

'Not the race of Dante': Southern Italians as Undesirable Americans

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Boston College

The Graduate School of Arts and Sciences

Department of History

“‘NOT THE RACE OF DANTE’: SOUTHERN ITALIANS AS
UNDESIRABLE AMERICANS”

A Doctoral Dissertation

by

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‘NOT THE RACE OF DANTE’: SOUTHERN ITALIANS
AS UNDESIRABLE AMERICANS

Michael John Mezzano Jr.

Dissertation Abstract

Main Advisor: James M. O’Toole, Ph.D.

This dissertation argues that the movement to restrict European immigration to the United States in the early 1900s was critically supported by a set of ideas that the dissertation refers to as “classic racialism.” Derived from several intellectual traditions – such as anthropology, biology, criminology, eugenics and zoology – classic racialism posited that differences in human population groups were biologically determined and hereditary, and because of this fact, American nativists held that the “new” immigration to the United States had to be curtailed in order to save the American Anglo-Saxon racial stock. The dissertation uses Italian immigration to the United States as a case study for understanding the fluidity of racial and biological thought. While classic racialism played a key role in supporting nativists’ calls for immigration restriction, advances in methods of scientific research were revolutionizing the fields of biology, genetics and anthropology. Research in these fields cast doubts on the veracity of intellectual claims made by classic racialists, which were increasingly untenable in the light of advancing scientific knowledge. The tensions between these competing intellectual paradigms of classic racialism and modern experimentalism in the late nineteenth- and early twentieth-centuries reveal the esoteric nature of scientific revolutions, in that the uncertainty and complexity of the developing biological and genetic sciences kept knowledge of scientific advances in these fields restricted to a narrow audience of professional scientists

and academics. While modern experimental biology raised significant scientific doubts about the principles of classic racialism, it was the latter that influenced American immigration policy in the 1920s because of classic racialism's simplicity and the broad public recognition that "like produces like."

Table of Contents

<i>Table of Contents</i>	<i>i</i>
<i>Acknowledgements</i>	<i>ii</i>
<i>Introduction</i>	<i>1</i>
The Heredity of the Genius and the Body of the Criminal	17
Part I: The Progressive Impulse of Restriction	61
<i>Chapter 1: The Problem of Immigration</i>	62
Biological Problems of Immigration	87
<i>Chapter 2: The Value of Scientific Cosmopolitanism</i>	109
The Problem of Heredity	122
<i>Chapter 3: Science, Prejudice and Politics: The United States Immigration Commission</i>	154
<i>Chapter 4: The Immutable Basis of Science?</i>	206
The Political Uses of Science	253
Part II: The Uncertainties of Scientific Certainty	287
<i>Chapter 5: The Inheritance of Intelligence</i>	288
Developing the Congressional Quota System	322
<i>Chapter 6: “There is no science of propaganda”</i>	391
“All the respectable people are besmirched by the contact”	405
“One of the most useful citizens in the United States”	426
<i>Chapter 7: The Battle for National Origins</i>	441
The Importance of Blood	462
<i>Chapter 8: The Futility of Criticism</i>	495
<i>Conclusion: “Eternal vigilance is the price of safety”</i>	544
<i>Bibliography:</i>	563

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Introduction

In early April 1897, Herbert Spencer Jennings, a Harvard Ph.D. candidate and recipient of a Parker Traveling Fellowship, wrote to a friend his perceptions of Naples. It was, he explained, a “fearful contrast” to Florence and Venice: “The people are the most worthless, dirty, indecent, vicious lot of creatures it is possible to imagine. It really has a very depressing effect on one, the first few days he walks about the city.” He wrote similar thoughts to an intimate female friend, noting that the people in Naples, as opposed to those he had seen in Florence, “are almost like the Digger Indians—they are not even pretty, considered merely as little living things, like dogs or cats, as some of the lower class people in North Italy are— [in Naples] they seem stupid, brutalized and miserable.” But at the end of his letter to the woman who would later be his wife, he moderated his criticism. Jennings said that he feared he had been unduly harsh on the Neapolitans. Although they gave one a “dreadful impression,” the future Johns Hopkins Professor wrote, “I am in hope that I was partly wrong however about their not being happy in their worthless lazy way.” Jennings’s statements have significance beyond providing commentary on the qualities of Neapolitans. His perception of the difference between lower-class Neapolitans and northern Italians is significant for understanding the growing impetus toward restricting “new” immigrants to the United States. His lessons and experience at the Naples Zoological Station where he conducted biological research on single-celled organisms are critical for understanding a dramatic shift that occurred during his lifetime in the way that scientific knowledge was obtained. Finally, Jennings also

illustrates important trans-Atlantic intellectual connections between the United States and Europe in the period between the American Civil War and World War I.¹

This dissertation argues that the success of the American immigration restriction movement in 1929 depended heavily on what the historian Elazar Barkan has called a “restrictionist language and epistemology” that is called here “classic racialism.”² This loose amalgam of ideas was oriented around a simple,

¹ Herbert Spencer Jennings to Joseph Brennemann, 1 April 1897; Herbert Spencer Jennings to “Jess” (Mary Burridge), 18 April, 1897, both in Herbert Spencer Jennings Papers, American Philosophical Society Library, Philadelphia, PA, [hereafter Jennings Papers, APS].

² In the majority of the historiography of both eugenics and immigration restriction, the loose affiliation of ideas that I have termed “classic racialism” is most often referred to as “scientific racism.” Two essays in particular have influenced the use of my alternate term. The difference between “racism” and “racialism,” though seemingly slight, is a significant one. Racism implies an antipathy or action based upon an individual’s racial identity. I am more concerned with the formulation of the racial identity itself, that is, the formulation of ideas of group identity rather than the attitudes that grew from awareness of group difference. For example, the United States Immigration Commission believed it was possible to compile a definitive “Dictionary of Races or Peoples” that specifically and precisely listed the ethnological characteristics of various European population groups and their racial qualities. I aim to understand both how this racial taxonomy was achieved and how policy makers understood it as a precise set of ideas with specific intellectual content. “Racial” characteristics included physical qualities like head or skull shape, and mental or moral qualities like an inclination to crime or an inability to read. I am less interested in the attitudes that arose from those categories (although certainly that is an important part of this project as well). Historian Peggy Pascoe’s 1996 article employs the term “racialism” to “designate an ideological complex that other historians often describe with the terms ‘race’ or ‘racist.’” John Higham also uses racism and racialism as precise and distinct terms, talking of “racial science increasingly intermingled with racial nationalism” to describe the intellectual foundations of racism that were perceived to have important biological or hereditary sources along with tangible physical differences. Additionally, a term is needed to convey the specific scientific language that restrictionists and racial nativists employed to justify restriction, the “restrictionist language and epistemology,” which was heavily scientifically oriented. Elazar Barkan’s article studying Jennings’s criticisms of eugenics describes Jennings’s early awareness of his teacher Charles B. Davenport’s “metaphysical reductionism”: it was based upon biological concepts, but bent them into the flat and simple faith that like produces like. The basic argument that I am making, that the conditions and methods of conducting scientific research were changing, precludes using the term “scientific”—what “science” was and held to be true in this period was contested and undergoing reformulation, so it cannot be used. The term “classic” suffices to signify older methods of scientific investigation based on description, collection, and inference by analogy rather than experimentation. The term is employed in a similar fashion by the historian-scientist Elof Axel Carlson in *Mendel’s Legacy* to differentiate the experimental sciences that were replacing descriptive science. Keith Benson also describes American biological sciences experiencing the “gradual transformation... from its primary location in museum-oriented natural

biological determinism that was often expressed in the axiom that “like produces like.” While the modern experimental sciences of biology and anthropology proved this to be a dramatic and inaccurate oversimplification, government policy was shaped and justified by this simpler, more familiar set of ideas. The methods that Jennings absorbed in Naples, part of a larger epistemic shift toward modern experimental and laboratory sciences, ultimately showed classic racist principles to be in error. Like did *not* always produce like. But classic racism still remained a central rationale for excluding immigrants to the United States. Why was the more accurate method not used? Part of the reason is the uncertainty and the complexity of the new methods. The dissertation therefore also examines the inability of professional academics to successfully debunk the restrictionist language and epistemology of classic racism and how “revolutions” in scientific knowledge are more accurately “evolutionary,” in that their widespread acceptance often takes time to diffuse from acceptance in professional communities to popular understanding. Support from the eugenic community is an essential component for understanding the passage of the Johnson-Reed Act in

history to its eventual setting within academic and research institutions.” “Biological determinism” is inadequate because some of the researchers recognized that environment did play at least some role in shaping the development of organisms. By using the term “classic racism” then, I distinguish between this set of older ideas and language and the more recognizable “modern” experimentalism of laboratory sciences. Peggy Pascoe, “Miscegenation Law, Court Cases, and Ideologies of ‘Race’ in Twentieth-Century America” *The Journal of American History* v. 83, n. 1 (June 1996), 44-69, 47-8; John Higham, *Strangers in the Land: Patterns of American Nativism, 1860-1925* (New Brunswick, NJ: Rutgers University Press, 1992; [orig. 1955]), 133; 336. Elazar Barkan, “Reevaluating Progressive Eugenics: Herbert Spencer Jennings and the 1924 Immigration Legislation” *Journal of the History of Biology* v. 24, n. 1 (Spring 1991), 91-112, 103 for restrictionist language, 93 for Davenport; Elof Axel Carlson, *Mendel’s Legacy: The Origin of Classical Genetics* (Cold Spring Harbor, NY: Cold Spring Harbor Press, 2004), chapter 1; Keith R. Benson, “From Museum Research to Laboratory Research: The Transformation of Natural History in Academic Biology” in Ronald Rainger, Keith Benson, Jane Maienschein, eds., *The American Development of Biology* (Philadelphia: University of Pennsylvania Press, 1988), 50.

1924 (the “National Origins Act”). By considering the history of science, rather than just the history of eugenics, this dissertation shows that the state of knowledge in biology and genetics after World War I had advanced to such an extent that eugenic theories were regarded by professionals as scientifically untenable. This not only led many “positive” eugenicists to distance themselves from the movement, but also slowly eroded any scientific respectability for the movement. When, as Jane Maienschein suggests, the basis of biological knowledge had shifted to an experimental and laboratory phase, which required advanced techniques, equipment, and institutional support, “negative” eugenicists had little legitimate scientific authority.³

Although Jennings was an American scientist, born in Tonica, Illinois, European observers had already articulated the thoughts that he communicated on the dirty and brutalized Neapolitan population. Creuze de Lesser, a French tourist to the south of Italy in 1806 had observed, “Europe ends at Naples, and ends badly. Calabria, Sicily, and all the rest belong to Africa.” After the failed Italian

³ As the history of eugenics shows, there were two strands of eugenic thought: positive eugenics and negative eugenics. The former focused on the improvement of man’s innate capacity—making all people “well-born.” Negative eugenics was more intense and racist, designed to prevent the “dysgenic” or “cacogenic” from reproducing, and increasing the propagation of the “aristogenic.” Many of the classic racialists and eugenic-minded restrictionists advocated negative eugenics, which encompassed forced sterilization, immigration restriction, and segregation of degenerates. As genetic science became increasingly sophisticated, however, many of the positive eugenicists—Jennings and Raymond Pearl, for instance—distanced themselves, or in Jennings’ case, became highly critical of the sloppy and racialized theories of negative eugenicists. Jane Maienschein, *Transforming Traditions in American Biology* (Baltimore: Johns Hopkins University Press, 1991). Also Kenneth Ludmerer, *Genetics and American Society: A Historical Appraisal* (Baltimore: Johns Hopkins University Press, 1972); Daniel J. Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (New York: Alfred A. Knopf, 1985); Mark H. Haller, *Eugenics: Hereditarian Attitudes in American Thought* (New Brunswick: Rutgers University Press, 1984 [orig. 1963]).

revolutions in 1848, the Prime Minister of the Kingdom of Sardinia, Massimo d’Azeglio, commented “In every way the fusion with the Neapolitan makes me afraid; it is like putting yourself to bed with a smallpox patient.” In August 1861, Carlo Farini, the chief administrator of southern Italy for Prime Minister Camillo Cavour after unification, wrote his superior that the south “is not Italy! This is Africa: compared to these peasants the Bedouins are the pinnacle of civilization.” Jennings was merely one in a long line of observers who noticed the “difference” in the population of the Italian south.⁴ This perceived difference, largely believed to be biological and hereditary, followed southern Italians as they emigrated to the United States. It would become a key justification for excluding them and other “inferior” southern and eastern Europeans from admission to the United States.

During his time in Naples, Jennings continued to develop his proficiency with experimental research methods. But his scientific training was important not only for the history of biology but for immigration restriction as well. Jennings was the only professional biologist to contest Harry H. Laughlin’s testimony before the 1924 House Committee on Immigration and Naturalization. Laughlin was the “Expert Eugenical Agent” whom the House Committee commissioned to research biological aspects of immigration. Laughlin described his research project as “a study in comparative degeneracy. It is one of the several

⁴ Creuze de Lesser, “Voyage en Italie et en Sicile,” (1806), cited in Gabriella Gribaudi, “Images from the *Mezzogiorno* as seen by Insiders and Outsiders” in Robert Lumley and Jonathan Morris, eds. *The New History of the Italian South: The Mezzogiorno Revisited* (Exeter, UK: University of Exeter Press, 1997), 87; D’Azeglio quoted in Don Doyle, *Nations Divided: America, Italy, and the Southern Question* (Athens, GA.: University of Georgia Press, 2002), 71-2; Carlo Farini to Camillo Cavour, from *Carteggi di Camillo Cavour, La Liberazione del Mezzogiorno e la formazione del regno d’Italia*, III, 208, cited in Nelson Moe, *The View from Vesuvius: Italian Culture and the Southern Question* (Berkeley, CA: University of California Press, 2002).

investigations into the biological or eugenical aspects of immigration which I am conducting under the immediate auspices of this committee.” He continued, “The idea of course is to use such data to gauge the relative soundness and stability of the different racial and nativity groups in the United States, which gauge, in turn, would constitute a measure of their relative long-time value to the Nation, especially, when viewed in the light of *the inborn quality of future generations*. More specifically, it is a measure of the relative soundness of recent and older immigrant stocks.” He devised statistical studies that purported to demonstrate that the southern and eastern immigrants were exceeding what should be their quota of socially inadequate and undesirable classes.⁵ Jennings appeared to try to correct the Committee’s approval of Laughlin’s incorrect biological determinism. Heredity was a highly complex phenomenon, Jennings argued, and heredity, the environment and culture worked together to shape human development. Where Laughlin wanted to argue for precise, biological “racial” characteristics, which then made ranking superior and inferior racial stocks easy, Jennings argued that nature and nurture were two essential, interdependent components of human development.⁶ Jennings believed this because he had spent almost thirty years

⁵ Harry H. Laughlin, “Analysis of America’s Modern Melting Pot” Hearings before House Committee on Immigration and Naturalization, 67th Congress, 3rd Session, 21 November 1922 (Washington, D.C.: Government Printing Office, 1923). 731, emphasis added.

⁶ Statement of Herbert Spencer Jennings, “Restriction of Immigration” Hearings before the Committee on Immigration and Naturalization, House, 68th Congress, 1st Session, Dec. 26, 27, 31, 1923; Jan. 2-5, 7, 8, 10, 19, 1924 (Washington, D.C.: Government Printing Office, 1924), 510-13. On Jennings and Laughlin, crucial are Bentley Glass, “Geneticists Embattled: Their Stand Against Rampant Eugenics and Racism in America during the 1920s and 1930s” *Proceedings of the American Philosophical Society* v. 130 (1986), 130-54; Frances Janet Hassencahl, “Harry H. Laughlin, ‘Expert Eugenics Agent’ for the House Committee on Immigration and Naturalization” (Ph.D. diss.: Case Western Reserve University, 1970); Elazar Barkan, *The Retreat of Scientific*

after his Naples trip practicing the methods of laboratory experimentation he learned there. Jennings was part of a larger epistemic shift within the natural sciences in the West at the turn of the twentieth century. New ways of knowing were transforming the pursuit of scientific knowledge, as more modern laboratory, chemical and microscopic methods gradually displaced the older, traditional methods of what may be called a natural history or philosophical approach to science.⁷

Professional academics in the United States at the end of the nineteenth century were experiencing a change in accepted methods, particularly in the fields of the natural sciences. “Naturalists” were becoming zoologists, cytologists, and biologists, examining the biological development of creatures in laboratory settings, performing experiments and closely observing their development. Philip Pauly describes the emergent “division of labor between the field [researcher] and the naturalist’s ‘closet,’ combined with the increasing emphasis on painstaking microscopic observation,” as indicative of a new urban, professional class of

Racism: Changing Concepts of Race in Britain and the United States Between the World Wars (Cambridge: Cambridge University Press, 1992); *Idem.*, “Reevaluating Progressive Eugenics: Herbert Spencer Jennings and the 1924 Immigration Legislation” *Journal of the History of Biology* v. 24, n. 1 (Spring 1991), 91-112; Michael Mezzano, “The Progressive Origins of Eugenics Critics: Raymond Pearl, Herbert Spencer Jennings and the Defense of Scientific Inquiry” *Journal of the Gilded Age and Progressive Era* v. 4, n. 1 (January 2005), 83-97.

⁷ Philip Rehbock describes the “philosophical naturalists” in Britain who in the early nineteenth century –Darwin’s book was only “the climax of a half a century of searching for the philosophical foundations of the new science, ‘biology’” – set out to discover natural laws of development and to understand the morphology and distribution of organisms in space and time. Philip Rehbock, *The Philosophical Naturalists: Themes in Early Nineteenth-Century British Biology* (Madison: University of Wisconsin Press, 1983), 6, 9.

biologists.⁸ This “transformation” in scientific knowledge, as Jane Maienschein terms it, is central for the dissertation’s argument that the more easily comprehended, simple hereditarian language of biological determinism was easier to communicate to politicians and laypersons; while it was widely regarded with

⁸ Philip Pauly, “Summer Resort and Scientific Discipline: Woods Hole and the Structure of American Biology” in Rainger, et al, eds., *The American Development of Biology*, 126. I argue that just as the scientific language in the United States was undergoing a significant epistemological shift that precludes talking about a monolithic community of researchers in the United States, these shifts were also occurring in the main European communities as well. Thus, Italy and Great Britain both had their divergent camps. On Italy’s competing scientific camps, see Steven C. Hughes, *Crime, Disorder and the Risorgimento: The Politics of Policing in Bologna* (Cambridge: Cambridge University Press 1994); Silvana Patriarca, *Numbers and Nationhood: Writing Statistics in Nineteenth-Century Italy* (Cambridge: Cambridge University Press, 1996); Mary Gibson, “Biology or Environment? Race and Southern ‘Deviancy’ in the Writings of Italian Criminologists, 1880-1920”; Nelson Moe, “The Emergence of the Southern Question in Villari, Franchetti, and Sonnino”; Jane Schneider “The Dynamics of Neo-orientalism in Italy (1848-1995)” all in Jane Schneider, ed. *Italy’s “Southern Question”: Orientalism in One Country* (New York: Berg, 1998); Gabriella Gribaudi, “Images of the South: The *Mezzogiorno* as seen by Insiders and Outsiders” in Lumley and Morris, eds. *The New History of the Italian South*; Aaron Gillette, *Racial Theories in Fascist Italy* (New York: Routledge, 2002). On Britain see Hamish Spencer and Diane B. Paul, “The Failure of a Scientific Critique: David Heron, Karl Pearson and Mendelian Genetics” *The British Journal for the History of Science* v. 31 (1998), 441-52; Edward J. Larson, “The Rhetoric of Eugenics: Expert Authority and the Mental Deficiency Bill” *The British Journal for the History of Science [Br J Hist Sci]* v. 24, n. 1 (March 1991), 45-60; David Barker, “The Biology of Stupidity: Genetics, Eugenics and Mental Deficiency in the Inter-War Years” *Br J Hist Sci* v. 22, n. 3 (September 1989), 347-75. In terms of intellectual or epistemic change in methods of inquiry or general, guiding theoretical principles, I am guided by Thomas Kuhn’s insightful analysis of “scientific revolutions.” Kuhn does not suggest that in an instant, one scientific paradigm or theory displaces another as *the* reigning paradigm. Rather, as crises in the “normal science” paradigm grow, new methods or ideas are introduced, eventually generating competing, incommensurable theories. Ultimately it is individual practitioners – very often members of a younger generation – who undergo a “conversion experience” or “*gestalt-switch*” that compels them to embrace the new paradigm. This difficulty of the “conversion” process – the physicist Max Planck declared that “a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.” – is one of the reasons that change in scientific knowledge is gradual, a process of “speciation” as Kuhn later suggested. Max Planck, *Scientific Autobiography* quoted in Thomas S. Kuhn, *The Structure of Scientific Revolutions* 2nd ed, enlarged (Chicago: University of Chicago Press, 1970 [orig. 1962]), on 151; crises on 77-91; gestalt switch 121; incommensurability from 150; generations 144; conversion experience 151-59. Speciation is from Thomas Kuhn, *The Road Since Structure: Philosophical Essays, 1970-1993, with an Autobiographical Interview* James Conant and John Haugeland, eds. (Chicago: University of Chicago Press, 2000).

skepticism by the professional scientific community, the general acceptance of classic racialism by the public remained undiminished.⁹

Writing from the Naples Zoological Station, Jennings finally highlights the trans-Atlantic nature of the scientific communities in this period. Participating in international meetings of their professionalizing disciplines, exchanging data and publications across the Atlantic, and traveling to the best European institutions to train, there was a sizable international community of researchers who contributed to advancing the knowledge of nature.¹⁰ Classic racialism, so central to the world-view of what historian John Higham calls America's "racial nativists" was ironically partly European in origin. Two strands, one from

⁹ Maienschein, *Transforming Traditions in American Biology*. See also Pascoe, "Miscegenation Law, Court Cases, and Ideologies of 'Race'"; Sharon Kingsland, "The Battling Botanist: Daniel Trembly MacDougal, Mutation Theory, and the Rise of Experimental Evolutionary Biological in America, 1900-1912" *Isis* v. 82, n. 3 (September 1991), 479-509; Garland Allen, "Naturalists and Experimentalists: The Genotype and the Phenotype" *Studies in the History of Biology* v. 3 (1979), 179-209; Philip Pauly, *Controlling Life: Jacques Loeb and the Engineering Ideal in Biology* (New York: Oxford University Press, 1987); Jan Sapp, *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics* (New York: Oxford University Press, 1987); William Provine, *The Origins of Theoretical Population Genetics* (Chicago: University of Chicago Press, 1971).

¹⁰ On the Naples Zoological Station, see (n.a.) "The Naples Zoological Station. I." *Science* v. 1, n. 17 (1 June, 1883), 479-81; Emily A. Nunn, "The Naples Zoological Station. II." *Science* v. 1, n. 18 (8 June, 1883), 507-10; C. O. Whitman, "The Advantages of Study at the Naples Zoological Station" *Science* v. 2, n. 25 (27 July, 1883), 93-97; Henry F. Osborn, "American Students at the Naples Zoölogical Station" *Science* n.s. v. 1, n. 9 (1 March, 1895), 238-39; (n.a.) "The Naples Zoological Station" *Science* n.s. v. 9, n. 225 (21 April, 1899), 596-97. On the nature of the change in biology and the natural sciences, see Maienschein, *Transforming Traditions in American Biology*; Keith Benson, "The Naples Stazione Zoologica and Its Impact on the Emergence of American Marine Biology" *Journal of the History of Biology [Jl H Bio]*, v. 21, n. 2 (June 1988), 331-41; *idem.*, "American Morphology in the Late Nineteenth Century: The Biology Department at Johns Hopkins University" *Jl H Bio*, v. 18, n. 2 (June 1985), 163-205; Philip Pauly, "The Appearance of Academic Biology in Late Nineteenth Century America" *Jl H Bio* v. 17, n. 3 (September 1984), 369-97; Robert Kohler, *All Creatures: Naturalists, Collectors, and Biodiversity, 1850-1950* (Princeton, NJ: Princeton University Press, 2006); Philip Pauly, "Summer Resort and Scientific Discipline: Woods Hole and the Structure of American Biology"; Keith R. Benson, "From Museum Research to Laboratory Research: The Transformation of Natural History in Academic Biology" and Toby A. Appel, "Organizing Biology: The American Society of Naturalists and its 'Affiliated Societies,' 1883-1923" all in Rainger et al, eds., *The American Development of Biology*.

England and one from Italy, had a powerful influence in the late nineteenth century in the direction American restrictionists would take nativism. Yet at the same time that classic racialism was so powerful, it was also methodologically wrong. This dissertation also examines a shift in scientific language, a scientific revolution – what Thomas Kuhn called “those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one” – that disproved classic racialism.¹¹ It finds that while proper “scientific” research was now conducted in a more modern, experimental method, the complexity of modern experimental genetics and biology was too esoteric to displace classic racialism from the public’s mind. The halting, uncertain answers of modern biological science as they gradually developed were too complex as a base for policy or public understanding.

Two of the foundations of the biological determinism of Laughlin and other American classic racialists came from an English relative of Charles Darwin, the Victorian polymath Sir Francis Galton, and an Italian physician, Cesare Lombroso. Galton and Lombroso contributed theories that certain characteristics of the human organism were determined biologically through heredity or evolutionary atavism. Their ideas diffused throughout the “world of common referents,” in Daniel Rodgers’s phrase, via magazine and journal articles, personal correspondence, books and study, and international meetings.¹²

¹¹ Higham, *Strangers in the Land*, 133-36; Kuhn, *Scientific Revolutions*, 92.

¹² Daniel Rodgers, *Atlantic Crossings: Social Politics in a Progressive Age* (Cambridge, MA: The Belknap Press of Harvard University Press, 1998), 3. Rodgers’s book was a key inspiration to
Introduction

The “transformation” occurring in the way science was conducted in this period, personified by an image of Jennings standing over a microscope at a table in the Naples Zoological Station, surrounded by aquaria with live specimens from the waters around Italy’s largest city, would eventually establish experimental, laboratory research as the foundation for all modern biological knowledge. A student and friend of Jennings, Raymond Pearl, discussed “Trends in Modern Biology” in 1922, in which he described how this modern form of an experimental research methodology was fundamental because “the essential problems of biology are questions of dynamic relationships and not of static phenomenon.” Because biology was dynamic, it had to be observed and studied experimentally. Pearl appreciated the difficulty in displacing classic racialism’s older language, describing “the seductive lure of certain rules by which the game was played, which rules (such as ontogenetic recapitulation of phylogeny, certain aspects of homology, etc.) were mistakenly supposed to be natural laws, whereas in point of fact, at best, they were only imperfect expressions of certain inherent necessities of the philosophic principle of organization, and at the worst just plain buncombe.”¹³ Biological principles could not be inferred from what Pearl explained was “purely static phenomena (the intimate structure of the body)” via description or classification, or the other methods of natural history.

search for some of the roots of American ideas in this period in the “largely forgotten world of transnational borrowings and imitation.” 7.

¹³ Raymond Pearl, “Trends in Modern Biology” *Science* n.s. v. 56, n. 1456 (24 November, 1922), 581-92; “dynamic” from 585; “intimate structure” from 584.

This scientific language of experimentalism was ultimately restricted to a small number of its professional practitioners. It was a complex, sophisticated method that functioned best in dedicated research institutions or academic laboratories, and it was hesitant of prematurely declaring veracities and truths. This uncertainty, complexity and esotericism would have fateful implications, which crystallized when American immigration policy reversed course in 1924 with a dramatic limitation on the number of European emigrants the nation would admit. The movement to restrict immigration, active for over thirty years, was significantly supported and legitimized by a language that declared certain “races” of Europeans to be biologically and hereditarily inferior, but which experimental research had proven to be flawed.

The language of classic racialism was based heavily on the language of eugenics, a theory that human beings could direct human evolution through judicious and fertile marriages of talented and healthy stock, while limiting the reproduction of inferior or degenerate branches of the human stock. In turn, eugenics was biologically determinist, based on the belief that an individual’s basic biological composition – sealed, somehow, at the moment of conception – determined irrevocably that individual’s development. A third basic component of classic racialism was the belief in fixed, immutable, unchanging or static population groups that passed on their racial qualities – the good and the bad – to their progeny with no change or moderation. It believed, in short, “that all men are created *bound* by their protoplasmic makeup and *unequal* in their powers and

responsibilities.”¹⁴ The professional, academic and institutional scientific community had, during the 1920s, largely disproved this simplistic approach to heredity and human development, but was unable to influence Congressional control of immigration to the United States.

It was not only biologists that combated this misperception of heredity and unchanging racial types. Cultural and physical anthropologists like Franz Boas and Ales Hrdlicka, along with biologists like Jennings and Pearl, criticized the “bad science” of the eugenicists, although they too were ineffective at stopping Congress from passing immigration quotas on the basis of racial identity in 1924. Despite their best efforts to oppose the simplistic eugenic theories—Jennings wrote an article in *Scientific Monthly* in 1924 in which he insisted that “no single thing that the organism does depends alone on heredity or alone on environment; always both have to be taken into account”—they were unable to counter the easily comprehensible scientific “laws” the eugenicists insisted on.¹⁵ Though academics like Boas and Jennings continually argued that culture and environment were as essential to understanding human development as biology and heredity, heredity was more easily “proven.” Although scientific knowledge of human development in the period between 1860 and 1930 ranged from narrow

¹⁴ Charles Benedict Davenport, *Heredity in Relation to Eugenics* Introd. Charles Rosenberg (New York: Arno Press, 1972 [orig. 1911]), iv, emphasis in original.

¹⁵ Herbert Spencer Jennings, “Heredity and Environment” *The Scientific Monthly* v. 19, n. 3 (Sep. 1924), 225-38; 225. Jennings also pointed out that “We are warned not to admit to America certain peoples not differing from ourselves, on the basis of the resounding assertion that biology informs us that the environment can bring out nothing whatever but the hereditary characters. Such an assertion is perfectly empty and idle....” 237.

biological determinism to theories of environmental and biological interdependence, simple determinism was easier to communicate to—and thereby persuade—political leaders and the general public. As Jennings lamented, “to clear and definite questions nature declines to give clear, definite, and generally valid answers. The evidence, so far as it goes, is that in some degree or in some organisms, both doctrines are correct.”¹⁶ Classic racialism suggested that it did have clear and definite answers to these questions.¹⁷

The definitive work on American attitudes toward immigration in this era remains John Higham’s *Strangers in the Land* (1955), which suggests that three distinct types of nativism circulated through the United States in the years between the Civil War and 1925: anti-radical, anti-Catholic, and racial. Each of these iterations of xenophobia was driven by political, social or economic crises.¹⁸ Higham argues that the outbreak of World War I and the attendant desire for “100 Per Cent Americanism”—a uniform nationalist ideology—ultimately obliged policy makers to exclude the immigration of Europeans who were perceived as racially different, and thus unable to assimilate to American ideals and identity.¹⁹

¹⁶ Herbert Spencer Jennings, “The Inheritance of Acquired Characters” *The Forum* v. 76 (Nov. 1926), 702-11; 707.

¹⁷ For instance, consult John S. Haller, *Outcasts from Evolution: Scientific Attitudes of Racial Inferiority, 1859-1900* (Urbana: University of Illinois Press, 1971) for an overview.

¹⁸ Higham, *Strangers in the Land*.

¹⁹ *Ibid.* Some notable contributions to this field are Reginald Horsman, *Race and Manifest Destiny: The Origins of American Anglo-Saxonism* (Cambridge, MA: Harvard University Press, 1981); Alexander Saxton, *The Rise and Fall of the White Republic: Class, Politics and Mass Culture in Nineteenth Century America* (New York: Verso Press, 1990); Matthew Frye Jacobson, *Whiteness of a Different Color: European Immigrants and the Alchemy of Race* (Cambridge, MA: Harvard University Press, 1998); Desmond King, *Making Americans: Immigration, Race, and the Origins of the Diverse Democracy* (Cambridge, MA: Harvard University Press, 2000); Thomas A.

Higham's narrative, however, is insufficiently attentive to developments in scientific language. Although he correctly places great emphasis on the complexities of "racial nativism," his depiction of science leaves the reader with the impression that, like nativism itself, scientific knowledge waxed and waned along with nationalist emotions and sentiments. The racial nativism in Higham's account is based in large part on cultural descriptions and stereotypes; Higham recounts Laughlin's "expert" eugenics testimony before Congress, but does not record the academic witnesses who strongly criticized the scientific content of Laughlin's testimony. He examines the "scientific race thinking" that was the critical component of racial nativism without recognizing that there were professional critics who dismissed it as non-scientific.²⁰ Higham himself is aware of these shortcomings and in a new afterword to the 1992 edition admits that his chapter on Nordic racism in the nineteenth- and twentieth-centuries was "only partly successful in analyzing the scientific ideas that affected race thinking."²¹ This dissertation will examine these scientific ideas regarding race more closely

Guglielmo, *White on Arrival: Italians, Race, Color, and Power in Chicago, 1890-1945* (Oxford: Oxford University Press, 2003); Mae Ngai, *Impossible Subjects: Illegal Aliens and the Making of Modern America* (Princeton, NJ: Princeton University Press, 2004).

²⁰ Higham, *Strangers in the Land*, 313-14 for Laughlin; 273ff for "race thinking."

²¹ *Ibid.*, 336. See also John Higham, "Instead of a Sequel, or How I Lost My Subject," *Reviews in American History* v. 28, n. 2 (June 2000), 327-339. Higham explained that to keep American nationalism at the center of the narrative "I treated the ethnocentric attitudes displayed in stereotypes as a relatively stable precondition for the much more volatile passions of nativism." Higham *Strangers in the Land*, 343. Two important examples of the fragile state of American racial ideologies are presented in Kristin Hoganson, *Fighting For American Manhood: How Gender Politics Provoked the Spanish-American War* (New Haven: Yale University Press, 1998); Gail Bederman, *Manliness and Civilization: A Cultural History of Race and Gender in the United States, 1880-1917* (Chicago: University of Chicago Press, 1995).

by looking at their formulation and the professional academic critics who had a different, more modern understanding of “scientific” knowledge and research.

Other historians of immigration restriction have examined the prominence of eugenic theorists in the restrictionist movement. While the eugenics movement within these accounts is frequently (and rightly) branded as pseudo-scientific, that it did have scientific credentials in an era dedicated to scientific management, efficiency, and the central role of the “expert” in a complex society is significant.²² The founder of eugenics, Francis Galton, and his student Karl Pearson, were talented theorists and mathematicians. Eugenics advanced the idea that gifted people of superior stock, through careful use of their desirable hereditary material, could increase their number in the population. It was also important to limit the reproduction of the unfit and undesirables. And eugenicists took that “good” or “bad” qualities would be passed on to future generations

²² E.g. John S. Haller, *Outcasts from Evolution: Scientific Attitudes of Racial Inferiority, 1859-1900* (Urbana: University of Illinois Press, 1971); Alan M. Kraut, “Southern Italian Immigration to the United States at the Turn of the Century and the Perennial Problem of Medicalised Prejudice” in Lara Marks, Michael Worboys, eds. *Migrants, Minorities and Health: Historical and Contemporary Studies* (NY: Routledge, 1997), 228-50; Alan M. Kraut, *Silent Travelers: Germs, Genes, and the “Immigrant Menace”* (New York: Basic Books, 1994); Amy Lauren Fairchild, “Science at the Borders: Immigrant Medical Inspection and Defense of the Nation, 1891 to 1930” (Ph.D. diss. Columbia University, 1997); idem., *Science at the Borders: Immigrant Medical Inspection and the Shaping of the Modern Industrial Labor Force* (Baltimore: Johns Hopkins University Press, 2003); Leila Zenderland, *Measuring Minds: Herbert Henry Goddard and the Origins of American Intelligence Testing* (Cambridge: Cambridge University Press, 1998); Nancy Leys Stepan, *‘The Hour of Eugenics’: Race, Gender, and Nation in Latin America* (Ithaca: Cornell University Press, 1991); Charles Rosenberg, *No Other Gods: On Science and American Social Thought* (Baltimore: Johns Hopkins University Press, rev. ed. 1997 [orig. 1976]); Robert F. Zeidel, *Immigrants, Progressives, and Exclusion Politics: The Dillingham Commission, 1900-1927* (Dekalb, IL: Northern Illinois University Press 2004); David M. Reimers, *Unwelcome Strangers: American Identity and the Turn Against Immigration* (NY: Columbia University Press, 1998); John Higham, *Send These to Me: Immigrants in Urban America* rev. ed. (Baltimore: Johns Hopkins University Press, 1984 [orig. 1975]); Barbara M. Solomon, *Ancestors and Immigrants: A Changing New England Tradition* (Cambridge: Harvard University Press, 1956); Dale Knobel, *“America for the Americans”: The Nativist Movement in the United States* (New York: Twayne Publishers, 1996).

unchanged, was an established fact. Men were born unequal in gifts, and so they and their progeny would remain.²³

Galton's studies of hereditary characteristics by family pedigree were an important contribution to classic racialism. Italian criminal anthropologists, a field pioneered by Cesare Lombroso and then Giuseppe Sergi, also had influence on American immigration restriction. The search for the origins of the scientific language of classic racialism, which was so central to American immigration restriction, begins not in America but in Europe. Within Britain and the young Kingdom of Italy, ideas emerged that would heavily influence the classic racialism that nurtured American xenophobia.

The Heredity of the Genius and the Body of the Criminal

The publication in 1869 of *Hereditary Genius* provides an essential beginning point for the intellectual theories that would underpin and support the American immigration restriction movement. In the book, the English gentleman Francis Galton began laying the groundwork for what would become the scientific language of "eugenics." Galton's scientific researches, like Cesare

²³ Richard Hofstadter, *Social Darwinism in American Thought* (New York: George Braziller, Inc., rev. ed., 1955). Diane Paul, "Eugenics and the Left" *Journal of the History of Ideas* 45 (October-December, 1984), 567-90; Donald Pickens, *Eugenics and the Progressives* (Nashville, TN: Vanderbilt University Press, 1966); Mark H. Haller, *Eugenics: Hereditarian Attitudes in American Thought* (New Brunswick: Rutgers University Press, 1984 [orig. 1963]); Barry Mehler, "A History of the American Eugenics Society, 1921-1940: (Ph.D. dissertation, University of Illinois-Urbana, 1988); Daniel J. Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (New York: Alfred A. Knopf, 1985); Robert C. Bannister, *Social Darwinism: Science and Myth in Anglo-American Thought* (Philadelphia: Temple University Press, 1979); Thomas Gossett, *Race: The History of an Idea in America* 2nd ed. (New York: Oxford University Press, 1997; [1963]); Barkan, *The Retreat of Scientific Racism*; Kenneth Ludmerer, *Genetics and American Society: A Historical Appraisal* (Baltimore: Johns Hopkins University Press, 1972); Elof Axel Carlson, *The Unfit: The History of a Bad Idea* (New York: Cold Spring Harbor Laboratory Press, 2001).

Lombroso's, mark the beginning of a shift in methods that would ultimately lead to many of his scientific theories being discredited. In the preceding centuries in Western civilization, what one might call "gentlemen scientists," had made extraordinary contributions to the advance of scientific knowledge.²⁴ The generations that followed Galton were of a different breed, carefully trained in research universities on narrow topics to enable the mastery of both method and content. Like Jennings in Naples, these "new" scientists would rely upon their professional training and credentials rather than their social position to prove the value of their scientific work. Their academic positions at research universities and the countless hours spent in laboratories conducting careful experimental research marked the new generation as fundamentally different from Galton's in methodological approach.

Galton made two absolutely crucial contributions that would flower in the immigration restriction movement in the United States: the fields of eugenics and pedigree analysis. After his death, Galton bequeathed a significant portion of his wealth (£45,000) to the University of London to endow a chair in Eugenics and support the Eugenics Laboratory (at present the Galton Laboratory of the

²⁴ Two critical books that advance the importance of social position, trust, and knowledge are Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994) and Peter Burke, *A Social History of Knowledge: From Gutenberg to Diderot* (Cambridge, MA: Polity Press, 2000). Shapin describes the early community of scientific practitioners: "all the individual can do is offer claims, with evidence, arguments, and inducements, to the community for its assessment. Knowledge is the result of the community's evaluations and actions, and it is entrenched through the integration of claims about the world into the community's institutionalized behavior. Since the acts of knowledge-making and knowledge-protecting capture so much of communal life, communities may be effectively described through their economies of truth." By contrast, "Modern epistemology has systematically argued that legitimate knowledge is defined precisely by its rejection of trust: if we know something on basis of trust, we do not possess genuine knowledge." Shapin, *Social History of Truth*, 6, 16.

Department of Human Genetics and Biometry) that he had previously created. Galton had also organized a Eugenics Record Office in 1904 (identical in name to that founded by Charles B. Davenport at Cold Spring Harbor, New York in 1910) to facilitate the development of eugenics in Britain and to assess and organize the pedigree data that provided the evidence in favor of eugenic advancement.²⁵

Hereditary Genius represented Galton's first attempt to analyze the inheritance of specific qualities within family lines, what is also known as "pedigree analysis." The book analyzed biographical and genealogical data of men of "high reputation" that Galton had gathered from dictionaries of biography, encyclopedias and the London *Times* obituary index of 1868. His interest in the inheritance of certain mental characteristics emerged out of an earlier ethnological

²⁵ D. W. Forrest, *Francis Galton: The Life and Work of a Victorian Genius* (London: Elek Books, 1974); Michael Bulmer, *Francis Galton: Pioneer of Heredity and Biometry* (Baltimore: Johns Hopkins University Press, 2003); Nicholas Wright Gillham, *A Life of Sir Francis Galton: From African Exploration to the Birth of Eugenics* (New York: Oxford University Press, 2001). The essential starting point for any understanding of Galton's work and life is Karl Pearson, *The Life, Letters and Labours of Francis Galton* 3 vols. (London: Cambridge University Press, 1914, 1924, 1930). Pearson, Galton's protégé, was named the first Galton Professor of Eugenics at University College London by the terms of Galton's bequest. Galton's biographers note Galton's quintessential "Victorian" identity as well as his scientific achievements, and it is as a Victorian that Galton's position as a scientific pioneer becomes important. Galton was a polymath, with wide-ranging interests: his economic security enabled him to conduct investigations and his intellectual ability – Louis Terman, one of the chief American proponents of intelligence testing later pegged Galton's Intelligence Quotient at 200 based on a letter Francis wrote to his sister Adele when Galton was four – led him to not only pioneer the field of eugenics, but also the use of fingerprints in law enforcement, statistical theories of inheritance, and contribute to geographical knowledge of Africa. Gillham describes that the purpose of the Galton Laboratory was "to permit highly trained experts to gather and analyze masses of data pertinent to eugenics." Gillham, *A Life of Sir Francis Galton*, 342. See also 330-357 for an assessment of Galton's influence on eugenics worldwide. Garland Allen, one of the United States's best historians of eugenics and genetics, has an excellent article on the American version of Galton's lab, "The Eugenics Record Office at Cold Spring Harbor, 1910-1940: An Essay in Institutional History" *Osiris* 2nd series, v. 2 (1986), 225-64. By the time the American Eugenics Record Office was closed by the Carnegie Institution of Washington, it had amassed over a million cards containing pedigree information and family data, nearly all of which Allen explains were "of a subjective, impressionistic nature" and "worthless from a genetic point of view." 242-43. Terman's assessment of Galton's I.Q. rating from Forrest, *Francis Galton*, 7.

inquiry, and came to convince himself that “genius was hereditary.”²⁶ He aimed to prove his thesis by “showing how large is the number of instances in which men who are more or less illustrious have eminent kinsfolk.” He broke down the biographical data of male ancestors and descendants of talented men, enabling him, Galton believed, to compute statistically the extent to which eminence and genius were inherited.²⁷ Beginning with an historical investigation of biographies of English judges from the English Revolution to 1865, Galton maintained that the historical examples proved that “ability is not distributed at haphazard, but that it clings to certain families.” So statesmen, English peers, military commanders, literary men, scientific men, poets, musicians, painters, theologians, oarsmen and “wrestlers of the North country” were all testimony of the extent that great ability ran in immediate family lines. But although the scientific community in Britain politely received *Hereditary Genius*, Galton’s underlying premise was flawed.²⁸

²⁶ Francis Galton, *Hereditary Genius: An Inquiry into Its Laws and Consequences* (London: MacMillan and Co., 1869), v for origins of study; 1-6, 8-10 for the sources and method.

²⁷ One of Galton’s more fantastic statistical calculations was to compute the likelihood of a military man being shot in the field, either by accident (i.e. not specifically aimed at) or by specific effort. The statistical risk of being accidentally shot was the square root of the product of a man’s height and his weight – a man of 16 stone standing 6 feet 2.5 inches, would escape harm for 2 years; a man of 8 stone standing 5 feet 6 inches would be safe from chance wounds for three years. Except, Galton pointed out, the taller and heavier man, because of his conspicuousness, would more likely be assumed to be an important personage and “made the object of special aim.” Galton, *Hereditary Genius*, 144.

²⁸ Galton, *Hereditary Genius*, “kinsfolk” from 6; 50-54 for notation of study; 67 for distribution of heredity. Also, Forrest, *Francis Galton*, 101 for reviews. Galton later expressed regret for using “genius” instead of “talent” which he felt would have been more accurate. Accuracy of term in Forrest, *Francis Galton*, 101. Gillham records a diary entry of Galton’s wife Louisa in which she notes that “Frank’s book not well received, but liked by Darwin and men of note.” Quoted in Gillham, *A Life of Sir Francis Galton*, 169. Gillham records other reviews and reactions to *Hereditary Genius* on the following pages. *Hereditary Genius* also had, as a biographer points out,

Galton's greatest problem was that he had no vector for inheritance. Throughout *Hereditary Genius*, Galton affirmed that special ability – whether called eminence, distinction, genius or talent – was inherited along the male line, and tended to pass from father to one son. He was certain that the proof of his theory lay in the weight of accumulated examples. At the conclusion of *Hereditary Genius*, Galton confidently declared, “There cannot, therefore, remain a doubt as to the existence of a law of distribution of ability in families.”²⁹ Yet the difficulty of proving, through an analysis of pedigree or ancestry, that ability was inherited was nearly impossible without any idea of a vector of that ability; before the rediscovery of Gregor Mendel's work on recessive and dominant inheritance strands (which, ironically, was taking place in Germany at exactly the same time, only to slip into oblivion), or August Weismann's theory of the germ-plasm, no one could precisely identify *how* these characters were biologically inherited. For Galton to leap to the conclusion that eminence or genius was a purely biological quality, and not the result of parental influence, training and encouragement, was, in the context of his social relationships, partly understandable. After all, in his own family tree, Galton had his own gifted intellect, his cousin Charles Darwin, and their shared grandfather Erasmus Darwin. When Galton's work began to achieve significant influence in the United States, this flaw became the basis for

several minor errors that in the context of the work had a significant effect in altering the study's results. Forrest, *Francis Galton*, 90-91.

²⁹ Galton, *Hereditary Genius*, 318.

restricting the admission of the non-gifted inferior immigrants from southern and eastern Europe.³⁰

In Galton's mind, however, there were no doubts or flaws. The results from his biographical study suggested to him not only the idea that superior ability could be inherited; it also created the possibility that it could be intentionally bred in future generations. His introduction to *Hereditary Genius* declared "it would be quite practicable to produce a highly-gifted race of men by judicious marriages during several consecutive generations."³¹ In his second book on heredity Galton tried to ascertain the practicability of this goal by weighing the relative influence of "nature" and "nurture."

Another Galton book, *English Men of Science*, published in London in 1874, was a more focused investigation of the inheritance of ability, though it was equally problematic. Galton based his study on membership lists of Fellows of the Royal Society, and winnowed the list of subjects whose genealogical backgrounds

³⁰ For an outstanding example, see the book by Charles B. Davenport – whom Garland Allen calls the "chief American advocate" of eugenics (Allen, "Eugenics Record Office," 225) – *Naval Officers: Their Heredity and Development* (Washington, D.C.: The Carnegie Institution of Washington, 1919). The book is absolutely identical in method to Galton's *Hereditary Genius* with the sole exception that Davenport also included the maternal side of a man's inheritance. Davenport argued that "Since heredity is so potent in determining the product, and particularly the vocation which a man selects and in which he is more or less successful, it is worth while to consider the occupations of close relatives of the propositus." 9. Davenport then examined the biographical information of 67 noted naval officers – with one Dutch exception, all American or English – to see what they inherited from their parents. The officers inherited various qualities, including *restlessness*, *nomadism*, *adventuresomeness*, *hyperkinesis* (energy), and *combative*, *diplomatic*, *courageous*, or *administrative* qualities (Italics in original, p. 24). Ultimately, however, Davenport found that the most important characteristic great naval officers inherited from their parents was *thalassophilia* (defined as "sea-lust" – p. 25). He pronounced *thalassophilia* to be a recessive trait, "so that, when the determiner for it (or the absence of a determiner for dislike) is in each germ-cell the resulting male child will have a love of the sea. Sometimes a father who shows no liking for the sea, like [Commodore George Hamilton] Perkins's father, may carry a determiner for sea-lust recessive." 29

³¹ Galton, *Hereditary Genius*, 1.

he would investigate by adding points for winners of scientific medals, presidents of learned societies, and professors. Thus supplied with the names of 180 men, most residents of London, Galton wrote to these distinguished men asking for them to supply him with any and all information about their ancestors they felt comfortable providing. Galton required his readers to have faith in his scrupulousness; all of his respondents would remain anonymous, so no verification of his computations or analysis was possible.³²

Galton attempted a sophisticated statistical approach to heredity, describing in the introduction that nature and nurture (a phrase he coined) were both important influences on a human being's development, although he was, like later classic racialists, unable to remain consistent regarding the interdependence of biology and the environment.³³ When he began to discuss the Darwin family, and that family's love of natural history and theory, he expressed his conviction that "I am sure that these characteristics are hereditary rather than traditional." Analyzing family traits, or studying an individual's hereditary pedigree, would in Galton's mind foreshadow that individual's development. Thus, taking into account the intellectual ability of Erasmus Darwin, the maternal grandfather he shared with cousin Charles, it was not surprising that his offspring achieved

³² Francis Galton, *English Men of Science: Their Nature and Nurture* (London: Macmillan and Co., 1874), 11. Also, Shapin, *A Social History of Truth*.

³³ Galton described nature as "all that a man brings with himself into the world; nurture is every influence from without that affects him after his birth. The distinction is clear: the one produces the infant such as it actually is, including its latent faculties of growth of body and mind; the other affords the environment amid which the growth takes place, by which natural tendencies may be strengthened or thwarted, or wholly new ones implanted." Galton, *English Men of Science*, emphasis added, 12.

eminence in science, medicine, and other fields.³⁴ Ultimately, Galton believed the ability to be hereditary, and not due to education or socialization.³⁵

The eminent Darwin family was not the only hereditarily gifted brood in England though, and Galton's study of English scientific men tried to prove the hereditary character of intellectual interests. Galton pointed to several qualities that he felt his respondents had proven were inherited: great energy, good constitutions, perseverance, practical mental habits, "good verbal memory, as for prose and poetry," facility with numbers and figures, mechanical aptitude, secular inclinations, and "good memory for form."³⁶ Hampered by the difficulty of not knowing how these qualities were transmitted from generation to generation, Galton assumed that the coincidence of ability or particular aptitudes in a family line was not a coincidence or a result of socialization, but was the result of heredity. But since the hereditary transmission could not be proven, Galton included lengthy quotes from the letters he received from his subjects to verify his

³⁴ *Ibid.*, 45. The historian Stephen Jay Gould points out that actually, Darwin was *not* the naturalist on board the *HMS Beagle* – the ship's physician and naturalist was Robert McCormick. Darwin was instead merely the personal companion to the ship's captain Robert Fitzroy. But significantly, Gould also points out that "this story illustrates the importance of social class as a consideration in the history of science. How different would the science of biology be today if Darwin had been the offspring of a tradesman and not the son of a very wealthy physician. Darwin's personal riches gave him the freedom to pursue research without encumbrance." This is the same point that Steven Shapin makes, and a very significant component to Galton's own social position and research as well. See Stephen Jay Gould, *Ever Since Darwin: Reflections in Natural History* (New York: W. W. Norton, 1977), quote from 33; 28-29 for Darwin's role on board the *Beagle*. Also, Shapin, *Social History of Truth*. Charles Darwin's son Leonard later became the head of the Eugenics Education Society in England, and supervised over the first International Congress of Eugenics as its President, although he made few original intellectual contributions to the study of heredity, evolution, or natural science.

³⁵ Karl Pearson, in his biography of Galton attributed the family's scholarly success as due to the inheritance of a "scientific imagination." Pearson, *The Life, Letters and Labours of Francis Galton*, I, p. 58. Discussed further in Gillham, *A Life of Sir Francis Galton*, 22ff.

³⁶ Galton, *English Men of Science*, ch. 2.

assertions. He relied on an uncritical acceptance of anecdotal evidence as “proof” of his theory.³⁷

Finally, in *English Men of Science*, Galton expressed an underlying assumption of a connection between an individual’s social position and their societal value. The historian Steven Shapin suggests that the high social position of scientific investigators in the pre-modern period was absolutely crucial to assessing the veracity of their work. Because his respondents were all men of high intellectual and social reputation, there was no need to critically evaluate their responses, so Galton did not. He was convinced that the replies he received were unimpeachably accurate: “As regards the scientific men, I find, as I had expected, vanity to be at a minimum, and their returns to bear all the marks of a cool and careful self-analysis. My bias has always been in favour of men of science, believing them to be especially manly, honest, and truthful, and the results of this inquiry has [sic] confirmed that bias.”³⁸ This lays bare one of the critical distinctions between the methods employed by classic racialists and the modern experimentalists. For the latter, the veracity, utility, and accuracy of a scientific experiment or truth-claim resided in the ability of any investigator to replicate the results in a duplicated experiment. As Jennings would later insist, “we must study biology in such a way that what we find out can either be proved or disproved, not merely left hanging in the air. We must find out things that we can show people;

³⁷ One respondent to Galton’s inquiry replied that “I seem to possess the same unweariedness as my father, and find myself trotting in the streets as my father used to do.” This struck Galton as an indicator of inheritance of intellectual ability. Galton, *English Men of Science*, quoted on 83.

³⁸ Ibid., 148. Also, Shapin, *A Social History of Truth*.

we must be able to say to doubters—you try it yourself and you will see that it's true."³⁹ Jennings absorbed these methods and techniques in part at the Naples Zoological Station, where laboratory research was the *sine qua non* of scientific investigation. Galton, airing one of the final gasps of pre-modern science, expected his reputation, and the reputation and social positions of his respondents, to satisfactorily stand as truth claims on their own. His results were true because he and his respondents – all men of high Victorian social standing – said they were.

In *Hereditary Genius* and *English Men of Science*, Galton evolved toward an understanding of heredity and inheritance that culminated in one of the most important books of the late nineteenth-century, *Inquiries into Human Faculty and Its Development*, published in 1883. *Human Faculty* was enormously important for the development of American classic racialism. Indeed, it could be argued that it was the single-most influential book for the entire American immigration restriction movement, as it created the scientific language of eugenics, which in turn held a profound sway over American immigration policy. Within this book, Galton unified his hereditary assumptions of pedigree in the description of the new field of eugenics.⁴⁰

³⁹ Herbert Spencer Jennings, "Some Unsettled and Unsettling Questions in Biological Science: An Unpopular Talk" Lecture n. 37, unpublished manuscript, Jennings Papers, APS, delivered at University of Pennsylvania, 11 March 1926, p. 37, emphasis in original.

⁴⁰ Despite the sloppiness with which he employed it, Nicholas Gillham declares Galton's work in pedigree analysis to be a tremendous contribution to modern genetic science: "Today pedigree analysis is the essential analytical tool that human geneticists use in localizing the genes responsible for different human maladies both physical and mental." Gillham, *A Life of Sir Francis Galton*, 4; also 171.

Galton explained that his intent in the book was “to take note of the varied hereditary faculties of different men, and of the great differences in different families and races, to learn how far history may have shown the practicability of *supplanting inefficient human stock by better strains...*” and the extent to which this direction of evolution was feasible.⁴¹ This was the kernel of eugenics that Galton developed throughout the book. It built on the arguments of his previous books – namely that ability, talent, and eminence were inherited – to advance the proposition that these superior characters could somehow, because of their hereditary basis, be augmented and propagated through judicious and careful procreation of superior stocks. This hope would play directly on the fears and aspirations of native-born Americans in the future.

Galton used the word “eugenics” to describe this process of breeding superior stocks. “We greatly want,” he explained, “a brief word to express the science of improving stock, which is by no means confined to questions of judicious mating, but which, especially in the case of man, takes cognizance of all the influences that tend in however remote a degree to give to the more suitable races or strains of blood a better chance of prevailing speedily over the less suitable....” The Greek word *eugenes*, meaning “good in stock, hereditarily

⁴¹ Francis Galton, *Inquiries into Human Faculty and Its Development* (Bristol, UK: Thoemmes Press, 1998 [orig. New York: MacMillan and Co., 1883]), 1-2, emphasis added. In the introduction, Galton explained his method for obtaining composite portraits that he put on the frontispiece and which demonstrated the continuity of the physical structure in family lines: he would collect photos, focus, crop, or reduce them to be of similar size, set up his photographic equipment, place the photos on a spot on the wall, and over-expose one photographic plate while inter-changing the photos. This, he claimed, would “bring into evidence all the traits in which there is agreement, and to leave but a ghost of a trace of individual peculiarities.” 10. This is a rather surprising reversal from *Hereditary Genius*, wherein Galton declared “features and mental abilities do not seem to be correlated.” 333. One of the “Personal and Family Specimens of Composite Portraiture” was of Alexander the Great, derived from six different medals or coins.

endowed with noble qualities,” he stated, adequately expressed that science.⁴² So was born a concept, a scientific language, that would have dramatic implications. But almost immediately after introducing this nettlesome neologism—a mere three pages—Galton began to undercut the “science” he just created. What exactly was a “good” stock? What were the desirable, “noble qualities”? These were not scientifically precise terms; they were subjective evaluations. And how, exactly, were these noble qualities “hereditarily endowed”?

As many of his acolytes would, Galton ran into a severe difficulty in trying to measure or quantify something that defied easy empirical analysis when applied to human beings. After describing this new, promising “science” of eugenics, Galton wrote that he believed that in any scheme of improving the hereditary capabilities of future human beings, “energy is the most important quality to favor.”⁴³ Galton meant not only physical energy, the body’s capacity to perform a given physical task, but also mental energy, those traits that he had laid out in *English Men of Science*: practical business habits, perseverance, good verbal and mathematical memory, and a secular inclination, among others. But with mental testing and psychology in their infancy, there was great difficulty in quantifying, let alone understanding, how mental energy worked.

Galton aimed to remedy this difficulty of quantification by recommending extended generational studies of human beings. Pedigree analysis was the core of

⁴² Galton, *Human Faculty*, fn. 1, pp. 24-25.

⁴³ *Ibid.*, 27.

his work in heredity; *Hereditary Genius* and *English Men of Science* were at heart studies of the genealogies of gifted men. As he explained in *Human Faculty*, the principal limitation in advancing the goal of eugenics, of mastering “the conditions under which men of a high type are produced” was the lack of “full family histories, both medical and general, extending over three or four generations.”⁴⁴ Galton was confident that he had already established the hereditary nature of ability and eminence. In *Human Faculty* he tried to broaden his investigation to more general characteristics, so that the possessors of desirable traits could be encouraged and appropriately supported to produce offspring of “a high type.”

Surprisingly, Galton turned to the United States for evidence in support of his argument. The American “Juke” family provided theoretical proof for Galton’s pedigree studies. While the British polymath was investigating the pedigree of English scientific men and distinguished Britons from history, an American researcher, Robert Dugdale, was uncovering a hereditary lineage not of ability or eminence, but of degeneracy. Dugdale wrote that the several generations descended from “Max Juke” “run along lines of descent so that you can follow them from generation to generation,” and that the criminality, poverty and immorality of the family’s descendants could be traced through this genealogy.

⁴⁴ *Ibid.*, 44.

Galton approvingly cited the Jukes as an example of the usefulness of pedigree analysis to show that both ability and criminality were inherited.⁴⁵

But this trans-Atlantic affirmation of the heredity of moral and mental characteristics did not help Galton solve all of his methodological or theoretical problems. His difficulty in quantifying heredity extended beyond attempting to measure things like energy, perseverance, or practical business habits. His biographer Nicholas Gillham notes that the “diversity of Galton’s interests was not atypical for a Victorian scientist,” but that diversity, and Galton’s “enthusiasm for analyzing scientific problems quantitatively” in many ways limited the utility of Galton’s work on heredity on a conceptual level.⁴⁶ His diverse interests drove his work into unusual places; he wanted to measure and quantify *everything*. In *Human Faculties* he described “Psychometric Experiments,” an intriguing anticipation of Sigmund Freud’s “free-association,” wherein Galton would go for leisurely walks and record his thoughts and the influences on these thoughts from his immediate environment. He tried to quantify color associations and mental

⁴⁵ Robert Dugdale, *The Jukes: A Study in Crime, Pauperism, Disease and Heredity* Fourth ed., Intro. by Franklin Giddings, Foreword by Elisha Harris, M.D. (New York: G. P. Putnam’s Sons, 1910 [orig. 1877]), quote from 16. Significantly, Dugdale very frequently ascribed a high importance to environmental influence, which Galton did acknowledge, though to a diminished extent. Galton, *Human Faculty*, 63. On the Jukes see also Carlson, *The Unfit*, esp. ch. 10.

⁴⁶ Gillham, *A Life of Sir Francis Galton*, 3. I distinguish the “conceptual level” here to acknowledge that Galton did make significant theoretical and practical contributions to the study of heredity (e.g. the law of ancestral inheritance) as well as significant contributions to statistics in helping to develop standard deviation, regression and error analysis, correlation and others. The historian Theodore Porter describes Galton’s statistical contributions as leading to quantitative genetics and biometrics. Theodore Porter, *The Rise of Statistical Thinking, 1820-1900* (Princeton: Princeton University Press, 1986), 129-30. Sharon Kingsland argues that Galton’s statistical ability was central in making the study of heredity a “positivist science”; Lombroso aimed to do the same for the study of criminology. See Sharon Kingsland, “Evolution and Debates over Human Progress from Darwin to Sociobiology” *Population and Development Review* v. 14 Supplement: Population and Resources in Western Intellectual Traditions (1998), 167-98; 186. See also Gillham, *A Life of Sir Francis Galton*.

imagery and its vividness. He tried to objectively determine the efficacy of prayer by examining the average age at death of a variety of professions (he found it ineffective).⁴⁷ Galton's conceptual approach to the world was effectively pre-modern, in that he still practiced a method of scientific investigation and inquiry that, despite its positivist ambitions, was really about observing and describing phenomena and making inferences from those phenomena.⁴⁸

An example will help illustrate this point. As a well-traveled Victorian gentleman, Galton was certainly familiar with the processes of emigration and colonization.⁴⁹ But what inspired or compelled a man to leave the comforts of his familiar home and environment for the great uncertainty and hazards of relocating? For Galton, an "exile," as he called emigrants, left all that was safe

⁴⁷ Galton was not a particularly religious man, but he understood that very frequently, great statesmen, religious leaders, and monarchs were often (though perhaps insincerely) the recipients of hopeful, well-wishing prayers from their subjects or congregants. He found that 97 members of royal households lived to an average age of 64.04; nearly 1000 members of the clergy lived an average of 69.49 years; lawyers, of whom he had 294 records, lived an average of 68.14 years; and noted contributors in fields of literature or science, whose sample numbered 395, lived to an average of 67.55 years old. So clergy and kings who received prayers did not live any longer than those anonymous members of society that did not. He thus logically concluded, "If prayerful habits had influence on temporal success, it is very probable, as I must again repeat, that insurance offices...would long ago have discovered and made allowance for it." Galton, *Human Faculty*, table on p. 281; quote 292.

⁴⁸ Galton did perform some experiments. For instance, to test Darwin's theory of pangenesis, Galton transfused blood from rabbits. See the description in Gillham, *A Life of Sir Francis Galton*, 174ff. Galton also presented the results of a two-year surveying trip in Africa, charting and mapping parts of what is now Namibia to the Royal Geographical Society in the early 1850s. This presentation, Gillham argues, "launched his scientific career." *Ibid.*, 91.

⁴⁹ Another useful example that Galton used can be found in *Human Faculty*. He used an analogy of sticks thrown into a stream to demonstrate the co-operative influence of heredity and environment. A person throwing bits of a stick into a stream, he explained, would observe the halting progress of the sticks as they were variously arrested, deflected, or facilitated in their course by "a combination of circumstances." The observer might think "how largely the destiny of the stick has been governed by a series of trifling incidents. Nevertheless," he wrote, "all the sticks succeed in passing down the current, and in the long run, they travel at nearly the same rate. So it is with life, in respect to the several accidents which *seem* to have had a great effect on our careers." Galton, *Human Faculty*, 241, emphasis added.

and easy because he was a man of great character, strength and virtue. They were by definition great, eminent men, because “a quiet man would endure and succumb” to inertia or oppression, and remain safe at home. Galton declared that “exiles are on the whole men of exceptional and energetic natures, and it is especially from such men as these that new strains of race are likely to proceed.”⁵⁰ Certainly, this *could* be true; the settlement of British North America showed many such examples of gifted and exceptional men taking great risks. (This would be precisely the argument American supporters of immigration restriction would make, that the early colonists and pioneers in what became the United States were a special, select, and highly superior breed whose offspring were tragically being overwhelmed by inferior races.) But it could also be said that a great number were not gifted or exceptional at all, but were compelled to leave England behind because of desperation, crushing poverty, criminal prosecution, or were searching for significant economic gain. Ireland would have provided a perfect example of some of these “other” factors that led Irish men and women to uproot themselves from their homeland and loved ones. Galton’s great logical flaw, and it was Lombroso’s too, was to assume or infer that qualities or causes that may or *may not* have been true were indeed true.

The trans-Atlantic impact of Galton’s work can be seen in the reception of his theories in America. It attracted the attention of Charles B. Davenport, an American zoologist also interested in human heredity and biology, who would in time become the leader of eugenics in the United States. Davenport felt that

⁵⁰ Galton, *Human Faculty*, 308.

Galton's application of statistical analysis to biological inheritance held great promise. The American aimed to emulate Galton's work, writing a book of his own titled *Statistical Methods: With a Special Reference to Biological Variation* (1899) to use in his zoology classes at Harvard University and at research laboratories in the United States. When Davenport sent a copy of the book to London, Galton replied, "Your beautiful little book with its kindly & charming lines of gift to me, has of course interested me greatly. I am delighted to see how steadily the application of the higher statistics is making its way in biological and social matters."⁵¹ Davenport shared many qualities with the English gentleman, such as a high confidence in the efficacy of eugenics, an appreciation of the importance of statistical analysis in biological studies, a conviction that many characteristics in human beings were directly inherited, and the utility of pedigree analysis in proving the heredity of those characteristics. But perhaps their most important similarity was a like tendency toward sloppy analysis that, over time, radically undercut the scientific standing of eugenics.

One might describe Galton as a late pre-modern scientist. His social standing and position as a Victorian gentleman was a more important endorsement of the veracity of his scientific statements than his institutional

⁵¹ Francis Galton to Charles Benedict Davenport, 20 October 1899, Charles B. Davenport Papers, American Philosophical Society, Philadelphia, PA [hereafter Davenport papers]. See also Charles B. Davenport, *Statistical Methods, with a Special Reference to Biological Variation* (New York: J. Wiley and Sons, 1899). Davenport explained that the book was "issued in answer to a repeated call for a simple presentation of the newer statistical methods in their application to biology.... [particularly] for use at summer laboratories where material for variation-study abounds." iii.

affiliation or his academic credentials.⁵² His research consisted of poring over biographies and pedigrees of socially or historically notable men, conducting calculations of correlated qualities and characteristics, and generalizing findings from there. He had certainly put, in Theodore Porter's phrase, his "trust in numbers," and his empiricism and positivism reflected a commitment to the development of objective knowledge. But he was more a natural scientist than a zoologist or biologist. Particularly in dealing with heredity and inheritance, there was no knowledge of *how* heredity actually worked, though the framework of Darwinian evolution was certainly a tremendous intellectual advance. Galton tried to express hereditary inheritance by distinguishing between "latent" and "patent" characteristics.⁵³ With Mendel's results from his work on peas unknown, and before the "germ-plasm" theory of inheritance, Galton groped in the dark – albeit with modest success.

⁵² See Gillham, *A Life of Sir Francis Galton*, chapters 2-3 for Galton's education. Steven Shapin describes early modern science as implicitly founded on trust. Many scientists in the period "were gentlemen and their codes of scientific conduct were adapted from those circulating in gentlemanly society," such as integrity, honor and free action. But he also notes that the ability of empirical scientific ideas to "travel," to diffuse from local and particular contexts and circumstances, is reliant on "a trust relationship" to an even greater extent than modern scientists. Steven Shapin, "Placing the View from Nowhere: Historical and Sociological Problems in the Location of Science" *Transactions of the Institute of British Geographers* n.s. v. 23, n. 1 (1998), 5-12; 8. Also Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, NJ: Princeton University Press, 1998).

⁵³ "Latent" and "patent" were adequate terms to describe what Mendel had named "dominant" and "recessive" traits. As Mendel's work was not rediscovered until 1900, Galton was unable to use the terms. Darwin also tried to explain reversion (the appearance of characteristics from an earlier generation), and described two separate classes of "gemmules," one of which was dormant, the other active. In an essay titled "On Blood-relationship," Galton introduced his alternate terminology to explain the problem of reversion. Francis Galton, "On Blood-relationship" *Proceedings of the Royal Society* v. 20 (1872), 394-402. Gillham describes the essay as "verg[ing] on the incomprehensible." Gillham, *A Life of Sir Francis Galton*, 179. Carlson, *Mendel's Legacy* describes Weismann's alternative theory of inheritance via the germ-plasm theory of heredity.

In contrast to Galton's methods, Herbert Spencer Jennings conducted scientific investigations in a completely different, "modern" way.⁵⁴ Although he spent his summers as an undergraduate working for the Michigan Fish Commission's survey of the Great Lakes doing mostly descriptive work (his advisor at Ann Arbor was its director), Jennings used the materials to further his knowledge and interest in morphology and developmental mechanics. His desire to go to Naples to conduct research stemmed from a laboratory and experimental approach to science. He told a friend that "a combination of the natural history way of looking at things with the morphological or philosophical way is the ideal condition for one who is going to do something in science." In Naples Jennings continued his training as a zoologist using modern experimental methods, and after getting his Ph.D. in zoology at Harvard, he taught courses at the University of Michigan in Experimental Morphology. He was ultimately lured to Johns Hopkins University – one of the country's preeminent research universities – to reorganize the zoology department. He taught there for over thirty years, and his approach to science remained what it was at Naples: study the processes of development by experimental research. "I have been more and more attracted," he explained to the zoology chair at Michigan before he left for Italy's largest city,

⁵⁴ On this general intellectual and epistemic shift, see the following: Hamilton Cravens, *The Triumph of Evolution: American Scientists and their Heredity-Environment Controversy, 1900-1941* (Philadelphia: University of Pennsylvania Press, 1978); Jane Maienschein, *Transforming Traditions*; Kohler, *All Creatures*; and Keith Benson, "From Museum Research to Laboratory Research"; Pauly, "Summer Resort and Scientific Discipline"; and Diane Paul and Barbara Kimmelman, "Mendel In America: Theory and Practice, 1900-1919" all in Rainger, et al, eds. *The American Development of Biology*; Philip Pauly, *Biologists and the Promise of American Life: From Meriwether Lewis to Alfred Kinsey* (Princeton: Princeton University Press, 2000); Paul, *Controlling Human Heredity*.

“to the line of work often called Developmental Mechanics—the work of the ‘new school.’” Jennings was of a generation that helped gradually shift American biology from its natural history orientation to an academic and research orientation.⁵⁵

The “new school” that Jennings described, however, faced several difficulties in completely displacing a language of scientific inquiry based in part on natural history and natural science. It was competing against an entrenched understanding of the “restrictionist epistemology,” the classic racialism of the early 1900s. As the long-lasting influence of eugenics and classic racialism in the United States shows, although experimentalism may have been epistemologically “better,” it was highly complex and esoteric. By contrast, Galton’s and Lombroso’s simple theories that men of talent and born criminals were naturally gifted or depraved were easy to digest and understand. Classic racialism argued that the blood of the Jukes, and later the Kallikaks, would carry their degenerate characters into future generations because of the immutability of heredity. Classic racialism’s essence was easily reduced: like produces like. Modern biologists could produce no similar dramatic argument of their science’s essence. Jennings later lamented that “Heredity is not the simple, hard-and-fast thing that old-

⁵⁵ For Jennings’s biography and early training, see Mezzano, “The Progressive Origins of Eugenics Critics”; on reorganizing Hopkins consult T. M. Sonneborn, “Herbert Spencer Jennings, 1868-1947: A Biographical Memoir” *Biographical Memoirs* v. XLVII (Washington, D.C.: National Academy of Sciences of the United States, 1975). For his interests in Naples, see, Jennings to Mary Louise Burridge, 20 October, 1895; “morphological” work in Jennings to Joseph Brennemann, 1 August 1894, Jennings papers. *Calendar of the University of Michigan, 1899 - 1900* (Ann Arbor, MI: 1899), Bentley Historical Library, University of Michigan, Ann Arbor, MI [hereafter Bentley], 103-04 for Jennings’s courses at the University of Michigan. For “Developmental Mechanics” see Jennings to Jacob Reighard, 29 December 1895, Box 1, Jacob Reighard Papers, Bentley.

fashioned Mendelism represented it [to be],” but disabusing the public of this beguiling simplicity was not easy.⁵⁶ Modern biology did not reduce to aphorisms. The “new school” could not displace the old outside the narrow audience of its practitioners.⁵⁷

Another essential contribution to classic racist language and ideas involved a biological determinism similar to Galton’s. An Italian medical doctor, working amidst an allegedly unlawful and rebellious population in southern Italy after unification in 1861, found physical predictors of the criminal behavior of southern Italian brigands. Deviants and criminals had physical stigma and abnormalities that manifested their antisocial inclinations. His ideas became so popular and compelling that Cesare Lombroso’s criminal anthropology appeared in fiction: “The Count is a criminal and of criminal type. Nordau and Lombroso would so qualify him, and *quâ* criminal he is of imperfectly formed mind. Thus, in a difficulty, he has to seek resource in habit.” In that habit laid the method of tracking down and destroying the fictional fiend Dracula.⁵⁸ Published in 1897, Bram Stoker’s novel *Dracula* at once attests to the diffusion and persuasiveness of the theory of the “born criminal,” first articulated by Lombroso in 1876 in *L’uomo delinquente* [*Criminal Man*]. Lombroso’s theory, that there existed a type of criminal whose actions were determined by an inherent, biological disposition to depravity and delinquency that manifested itself in physical form was an

⁵⁶ Jennings, “Heredity and Environment” 230.

⁵⁷ This dilemma is discussed in Kuhn, *Scientific Revolutions*; idem., *The Road Since Structure*.

⁵⁸ Bram Stoker, *Dracula* Introduction by Leonard Wolf (New York: Signet Classic, 1992 [orig. 1897]), 343.

essential and fundamental contribution to classic racialism, and in turn had a significant impact on the American immigration restriction movement. As the founder of the “Italian school” of criminal anthropology, Lombroso’s own writings, as well as those of his followers Giuseppe Sergi and Alfredo Niceforo contributed to the trans-Atlantic origins of American racial nativism, and provided an additional pivot from pre-modern methods of scientific investigation to modern laboratory- and experimentally-derived scientific truths.⁵⁹

Lombroso made several key contributions to the foundations of classic racialism, but of primary importance was his work in criminal anthropology. Reacting against the Enlightenment approach of Cesare Beccaria and others, who attempted to rationalize and make uniform the punishment of crimes, Lombroso’s training as a medical doctor oriented his approach to the criminal rather than the crime. Serving as a doctor in the newly-unified Italian army, Lombroso came across the bodies of “brigands,” Southern Italian peasants who refused to submit to the Piedmont-dominated Royal government, which southerners viewed as another manifestation of foreign domination of the Italian south (*il Mezzogiorno*).⁶⁰ After a few years’ service in the Italian army, Lombroso worked

⁵⁹ I maintain this trans-Atlantic connection despite the good suggestion of Frederick Churchill for greater scrutiny of local and national contexts and institutions to escape the competing dyad of “naturalists” versus “experimentalists.” See Frederick Churchill, “In Search of the New Biology” *Jl H Bio*, v. 14 (1981), 177-91. Also, Kingsland, “The Battling Botanist”; David Hull, *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science* (Chicago: University of Chicago Press, 1988).

⁶⁰ For the origins of *il questione meridionale* – what Italians called the “Southern Question” – see the essays in Robert Lumley and Jonathan Morris, eds. *The New History of the Italian South: The Mezzogiorno Revisited* (Exeter, UK: University of Exeter Press, 1997); Nelson Moe, “‘This is Africa’: Ruling and Representing Southern Italy, 1860-61” in Albert R. Ascoli and Krystyna von Henneberg, eds. *Making and Remaking Italy: The Cultivation of National Identity around the Risorgimento* (Oxford: Berg, 2001); Nelson Moe, *The View from Vesuvius: Italian Culture and the Introduction*

with mental patients, and then took a position at the University of Turin in 1876 as professor of legal medicine.⁶¹ During this time, Lombroso examined the body of an executed brigand, Giuseppe Villella, and found several strange anomalies in the dead criminal's skull. It became, as Mary Gibson describes, Lombroso's "foundation myth" for criminal anthropology.⁶²

Villella's delinquency was inscribed on his body, and ultimately pre-determined the course of his life as a criminal. The physical body of Villella, Lombroso argued, betrayed his criminal behavior.⁶³ Lombroso then began to examine and measure the bodies of other criminals and delinquents, amassing enough evidence to publish *L'uomo delinquente* in 1876 in which he described the new field of positivist criminal anthropology. The argument of *L'uomo delinquente* was simple: most criminals possessed abnormal physical features and characteristics that demonstrated them to be evolutionary throwbacks or atavisms, which disposed them to delinquent and criminal behavior. The circumference of their skulls tended to be abnormally small, their cheekbones wide or

Southern Question (Berkeley: University of California Press, 2002); John Dickie, *Darkest Italy: The Nation and Stereotypes of the Mezzogiorno, 1860-1900* (New York: St. Martin's Press, 1999); and the essays in Jane Schneider, ed. *Italy's "Southern Question": Orientalism in One Country* (New York: Berg, 1998).

⁶¹ See Peter D'Agostino, "Crania, Criminals, and the 'Cursed Race': Italian Anthropology in American Racial Thought, 1861-1924" *Comparative Studies in Society and History* v. 44, n. 2 (April 2002), 319-343; 322; Mary Gibson, *Born to Crime: Cesare Lombroso and the Origins of Biological Criminology* (Westport, CT: Praeger Press, 2002).

⁶² Gibson, *Born to Crime*, 20.

⁶³ What Lombroso specifically found was that Villella's skull had an abnormal indentation at the base of his brain, which he believed suggested the existence of "a fairly small cerebellum." Cesare Lombroso, *Criminal Man* translated and edited by Mary Gibson and Nicole Hahn Rafter (Durham: Duke University Press, 2006) [Hereafter Lombroso, Gibson/Rafter, *Criminal Man*], 48. Also, Cesare Lombroso, *L'uomo delinquente: in rapporto all'antropologia, alla giurisprudenza, ed alle discipline carcerarie* (Rome: Napoleone Editore, 1971 [orig. 1876]).

overdeveloped, and they had a high incidence of prognathism (a jaw that projects beyond the nose). Certain classes of criminals, Lombroso found, had specific physical similarities. Thieves had thick and close eyebrows and expressive faces accented by small, darting eyes. Their beards and hair were scanty, and they had large ears. Rapists, too, had large ears, but also had “sparkling eyes, delicate features, and swollen lips and eyelids.”⁶⁴ Criminals also frequently had tattoos, they spoke in a jargon or vernacular particular to their deviant profession, and they tended to have a notable lack of affect.⁶⁵ These characteristics, Lombroso declared, “clearly prove that the most horrendous and inhuman crimes have a biological, atavistic origin in those animal instincts that, although smoothed over by education, the family, and fear of punishment, resurface instantly under given circumstances.”⁶⁶ The dangerous class of criminals, to an expert who knew how to properly read the human body for these stigmata and marks of atavism, was readily visible. The danger they represented, for Lombroso and his followers, lay in the fact that their propensity to crime was innate and biological.⁶⁷

⁶⁴ These descriptions are all from Lombroso, Gibson/Rafter, *Criminal Man*: circumference from 45; cheekbones, 49; rapists and thieves, 51. Lombroso declared that “It can even be said that each type of crime is committed by men with particular physiognomic characteristics...” *Ibid.*, 51.

⁶⁵ *Ibid.*, chapters 3 and 4.

⁶⁶ *Ibid.*, 91. This emphasis on “given circumstances” ultimately made Lombroso’s theory slightly less deterministic than Galton’s theory of hereditary ability, though, as I have suggested, Galton was at least aware of some role for environmental factors.

⁶⁷ Lombroso did not use the phrase “born criminal” [*delinquente nato*] until the third edition of *L'uomo delinquente*, which appeared in 1884. One of Lombroso’s students, Enrico Ferri, first coined the phrase. See the explanation in the editors’ introduction to the third edition in Lombroso, Gibson/Rafter, *Criminal Man*, 9; 161. Lombroso also had difficulty solving the problems this class represented. He suggested that crime had a symbiotic relationship to society, but since born criminals could not be rehabilitated, this concept ends logically in negative eugenics or extermination. Lombroso stated at the end of the fifth edition of *Criminal Man* that “born

But Lombroso's conclusions were problematic. He often seemed confused regarding the efficacy of the environment, socialization, and other cultural factors in explaining crime; Lombroso was a committed socialist (as were most of his followers), and tended to also perceive an economic basis for some types of crime. Again, certain criminals acted on their innate dispositions only in certain circumstances. Lombroso eventually devised four typologies of male criminals, of which the born criminal was only one type. The Italian physician used anecdotes, paintings, literature, and ancient history as evidence in support of his theory.⁶⁸ The greatest problem in his positivist criminology, however, was also its greatest strength: simplicity. Lombroso tried to compress what is ultimately a very complicated phenomenon (crime and criminal behavior) into a very concise explanation (biology). In this way, what may have been merely coincidental correlations, or random chance, became elevated to a type of scientific law.

Lombroso's positivist criminology was challenged by the emergence of a "French" school of criminology at the Second Congress of Criminal Anthropology in 1889 that emphasized the sociological origins of crime, rather

criminals are impervious to every social cure and must be eliminated for our own defense, sometimes by the death penalty." *Ibid.*, 354.

⁶⁸ Typologies from the editors' introduction to the fourth edition of *L'uomo delinquente* in Lombroso, Gibson/Rafter, *Criminal Man*, 228; anecdotes and proverbs from *Ibid.*, 311. Lombroso suggested that archeologists had demonstrated that the worst of the Roman Empire's leaders – Commodius, Nero, and Tiberius – all had "jug ears and swollen temples." *Ibid.*, 53. Lombroso also declared that "Great artists depicted criminal physiognomy in their painting long before criminal anthropologists demonstrated scientifically the existence of a criminal type." Among those he singled out were the depraved executioners in the work of Mantegna, Titian, Rubens and Goya. *Ibid.*, 312. See also Lombroso, *L'uomo delinquente*, 88-89 for a variety of regional proverbs in Italy that also allegedly provided evidence of the anthropological theory of crime; on p. 90 Lombroso lists Dante, Shakespeare and Dostoyevsky as artists especially skilled at accurately describing the physical characters of criminals.

than the biological.⁶⁹ Other Italians, particularly the sociologist Napoleone Colajanni, also disputed his biological explanations for crime. But Lombroso made significant efforts to diffuse his work across the Atlantic, and in this effort, he found receptive audiences in the United States. Although there was no adequate English translation of *L'uomo delinquente* until Lombroso's daughter's abridged translation was published in 1911, early on Lombroso tried to disseminate his ideas to Americans through short articles in journals.⁷⁰

In the introduction he wrote for his daughter's abridgement, Lombroso pointed out that the United States had always provided "a warm and sympathetic reception to the ideas of the Modern School which they speedily put into practice."⁷¹ His first English description of criminal anthropology was published

⁶⁹ At the First International Congress of Criminal Anthropology at Rome in 1885, Lombroso's theory had no opponents and very few critics. At the Second Congress, in Paris in 1889, the physician Alexandre Lacassagne offered an alternative "French School" of criminal anthropology that was heavily environmentalist and sociological rather than anthropological. See Gibson, *Born to Crime*, 43. There was also an Italian camp of sociological criminology, in which Napoleone Colajanni was a significant presence. See Gabriella D'Agostino, "Napoleone Colajanni and Human Sciences" *Europaea*, v. 3, n. 3 (1997); also Piers Beirne, *Inventing Criminology: Essays on the Rise of Homo Criminalis* (Albany: State University of New York Press, 1993). After the Fourth International Congress of Criminal Anthropology in 1896, the American magazine *The Nation* detailed the meeting in Geneva: the French and Italian schools continued their rivalry, and Lombroso again showed "that lamentable weakness known as hasty generalization" but also showed great facility with "scientific 'accommodation.'" Lombroso's positivist criminology had evolved over time, the article reported, incorporating criticisms and refinements, "and now, at the fourth congress, applauds the proposition of the men who are seeking to substitute a criminal and social psychology for the anthropology which he (Lombroso) started out." "Fourth International Congress of Criminal Anthropology" *The Nation* v. 63, n. 1631 (1 October 1896), 247-8; 247.

⁷⁰ I take the two following definitions of positivism to be excellent: "Positivism was an intellectual frame of mind—rationalistic, naturalistic, attentive to collecting facts, critical of metaphysics, and prone to draw analogies between nature and society. Grounded in Social Darwinism, it captivated a broad spectrum of liberal and socialist intellectuals in the late nineteenth century." P. D'Agostino, "Craniums, Criminals" 321; and defined as a philosophy of history, a philosophical system that offered a structured way of thinking about the nature of the world, and a moral philosophy. Teresa Neyhouse, *Positivism in Criminological Thought: A Study in the History and Use of Ideas* (New York: L.F.B. Scholarly Publishing, 2002).

in 1891, where he described the field in relation to Emile Zola's fictional criminals. Lombroso felt that although Zola had certainly done his homework regarding the physical anomalies of criminals, the French writer placed too much emphasis on contingent, dramatic props in the story, which suggested that criminal acts were contingent.⁷² For Lombroso, there was no contingency. Murder most often occurred because of the biological disposition of the criminal. Using this popular, fictional manifestation of his positivist criminology, Lombroso took the occasion to introduce the principles of criminal anthropology to the American reader. In the second part of his article, Lombroso described experiments he performed in his laboratory on the chemical composition of criminals' urine, their scientifically tested power of smell (using an "osmometer") and taste, and the walk and gestures of criminal subjects. Finally, Lombroso discussed the morphology and the physical abnormalities of criminals, particularly their skull size and shape.⁷³ These helped an expert observer to know that the subjects *were*

⁷¹ Gina Lombroso-Ferrero, *Criminal Man: According to the Classification of Cesare Lombroso* intro. Cesare Lombroso (Montclair, NJ: Patterson Smith Publishing Corp., 1972 [orig. 1911]), xxix. Lombroso defined his criminal anthropology as "modern" to contrast it to Beccaria and Enlightenment theories of penology and criminality. It is not used in the same sense as I am using "modern" to distinguish from "classic racialism." The methods and key concepts were still derived by analogy, description or measurement, not through chemical and laboratory experimentation.

⁷² "In 'La Fortune des Rougon,'" Lombroso explained, "there is a certain musket which serves for the murder of gendarmes...; as if the cause of the fatality was not the hereditary instinct, but this silent and unconscious instrument." Cesare Lombroso, "Illustrative Problems in Criminal Anthropology, Part I and II" *The Monist* v. 1, n. 2 (January 1891), 177-96; 178. Lombroso liked Zola's work in general, describing him as "the only one of the Latin race who endeavors to introduce the scientific method into his literary work." 177. The essay is also reprinted in Cesare Lombroso, *The Criminal Anthropological Writings of Cesare Lombroso Published in the English Language Periodical Literature During the Late 19th and Early 20th Centuries* David M. Horton, Katherine E. Rich, eds. *Criminology Studies*, v. 22 (Lewiston, NY: The Edwin Mellen Press, 2004). Preface by Nicole Rafter.

⁷³ *Ibid.*, 187-88 for osmometer; 190 Wrinkles, tattoos, and the relative infrequency of "canities" (one's hair turning grey) were also described as markers of criminality. In his own schema, two of

criminals, he argued, because their measurement in these categories regressed from normal measurements.

In a later issue the same year, Lombroso discussed his anthropological measurements of political criminals during Italian unification, along with the physical appearances of nihilists and Communards, and suggested that political criminals were ultimately different from born criminals. He pointed to anarchists detained in Turin and Chicago who did not universally display the attributes of the “born criminal.” Muddying the relative influence on human development of heredity and the environment, Lombroso wrote that “I do not mean that political criminals, even the most violent anarchists[,] are true criminals; but that they possess the degenerative characters common to criminals and the insane, being anomalies and possessing these traits by heredity.” So they had abnormal physical features, which were biologically inherited from their ancestors, but their ultimate compulsion to crime – and a particular type of crime directed against the “state” and not an individual – was the result of contingent or accidental circumstances. He mentioned that the father of Charles Guiteau was a “religious lunatic” who had passed on his mental and moral instability to his son and that the father of John Wilkes Booth contributed to his murderous son a radical ancestor’s mental instability.⁷⁴ But ultimately it was a contextual event, not biology that drove these two American political assassins to act. They were not “born” murderers.

the fictional examples of murders from Zola’s work could easily be classified as a criminals of passion, or an “occasional criminal,” one “driven to crime through external circumstances.” Lombroso, Gibson/Rafter, *Criminal Man*, 288. Gibson and Rafter point out Lombroso’s inconsistencies in their introduction to *Criminal Man*. See especially 12, 18.

A German-born British national, who was a well-known translator of Italian, also assisted with the dissemination of Lombroso's ideas throughout the United States. Helen Zimmern published an article in 1898 in the American magazine *Popular Science Monthly* describing the field of Italian criminal anthropology, and Lombroso's accomplishments in establishing the scientific study of criminals and their pathological abnormalities. While impressed with Lombroso's new field, Zimmern did point out problems in the Italian's research, problems that would plague many of the Americans who followed in his steps. She explained that "His work is by no means perfect; he is apt to jump too rapidly at conclusions, to accept data too lightly; thus he was led at the beginning to overestimate the atavistic element in the criminal, and at a later date he has pressed too strongly the epileptic affinities of crime." Just as Galton would do, and later the American eugenicists under Charles Davenport (whose work built on these classic racist foundations), Lombroso interpreted coincidence as causation. But as a trailblazer, opening up new lines of investigation, Zimmern had unqualified admiration for Lombroso, and she passed those sentiments on to the American readers.⁷⁵

The intellectual connection between Lombroso and the United States was strong. He followed developments in the American criminal justice system very closely. He favorably mentioned the American reformatory system, which was

⁷⁴ Cesare Lombroso, "Illustrative Studies in Criminal Anthropology, Part III: The Physiognomy of the Anarchists" *The Monist* v. 1, n. 3 (April 1891), 336-43; 339.

⁷⁵ Helen Zimmern, "Criminal Anthropology in Italy" *Popular Science Monthly* v. 52 (April 1898), 743-60; quote from 752.

designed to rehabilitate what Lombroso called “occasional criminals.”⁷⁶ In an English article, Lombroso stressed that “All efforts at reform should be concentrated upon occasional criminals. They are the only ones for whom much can be done.”⁷⁷ But his American biographer Mary Gibson points out that his positivism and socialism were ideologically at odds; his explanations for Jewish criminality tended almost exclusively to environmental or social causes of crime rather than biology (Lombroso was Jewish). At times he emphasized very stark differences in behavior between whites and non-whites, whom he held to be biologically different and more primitive.⁷⁸

Through the various editions of *L'uomo delinquente*, Lombroso evolved his schema on criminality, and the late-emerging category of occasional criminals again shows some of the contradictions and confusions in Lombroso's own writing. He consistently argued, however, that there *was* a category of born criminals, and like Galton used *The Jukes* as evidence of its biological basis. In his second edition of *L'uomo delinquente* (1878), Lombroso incorporated Dugdale's study of the Juke family as evidence of hereditary criminality.⁷⁹ There

⁷⁶ Lombroso describes this class in great detail in Lombroso, *L'uomo delinquente*, Parte VIII. See also Lombroso, Gibson/Rafter, *Criminal Man*, Chapters 32-41. Lombroso further breaks down *delinquenti d'occasione* into pseudo-criminals, criminaloids, latent criminals, and epileptoids. See Lombroso, *L'uomo delinquente*, 327-46.

⁷⁷ Cesare Lombroso, “Criminal Anthropology: Its Origin and Application” *Forum* v. 20 (September 1895), 33-49; 46.

⁷⁸ Gibson/Rafter, *Criminal Man*, 106-7; 100. In 1871 Lombroso published *L'uomo bianco e l'uomo di colore. Letture sull'origine e varietà delle razze umane* [The White Man and the Man of Color: Lectures on the Origin and Variety of Human Races] (Padua: F. Sacchetto, 1871).

⁷⁹ Dugdale, *The Jukes*, 8 for list of crimes committed by one sample of the family. One historian has claimed of Dugdale that “there was no one of his generation, not even Lombroso, whose work had greater impact on the new sciences of eugenics and criminology.” Ysabel Rennie, *The Search*

was a certain appropriateness to his utilization of Dugdale, as Lombroso's deepening socialist convictions coincided with the American's recommendations for penal reform, and their descriptions of the causes of crime were very close as well.⁸⁰ There was one crucial difference between Dugdale and Lombroso, though. Dugdale traced the pedigree of the Juke family, noting the consistent appearance in each generation of criminals, delinquents and paupers. But Dugdale did not *explain* this to be solely due to their heredity or a biological disposition to criminality. Rather, Dugdale explained that "the logical induction seems to be, that environment is the ultimate controlling factor in determining careers, placing heredity itself as an organized result of the invariable environment."⁸¹ The multi-causal etiology of degeneracy was, we have already seen, attended to, to an extent, in Lombroso's work to an extent. Yet when it was deployed in classic racist thought in the United States, it would be almost completely dropped, with a singular emphasis on the biological causes.⁸²

for *Criminal Man: A Conceptual History of the Dangerous Offender* (Lexington, MA: Lexington Books, 1978), 79. See also Mark Haller's history of American eugenics, which describes the quick acceptance of Dugdale in America as due to medical and scientific assumptions of heredity and an unconscious bias of college-trained middle class physicians and social workers. Haller, *Eugenics: Hereditarian Attitudes in American Thought*.

⁸⁰ Near the end of *The Jukes* Dugdale refers to "first offenders," a category very similar to Lombroso's "occasional criminals." He also recommended both "immediate" or "correctional" and "remote" or "preventive" methods of penal reform. Among the latter he included greatly enhanced sanitary improvements and child education. *Ibid.*, 110-12; 117-19.

⁸¹ Dugdale, *The Jukes*, 66.

⁸² See, for instance, Arthur Estabrook, *The Jukes in 1915* (Washington, D.C.: Carnegie Institution of Washington, 1916). In revisiting the degenerate New York family, Estabrook, who was a paid researcher of the Eugenics Record Office at Cold Spring Harbor, concluded that the family was still propagating the bad heredity. Estabrook noted that heredity "has its complementary factor in environment. The two determine the behavior of the individual." However much the environment might be improved or perfected, though, "the response of the individual will still depend on its constitution" which was more important than environmental changes or improvements. 85. See

Dugdale impressed Lombroso, as the Jukes seemed to verify his findings of *il delinquente nato*, and Lombroso consistently expanded his analysis of Dugdale's work on heredity in his books on criminology. In an extended treatment of the etiology of crime in a later edition of *L'uomo delinquente*, Lombroso devoted an entire chapter to crime and heredity, race, and disease.⁸³ After describing the effects of direct heredity – those characters directly passed from father to son – Lombroso inserted a large table that graphically showed the criminal heredity of the Jukes.⁸⁴ The bottom of the chart added up the totals of the various crimes by bloodline – the criminality of Juke blood far outstripped the ratio of delinquency of non-Juke blood. Before the rediscovery of Mendel's inheritance ratios, however, like Francis Galton, Lombroso's understanding of hereditary mechanisms was highly flawed.⁸⁵ He presumed a hereditary basis of criminality because a family had an abnormal number of criminals in it. He discussed parents that were insane, epileptic, alcoholic, and aged passing on their criminal dispositions to their children.⁸⁶ But he lacked any mechanism of

also Nicole Hahn Rafter, ed. *White Trash: The Eugenic Family Studies* (Boston: Northeastern University Press, 1988) for a collection of other family studies published in the period.

⁸³ See Lombroso, *L'uomo delinquente*, part IX, “eziologia del delitto,” Chapter 2, “Fattori ereditari ed ethnici.”

⁸⁴ *Ibid.*, 356-57.

⁸⁵ Lombroso, *L'uomo delinquente*, 356-57. Lombroso's column was broken down into two groups – the Jukes and “X.” Lombroso explained that “By X is meant the outsiders or those married into the Jukes but not originally derived from them.” 357.

⁸⁶ *Ibid.*, 360-61. Lombroso pointed out that the dual threat of aged criminals having offspring was not only that their children tended to be violent criminals (old fathers sired 52.9% of the murderers, 40% of assailants, 37% of the swindlers, and 30% of the rapists), but the longevity of the parents meant that they produced more children as well. 360. Issues of fecundity would be of paramount concern in American classic racialism.

transmission of the inheritance, any real concept of how heredity functioned on a cellular level, nor a consistent appreciation of non-hereditary factors that may have been equally important in developing the criminal tendencies of the family.

In the etiology of crime Lombroso linked criminal tendencies to races. For Lombroso, racial identity was organized around cranial circumference. Geographic regions with predominantly dolicocephalic cranial indices had homicide rates nearly double the Italian national average.⁸⁷ He pointed out that a large proportion of London's thieves were Irish, and that the most skilled English thieves hailed from Lancashire. He cited Alfredo Niceforo's book *Delinquenza in Sardegna* as documenting the importance of race as a factor in Sardinian crime. He argued that African and Oriental elements in the population were the major source of murder in Calabria, Sicily and Sardinia.⁸⁸ Albanians generated a bloodline of notable criminals in Calabria and Molise. Even Jews as a race had slight hereditary dispositions to lesser crimes as swindlers, perjurers, slanderers, usurers, and conspirers with brigands, but they were rarely assassins or murderers. Two pages later, though, Lombroso discussed a wide variety of social factors that also impacted the rates of criminality, which included economic conditions and

⁸⁷ Lombroso published a whole study of criminal and racial anthropology, *In Calabria (1862-1867)* (Catania: Nicolo Giannotta, 1898). Gibson describes Lombroso's racial anthropology in *In Calabria* as "this scientifically unsteady base of racial classification," upon which Lombroso built "elaborate and colorful typologies of criminal behavior." Gibson, *Born to Crime*, 105.

⁸⁸ Lombroso, *L'uomo delinquente*, 360-2. Curiously, Lombroso does not say elements of African or Oriental *blood*, though just pages before had mentioned the delinquency in the Jukes' bloodlines. American classical racialists would later claim that one of the reasons southern Italians, particularly Neapolitans and Sicilians, were undesirable was an infusion of African blood through their ancestors, so that their blood was corrupted and therefore racially inferior to that of the Anglo-Saxon or Teutonic races.

religion.⁸⁹ This confusion and ambivalence toward what were the decisive causes of crime – heredity or environment? – reflect the pre-Mendelian confusion about how heredity worked. When Mendel’s findings were rediscovered at the turn of the century, some Americans would take it as proof that there was no ambiguity or confusion, because they felt that Mendel had decisively shown how these characters were transmitted.⁹⁰

Because of this brief attention to the issues of race and heredity in a book ultimately numbering over 1000 pages, it fell to one of Lombroso’s students to explore more fully this expression of biological racial difference. Giuseppe Sergi provided a key conduit of Lombrosian determinism to the United States, through his participation in international conferences and the acquaintances he made with American researchers, and through his own writings on Italian racial anthropology. In this latter instance, we see the trans-Atlantic language of biological determinism migrating directly into the classic racialism of American nativists.

⁸⁹ Lombroso, *L'uomo delinquente*, 363 for Jews; homicide rates 362; 365-72 for social factors.

⁹⁰ On Mendel, Diane Paul suggests that Mendel seemed to seal the affirmative case for heredity. Paul, *Controlling Human Heredity*, 48. Also relevant is Elof Axel Carlson, *Mendel's Legacy: The Origin of Classical Genetics* (Cold Spring Harbor, NY: Cold Spring Harbor Press, 2004). Carlson argues that the period of “classical genetics” spanned the rediscovery of Mendel in 1900 to 1953, with the announcement of the Watson-Crick DNA model. Classical genetics fused breeding analysis, cytology, evolution, and reproductive and developmental biology into a scientific language that was assimilated into modern molecular genetics. Importantly, Carlson describes four phases of the “life sciences” in the 19th century, describing the fourth phase from roughly 1880-1900 as one in which experimental science was gradually displacing descriptive science in biology. Carlson uses “classical” in the same sense in which I use it in describing “classic racialism” – a method or language that had certain scientific foundations, but was ultimately supplanted by a more superior language (in this case of chemical strands of DNA). Also helpful for Mendel’s impact is Cravens, *The Triumph of Evolution*.

Giuseppe Sergi was born in Sicily, and left the island to assume the chair in anthropology at the university in Bologna in 1880. After moving to Rome to take the same position in 1884, he began to collaborate with Lombroso in the latter's journal, *Archivio di psichitria, antropologia criminale e scienza penali*. Although they shared the key assumptions of positivist criminology, namely, that physical form determined development and characteristics, Sergi and Lombroso used different measurements to determine racial identity. Whereas Lombroso and many other racial anthropologists utilized the cephalic index (the ratio of the width to the length of the skull), Sergi used only cranial morphology (the physical shape of the skull).⁹¹

Sergi's alternate method shows the lack of unity or agreement in physical anthropology that weakened the ability of opponents of classic racialism like Franz Boas to combat arguments of fixed racial identities. That there were several competing methods made it difficult to assert decisively that the essential idea was wrong. In 1893 Sergi described his new method, in which the shape of the skull and its volume capacity were indicators of separate human population groups. The volume capacity of the skull, Sergi believed, limited the size and development of the brain, in his mind the most important human organ. Sergi believed his new method to be superior because his system was attuned to measurements and dimensions of internal skeletal structure, which, he thought,

⁹¹ See P. D'Agostino, "Crania, Criminals"; Gibson, *Lombroso*, and Gould, *Mismeasure*. The German-born American anthropologist Franz Boas would ultimately demonstrate the cephalic index to be a very poor indicator of racial identity in his studies for the United States Immigration Commission in the late 1900s, the same Commission that used Sergi's work to affirm the racial difference of Southern Italians.

were fixed and unchanging. While internal skeletal structure – especially cranial capacity – divided all human beings into two great “species,” other physical aspects like skin or hair color were more fluid and changeable. The environment had a pronounced effect on these external elements of population groups, but never changed the internal skeletal structure.⁹²

Because he believed the skeletal structure was unchanging, Sergi was able to talk about population groups on an archeological or geologic scale of time. He posited two fundamental, basic human species, the Eurafrican and the Eurasiatic, organized around four primary cranial forms. These great species then broke down further into varieties, marked by external characters like skin, hair, and eye color. From these varieties, population groups were then subdivided into ethnic groupings, primarily arranged by linguistic, cultural and historical associations. After examining 3500 human crania, Sergi noted that he could not answer how many different varieties there ultimately may be, but explained that “I affirm with some personal satisfaction that, as regards the new anthropological method, I have surmounted its uncertainties.” Sergi insisted that his method was superior because its measurement of the cranial capacity showed “the persistence of forms from immemorial epochs, and their reproduction through numerous generations

⁹² Giuseppe Sergi, *La varietà umane: Principi e methodo di classificazione* (Turin: Bocca, 1893); also published as “The Varieties of the Human Species: Principles and Methods of Classification” *Smithsonian Miscellaneous Collections* v. 38, n. 969 (November 1894); Giuseppe Sergi, *Origine e diffusione della stirpe mediterranea* (Rome: Dante Alighieri, 1895); Giuseppe Sergi, *The Mediterranean Race: A Study of the Origin of European Peoples* (London: Walter Scott, 1901); Giuseppe Sergi, “My New Principles of the Classification of the Human Race” *Science* v. 22, n. 564 (24 November 1893), 290. See also John Weeks, *The Library of Daniel Garrison Brinton* (Philadelphia: University of Pennsylvania Museum, 2003) which lists several of Sergi’s books in the American anthropologist’s library. Brinton is discussed later.

notwithstanding amalgamation with other types.” Sergi claimed that he had “clearly demonstrated” this persistence, which had the ultimate implication of supporting the belief in immutable and unchanging skeletal forms, a significant component of classic racialism.⁹³ And with English translations of his books, Sergi made these ideas accessible to the American community.

Sergi ran into two difficulties, however, in presenting his new classification scheme. The first was the large number of population groups his measurements showed – at least sixteen groups of human varieties, which made it an unwieldy classification scheme. Second, in to introduce a new method of measurement, Sergi would have to displace the old. This, he was aware, would not be easy. In training and education, scientists absorb a particular approach and methodology to science, which Thomas Kuhn has described as a paradigm. New paradigms replace “normal” scientific paradigms when the explanatory utility of the normal paradigm begins to falter. But the old method was not particularly vulnerable, and Sergi noted, “The scientist cannot, indeed, free himself of certain sentiments which are acquired in following scientific habits and which have become a part of scientific and public opinion...” In this capacity a scientist’s training became essential in structuring his or her openness to new ideas.⁹⁴ Sergi was personally willing to make this leap to the new method that he proposed in *La varietà umane*, but gaining followers and adherents for such a complex scheme was another matter.

⁹³ Sergi, “Human Varieties,” “surmounted” from 47; “amalgamation” from 12.

⁹⁴ Ibid., 20-21. See also Kuhn, *Scientific Revolutions and The Road Since Structure*.

It was not Sergi's method that was particularly dramatic, though. It was the way he used it to recast European racial history. Sergi tried to combat the perception that the Italians of the south (Sergi was Sicilian) were racially different from Nordics or northern Europeans. In Sergi's scheme bands of Eurafricans emerged from Africa and made their way into Europe. The physical characteristics of the skull and face of the Eurafrican species, which stretched from the Red Sea to the Atlantic, was "uniform." But the physical characteristics of the skin and intermediate parts, "that is to say the development and form of the soft parts, vary."⁹⁵ Elements of this Eurafrican race peopled the British Isles, France, Italy and Africa; from this foundation grew Greco-Latin civilization. The barbarians and savages that filtered into Germany from the Russian borderlands were the Eurasiatics who destroyed the Roman Empire. Sergi was trying to create a racial affinity between Italians, the French and British by suggesting that in some primeval way, they developed from the same species (the Eurafricans), which then splintered into African, Mediterranean, and Nordic varieties. The "Germans and Scandinavians," (i.e. Nordics) were the "primitive" inhabitants of the Eurafrican species that were concentrated in the Scandinavian peninsula. At the end of the Neolithic period, the Eurasiatic species that formed various Celtic, Slavic and Germanic varieties emerged into eastern Europe. This made the Aryans, in Sergi's scheme, Asiatic. So although he had based his classification on observation, his work was ultimately driven by an agenda.⁹⁶

⁹⁵ Sergi, *The Mediterranean Race*, 249.

Sergi's project had additional significant impacts other than to buttress Italian nationalism. Peter D'Agostino argues that Sergi's "browed Europe" had strong nationalist overtones combating the emergence in northern Europe of pro-Aryanism or Indo-Germanism.⁹⁷ In his attempt to displace Aryans from the center of the narrative of the development of European civilization, Sergi offered members of the United States Immigration Commission a theory that skeletal forms of the two different species did not change. That the physical structure of these different races had remained uniform, unchanging and trans-historical implied that the physical differences *between* species might also be permanent and unchanging. And likely his association of Mediterraneans with a "Eurafrican" species did not bode well for dark-skinned Italian emigrants arriving in the United States, which was already acutely aware of skin color and race.⁹⁸

Sergi's Italian edition of *Specie e varietà umane* of 1900 was used, like *The Mediterranean Race*, by the United States Immigration Commission to devise a definitive classification of immigrant population groups. Sergi's tri-partite system of classification of species, variety or race, and physiognomic type could then be further organized by non-physical factors like language, social form,

⁹⁶ *Ibid.*, v-vii.

⁹⁷ See P. D'Agostino, "Craniums, Criminals," 24-27; 25.

⁹⁸ Sergi, *The Mediterranean Race*, especially chapter 13. These would effectively be "Alpines," who occupied the Alps, parts of Germany and France, plains of Russia, and a great part of Central Europe and the Po Valley. *Ibid.*, 263. For the Eurasiatrics, four principle skull shapes linked them together: cuboid, cuneiform or sphenoid, spheroid and platycephalic. For the Eurafricans the four types were: pentagonoides, ellipsoidal, ovoid and beloides (arrow-shaped). Sergi, *The Mediterranean Race*, 263 for Eurasiatrics; 256 for Eurafrican. See also Andrea Orsucci, "Ariani, indogermani, stirpi mediterranee: aspetti del dibattito sulle razze europee (1870-1914)", *Cromohs*, vol. 3 (1998), 1-9.

family form, intellectual and emotional characteristics, and forms and modes of activity.⁹⁹ While the Immigration Commission's "Dictionary of Races or Peoples" tended more to the older methods of anthropological measurement, Sergi's distinction between two main human species that subdivided into varieties or races, which were then further subdivided into physiognomic types, did set a template for the organization of European population groups. *Specie e Varietà* closed by reiterating Sergi's belief in the utility of cranial and facial measurements as markers of unchanging species types. He insisted that the internal characteristics that were determined by skull and facial measurements (the two absolutely primary skeletal structures in his scheme) were "constant and persistent in their form and do not change from exterior influences of habitat, climate or diet..."¹⁰⁰ His insistence on immutable and unchanging physical forms would ultimately provide compelling arguments in favor of excluding certain types of immigrants from entering the United States.

Sergi's system was meant to abolish confusion about population groups by classifying "species" as population groups fundamentally linked by skull shape and capacity. But moving on to varieties and then to types was riddled with confusion, because Sergi argued that the characteristics of varieties and types could vary and change. He ended the book by effectively endorsing the American

⁹⁹ Sergi, *The Mediterranean Race*, 171-72. Lombroso's English biographer Mary Gibson sees in Sergi's assertion of psychological differences between the Eurasiatic species and Eurafrikan species, which he claimed were based on their physical differences, to be "Undocumented by any rigorous data, [and his] analysis showed that Sergi was as careless as his colleagues in assuming that biology determined psychology." Gibson, *Lombroso*, 114.

¹⁰⁰ Sergi, *Specie e Varietà*, 168. The verb Sergi uses is "subiscono" to express the cranial resistance to change.

“melting pot” theory by suggesting that the original English population of North America, leavened with Irish, Scandinavian, German, French, Italian and Russian varieties, among others, would ultimately merge [“assumeranno”] into a distinctly American race.¹⁰¹

So how stable was physical form then? If the kindred races of the Eurafrican species diverged to form varieties of Africans, Italians, and Anglo-Saxons, but the only place their similarities could be seen was in their skeletal structure, how practical a system was this? The contradictions and complexities of Sergi’s system limited its utility in terms of classifying and organizing human population groups. Physical anthropology was plagued by multiple organizational schemes including the cephalic index and cranial capacity, among others. However complex this system of Sergi’s may have been though, the basic agreement he had with Lombroso was that the physical structure of bodies dictated what an individual was, whether that was a criminal, a Eurafrican or Eurasiatic. These essentialized concepts would make important contributions to American racial thought in the early decades of the twentieth-century.

Unfortunately, although both Galton and Lombroso made significant use of Robert Dugdale’s study of the Juke family, they appear to have skimmed past Dugdale’s explanation of his method of research and his cautionary warnings. The American described two potential statistical methods that could be used to examine the genealogical data of the family that he had accumulated: Positive Statistics and Conjectural Statistics. For *The Jukes* he chose to use the former – a

¹⁰¹ *Ibid.*, 173.

method like that employed by the Census Bureau, which enumerated and coordinated facts of similar type to reduce them to a common measure of quantity, frequency, time and place.¹⁰² He perceived positive statistics to be the simplest and safest scientific method for handling his data. But he emphasized the hazards of Positive Statistics, namely its liability to “a fundamental error, that of comparing similar facts which are not identical because they do not occur under similar conditions...” He expressly warned that Positive Statistics “does not explain the causes or consequences of facts, therefore conclusions drawn from its figures are inferential and *may lead to mistaking coincidences for correlations*, as where it is concluded that because criminals show a larger percentage of illiteracy than the average of the community, therefore illiteracy is a cause of crime.”¹⁰³ This was precisely what Galton, Lombroso, and the American classic racist community did.

Dugdale’s use of illiteracy as causing crime for an example was eerily prescient for the American community. When southern Italians began to emigrate to the United States in large numbers at the end of the century, the high rates of illiteracy in the Italian south, Americans believed, was directly correlated to the

¹⁰² Dugdale, *The Jukes*, 9. Dugdale’s Positive Statistics and Conjectural Statistics are described on 9ff. Giddings, a professor of Sociology at Columbia University, described Dugdale’s work as “the best example of scientific method applied to a sociological investigation” in his foreword to the 1910 edition. Page iii. Giddings also reminded his readers that “Far from believing that heredity is fatal, Mr. Dugdale was profoundly convinced that ‘environment’ can be relied on to modify, and ultimately to eradicate even such deep-rooted and wide-spreading growths of vice and crime as the ‘Jukes’ group exemplified.” iv. This would be precisely the avenue that more progressive scientists would explore as “positive eugenics.” Dugdale described “Conjectural statistics” as made up of largely political arithmetic and probability theory. 10.

¹⁰³ *Ibid.*, 10, emphasis added.

growth of crime in urban areas with Italian settlements. Furthermore, that so many were illiterate suggested some type of inferiority or backwardness. Perhaps this was due to a biological difference, one that made them racially incompatible with the American Anglo-Saxon stock. If criminality and illiteracy could be linked in the minds of American xenophobes, perhaps they could be linked to biology or heredity too.

Galton, Lombroso and Sergi all laid out schemes whereby heredity and biology formed specific types of people. Whether by talent, depravity, or physiognomy, the physical and mental characteristics of human beings were determined by their basic, biological inheritance. These ideas were rapidly assimilated into the consciousness of an American public inclined to practical scientific thinking. They formed a cornerstone of classic racialism that the American public began to understand in the first decades of the twentieth century. But as an example of the evolutionary nature of scientific revolutions – what Peggy Pascoe calls “intellectual trickle-down theory” – the public’s understanding was wrong.¹⁰⁴ Modern biology had no certainty as to how exactly heredity worked, but modern biologists knew the methods to achieve certainty, and these were derived from experiment and research. Jennings may not have known how inheritance passed from generation to generation the natural characteristics of his little *paramecia*, but he knew that the only way to understand their development was microscopic research and experimentation.

¹⁰⁴ Pascoe, “Miscegenation Law,” 97.

At his Naples research tables, at the Great Lakes, and researching in laboratories in Ann Arbor, Philadelphia and Baltimore, Jennings embraced the new experimental methods. But the public largely ignored Jennings's science. They could not grasp the complex associations of germ-plasm or genes when even professional scientists only dimly apprehended them. When the American biological community did finally begin to understand the mechanisms of heredity, the complexity of chromosomes and genes prevented the public from solidly grasping the principles. The key was simplicity. Modern experimental science simply could not yet describe or explain succinctly and simply how heredity worked or was transmitted. Although they were wrong, classic racialists's principal idea was elegantly simple: like produces like. This dissertation is a study of these tensions over the bodies and biology of immigrants into the United States.

PART I: THE PROGRESSIVE IMPULSE OF RESTRICTION

Chapter One examines ways Americans conceptualized immigration to the United States, in terms of demographic, racial, economic and social impact, and how these fears shaped the development of classic racialism. It establishes a template for regarding immigration as a “problem,” something that increasingly disrupted the stable development of American civilization as the sources of immigration became less homogeneous – that is, as immigration shifted from its “old” sources to “new” sources in southern and eastern Europe. The formation of the Immigration Restriction League and their focus on the passage of an “illiteracy” test to restrict immigration is one of the key developments the chapter explores, along with the intellectual contributions of William Z. Ripley and Francis A. Walker, who provided the critical base for the development of racialized nativism in the United States.

Chapter Two considers the role of critics of American classic racialism, examining the intellectual training that Herbert Spencer Jennings, Raymond Pearl and Franz Boas received, which later compelled them to criticize the highly problematic assumptions of classic racialism. The sophisticated scientific and intellectual training they (and others) received predisposed them to approach biological and hereditary issues of race and human development in fundamentally different ways from determinists and classic racialists like Charles Davenport and Prescott Hall.

Chapter Three follows the work of the United States Immigration Commission’s comprehensive investigation of immigration to America. The pressure that restrictionists placed on commissioners to secure a report favorable to excluding certain races of immigrants is an important manifestation of the political influence groups like the Immigration Restriction League wielded. Of especial importance are three volumes that the Commission published – “Immigration and Crime,” the “Dictionary of Races or Peoples,” and Franz Boas’s volume on the “Changes in the Bodily Form of Descendants of Immigrants.” The Commission’s final reports demonstrate in particular the influence that classic racist ideas had on influencing the recommendations of the special Congressional committee.

Chapter Four looks at how inaccurate scientific knowledge was disseminated through organizations like the Race Betterment Conferences, and how the assumptions of classic racialists were increasingly challenged by professional academics, who were in the final analysis unable to definitively state with confidence what exactly scientific knowledge had established as true. The critical professional reception of work by Davenport and Madison Grant in particular show an increasing understanding that classic racist ideas regarding heredity and development were wrong, but modern experimentalism was as yet unable to declare what was correct and scientifically true.

Chapter 1: The Problem of Immigration

Francis Galton may have believed that emigrants were “on the whole men of exceptional and energetic natures,” but in the United States at the end of the nineteenth century, he would have held a minority opinion. Native-born Americans viewed with alarm the rapidly increasing numbers of European migrants landing on American shores. “So broad and straight now is the channel by which this immigration is being conducted to our own shores,” one concerned American noted in 1891, “that there is no reason why every stagnant pool of European civilization, the worst defeats in the struggle for existence, the lowest degradation of human nature, should not be completely drained off into the United States.”¹ So feared General Francis A. Walker, former superintendent of two censuses of the United States, and in 1891 the President of the Massachusetts Institute of Technology. Walker knew firsthand the impact that immigration to the United States was assuming in the 1890s from his work on the Census. As he began laying out the demographic and statistical implications of the human tide crashing on American shores, Walker found an increasingly receptive audience among native-born Americans from “old” families, whose Anglo-Saxon and Protestant heritage were critical elements of their self-identity, which simultaneously reflected how they perceived the racial composition of the United States as well. These “racial nationalists,” as John Higham called them, turned

¹ Francis Galton, *Human Faculty*, 308; Francis Amasa Walker, “Immigration and Degradation” *Forum* v. 11 (August 1891), 634-44; 644.

increasingly to an idiom of science to both articulate and understand the changes occurring in the United States around the turn of the century.²

The introduction of biological definitions of “race” began shifting the meaning of American nativism. “Racial nationalism,” Higham explains, “having arisen out of political and literary speculation, not out of scientific inquiry, displayed a characteristic vagueness.” A romantic or literary origin had contributed, both to the United States and England, an Anglo-Saxon tradition or ideology that embraced “the inner vitality of one’s own culture.” But at the turn of the century, the character of racial nationalism began to change, and as it incorporated classic racist ideals – Higham calls it “racial science” – it transformed Anglo-Saxon nationalism from a vague concept into “a sharp-cutting nativist weapon...a completely racist philosophy.”³ This chapter examines the role that men with professional and scientific credentials played in articulating the problems of immigration to the United States.

The clarity with which statistics depicted trends in the development of the United States made it an important source of restrictionists’ anxieties over the quality of immigration. Administrative needs of the state and economic interests (for example, life insurance companies) needed regular statistical information to function properly, and the growing social sciences helped articulate methods and categories of study like demography, moral statistics (e.g. criminality and

² See Higham, *Strangers in the Land*, chapter 1 for an introduction to the various strands of American nativism.

³ *Ibid.*, 11; 133; 134. I would again emphasize here that defining these sentiments as “scientific” is inadequately imprecise in the context of the changes occurring in methods and forms of scientific investigation. It *was* racial, but *not* “science” to men like Jennings, Pearl, or Boas.

pauperism) and medical and psychological statistics (insanity and mortality and birth rates). The social and racial implications of the statistics were important concerns to the public.⁴ One of the core fears of the opponents of immigration was that these aliens would outbreed the native-born Americans, fundamentally changing what nativists saw as the basis of the American racial stock. Walker played a central role in articulating in the late nineteenth century a theory that the “old stock” was being “replaced” by the arrival of different immigrant stocks, a theory that reinvigorated the immigration restriction movement in the United States.⁵

Born in Boston in 1840 to a father who was an economist, Francis Amasa Walker became one of the premier American statisticians of the nineteenth century. His contributions to statistics were well received on the European side of the Atlantic where theoretical work in statistical science was more advanced than in America. Walker’s 1875 book *The Statistical Atlas of the United States* won first prize at the Paris Geographical Congress, despite the fact that Walker had never taken any formal statistical training. In the United States, though, as the historian Patricia Cline Cohen has argued, statistics were more practical and

⁴ Robert C. Davis, “The Beginnings of American Social Research,” in George H. Daniels, ed. *Nineteenth Century American Science: A Reappraisal* (Evanston, IL: Northwestern University Press, 1972), 152-78. See also the recent book by Sarah Igo, *The Averaged American: Surveys, Citizens, and the Making of a Mass Public* (Cambridge, MA: Harvard University Press, 2007), which argues that between the 1920s and 1950s, empirical surveys came to be seen as increasingly effective ways to create a “mass society,” and particularly disclose what existed as statistically “normal” behavior, as in the Lynd’s survey of *Middletown* and the Kinsey sex studies.

⁵ John Higham explains that “Since no clear-cut difference of complexion was apparent between native Americans and any European group, the old instincts of white supremacy did not extend to the new immigration as easily as they did to the Chinese. To a large extent, race lines would have to be manufactured.” John Higham, *Send These to Me: Immigrants in Urban America* rev. ed. (Baltimore: Johns Hopkins University Press, 1984 [orig. 1975]), 45.

utilitarian. The strong capitalist base of the United States economy in the mid-nineteenth century required strong calculating skills, so the origin of statistical thinking in the United States was centered more on practical business mathematics rather than on complex theoretical propositions. European observers, she explains, were impressed by the extent to which ordinary Americans thought in terms of calculations and computations, but Americans rarely produced a theoretical thinker of the stature of Galton, Adolphe Quetelet or Joseph Fourier. It was, in essence, a fundamental difference between theoretical and applied statistical sciences.⁶ Walker's international recognition made him a logical choice to direct the 1870 census of the United States. His effort, however, was plagued by methodological and computational errors, though this was not particularly the fault of Walker but was instead due to the lack of professional training for the enumerators and statisticians who copied the final results. Walker was retained for the 1880 census, and under a new law that gave him greater control over the selection of enumerators, helped create a more professional and accurate enumeration.⁷ But lingering problems with analysts manipulating statistical

⁶ Patricia Cline Cohen, *A Calculating People: The Spread of Numeracy in Early America* (Chicago: University of Chicago Press, 1982); see also Margo Anderson, *The American Census: A Social History* (New Haven: Yale University Press, 1988); Theodore Porter, *The Rise of Statistical Thinking, 1820-1900* (Princeton, NJ: Princeton University Press, 1986). Anderson attributes some of the severe methodological problems in the 1840 and 1870 censuses to the shortage of sophisticated theoretical methods for accurately obtaining and analyzing census data despite this widespread numeracy.

⁷ A. W. Coats and Patrick Williams, "Walker, Francis Amasa" John A. Garraty, Mark C. Carnes, eds. *American National Biography* (New York: Oxford University Press); Jeannette P. Nichols, "Walker Francis Amasa" Allen Johnson et al, eds. *Dictionary of American Biography* (New York: Charles Scribner's Sons, 1958-1964). See also Hyman Alterman, *Counting People: The Census in History* (New York: Harcourt, Brace and World, 1969); Cohen, *A Calculating People*; Anderson, *The American Census*; Carroll D. Wright, *The History and Growth of the United States Census* (Washington, D.C.: Government Printing Office [GPO], 1900) which are all useful for studying

correlations into causation and statistical inaccuracies persisted, reflecting many of the later problems eugenicists and classic racialists would have with their data, which like the census, was also collected by field investigators. As the problems in Galton's work showed, the tendency to conflate correlation and causation was a difficult conceptual obstacle, one not always easily surmounted.

Though he left the Census Bureau in 1881 to become the president of the Massachusetts Institute of Technology, Walker continued to follow developments in American statistical science, and began digesting and analyzing some of the trends he found in the enumerations and tables. John Higham considers Walker's statistical and economic contributions to be the only "original contribution to nativist thought" in the United States, and that Walker exerted a "telling intellectual influence" on racial nativism by coupling it with Darwinian fears of the survival of the fittest.⁸ The publication of the census of 1890 gave the MIT president plenty of material to ponder, as it did many other academics. Of the larger demographic trends taking place in the country, Superintendent Robert Porter's introduction pointed out that "more than one-third of the total immigration to this country since 1820 has come during the last ten years," and the vast majority, the returns showed, lived in urban areas. Although the percentage of foreign-born aliens in the United States arriving from southern and eastern Europe had not yet surpassed the percentage from the north and west of

the growth of Census categories. The Appendix of the latter is particularly helpful, as it republished many of the schedules of inquiry of the early censuses. The U.S. Census of 1850 similarly republished the early inquiries.

⁸ Higham, *Strangers in the Land*, 142.

Europe, the tendency of the new immigrants to settle in cities made them more visible.⁹ Additionally, the rapid growth of urban areas, particularly in the northeast, created many problems with effective management of urban services, housing, poverty, crime and corruption.

Statistical evidence showed that native-born Americans, particularly those of an Anglo-Saxon and Protestant background were not reproducing at the rate they previously had. In terms of population growth, no country had ever experienced such demographic expansion as the United States did in the century after its independence. While much of the gain was due to natural increase, there were important contributions to the nation's human stock from European immigration even in the first decades of the country's existence, although the contribution was difficult (if not impossible) to accurately determine because no official records of immigration were kept until the middle of the nineteenth century. As the population of the country grew, so did the categories that the United States Census enumerated. In an effort to determine the origins of the immigrant population, in 1850 enumerators were instructed to ask about the nativity of respondents. In 1870 the accumulated statistics of criminality and

⁹ United States Department of Commerce, Bureau of the Census, *The 11th Census of the United States* (Washington, D.C.: Government Printing Office, 1895), vol. I, p. lxxix. On page lxxx of the introduction, the total number of immigrants arriving between 1881 and 1890 was listed by country of origin: out of a total of 5,246,613 immigrants in those years, almost three million originated in the United Kingdom and Germany. Italy provided only 307,309 of the remainder, and Russia and "Poland" combined for 265,088. Although the population of the country was only 22.34% urban (defined as an incorporated area with a population greater than 2500), 44.13% of immigrants lived in urban areas (p. xc). *Ibid.*, V. 2, part 2 ("Social Statistics of the Population") listed the literacy of America's population, and showed a much higher proportion of illiterate immigrants than native-born illiterates (pp. xxxi-xxxv).

pauperism were broken down by color and nativity.¹⁰ These early census returns still showed, however, that the geographic origins of the immigrants to the United States was sufficiently similar to enable the forging of a particularly robust “old stock” of Anglo-Saxon and Teutonic origins. This was the beginning of what Higham describes as the “recasting of the Anglo-Saxon tradition into the mold of a gloomy, scientific naturalism,” and statistical evidence was key to that transition and the anxieties that arose from fears of natural selection.¹¹

Statistics showed that this demographic growth was primarily in urban areas, which created a highly visible foreign-born population. In 1900 the entire urban population of the country was 20 million, out of a total population of 76 million.¹² The proportion of the urban population was especially high in the North Atlantic region, where it was 58.6% of the total population, but the national total was also inching upwards, standing at 32.9% in 1900.¹³ It is not unusual, then, that anti-immigrant agitation in the late 1890s gathered steam in these northeastern states as native-born Americans realized this new change in the basic demography of the country. Furthermore, between 1890 and 1900 it became clear that the source of immigration was shifting to the southern European “races.” Politicians

¹⁰ United States Department of Commerce, Bureau of the Census, *Seventh Census of the United States, 1850* (Washington, D.C.: R. Armstrong, 1853) v. 1, p. xxxvi; United States Bureau of the Census, *Ninth Census of the United States, 1870* (Washington: Government Printing Office, 1872), v. 1, p. 568ff.

¹¹ Higham, *Strangers in the Land*, 144.

¹² United States Department of Commerce, Bureau of the Census, *The Twelfth Census of the United States, 1900* (Washington, D.C.: Government Printing Office, 1901), vol.1, pp. lxxii-lxxiii. The exact numbers are 19,757,618 urban residents out of 76,303,387.

¹³ *Ibid*, p. lxxxii.

like Representative Henry Cabot Lodge of Massachusetts argued that these groups were racially inferior and that politically the United States had every right to restrict them, while academics like William Ripley their racial and biological difference, and the Census showed their dramatic growth in numbers.¹⁴

The dramatic increase in immigration and its changing characteristics led Walker to publish articles in 1891 and 1896 in which he described, for “old stock” native-born Americans, a new and worrisome development in American demographics. In “Immigration and Degradation,” published in the popular magazine *Forum* in August 1891, Walker showed that between 1790 and 1830, the population of the United States grew by 227%, from just under 4 million to over 12 million in 1830.¹⁵ This was by and large a natural increase of the native born population, since immigration in these early decades, despite very poor record keeping, never exceeded 151,000 over a ten-year period. In the 1830s, immigration increased to almost 600,000 for the decade. Walker found that over time the growth rate of the native population, which averaged over 30% in each of the first four decennial enumerations, began to slacken. Although the population of the country “was almost exactly what it would have been had no increase in foreign arrivals taken place,” demographic growth after mid-century was not from natural increase of the native-born population, but from the addition of these alien

¹⁴ For instance, see Henry Cabot Lodge, “Lynch Law and Unrestricted Immigration” *The North American Review* v. 152, n. 414 (May 1891), 602-13; *idem.*, “A Million Immigrants a Year: I. Efforts to Restrict Undesirable Immigration” *The Century Magazine* v. 67, n. 3 (January 1904), 466-9; William Z. Ripley, *The Races of Europe: A Sociological Study* (New York: D. Appleton and Co., 1899); *idem.*, “The European Population of the United States” *Journal of the Royal Anthropological Institute of Great Britain and Ireland* v. 38 (July-December 1908), 221-40.

¹⁵ Francis A. Walker, “Immigration and Degradation” *Forum* v. 11 (August 1891), 634-44; 634.

immigrants.¹⁶ Walker argued that the decline in native-born fecundity was not due to the inability of the native stock to reproduce, but rather was caused the “*appearance of the foreigners themselves.*”¹⁷ He suggested that arrival of immigrants was inhibiting the native Anglo-Saxon stock’s reproduction, which would have the alarming effect in the long run of replacing the Anglo-Saxon stock. While it was statistically true that native-born Americans were producing fewer offspring than previously, the cause of that decline was not clear-cut as Walker posited.

Walker wrote that this conclusion could be explained only three ways: by coincidence, by an actual decline in vitality of the native population, or by his radical idea that immigration constrained native-born reproduction. After dismissing the first two by using statistical data, Walker demonstrated his “replacement thesis” by utilizing both economic and moral arguments. The native stock, he explained, expected a standard of living that was much higher than the expectations of the immigrants. Americans would not lower themselves to compete in an economic sphere with immigrants who would labor at dramatically lower wages. Ethically, he claimed, the native-born American “was even more unwilling to bring sons and daughters into the world to enter into that competition.”¹⁸ Walker made the implication of this moral and economic constraint explicit: “foreign immigration into this country has, from the time it

¹⁶ Walker, “Immigration and Degradation,” 637-38; 638.

¹⁷ *Ibid*, p. 637. Emphasis in original.

¹⁸ *Ibid*, p. 641.

first assumed large proportions, amounted not to a re-enforcement of our population, but to a replacement of native by foreign stock.”¹⁹ Walker also detected a troubling difference in the type of immigrant that was now arriving on American shores. No longer were immigrants the “more alert and enterprising members of their respective communities,” those men of ability and exceptional energy that Galton had described. Because modern technology made the Atlantic passage a quick and relatively safe journey, immigrants arriving now were “the unlucky, the thriftless, the worthless...”²⁰ Aided by statistical data, Walker’s “replacement” thesis suggested that the long-term extermination of the old native stock of the American people was, in the face of unrestricted immigration, inevitable. Yet when he wrote his first essay in 1891, few heeded his warnings.²¹

After Walker’s 1891 essay on immigration and racial degradation, several events transpired that would invigorate the nascent American statistical movement and refocus the public’s attention on demographic figures. In an 1893 lecture to the American Historical Association, the then-unknown young historian Frederick Jackson Turner used demographic data from the 1890 census to learn about the main characteristics of American development. Turner’s “frontier

¹⁹ *Ibid*, p. 642.

²⁰ *Ibid*, p. 643.

²¹ Historian Theodore Porter’s study of the development of statistical thinking places much emphasis on the European Enlightenment as the genesis of proper theoretical statistics, particularly as seen with Adolphe Quetelet’s *l’homme moyen*. But it was Francis Galton, the British polymath, who first used statistics to study natural variation, which Porter argues “also provided the key in biology of the quantitative study of heredity, leading to what is now the most purely statistical of the natural sciences, quantitative eugenics.” Porter, *The Rise of Statistical Thinking, 1820-1900*, 110.

thesis” suggested that the western expansion of the country acted as a school for Americanizing immigrant settlers on the frontier, forging a new breed of “Americans” that were rugged, independent, hard-working and resourceful. The frontier, for Turner, is what *made* an American; by pushing further and further away from the east coast and its continuing attachment to Europe, truly American institutions emerged and took hold in the western frontier. For old-stock natives fearful of the invading immigrant horde, Turner declared “in the crucible of the frontier the immigrants were Americanized, liberated, and fused into a mixed race, English neither in nationalities nor characteristics.”²² Dramatically, Turner reiterated the 1890 Census Superintendent’s declaration that the frontier was now “closed”—the open tracts of unsettled land had reached a population density that technically defined the western frontier territories as settled and inhabited. There no longer existed that open area which forged a hardy American population. Turner closed his address by noting that “now, four centuries from the discovery of America, at the end of a hundred years under the Constitution, the frontier has gone, and with its going has closed the first period of American history.”²³ This served to fuel the anxiety of the old-stock American population.

With the description of the close of the frontier, and in the aftermath of a severe cholera outbreak in Europe in 1891, the Panic of 1893, and widespread labor unrest in 1894, Walker revisited immigration in an article for *The Atlantic*

²² Frederick Jackson Turner, “The Significance of the Frontier in American History,” in Frederick Jackson Turner, *The Frontier in American History* Introduction by Ray Allen Billington (Huntington, New York: Krieger Publishing Co, 1976), 4, 23. Turner’s lecture was originally delivered at the AHA in Chicago, July 12, 1893 during the World’s Columbian Exposition. Note that Turner imagined the ethnic base of the American population to be fully English *in origin*.

²³ Turner, “The Significance of the Frontier,” 38.

Monthly in 1896.²⁴ The volume of immigration had recently become so great, he warned, that the new arrivals, some “unfit to be members of any decent community” were not being carefully examined as they passed through entrance ports, which served as the first defensive bulwark against the admission of inferior, diseased, criminal, or subversive immigrants. Although they contributed an increasing proportion of the population of prisons, asylums, and almshouses, this was not his greatest fear of the new increased volume of immigration. Instead, he explained, it was the “vast throngs of ignorant and brutalized peasantry” that threatened the wages of American laborers, the American standard of living, and the quality of American citizenship. Walker expressed a note of urgency in this article, explaining that current conditions in the United States necessitated swift action to protect the quality of American life and its population.²⁵

Demographic data was central to fueling anxiety over immigration. The historian Mae Ngai has suggested that demographic statistics “was to twentieth-century racists what craniometric data had been to race scientists during the nineteenth....If statistics showed that immigrants were less healthy, less educated, and poorer than native-born Americans, the data were deemed to be evidence of

²⁴ On the cholera outbreak and its effects on immigration, see Alan M. Kraut, *Silent Travelers: Germs, Genes, and the “Immigrant Menace”* (New York: Basic Books, 1994); Frank M. Snowden, *Naples in the Time of Cholera, 1884-1911* (Cambridge: University of Cambridge Press, 1995).

²⁵ Francis Walker, “Restriction of Immigration” *The Atlantic Monthly* v. 77, n. 464 (June 1896), 822-29; 822; 827.

the immigrants' inferior physical constitution, intelligence, and ambition.”²⁶ The close of the frontier, the fall of agricultural prices, labor problems, the economic depression after 1893 and public health threats from immigration suggested that the immigration question demanded new answers. The old policy of welcoming anyone to American shores, Walker insisted, must be curtailed. The old immigrants that first populated the North American continent had had to be “enterprising, thrifty, alert, adventurous and courageous” – Galton would have been in agreement – in order to survive the rugged, undeveloped American environment. Now, the new arrivals did not have to worry about these difficulties that old stock pioneers had faced, and consequently, were of poorer quality.²⁷ Walker hoped his article would finally force Americans to seriously address the immigration problem. In 1896 the Immigration Restriction League was listening, and ready to help.

The Immigration Restriction League [IRL] was founded in 1894 by “five young blue bloods” who, John Higham notes, came from “well-to-do, long-established families, steeped in Boston ways and Boston ideas....determined to mount a counteroffensive against the strange invaders who seemed so grave a threat to their class, their region, their country, and their race.”²⁸ Prescott Hall, Robert DeCourcy Ward, Joseph Lee, Robert Treat Paine and Richards Bradley

²⁶ Mae Ngai, *Impossible Subjects: Illegal Aliens and the Making of Modern America* (Princeton, NJ: Princeton University Press, 2004), 31-2.

²⁷ Walker, “Restriction of Immigration,” 827.

²⁸ Higham, *Strangers in the Land*, 102.

would be actively involved in protecting America from these alien immigrants for decades. The historian Barbara Solomon, who studied the League extensively, argued that an “Anglo-Saxon complex” underpinned the activities of League members, who were buffeted by social, economic, and racial anxieties in their long fight to secure restriction. They were sophisticated in terms of their intellectual approach to achieving restriction, and they were well connected to political power through their professional paid lobbyist, James Horace Patten.²⁹

One of the most complex facets of the Immigration Restriction League is their racial ideology. Numerous scholars in the last ten years have commented on the multiple valences that “race” as an identity marker carried. For members of the Immigration Restriction League, “race” existed not in an abstract, intellectual sphere, but in the day-to-day realities of their lives. Races of inferior immigrants settled in their cities, corrupted their politics, and intensified rates of poverty. Yet “race” also existed as something to be handled cautiously because it was a very amorphous concept. As they preached their restrictionist doctrine, many of the core members showed confusion about what constituted a “race,” what characters were peculiar to different races, and how immutable, unchanging, and biology racial identity actually was. By looking closely at the language they used to alert

²⁹ Biographical information on Lee: Neva R. Deardoff, “Joseph Lee” *Dictionary of American Biography*, 374-5; *The National Cyclopedic of American Biography* v. 33, 66-7; *Biographical Dictionary of Social Welfare in America* ed. Walter Trattner; on Warren, *Dictionary of American Biography* supplement 5 (New York: Charles Scribner’s Sons, 1977), 730-31; biographical information for Ward from *The National Cyclopedic of American Biography*, v. 24; *Who Was Who in America*, v. 1; *Dictionary of American Biography*; and Robert Tracy Jackson, “Robert DeCourcy Ward” *The Scientific Monthly* (February 1932), 192-94. See, in addition, Solomon, *Ancestors and Immigrants*, where the author describes Patten, who was paid a yearly salary of \$3,000, as a key figure in ending the “amateur standing of the League.” 124. See also Matthew Frye Jacobsen, *Whiteness of a Different Color: European Immigrants and the Alchemy of Race* (Cambridge: Harvard University Press, 1998).

American Anglo-Saxons to the perils of the immigrant tide, we gain a new understanding of the League and its members which attempts to read through their racism, and see their own imagined racial identity.

The League was founded, as its Constitution stated, to “advocate and work for the further judicious restriction or stricter regulation of immigration...and to arouse public opinion to the necessity of a further exclusion of elements undesirable for citizenship or injurious to our national character.”³⁰ Its fourth publication, from around 1894, listed “Twenty reasons why Immigration should be further restricted now.” These twenty reasons were reducible to largely one key idea: the immigrants did not measure up—morally, economically, socially, or politically—to the standards of Anglo-Saxon America.³¹ One of the League’s first methods to achieve restriction was through recommending that Congress pass an immigration bill that excluded immigrants unable to read. The “literacy restrictionists,” as Higham describes them, saw this test as an effective way of weeding out aliens who were perceived to be of this “new” immigration; it was he explains, “a highly ‘respectable’ cultural determinant which would also minister

³⁰ *Constitution of the Immigration Restriction League*, box 3, Joseph Lee Papers, Massachusetts Historical Society, Boston, MA [hereafter Lee papers]. Barbara Solomon described Walker’s impact on the group: “The happy ideal of assimilation...disintegrated under Walker’s cogent proofs, and, for old New Englanders, immigration became a matter of racial preservation.” Solomon, *Ancestors and Immigrants*, 77. Solomon described the young founders of the League being buffeted by “the conviction that neither the economic nor the social promises of democracy seemed to work in the divided society of rich and poor, native and foreigner, educated and illiterate, Anglo-Saxon and scum of Europe.” 102. The medical historian Alan Kraut describes the League as “a highly organized, extraordinarily tenacious lobbying group.” Alan Kraut, *The Huddled Masses: The Immigrant in American Society, 1880-1921* (Arlington Heights, IL: Harlan Davidson, 1982).

³¹ “Publications of the Immigration Restriction League, Twenty Reasons why Immigration Should be Further Restricted,” n.d., box 3, Lee papers.

to Anglo-Saxon sensibilities.”³² Statistics and demographics quickly became the preferred way of the League to warn the public of the dangers of immigration.

These were well-educated men – three of the League’s founders (Robert DeCourcy Ward, Prescott F. Hall, and Charles Warren) were all members of the Harvard Class of 1889. Nearly all of the active members of the League, throughout its forty-three year history, were somehow affiliated with Harvard.³³ The members of the IRL also had significant social and political connections that served them well. One of the League’s chief financial backers, Joseph Lee (A.B. Harvard, 1883; L.L.B. Harvard, 1887), was a cousin of Henry Cabot Lodge, long-time congressional representative from Massachusetts. After getting his Harvard Law degree in 1892, Charles Warren served as the private secretary to Massachusetts Governor William Russell. The League’s Washington, D.C. lobbyist, James H. Patten (A.B. Harvard, 1897, A.M. Harvard, 1899, L.L.B. Harvard, 1905), married the daughter of Senator A. C. Latimer of South Carolina. Robert DeCourcy Ward was the son of the representative of the Republic of Chile

³² Higham, *Strangers in the Land*, 101.

³³ The youth of most of the League’s early leaders was important as well. For the cultural anxieties of the League, see the letter from Charles Francis Adams to Prescott Hall, July 28, 1905, Box 1, Immigration Restriction League papers, Houghton Library, Harvard University, Cambridge, Massachusetts [hereafter IRL papers]. Concerning immigration, Adams noted, “the question is too big and too intricate for me to meddle with. I have gone on the retired list, and a generation other than that to which I belong must in future, if possible, save the country from the ‘eternal bow-wows.’ My generation, for better or worse, has done its work.” This is remarkably similar in theme to a note to Lee almost thirty years later: “it would be a tremendous job to start propaganda necessary to excite people on the dangerous legislation now threatened when the whole world is so stirred by other and more pressing problems. I’ll keep on thinking what, if anything, we three old gentlemen can do, but it is certainly a hard task.” John F. Moors to Lee, August 31, 1934, box 2, Lee papers; also Solomon, *Ancestors and Immigrants*.

to Germany. The political savvy and experience these men possessed was impressive, and they used this knowledge remarkably well.

Socially, the core members of the Immigration Restriction League were decidedly upper class, and they engaged in various reform movements in the United States, suggesting that restriction was not exclusively a reactionary or conservative mindset. Lee organized the Massachusetts Civic League in 1897, published the book *Play in Education* in 1915, and became President of the Playground Association of America in 1910, which he nurtured into the National Recreation Association. During World War I, Lee organized the War Camp Community Service within the Recreation Association. He was active on the Boston School Board, and got Massachusetts to adopt annual vision and hearing tests for its students. He was, as one historian put it, “the quintessence of the creative social conscience of New England.” Hall practiced law part-time in his downtown Boston office, which doubled as the headquarters for the Immigration Restriction League. In 1910 Ward became the first Professor of Climatology in the country. He taught at Harvard University for over thirty years, was on the administrative board at the College, and for several years chaired the Department of Geology and Geography. He was a Fellow in the American Academy of Arts and Science, the Royal Meteorological Society, the Association of American Geographers, and the American Meteorological Association. For four years in the late 1890s he edited the *American Meteorological Journal*. His 1925 book *The*

Climate of the United States became one of the standard works on American meteorology.³⁴

These men were not amateurish intellectuals. Their education and social status gave them powerful outlets to act on their racial ideologies. Their racial nativism was not the same kind as that expressed by other groups like the defunct American Protective Association or the Ku Klux Klan. The League's sophisticated racialism gave them an edge in lobbying for Congressional legislation that they deemed was necessary to protect Anglo-Saxon America from the invasion of inferior, criminally-inclined, feeble-minded hordes of non-Nordic European aliens.³⁵ And at the foundation of that opposition to immigration were the principles of classic racialism. By looking closely at the language they used to

³⁴ Biographical information on Hall in *Who Was Who in American History*, v. 1; and Prescott F. Hall, *Immigration and Other Interests of Prescott Farnsworth Hall*, compiled by Mrs. Prescott Hall (New York: Knickerbocker Press, 1922).

³⁵ A short list of their main writings, to appreciate the ostensible "scientific" character of their racial anxieties would include Prescott F. Hall, "Selection of Immigration" *The Annals of the American Academy of Political and Social Science* 24 (July 1904), 169-84; Hall, *Immigration and Its Effects on the United States* (New York: H. Holt and Co., 1906); Hall, "Eugenics, Ethics and Immigration," *Publications of the Immigration Restriction League no. 51* (Boston: ca. 1910); Hall's posthumous book, *Immigration and Other Interests* republished several of these essays, listed here in order of their publication: "American Ideals" 1912, "World Eugenics" 1919, "Immigration and World War" 1921, "Present and Future of Immigration" 1921; Ward's writings include Ward, "Immigration and the South" *The Atlantic Monthly* v. 96 (November 1905), 611-17; Ward, "National Eugenics in Relation to Immigration" *North American Review* v. 192 (July 1910), 57-65; Ward, "Race Betterment and Our Immigration Laws" *Proceedings of the First National Conference on Race Betterment* (Battle Creek, MI: Race Betterment Foundation, 1914); Ward, "Fallacies of the Melting Pot Idea and America's Traditional Immigration Policy" in Madison Grant's and Charles Stewart Davison's collection, *The Alien in Our Midst, or "Selling Our Birthright for a Mess of Pottage."* (New York: Gallatin Publishing Company, 1930); Ward, "The Acclimatization of the White Race in the Tropics" *The Smithsonian Report for 1930* (Washington, D.C.: Government Printing Office, 1931 [orig. published in *New England Journal of Medicine*, v. 201 (26 September 1929)]), 557-76. Later members like Madison Grant and Lothrop Stoddard would be less successful at discretely couching their nativism and racial ideologies in their writings.

alert other proud Anglo-Saxon elites to the perils of the immigrant tide, one can begin to perceive the efficacy of their racial thought.

The League's goal was simple: the preservation of the United States as a white, Anglo-Saxon Protestant nation. In articulating their fears, the League used three primary strategies: to cast the immigrants as inferior because of their unsuitability for participation in a democracy; on a "scientific" basis, which cast them as biologically inferior; and as an economic threat to native-born Americans. Hall commented in a 1904 article that, "We are trustees of our civilization and institutions with a duty to the future, and as trustees the stocks of population in which we invest should be limited by the principle of a careful selection of immigrants."³⁶ By arguing that new immigrants were biologically inferior, the United States was endangering, as the League's leaders felt, American civilization. These immigrants therefore must be excluded to preserve the character of the United States.

To achieve this, and solve what they believed was the problem of immigration, League members turned to a sympathetic legislator from their own backyard – Henry Cabot Lodge of Boston. In an 1891 article in *The North American Review*, Lodge had described a mechanism for restriction based on basic literacy skills that the League later endorsed. As he commented on the apparent decline in the quality of immigrants, Lodge drew the same comparisons between northern and southern Italians that were being made in Italy. He

³⁶ Prescott F. Hall, "Selection of Immigration" *Annals of the American Academy of Political and Social Science* v. 24 (July 1904), 184.

explained that although immigrants from the northern peninsula were desirable additions to the United States, those from the southern districts and Sicily were inferior and undesirable. The latter hailed from “the most illiterate parts of Italy,” regions that he explained had also long been plagued by brigandage. Lodge associated literacy with desirability. And as a sovereign country, Lodge maintained, “We have the right to exclude illiterate persons from our immigration, and this [literacy] test...would, in all probability shut out a large part of the undesirable portion of the present immigration.”³⁷ This became the central thrust of the League’s strategy – secure passage of an immigration bill that contained a literacy test, which would go a long way toward solving the immigration problem.

Lodge was also quick to absorb Galton’s lessons on the heritability of ability and genius. In 1891, he conducted his own historical study of the distribution of ability in the United States, using, just as Galton had, dictionaries and encyclopedias of biography. Armed with the names of 14,243 American men of ability, Lodge broke down both by geography and race the character of these desirable American-born men. The vast majority of these men of ability were geographically located in the New England and Middle-Atlantic states, and preponderant number were of English descent.³⁸ Lodge defined as an “immigrant”

³⁷ Henry Cabot Lodge, “The Restriction of Immigration” *The North American Review* v. 152, n. 410 (Jan. 1891), 27-36. Quote from p. 36.

³⁸ Henry Cabot Lodge, “The Distribution of Ability in the United States” *The Century* (September 1891), reprinted in Henry Cabot Lodge, *Historical and Political Essays* (Boston: Houghton Mifflin Co., 1892), 138-39 for sources and the total number of names; 147 for geographical and racial breakdown. 5,456 came from New England, 5,021 from the Mid-Atlantic states; 10,376 of

any man on the list who came to the United States after the Constitution was adopted, and found that “the production of ability has been remarkably concentrated” in descendants of non-immigrants.³⁹ Lodge took a curious position on slavery; despite the fact that the slave-holding aristocracy produced, in relation to New England and the Mid-Atlantic states, a very small number of men of ability, Lodge argued that “no finer people ever existed than those who settled and built up our Southern States.” He explained that despite laboring “beneath the burden of a slave system,” they had achieved a remarkable degree of success, “the mass of ability they produced under such adverse conditions is a striking proof of the strength of the race.”⁴⁰ The superior characteristics of these men could have not be blunted or submerged beneath the burden of running a slave society. These qualities were hereditary, and could not be suppressed.

Lodge provided a direct connection between the inheritance of ability and the dangers of immigration. In explaining why the Germans and French Huguenots produced a comparatively small proportion of men of ability in relation to their numbers in the population, Lodge said the answer was because these immigrants tended to concentrate in groups. These immigrant populations established themselves in two or three isolated communities, and retained their social networks and their native languages. In short, “they did not quickly become

the 14,243 were of English heritage, 1,439 of Scotch-Irish ancestry, 659 German, 589 French Huguenot, and 7 Italian.

³⁹ *Ibid.*, 142 for “immigrant”; 160 for concentration.

⁴⁰ *Ibid.*, 163.

Americans.”⁴¹ Lodge regarded the ability or capacity to become “American” – which he never precisely defined – to be the single most important factor in creating men of ability. Lodge closed his Galtonian exercise by invoking, just as Galton had, impartiality based upon his social standing as a genteel, upper-crust American.⁴²

In 1891 Lodge also wrote an article concerning the recent lynching of a group of Italian immigrants in New Orleans, who were suspected of murdering the local chief of police. Of the eleven Italians that the New Orleans police rounded up, six were acquitted, three freed as the result of a mistrial, and two were still awaiting trial in March 1891 when a mob broke into the prison and lynched all eleven. The Italians were suspected of belonging to the Italian criminal organization known as the *Mafia*, which would provide additional evidence of the criminal tendencies of Italian immigrants; Lodge wrote that “The men were not killed in the New Orleans prison because they were Italians, but because they were believed to be members of a secret-assassination society responsible for a brutal murder.” He used the occasion to demand the restriction of undesirable European immigration: “it certainly is madness to permit this stream [of Italians] to pour in without discrimination or selection, or the exclusion

⁴¹ *Ibid.*, 163.

⁴² Lodge explained, “I did not create the figures. I merely collected and tabulated them.” *Ibid.*, 165 for Germans and Huguenots; 166 for assimilation and tabulation.

of dangerous and undesirable elements.”⁴³ Lodge would come to support a literacy test as the most effective way to achieve this.

In a piece written for the *Century Illustrated Magazine* in 1893, the Massachusetts Representative maintained his position on the necessity of restricting undesirable immigration, which he justified by using the returns of the newly available 1890 Census. because Lodge argued that restriction was justified on two grounds: the quantity of immigration was excessive, and at the same time its quality was decreasing. He added that the “very undesirable” immigration then arriving in the United States was lowering the quality of “our citizenship” and wages.⁴⁴ In a letter to Prescott Hall in 1896, Lodge hoped that the IRL could help him protect “our citizenship.” He encouraged Hall “to urge, by every method at your command the importance of passing the bill reported from the [Congressional] conference....”⁴⁵ The bill he referred to was the first proposed bill containing a provision barring illiterate immigrants from entering the country to make it through Congress, but it was vetoed by President Cleveland in 1897. Although they were defeated in this first attempt to change immigration policy, the League had found an irreplaceable ally whose cooperation would dramatically increase the visibility and influence of the League.⁴⁶

⁴³ Henry Cabot Lodge, “Lynch Law and Unrestricted Immigration” *The North American Review* v. 152, n. 414 (May 1891), 602-13; 602 for background; quotes from 604; 611.

⁴⁴ Henry Cabot Lodge, “The Census and Immigration” *Century Illustrated Magazine* v. 46, n. 5 (Sept. 1893), 737-39, 737.

⁴⁵ Henry Cabot Lodge to Prescott F. Hall, 24 Dec. 1896, box 2, IRL papers.

⁴⁶ See E. P. Hutchinson’s excellent resource *Legislative History of American Immigration Policy, 1798-1965* (Philadelphia: University of Pennsylvania Press, 1981). Cleveland opposed the “radical

The League's connection with Lodge paid quick dividends. League members Prescott Hall, Robert DeCourcy Ward and Robert Treat Paine secured a private audience at hearings of the House Committee on Immigration and Naturalization in 1896 where they presented a petition in favor of immigration restriction. After his appointment to the Senate, Lodge continued to press the literacy test, arguing in a report appended to Senate bill 4127 that adopting the literacy test "will tell exclusively on the most undesirable portions of immigration."⁴⁷ As the House of Representatives again debated immigration restriction in 1898 – after President Cleveland's veto – the Immigration and Naturalization Committee received thousands of petitions in favor of restriction from across the country. The petitions show the success organizations like the IRL had in unifying opinion in favor of the literacy test, in particular the effect it would have on keeping out only the "undesirable" immigrants. Petitioners endorsed a statement that declared their opposition to "the illiterate, pauper and criminal classes" of immigrants that came from the southeast of Europe. These immigrants were explicitly contrasted with those "intelligent, industrious and law

departure" in policy that the literacy test would force. "In my opinion it is infinitely more safe to admit a hundred thousand immigrants who, though unable to read and write, seek among us only a home and opportunity to work," the President explained, "than to admit one of those unruly agitators and enemies of governmental control, who can not only read and write but delights in arousing by inflammatory speech the illiterate and peacefully inclined to discontent and tumult." Senate Document n. 185, 54th Congress, 2nd Session, "Message from the President of the United States" 3 March 1897, p. 3.

⁴⁷ Senate Report n. 290, to accompany S. 4127, 54th Congress, 1st Session, 18 February 1896; *Minutes of the House Committee on Immigration and Naturalization*, 54th Congress, 1st Session, 23 January 1896, in Record Group 233, Records of the House of Representatives, National Archives and Records Administration, Washington, D.C.

abiding classes” that typically had northern, Anglo-Saxon origins.⁴⁸ These petitions resulted in little significant change in immigration policy, but the passage of the Immigration Act of 1907 established a special committee to investigate immigration whose final report in 1911 legitimized many of these popular opinions.

Lodge utilized the distinctions that Italians had already been making about northern and southern Italian immigrants. He wrote an article favoring immigration restriction in 1904 that explained that the great surge in immigration from southern Italy, which, “without exaggeration, may be described as appalling in quality and in amount...” necessitated that the United States act to exclude a significant number of these new arrivals. Southern Italian immigration, which was comparatively new and growing rapidly, represented a people “with whom the English-speaking people have never before amalgamated,” he noted. It was unfortunate that it was not immigration from the north, because with them, “one can say at least that they are a people of the Western civilization like our own, [and] that there is among the northern Italians an infusion of Germanic blood, and that they present in themselves no alarming feature.” But other races however, like Bohemians, Jews, Southern Italians, Hungarians, Slavs, Russians, Greeks,

⁴⁸ Petitions in files of House Committee on Immigration and Naturalization, RG 233, HR 55A – H 7.2, 1898. The petitions the Committee received in two days in early February 1898 filled one entire box. The petitions from 1898 span over 40 boxes, with each petition containing hundreds of signatures.

Syrians and Armenians, Lodge was not willing to admit, because they were racially different, and because they had no infusion of Germanic blood.⁴⁹

The statistical evidence of the declining rate of natural increase of their talented, Anglo-Saxon stock, and the growing volume of immigration that was illiterate and non-Anglo-Saxon, created significant worries for the social elites of the IRL. Equipped now with ample statistical evidence, and with an ambitious and devoted senator favoring restriction, the Immigration Restriction League had two of its major weapons in hand. Now the League's leaders needed convincing scientific proof of the inferiority of these new immigrants to provide additional support and legitimacy for restriction. They did not have to wait long.

Biological Problems of Immigration

In 1899, William Z. Ripley was dividing his teaching duties between the Massachusetts Institute of Technology, where he was an instructor in sociology and economics, and Columbia University, where he was the Prize Lecturer in Physical Geography and Anthropology. In 1901 he was named professor of economics at Harvard University, where he taught for the next thirty-two years, eventually holding an endowed chair in the department.⁵⁰ But the publication of his book, *The Races of Europe* in 1899 had much larger significance than anything he did at Harvard, Columbia, or M.I.T. *The Races of Europe* generated tremendous impact on political and intellectual arguments in favor of restriction.

⁴⁹ Henry Cabot Lodge, "A Million Immigrants a Year: I. Efforts to Restrict Undesirable Immigration" *The Century Magazine* v. 67, n. 3 (January 1904), 466-69. The distinctions between the northern and southern Italian population will be discussed further in chapter 3.

⁵⁰ John Stover, "Ripley, William Zebina" in John A. Garraty, Mark C. Carnes, eds. *American National Biography* (Oxford: Oxford University Press, 1999) v. 18, 540-42.

Yet the evidence in Ripley's book, significantly, was not collected from first-hand examination or study. For much of the data used in *Races*, Ripley relied on monographs written by other specialists, and personal contacts through which he was able to obtain his desired information. His membership in international societies like the Royal Anthropological Institute and the Anthropological Societies of Rome and Paris provided him with distinguished contacts, but, as with Charles Davenport, Ripley's work was predominantly correlative. He did not have the extensive field-work experience and experimental background that would later mark the differences between his and Davenport's work and that of the anthropologists Franz Boas and Ales Hrdlicka, and the biologists Jennings and Raymond Pearl.⁵¹

Ripley opened the book by listing the factors that shaped human history: race, environment, and epoch. Unlike many of the classic racialists, Ripley emphasized the importance of the built and social environment on human beings, since these often had significant impact on human physical development, but the individual was fundamentally limited and constrained by their biological makeup. "Race," he pointed out, "denotes what man *is*; all these other details of social life represent what man *does*."⁵² Ripley's book aimed to classify the major racial types of the European population, and he insisted that there was no monolithic European race. Instead, there were three ideal types of European races, each

⁵¹ Ripley's biographical entry in *American National Biography* lacks mention of any European training, just as with Davenport.

⁵² William Z. Ripley, *The Races of Europe: A Sociological Study* (New York: D. Appleton and Co., 1899), 32, emphasis original.

essentially geographically isolated and identifiable by cephalic index, and these races were then further broken down by linguistic and cultural attributes. Although he placed a strong emphasis on the shape of heads, Ripley declared that head shape was useful only for identifying these ideal racial types, and did not in any way correlate to intelligence.⁵³ The three major European races he named were the Teutonic, the Alpine, and the Mediterranean; each race had corresponding geographical bases of population, and although the racial identity was not static, there were significant and inherent social characteristics associated with each type. He explained that the physical character of populations groups “often changes at the line which divides the hills from the plains” and that there were profound “structural” differences between eastern European populations and western European populations.⁵⁴

The Races of Europe was profoundly influential on the emergence of American classic racialism in two ways: it established for an American audience the principle of separate and distinct racial identities of European population groups, and it provided a direct conduit for Giuseppe Sergi into American race thinking. Every book on immigration or racial types – Prescott Hall, the United States Immigration Commission or Madison Grant – that relied on Ripley imported, indirectly or directly, the Italian’s description of the separate racial

⁵³ *Ibid.*, chapter three for head form; chapter six for ideal racial types.

⁵⁴ *Ibid.*, 31. Despite his lack of primary observational and experimental data, Ripley’s book became, as John Higham notes in *Strangers in the Land*, one of the landmark intellectual contributions to racialized nativism. Higham, *Strangers in the Land*, 154-5. But Higham also claims that Ripley introduced American readers to the Teutonic, Alpine, and Mediterranean races that he claimed compromised the European populations, where Ripley had been, in reality, pre-empted by Italians like Sergi.

groups within Italy. In Ripley's scheme, Italians were of two separate, distinct racial groups. In the north, the Alps blocked the migration of the Teutonic race into the Italian peninsula, so a distinct Alpine race developed near the mountains and in the Po River valley. "As we leave the river," he pointed out, "and rise slowly across Emilia toward the mountain range the heads gradually become less purely Alpine; and then suddenly as we cross the watershed we step into an entirely different population."⁵⁵ Among Ripley's sources for making this claim: Giuseppe Sergi, whom he described as a "brilliant anthropologist" and Cesare Lombroso.⁵⁶

Ripley's division of Europe into three distinct, fixed racial types would reinforce the attitudes of American nativists toward European immigrants from southern and eastern Europe. The Teutonic, Alpine, and Mediterranean races all had specific geographic origins – the Teutons dominating the northern countries

⁵⁵ Ripley, *The Races of Europe*, 260.

⁵⁶ *Ibid.*, 592 for "brilliant." *The Races of Europe* supplied footnotes for source information, but did not contain a bibliography; that was printed as a separate volume that contained over 2000 titles: William Z. Ripley, *A Selected Bibliography of the Anthropology and Ethnology of Europe* (Boston: Trustees of the Boston Public Library, 1899). Herbert Putnam, the librarian who also served as an editor of the book, noted that the BPL's holdings had recently been "materially strengthened...in this branch of anthropological literature." Herbert Putnam, in editor's preface to Ripley, *A Selected Bibliography*. Of Lombroso's books, Ripley did not use *L'uomo delinquente* but did make use of Lombroso's *L'uomo di genio in rapporto alla psichitria, all storia ed all'esteca* (1888) and *L'uomo bianco e l'uomo di colore*. He listed nine titles of Lombroso's altogether. For Sergi, the list was extensive: twenty-six works were listed, most of them in Italian, and including *La varietà umane, Origine e diffusione della stirpe mediterranea*, and *Arii e italici* (1898). The work of Napoleone Colajanni, who argued against the biological determinism of the Italian population, was not cited, although he and Ripley corresponded. Sergi was also kind enough to supply the American researcher "a complete catalogue of my works," from which, he told Ripley, "you can know better my ideas." Napoleone Colajanni to William Ripley, 12 January 1899; Colajanni to Ripley 25 October 1898; quote from Giuseppe Sergi to William Ripley, 21 April 1898, but see also Sergi to Ripley 14 May 1898; Sergi to Ripley 25 May 1898; and Cesare Lombroso to William Ripley 11 June 1898, all in William Zebina Ripley Papers, box 1, Harvard University Archives, Harvard University, Cambridge, MA.

from their “historical” origins in Scandinavia and Germany (as Sergi had posited) and including the Anglo-Saxons, while the Mediterranean race’s concentration in Spain, Italy, and Africa was due to the mountainous obstacle of the Alps which produced its own distinct racial type. Despite superficial physical similarities in racial characteristics between the Teutonics and Alpines, Ripley asserted, “there can be no doubt of two distinct races of men.”⁵⁷ This alleged distinctiveness lent scientific credibility to fears of American nativists that their Anglo-Saxon racial identity might be overwhelmed.

Transitioning from general racial types to specific national racial identities, Ripley selected the British Isles as a quintessential example of geographic isolation producing a stable racial type. The British population, he argued, was homogeneous – though his argument did not include the Irish population – and was, by measuring its remarkable stability of head form, uncorrupted by other racial influences. “We are,” he declared, referring to Britons and their descendants, “generally known as Teutonic by descent.”⁵⁸

Despite the shift in his professional interests to economics after publishing *The Races of Europe*, Ripley revisited European immigration in a paper for the Royal Anthropological Institute’s Huxley Memorial Lecture in 1908. “The European Population in the United States” moved away from biological races to incorporate an environmental explanation for somatic differences, which Ripley was increasingly embracing as an equal factor in human racial development.

⁵⁷ Ripley, *The Races of Europe*, 218.

⁵⁸ *Ibid.*, 318.

European populations, he argued, “are the product of their immediate environments; dark in the southern half, blonde at the north, stunted where the conditions are harsh, well developed where the land is fat. Even as between city and country,” he continued, “conditions have been so long settled that one may trace the results in the physical traits in the inhabitants.”⁵⁹ The European population in the United States, however, was as yet unrelated to its physical environment because its movement to the other side Atlantic was comparatively recent. But the geographical differences in Europe, he insisted, were partly the cause of the difference in racial types on the continent. In this lecture, Ripley transitioned away from a strict biological determinism.

Ripley’s lecture showed the difficulty of trying to precisely define and codify “racial” identities among human populations. When environmental conditions were considered, and the somatological effects on human populations of the surrounding geography were taken in to account, Ripley conceded that his racial groupings in *The Races of Europe* needed to be reclassified. Mediterranean, Alpine, Slavic, Teutonic and Jewish types, divided on the basis of physical features, and “disregarding...mere linguistic affiliations” seemed to provide a more accurate system of classification than the three types that he posited in *The*

⁵⁹ William Z. Ripley, “The European Population in the United States” *Journal of the Royal Anthropological Institute of Great Britain and Ireland* v. 38 (July-December 1908), 221-240; 221. Ripley’s move toward a more environmental explanation for racial differences is not surprising considering the role that geographic isolation played in the emergence of racial types that he had described in *The Races of Europe*. Yet this also reflected a better appreciation for environmental impact that professional academics were moving toward in the first decade of the twentieth century. It had also, as suggested in the Introduction, been a central feature of Dugdale’s work on the Jukes.

Races of Europe.⁶⁰ Still, Ripley did argue in favor of specific racial characteristics and qualities, and ended his lecture with a declaration that while environmental influences did have an impact on the physical characteristics of racial groups, there were racial distinctions in degree of physical and mental evolution that could be utilized in a classificatory system.⁶¹ The lecture presented some distinct difficulties in precise classification because of environmental influences on human bodies and human evolution. What is significant about Ripley's Huxley lecture was that an internationally respected American academic was unwilling to declare the existence of fixed, immutable racial types that eugenicists and restrictionists argued so heavily in favor of. Ripley, whose book would become so central to American classic racist thinking, seemed to back away from his earlier suggestions of a biological determinism. But those who found this new position unsettling conveniently overlooked his Huxley lecture.

Ripley was not the only American investigating the ethnology and racial background of European populations in the late 1800s although he had the largest influence on American racial thought. Another investigator, a Pennsylvanian from an old Quaker family, Daniel Garrison Brinton, studied medicine at the Jefferson Medical School in Philadelphia before traveling to Europe in the early 1860s to study ethnology at Paris and Heidelberg. After serving in the Union Army as a surgeon in the Civil War, Brinton embarked on a career in ethnology. As the first American university professor in anthropology (at the University of

⁶⁰ Ripley, "European Population," 225.

⁶¹ Ripley, "European Population," 240.

Pennsylvania), Brinton had the makings of being a leader in the field. He was one of the American Philosophical Society's most eminent ethnologists after his first two books, *Anthropology and Ethnology* (1886) and *The American Race* (1891) were published, and he was the President at the International Congress of Anthropology in Chicago in 1893. Brinton, however, failed to grasp the intellectual shift that was then occurring in the scientific community, despite his European training. Although he was a member of the University of Pennsylvania's faculty, he worked mostly in the University Museum, and taught no students. Franz Boas, after he was established at Columbia University as a professor of anthropology, used Brinton as an example of – and a warning of the dangers of – anthropological work that was based principally on theory instead of fieldwork.⁶²

Two books of Brinton's assumed importance in creating a coherent classic racist position for American nativists and later influenced the federal government: *Anthropology and Ethnology* and *Races and Peoples* (1901), the latter a posthumous collection of lectures he delivered at the Academy of Natural Sciences in Philadelphia in 1890. In *Anthropology and Ethnology*, the Pennsylvanian defined anthropology as a field studying physical differences, whereas ethnology was an examination of social and mental characteristics. In characterizing the physical differences between types of people, Brinton used two systems: the Linnaean system of geographical location (European, Asiatic, African, American, Australian, Oceanic), and Blumenbach's system of skin

⁶² Noted in Regna Darnell, "Brinton, Daniel Garrison," in *American National Biography*, John A. Garraty, Mark C. Carnes, eds. (Oxford: Oxford University press, 1999), vol. 3, 559-60.

assortment (white, yellow, black, brown, red). This was a different method than that used by Ripley or Sergi, which relied on physical structure to determine racial differences. Although he bucked the strictures on racial purity of some of his contemporaries by noting that race mixing can be beneficial, he meant it only in terms of mixing *within* racial groups. The English, for example, the “most vigorous and energetic” of all races, were the product of numerous crossings of Celtic, Romance, and Teutonic breeds. But, Brinton insisted that the “highest race will, however, always preserve the purity of its blood... [due] to the abhorrence of its females to mingling with the lower stock....”⁶³ Like his focus on theoretical outlines of anthropology and ethnology rather than field work in this methodological treatment of these developing fields, Brinton relied on long-accepted cultural sentiments rather than observed and measured facts. This became the hallmark of American classic racialism, particularly when it came to understanding the problems of immigration.

Races and Peoples was another important contribution to American classic racialism, but was more specifically geared to applying some of the theoretical arguments he had laid out in *Anthropology and Ethnology*. The ten lectures that Brinton delivered in the early months of 1890 covered many topics in the field of ethnography.⁶⁴ In his first lecture, Brinton laid out the core classic

⁶³ Daniel G. Brinton, *Anthropology and Ethnology* (Philadelphia: Iconographic Publishing Company, 1886) v. 1, p. 56.

⁶⁴ Daniel G. Brinton, *Races and Peoples: Lectures on the Science of Ethnography* (Philadelphia: David McKay, 1901). The major ethnological groupings in these lectures were: Eurafrikan, Asian, Insular and Littoral, and the American races.

racialist belief: “The traits of the race thus [overwhelm] the variable characters of the family, the sex or the individual, and maintain themselves *uniform and unalterable in the pure blood of the stock through all experience*.”⁶⁵ An individual’s “nature” could never be changed. The mental and physical differences between races might escape the notice of a casual observer, but this did not make them any less permanent. Cranial measurements, facial and maxillary [jaw] angles, teeth, bones, and skin color all helped the skilled student identify and separate racial groups.⁶⁶ The mental and psychological characteristics of racial groups were also, just as it was for Lombroso, inscribed on the physical body. Brinton tried to convince his audience that culture, environment, and education had a negligible effect on racial characteristics. “We are accustomed familiarly to speak of ‘higher’ and ‘lower’ races, and we are justified in this even from merely physical considerations. These indeed bear intimate relations,” he confidently stated, “to mental capacity, and where the body presents many points of arrested or retarded development, we may be sure that the mind will also.”⁶⁷

Brinton’s second lecture continued the physical elements of ethnography, but

⁶⁵ *Ibid.*, p. 18, emphasis added. Lectures 4-9 studied particular ethnological groups, while the remaining lectures provided syntheses and overviews of ethnology. Brinton used the word “overslaugh,” which is substituted here for clarity.

⁶⁶ Brinton’s exposition of the importance of skin color shows very clearly the importance that classic racialism maintained for some students of ethnology. The coloration of Africans, he explained, “extends much more beyond the skin. It is found in a less degree on all his mucous membranes, in his muscles, and even in the pia mater and the grey substance of his brain.” The intrinsic composition and difference between races was, for Brinton as for other classical racialists, fundamentally and irrevocably established in every cell in the body, and could, therefore, never be modified or changed. When he shifted his lecture to talk about mental traits, the importance of this unchanging conception of racial identity became clear. *Ibid.*, 30.

⁶⁷ *Ibid.*, 47. He also explained that measured by these objective physical measurements of capacity and quality, “the European or white race stands at the head of the list, and African or negro at its foot.” 48.

considered more the mental and psychological differences between races, which “are real and profound” and hereditary.⁶⁸

Though Brinton was attempting to create a workable and sophisticated scientific classification system, he was unable to divorce himself from certain cultural assumptions. When he discussed gradual changes between races in the lectures, he elevated the white race as the “leading race in all history,” and thus justified his extended treatment of physical differences of the various stocks of the white race. His description of the Celtic and Italic stocks reflected the biases of an old-stock American, proud of his long lineage back to England. The Celtic stock was turbulent, boastful, alert, courageous, and deficient in caution, persistence, and self-control. They were “a dangerous element in the body politic of a free country. In religion they are fanatic and bigoted.”⁶⁹ “Italic peoples” were divided into two population groups, Umbrians in the north, and Latins in the south, again suggesting the influence of Italian racial anthropology; Brinton argued that Latin southerners were very different racially from Americans.⁷⁰ He closed his last lecture, in which he discussed problems of acclimation, amalgamation and civilization, with one last paean to the perfection of the European white race. “Every navy and every army of any fighting capacity belong to the European whites and their descendants,” he claimed. “No nation and no race or other

⁶⁸ *Ibid.*, 51.

⁶⁹ *Ibid.*, 103, 155.

⁷⁰ This is difficult to prove, however, as Brinton did not provide any footnotes, and his List of Authors at the end of the book did not mention Lombroso, Sergi, Niceforo or the Italian anthropologist Rudolfo Livi (who was an important source for Ripley’s book).

lineage dare withstand an attack or disobey an order from a leading European power.”⁷¹ Madison Grant and Lothrop Stoddard would later repeat this conceit, which had no basis in actual scientific or objective evidence, in the late 1910s and 1920s, and which helped support the entire rationale for immigration restriction in the United States. To maintain American civilization, its homogeneity had to be preserved.

One other important ethnographical source for the creation of classic racialism, and which influenced the development of solutions to the “immigration problem,” was the product of a Russian-born Frenchman, Joseph Deniker. Deniker’s outline of anthropology and ethnography, *The Races of Man*, grew from his doctoral dissertation for the Faculté des Sciences in Paris, and was released in an English translation in 1900. Deniker was born to French parents in Astrakhan, Russia in 1852, and spent several years after his graduation from the Technological Institute in Petrograd traveling the Caucasus, Persia, and Europe researching the major ethnographic features of the inhabitants. His doctoral dissertation for the Faculté des Sciences in Paris (1886) was the result of these researches, and Deniker quickly became one of the premiere theoreticians of human races.⁷² Like Ripley, Deniker’s work tried to organize the varieties of human beings into coherent groups, while recognizing the remarkable variations

⁷¹ *Ibid.*, 298.

⁷² “Deniker, Joseph” in *Who Was Who, 1916-1928*, v. 2 (London: Adam and Charles Black, 1929), 279; St. Le Tourneur, “Deniker, Joseph” *Dictionnaire de biographie française* Roman D’Amat and R. Limouzin-Lamothe, eds. (Paris: Librairie Letouzey et Ané, 1965), v. 10, 1026. Deniker’s professional position was Chief Librarian at the Museum of Natural History in Paris.

within these groupings. To create associations, he relied principally on language, cranial measurements, and cultural characteristics, which were, as I have suggested, were pretty common metrics in early racial anthropology. But Deniker's work was very much ethnography in its original sense: much of the book detailed the characteristics and qualities of "primitive" and "uncivilized" cultures.⁷³ His chapter on ethnic characters, for instance, described the linguistic attributes of non-literate cultures, and how inflection, signals, glyphs, and pictographs functioned as modes of communication in human groups that lacked written alphabets.⁷⁴

Deniker's book offered a cautious approach to racial groupings of human populations. He hesitated to deny influence of the environment on human groups, and conceded that biological laws regarding inheritance and heredity were "still very dimly apprehended." He argued against Lamarckian inheritance, and mocked the phrenology of Franz Joseph Gall.⁷⁵ Sociological characteristics of racial groups were discussed in three lengthy chapters, demonstrating Deniker's appreciation that while "race" was largely a physical artifact, grouping "peoples" or nations together was much more complex. But deviating from common anthropological metrics, Deniker's organizing principle of racial groups was hair

⁷³ Ripley addressed Deniker's forthcoming book in an appendix in *The Races of Europe*.

⁷⁴ Joseph Deniker, *The Races of Man: An Outline of Anthropology and Ethnography* (New York: Charles' Scribner's Sons, 1900).

⁷⁵ *Ibid.* On Gall and early American ethnology, anthropology and phrenology, see John S. Haller, *Outcasts from Evolution: Scientific Attitudes of Racial Inferiority, 1859-1900* (Urbana, IL: University of Illinois Press, 1971); Stephen Jay Gould, *The Mismeasure of Man* (New York: W. W. Norton Co., 1981); Paul, *Controlling Human Heredity*.

type, which resulted in six large “types” of population, which were then broken down into races and sub-races by eye color, skin type, and cranial measurement.

This led to a wildly complicated organization of world racial populations. It demonstrates two important aspects of classification schemes of “races” at the turn of the century: that there was no agreement in principle on standards of evidence, and there were a number of strange classification schemes. This complexity is significant because it demonstrates a lack of an academic or professional consensus, one of the keys for the emergence of a new scientific paradigm in Kuhn’s framework. Without agreement in what constituted the organizing principles of racial classification, professional academics and researchers had difficulty talking about “scientific” systems of race. Ripley, Brinton, Deniker, Sergi and others each suggested their own independent method of grouping populations by physical and mental characteristics. Thus, when historians refer to the ideological complex of “scientific racism” they obscure the divergence of opinion on what constituted a “race.” This complexity allowed less reticent advocates to flatten out these complexities into the convincing and simple aphorism that like produced like.

Nativists like the IRL’s Prescott Hall used this lack of consensus to advance claims that had dubious scientific credibility, but in the context of this uncertainty, scientists had inadvertently created space for amateurs and non-scientists to declare that there was consensus and certainty with little fear of opposition. Hall’s 1906 book *Immigration and Its Affects* was forthright in the danger that the new immigration presented; instead of contributing an analysis

based on first-hand research, he merely expanded many of the basic points he made in a 1904 article. Noting the increase in illiteracy, disease, insanity and crime that the southeastern immigrants brought with them, as well as the danger – as Walker pointed out – of racial degradation, Hall argued that failing to curtail immigration, “will probably mean, not merely a change of race but a change in average quality.” He burnished Walker’s replacement thesis as proof that undesirable immigration was a great peril to the country. Despite the fact that he was a lawyer by training, Hall felt compelled to speak on “recent discoveries in biology [that] show that in the long run heredity is far more important than environment or education.”⁷⁶ There was no mention of the complexity of the racial sciences or the disagreements by professional students over proper and accurate classification schemes, or that academics were unwilling to dismiss the importance of environmental influences on individual development.

While Hall’s book *Immigration* became one of the standard texts the federal government would use in formulating its immigration policy for decades (Madison Grant still listed it on his recommended reading list to the House Committee on Immigration in 1925⁷⁷), the assumptions that underlay the book put it at odds with the objective, experimental trend developing in modern science in the early-twentieth century. It was quintessential classic racist thought. Hall claimed that the “Keltic” and “Iberic” populations within Italy reflected the

⁷⁶ Hall, *Immigration* pp. 99-110; quote from 99.

⁷⁷ “A List of Authoritative Books on Immigration compiled by Madison Grant for the Immigration Committee of the American Defense Society, Inc.” RG 233, HR 68 A – F. 18. 1 box 264.

general geographical divisions that the United States Immigration Bureau was using at that time to distinguish between north Italians and south Italians. Hall's Keltic element comprised any immigrant from Tuscany, Emilia, Liguria, Venice, and Lombardy, and the Iberic element made up all other territories to the south. Those Italians from the north, Hall described, were of "much better stock, and are more enterprising, thrifty, and intelligent than their southern fellow-countrymen."⁷⁸ Curiously, Hall's historical analysis of Italian immigration to the United States conflicted with the statistical evidence gathered by the Immigration Bureau in the nineteenth century. Hall claimed that, "The earliest Italian immigration to the United States," he argued, "was of a very low class" and included the organ-grinders and rag-pickers featured so prominently in Jacob Riis's book *How the Other Half Lives* (1890); Italian immigrants that followed comprised the better class of northerners, who took jobs as barbers, bootblacks, fruit vendors and shoemakers – people who were, "on the whole, peaceable and industrious."⁷⁹ Statistically, however, Hall was wrong – it was the northern Italians who had arrived in the United States first, and northern Italian immigrants tended to move away from east coast cities to the interior of the United States.⁸⁰

⁷⁸ Hall, *Immigration*, 54. This effectively made everything south of Siena "Iberic." His ethnographical definitions reiterated many of the distinctions that the Italian follower of Lombroso, Alfredo Niceforo, used in *Italiani degli nord et degli sud* (1898).

⁷⁹ Hall, *Immigration*, 55.

⁸⁰ For instance, see United States Immigration Commission, *Reports of the Immigration Commission* v. 5 "Dictionary of Races and Peoples" (Washington, D.C.: Government Printing Office, 1911), 83-86 for distinction between northern Italian and southern Italian immigrants; on their longevity residing in the United States, *idem.*, *Reports of the Immigration Commission* v. 8 "Immigrants in Industries, Part 2" pages 15-16, for non-Italians. More than 1/3 of northern Italian immigrants are listed as residing in the United States longer than five years; on distribution of immigrants, see United States Department of the Interior, *The Eleventh Census of the United*

Despite having no formal training in the natural sciences, Hall devoted a thirty-page chapter to the biological and racial effects of immigration on the United States. One of the central elements of this chapter was General Walker's replacement thesis. Recent biological discoveries, Hall explained, showed that in the long run, it was heredity, and not environment, that was of paramount importance in determining the racial composition of a country. Hall and his colleagues in the Immigration Restriction League would maintain this position – despite growing scientific evidence to the contrary – for the next two decades. Using statistics from the Immigration Bureau for 1904, Hall emphasized the non-Teutonic racial background of the majority of immigrant arrivals: Slavic and Iberic immigrants totaled 56.4% of all arrivals, while only 24% had a Teutonic background.⁸¹

Hall discussed possible legislative action that would preserve the Teutonic racial composition of the United States. Since its formation in 1894, the Immigration Restriction League had advocated a literacy requirement for all arriving immigrants. As he traced the development of legislation that included this

States, 1890: Part I (Washington, D.C.: Government Printing Office, 1894), lxxxii for the urban/rural breakdown of native-born and foreign-born residents; xc for the proportion of immigrants in cities. In the 1890 census, Italians were not distinguished by “race” – i.e. were not listed as “Italian (North)” and “Italian (South)” but described as from the “Latin Nations.” For instance, see 608.

⁸¹ Hall, *Immigration*, 103. Hall's two sources for the background of the racial composition of the European races were Dr. K. H. Claghorn's 1905 essay in *Outlook* (vol. 13, 4 February 1905), 453, and Ripley's *Races of Europe*. In a footnote on page 103, Hall included a lengthy exposition on the racial background of immigrants, and emphasized the inadequacy of a definition that rested on nationality and linguistic groupings. Language and nationality, he explained, were chiefly acquired characteristics, while “a true racial classification tells of the inherited tendencies which are likely to be developed in [an immigrant's] descendants.” Footnote 4, p. 103. Hall devoted two pages in the chapter to a careful analysis of Walker's “Replacement Thesis.”

provision, Hall noted that it was two Massachusetts congressmen – Senator Henry Cabot Lodge and Representative Samuel W. McCall (later the governor of Massachusetts for two years) – who consistently favored this form of “selection.” Hall spent several pages emphasizing the importance of the literacy requirement. The League faced obstacles in securing this provision; President Grover Cleveland had already vetoed a literacy clause in an immigration bill in 1897, because it represented a “radical departure” from policy that could only be justified by “the necessity of protecting our population against degeneration and saving our national peace and quiet from turbulence and disorder.” Enacting a clause restricting immigrants based on their ability to read would be no protection “against these evils.”⁸² Such an act by the President led Hall to conclude that Cleveland simply misunderstood the idea behind the literacy test, and the League’s role was to educate not only political leaders, but the public as well, about the true benefits of a literacy clause: it would preserve the racial character and homogeneity of the United States.

Manipulating the description of it, Hall explained that the premise behind the “educational test” was that “it furnishes an indirect method of excluding those who are undesirable, not merely because of their illiteracy but for other reasons.”⁸³ Illiteracy also correlated with a low amount of money an immigrant brought, making him (Hall specifically used the male pronoun) more susceptible to

⁸² Senate Document n. 185, 54th Congress, 2nd Session, “Message from the President of the United States” 3 March 1897, 2-3.

⁸³ Hall, *Immigration*, 273. He did not explain what the “other reasons” might be.

pauperism, crime, a low standard of living, and an inability to assimilate politically and socially to his adopted country. Since Hall took illiteracy to also be a sign of hereditary mental inferiority, the test would presumably (and conveniently) exclude racially undesirable immigrants too. Significantly, the Boston lawyer attributed illiteracy not to environmental or social causes, but to heredity. Illiteracy can be conquered in certain instances, Hall explained. But the “hereditary tendencies of the peoples illiterate abroad, and especially of their uneducated classes, cannot be overcome in a generation or two.” Their hereditary inferiority was permanent, and since Hall maintained the classic racist belief that like produces like, their offspring, though born in America, would always inherit these inferior, undesirable qualities.

Yet there were American academics capable of performing solid analyses of immigration problems that could still be used to justify the exclusion of the new immigrants. Columbia University Political Economist Richmond Mayo-Smith was one, and he wrote a balanced and sophisticated book with a wealth of statistical information on immigration. Mayo-Smith, who was named a full professor of Political Economics and Social Science in 1883, was another highly regarded American statistician in the late-nineteenth century. Barbara Solomon notes that he was “the most important” member of the American Economic Association to affect “the intellectual development of the restriction movement.”⁸⁴ He made key contributions to the development of classic racialism and its incorporation by the government into immigration policy by providing an

⁸⁴ Solomon, *Ancestors and Immigrants*, 77.

alternative language to xenophobia or racism. An Ohioan of Puritan descent, Mayo-Smith spent two years in Germany after his graduation from Amherst College studying statistics and political economy. He improved his international credentials throughout his career; he joined the International Statistical Institute in 1889, and was named an Honorary Fellow of the Royal Statistical Society in 1890. His two-volume *Science of Statistics* (1895, 1899) became one of the foundational texts for statistical instruction in the early twentieth century.⁸⁵

In 1898 Mayo-Smith addressed European immigration to the United States in an important book, *Emigration and Immigration*. The sources that the Columbia professor used in compiling his book on immigration were varied: the Tenth Census of the United States (1880), the New York Commissioners of Emigration reports, *Statistical Tables relating to Emigration and Immigration from and Into the United Kingdom* (1887), the *Bulletin de l'Institut international de statistique* (1887), *Statistica della emigrazione italiana per gli anni 1884 e 1885*, the Massachusetts census of 1885, and the New York State Board of Charities Annual reports among others. While this gave the book a strong statistical background, its “whole argument...has been to show that it is desirable to correct certain evils which flow from the perfect freedom of immigration.”⁸⁶ While he insisted that the country restrict immigration, Mayo-Smith deplored nativist justifications based on the racial difference of immigrants. Mayo-Smith

⁸⁵ W. R. L., “Mayo-Smith, Richmond” *Dictionary of American Biography* Dumas Malone, ed. (NY: Charles Scribner’s Sons, 1933) Vol. 12, 467-68.

⁸⁶ Richmond Mayo-Smith, *Emigration and Immigration: A Study in Social Science* (New York: Charles Scribner’s Sons, 1898), p. 277-78.

was not so concerned with the racial degradation of the country as the economic instability that the immigrant labor pool created. The desired end of restriction was to exclude “elements incompatible with our civilization,” defective and delinquent classes, and large numbers of immigrants that would “threaten the integrity of our political institutions or to cause economic disturbances.”⁸⁷ But in *Emigration* he took pains to demonstrate the positive impact that immigrants had on the United States, particularly its economic and industrial development.

Yet at the same time he celebrated immigrants as one of the three chief factors responsible for the country’s growth, Mayo-Smith insisted on the importance of the immigrant’s impact on the racial composition of the nation. Noting that the African-American population in the south would “always be a problem for us” – they were “by birth and race and previous condition of servitude incapable of representing the full American capacity for political and social life” – Mayo-Smith recommended a careful approach to restricting the admission of European aliens, although he declined to provide an exact method. He cautioned that absolute prohibition of immigration was neither necessary nor desirable.⁸⁸

⁸⁷ *Ibid.*, p. 278-9.

⁸⁸ *Ibid.*: 64-6 for the African-American population; 279 for opposition to absolute prohibition. “The end to be desired is perfectly plain. It is, that immigration shall be controlled in such a way that elements incompatible with our civilization shall be excluded; that the defective and delinquent classes, who are only a burden and a danger to us, shall also be excluded; and that the immigration shall not be on such a scale as to threaten the integrity of our political institutions or to cause economic disturbances. The general method is to establish some process of selection by which the immigration of undesirable persons shall be discouraged.” *Ibid.*, 278.

At the end of the nineteenth century, the increasing volume of immigration to the United States came to be defined as a “problem.” Professional academics and non-scientists alike examined the apparent differences between the aliens arriving on American shores and the characteristics of its native-born population. Professional academics like Ripley and Brinton relied upon second-hand analysis and observations to compile their organizational schemes of the world’s racial groups. Others approached the issue in sophisticated and cosmopolitan ways, examining immigration and racial typologies, as Mayo-Smith and Deniker did, cautiously and with an objective eye. The wide variety of interpretations, the lack of certainty or proof, created ambiguity in the intellectual approach to racial thought. This ambiguity and uncertainty enabled non-scientists, like Prescott Hall and his associates in the Immigration Restriction League, to insist that there were specific biological population groups whose dangerous and undesirable attributes and characteristics would be propagated in future generations. Once those groups could be defined as “undesirable” or biologically different, the “problem” of immigration could be advanced as a danger to the American native-born population. Using fears articulated by men like Walker, Lodge and Turner, the differences between the new immigration and the old immigration could be portrayed as threatening and dangerous. And the lack of a coherent scientific opposition to the deterministic thinking of classic racialism gave it a degree of influence that ultimately enabled restrictionists to propose their specific solutions to the problem of immigration.

Chapter 2: The Value of Scientific Cosmopolitanism

In 1904 Immigration Restriction League founder and Boston lawyer Prescott Hall published an essay in *The Annals of the American Academy of Political and Social Science* that defined the nature of inferior immigration. Any immigrant is undesirable, he explained, “which is ignorant of a trade; which is lacking in resources; which has criminal tendencies; which is averse to country life and tends to congregate in the slums of large cities; which has a low standard of living and lacks ambition to seek a better [standard]; which fails to assimilate within a reasonable time, and which has no permanent interests in this country.”¹ Several of those categories could be determined statistically; the urban concentrations of population and the nativity of criminals in particular were clearly listed in the census. These two categories became two of the more frequently cited empirical “proofs” of the undesirability of the “new” immigration.

For Hall and his associates in the Immigration Restriction League, southern Italian immigrants were among the most undesirable elements of new arrivals because they were perceived to embody several of these categories. The association of southern Italians and crime – as witnessed in the vigilante action in New Orleans – was particularly powerful. Hall’s colleague Joseph Lee made the case explicitly, stating in a committee report of the National Conference of

¹ Prescott F. Hall, “Selection of Immigration” *The Annals of the American Academy of Political and Social Science* (July 1904), 175. This essay was a slight variation on arguments Hall had been publishing for a decade. See, for instance, Prescott F. Hall, “Immigration and the Educational Test” *North American Review* v. 165, n. 4 (October 1897), 393-403.

Charities and Correction that “It is generally conceded that the North Italian is a much more desirable immigrant than the South Italian. He is stronger, more enterprising, has laid aside much more property, and is better educated.”² The southern Italians’ overrepresentation in the criminal and illiterate populations suggested to restrictionists an eminently justifiable cause for their exclusion from American’s shores. Their criminality threatened law-abiding native-born Americans with violence and theft. Their illiteracy made them unable to secure good jobs, which left them economically vulnerable, which in turn reinforced an innate propensity to crime. Their urban concentration retarded their assimilation of American ideals and values. All of these had statistical evidence to support it. These tendencies – to say nothing of their non-Nordic racial identity – made them undesirable Americans. The same *Proceedings* that published Lee’s report to the Charities of and Correction conference carried a defense of recent immigrants by New York medical doctor Maurice Fishberg. “Many writers on the racial effects of the recent immigration have alleged that ‘inferior racial elements’ are likely to deteriorate physically the people of the United States,” he explained. “None of these writers have to my knowledge made an effort to study the problem *directly* from the strictly scientific standpoint, so that their statements on the subject can be considered merely opinions not necessarily based on facts.” One writer that had studied the problem directly would have strongly disagreed with Lee’s assertion, and in fact made a concerted effort to correct these misunderstandings.

² Joseph Lee, “Report of the Committee” in *Proceedings of the National Conference of Charities and Correction* v. 33 (May 1906), 1-17; 9; Dr. Maurice Fishberg, “Ethnic Factors in Immigration—A Critical View” in *ibid.*, 27-36; 27, emphasis added.

The critic, a sociologist and statistician, was an Italian named Napoleone Colajanni.

Just as Galton and Lombroso reached across the Atlantic Ocean to spread their ideas, Colajanni published essays in the United States; Colajanni's essays, however, tried to combat the perceptions that southern Italians were undesirable immigrants. He aimed to cast doubt on the validity of the arguments made in favor of their exclusion by criticizing the alleged biological and hereditary permanence of racial identity. He was not simply an apologist for Italian immigrants, however – Colajanni used a sophisticated (or at least less simplistic) approach to social phenomenon like crime and illiteracy. In this regard he created important precedents for later critics of classic racist thought in the United States.

Colajanni first wrote an English article to reexamine the statistical evidence used to allegedly prove the criminality of many southern Italian immigrants in the magazine *The Forum* in March 1901. He emphasized the importance of social and cultural context in understanding empirical facts, and consistently argued against a racial determinism that presumed a perpetual inferiority of certain population groups. Colajanni admitted that the assassination of Italy's King Umberto in 1900 had reinforced the perception, even of sympathetic observers, that "our beautiful country is privileged ground for the production of criminals," but his essay marshaled statistical evidence to disprove that perception. Colajanni took aim at the Lombrosians and their "poorly collected" and "still more poorly interpreted" statistical evidence of Italian

criminality, which had been, through Lombroso's and Zimmern's articles, accessible to American audiences.³ Although Colajanni could not dispute the fact that Italy did have a high rate of homicide (compounded in the public's mind by an association with organized crime), he took pains in the essay to shift the explanation from a racial or biological source to an economic and educational problem, and tried to undermine Lombroso's and Sergi's arguments about the "characters" of the Italian people and their immigrants. That the highest rates of homicide in Italy at the end of the nineteenth century were in the regions of the south was true, he noted, but a similarly high rate was found in northern Italy at the end of the eighteenth century. There was, he insisted, no racial predisposition to criminality in the Italians; crime was primarily a primarily of poverty and environmental influences.⁴

Colajanni was a professor of statistics at the University of Naples and deputy of parliament in 1906 when he republished his book *Latini e anglo-sassoni: razze inferiore e superiore* (*Latins and Anglo-Saxons: Inferior and Superior Races*) [orig. 1903], which sought to combat the racially determinist arguments that pervaded European and American thought. In the late 1890s, Colajanni, like Giuseppe Sergi and Cesare Lombroso, had been in touch with the American economist, sociologist and physical geographer William Z. Ripley, exchanging with him publications and bibliographies of racial anthropology. Colajanni even offered to review Ripley's 1899 book *The Races of Europe* in the

³ Napoleone Colajanni, "Homicide and the Italians" *Forum* (March 1901), 62-68; pp. 62-3.

⁴ *Ibid*, 67.

Italian journal *Rivista popolare di politica lettere e scienze sociale*.⁵ But Colajanni's own view about European races, which he laid out in the literature review in *Latini e anglo-sassoni*, suggested that discussing "race" was inherently difficult because even among students of human varieties (as the last chapter showed), there were "profound differences" in the classification systems. Colajanni told of the wild variations in the classification systems of Buffon, Aggassiz, Huxley, Sergi, Ripley, Keane, Deniker and many others.⁶ The important point for Colajanni – as with anthropologists other later critics of classic racialism – was that environmental influences on an organism could not be ignored, and that "pure" races had not existed for tens of thousands of years, so there was little sense in trying to preserve the racial homogeneity of the United States. American authors who favored a purely biological or hereditary classification were missing an essential component of the development of human population groups. Although Colajanni was a statistician, he approached biological problems with an open and flexible mind, suspicious of what statistics purported to "prove." He had a somewhat cosmopolitan perspective like Boas, Jennings and Raymond Pearl, which inclined him to believe in multicausality. Ripley's book had some of these same cosmopolitan inclinations – despite the fact

⁵ Napoleone Colajanni to William Z. Ripley, 12 January 1899, Papers of William Z. Ripley, Correspondence: Box 1, Harvard University Archives, Harvard University, Cambridge, MA. Colajanni noted that he was opposed to the racial theories of Vacher de Lapouge, who would become an important influence on American nativists like Madison Grant. See also Giuseppe Sergi to Ripley, 21 April 1898; 14 May 1898; 25 May 1898; Cesare Lombroso to Ripley, 11 June 1898, all in Ripley papers.

⁶ Napoleone Colajanni, *Latini e anglo-sassoni: razze inferiori e razze superiori* Second ed. (Roma: La rivista popolare, 1906), 4.

that Ripley spent the rest of his career in economics and not in anthropology – but which were wholly absent in the claims and publications of Hall and restrictionists. American classic racialists would abandon this cosmopolitanism and Colajanni serves as a useful point of departure for later critics of the determinism of immigration restrictionists.

As he debunked each of the arguments leveled against the “new” immigration, Colajanni relied on a wealth of American sources that showed that global migrations involved not only individual immigrants, but also abstract representations of them that also migrated around the world. The statistical sources that Colajanni cited – the reports of reformatories in upstate New York, annual reports of the United States Commissioner of Immigration, the Secretary of Commerce’s *Prisoners and Juvenile Delinquents, 1904* (one of his favorites) – demonstrates two points: the assertion that Italian immigrants were “undesirable” was contested on both sides of the Atlantic; and statistical proof of inherent characteristics of racial “types” was vulnerable to manipulation.⁷ As scientific knowledge lurched fitfully toward an understanding of heredity and genetics in the early twentieth-century, the ambiguities of what scientific knowledge had established as proven left a space for statistics (which were perceived to be “harder” or more accurate because of their numerical basis) to be utilized to prove simple arguments of heredity and racial type. Classic racialists, as Galton showed,

⁷ In one of Hall’s first public articles opposing Italian immigration in 1896, his piece was preceded by two months by an article written by a medical doctor and a United States Commissioner of Immigration, which declared Italian immigrants to be generally desirable additions. “Italian immigrants, even in the first generation,” he pointed out, “succumb sooner or later, like those of other European nationalities, to the irresistible influence of freedom and prosperity.” Joseph H. Senner, “Immigration from Italy” *North American Review* v. 162, n. 6 (June 1896), 649-57; 651.

avored a statistical analysis of heredity because it was perceived to be less ambiguous. Colajanni used statistical evidence to counter that, and to demonstrate that Southern Italians were not biologically predisposed to violent crime. Colajanni used statistical evidence to demonstrate that Southern Italians were not biologically predisposed to violent crime. But it was not so easy to disabuse a hazy understanding of Italians' alleged hereditary criminality in the popular imagination, as the reports of the Dillingham Commission would later show. Statistics could be made to show just about anything.

Some American magazines also tried to deter this perception of Italian criminality, as *The Independent* did in a 1909 editorial. Writing on "Undesirable Citizens," the journal described that murderers, of all undesirable immigrants, were certainly the worst. And, it reported, the most "hateful" murderers were "the banded, cowardly professional murderers represented by the Camorra, the Mafia and the Black Hand oath-bound societies." The editorial described how these underground, secret societies effectively cowed the law-abiding citizens of Italian colonies in the United States by intimidation, threats, and violence. The tragedy of the situation, *The Independent* informed its readers, was that the honest Italian workers, the desirable immigrants from the Italian peninsula, lived in perpetual fear of retribution, which had the added effect of preventing the American police forces from more effectively enforcing law and order. The editorial took a cosmopolitan, sophisticated approach to the issue of Italian immigration. "Far be it from us," it stated, "to say one word against Italians as Italians. There is no nobler or abler stock out of which to make citizens. The race of Cicero and

Caesar, of the Gracchi and the Scipios, of Dante and Galileo, of Raphael and Michael Angelo [sic], of Cavour and Garibaldi is no mean stock.”⁸ It was unfair, irrational, and wrong to presume that an entire race could not possibly breed good and noble men because of its inferiority. But despite the fine qualities of this race, there were also negative and dangerous qualities in it that necessitated careful attention. What mattered was the cause of crime: was it biological or contextual? *The Independent* editorial clearly leaned to the latter.

Italian immigration, which was at this time being studied by the United States Immigration Commission, was a concern for the Italian government as well. In fact, as the reports on educational efforts in the south of Italy in the *Bolletino dell'emigrazione* reflect, the Royal Government tried to extend education and literacy programs throughout the rural Italian south after the American government passed the literacy requirement in its 1917 immigration bill.⁹ In 1909 Colajanni published *Gli italiani negli stati uniti*, a remarkable manifestation of a trans-Atlantic concern over immigration problems, but *Gli italiani* was not the work of Colajanni alone; it was a collection of documents, some originally in English, which touched on broad immigration matters. In publishing the essays, Colajanni noted in the preface that the goal of the collection was to refute the statistical, historical, and scientific arguments used to support

⁸ “Undesirable Citizens” *The Independent* v. 66 (1 April, 1909), 712-13.

⁹ For instance, see “Scuole per emigranti istituite dal Commissariato” *Bolletino dell'emigrazione* (Roma: Commissariato Generale dell'Emigrazione) 1921, n. 3, doc. 1384. These were the same programs that Colajanni criticized in his essay “Homicide and the Italians.” In 1911, a whole issue was dedicated to describing the work and reports of the United States Immigration Commission. “Lavori della Commissione federale per l'immigrazione negli Stati Uniti” *Bolletino dell'emigrazione*, 1911 n. 4, doc. 527. The *Bolletino* will be discussed in more detail below.

“racial prejudice” against Italians. “This prejudice,” he explained, “has condemned the Italians, especially from the South, in North America as ‘undesirable’ elements, that could not assimilate to civilization in the [American] Republic.”¹⁰ *Latini e anglo-sassoni* had been specifically written to combat this perception, but Colajanni noted that it garnered little attention in the United States; *The Nation*, while unable to confute “a single line in the book,” still published a negative review of the book.¹¹ Colajanni’s purpose in *Gli italiani*, then, was to bring together American authors that also tried to dispute the “undesirability” of Italian immigration, and to give Italians living in the United States a truer idea of how desirable they were as immigrants.¹²

Colajanni’s volume included American authors Frank Sheridan, Amy Bernaby and Emily Meade. All had backgrounds in dealing with immigration matters. Sheridan published a report he wrote for the Bureau of Labor in 1907 that described the valuable economic contribution unskilled Italian, Polish and Slavic immigrants made to the United States, and he summarized some of his findings in *Gli italiani*. Bernaby’s contribution to the collection was an examination of the reasons immigrants tended to group together in urban areas – it was primarily to retain peer networks that provided crucial social services. Meade’s essay used statistical returns of the 1900 census to document sizable Italian participation in

¹⁰ Napoleone Colajanni, “Prefazione” in Francis J. Sheridan and Napoleone Colajanni, eds. *Gli italiani negli stati uniti: lavoro, salari, risparmi; agglomeramento nelle città; vita rurale; laboriosità, delinquenze ecc.* (Roma: Rivista Popolare, 1909), 3.

¹¹ Colajanni, “Prefazione,” 3.

¹² *Ibid.*, 5.

agricultural industries rather than just as unskilled industrial and urban laborers.¹³ These were in essence refutations of Hall's reasons for classifying certain immigrants as undesirable.

But it was Colajanni's essay that addressed head-on the arguments made by restrictionists that Italian immigration was undesirable. The list of "undesirable" immigrants in the United States, he explained, included "the Chinese, the Japanese, the Slavs, Hungarians, and above all the Italians." Most alarming to American nativists, as Colajanni perceived, was the size and the ethnic composition of the Slavic, Hungarian and Italian immigration as opposed to that "old" immigration of northern Europeans of British, German, French and even Irish background – precisely the argument that the United States Immigration Commission would support when it issued its report two years later.¹⁴ Colajanni acknowledged the point, noting that there was an obvious growth in the number of immigrants, as well as a shift in the country of origin of immigrants in the preceding twenty years. But since he did not believe in fixed and "pure" racial types, Colajanni would not concede that this geographical shift in and of itself was undesirable. This begged the question: where did this aversion to the "new" immigration come from?

¹³ Francis J. Sheridan, "Salari, ore di lavoro, consumi e risparmi degli italiani negli stati uniti"; Amy A. Bernardy, "Perchè gli italiani si addensano nelle città americane"; Emily Fogg Meade, "Gli italiani nell'agricoltura" all in Sheridan and Colajanni, *Gli italiani negli stati uniti*. See also Francis J. Sheridan, *Italian, Slavic and Hungarian Unskilled Immigrant Laborers in the United States* (Washington, D.C.: Bulletin of the Bureau of Labor, no. 72, 1907).

¹⁴ Napoleone Colajanni, "I non desiderabili" in Sheridan and Colajanni, *Gli italiani*, 85.

Colajanni saw that the aversion came from racial antagonisms, particularly those laid out by General Francis Walker, Henry Cabot Lodge, Prescott Hall, and “a hundred others.” These authors argued that the “new” immigrants – Colajanni once again noted “above all the Italians of the south” – were undesirable because they were poor and lived as burdens on state and local government; they were illiterate; they were unskilled and had a low standard of living. Keying in on Walker’s “replacement thesis,” Colajanni explained that the “new” immigrants were represented as being highly fecund, and were changing the composition of the population with their “rapid multiplication.” Restrictionists claimed that new immigrants refused to work in agriculture, concentrating instead in cities where they created and aggravated social instability. Opponents of immigration, the Italian wrote, complained that the immigrants were birds of passage, traveling back to Europe with American capital. New immigrants were represented to be criminally inclined, they did not assimilate, and they would not Americanize.¹⁵ Those were the reasons nativists saw the new immigrants as undesirable, but Colajanni felt that they had a questionable basis in scientific fact.

In the ensuing pages, Colajanni demolished each of the representations that Walker, Lodge, Hall and the “hundred others” had made, one by one. Utilizing statistical reports from public charities, for instance, he reported that Italians actually made up a tiny proportion of inmates in poor-houses and insane asylums. Compared to the proportion of Irish, German and Scandinavian inmates

¹⁵ *Ibid.*, 87-88. Colajanni noted the “special preoccupation with ‘undesirable’ Italians” as evinced by the extended examination of the conditions in Italy conducted by Immigration Commission members Dillingham, Latimer, Howell, Bennet and “Burdet” [sic]. See footnote 1, p. 88.

on some form of public support, Italians were a negligible drain. Italians, he noted, relied on other Italians for help. The statistics showed that in that respect, they were actually the “most desirable” immigrants.¹⁶

Colajanni was not the only Italian with an interest in protecting the reputation and managing the perception of Italian immigrants. In 1901 the Kingdom of Italy created the Commissariato Generale dell’Emigrazione within the Ministero degli Affari Esteri to supervise and enforce Italian regulations regarding the emigration of Italian nationals. The Commissariato published a bulletin of issues concerning Italian emigration to help officials and interested parties understand the policies of the Italian state regarding emigration, but also presented valuable and important information about the attitudes of the nations that Italians were emigrating to. For example, the 1903 issues of the *Bolletino dell’Emigrazione* provided information on aid societies for Italian immigrants in the main ports of entry in the United States, updates on legislative trends in the United States Congress – particularly concerning the progress of the literacy clause – and conditions in labor camps in the mines of West Virginia.¹⁷

¹⁶ *Ibid.*, 89-90.

¹⁷ Egipto Rossi, “Istituto di patronato dell’emigrazione italiana negli Stati Uniti” doc. 73, v. 2, n. 4 (1903) *Bolletino dell’emigrazione*, 29-47; “Nuova legge sull’immigrazione negli Stati Uniti” doc. 86, v. 2, n. 6 (1903) *Bolletino dell’emigrazione*, 94-106; Gino Speranza, “Inchiesta sugli abusi contro gli italiani nel West Virginia” doc. 118, v. 2, n. 14 (1903), *Bolletino dell’emigrazione*, 10-23. Information on the Commissariato and a tremendously helpful index can be found in Francesco Cordasco, *Italian Mass Emigration: The Exodus of a Latin People: A Bibliographical Guide to the Bolletino dell’Emigrazione, 1902-1927* (Totowa, NJ: Rowman and Littlefield, 1980). The immigration historian Donna Gabaccia warns against placing too sharp an emphasis on the “tyranny of the national,” reminding readers that migration is at its core a personal and human story. Still, she insists that “When we write transnational histories of modern migration, we produce a world history in which nations and nation states continue to be important constituent elements and explanatory forces.” Donna Gabaccia, “Is Everywhere Nowhere? Nomads, Nations, and the Immigrant Paradigm of United States History” *The Journal of American History* v. 86, n.

The *Bolletino* also reprinted and translated documents from immigration officials in the United States for Italian emigrants. Robert Watchorn, who was appointed to the immigration commission in New York City in 1905, published an article in the American *Metropolitan Magazine* in 1909 that the *Bolletino* translated so Italians could understand American attitudes toward the arriving Italians. Watchorn defended the tendency of the new immigrant arrivals to settle in cities along the eastern seaboard by pointing out that most of the available unskilled jobs were concentrated there. He explained that the only major differences between the native-born unskilled workers and foreign-born unskilled workers were the language barrier and differences in standards of hygiene and living. There were, in Watchorn's mind, no significant problems of race.¹⁸ The *Bolletino* also reprinted Colajanni's essay on the alleged criminality of Italians in the United States, to prepare emigrants from the peninsula of the attitudes they may encounter in America.¹⁹

Throughout the period of Italian immigration to the United States, the Italian government kept its citizens informed of changes in American immigration policy and American attitudes toward immigrants. Particularly in the early 1920s, when American policies against unregulated immigration hardened, the *Bolletino*

3 (December 1999), 1115-1134; "tyranny of the national" discussed on 1116-1117; quote from 1117. See also Donna Gabaccia, *From Sicily to Elizabeth Street: Housing and Social Change Among Italian Immigrants, 1880-1930* (Albany: State University of New York Press, 1984); idem., *Italy's Many Diasporas* (Seattle: University of Washington Press, 2001).

¹⁸ Robert Watchorn, "La verita sugli immigranti negli Stati Uniti" *Bolletino dell'emigrazione* n. 17, doc. 456 (1909), 87-94.

¹⁹ "La criminalita degli Italiani negli Stati Uniti. Studio dello Napoleone Colajanni" *Bolletino dell'emigrazione* n. 4, doc. 477 (1910), 3-61.

sought to inform Italians of the conditions and attitudes in the United States. Colajanni forged a path for later opponents of the immigration restriction movement and classic racialism by showing how empirical analysis that purported to show that Italians were undesirable did not demonstrate that with the certainty that it was presented. Ultimately, however, the most important criticism of classic racialism was from the American scientific community itself. To this end, Herbert Spencer Jennings's cosmopolitan training and devotion to the experimental basis of science became critical.

The Problem of Heredity

Early in his career Jennings embraced the new experimentalist methods in biological research that he was exposed to in Naples.²⁰ After Jennings left the University of Michigan for graduate work at Harvard in 1895, he wrote a note to his former advisor in Ann Arbor to discuss on his interests in experimentalism. He told Jacob Reighard that his work at Harvard was becoming more and more focused on mechanics, what he described as “the processes of development...with a view to presenting some evidence as to mechanical and other explanations of morphological processes.”²¹ Before he left for Naples he explained his basic approach to science in a letter to Mary Louise Burrage, his future wife. “Careful

²⁰ Keith Benson cautions against assigning too much importance to Naples as the “Mecca” for biological investigations. He notes that Johns Hopkins University in the 1880s was critical for establishing the standard of professional graduate training in biological science in the United States. He correctly points out that the formation of academic standards in the biological sciences was a nonlinear process, one that borrowed scientific traditions used in cell-lineage and developmental mechanics and grafted them into a new tradition of biological research. See Keith R. Benson, “Review Paper: The Naples Stazione Zoological and Its Impact on the Emergence of American Marine Biology” *Jl Hist Bio*, v. 21, n. 2 (Summer 1988), 331-41.

²¹ Herbert Spencer Jennings to Jacob Reighard, 29 December 1895, Jacob Reighard Papers. Emphasis original.

observation shows,” he told her, “that when an animal is put into new conditions, it begins to change to suit these conditions.” He then asked the key question that showed his underlying orientation to scientific knowledge of biological principles: “what makes it change that way?”²² By 1896, Jennings was already essentially opposed to one of the core principles of classic racialism, namely, that the characters of organisms were fixed and unchanging. This was something that direct observation and experimentation proved to him to be wrong. The Naples Zoological Station provided him the opportunity to further his work studying processes of development and the way stimuli affected the behavior of unicellular organisms.²³ While in Europe he also studied physiology in Germany under Max Verworn at Jena.²⁴ Upon his return to the United States, Jennings moved to Montana to take a position as instructor of botany at Montana State Agricultural and Mechanical College in Bozeman, which at least afforded him a practical environment to continue his experimental work in developmental mechanics.

But Jennings desired to return to Ann Arbor to be closer to Mary, whose family lived in Tecumseh, Michigan, a small town about thirty miles south of Ann Arbor. After a detour to Dartmouth College for a year, as an instructor in zoology, he returned to Michigan. While at Dartmouth, he made the acquaintance of a

²² Jennings to Mary Louise Burridge [“Jessica”] 1 March, 1896, Jennings Papers. Emphasis original.

²³ Elizabeth Noble Shor, “Jennings, Herbert Spencer” *American National Biography*. On the Naples Zoological Station, see Henry F. Osborn, “American Students at the Naples Zoölogical Station” *Science* n.s. v. 1, n. 9 (1 March 1895), 238-39; C. O. Whitman, “The Advantages of Study at the Naples Zoological Station” *Science* v. 2, n. 25 (27 July 1883), 93-97; Benson, “The Naples Stazione Zoologica.”

²⁴ Shor, “Jennings, Herbert Spencer.” *Entwicklungsmechanik*, or developmental mechanics, was a German method of biological investigation.

gifted mathematician named Raymond Pearl. When he returned to the University of Michigan, Jennings encouraged Pearl to come along, and working with Jennings and Reighard, Pearl was awarded the University's second Ph.D. in zoology in 1902.

Although Jennings and Pearl worked closely together as instructors and researchers, their research interests were different. Jennings's interest, as he had explained to Mary, was of individual organisms' processes of development. Pearl, on the other hand, was more interested in changes in population groups, or as Garland Allen describes, "selection acting on large-scale groups of organisms, rather than on individual breeding pairs." One worked with microscopes, the other with statistics. Despite this divergence, however, they embraced the same general principal of the importance of careful, exact measurement and experimentation, an epistemological mind-set that they developed and frequently discussed over their careers.²⁵ And it was this epistemological cosmopolitanism that underpinned their opposition to biological arguments in favor of immigration restriction in the 1920s.

Pearl's interest in using biometrics to understand changes in population groups led him to inquire, in 1904, about obtaining a laboratory position at research facilities at in Cold Spring Harbor, New York, that were being organized

²⁵ Garland Allen, "Old Wine in New Bottles: From Eugenics to Population Control in the Work of Raymond Pearl" in Keith Benson, Jane Maienschein, Ronald Rainger, eds., *The Expansion of American Biology* (New Brunswick, NJ: Rutgers University Press, 1991), 235. See also Kathy J. Cooke, "From Science to Practice, or Practice to Science? Chickens and Eggs in Raymond Pearl's Agricultural Breeding Research, 1907-1916" *Isis* v. 88, n. 1 (March 1997), 62-86; Barkan, "Reevaluating Progressive Eugenics"; Mezzano, "Progressive Origins of Eugenics Critics."

by Charles Davenport, Jennings's old Harvard instructor. Pearl wrote to Davenport to ask if there were openings at the "biological farm," and explained that ideally he would "like nothing better than an opportunity to do uninterrupted research work along biometrical and experimental lines..."²⁶ When Davenport replied that he had given the biometrical position to his long-time assistant, he still encouraged Pearl to come anyway to conduct new investigations over the coming summer.²⁷ But because Davenport expected Pearl to work on research specific to work being conducted at Cold Spring Harbor, rather than Pearl's own interests, in early March Pearl finally informed Davenport that he would have to decline the offer. Pearl then prepared to return to teaching at the University of Michigan, while he filed for grants that would eventually enable him to go to London to study under the pioneer of biometrical studies, the gifted mathematician Karl Pearson, which he did in 1905.²⁸

When Pearl returned from London the following year, Jennings had already accepted an appointment as associate professor of experimental zoology at Johns Hopkins University, where he would teach until 1938.²⁹ By the time he assumed a stable, professional position, Jennings had embraced the new experimental methods, and instructed younger researchers that the best methods

²⁶ Pearl to Charles B. Davenport, 6 January 1904, Raymond Pearl Papers, American Philosophical Society Library, Philadelphia, PA [hereafter Pearl Papers].

²⁷ Davenport to Pearl, 15 January 1904, Pearl Papers.

²⁸ Mezzano, "Progressive Origins of Eugenics Critics"; Allen, "Old Wine in New Bottles"; Theodore Porter, *Karl Pearson: The Scientific Life in a Statistical Age* (Princeton: Princeton University Press, 2004). Also helpful for the development of statistical analysis in this period is Porter, *Trust in Numbers*.

²⁹ Shor, "Jennings, Herbert Spencer."

for performing useful scientific work were for them to do the same. When another young zoologist prepared to move from the Midwest to the mid-Atlantic to train in the new investigative and experimental methods, he approached Jennings for advice. The newly established professor insisted to the student, Robert Mearns Yerkes, that “in attempting to classify and draw conclusions from objective investigations, we must be true to the objective point of view... we must not get the impression that this gives us certain distinctions which we have really brought to the matter a priori.” Jennings had been exposed to research conducted that way at Harvard, when he worked under Davenport, and he found it to be very frustrating.³⁰ Almost two years later, he repeated the advice to Yerkes, who was now using his Ph.D. in zoology to work in animal behavior and psychology. “My general principle would be, that we are always to begin with the facts, proceeding only very gradually to definitions, and that we should keep our definitions always in close touch with the facts, and regard them simply as ways of expressing the facts,” he counseled Yerkes. “We should recognize that fixed definitions are then hardly possible.”³¹ Jennings believed in this sophisticated approach and counseled others to adopt it. In terms of how to learn the “facts,” the advice and instructions Jennings gave to his younger colleagues was clear and unambiguous.

Pearl’s understanding of the proper method of learning the facts shaped his career decisions as well. After his return from Europe, Pearl accepted a job at

³⁰ Jennings to Robert M. Yerkes, 11 February, 1905, box 27, Robert M. Yerkes Papers, Manuscripts and Archives, Yale University Library, New Haven, CT [hereafter Yerkes papers]. Emphasis original. On his sentiments to Davenport, see Jennings to Mary Louise Burridge, 10 February 1895; Jennings to Burridge, 24 February 1895, both in Jennings Papers; Barkan, “Reevaluating Progressive Eugenics”; and Mezzano, “Progressive Origins.”

³¹ Jennings to Yerkes, 11 January 1907, Yerkes Papers.

the University of Pennsylvania for one year, and in the following year accepted a position from the University of Maine, to be the director of biology at the Maine Agricultural Experiment Station at Orono. Pearl relished the prospect tremendously, telling Yerkes, whose acquaintance he too had made, that he now hoped to be done with teaching, “I hope for evermore.” “The prestige which is supposed to go with a big university place has no attraction for me in comparison with the opportunity to do research work...” which in Maine took the form of studying the inheritance of characteristics in chickens.³² But Pearl was not quite as secure as Jennings in his knowledge of the mechanisms or rules of inheritance, and turned to Jennings for help, which prompted a lengthy exchange between the two men that demonstrated the developing chasm between the old investigative methods of classic racialism and the new paradigm of research experimentalism.

The context of this remarkable exchange – really a snapshot of the process of “speciation” in the biological sciences – was an article that Pearl was drafting about the results he was getting from his chicken research.³³ In late 1907, Pearl had dispatched a draft to Jennings, and Jennings replied with helpful criticisms of some of Pearl’s theoretical formulations. In the second week in January, Pearl had an opportunity to substantively engage Jennings’s critique. The theoretical problem Pearl faced was how to explain the origin and development of an

³² Raymond Pearl to Robert Yerkes, 25 November 1906, box 39, Yerkes Papers. See also Allen, “Old Wine in New Bottles”; Kathy J. Cooke, “From Science to Practice, or Practice to Science? Chickens and Eggs in Raymond Pearl’s Agricultural Breeding Research, 1907-1916” *Isis* Vol. 88, n. 1 (March 1997), 62-86.

³³ Speciation is taken from Thomas Kuhn, *The Road Since Structure*, 98. It refers to Kuhn’s modified approach to scientific “revolutions” and denotes a much more gradual process of scientific *evolution* that then leads to an irreparable break between two paradigms.

individual organism, a process also known as ontogeny. How was it, Pearl asked, that a cat developed to be a cat? There were clearly a host of organs that had to develop properly for the cat to develop – how did that happen? What if dietary habits changed or food availability shifted so that internal organs like the pancreas had to supply higher levels of digestive enzymes – how did the pancreas know how to do that? How did blood vessels in the heart “know” what angles to form as the organ developed in embryo? Answers to these questions, Pearl wrote, could only be obtained experimentally.³⁴ But therein lay the rub: you could see the angles develop, or the pancreas secrete extra enzymes, but how this *capability* developed, Pearl was struggling to understand.

Embryology was an essential avenue of investigation to answer these questions. Examining and studying the *processes* of development were the only ways to achieve results. But lacking knowledge of genetics or chromosomes, “certain proof” was impossible.³⁵ Pearl told Jennings that a lot of the theoretical problems in ontogenetic development were because “there are certainly a great many biologists who have not particularly followed the work in experimental embryology.” How organisms adapted to changed conditions as they developed, but still followed their normal development into the finished organism – what Pearl referred to as “internal modes of selective action” – were, if not mysterious, still rather opaque. “I do not know of one single bit of concrete, definite, tangible

³⁴ Pearl to Jennings, 13 January 1908, Pearl Papers. The dates of the letters referred to in this paragraph are derived from this letter from Pearl to Jennings.

³⁵ See Stephen Brush’s very interesting article on Thomas Hunt Morgan’s chromosome theory: “How Theories became Knowledge: Morgan’s Chromosome Theory of Heredity in America and Britain” *Jl Hist Bio* v. 25, n. 3 (Fall 2002), 471-535.

evidence,” he wrote to Jennings, that explained adaptiveness in organisms. Selection, Pearl pointed out, seemed to be mostly trial and error, but “when an animal developes [sic] or the pancreas pours out just the right kind of juice at a moment’s warning...where is the evidence of ‘trials,’ the failure or ‘errors,’ and the persistence of the right, i.e. the [teleological]?”³⁶ Pearl explained that one would see examples of the “right,” but of the failures or of the trials, one would see nothing. What is necessary, Pearl insisted repeatedly throughout the letter, was “evidence.”³⁷

In his letter, Pearl repeatedly insisted that evidence could only be obtained through experimentation, which he said was to “observe with precision” how certain processes unfolded. In a chick blastoderm, he explained, how is it that blood vessels formed in the way they did? Laws of hydrodynamics provided “absolute criteria as to the proper angles of branching to give the best” results, but as Pearl examined teratological developments – malformations and defects in development – he had no evidentiary cause of the defects. Pearl suggested that if one biometrically studied the process, and then experimentally made new conditions, worked the processes out again, and compared the results, that “this ought to throw a lot of light where it is needed.”³⁸ He expressed his confidence in

³⁶ Pearl to Jennings, 13 January 1908, Pearl Papers. Pearl’s letter, which contains many hand-corrected typing errors, has “teteological.” Emphasis original.

³⁷ *Ibid.* Pearl placed the emphasis on “evidence” – which he used over ten times in the letter.

³⁸ *Ibid.*, 2.

experimentation, observation, and manipulation – these were the methods that would produce adequate evidence.

But science could offer no certainty, so Jennings wrote a very long, handwritten reply to Pearl, first offering hope that Pearl would not put off publishing his paper, but publish it as it was, in the hope of generating both discussion and new experimental and theoretical work on individual selection. The trouble at that time was that since so many of the processes biology and zoology were studying were internal and microscopic, observations were exceedingly difficult. But conceptualizing the developmental process in organisms, Jennings told Pearl, might best be done by analogy. And for the analogy, he turned to the same example that Francis Galton had used: a stick traveling down a stream.

Jennings explained the development of organisms in very practical terms that obscured the larger difficulties of modern experimentalism. If one thought of physical development in an organism as *entelechy*, a process requiring energy for actualization, Jennings explained that any interference with the flow of energy results in “starting up new things till something relieves the stoppage...” at which point the organism continues its normal development. A stream of water behaved the same way. He told Pearl that if anything interferes with the flow of a stream, the stream will regulate itself and continue on its course: “It is not limited to one method of accomplishing its results, any more than the organism is. Some obstacles it carries away; some it digs under and buries; some it dissolves; some it goes around... one [method] finally prevails, because it works best. I can’t help but believe that there is a really great similarity here with regulation in

organisms.”³⁹ Jennings used this analogy to distinguish between end results and a process of development, and in many ways, made a distinction that stands for the elemental difference between classic racialism and modern experimentalism.

Jennings, Pearl, Boas and Colajanni were interested in the *processes*, the way things developed and how they developed. Classic racialists like Davenport, his associate at his research station in New York Harry H. Laughlin, Madison Grant, and the members of the Immigration Restriction League were interested only in the results. This led the latter group to minimize the processes; it *assumed* that it would always, and universally, produce the same end result. Jennings and Galton both used a stream analogy to explain development and heredity. But there was an absolutely key difference: Galton was interested in the stream and sticks on a meta-level. Jennings was interested in the stream on a specific, immediate level. For Galton, the stick would always reach the end of the stream because of its natural weight and shape, so how it got there was irrelevant. It simply would. You could, because of the nature of the stick and the water in the stream, assume that the stick would remain a stick and inevitably ricochet down the stream. The experimentalists, these “new” biologists, these cosmopolitan, modern researchers, did not share that conviction or assumption, because they understood the importance of the process – the path the stick traveled downstream was just as important as the fact that it did travel downstream. Perhaps it cracked or broke on a rock or became pinned against a bend in the stream. One could not, therefore,

³⁹ Jennings to Pearl, 11 February 1908, Pearl Papers.

accurately deduce from the qualities of the stick or the characteristics of the water, what would happen to the stick without carefully observing it.

The example of the stream illuminates the principal differences between these competing languages or idioms of science – between classic racialism and experimentalism – which were two-fold: the nature of proof, and the subject of investigation. Classic racialists took patterns, correlations or associations to be adequate proofs, and utilized broad non-controlled samples as subjects for investigation. From Galton to Lombroso to Davenport, the main question was “what rules can be deduced from a given phenomenon?” Galton would answer that one can deduce, from pedigrees of 300 men, that talent and genius is inherited; Lombroso would reason that a sample of 800 criminals shows that they are deviant by nature and atavistic in form; and Davenport would argue that a sample of 60 naval officers’ pedigrees shows *thalassophilia*, or “love of the sea,” to be hereditary. In a pronounced way, classic racialism was a syllogistic or deductive method of inquiry into biological processes and phenomena.

The developing alternative, seeking to provide answers that the classic racist paradigm could not adequately solve, took tangible, direct, and experimental evidence to be the essential proof, and frequently took the individual as the principal subject of investigation. Definitive evidence of biological laws of development was best – or most accurately – obtained through experimentation and the replication and verification of its results. Its reasoning and logic were primarily inductive. But it faced a tremendous obstacle in that it was very methodologically complicated and esoteric, and the correspondence between

Pearl and Jennings shows this. The medical historian Charles Rosenberg describes the difficulty in this way: “The more tenuous an area of scientific knowledge, the smaller its verifiable content, [and] the more easily its data may be bent to social purposes.”⁴⁰ The professional scientific community, with only a very tenuous grasp on the mechanisms of hereditary transmission, and an absence of consensus in method and theory (which would not develop and crystallize until the 1920s) lacked a universal agreement of “its verifiable content” of biological principles. And as Rosenberg suggests, biological knowledge was thus quite easily bent to social purposes, particularly the restriction of immigration based upon spurious accusations of biological and heredity “racial” inferiority.

This dilemma between inductive and deductive reasoning in the biological and natural sciences can also be seen in the association and interaction between two additional exemplars of each method: Franz Boas and Charles Davenport. Boas represented the new method of inductive research and reasoning; Davenport the older method of deductive inference and association.

The emerging science of anthropology examined the development and origin of human beings, and was in essence, designed to study differences among population groups. It should not be surprising, then, to find anthropologists among the early defenders of the boundaries of proper scientific method: advocating careful assertions of truth, verified by experimental research and proofs. Two of these defenders—who were both immigrants themselves—were Columbia

⁴⁰ Rosenberg, *No Other Gods*, 10.

University anthropologist Franz Boas, and the head of the Smithsonian Institution's Division of Physical Anthropology, Ales Hrdlicka.

Boas was an early and consistent critic of the methods and conclusions derived from classic racialist principles. He was particularly prominent in criticizing the biological determinism of immigrants that restrictionists relied upon to support exclusion. Born and educated in Germany, Boas immigrated to the United States in 1887. He was appointed a full professor of anthropology at Columbia University in 1899, and remained at the university for the rest of his professional career. His early ethnological research was among Native Americans of the northwest, and it imparted to Boas a firm belief in the malleability of human body form and a wide variety of intellectual ability in population groups. His early research exploded the classic racialist myth that like would always produce like, regardless of environmental influence; his examinations of primitive peoples showed that the form of human bodies was elastic, and environmental influences were profoundly important in determining physical development.⁴¹ As he accumulated evidence throughout the early twentieth century, Boas tried to refute the arguments of nativists that the new immigrants from southern and eastern Europe were unassimilable and undesirable. Just as with Italian anthropologists who argued for a loose-fitting categorization of the Italian population in opposition to their racial determinist countrymen, and American

⁴¹ See for instance Franz Boas, "Instability of Human Types" *Papers on Interracial Problems at the First Universal Racial Congress, University of London, July 26-29, 1915*; idem., *The Mind of Primitive Man* (New York: MacMillan Press, 1911).

biologists who cautiously worked toward a heredity theory of chromosomal inheritance and developmental mechanics, Boas had difficulty in combating classic racialism's theory of fixed racial identities in the immigrant populations in the United States.⁴²

One of the first opportunities Boas had to examine changes in immigrant body types was through his appointment to the United States Immigration Commission in 1908. When Cornell political economy professor Jeremiah Jenks was named to the federal commission in 1907, Boas used his contact with Jenks to suggest a detailed anthropological study of the physical changes that immigrants underwent after they arrived in the United States and what, if any, physical changes occurred in their children.⁴³ In mid-March 1908, Boas outlined his plan to Jenks for investigating five immigrant racial types, hoping that the political economist could secure Congressional funding for the project as part of the Commission's larger study of immigration. Boas's proposal called for an examination of about 120,000 immigrants, studying their physical development to

⁴² Boas's role as a leader in the critiques of "scientific racism" has been very well-documented. For example, consult George W. Stocking Jr., *The Shaping of American Anthropology, 1883-1911: A Franz Boas Reader* (New York: Basic Books, 1974); idem., *Delimiting Anthropology: Occasional Essays and Reflections* (Madison: University of Wisconsin Press, 2001), esp. Part I; Regna Darnell, *Invisible Genealogies: A History of Americanist Anthropology* (Lincoln, NE: University of Nebraska Press, 2001); Vernon Williams, *Rethinking Race: Franz Boas and His Contemporaries* (Lexington: University of Kentucky Press, 1996); Jacobsen, *Whiteness of a Different Color*, esp. chapter 3; George Stocking Jr., "Lamarckianism in American Social Science: 1890-1915" *Journal of the History of Ideas* v. 23, n. 2 (April-June 1962), 239-56; idem, ed. *Bones, Bodies, Behavior: Essays on Biological Anthropology* (Madison, WI: University of Wisconsin Press, 1988); idem., ed. *Volksgeist as Method and Ethic: Essays on Boasian Ethnography and the German Anthropological Tradition* (Madison, WI: University of Wisconsin Press, 1996); Higham, *Strangers in the Land*, esp. chapter 6; Solomon, *Ancestors and Immigrants*.

⁴³ Robert F. Zeidel, *Immigrants, Progressives, and Exclusion Politics: The Dillingham Commission, 1900-1927* (Dekalb, IL: Northern Illinois University Press, 2004).

determine the extent, and the success, of assimilation. The size of the sample was immense, the anthropologist acknowledged, and although he would reduce the number in the study if it was necessary, doing so would “seriously reduce the usefulness of the investigation.”⁴⁴ Boas intended his report to be thorough and scientifically rigorous.

Boas was confident that his study would generate important results that may help undercut nativist opposition to immigration. Shortly after outlining the study, he tried to reassure Jenks of the study’s likely success. Noting that there was indeed a physical difference between the newer immigrants and those who had arrived in the nineteenth century, determining the ability of the new immigrants to be amalgamated and assimilated was of great importance. Modern anthropological methods of carefully and precisely conducted measurements of a wide variety of physical characteristics, Boas said, were sufficiently sophisticated to provide a definitive answer to this complex problem. The study would examine emigrants, physical changes in their children, and the effects of intermarriage between North, Central, East, and Southern Europeans.⁴⁵ Boas was confident in

⁴⁴ Boas to Jeremiah Jenks, 19 March 1908, Franz Boas, *The Professional Correspondence of Franz Boas* (American Philosophical Society Library, Philadelphia; Wilmington, DE: Scholarly Resources, 1972), Microfilm [hereafter *Boas Film*].

⁴⁵ Boas to Jenks, 23 March, 1908, *Boas Film*. Interestingly, Boas noted that the South European type would be primarily “Italians of the region south of Rome.” Boas explained his methodology in United States Immigration Commission, *Reports of the Immigration Commission* v. 38, “Changes in the Bodily Form of Descendents of Immigrants” by Franz Boas, Sen. Doc. 208, 61st Congress, 2nd Session (Washington, D.C.: Government Printing Office, 1911), 82-84. For earlier anthropological methods, in addition to Stocking’s body of work, see also Roger Stanton, *The Leopard’s Spots: Scientific Attitudes Toward Race in America, 1815-59* (Chicago: University of Chicago Press, 1960); John S. Haller, *Outcasts from Evolution: Scientific Attitudes of Racial Inferiority, 1859-1900* (Urbana, IL: University of Illinois Press, 1971). (Charles Darwin’s book *On the Origin of Species* was published in November 1859.)

anthropological science and quantification, though not completely so: “I believe I can assure you that the practical results of this investigation will be important in so far as they will settle once and for all the question whether the immigrants from southern Europe and from eastern Europe are and can be assimilated by our people.” Jenks agreed, writing back that he was convinced of the utility and import of the proposal, and the Cornell professor recommended the proposal strongly to the other eight members of the Immigration Commission. Yet when they voted on the project on April 29 1908, with one member absent, the Commission deadlocked at four in favor, four opposed. The reason for the opposition, Jenks wrote to Boas, was the opinion that the results would not be clear enough to justify the outlay of money. The critical factor was what some members felt was the inability, even of modern anthropological methods, to provide definitive, certain proof: “there would be so much dispute over the significance of the figures that the results would probably be considered barren and would not be of any special credit to the Commission.”⁴⁶ But Boas believed

⁴⁶ Jenks to Boas, 29 April, 1908, *Boas Film*. Also, Boas to Jenks 2 May 1908; Jenks to Boas 12 May 1908; Boas to Jenks 13 May 1908; Jenks to Boas 20 May 1908; Boas letters to Jenks of 3 June, 9 June, and 26 June, and Jenks’s replies of 5 June, 16 June, 29 June, 1908, all from *Boas Film*. Dorothy Ross ties together the emergence of social science with the dominant American cultural values of exceptionalism and the tension it maintained with a pluralist thread of American culture. She notes that the development of American social sciences was part of a larger trend of all sciences searching for sources of intellectual and professional authority, but was also a function of the structure of American society. Ross, *The Origins of American Social Science*, 160. Also instructive are the essays in Sally Gregory Kohlstedt, Michael M. Sokal, Bruce Lewenstein, eds., *The Establishment of Science in America: 150 years of the American Association for the Advancement of Science* (New Brunswick, NJ: Rutgers University Press, 1999); Thomas L. Haskell, *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority* (Urbana, IL: University of Illinois Press, 1977); and the essays in Thomas L. Haskell, ed. *The Authority of Experts: Studies in History and Theory* (Bloomington, IN: Indiana University Press, 1984). For the development of anthropology and Boas’s role, see Regna Darnell, *And Along Came Boas: Continuity and Revolution in Americanist Anthropology* (Philadelphia: John Benjamins Publishing Company, 1998).

his methods could show definitively the capacity for immigrants to be assimilated. He argued that the congressional opponents lacked any real understanding of anthropology (and none were professional anthropologists), and Boas felt their opposition was based on this ignorance. In an instance like this, whose scientific authority would be more convincing?

Apparently, it was Boas's. Throughout May and June he and Jenks continued to flesh out the anthropologist's plans for the immigrant study, outlining reasons why the study would contribute positively to the scientific knowledge of immigration to the United States and the capacity of immigrants to assimilate, and hoping Jenks could persuade enough members of the Commission to back it. Boas already had a reputation as a solid anthropologist, which helped Jenks ultimately win a majority of votes from Commission members to approve the study. Boas set to work. At schools throughout the New York City area, the Columbia anthropologist and his team conducted careful measurements on immigrant children, and in some cases extended the physical measurements to the parents and extended family. Cooperation from the foreign-language press smoothed the way for these important measurements to be taken.⁴⁷

In February 1909, Boas wrote a preliminary report for distribution to Congress. The early results of the study, the report explained, indicated that the American environment might act favorably upon some immigrant types, and unfavorably on others.⁴⁸ Shortly after submitting it to Congress, Boas wrote a

⁴⁷ Franz Boas, "Changes in the Bodily Form of Descendants of Immigrants."

more elaborate statement to Jenks in an attempt to prevent Congress from cutting back on appropriations for the Commission's work. The anthropologist explained that his investigation had shown that descendants of immigrants born in the United States developed physically earlier and better than the immigrants themselves or their ancestors in their native countries, that the environmental effect on body form was remarkably strong – the longer the parents resided in the country before reproducing, the more noticeable the influence of environment – and finally, that the children of immigrants born in this country showed a change in form approaching an American type.⁴⁹

Boas's final report to the Commission, published by Congress in 1911, provided a wealth of statistical and anthropometric data on the physical changes that immigrants and their children underwent in their new homeland. Over 400 pages of the appendix were devoted to computations and analysis of the measurements Boas and his carefully selected researchers obtained. He presented a rather shocking finding: "not even those characteristics of a race which have proved to be most permanent in their old home remain the same under the new surroundings; and we are compelled to conclude that when these features of the body change, the whole bodily and mental make-up of the immigrants may change."⁵⁰ There was a direct correlation between period of residency of the parents in the United States and the plasticity of the bodily form of their children.

⁴⁸ Boas to William W. Husband, 24 February 1909, *Boas Film*.

⁴⁹ Boas to Jenks, 11 March 1909, reprinted in Stocking, *Franz Boas Reader*, on 210. Stocking points out that in all of these writings, Boas never defined exactly what constituted "the American type," which he considers an important flaw in Boas's analysis.

⁵⁰ Boas, "Changes in the Bodily Form of Descendants of Immigrants," 5.

If classic racialism assumed that body form and mental characters were unchanging and stable, if they were fixed and hereditary qualities, Boas's study results seemed to dramatically explode these claims. Yet despite a favorable academic reception, despite an additional Boas article in *American Anthropologist* in 1913 that provided further evidence for plasticity of body type, the perception of fixed and unchanging racial types persisted in the public mind and the minds of policy makers.⁵¹ Why was this careful scientific cosmopolitanism not more effective?

One of the difficulties was that the dominant physical perception of the parents, the immigrants themselves, was that they were so different and alien. But a larger difficulty was a lack of answers: how did this adaptation occur? Was it a reversion to a mean or a product of dietary improvement? Would these anthropometric changes be passed on to later generations? If so, how? Anthropology could provide no positive answers. In contrast, though not exactly based on certainty, classic racialism *would* provide these answers, even if they seemed to be wrong.

⁵¹ See, for instance, Franz Boas and Helene Boas, "The Head-Forms of the Italians as Influenced by Heredity and Environment" *American Anthropologist* n.s. v. 15, n. 2 (April-June 1913), 163-88. Boas was still defending the results of his immigrant body studies in the 1930s. Franz Boas, "The Effects of American Environment on Immigrants and Their Descendants" *Science* n.s. v. 84, n. 2189 (11 December 1936), 522-25; idem., "Changes in Bodily Form of Descendants of Immigrants" *American Anthropologist* n.s., v. 42, n. 2 Part I (April-June 1940), 183-89; idem., "Race and Progress" *Science* n.s. v. 74, n. 1905 (3 July 1931), 1-8, which was a reprint of Boas's Presidential Address to the American Association for the Advancement of Science given on 15 June 1931. For the reception of Boas's report, see G. U. Y., review of Franz Boas, *Changes in the Bodily Form of Descendants of Immigrants* in the *Journal of the Royal Statistical Society* v. 76, n. 4 (March 1913), 417-18; Review of Franz Boas, *Changes in the Bodily Form of Descendants of Immigrants* in *Bulletin of the American Geographical Society* v. 46, n. 1 (1914), 62; H. J. Fleure, review of Franz Boas, *Changes in the Bodily Form of Descendants of Immigrants* in *Man* v. 14 (1914), 206-08.

Eugenics played an essential role in legitimizing the answers that classic racialism provided. The American Breeders' Association (ABA), founded in 1903, was designed to help farmers and agriculturalists apply the principles of genetics to plant and animal stocks. The establishment within the ABA of a Committee on Eugenics in 1906 offered an opportunity for scientists to not only explain the goals and methods of eugenics to a lay-audience, but also provided themselves with living laboratories to test out the mechanisms of heredity and inheritance.⁵²

Charles Davenport understood early on the opportunities that plant and animal husbandry provided for testing theories of inheritance and heredity. Writing to the American inventor Alexander Graham Bell in 1907 for advice on how to prevent the eugenics section of the ABA from splintering off into a separate organization, Davenport emphasized the importance experimental research had for genetics, and in fact, how the whole idea of hereditary science had come from work done on inherited characteristics in peas. "Students of eugenics must be scientifically trained," explained Davenport, and have particularly rich experience in experimentation. "A separate eugenics society would be cut off from the main body of experimentalists and would attract rather the speculative and those with a sentimental interest in the subject who have no

⁵² Barbara Kimmelman, "The American Breeders' Association: Genetics and Eugenics in an Agricultural Context, 1903-13" *Social Studies of Science* v. 13, n. 2 (May 1983), 163-204.

training to advance it as a science.”⁵³ Ironically, though the split was prevented, this was exactly what happened to the eugenics movement in the United States. Heredity could very easily be studied, over long generational lines, in plants and animals. This was the same idea that induced Pearl to move to the Maine agricultural experiment station. Experimental proof from breeding plants and farm animals was essential to advance the knowledge of heredity, and the thousands of farmers and farms throughout the country could provide a wealth of experimental data.

Therein lay the great difficulty: farmers did not care about the theoretical or scientific aspects of breeding. They were not scientifically trained. They were farmers, not experimentalists. Farmers wanted practical and useful knowledge of how to improve crop yields or get cows to produce more milk. Relying on farmers to produce rigorous scientific data was futile – most had zero interest in ensuring the purity of their breeding specimens or maintaining the complex records of generational characteristics and measurements. That was one of the things Davenport liked about Bell – Bell did maintain detailed records that were of genuine scientific utility. So keeping eugenics within the ABA, while important for Davenport, ultimately inhibited the proper scientific and experimental growth of knowledge of heredity. Thomas Hunt Morgan, who studied the simple fruit fly, discovered the great advances in heredity. Morgan kept meticulous records,

⁵³ Charles Davenport to Alexander Graham Bell, 29 January 1907, Davenport papers. The correspondence between the two men was rather extensive, but it most often contained advice and data on Bell’s genealogical and inheritance studies of sheep. Bell was a supporter of eugenics; he served as the Chair of the Board of Scientific Directors of the Eugenics Record Office from 1912 to 1915.

examined with microscopes the changes in his fruit fly populations, and searched for chromosomal markers for changes, modifications or mutations in the *drosophila* populations he bred in his lab. Farmers and breeders in the ABA not only lacked the scientific knowledge and equipment to conduct productive studies; they had no interest in doing so.

Despite Davenport's protestations that eugenics work be carried out carefully, and integrate multiple methods and approaches (i.e. both analytical and experimental work by trained professionals), his inability to follow his own advice in the ensuing decades would be partly responsible for the decline of the legitimacy of the movement. In fact, it quickly became quite clear that Davenport was merely paying lip-service to the high standards that he espoused for eugenic knowledge. When a permanent subcommittee on immigration was established within the Committee on Eugenics in the ABA in 1911, Davenport, who was serving as Secretary under Chairman David Starr Jordan, wrote to an old Boston friend with a good deal of experience in immigration matters for help filling out the sub-committee's membership. Prescott Hall, one of the founders of the Immigration Restriction League, received from Davenport a possible list of members, which included Franz Boas, William Williams (the Ellis Island Immigration Commissioner), and Commissioner General of Immigration Daniel Joseph Keefe. Although Davenport explained that the sub-committee's main object would be "a more detailed study of the hereditary traits that immigrants are bringing into this country," he insisted that the group not commit itself in advance to any position of liberalizing or restricting immigration, and that it especially

avoid propaganda.⁵⁴ In asking for Hall's input on men to serve on the immigration sub-committee, however, Davenport was either acting very shrewdly, or was being incredibly naïve. A man with Hall's record of opposition to free immigration would not be likely to appoint objective workers.

As one of the founders of the Immigration Restriction League, Hall's opinion on what the sub-committee should ultimately recommend was a foregone conclusion. His 1906 book *Immigration and Its Effects upon the United States* argued that the "new" immigrants arriving on American shores posed a great peril to the American way of life. It argued that many of the races of the new immigrants, particularly southern Italians and Jews, were incapable of assimilating to the core of "old stock" Americans because they were biologically different. *Immigration and Its Effects* used Darwinian and classic racist arguments that, in alarmist tones anticipating Madison Grant, unrestricted immigration would mean the degeneration of the "average quality" of the American stock.⁵⁵ In his letter, Hall assured Davenport that he agreed that the work of the committee should be "strictly impartial," but in the same sentence undermined that impartiality by opposing recommended members that would likely disagree with his desired recommendations. Although the Boston lawyer conceded Davenport was in a "much better position" to know who would make suitable candidates, he strenuously opposed the selection of Boas. Hall was far too

⁵⁴ Charles Davenport to Prescott F. Hall, 20 May 1911, box 1, IRL Papers.

⁵⁵ Prescott F. Hall, *Immigration and Its Effects upon the United States* (New York: Henry Holt Co., 1906).

deeply enmeshed in immigration matters and too well connected not to have been up-to-date on Boas's work for the Immigration Commission of the United States.⁵⁶

Hall's suggestions reflected the League's overall attitude to science: it was a tool to be used to justify their ideological positions against immigration. Instead of Davenport's list of candidates, Hall suggested several of his close associates in the IRL. "Of course, it always sounds ungracious to say that people on one's own side are patriotic and sincere, and the people on the other side are not," explained the Boston lawyer, "but I know the men who have worked with me for many years...and I know that while they may be prejudiced, they are sincere." How his recommendations of prejudiced but sincere men would establish an impartial, unbiased investigation by the committee, Hall did not say. Speaking specifically of Boas, Hall conceded that his technical qualifications were suitable, and that he himself was not competent to pass on the results of Boas's work. But like his preceding objection to another suggested candidate, Herbert Sherwood, Hall questioned Boas's ability to be neutral because Hall believed them to be opposed to restriction because of previous associations with pro-immigrant groups.⁵⁷

⁵⁶ Hall to Davenport, 22 May 1911, box 1, IRL Papers. Hall's and the IRL's involvement with the Dillingham Commission are explored in the following chapter. See also Robert F. Zeidel, *Immigrants, Progressives, and Exclusion Politics: The Dillingham Commission, 1900-1927* (Dekalb, IL: Northern Illinois University Press 2004).

⁵⁷ Hall to Davenport, 22 May 1911, box 1, IRL Papers. Hall explained that Sherwood represented the National Liberal Immigration League and had worked closely with one of the members of the Dillingham Commission, Representative William Bennet (who dissented from the Commission's recommendation in favor of the literacy test). The National Liberal Immigration League, he pointed out, "has been stated by the Chairman of the House Immigration Committee to be organized and financed by the steamship companies in order to fight all immigration legislation, and I should personally feel some doubt as to Mr. Sherwood's ability to be neutral on any investigating committee." Of Boas, "Of course, he has certain technical training for such work," but Hall thought him to be related to an agent of the Hamburg-American steamship line to whom

Davenport was unable to completely accept Hall's suggestions, as he desired the subcommittee to have at least some professional credibility. When the list of members for the sub-committee on immigration finally came from Davenport, who was still acting as secretary of the Eugenics Section, it included the following names: Hall (as secretary), Boas, Alexander Cance, James Field, and Robert DeCourcy Ward. Davenport explained that he felt it was necessary to have "Boaz" on the committee, "even if he has liberalizing tendencies," because he was such a well-known leader in anthropology. He could, Davenport hoped, add some scientific gravitas and respectability.⁵⁸ Hall had no need for respectability. He wanted a report from the ABA to affirm the position of his Immigration Restriction League. Ward, Hall's associate and friend, supported him in this approach. In the following year, as the sub-committee drafted a report for the ABA on what policy solutions and field examinations should be studied, Ward laid out suggestions: "I think we ought to make as strong a report as possible...and give the impression that we have done a lot of thinking, if not any original investigation." With Ward and Hall on the committee, it seems in advance that the "thinking" would be geared specifically toward finding suitable

Bennet had given a "fat job" to placate the company. For Bennet's position on the Dillingham Committee's recommendations, see United States Immigration Commission, *Reports of the Immigration Commission* v. 1 "Abstracts of Reports" Sen. doc. 747/1, 61st Congress, 3rd Session (Washington, D.C.: Government Printing Office, 1911), 49.

⁵⁸ Davenport to Hall, 17 November 1911, box 1, IRL Papers, Harvard. Ward was a professor of Climatology at Harvard; Field taught at the University of Chicago, and Cance worked for the U.S. Department of Agriculture.

justifications in favor of restriction.⁵⁹ Armed with this suggestion, Hall prepared to write out the draft of the report.

He drafted it in about five weeks, which suggests that no original or deep research had been conducted in the matter.⁶⁰ He sent out the draft to the members of the immigration committee for their comments; the copy for Boas arrived at Columbia while the professor was out of the country. When he finally returned and read the draft, the anthropologist was not pleased. Firing off a terse note to Hall, Boas regretted to inform him that “I do not consider the report acceptable, since I disagree with practically all the positions taken in it. If a serious report is to be drawn up, I am ready to attend a meeting, but I cannot approve, much less sign your draft.”⁶¹ What could there have been to object to? For starters, Boas likely resented the implication that certain “elements of immigration” were dangerous to the “well being of the nation for eugenic reasons.” But it is likely that the assumptions underlying the report were even more unacceptable to Boas. The arguments of restrictionists like Hall and Ward, particularly later in the post-war years, centered around classic racist precepts that biological identity, physical stature and mental ability were unchanging and static, and the draft implied that the entire subcommittee worked with the assumption that this was

⁵⁹ Robert DeCourcy Ward to Hall, 28 June 1912, box 1, IRL papers. Ward would be unable to help much with the drafting, as he was going away for the summer, but he suggested to Hall that the evidence used in the report should come as much from official government reports as possible. This would, he believed, show that it was not only the Committee on Eugenics in the ABA that was worried about the racial degeneration of the country, but the government as well.

⁶⁰ There are no early drafts of the first report in the papers of the IRL; the final “First Report of the Immigration Committee of the Eugenics Section, American Breeders’ Association” is in box 4, IRL Papers: Compositions.

⁶¹ Franz Boas to Prescott Hall, 23 August 1912, *Boas Film*.
Chapter 2

true.⁶² Boas's research with immigrant body form for the Immigration Commission had proven this assumption wrong: he knew that human body form was quite malleable, and subject to both positive and negative environmental influences that impacted its development and abilities.

Boas, Hall and Davenport represented competing thrusts of the Progressive movement, though they were not mutually exclusive. Boas represented the professionalizing impetus of the period. Professional academics were able to provide expertise to address the social problems that Progressives tried to confront. One of the mechanisms to create consensus on the new professionalizing fields was through the establishment of disciplinary journals, like the journal *Science*, which in 1894 was taken over by the American Association for the Advancement of Science and its editor James McKeen Cattell, a professor of experimental psychology at Columbia.⁶³ Settlement house workers, social workers, academics in newly articulated and defined disciplines, statisticians and political economists, among many others, offered specific areas of knowledge that they were willing and able to deploy for the benefit of society. Hall's social circle, which contained many old families and blue-blood pedigrees, manifested the social anxieties of a rapidly changing society that was described in the early historiography of Progressivism, but also fits in with what the historian

⁶² "First Report of the Immigration Committee of the Eugenics Section, American Breeders' Association" IRL Papers.

⁶³ See Michael M. Sokal, "Promoting Science in a New Century: The Middle Years of the AAAS" in Sokal et al eds., *The Establishment of Science in America*, 52ff; Peter Dobkin Hall, "The Social Foundations of Professional Credibility", Dorothy Ross, "American Social Science and the Idea of Progress" both in Haskell, ed. *The Authority of Experts*.

Michael McGerr has recently described as an exclusionist and segregationist impulse within certain Progressive circles.⁶⁴ Joseph Lee, one of Hall's colleagues on the Executive Committee of the IRL was deeply involved in the playground reform movement as part of a broader involvement in helping children "become productive citizens who would contribute to the continuing development of a democratic society."⁶⁵ Madison Grant, who came to the IRL somewhat later and penned *The Passing of the Great Race*, was also a dedicated conservationist, a movement that unified the scientific management and preservation of the

⁶⁴ Michael McGerr, *A Fierce Discontent: The Rise and Fall of the Progressive Movement in America* (Oxford: University of Oxford Press, 2003), esp. 211-14. Richard Hofstadter began the "status anxiety" argument, explaining that members of the established American Protestant middle led the reform movement in the early 1900s in response to what they perceived as dramatic changes in their social status and influence in *The Age of Reform: From Bryan to F.D.R.* (New York: Vintage Books, 1955); this was followed by Robert Wiebe's *The Search for Order, 1877-1920* (New York: Hill and Wang, 1967) which suggested that this middle class was not established, insecure, and backward-looking, but rather dynamic and forward looking professionals; Samuel Hays, *The Response to Industrialism, 1885-1914* 2nd ed. (Chicago: University of Chicago Press, 1995 [orig.1957]) was an important contribution as well. Urbanites received close attention in John Buenker, *Urban Liberalism and Progressive Reform* (New York: Scribner's, 1973) and Paul Boyer, *Urban Masses and Moral Order in America, 1820-1920* (Cambridge, MA: Harvard University Press, 1978). Arthur Link and Richard McCormick provide a solid overview in *Progressivism* (Wheeling, IL: Harlan Davidson, Inc., 1983). Robert Johnson, "Re-Democratizing the Progressive Era: The Politics of Progressive Era Historiography" *Journal of the Gilded Age and Progressive Era* v. 1, n. 1 (January 2002), 68-92 for an examination of shifts of interpretations of the period.

⁶⁵ Maureen Flanagan, *America Reformed: Progressives and Progressivisms, 1890s-1920s* (New York: Oxford University Press, 2007), 68. Flanagan describes immigration restriction as a progressive impulse rationalized as acceptable "as part of the struggle to end laissez faire liberalism and create a more orderly society," although an equal number of progressives opposed restriction. Quote from 262; see 262-64. Also see Donald Pickens, *Eugenics and the Progressives* (Nashville: Vanderbilt University Press, 1968); Robert F. Zeidel, *Immigrants, Progressives, and Exclusion Politics: The Dillingham Commission, 1900-1927* (DeKalb, IL: Northern Illinois University Press, 2004); also Mezzano, "The Progressive Origins of Eugenics Critics" and Barkan, "Reevaluating Progressive Eugenics" for the role of scientific knowledge in the progressive era. Leon Fink's recent book *Progressive Intellectuals and the Dilemmas of Democratic Commitment* (Cambridge, MA: Harvard University Press, 1997) considers how public intellectuals (not necessarily academics) also shaped the reforms of the period.

country's natural resources.⁶⁶ Davenport's facilities at Cold Spring Harbor, which were partially funded by the Carnegie Institution of Washington manifested the power of philanthropy to address social problems. The knowledge of eugenics that emanated from his New York facilities was aimed at improving the public health and the quality of life for American families.⁶⁷

If the subcommittee represented the broad thrust of Progressivism, it also reflected some of its tensions. Boas refused to sign the report that was based on this assumption of fixed and permanent racial types. Hall took offence that Boas implied it was not a "serious" report: "I do not think it is very courteous to the three members of the committee who will sign the draft sent you, to imply that it is not a serious report." (It was apparently not necessary to inform Boas that only one member wrote the bulk of the draft.) To placate the distinguished anthropologist, Hall suggested that perhaps a few changes could be made — if Boas would suggest some — that would be amenable to all. Boas does not appear to have replied, which did not surprise or disappoint Hall. After writing his reply to Boas, the Bostonian immediately penned a gossipy letter to another member of the immigration sub-committee, James A. Field. After recounting the letter he

⁶⁶ See Jonathan Spiro, "Patrician Racist: The Evolution of Madison Grant" (Ph.D. Diss.: University of California, Berkeley, 2000); Flanagan, *America Reformed*, chap. 6 for conservationism. Spiro's impressive biography of Madison Grant was published just as this dissertation was being filed. Jonathan Spiro, *Defending the Master Race: Conservation, Eugenics, and the Legacy of Madison Grant* (Burlington, VT: University of Vermont Press, 2009).

⁶⁷ Allen, "The Eugenics Record Office at Cold Spring Harbor, 1910-1940: An Essay in Institutional History" *Osiris* 2nd series, v. 2 (1986), 225-64 for the history of Davenport's facilities; Wendy Kline, *Building a Better Race: Gender, Sexuality, and Eugenics from the Turn of the Century to the Baby Boom* (Berkeley: University of California Press, 2001); Edward Larson, *Sex, Race, and Science: Eugenics in the Deep South* (Baltimore: Johns Hopkins University Press, 1995)

received from Boas, Hall was able to take satisfaction in being proven right. “I told Davenport at the start there would be trouble if Boas was put on the committee,” he explained, “not because I don’t agree with [Boas], but because very few Jews have any manners.” Why Davenport would have even recommended Boas to serve on the committee in the first place had been a mystery to Hall. Despite the modest acclaim Boas received from his work for the Dillingham Commission, Hall told Field that “Madison Grant tells me the biologists are all laughing at his measurement of school children’s skulls.”⁶⁸ If Grant or Hall had read any of the reviews of *The Mind of Primitive Man* that were circulating, however, perhaps they have would re-assessed their position on Boas’s professional work.⁶⁹

Ultimately, the report was issued and printed in *The Journal of Heredity* in 1913 as the official position of the Committee on Immigration of the Eugenics Section of the American Breeders Association, the objections of Boas notwithstanding.⁷⁰ It appeared with a brief notice that “Professor Boas dissents from the conclusions and recommendations of the Committee.” The

⁶⁸ Hall to Boas, 28 August 1912, box 1, IRL papers; Hall to James A. Field, 28 August 1912, box 2, IRL papers. Hall, Grant, and most other core members of the IRL made similar comments about Jews, although they typically tried to mask their anti-Semitism. See, for instance, Prescott Hall to Maxwell Beals, 14 February 1910; Hall to Charles Fleischer, 23 February 1910, both box 2, IRL papers. This would be a persistent strategy of the IRL—agree with and support those that agreed with their agenda; condemn and sabotage those that did not agree with immigration restriction.

⁶⁹ See, for instance, Robert H. Lowie, review of *The Mind of Primitive Man*, by Franz Boas, *The American Journal of Sociology*, v. 17, n. 6 (May 1912), 829-35; Albert Keller, review of *The Mind of Primitive Man*, by Franz Boas, *The American Historical Review* v. 17, n. 3 (April 1912), 587-89; “The Scientists Speaks to Us” *The New York Times Book Review*, 11 February 1912, BR72.

⁷⁰ “First Report of the Committee on Immigration of the Eugenics Section” *American Breeders Association Magazine* v. 3, n. 4 (Fourth Quarter, 1912), 249-55; quote from 255.

recommendations themselves were in favor in increased proscriptions against admitting “unfit” immigrants, increased inspection facilities and more immigrants inspectors, a restriction on the use of discretionary admissions by the Secretary of Commerce and Labor, extensions of the period of deportation in the cases of insanity and public charges and greater fines on steamship companies that transported knowingly inadmissible immigrants.⁷¹

The report reflected another instance of tension between theoretical and applied science, in this case between the axiomatic principles of classic racialism against the anthropological and biological results gained from experimentalism. As Boas applied theory in his anthropological researches, he occasionally found theory to be inadequate to explain the observed phenomena. For a professional researcher, this meant that the theory was either wrong, or needed to be modified to incorporate the observed and experimental anomalies. In certain fields, like mathematics or theoretical physics, deduction and inference was a necessary and essential way to develop theories. But in matters of biology and the life sciences, where inductive research could be conducted in laboratories, it was possible to achieve confidence in the answers that nature provided. To non-scientists like Hall, however, the theory was the theory, and it could therefore be used in all cases, all the time. And when such a theory could be used to advocate a certain policy goal, it would be.

⁷¹ *Ibid.*, 250, 255. The Second Report noted that Boas had left the committee, and been replaced with Irving Fisher. “Second Report of the Committee on Immigration of the Eugenics Section of the American Genetic Association” *Journal of Heredity* v. 5, n. 7 (July 1914), 297-300. The *American Breeders Magazine* changed its name to the *Journal of Heredity* in the first number of 1914; the American Breeders Association had also changed its name to the American Genetic Association at the end of 1913.

Hall witnessed the success of manipulating theories to fit goals by isolating Boas and his dissenting opinion, and was therefore able to present a unified, unanimous report of the Subcommittee on Immigration in the ABA. Advocates of restriction like Hall would use these lessons to influence politicians to support the League's policy goals, as they did with the Dillingham Commission and during the Congressional debates on immigration restriction in the 1920s. To members of the ABA, perhaps this would imply that there was in fact scientific unity on the eugenic aspects of immigration. Certainly, non-academic readers of the *American Breeders' Magazine* or its later guise, the *Journal of Heredity* would be inclined to believe it; their subscription demonstrated their support of hereditary and breeding studies. But the scientific unanimity they presumed was wrong and opposed by actual scientists who worked in the field. Hall and the other members ignored the dissent of professionals whose research and cautious conclusions were often a function of their professional training they utilized sophisticated approaches to accumulating small and modest solutions to biological and hereditary problems. Many, like Jennings and Boas, were cosmopolitan in that they perceived a complex interplay of factors combining to direct the development of organisms like the stick traveling downstream. They understood that many elements impacted and shaped the journey toward definitive biological answers, and used caution in declaring scientific truths because of the complexity of hereditary and environmental influences on development.

Chapter 3: Science, Prejudice and Politics: The United States Immigration Commission

In 1907 the United States Congress passed a new immigration bill. In the debate over the bill, Republican Charles Henry Grosvenor, a House member from Ohio, had the clause featuring a literacy test deleted, and replaced it with a clause establishing an investigative body to examine all aspects of immigration to the United States. Funded by revenues from the head tax imposed on steamship companies and Congressional appropriations, the United States Immigration Commission played a central role in the legislative history of restriction. A recent history of the commission places it firmly within the Progressive tradition of having experts examine social and policy issues, and the broad statistical and empirical base, and its endorsement of a literacy test as a practical policy of limiting immigration, provided key support for the American nativist community. The final report, the historian Robert Zeidel notes, was crucial in rejuvenating the “stagnant” restrictionist movement.¹

But the Commission’s findings also played an important role in enabling the members of the Immigration Restriction League to justify their policy goals against the dark-skinned, broad-headed immigrants from southern and eastern Europe that were arriving in larger and larger numbers on American shores. The final report of the Congressional body enabled them to talk more about the qualities the immigrants lacked, along with their perceived racial differences. The

¹ Robert Zeidel, *Immigrants, Progressives, and Exclusion Politics: The Dillingham Commission, 1900-1927* (DeKalb, IL: Northern Illinois University Press, 2004), 115; also E. P. Hutchinson, *Legislative History of American Immigration Policy, 1798-1965* (Philadelphia: University of Pennsylvania Press, 1981), 141-43 for the 1907 Immigration Act.

Commission's work, in essence, proved that the attitudes of the League's leaders were not those of a marginal or fringe group, but attitudes that had broad support throughout the country.

Leaders of the Immigration Restriction League understood the importance of perceptions. One of the reasons they repeatedly used Francis Walker's replacement thesis to warn about the perils of swarthy and non-Anglo-Saxon immigrants was because it was a statistical or a demographic argument and not overtly an argument about "race." The IRL sent out a brief questionnaire to its supporters in early 1905, asking their opinion on certain restrictionist stances in an attempt to increase their mailing list and solicit, if possible, donations from supportive respondents. When replies from upper-class elites arrived that suggested confusion or ambivalence to the restrictionist position, Joseph Lee took up the task of replying to clarify the reasons why the League advocated the dramatic tightening of the immigrant flow. Walker's thesis was one of his favorite weapons. One respondent was puzzled why the IRL's animosity seemed to be directed in particular at Jews and Italians, who seemed to the author to be good types of people. Lee replied that he agreed that both groups were "excellent people," but that the essential question was "are they better than the native American? The statistics show that we shall not have both, or at least, that in proportion as we have Jews and Italians we shall not have the native."² In another letter, Lee invoked Walker explicitly. The respondent contended that immigrants should be excluded only if there was no likelihood that they could assimilate to

² Joseph Lee to Mr. Barrows, 13 December 1905, box 1, Lee papers.

the native standards and values. To Lee, thinking along these lines was completely wrong. “General Walker,” he explained, using the M.I.T. President’s military rank from the Civil War, “showed that the bringing in of immigration means the sterilization of the old stock[;] at least there has been a great fall in the native birth rate with each great wave of immigration...”³ The advantage “General Walker’s” argument provided was that it did not blame the immigrants for social problems like crime, poverty, or labor unrest, which as Colajanni was trying to point out, could be attributed to a variety of factors. It simply used statistics to demonstrate the impact of immigration on native-born Americans, although the causal connection was typically assumed rather than proven.

But what happened when members of the old stock, who Walker said were metaphorically sterilized by immigrants, opposed the League’s goals? This happened in 1906, when the President of Harvard University, Charles Eliot, sent a letter to League member Richards Bradley, criticizing the group’s activities.⁴ Eliot was an important advocate for the reform of higher education in the United States, and was president of the University when many of the leaders of the League were undergraduates in Cambridge.⁵ The tone of Eliot’s reply must have been

³ Lee to Mr. McMahon, 14 December 1905, box 1, Lee papers.

⁴ Bradley would later become an influential and important member of the IRL in the 1920s, after the death of Prescott Hall in 1921. He was born in Vermont in 1861, graduated from Harvard in 1882 and then spent two years studying law at Harvard. He formed a law firm in 1897, and a real estate company in 1900, around the same time that he joined the League. By 1913 he was the League’s treasurer. “Bradley, Richards Merry” *The National Cyclopaedia of American Biography* (New York: James T. White and Co., 1962), v. 45. He was listed as Treasurer of the League after the reorganization in 1913. See the draft of letterheads in Box 1, “Immig. Rest. League – List records” folder, IRL papers.

⁵ Philo A. Hutchinson, “Eliot, Charles William” *American National Biography*; see also Kim Townsend, *Manhood at Harvard: William James and Others* (New York: W. W. Norton, 1996);

discouraging for the members of the League, considering how important Harvard University was to them. Eliot had received a copy of the League's 1905 survey on immigration matters in December, and his February reply to Bradley was inflexible in its opposition to further restriction.⁶

Certain legislative aspects of the IRL's agenda did meet with the Harvard president's approval, such as the closer medical inspection of arriving immigrants to prevent the arrival of criminals, paupers, and diseased immigrants. Eliot also favored further regulations on steamship companies to improve conditions in steerage, where most immigrants traveled. The other methods of restriction the League favored, such as an increased head tax, literacy tests, property qualifications, "and all exclusion on the ground of race" Eliot rejected. His opposition to restriction rested upon fundamental philosophical differences with the leadership of the Boston-based group. Describing the attitude of the League as "vicious, economically, politically, and sentimentally," Eliot stated that the previous 300 years of immigration to the United States by foreigners had been essential to the improvement and development of the country. He noted that these continuous infusions of immigrants had, "on the whole," allowed the country to be "safely and well developed, and never faster or better than in the last fifty years." The correct attitude toward immigration, Eliot counseled Bradley, was to continue the "hospitable, generous, freedom-trusting attitude" that had guided immigration policy during those 300 years. Eliot was not criticizing the League

Louis Menand, *The Metaphysical Club: A Story of Ideas in America* (New York: Farrar, Straus and Giroux, 2001). Eliot was president between 1869-1909.

⁶ Charles W. Eliot to Richards Bradley, 7 February 1906, box 1, Lee papers.

members personally. “I regret to differ in this subject from you and Messrs. Moors and Lee. I should feel safer if I agreed with you and them, but it seems to be a real case of different faiths and expectations.”⁷ As Eliot’s letter shows, some old stock elites did have different attitudes toward arriving immigrants. Even Lee, whose social conscience manifested itself in additional matters besides immigration restriction, believed, in part, that the proper environmental and cultural influences could improve *all* human stock.⁸

Eliot’s opposition to the League’s policy goals presented a significant threat to the success of restriction. On one hand, it clearly demonstrated the importance of not using intemperate or discriminatory language in their efforts to gain new supporters, and League members took this lesson to heart. In 1910, for instance, Lee was referring to the group not as a “Restriction” league, but as an organization that favored the “selection” of immigrants.⁹ Lee wrote to Gustave

⁷ *Ibid.* Moors was another of the founders of the League. See Solomon, *Ancestors and Immigrants*.

⁸ Historian Kim Townsend’s study of Harvard University suggests that Harvard graduates in the post-Civil War years were “exceptional in that they thought of manhood not merely as an advantage in the struggle for survival but rather as an opportunity and a responsibility to foster and spread what they thought of as civilized life.” Townsend, *Manhood at Harvard*, 11. In a short letter to a friend, Lee once remarked that Havelock Ellis’s *The Dance of Life* was “pretty nearly my own doctrine.” Lee to Ellery, 24 March 1928, Box 2, Lee papers. Ellis’s *The Dance of Life* (Boston: Houghton Mifflin Company, 1923) is a collection of essays that describe ways to enjoy the full meaning of life, including dancing, thinking, writing and religion as forms of art that enrich one’s experience. It contrasts remarkably with his bleak writings in *Essays in War-Time: Further Studies in the Task of Social Hygiene* (Boston: Houghton Mifflin, 1917) and *The Philosophy of Conflict and Other Essays in War-Time* (Freeport, NY: Books for Libraries Press, 1970 [1919]).

⁹ One of the methods they used to garner support was a type of discursive or rhetorical manipulation. Describing the reading test as a “literacy” test might not convey the same *inferior* connotations as an “illiteracy” test would. League members alternated between the two descriptions of it, though privately they used the “illiteracy” test far more frequently. Clearly, with the latter terminology, the prospective immigrant *lacked* some quality, and was inadequate or inferior in some way; “illiterate” was a more powerful adjective. Yet, in other situations where greater tact was required, the League described it as a “literacy” test.

Hitzel, a German immigrant in Buffalo, to express Lee's support for methods of promoting German immigration. Lee introduced himself in the letter by explaining that he had been given Hitzel's name, and explained that the IRL, despite its official title, "is really an immigration selection league." The goal of his group, Lee suggested, was to encourage the government select the types of immigrants that were admitted to the country, rather than admitting any and all aliens with no evaluation of quality. The goal was to change the source of immigration back to the original northern and western regions of Europe, since these immigrants were more easily assimilated, and of kindred racial stock. The favored method at the moment was the "illiteracy" test, but something else, Lee said, would have to be tried since this would not exclude a sufficient number of undesirable immigrants. The Walker thesis applied not only to old-stock natives who would not reproduce if forced to compete with "inferior" immigrants, but in Lee's mind, applied to other immigrants as well. In the IRL pamphlets that he was enclosing, Lee assured Hitzel that the New Yorker would notice "an extraordinary coincidence between the increase in immigration from southern and eastern Europe and the falling off of immigration from Germany."¹⁰ German immigrants were desirable because Germans and Anglo-Saxons shared similar racial origins, and Lee believed that two thousand years of history had proven that southern and eastern Europeans lacked a capacity for self-government and building effective and stable political institutions, while the recent history of Germany and German-Americans had shown these attributes to be "eminently present" among the

¹⁰ Lee to Gustave A. Hitzel, 16 February 1910, box 1, Lee papers, emphasis in original.

Teutonic and Aryan races. In closing, Lee hoped Hitzel's German-American Association would make an effort to study the characteristics of contemporary German immigration and strategies for increasing it.¹¹ The League was not opposed to *all* immigration, just certain *kinds* of it.

On the other hand, Eliot's opposition to the League's agenda was dangerous because it showed how difficult it was to make compelling arguments in favor of restriction. Using abstract statistical data lacked any real punch. Eliot could object on philosophical grounds, or on political, economic or sentimental grounds. Describing the League's goals as favoring "restriction" or "selection" may help it gain supporters and economic contributors, but it was unlikely to generate fear or distrust of the new type of immigrants that were arriving in the United States. It was fear that would rouse the conscience of political and social leaders to move decisively to exclude certain types of immigration. And stoking fear of racial impurity, and racial contamination or degradation of biologically different and inferior races, as they knew from Eliot's reaction, had to be achieved carefully. It had to be presented as unimpeachably accurate scientific knowledge.

In an undated typescript in Lee's personal papers, a short essay called "The Ethics of Immigration Restriction" drew these ideas together, and placed them within a classic racist frame. The science of genetics and breeding, Lee suggested, gave man a high degree of control over nature. Darwin's theory of natural selection allied with this theory to show how certain species succeeded,

¹¹ *Ibid.*

while others died out. In its application to the human races, however, Lee's essay argued that it was not necessarily the best individuals that survived, but merely those "better fitted to perpetuate themselves." The science of eugenics as advanced by "Messrs. Pearson and Galpon [sic]" provided a method for improving racial stocks and maintaining their purity, although Lee conceded that "probably the world is not yet ready" to put these ideas into practice. But as he discussed the hierarchy of races, Lee argued precisely for such a eugenic strategy.¹² The Teutonic and Anglo-Saxon races must be encouraged to breed prolifically (and maintain the purity of their stock), and the "Iberic" race, whose days of glory had long passed, must be limited.

"Just at this point," Lee wrote, "I seem to hear something said of the colossal Teutonic conceit which thinks its race better than others. I frankly accept the challenge." The history of recent centuries had shown the Teutonic stock to be "the finest in the world." When compared to the Spanish American republics – peopled by the "Iberic" racial stock – England, Germany, Scandinavia and the United States all presented superior examples of development. Lee ended his draft by posing a rhetorical question: "If our country had been settled by the Galicians, Croatians, Sicilians or Greeks, can anyone suppose that our institutions and achievements would have been what they have, or that the movement toward political and religious liberty throughout all the world would have been the

¹² Joseph Lee, "The Ethics of Immigration Restriction" Typescript, n.d. [ca. 1907], box 3, folder: J. Lee—Immigration restriction (writings)-additions, p. 1.

same[?]"¹³ Despite the intense contradictions in his concluding statement – American anti-Catholicism and international isolationism call into serious question a genuine movement to spread political and religious liberty – Lee took the superiority of the Teutonic race to be a certified truth. These contradictions would not prevent the “Teutonic conceit” from being disseminated on a wide scale.

The members of the League obviously did not agree with Eliot’s assessment of their “viciousness.” For members, theirs was a patriotic duty to protect the American experiment in democracy. In their eyes, the “new” immigrants lacked any historical or cultural experience with republican or democratic ideals. Their undesirability was, in part, due to their perceived racial difference, but in the letters of League members to supporters and potential supporters, a related argument was the new immigrants’ inability to appreciate and perpetuate democratic institutions. Old-stock Americans found the statistical and demographic implications of unrestricted immigration powerfully persuasive as they attempted to navigate the dramatic economic and cultural changes of the Gilded Age and Progressive Era. Describing qualities that immigrants *lacked*, such as the ability to read or a history of participating in democratic government was a compelling, non-offensive or non-discriminatory rationale for restriction. It provided a safe rhetorical tool for the League to get support for restriction without seeming to play on racial antipathies.

¹³ *Ibid*, p. 5.

For the core leadership of the League, the prospect of Mediterranean and Alpine races, swarthy, short, broad-headed immigrants, “beaten men from beaten races,” in Walker’s phrase, supplanting the Teutonic and Anglo-Saxon core of United States was terrifying. Their strategies were driven by discretion and tact; their motivations were driven by racism. In this context, classic racialism, because it was based on scientific principles and a language of biology and heredity, was able to unify both strategy and motivation. With the establishment of the United States Immigration Commission in 1907, the League made a concerted effort to use the classic racialism to influence the immigration policy of the national government.¹⁴

The Commission, which was charged with conducting a “full inquiry, examination, and investigation...into the subject of immigration” was made up on nine members: three Senators, three Representatives, and three Presidential appointees.¹⁵ In addition to Vermont Senator William Dillingham, who chaired the

¹⁴ In one of their first circular letters, Hall, acting as the League’s secretary, compiled a list of twenty reasons why immigration to the United States had to be further restricted. The “quantity of our immigration has been increasing and its quality decreasing,” and American character, as a result, was being threatened. Immigrants because of “their foreign language, habits, customs and political beliefs” were undermining American institutions, morals, and standards of living. This was the tactful approach. The threatening approach can be seen in a survey of the eleven members of the League’s executive committee, conducted by Lee around 1906, which showed the tendencies of the League to carry “exclusion [of certain immigrants] considerably further.” Seven of the members, Lee’s survey found, “believe that there are races which, whether or not inferior to the preset American race, are not adapted by their ideals and traditions to combine with [the American] race in building up a better national ideal and national life, and that such races should accordingly be excluded.” “Publications of the Immigration Restriction League, no. 4” n.d. [ca. 1894], Lee papers, box 3; “Report of the Committee on Immigration” n.d. [ca. 1906], Lee Papers, box 1.

¹⁵ The Commission, which was referred to as the “Dillingham Commission” after its chairman, Senator William Dillingham, was created by the Immigration Act of 20 February 1907 (34 Statutes-at-Large 898). Quote from United States Immigration Commission, *Reports of the Immigration Commission* v. 1 “Abstracts of Reports” (Washington, D.C.: Government Printing Office, 1911), Senate Document 747/1, 61st Congress, 3rd Session, ii.

commission, Massachusetts Senator Henry Cabot Lodge and Democratic Senator Le Roy Percy from Mississippi served with Representatives Benjamin Howell, William Bennet, John Burnett and the three Presidential appointees, Charles Neill, Jeremiah Jenks and William Wheeler. Zeidel describes how opponents of immigration realized they “would need to use more progressive approaches, not just emphasize immigrants’ alleged differences from natives” to justify their exclusion. “Even passionate xenophobes,” he points out, “were coming to realize that they needed more convincing ways to make their arguments.”¹⁶ The Immigration Restriction League’s interest in the Immigration Commission’s work was not limited its “full inquiry” into immigration. They used political connections and influence to help direct the Commission to a recommendation favorable to restriction, and worked hard for an explicit endorsement of the literacy test. The selection of their senator from Massachusetts to serve on the Commission – Joseph Lee’s cousin Henry Cabot Lodge – gave them a powerful reach into the heart of the Commission’s investigations.

The League used a variety of strategies to gather support for restriction, and one of their most successful was the exploitation of their political ties. The IRL had forged close relationships with several other members of the United States Congress in addition to Lodge. Dillingham was already meeting with the League’s paid lobbyist James H. Patten in 1906. (Hall in fact tried to get Patten

¹⁶ Robert F. Zeidel, *Immigrants, Progressives, and Exclusion Politics*, 26; 20.

appointed to the Commission.¹⁷) New York Representative William S. Bennet, who opposed restriction and authored the Commission's dissenting minority report, took note of the IRL's influence in his dissent. He pointed out that all testimony from professionals involved in immigration matters before the House Committee in 1905 was opposed to the literacy test. But one group outside Congress was particularly active in overcoming opposition to the reading test, an organization represented by "Mr. Patten, who is the secretary [sic] of the Immigration Restriction League of Boston."¹⁸ Bennet correctly noted the political influence the League exercised, and he felt that that influence jeopardized the objective investigation the commissioners tried to conduct. The League was active in trying to influence the recommendations of the commissioners because they knew that its support for a literacy test might help encourage Congress to pass another bill containing a literacy requirement. So they worried about its final recommendations.

For Lee, restriction was justified partly on the grounds that it was necessary to preserve American democratic institutions. Although statistics had become highly important in the movement, providing an empirical demonstration of the threat immigrants posed to Anglo-Saxon American civilization, Lee argued

¹⁷ Hall informed Patten that he had written to Lodge and Representative Gardner to "use their influence with the President to have you appointed.... I can think of no more valuable work you can do for the U.S.A....Most of such bodies bring in fool reports, and you will have a chance to see a lot on our side & get it in." Hall to Patten, 4 February 1907, Lee papers, box 1; William Dillingham to James H. Patten, 5 January 1906, box 1, IRL papers.

¹⁸ "Immigration of Aliens into the United States" House Report n. 3021, part 2, "Views of the Minority" 59th Congress, 1st Session, 27 April 1906, p. 3. Hall was the League's secretary; Patten was its congressional lobbyist.

that statistics omitted the political capacity of immigrants, which could not be reduced to mere figures. In this situation, literacy became increasingly important, as it indicated their unsuitability to perform their duties as American citizens. Illiterate immigrants were dangerous to admit because they were intellectually unprepared for – if not incapable of – the political responsibilities of American citizenship. As Lee had explained to Jeremiah Jenks, one of President Roosevelt's appointees to the Immigration Commission, "The facts most relevant to the question [of immigration] seem to me to be those indicating the capacity of these races for self-government in those countries where these races are predominant." He suggested that political self-government was a racial attribute, and monarchical or autocratic governments were best suited for less civilized peoples.¹⁹

Lee felt that the question of immigrants' suitability for political freedom must be carefully examined. Once again relying on Francis Walker's replacement thesis, the Boston philanthropist explained that "If upon the whole such a study [as the Commission's] shows that the Italian and Slavic races are superior to the Anglo-Saxon in carrying on a democratic government, then the present immigration must be considered a great benefit to mankind. If, however, these races are less successful than ours, then such immigration must be considered the greatest misfortune the world has ever suffered."²⁰ This alarmism became a staple in the League's campaign for restriction, with the statistical proofs supplied by

¹⁹ Lee to Jeremiah Jenks, August 9, 1906, Box 1, Lee papers.

²⁰ Ibid.

General Walker, the Census and federal statistics on immigration. But since they began with the conviction that the Italian and Slavic races were not superior to the Anglo-Saxon in carrying on a democratic government, the League's leaders worked zealously to secure a final report from the Dillingham Commission that agreed with them.

That Lee singled out the political behavior of Southern Italians demonstrates the impact that classic racialism had on North American restrictionists. The biological and racial differences between southern and northern Italian were also visible in their cultural characteristics, a theme Lombroso, Sergi, and Alfredo Niceforo had been making for decades. Lee explained in a letter that his support for restriction originated in a fear that if the influx of the new immigrants continued, "together with the decreasing birth rate of the old American stock," in a few years the effect would be to "make our country predominantly of the Alpine (roughly of the Slavic) and the Mediterranean—chiefly South Italian—races." This substitution of racial stocks would be ruinous to American democracy. Urban political corruption – a special target of progressive reform – was, as Lee saw it, a function of the corruption of the voters, not the political boss. "If a hundred Italian votes, for instance, are bought by an American boss, the evil is sometimes attributed to the American boss rather than to the one hundred Italians. But where the [United States] is predominantly South Italian, as is true of South Italy and Sicily," he explained, "there the conditions of politics must be attributed chiefly to the kind of people."²¹

²¹ *Ibid.*

Lee showed a solid grasp of the ideas of the Italian racialists, while showing himself completely ignorant of the complex history and scholarship of Italian scientists who disputed the determinist arguments of Lombroso and Sergi. It would not be the first time they would selectively choose scientific studies to buttress their arguments.

While the Dillingham Commission was studying immigration, League members reached out nationally to try to influence the Commission. Correspondence between Hall and Alexander Graham Bell linked the Immigration Restriction League with the emerging field of eugenics. William Ripley, whose book *The Races of Europe* was profoundly influential for Anglo-Saxonists and the members of the Dillingham Commission, replied to a letter from Hall with the suggestion that the League use an economic argument for restriction, not a racial one, as economic security was much more tangible to the average American. Ripley also cautioned Hall about utilizing “scientific” works whose quality was questionable. Hall had asked Ripley’s opinion on Alfred Schultz’s book *Race or Mongrel* (1908), and Ripley explained that though he had not seen the volume in question, he was confident that it was unreliable. A member of the Florida Supreme Court and a judge from the Supreme Court of Idaho wrote the secretary of the League supporting efforts to tighten immigration regulations. David Starr Jordan, President of Stanford University, conveyed his support for restriction. Dr. John D. Adams, a Boston charity worker, advocated restriction, which must have been particularly gratifying for the League since they

typically had a difficult time attracting the support of social and settlement house workers.²²

The League's attitude toward using the Commission to support their own attitudes and policies began with the composition of the Commission itself. With the study underway in 1908, Hall wrote a letter to President Roosevelt implicitly questioning the qualifications of some of the Commission's appointed members.²³ Hall complained that New York Congressman William Bennet "represents a foreign-born district and is the spokesman of the National Liberal Immigration League, a society started by certain Jews in New York to oppose restrictive laws."²⁴ How, Hall wondered, could such a man serve on such an important governmental body? League activists found much more favorable the appointment of Alabama representative John Burnett, who was outspoken in his restrictionist views. Burnett made statement before the House Committee on Immigration and Naturalization in 1908 that reflected the American incorporation of Italian arguments of two racially distinct Italies. There was one cause that best

²² Prescott Hall to Alexander G. Bell, 31 March 1908; Bell to Hall, 28 May 1908; Hall to Bell, 1 June 1908, all box 1, Immigration Restriction League papers; William Ripley to Prescott Hall, 16 February 1909, box 3; R. F. Taylor to Hall, 24 March 1910, box 3; James F. Ailshie to Hall, 8 April 1910, box 1; David S. Jordan to Hall, 28 February 1910, box 2, all in IRL papers. In 1915, during more Congressional hearings on restriction, Hall wrote to Louis Brandeis complaining of his testimony, which opposed restriction. "It is very unfortunate the prominent Jews like yourself should take this attitude, as it is the Jews of all people, it seems to me, who should be on the side of progress and eugenic selection of immigrants." Hall to Louis Brandeis, 7 April 1915; Brandeis to Hall, 8 April 1915, both box 1, IRL papers.

²³ Prescott Hall to Theodore Roosevelt, 27 June 1908, box 3, IRL papers. Roosevelt frequently talked about the perils of "race suicide." A particularly thoughtful treatment of Roosevelt is Gail Bederman, *Manliness and Civilization: A Cultural History of Gender and Race in the United States, 1880-1917* (Chicago: University of Chicago Press, 1998), chapter 5.

²⁴ Hall to Roosevelt, 27 June 1908, IRL papers.

explained the Italian government's difficulties trying to improve the conditions in southern Italy, Burnett announced at a House Committee hearing. "They have the same King, the same Parliament, the same laws," he explained, "and yet the man north of the mountains is of the Caucasian, the white race, and the other is of the mixed race." His authority for such a declaration was a conversation he had with Egisto Rossi, the Italian Commissioner of Emigration. Burnett told the committee that the literacy test under debate would have the effect of reducing by more than 50% the number of Southern Italians eligible to land in the United States, but would only impact 10% of North Italians.²⁵ For a society acutely aware of racial mixing, to allow the entrance into the United States of a mixed race of people – who had no inherent claim to admission anyway – was unthinkable.²⁶

Fear that the Anglo-Saxon stock of the American population would subsumed by a mass of racially inferior immigration was the essence of the League's motivations, and reflects the way the language and epistemology of classic racialism had significant purchase. Immigration was a biological threat. Historian Matthew Jacobsen describes this general process of "racial refinement from 'white' to 'Anglo-Saxon'" that created a hierarchy of distinct white races.²⁷ When coupled with statistical evidence of the decline of the Anglo-Saxon

²⁵ "Hearings before the Committee on Immigration and Naturalization, House of Representatives" 60th Congress, 1st Session, Hearings on HR 11326, 18 February 1908, p. 7. Burnett never clarified what "mountains" drew the dividing line between north and south Italy. The Alps mountain range would limit the number of "north Italians" to a number so relatively small as to be almost insignificant; the path of the Apennines down the peninsula would have a similar effect. Effects of literacy test from p. 8.

²⁶ Useful here are Guglielmo, *White on Arrival*; Jacobsen, *Whiteness of a Different Color*.

²⁷ Jacobsen, *Whiteness of a Different Color*, 41-3.

birthrate, the mental inferiority of the new immigrant races that high rates of illiteracy seemed to demonstrate, and the persuasive biological determinism of classic racialism, the dangers of unrestricted immigration seemed clear to members of the League. Convincing the Congressional investigators and securing a favorable report of the Commission became imperative.

As the Commission began writing its report in 1910, the Immigration Restriction League's leadership concentrated their activities to secure a report that would recommend the literacy test to reduce southern and eastern European immigration. What is particularly striking about this period is how the League behaved, leaving no stone unturned. League members requested friends write to Commission members to pressure them to favor the "illiteracy test." The IRL's leadership shifted political alliances in an effort to get what they wanted out of the report. Restrictionists like these were not simply trying to ensure that immigrants to the United States would be able to get jobs and assimilate; they were crusading to preserve what they perceived as the Anglo-Saxon character of the country.

Writing a letter to new Harvard President A. Lawrence Lowell in 1910, Joseph Lee requested that he write to Senator Lodge, urging him to make sure the Commission favored the literacy test. Lee argued that, "If their report is negative, it will hurt us for many years whereas a positive report, favoring especially the illiteracy test, would be a considerable help. Lodge, though our professed champion, has shown a tendency to wobble...and thereby needs stiffening of the backbone." Lowell not only complied, but sent a draft of the letter for Lee's approval before he sent it off to the Senator. Three months later, Lee wrote to

Lowell again, addressing him informally as “Dear Lawrence,” asking him to now write to President Taft to impress upon the President the danger of immigration. Lee offered some “suggestions” on things the League would favor, and hoped that Lowell could work them into the letter and thereby encourage the President to support the policies regardless of the Commission’s findings. Lee couched his suggestions in careful terms (for example, that a democracy can only work in a largely homogeneous society), but made it clear that a literacy test was an essential element of immigration policy. Taft’s brief, handwritten reply explained that although he had initially favored a literacy test, “after hearing pro and con in the Congressional debates, I am not quite so clear in my mind now.”²⁸

The League threatened to oppose Lodge’s reelection if their senator did not deliver a report that was friendly to their goals. With Taft’s uncertainty, it became imperative for the League to get a report from the Immigration Commission in support of the literacy test. League leaders pressured Lodge to secure it. James Bronson Reynolds, Assistant District Attorney for New York County, wrote to Lee in November 1910 (shortly before Lodge was to face reelection in the Massachusetts legislature), stating, “I have just seen Patten who has told me that you and others connected with the Immigration Restriction League are considering your attitude towards the re-election of Senator Lodge.” Reynolds embarked on a 3-page critique of Lodge’s actions, and his unsuitability for reelection. “I regard him as a weak friend of our cause, and am unable to

²⁸ Joseph Lee to A. Lawrence Lowell, 4 August 1910; Lee to Lowell, 3 November 1910, both box 1, Lee papers. Lowell’s note to Taft and Taft’s reply were undated, and are placed with Lee’s November 3 letter to Lowell.

place my finger on a single strong measure which he has advocated. I therefore personally believe that his removal from the Senate would be a victory for good government, and a gain rather than a loss to restriction,” he closed. Lee was less aggressive in his attitude towards his cousin, replying that “we shall know better what his real usefulness is when the commission makes its decision, as he can pretty nearly swing it. Anyway his own vote will be seen.”²⁹ The League’s leaders regarded Lodge’s usefulness as reducible solely to his position on immigration, and his ability to advance their interests.

To the leadership of the IRL, political influence was a means to an end, just like scientific investigations were. They adopted a wait-and-see attitude towards Lodge’s reelection; if he supported the literacy test, they would support Lodge. If he did not, Lee wrote to Patten, the League would “soak him. Anyway, I think he realizes we have our eye on him, and I don’t know any way to make that more evident than it is.” When the Immigration Commission endorsed the principle of restriction, and suggested that the literacy test be enacted in the next immigration bill sent to the President (Bennet was the sole dissenter of this recommendation), the attitude of the League towards Senator Lodge changed dramatically. Patten wrote to Lee on December 11, “WE MUST do everything we can for Senator Lodge....It is simply up to every one of us to get into the game and see if we cannot show some political influence. The Senator did the Commission trick.” With this fine news, Lee wrote to “Cousin Lodge” expressing

²⁹ James Bronson Reynolds to Joseph Lee, 19 November 1910; Lee to Reynolds, 22 November 1910, both box 1, Lee papers. See also Zeidel, *Immigrants, Progressives and Exclusion Politics* for a solid treatment of the Commission’s work and the League’s involvement with Lodge.

his appreciation, and wishing him luck in the election.³⁰ Lodge had proven his usefulness.

Lodge not only prodded the Commission to unanimous endorsement of restriction – as Lee wrote to Lowell, he “made good from Goodville” – he couched restriction in non-racialized terms. That is, he guided the Commissioners to support the position that immigration restriction should be American policy, but not because of biological theories of inferiority or classic racist ideas. Instead, restriction was supported, as Lee explained, “on the *popular* basis of labor and industrial effects rather than on the *unpopular* one of race discrimination.”³¹ This suggests that the members were concerned about how the League was perceived, that they did listen to criticisms by non-members like Eliot, and they made efforts to temper their rhetoric and strategies at times, though their ultimate goal always remained unchanged.

The Dillingham Commission’s final report, issued in 1911, was a triumph for the IRL. The Commissioners endorsed the principle of restriction, advocating “the reading and writing test as the most feasible single method of restricting undesirable immigrants.” The report also suggested a method of excluding what they called “birds of passage,” and endorsed a long-term policy goal of limiting “the number of each race...to a certain percentage of the average of that race

³⁰ Lee to Patten, 28 November 1910; Patten to Lee, 11 December 1910; Lee to Lodge, 13 December 1910, all box 1, Lee papers. Emphasis in original.

³¹ Lee to Lowell, 13 December 1910, box 1, Lee papers. Emphasis added.

arriving during a given period of years.”³² Founded to “arouse public opinion to the necessity of a further exclusion of elements undesirable for citizenship injurious to our national character,” the League’s goal of immigration restriction was given federal support by the Dillingham Commission.³³ League leaders had had private access to members of the Commission, and were able to exercise influence on the final report, although they were not able to prevent the publication of a minority report that opposed the literacy test. Their views were printed, along with many other “patriotic” organizations, in the final volume of the reports, which suggests that their views were mainstream.³⁴

In Italy, the contents of the report were related to the *Bolletino*’s readers in a special volume published in 1911. The subjects that received special attention in the Italian journal of immigration involved the commissioners’ reports on immigrant banks and the history of steerage and transportation legislation, and the endorsement of the literacy test. The Italian Commissariato was well informed on Congressional debates leading up to the creation of the Dillingham Commission,

³² United States Immigration Commission, *Reports of the Immigration Commission* v. 1, “Abstracts of Reports” (Washington, D.C.: Government Printing Office, 1911), Senate document 747/1, 61st Congress, 3rd Session, pp. 48, 47. The sole dissent from the endorsement of the literacy test came from New York Congressman William Bennett, although he found “a slowing down of the present rate of the immigration of unskilled labor” to be “justified by the report.” *Ibid.*, 49.

³³ “Constitution of the Immigration Restriction League,” box 3, Joseph Lee Papers. See also “The educational test as a means of further restricting immigration, 1895” *Publications of the Immigration Restriction League*, n. 6, IRL papers; Higham, *Strangers in the Land*; Jacobsen, *Whiteness of a Different Color*.

³⁴ United States Immigration Commission, “Abstracts of Reports,” 45-48 for recommendations; 49 for opposition of Rep. Bennet. United States Immigration Commission, *Reports of the Immigration Commission* v. 41, “Statements and Recommendations Submitted by Societies and Organizations Interested in the Subject of Immigration,” Senate Doc. 764, 61st Congress, 3rd Session (Washington, D.C.: Government Printing Office, 1911). The League’s recommendations and the petitions of other groups in favor of restriction are discussed further below.

and the legislation that had been proposed in the years before the passage of the 1907 Immigration Act. But the *Bolletino* offered no real editorial comment, nor did it express any reaction to the Commission's report. The Commissariato's writers explained that Franz Boas's investigations had provoked great interest in scientific circles, both in America and abroad, although the Commission did not declare Boas's findings to be definitive. Bennet's dissent from the final recommendation of the literacy test was also noted.³⁵ But there was simply nothing for the Italian government to do. Immigration regulation was strictly a sovereign issue, and the Royal government made great efforts to abide by the letter of the American laws so that American legislators, policy makers, and foreign diplomats would have no complaints.

As the Immigration Commission's members compiled the data to fulfill their charge under the 1907 act, they and their staff relied heavily on statistics to depict the immigration situation in the United States. The majority of the 41 published volumes were heavily statistical; besides volume three ("Statistical Review of Immigration"), volumes six through twenty-five statistically depicted, in twenty-five parts, the role of immigrants in industries. Tables were broken down by nativity, emphasizing the tendency of certain immigrant groups to concentrate in specific kinds of production and in certain geographic areas.³⁶ Advocates of restriction, however, did not emphasize this statistical data to

³⁵ "Lavori della Commissione federale per l'immigrazione negli Stati Uniti" *Bolletino dell'Emigrazione* Doc. 527, n. 4 (1912). Boas's study described on 55-6.

³⁶ The series of "Immigrants in Industries" officially comprises one lengthy document: Senate Document 633, 61st Congress, 2nd Session.

support their policy goals. Restrictionists most effectively used three particular volumes of the *Reports* to portray the undesirability of the “new” immigration: “Emigration Conditions in Europe” (volume 4), the “Dictionary of Races or Peoples” (volume 5), and “Immigration and Crime” (volume 36). Collectively, these three volumes portrayed the new immigration as racially incapable of assimilating into the “Anglo-Saxon” population of the United States and partially responsible for increases in crime, poverty and social disorder. The unmistakable policy prescription of these volumes for the IRL was a policy of restriction.

In the summer of 1907, six of the nine members of the United States Immigration Commission spent the summer in Europe studying the natural and artificial causes of emigration, the class and character of emigrants, as well as the attitudes of, and impact on, the respective countries of emigration. Senators Latimer and Dillingham, Representatives Howell, Bennet and Burnett, and William Wheeler, one of the Presidential appointees, departed Boston for Naples on the 18th of May. Dillingham, Howell and Wheeler were dispatched to Rome to meet with representatives of the Royal government while the rest began collecting information about Italian immigration from the south of Italy. The six commissioners spent about two and a half weeks in Italy, and then dispersed to other parts of Europe.³⁷ Zeidel notes that the trip was undertaken to ensure that the investigations of the commission were comprehensive, to test allegations of abuse

³⁷ United States Immigration Commission, *Reports of the United States Immigration Commission*, vol. 4, “Emigration Conditions in Europe” (Washington, D.C.: Government Printing Office, 1911), Sen. Doc. 748, 61st Congress, 3rd Session, 3-4; Zeidel, *Immigrants, Progressives, and Exclusion Politics*, ch. 3.

of steamship and local officials, and to confront prejudices at the source. But despite the time the members spent in Italy, he notes, they could find no empirical support of Italian criminality, and that several of the commission's members saw that southern Italians were industrious, but just unfortunate to live in an impoverished region.³⁸

The first part of volume 4 was a general survey of immigration drawn in part from statistics provided by these countries of emigration and which included a yearly breakdown of alien arrivals over the previous 90 years. These statistics demonstrated the shift in the origins of immigrants and drew an explicit difference between the "old and new European immigration." "In studying the emigration situation in Europe," the report noted, "the Commission was not unmindful of the fact that the widespread apprehension in the United States relative to immigration is chiefly due to this change in the character of the movement of population from Europe in recent years."³⁹ The greatest difference Commission members found between the old and new immigrants was the degree of education. One table early in the volume demonstrated what the members of the Immigration Restriction League had been claiming for over a decade: a literacy test would exclude a sizable number of the "new" immigrants. Later in Part II of "Emigration Conditions," the report stated, "if illiterates, without exceptions, were denied admission Italian immigration undoubtedly would be reduced to about one-half its

³⁸ Zeidel, *Immigrants, Progressives, and Exclusion Politics*, 51-68 for the European investigations of the Commission.

³⁹ United States Immigration Commission, "Emigration Conditions," 12.

present volume.”⁴⁰ Achieving this reduction was what the League had long been advocating.

“Emigration Conditions in Europe” highlighted the four specific European countries that were furnishing the bulk of the immigrants to the United States: Italy, Russia, the Austro-Hungarian Empire, and Greece. The Commission’s report reinforced many of the perceptions of the undesirability of Italian immigration. In discussing the character of Italian immigration, Commission members followed what they described as the “practice of ethnologists” in examining the characteristics of south Italians. The fourth volume of the report explained, “It is generally accepted that the North Italians make the most desirable class of immigrants. They are more progressive, enlightened, and it is claimed are more easily assimilated than their southern countrymen, who, because of their ignorance, low standards of living, and the supposedly great criminal tendencies among them are regarded by many as racially undesirable.”⁴¹ Instead of examining, as Napoleone Colajanni and others did, the root causes of the allegedly high incidence of criminality in the *mezzogiorno*, the Commissioners attributed it to an inherent biological disposition. The report on European conditions was influenced strongly by classic racist ideas; its principal sources for the racial definition of “Southern immigrants” was the Italian determinist

⁴⁰ The table noted, for instance, that only 1.1% of the 302,657 English immigrants over the age of fourteen that had arrived in the United States between 1899 and 1909 were incapable of reading or writing. The comparable figure for German immigration at the same time was 5.1%; French 5.4%; Dutch and Flemish 4.7%; and Scotch .4%. The 1,517,768 arriving “South Italians” however, had an illiteracy rate of 54.2%. “Emigration Conditions in Europe,” *Ibid.*, Table 15, p. 30; quote on 192.

⁴¹ *Ibid.*, 177-78.

Giuseppe Sergi.⁴² The report explained that the “prevailing alarm” created by the high number of southern Italian immigrants was “not occasioned entirely by the fact that a good many actual criminals come to the United States from Italy, but also by the *not unfounded belief* that certain kinds of criminality are inherent in the Italian race.”⁴³ It was precisely this “not unfounded belief” that Colajanni had sought to combat in his essays. Furthermore, while Colajanni had criticized the Italian government’s inadequate attention to educational opportunities as the cause of Southern Italian criminality, the American report instead declared that “under the enlightened government” of the Italian state, conditions had sufficiently improved in the South, and “the people are said to be slowly losing the old characteristics of lawlessness which have made members of the race so conspicuous in the criminal element of the United States during recent years.”⁴⁴

The apparent contradiction in the two statements above – that criminality was inherent in Southern Italians, but they were slowly losing their natural proclivity to crime – reflects one of the fundamental tensions in scientific arguments favoring immigration restriction. Certain qualities like skin, eye, or

⁴² The section detailing the “Character of Italian immigration” (177ff) referred readers to the “Dictionary of Races or Peoples,” which as discussed below, drew heavily on the work of Giuseppe Sergi. It explained: “It is said that the people of Piedmont have the largest admixture of Celtic blood, and that they resemble the French in many respects. The Lombards have both *Teutonic and Celtic* strains and consequently the men are generally tall and of a powerful build as compared with most other Italians, while fair hair and blue eyes frequently occur. A *different type* occurs in the Sicilians, who are said to be largely a mixture of Italian, Greek, Spanish, and Arab with some infusion of Teutonic blood. Sardinians have a considerable infusion of Spanish blood, while the Neopolitans [sic] are said to incline slightly toward the African or negro type.” United States Immigration Commission, “Emigration Conditions,” 177. Emphasis added.

⁴³ *Ibid.*, 209. Emphasis added.

⁴⁴ *Ibid.*, 209.

hair color were easily visible and seemingly hereditary. But it was harder to assess the inheritability of other qualities like intelligence and predisposition to crime or delinquency, and indeed, to even measure them accurately. And these aspects were more socially significant than physical characteristics. It was difficult to determine where “nature” ended and “nurture” began; the complexity of the hereditary patterns made it problematic to implement policy, and often led, as will be shown with mental testing, to exaggerated claims of scientific knowledge. On balance, the reports of the Dillingham Commission tended toward accepting a major role for hereditary influence, and a diminished role for socialization and environmental influence (though Boas’s report on descendants of immigrants was a notable and important exception).

Ultimately, it was the thirty-sixth volume of the reports, “Immigration and Crime” that most explicitly argued that the southern Italian immigrant was an undesirable addition to the country’s population. The report did note that immigrants were statistically less prone to commit crime than native-born Americans. But using court reports, records of penal institutions and police records, the Commission found that immigrants committed crimes against persons in greater proportion than native-born persons. Italian immigrants in particular, it reported, deserved special attention in that category. Immigrant children however, were more criminally inclined than the children of native-born Americans – the Commission’s report suggested that this could be attributed to the disproportionate concentration of immigrants in the cities of the North Atlantic states, so “the criminality of the children of immigrants is largely a product of the

city.”⁴⁵ Again, the confusing roles of nature and nurture complicated a solid understanding of human development.

The Commission decided to single out Italian immigrants because “of the popular opinion, voiced in the press, that large numbers of Italians having criminal records in Italy come to the United States, and that Italian crimes of violence in this country are in large measure due to them...”⁴⁶ Here the Commissioners themselves fell prey to popular prejudice and the association in the public’s mind of Italians with organized crime. Spectacular events like the lynching of eleven Italians in New Orleans in 1891, the murder of New York Police Lieutenant Joseph Petrosino in Sicily in 1909, and the assassination of the Italian King Umberto I in 1901 had created a perception in the United States of southern Italian immigrants as wildly violent. The thrust of the investigation of crime and emigration in Italy, however, was limited to uncovering what role, if any, the Royal Government played in allowing criminals to emigrate. It found that the Italian government largely abided by American immigration law. As “Emigration Conditions in Europe” had noted, the Italian government had enacted specific regulations to prevent the emigration of convicted criminals – intending emigrants were required to obtain a *certificato penale* declaring that they had

⁴⁵ United States Immigration Commission, *Reports of the Immigration Commission*, vol. 36, “Immigration and Crime,” (Washington, D.C.: Government Printing Office, 1911), Senate Doc. 750, 61st Congress, 3rd Session, p. 1.

⁴⁶ *Ibid.*, 277.

committed no crimes to be able to obtain their passport to leave Italy.⁴⁷ These aspects of American immigration law were also discussed in the *Bolletino*.⁴⁸

Commissioners feared, though, that this did not restrain criminals from embarking from other ports within Europe. While American immigration law prohibited the entry of one of the classes of criminal Italians – those convicted in Italy who had served their sentence – it did not prohibit the entry of those convicted *in absentia*, those tried in Italy but not convicted, or those under surveillance in Italy. The Commission members struggled to suggest changes to the laws that would protect Americans and “the great majority of the Italians in this country [who] are law-abiding and industrious” from the “small proportion of [Italian] criminals.” While the Italian government was helping limit the emigration of criminals to the United States as best it could, the Commission concluded that these efforts were insufficient: “the Italian criminals are largest in numbers and create [the] most alarm by the violent character of their offenses in this country.”⁴⁹ Such a class of immigrants would certainly be an undesirable addition to the American population. The hand of the IRL in favor of restriction strengthened.

The League was even included in the Commission’s reports. The final volume, “Statements and Recommendations Submitted by Societies and

⁴⁷ “Emigration Conditions in Europe,” 67. A copy of a *Certificato Penale* is in Record Group 233, HR 66A – D 10, HR 12320, National Archives and Records Administration, Washington, D.C.

⁴⁸ “Lavori della Commissione federale per l’immigrazione negli Stati Uniti” *Bolletino dell’Emigrazione* Doc. 527, n. 4 (1912).

⁴⁹ “Immigration and Crime,” 286; 278.

Organizations Interested in the Subject of Immigration” featured a wide variety of opinions on immigration, including the League’s. Their statement to the Dillingham Commission included attitudes of their members who had responded to a survey the League had sent around in 1910. The League’s position was “that the present laws are inadequate and that further selection of immigration is necessary and desirable from (a) the social and moral standpoint, (b) the economic standpoint, (c) *the eugenic standpoint*.”⁵⁰ They endorsed passage of a literacy test, an increase in the head tax on immigrants to at least \$10, an unlimited period for deportation, and racial selection of immigrants so that kindred racial stocks from the north of Europe were admitted, and the races of the south of Europe excluded. They explained, “we should do as we do in breeding any other species than the human, viz., secure the best specimens to breed from.” Southern Italians and other “new” immigrants were not the best specimens.⁵¹ Other organizations advocated similar restrictive policies; the Junior Order of the United American Mechanics (who also favored a “money test” that required that immigrants must arrive with at least \$25) made the most explicitly classic racist argument, demanding that “Our grand Anglo-Saxon character must be preserved and the pure, unmixed blood flowing down from our Aryan progenitors must not be mingled with the Iberic race...”⁵² The American Jewish Committee, the Immigrants Protective League, the Young Men’s Christian Association and the

⁵⁰ United States Immigration Commission, *Reports of the Immigration Commission* v. 41, “Statements and Recommendations Submitted by Societies,” 103, emphasis added.

⁵¹ *Ibid.*, quote from 106; statement and recommendations 103-06.

⁵² *Ibid.*, quote from 24; statement and recommendations 16-26.

National Liberal Immigration League all presented statements that opposed restriction. But their opposition would be ignored.⁵³

The influence of classic racialism can be seen most clearly and explicitly in the Dillingham Commission's fifth volume, the "Dictionary of Races or Peoples." On the first page of the "Dictionary," the Commissioners declared that caution must be used in describing ethnological attributes of the newer immigrants, because "the true racial status of many of them was imperfectly understood even in communities where they were most numerous...."⁵⁴ The purpose of the Dictionary was to clarify the ethnological and racial differences between the population groups that the Bureau of Immigration had been using for record-keeping since 1899. To accomplish this, it relied chiefly on the works of William Ripley, Daniel Brinton, Joseph Deniker, and Giuseppe Sergi. For compiling statistics to empirically demonstrate the effects of immigrants and immigration on the U.S., its sources were Prescott Hall, Richmond Mayo-Smith and John Commons, odd choices considering that all three dealt more with the social, political and economic impacts of immigration than compiling statistics. Nearly every one of the works the Commission relied on for its explication of races and ethnicities began with an assumption of fixed racial identities, and the

⁵³ *Ibid.* See 15-26 for the JOUAM, 55 for the position of the IPL, 83 for the YMCA, 103-07 for the statements and recommendations of the IRL, 262 for the AJC.

⁵⁴ United States Immigration Commission, *Reports of the Immigration Commission*, vol. 5, "Dictionary of Races or Peoples," (Washington, D.C.: Government Printing Office, 1911), Senate Document no. 662, 61st Congress, 3rd Session, p. 1

sources of their statistics – particularly Prescott Hall – were biased against the “new” immigrants.⁵⁵

In his paper for the Royal Anthropological Institute’s Huxley Memorial Lecture in 1908, William Ripley had revisited his work on European population groups, despite the shift in his professional interests to economics after publishing *The Races of Europe*.⁵⁶ Ripley had been cautious of declaring the existence of fixed and immutable races in his 1899 book. Now, less than ten years later, his cautiousness increased. He was turning away from certainty over racial definitions and identities. Although the Commissioners used *The Races of Europe* as a key source in compiling an ethnological and anthropological guide, they did not use Ripley’s more recent investigation of the European races.

Ripley’s essay showed a reversal of his earlier position on race as a fixed or easily identifiable category. His work had a significant impact on the Commission’s report on race and peoples, but his new attitude was an unwillingness to declare the existence of fixed, immutable racial types that eugenicists and restrictionists argued so heavily in favor of. Ripley’s essay presented some distinct difficulties in precise classification because of environmental influences on human bodies and human evolution. Perhaps that is why the Commissioners, in compiling the “Dictionary,” did not utilize this more recent work.

⁵⁵*Ibid.*, 8-12 for a bibliography used in the Dictionary.

⁵⁶ William Z. Ripley, “The European Population in the United States” *Journal of the Royal Anthropological Institute of Great Britain and Ireland* v. 38 (July-December 1908), 221-240; 221.

In fact, the Dillingham Commission's decision to use Ripley, Deniker (who had died in 1899) and Brinton for ethnological classifications was extraordinary considering that, working with the Commission on a study of the descendants of immigrants, was Franz Boas, one of the most respected – and controversial – anthropologists in the United States. Although his best work was still to come, Boas had already delivered a paper on “Human Faculty as Determined by Race” at the American Association for the Advancement of Science in 1894, lectured on the “History of Anthropology” at the International Congress of Arts and Science in 1904, and had, with the aid of Jeremiah Jenks, secured significant funding for his anthropometric studies from a reluctant Immigration Commission based largely on the basis of his professional standing as an ethnologist.

In the final analysis, all four of the principal authors that the members of the Immigration Commission relied on simply to classify and codify the anthropological races of immigrants offered incredibly complicated schemes of racial anthropology that featured divergent metrics for racial classification. Ripley used the cephalic index to associate racial types into three categories, although he cautioned that no pure racial types existed, and that environmental influences on physical form could be pronounced. Brinton's three books noted that other skeletal features, like jaw and facial angles, were the essential markers of the four racial types that he described. Deniker used hair type as an organizing principle, which generated six large “types” of human populations, which were then broken down further into by eye and skin color and cranial measurement. Sergi's system,

which the Commissioners also relied heavily upon, outlined only two major types – the Eurafrian and the Eurasiatic – that were differentiated by interior skeletal structure. There was no consensus on what constituted an appropriate scheme of racial classification. Six of the nine members of the Commission charged with devising the “Dictionary of Races and Peoples” were politicians, and the remaining three members were not anthropologists.⁵⁷ Yet these men were given the Herculean task of sifting through these highly complicated and contested schemes of racial anthropology to compile a definitive list of the immigrant races that were arriving in the United States. The simpler theories of group and racial identities thus became the most appealing, regardless of the genuine scientific accuracy. As Franz Boas would discover after he submitted his report on “Changes in the Bodily Form of the Descendants of Immigrants,” the Commission’s members lacked the scientific knowledge to sift through these divergent systems of racial organization. The “Dictionary,” then, ended up simply recapitulating, uncritically, the dominant perceptions of racial identity, regardless of its accuracy. To compile the “Dictionary,” simplicity worked best.

For at least some Commission members, Boas’s work was of sufficient quality that he was asked to consult on the “Dictionary of Races or Peoples.”

Executive Secretary William W. Husband asked Boas to examine an early draft of

⁵⁷ The three Presidential appointees, Charles Patrick Neill, William R. Wheeler, and Jeremiah Jenks were, respectively: the United States Commissioner of Labor, the California Commissioner of Immigration, and a professor of political economy at Cornell University. For biographical information, see Neill’s obituary in *The New York Times*, 5 October 1942, 53; John Lund, “Boundaries of Restriction: The Dillingham Commission” *The History Review* v. 6 (December 1994) [which can be found online at the University of Vermont at: <http://www.uvm.edu/~hag/histreview/vol6/lund.html>]; Percy Bidwell, “Jenks, Jeremiah W.” *Dictionary of American Biography*.

the Dictionary to ensure “its accuracy and scientific value from an ethnological standpoint.” Husband recognized that there was considerable disagreement on various racial classifications, and that ethnologists might not agree on every detail or group, but he still desired the document be as accurate and scientific as possible, and anticipated that Boas’s input, considering his reputation, would make it so. But Boas was skeptical of universally declaring and defining racial identity. He sent the draft of the “Dictionary” to an associate, Dr. Sinkhovitch, for comment on the entry for Ruthenians, which Boas then forwarded to Husband. Husband answered Boas in a note a few days later that said, “Although Dr. Sinkhovitch’s letter is one of condemnation rather than criticism, still I am exceedingly glad to have it.” He assured Boas of his own commitment to accuracy, as he continued, “of course the Commission would not authorize the publication of inaccurate and unscientific material upon this or any other subject.” Yet this is largely what it seemed to do.⁵⁸

When it came to describing particular immigrant groups, the Commissioners’ classification of Italian immigrants repeated many of Sergi’s and Niceforo’s assertions of fundamental racial differences in the Italian peninsula. It reported that Sergi, “the foremost Italian ethnologist” had traced the origins of southern Italians to the Hamitic stock of North Africa.⁵⁹ For the Commission, Southern Italy started south of the Po River, and excluded only the *compartamenti*

⁵⁸ William W. Husband to Boas, 29 September 1909; Boas to Husband, 9 October 1909; Husband to Boas, 12 October 1909, all in *Boas Film*. See also Zeidel, *Immigrants, Progressives*.

⁵⁹ United States Immigration Commission, “Dictionary of Races or Peoples,” 82.

of Piedmont, Lombardy, Venetia, and Emilia (almost exactly the same regions that Hall had laid out in *Immigration*). Everything south of the Po, the Commission said, was southern. Florence, Genoa, Bologna – any part of the actual Italian peninsula – was defined as southern, and by extension, of the Iberic or Mediterranean race, making them racially separate from and inferior to the Anglo-Saxon and Teutonic population in the United States. The “Melting Pot” of American society would be unable to assimilate such fundamentally different racial stocks into an “American” race. Yet no mention was made of the hostility and opposition previously directed toward Irish immigrants when they began arriving in the United States in the mid-nineteenth century, nor how they had been considered racially inferior to the Teutonic or Anglo-Saxon but had somehow later proven themselves to be eminently desirable additions. In fact, by 1911, the Irish had become welcome immigrants; the “Dictionary” noted that, “Like the English, the Irish come to the United States speaking our own language and imbued with sympathy for our ideals and our democratic institutions.”⁶⁰ This fluidity of prejudice – first the Irish need not apply for admission, then their cultural and political experiences as part of the United Kingdom suited them perfectly to be beneficial members of American society – reflects the ambiguity of racial science in the early 1900s.

The final version of the “Dictionary,” in dividing up the “great races” of mankind, used Johann Blumenbach’s five categories of Caucasian, Ethiopian, Mongolian, Malay, and American – the white, black, yellow, brown and red

⁶⁰ *Ibid.*, 79.

races.⁶¹ Italians clearly fit into the Caucasian category, but the “racial” attributes of southern Italian immigrants were, when juxtaposed to those of the “English or Anglo-Saxon,” undesirable. Describing the English, the “Dictionary” stated “there is no necessity...for discussion of a subject so well understood by all as [to] the character, social institutions, and other qualities of the English as an immigrant people.”⁶² In the minds of restrictionists, the original English settlers of North America embodied all the characteristics – “intelligence, manliness, cooperation” – that made the United States strong.⁶³ Southern Italians, on the other hand, were described as “excitable, impulsive, highly imaginative, and impracticable; as an individualist having little adaptability to highly organized society.” In drawing a stark distinction between the Southern and Northern Italian – the latter were “cool, deliberate, patient, practical, and as capable of great progress in the political and social organization of modern civilization” – the Commission articulated the risks the country ran by not restricting this undesirable, “excitable” immigrant stream.⁶⁴

Using Darwinian language, the entry on the Italians closed by warning of their great fecundity and high birth rate. The prodigious reproductive capacity of Italians had enabled them to outnumber the “Spanish race in Spanish Argentina

⁶¹ Thomas Guglielmo’s *White on Arrival* makes an important point regarding the primary racial identity of Italian immigrants to the United States: Italians, particularly Southern Italians, may have been *racially* undesirable, but they were never considered not-*white*. Guglielmo, *White on Arrival*.

⁶² United States Immigration Commission, “Dictionary of Races or Peoples,” 54.

⁶³ See e.g. John Commons, *Races and Immigrants in America* (New York: Augustus M. Kelley Publishers, 1967 [Reprint: orig. 1907]), 7.

⁶⁴ United States Immigration Commission, “Dictionary of Races and Peoples,” 82

and the Portuguese race in Brazil, a ‘Portuguese’ country.”⁶⁵ Unlike the Irish, who depleted their island’s population as their men and women left for America, the Italians’ fecundity constantly replenished the peninsula, creating a never-ending pool of potential emigrants. An opponent of unrestricted immigration would immediately see the implications of Walker’s replacement thesis: a perpetual stream of Southern Italian immigration would eventually overwhelm the “English or Anglo-Saxon” stock of the United States.

Italians were not the only immigrant group that received extensive attention from the writers of the “Dictionary of Races or Peoples,” of course. The entry for Jews, while explicating their distinct racial character and composition, explained that Jewish immigration need not concern Americans, since the total Jewish population in the world was relatively small. Italian immigration received the most attention because of its volume, and their alleged criminality, high illiteracy, fecundity and non-Anglo-Saxon racial identity. There were also the 35 million Italians who remained in Europe who may in the future emigrate to the United States.⁶⁶

To gauge the impact of immigrants on the United States, the members of the Dillingham Commission turned to statistics. In addition to Prescott Hall’s 1906 book *Immigration*, the two books Commissioners used to create the “Dictionary” were works of economists, not statisticians: John Commons and

⁶⁵ *Ibid*, 84

⁶⁶ *Ibid.*, 79.

Richmond Mayo-Smith. Commons had trained at Johns Hopkins University with the noted economist Richard T. Ely. As he worked with the United States Industrial Commission around the turn of the century, Commons came upon a wealth of statistical data that he used for his book *Races and Immigrants in America*. *Races and Immigrants* was suffused with a theoretical foundation of classic racialism. But Commons viewed “race” in a unique way that escaped the attention of all of the members of the IRL in the books they published in the 1900s and 1910s. Commons included southern African-Americans in his discussion of race, which was rare in the writings of advocates of restriction. His book was laced with the standard racialized dogma about the delinquency and inferiority of non-whites, particularly regarding African-Americans, whose alleged sexual depravity and cultural backwardness he emphasized.⁶⁷ Still, Commons noted immediately in the first chapter, “Race differences are established in the very blood and physical constitution” of population groups.⁶⁸ Regarding immigrants, Commons ruminated on the essential economic contribution of the various immigrant races, declaring that immigrants were “the hardest, if not the most intelligent worker in our industries.”⁶⁹ But racially they were fundamentally different. And this racial difference, compounded by the tendency of immigrant laborers to lower wages, concentrate in cities, and fall prey to corrupt political machines in urban areas led Commons to the same conclusion

⁶⁷ H. M. Gitelman, “Commons, John Rogers” *American National Biography*.

⁶⁸ John R. Commons, *Races and Immigrants in America* (New York: Reprints of Economic Classics, 1967 [orig. 1907]), 7.

⁶⁹ Commons, *Races*, 126.

that Hall reached: immigration must be either improved in quality or restricted.⁷⁰

It was only in their utilization of the sophisticated and cosmopolitan work of Columbia professor Richmond Mayo-Smith that the Commission's report on race provided any semblance of balance to the statistical data the commissioners relied on.

Ultimately Franz Boas's final report on the descendants of immigrants suggested that there was really very credibility to fears that immigrants were ruining the quality of American racial stock. Although body form seemed to be the most stable element of racial identification, and this belief was at the heart of the "Dictionary of Races and Peoples," Boas argued that his research on the children of immigrants in the United States had shown that "There are not only decided changes in the rate of development, but there is also a far-reaching change in type – a change which can not be ascribed to selection or mixture, but which can only be explained as due directly to the influence of the environment.... The adaptability of the immigrants seems to be very much greater than we had a right to suppose before our investigations were instituted."⁷¹ By examining stature, weight, physiological development, the length and width of the head and face, Boas had found that American-born children of immigrants had a marked divergence from the body type of their parents.

⁷⁰ Commons, *Races*: p. 148 for reduction in wages; 168-70 for the urban experience of immigrants; p. 182 for "boss" system in politics; p. 238 for necessity of legislation improving immigration. Interestingly, Commons also recapitulated Walker's "replacement thesis" on page 214.

⁷¹ United States Immigration Commission, *Reports of the Immigration Commission* v. 38, "Changes in Bodily Form of Descendants of Immigrants" by Franz Boas, Senate doc. 208, 61st Congress, 2nd Session (Washington, D.C: Government Printing Office, 1911), quote from 2; body form from 1.

The extent of change in physical structure was a direct function of the length of time that the immigrant parents resided in the United States, and pointed to a dramatic anthropological fluidity in all population groups, an argument clearly at odds with classic racist ideas. Southern Italian immigrants, Boas found, tended to improve the least physically in the congested urban areas of the United States, but children of Jewish immigrants tended to develop better. He stated that the measurements of children taken at public schools showed “a great plasticity of human types, and permanence of types in new surroundings appears rather as the exception than as the rule.”⁷² But the cause of these improvements could not be precisely documented; the influence of the environment could not be positively demonstrated. Was the change attributable to heredity? Boas examined this possibility, noting that the characteristics of parents seemed to not be blended or averaged in their offspring, so were perhaps a function of some alternative that could be a result of dominant or recessive characters. The Columbia anthropologist noted that man’s understanding of heredity was inadequate at the moment to state with certainty how it affected the development of the children of immigrants.⁷³

⁷² *Ibid.*, quote from 5; Jewish and Italian children’s physical elasticity from 62. Boas described the method on page 82: “In order to insure greater uniformity, three observers were sent to each school in which measurements were taken, and in these schools a considerable number of control measurements were made in order to test the uniformity of observers. On the whole, the results of these tests were perfectly satisfactory. Where any doubt arose, which was the case particularly in some measurements of the width of the face, these measurements were eliminated.”

⁷³ *Ibid.*, 153. Boas had devised a complicated mathematical formula to try to determine whether or not the physical form of the children reverted to one parental type or was the result of a mixture of both parental types.

For opponents of immigration, Boas undermined their simplistic argument that like created like. If the bodies of descendants of immigrants were highly variable, how could one speak of the perils that immigration presented for America's Anglo-Saxon stock? Boas included over 400 pages of statistical tables in his appendix to provide specific empirical support for his argument of elastic human body form. This proved to be important for the professional reception of Boas's report. A reviewer in the *Journal of the Royal Statistical Society* called Boas's findings "distinctly remarkable" and explained that the statistical evidence contained in the appendix was critical for verifying his conclusions. Another review pointed out that the "enormous mass of figures" Boas provided to support his argument, "if accepted and developed, would make a considerable differences [sic] in our anthropological outlook." The reviewer also commended Boas's position, which was "wise in its moderation," but despite the application with "evident care and manifest anxiety to avoid influences of preconceived notions," felt that his chosen statistical method and tables were perhaps not best adapted for his purposes. Still, Boas's contribution to scientific knowledge was important and impressive.⁷⁴ The American press also favorably covered Boas's study, giving him a wide popular audience.⁷⁵

⁷⁴ G. U. Y., review of *Changes in Bodily Form of Descendants of Immigrants* by Franz Boas, *Journal of the Royal Statistical Society* v. 76, n. 4 (March 1913), 417-18; quote from 417; H. J. Fleure, review of *Changes in Bodily Form of Descendants of Immigrants* by Franz Boas in *Man* v. 14, n. 101 (1914), 206-08; quote from 207.

⁷⁵ For examples of press coverage, see the *New York Times* for the following dates: 17 December 1909, 18:1; 18 December 1909, 12:3; 26 December 1909, part 5, p. 3; 30 December 1909, 8:1.

Not all of the American professional community was convinced of Boas's findings, however. After the Immigration Commission's reports were printed in 1911, Henry Fairfield Osborn, President of the New York Museum of Natural History, wrote to Boas's fellow anthropologist Ales Hrdlicka at the Smithsonian Institution, to determine how other anthropologists judged the work of the Columbia professor. Osborn found particularly troubling the suggestion that head form, which Osborn previously thought to be an highly stable physical feature, was in fact changeable. Osborn asked if Hrdlicka would agree that head-form was a stable or "unit" character that would thus be inherited by offspring, or if it was an unstable physical feature. Hrdlicka's perspective on anthropology was similar to Boas's, in that the research he had done on primitive peoples suggested to him—as it did to Boas—that physical structure was fluid. Compelling proof of this fluidity would greatly damage the argument of restrictionists, who claimed that the inability of immigrants to physically and culturally assimilate necessitated their exclusion. Hrdlicka's reply to Osborn on Boas's work was somewhat guarded. He declined to comment on the relative "proof" that the immigrant study had provided, because there were several defects in the study, Hrdlicka explained, resulting in uncertainty as to what, if anything, Boas's project proved.⁷⁶

Regarding the "stability" of head form, however, Hrdlicka was more helpful. He preferred "persistence" as a term for describing head form, but insisted that persistence must be qualified to include "under no greatly differing

⁷⁶ Henry Fairfield Osborn to Ales Hrdlicka, 31 October 1911; Hrdlicka to Osborn, 1 November 1911, both box 58, Papers of Ales Hrdlicka, National Anthropological Archives, Smithsonian Institution, Washington, D.C. [hereafter Hrdlicka papers].

environmental conditions.” He added, “there can be no doubt but that much differing environments such as will induce important physical modification in stature, strength and especially in the development of organs of mastication, will also secondarily affect the form of the skull.” Hrdlicka explained that he had clearly seen this plasticity in his research on Eskimos. Regarding the inheritance of head shape, he said that it was not a particular “unit character” – such as eye color might be – that was unchanging, and found arguments of generational stability to be a highly suspicious claim. The complexity of the subject, the Smithsonian curator argued, made it impossible to say with any certainty if there was continuity in head form, and the exact truth could only be known after “considerable investigation in different localities and with different peoples.” Certainly this would be outside the scope of the Dillingham Commission, or any other modestly sized research group.⁷⁷ Notwithstanding these cautions, though, Hrdlicka sided with Boas against classic racialism.

Hrdlicka’s caution reflected partly his professional position at the Smithsonian, but it also resulted from several extended sojourns among aboriginal people in North and South America. Hrdlicka had emigrated from Bohemia when he was thirteen, and trained in American medical colleges until 1896 when he traveled to Paris to study with French anthropologist Leon Manouvrier.⁷⁸ Hrdlicka consistently maintained high levels of proof for scientific knowledge, and insisted that both observed and theoretical phenomena must agree for a scientific claim to

⁷⁷ Hrdlicka to Osborn, 1 November 1911, box 58, Hrdlicka papers.

⁷⁸ From “Register to the Papers of Ales Hrdlicka” in Hrdlicka papers.

have validity. In a lengthy paper for the journal *American Anthropologist* in 1914, Hrdlicka noted that in the recent development of the discipline, through the “systematic training and regulation of methods,” and cooperation “with closely allied branches of science” anthropology had finally made some secure advances in providing evidence for the cultural and biological development of population groups.⁷⁹ Physical and cultural anthropology were developing as coherent scientific disciplines.⁸⁰

This emergent consensus predisposed Boas and Hrdlicka to expect agreement among scientific investigators, since proper scientific research would generate agreement on empirical and scientific truths. Yet they were not surprised when they did not get agreement, as letters between Hrdlicka and Davenport show. Hrdlicka wrote to Davenport in 1915 to request some scientific instruction on hereditary mechanisms. Hrdlicka noted that ordinary Mendelian patterns may hold true for some specific characteristics, but “it certainly does not in its simple form rule the transmission of...the color of the skin, color, nature and quantity of hair, and the color of the eyes, gums and other special parts of the body.” Hrdlicka needed to be able to explain the inheritance of these physical characteristics in his work, but was unclear on the mechanism of transmission. He hoped Davenport

⁷⁹ Ales Hrdlicka, “Anthropology in America” *American Anthropologist* n.s., v. 16, n. 4 (October-December 1914), 508-54; quote from 552-3. Also instructive are “Letter to G. E. Hale from Franz Boas, Ales Hrdlicka, and Alfred M. Tozzler” *American Anthropologist* n.s. v. 21, n. 2 (April-June 1919), 216-18, which called explicitly for more sophisticated and integrated approach to anthropological work.

⁸⁰ For instance, Stocking, ed. *The Shaping of American Anthropology*; Hamilton Cravens, *The Triumph of Evolution: American Scientists and the Heredity-Environment Controversy, 1900-1941* (Philadelphia: University of Penn Press, 1978); Williams, *Rethinking Race*; Ivan Hannaford, *Race: The History of an Idea in the West* (Washington, D.C.: The Woodrow Wilson Center Press, 1996); Darnell, *Invisible Genealogies*.

could help him understand how inheritance may work in these and other cases. Davenport replied that through his experience, he *was* convinced that hereditary traits “which have been applied so successfully to plants and animals apply just exactly as well to man.” He assured Hrdlicka that he had come to the right man with his questions, as the eugenicist corrected him that the inheritance patterns of eye color, skin color, hair color and pigmentation were all, in fact, quite well understood.⁸¹

Davenport’s position on inheritance in 1915 was substantially what it was when he published *Heredity in Relation to Eugenics* in 1911, the same year that Boas’s report on immigrant body form was printed. The thesis of Davenport’s book was summed up quite easily: “all men are *bound* by their protoplasmic makeup and *unequal* in their powers and responsibilities.”⁸² Hrdlicka, however, was not convinced. He could not share Davenport’s certainty on the inheritance of eye and skin color, he explained, because “I constantly meet with cases which elude satisfactory explanation.” Hrdlicka’s fieldwork—by definition the practical application of theoretical knowledge—proved far more complex than the theory permitted. Replying to Davenport’s criticism of anthropologists in his previous letter, where Davenport attacked the anthropological profession’s unwillingness to use new hereditarian hypotheses on physical form, Hrdlicka defended his discipline. He told Davenport that perhaps anthropologists had good reason to be

⁸¹ Hrdlicka to Charles B. Davenport, 3 May 1915; Davenport to Hrdlicka 5 May 1915, both Davenport papers.

⁸² Charles B. Davenport, *Heredity in Relation to Eugenics* Introduction by Charles Rosenberg (New York: Arno Press, 1972 [orig. 1911]), iv. Emphasis in original.

reluctant to adopt the strict Mendelian inheritance patterns that Davenport was “sure” were proven. In the course of field work, Hrdlicka explained, an anthropologist “finds so little [in the theory] that agrees with his more general observations...”⁸³ These differences suggest a fundamental problem: if two men in related disciplines could not agree on a principle, who was right? Here the reputation of the researcher became important: wild generalizations based on non-empirical evidence threatened the accumulation of scientific knowledge—it misdirected efforts and retarded genuine intellectual advance. On the contrary, careful measurement and experimentation enabled scientific knowledge to advance because it could be tested and verified. It could, as Jennings had pointed out, be shown to be true.

Hereditarian theory based on Mendelian principles was useful only to the extent that observed phenomena supported it. Some scientists, who approached research with an open mind and a willingness to temper theory with observation, saw that predicting inheritance was a highly complex problem. For others, like Davenport, theory was, *ipso facto*, the theory: it had withstood tests and replication, and would not need to be changed. Davenport’s inability to recognize the importance of this alternative approach to scientific knowledge, however, would not hinder the success of movements that he supported. When Davenport’s interests began to include the eugenical qualities of immigrants (already apparent in *Heredity in Relation to Eugenics*), he made sure that the theory could be used to support his position.

⁸³ Hrdlicka to Davenport, 19 May 1915, Davenport papers; also in box 15, Hrdlicka papers.

As Congress acted upon the recommendations of the Dillingham Commission, and passed an immigration bill featuring a literacy test, the IRL's leaders discussed a range of strategies for gaining President Taft's support. In the end, they settled on attempting intimidation. On April 15, 1912 Patten wrote a furious letter to Prescott Hall from D.C. "Taft has lied to us. He has had senators lie to us. He has not said a word, even though the Commission has reported. He slipped off to New York and has members of his Cabinet slip off and lolly-gag the foreigners and certain religious influences that ought to stay out of politics." Patten's note reflected the objections Hall had made about the selection of Commissioners that were more loyal to the immigrant communities in their districts instead of the best interests of the country. The dissenting minority report proved this, in their minds. And Taft too, League members felt, was pandering to the interests of alien communities in the United States, and not protecting the best interests of Americans. The IRL's lobbyist fulminated that, "In all his speaking Teddy [Roosevelt] has not pandered or cattered [sic] or stooped to any such palavering and un-American foreign slobbering." For Patten, there was only one solution: "to hammer the life out of Taft, Taft senators, and those in opposition to this legislation, regardless, I say, of other issues." But Patten's attempt at intimidation failed.

On February 14, 1913, President Taft vetoed the bill specifically because of the literacy provision it contained. The President explained that, although he vetoed the bill with "great reluctance," the bill's literacy provision "violates a principle that ought, in my opinion, to be upheld in dealing with our

immigration.”⁸⁴ Although the vote to override the veto passed the Senate by a large margin, it failed by three votes in the House. Madison Grant, a League vice-president, would later suggest that Taft vetoed the bill because he “did not understand the issues involved, and was confused by conflicting advisors.”⁸⁵ In Grant’s mind, had Taft appreciated and understood the danger the alien immigrants posed to the country, he would have acted to save the nation’s racial homogeneity and signed the bill.

Along with their intense efforts to influence the final report of the Dillingham Commission, the League’s leaders cultivated alliances among sympathizers who might later be useful all the time. Even before the Washington state newspaperman Albert Johnson began serving in the House of Representatives in the sixty-third Congress in March 1913, Prescott Hall wrote to him as a potential supporter of restriction. Hall approached Johnson hoping that the west coast resident would join the League and help spread its geographical reach beyond the east coast. Although Johnson was not really familiar with the League, he told Hall that his own thoughts on immigration “are exactly in line with the basic arguments” of the League and its supporters. Offering to send a separate letter with his membership fee, and a promise to send future copies of his

⁸⁴ “Regulation of the Immigration of Aliens: Message from the President of the United States” Senate Doc. 1087, 63rd Congress, 1st Session, 14 February 1913. Taft also relied on Labor Secretary Charles Nagel’s recommendation for veto. Nagel was long a thorn in the side of the Immigration Restriction League.

⁸⁵ Patten to Hall, 15 April 1912, box 1, Lee papers; vote totals from Hutchinson, *Legislative History of Immigration*, 154; Madison Grant in the Foreword to *Immigration and Other Interests of Prescott F. Hall*, compiled by Mrs. Hall (New York: Knickerbocker Press, 1922), viii. Grant was far less kind in his comments about President Wilson: he was not from “native American stock” and therefore lacked any interest in protecting it from degradation at the hands of European aliens. *Ibid.*, viii.

monthly paper, *The Home Defender*, Johnson closed his letter with a declaration that “I shall proceed along the lines marked out [i.e. to favor immigration restriction and a literacy test] and do all I can for the restriction of undesirable immigration.”⁸⁶

Just as the League leaders had done with President Lowell of Harvard, a sympathetic person might one day be useful in one day achieving restriction. As they left no stone unturned in pressuring the members of the Dillingham Commission to endorse their policy goals, they reached out to broaden support for the movement. But when Hall wrote Johnson, he likely had no inkling as to how useful Johnson would prove to be. Johnson would later chair the House Committee on Immigration and Naturalization, and author several bills that the Immigration Restriction League favored, including the 1921 and 1924 Immigration Acts that effectively ended the open-gate policy of the United States. Cultivating his friendship was time well spent, and demonstrates the devotion these men had to restricting the flow of immigration into the U.S., and the powerful political alliances they forged to achieve that goal.

Having stamped its imprimatur on the forty-one volumes of the Immigration Commission’s reports, the United States Congress provided American restrictionists with a wealth of evidence that supported their assertions that the “new” immigrants arriving on American shores were undesirable. The particular attention the Commissioners gave to southern Italian immigration reflected the anxieties of the American public. Throughout the 1910s and 1920s,

⁸⁶ Albert Johnson to Prescott Hall, 13 June 1912, box 2, IRL papers.

members of the Immigration Restriction League would use the statistics from the reports, as well as the racial definitions and criminal findings, to urge members of Congress to prohibit the entry of undesirable aliens. In publications, magazines, private correspondence and Congressional testimony, the League utilized the Dillingham reports to garner additional public support for restriction. The importance of immigration restriction to them was plainly clear. Now, with Congressional endorsement of their principles, they aimed to secure the passage of restrictive legislation by making the importance of restriction plainly clear to the rest of the country.

Chapter 4: The Immutable Basis of Science?

After the Dillingham Commission endorsed the literacy test to restrict immigration to the United States, anti-immigration groups like the Immigration Restriction League increased pressure on Congress to pass a bill featuring it. The arguments the League made in support of the reading requirement show two significant components of restriction arguments: the continued emphasis on the racial or biological inferiority of the “new” immigrants to the “old” immigration (of which illiteracy was only one manifestation), and the importance of statistical or empirical proof provided by “experts” to demonstrate this assertion. Since this method of exclusion now had the sanction of an “expert” governmental body, other academics and authors joined in support of the League’s preferred method in the 1910s. Despite the affirmation of a policy of restriction by a governmental committee of investigators, however, the legitimacy of a policy of restriction remained contested in the ensuing years. This would be particularly apparent when supporters of restriction tried to add dubious scientific claims to support the change in policy.

Congressional action on the recommendations of the Dillingham Commission began in January 1911 when the House Committee on Immigration and Naturalization favorably reported on an immigration bill (H.R. 15413). The majority of the House committee had declared the “illiteracy test” to be the most “complete method for sifting out undesirable immigrants...” and the bill contained a provision requiring admissible immigrants to be able to read a brief passage in a language of their choosing. On page three of the House Report, a

“racial” ranking of illiteracy among adult aliens demonstrated the outcome the bill would have in restricting immigration: large numbers of immigrants from southern and eastern Europe would be effectively excluded by the literacy test; immigrants from northern and western Europe would not. The report specifically linked the literacy and desirability of immigrants by asking, “Is it probable that 1,000,000 men who can not read will make as good citizens as 1,000,000 men who are able to do so?”¹ The majority of committee members felt that the answer was certainly “no,” and used the statistical evidence to document the larger implications of illiteracy on the desirability and suitability for citizenship. Southern Italian immigrants topped the list of illiterate immigrants, as 51% of all adult males from the *mezzogiorno* were unable to read any language and they were also, as the “Dictionary of Races or Peoples” had shown, racially different from the American Anglo-Saxon stock. Their countrymen from the north were much further down the list, with only 12% of northern Italian immigrants registering as illiterate, and they were, therefore, more desirable to admit. They were also, according to the “Dictionary,” of a “Keltic” racial stock.² Unfortunately, the bulk of immigration from Italy was coming from the southern

¹ House Report n. 1956, “Immigration of Aliens into the United States” 61st Congress, 3rd Session, 20 January 1911, 1, 3. Four members of the committee issued a minority report that opposed the literacy test on the grounds that the test would be ineffective at preventing the admission of immoral, diseased, criminal or vicious immigrants. The minority view was also recognition of the inadequate resources and infrastructure that some nations had to devote to education, and not a sign of racial inferiority. House Report n. 1956, Part 2, “Immigration of Aliens into the United States, Views of the Minority,” 61st Congress, 3rd Session, p. 1. The four members were Gustav Küstermann, a Republican from Wisconsin who had emigrated from Germany in 1868, Adolph Sabath, an Illinois Democrat who emigrated from Bohemia in 1891, and Henry Mayer Goldfogle, Democrat from New York, with William Bennet from New York presenting the dissenting report.

² United States Immigration Commission, “Dictionary of Races or Peoples,” 82.

part of the peninsula, not the northern part. Restrictionists took this high level of illiteracy in the south of Italy as proof of the racial inferiority of southern Italians, which made them undesirable additions to the American population.³ Colajanni had tried to emphasize in the first decades of the twentieth-century that the logical association of illiteracy with racial inferiority was highly problematic, and in reality was due to complex causes (a theme that Jennings and other critics of classical racialism would pick up on later). But the simplicity of the argument that illiteracy was a manifestation of the backwardness of a “race” of peoples was compelling.

Despite the Commission’s recommendation, and the position of the House Immigration and Naturalization Committee, immigration legislation stalled in the 61st Congress and no significant action was taken in the third session. John Higham suggests that this was due to the presidential election of 1912: because of the emergence of Teddy Roosevelt’s Progressive Party as a viable third party, immigrant voters became key to an electoral victory. It was, he explains, “no time to antagonize the foreign vote,” so national politicians took pains to not alienate them by classifying certain groups of them as undesirable or inferior.⁴ This left it to the men outside Washington, D.C. to press home the importance of the literacy test as a restrictive mechanism. The members of the Immigration Restriction League, maintaining their long-standing position favoring the “illiteracy test” as

³ House Report n. 1956, “Immigration of Aliens into the United States,” 3.

⁴ Higham, *Strangers in the Land*, 189-90.

the most effective method of restriction, issued a flurry of publications laying out the positive impact the literacy test would have if it were enacted into law. In a newsletter issued in late 1912, the League's secretary provided immigration figures for fiscal years 1911 and 1912 and reprinted the Commissioner-General's 1912 report on immigration, in which the Commissioner-General endorsed a policy of immigration restriction. Hall quoted the government bureaucrat explaining that the "literate alien" was "better qualified than the illiterate to acquire a knowledge of and respect for our political and social institutions, and may, therefore, be more readily assimilated." The League's pamphlet reiterated the arguments of the House committee's report and noted that restriction based on literacy would not impact the races from northern and western Europe – which included Northern Italians – but that the number of immigrants from southern and eastern Europe, who were "Chiefly Slavic and Iberic," would be dramatically reduced.⁵ League members tried to make clear the fact that enacting the literacy clause would restrict racially undesirable immigrants efficiently and effectively.

Leaders of the League pressed this issue energetically in the early 1910s, driven by the belief that certain immigrants were biologically and permanently inferior, and that an immigrant's inability to read was a quick, simple illustration of that inferiority. The League's tactic of endorsing the literacy test enabled it to avoid making these racial claims explicit: by requiring immigrants to have

⁵ *Publications of the Immigration Restriction League No. 60*, n.d., box 3, Lee papers, n.p. (third page of document for Commissioner-General's statements, fourth page for illiteracy rates by race). The statistical reports date the publication to the second half of 1912.

reading skills, supporters of restriction could plausibly argue that their opposition to certain immigrants was objective and had nothing to do with their race. But when Joseph Lee drafted a paper for publication in late 1912, he had great difficulty in making this rhetorical trick work. A year earlier, the League's Treasurer Richards M. Bradley had written to Lee from his office on Boston's State Street expressing his great fear of making "race" too prominent an element for advocating restriction. Bradley worried that Prescott Hall's recent writings were making a "tactical mistake, and a serious one [at that]," by using the "race issue" to justify restriction. "This immediately and necessarily ranges against us the races involved, who are becoming numerous and influential in the pivotal states," Bradley wrote, and negated the help from the Dillingham Commissioners "who have put the thing on economic and sociological grounds...which seems to be much better for all purposes, including fighting purposes."⁶ Like Hall, Lee was unable to completely divorce himself from utilizing the racial arguments in favor of restriction. He believed that this was ultimately the most important reason to restrict immigration. Lee's draft of "The Illiteracy Test" shows the difficulty the League's members had resisting the arguments of classic racialism.

Lee tried to write an article that engaged opponents of restriction. Most objections to the literacy test, he pointed out in the draft, were that "It would exclude the countrymen of Dante, Columbus, Michael Angelo [sic], Cavour, Garibaldi, Mazzini, etc." This was wrong, Lee explained, because southern

⁶ Richards M. Bradley to Joseph Lee, 16 March 1911, box 1, Lee papers.

Italians may be “Countrymen, yes, but [are] not those of the same race, namely, that of North Italy.” Northern Italians, he claimed, reaching back to Ripley’s *The Races of Europe*, were of the better Alpine race, whereas southern immigrants were Mediterranean or Latin. Lee’s argument also remained tautological: north Italians were racially superior because the race of Dante, Michelangelo and Columbus had produced these great artists, statesmen and explorers; but these men were exceptional because of their race.⁷ The north Italian race, Lee wrote, was literate and desirable, but “It is in South Italy[,] whence none of these great men came[,] that illiteracy is 56.9%.” Lee tried to make this issue of desirability not a racial issue – he printed a marginal note that admitted: “(This point not to be used unless in repartee with somebody.)”⁸ When Lee published his drafted essay in *The Survey* in January 1913, he softened his criticism of the southern Italian race slightly, but maintained the essence of his argument that northern Italians were desirable immigrants and southern Italians were undesirable immigrants.

Lee tried to sidestep racial issues in the published article by coupling reading ability with democratic values, and arguing that illiterate citizens could not sustain a democracy. He cautioned that the total exclusion of all European races was a poor method of restriction because “there is no European race of

⁷ Joseph Lee, “The Illiteracy Test” n.d., box 3, Lee papers, folder: “The Illiteracy Test” ms. note, typescript, 2. There was some irony in making Columbus a North Italian – although he was Genoese – because he sailed and claimed the New World for the Spanish, whom the “Dictionary of Races and Peoples” described as very physically similar to southern Italians and were part of the “Iberic” race. United States Immigration Commission, “Dictionary of Races or Peoples,” 136-7. See also Alfredo Niceforo, *Italiani del nord e italiani del sud* (Torino: Fratelli Bocca, 1901).

⁸ Joseph Lee, “The Illiteracy Test” 2.

which the best [members] are not desirable.” Using Italy as an illustration, Lee explained the race of northern Italians – “the race that produced Columbus, Dante, Michael Angelo [sic], Garibaldi, Mazzini and Cavour” – was desirable because their low level of illiteracy (5.6%) showed them to have the potential to be good democrats. Enacting the literacy test as a restrictive mechanism would exclude 42.8% “of the very different race of southern Italy, the race...whose most notable political and social institution of the present day is the Camorra...”⁹ Lee asked, “Which of these two classes would the reader choose if he were starting out to select material for a democracy, the people of Florence, Genoa and Lombardy, or those of Sicily and Naples; the German burgher or the Russian peasant?”¹⁰ Although the Bostonian believed that Italians not belonging to the race of Dante were racially inferior and would make poor specimens to build or sustain a republic, he was able to make the issue of desirability a function of political capacity and not racial identity.

Other articles published by restrictionists in the period emphasized these aspects of the literacy test. Henry Pratt Fairchild, a sociologist at Yale University, used the pages of *The American Journal of Sociology* to express support for the Dillingham Commission’s final recommendations. Fairchild, who had grown up in the Midwest, became affiliated with the IRL after the publication of his book *Immigration* in 1913. Fairchild felt that it was the duty of academics, social

⁹ Joseph Lee, “Democracy and the Illiteracy Test” *The Survey* 18 January 1913, 497-99; 498.

¹⁰ *Ibid*, 498.

scientists, and experts to make a persuasive case to the public in favor of immigration restriction. He feared that if social scientists would “hold aloof” on immigration legislation, restriction would be pushed by “selfish interest[s] and quack politicians.” Fairchild listed several “evils” of immigration: there were too many immigrants arriving in the United States, and they concentrated in cities, which retarded their successful assimilation, lowered the standard of living, and increased crime. The Dillingham Commission’s recommendation of the literacy test as a method of restricting immigration was good, he wrote, because at least 25% of all arriving immigrants over the age of fourteen could not read or write. Passing a law with a literacy clause would immediately cut the volume of immigration by one-quarter. But the Yale sociologist felt that none of the methods under Congressional consideration were perfect. The government should do more about the problem of immigration. Administratively, he argued that the federal government should take a more active role in distributing and spreading immigrants throughout the country, although this was a suggestion that the IRL consistently opposed.¹¹ Advocates of “distribution” felt that spreading immigrants out geographically would ameliorate several of the problems with immigration

¹¹ Henry Pratt Fairchild, “Restriction of Immigration” *The American Journal of Sociology* v. 17, n. 5 (March 1912), 637-46; 637 for quack politicians, 638-9 for “evils” of immigration and benefits of literacy test; 643 for increased federal intervention; idem., *Immigration: A World Movement and its American Significance* (New York: MacMillan Co., 1913). Regarding the distribution of immigrants, Prescott Hall had previously informed a correspondent that the League “has always opposed” such a provision, and that personally, Hall viewed it as “mostly a bluff of the Jews and steamship companies who throw dust in the eyes of the ignorant and prevent proper legislation; while those who have not thought much about the matter say ‘of course, that is the very thing to do.’” Prescott F. Hall to Maxwell F. Beals, 14 February 1910, IRL papers, box 2; see also Hall’s book *Immigration and Its Effects Upon the United States*.

that Fairchild had pointed out. The League's opposition to such a program suggests an inflexibility on immigration matters, and their distinct antipathy to immigrants, regardless of where they were located, that were racially inferior.

As a sociologist, Fairchild did not place great emphasis on the blood and race of immigrants. As a student of society, he instead saw the negative consequences of unrestricted immigration on crime, urban disorder and degraded labor conditions. Advocating restriction was partly a reflection of cultural and social anxieties, but it also demonstrated a humanitarian interest as well. Fairchild's support for distribution shows that he was not motivated solely by racial fears, and that he believed that environmental and educational influences could be effective assimilating agents if the total numbers of immigrants were limited and their distribution better controlled.¹²

Fairchild's nuanced position was a minority one, though. The University of Wisconsin sociologist Edward A. Ross maintained a different attitude on immigration that manifested the influence of classic racialism. Historian John Higham calls Ross "one of the most race conscious of American social scientists."¹³ Ross's 1914 book, *The Old World in the New* was one of the last publications to deal with immigration before the war, and despite the relative sophistication of the book, it maintained strict allegiance to the primacy of nature over nurture, of biology over culture. Ross opened the book with a commentary

¹² Fairchild, "Restriction of Immigration."

¹³ Higham, *Strangers in the Land*, 109.

on the original composition of the American people, and the positive impact of the virgin environment on all the early settlers, the religious dissenters and the other less savory elements. “The regenerative stimulus of opportunity” he explained, had the remarkable effect of regenerating the positive characteristics of the settlers, effacing the moral taint of the relatively few convicts that were banished from Great Britain. In the New World, Ross claimed (with little supporting evidence), this settler population was reformed into good racial stock. The English criminals that arrived were either redeemed or, for those that were incurably lazy and shiftless, they became the progenitors of what Ross called the “poor whites,” “crackers” and “sandhillers” of southern Appalachia. French Huguenots, Germans, and the Scotch-Irish joined the British, the criminal and innocent alike, to forge a hardy nexus of population that would create the American republic.¹⁴ And each settler group had unique and particular characteristics that they contributed to the developing “American” population. Celtic Irish immigrants contributed poetry and eloquence, courage and loyalty, along with an unfortunate propensity to drink; German immigrants throughout the Midwest contributed conservative, sober, and respectful values. Scandinavians

¹⁴ Edward Alsworth Ross, *The Old World in the New: The Significance of Past and Present Immigration to the American People* (New York: The Century Co., 1914), 9 for stimulus; 10-13 for population attributes. The French Huguenots contributed a “sensibility, a fervor, and an artistic endowment all their own,” the Germans fantastic economic virtues, and the Scotch-Irish an iron will. English criminals, 9. Once the war began, restrictionists’ books became increasingly alarmist, and the “scientific” basis of it more spurious.

were desirable immigrants because they were highly literate and assimilated with speed, although they were prone to melancholy and insanity.¹⁵

For the Wisconsin sociologist, these contributions to the American population were good. The Italians, Slavs and “East European Hebrews,” however, were a mixed addition to the American population. Ross’s racial description of the Italian immigrants reflected the heavy influence of William Ripley. Ross drew a distinction between populations on the Italian peninsula, describing how the *compartamenti* of Lombardy, Venetia, and the Piedmont provided emigrants with “Celtic, Gothic, Lombard, and German” blood, while emigrants of the “Mediterranean race” in the south only added to America a dark-skinned population that contained in it vestiges of Greek, Saracen, and African blood.¹⁶ Ross pointed out in his book that the Italian anthropologist Alfredo Niceforo himself deplored the inferiority of southern Italians. These swarthy, illiterate, undesirable immigrants, Ross declared, disembarked from steerage holds with low foreheads, open mouths, weak chins and skew faces, physical features that obviously showed an inability to “take rational care of themselves.”¹⁷

But despite the southern Italian immigrants’ obvious racial inferiority, Ross found Jewish immigrants to the United States far more dangerous. The cause

¹⁵ Ross, *The Old World in the New*, ch. 2 for Irish, ch. 3 for Germans, and ch. 4 for Scandinavians.

¹⁶ *Ibid.*, 97.

¹⁷ *Ibid.*, 8, 113. Ross also alarmingly saw southern Italian immigrants breaking down color distinctions in the South, where farmers welcomed them as replacements for troublesome black laborers. “A fear has sprung up,” he noted, “lest the Italians, being without the southern white man’s strong race feeling, should mix with the negroes and create a hybrid.” 104. Niceforo in *Italiani del nord e italiani del sud* provided anthropological support for this distinction as well.

of alarm was the economic and political power the Hebrews wielded. Jews in America, one-fifth of the world's total population of Jews when Ross wrote, were the sole source of opposition to immigration restriction, he claimed. "The systematic campaign in newspapers and magazines to break down all arguments for restriction and to calm nativist fears is waged by and for one race," the Jews. Ross accused all of the Jews in New York City, who allegedly controlled New York's Congressional delegation, of opposing the passage of a literacy test. Ross's Jews had a "herding instinct" from centuries of enforced ghetto life.¹⁸ But the professor maintained he was not an anti-Semite, because what he disliked about the Jews had nothing to do with their religion, but was the "certain ways and manners" of "the vulgar upstart parvenus..."¹⁹ These attitudes and his descriptions of the racial background of the old and new immigrants echoed the findings of the United States Immigration Commission, and the long reach of classic racialism. Ross argued that these undesirable groups – in particular the southern Italians, Slavs, and Jews – carried the racial inferiority in their bloodstream. Ripley had made the point that differences in racial groups were physical or anthropological. Ross, like Prescott Hall and Madison Grant after him, transmuted this physical difference to an evaluation of quality. Because they were different, they were undesirable and inferior, since they differed from what was regarded as the best or highest quality stock.

¹⁸ *Ibid.*, 143-45.

¹⁹ *Ibid.*, 165.

Ross's book was significant because he was a sociologist, not an anthropologist, but was nonetheless heavily influenced by classic racialism. In the second part of his book, the professor turned to the social, political, economic and racial effects of immigration. The mere presence of inferior immigrants in the United States lowered the "general plane" of intelligence, self-restraint, refinement, orderliness, and efficiency. Immigrants, he believed, raised the levels of illiteracy, urban crime and poverty, and degraded the general position of women. Ross also predicted "Not until the twenty-first century will the philosophic historian be able to declare with scientific certitude that the cause of the mysterious decline that came upon the American people early in the twentieth century was the deterioration of popular intelligence by the admission of great numbers of backward immigrants."²⁰ When Ross turned the discussion to the effect of immigration on American racial stock and the descendants of the "pioneering breed," the deep influence of classic racialism became clear. Ross explained how he stood in Union Square one afternoon at about the time the Fifth Avenue garment workers left work, and observed 368 people pass by. He maintained that a practiced eye – as many before had testified to the efficacy of experts – could immediately identify the undesirables among the crowd. Of all the workers on the street, only thirty-eight of them had what he considered to be an American visage. Twenty-percent of the total group were "hirsute, low-browed,

²⁰ *Ibid.*, 228 for plane of intelligence; 230 for illiteracy; 235 for women; 239-40 for urban congestion and pauperism; twenty-first century 256.

big-faced persons of obviously low mentality.”²¹ Ross had also fallen under the sway of Walker’s demographic arguments: “William does not leave as many children as ‘Tonio because [William] will not huddle his family into one room, eat macaroni off a bare board, work his wife barefoot in the field, and keep his children weeding onions instead of at school.”²² A reader of Professor Ross’s book could only arrive at the conclusion that unrestricted immigration to the United States was a bane to the country. It was generating very few positive contributions while dramatically eroding the quality, stability, and promise of American life.

These arguments and the recommendations of the Immigration Commission were, however, unable to compel presidential support to make immigration policy more restrictive. Under the Wilson administration, in early 1913 the IRL had begun to consider reorganizing, in an effort to make the League less likely to “offend many otherwise friendly people,” hoping that a “positive name” would be better suited to garner support for restriction than a “negative name.” The suggestion of Robert DeCourcy Ward to rename the League as the “National Committee for Regulating Immigration” would enable the members to attract more people, and as Ward noted to Lee (a point that he obviously knew would garner Lee’s support) it would help convince “more people to give

²¹ *Ibid.*, 285-86.

²² *Ibid.*, 303. Ross’s book could be comical and perplexing. While discussing the physiognomy of immigrant types, Ross decried that foreigners were decreasing the “good looks” of the American race. (293) His veneration for the American race could also manifest itself in a cryptic, obscure manner: “Among all nationalities the Americans bear the palm for coolness, orderly saving of life, and consideration for the weak in shipwreck.” (295)

money.” Lee replied that he supported reorganization because it would give the Immigration Restriction League a chance for “roping in new people,” and he suggested specifically trying to recruit Ross and Commons, because they were successful in couching restriction in academic terms rather than on the basis of the immigrants’ alleged racial inferiority.²³

Hall joined the public debate over immigration policy with an article in the *Journal of Political Economy* in late 1913, just before Ross’s book was published. A new bill that Congress was considering in 1913, introduced by Senator Dillingham, used a different strategy to achieve immigration restriction. The bill replaced the literacy test with a method of numerical restriction, in the hope that some method of restriction would finally meet with Presidential approval. Despite this tactical shift Hall saw numerical restriction as inadequate to the task, as it “might not curtail immigration from southeastern Europe and Asia to any considerable extent, and it is therefore inferior to the reading test as a means of restriction....”²⁴ Having seized upon the literacy test as the most desirable, effective, and simple method of restricting southern and eastern European

²³ Ward to Lee, 27 February 1913; Lee to Ward, 3 March 1913, both in box 1, Lee papers. Emphasis in original note. From Washington, D.C., the League’s Congressional lobbyist James H. Patten wrote to the Executive Committee to inform them that in his opinion, Albert Johnson, the League’s closest Congressional ally “is against us” because, although he publicly opposed immigration to his constituents, his objections were pragmatic, local, and particular. Patten began regarding Johnson as “a wolf in sheep’s clothing” because Johnson did not support restriction on fundamental principles, such as their impact on American racial characters. See James H. Patten to the Executive Committee of the Immigration Restriction League, 10 December 1913, box 3, IRL papers. Johnson maintained a complicated attitude toward immigration, particularly in regard to National Origins.

²⁴ Prescott F. Hall, “The Recent History of Immigration and Immigration Restriction” *Journal of Political Economy* v. 21, n. 8 (October 1913), 735-51; 744-45.

immigration, League members supported it above all other methods. The literacy test would have the effect of immediately lowering, by a significant number, the volume of immigration from these undesirable areas, but it would not affect, as a numerical limit would, desirable immigration from the north and west of the continent. The League's members did not mind how many of these "good" European immigrants might come, only how many of the bad Europeans came. The IRL's leadership did not oppose immigration *per se*, but only certain types of immigration. If the numerical quotas would not limit the overall numbers of undesirable immigrants, it would be useless. The literacy test, because it would effect largely only southern and eastern European immigrants, was therefore more desirable a policy initiative, at least until the public's attitude could be changed to think more like the IRL in excluding specific population groups that were biologically or eugenically dangerous, and the public would not change positions, Hall felt, until scientific evidence of the differences of the new immigrants could be provided. Eugenic science was to provide this evidence.

But eugenics lacked a consensus of its laws and principles. The theoretical foundation of eugenics, that human organisms could be improved with careful breeding, lacked viable, experimental laboratory results to demonstrate that this improvement was possible. It could not, practically, be tried out and proven with human beings. Charles Rosenberg, a historian of American medical science, explains that, "Unfortunately, human beings are not so easily studied as garden peas or drosophila." This difficulty did not prevent Davenport and his eugenic and restrictionist friends from maintaining "a rigid biological mechanis[m]" and

insisting “Every aspect of human behavior had its origins in definite physiological and anatomical mechanisms.”²⁵ Because of the difficulty in proving the laws of inheritance and heredity in human beings, the focus of early eugenics occurred in several broad areas: animal breeding, race betterment and human sterilization, and pedigree analysis. Rich with possibilities for alleviating human afflictions, eugenics attracted a great deal of attention and enthusiasm from both professional scientists and untrained laypersons. But ironically, as historian Philip Pauly observes, “what is notable” about American eugenic biologists was “how little eugenics research they did.”²⁶ One of the ways advocates of eugenics popularized and explained the movement’s goals, aside from the work that Davenport and the Eugenics Record Office conducted, was through educational seminars and public events. The movement’s close ties with the American Breeders’ Association presented opportunities for discussions of eugenics at state and county fairs, and exemplified breeding and good heredity through “Better Baby” contests. With the creation of the Race Betterment Foundation in the early 1910s, a partner organization for eugenics, with the specific goal of educating the public on the methods of racial improvement, was born. The members of the Immigration Restriction League would work closely with the Foundation to both prove the

²⁵ Rosenberg, *No Other Gods*, peas from 91; mechanisms from 93. *Drosophila* were the fruit flies on which Thomas Hunt Morgan made his extraordinary discoveries of chromosomes and genetics. For an essential introduction, see Garland Allen, *Thomas Hunt Morgan: The Man and His Science* (Princeton: Princeton University Press, 1978).

²⁶ Pauly, *Biologists and the Promise of American Life*, 225.
Chapter 4

efficacy of eugenics and to educate the public about the connection between eugenics and immigration.

The Race Betterment Foundation had difficulty in maintaining the scientific integrity of its meetings though. The First National Conference on Race Betterment was held in January 1914, at the Battle Creek Sanitarium in Battle Creek, Michigan. The director of the Sanitarium, John Harvey Kellogg, served on the Executive Committee, along with Irving Fisher, Rev. Newell Hillis, Sir Horace Plunkett, and Jacob Riis. Fisher was a political economist from Yale with restrictionist interests, Hillis was a pastor at a Brooklyn church, Plunkett an agricultural minister for the Irish territories, and Riis was associated with New York's Henry Street Settlement, and was also the author of *How the Other Half Lives* (1890). The disparate interests and backgrounds of the Executive Committee reflected the various tensions within the Race Betterment Foundation; the goal of the Conference was "To assemble evidence as to the extent to which degenerative tendencies are actively at work in America, and to promote agencies for Race Betterment," but it gave no concrete or effective strategies to achieve this. As presented at the conference, "Race Betterment" seemed to include just about every type of reform, regulation, and recommendation.²⁷ From the varied

²⁷ Race Betterment Foundation, *Proceedings of the First National Conference on Race Betterment* (Battle Creek: The Race Betterment Foundation, 1914), xi (hereafter *First Conference Proceedings*). Riis's book *How the Other Half Lives* was a shocking portrayal of the predominantly immigrant slums in New York City. His camera captured the despair, plight, poverty – and for nativists, the danger – of the immigrant classes in and around the city.

associations at the upper levels of the Foundation's Executive Committee, the methods of racial improvement splintered almost uncontrollably.

The organization and interests represented in Battle Creek were almost chaotic. The program of the conference was divided into several categories: Statistical Studies; General Individual Hygiene; Alcohol and Tobacco; Child Life; Sex Questions; School and Industrial Hygiene; City, State and National Hygiene; and Eugenics and Immigration.²⁸ The last set of papers listed in the program was of critical importance for advocates of immigration restriction, and the papers presented there constituted one of the first large public forums after the Dillingham Commission's report for restrictionists to make the case for changing American immigration policies. Four of the speakers for that segment of the Conference were deeply involved in the restriction movement until the 1930s. The essential difficulty of the conference, however, was made clear in the opening address of Stephen Smith, the president of the conference. A medical doctor, Smith was also a vice president of the New York State Board of Charities, and he came to race betterment with a decidedly environmentalist approach, which at its core opposed classic racialist principles. Although the doctor suggested to his audience that an important goal of the movement was to prevent the birth of "degenerates," he also insisted that there was an obligation to improve the living conditions and environment of people. For Smith, the environment was the most important facet of racial uplift and one of the principal causes of degeneration. While this might have rankled some of the attendees like Charles Davenport,

²⁸ *Ibid*, v-x.

Harry H. Laughlin, and Robert DeCourcy Ward, all of whom spoke on “Immigration and Eugenics,” Smith insisted that race betterment could only work if rooted in the “immutable basis of science.”²⁹ For Davenport, Laughlin and Ward, Smith’s recommendation was highly dubious, since in their minds, the environmental influence on personal physical development was minimal at best.

The presentations of several of the speakers at the conference raised troubling questions of just how immutable the basis science was. Scientific knowledge seemed, in fact, to be unclear. For instance, Walter Wilcox, a professor of Political Economy and Statistics at Cornell University pointed out that the proportion of sterile marriages was highest among marriages where the wives were born in the United States. This provided additional statistical evidence for Walker’s replacement thesis, although Wilcox did not offer an explanation as to why these women became infertile.³⁰ Several of the speakers offered positive solutions for racial improvement, particularly those who spoke on “School and Industrial Hygiene” and “City, State, and National Hygiene.”³¹ Yet the majority of the papers dealt in some way with the specter of racial degeneration in the United States. In particular, connections between degeneration and immigration were

²⁹ Stephen Smith, “The Basic Principles of Race Betterment” *First Conference Proceedings*, 5-22.

³⁰ Walter Wilcox, “Differential Fecundity” *First Conference Proceedings*, 79-89.

³¹ See, for instance, F. O. Clements, “Industrial Welfare” *First Conference Proceedings*, 356-64; Stephen Smith, “The National Department of Health” *ibid.*, 376-79; S. S. McClure, “Government” *ibid.*, 393-400. One of the more interesting panelists for “City, State and National Hygiene,” which lies beyond the scope of this paper, was Booker T. Washington’s lecture on “The Negro Race.” See also the discussion of “positive” and “negative” eugenics in Ludmerer, *Genetics and American Society*.

made throughout the entire conference. Dr. H. W. Austin, who worked for the United States Public Health Service (USPHS) at the Detroit-Windsor border, explained that the USPHS was involved not only with problems of disease outbreaks, water pollution, and milk cleanliness throughout the country. One of the most important tasks of the Service was the exclusion of diseased, insane, or physically disabled immigrants.³² Exclusion of immigrants of this type was key for preventing the further racial deterioration of the country.

The medical inspection of immigrants was an important concern for restrictionists. It served a critical function of protecting the health of the American population by preventing the landing of diseased and contagious immigrants on the shores on the United States. Western scientists had gradually developed a chemical and bacteriological understanding of disease and contamination, replacing the older language of miasmatic contamination. Historian Amy Lauren Fairchild points out that this new language reinforced the perception of immigrants as pathogenic threats.³³ In 1892, the United States Secretary of the Treasury, Charles Foster, explained to Congress, “Every serious epidemic that this country has ever known has been traced to the immigrant and it must be apparent that the same authority that controls immigration should control

³² H.W. Austin, “What the United States Public Health Service is doing for Race Betterment” *First Conference Proceedings*, 385-90.

³³ Amy Lauren Fairchild, “Science at the Borders: Immigrant Medical Inspection and Defense of the Nation, 1891 to 1930” (Ph.D. Diss., Columbia University, 1997), 33, 38; idem., *Science at the Borders: Immigrant Medical Inspection and the Shaping of the Modern Industrial Labor Force* (Baltimore: Johns Hopkins University Press, 2003).

quarantine.”³⁴ As medical science became more advanced and sophisticated at understanding the origin of diseases, there grew alongside it a language that associated immigrants specifically with disease, what the medical historian Alan Kraut calls “medicalized nativism.”³⁵

Medicalized nativism reflected genuine and legitimate concerns for the public health with increasing immigration at the end of the 1800s. Beginning in 1891, the federal government mandated that immigrants with “loathsome and dangerous diseases,” such as trachoma, favus, cholera, venereal disease, insanity, feeble-mindedness and epilepsy be excluded from entry. In the early twentieth century, provisions extending exclusion to include diseases that rendered the immigrant likely to be a public charge – heart disease, poor eyesight, senility, poor physique and others – were also put in place. Fairchild notes that the effect of these clauses reinforced the fears that immigrants were dangerous threats to the United States; she explains, “when groups of immigrants failed to conform to societal expectations about the industrial worker, the [medical] examination worked to *exclude* those groups at the nation’s borders on the understanding that they were not racially fit for industrial labor.” Science and medicine played a key role, she suggests, “in rationalizing these exclusions, and the medical examination served as a flexible tool to achieve higher exclusion rates in regions of the country

³⁴ Bess Furman, *A Profile of the United States Public Health Service, 1798-1948* (Washington, D.C.: Government Printing Office, 1973), quoted on 210-11.

³⁵ Alan Kraut, *Silent Travelers: Germs, Genes, and the ‘Immigrant Menace’* (New York: Basic Books, 1994), 4.

receiving greater shares of ‘undesirable’ immigrants.”³⁶ The Immigration Restriction League in fact frequently targeted the inspection facilities at Ellis Island and in Boston as places where political pressure displaced expert medical knowledge to admit immigrants who otherwise would be inadmissible because of their physical or medical conditions.³⁷

The role that medical doctors played in protecting the public health from diseased immigrants provides another instance of the increasing role that professionals and experts played in immigration policy. Like the Dillingham Commission, which the American Genetics Association had called “an unprejudiced and competent body of experts” in 1914, the bodies and health of immigrants became subject to the examination of trained medical experts. Alan Kraut describes the professionalism of the medical examiners, who “were acutely aware of refusing to permit an ever-louder chorus of restrictionists from influencing their medical diagnoses, even if those diagnoses were from time to time unconsciously shaped by their own ethnic biases.”³⁸ Two physicians who

³⁶ Amy Lauren Fairchild, “Policies of Inclusion: Immigrants, Disease, Dependence, and American Immigration Policy at the Dawn and Dusk of the 20th Century” *American Journal of Public Health* v. 94, n. 4 (April 2004), 528-39; quote from 532; classes of exclusion from 531.

³⁷ For instance, see Joseph Lee to Mr. Crane, 9 December 1912, Lee papers, box 1; M. Victor Safford to Prescott Hall, 2 April 1912, IRL papers, box 3; Lodge to Lee, 9 April 1921 and Lee to Lodge, 12 April 1921, both Lee papers box 3.

³⁸ “Second Report of the Committee on Immigration of the Eugenics Section of the American Genetic Association” *Journal of Heredity* v. 5, n. 7 (July 1914), 297; Kraut, *Silent Travelers*, 57. Also helpful are Elizabeth Yew, “Medical Inspection of Immigrants at Ellis Island, 1891-1924” *The Bulletin of the New York Academy of Medicine* v. 56, n. 5 (June 1980), 488-510; Anne-Emanuelle Birn, “Six Seconds Per Eyelid: The Medical Inspection of Immigrants at Ellis Island, 1892-1914” *Dynamis* v. 17 (1997), 281-316; Howard Markel, “‘The Eyes Have It’: Trachoma, the Perception of Disease, the United States Public Health Service, and the American Jewish Immigration Experience, 1897-1924” *Bulletin of the History of Medicine* v. 74, n. 3 (2000), 525-

were engaged in the medical inspection of immigrants talked about their experiences and made this point explicitly. Doctor E. H. Mullan argued that the expertise of medical doctors enabled them to not only assess the physical fitness of an immigrant, but also insisted “Experience enables the inspecting officer to tell at a glance the race of an alien.”³⁹ Mullan’s training and expertise, the immutable scientific basis for his decisions to admit or exclude immigrants, suggested that policy or politicians could not overrule the doctor’s opinions. Victor Safford, who worked in both Boston and Ellis Island, similarly insisted that a glance enabled the trained expert to recognize the physical ailments, race, and desirability of the immigrant. Safford explained that the danger of not excluding these “defective or inferior types” was because they would replicate the conditions of their homeland in the United States, and further endanger the public health of their new country.⁴⁰ They threatened the racial progress and improvement of the country.

60; Nayan Shah, *Contagious Divides: Epidemics and Race in San Francisco’s Chinatown* (Berkeley: University of California Press, 2001); Alan M. Kraut, “Southern Italian Immigration to the United States at the Turn of the Century and the Perennial Problem of Medicalised Prejudice” in Lara Marks, Michael Worboys, eds. *Migrants, Minorities and Health: Historical and Contemporary Studies* (NY: Routledge, 1997), 228-46.

³⁹ E. H. Mullan, “Mental Examination of Immigrants” *Public Health Reports* n. 398 (18 May 1917), 733-46; 737. Mullan noted that “After constantly observing the passing of thousands of immigrants the experienced eye of an examiner will quickly detect the slightest irregularity in gait.” 735. He also provided a list of the markings that immigrants suspected of being medically inadmissible received while undergoing cursory medical inspection – which happened to roughly 15-20% of arriving aliens. 736.

⁴⁰ Victor Safford, *Immigration Problems: Personal Experiences of an Official* (New York: Dodd, Mead and Co., 1925), 15; 238 for replicating conditions. Also, Fairchild, *Science at the Borders*.

Outbreaks of contagious diseases, such as the severe cholera epidemics in Italy in 1884 and 1911, also reflected poorly on the characteristics of the European nations that sent immigrants to the United States and raised significant worries about the safety of public health in America. The historian Frank Snowden's study of cholera epidemics in Naples argues that the persistence of this disease of poverty – spread by poor sanitation, housing and sewer systems – served to reinforce perceptions of southern Italians as different from the northerners. "Cholera crystallized the political and cultural current known as the 'Southern Question,'" he writes, explaining that the political problems in the Kingdom were compounded by the alleged neglect of the liberal state to improving the environmental and economic conditions in the *mezzogiorno*. After the 1884 epidemic the Kingdom of Italy began a large-scale urban renewal program that tried to modernize the infrastructure of Italy's largest city. But when another outbreak hit the city in 1911, the Italian government concealed it from the international community, fearing that the failure of public health reforms would reflect poorly on the character and quality of Neapolitan immigrants.⁴¹ Combined with the anthropological descriptions of southern Italians as "Mediterranean" or "Latin," and thus different from the Nordic or Anglo-Saxon population base of the United States and its "old" immigrants, concerns over medicine played

⁴¹ Frank Snowden, *Naples in the Time of Cholera, 1884-1911* (Cambridge: Cambridge University Press, 1995), quote from 5; see Part IV for the "secret epidemic"; Part III for the *risanamento* program.

another important role in rationalizing, on a scientific basis, the exclusion of certain races of immigrants.

If the medical fitness of immigrants was an important axis for justifying restriction, the association between immigration and eugenics was even more important. The argument in this case concerned the role of immigrants as future parents of native-born Americans. At the First Race Betterment Conference in Battle Creek, Michigan, a panel on Eugenics and Immigration had tried to provide reassurance that the eugenic quality of immigrants was being taken seriously by advocates of restriction. Men like Harry H. Laughlin who had absolute confidence in the scientific basis for studying and improving the American racial stock, gave lectures about the progress of eugenics in the United States. Since 1910 Laughlin had been Superintendent of the Eugenics Record Office at Davenport's Cold Spring Harbor facilities, and he described the success of sterilization legislation like the one passed by Indiana in 1907. Laughlin detailed other methods that could be used to design more effective programs for preventing undesirable parents from producing undesirable offspring. Another medical doctor, C. W. Saleeby, who discussed methods of racial regeneration, followed Laughlin's talk. Two of the panelists on Eugenics and Immigration explained ways to determine and encourage "sound" marriages – by which both meant fruitful marriages between persons of good stock.⁴² While some of these presentations were

⁴² H. H. Laughlin, "Calculations on the Working Out of a Proposed Program of Sterilization" *First Conference Proceedings*, 478-94; C. W. Saleeby, "The Methods of Race Regeneration" *ibid.*, 476-77; Rev. Walter Taylor Sumner, "The Health Certificate—A Safeguard Against Vicious Selection in Marriage" *ibid.*, 509-13; Roswell Johnson, "Marriage Selection" *ibid.*, 515-32. One of

optimistic, many told of impending ruin for the United States. When Robert DeCourcy Ward took the podium to speak on “Race Betterment and Our Immigration Laws,” he made the connection between immigration restriction, eugenics and classic racialism clear by attributing American racial degeneration to the “new” immigration.

Racial improvement must mean more than preventing unfit native-born persons from breeding, Ward cautioned. It must also mean selecting immigrants of only the highest quality for admission. It was the prerogative of the United States, he insisted, to determine which immigrant types should be admitted, and which immigrants should not: “We can decide upon what merits—physical, mental, moral—these incoming aliens shall be selected.” Failing to do so, he warned, would adulterate the pure lines of old stock Americans, and lower the physical and mental standards of the country. “The real wealth of a nation,” he explained, “is the quality of its people.”⁴³ As policy makers and the American

Laughlin’s suggestions, which was adopted as a resolution, was for the forced sterilization of degenerate stocks after a process of “racial” segregation. “Resolutions” *First Conference Proceedings*, 592. On sterilization laws, see Philip Reilly, *The Surgical Solution: A History of Involuntary Sterilization in the United States* (Baltimore: Johns Hopkins University Press, 1991); Mark Largent, *Breeding Contempt: The History of Coerced Sterilization in the United States* (New Brunswick, NJ: Rutgers University Press, 2008).

⁴³ Robert DeCourcy Ward, “Race Betterment and Our Immigration Laws” *First Conference Proceedings*, 542-46. “Merits” from 542; “Wealth of a nation” from 546. In Davenport’s discussion on “The Relative Effects of Heredity and Environment” (*ibid.*, 471-72), he applauded the previous lecturer’s admission that psychological defects seem, in many cases, to be inherited (the lecture was by a sociologist, Herbert Adolphus Miller, “The Psychological Limits of Eugenics” *ibid.*, 456-64). But he then proceeded to use the example of the Kallikak family to prove, even more forcefully—and frighteningly—the “fact” that feeble-mindedness, as was shown in the two branches of the Kallikak family, was inherited, and was predetermined by the mental quality of only *one* of the parents. See Herbert Henry Goddard, *The Kallikak Family: A Study in the Heredity of Feeble-Mindedness* (New York: The Macmillan Company, 1912); Leila Zenderland, *Measuring Minds: Herbert Henry Goddard and the Origins of American Intelligence Testing* (Cambridge: Cambridge University Press, 1998), esp. chap. 5.

public began paying more attention to conservation of forests, parks, and open-space, they were missing the most important conservation of all: that of the pure racial blood of America.

But the scientific character of some of the papers at the conference was spurious even by the standards of the time. Shortly after its conclusion Irving Fisher sent out a questionnaire to attendees to get their reactions and thoughts on the conference. Davenport replied in early March and suggested that he was not overly impressed with many of the papers. The value of the papers “and [their] contributions to knowledge” was decidedly varied. This was to be expected, the eugenicist noted, “owing to the fact that a good many who spoke were not at all men of science, merely expressing very emphatically their opinion.” Should there be another conference, he told Fisher that he leaned toward making the program a little more scientific. Davenport did concede, though, the importance of keeping the conference intellectually accessible to the public, and keeping the papers from being so esoteric as to be incomprehensible to non-experts. As an example of the type of papers that he had in mind, Davenport pointed to J. McKeen Cattell’s statistical paper on “The Causes of the Declining Birth Rate” which was empirically rigorous but also easily understood.⁴⁴ Making their complex scientific theories easily accessible to the lay public was one of the key obstacles that men like Jennings and Boas faced in combating classic racialist ideas.

⁴⁴ Charles Davenport to Irving Fisher, 3 March 1914, Davenport papers; J. McKeen Cattell, “The Causes of the Declining Birth Rate” *First Conference Proceedings*, 67-72.

At the time he made these observations, Davenport was rather sensitive about the quality of scientific work coming from the eugenics movement. Because there was no scientific consensus on what the mechanism or vector of inheritance was, a number of ideas were circulating that purported to explain it. As Davenport had noted about the Battle Creek conference, some of these explanations were not scientific at all, but were merely strenuous assertions of opinion. But Davenport was equally guilty of this. Although Thomas Hunt Morgan and others were putting the final touches on what the historian Stephen Brush refers to as “Morgan’s Chromosome Theory of Heredity” at the time the Race Betterment Foundation was meeting, the chasm between the quality of Morgan’s research work and Davenport’s was tremendous.⁴⁵

Davenport had recently published two books that were, in some circles, very critically reviewed. David Heron, who worked at the Galton Laboratory at University College London, *the* source of Anglo-American eugenics work, attacked Davenport’s work publicly in *The New York Times* in late 1913.⁴⁶ Two of

⁴⁵ Stephen Brush, “How Theories became Knowledge: Morgan’s Chromosome Theory of Heredity in America and Britain” *Jl Hist Bio* v. 35, n. 3 (Fall 2002), 471-535. Morgan and his coauthors explained that “every character is the realized result of the reaction of hereditary factors with each other and with their environment.” T. H. Morgan, A. H. Sturtevant, H. J. Muller, C. B. Bridges, *The Mechanism of Mendelian Heredity* rev. ed (New York: Henry Holt and Co., 1922 [orig. 1915]), 46.

⁴⁶ The two works by Davenport are Charles Davenport, *Heredity in Relation to Eugenics* (New York: H. Holt and Co., 1911) and idem., *Inheritance of Characteristics in Domestic Fowl* (Washington, D.C.: Carnegie Institution of Washington, 1909); David Heron, “English Expert Attacks American Eugenics Work” *The New York Times* 9 November 1913, SM2; Charles Davenport, “A Reply to Dr. Heron’s Strictures” *Science* n.s. vol. 38, n. 987 (November 1913), 743-44; David Heron, “A Rejoinder to Dr. Davenport” *Science*, n.s. vol. 39, n. 992 (January 1914), 24-25; idem., “English Eugenics Expert Again Attacks Davenport” *The New York Times* 4 January 1914, SM14. This will be discussed further below.

Davenport's professional friends, Jennings and Pearl, were becoming increasingly suspicious of the quality of the work coming out of Cold Spring Harbor under Davenport's name.⁴⁷ Fisher's elevated position as a vice president and member of the Executive Committee of the First National Conference on Race Betterment should have given Davenport pause as well, since Fisher—whose native discipline was political economy—had become interested in eugenics only in 1911. His knowledge of the principal ideas and theories behind eugenics was superficial.⁴⁸ Davenport should have been cautious with Fisher regardless, because of the attitudes toward popularizing eugenics that Fisher expressed to Davenport in December 1913.

But Davenport and eugenics supporters were in a bind. They needed to inform and educate the public about the main ideas of eugenics in an accessible language, but this tended to flatten out the genuine scientific complexity of heredity, which they evidently did not fully understand themselves. Throughout the summer of 1913, Davenport and Fisher discussed these difficulties. By December, Davenport wrote to unequivocally decline a role in propagandizing the movement. As he explained that the public's perception of eugenics was that it was synonymous with sex hygiene, Davenport began distancing himself from

⁴⁷ Discussing some of Karl Pearson's recent critiques of Mendelists, Pearl described the "horrible drubbing" that Heron gave Davenport's work on canaries. "It brings out in brutal fashion what I suppose anyone who has critically followed up Davenport's work has however, namely that it is very careless and slipshod. I know nothing about canaries," Pearl told Jennings, "but it is certain that much of Davenport's poultry work is, in plain language, 'no good.'" Pearl to Jennings, 9 July 1910, Jennings papers, emphasis original. See also Mezzano, "The Progressive Origins of Eugenics Critics."

⁴⁸ Fisher to Davenport, 17 October 1911, Davenport papers.

public statements in favor of the movement. “If it becomes clear,” he wrote, “that the Eugenics Record Office must go into propaganda that means that I have to part company with it....I have been close enough to the whirling maelstrom in eugenics to want to keep as far away from it as possible.” Fisher was undeterred, and insisted to his associate that one could not have it both ways, so to speak: one could not expect the scientific aspect of eugenics to be understood by the public, which is what the ultimate goal of eugenics required, without dealing in propaganda. Fisher felt himself being drawn more and more into propaganda work, because it was an essential component of the movement. Knowing Davenport’s affinity for the Austrian monk whose breakthrough research on peas had languished in obscurity for over forty years, Fisher closed the letter with his trump card: “Mendel gives an excellent example of how the practical value of scientific work may be overlooked and its utilization deferred for lack of machinery ready [at] hand to convey the result of such researches as Mendel’s to the people and places where it can be made use of.”⁴⁹ Despite Fisher’s use of one of Davenport’s great intellectual heroes, Davenport remained steadfast; he would continue, for the most part, to leave much of the work of popularizing eugenics to others. When the Second National Conference on Race Betterment was held eighteen months later in San Francisco, Davenport did not give a paper.⁵⁰

⁴⁹ Davenport to Fisher, 16 December 1913; Fisher to Davenport, 18 December 1913, both Davenport papers.

⁵⁰ *Official Proceedings of the Second National Conference on Race Betterment* (Battle Creek, MI: Race Betterment Foundation, 1915). If Davenport attended, in the context of the above exchange, he most likely regretted it. The quality of papers seems as at least as varied and scientifically suspect as the first conference, and the inclusion of the “Playground Pentathlon” and the “Morality

When men of the biological sciences spoke in public about the possibilities of scientific knowledge, even if they spoke inaccurately, their scientific credentials and standing would at least give the impression of scientific truth. But who would perform this popularizing role? Davenport may have wavered, but he recognized the risk when “men of science” did not get involved. He would witness these difficulties in the 1920s in the work of Albert Wiggam, who was not a scientist, but who advanced extravagant claims of what eugenics could do. Controlling the information that was spread about eugenics became almost as important as conducting research in its constituent fields.

The allure of the eugenics movement was its promise, as Davenport explained, to improve the quality of human existence. Yet the non-scientific followers of the movement misrepresented – sometimes inadvertently, sometimes intentionally – what eugenics was able to prove. Occasionally, the efforts of eugenic propagandists met with swift criticism from the actual scientific practitioners. Intellectual leaders of eugenics were aware of these fringe followers, and tried to constrain how they represented eugenics to the public. This was not always an easy feat. With the work of propagandists like Madison Grant, Albert Earnest Wiggam, and Lothrop Stoddard, all of whom raised the shrill cry of racial degeneration, the dissemination of eugenic ideas and precepts reached a

Masque”—a play in two acts performed by over 200 students from the University of California designed to be a “dramatic representation of the Great Truths for which the National Conference on Race Betterment Stands”—made the whole conference seem like a farce. The conference was held in San Francisco to coincide with the Panama-Pacific International Exposition held in August 1915.

large audience. But the scientific accuracy of their claims was very problematic. Davenport and other researchers in eugenics faced a double-edged sword.

Davenport had great difficulty trying to manage these two competing impulses. His position on immigration shows some of the obstacles he and other eugenicists faced. They needed scientific proof in support of eugenics, but had little idea of what that scientific proof might be. When Henry H. Goddard went out on a research trip in 1909, Davenport instructed him “to collect facts supporting the conclusion that the offspring of two imbecile parents are all imbecile.” Although science was supposed to be a cautious empirical study, Davenport wanted to deduce facts that fit his *a priori* beliefs. Goddard was just beginning his career in eugenics, and was appreciative of Davenport’s help. “I must depend on you for much guidance. I have much more zeal than knowledge in this field, tho [sic] I hope to practically remedy that in time.” The following spring, after Goddard had collected some information on feeble-mindedness and its hereditary lineage in a rural New Jersey family, he expressed concern to Davenport that publishing the material might allow opponents of eugenics to “queer” the whole country against the movement. Davenport’s reply to Laughlin is significant, in that it explains the basic position of the former Harvard professor on propaganda and scientific knowledge. “There is no use in collecting facts,” he told Laughlin, “unless they can be put before at least the scientific public to form

the basis of eventual action.”⁵¹ For Davenport, knowledge was a means to an end, not an end in itself.

This attitude had gotten Davenport into trouble before. Jennings had noticed it privately back in 1896 when he studied with Davenport in Cambridge, but he was not the only one to do so. Karl Pearson, one of England’s most gifted mathematicians, was one of the first academics to roundly criticize Davenport’s scientific work. Davenport and Pearson had a stormy correspondence in the early century that foreshadowed many of the problems scientists would have with Davenport’s work. In the first years of the twentieth century, Pearson had turned to Davenport to recommend American students to study biometry in London, with Pearson. The English mathematician hoped the students would then return to the United States to spread the new science of biometrics throughout North America.⁵² In October 1902, Pearson wrote a testimonial in support of Davenport’s application for funding from the Carnegie Institute in Washington, in which he praised Davenport’s “résumé of recent statistical theory.”⁵³ In the following spring, however, Pearson’s attitude began to change. Responding to a

⁵¹ Davenport to Henry H. Goddard, 9 July 1909; Goddard to Davenport 18 July 1909; Goddard to Davenport 13 April 1910; Davenport to Goddard, 15 April 1910, all in Davenport Papers. Goddard, whose principle training was in psychology, studied mental traits and gained notoriety when he published *The Kallikak Family: A Study in the Heredity of Feeble-Mindedness* (New York: MacMillan Co., 1912). See also Leila Zenderland, *Measuring Minds: Henry Herbert Goddard and the Origins of American Intelligence Testing* (Cambridge: Cambridge University Press, 1998).

⁵² Karl Pearson to Davenport, 27 January 1902, Davenport papers. For Karl Pearson’s role in the development of statistical theories and biometry, see Theodore Porter, *Karl Pearson: The Scientific Life in a Statistical Age* (Princeton, NJ: Princeton University Press, 2004).

⁵³ Pearson to unnamed Carnegie Institute employee, 7 October 1902, Davenport papers.

request from the American eugenicist to cooperate on a book of statistical methodology, Pearson refused, since Pearson felt that his own biometrical methods were far in advance of what Davenport would be relating to American pupils. The Englishman also warned Davenport of a trend that he perceived in the American scientific community that often led scientific men “to accept incautiously and without due criticism results which will not really bear investigation.” In particular, Pearson singled out the privileged role that Americans gave to Mendel’s theories without proper scientific verification.⁵⁴ In a telling commentary on a paper that Davenport submitted to Pearson’s journal *Biometrika* in 1903, Pearson crisply summarized the difficulties Davenport had throughout his career of over-drawing conclusions: “The opinions you give there in no way follow from your measurements, in fact have nothing whatever to do with them. There is no objection to a contributor drawing conclusions from the results of his investigations, but we have in the interests of the journal set our face against mere expression of opinions.”⁵⁵ This submission by Davenport was not published.

But these remained private rebukes. Public criticism of Davenport’s work did not begin until A. Rudolf Galloway, a researcher based in Aberdeen, published his research on canaries in *Biometrika* in late 1909. In 1908 the Carnegie Institution of Washington published some of Davenport’s research on

⁵⁴ Pearson to Davenport, 22 March 1903; Pearson to Davenport 23 May 1903, both Davenport papers.

⁵⁵ Pearson to Davenport, 16 June 1903, Davenport papers.

canaries, which was the principal source of financial support for the Eugenics Research Office at Cold Spring Harbor. Davenport's paper was an attempt to examine the rules of inheritance in canaries of certain physical attributes like plumage, coloring, feather distribution, and the shape or form of crests on the breast of the small birds.⁵⁶ The certainty with which Davenport believed rules of inheritance could be precisely documented – which he insisted followed strict Mendelian patterns of dominant and recessive inheritance – was misplaced, and his *a priori* method of scientific investigation sparked criticism from the scientific community on both sides of the Atlantic.

Davenport's sloppy research generated a specific investigation to refute it; Galloway began his study in response to serious flaws that he found in Davenport's research sample in *Inheritance in Canaries*. The British researcher explained that, "Most of the original birds used [by Davenport] were of the Hartz mountain variety...bred purely for song quite regardless of colour and crest, the two points concerning which the author wished to test Mendel's theories..." So the inheritance patterns that Davenport was trying to prove were not likely to be shown in the species that Davenport had selected for study. But the poor choice of experimental subject was the least of the problems Galloway found with Davenport's work. The British researcher refuted many of the claims that

⁵⁶ Charles B. Davenport, *Inheritance in Canaries* Carnegie Institution of Washington Publication #95 (Washington, D.C.: Carnegie Institute of Washington, 1908). It was at about this time that Raymond Pearl headed out to the University of Maine's research station at Orono to begin his research on the egg-laying capacity of chickens, research that would dramatically complicate Pearl's understandings of heredity and genetics and provide the empirical basis for Pearl's eventual break with the eugenics movement. For Davenport, however, his experience with canaries caused him no such reevaluation.

Davenport advanced by noting the American's statistical sloppiness, careless classifications, and general mathematical errors. He found little scientific value in Davenport's paper.⁵⁷

In spite of this harsh rebuke, *Biometrika* was good enough to allow Davenport to respond to Galloway's criticisms, and printed Davenport's self-defense in the April issue, along with further rebuttals from Galloway. The American eugenicist maintained that his specimen sample was carefully chosen, because he was interested in obtaining the evidence for the inheritance of color. To Davenport, Galloway's criticisms were like those of a bird fancier who was objecting to the imprecise use of terminology by a scientist. Davenport defended his claims, stating "I know more about [the canaries'] germ plasm than most punctilious fanciers do about that of their pure breeds. And a knowledge of performance of germ plasm, not capacity for satisfying the conditions of the 'Standard' is what I have worked for." Writing for an international scientific community, Davenport viewed his work as far more important than utilizing the precise vernacular of bird watchers and hobbyists.⁵⁸ He reacted defensively to this

⁵⁷ A. Rudolf Galloway, "Canary Breeding: A Partial Analysis of Records from 1891-1901" *Biometrika* v. 7, n. 1/2 (July – October, 1909), 1-42. Quote from p. 2.

⁵⁸ Charles Benedict Davenport, "Dr. Galloway's 'Canary Breeding'" *Biometrika* v. 7, n. 3 (April 1910), 398-400. Davenport also included at the conclusion a jibe at editorial staff at *Biometrika*, which included Karl Pearson. Galloway's negative reaction in the 1909 article surprised him, as it showed the author failed to understand the Mendelian laws in a scholarly journal where, Davenport sarcastically noted, "the Mendelian theory is so often appreciated and supported." Seeing his Mendelian paper on canaries in print in *Biometrika* gave him satisfaction, as it showed "the increasing catholicity" of the editors to publish Mendelian papers. This elicited a harsh editorial rebuke from Pearson, who wrote in a footnote at the conclusion of the paper that "No paper dealing with heredity from the Mendelian standpoint has ever been refused by this journal,

criticism because of the importance of maintaining scientific authority for his work at Cold Spring Harbor.

Galloway was not swayed by this defense. Scientifically, in “all probability” Davenport’s statements were “valueless” and his interpretations “extremely faulty.” Davenport’s essay had argued that his findings overturned previous conclusions regarding the inheritance of plumage and crest color, despite the fact that these older conclusions were based on a specific sample of pure-bred birds, and a similarly carefully chosen sample would be necessary to verify the inheritance. “From his assumptions both in the matter of crest, and plumage-color,” Galloway wrote, “it is quite impossible for [Davenport] to arrive at any scientifically correct, or practically useful conclusion.”⁵⁹ Not only was the scientific contribution of Davenport’s work suspect, it also began a thunderous dispute with David Heron – another Englishman who worked with Karl Pearson – that would last for almost five years.

Heron jumped on Davenport’s research in the same issue of *Biometrika*, and like Galloway, Heron provided significant criticisms of Davenport’s sloppy research on canaries. Heron’s criticism was limited to the specific arguments of inheritance of crests and lists of matings that Davenport made, in order to “measure the scientific weight of [Davenport’s] paper.” He found it significantly

although such papers would be declined if they were considered inadequate experimentally or theoretically.” 400. These were precisely the problems that Davenport’s work often had.

⁵⁹ A. Rudolf Galloway, “Canary Breeding. A Rejoinder to C. B. Davenport” *Biometrika* v. 7, n. 3 (April 1910), 401-03, quotes from 403.

lacking in any scientific weight at all. The presentation of the material itself was “very defective,” and many of the statistical tables were “self-contradictory.” Heron pointed out instances where Davenport’s conclusions could not possibly be supported from the evidence he provided. He expressed a tone of exasperation at the terrible quality of work that Davenport published, and insisted that the publication of material like Davenport’s canary work was retarding the scientific verification of the Mendelian theory.⁶⁰ The Englishman’s campaign against Davenport’s work peaked in late 1913, when Heron voiced his criticisms in the pages of *The New York Times*.

Davenport’s sloppy work risked discrediting all of the research being done on Long Island under his guidance. So in January 1913, *The New York Times* interviewed Davenport for a story defending Cold Spring Harbor’s research projects, calling the complex “the country’s clearing house for scientific race investigations.” The entire purpose of the Eugenics Record Office and the Station for Experimental Evolution, the newspaper reported, was to collect data to prove the possibility of bringing about “race progress” and combating “race deficiency.” The article explained that one of the ERO’s current investigations involved the inheritance of musical talent. Following Galton’s work in the 1860s and 1870s on inheritance of ability, Davenport declared in the interview that the history of North America proved that “like marries like,” which had the beneficial effect of keeping hereditary strains pure in respect to certain characters. The old

⁶⁰ David Heron, “Inheritance in Canaries: A Study in Mendelism” *Biometrika* v. 7, n. 3 (April 1910), 403-410.

families that settled the Massachusetts Bay Colony and the Virginia Colony, Davenport told the interviewer, “constituted nearly pure strains of scholarship and social leadership respectively. And the characteristics of these strains are inheritable.”⁶¹ But in 1913, Davenport still lacked, as Galton did, a vector for transmission. His assumption reflected not cautious or thoughtful scientific research, but Davenport’s commitment to classic racialist principles, and the certainty that these characteristics were biologically inevitable given the high quality of the early settler stock. Recent advances in genetics and additional experiments in Mendelism, however, had disproved Galton’s findings from the nineteenth century. Pearl’s work on chickens in Orono, Maine was providing ample proof that inheritance of characteristics was far more complex than the simple Mendelian recessive-dominant pattern that Davenport believed in. Yet the *Times* article expressed Davenport’s inflexible belief in the veracity of Mendelian inheritance. The continued sloppiness of Davenport’s scientific approach and the claims he made in the newspaper prompted Heron to move his criticism from a professional journal to denounce him publicly.

Heron prepared for the denunciation of Davenport’s work for months, carefully examining it through the Carnegie Institution of Washington’s publications. Ten months after the *Times* first introduced Davenport’s work at Cold Spring Harbor to the American public, Heron submitted his criticisms. When Davenport retrieved his Sunday *New York Times* on the morning of

⁶¹ “Social Problems have Proven Basis of Heredity” *The New York Times* 12 January 1913, p. SM10.

November 9, the Sunday Magazine featured Heron's blistering criticism of the ERO, and Davenport's own defense of his cherished institution. The editorial introduction the paper gave to the two contributions prepared a casual reader for the acrimonious scientific dispute that followed.

Heron made the accusation that Davenport's work, which he had already characterized as very defective in scientific value, was misrepresenting the facts of Mendelism to the lay public. Heron began his article by ridiculing the meeting, in the summer of 1912, of the First International Eugenics Congress, which the University College of London had hosted. Heron felt that the importance the public attached to racial improvement and deterioration necessitated that work in eugenics be conducted with great care and caution, and the quality of the papers was so poor that the biggest detriment to the advance of scientific eugenics was the work of the Congress itself. The lay public, "who have no means of ascertaining whether a given statement is or is not a great scientific verity" was done a tremendous disservice by the Congress, Heron explained, which gave a forum to many scientists whose grasp of heredity and inheritance was feeble and problematic.⁶² Despite the shortcomings of the Congress's papers, Heron's wrath was particularly focused on Davenport's institution:

⁶² David Heron, "English Expert Attacks American Eugenic Work" *The New York Times* 9 November 1913, SM2. The papers presented at the International Eugenics Congress were published by the London-based Eugenics Education Society under the title *Problems in Eugenics: Papers Communicated to the First International Eugenics Congress* (London: Eugenics Education Society, 1912). Davenport, who was a Vice President of the Congress, a member of the Consultative Committee and the General Committee, gave a paper titled "Marriage Laws and Customs" (pp. 151-155). Giuseppe Sergi and Alfredo Niceforo were also Vice Presidents of the Congress; Niceforo was on the General Committee with Davenport. Both Italians also gave papers at the Congress, which were also translated into English: Sergi, "Variazione e eredità nell-uomo"

“The Eugenics Research [sic] Office has issued a series of bulletins and memoirs, and we shall now proceed to show that the material on which these papers are based has been collected with a *decided bias in favor of a particular theory of heredity*; that it is presented with extraordinary carelessness; that it is, on internal evidence, repeatedly contradictory; that it is not treated in any adequate statistical manner, and that the conclusions reached are not justified by the data.”⁶³

Heron pointed to two problematic bulletins from the Eugenics Record Office [ERO] in particular, “The Study of Human Heredity” and “The Trait Book,” which were guides written to provide instructions to ERO field workers. “The Study of Human Heredity” was instruction for conducting personal interviews with subjects to determine the familial inheritance of characters like neuroticism, sexual immorality, vagrancy, criminality, eccentricity, and shiftlessness, as well as the background of family members that were “decidedly intemperate.” The “research” conducted was not scientific, but was instead the results of assorted interviews with subjects who were “encouraged to talk freely while the field worker records the essential points in the description.” Had Heron known the intended audience for “The Study of Human Heredity” he would have had an even stronger case for demonstrating the carelessness of the work being conducted under Davenport. The researchers of these family traits tended to be younger, unmarried women that Davenport would employ for no more than three

and Niceforo, “La cause de l’infèriorité des charactères psycho-physiologiques des class infèrieurs.” Sergi’s paper is of particular interest, as he claimed to disprove Boas’s recently published work on the body form of immigrants for the United States Immigration Commission.

⁶³ Heron, “English Expert Attacks American Eugenic Work” *The New York Times* 9 November 1913, SM2. Emphasis added.

years. As Davenport explained to his colleague Herbert H. Goddard, “tho [sic] the work may suffer the loss of experience [due to this policy], yet we hope that this loss will be compensated in other directions.”⁶⁴ They should go forth from Cold Spring Harbor, marry, and breed.

The “Trait Book” represented a different magnitude of incautious and careless science. It was an attempt to categorize certain human characteristics for the field workers in order to detail their inheritance. Although Heron took particular exception to Davenport’s use of terms like “impracticalness,” “inadventuresomeness,” “disheartedness,” “unconversationableness,” and “unanecdotedness,” in truth he could have used any number of examples.⁶⁵ For Heron, the work conducted at Cold Spring Harbor was doing a “disservice to knowledge,” and committing “a serious offense against the infant science of eugenics. Every piece of unthorough work ‘dominates’ in research, for it begets its like; others find it equally easy to reach similar spectacular conclusions by loose methods applied to inadequate data.”⁶⁶ Criticizing Davenport’s scientifically

⁶⁴ Charles Davenport, Henry H. Laughlin, Herbert H. Goddard et al., “The Study of Human Heredity: Methods of Collecting, Charting and Analyzing Data” *Bulletin #2 of the Eugenics Record Office* (New York: Cold Spring Harbor, 1911); Charles Davenport to Herbert H. Goddard, 29 July, 1912, Davenport Papers. The reason female field workers were employed for only three years was that discontinuing the financial support would encourage the woman to marry, and “thus the Eugenics Record Office can not be charged with working cacogenically in inducing the excellent field workers whom we have secured to neglect more important social duties.”

⁶⁵ Heron, “English Expert Attacks American Eugenic Work” *NYT*, 9 November 1913, p. SM2. Charles B. Davenport, “The Trait Book” *Bulletin #6 of the Eugenics Record Office* (New York: Cold Spring Harbor, 1912).

⁶⁶ Heron, “English Expert Attacks American Eugenic Work,” SM2. See also Hamish Spencer and Diane B. Paul, “The Failure of a Scientific Critique: David Heron, Karl Pearson and Mendelian Genetics” *The British Journal for the History of Science* v. 31 (1998), 441-52. Spencer and Paul

sloppy work was, for Heron, the only sure way to keep scientific knowledge advancing.

The methodological flaws in his approach to research were the essential difficulties Davenport faced in conducting eugenics research. Men like Heron, Pearson, Pearl and Jennings, wanted science to be advanced cautiously and carefully by indisputable experimental accuracy. A scientific theory had to match the experimental results for it to be considered true. Davenport, on the other hand, aimed to provide practical advice and easily comprehensible rules and guidelines for a wide audience of non-scientists. In carrying out Galton's admonition that the science of eugenics could only succeed when it was internalized like a religion, Davenport loosened the standards of scientific knowledge to enable the public's understanding of highly complex biological and genetic associations. His defense against Heron's attack reflected this. "There has been a concerted effort," he explained, "to make the work of the Eugenics Record Office *accessible for every one, not for scholars and scientists alone*. We have tried to place our findings at the disposal of the people who need them, the people who know the least about things biological."⁶⁷ Here was the problem of establishing the "immutable basis of science." Heron, Pearl, Jennings and other experimental researchers were trying to advance scientific knowledge. Davenport, Laughlin, and other eugenicists wanted the ideas to be accessible and useful to the lay public, even if that meant

suggest that the ineffectiveness of Heron's critique undermines the conventional causal link between the progress of genetics and the decline of eugenics. 452.

⁶⁷ Charles B. Davenport, "American Work Strongly Defended" *The New York Times* 9 November, 1913, SM2-3. Emphasis added.

the scientific complexity had to be flattened out or ignored. Two historians that have examined this particular split between Heron and Davenport suggest that Heron's difficulty was that "even the soundest scientific arguments will fail to convince in the absence of a suitable culture in which the argument can take root."⁶⁸ American culture was amenable to classic racist arguments in part because the ideas were practical and accessible. This easily understood, superficial knowledge of genuinely complex biological principles was the elemental difficulty that critics of eugenics and classic racialism faced into the 1920s.

Ultimately, *The New York Times* ended up siding with Davenport in this dispute with Heron, and in doing so provided him and his work with public legitimacy. Heron's attack was bitter and unfair, an editorial reported, and was the result of the intellectual inadequacies of the current crop of British biometricians. British researchers, the *Times* reported, had spent years collecting "a vast amount of material, and they have an easily explicable reluctance for confessing that it is almost worthless, and that they have been wasting their time and toil."⁶⁹ The genesis of Heron's harsh attack on Davenport's work, the editorial explained, was not because research at the ERO itself was inaccurate, but because the British

⁶⁸ Spencer and Paul, "The Failure of a Scientific Critique," 452.

⁶⁹ "Topics of the Times" *The New York Times* 11 November 1913, p. 12. In the final analysis, the charges the editorial levied against Heron, Pearson, and the British biometricians would ultimately – and accurately – be directed at the American eugenicists in the coming decades. The Eugenics Record Office's collection of pedigree cards was equally worthless. See Allen, "The Eugenics Record Office," 243.

biometricians felt insecure about the quality of their own work. In this case the *Times* did a tremendous disservice its readers. For a public reading this spirited scientific exchange, the judgment of the editorial staff of a major American newspaper in support of American eugenicists would surely have encouraged many of them to believe and accept fundamentally incorrect scientific ideas. When these spurious eugenic claims were being allied to very serious and weighty public debates like the regulation of immigration into the United States, the paper's endorsement of Davenport's inaccuracies seems reckless by the standards of the time. As his difficulties with *Biometrika* show, Davenport had increasing problems publishing his work in reputable academic journals. *The New York Times*, however, gave Davenport what he wanted most: publicity about the work being conducted in eugenics to a large, non-scientific community who neither understood nor cared to follow complex and esoteric scientific disputes.

Davenport remained popular with the New York newspaper. Not only did it side with him in the public exchange with Heron, it published Davenport's review of Frederick Adams Woods's book *The Influence of Monarchs* later in the month. Woods's book was very much in line with the problematic studies of inheritance conducted by Galton, Lodge, Lombroso, and others. It also, not coincidentally, affirmed many of the arguments about inheritance of family traits that Davenport made in 1911 in his book *Heredity in Relation to Eugenics*.⁷⁰

⁷⁰ Charles Davenport, "Eugenics" *The New York Times* 30 November 1913, BR662. It was not a coincidence, as Woods was an ardent supporter of eugenics. For the First International Eugenics Congress, Woods sat on the board of the Consultative Committee with Davenport, and gave a paper titled "Some Interrelations between Eugenics and Historical Research." *Problems in*

Woods's argument was simple: royal families produced illustrious and eminent offspring because of their superior germ plasm. Davenport described the book as "objective and [as] free from bias as possible," and endorsed Woods's conclusion that royalty was superior because of its innate, natural qualities. From the endorsement of the book's thesis, Davenport moved quickly to connect eugenics and immigration restriction. A nation's principal concern should be the multiplication of the strong and gifted stock, he opined. Those of weak and inferior stock should be prevented from increasing. Considering demographic and statistical trends, immigration was nearly as important as reproduction, and the imperative for the United States was "securing a relatively greater fecundity of the best stock already in the country and a relatively greater migration of the better strains into the country."⁷¹ He invoked, once again, Walker's replacement thesis.

Eugenics: Papers Communicated to the First International Eugenics Congress (London: Eugenics Education Society, 1912). Woods was also a member of the American Eugenics Society Advisory Council in the 1920s. See the Appendix of Barry Mehler, "A History of the American Eugenics Society, 1921-1940," (Ph.D. dissertation, University of Illinois-Urbana, 1988). Frederick Adams Woods, *The Influence of Monarchs* (New York: MacMillan Co., 1913); Davenport, *Heredity in Relation to Eugenics*. Rosenberg's introduction described *Heredity* as "the most comprehensive and aggressively assured summary of eugenical doctrines at what was probably the height of their public acceptance." (p. i) Yet the criticisms of Heron, Pearson, Jennings, Pearl, and many others had, by the mid-1920s "demonstrated the arbitrary and ingenuous quality of Davenport's arguments." (p. ii).

⁷¹ Davenport, "Eugenics" *The New York Times* 30 November 1913, BR662. Six weeks later Heron would again denounce Davenport's endorsement of the book and his eugenic contributions in the newspaper's pages. He wrote, "To a great nation willing to experiment it is absolutely essential that its scientific knowledge should be above suspicion." Clearly, he believed that Davenport's work was not. David Heron, "English Eugenics Expert Again Attacks Davenport" *The New York Times* 4 January 1914, SM14. Davenport advanced arguments in *Heredity in Relation to Eugenics* that were identical to those Woods made. He claimed, for instance, that modern medicine had "forgotten the fundamental fact that all men are created *bound* by their protoplasmic makeup and *unequal* in their powers and responsibilities," (iv), while also arguing the point that artistic, literary, and musical talents are unit characters hereditarily transmitted to offspring, although "the common inherited factor may be only a highly developed imagination." (60) Davenport, *Heredity in Relation to Eugenics*, emphasis in original.

The difficulties that Davenport faced in publishing regularly in reputable scientific journals, and the public criticism leveled at him in *The New York Times*, had surprisingly little effect on his work. Advancing his mission to disseminate eugenic knowledge to “the people who know the least about things biological,” Davenport organized the publication of his own journal from Cold Spring Harbor in early 1916. The inaugural issue of the *Eugenical News* in January 1916 stated that its aim was to report news of the ERO’s resident and field staff, foreign and domestic news of eugenic studies, and “other attempts at social control of the selection of mates, including the growth of state institutions.” The reviews contained in the *Eugenical News* shed light on the difficulties Davenport and other scientific-minded eugenicists experienced in policing the borders of proper scientific knowledge and work.⁷² That the journal was published and controlled by Davenport gave him a forum to disseminate “good” propaganda, and to articulate the boundaries of what constituted “bad” propaganda.

The Political Uses of Science

While Davenport was consolidating eugenic knowledge in Cold Spring Harbor and trying to establish unimpeachable scientific credentials for eugenics, the IRL continued its agitation for immigration restriction. National events seemed to play into the League’s hand in the early 1910s, and gave them many opportunities to oppose immigration on the grounds that Bradley had pointed out, namely, using sociological and economic justifications. The League’s paid

⁷²*Eugenical News* v. 1, n. 1 (January 1916), 1; *Eugenical News* v. 1, n. 7 (July 1916), 48.

lobbyist, James Horace Patten, boasted that the Lawrence strike of 1912 had brought the older Irish immigrants into the restrictionist camp. He claimed that Jewish gangsters were responsible for the Arnold Rosenthal murder in New York City, and highlighted the alleged criminal tendencies of New York City Jews to broaden the allure of restriction.⁷³ World War I fueled American xenophobic tendencies against the foreign-born, and “100% Americans” kept watch over the loyalties of these alien, “hyphenated” Americans.⁷⁴

The outbreak of war in Europe in August 1914 was decisive for members of the IRL. Although immigration quickly diminished to a trickle, members feared that immigration would burst forth again when the war ended. They anticipated that the physical devastation of European lands, and the attendant economic dislocation of demobilization would encourage many immigrants of poor stock to depart Europe for America. Thus when the war in Europe began, the League felt a tremendous sense of urgency to secure restriction while the volume of immigration was low. Patten recommended a dramatic shift in tactics, since Congress had been consistently unable to overcome Presidential objections to the literacy provision. When Taft expressed his opposition to any future bill containing a literacy provision, Patten felt this made the League’s strategy futile and he began to reconsider the League’s legislative options. Additionally, anti-restrictionists in Congress were adopting new tactics in an attempt to divide

⁷³ Lee to Hall, 5 September 1912, box 1; Patten to Lee, 29 September 1912, box 1, both Lee papers.

⁷⁴ Higham, *Strangers in the Land* has a good description of the nationalist ferment of the period.

restrictionist forces. The policy of “numerical limitation” that the Dillingham Commission had suggested as another element of immigration policy was being discussed by New York Representative William Bennet, who was opposed to restriction.⁷⁵ With the anti-immigrant forces fragmenting because of these new alternative strategies and the general distraction of the war in Europe, Patten felt that if the bill that reintroduced the literacy test, which Congress was then considering, failed, “we cannot put any restrictive proposition across, in my opinion, and it would be futile and a waste of time and good money to try.”⁷⁶ But what Patten and several others who favored restriction underestimated was the psychological impact of the war. As nativists and Anglo-Saxonists pondered potential outcomes of the Great War as it settled into static trench warfare, one consequence of it became apparent: the war would have tremendous racial and demographic implications.

This was not an entirely new revelation. Stanford University President David Starr Jordan, long a supporter of immigration restriction, had discussed hereditary implications of warfare in 1902 and 1912. The blood of a nation, Jordan wrote, was its most important vital resource, and the quality of blood – and blood alone, since “as science knows, education and training play no part in

⁷⁵ James H. Patten to Joseph Lee, 29 September 1912, Lee papers. The policy of numerical restriction set a numerical limit on the total number of immigration the United States would accept, and apportioned this limit by “quotas.” Patten did not explain why an opponent of restriction would recommend this policy, only that it was confusing and dividing legislators who favored restriction.

⁷⁶ James H. Patten to Prescott F. Hall, 24 November 1914, box 3, IRL papers.

heredity” – was solely responsible for the fate of nations. In *The Blood of the Nation* (1902), Jordan offered a historical lesson of war, explaining that Rome had fallen not because of corruption, indolence, imperial overextension, Nero or Caligula, but because the quality of the breeding population declined. Roman conquests required the abilities of *Vir*, the real man, to go “forth to battle and to the work of foreign invasions; *Homo*, the human being, remained on the farm and in the workshop and begat the new generations.” Virtuous, strong men fought in the Roman legions and maintained control of the Empire’s vast holdings. Effeminate, shiftY males avoided military service and remained at home to enjoy the sacrifices of the real men. The death of virtuous men was therefore doubly tragic: not only was Rome deprived of the superior characteristics from their superior blood, but their deaths also deprived Rome of future offspring with these superior characteristics.⁷⁷ This echoed the idea of what could be called “settler selection” which Commons had alluded to: the early immigration to North America had been undertaken by hardy, strong, virtuous souls, and the conquest of the New World had made them even stronger. Once the difficult work of subduing the continent and establishing a new republic had concluded, shiftY and worthless men, beaten men from beaten races (as Walker called them), traveled to the already-established United States, contributing little to the country except undesirable, inferior offspring. What made the present particularly alarming and

⁷⁷ David Starr Jordan, *The Blood of the Nation: A Study of the Decay of Races Through the Survival of the Unfit* (Boston: American Unitarian Association, 1902), 15-16 for heredity; idem., *The Human Harvest: A Study of the Decay of Races Through the Unfit* (Boston: American Unitarian Association, 1912), 25 for decline of Rome; 24 for “Reversal of selection.” *Human Harvest* was an expansion of *Blood of the Nation* with slight alterations to the original text.

terrifying for classic racialists was the casualty rate of the Great War. Artillery and machine guns had made it clear as early as October 1914 that industrialized warfare would carry off hundreds of thousands of *Vir* to their deaths in rapid fashion.

The European war provided the backdrop for the expression of these racial concerns at the Second Race Betterment Conference, which opened in San Francisco in August 1915. The First Conference had provided a forum for a discussion of problems of eugenics and racial health. Several social scientists used the Battle Creek meeting to urge audience members to be cautious in their expectations of eugenics, and even Davenport criticized some of the contributions of unscientific men.⁷⁸ There was no such caution at the San Francisco conference. After the President of the Race Betterment Foundation provided some brief opening remarks, David Starr Jordan spoke to attendees about the eugenic implications of war once again. But now Jordan was not limited to ancient examples of the dysgenic consequences of militarism. He and all the other attendees had a contemporary example on the European continent, although this did not change his basic message from 1902. The greatest tragedy of any war, Jordan explained, was that it slaughtered the best breeding specimens, leaving “the second best” classes as the only genetic resource. Because of this, every war-like nation in history became decadent in time, as the superior classes were

⁷⁸ See, e.g. Herbert A. Miller, “The Psychological Limit of Eugenics” *First Conference Proceedings*, 8-12 January 1914 (Battle Creek, MI: The Race Betterment Foundation, 1914), 464-71; Davenport to Fisher, 16 December 1913; Fisher to Davenport, 18 December 1913, both Davenport papers.

killed and outbred by inferior classes.⁷⁹ War, Jordan claimed, was “the great ‘mollycoddle’ factory – the great producer of men that are too weak and too dull to think.” Not because their senses were deadened or blunted by the savagery of war, but because the only fathers for future generations that survived lacked all the vigorous qualities of the *Vir*. He insisted that had 500 of the most prominent men of British North America died in battles before American independence, American history would have been radically different because the country would have been deprived of the hereditary contributions of the blood from those 500 men. This conception of the racial history of the United States dovetailed with Walker’s fears of the replacement of the Anglo-Saxon stock.⁸⁰ Although his historical exposition was dubious, Jordan tapped into a very real anxiety that the war was destroying the best racial specimens of northern Europe. Those that survived the war, those “too weak and too dull to think” would be unable to replace that exceptional class in terms of leadership and hereditary gifts. But this message was reaching a very narrow audience. The leadership of the IRL knew that they needed to reach a very wide, general audience to achieve real restriction.

For restrictionists, these public conferences, private correspondence, and articles published in erudite journals had their desired effect on garnering support for immigration restriction from a portion of the learned population. But they

⁷⁹ There is an obvious tautology in this line of thinking as well: the men who stayed at home were unfit for soldiering, since every fit, patriotic male would enlist, fight, and bravely die.

⁸⁰ David Starr Jordan, “Eugenics and War” *Official Proceedings of the Second National Conference on Race Betterment* [hereafter *Second Conference Proceedings*], 4-8 August 1915 (Battle Creek, MI: Race Betterment Foundation, 1915), 13 for ‘mollycoddle’; 15 for American history.

needed different tactics to reach a wider audience. One book by an unmarried New York patrician helped them achieve this, and consequently had a greater impact on the history of American immigration: Madison Grant's *The Passing of the Great Race*. Published in the fall of 1916, Grant's book proposed to explain the wide expanse of European history through the lens of racial anthropology. Although his professional training in the academic field of anthropology was quite limited, Grant's writing style – a biographer describes his “passages [that] sparkle with energy and wit” – made the book far more accessible than esoteric and complex scientific treatises on anthropology, biology, and heredity.⁸¹

Grant, a “Park Avenue bachelor” and “the most lordly of Patricians” as John Higham describes him, was a central figure in the restriction movement. Grant's definitive biography has only just been written by the historian Jonathan Spiro, who describes Grant's family background as including “Dutch grandees, Puritan divines, colonial magistrates, revolutionary patriots, and decorated soldiers.” Grant spent his formative teen years in Dresden, Germany, where he received a classical education before returning to the United States in 1884 to enter Yale University where he studied under the sociologists and Social Darwinist William Graham Sumner. After graduating, Grant returned to New York, attended Columbia Law School and was admitted to the bar in 1890, after which he established a legal practice next to the New York Stock Exchange. He became a member of all the elite men's clubs in Manhattan – the Union,

⁸¹ Spiro, “Patrician Racist,” 338.

Knickerbocker, University, Down Town, and Tuxedo – through which he cultivated his interests in hunting, conservation, and immigration restriction.⁸²

Grant's fundamental supposition, which Professor Henry Fairfield Osborn endorsed in the preface that he wrote for Grant's book, was that heredity was all-important in the field of human history, and that the germ plasm contained within each individual human being was immutable and unchanging, was the sole source of the individual's potential, and determined their development. His biographer notes that Grant's extraordinary contribution to eugenics was "to advance it from a war against individuals who were socially unfit into one against groups who were *racially* unfit."⁸³ Several of Grant's arguments illustrated the central problem of changes in scientific paradigms and the manipulation of scientific knowledge.

The Passing of the Great Race was the culmination of several intellectual trends. In its supposition of unchanging, immutable racial characters, it represented the quintessential classic racist argument. Grant's analysis of differential fecundity exhibited the long reach of the nineteenth century arguments of Francis Walker, as well as the English sociologist Herbert Spencer's Social Darwinism. The Great War raging in Europe, Grant predicted, would have

⁸² Higham, *Strangers in the Land*, 155; Spiro, *Defending the Master Race*, family from 7; 8-11 for New York life. Spiro mentions a curious inversion: "during the course of my research, if I told people that I was writing a biography of a leading conservationist, they would delightfully exclaim: 'How wonderful!' On the other hand, if I told people that my subject was a leading eugenicist, they would invariably respond: 'How dreadful!' It is instructive to remember that one hundred years ago, those reactions would have been reversed." *Ibid.*, xvi.

⁸³ Spiro, "Patrician Racist," 325, emphasis in original. Spiro suggests that Grant was the genesis of "scientific racism": an amalgam derived from wildlife management, paleoanthropology, race suicide, Aryanism, eugenics and genetics. *Ibid.*, 325. As I have tried to demonstrate, the worldview central to scientific racism – what I call classic racialism – preceded Grant by a few decades. Also, Madison Grant, *The Passing of the Great Race, or The Racial Basis of European History* (New York: Charles Scribner's Sons, 1916).

horrible dysgenic effects, since “the loss of life now going on in Europe will fall much more heavily on the blond giant than on the little brunet [sic].”⁸⁴ This combination of demographic replacement with fixed biological and anthropological types blended together aspects of Walker, Ripley and Galton’s writings.⁸⁵

Grant maintained an unshakable conviction throughout the book that the only element of human existence that mattered was the unchanging and immutable germ plasm of each individual. One of his most quoted declarations exemplified his approach to racial anthropology: “The cross between a white man and an Indian is an Indian; the cross between a white man and a Negro is a Negro; the cross between a white man and a Hindu is a Hindu; and the cross between any of the three European races and a Jew is a Jew.”⁸⁶ The germ plasm of the inferior race – the Indian, African, Hindu or a Jew – would irrevocably contaminate and degrade the blood of the superior race. And the question of which race was superior Grant answered with total certainty. The “absolutely fair skin” of the pure Nordic race was zenith of human perfection, although women of fair skin faced constant danger from the “keen envy” of men “whose skins are black, yellow, or red.” Offspring of such a union would tragically contain none of the

⁸⁴ Grant, *The Passing of the Great Race*, 73-4. Also see chapter 4 for implications of Walker and Spencer.

⁸⁵ See Higham, *Strangers in the Land*; Spiro, “Patrician Racist”; Spiro, “Nordic vs. Anti-Nordic: The Galton Society and the American Anthropological Association” *Patterns of Prejudice* v. 36, n. 1 (2002), 35-48.

⁸⁶ Grant, *The Passing of the Great Race*, 18. See discussions of Grant in Higham, *Strangers in the Land*; Jacobsen, *Whiteness of a Different Color*; Spiro, “Patrician Racist.”

Nordic racial characteristics from the mother. It was therefore essential for Nordic elements of the European and American population to breed only with other Nordics, and to have them protected from contamination.⁸⁷

Grant bent history to support his assertions. As he surveyed the expanse of European history, he argued that nearly every notable achievement in Western history was the product of the Nordic race. The organization of Rome, its legal and military efficiency, its ideals of family life and loyalty and truth suggested a northern origin.⁸⁸ The Mediterranean and Alpine races – whose geographic and anthropological descriptions he took from Ripley's *The Races of Europe* – were extensions of “Asiatic subspecies” and therefore, non-European. Sergi had also suggested this, although Grant provided no footnotes for verifying his claims. But the Nordic race, Grant argued, was “a purely European type, in the sense that it has developed its physical characters and its civilization within the confines of that continent. It is, therefore, the *Homo europeaus*, the white man par excellence.” Although the “Nordic Fatherland” was in the forest regions of eastern Germany, the great Italian artists of the Renaissance – Dante, Raphael, Titian, Michelangelo, and da Vinci – were all Nordics, “just as in classic times many of the chief men and of the upper classes were Nordic.”⁸⁹ Grant took the

⁸⁷ Grant, *The Passing of the Great Race*, 28.

⁸⁸ *Ibid.*, 154-5.

⁸⁹ *Ibid.*, 167; 215.

superiority of the Nordic race to be a given in every field of endeavor, and every great achievement of civilization, by definition, was a Nordic achievement.

But these were total fictions, a point the anonymous reviewer in *The American Historical Review* quickly pointed out. Though the book contained a few solid historical and scientific truths, the reviewer insisted that Grant's arguments that racial identity determined the success of a civilization were unconvincing and overstated. The book, the reviewer felt, "can hardly be regarded as an important contribution to historical science. Its dogmatic assurance and its partisanship impair its value to learning."⁹⁰ For a proper and careful study of European racial history, the "solid and discriminating *Races of Europe*" remained the best source of guidance.⁹¹

Even a sympathetic reviewer could be a bit put off by claims Grant made in *Passing*. Frederick Adams Woods, who wrote the review of the 1918 edition for *Science*, gave a cautious endorsement of the book. Woods – who admitted that he accepted the racial theory of European history – pointed out that Grant neglected to engage points of his argument that were scientifically disputed, and that Grant's book featured no references or footnotes for verification. Woods assured the readers of his own review that in support of Grant's argument – that race was immutable and all-important – "there is a mass of carefully finished statistical research on the problem of human heredity which tends to support the

⁹⁰ A. B. S., review of *The Passing of the Great Race, or the Racial Basis of European History* by Madison Grant, in *The American Historical Review* v. 22, n. 4 (July 1917), 842-44; 843.

⁹¹ *Ibid.*, 844.

whole theory...”⁹² But heredity was not solely a statistical field. It encompassed anthropology, biology and sociology (among others), and was a complex concept. Yet Woods could not have been expected to be impartial. In addition to his classic racialist book on *The Influence of Monarchs* that Davenport reviewed, in 1912 he had given a paper on “Some Interrelations between Eugenics and Historical Research” in which he proposed a science of historiometry to “prove that heredity is everywhere [the] chief force in determining nations.” He also joined the American Eugenics Society Advisory Committee in 1923, and remained affiliated with the group until 1935.⁹³

One academic who was not confused about the role of environment versus race was Horace Kallen, an immigrant Jew from Germany living in New York. Kallen, a philosopher, shared Franz Boas’s cultural pluralism and opposition to classic racialism, and his review of *Passing* reflected that opposition. The transplanted German understood the lure of the simplicity of classic racialism, describing the ideological shift from accepting inequalities within the human races to accepting the superiority of one’s own race as “easy and unconscious.” Kallen suggested that Houston Stewart Chamberlain’s *The Foundations of the*

⁹² Frederick Adams Woods, review of *The Passing of the Great Race* by Madison Grant, in *Science* n.s. vol. 48, n. 1243 (25 October 1918), 419-20; 420. Woods expressed disappointment that Grant did not proffer an answer as to how a country – he mentioned Germany – could so quickly improve the quality of their population at the instigation of a few leaders, or how upper classes would evolve, since presumably they were the highest evolved. Woods was on the consultative committee of the First International Eugenics Congress, along with Davenport, Jordan, Hrdlicka and Kellogg.

⁹³ See Barry Mehler, 309; See Frederick Adams Woods, “Some Interrelations between Eugenics and Historical Research” in Eugenics Education Society, *Problems in Eugenics: Papers Communicated to the First International Eugenics Congress* (London: Eugenics Education Society, 1912), 253ff.

Nineteenth Century, which Kallen described divided the world into two classes – “the things that [Chamberlain] liked, which he called good, and the things that he disliked, which he called evil” – must have been an inspiration for Grant, as “his own book on the passing of the great race echoes the absurdities of Mr. Chamberlain.”⁹⁴ The publicity around *Passing* celebrated it as a “scientific” book, but Kallen refused to evaluate Grant’s book on a scientific basis. It was bad enough to Kallen that Grant could not maintain a consistent argument: though Grant suggested every excellent thing in civilization was from the Nordic race, the philosopher pointed out that many of Grant’s statements showed that they originated with Alpine and Mediterranean stocks. The publisher’s notice to the public of the publication of *Passing*, Kallen pointed out, commended it as “an entirely new and original recasting of history on a purely scientific basis.” If that was true, Kallen wryly wrote, the science was so pure it was imperceptible. This was not science, it was fiction, and Kallen’s review reflected his dismissive attitude to Grant’s screed.⁹⁵ That Grant invoked science to legitimize his arguments and that the book was pushed as a work of science rankled legitimate academic scientists. Yet *Passing*, whatever its professional or scientific shortcomings, was a popular book. It contained none of the tentative hypotheses, cautious generalizations and insecure certainties of genetic research or biological

⁹⁴ Horace M. Kallen, “Racial Mythology” review of *The Passing of the Great Race* by Madison Grant, in *The Dial* v. 62, n. 742 (19 May 1917), 432-33. 432 for the “easy” transition; 433 for absurdities.

⁹⁵ *Ibid.*, 433.

laws. Grant had an audience, and he offered them easily comprehensible explanations using plausible or common sense proof. Clearly this was no way for science to be conducted, but Grant was not interested in the scientific accuracy of his books. He was interested in their efficacy.

Edwin Grant Conklin, however, was interested in the scientific accuracy of statements as well as their efficacy. On an evening in late December 1916, in a speech before the American Society of Naturalists in New York City, the respected Princeton biologist confessed uncertainty about how inheritance worked and shaped the development of human organisms. Like Jennings and Pearl, Conklin had a favorable but skeptical attitude toward eugenics. Conklin asked his scientific audience, if human life depended on the quality of its crops and herds, why biology could not show how to make better citizens and a better state? The problem, as the professor saw it, was that American social institutions were based upon the “antiquated and wholly erroneous opinion that ‘all men are born equal,’” and were at risk from individuals with inferior heredity who were not equal to Anglo-Saxons or Nordics. Conklin suggested that although good environment and education were important elements of human development, good heredity was of far greater importance. Certain races were capable of creating sophisticated civilizations, but for other, presumably inferior, races, sophisticated civilizations were impossible because of their heredity and inherited characteristics.⁹⁶

⁹⁶ Edwin Grant Conklin, “Biology and Citizenship” delivered at the American Society of Naturalists, 29 December 1916, Edwin Grant Conklin Papers, Series I: Professional Papers, Box 3, Rare Books and Special Collections, Firestone Library, Princeton University, Princeton, NJ [hereafter Conklin papers], p. 5.

It was the duty of biologists, Conklin explained, to “teach the nation that heredity is more potent than environment or education...”⁹⁷ Conklin’s talk seemed to provide academic credence to Grant’s ideas; he was making Grant’s claims scientifically legitimate. The Princeton biologist declared that the American experiment with immigration had been undertaken without due consideration to the biological consequences of admitting inferior racial stocks. Immigration legislation, which could obviously not undo “the evil that has been done” by the haphazard admission of immigrants, should now be utilized, he urged, to “exclude some of the worst hereditary lines.” Conklin mentioned that the immigration bill being finalized in Congress improved upon the mental examination of immigrants, and argued that although the literacy test, “upon which public attention has been focused...is not a very valuable one,” it was an improvement over existing legislation in that it at least addressed issues of mental ability. Conklin, like many opponents of the test, felt that literacy was not a hereditary quality but was a test of opportunity, and that other, more accurate tests of intellectual ability could be implemented. But as the subsequent mental exams of American army recruits showed, these were equally problematic methodologically and subject to misinterpretation.⁹⁸

Conklin’s address to the Naturalists exhibited other confusions about the role of heredity and environment, but he did try to correct certain misconceptions

⁹⁷ *Ibid.*, 5.

⁹⁸ *Ibid.*, 6. This is discussed more in the following chapter.

that were circulating through the literate public, particularly via Grant's book. Strictly speaking, he said that there were no "human pure breeds," but racial types did exist, and these types had distinct mental, social, and physiognomic differences. Conklin insisted that Grant's argument that any combination of superior stock with inferior stock yielded inferior offspring was an overstatement. Combinations of the best types, even of different racial stocks, he explained, generated offspring with the best tendencies of both types. But generally speaking, assimilation created an averaging of type, which created potential problems in that "the lower types drag down the average." Avoiding this decay was one of the great appeals of eugenics, of which "no other nation has a greater need" than the United States, according to Conklin.⁹⁹

In a nutshell, this was the dilemma of the experimentalist community: research in eugenics had to be conducted very cautiously and seriously in order to assess the scientific merits of its claims. But the promise of the eugenics movement was appealing and attracted a wide range of enthusiasts with little or no scientific background. Jennings, Pearl, Conklin and many other professional scientists who later broke with the movement were inspired in the beginning by the positive possibilities of genetic and eugenic science. At its heart, eugenics was a progressive, humane project. But the science in support of it was uncertain, and their attitudes toward eugenics became more critical as scientific knowledge was revealed to make the goals of positive eugenics impossible. Eugenics enthusiasts

⁹⁹ *Ibid.*, 8, 10. Absolute eugenic selection, however, was undesirable for Conklin, because "who would dig coal and build railroads and work in factories or farms?" 9.

on the other hand, had unrealistic expectations, and no sound scientific basis for their claims. Trying to limit those expectations by showing the scientific difficulty of the eugenics movement had to be done by accurately describing the true complexities of inheritance and heredity, but enthusiasts had no understanding or appreciation of this complexity, and frequently reverted to their appealingly simple claim: like produces like.

For many Congressmen who favored restriction, the coupling of literacy and desirability was a way to avoid becoming entangled in these complex debates over biological science, classic racialism, and eugenics, and to circumvent this esoteric scientific dispute. Literacy and desirability was a simple causal association. Alabama Democrat John L. Burnett, who had long supported excluding illiterate immigrants, used labor unrest in the American factory towns of Youngstown, Ohio and Lawrence, Massachusetts to garner support for the provision. Exclusion based on illiteracy would have kept out the strikers, seventy per cent of whom, he claimed, were illiterate. Burnett explained in a speech to the House of Representatives that the illiterate immigrant radicals, led by a small cadre of literate immigrants, marched behind the banner of the Industrial Workers of the World, which carried the words, “No God, no law, no master.”¹⁰⁰ (He never explained how the illiterate immigrants would have been able to read such a revolutionary exhortation, but the Alabama Democrat never claimed to be consistent or coherent in his pro-restrictionist arguments.) Another Representative, John Charles McKenzie, a Republican from Illinois, likewise

¹⁰⁰ *Congressional Record*, House of Representatives, 24 March, 1916, 4473.

declared that the literacy test would be the most effective method of protecting the standard of American labor. The “nature and characteristics” of immigrants from southern European and Asian races impeded their proper assimilation – in fact assimilation of these immigrants was a “practical impossibility” because they were “lacking in comprehension of the true spirit of our institutions, [and so] can not be elevated to the true standard of American citizenship...”¹⁰¹ The Illinois Republican expressed no hesitation or uncertainty that this was indeed a “practical impossibility.” McKenzie was certain that the nature and characteristics of human beings was unchanging and hereditary and that they would never be able to assimilate successfully.

In the midst of unease about the European war, Burnett introduced in Congress another immigration bill with a literacy test in 1916. The intent of Burnett’s bill was not, he explained, in favor of “striking down the Irishman and it does not touch one of them. It does not touch the Scotchman or the Englishman or the German or the Frenchman or the Hollander or any of the Scandinavian people, or the Swiss up in the midst of God’s snowy mountains.” Burnett reassured his Congressional colleagues that these men would find, should they decide to emigrate to the United States, that “our doors are wide open.”¹⁰² The problem, the

¹⁰¹ *Ibid.*, 4777. McKenzie also feared the political stability of the country with the influence of the “hot-blooded” races with their revolutionary tendencies.

¹⁰² *Congressional Record*, House of Representatives, 25 March 1916, 4855. See also “Il ‘Burnett Bill’ nuovamente in discussione agli Stati Uniti,” *Bolletino dell’Emigrazione* v. 16, n. 3, doc. n. 821 (March 15, 1916), 46-48. Along with being informed of Congressional progress on immigration restriction through their paid Washington D.C. lobbyist and their correspondence with Representative Albert Johnson of Washington, League Secretary Prescott F. Hall also

southerner informed Congress, was that these valuable and desirable races of men would not emigrate. Burnett recounted a story that would have been immediately understood by members that were familiar with the well-rehearsed lines of General Francis Walker. While on the Immigration Commission, Burnett was one of the members who traveled to the northern European states to discuss and investigate immigration with European officials. Questioning a German official as to why Germans were not emigrating to the United States, where they would receive a hospitable welcome, Burnett was told, as he related to his Congressional colleagues, that economic prosperity was one of the reasons the Teutonic and Anglo-Saxon race stayed put. But it was also, he quoted the German official, a matter of competition: the immigrants the Germans would be competing with for jobs were Mediterranean in origin, and the laboring people of Germany ““will not stand competition with them. We do not want our families raised in such surroundings and subject to *such contamination*.””¹⁰³

The war in Europe began to significantly change the political calculus of restriction. Undesirable immigration to the United States had to be stopped, according to Burnett’s accounting. Not only was it inhibiting the reproduction of the native-born stock, it was preventing the addition of desirable Teutonic and northern races of immigrants too. Restrictionists hoped that the “replacement thesis” might help bring the older immigrant groups already in the United States

corresponded privately with Representative Burnett. E.g. Burnett to Hall, 22 July 1912, box 1, IRL papers.

¹⁰³ *Congressional Record*, House of Representatives, 25 March 1916, 4855, emphasis added.

to support restriction, and aimed specifically at the impact these undesirable “new” immigrants were having on the “old” established immigrant communities. Patten sensed opportunity. Though President Woodrow Wilson had already vetoed one immigration bill because of a literacy test in 1915, Patten communicated to the League leadership in 1917 that the possibility of American involvement in the war might give the League the upper hand. President Wilson’s second veto of the literacy clause in January 1917 was centered upon the same objections he had made almost exactly two years earlier but Patten believed that Congressional attitudes were significantly different now. Days before the country broke off official relations with Germany, Wilson informed the Congress that he had declined to endorse Burnett’s bill because of the “radical change” the policy would introduce, which he felt was “not justified in principle.” The literacy test, the President said, “was not a test of character, of quality, or of personal fitness, but would operate in most cases merely as a penalty for lack of opportunity...”¹⁰⁴ This stood in opposition to the classic racist position that the literacy clause *was* a test of racial or biological ability and desirability.

As Patten intuited, the circumstances were now different. Congress marshaled the votes to finally override a presidential veto, and passed the Immigration Act of February 5, 1917, which culminated over twenty years of agitation by the IRL and other restrictionists.¹⁰⁵ After this triumph, however,

¹⁰⁴ House Doc. 2003, 64th Congress, 2nd Session, “The Immigration Bill H.R. 10384: Message from the President of the United States” 30 January 1917, 3.

League members did not stop. The Immigration Restriction League's leaders wanted the total exclusion of non-Anglo-Saxon immigrants whether they could read or not. They wanted to keep out these new immigrants because of their belief in Walker's replacement thesis, that these new immigrants would alter the racial composition of America. They wanted to retain the racial purity—their version of racial purity—of the country.

Once Congress overrode Wilson's veto, the IRL's executive committee turned its attention to closing loopholes and working to further restrict immigration. They quickly noticed that the literacy provisions did not apply to immigrants within the Western hemisphere, so that "illiterate and other Mexicans" were now being admitted under a regulation issued by the Commissioner General of Immigration. The League's leaders began to advocate restriction on a numerical basis, recommending to its members support for a bill that Massachusetts Representative Augustus P. Gardner introduced in late 1917, which had previously been put forth by Senator Dillingham. Representative Burnett, assured of the League's continued support for restriction, wrote to Hall in 1918 and warned of a need for "some strong restrictive legislation...against the flood of immigration that will try to come into this country very soon." He supported basing restriction on the numerical basis that Hall had recommended he support.¹⁰⁶

¹⁰⁵ See Hutchinson, *Legislative History of Immigration*, 163, 167.

Numerical limitation became the next tool for the League's leaders. Burnett soon introduced a bill with this provision, which based restriction on a fixed percentage of the number of a country's naturalized citizens in the United States. League leaders hailed its proposal as the best legislative option, since "The effect of the bill will be similar to that of the reading test, in that it will discriminate in favor of immigrants from northern and western Europe, thus securing for this country aliens of kindred and homogeneous racial stocks."¹⁰⁷ Congress approved this numerical principle in the emergency Immigration Act of May 19, 1921, which limited the number of any nationality admitted to 3% of immigrants from the same country that had been naturalized as U.S. citizens as of the 1910 census.¹⁰⁸

This bill, the first quota act, represented the principal thrust of the Immigration Restriction League's goals. It nearly eliminated the arrival of immigrants who did not, in their minds, have a similar racial identity with the American population. But even with this success members of the League did not

¹⁰⁶ Patten to Lee, 25 May 1917, box 1, Lee papers; Hutchinson, *Legislative History*, 168 for Gardner bill (not reported out of committee); and John L. Burnett to Hall, 29 November 1918, box 1, IRL papers.

¹⁰⁷ "Publications of the Immigration Restriction League. No. 69: The League's Numerical Limitation Bill" n.d. Box 1, Lee papers. The bill, H.R. 11280, is briefly discussed in Hutchinson, *Legislative History*, 169. Though no action was taken, it established the fundamental ideas for future exclusion bills *not* based on ability to read. See also Kenneth Roberts, *Why Europe Leaves Home* (Brooklyn: Boobs-Merrill Company, 1922). Roberts spent 1920-21 in Europe "observing" the torrent of immigrants heading for ports of departure for America, and expressed grave concern over the number of Jewish emigrants. "An ostrich could assimilate a croquet ball or a cobble-stone with about the same ease that America assimilated her newcomers from Central and Southeastern Europe.... Their standard of living in their home countries was as low as any standard of living could possibly be." Page 4.

¹⁰⁸ Hutchinson, *Legislative History*, 176-80 for an excellent overview of the bill and its passage. This quota provision will be discussed in chapter 7.

relax. An exchange with a supporter shows the ongoing commitment of the League's leadership to maintaining the racial homogeneity of the country. Cameron Forbes had donated \$500 to support the League's activities in 1913 and \$100 in 1916. When Lee asked him for additional money in 1921, while the emergency immigration bill was being debated in Congress, Forbes explained that he was puzzled as to why the League was still fighting. Forbes was "pretty well satisfied" with the literacy test, but the "Immigration Restriction League seemed not to be." He felt that the League's leadership kept fighting for restriction because they did not know what else to do. He declined to contribute any more money to the League, though he did tell Lee that he would continue reading their literature if it was sent to him.¹⁰⁹

Lee was appalled. He could not believe that a concerned Anglo-Saxon American would simply consider the fight won, pack up, and go home just because the literacy test had finally passed. That was certainly not the intention of the Immigration Restriction League. Lee wrote to Hall, asking for his help in creating a "good answer" to Forbes. Hall's reply, which Lee forwarded to Forbes with only minor corrections, showed the extent of the IRL members' commitment to protecting white Anglo-Saxon America. Hall explained that the literacy test was only of temporary value, and the Immigration Restriction League understood it as such. "If we are to keep the institutions and habits built up by our ancestors

¹⁰⁹ Cameron Forbes to Lee, 31 March 1921, box 2, Lee papers. Forbes was a Harvard Graduate (Class of 1892) and high-ranking diplomat who served in the Philippines, eventually as Governor-General.

in this country, it is absolutely essential to limit the numbers, because the bulk of immigration is now Slavic, Semitic, and Mediterranean. Up to the Civil War our population was almost entirely Nordic. Now not more than one-half is Nordic.” Additionally, in a private reply to Lee, Hall noted that Forbes’s objection was probably due to his experience in the Philippines, where he was used to being surrounded by large numbers of “inferior peoples.” He concluded, “I don’t think any of us felt that the reading test was all we wanted or all that was needed.”¹¹⁰ The large numbers of immigrants meant that not all of them could be assimilated and Americanized properly. So limiting the volume was also crucial.

The attention of the League’s leaders also shifted to Asian sources of immigration to ensure the continuation of the policies excluding Asians from entering the United States, reflecting a broader concern over maintaining the color of the country.¹¹¹ New League member Lothrop Stoddard tackled Japanese immigration in his 1920 book, *The Rising Tide of Color*. Hall spoke of Stoddard’s excellent potential as a member of the Executive Committee of the League,

¹¹⁰ Lee to Hall, 4 April 1921; Hall to Lee, 5 April 1921, both box 2, Lee papers. The California sociologist Emory Bogardus proffered a similar argument in 1919. The numbers of immigrants had to be controlled, because they needed to be Americanized. The emergency bill proposed to “allow all who wish to do so to come from the countries of Northwestern Europe whose peoples are most akin to us...while it will reduce to manageable proportions the Jews, Greeks, Syrians, South Italians and other Southern and Eastern peoples, who tend to form separatist groups, and have a lower standard of living.” Bogardus’s opinion of Americanization was that native-born Americans needed it (including the Indians, blacks, and “Appalachian mountaineers”) as much as the immigrants did. The four fundamental points of Americanization were teaching American ideals of Liberty and Self-Reliance, Union and Co-operation, Democracy and the Square Deal, and Internationalism and Brotherhood. Emory Bogardus, *Essentials of Americanization* (Los Angeles: University of Southern California Press, 1919),

¹¹¹ Two important recent works on “color” in this period are Thomas Guglielmo, *White on Arrival: Italians, Race, Color, and Power in Chicago, 1890-1945* (Oxford: Oxford University Press, 2003); Mae Ngai, *Impossible Subjects: Illegal Aliens and the Making of Modern America* (Princeton, NJ: Princeton University Press, 2005).

describing him as “just the new blood we want. He is well up on racial questions, and he can write.” Stoddard’s book urged the United States to stand up against the “Yellow races,” and protect America’s Anglo-Saxon purity. Stoddard felt that he had divined the imperial Japanese government’s intentions from their international actions and that these intentions threatened American security and racial homogeneity. “Japan must find lands where Japanese can breed by the tens of millions if she is not to be automatically overshadowed in course of time,” he noted. “This is the secret of her aggressive foreign policy, her chronic imperialism, her extravagant dreams of conquest and ‘world-domination.’” In the early 1920s, Stoddard came to play an increasingly prominent role in the League, as his books kept concerns over immigration and race prominent in the public mind. Though they had achieved restriction of immigration, the League fortunately had men like Stoddard, who “can write,” to persuade the public to remain vigilant, while other members lobbied politicians.¹¹²

¹¹² Lothrop Stoddard, *The Rising Tide of Color Against White World-Supremacy* (New York: Charles Scribner’s Sons, 1920), 48-9; Prescott F. Hall to Joseph Lee, 15 October 1920, box 1, Lee Papers. Hall’s conference with Robert DeCourcy Ward, which was the subject of his letter to Lee, described two further strategies the League should embrace to achieve further restriction – increasing the frequency with which the League mailed short contributions to newspapers so that they could “comment on things while they are still fresh” and that the United States government “as I originally suggested when [Senator] Lodge told us to wait” should make a deal “with the Slope people, that we will go as far as they like on Japanese, if they will back our percentage bill for Europe.” Hall’s reference to “the Slope people” was apparently a witty play on anti-Asian slang, as he was referring to restrictionists in the western United States who were unconcerned with European immigration, but who consistently agitated for restrictions against Asian immigration.

Even with the passage of the heavily restrictionist Emergency Immigration Act of 1921, the League pressed for more restriction.¹¹³ Ward wrote to Lee in May 1922 to discuss strategies and a “general consensus” for an upcoming article he was writing on immigration and future legislation. Ward hinted at the possibility of introducing some type of eugenic selection of aliens, and considering mental tests for immigrants as well. In November of the same year, Patten continued informing League members of the ongoing hearings for increased restrictions on immigration in Congress, noting that H. H. Laughlin’s testimony to the House Immigration and Naturalization Committee—of which Albert Johnson was still chairman—was a “wonderful presentation,” and a “corking study of the alien inmates of public institutions.”¹¹⁴ Laughlin’s data sample was highly problematic, a point that Herbert Spencer Jennings took pains to elaborate later in the 1920s, but which did not trouble League leaders in the least.

As various restrictive mechanisms designed to curtail immigration continued coming out of Congress, Patten kept the League’s members informed of all the suggestions and policy possibilities. Their lobbyist was on top of the political discussions involving the 1890 Census as the basis for new numerical quotas, and the implications that would have for the flow of Anglo-Saxon immigrants. Patten knew which provisions would increase the percentage of immigrants allowed under the various numerical limitation schemes which were

¹¹³ The first quota provision was pocket-vetoed by President Wilson in February 1921; President Harding signed the bill in mid-May 1921. See Hutchinson, *Legislative History of Immigration*.

¹¹⁴ Patten to Ward, 21 November 1922, box 2, Lee papers.

designed to address economic demands for certain kinds of manual labor. As the House Committee was getting ready to report out a bill in 1923, Patten described it to his colleagues as a “splendid bill.” “The whole theory of the bill is scientific,” he excitedly told Lee, as the bill explained, “that we have been getting too many S.E. Europeans, and that S.E. European immigration is to be cut down as much as possible.”¹¹⁵ This association with science, and how it proved the undesirability of southern immigrants shows how effective restrictionists were at using classic racialist ideas.

The League’s base of support was very broad in the 1920s, and not all of its political influence in Washington D.C. originated with Patten. Lee sent a letter to James J. Davis, the Secretary of Labor, in September 1923 to correct what Lee believed was a misunderstanding on the part of the Secretary.¹¹⁶ Davis believed that social workers were against immigration restriction, and Lee maintained that by and large, the opposite was the case. He cited personal acquaintances, like Edward Devine (General Secretary of the Charity Organization Society of New York) who was a restrictionist, and John Moors, President of the Boston Public School Association and the Family Welfare Society of Boston, who just happened to also be titular President of the Immigration Restriction League. Lee said that he himself, who was the President of the Playground and Recreation Association of

¹¹⁵ Patten’s unaddressed memos to Lee from 18 December 1922; 5 January 1923; and 26 January 1923, all from box 2, Lee papers.

¹¹⁶ James Davis was the author *Selective Immigration* (St. Paul, MN: Scott-Mitchell Publishing Co., 1925).

America, had “always been a restrictionist.” But Lee also noted, “Of course, the Jews, social workers or otherwise, are against us.” In mid-October the Boston philanthropist wrote to Speaker of the House Frederick Gillett to oppose the appointment of Fiorello LaGuardia to the House Immigration Committee because LaGuardia was sympathetic to immigration of the wrong type.¹¹⁷

Members of the Immigration Restriction League also participated in a number of outside concerns, most of which also added to their political influence. Throughout the course of the restrictionist movement, members approached labor organizations to join with them in excluding immigrants. They also turned to scientific interests to advance restriction. Hall was active on the Immigration Committee of the American Breeder’s Association. League members maintained correspondence with Charles Davenport in the 1920s, and their connections with other organizations continued to expand.¹¹⁸

Lee became involved with organizations supporting birth control because he saw it as a method of demographic control: “We want birth control taught to the non-Nordics in the country to put them on an equal reproductive basis with the Nordics. Yet against the brown and yellow races, we need all the white stock we can get, even if some of it is second rate.” The leaders of the League made a concerted effort in 1922 to garner the support of women’s clubs, because “ladies are going to run this country during the next few years” (or at least until they got

¹¹⁷ Lee to James J. Davis, 19 September 1923; Lee to Frederick Gillett, 18 October 1923, both box 2, Lee Papers.

¹¹⁸ E.g. Lee to John Golden [President of the United Textile Workers of America], 18 June 1912, box 1, Lee papers; Charles B. Davenport to Prescott Hall, 28 September 1920, box 1, IRL papers.

absorbed in “regular politics like men”).¹¹⁹ In 1925, Lee contributed \$500 to the International Migration Service because they helped immigrants move to better living arrangements, but also diminished the “output of sob stuff, which is the principal weapon of those who want infinite cheap labor.”¹²⁰ Not all outside affiliations were constructive. Patten’s involvement with the Junior Order of United American Mechanics and the World’s Purity Foundation nearly caused his removal as the group’s legislative lobbyist in Washington D.C. His affiliation with the Farmer’s National Congress, and his commission by the Secretary of Labor to investigate conditions in Hawaii, Guam, Japan, China and the Philippines in 1923 were more palatable.¹²¹

These affiliations demonstrate the activism of members of the League, and their efforts to fight for restriction on multiple fronts. It also aided their efforts at

¹¹⁹ Hall to Lee, 8 April 1921 [on the Birth Control League of Massachusetts], box 2, Lee papers; Lee to Patten, 29 November, 1922, box 2, Lee papers; Lee to Emma Knight [member of the Women’s Municipal League of Boston], 10 February, 1923, box 2, Lee papers; Lee to Mrs. Tilton, 1 February, 1923, box 2, Lee papers. Birth control sustained Lee’s interest. In 1935, writing to Robert Paine, a member of the IRL’s Executive Committee, Lee said, “Pretty nearly ever since I can remember I have been in favor of birth control as well as any other method of lessening the number of undesirable people coming into this country, and I hope the Immigration Restriction League will soon be able to take hold of this new way of tackling the problem.” Lee to Robert Paine, 15 July, 1935, box 2, Lee papers.

¹²⁰ Lee to Miss Hurlbutt [of the International Migration Service], 8 December, 1925, box 2, Lee papers.

¹²¹ *Who Was Who in America*, v. 1 (Chicago: A.N. Marquis Co., 1943). Hall and Ward published numerous articles relating to restriction throughout the course of the 1920s. For instance, see Robert DeCourcy Ward, “The New Immigration Law and Its Operation” *Scientific Monthly* Vol. 21, no. 1 (July 1925), 45-53; Robert DeC. Ward, “Higher Mental and Physical Standards for Immigrants” *Scientific Monthly*, v. 19, n. 5 (Nov., 1924), 533-47; Robert DeCourcy Ward, “Some Thoughts on Immigration Restriction” *Scientific Monthly* v. 15, n. 4, (Oct. 1922), 313-19; Prescott F. Hall, *Immigration and Other Interests of Prescott Farnsworth Hall* compiled by Mrs. Prescott F. Hall (New York: Knickerbocker Press, 1922), among others. As its secretary, Hall also penned the vast majority of the Immigration Restriction League’s publications.

political pressure. By cooperating with groups that had similar interests, or through their own membership in them, League members used every conceivable avenue to advance restriction. Though not all of these groups demanded a large time commitment, the core of the Immigration Restriction League was extraordinarily active. As Madison Grant said of Hall after his death in 1921, Hall supported restriction tirelessly because he deeply believed in it. “To the day of his death [Hall] asked for nothing for himself and everything for his country.... If America goes wrong and allows our native Nordic stock to be replaced by half Asiatic mongrels the hope of the world is gone. This issue Prescott Hall saw and saw with the vision of a prophet a full generation ahead of his countrymen.... I know of no one of our generation to whom America owes so much.”¹²² In this regard, the scientific underpinnings of the movement, and the influence of classic racialism played a key role in convincing League members that they were working toward good ends. It helped them believe that they were acting upon

¹²² Madison Grant, foreword to Hall’s *Immigration and Other Interests*, x. League correspondence shows several references to the ill health of members, some physical effects of strain, some mental and psychic strains. Richards Bradley, Lee, Ward, and Hall were born in 1861, 1862, 1867, and 1868, respectively. Other members of the League were born around the same time: Madison Grant, 1865, John F. Moors, 1861, Charles Warren, 1868, Thornton Cooke, 1873. James Patten was the youngest of the group, born in Kansas in 1877. In a collection of his writings published in 1922, shortly after he died, Hall’s wife noted that he was an insomniac, and had grown up as a “frail little hothouse plant.” On his death, his doctor noted that “in him was merged the analytical mind in a supersensitive body.” Similarly, in a memorial published shortly after Ward’s death in *Scientific Monthly*, Robert T. Jackson described him as having “a sensitive, nervous, highstrung temperament.” Hall, *Immigration and Other Interests*, 119, xviii; Robert Jackson Taylor, “Robert DeCourcy Ward,” *The Scientific Monthly*, February 1932, 192-94; on Madison Grant, Fairfield Osborn, “Madison Grant” *Dictionary of American Biography* supplement 2, (New York: Charles Scribner’s Sons, 1958). Interestingly, for all Grant’s lamentations of the inability of the Nordic Race to reproduce, Grant never married. He was the son of a Congressional Medal of Honor winner in the Civil War, and his mother was from a wealthy family from Jamaica, Long Island, N.Y. Along with founding the N.Y. Zoological Society, Grant was also President of the Bronx Parkway Commission, and with Henry Fairfield Osborn (the Director of the American Museum of Natural History) founded the Save the Redwoods League in 1919. Spiro, “The Patrician Racist”; idem., *Defending the Master Race*.

scientific principles, not prejudice or discrimination. It was wholly irrelevant if the science was actually wrong.

The importance of their continued activism and vigilance was clear. But equally clear for the IRL elite was the importance of avoiding unsavory entanglements and keeping restriction predicated upon a “scientific” basis. Richards Bradley wrote to Patten in October 1922 that he was concerned about groups like the Ku Klux Klan may ruin support for restriction. “Not many years ago we had the Know Nothing movement, which died out after being too intense,” Bradley explained, “and came to nothing, but is always used by the anti-restrictionists as an awful example of restriction advocacy. Later we had the [American Protective Association]...and now we have the K.K.K. likely to do the same things.”¹²³ By having such groups side with the Immigration Restriction League, League leaders feared the “noble” purpose of the organization might be tainted as reactionary or racist. Thus it was important to carefully guard against racist rhetoric and attitudes, and to be moderate in their choice of written or spoken language. The scientific language and epistemology of restriction was a critical tool for that. Thus, in the mid- to late-1920s, many of the League’s core members shook off their “offensive” strategy and focused on convincing the public of the importance of preserving restriction. Madison Grant’s collections of essays, *The Founding Fathers of the Republic on Immigration, Naturalization,*

¹²³ Richards Bradley to Patten, 11 October 1922, box 2, Lee papers.

and Aliens and *The Alien in Our Midst* attempted to ground restriction in the discourses of the Founding Fathers and early social leaders.¹²⁴

The education and social status of these men gave them powerful tools to express their racial ideologies in discrete and subtle ways. Their nativism was not the same kind as that expressed by other groups, a point that becomes clear when read against Donald Kinzer's history of the American Protective Association.¹²⁵ The League's sophisticated racist epistemology gave them an edge in lobbying for Congressional legislation that they deemed necessary to protect Anglo-Saxon America from the invasion of inferior, crime-prone, feeble-minded hordes of non-Nordic European aliens.

The members of the Immigration Restriction League felt very deeply about their cause. They were middle and upper class elites who saw a dramatic change in society in the course of their lives. They experienced nervous tension and physical dis-ease partly as a function of their involvement in trying to stop the flow of undesirable immigrants into the United States.¹²⁶ As well-educated and

¹²⁴ "Immigration Restriction League, Annual Report of the Executive Committee for 1925"; Transcript of "Our Immigration Problem", delivered by Patten on 27 October 1927, both box 2, Lee papers; Madison Grant and Charles Stewart Davison, eds. *The Founding Fathers on Immigration, Naturalization, and Aliens* (New York: Charles Scribner's Sons, 1928); Grant, *The Alien in Our Midst*.

¹²⁵ Donald Kinzer, *An Episode in Anti-Catholicism: The American Protective Association* (Seattle: University of Washington Press, 1964).

¹²⁶ The internal correspondence of the League and the Executive Committee reports clearly show that legislative victories were not what stopped League activism. The old stock identity was too precious to these men to think that immigrant restriction itself would preserve it. Their continued activism after 1917, 1921, and 1924 belie the standard narrative that with legislation, groups like the League gave up and enjoyed their success. It was ultimately physical attrition that stopped the League. The generation cohort that made up its core membership simply began to die off—Ward in 1931, Lee in 1937, Patten in 1940, and Richards M. Bradley in 1943.

well-connected men, they used all the tools at their disposal to affect a dramatic change in American immigration policy.¹²⁷ For over twenty years, they had advocated restricting immigration to literate immigrants because in their minds basic reading skills were an indicator of racial quality. It was not, however, the sole indicator. As the literacy clause took effect, they knew it would not limit numbers to the extent that they had hoped. For this purpose, the 1921 bill, limiting the total number of undesirable immigrants, represented a good starting point in their minds.

Continued action was of paramount importance for League members because the first numerical quota provisions were designed to be temporary; a May 1922 bill extended it to 1924. The Johnson-Reed Act superseded it in 1924 (the “National Origins Act”), which the League’s annual report described as “the end of one epoch and the beginning of another.” In 1924 they argued that the new law must be defended against repeal, and Congressional advocates must be supported. “Your Committee does not believe in resting on the ground already gained. The enemy is still very much alive, and we hope for your support in

¹²⁷ Without careful attention to the nuances and distinctions of immigration restriction and the science of eugenics, one risks collapsing an entire ideology into a monolithic and static movement. The most egregious example of this type of over-simplification is Allan Chase’s 600 page *The Legacy of Malthus: The Social Costs of the New Scientific Racism* (New York: Alfred A. Knopf, 1980). Chase accuses English political economist Robert Malthus of being the founder of over 200 years of scientific “racists” and “quacks,” whose “useless nostrums” shows an absolute disregard for true Knowledge. Malthus’s *Essay on the Principle of Population* Chase claims, was actually *not* about demography, but was rather a tract favoring the racial status quo. Though it was Francis Galton—a “wealthy Victorian dilettante” who did not understand that “correlation is not causation”—that actually described many of the principles of eugenics, that does not seem to matter to Chase. Karl Marx, Francis Galton, Harold Laski, Justice Oliver Wendell Holmes “were united in their contempt for the humanity and the gonads of the wretched of the earth,” (316) which led to the “Genocidal 1924 Immigration Quotas.”

seeing the thing through.”¹²⁸ They looked to science as an essential tool for encouraging people to “see the thing through.”

¹²⁸ “To the Members of the Immigration Restriction League” June 14, 1924, box 2, Lee papers.
Chapter 4

PART II: THE UNCERTAINTIES OF SCIENTIFIC CERTAINTY

The four chapters in Part II examine specific disputes between practitioners of the methods of scientific investigation of classic racialism and the new forms of laboratory and experimental research. Chapter 5 analyzes the ways that mental and intelligence tests, which were only in their infancy, were used as empirical evidence of racial inferiority of immigrant groups, and how they were impacting the intellectual quality and character of American society. The influence of classic racialists (especially Harry H. Laughlin) on Congress is an important aspect of the ways these imperfect tests were used to generate political support for changes in immigration policy.

Chapter 6 details how the slow, gradual search for a mechanism of natural selection by professional scientists enabled many non-academic researchers to make sweeping – and erroneous – claims about the processes of evolution and degeneration to the public. The popular propaganda of non-scientific writers like Albert Wiggam was important in shaping the understanding and perceptions of the public and their representatives in Congress.

Chapter 7 deals specifically with the difficulties of modifying American immigration policy to the basis of National Origins. This problematic method of exclusion, which was the signal feature of the 1924 Immigration Act, generated significant dissent from professional biologists and statisticians who believed that computing quotas on the basis of the racial composition of the American population in 1790 was impossible. Yet these were the immigration quotas that went into effect in 1929 and remained American policy until the passage of the Immigration and Naturalization Act of 1965.

The final chapter of the dissertation studies several debates within professional scientific communities in the 1920s, arguing that with a lack of consensus even among professional researchers (particularly in the field of physical anthropology) severely limited the effectiveness of scientific critiques of classic racist principles. Chapter 8 considers some of the complexities of “scientific revolutions” and paradigmatic shifts.

The conclusion takes the restrictionist movement forward into the 1930s, demonstrating that the coalition in support of immigration restriction, led by the IRL, did not disband after the implementation of the National Origins quotas in 1929, but continued working throughout the decade to close policy loopholes and maintain the white, Anglo-Saxon character of the American population.

Chapter 5: The Inheritance of Intelligence

In 1906, Herbert Henry Goddard, a native of Maine with roots to the Puritan settlers of New England, arrived at the Vineland Training School for Feeble-Minded Boys and Girls in New Jersey. Having trained with the American psychologist G. Stanley Hall at Clark University – Goddard wrote a dissertation analyzing the scientific basis for “faith cures” of hypnotism and the power of suggestion – Goddard began to introduce a system of intelligence testing to establish the mental ability and age of the residents of the facility. Hoping to establish the efficacy and utility of mental testing, his biographer notes, “Goddard would become America’s most avid convert to [Alfred] Binet’s ‘psychological method’ of classifying the feeble-minded. From his position at Vineland, he would also be able to explain his new practices to institutional physicians.”¹ Despite Goddard’s contribution to the establishment of mental testing in the United States, the key contribution the new Vineland director made was not to demonstrate *how* intelligence could be measured, but instead was to allegedly demonstrate its heredity. Goddard’s discovery of one particular feeble-minded young woman at Vineland provided an essential foundation for the dominance of classic racist thought in the United States.

Goddard argued for the hereditary basis of feeble-mindedness in his book *The Kallikak Family*, published in 1912. Deborah Kallikak, a pseudonymous

¹ Zenderland, *Measuring Minds*, 98; biographical information in chapters 1 and 2. See also J. David Smith, *Minds Made Feeble: The Myth and Legacy of the Kallikaks* (Rockville, MD: Aspen Systems Corp., 1985); Paul, *Controlling Human Heredity*; for G. Stanley Hall, see Bederman, *Manliness and Civilization*.

resident at the training center, illustrated for Goddard a few simple yet essential truths. She manifested how decisively heredity contributed to the perpetuation of feeble-minded offspring. Goddard investigated Deborah's family background much like Galton had done with his Englishmen, viz., by inquiring into the mental conditions of Deborah's parents and grandparents. What he found was that Deborah's ancestors were disproportionately feeble-minded. Goddard believed that she therefore provided proof that a feeble-minded parent would always create feeble-minded children; like would produce like, and Deborah's mental defects could never be overcome. She exemplified the need for eugenics, for had her parents not reproduced, they would not have contributed this burden to the state of New Jersey. Deborah also demonstrated how important experts were in recognizing what Goddard called, "the unmistakable look of the feeble-minded," the vast majority of whom "are persons who would not be recognized as such by the untrained observer."² In her pedigree Deborah represented a critical danger to the United States, and Goddard meant to use her as an example of how to stop it.

Her biography was essential in demonstrating what Goddard believed was the Mendelian inheritance of feeble-mindedness. Using an elaborate system of charts that graphically represented Deborah's ancestry – the genealogical research into Deborah's background was performed by Elizabeth Kite, a field worker that Goddard employed to trace Deborah's lineage through interviews with neighbors of Deborah's ancestors – Goddard traced to a Revolutionary War soldier, Martin

² Henry Herbert Goddard, *The Kallikak Family: A Study in the Heredity of Feeble-Mindedness* (New York: MacMillan Co., 1912), 77 for look; 104 for observer.

Kallikak, the origin of an entire family tree of feeble-minded, criminally inclined, and socially inadequate people that culminated in Deborah. Martin had had a brief liaison with a nameless feeble-minded girl who bore him an illegitimate son, who was named for his father. Martin Sr. later married a proper, upright female, and with her had a long line of legitimate descendants in which there was, Goddard wrote, no trace of degeneracy or feeble-mindedness. But from this first union, Goddard and his field worker traced 480 direct descendants, for 143 of whom he had “conclusive proof” of hereditary feeble-mindedness. These direct descendants in turn married into other families, and produced an additional 1146 offspring, of whom 246 were feeble-minded. Deborah, who had arrived at Vineland at age 8, had shown no capacity to improve her mental ability. Photographs that he included of Deborah and her ancestors – doctored to accent their threatening defectiveness – functioned in Goddard’s mind, along with this complex genealogical history, to prove his conclusion that “all this degeneracy has come as the result of the defective mentality and bad blood having been brought into the normal family, first from the nameless feeble-minded girl and later by additional contaminations from other sources.” Goddard’s study became wildly popular and persuasive in terms of rationalizing a greater control over the reproduction of these mental defectives, at the same time that it helped propel the legitimacy of restrictionist’s literacy tests and examinations of mental ability. It was also entirely wrong.³

³ Goddard, *The Kallikak Family*, quote 69; family data of Martin Sr. and Martin Jr. from 18-20; charts take up pages 33-50. On the doctoring of the photographs of Deborah and her ancestors, see William Grazter, *The Undergrowth of Science: Delusion, Self-Deception, and Human Frailty*

It was not only the physical or biological aspects of immigrants that made them seem threatening to nativist-minded Anglo-Saxons. While a trained medical expert might be able to detect diseases or abnormalities of immigrants at medical inspection stations like Ellis Island, not every disability or undesirable quality was easily perceived. Their mental characteristics also made them undesirable – their inability to read demonstrated their mental inferiority, which was now taken to be hereditary. As the Kallikaks showed, however, not all undesirable population groups were alien. Mental testing and psychological exams came of age during World War I when millions of American draftees were required to take mental exams to determine their best and most effective function for the Army. These tests provided an additional metric for assessing the desirability of immigrant groups, as native-born Americans performed much better on the tests than immigrants. As Galton's work with the inheritance of talent demonstrated, though, the scientific quality of mental testing remained suspect in the immediate post-war period.

(Oxford: Oxford University Press, 2000), 288. Gratzner also makes the link with the “propensity towards criminal or other antisocial behavior...[which] dated back to the writings of Cesare Lombroso.” 288; also, Gould, *The Mismeasure of Man*, esp. chapter 5 where Gould notes that Goddard's work on associating immigrant races and their intelligence, which followed from his Kallikak studies, were “little more than guesswork rooted in conclusions set from the start.” On 188. Zenderland, *Measuring Minds* is similarly critical of Goddard's research, and Diane Paul explains that the “real menace” of the feeble-minded was the heterozygous Mendelian population that carried the genes but did not manifest the feeble-minded mental defects. They lurked instead as silent vectors for future generations. See Paul, *Human Heredity*, chapter 4, esp. 68-70. Nicole Hahn Rafter suggests that Goddard's contribution to the heredity of the feeble-minded was to supplant Lombroso's criminal anthropology of the born criminal a psychological definition of the moron and criminal. See Nicole Hahn Rafter, *Creating Born Criminals* (Urbana, IL: University of Illinois Press, 1997), 137. Smith points out two key aspects of Deborah's story: she most likely had a basic learning disability, and was not feeble-minded; and that the lure of Goddard's Kallikak family study was that it provided a “*simplistic explanation* that social ills like poverty, prostitution, crime, and alcoholism were the result of feeble-mindedness—specifically the high-grade, moron type—was *appealing to the spirit of the time*.” Smith, *Minds Made Feeble*, disability from 23; quote from 61, emphasis added.

There were competing agendas and plans for restriction that complicated the movement's goals for protecting the American people; there was not a monolithic nativist movement. Some restrictionists favored using mental tests on immigrants as another mechanism of restriction, believing that it would provide further proof of the undesirability of certain "races" of immigrants. Others began to favor a process of biological or eugenic selection of immigrants that investigated the genealogical and racial background of immigrants so that only Nordic or Alpine races were admitted, in their minds thus preserving America's racial homogeneity. Despite these differences, however, the restrictionist movement in the 1920s began to use effective tactics to achieve its aims. Nativists utilized evidence from the army mental tests to "prove" the intellectual inferiority of immigrants, and made their findings available to wide segments of the public. They combined the infant science of psychology with a popularizing thrust that described the dangers of hereditary inferiority, exemplified by the Kallikaks and the undesirable immigrants from southern and eastern Europe. The assertions in favor of restriction that were based on the "restrictionist epistemology" of classic racialism began to take on the weight of "scientific" fact. But these assertions in the 1920s were increasingly refuted by professional scientists and academics who demanded a much higher burden of proof and accuracy than Goddard's field workers could provide, or that haphazardly administered mental tests to army recruits would generate.

While the restrictionist movement disagreed on some strategies, they were united in the way they perceived the threat. Along with the perceived racial

degradation that unrestricted immigration created was the fear of the mental degeneration of the country caused by immigrants whose psychological temperament and mental ability made successful assimilation impossible. The racial and mental dangers often worked hand in hand for restrictionist's arguments, and it is worth considering the ways in which early psychological testing was also used as a justification for restriction. In this activity, Davenport and the Eugenics Record Office were again in the front lines of research, and many of his flawed assumptions about heredity meshed with his flawed assumptions about mental ability. But despite increasingly public criticism of the quality and standards of his work – which Heron, Galloway and Pearson had already demonstrated – these flaws had little real negative impact on the public's reception of his work.

The ERO's "Trait Book," which Heron had ridiculed in his *New York Times* review in 1913, was one of many products of research that coupled heredity and mental ability in the early twentieth-century. The "Trait Book" was designed to give eugenic field workers a precise vocabulary to classify physical, mental, and social attributes in families, thus providing living "breeding experiments in which the true units reveal themselves as relatively, if not absolutely, constant, unalterable, indivisible things."⁴ Francis Galton, Cesare Lombroso, Henry Cabot Lodge, and many others had for years been making facile associations between heredity and "intelligence"; Davenport would make his own contribution in 1921

⁴ Charles B. Davenport, "The Trait Book," *Bulletin no. 6 of the Eugenics Record Office* (Cold Spring Harbor, NY: 1912), 1.

with *Naval Officers: Their Heredity and Development*.⁵ The “Trait Book” was a guide for mostly female field workers with little academic or experimental training in biology, to help them register attributes that could then be tabulated and correlated at Cold Spring Harbor. Coming up with succinct descriptions and symptoms was challenging, however. Describing the physical traits and occupations of individuals was relatively straightforward – though certainly some of the evaluated physical traits like “Physical Beauty” were highly subjective – but describing mental attributes was much more difficult. For the section of the brief book dealing with mental traits, Davenport modified recommendations from E. L. Thorndike and Robert M. Yerkes, two men closely involved with the emerging field of psychological testing.⁶ Being able to classify the mental ability of individuals and groups was critical for restrictionists and classic racialists. Davenport’s association with Goddard had frequently impressed on Davenport the importance of being able to precisely calculate the likelihood of mental inferiority developing in offspring. Additionally, federal immigration policy already excluded immigrants with certain physical diseases – if more mental diseases could be added and their detection accurately improved, the country’s racial quality and characteristics could be that much better protected.

Psychological experts believed that a professional could immediately and easily spot some mental deficiency, just as Goddard predicted. But most mental characteristics were not physically apparent, and they were impossible to catalog

⁵ Charles B. Davenport, *Naval Officers: Their Heredity and Development*.

⁶ See, for instance, the essays in Michael Sokal, ed., *Psychological Testing and American Society, 1890-1930* (New Brunswick: Rutgers University Press, 1987); Zenderland, *Measuring Minds*.

without some master list. Such was the purpose of the “Trait Book.” Turning to Yerkes for assistance was an obvious solution for Davenport, since Yerkes had many of the same hereditary assumptions that Davenport held, and as he had expressed to Davenport, was “keenly interested in psychic heredity.”⁷ Davenport’s hope was that a precise terminology would help illuminate the hereditary transmission of specific mental characteristics, and he enlisted Yerkes’s expertise in devising a list.

The list of mental traits Yerkes and Thorndike provided for Davenport was subdivided in several broad categories. The general categories were then further broken down, so, for instance, Intellectual Faculties examined General Mental Ability, Imagination, Reasoning and Logicalness, Sense-Perception and Organization. Field workers recorded on an index card characteristics like “retentiveness,” “logicalness,” “suggestiveness,” which were then collated and analyzed at Cold Spring Harbor for a hereditary basis.⁸ As might be imagined, measuring mental traits regarding an individual’s “Feelings” were quite difficult to describe, and considering the conditions under which such questions were

⁷ Robert Yerkes to Davenport, 17 July 1911, Davenport Papers. Around the turn of the century Davenport had introduced Yerkes to Galton’s work on eugenics and inheritance of certain “traits” in distinguished families. The two corresponded in the late summer and early fall of 1911 about the best way to disseminate knowledge; see in addition to above, Davenport to Yerkes 22 July 1911; Yerkes to Davenport 16 September 1911; Davenport to Yerkes 18 September 1911; Davenport to Yerkes 25 September 1911, all in Davenport papers. Yerkes was a former student of Davenport’s, and had conducted research at Harvard’s Psychological Laboratory in the late nineteenth century. See Davenport to Yerkes, 1 December 1899; Davenport to Yerkes, 11 December 1900, both in Robert M. Yerkes Papers, Professional Correspondence, Yale University Archives, Yale University, New Haven, CT [hereafter Yerkes papers].

⁸ Davenport, “The Trait Book,” 22. See also Garland Allen, “The Eugenics Record Office at Cold Spring Harbor, 1910-1940: An Essay in Institutional History” *Osiris* 2nd series, v. 2, (1986), 225-64.

asked, their reliability should have been immediately suspect to any careful observer or empirical scientist.

Instructions for measuring “Feelings,” under the broad category of “General Tastes,” presented the fieldworker with a list of oppositional binaries: “detail vs. generalization,” “excitement vs. quiet,” “knowledge vs. lack of intellectual interests,” “truth vs. deception,” and “ludicrousness vs. absence of sense of humor.”⁹ The sequence following “Special Pleasures or Passions” featured easily recognizable attributes like the use of tobacco, narcotics, alcohol or stimulants, but also listed obscure traits like “Getting Money,” “Hoarding Money,” “Power, social,” and “Sex Indulgence” without providing any real definition.¹⁰ The entire list of mental attributes was, in reality, a seemingly random compilation of behavioral attitudes that had little observable basis. Considering Davenport’s position on the longevity of female field workers, the women sent out to gather the information frequently lacked experience in assessing personality and behavioral traits, making their evaluations of subjects intensely problematic.¹¹ How could they accurately determine an individual’s “Anger (liability to) vs. unruffledness,” “boldness vs. timidity,” their love of action, approbation, sympathy, or power, their “envy vs. unenviousness,”

⁹ Davenport, “The Trait Book,” 23.

¹⁰ Under Money, coded 4244, was listed Getting (42442) and Hoarding (42444). Davenport, “The Trait Book,” 24.

¹¹ Davenport explained the policy to Goddard: “We regard our appointments as temporary and have adopted the general principle that we shall employ a field worker for only three years...in the hope that she will marry...[and the ERO] can not be charged with working cacogenically...” Davenport to Goddard, 29 July 1912, Davenport papers. There was a profound irony that Davenport never had children of his own.

“jealousness vs. unjealousness,” “trustfulness vs. suspiciousness,” or their “philoprogenitiveness”?¹² These “traits” were based purely on speculation and subject responses.

Yet like Galton, the Eugenics Record Office regarded these records and replies to be indicators of hereditary mental ability, and thus an effective way to biologically rank inherited mental characteristics. Other behavioral attributes were carefully indexed to aid the field workers, including opposites like “gracefulness vs. awkwardness,” “stereotype vs. variableness,” as well as variations in quality and degree of concentration, apprehension, observation, imitation, and suggestibility.¹³ Characteristics related to egotism and temperament, self-assertion, altruism, ethics, sexuality, and speech were all carefully named, listed, categorized and indexed for ease of classification. The instructions laid out to field workers in the May 1911 bulletin “The Study of Human Heredity” gave explicit instructions for conducting interviews of willing family members, and give a real sense of the problematic nature of the data gathered. There was no chemical testing; there were no long periods of observation of familial groups as there would be in anthropology or sociology. Recording and cataloguing family traits was conducted by oral conversations with not-disinterested persons (both the interviewer and interviewee), with the interviewer operating under the goal “to establishing a feeling between the family and Institution that will assure [the field

¹² *Ibid.*, 24-5. Philoprogenitiveness would relate to an individual’s affect toward its offspring.

¹³ *Ibid.*, 24-25. The subdivision “Movements in Relation to Productiveness” in the subheading “Movement: Behavior” supposed inherited characters leading to indolence, vacillation, tardiness, procrastination, rashness, “coolness in emergency vs. loss of head,” “impracticalness,” “definiteness of purpose vs. desultoriness,” fickleness and slovenliness. *Ibid.*, 25-26.

worker] of a welcome at any time...and to this end she sacrifices minor details that would naturally come on return visits.”¹⁴

At this time, human understanding of the mechanisms of inheritance was dim at best. Confusion over terminology, inadequate methods for gathering data, and unwieldy statistical tools of association and deviation made even the cautious study of heredity problematic. In this context, it is difficult to ridicule the efforts that Davenport and his colleagues made to advance the study of genetics by recording and analyzing extended familial lines for manifestations of behavioral, physical, mental or moral similarity. Yet when one considers the implications that these studies of mental behavior had for thousands of individuals, the unwavering faith that Davenport and others put in their strict Mendelian determinism, and the way they “used” their knowledge of heredity, their efforts take on a significance more tragic than comic. There was never a sense of cautiousness, hesitation or critical examination of the results. There was only a tremendously misplaced certainty.

In the realm of mental testing, one of the more significant of Davenport’s associates was the Director of the Vineland Training School. In their early interactions, Goddard turned to Davenport for instruction on heredity and eugenics. Both men perceived a danger to the racial character of the United States if mentally inferior individuals reproduced and passed their afflictions on to future generations; both also tended to support classic racist axioms. Davenport’s

¹⁴ Davenport et al, *The Study of Human Heredity*, 2.

certainty in the Mendelian patterns of inheritance assured Goddard that a eugenic solution to the problem was possible. And although Goddard confessed “much more zeal than knowledge in this field,” he quickly made up ground, aided by his discovery of the notorious Kallikak family of New Jersey.¹⁵

While Richard Dugdale’s Jukes family was the first shocking manifestation of the inheritance of delinquency and degeneration in the United States, it was only in the first decades of the twentieth century that public awareness of the inheritance of feeble-mindedness began to grow rapidly.¹⁶ The Kallikak discovery provided what appeared to be an extraordinary example of the inheritance of feeble-mindedness. The family was a perfect example for the utility of eugenics. It was important then, that Goddard’s case study of the inheritance of feeble-mindedness come to the public carefully, in an unimpeachable way.¹⁷ Goddard had to be extremely careful to not, as he put it, “queer” the country against eugenics by exaggerating his findings with sloppy analysis. He turned to Davenport for advice on how to handle the publicity regarding the study. Their private correspondence on this matter is worth examining closely, as it shows

¹⁵ Goddard to Davenport, 18 July 1909, Davenport papers.

¹⁶ Richard Dugdale, *The Jukes: A Study in Crime, Pauperism, Disease and Heredity* (New York: G. P. Putnam’s Sons, 1877). Also Christine Rosen, *Preaching Eugenics: Religious Leaders and the American Eugenics Movement* (New York: Oxford University Press, 2004).

¹⁷ When David Starr Jordan asked Goddard to take a place on the Committee on Feeble-Mindedness of the Eugenics Committee of the American Breeders Association in 1909, Davenport and Goddard discussed the importance of issuing a quick preliminary report “concerning our knowledge of inheritance of feeble-mindedness...” Davenport to Goddard 9 July 1909; Davenport to Goddard, 24 May 1909; Goddard to Davenport 27 May 1909; Davenport to Goddard, 26 October 1909, all in Davenport papers. In the October letter, Davenport told Goddard that a preliminary report, which they would ensure would “show the importance of blood in cases of defectives, delinquents and diseased...to the Sage Foundation and make a request which the Trustees of that Foundation will find it difficult to refuse.”

private hesitations about a movement for which they showed no doubts or uncertainty in public.

On Wednesday April 13, 1910, with his research on the family underway, Goddard wrote Davenport asking about the advisability of publishing family charts on the feeble-mindedness of the Kallikaks and their heredity in a popular magazine. While the increased visibility of the early results of his hereditary study of the family could generate public support for mental testing, Goddard worried that the content could be manipulated or exploited by a writer hoping to gain a type of “shock” value from the story. By bowdlerizing the scientific content of the charts, the Vineland superintendent worried that a story by an unscrupulous writer “might prove greatly to the disadvantage of the world,” and “‘queer’ the whole country...in regard to this whole matter of Eugenics.” He felt it most important to protect the integrity of future investigations, whether they were conducted by Goddard himself, or by Davenport and his field workers at the ERO. He added a hand-written post-script noting that despite his inclination to withhold the letter – “it seems to me a needless worry” – he would send it anyway, as Davenport’s answer would reassure him that they were doing the right thing.¹⁸ Instead of reassuring him that publishing the information was advisable, Davenport wrote of the tremendous perils that eugenicists faced in popularizing the movement, along

¹⁸ Goddard to Davenport, 13 April 1910, Davenport papers.

with a basic uncertainty of the hereditary basis of mental ability that they did not often make in public.¹⁹

The importance of spreading public knowledge about eugenics was essential, but managing its dissemination was even more central to the success of the movement. “There are scores of persons in the newspaper offices with the power and many of them I regret to say who would not scruple to use their power to damage any good cause,” Davenport wrote. The risk of serious damage to “our whole work” was very significant, and although there was “increasing popular appreciation of the true worthlessness of such [critical] writings...the great uneducated masses are still fooled,” and the possibility of “queering” the country against eugenics was very real. But the eugenicist firmly believed that “our duty as men of science [is] to steer our course perfectly direct” and publish the charts. “There is no use in collecting facts unless they can be put before at least the scientific public to form the basis of eventual action.”²⁰ This tension between controlling information about eugenics while simultaneously trying to educate the public about it plagued the movement throughout its history, and the precise definition of who constituted the “scientific public” was never clear.

In July, Goddard and Davenport again debated the virtues of teasing out information about Goddard’s dysgenic discovery, this time at a child welfare

¹⁹ They did acknowledge uncertainty as to the mechanism of inheritance – Goddard explained in *The Kallikak Family* that “We are still ignorant of the exact laws of inheritance. Just how mental characteristics are transmitted from parent to child is not yet definitely known.” – but he was relatively convinced that it was a basic Mendelian unit character, and he spent several pages after expressing that caution describing how it followed the laws of dominant and recessive inheritance. Goddard, *The Kallikak Family*, quote from 109; examination of Mendelian nature 110ff.

²⁰ Davenport to Goddard, 15 April 1910, Davenport papers.

conference that was being organized in New York City for November 1910. Davenport requested a photograph of an “imbecile with extreme features or possibly a microcephalic idiot” to use as the icon for imbecility in family pedigrees for an exhibit for a mental health organization. Goddard demurred, and his negative reply showed the efforts that eugenicists were making to impress on the public the horrifying truth that “the most dangerous children in a community are those that look entirely normal,” and were not like Lombroso’s stigmata-scarred subjects. Goddard told Davenport that instead of using a photo of an obviously feeble-minded person, the movement’s goals would be better served by using the photograph of a “fine looking, normal appearing boy or girl and lay the emphasis on the fact that they are really feeble-minded and incapable of taking care of themselves.”²¹ But this did not convince Davenport. The importance of the work lay in eugenics’ ability to “bring home to the American people *and their legislators* the importance of heredity as a main source of the stream of degenerates, defectives and delinquents.” The public’s understanding at present, he lamented was that delinquency and defectiveness were products of environment.²² Visually representing the dangers, Davenport believed, would add urgency to the movement and assist the public’s direct association between heredity and feeble-mindedness.

²¹ Davenport to Goddard, 1 July 1910; Goddard to Davenport, 4 July 1910, both Davenport papers.

²² Davenport to Goddard, 15 September 1910, Davenport papers. Emphasis added.

Deborah Kallikak perfectly portrayed Goddard's point about feeble-minded "passing." In most of the photographs of the book once it was published, she appeared as a perfectly normal looking person. Deborah arrived at Vineland when she was 8 years old, the daughter of a woman who had several children by different fathers. Goddard explained that although she had been persistently trained in the years she had been at the center, Deborah's general intelligence had not improved. Lacking what the doctors described as the mental capacity to resist vice, she was consigned to be a perpetual resident at the Training School. The reason for her inferior mental condition, Goddard declared, was bad heredity, and no amount of training or education could improve her condition. But she *looked* normal. Goddard maintained that a feeble-minded person could never be made into a useful citizen (or parent) but only a trained professional was able to easily identify feeble-minded persons who seemed, to the untrained, to be normal.²³

It was this privileged position of expert authority that became highly significant in coupling mental deficiency with immigration restriction. Writing to Robert Yerkes in late 1912, Goddard declared the special province of scientific experts in assessing the mental capacity of individuals. Leila Zenderland, who has closely studied Goddard's work on mental testing, describes his position as one removed from academic psychology and more a part of the medical world.²⁴ Using the example of medical inspections of immigrants on Ellis Island, Goddard

²³ Goddard, *The Kallikak Family*. See also Zenderland, *Measuring Minds*.

²⁴ Leila Zenderland, "Debate over Diagnosis: Henry Herbert Goddard and the Medical Acceptance of Intelligence Testing" in Michael M. Sokal, ed. *Psychological Testing and American Society, 1890-1930* (New Brunswick, NJ: Rutgers University Press, 1987) p. 50; see also James Reed, "Robert M. Yerkes and the Mental Testing Movement," *Ibid*.

admitted that he was mystified when psychiatrists on the inspection line could look at an immigrant and know that the new arrival was an insane person. “I don’t understand how he does it,” he confessed to Yerkes. “I cannot believe but that he is guessing, nevertheless the results prove that he is accurate.... I come to conclude that the expert has an experience that is not understandable but whose results we must accept.” Addressing the work of his own field researchers, Goddard emphasized the same special ability, noting that they did not hunt for feeble-mindedness, but were carefully trained to recognize it.²⁵

Professional reviews of *The Kallikak Family* by and large validated Goddard’s thesis of the special skill of properly trained experts. *The American Journal of Psychology* stressed that although the book was written for a lay audience, the scientific evidence presented in the technical volume that was forthcoming would prove the conclusions Goddard drew. When this was published as *Feeble-Mindedness: Its Causes and Consequences* in 1914, the reviewer in *International Journal of Ethics* endorsed Goddard’s conclusions, and even included a variant of Davenport’s favorite hereditary claim: “we may say, it is clear that children in these cases are like their parents.”²⁶ Even the review of an Oberlin College professor that was harshly critical of Davenport’s *Heredity in Relation to Eugenics* and the published papers of the first International Eugenics

²⁵ Goddard to Robert M. Yerkes, 5 November 1912, Robert Mearns Yerkes Papers, Manuscripts and Archives, Yale University Library, New Haven, CT.

²⁶ Review of *The Kallikak Family: A Study in the Heredity of Feeble-Mindedness* by H. H. Goddard, in *The American Journal of Psychology* v. 24, n. 2 (April 1913), 290-1; J.H.T., Untitled review of *Feeble-Mindedness: Its Causes and Consequences* by H. H. Goddard, in *International Journal of Ethics* v. 25, n. 3 (April 1915), 423-24, quote from 423.

Congress, warmly endorsed the Kallikak study. Goddard proved, he noted, the heritability of mental defects “beyond possibility of reasonable doubt.”²⁷

When the empirical evidence to support Goddard’s claims for the heredity of feeble-mindedness was published in a separate volume in 1914, it aimed to demonstrate the heritability of feeble-mindedness by sheer weight of evidence. Goddard performed a pedigree analysis of other Vineland residents just as he had with Deborah. The book contained over 400 pages of family pedigrees and charts to emphasize that mental defects and incapacity were confined to family lines, transmitted, he insisted, as a unit character by the Mendelian ratio. By this Goddard meant that “intelligence” was a singular quality, that the intellectual development and capacity of a human being was the result of one specific and separate part. It was believed at this time, erroneously it turned out, that things like eye color and hair color were “unit characters,” that were either dominant or recessive, so that they were transmitted by precise mathematical ratios. Jennings would later describe this idea as having no scientific basis even as it pertained to simple phenomena like eye color.²⁸ Thomas Hunt Morgan’s research on fruit flies also unequivocally disproved the concept of unit characters, explaining that a given characteristic “is the product of a number of genetic factors and of

²⁷ A. B. Wolfe, review of *The Kallikak Family* by H. H. Goddard, in *The American Economic Review* v. 3, n. 1 (March 1913), 165-70. John Lisle, the *Kallikak* reviewer in the *Journal of the American Institute of Criminal Law and Criminology* declared that the book’s value in the study of heredity could not be overestimated, and that the standing of the authority and his professional position “are such guarantees of the authenticity of its data and the correctness of the work as to make it invaluable.” John Lisle, review of *The Kallikak Family* by H. H. Goddard, in the *Journal of the American Institute of Criminal Law and Criminology*, v. 4, n. 3 (September 1913), 470-71.

²⁸ Henry Herbert Goddard, *Feeble-Mindedness: Its Causes and Consequences* (New York: The Macmillan Co., 1926); Herbert Spencer Jennings, “Heredity and Environment” *The Scientific Monthly* v. 19, n. 3 (September 1924), 225-38; 228.

environmental conditions” and that the “careless use of the term [unit character]” in the context of his chromosome theory “deserves the disrepute into which it is falling.”²⁹ But for Goddard, Harry Laughlin and Davenport, unit characters were essential to the eugenic project; the movement could not function without them or the transmission of characteristics along readily predictable Mendelian lines. “Proving” the inheritance of feeble-mindedness by Mendel’s ratios was imperative. This was the whole function of *Feeble-Mindedness*: once the inheritance of feeble-mindedness in this fashion could be demonstrated, once it was proven to be a biological law, the feeble-minded could be isolated from society, because nothing could be done to improve their lives or their intelligence.³⁰ Importantly, however, it must be pointed out that at this point that the eventual eugenic solutions introduced in Nazi Germany parted ways with its American counterpart, for Goddard, Laughlin or Davenport never took the next logical step of exterminating these defectives.³¹

²⁹ Thomas Hunt Morgan, A. H. Sturtevant, H. J. Muller, C. B. Bridges, *The Mechanism of Mendelian Heredity* rev. ed. (New York: Henry Holt and Co., 1922 [orig. 1915]), 264.

³⁰ Goddard, *Feeble-Mindedness*, 556 for Goddard’s assertion that it is “transmitted in accordance with the Mendelian formula.” On the logical conclusion of negative eugenics – eliminating these members from society – Goddard wrote: “The feeble-minded person is not desirable, he is a social encumbrance, often a burden to himself. In short it were *better both for him and for society had he never been born*. Should we not then, in our attempt to improve the race, begin by preventing the birth of more feeble-minded? This is a program which is attracting much attention.” *Ibid.*, 558, emphasis added.

³¹ Stefan Kühl’s very careful analysis of Goddard’s book shows that there were significant connections between the American and German eugenics communities that preceded the Nazi party’s takeover of the German state; he notes that American eugenicists were among the strongest foreign supporters of Nazi racism, and that American immigration laws were viewed in Germany particularly well. The Nazis, Kühl points out, embraced the stories of the Jukes and Kallikaks to legitimize their own programs. Stefan Kühl, *Eugenics, American Racism, and German National Socialism* (New York: Oxford University Press, 1994), especially chapter 4. Stephen Jay Gould points out that the Nazis ultimately implemented the Eugenic Record Office’s Model Sterilization Law, written in 1922, in September 1939. Stephen Jay Gould, *The Flamingo’s Smile: Reflections*

It was not just the ominous outlook on hereditary mental defects in *Feeble-Mindedness* that made it troubling. It was also the way the evidence was obtained. The subjects that provided the family pedigrees that Goddard used to “prove” the heritability of mental defects were the young patients at the Vineland facility. Very often, Goddard explained, the children were admitted without full examinations of the family histories. The pedigrees were essential not so much for the care of the children – the alleged biological basis of feeble-mindedness made that futile. But the pedigrees *were* important for the establishing the proof of hereditary feeble-mindedness. It seems strange, then, how the data and information were gathered. In this respect, Goddard learned from Davenport very well, for the information that his researchers gathered was obtained in exactly the same way that Davenport’s conducted research through the ERO: by female field workers who conducted interviews with family members. Selected for their “pleasing manner and address,” which was supposed to inspire comfort in the subject’s family, the female researchers did not have any professional or even systematic training. They were given brief instruction and special training to conduct interviews, but they mostly lacked any knowledge of the scientific nature of the problems they were investigating, the alternative factors that might also have caused the mental defects, or any genuine ability to be critical of the information they gathered from family members.³² Yet these were the women that

in *Natural History* (New York: W. W. Norton, 1985). Kevles, *In the Name of Eugenics*, especially chapter 8, correctly points out that the public erosion for support in the United States was not due to the Nazi policies of the 1930s, but was the result of a coalition of critics, which will be discussed further below.

³² The role of the women is from Goddard, *Feeble-Mindedness*, 24.

Goddard, like Davenport, selected to examine family histories for evidence of feeble-mindedness back three, four, or even six generations.³³ This was science conducted in the quintessential classic racist method: knowledge creation by virtue of social skills, not academic proficiency; examining as unit characters highly complex phenomena and processes; “proof” provided by anecdotal evidence and sheer volume rather than experimental viability; and heavily influenced by *a priori* assumptions. Surprisingly these shortcomings would not inhibit the success of the movement at all.

Ultimately, the significance of *The Kallikak Family* was not the discovery of a long line of feeble-minded ancestors that consumed public resources, or the proof it allegedly supplied for the inheritance of feeble-mindedness. The real significance lay in the affirmation of the positive role that experts could play in discerning the mental ability of individuals. In the wake of Goddard’s book – much more so than after Dugdale’s or Davenport’s exposés – the practice of psychological testing became mainstream. And with the affirmation of the ability of experts to detect mental deficiencies, the coupling of mental ability to immigration restriction became legitimized.

The Immigration Act of 1882 first listed psychological reasons that an arriving immigrant may be denied admission to the United States.³⁴ Members of

³³ Goddard insisted, “The idea that it is impossible to determine the mentality of a person who is three or four generations back to the present is partly an ill-considered one and partly the result of erroneous logic.” The relatively closer family ties in defective families, and their limited geographical range (because of their feeble-mindedness) also improved, in his mind, the accuracy of these extended inquiries into family backgrounds. *Ibid.*, 27-8.

³⁴ Hutchinson, *Legislative History*.

the Immigration Restriction League frequently used their political connections to influence the appointment of immigrant inspectors in eastern ports, particularly at Ellis Island. They viewed it as essential that officials in charge of the examinations follow, to the letter, the policy laid out in the various immigration laws. In April 1912 Prescott Hall had asked United States Public Health Service physician M. Victor Safford to look into cases where the Secretary of Labor, who had oversight over the entire immigration inspection process, had overturned decisions made by medical physicians and landed imbecilic or feeble-minded immigrants. Safford found that in multiple cases the Secretary enabled relatives of the excluded aliens to secure their own physician to provide medical testimony that under the law, the immigrant *was* admissible. In one case in particular, that of Mosche Rabinowitz, the medical doctor secured by the immigrant's family succeeded in convincing Secretary Charles Nagel "that the alien was only deaf and dumb," but once he was admitted, he quickly became an inmate at the Kansas City, Missouri insane asylum.³⁵ The implication Hall drew was that Secretary Nagel was endangering the country by disregarding the professional and

³⁵ M. Victor Safford to Prescott Hall, 2 April 1912, box 3, IRL papers. Safford's experience in the Marine Hospital Service (later the United States Public Health Service) was detailed in a 1925 book, *Immigration Problems: Personal Experiences of an Official* (New York: Dodd, Mead and Co., 1925). Safford, like many of his colleagues, believed in the ability of the expert medical examiner to quickly discern racial identity, mental capacity, and desirability of immigrants. The racial and national characters of an alien were apparent from physical appearance, difference in build, shape of head, and facial features. Even on his first visit to Ellis Island he noted that "A glance was almost always sufficient to tell that the group was composed of different varieties of Eastern European Slavs.... Trailing along behind was a broad-shouldered, thick-set, swarthy sun-burned man who seemed out of place in the group. He was easily recognizable as a Dalmatian." 5. Henry Cabot Lodge and Joseph Lee took interest in the appointment of Commissioner of Immigration in Boston, and Safford was at the top of their list. See, e.g. Lodge to Lee, 9 April 1921; Lee to Lodge 12 April 1921, both in Lee papers.

unbiased expert medical testimony provided by government employees. In 1917 Public Health Service physician E. H. Mullan published an article on “Mental Examination of Immigrants” in which he described the process of examining the mental capacity of alien arrivals, with particular reference to their physical appearance, to demonstrate the special skill that experts had in detecting an immigrant’s mental inferiority.

In precisely the same style as Lombroso, Mullan maintained that the physical appearance of an individual reflected mental qualities or tendencies that may be grounds for exclusion. A well-practiced medical examiner could quickly and easily detect physical infirmities, and in many cases even the race of the alien, enabling the examiner to discern the difference between a northern Italian girl “whose appearance indicates that she has some schooling,” and an illiterate male from southern Italy. Physical signs of degeneration suggested to examiners that the alien may be psychologically inferior; many still believed physical features shaped the mental characteristics. Correctly diagnosing feeble-minded aliens, Mullan elaborated, was confirmed by “his appearance, stigmata, and physical signs,” just as Lombroso had predicted.³⁶ It is apparent that six years after the governmental publication of Boas’s study of the malleability of immigrant body forms, the determinism of classic racist thought retained great purchase in immigration policy.

³⁶ E. H. Mullan, “Mental Examination of Immigrants” *Public Health Reports* n. 398 (18 May 1917) (Washington, D.C.: United States Public Health Service/Government Printing Office), 733-46; pp. 737, 746. Cesare Lombroso, *L'uomo delinquente*, esp. Parte III, cap. 7, which considers tattoos and their links to delinquency.

While mental exams were being applied to immigrants with increasing rigor, the most important study in terms of public attention to the mental inferiority and undesirability of immigrants came from the army. The chief advocate for and the leader of mental tests was Robert M. Yerkes, the erstwhile correspondent of Jennings, Pearl and Davenport. When the war began, Yerkes resigned his chair in psychology at the University of Minnesota to work for the Surgeon General's Sanitary Corps testing the intellectual abilities of army recruits, although his early training was in the behavioral tendencies of higher animals (he published *The Dancing Mouse* in 1907 and edited the *Journal of Animal Behavior* between 1911 and 1919). When he began working at the Boston Psychopathic Hospital in 1913, the young psychologist's interests turned to perfecting Alfred Binet's mental testing system. What Yerkes's studies on human intelligence showed him, he explained in an article for the *National Academy of Sciences* in 1915, was that a person's intellectual capacity, or "degree of mental development" could be reliably—and quickly—determined to ascertain their skill and ability to perform certain tasks. This seemed an essential requirement in 1916 for a nation that was mobilizing hundreds of thousands of soldiers.³⁷

Yerkes's investigations were almost compromised at the outset of his investigations. As Yerkes and his staff in the Sanitary Corps began testing the mental aptitude of soldiers, Charles Davenport approached him with a

³⁷ Robert M. Yerkes, "A Point Scale for Measuring Mental Ability" *Proceedings of the National Academy of Sciences of the United States of America* vol. 1, no. 2 (15 February 1915), 114-17; see also idem, "Measuring the Mental Strength of an Army" *Proceedings of the National Academy of Sciences of the United States of America* vol. 4, no. 10 (15 October 1918), 295-97. For Yerkes's biographical information, see James W. Reed, "Yerkes, Robert Mearns" *American National Biography Online* February 2000, <http://www.anb.org/articles/14/14-00861.html>, Access Date: Tue Sept 07 2004.

proposition. Was it possible, he asked Yerkes, to ascertain not only the mental aptitude of the recruits, but also their racial identity? If information could be simultaneously gathered on the birthplace of the subject's four grandparents, Davenport said, eugenicists in the country would finally have "adequate analytical data" to back up assertions that "this or that race is a good fighting race." This did not seem to be Davenport's primary concern, though. Davenport was confident that "very interesting results would follow [such] a study of the racial differences." Davenport was hoping for racial information to provide additional support of the undesirability and inferiority of immigrants. In fact, he was so confident that these results would show this, that soon after he wrote Yerkes, he petitioned the Surgeon General to create a division of anthropology within the medical branches of the Army and Navy departments, to measure the physical characteristics of the recruits. Using anthropological measurements would unequivocally link physical stature and the mental characteristics of certain races. The Surgeon General already accumulated information relating to the height, weight, and physical fitness of recruits for the purposes of exempting or refusing enlisting soldiers. Davenport warned the Surgeon General, however, that the "Identification Records" being taken by the Adjutant General's office were "untrustworthy" and inadequate. Professionally trained scientists were not conducting the examinations, so the results might not be conclusive. There was of course a certain irony in Davenport making this criticism. And, he scribbled in the margin that if the Surgeon General approved the new study, the new schedules should also include the recruit's race, and the birthplace of both parents.

Davenport's study was approved. In a memo written after the armistice, Davenport pleaded that the compilation of information continue, despite the end of hostilities. Dr. B. A. Gould had done similar measurements of soldiers after the American Civil War, Davenport explained, and the opportunity to "'take stock' of the racial constitution of the United States" could not be passed up.³⁸

After the armistice, the War Department authorized the printing of the results of the Army mental tests. Yerkes and Clarence Yoakum undertook the task of compiling the data on 1.75 million men. When it was finally published in 1920, it became, one historian described, the "nexus" of all future mental tests. Unfortunately, it also became useful empirical evidence for American restrictionists to use in their campaign to stop immigration.³⁹

Robert Yerkes's and Clarence Yoakum's book *Army Mental Tests* analyzed the administration of the tests to military recruits, and expressed great confidence in the tests' ability to accurately segregate the mentally incompetent and to statistically classify men according to mental ability. Two types of exams were given, an Alpha and Beta, the first for literate recruits, the second for recruits who were wholly or partly illiterate. The results showed that mental tests were effective in selecting officer candidates who would succeed in the field, and fitting recruits to an appropriate function within the army. This led to the

³⁸ Charles Davenport to Robert Yerkes, 18 December 1917; Charles Davenport to Rupert Blue, Surgeon General of the United States, 2 February 1918, all Davenport papers; "Suggested Data for Anthropological and Ethnographic Survey of the Army," 15 November 1918, Davenport papers - Lectures. The results of the Army studies will be considered more fully in the following chapter. Note the evidence that Davenport asks for in order to fit his hypothesis that certain races are better fighters than others.

³⁹ Stephen Jay Gould, *The Mismeasure of Man* (New York: W. W. Norton Co., 1981), 222-33.
Chapter 5

conclusion, which the authors expressed in the second chapter, that the group examinations were successfully designed, “and are now definitely known, to measure native intellectual ability. They are to some extent influenced by educational acquirement but in the main the soldier’s inborn intelligence and not the accidents of environment determines his mental rating or grade in the army.”⁴⁰ But the reprints of the exams, which the book also included, suggest a very different type of examination.

As one example of what Yerkes’s examinations actually measured, the third test in the battery was designed to assess an examinee’s “common sense.” Though several questions were straightforward, many were ambiguous and subjective. One question asked why a man who could afford a house would choose to rent instead. Respondents had to choose between the following answers: “they don’t have to pay taxes,” “they don’t have to buy a rented house,” or “they can make more by investing the money the house would cost.” This seems more an interpretive rather than factual question, but mysteriously assessed mental intelligence. There are many subjective answers to this question, especially for an immigrant who had been in the country for a decade or less. A question asking why parents should send their children to school offered only the following answers: “it prepares them for adult life,” “it keeps them out of mischief,” or “they are too young to work.”⁴¹ For immigrant communities, the children were in

⁴⁰ Clarence S. Yoakum and Robert M. Yerkes, compilers and editors, *Army Mental Tests* (New York: Henry Holt and Co., 1920), 17.

⁴¹ *Ibid.*, Test 3, Form 6, p. 222-223. The official answers were “they can make more by investing the money the house would cost,” and “it prepares them for adult life”, p. 71.

some circumstances important economic contributors. Not being assimilated to American norms completely, the answers may not have seemed like common sense knowledge at all. When the results of the tests were discussed in publications after the war, John Higham points out how “eugenicists seized avidly on these findings as a clinching proof of the racial philosophy and of the inferiority of the new immigration.”⁴² But the questions asked were more questions of culture and educational background than any assessment of natural mental ability.

The problem with the state of scientific knowledge in the early twentieth century was that it was quite uncertain. Advances in the fields of genetics and biology had not yet filtered through many of the disciplines that allied with eugenics, leaving the veracity of eugenic arguments open to debate but with no overwhelmingly convincing new paradigm. This was particularly true as it concerned mental testing, and the “intelligence” of native-born and foreign-born was hardly definitively or decisively established. That did not stop scientific ideas – whether they concerned demographics and statistics, ethnology, mental ability or intelligence – from being coupled to cultural fears about racial degeneration and the alleged inferiority of the new immigration.

The army results were manipulated in several ways once they were published. Lothrop Stoddard’s *The Revolt Against Civilization* declared in alarmist tones that the tests proved that “intelligence is steadily being *bred out* of the American population,” and it was largely the fault of uneducated, mentally

⁴² Higham, *Strangers in the Land*, 275.

inferior, and highly fecund immigrants, who came from non-Nordic stocks. Some books, like Carl C. Brigham's *A Study of American Intelligence* (1922), which one historian describes as "a benchmark in the history of scientific racism," specifically used and manipulated the Army intelligence data to make the case that immigration was a detriment to the United States.⁴³ The profusion of books that misused this data prompted Walter Lippmann, the editor of *The New Republic*, to write a series of articles in the fall of 1922 denouncing the faulty generalizations made from the Army data. It was one of the first criticisms of the methods of classic racialists to appear publicly. Lippmann's first target was Stoddard's *Revolt Against Civilization*, in which Stoddard used the Army tests to show that the average mental age of Americans was fourteen years.⁴⁴ This was, Lippmann pointed out, "not inaccurate. It is not incorrect. It is nonsense." While he conceded that the Binet system for measuring intelligence had real promise and genuine utility, the New York-born journalist decried its "gross perversion by muddleheaded and prejudiced men."⁴⁵ Lippmann would soon have plenty of examples of such "gross perversion."

After the first salvo at one of the most egregious examples of the manipulation of scientific data, Lippmann used the next issue of *TNR* to explain

⁴³ Carl C. Brigham, *A Study of American Intelligence* Introduction by Robert M. Yerkes (Princeton, NJ: Princeton University Press, 1922); Clarence S. Yoakum and Robert M. Yerkes, compilers and editors, *Army Mental Tests* (New York: Henry Holt and Co., 1920). Quote is from Reed, "Yerkes and Mental Testing," 82.

⁴⁴ Lothrop Stoddard, *The Revolt Against Civilization: The Menace of the Under Man* (New York: Charles Scribner's Sons, 1922), 69.

⁴⁵ Walter Lippmann, "The Mental Age of Americans" *The New Republic* vol. 32, no. 412 (25 October 1922), 213-15. "Nonsense" on 213; "muddleheaded" on 215.

the complexity of intelligence testing and why Stoddard's book should be denounced and ignored as nothing other than a useless polemic. There was no objective standard by which to measure intellectual ability, he explained; intelligence is "not an abstraction like length and weight." The results of the Army's examinations, the editor pointed out, measured the intelligence only of men in the Army, not the nation at large.⁴⁶ Furthermore, he explained in the following week's issue that the results from the tests were simply "a fair guess at intelligence" and were not an objective or accepted mechanism for measuring intelligence. Again, Lippmann conceded that intelligence testing had its proper place and use, like classifying the abilities of schoolchildren, but he was insistent in opposing the tendency to generalize overall intellectual ability and capacity from just one test. In an article from the November 15 issue, Lippmann took aim at the general eugenicist strategy of coupling heredity and ability in an effort to portray certain groups of people, immigrants especially, as hereditarily inferior. Singling out Stoddard once again, Lippmann accused the Immigration Restriction League member of turning intelligence tests into "an engine of cruelty" and lamented how easily beneficial and useful science could be perverted for pernicious ends "in the hands of blundering or prejudiced men."⁴⁷ The Immigration Restriction League had faced criticism like this before, and as they did with President Eliot of Harvard, they remained uncowed. In his last article on

⁴⁶ Walter Lippmann, "The Mystery of the 'A' Men" *The New Republic* vol. 32, no. 413 (1 November 1922), 246-48.

⁴⁷ Walter Lippmann, "The Abuse of the Tests" *The New Republic* vol. 32, no. 415 (15 November 1922), 297-98.

intelligence tests, Lippmann flatly declared that the ability to measure hereditary intelligence “has no scientific foundation.”⁴⁸ In this series of articles in a popular magazine over the course of a month, Lippmann tried to point out the dangerously false claims that were being made in the name of science. But he had little effect.

In this public criticism of the men who utilized scientific knowledge for their own personal prejudices, Lippmann anticipated Jennings and Pearl, and likewise their ineffectiveness. Why a journalist would be among the first to condemn the “blundering or prejudiced men” that manipulated statistics and science is curious. But considering Stoddard’s own position within the scientific field makes the case a bit more intelligible: Stoddard had absolutely no professional scientific standing. Although he held advanced degrees (an M.A. and a Ph.D. from Harvard), they were in history, not in science. For Lippmann to square off against Stoddard in a public, popular journal, then, was effectively a battle between two like-equipped men. Lippmann was not writing his articles as a psychological expert, but merely as an educated person who saw the flaws in logic and method in the work. Seeing the ways that the data from the Army tests was being manipulated for a specific goal – immigration restriction – Lippmann merely wrote as a concerned observer.

Lippmann’s articles prompted a response from Yerkes, who wrote to *The New Republic*’s editor mid-way through the series of articles. In his first letter, Yerkes politely suggested that Lippmann was damning *all* intelligence testing

⁴⁸ Walter Lippmann, “A Future for the Tests” *The New Republic* vol. 33, no. 417 (29 November 1922), 9-11.

because of the perversions that men like Stoddard and Brigham were making of it. The Yale psychologist noted that while Lippmann was preparing his *TNR* articles, Yerkes too was preparing a manuscript, which was printed in March 1923 in *The Atlantic Monthly*, “the principal purpose of which was to indicate the unreliability of mental age and the uselessness of letter-grade distribution as an indication of the status of mental alertness...”⁴⁹ But while Yerkes had intended that caution be exercised in making conclusions from the tests, the ways the data was being misused required more forceful rebuttals that he was unwilling to provide. Yerkes had the authority of the expert – they were his tests and data, after all – yet he did nothing to counter the propaganda that men like Stoddard were making with them. Lippmann, for his part, defended his series of articles. “I was quite careful in the articles,” the critic countered, “to distinguish you and your work from the Binet-Terman business.” Lippmann offered his compliments on Yerkes’s editing of the army tests, saying that the work was “so admirably done that it should have prevented the abuse of them...” Still, the *New Yorker* lamented, it must be conceded that Terman’s works had captured the public’s attention, and “taken hold in practical affairs.”⁵⁰ And once there, they increased in influence.

⁴⁹ Yerkes to Lippmann, 28 November 1922, box Yerkes papers. The “Binet-Terman business” that Lippmann spoke of was Alfred Binet, Théodore Simon and Lewis M. Terman, *The Development of Intelligence in Children* (Nashville, TN: Williams Printing, 1916). Terman utilized the scale that Binet and Simon had devised and modified it as he applied it to the children at the Training School in Vineland, New Jersey. Vineland was also the base of Herbert H. Goddard before he moved to Cold Spring Harbor with Davenport; it was here the Goddard came across the “Kallikak” family.

⁵⁰ Lippmann to Yerkes, 6 December 1922, Yerkes papers.

With this note, Lippmann raised a fundamentally important issue, which would propel men like Jennings, Pearl, Boas, and Hrdlicka to action in succeeding years. Once a theory, a series of statistics, a hypothesis, or even a carelessly worded idea, had been put into the public domain, whether in a textbook, book, scientific journal, or popular article, it became fair game for selective quotation by anyone else. One had to be quite careful speaking in the name of “science,” for once something was published, its use (or in this case, misuse) could not be prevented. When Yerkes sent Lippmann an advance copy of his manuscript for *The Atlantic Monthly* in late December 1922, Lippmann pointed this out.⁵¹

Yerkes’s article in its published form, suggested that there were indeed serious errors being made with the interpretations of the Army tests. He began by quoting an unnamed article that declared that there were roughly “forty-five million people in this country who have no sense.” (This at a time when the total population of the United States was 106 million.) The uncited article went on to alarmingly suggest that there were, in addition, twenty-five million that had only “a little sense.” These twenty-five millions had a capacity for “mental and spiritual growth [that] is only that of a thirteen- or fourteen-year old” child. Yerkes pointed out that in the unnamed article, the hysteria worsened, and it claimed that there were another twenty-five million in the country with only “fair-to-middling-sense.” So as to not be entirely pessimistic and apocalyptic, the anonymous author explained that thankfully, there were “a few over four millions who have a great deal of sense. They have the thing we call ‘brains.’” The

⁵¹ See Lippmann to Yerkes, 9 January 1923, Yerkes papers.

professional psychologist debunked these absurd contentions—which Yerkes eventually revealed were written by Albert E. Wiggam—by explaining the importance of both the context and specificity of the tests, which Wiggam had blithely ignored. Despite the fact that the Army tests gave no evidence to support Wiggam’s statements that only four million Americans had any intellectual “brains,” Yerkes concluded his article echoing by Stoddard’s fear of declining intelligence in the United States and also recommending immigration restriction.⁵² When he could have spoken strongly against Wiggam’s careless work, he did not.

This did not sit well with Lippmann. *The New Republic*’s editor responded to the December draft he received very harshly. Not only were Yerkes’s generalizations “exceedingly unfortunate,” they rested upon statistics that seemed reasonable, so they would likely carry great weight with the public. What Yerkes did not understand, Lippmann complained in a letter, was “the implied interpretation. You lend your name, for example, to the impression that a child born of Irish parents is intrinsically inferior to a child of English parents. This might be true. But you are in no position to prove it. You are in no position to assess the effects of the history of Ireland upon the Irish intellectual behavior.”⁵³ This was the precise point that sociologists, anthropologists, and eventually even biologists, geneticists, zoologists, and biometricians would make: there was a

⁵² The uncited article appears in the first paragraph of Robert Yerkes, “Testing the Human Mind” *The Atlantic Monthly* vol. 131 (March 1923), 358. Yerkes explained in the subsequent paragraph that these statements that “I venture to quote from a popular magazine, are typical of much that has been written about ‘army mental tests.’ Are they true? No. Is there any truth in them? Just enough to make them worse than false.” 358. Yerkes identifies Wiggam as the author of the “unnamed” article on p. 360.

⁵³ Lippmann to Yerkes, 9 January 1923, Yerkes papers.

coequal role that environment, culture, and history play in determining the general behavior of groups of people and of individuals. Nature and nurture could not be that easily disentangled, they all declared. Science had in no way proven that nature was so preponderantly influential in development as to negate the influence of the environment and socialization. Italians like Colajanni, Sidney Sonnino, Leopoldo Franchetti, and Pasquale Villari, who all approached the Italian problems from a sociological rather than a biological perspective, had expressed these exact thoughts decades before in trying to solve *il questione meridionale*.⁵⁴ And herein lay the problem: it was much easier to communicate to the public one simple argument – namely, that heredity and inheritance predetermined an individual’s physical and mental make-up – than to give the necessary cautions about the significant impact of the environment, cultural and social backgrounds, and individual determination on a human being’s development. Nature, it seemed, was much easier to explain than nurture.

Developing the Congressional Quota System

After Congress passed the literacy test in the Immigration Act of 1917, legislators continued attempts to further restrict immigration with a variety of proposals that reflect the range of nativist sentiment and influence in Congress. Despite the cautions that writers like Lippmann offered, the fears and hysteria

⁵⁴ See especially John Dickie, “Stereotypes of the Italian South, 1860-1900” and Gabriella Bribaudi, “Images of the South: The *Mezzogiorno* as seen by Insiders and Outsiders” both in Lumley and Morris, eds. *The New History of the Italian South*; John Dickie, *Darkest Italy: The Nation and Stereotypes of the Mezzogiorno, 1860-1900* (New York: St. Martin’s Press, 1999); Nelson Moe, “The Emergence of the Southern Question in Villari, Franchetti, and Sonnino” in Schneider, ed. *Italy’s Southern Question*; Moe, *The View from Vesuvius*.

cultivated by alarmists like Stoddard and Wiggam retained much greater public influence. The neat, concise arguments of classic racialism were eminently better as a basis for drafting legislation because they were not complex. And Harry H. Laughlin, the longstanding associate of Charles Davenport, proved an effective conduit for the influence of classic racialism on the American legislative branch. Representative John Burnett, a Democrat from Alabama and former member of the Dillingham Commission, introduced two bills in the final session of the 65th Congress in 1919 to totally prohibit immigration to the United States for a period of four years. Many of the proposals introduced by legislators were motivated by the fear of a return to pre-war levels of immigration that they believed demanded greater restriction on immigration. But the bills were unable to receive a hearing in the House because of other legislative issues, dealing with demobilization, war debts, and the Versailles Treaty, and no immigration bills were enacted.⁵⁵

In Congress's next session, immigration remained an important legislative topic, and several bills were again introduced to further limit, regulate, temporarily suspend, or prohibit immigration altogether. The legislative tendency was decidedly in favor of further restriction, though one member did introduce a bill to repeal the literacy test (H.R. 3474).⁵⁶ Representative John J. Rogers introduced H.R. 9782, which became law without President Wilson's endorsement; the bill was a temporary measure to continue wartime regulations

⁵⁵ Albert Johnson's Committee report on H.R. 14461 on 6 December 1920 contained a thorough history of legislation introduced regarding immigration restriction in the 65th and 66th Congresses. House Report no. 1109, "Temporary Suspension of Immigration," 66th Congress, 3rd Session, 6 December 1920, 1-2.

⁵⁶ *Congressional Record*, 66th Congress, 1st Session, Index and Bill History.

on passport and visa control, and was referred not to Johnson's Committee on Immigration and Naturalization, but to the House Committee on Foreign Affairs.⁵⁷ Albert Johnson introduced three bills to temporarily suspend immigration for two years, and Representative Samuel Nicholls (D-SC) introduced a bill to regulate immigration on a percentage basis (H.R. 10837). The Immigration and Naturalization Committee only reported out Johnson's bill imposing a temporary suspension.⁵⁸

While the House Committee on Immigration and Naturalization was considering these new immigration laws, Harry H. Laughlin, the Superintendent of the Eugenics Record Office at Davenport's New York complex, was called to testify before Johnson's committee. Laughlin's appearance was fundamentally important: his testimony provided the "scientific" rationale for increasing the restrictive measures regarding immigration to a committee already predisposed to enact further restrictive legislation. The literacy test, Laughlin explained, was not having the desired effect of preserving the biological and racial character of the American people because it was not excluding, and would not exclude, a sufficient number of immigrants. Over the course of a weekend in mid-April

⁵⁷ The second session of the 66th Congress witnessed an interesting discursive shift in the description of foreign arrivals. "Immigrants" were no longer used in the index in the *Congressional Record* – "Aliens" was the new term of preference. This shift, which was echoed in similar discursive debates within the Immigration Restriction League, served to emphasize the foreign-ness of immigrants, a matter of particular importance during the Red Scare. See the *Congressional Record*, 66th Congress, 2nd Session, 9420-21.

⁵⁸ House Report no. 1109, "Temporary Suspension of Immigration," 66th Congress, 3rd Session, 6 December 1920, 1-2.

1920, Laughlin convinced the committee to regard the hereditary and eugenic character of immigrants as critically important when contemplating legislation.

Laughlin's position as the head of the ERO vouched for his "scientific" credentials, although in truth he had no real scientific training. At the opening of the morning session on Friday, Laughlin began his testimony by beseeching the members of the committee to think about the "racial qualities" of immigrants in terms of their hereditary physical, mental, and moral traits. "It is now high time," he told the Representatives, "that the eugenical element, that is the factor of natural hereditary qualities which will determine our future characteristics and safety, receive due consideration" in crafting immigration policy. To this end, Laughlin proposed that two policy initiatives be added to the bills before the committee: the overseas physical and mental examination of intending emigrants by the United States Public Health Service, and genealogical examinations of immigrants' family backgrounds by field workers credentialed by the State Department to ensure "the possession...of such physical, mental, and moral qualities as the American people desire to be possessed inherently by its future citizenry."⁵⁹ The influence of classic racialism was clear: such exams could, Laughlin believed, discern what characteristics immigrants would contribute to the American population in their future offspring. In Laughlin's eugenic worldview, good citizens and good Americans were not born; they were bred.

⁵⁹ Harry H. Laughlin, "Biological Aspects of Immigration," *Hearings before the House Committee on Immigration and Naturalization*, 66th Congress, 2nd Session, (Washington, D. C.: Government Printing Office, 1921), 16-17 April, 1920, pp. 3, 4.

Some eugenicists in the pro-restriction camp insisted that this overseas examination into the hereditary stock of intending immigrants was absolutely critical. Laughlin elaborated on why it was so important: “The importance of this condition of admission is driven home when we recall that immigrants are going to add to the breeding stock of the American people in greater proportion than their immigrant numbers bear to the total population, *because statistics have shown* that immigrant women are more prolific than our American women.” Nearly thirty years after its first iteration, Walker’s “replacement thesis” was still haunting old-stock Americans. Once again trotting out the social burden created by inferior families like the Jukes, Ishmaels, and Kallikaks, Laughlin warned the Committee of the imminent degeneration of the American population if immigrants were not carefully selected or restricted from admission.⁶⁰

Laughlin’s Saturday testimony pressed the call for investigating the hereditary background of immigrants. Because the “foundation stock” of the United States was largely from northwestern Europe, he explained, assimilation and Americanization were much easier for population groups from that area in Europe than it was for “Latin or other stocks less closely related to us....” Laughlin offered a eugenic utopian vision of America if only the Committee would follow his recommendation. By selecting immigrants on the basis of their hereditary background and “setting up an eugenical standard for admission demanding a high natural excellence of all immigrants regardless of nationality and past opportunities, we can enhance and improve the national stamina and

⁶⁰ *Ibid.*, 4. Emphasis added.

ability of future Americans.” He warned that the United States’ failure to sort immigrants at the point of European departure “on the basis of [their] natural worth is a very serious national menace.”⁶¹ Laughlin aimed to establish the necessity of scientific knowledge for formulating immigration policy and he largely succeeded. Months later, he bragged to Charles Davenport that the Committee had decided to print his “eugenical testimony,” and Laughlin declared that “I look upon the Committee’s newly acquired interest as an opening wedge for the presentation of further eugenical facts when the new hearings are held.”⁶² And when they were held, Laughlin would be ready to play an even greater role in advising the committee on “scientific” approaches to immigration.

All restrictionists did not, however, universally support Laughlin’s ideas. Shortly after the testimony of the ERO’s superintendent, Prescott Hall wrote to Davenport to “dissent as strongly as possible” from the endorsement given in *The Eugenical News* to overseas inspection of immigrants “to investigate the eugenical qualities of the stock from which the individual is derived.”⁶³ Hall wrote that such a policy had been thoroughly discussed by League leaders between 1894 and 1902, and was found to be impractical logistically and diplomatically, and

⁶¹ *Ibid.*, 17. Significantly, however, this provision did not meet with the support of all restrictionist groups.

⁶² Laughlin to Charles B. Davenport, 8 January 1921, Davenport papers.

⁶³ Prescott Hall to the Editor of *The Eugenical News*, 31 July 1920, in Davenport/Hall folder, Davenport Papers; *The Eugenical News* v. 5, n. 7 (July 1920), 57. The statement under the heading “Regulation of Immigration” followed Laughlin’s endorsement of National Eugenics in Germany – “Other nations would do well to embody in their fundamental laws, and live up to, such provisions for their racial needs as Germany has incorporated into her new democratic constitution.” 56. Hall’s letter to the editor noted his position as the Chair of the Immigration Committee of the American Genetic Association, despite the fact that he had no professional or scientific training in genetics.

would ultimately be “destructive of any real selection of aliens” especially in the postwar period. Hall referred Davenport to his own 1906 book *Immigration* for a thorough analysis of the matter.⁶⁴ Davenport wrote back quickly to reassure Hall that they both were in favor of protecting the racial quality of the American people, but explained his and the Eugenics Record Office’s position in better detail. The problem, Davenport said, was that in the past the lack of a hereditary history of arriving immigrants had allowed entry of “many perfectly good looking persons [despite their] belonging to families with high incidence of gross hereditary defects of a physical, mental, or temperamental sort.” Davenport’s statement revealed a fundamental tension behind his classic racialism: although he disbelieved Lombroso’s argument about stigmata and physical manifestations of criminality, he clung determinedly to the underlying premise of defectiveness being biological, something permanent in the blood. And he agreed with Goddard, that undesirable, inferior, or defective immigrants could pass as normal immigrants without an extensive investigation into their genealogy. In any event, Davenport noted that the House Committee expressed interest in the idea of selecting immigrants on the basis of their “germ plasm” and hoped that if restrictionists could unite behind the plan, it could make it into the immigration bill.⁶⁵ What was needed, Davenport argued, were simply better trained U.S.

⁶⁴ Hall to Editor, 31 July 1920, Davenport papers; Hall, *Immigration and Its Effects*.

⁶⁵ Davenport to Hall, 7 August 1920, Davenport papers. Davenport crisply encapsulated his beliefs in the possibilities of eugenics in a post-war lecture. He discussed how the laws of inheritance had been worked out in plants, animals and man, and all functioned alike, “in the same way in rats and man.” The anthropological and mental assessments the army conducted on military recruits had demonstrated that there were far too many physically unfit males in the United States (men with

officials at American immigration stations, and a more thorough enforcement of the law. Since he was trained as a lawyer, Hall conceded that the plan might work in theory, but that the financial cost of getting family data made it completely impractical.⁶⁶

But Davenport disagreed and maintained that it was possible and feasible to investigate the hereditary background of emigrants, prompting a prolonged exchange with Hall and the IRL. The dispute raises a curious question as to the extent of influence the League was now exercising in Congress. Was Laughlin's scientific testimony eclipsing the influence of Patten and the League at the Capitol? If Laughlin was wielding more influence than the League's lobbyist, and the provision for overseas restriction passed, it might impair effective action on more feasible and practical elements of policy. Hall implored Davenport to retreat from this policy provision. Certainly, Hall told his old classmate, the League would not oppose anything designed to improve inspection of immigrants. But the lawyer feared that the impending volume of European immigrants would be so large that it was inconceivable that one American field worker attached to the staffs of U.S. consulates – or even ten people – could perform the necessary investigations. Even if they could, Hall pointed out, American agents in foreign

diseased lungs, bad hearts, hernias), men whom Davenport regarded as “scrubs.” Worse still, he told how feeble-minded persons were breeding with their like, producing feeble-minded offspring. He explained: “The modern science of heredity shows the cause of this wide dissemination of defects and the remedy. Typical feeble-mindedness is due to a defect in the germ plasm; if the same defect is in both germ cells that initiate the offspring that child will also be defective. So with melancholia and many other defects.” Charles B. Davenport, “About Eugenics” (n.d.), Davenport papers – Lectures. Statements like this were later refuted by professional scientists, and used as examples of how sloppy and inaccurate was the work coming out of Cold Spring Harbor.

⁶⁶ Hall to Davenport, 9 August 1920, Davenport papers.

territories could not be trusted to maintain a vigilant guard at the gates; they would, because of their isolation, be vulnerable to bribery. Not even bureaucrats in America – he singled out the Assistant Secretary of Labor Louis F. Post – could be trusted to carefully and thoroughly uphold the regulations when it came to immigration. Davenport’s problem, Hall informed him, was that “you are looking at this thing from a purely laboratory point of view.” In practice, the Bostonian argued, Americans would prove to be “dreadfully inefficient” at carrying out such an enterprise. In practice, European nations like Italy, Austria, and Russia would not tolerate a policy that would “keep their bad people at home and let the good ones go. They want just the reverse.” In practice, he insisted that Davenport’s theoretical scheme was fundamentally unworkable, or so he argued.⁶⁷ Hall referred to the excessive “laboratory” influence in Davenport’s policy suggestions again in an October letter. There was a much easier, more practical solution to selecting immigrants, Hall insisted: simply prohibit the entry of large numbers of southeastern Europeans.⁶⁸ This disagreement over policy reflects a critical point overlooked in much of the historiography of immigration restriction: there was not a unified, monolithic “restrictionist” movement, but rather different

⁶⁷ Hall to Davenport, 20 August 1920, Davenport Papers; see also Davenport to Hall, 28 September 1920, box 1, IRL papers. The League disliked Post because he had a tendency to over-ride individual exclusions of immigrants on appeal. See, e.g. Patten to Hall, 14 March 1921, box 3, IRL papers.

⁶⁸ Hall to Davenport, 1 October 1920, Davenport papers. See also Robert DeCourcy Ward, “Immigration and the Three Per Cent Restrictive Law,” *Journal of Heredity* v. 12, no. 7 (August-September 1921), 319-26 which describes elements of policy that the League members favored.

background interests that gave several anti-immigrant groups differing strategies to protect old stock Americans.⁶⁹

The introduction of “Emergency Immigration Legislation” in the third session of the 66th Congress in 1920 saw the first serious push for the “quota” principle in immigration legislation, an implicit recognition of Laughlin’s influence.⁷⁰ Under this provision, immigrants must not only be literate, but could now only arrive in proportionate numbers to their share of the American population as represented in Census statistics. The Dillingham Commission had recommended restriction based on numerical proportion, but it conceded that in 1911 such regulations were not practicable.⁷¹ H.R. 14461, introduced in the House by Albert Johnson and referred to his Immigration and Naturalization Committee, proposed to temporarily suspend immigration “for the protection of the citizens of the United States.”⁷² When it arrived in the Senate, the bill was referred to the

⁶⁹ See Higham, *Strangers in the Land*; Jacobsen, *Whiteness of a Different Color*; Ngai, *Impossible Subjects*. This is only to point out the different strategies of restriction, and not to suggest these works are problematic. Restrictionists *were* unified on their goal: prohibit undesirable alien immigration. But examining in greater detail the intellectual rationales they provided yields a more nuanced picture.

⁷⁰ The goal of Laughlin’s testimony in “Biological Aspects of Immigration” was to convince the Committee on Immigration and Naturalization to “prevent any deterioration of the American people due to the immigration of inferior stock.” The quota provision, because it would “eugenically” pre-select its immigrant population from northwestern Europe would stem the deterioration of the American people by maintaining a consistent racial base (of superior character, of course, over the non-northwestern immigrant) to the population.

⁷¹ Restriction was recommended on a literacy test, exclusion of ‘birds of passage,’ and limiting “the number of each race...to a certain percentage of the average of that race arriving during a given period of years.” United States Immigration Commission, *Reports of the Immigration Commission*, v. 1, “Abstracts” (Washington, D.C., Government Printing Office, 1911), 48. Dillingham had proposed legislation containing a quota provision in 1916, but as the war had shut off most immigration, it received little attention. See Hutchinson, *Legislative History*, 166.

Committee on Immigration where it was dramatically reshaped. Chairman William Dillingham revived the quota idea, and replaced the bill with one that limited the number of “aliens” admitted to five percent of the number of “foreign born persons of such nationality resident in the United States as determined by the United States Census.” The Secretaries of State, Commerce, and Labor were directed to publish, as soon as feasible, the 5% quotas based on the census of 1910. The substitution presented the League leaders with a dilemma. This version would have been more palatable, because it did not contain any provisions for the overseas examinations that Hall and the IRL opposed, but the bill was still less acceptable because the original version of H.R. 14461 would have prohibited all immigration into the United States for fourteen months. Yet in either event, the League was nearing the success of their ultimate goal of restricting non Anglo-Saxon immigrants.

Dillingham stated that the key issue for supporting the quota bill was whether an emergency existed regarding immigration that would require such a dramatic change in policy as the complete suspension of all immigration. Relying on protracted hearings and testimony from “various interests,” as well as the 41 volumes of the *Reports of the Immigration Commission*, the Senate committee decided, Dillingham explained, that “no emergency exists at this time to warrant the adoption of the House bill...” prohibiting all immigration. At the same time, his report belabored the point that the new immigrants who were arriving, those

⁷² House Report no. 1109 parts I and II, “Temporary Suspension of Immigration,” 66th Congress, 3rd Session, 6 December 1920; House Conference Report no. 1351, “Temporary Suspension of Immigration,” 66th Congress, 3rd Session, 22 February 1921.

whom the legislation was aimed at excluding, were fundamentally different in kind from the “old immigration” that “consisted almost wholly of families who came to this country with the full intention of making it their home and becoming American citizens.” For Dillingham, it was this immigrant stock that was responsible for the successful development of agriculture in the “great Central West,” and the “construction of our incomparable transportation system.”⁷³ If immigrants of this type – families or pioneers, northern in European origin – could be induced to come, Congress was willing to let them in. Under the 5% quota regime, these families would have gotten the bulk of the quota.

The Senate Committee’s report included three tables that dramatically depicted the shift from “old” to “new” immigration that the Dillingham Commission had been so central in making. The principal character of the new immigration, Senator Dillingham explained, was that it consisted mostly of young, single males who came “to seek profitable employment,” and not to make homes and become citizens. While there was at present no emergency that would necessitate total prohibition of immigration, Dillingham reported that committee members felt that one was imminent, as many Europeans sought escape from the war-ravaged continent. Once business revived in the United States, the Vermont senator warned, immigration would likely exceed one million annually.⁷⁴

⁷³ Senate Report no. 789, 66th Congress, 3rd Session, “Emergency Immigration Legislation,” 14 February 1921 (Calendar day 15 February), 3 for “no emergency” and “wholly of families.”

⁷⁴ Senate Committee on Immigration, “Emergency Immigration Legislation,” 3-4 for tables; economics aspects discussed 5-6; 1 million immigrants from 7.

With this level of potential immigration in mind, returning to the quota principle was essential, Dillingham's Senate Report declared, because the 1910 census "afforded a new and more equitable basis for the purpose, and the plan was accordingly revised so as to limit immigration to a fixed percentage of the foreign born of each nationality resident in the United States."⁷⁵ Again, statistical data was of paramount importance. It provided a clear rationale and basis for restriction, since it was limited to proportionate population, and it sidestepped issues of quality and desirability that the literacy provision raised. The Senate Committee, with this allegedly more precise method in mind, recommended their bill be substituted for the House bill, while emphasizing that it would still have the effect of reversing the preponderance of southern and eastern European immigrants arriving in the United States. Northwestern European immigration averaged 182,850 per year between 1910 and 1914. During the same period, southeastern immigration averaged 738,612. With the Senate Committee's five percent plan, the number of northern immigrants that could be admitted was 337,020; the quota for southern immigrants was only 255,416. After being passed in the Senate, the lower chamber assented to the substitution after a conference committee recommended the Senate bill. Yet when it arrived at the White House on 28 February, President Wilson, incapacitated and a week from leaving office, refused to sign or veto the bill, and so the restrictionists waited for President-elect Harding to take office.⁷⁶

⁷⁵ Senate Committee on Immigration, "Emergency Immigration Legislation," 8.

In the opening session of the subsequent Congress, Johnson tried again and introduced a nearly identical percentage bill. It was again referred to his Committee on Immigration and Naturalization. The new bill reduced the percentage limit from five to three, but still used the 1910 census as the basis for quotas (the 1920 census remained in preparation). Johnson also explained that the need to use the older Census was a result of the war: the changes in geographical boundaries of European nations would, Johnson believed, render the 1920 census returns highly problematic in terms of determining nationality. The 1910 census also had the added advantage of giving a more substantial representation to the old immigration, which was more permanently settled than the transient “new” immigration during the same period, a point that Johnson made explicit in the report. Under the new percentages, 202,212 immigrants would be allowed from northwestern Europe, but only 153,249 from all areas “outside northwestern Europe.” The report justified this move by citing the lack of housing in urban areas where the new immigrants tended to settle, high American unemployment, the presence of large numbers of unnaturalized southern immigrants, increased dangers of communicable disease (an acutely sensitive issue in the wake of the influenza epidemic), and difficulties with assimilation to American values as reasons that immigration should be restricted more severely. Restricting the flow of immigration by the new quota system provided an essential appeal for eugenic-

⁷⁶ *Ibid.*, 9. Italian immigration under this formula would go from an annual average of 220,967 (nearly the total quota for southern immigration under the new plan) to 67,156. Hutchinson, *Legislative History*, 176. Also see the *Congressional Record*, 66th Congress, 3rd Session, Bill History: H.R. 14461.

minded nativists. “Any measure which checks the flow of immigration generally,” the report stated, “must necessarily result in the admission of fewer mentally and physically undesirable immigrants.”⁷⁷ The association was simple, if circular: by drastically reducing immigration from southeastern Europe, mentally and physically undesirable immigrants would be curtailed as well, because most immigrants from southeastern Europe were mentally and physically undesirable.

The competing plans to address immigration represented a wide divergence of Congressional opinion, although there was a decidedly pro-restrictionist sentiment in Congress. Johnson’s quota plan in the House was a compromise to the Senate plan that had failed to gain President Wilson’s approval. But when the House quota bill arrived in the Senate, members of the Committee on Immigration in the upper chamber performed an unusual about-face. Now the Senate, which in the previous session had substituted the House’s temporary prohibition on all immigration in favor of quotas, proposed a temporary prohibition on immigration, and substituted a bill to that effect for the House quota principle. The resulting conference between House and Senate members restored the quotas from the House bill and modified a few particular clauses dealing with aliens returning from visits to Europe. The House managers reported that the bill with these changes “makes H. R. 4075 more rigid in its restrictive effect than when it left the House.”⁷⁸ Passing the House and Senate, and

⁷⁷ House Report no. 4, “Restriction of Immigration,” 67th Congress, 1st Session, 19 April 1921, 2,3.

⁷⁸ House Conference Report, no. 62, 67th Congress, 1st Session, 12 May 1921, 5; Senate Committee on Immigration, “Immigration of Aliens,” Senate Report no. 17, 67th Congress, 1st Session.

no longer impeded by an anti-restrictionist Wilson, President Warren Harding signed the bill into law on 19 May 1921. American immigration policy had decisively changed. No longer were immigrants being screened or prohibited for particular deficiencies of quality (literacy, disease or poverty). Immigrants were now simply restricted because of their geographical origin or national identity, which were understood to be indicators of their racial character. In the ensuing years, the racial element of restriction would become less subtle and more drastic.

Laughlin played an important role in this gradual policy shift. After his weekend appearance before the House Committee on Immigration and Naturalization in 1920, Albert Johnson appointed Harry Laughlin the “expert eugenics agent” of the Committee, gave him Congressional franking privileges, and charged Laughlin with the task of conducting “further scientific studies” into immigration matters. When summoned before the Committee in November 1922, Laughlin presented some preliminary “scientific” findings to members, data that was sufficiently compelling to influence bills in the final session of the 67th Congress and the 68th Congress as well. Laughlin explained that the immigration act of 1921 and those proposed in 1922 had several promising features: the bills effectively limited total immigration to less than 400,000, they endeavored to geographically distribute immigrants so that they did not congregate in urban areas, and they contained regulations designed to select immigrants based on the “potentiality of the immigrant as a parent of desirable Americans of the future.” But all the bills introduced were temporary, and Laughlin insisted that a permanent, scientific solution needed to be found. After receiving a hearty

endorsement of his work by the Committee's chair – Johnson described Laughlin's data as "both biologically and statistical thorough" – Laughlin quickly moved to the crux of his policy suggestions.⁷⁹

Laughlin used statistical data to shore up support for his exclusionary suggestions. In the policy provisions he presented to the Committee, the expert eugenical agent invented his own quota to assess the "comparative degeneracy" and "the relative soundness of recent and older immigrant stocks." Compiling statistics of nativity from the 1910 census, Laughlin computed the relative proportion of foreign-born residents in public custodial institutions. Using Italian immigrants as an illustration, Laughlin explained that Italian-born immigrants to the United States numbered 1,343,123 in the 1910 Census, which was 1.46% of the entire American population. By his computation, Italians should thus only represent 1.46% of the population in custodial institutions, which would be 100% of their quota. Instead, in the 93 institutions he surveyed, out of 84,106 total inmates there were 1,938 Italians – 157.53% of their quota.⁸⁰ Laughlin also used results from the Army intelligence tests as further evidence of the mental inferiority and undesirability of the new immigrant groups, and thus emphasized the threat they posed to the United States. But first, to justify greater exclusionary policies, he turned to an examination of the literacy rates of the new immigration,

⁷⁹ Harry H. Laughlin, "Analysis of America's Modern Melting Pot," *Hearings before the House Committee on Immigration and Naturalization*, 67th Congress, 3rd session, 21 November 1922 (Washington, D.C.: Government Printing Office, 1923), 731, 729.

⁸⁰ *Ibid.*, 732. Laughlin's sources were state hospitals for the insane, federal and state institutions for feeble-minded individuals, and state and federal prisons. Herbert Spencer Jennings would severely criticize the sample Laughlin used in his own testimony before Congress, and in print as well.

and to justify more exclusionary policies, analyzed the impact the 1917 Immigration Act was having on improving the quality of immigration to the United States.

If the goal of immigration policy was to exclude undesirable immigrants, Laughlin testified, the policy was failing. The literacy provision was not having its desired effect. Literacy represented only “a partial, occasional, and indirect criterion of inborn mental capacity of a rather low level.” It was ultimately “more an examination of opportunity...than it is into native inborn and hereditary mental ability.” (Ironically, this was precisely the point that Colajanni and the American presidents had been making against the literacy test.) Many immigrants, Laughlin suggested, were carefully coached to read the few printed words required to demonstrate literacy, and were therefore circumventing the intent of the 1917 bill. When Representative John Cable (R-OH) asked if there might be a superior method of assessing the intellectual abilities of immigrants, Laughlin answered affirmatively, quickly suggesting an adaptation of the Army Alpha and Beta tests to be administered at immigration stations. These tests would definitively determine the mental capability of immigrants. If intelligence tests were applied to immigrants, he suggested, and the line for exclusion drawn between “low average (C-)” and “inferior (D),” 6,347,835 aliens would have been refused admission to the United States since 1917. “Native intelligence,” he informed the members, “does not depend upon opportunity or education. It is inborn;

consequently, it is transmitted from generation to generation.”⁸¹ Literacy was a test of opportunity; “intelligence” was a biological characteristic, something that was unchanging. Again, the paramount importance of hereditary ability, adhering to the lines laid out by Galton and Lodge in the late nineteenth century, and reiterated by Davenport’s *Naval Officers* (1919), was the principal thrust of the restrictionist rationale for exclusion. Immigrants were not only laborers and residents; they were also the parents of future generations of Americans. And, limited by low hereditary mental ability, future American generations from this inferior immigrant stock posed a considerable danger to the United States by reducing its intellectual quality.⁸²

Johnson pressed this line of questioning to get a better sense of the eugenic dangers of immigration into the record. He asked Laughlin to comment on the “biological or racial value of, or rather, the damage wrought by this type of [new] immigrant?” The Congressman feared that although individual feeble-minded aliens might be kept out by an intelligence or literacy test, lurking beneath the surface of every immigrant with normal intelligence that passed the tests could be a recessive trait that would produce degenerates in future generations. Laughlin answered that Americans looked past the inherent biological danger of immigrants because most of the public believed in the natural equality of all men, something that both Laughlin and Johnson believed that science and heredity

⁸¹ *Ibid.*, 736. This connection between heredity and intelligence – still heavily disputed at present – was one of the anchors of the classic racialist position, going back to Galton’s work on genius and talent.

⁸² Davenport, *Naval Officers: Their Heredity and Development*.
Chapter 5

disproved. Rather, they believed that all men were not created equal; some were biologically better than others. Laughlin cited as statistical proof of his argument the high proportion of the foreign-born insane in state and federal hospitals – they were 2.85 times more populous than the native-born population. He pointed out that although there was to some extent, a psychological and cultural adjustment and assimilation that did take place over time, the American-born children of foreign-born parents still had an incidence of insanity twice as high as the children of older immigration stocks. The criminality of alien stocks was also a pressing concern, which Laughlin suggested in a slight modification of Lombroso, was also inherited. The collective weight of the “expert” eugenical agent’s testimony was to impress on congressmen the extreme biological danger of inferior immigration to the nation – they could produce hereditary criminals or feeble-minded or insane offspring, which would burden the United States and lower the capability and quality of the American population.⁸³

Laughlin wrapped up his testimony by insisting that his research had proven the biological mental and physical inferiority of the “new” immigration. The institutional population of foreign-born aliens spoke to real differences in social values, “which represented, in turn, real differences in the inborn values of the family stocks from which the particular inmates have sprung. These degeneracies and hereditary handicaps are inherent in the blood.”⁸⁴ When asked

⁸³ *Ibid.*, 741.

⁸⁴ *Ibid.*, 752.

by a committee member if he had any policy suggestions, Laughlin obliged by endorsing increased restriction, improved latitude for American officials to deport aliens, a system of universal registration of the foreign-born, and Davenport's suggestion of overseas inspection of immigrant's family lines. "The conclusions which one may logically find at the termination of a research," he testified, "are those which are based upon data and conditions; they should not be influenced by sentiment or previous attitude."⁸⁵ Laughlin believed that he had demonstrated unequivocally the biological danger that immigration posed to the United States. If this new and undesirable immigration were not effectively excluded, he argued, the consequences to the American population would be grave. He believed these new immigrants were hereditarily inferior and irredeemably inadequate, and scientific knowledge and statistical evidence had proved this beyond a shadow of a doubt. But Laughlin, Davenport, Grant, Stoddard and a host of others were obviously influenced by sentiment or previous attitude. They intentionally selected certain types of evidence that would "prove" their claims, a point that professional academics would point out in the coming years. Real biologists and scientists knew that science and heredity had proven no such thing. Laughlin's disingenuous testimony drove one thoughtful biologist, Herbert Spencer Jennings, to re-examine Laughlin's Congressional data with a critical and scientific eye, and publish an opposing interpretation.

In addition to securing Laughlin's "expert" scientific testimony, Albert Johnson sought to influence immigration legislation in the 1920s in other ways.

⁸⁵ *Ibid.*, 755.

To secure more drastic limits on immigration, Johnson began packing his House Committee on Immigration and Naturalization with restrictionists. In previous sessions of Congress, the Democratic party had been demanding additional representation for two members from large east coast cities who would presumably be opposed to restriction, and Johnson had to oblige. Still, the Committee chair could secure ten restrictionist votes on a committee made up of seventeen members. As he recast the Committee's membership, Johnson informed Robert DeCourcy Ward, the two additional Democratic seats would probably have to go to anti-restrictionists, "in order that it may not be charged that the Committee is absolutely packed." Securing membership on the committee favorable to restriction was crucial for opponents of immigration since the 1921 bill was only an emergency measure that had to be re-authorized every fiscal year. So a majority vote from the Immigration and Naturalization Committee in favor of reversing the emergency bill was critical. But while preserving this significant gain for restriction was important, the long-term plan of Johnson was to secure something much more far-reaching and permanent. He needed sympathetic minds on his committee. And he worked to make sure that the 68th Congress would deliver.⁸⁶

⁸⁶ On the enlargement of the Committee, Johnson said that all Republican members had a restrictionist inclination, which, "in my opinion, will make the work of the Committee easier than before." Albert Johnson to Robert DeCourcy Ward, 3 December 1923, Records of the House of Representative, Record Group 233, Committee on Immigration and Naturalization, National Archives and Records Administration, Washington, D.C. [hereafter RG 233], box 262, NARA. See also, in the same box, Johnson to Richards Bradley 10 December 1923, in which Johnson details a meeting with Patten at his Washington, D.C. residence in which they discussed "several matters which may have to be offered in Committee as amendments to the restrictive bill which we are now trying to perfect."

First, however, the members of the House Committee on Immigration and Naturalization had to sort out desirable elements from bills proposed in the 67th Congress, and unify behind one policy. The various political approaches to immigration left many options. Although the 3% quota provision from the 1921 bill had a significant impact on reducing the total number of European immigrants, its statistical foundation on the population from the 1910 census meant that large numbers of undesirable European immigrants were still to be admitted. Because of the large volume of southern and eastern European immigration that arrived after 1880, when the census basis moved to more recent enumerations, southern European immigrants would be accorded, based on their statistical representation in the population at large, a greater proportion of the quotas. Thus, a quota based on the 1900 census would allow a smaller proportion of southern immigrants than one based on the 1920 census because of the smaller statistical representation of these populations in the older Census. This would come to be of paramount importance in the 68th Congress.

Advocates of further limits on immigration reached out to Johnson as he sifted through the various proposals to ensure that he selected a mechanism that would be sufficiently restrictive. Madison Grant, who had become close to Johnson, used the winter of 1923 to try to influence Johnson's actions on the upcoming legislative agenda and to encourage politicians to pass a law that would save the racial basis of the American population. As he reviewed the immigration bills that had been introduced in the House and Senate, Grant found a particularly troubling clause in a bill offered by Pennsylvania Senator David Reed. Grant

believed that basing an immigration quota on the percentage of the rates of naturalization by immigrants – as Reed’s bill proposed – was counter-intuitive. Race was a biological category and could not be changed by adopting American citizenship; simply because an immigrant had become a naturalized citizen did not, in Grant’s mind, make the immigrant an “American.” “From the scientific point of view,” Grant explained, “this basis is a thoroughly wrong one and does not give us racial selection which, after all, is what we are after and is our strongest card.” The right to vote, a privilege of a naturalized citizen, he told Johnson, did not bring immigrants “any nearer in sympathy with our institutions and methods of thought.” Careful selection of which immigrant groups would be admitted was absolutely crucial to ensure that immigrants had the same philosophical outlook, which in Grant’s mind was inextricably related to their racial identity. Even granting Ireland a separate quota from the British Isles would be dangerous; Grant worriedly pointed out the shaky performance of the Irish on the Army Intelligence Tests, and Laughlin’s study of the racial background of immigrants in custodial institutions had shown the Irish “in a very undesirable light.” They tested poorly and made up a disproportionate share of the population of custodial institutions, two empirical “proofs” of their inferiority in Grant’s mind.⁸⁷

The IRL members understood the appeal of quotas too. In several of their mailings and popular articles, they favored the census of 1890 as a baseline for

⁸⁷ Grant to Johnson, 22 December 1923, RG 233, box 264, NARA. Grant also tried to bring the case to a broader public; see also Madison Grant, “America for the Americans” *The Forum* v. 74, n. 3 (September 1925), 346-55.

computing immigrant quotas, since the number of immigrants from the south and east of Europe was, in that enumeration, comparatively small. Correspondence between Johnson and League members in the spring of 1924 made the concerns of the League explicit, but also showed their policy flexibility along with the tenacity of the League's leadership. "The whole controversy," Richards Bradley pointed out to the Washington congressman, "is over which quota, and what percent." For Bradley, choosing the 1890 or 1910 census was irrelevant; he felt no "strong preference" for either basis provided, as he emphasized, "the percentage in either can be kept down."⁸⁸ The League's leaders were willing to be flexible as to which census was used to calculate the quota, so long as the total number of immigration was kept as low as possible. The League membership's ultimate desire was not to ensure a higher quality of all immigrants, which is what the literacy test tried to establish, but to exclude, to the greatest extent possible, non Anglo-Saxon races from entering the country (though these mechanisms were not mutually exclusive). Six weeks later Bradley again wrote Johnson imploring him to have the final conference bill provide "as good a fighting position for the future as possible." While this encompassed a combination of the literacy test and strict quotas, Bradley mentioned a better strategy that the League's leaders were now considering. Bradley endorsed a "Racial Origin Provision" – which would eventually culminate in the National Origins quotas – since it would make "very superior fighting ground....It effectively knocks the ground from under those who

⁸⁸ Bradley to Johnson, 4 March 1924, RG 233, box 262, NARA. Emphasis original.

have attacked the 1890 apportionment as discriminatory even though it is less favorable to what they want.”⁸⁹ On this basis, quotas would not be determined “nationally,” based on the number of immigrants from European countries that were counted in any given census, but would instead be based on preserving determine the racial composition of the population already in the United States. “Racial Origins” would restrict immigration to the United States to those races that were similar in character to the American population.

This was recognition of Grant’s critique that rates of naturalization had nothing to do with biological or racial identity. This was a plan that explicitly attempted to preserve the racial homogeneity of the United States’ population as nativists imagined it. Classic racialism in this case would come to the aid of legislators to help them craft an ostensibly objective, non-discriminatory system of immigration regulations – it would be objective and non-discriminatory because it was to be based upon scientific principles of racial biology. But there *was* no actual science of racial biology. Biologists and geneticists were slowly discovering that physical and mental characteristics were not “racial” but were instead the complex results of chemical interactions of genes and chromosomes. Professional scientists understood that it was almost impossible to predict a person’s development or characteristics by examining their ancestors or geographic origins alone. Heredity was vastly more complicated than the simple, flat axioms of classic racialism that like produced like, and that blood and race were immutable, unchanging characteristics. Restrictionists and nativists were

⁸⁹ Bradley to Johnson, 20 April 1924, RG 233, box 262, NARA. Emphasis original.

proposing to devise immigration policy on wholly incorrect understandings of biological theories, and actual biological researchers finally began to push back. As later chapters will demonstrate, however, this resistance had no effect. The erroneous claims of classic racialism would instead become the foundation for immigration policy.

Nationally, restriction was gaining support. As Bradley was laying out potential legislative strategies, the House Committee was inundated with petitions from organizations and individuals seconding the IRL's agitation for increased restriction. John A. Jeffrey, the Exalted Cyclops of the Seattle Ku Klux Klan, dispatched a telegram to his Congressional representative, Albert Johnson, informing him of the unanimous support of the Seattle Klan for the 1890 census as the basis for an immigration quota. The Klansman also voiced the concern of his hooded-brethren that the complex issue of Asian immigration also had to be addressed in future legislation. "The safety of the most treasured ideals and institutions of the white race," he wrote, "as well as the purity of the same are at stake."⁹⁰ Restrictionists tried to avoid having policy based on emotional or visceral appeals to racial prejudice, so they consistently pushed the "scientific" claims and evidence that they believed biology provided. They could more easily avoid charges of racial discrimination by opponents if restriction was not attributed to emotion, but to objective, rational "science" instead. The great appeal of using statistical calculations as a basis for restriction was that it bypassed

⁹⁰ John A. Jeffrey to Albert Johnson, 24 March 1924, RG 233, box 262. See also Ngai, *Impossible Subjects*.

discussions of race, quality, and desirability with a simple statistical representation that could be presented as unbiased and non-discriminatory because it *was* statistical.

Laughlin again played a key role in providing evidence in support of further restriction even after the first quota acts went into effect. After his presentation before the Committee on Immigration and Naturalization in 1922, Johnson secured government credentials for the eugenicist to travel to Europe as a U.S. “immigration agent.” For six months, Laughlin met with European policy makers, diplomats, and parties concerned with immigration. As the Dillingham Commission had done, Laughlin tried to gain first-hand knowledge of the realities of the immigration situation in Europe, and to gauge the attitudes of European nations to potential American policy changes. But Laughlin’s trip was not necessarily a fact-finding mission so much as it was a chance for him to secure information to justify the positions restrictionists like Albert Johnson wanted. When Laughlin was invited to provide a preliminary report after six months in Europe in March 1924, Johnson introduced Laughlin by laying great importance on the “value of [Laughlin’s] first hand field-data skillfully collected and scientifically analyzed...” Laughlin began his presentation with an overview of the historical immigration policies in America, beginning with the asylum ideal, economic migration and finally, the most important immigration policy, the biological or eugenical basis. By now his lines were well rehearsed.

Laughlin argued that American immigration policy should be fundamentally selective. “Whenever a person who is a potential parent is admitted

to the country,” he said of immigrants, “and this person is not a member of the racial groups already established, then this new arrival, in order to compensate for the racial differences, should possess inborn and hereditary traits of most superior value.”⁹¹ Laughlin laid out the character traits that he believed were most desirable for parents of future Americans: “truthfulness, inventiveness, initiative, dependability, altruism, honesty, religious feeling, artistic sense...the bases of which are inborn in the individual and which vary greatly in family strains, can be determined by field studies of individuals and families.” There were also physical qualities of comeliness, longevity, strength and stature, and immunity to disease that would be desirable characteristics for immigrants. Laughlin explained that with all of these qualities, an immigrant’s “hereditary aspect can be located and measured not so well by clinical examination in a strange laboratory as by such examinations when supplemented strongly by field investigations.”⁹² It had worked with the Kallikaks and the Jukes, and Laughlin believed investigations into immigrant genealogies would work in a similar way. Immigrants with undesirable characteristics in their families would be excluded, on the basis of “expert” investigators.

This was the basis of the work being conducted by Laughlin’s home institution, the Eugenics Record Office, which correlated and sorted genealogical

⁹¹ Harry H. Laughlin, “Europe as an Emigrant-Exporting Continent and the United States as an Immigrant-Receiving Nation,” *Hearings before the House Committee on Immigration and Naturalization*, 68th Congress, 1st Session, 8 March 1924 (Washington, D.C.: GPO, 1924), 1260, 1236-37. This eugenic ideal of breeding a population with superior characteristics was to be, of course, mirrored in the Nazi regime in Germany, which ardent American eugenicists closely followed. See Stephan Kühl, *The Nazi Connection: Eugenics, American Racism, and German National Socialism* (New York: Oxford University Press, 1994).

⁹² *Ibid.*, 1277.

data into familial lines, which proved, in Laughlin's mind, the immutability of degenerate and inferior stocks. To demonstrate the potential dangers of not selecting immigrants with these superior qualities in the way he recommended, Laughlin again turned to population statistics and Walker's law, which he noted, "has never been disproven...."⁹³

Laughlin consistently mishandled and misused scientific evidence to support his policy goal of the eugenic selection of immigrants. He had no genuine scientific training, no research experience in modern laboratories, and no grasp of modern methods of scientific investigation. Convinced of classic racist notions that liked always produced like, Laughlin flattened out significant complexities in scientific information and knowledge to advocate for and influence Congressional support for greater immigration restriction. He regarded science as a tool, an implement, that could – and should – be used to guide policies toward immigration. Laughlin was not interested in scientific knowledge for its own sake. Jennings, however, was. Going back to his early training, Jennings understood the importance of experimental research to obtain knowledge of scientific principles, and the necessity of using precise, modern scientific methods to carefully build up the edifice of biological and genetic knowledge. That was the goal of scientific research. For Laughlin to misrepresent and misconstrue what truths and proof scientific research had established drove Jennings to action.

Laughlin's "expert" testimony and the forum that Johnson gave him before the House Committee was intended to demonstrate their scientific

⁹³ *Ibid.*, 1299; 1308-09.

arguments in favor of restriction. While before the Committee, Johnson also provided Laughlin the opportunity to refute criticism of his previous testimony from actual professional scientists. “Let me say,” Laughlin declared, “that my studies were made with no preselected theory or policy to support. There were no conclusions drawn before the data were in hand and analyzed, and the conclusions were in sound keeping with the first-hand facts.” Most critics of his previous testimony (and there were several), Laughlin noted, had done no research in the field, and future critics should be expected to disprove his assertions not by “badgering or guesses,” but by assembling facts derived from first hand research. “A critic who had made no field studies of the sort could hardly be taken seriously when he advises what might have been found had studies been made under different conditions. He should, by all means, proceed to make studies under those conditions.”⁹⁴ There was only one critic, Laughlin felt, whose dissent should be taken seriously: the biologist Herbert Spencer Jennings.

In 1923, Jennings wrote an article for *The Survey* in which he challenged the findings and evidence that Laughlin had presented to the House Committee in 1922 as Expert Eugenical Agent. When Laughlin described Jennings’s *Survey* article to the Committee, he insisted that Jennings had not challenged the “honesty of the survey nor the representative nature of the population studied.” Before the Committee, Laughlin twisted and misrepresented Jennings’s paper to

⁹⁴ *Ibid.*, 1311-12. This problem of “first-hand” research was fundamental to the classical racist/modern experimentalist conflict. As Ripley’s work showed, accurate measurement of a wide variety of samples conducted first-hand were practically impossible. Yet it was essential for any compilation and analysis that the measurements were accurately conducted, a problem critics of recent studies, like Davenport’s *Army Anthropology*, pointed out.

suggest that the biologist was actually in agreement with Laughlin's findings, which Jennings was not. "In the end Professor Jennings comes to the same main biological conclusion...namely, that taking into consideration the racial distribution of the inmate populations of our present institutions for the socially inadequate, it would probably be necessary for our future immigration policy to consider the matter of family stocks in admitting immigrants." This was a gross distortion of Jennings's paper. But, as was so often the case for Laughlin and his eugenicist cohorts, facts and reality could not be allowed to interfere with their mission of protecting the Anglo-Saxon racial stock in the United States. When Representative Samuel Dickstein (D-NY) asked him if "the immigrant from northern Europe is more desirable than from southern and eastern Europe?" Laughlin avoided answering, although the whole of his testimony had implicitly argued in the affirmative. "Desirability," he tactfully stated, "is a matter of policy. The races of northern and western Europe, more closely than the races of southern and eastern Europe, resemble the main body of the American people. I made this biological investigation and put the facts on record here for the benefit of the committee, which must draw its own conclusion. I am not here as an advocate for or against any race." But he was an advocate. He advocated using eugenical and class racist principles, which were disputed in the professional scientific community, as a basis for immigration restriction. His recommendations were before the Committee: immigrants should "possess inborn and hereditary traits of most superior value." He may have denied having any special interest, or being "a

special pleader,” but the classic racist ideas that he believed confined those superior traits only to the northern and especially Anglo-Saxon races.⁹⁵

In an attempt to seem objective, and perhaps to placate the anti-restrictionists on the Committee like Dickstein, Jennings was called to offer testimony in January 1924. The biologist, however, was only allotted a short amount of time to present his evaluation of Laughlin’s evidence and to correct Laughlin’s interpretations of his work.⁹⁶ After making his public critique of Laughlin’s research in *The Survey*, Jennings had prepared additional statistical evidence for the committee members that analyzed the implications of shifting the census base for computing quotas from 1910 to 1890. Changing the quota basis from 1910 to 1890 would decrease the number of criminal, tuberculous-infected, and feeble-minded immigrants, Jennings demonstrated, but the number of insane, epileptic, and dependent immigrants would proportionately increase. These defects were not due to *racial* failings of immigrant groups, but were instead to be attributed to environmental, cultural and contextual issues that all immigrants faced. The net effect of moving the census basis, he suggested, would be that “these various increases and decreases would offset one another, so that the total

⁹⁵ *Ibid.*, 1318; 1236-37. Dickstein was one of the seven “antis” that Johnson had on the Committee so that it was not “completely packed” See Johnson to Robert DeCourcy Ward, 3 December 1923, RG 233, box 262, NARA. Johnson had tried to prevent Laughlin from answering the New York congressman’s question by declaring that Dickstein’s query was not “a fair question.” Laughlin, “Europe as an Emigrant-Exporting Continent,” 1318. Johnson also reassured “Doctor Laughlin” not to worry about criticism: “you have developed a valuable research and demonstrated a most startling state of affairs.” 1311. Herbert Spencer Jennings, ““Undesirable Aliens’: A Biologist’s Examination of the Evidence before Congress” *The Survey* v. 51 (Dec. 15, 1923), 309-12, 364.

⁹⁶ House Committee on Immigration and Naturalization, “Restriction of Immigration,” *Hearings before the House Committee on Immigration and Naturalization*, 68th Congress, 1st Session, December 26, 27, 31, 1923 and January 2-5, 7, 8, 10, 19, 1924 (Washington, D.C.: Government Printing Office, 1924), 511-13.

number of defectives would remain practically the same.” To move the quota’s foundation from the 1910 to the 1890 census would not limit the aggregate number of defectives. This substantially undermined a justification for the shift in the census. Moving the census base had only one real reason behind it, namely that it would limit the number of non-Anglo-Saxon immigrants. Yet Jennings remained cautious, unlike Laughlin, of making sweeping declarations. He recognized that compiling statistical data on immigration and the mental and physical characters of immigrants contained many uncertainties.⁹⁷

Jennings’s hesitations reflected a professional recognition of the uncertainties of biological laws of heredity and genetics. Scientists did not yet know specifically how chromosomes and genetics worked on a cellular level, so their understanding of hereditary transmission remained hazy and tentative. But how could uncertainties and ambiguities help in crafting policy? If genuine “experts” could present no consensus or authoritative conclusions, what was the use of enlisting their help? Laughlin was *created* an expert by the chair of the Committee despite no actual professional training in experimental methods, biology, zoology or genetics. Eugenics was not an established or really viable field of scientific research, as Morgan’s results on chromosomes were gradually making clear. He was willing to help by declaring certainties regarding biological development of human beings. Jennings *was* an expert by training and

⁹⁷ *Ibid.*, 511-13. Jennings’s written report, titled “The Relative Numbers of European-Born Defectives from the Chief Sources of European Immigration and the Effect of a Change in the Basis of Admission, from the Census of 1910 to that of 1890” was submitted to the committee on 4 January 1924.

temperament. He understood that experimentation and careful observation were the key components of legitimate scientific knowledge. But Laughlin afforded certainty, and Jennings did not. Laughlin was not committed to high standards for scientific knowledge and research so he could manipulate or misrepresent what science proved. Jennings could not.

Jennings was not the only one to criticize the methodology and assertions that Laughlin had made in his “Melting Pot” testimony. At hearings before the Committee in December and January, Rufus R. Lutz declared that the sample Laughlin used resulted in findings that are “manifestly erroneous.” As Jennings also pointed out, Lutz demonstrated that Laughlin’s selection of state and federal custodial institutions neglected private custodial institutions, where more affluent and established “defectives” could take refuge. Laughlin, Lutz charged, was cherry-picking information to support Laughlin’s *a priori* conclusion that immigrants were undesirable because so many of them became public burdens. “This fundamental mistake,” he insisted, “affects the accuracy of every quota fulfillment figure in the report.” Johnson tried to deflect Lutz’s criticism by insisting that limitations of time precluded calculations involving anything more than county institutions and asylums, poorhouses, minor jails and tuberculosis asylums. This was unacceptable, Lutz maintained. “I take the point of view of the scientist in these matters, that after all, if we are going to think about this, let us get our figures right. Do not use statistics that will not stand careful scientific examination.” Laughlin’s facile manipulation of statistical data to support further restriction of immigration was particularly egregious, according to Lutz. After he

pointed out some glaring inaccuracies in the Army mental tests and in Laughlin's computation of quota-fulfillment of inadequacies and inferiority, Committee member John Box (D-TX) attacked the personal credibility of Lutz in an attempt to undermine his criticism.⁹⁸

Not all of the debate on the legislation that was pending in the Sixty-Eighth Congress's first session took place in the chambers of the House Committee on Immigration and Naturalization. In late 1923, the Committee on Selective Immigration of the Eugenics Committee of the United States of America – of which Albert Johnson was a member, Madison Grant the Chairman, Robert DeCourcy Ward the Vice-Chairman and Laughlin the Secretary – issued a draft report on immigration legislation before Congress. The report endorsed using the 1890 census as the basis for quota computations and provided for the overseas inspection of intending emigrants, acting on Laughlin's suggestions to Johnson's Committee in 1921. The draft report justified the shift from 1910 to 1890 specifically because using 1890 “would decidedly cut down on the numbers of [southeastern] immigrants. This provision would change the character of immigration, and hence *our future population*, by bringing about a preponderance of immigration of the stock which originally settled this country.” Statistics, immutable racial characteristics, the paramount role of biology and heredity in shaping human development – the Committee on Selective Immigration's report

⁹⁸ House Committee on Immigration and Naturalization, “Restriction of Immigration,” *Hearings before the House Committee on Immigration and Naturalization*, 68th Congress, 1st Session, 253, 255, 256. Lutz testified that the Army intelligence tests were highly flawed methodologically: one-third of the subjects for the Beta test were Italian, but they constituted only 14% of the immigrant population. 268-9.

brought all these facets together to justify more drastic exclusion. Aided by classic racist ideals, the Committee on Selective Immigration declared that changing the character of immigration was especially desirable since “immigrants from northwestern Europe furnish us the best material for American citizenship and for the future upbuilding of the American race.” These immigrants, the report noted, had higher standards of living, “are of a higher grade of intelligence,” were skilled in trades, and “better able to understand, appreciate, and support our form of government.” The members also cited the work of Robert M. Yerkes and Carl C. Brigham on intelligence in army recruits, as well as Laughlin’s testimony before the House Committee on Immigration and Naturalization on “The Melting Pot.”⁹⁹ When the Smithsonian Institution’s Ales Hrdlicka received a copy of the draft from Yale professor Irving Fisher, both of whom were members of the Selective Immigration Committee, he scribbled several marginal notes in the sections where the “scientific” arguments were most spurious. These the anthropologist passed back to Fisher in November 1923.

As a member of the Committee on Selective Immigration, Hrdlicka had initially accepted the position “with the hope that it would lead to a thoroughly unbiased scientific research in the field of the many serious problems that relate to the future development and betterment of the American people.” While some of the material in the report was good, Hrdlicka felt that “the recommendations are more of a political than scientific nature.” Especially troubling was the

⁹⁹ Committee on Selective Immigration of the Eugenics Committee of the United States of America, “Report of the Committee on Selective Immigration,” n.d. [ca. late 1923], 2, copy in Jennings papers. Yerkes, Brigham and Laughlin are cited on page 3 of the report. Emphasis added.

“particularly unfortunate” utilization of Brigham’s work, which the anthropologist noted was “imperfect and already rather discredited.” The political use of questionable scientific claims to influence the public and Congress led Hrdlicka to request that his name be removed from the membership roll of the Committee.¹⁰⁰

Before the Committee on Selective Immigration circulated the draft report, Hrdlicka had warned Fisher of the fundamental uncertainties at the heart of eugenic claims that the group was using to justify their positions. Eugenacists should not be interested in influencing legislation, Hrdlicka said, but should rather earnestly investigate the nature of the germ plasm, the natural and environmental influences that influence it, and the mechanisms of transmission of hereditary qualities. These advances in knowledge, the anthropologist insisted, could only be gained by “continued research surpassing in quality and intensiveness everything done before.” Convinced of the superiority of work conducted in this way, Hrdlicka told Fisher that he could not help but disapprove of the preliminary report that was based on faulty and deductive reasoning, and urged care in dealing with immigration matters from a eugenic standpoint, singling out in particular Grant’s *Passing of the Great Race* as a major source of misinformation about eugenics and its possibilities. Bristling at the accusation that the Selective Immigration committee was manipulating scientific knowledge, Fisher replied that Hrdlicka was mistaken in his perception: “it is the object of our Committee to secure as thoroughly unbiased a scientific research and action in all eugenic fields

¹⁰⁰ Irving Fisher to Ales Hrdlicka, n.d. [ca. late November 1923]; Hrdlicka to Fisher, 22 November 1923, box 24, Papers of Ales Hrdlicka, National Anthropological Archives, Smithsonian Institution, Washington, D.C. [hereafter Hrdlicka papers].

as we can get and not to pursue political purposes.” It was essential for eugenicists to assist Johnson in making “his bill as eugenical as possible,” and Fisher declared that Johnson would introduce the legislation with or without the support of the eugenical movement. A bit of bias and race prejudice, Fisher felt, was unavoidable when it came to the practical application of eugenic principles. All the more important, he felt, to have properly scientific men assist in the creation of legislation.¹⁰¹

Hrdlicka found Fisher’s position to be absurd on many levels. The purported neutrality of the committee’s leadership was especially farcical, considering the composition of the Committee; there were very few professional scientists on it. Particularly egregious was the appointment of Madison Grant to be the Committee’s chair. Hrdlicka pointed out that Grant’s “widely known” bias in favor of “the northern peoples” would completely undermine the objectivity of the Committee and raise “much deserved antagonism and create suspicion as to the true aims of the Committee.” Johnson’s position on the Committee was equally problematic – the Congressman had attended a series of Hrdlicka’s lectures two years before and “showed that he held strong pre-formed opinions.” Of course, this was exactly the reason that restrictionists worked with the House Committee on Immigration and Naturalization. The only thing the Committee on Selective Immigration should do, Hrdlicka said, was admit that “we have thus far

¹⁰¹ Hrdlicka to Fisher, 7 June 1923, box 24, Hrdlicka papers. As Fisher was not swayed by Hrdlicka’s suggestions, in October Hrdlicka reiterated his warning that certain members of the Committee on Selective Immigration “are men who by their recent publications have shown themselves either not sufficiently careful or decidedly biased.” Hrdlicka to Fisher, 12 October 1923, box 24, Hrdlicka papers.

no certain knowledge about the mental superiority or inferiority of any branch or branches of white people.” To assume that the geographic origins of a people consigned them to permanent racial inferiority was ridiculous and untenable because there was no scientific evidence, anthropological, biological or genetic, that could compellingly prove “that any one group has superior or inferior endowments mentally or even physically.” If the Committee were to issue a genuinely scientific report, they would have to respect the limitations of scientific knowledge.¹⁰²

Hrdlicka’s criticisms highlight the key elements of the emerging chasm between classic racialism and experimentalist methods. The importance of objectivity in light of the problematic testimony given by Laughlin and popularized in Grant’s polemical books undermined any scientific value of the comments or arguments they made. There was a pronounced difference between following data and manipulating or perverting data, and objectivity remained the key aspiration in the new methodologies. Advances in knowledge of genetics in this period was unstable and complex, there was a lack of consensus in anthropology as to how stable and unchanging psychical body form was, and biologists still did not fully understand the relative influences of the environment on the development of organisms. To speak of scientific knowledge as establishing or proving the biological inferiority of certain population groups undermined the slow process research was making. Hrdlicka’s hesitancy to

¹⁰² Hrdlicka to Fisher, 27 November 1923, box 24, Hrdlicka papers. Fisher was a long-time professor of Political Economy at Yale University.

ascribe characteristics to a purely biological origin, to privilege nature over nurture, reflected the increasing appreciation of professional scientists had for the complexity of development. With all these things in mind, it is no wonder Hrdlicka began to untangle himself from the Selective Immigration Committee. But the unwillingness of the Yale professor to follow suit shows that Hrdlicka's criticisms lacked any real impact, a problem critics of classic racialism would continue to have.

When the Committee on Selective Immigration finalized its report, members tried to keep it confidential so as to maximize its political impact. The hope was that it would help consolidate public opinion behind the 1890 census proposal and thus justify a more severe level of restriction for the "new" immigration.¹⁰³ Keeping the report under wraps was important, since at the same time that it was being prepared, Johnson's congressional committee was holding hearings on immigration restriction, and petitions from pro- and anti-immigrant forces were flooding into Congress. Since Johnson was on the Selective Immigration Committee, it was important to maintain the appearance of open-mindedness on the part of legislators. A sample of the petitions shows that anti-immigrant forces consumed, completely and uncritically, the determinist logic of classic racialism, and pro-immigrant forces tended to the environmentalist side. For instance, a petition to Johnson in January 1924 from the Napoleone Colajanni Lodge of the Order of the Sons of Italy despaired of being able to correct Johnson's preconceptions. "Prejudice has moved the framing of your bill," its

¹⁰³ Robert DeCourcy Ward to Albert Johnson, 17 December 1923, RG 233, box 267, HR 68A – F 18.1, NARA.

secretary declared, “poor data and perverted statistics have given it shape and a misconception of ideals and of human wisdom shall attempt to have it passed.... We cannot urge the remodeling of a fixed idea, we can only hope that your bill shall not pass, and we wait to prove that our hopes will not have been in vain.”¹⁰⁴ An Italian aid society from Youngstown, Ohio appealed to Johnson to use the 1920 census as the basis for computing quotas, which the group felt would be more impartial and fair because it was simply the most recent and accurate accounting; the group felt that the movement to the 1890 enumeration was being facilitated by the use of misleading propaganda.¹⁰⁵

At the other end of the spectrum, petitioners in favor of exclusion trotted out classic racist orthodoxy to insist that draconian immigration restriction be placed on a permanent basis, and admit as few immigrants as possible. The Spokane, Washington chapter of the Daughters of the American Revolution wrote to Johnson expressing their full support of his bill. Some petitioners wrote missives that made little sense logically: the editor of the Shelby, North Carolina newspaper insisted that if immigration were not curtailed, the United States would go the way of Ancient Greece and Rome. These illustrious civilizations, he explained, “were the homes of greatness only so long as the Nordic strains of blood survived in the blood of their peoples. In a very few centuries

¹⁰⁴ Samuel Albo to Albert Johnson, 26 January 1924, RG 233, box 264, HR 68 A – F 18.1, NARA.

¹⁰⁵ Michele Colomma to Albert Johnson, 4 February 1924, RG 233, HR 68 A – F 18.1, box 263, NARA. Johnson replied that once the restrictive quotas were enacted, Italians already present in the United States would be among the chief beneficiaries of the law, as they would enjoy increased prosperity, “In my opinion, the children of the Italians now in the United States will wish that a restrictive immigration law had been passed earlier.” Johnson to Colomma, 9 February 1924, *ibid.*

MONGRELISM did its deadly work and destroyed their greatness.”¹⁰⁶ Grant had made this case, with no evidence in support of it, that ancient Rome was a Nordic civilization. Jordan had attributed the decline of Rome to the absence of the *Vir*. Although completely spurious arguments from a historical perspective, they were influential.

Johnson and his Congressional committee continued drafting a bill as these petitions arrived in D.C. When H.R. 7995 was reported out of the House Committee on Immigration and Naturalization, the confusion in racial thinking followed the bill to the floor of the House and the Senate. Just as it was being played out in the private meetings of the Galton Society in New York, in the Committee on Selective Immigration of the Eugenics Committee of the United States of America, and in Congress, uncertainty over what role heredity and environment played in human development impaired action on the bill. The House Report attached to the bill contained a strong minority report that disagreed with major provisions in the bill, particularly the shift from the 1910 Census to the 1890 Census which the minority hinted was undertaken for no legitimate reason other than the nativist and restrictionist agenda.¹⁰⁷ Immediately upon its reception in the House chamber, members began tensely arguing about the shift

¹⁰⁶ B. H. DePriest to Albert Johnson, 12 March 1924, RG 233, HR 68 A – F 18.1, box 263, NARA, emphasis in original.

¹⁰⁷ House Report no. 350, Part 2, “Restriction of Immigration: Minority Report” submitted by Adolph Sabath, 68th Congress, 1st Session, 27 March 1924, 2. Sabath’s report insisted that “There is an avowed reason for changing the basis of calculation to the census beginning 34 years ago.... It is to admit a minimum of immigrants from eastern and southern Europe and a maximum from northern and western Europe.” 3.

from 1910 to 1890, and others pointed out that the quota provisions left a gaping hole in the western hemisphere, where migration from Canada, Mexico and the Caribbean were not restricted at all.

Illinois Representative Adolph Sabath, an opponent of restriction who co-wrote the minority report, decried the false racial anthropology that supported the claims of restrictionists. Sabath was himself an immigrant from Bohemia, settling in Chicago after his arrival in the United States in 1881. He curtly pointed out that the immigrants whose racial inferiority now deemed them undesirable had been very desirable allies in the war against Germany. But this hypocrisy, he noted, was minor compared to the “unfounded anthropological theory” that underlay the antipathies toward southern and eastern European immigrants, and that venerated the Nordic race as the highest point of human development. “No scientific evidence worthy of consideration,” Sabath criticized, “was introduced to substantiate this pseudoscientific proposition.” The Illinois Democrat declared to members that he found it “amusing” to see the majority report declare northern immigrants so superior, when anyone with knowledge of immigration would have seen Americans fulminating in the last 100 years against Irish, German, Austrian and Scandinavian immigrants who were now lumped in with the Nordic races.¹⁰⁸ With that opening salvo fired against the restrictionist camp in Congress, debate in the chamber over the next few days became increasingly contentious.

¹⁰⁸ *Congressional Record*, 68th Congress, 1st Session, House of Representatives, 4 April 1924, 5582 for Know Nothings, 5578 for pseudoscience.

Debate on the morning of the 5th of April featured statistical data from the United States census again being manipulated. Speaking in favor of the 1890 census bill, Representative William Vaile (R-CO) dismissed the Polish government's complaints of discrimination against Polish immigrants that would result from Congress using the 1890 census as the quota base. Vaile declared that Buffalo, New York, was an excellent example of the need to restrict the new immigration, as recently arrived Polish immigrants outnumbered the native stock by 20,000, although he never stated which census he took the information from, nor how he was knowledgeable about conditions in a Congressional district on the opposite side of the country. Representative Clarence MacGregor (R-NY) immediately rose to challenge the accuracy of Vaile's claim. The Colorado representative invoked what he thought was the unimpeachable authority of statistical evidence, mocking MacGregor by declaring, "Then the gentleman does not agree with the census." He continued, to the applause of members, to reiterate that the "greedy complaints" by national governments about the alleged discrimination in using the 1890 census was, "when it is remembered that no one is entitled to enter the United States at all to become a part of our permanent population except at the will and pleasure of the United States...particularly bad grace."¹⁰⁹ That Vaile felt so confident in the empirical evidence of the United States Census to stifle any debate in favor of open immigration shows the power

¹⁰⁹ *Congressional Record*, 68th Congress, 1st Session, House of Representatives, 5 April 1924, 5647.

that statistical data held. And that Vaile grossly perverted the evidence in favor of his argument shows how vulnerable statistical data was to distortion.¹¹⁰

MacGregor was not the only Representative to point out how restrictionists manipulated data and scientific theories to advance their legislative agenda. As soon as the Colorado Republican yielded the floor, John Joseph O'Connor (D-NY) rose to denounce the discrimination that he perceived in the bill "in favor of blonds against brunettes." If the Johnson bill was not designed to discriminate against southern and eastern European immigrants, he asked, "why the report of Doctor Laughlin, why all the scientific investigation under the Carnegie Foundation to prove that those races were inferior, socially and nationally; that they were inferior stock?" The New Yorker then launched a personal attack against Laughlin – "and the first syllable is spelled L-a-u-g-h" – and his "so-called scientific study," declaring the eugenicist's report to the Immigration and Naturalization Committee "the greatest joke book that has been published during this session of Congress. It is founded on fallacies from

¹¹⁰ There is zero evidence to support Vaile's claim based on census returns for Buffalo; like Laughlin and many others before him, he invented the assertion out of whole cloth. The 1890 returns for Buffalo indicate that only 8,879 residents reported Poland as their country of birth (p. 672), out of a total foreign-born population of 89,485 (p. 670). The total native-born white population of Buffalo in 1890 was 165,295 (p. 543), all from United States Census Office, *Eleventh Census of the United States, 1890*, (Washington, D.C.: Government Printing Office, 1892-1897) v. 1. The 1910 census showed that native-born residents of native parents numbered 119,692, with an additional 183,673 native-born residents of foreign-born parents (vol. 3, p. 238). The total of foreign-born residents in Buffalo in 1910 numbered only 118,444, although "Poland" as a country of origin was not listed, as Poland was not an independent nation, so "Poles" were subsumed by national identities as Russian, German, or Austrian. Classified by "mother tongue," Russian, German, Austrian and Hungarian Poles numbered 425,607 for the entire mid-Atlantic region (vol. 1, pp. 192-3), United States Census Bureau, *Thirteenth Census of the United States, 1910* (Washington, D.C.: Government Printing Office, 1910). The 1920 census listed 83,344 Polish speakers in Buffalo (vol. 2, p. 1007), but counted only 31,406 of them as "Polish." Buffalo's entire population in 1920 numbered 506,775, of whom 380,512 were native-born (vol. 3, p. 708), United States Census Bureau, *Fourteenth Census of the United States, 1920* (Washington, D.C.: Government Printing Office, 1921-1922).

beginning to end.”¹¹¹ Although this was largely true, based on the current knowledge of the science of genetics, Johnson immediately sprang to Laughlin’s defense, declaring that any bias toward the blond race was “all in your eye. Your committee,” he disingenuously stated, “is not the author of these books on the so-called Nordic race.”¹¹² Midway through the day’s debate, Jasper Napoleon Tinch (R-KS) rose to ask Sabath, who now controlled the floor, why Sabath’s colleagues on the committee wanted to keep out the “new” immigrants? “Because,” the Czech immigrant replied, “they believe these people are inferior.

¹¹¹ *Congressional Record*, 68th Congress, 1st Session, House of Representatives, 5 April 1924, 5647-48.

¹¹² *Ibid.*, 5648. Johnson’s statement was the height of hypocrisy. He was not only the member several anti-immigrant and eugenical committees, he also endorsed Madison Grant’s “A List of Authoritative Books on Immigration” for the Immigration Committee of the American Defense Society. The purpose of the list was “to counteract the vague, wild and unscientific theories which have gained currency latterly through their circulation by interested groups.” By this was not meant the classic racist theories, but rather those that suggested that all races were basically equal; the recommended books to counter the vague theories would have never had that effect, as they were the ones that were vague and unscientific. Among the titles recommended to be “kept on the shelves of all libraries” were Grant’s *Passing of the Great Race*, the House Committee Hearings on Immigration, John Trevor’s *Analysis of the American Immigration Act of 1924*, all three of Lothrop Stoddard’s books: *The Rising Tide of Color*, *The Revolt Against Civilization*, and *Racial Realities*, Carl C. Brigham’s *American Intelligence*, C. W. Gould, *America: A Family Matter*, Earnest S. Cox’s *White America*, Kenneth Roberts, *Why Europe Leaves Home*, Seth Humphrey’s *The Racial Prospect*, A. E. Wiggam’s books, *The Fruit of the Family Tree* and *The New Decalogue of Science*, Michael Guyer’s *The Importance of Being Well-Born*, Clark Wissler’s *Mind and Culture*, Ripley’s *The Races of Europe*, Alfred P. Schultz’s *Race or Mongrel*, Prescott Hall’s 1906 book *Immigration*, Henry Pratt Fairchild’s two books *Greek Immigration to the United States* and *Immigration*, John Commons, *Races and Immigrants*, F. J. Warne’s *The Tide of Immigration* and *The Immigrant Invasion*, among others. At the end of January 1925, Johnson requested 1,000 copies of the list from Grant, and enclosed three franking slips for shipment back to the Committee. Johnson told Grant that when the supply was gone, he would have the Committee print additional copies at taxpayer’s expense. “A List of Authoritative Books on Immigration compiled by Madison Grant for the Immigration Committee of the American Defense Society, Inc.” in RG 233, HR 68 A – F 18.1, box 264; Johnson to Grant 30 January 1925, *ibid.* Two weeks earlier, Johnson told Grant that he intended to use the list of books to send to teachers. Johnson to Grant, 13 January 1925, RG 233, HR 68 A – D 11, box 262. Excepting Ripley’s book, which at this point was nearly thirty years old, hardly any of these books were based on accurate empirical or experimental data. Nearly all hewed, in some fashion, to the classic racist orthodoxy.

They have been fed by misinformation; they have been fed by new dope, as I may term it, by unreliable statisticians, and by Professor Laughlin's eugenic and anthropological false tests, until they themselves believe that there is some foundation for the unjustifiable conclusions in the so-called Laughlin report."¹¹³ Here, on the floor of the House, the Illinois Democrat called out the House members strung out on the dope of classic racialism. And it had no effect.

One problem in Sabath's assessment of the situation was that it implied some degree of collusion or mendacity that does not seem to have existed. The great power of classic racialism was that it *was* derived, albeit loosely, from scientific principles. It was scientifically plausible, especially to persons who lacked sophisticated intellectual and professional training in scientific methods.¹¹⁴ The "new dope" was also not "new" at all; it had been around for fifty years. Restrictionists believed that it was true, that science did suggest that biology was destiny and that like produced like. The alluring simplicity of this claim was the central problem with scientific knowledge in the 1920s. Within the professional community of scientists, academics and researchers, it had become clear that heredity, inheritance and the way genes and chromosomes functioned and interacted were far more complex than was previously understood. But without being able to explain exactly how these complex processes unfolded – which was partly due to the infancy of genetic science and partly to technological limitations

¹¹³ *Congressional Record*, House of Representatives, 5 April 1924, 5662.

¹¹⁴ This is a point that Nancy Stepan develops convincingly in Nancy Stepan, *The Idea of Race in Science: Great Britain, 1800-1960* (London: The MacMillan Press, 1982); see also idem., *The Hour of Eugenics: Race, Gender, and Nation in Latin America* (Ithaca: Cornell University Press, 1991).

– they could provide no definitive “proof” that they were right and the classic racialists wrong.

While Laughlin, Davenport, Johnson, Grant and others may have had their prejudices against certain groups of people and exaggerated the accomplishments of other groups, in their minds the sciences of classic racialism had established that like invariably produced like, and that the blood of an individual shaped and determined its development over the course of its life. New methods of scientific investigation may have been exposing weaknesses in the accuracy of this belief, but they had not decisively and absolutely demolished it. Anthropology could explain why different population groups were identifiable and had different cultural values, but even with Hrdlicka’s Eskimos or Boas’s American Indians, the fact was that they were *still* different. So how to explain that difference, with no compelling or clear scientific explanation? Men like Johnson, Laughlin and the IRL leaders were motivated by genuine concern for the quality of life of native-born Americans, and they feared the effects of large pockets of alien population in the country. Lee’s longstanding financial commitment to the organization suggests the seriousness with which the League’s elites viewed restriction. While scientists and restrictionists may not have understood hereditary mechanisms or how characteristics developed, one group admitted uncertainty because they lacked proof, and one group assumed, without any proof, that there were certain, definite answers.

The difficulty in managing this transition from classic racialism to modern experimentalism was also not particularly novel, as can be seen in the persistence

of Lamarckian explanations of evolution even after Darwin published *The Origin of Species* in 1859 and *The Descent of Man* in 1871. Thomas Kuhn later modified his theory of revolutionary paradigmatic shifts in structures of scientific knowledge to be more closely aligned with gradual evolutionary change rather than punctuated revolutions, citing the persistence of Lamarckism after Darwin's evolutionary theory was published in 1859. Darwin's theory of evolution by natural selection was not a decisive or revolutionary mutation, Kuhn argued, but was more a process of speciation, of gradual, evolutionary adaptation.¹¹⁵ In this sense, opponents of classical racialism, or of the claims in favor of immigration restriction that were made in its name, could not point to radical, decisive experiments that disproved the axiom that "like produces like." Instead they had to point out inconsistencies in that premise, and flaws in the understandings of heredity that only gradually became clear. And without this decisive "proof," the explanatory power of classic racialism remained superior for its simplicity as a basis of devising policy.

The rancorous debate in Congress that continued in early April 1924 reflects this difficulty of assessing scientific truth claims. With the new, more restrictive bill gaining support in the House, a group opposed to the perceived discrimination written into the bill began to consolidate. While restrictionists continued to utilize census data to illustrate the myriad problems that they associated with the new immigration, deploying statistics to bolster their arguments, the misuse of this data continued to be hotly contested and debated.

¹¹⁵ Thomas Kuhn, *The Road Since Structure*, 98.

For instance, when Meyer Jacobstein (D-NY) took the floor to contest one of the restrictionists' favorite arguments about immigrants concentrating in urban areas and creating problems of crime, poverty, overcrowding and disease, Representative John Cable (D-OH), interrupted to ask what was the total percentage of immigrants living in cities in 1860? Cable was hoping to demonstrate that the urban proportion of the foreign-born from the 1920 census – which he believed was much higher than 1860 – would prove this tendency of immigrants to settle in ethnic neighborhoods, which ultimately retarded their assimilation and thus may suggest a causal link between the immigrant communities and a rise in urban crime rates.¹¹⁶

Jacobstein answered that the census returns for the foreign-born population in urban areas in 1860 was 30% of the total population of urban areas. In 1920, he continued, the proportion was only 24%. Jacobstein's figures demonstrated that Cable's argument had no statistical basis – immigrants could not be the sole cause of these urban problems if they constituted a smaller proportion of the urban population; it was, rather, the general population growth in the cities. Incredulous, Cable denounced Jacobstein, insisting, "To-day [the proportion] is 75 per cent in cities of 2,500 or more." "The gentleman asked me for the facts," the erstwhile economics professor from New York retorted, "and those are the facts as given by the census for the principal cities listed in the census of 1860 (pages 31 and 32), and the cities listed in the 1920 census having a population of 100,000 and over." When Cable tried to defend his claim, New

¹¹⁶ *Congressional Record*, House of Representatives, 8 April 1924, 5860.

York Representative Fiorello La Guardia joined the fray, declaring, “The gentleman from New York [Jacobstein] takes his figures from the United States census and not from [Houston Stewart] Chamberlain or [John B.] Trevor.”¹¹⁷ The “Little Flower” touched upon an essential element of the debates in Congress as they would play out until 1929: statistics were a highly problematic basis upon which to base policy, because data sets could be easily manipulated.

Statistical data was slippery, so when Johnson took the floor shortly after this exchange, he tried to move the debate back to the problem of assimilation and the failures of the melting pot. Making a joke at anthropology’s expense, Johnson articulated the desire of nativists in Congress to act for – and only for – the interests of the United States, declaring that, “we do not care whether they are round heads, longheads, or bone heads. We are going to cut down the number that come here.” Not desiring to be an obstructionist, Jacobstein retook the floor to offer a compromise: using the 1890 census was unsatisfactory to the “new” immigrants, 1920 unsatisfactory for old-stock Americans, so an equitable solution

¹¹⁷ *Ibid.* The Eighth Census tabulated the proportion of foreign-born residents in the “principal cities,” the smallest of which was Manchester, New Hampshire, which had a total of 20,109 residents, 27% of whom were foreign-born. It did not calculate a total average proportion of native-born versus foreign-born in the cities, but Jacobstein was on the whole correct that roughly 30% of the population of major American cities in 1860 was foreign-born. United States Bureau of the Census, *Population of the United States in 1860* (Washington, D.C.: Government Printing Office, 1864), xxxi-xxxii. In the 1920 Census, the returns were enumerated by a division between “urban” and “rural” population. Foreign-born residents listed under the “urban” heading numbered less than 20% of the total (19.1%) in 1920; in 1910 the total was 22.6%, in 1900 22.2%. United States Bureau of the Census, *Fourteenth Census of the United States* (Washington, D.C.: Government Printing Office, 1922) v. 3 “Population,” Table I, p. 15. The difference was in what constituted an “urban” area, but Cable’s claims are difficult to verify with census data, since the low proportion of foreign-born in large cities made it impossible on an aggregate basis of all cities over 2500, but conceivable that in smaller towns the foreign-born comprised a higher proportion than the 19.1% in the major urban areas.

would be to take the total number of foreign-born residents (i.e. naturalized immigrants) in the United States in the census of 1890 and 1920 and average the two to arrive at an equitable quota basis for immigration.¹¹⁸

Instead of placating both groups, Jacobstein's solution enflamed the nativists. Representative Walter F. Lineberger (R-CA) dismissed the statistical compromise, asking if it was not better to be unfair to statistics than to the American people? When Jacobstein replied that one cannot violate a fundamental thing like arithmetic, New Jersey Republican Randolph Perkins recommended cutting off all immigration so that aliens already resident in the United States could be safely and properly assimilated. Jacobstein replied that Perkins was assuming that there were already too many foreigners in the country to safely assimilate, and that he simply did not agree with that assumption. "I am working on the assumption of America for Americans," Perkins shot back. "So am I," Jacobstein testily replied. Born and raised in New York, Jacobstein bristled at the implication that he was operating in the interests of the immigrants. He set off a fascinating exchange of race pride and racial identity:

Jacobstein: "But what is an American? How long do men have to be here to be Americans? How long has the gentleman been here?"

Perkins: "Since I was born, and my forefathers had been here for 260 years."

Jacobstein: "Does the gentleman think for that reason he is better than one whose ancestors did not come over in the *Mayflower*?"

Perkins: "I can lick them."

Jacobstein: "Then the gentleman must be Irish."

¹¹⁸ *Congressional Record*, House of Representatives, 8 April 1924, 5864.

[Adolph] Sabath (D-IL): “There may be some the gentleman can not lick.”
Perkins: “I will take on anyone who is not an American.”¹¹⁹

Perkins’s implication was that anyone who was opposed to restriction was not an American. But Jacobstein and LaGuardia were Americans too, both born and raised in the United States. Perkins’s objection was ultimately not about nativity; it was, as for Johnson, Cable, and others, a matter of race.

When Maine Republican Ira Greenlief Hersey took the floor and brought race openly into the debate, tensions in the House chamber escalated further. Hersey’s diatribe against immigrants neatly encapsulated several crucial elements of the nativist position. Though one had to concede that the Pilgrim Fathers arrived in the New World only to find the “red race” holding claims to the land, Hersey insisted that the Indian “cared nothing for civilization. He gave freely of his land to the white man for trinkets to adorn his person...” Like Robert L. O’Sullivan and a long list of nationalist writers before him, Hersey spewed forth the rhetoric of Manifest Destiny. It was imperative to preserve the racial heritage of the United States, which he thought was “God-intended, I believe, to be the home of a great people. English-speaking—a white race with great ideals, the Christian religion, one race, one country, and one destiny.” In his narrative, North America was “a mighty land settled by northern Europe from the United Kingdom, the Norsemen, and the Saxon, the peoples of a mixed blood. The

¹¹⁹ *Ibid.*, 5864-65. Interestingly, despite his family’s long lineage in the United States, Representative Perkins died in 1939 with \$2,000 in assets, and debts totaling \$223,000. See his obituary in *The New York Times*, 7 April 1939, 22.

African, the orientals, the Mongolians, and all the yellow races of Europe, Asia, and Africa should never been allowed to people this great land.”¹²⁰ Hersey thus articulated one of the core fantasies of nativists and classic racialists, namely that the United States was purely Anglo-Saxon at its founding, and only with the new immigrants that began arriving in the 1880s was the North American Eden contaminated. He urged his Congressional colleagues to stem the contamination before it was too late.

Members also used statistics to expose nativist prejudice. When James Willis Taylor (R-TN) took the floor to argue against the admission of inferior southern immigrants who were all, in his mind, poor and illiterate, LaGuardia interjected to ask about illiteracy rates in the southern states. Taylor confidently replied, “We have no illiteracy in Tennessee and Kentucky.” When LaGuardia skeptically pressed the Tennessean, Johnson came to his nativist colleague’s aid, insisting that the illiteracy rates in the south were not as high as those in New York City. Taylor and Johnson were both wrong, as LaGuardia knew. The census of 1920 showed that the illiteracy rate of adults over 21 in Tennessee was 12.6%, and in Kentucky 10.6%. The comparable figure for New York in the same census was 6.4%. LaGuardia was making the point that with the illiteracy provision of the 1917 Immigration Act, illiterate immigrants were excluded from coming in despite the fact that illiteracy was prevalent in the United States in poorer and

¹²⁰ *Congressional Record*, House of Representatives, 8 April, 1924, 5868. Hersey’s language eerily presaged the rhetoric the fascists would use in venerating and idolizing racial and national myths of purity and perfection. See also Alexander Saxton, *The Rise and Fall of the White Republic: Class, Politics and Mass Culture in Nineteenth Century America* (New York: Verso Press, 1990).

less-developed regions, just as it was in Europe; illiteracy therefore had nothing to do with the “racial” character of the population.¹²¹ Again, the manipulation of statistical data to support their agenda did not trouble congressional nativists. And although opponents of restriction took pains to demonstrate the inaccurate statistical associations that their restrictionist colleagues made, their corrections had little effect.

When LaGuardia finally took the floor, he addressed this issue of statistical manipulation by restrictionists. Pointing out that Vaile supported his restrictionist claims with statistical evidence not from the census but from John Trevor’s [sic] *A Century of Population Growth*, LaGuardia mocked “Trevor’s figures! Oh, yes, figures do not lie, but liars figure. This House is entitled to a more accurate basis than figures taken from a semifiction, semiscientific private publication.” Feeling that the opponents of restriction were gaining momentum, LaGuardia laid bare the prejudice that the House was attempting to enshrine into law: “I charge that your basis of 1890 is absolutely and intentionally discriminatory against the immigrants of Jewish faith and against the immigrants from Italy, Greece, Czechoslovakia, Poland, Russia, Hungary, and Austria, and I do not qualify that charge one bit. The basis of 1890 was taken only because it was the only census that would lend itself to the dirty work that is attempted to be done by the secret influences [in] back of this bill.”¹²² But while demonstrating

¹²¹ *Ibid.*, 5871. Statistics of illiteracy taken from *1920 Census*, v. 3, p. 958 for Tennessee, p. 366 for Kentucky, p. 678 for New York.

¹²² *Ibid.*, 5886. LaGuardia declared his opposition to H.R. 7995 “because it is unscientific, because it does not fit with the economic condition of the country, because it is the result of narrow-mindedness and bigotry, and because it is inspired, prompted, and urged by influences who dare

that the inaccurate use of statistics was one method pro-immigrant forces used to undercut the momentum of the restrictionist camp, the spurious scientific claims that lay at the heart of racial nativism were far more dangerous. Peter Francis Tague (D-MA) decried the “effort on the part of unscientific men to solve the problems of immigration by scientific method” and failed to find that the “pseudo scientific [sic] claims of the sponsors of the bill have been substantiated from any scientific source.”¹²³ This was the core of the debate over not only restriction, but also one that was occurring in science generally: who was able to properly speak in the name of science?

When the House reconvened for its evening session on the 8th, Ohio Democrat Charles Mooney attempted to redirect members’ attention back to the fundamentally flawed anthropological assumptions of superior and inferior races that the restrictive laws were based upon. A new word had infiltrated the English language, Mooney declared, that empowered the pseudoscientific basis for restriction. The venerated “Nordic” was suddenly appearing everywhere, and

not come out in the open, by the influences who have no intelligent information of conditions, but who have a fixed obsession on Anglo-Saxon superiority...” *Ibid.*, 5887. *A Century of Population Growth* was not written by Trevor but was issued from the Bureau of the Census, although it was a crucial resource for the eventual computation of National Origins Quotas. Trevor was, however, the author of two short reports on the racial character of the American population that were frequently quoted by members of Congress in defense of stricter quotas, and were highly influential in the eventual passage of the national origins quotas. He was also an intimate friend of Johnson’s, whose position on immigration was one of favoring only the admission “peoples who are closely related to our basic stocks and older immigration” to preserve “our racial homogeneity.” John B. Trevor to Albert Johnson, 31 December 1923; see also Trevor to Johnson 6 December 1923 which warned of the necessity of excluding Central and South American immigration, as “it would be just as objectionable to have a flow of Mexicans and Brazilians...as it is to have Greeks and Italians (south) pour in upon us.” Both from John B. Trevor Papers, box 2, Bentley Historical Library, University of Michigan, Ann Arbor, MI [hereafter Trevor papers]. Trevor’s work will be discussed in greater detail below.

¹²³ *Congressional Record*, House of Representatives, 8 April 1924, 5873.

there was not “a fifth-rate extension lecturer” – perhaps a jibe at A. E. Wiggam – “[who] does not speak of it with scientific exactness.” And yet the hearings of the Immigration and Naturalization Committee had “established beyond a doubt that this word ‘Nordic’ is an invention of the anti-Semite [Houston Stewart] Chamberlain, an English subject who expatriated himself at the time of his country’s need. He went so far in his rabid anti-Semitism as to make of the Christ a ‘Nordic.’”¹²⁴ Mooney wondered if a Nordic race even existed, and exactly how its superiority – or that of any race – was established? Franz Boas, America’s leading authority on anthropology as Mooney called him, had declared in *The Mind of Primitive Man* back in 1911 that all races were equal in gifts and potential, but some simply lacked opportunity. Mooney asked why Boas was not called before the Committee? Johnson rose and, perhaps tongue in cheek, insisted, “through the strenuous times of the hearings this committee undertook not to discuss the Nordic proposition or racial matters.”¹²⁵ This was again entirely

¹²⁴ *Ibid.*, 5910. Chamberlain’s two-volume *The Foundations of the Nineteenth Century* was published in its English translation in 1911 and immediately became a favorite of Madison Grant. Its original German version, *Die Grundlagen des neunzehnten Jahrhunderts* (1899) described all of history as the struggle of Aryans against the Jews; that Aryans were the saviors of mankind, the true heirs ancient Rome and Greece; and projected Christ’s values of love, compassion and renunciation of material ambition as fundamentally “Aryan.” Chamberlain was not only a favorite of Grant and American nativists; he also was highly esteemed by Adolph Hitler, who shared Chamberlain’s vision of an ancient and perfect Aryan tribe that originated in what eventually became Germany. They also shared a passion for Richard Wagner – Chamberlain married the German composer’s only daughter Eva in 1907, and received the German Military Cross (despite being born in Hampshire, England) in 1915. The accusation that Chamberlain abandoned his country was a reference to Chamberlain’s ex-patriot status in Germany during World War I. See Geoffrey Field, *Evangelist of Race: The Germanic Vision of Houston Stewart Chamberlain* (New York: Columbia University Press, 1981); George Mosse, *Toward the Final Solution: A History of European Racism* (New York: Howard Fertig, 1978). See also “The Dictionary of Races or Peoples,” which states that Nordic and Aryan were equivalent terms, but were both linguistic definitions that were further subdivided into anthropological categories. United States Immigration Commission, “The Dictionary of Races and Peoples.”

disingenuous: the fundamental point of the bill was to preserve the Anglo-Saxon or Nordic character of the American population. Johnson avowed this goal as a member of the Committee on Selective Immigration of the Eugenics Committee of America, which had declared its intent to preserve the Anglo-Saxon or Nordic character of the American population.¹²⁶

Emmanuel Celler (D-NY), Mooney's colleague who also an ardent opponent of restriction, continued to press Johnson on this critical issue of the proof of the inferiority of the new immigration. Celler described Laughlin's testimony before the committee as "redolent with downright and deliberate falsehoods. The gentleman from Washington [Johnson] admitted to me that it contained certain inaccuracies, yet he has made no attempt to check its circulation." In Celler's mind, Laughlin's project was pre-determined to show the supremacy of the Nordic race, and the researcher "hoodwinked" Johnson and the Committee. Celler feigned surprise that Grant – whose work was "dogmatic piffle" – and Stoddard ("a sort of 'Saturday Evening Post' scientist") were summoned to the Committee, but not someone like Boas or Hrdlicka. Ultimately Celler knew why: "the committee only wanted those who believed in 'Nordic'

¹²⁵ Mooney backtracked and paid deference to Johnson, telling the Committee's Chair that he was criticizing "the propagandist" and not the Committee. *Congressional Record*, House of Representatives, 8 April, 1924, 5910-11. Historian Vernon Williams has called *The Mind of Primitive Man* "complete in its refutation of crude racial determinist thinking, complete in its indictment of crude racial prejudice." Vernon J. Williams, Jr., *Rethinking Race: Franz Boas and His Contemporaries* (Lexington, KY: University of Kentucky Press, 1996), 22.

¹²⁶ Committee on Selective Immigration of the Eugenics Committee of the United States of America, "Report of the Committee on Selective Immigration," [n.d. but ca. late 1923], Jennings Papers.

superiority; men who deal in buncombe, like Grant and Stoddard.”¹²⁷ But these criticisms were not enough to stop the influence of classic racist “piffle,” and in a Saturday session, the House of Representatives passed H. R. 7995 by a vote of 323 to 71.¹²⁸ The Senate, however, had its own ideas on how to proceed to improve immigration restriction.

The senators considered a bill that was more exclusionary than the House bill. As they considered a bill introduced by David Reed (R-PA), Senator LeBaron Colt (R-RI) spoke out that he saw an intentional act of discrimination against southern and eastern European immigrants. Reed’s bill moved the Census basis for quota calculations back to 1890, and after two years, the system would be replaced by a “national origins” system of determining immigration quotas. The quota law already in effect – 3% of the number of immigrants enumerated in 1910 – had decreased immigration from southern and eastern Europe from 915,754 in 1913-1914 to 159,646 in 1923. Colt believed that “this 3 per cent has solved the problem of any menace of immigration.”¹²⁹ Reed’s proposal to move the quota to the 1890 census was an attempt to create a minimum number of southern immigrants, who were regarded as inferior, and a maximum number of northern immigrants, which was precisely “what one class of restrictionists wants.” But declaring the various countrymen of the 6,000,000 immigrants from southeastern Europe already in the United States to be “undesirable” was a great

¹²⁷ *Congressional Record*, House of Representatives, 8 April 1924, 5915.

¹²⁸ *Congressional Record*, House of Representatives, 12 April 1924, 6258.

¹²⁹ *Congressional Record*, Senate, 2 April 1924, 5411-12.

injustice and discrimination against them, the Rhode Islander argued. And Colt argued that Reed's provision to eventually compute quotas on the basis of national origins was futile: "It can not be done."¹³⁰

The national origins provision proposed to calculate the racial composition of the whole population of the United States by analyzing the origins of the population enumerated in the first census of 1790 and then calculate quotas for immigration upon that computation. This was a highly difficult endeavor, since the census made no inquiry of nativity until 1840.¹³¹ On Friday April 3, Senator Reed rose to defend his proposal to base quotas on national origins by describing how it could be effectively implemented. The Pennsylvania senator conceded that it was impractical to question "the individuals who are here to-day of the origin of their ancestors who first immigrated to this country," but utilizing the census returns from 1790 would provide a clear picture of the original national and racial composition of the American population. Restrictionists freely moved the census basis for calculating quotas to achieve their goal of preserving the racial homogeneity of the United States. Reed informed his colleagues that, "all the methods of ascertaining racial origins and national origins that have been used check with a surprising degree of accuracy." For advocates of additional restriction, the National Origins provision promised to eliminate accusations of discrimination and bias against certain population groups of immigrants arriving

¹³⁰ *Ibid.*, 5414.

¹³¹ See also Ngai, "The Architecture of Race in American Immigration Law"; *idem.*, *Impossible Subjects*, 22-37.

in the United States. Because the principle would establish quotas for immigration based on the nativity of the American population at its founding, it had the appearance of not being directed against any particular group of immigrants and used the language of nationality and not race. Except that it was intentionally directed at those not present in 1790 – the “new” immigrants. But it projected an image of fairness. As Massachusetts Senator Henry Cabot Lodge explained, the national origins quotas may not be precise in their ethnic divisions, “but as far as what are called scientific races, the races created by history, it comes pretty near to being exact—for all practical purposes sufficiently exact.”¹³² But Lodge’s statements on the Senate floor reflected major confusion in drawing this distinction between scientific, historical, national and racial identity that was never really resolved.

The 1921 quota act had the effect of collapsing the racial boundaries in Italy by subsuming the alleged racial differences between north and south into one national quota. When the quotas were announced, there was no distinction between the amount of northern and southern Italians that would be admitted. It was simply “Italian.”¹³³ But the debates in the Senate over national origins show

¹³² *Congressional Record*, Senate, 3 April 1924, 5462. Lodge defined scientific races by using an example of German and English immigration. He told Senators that the population in East Prussia, as it had descended from the ethnic Wends, was of a different race than the Germans, but in dealing with immigration, one would consider them nationally as Germans. Similarly, for the English – the Welsh and Irish were racially different from the English, but designating a quota for the “United Kingdom” would “convey a very good idea of what the immigration is and where it comes from.” Seventy years previously, this would have appalled the American Protective Association and the Know-Nothings.

¹³³ See Carl Ipsen, *Dictating Demography: The Problem of Population in Fascist Italy* (Cambridge: University of Cambridge Press, 1996) for an interesting discussion of the differences between *razza* and *stirpe* (“race” and “stock”) in Italy during the early fascist period.

the persistence of thinking in terms of race. Another significant aspect of the Senate's proposed legislation for 1924 was the rhetorical gymnastics the national origins provision enabled. As several critics had pointed out regarding the quotas that were established in 1921, the limits on immigration were surreptitiously discriminatory against southern and eastern immigrants. National origins inverted these charges – restrictionists suggested that the volume of immigration from non-northern areas of Europe actually discriminated against the “American” race, which was chiefly English in origin. On the third of April, Reed explained this more fully. Selective immigration policy, he explained, was not about the “relative merits of this race and that,” or claiming that any nation produces better emigrants than others. Every immigrant was “the equivalent of myself in general desirability.” But in fixing the quotas under the 1921 law, immigrants received “two or three times the consideration we have been giving to our own American stock, which is not fair, which is a discrimination against us.” Reed reiterated the advantages of the National Origins provisions again on April 9, insisting to his Senate colleagues that establishing restriction on this basis would obviate “the slightest accusation of discrimination,” and encouraged the support of the Senate for a proposal that would “make our immigration an exact cross section of our present population.”¹³⁴ The assumption at the heart of the National Origins principle, however, was that the national background of the “present population” of the United States could be determined, despite the fact that no evidence of its composition existed in the census data from 1790 or 1800.

¹³⁴ *Congressional Record*, Senate, 9 April 1924, 5943.

But before the national origins quotas would go into effect, Reed's bill changed the census base for the calculation of the existing quotas back from 1910 to 1890, a move that some senators charged manifested a malevolent intent to discriminate against the "new" immigration (the national origins provision would restrict the volume of "new" immigrants even further). Massachusetts's freshman senator David Walsh decried the "very apparent discrimination against immigrants of certain nationalities" that would result from using the 1890 census instead of the 1910 census that was currently used. Walsh decried the "popular and pseudoscientific propaganda" restrictionist congressmen introduced to justify the shift to 1890. Some senate members who supported Reed's bill denied believing that some races were inferior to others.¹³⁵ The appeal of national origins was to make this criticism a moot point: there was no particular justification for selecting either 1910 or 1890 as the census basis to compute quotas. To use national origins, however, one could argue that the goal of preserving the racial homogeneity of America was based upon an exact calculation of what the racial composition of the country actually *was*. Quotas and restriction would no longer be based on the census at all. And whenever critics denounced the national origins system as impossible, supporters nearly always referred to short studies written by a man named John B. Trevor ("A Preliminary Study of the Immigration Problem")

¹³⁵ *Congressional Record*, Senate, 15 April 1924, 6355, 6357 for science. Two days later, Walsh's anti-restrictionist colleague, Senator Colt declared that the new legislation "proposed to drift from broad American nationalism to racialism; it is proposed to adopt, so far as our foreign born are concerned, the policy of Germany. It was the racial feeling of Germany that Germans were a superior race in the world that led to their downfall." *Congressional Record*, Senate, 17 April, 1924, 6542. The long-serving senator from Rhode Island died in August 1924, unable to see how prescient his comments were.

and “A Study of the Population of the United States”), both of which confidently declared the viability of computing the national origins of the population from the census returns of 1790, and insisted that accurate quotas could be made upon this population base with precision.¹³⁶

National origins proposed was to discern the racial composition of the United States in its first statistical accounting, the original census of the population in 1790. As A. Warner Parker, a member of the District of Columbia Bar Association, explained, the intent was to achieve “a sufficient reduction in the volume of immigration to give assurance that the country will not admit more annually than it can accommodate; and... making the reduction fall most heavily on the “new immigration” countries (of southern and eastern Europe)...” The stated appeal of national origins was that “instead of arbitrarily reducing the volume of immigration...it is a scientific plan for keeping America American...”¹³⁷ Once this racial composition was determined, the proportion of immigration permitted to enter the United States would conform directly to that composition. So if it was discerned (“divined” may be a more appropriate phrase) that the American population in 1790 was 80% Anglo-Saxon, 15% Alpine, and 5% Latin, the quotas for admissible immigrants would reflect those proportions. Anglo-Saxon races would get 80% of the quota, Alpine races 15%, and Latin or Mediterranean races 5% in proportion to a maximum limit of around 150,000

¹³⁶ In the Senate, it appeared in the debate on April 3, 1924 (*Congressional Record*, Senate, 3 April 1924, 5469-71) and was attached as an appendix to House Report n. 350.

¹³⁷ A. Warner Parker, “The Quota Provisions of the Immigration Act of 1924” *The American Journal of International Law* v. 18, n. 4 (October 1924), 737-54; 737 for intent; national origins on 740.

immigrants. And from the Dillingham Commission's "Dictionary of Races or Peoples" the federal government now knew "definitively" which races came from which European countries. There was only one problem – the racial information was not contained in the 1790 census. There were no questions of nativity or country of origin or ancestry listed in the 1790 census – just six categories: family surname, number of free white males over 16 years of age, free white males under 16, free white females, free blacks, and slaves.¹³⁸ This shortcoming would almost defeat the entire restrictionist campaign, and is discussed in greater detail in Chapter 7.

Reassured by the practicality of this new national origins system, when the House bill (which passed the lower chamber on the 12th of April) arrived in the Senate, it was substituted for the Senate Immigration Committee's draft of legislation (S. 2576), which eliminated all quota exemptions for immigrants from the House bill and contained a provision for the gradual enactment of national origins quotas. Thus amended, it was passed in the Senate by a substantial margin, 62 in favor, 6 opposed (28 members did not vote).¹³⁹ The resulting conference between the two chambers generated a bill that established immigration quotas based upon 2% of the number of foreign-born aliens resident in the United States

¹³⁸ Regarding national origins, see Walker, "The Quota Provisions"; Act of 26 May 1924 (43 Stat. 153); John B. Trevor, "An Analysis of the American Immigration Act of 1924" *International Conciliation* n. 202 (September 1924), 5-80; Ngai, *Impossible Subjects*; Anderson, *American Census*, 14 for census categories.

¹³⁹ *Congressional Record*, Senate, 18 April 1924, 6649. Some of the exemptions permitted for exceeding quota limits included family reunification (which was a contentious issue for restrictionists) and spousal citizenship status or nativity.

in 1890 as enumerated in the census until 1 July 1927, at which time the National Origins plan would take effect. The bill also contained, in section 13, an exemption for Japanese nationals, continuing to honor the “Gentleman’s Agreement,” until the President and Secretary of State could properly abrogate the agreement.¹⁴⁰ As the bill was reintroduced to the House, pro-immigration forces, who were vastly outnumbered in both chambers, tried one last time to refute the possibility of accurately computing the quotas based on national origin. Representative Sabath informed his colleagues that both “the Director of the Census, Mr. Steuart, and Doctor Hill, his first assistant...declared they would be obliged to adopt arbitrary methods to arrive at the proper basis upon which the allocation will be raised.”¹⁴¹ On May 9, LaGuardia rose to dispute the mathematics of both types of quotas, which he believed were “worked out in order to discriminate against certain races.”¹⁴² Ultimately it was not the statistical calculations but the complex international and diplomatic issues of Japanese immigration that proved a sticking point in the bill; by a narrow vote (192 to 171, 69 not voting), the bill was recommitted to conference, where the provision exempting Japanese immigration was stripped.¹⁴³

¹⁴⁰ House Report No. 688, 68th Congress, 1st Session, “Immigration of Aliens,” 8 May 1924.

¹⁴¹ *Congressional Record*, House of Representatives, 8 May 1924, 8138. When the conference report was entered into the *Congressional Record* the following day, Sabath was joined by Representative John Raker in raising points of order against the conference report, as the provision for the abrogation of the Gentlemen’s Agreement exceeded the Managers’ authority. *Congressional Record*, House of Representatives, 9 May 1924, 8218ff.

¹⁴² *Congressional Record*, House of Representatives, 9 May 1924, 8244.

¹⁴³ *Congressional Record*, House of Representatives, 9 May 1924, 8249. For a description of the difficulties Japanese nationals caused the House and Senate, see House Report no. 350, 68th Congress, 1st Session, 24 March, 1924, 7-8; and John Raker’s detailed history of the bill in the

Improved in this way, H. R. 7995 came before the House of Representatives on Thursday May 15, and it was again handily passed, 308 to 62 (63 members not voting). Representatives Celler, Dickstein, LaGuardia, O'Connell and Sabath maintained their opposition to the bill.¹⁴⁴ The vote in the Senate on the same day passed by an equally large margin, 69 to 9, with 18 members not voting.¹⁴⁵ Sent to President Coolidge on the May 19, it received his endorsement and became law on May 26, 1924. Pursuant to subdivision (e) of § 11, a team was assembled to begin computing the new quota, on the basis of national origin, which the quotas based on 2% of the 1890 census went into effect.

At the conclusion of the Great War, the attention of many American congressmen had returned to immigration. Most members, representing constituents aroused by "100 per cent Americanism," supported greater restrictions on immigration. It was not only this feeling or sentiment that drove them to close the gates further, however. A significant base of empirical support emerged in the immediate post-war years from the Army mental tests. Now it appeared that a scientific field provided a means to assess and determine the intellectual desirability of immigrants too. Policy recommendations from outside groups also included pedigree analysis of European immigrants to ensure the proper detection of feeble-mindedness, imbecility or other mental defects. The

Congressional Record, House of Representatives, 9 May 1924, 8235-36, and Albert Johnson's comments which follow, 8236. See also Ngai, *Impossible Subjects*; Hutchinson, *Legislative History*.

¹⁴⁴ *Congressional Record*, House of Representatives, 15 May 1924, 8652.

¹⁴⁵ *Congressional Record*, Senate, 15 May 1924, 8589. The emergency Immigration Act of 1921, which had established the quota principle of 3% of the census of 1910, had been twice extended, and was set to expire on 1 July 1924.

categories for immigrant admission were slowly being tightened. Literacy tests were not keeping enough people out, so quotas were introduced, although as the debates in Congress showed, they could appear to be arbitrarily selected and computed, whether based on the censuses of 1920, 1910, 1890, or by the percentage admitted. These were contentious, so assigning quotas by national origin was introduced as a policy provision. This had the advantage of purportedly preserving the racial character and composition of the United States, which for racial nationalists like the IRL, was what they had favored all along. Always, the goal was the same – to protect the Anglo-Saxon population of America from being overwhelmed by inferior and undesirable immigrants. Whether the undesirability was justified by intelligence, racial characteristics or national identity was not important. It simply needed to be justified somehow. While Congress debated these bills to further restrict immigration, outside Congress a powerful propaganda movement emerged to help justify restriction to the public.

Chapter 6: “There is no science of propaganda”

In the early 1920s, as political support for immigration restriction grew, other developments threatened to derail some of the carefully constructed justifications for the anti-immigrant laws of 1917, 1921 and 1924. The classic racist ideas were increasingly vulnerable as the science of genetics and Morgan’s chromosome theory of heredity began to diffuse throughout the academic and scientific communities. Morgan’s work on fruit flies in particular was instrumental in propelling further experimental and laboratory investigations into heredity.¹ Significantly, however, this knowledge of the complexity of chromosomes and heredity remained very isolated and enclosed within a small community of practitioners. When the professional scientists did reach out to the larger public, as Franz Boas tried to in the mid-1920s, the complexity of scientific knowledge was very difficult to explain.

This created a significant disadvantage for professional scientists who tried to educate the public about recent discoveries and advances in knowledge about heredity and genetics. Their disadvantage lay in the genuine uncertainty and ambiguity of the processes they studied. When non-scientists, men who had no professional training in zoology, biology or any of the experimental sciences, described science, they expressed no uncertainty because they believed that none existed. As they deductively reasoned, and as they assumed correlations for

¹ See Allen, *Thomas Hunt Morgan*; Brush, “How Theories became Knowledge.” In an essay, Allen also suggests that the biological determinism that eastern and southern European immigration should be excluded was “mounted through two interrelated movements: the eugenics movement and the I.Q. testing movement,” both of which came under sustained fire in the 1920s. Garland Allen, “Essay Review: The Roots of Biological Determinism” *Jl His Bio* v. 17, n. 1 (Spring 1988), 141-45; 144.

causation, men like Madison Grant, Charles Davenport and Lothrop Stoddard and others who hewed to the classic racist orthodoxy that like produced like, convinced the public and policy makers that there was no complexity, ambiguity or uncertainty.

At one extreme of how superficially the public understood scientific principles was the Scopes trial in Tennessee in 1925. Although William Jennings Bryan's defense of the Biblical interpretation of man's origins was well beyond the pale even of the inaccurate orthodoxy of classic racialism, the trial itself presents a useful example of the dangers of controlling scientific knowledge and propaganda. Bryan's work for the prosecution crystallized the dangers of letting laypersons (in this case those without scientific credentials) speak to the public on scientific matters. Bryan saw the trial as a showdown between faith and salvation and godless Reason. For professional scientists, though, if Bryan's argument carried the day, the unfettered development of scientific knowledge in the country would be imperiled.²

The contours of the conflict between the religious fundamentalists and professional scientists had actually been laid out years before the trial began. In 1922 William Jennings Bryan was offered column space by *The New York Times* to comment on evolution, which Bryan denounced as only "a guess." Bryan claimed that this was the scientific consensus – that evolution was a hazardous

² See Edward J. Larson, *Summer for the Gods: The Scopes Trial and America's Continuing Debate over Science and Religion* (Cambridge, MA: Harvard University Press, 1997).

guess.³ The opportunity the *Times* afforded to Bryan to comment on a scientific theory appears to have been, in some manner, a publicity stunt. The paper issued a quarter-page advertisement a week before that cautioned readers to order their papers in advance, as “Mr. Bryan’s extraordinary explanation of his stand on evolution will create an unusual demand for the Sunday edition.”⁴ The advertisement itself provoked reactions from readers, some of whom angrily wrote the editor of the *Times*, criticizing such vulgar commercialism. One reader, a Congregational minister from Akron, Ohio, actually anticipated the criticisms of Bryan’s piece that Edwin Conklin and Henry Fairfield Osborn would make in the following weeks (which the *Times* also advertised in advance, though less conspicuously).⁵ Bryan, Reverend Lloyd Douglas pointed out, “has not standing either as a theologian or a biologist,” but despite this the former presidential nominee still “raced across the country perspiringly endeavoring to protect a sixteenth century ultra-orthodoxy from the subversive influences of latter-day science.” Douglas closed his acidic letter by declaring “Because he has willfully refused to inform himself on a subject of which he presumes to speak in tones of authority, Mr. Bryan is intellectually immoral, and the people who provide him a platform for the spread of his misinformation are rendering their patrons a disservice.”⁶ Bryan had no justification to speak on any aspect of scientific

³ William Jennings Bryan, “God and Evolution” *The New York Times* 26 February 1922, Section 7, 1;6.

⁴ *The New York Times* 22 February 1922, p. 12.

⁵ The advertisement for Osborn and Conklin was placed on page 20 of the 28 February 1922 edition of *The New York Times*, under the heading of “Champions of Evolution.”

authority. Douglas, in a few brief sentences, provided a crisp and effective critique of Bryan's pretended scientific authority. Conklin and Osborn, who responded with columns of their own, would be hard-pressed to put their objections to Bryan's piece as succinctly or powerfully with their professional and academic credentials.

When the anticipated Sunday arrived, Bryan's article thundered that the idea of evolution was harmful because it destroyed man's family tree as taught by the Bible. Bryan declared that scientists scandalously accepted man's descent from lower organisms "*without any explanation whatever* to support it." Darwin's theory of evolution, the Great Commoner insisted, "is only a guess and was never anything more." As if this were not damning enough, the second (and for Bryan, the most horrifying) objection was that evolution "has not one syllable in the Bible to support it....The Bible not only describes man's creation, but gives a reason for it; man is a part of God's plan and is placed on earth for a purpose." There had never been one instance of one species changing into another, which, Darwin's "guess" posited, was how better adapted species evolved. In fact, Bryan declared, Darwinian evolution requires believers to accept absurd, irrational explanations for man's existence. It is "*harmful*, as well as groundless." The Presbyterian populist anticipated the Scopes case, arguing that, "If the Bible cannot be taught [in public schools], why should Christian taxpayers permit the

⁶ Lloyd C. Douglas to the Editor, *The New York Times* 2 March, 1922, 20:7. Douglas's letter was dated 23 February 1922, from the First Congregational Church, Akron, Ohio.

teaching of guesses that make the Bible a lie?”⁷ But the theory of evolution was not a lie – most professional scientists believed that there was plenty of evidence to prove that evolution occurred. The difficulty remained the same that Galton had faced: they had no tangible proof of *how* it occurred.

Darwin’s theory of natural selection still seemed to provide the most compelling explanation for how evolution occurred, but it was very difficult to explain exactly *how* the mechanism worked. As Jennings intuited, scientists must be able to demonstrate processes to convincingly prove biological theories. But how did one show the process of natural selection in action? Without compelling proof, it was difficult to decisively and forcefully repudiate people like Bryan, who extrapolated from this lack of proof of natural selection occurring that the whole principle of evolution was wrong. This was a recurrent problem for scientists, whether they were describing the process of evolution, the existence of chromosomes, or the influence of environmental factors on the development of organisms. They lacked specific, tangible, direct proof of natural selection occurring. They could offer no detailed explanation of how the environment modified an organism’s development; as Pearl had learned with his chick blastoderms, it was still unknown how the organs developed in just the right way. They could only suggest that it *was* true. Morgan’s introduction of chromosomes went some way to providing direct evidence that evolution or nurture were true. But publicly, or at least for the readers of *The New York Times*, Bryan presented

⁷ William Jennings Bryan, “God and Evolution” *The New York Times* 26 February 1922, Section 7, pages 1, 11. Emphasis original.

reasonable doubts that evolution had any real merit because natural selection could not be “shown.”

To refute these accusations, the *Times* kindly offered two professional scientists, Henry F. Osborn and Edwin Conklin, equivalent space to respond to Bryan’s allegations in the following Sunday’s issue. Osborn, as President of the American Museum of Natural History in New York City and the Vertebrate Paleontologist of the United States Geological Survey, was an obvious choice to comment on evolution; Conklin’s selection was a bit more puzzling, as he was principally a biologist, though he had a very strong religious background.⁸ Osborn opened his rebuttal on a friendly note (which, as Osborn and Bryan continued to spar over the coming years, would degenerate into outright hostility), expressing admiration for Bryan’s familiarity with some of Darwin’s more complex arguments, like Sexual Selection. Again, it must be remembered that Bryan’s objections to Davenport’s theories originated from a specific theological or cosmological perspective. He was not a scientific expert. But, Osborn insisted, twenty-one years of research in the U.S. Geological Survey had convinced him that “Natural Selection is the only cause of evolution that has thus far been discovered and demonstrated.... No living naturalist, however, so far as I know, differs as to the immutable truth of evolution in the sense of the continuous fitness of plants and animals to their environment...” Bryan’s objection to evolution, that

⁸ Osborn biographical information from his byline for “Evolution and Religion” *The New York Times* 5 March 1922, Section 7, p. 2; for Conklin, see Jane Mainschein, “Conklin, Edwin Grant” *American National Biography Online* Feb. 2000. <http://www.anb.org/articles/13/13-00331.html> Access Date: Tue Nov 11 17:15:56 EST 2003. Conklin is treated with great sophistication in Jane Maienschein, *Transforming Traditions in American Biology, 1880-1915* (Baltimore: Johns Hopkins University Press, 1991).

it removed the necessity of Divine involvement, or otherwise inclined believers to atheism, Osborn insisted, was simply not true. Citing St. Augustine's respect for natural mysteries as a sign of God's power, Osborn tried to defuse the anti-religious tone that Bryan believed he saw in evolutionary theory.⁹ But Osborn insisted that there was scientific proof demonstrating the fact of evolution, and more importantly, Osborn invoked his professional scientific position to lend credence to evolution.

Conklin's article addressed the religious aspect of evolution more directly, although he also questioned Bryan's capacity to speak on scientific matters. The complexity of evolution, his article explained, did indeed lead to uncertainty among scientists as to the mechanism of evolution, which Bryan had correctly pointed out. This did not, however, cast doubt upon the truth of evolution, as many non-scientific persons like Bryan claimed. "One who desires to know the truth about this or any other subject," wrote Conklin, "should inquire as to the competence of a witness, his impartiality, the truthfulness of his testimony, and whether he has any new evidence to offer." Bryan lacked any competency to evaluate the scientific evidence in favor of evolution. Conklin criticized Bryan's lack of impartiality, a key requirement for accurate scientific knowledge, because the former Secretary of State "frankly confesses that his motives are not to find the truth," and so his explanation of scientific theories must be disregarded. To demonstrate the truth of evolution, Conklin laid out the scientific fields that

⁹ Henry Fairfield Osborn, "Evolution and Religion" *The New York Times* 5 March 1922, Section 7, p. 2.

provided strong evidence in favor of the theory – in addition to the fossil remains that Osborn primarily studied, morphology, physiology, embryology and genetics all provided compelling proofs of the truth of evolution. This was an overwhelming consensus, but it was inadequate for Bryan and “his kind,” who hurled their “medieval theology” of creationism against “the ramparts of science,” and broadly established scientific theories.¹⁰

Gauging the responses that the *Times* received following Bryan’s first essay from other scientists, one could hardly question how strong the “ramparts of science” actually were in terms of the consensus in favor of evolution. There was a broad accord with Darwin’s great theory, and the confidence in the ramparts was quite strong. Davenport gave a series of lectures in the early spring of 1922 with the aim of popularizing support for evolution that he titled “Population and W.J. Bryan.” In this address, Davenport railed against Bryan’s assertion that “all knowledge that is of value is intuitive; that one person’s knowledge is as good as another’s...” For the director of the Eugenics Record Office and the Station for Experimental Evolution, this was ridiculous. Bryan’s article was most upsetting to Davenport because Bryan had dismissed the specialized knowledge that scientists had. Davenport denounced this notion, insisting that the thirty-five years he had spent in professional study of scientific knowledge *did* give him a right to speak on scientific truth, particularly in opposition to one who “has obviously not spent 35 hours of critical observation in this field.” But this also forced Davenport to

¹⁰ Edwin Grant Conklin, “Bryan and Evolution” *The New York Times* 5 March, 1922, Section 7, p. 14.

confront the long-standing problem that scientists had, and one of the reasons that he recommended popular articles and books by Albert Wiggam, Madison Grant, and Lothrop Stoddard in his lecture: the esoteric knowledge of professional scientists was hard to communicate to the general, lay public.¹¹

If scientists had special truth claims by virtue of their professional training and expert knowledge, their opinions might carry more weight in policy approaches and attitudes to issues related to their fields, whether or not this involved matters of immigration, feeble-mindedness and sterilization or the teaching of evolution. Thus, when the young biology teacher John Scopes was charged with violating the Butler Act in Tennessee in 1925, the trajectory of the trial was already in a sense laid out. Osborn and Conklin were both approached by the defense team to testify on behalf of Scopes and explain the theory of evolution to the jury, to demonstrate that it was not “a guess,” as Bryan had asserted in 1922. If the scientific truth of evolution could be established, any religious objection to the teaching of evolution in public schools might violate the First Amendment and secure an acquittal for Scopes.

Conklin’s religious background made him a particularly desirable witness for the defense team. Forrest Bailey, the Associate Director of the American Civil Liberties Union [ACLU], asked Conklin at the end of May to “serve the cause of science” on behalf of Scopes. Conklin, despite being “intensely interested” in the

¹¹ Charles B. Davenport, Lecture “Population and W. J. Bryan, 1922” in Davenport papers. See also Davenport’s letter to the Editor of *The New York Times* [n.d., ca. 19 March 1922], also in the Davenport Papers.

trial, informed Bailey that he had already committed himself to a summer research program, and would not be able to appear in Dayton. Bailey refused to accept Conklin's negative reply, writing him two letters in June beseeching him to come south for Scopes. The attorney shrewdly put his plea in terms that he knew would resonate with Conklin: "The important thing is that the interests of science should be adequately protected by the presence of men who are competent for that purpose."¹² The historian Ray Ginger describes among the ACLU's goals in the trial as educating the public on evolution, to demonstrate the necessity of having education unfettered by laws based on religious faith, and to show that religion and science were not incompatible.¹³ Having a scientific man familiar with the theological issues, another historian has suggested, was a critical strategy for swaying the rural Rhea County jurors.¹⁴ Considering Conklin's 1922 article in *The New York Times*, his appearance before the jury would have been a tremendous asset for the ACLU. But appearing for Clarence Darrow raised significant complications. The historian Edward Larson explains that many of the academics and professional scientists that were approached by the defense team – Osborn, Conklin, Davenport, Luther Burbank, David Starr Jordan and J. McKeen Cattell – were reluctant to appear opposite a "showman" and radical agnostic like Darrow. And the support these six potential witnesses had for eugenic plans and policies,

¹² Forrest Bailey to Edwin Conklin, 29 May 1925; Conklin to Bailey 2 June 1925; Bailey to Conklin, 4 June 1925, all box 1, Professional Papers, Conklin Papers.

¹³ Ray Ginger, *Six Days or Forever? Tennessee v. John Thomas Scopes* (Boston: Beacon Press, 1958), 78.

¹⁴ Edward J. Larson, *Summer for the Gods: The Scopes Trial and America's Continuing Debate over Science and Religion* (New York: Basic Books, 1997), 135.

Larson suggests, was anathema to Darrow's own conception of universal human rights.¹⁵ So professional academics and researchers would only go so far in their defense and protection of academic freedom and scientific inquiry.

Conklin was tempted to assist the defense; despite his arrangements for a research trip to Woods Hole, he wrote to Osborn that his interests and the ACLU's overlapped to a great extent. The Princeton biologist had just given a commencement address at Ohio Wesleyan University, and found the university audience, and most of the people he encountered on his trip, to be deeply interested in the defense of scientific inquiry and academic freedom. For these northern audiences, he told Osborn in a private letter, Tennessee, and the south in general, appeared as backward states that are "trying to repeal laws of nature by the laws of the legislature." When Osborn replied, three weeks before the trial opened, he told Conklin that in recent, lengthy conferences with the Scopes's defense team in New York, Osborn argued that the best course for Darrow would be to argue that religion and evolution were "not in any way inconsistent." If this could be achieved, Osborn hoped, the lay public could obtain a broad understanding of what evolution really meant.¹⁶

Although neither man participated on behalf of the defense (Judge John Raulston ruled on July 17 that the defense would not be allowed to call scientists to testify on behalf of Scopes), they actively participated in the public debate

¹⁵ *Ibid.*, 135.

¹⁶ Conklin to Osborn, 19 June 1925; Osborn to Conklin 22 June 1925, both Conklin Papers, Princeton.

before, during and after the trial.¹⁷ Educating the public about the scientific merits of evolution was of paramount importance. Osborn was the first to strike in print, with an article in *The Forum* before the trial even began. In “The Earth Speaks to Bryan,” Osborn claimed that the importance of refuting Bryan’s dismissal of evolution was based on the arguments the Great Commoner made to thousands of Americans in his sermons. When Bryan preached that there existed some antagonism between the Creator and His Creation, between God and Nature, Osborn argued that Bryan corrupted true religion, morals and education. Science may not be incompatible with theological beliefs, but scientific inquiry had to be guided by secular investigators and methods. Osborn also defended educational freedoms, suggesting that Tennessee’s actions in determining what could and could not be taught in its schools and universities “puts the State back exactly four centuries to the inquisitorial period of Spanish history.”¹⁸ Neither the Tennessee school board nor Bryan could determine the intellectual merit of evolution, because the board members and Bryan were not professional scientists capable of

¹⁷ Before the trial started, Conklin wrote to a friend in the Geology Department at the University of Kentucky of his relief that he decided to remain in Woods Hole and not go to Tennessee: “I have been more or less disgusted by the comic aspects of the whole case as it has been reported in the newspapers and have been in doubt as to whether scientific men ought to take any part in a proceeding which looks as if it were intended to advertise Dayton, Bryan, Darrow et al.” Conklin to Arthur Miller, 6 July 1925, Conklin papers. See Ginger, *Six Days or Forever?*; Jeffrey Moran, *The Scopes Trial: A Brief History with Documents* (Boston: Bedford St. Martin’s, 2002); Larson, *Summer for the Gods*.

¹⁸ Henry Fairfield Osborn, “The Earth Speaks to Bryan” *The Forum* v. 73, n. 6 (June 1925), 796-804. Osborn also noted that “one of the leading American anthropologists, Dr. Ales Hrdlicka” recently posited Europe, and not Asia, as the cradle of the human race based upon recent fossil discoveries (p. 802).

evaluating that. Scientific inquiry had to be protected and defended from political or theological influence.

But Bryan was not swayed by the argument that scientific knowledge was the exclusive preserve of professional scientists. In a reply in *The Forum*, he again reverted to dismissing evolution by invoking the Biblical accounts of man's creation and simple common sense. To suggest that evolution was an established fact, like the law of gravity or the "roundness of the earth," Bryan complained, was presumptuous; the laws of gravity and the spherical shape of the earth could be witnessed by all. How could a person believe in evolution, Bryan asked, when it could not be demonstrably proven? In this regard, Bryan meant to point out that, because its mechanism of natural selection in man lacked demonstrable proof, evolution had to be accepted on faith. But how was this faith in natural selection, Bryan wondered, any different from faith in the Divine creation of Man? Osborn's rush to defend man's "brute ancestry" meant, the Commoner argued, that the New Yorker accepted as "proof" of evolution "the most absurd stories." Bryan then shifted his attack from Osborn, in particular, to "evolutionists" in general. They built up a philosophy of life, he explained, based on "a brute hypothesis, wholly unproved," and they eliminated all need of the Bible and religious faith because these were inconsistent with their hypothesis that man evolved over centuries from lower organisms.¹⁹ Without proof, how was this theory of man's animal origins any more plausible than the Bible's account? Although it brought to the public's attention the issue of evolution, Darwin's

¹⁹ William Jennings Bryan, "Mr. Bryan Speaks to Darwin" *The Forum* v. 74, n. 1 (July 1925), 101-08.

theory was not very effectively defended by Osborn because he lacked the genetic evidence of similarity in human beings that DNA would later provide. But it was critical for the public to understand and appreciate evolution, because it was key for classic racist arguments in support of restriction.

Perhaps the most salient statement of the stakes in the creation/evolution argument occurred in an exchange between Bryan and lead defense attorney Clarence Darrow on the last day of the trial, though the printed arguments in favor of evolution from Osborn and other scientists continued for the remainder of the decade (including Osborn's collection of essays in defense of evolution, *The Earth Speaks to Bryan*, which he dedicated to Scopes). On the stand, Bryan declared that the strategy of the ACLU was not to defend the freedom of teaching, or to instruct the public on the "guess" of evolution. Rather, he claimed, "the purpose is to cast ridicule on everybody who believes in the Bible..." Bryan maintained that religion, as a source of knowledge, ought to be privileged over all other forms of knowledge. Darrow refused to let this comment stand, and countered that the defense team, and the scientists who wrote briefs for the defense and published articles in journals and magazines, had a much more important purpose: "We have the purpose of preventing bigots and ignoramuses from controlling the education of the United States and you know it, and that is all."²⁰ In this endeavor, Darrow, Osborn and Conklin were of one mind: scientists *did* have a special role to play in advancing scientific knowledge because of their

²⁰ Moran, *The Scopes Trial*, quoted on 156; Henry Fairfield Osborn, *The Earth Speaks to Bryan* (New York: Scribner's Sons, 1925). In addition to Osborn's academic/professional positions that *The New York Times* listed in 1922, this volume included a Doctor of Laws, and Honorary Doctorates of Science from Cambridge University and Yale University.

professional training. They were qualified to speak definitively about theories of knowledge, not Bryan.

Darrow had captured a complex situation in a pithy statement with more significance than even he perhaps knew. It was not only the members of the ACLU and certain groups of esoteric scientists that were trying to prevent “bigots and ignoramuses” in Tennessee from controlling American education. Large numbers of academics and scientists who were concerned over the ways that scientific knowledge was being popularized, misrepresented and exploited throughout the country. It was not the propagandizing itself that rankled; it was that in communicating the principles of scientific investigation, whether of evolution, racial science, feeble-mindedness or intelligence testing, to a non-scientific public, popularizers exaggerated and in some cases, baldly misstated the factual scientific content. Jennings explained this fundamental problem to public audiences throughout the 1930s, when he argued consistently and unyieldingly for the empirical basis of scientific knowledge.²¹ When the preaching ran too far ahead of the empirical, experimental, and observed basis of the teachings for the viability of the intellectual program of scientific examination, it had to be stopped. Even among the adherents of classic racialism, some fringe elements needed to be expunged from association because they were too wild.

“All the respectable people are besmirched by the contact”

²¹ Herbert Spencer Jennings, Lecture 73, “Biological Recommendations for the Ills of Mankind” (Oxford University, 3 June 1936); idem, Lecture 53, “Biologist’s Outlook on the World” (Philadelphia, 18 May 1933); idem, Lecture 48, “Can Men be Bettered Psychologically and Ethically by Better Breeding?” (Rand School, New York, 17 October 1932), all Jennings papers – Lectures.

As the restrictionists changed their tactics in the postwar decade, many continued to frame their strategic arguments in scientific terms. For academic scientists, the most troubling aspect of the manipulation of scientific knowledge for political ends was the way biological knowledge was presented the public. Immigration restrictionists continued to insist that classic racist principles, and scientific evidence in support of them, proved the danger that immigration presented to the American racial stock, the nation's intelligence, American political and social stability, and even its economic well-being. Because biology, demonstrated the pivotal role that heredity played in an organism's development, these nativists explained, the United States was undermining its racial heritage by were allowing immigrants to pollute the blood stream of the nation. This was not, however, what scientists believed in the 1920s. Heredity was very complicated, and ontogeny did not recapitulate phylogeny precisely. During this crucial period, Pearl, Jennings, and others intensified their supervision of who claimed to speak with scientific authority and the claims that they made. One case in particular showed why this was so important. Casper Redfield, an eccentric amateur investigator of heredity, threatened to bring the whole scientific investigation of biology and heredity into disrepute.

In the 1910s, Redfield, who had no professional scientific training, began making dramatic claims about inheritance that brought him to the attention of Stanford President David Starr Jordan, who came across some of his work in

1922.²² Redfield's 1914 book, *Dynamic Evolution* had attempted to refute the evolution of man from lower organisms and retain the Mosaic account of modern man's emergence. "To say that intelligence is a function of the brain, is to say that the brain creates something out of nothing. The brain is clearly a mechanism employed in the process of making intelligence out of some pre-existing thing, but science has indicated nothing out of which it is made, or any work involved in the making." The improvement of man's intellect and his physical strength had to be increased *by* something, he argued. The physical laws of conservation of energy meant that some divine form equipped man, and modern animals, with a surfeit of intellectual energy that then expanded and developed to form the modern beasts and humans that were seen in the surrounding natural world.²³

Jordan asked his friend, the Princeton biologist Edwin Conklin, if Redfield's work had any scientific merit. In an essay published right after the war, Redfield took Galton's and Davenport's analysis of family pedigrees and genealogies of notable men to an unusual extreme, arguing that talent and superiority was a direct function of the time elapsed from the birth of the grandfather to the birth of the grandson. Conklin denounced Redfield's work, but refused to get involved in any further discussion or correspondence regarding him. Redfield had earlier insisted to Conklin that an evil cabal of professional

²² Among Redfield's books: Casper L. Redfield, *Control of Heredity: A Study of the Genesis of Evolution and Degeneracy* (Philadelphia: Monarch Book Co., 1903); idem., *Dynamic Evolution: A Study of the Causes of Evolution and Degeneracy* (New York: G. P. Putnam's Sons, 1914); idem., *Human Heredity* (Chicago: Heredity Publishing Co., 1921); idem., "Great Men and How They are Produced" (Chicago: privately published, 1915); idem., "Descent from Educated Grandfathers" reprint from *The Indianapolis Medical Journal* (ca. 1918/1919), in box 3, Lee papers.

²³ Redfield, *Dynamic Evolution*, quote on v.

scientists was conspiring to suppress Redfield's genius – “he assured me that he was a greater man than Sir Isaac Newton and that I was one of those responsible for blinding the present generation to his greatness,” Conklin told Jordan – and the Princeton professor wanted nothing to do with him. Redfield refused to go away.²⁴

Redfield first came to the attention of professional scientists in the early 1910s, when he published a curious theory of heredity that Pearl harshly reviewed in *The Journal of Heredity* in 1915.²⁵ A short pamphlet, “Great Men and How They are Produced,” issued a challenge to the biological and genetic community to refute Redfield's argument that the production of offspring with superior qualities was a function of the age of the father and the rate at which the generations reproduced. He even offered a financial incentive for a successful rebuttal of his theory that “Rapid breeding inevitably and necessarily leads to the production of inferior stock, no matter what the original stock may be,” and that “Slow breeding is an essential [sic] to the production of superior stock, and, when properly used, inferior stock can be transformed into superior stock in about 100

²⁴ Conklin to David Starr Jordan, 14 July 1922. Edwin G. Conklin Papers, Personal Correspondence, box 12, Series II, Rare Books and Special Collections, Firestone Library, Princeton University, Princeton, N.J [hereafter Conklin Papers].

²⁵ Raymond Pearl, “Dynamic Evolution: Redfield's Theory of Inheritance of Results of Training and Use Not Supported by Adequate Biological Evidence” review of *Dynamic Evolution* by Casper Redfield in *Journal of Heredity* v. 6, n. 6 (June 1915), 254-56. Pearl said: “As a scientific investigator Mr. Redfield labors under at least one very serious handicap.... [H]e is firmly committed to a thesis in advance of the investigation. This assertion he would no doubt deny vigorously.” (254) “It would require altogether too much space to discuss critically all the points of alleged fact and their interpretation brought out in this book,” Pearl insisted. (255) He also called Redfield “either grossly ignorant of the literature of biology and physiology or else feels impelled in his theorizing to soar above all paltry consideration of ascertained biology fact.” (256)

years, and into eminent men in less than 200 years.”²⁶ Redfield’s method was argument by inference and analogy, and associations and correlations provided the physical and empirical proof. Moreover, the associations and correlations were most often gathered from correspondence from professionals, not from any original or institutional research that Redfield conducted.

In this regard, his work was just like Galton’s work on *English Men of Genius*. That professional scientists in the United States now sharply criticized such work shows how far scientific understanding of heredity and biology had come in advance.²⁷ But Redfield continued to gather data and generalize from it with no original effort or critical examination. While this maddened professional scientists, others outside of strictly academic fields did not find Redfield’s sloppy work troubling. In January 1925 Leon Whitney, the President of the American Eugenics Society wrote to Conklin suggesting that, although Redfield’s work was wrong, the man was expending genuine effort that if harnessed properly, could perhaps assist the eugenics movement. Whitney noted that “No matter if we agree with them or not (and I do not, from my present knowledge), we must not laugh at them.” Much like the way Laughlin got involved in the eugenics effort, Whitney felt that enthusiasm for the subject was key. Inaccurate knowledge or hazy misunderstanding could be remedied by training and education; the passion could

²⁶ Redfield, *Great Men*, 10.

²⁷ A notable exception would be Davenport’s *Naval Officers*.
Chapter 6

not. Whitney hoped that the respected Princeton geneticist would assist him in directing Redfield to more productive and more accurate claims.²⁸

This was the last thing the professor wanted to hear, and in his swift reply, Conklin angrily refused. Redfield had no respect for the proper way to conduct research, and his zeal (and ego) blinded him to factual realities that were inconvenient to proving his assumptions. Conklin explained that Redfield had, over the previous four or five years, been “employing stool pigeons in all parts of this country and Canada” who wrote to professional biologists asking their opinions on Redfield’s theory. “Redfield has then taken the letters, garbled the replies by quoting isolated sentences and parts of sentences and thereby has attempted to show that the biologists of the country are fakirs,” conspiring to “suppress the only real genius in this country, namely [Redfield].” Conklin said that the fundamental problem with the amateur’s work was that he selectively employed statistics to demonstrate his arguments. This was, he insisted, no way to conduct proper research. He explained to Whitney, as Pearl and Jennings had thrashed out between themselves almost twenty years earlier, that only integrating methods of statistical analysis and the experimental method advanced scientific knowledge.

Despite this harsh rebuke of Redfield’s work, Whitney persisted, informing Conklin that Dr. John Harvey Kellogg, the Superintendent of the Battle

²⁸ Leon Whitney to Edwin Conklin, 16 January 1925, Conklin Papers, box 19. From 1924-1934, Whitney was the Executive Secretary of the American Eugenics Society, a farmer and D.V.M. (Alabama Polytechnic Institute, 1940). See his brief biography in *Contemporary Authors Online*.

Creek Sanitarium, had once thought that Redfield's work was more or less solid, and his conclusions reasonable, although Kellogg had since changed his mind. Kellogg too, however, was not a scientist and his opinion was unlikely to sway Conklin in either direction. Whitney closed his letter by stating that he would search out Pearl's article in the *Journal of Heredity*, because it was "the only published record which I know to date to disprove the assertions of Redfield."²⁹

Whitney seemed to be suggesting an important and troubling matter to professional biologists: if Redfield was so wrong, why was there only one published rebuke of his theory, and that from ten years ago? Why weren't professional scientists more concerned about setting the scientific record straight? Were Redfield's theories legitimate? Redfield's own letters to academics and professional scientists suggested one answer: the man was not only persistent, but so utterly certain that he was correct that he refused to listen to the criticisms that the actual professionals advanced. Arguing with and refuting Redfield was utterly pointless. But this, as Whitney hinted, lent a dangerous impression of accuracy and legitimacy to Redfield's work. Exposed to this frustrating irony, Jennings and Pearl would later publicly clamp down on the non-scientists that purported to be scientific like Laughlin and Goddard while trying to ignore and avoid any entanglement with folks Pearl to as "cranks."³⁰

²⁹ Conklin to Whitney, 19 January 1925; Whitney to Conklin, 20 January 1925, both box 19, Conklin papers, Series II.

³⁰ Pearl maintained a separate folder in for "Cranks" in his professional papers.

Protecting the professional integrity of scientific organizations and institutions from “cranks” like Redfield became especially important in the post-war decade. When the Science League of America began soliciting membership amongst professional scientists to its Advisory Board, Redfield’s name appeared on the letterhead sent out to members. The Science League of America [SLA] was formed in September 1924 to help educate the public about evolution in particular, and scientific knowledge in general. It was “a nonsectarian and nonpolitical association...formed to protect the rich fruits of scientific freedom, to combat the attacks of so-called fundamentalists who insist on accepting the Bible as an authentic text-book on biology, and who would take away from thousands of earnest and progressive students the right to investigate the phenomena of nature and announce their discoveries.”³¹ It advocated the unfettered teaching of science regardless of the theological or moral implications of valid scientific theories.

Davenport, Pearl, and Jennings assented to membership in the SLA, until they learned of Redfield’s presence on the advisory board. Pearl was furious. He sent a note to Conrad Biron, the Secretary of the SLA, resigning his position on the board. Pearl told Biron that he was resigning because he refused to serve in any organization that Redfield belonged to, and the Hopkins professor then forwarded his resignation to fellow scientists, encouraging them to follow suit. It worked. Jennings replied to his old friend that “I have decided to follow your lead

³¹ “The Science League of America” *American Journal of Public Health* v. 15, n. 7 (1 July 1925), 631-32; also Maynard Shipley, “The Science League of America” *Science* n.s. v. 62, n. 1601 (4 September 1925), 221-222.

in this matter,” and he resigned too. Jennings noted that he had been contemplating what action to take for a few days before Pearl’s letter arrived, but told Pearl that he felt that being listed with someone like Redfield “is certainly too much to ask; they will have to take care if they expect to retain an advisory board of any standing.” Jennings’s note to Biron was also explicit in the reason for his resignation: “If you consider him the type of man for your Board, I certainly have no wish to be connected with it....I object to being listed along with him.”³² The academic and scientific integrity of the SLA was important to maintain. Though both Pearl and Jennings consistently engaged in work just like the SLA undertook – educating the lay public about biology and heredity and describing the limitations and uncertainties of scientific knowledge along with its achievements – to be lumped together with a character like Redfield, who had no scientific training or any inkling of genuine scientific knowledge of heredity, genetics or modern scientific methods, was absolutely unacceptable.

Several of Pearl’s and Jennings’s professional colleagues agreed with their sentiments, but not their action. Conklin suggested to Pearl that there were perhaps enough people of good scientific standing on the Board to cancel out Redfield’s presence, and counseled that the Hopkins biometrist should remain a member of the organization. Pearl would hear none of it. He told Conklin that he had followed Redfield’s work for twenty years, and his opinion of Redfield was that “He is, without any question whatever, a quack and a crook. Every decent

³² Jennings to Conrad J. Biron, 26 October 1925, Jennings papers (folder 16, “Raymond Pearl”). Pearl forwarded his note to Jennings; Jennings’s note to Pearl was typed on a carbon of the letter to Biron.

animal breeding organization in the country...have one by one kicked him out, and he is unable today to publish anything in any of the trade papers of these associations.” Pearl’s change in tone here is emblematic of a larger frustration for him, Boas, Jennings and others in the 1920s. Genetics, zoology, biology and other natural sciences were making significant strides in the range of natural phenomenon that they now understood. They were also acutely aware of lacunae in them. They admitted that there were things they did not yet know, but their insistent, unwavering belief in the proper way to overcome these gaps and uncertainties went back to Jennings’s experiments in Naples. Jennings, in a address at the dedication of the Whitman Lab of Experimental Zoology at the University of Chicago in June 1926, insisted that “We must be able to say: Such and such things happen under such and such conditions, and if you don’t believe it you may supply the conditions, you may try it for yourself, and you will find it to be true.”³³

Men like Redfield had nothing to show except correlations that they took to be causations. Redfield had nothing to say in terms of proper experimental results, just deductions from sloppily gathered data. Redfield had no methods or processes to show how he arrived at his conclusions to enable verification of them. It was important for scientists in the 1920s to protect and define for the

³³ Herbert Spencer Jennings, “Biology and Experimentation” *Science* n.s. v. 64, n. 1648 (30 July, 1926), 97-105; 98. This was a published version of Jennings’s speech, and a very close copy of what became for him, in the mid-1920s, a “stump speech” – see Herbert Spencer Jennings, Lecture n. 37, “Some Unsettled and Unsettling Questions in Biological Science: An Unpopular Talk” delivered at the University of Pennsylvania, March 11, 1926, unpub. mss. in Jennings papers: Lectures.

public what were acceptable parameters for scientific research, and what were not. It was critical that Redfield be labeled a quack and a crook. Otherwise, as Pearl told Conklin, “all the respectable people are besmirched by the contact.”³⁴

But Conklin did not share this position, so the professional scientific community must not be regarded as having adapted and accepted universally these methods and cautions. Conklin needed further convincing. He wrote to Jennings that, despite his displeasure with some of the problematic information the Science League had been putting out, he had doubts as to the wisdom of resigning. Jennings insisted that the placement of Redfield on the Board confirmed his impression that “the people who are running the League know nothing about science, and they are likely to let us in for other things we don’t wish to stand for. It seems to me, too, that they really must choose between scientific men and such as Redfield.” Conklin, finally convinced, then submitted his resignation to Biron as well.³⁵

If Conklin, who did take the responsibilities of accurate scientific work very seriously, was ultimately swayed, others were not. Davenport, perhaps not surprisingly, was more forgiving of associations with unscientific men. Although he submitted his resignation to the SLA’s President Maynard Shipley in late October along with Pearl, Jennings, and Conklin, Davenport’s own attempts to organize broad coalitions between scientists and non-scientists made him

³⁴ Conklin to Pearl, 23 October 1925; Pearl to Conklin, 27 October 1925, both in box 18, Conklin Papers, Series II.

³⁵ Conklin to Jennings, 26 October 1925; Jennings to Conklin, 27 October 1925, both Conklin Papers, Series II, box 12.

sympathetic to Shipley's dilemmas. When Shipley replied to Davenport that the League ideally desired "as broad a representation of the various groups of citizens as possible," to combat the religious fundamentalists like Bryan, who believed the Bible was the source of all knowledge, Davenport's own work in the International Eugenics Congresses, the Eugenics Research Association, and his other eugenic groups gave him first-hand experience in creating coalitions between uneasy partners for the goal of popularizing esoteric scientific knowledge. In the past, Davenport had forged many unstable associations between men of suspect credentials, and he would continue to do so. When Redfield was finally removed from the Advisory Board, only Davenport felt compelled to withdraw his resignation.³⁶

For Davenport, Redfield posed a complicated challenge. While academic scientists like Pearl and Jennings absolutely refused to be associated in any way with him or his work, Davenport saw a degree of usefulness in Redfield's publications and pronouncements. Like Albert Wiggam, Redfield was, right or wrong, ultimately successful in raising public awareness of the possibilities of eugenics and heredity. When Redfield's book *Human Heredity* was published in 1921, Davenport saw to it that the book was reviewed in *The Eugenical News*. Although critical, describing the book as mostly "expression of opinion with out evidence," the reviewer described Redfield as a "trained advocate." But Redfield had had no proper scientific training – the review, in fact, stated that he was a

³⁶ Maynard Shipley to Davenport, 28 October 1925; Davenport to Shipley, 3 November 1925, both in Davenport Papers.

patent lawyer. This was the crux of the issue for professionals like Jennings and Pearl. It was simply not possible that a patent lawyer could competently speak on the complex scientific theories of heredity.

Garland Allen notes how Thomas Hunt Morgan in the aftermath of his *drosophila* studies in the mid-1910s began discussing on occasion his “new biology” and his philosophy of science. Morgan criticized the abstract idealism of men like Henry Fairfield Osborn in the late in 1910s, whose work he felt had little comprehension of genetic and embryological science. Having made tremendous gains as a result of carefully experimental work in the second decade of the twentieth-century, in the 1920s the “new” biologists were not about to be drawn backwards.³⁷ Although the brief article in the *Eugenical News* explained, “it is true that others have investigated some of the same data as he and got different results,” it did so in such a way that it seemed that this was a basic problem in scientific inquiry and did not itself make the book’s conclusions problematic. The correspondence between Jennings and Pearl in 1908 attests to this problem. Davenport seems to have made a calculation that Redfield’s “Lamarckian” views (which were disproved by much of the then-current genetic research, as Pearl learned with his chickens at Orono) were an acceptable inaccuracy when in reality they were unequivocally wrong. As long as the public was cautioned not to believe everything the patent lawyer wrote—as the review in the *Eugenical News* advised—raising awareness of scientific knowledge was a worthy endeavor, even

³⁷ Allen, *Thomas Hunt Morgan*, 318ff. It is interesting to note that Morgan did not win the Nobel Prize for his work in genetics until 1933, although he had been nominated for it in 1919 and 1922. *Ibid.*, 372-73. See also Brush, “How Theories became Knowledge.”

if that knowledge was partly, or mostly, inaccurate. Propaganda, for men like Davenport, had its place and its utility.

This is not to suggest that Davenport rubber-stamped any book dealing with eugenic or genetic claims in *The Eugenical News*. In an acid review in 1920, the Cold Spring Harbor Director had damned Ellis B. Gould's book *The Science and Philosophy of Eugenics*, published in the same year. The book never should have been written, especially by an author, who "merely reveals himself as a person untrained in science, distorted in judgment, accepting as established the most questionable things..." The book, the review described, was laden with super-natural references to wraiths, ghosts, and the influence of signs of the zodiac on the development of organisms. "The pity is there are so many people who will think this worthless book 'perfectly splendid' just as they pin their faith to the airy nothingness of Christian Science."³⁸ It was this Christian Science that took center stage in Dayton in 1925. Davenport would employ an equally critical tone later in the decade when a man based in Oakland, California, R. Clay Jackson, attempted to secure Davenport's endorsement for his pamphlet on the role of sunshine in determining the sex of offspring. Davenport functioned as a gatekeeper, establishing boundaries for what was acceptable scientific propaganda and what was not. For nearly a decade Jackson taunted Davenport, daring him to disprove his theory, and like Redfield, implicated a cabal of scientists that were suppressing marvelous scientific discoveries. When Davenport tried to close the

³⁸ "The Science and Philosophy of Eugenics" Review of Ellis B. Gould, *The Science and Philosophy of Eugenics* (Kansas City: Brinton Publishing Co., 1920) in *The Eugenical News* V. 5, n. 11 (November 1920), 85.

circle of scientific knowledge to this obvious outsider (Jackson's pamphlet "Sex, Shelter, and Sunshine" suggested that men who worked in the evening, and had less exposure to sunshine, tended to produce male children), he exposed his own hypocrisy.³⁹ Work by "trained" men like Albert Wiggam, Lothrop Stoddard, Madison Grant, and Ernest S. Cox would receive favorable reviews—the recommendation of Cox's book, *White America* was particularly outrageous—in *The Eugenical News* despite the fact that their arguments were no more scientifically accurate than those of Redfield, Jackson or Gould.⁴⁰

What explains this varying approach to scientifically inaccurate books? Why did Davenport's organization recommend certain arguments based loosely on science, and condemn others equally loose in scientific accuracy? One of the clearer explanations for this approach by Davenport is gained through his correspondence. Davenport recommended, and supported the work of, men whom he knew personally. It is likely that Davenport felt, because he *knew* them, that he could be confident they at least had a rudimentary understanding of heredity and inheritance. Davenport's approach to science in this respect was largely correlative, and for Davenport, like Galton, the social position of a writer was an

³⁹ Jackson's correspondence with Davenport began in 1927, and continued through the mid-1930s. R. Clay Jackson to Davenport, 6 October 1927; Davenport to Jackson 15 October 1927, both Davenport papers. All correspondence is located in folder "R. Clay Jackson," Davenport papers, which also contains the pamphlet "Sex, Sunshine, and Shelter."

⁴⁰ Cox's book was a paean to white supremacy, and argued that the elimination of all non-white (i.e. black) members of the United States was the only way to save America from destruction. The review noted that "It is to be imagined that many of the negroes and their parasites will object strenuously. But America is still worth saving for the white race and it can be done. If Mr. E. S. Cox can bring it about he will be a greater savior of his country than George Washington. We wish him, his book and his 'White America Society' godspeed." "White America" Review of Earnest Sevier Cox, *White America* (Richmond, VA: White America Society [n.d.]) in *The Eugenical News* v. 9, n. 1 (January 1924), 3.

indicator of his scientific veracity.⁴¹ Eugenics was not an particularly complex science as long as one assumed that heredity always operated according to the same principles; as long as like continued to produce like, eugenics was “proven.” This was the kernel of classic racist belief, and although the true mechanisms of transmission were becoming better understood because of Morgan’s chromosome and genetic research, the similarities between parent and offspring functioned as suitable evidence of the truth of classic racialism and biological determinism. Eugenics was allegedly a science – mysticism and sunshine were not. Finally, and somewhat more speculatively, Davenport struck up correspondence only with those men who demonstrated the necessary deference to him. If Davenport had been involved in “training” them—and he often mentioned their “trained” or “untrained” status in reviews, as with Jackson and Wiggam—their work proved acceptable. If not, he deemed the work “worthless.” Herbert Spencer Jennings, as far back as 1896, had seen this closed-mindedness in Davenport. By the late 1920s, this closed-mindedness had evolved into such self-certainty and self-righteousness that Davenport could not feel the movement slipping away from him.⁴² The way that he responded to popularized and exaggerated accounts of

⁴¹ Shapin, *The Social History of Truth*.

⁴² Garland Allen, the chief historian of scientific opposition to eugenics, has written in his institutional history of the Eugenics Record Office that Davenport and Harry H. Laughlin (the ERO’s Superintendent) were determined to keep the ERO strictly scientific. But in perusing the actual records and reports submitted by field workers, Allen found that most of the data gathered by the ERO “were of a subjective, impressionistic nature” (242-3), which rendered the data sets genetically useless. He singles out data on height to demonstrate that an entry on the height of a person did not mean it was measured, or measured with any accuracy but was instead *reported*. He also notes Davenport’s tendency to argue by analogy, not direct evidence. The Carnegie Institution sent a “visiting” committee to Cold Spring Harbor in February 1929 to evaluate the ERO’s work; the report of the second committee in 1935 recommended closing the Record Office completely.

science is nicely embodied in the Cold Spring Harbor superintendent's attitude toward the work of Albert Wiggam.

"That article by Wiggam in the March 'Century' is the most important contribution to popular education that has been made in fifty years," gushed a Political Science professor at Columbia University to the editor of *The Century Magazine*. The author continued: "I am not speaking hastily or recklessly but after full consideration and weighing my words." The article that Franklin Giddings referred to, "The New Decalogue of Science," went from brief article, to book, to lightning rod for professional scientists who hoped to curtail the misuse of scientific information in the span of a few short years.⁴³

The article, which was reprinted as the first chapter of Wiggam's book of the same title, was "An Open letter from the Biologist to the Statesman" encouraging political leaders to utilize the conclusions of modern scientific knowledge and scientific tools to recast society along rational, scientific principles.⁴⁴ The article was wildly popular with non-biologists. As soon as the article appeared, Charles Davenport wrote to its author to thank him for the gratis copy sent to him at Cold Spring Harbor. "You have said very vigorously what I have often wished somebody would say," he told the writer. Requests for reprints

Garland Allen, "The Eugenics Record Office at Cold Spring Harbor, 1910-1940: An Essay in Institutional History" *Osiris* 2nd series, v. 2 (1986), 225-64.

⁴³ Franklin Giddings to Glenn Frank [editor of *The Century Magazine*] 14 March 1922, Davenport papers; Albert E. Wiggam, "The New Decalogue of Science" *The Century Magazine* (March 1922).

⁴⁴ Albert E. Wiggam, *The New Decalogue of Science* (Indianapolis: Bobbs-Merrill Co., 1923), see 15-22 for the text of the article.

of the article came from Davenport, who asked for “several thousand,” Franklin Giddings, who said he would make it required reading if he could secure the article “in quantity,” and even the psychologist and Clark University President G. Stanley Hall, who lamented that Wiggam had written what Hall had long wanted to.⁴⁵ Wiggam was thrilled with the attention. He had, for nearly a decade, been a lecturer on the Chautauqua circuit where he spoke frequently on eugenics. Wiggam also wrote a nationally syndicated science column called “Let’s Explore Your Mind” for the Adams News Service for over thirty years. In 1914 he had struck up a correspondence with Princeton geneticist Edwin Conklin in an effort to stay abreast of developments in genetics and heredity.⁴⁶ When Davenport, in his capacity as the Director of the Station of Experimental Evolution at Cold Spring Harbor praised Wiggam’s article, Wiggam felt that he had finally attained a degree of scientific legitimacy and authority. The reactions to Wiggam’s book from professional academic scientists, however, would prove these hopes to be unfounded.

Davenport’s consistent praise of Wiggam’s writings demonstrates one of the essential difficulties that scientists in the period had in communicating complex ideas to the lay public. When the article was expanded into a book, *The New Decalogue of Science*, Wiggam continued his plea for the use of scientific

⁴⁵ Charles Davenport to Albert Wiggam, 9 March 1922; Giddings to Frank, 14 March 1922; G. Stanley Hall to Albert Wiggam [no date], all in Davenport papers. Copies of all of these notes are contained in Charles Davenport’s correspondence file with Wiggam.

⁴⁶ “Albert E. Wiggam” *The National Cyclopaedia of American Biography* v. 44 (New York: James T. White and Co., 1962), 542-3. Albert Wiggam to Edwin G. Conklin, 9 April 1914, Conklin Papers, Personal Correspondence, box 23, Princeton.

knowledge to stem the collapse of human civilization. Science was an inspired tool, he insisted, ready to be used by thoughtful statesmen to arrest the collapse of humanity. “[W]hen you take man out of the bloody, brutal but beneficent hand of natural selection,” he wrote, channeling the spirit of Herbert Spencer, “you place him at once in the soft, perfumed, daintily gloved...hand [of] artificial selection.” This was, of course, an unnatural process. Wiggam explained that reformers and progressives, who had sought to use their expertise to alleviate the suffering of all peoples, were actually destroying humankind by preventing the weak from being killed off. He implored his readers that “unless you call science to your aid and make this artificial selection as efficient as the rude methods of nature, you bungle the whole task.” Wiggam argued that policies designed to “nurture” human organisms – reforming their environment or educating them – were a catastrophe on an unimaginable scale, and fervently pleaded that it was not yet too late to abandon this coddling of inferior stocks who otherwise were consigned by nature to extinction. The scientific facts behind these ideas, however, were highly suspicious. They were so suspicious, in fact, that they prompted the otherwise discreet Herbert Spencer Jennings, ironically named for the founder of Social Darwinism, to publicly ridicule Wiggam’s writings.⁴⁷

The review for Wiggam’s second book, *The Fruit of the Family Tree* (1924) in *The Eugenical News* was generally positive. *Fruit*, which described the

⁴⁷ Wiggam, *The New Decalogue of Science* (Indianapolis: Bobbs-Merrill Press, 1923), quote from 34; Herbert Spencer Jennings, Lecture n. 37, “Some Unsettled and Unsettling Questions in Biological Science: An Unpopular Talk” delivered at the University of Pennsylvania, March 11, 1926, unpub. mss. in Herbert Spencer Jennings Papers: Lectures.

latest theories of the heredity of disease, twins, genius and beauty, was less “pro-science” than *Decalogue*, and was more in line with specific eugenic goals. The review emphasized that Wiggam was a lecturer, not a scientist, but praised Wiggam’s effective work in popularizing complex scientific theories. The brief notice described the book as subject to a “few lapses of memory or confusion of facts, [but] such is inevitable when one is digesting a great mass of them.”⁴⁸ Davenport’s praise for Wiggam extended beyond the printed word; in an undated lecture, the ERO Director spoke on “Albert Wiggam and Eugenics,” where he complimented the author for being the popular spokesman that eugenics needed. Francis Galton, Davenport told his crowd, was “a poor propagandist,” and Karl Pearson “an esoteric.” Their theories – particularly Pearson’s – were too complex for a lay public to understand. Therefore, all eugenic enthusiasts “must rejoice” that there is Albert Wiggam, who “can put the facts of heredity so forcibly and convincingly before the public.”⁴⁹

Despite these praises, Davenport’s association with Wiggam began on a rocky footing. After the 1922 *Century* article came out, the Adams Science Service for which Wiggam wrote his regular column ran a large ad mentioning Wiggam’s “Decalogue” article. Under a large, bold headline the advertisement stated “The CARNEGIE INSTITUTION puts its stamp of approval on Mr. Wiggam as a scientist and writer by ordering for distribution all over the world

⁴⁸ Albert E. Wiggam, *The Fruit of the Family Tree* (Indianapolis: Bobbs-Merrill, 1924); “Fruit of the Family Tree,” review of Albert E. Wiggam, *The Fruit of the Family Tree* (Indianapolis: Bobbs-Merrill, 1924) in *The Eugenical News* v. 10, n. 2 (February 1925), 21.

⁴⁹ Charles Davenport, Lecture “Eugenics and A. E. Wiggam” [n.d.] Davenport papers.

5,000 reprints of his article...” This put Davenport in an awkward position: the Carnegie Institution never gave Wiggam any authorization to use its endorsement to recommend the book; indeed, never *had* given its endorsement to Wiggam’s article. Davenport’s own personal note to Wiggam, which had asked for the reprints, was mischievously used by the managers of the Adams syndicate to suggest that a notable, important institution like the Carnegie Institution had approved of Wiggam’s writings. The Washington, D.C.-based Carnegie Institution, which was one of Cold Spring Harbor’s most important sources of funding, came down on Davenport to force Wiggam to stop using their name in promoting his article. As Wiggam explained in a frantic series of letters in September, it was merely an oversight that this had occurred, and he quickly pled ignorance to the inner workings of press syndicates and how they marketed and advertised the articles they published. But, Wiggam explained, much of this could have been avoided if scientists were better at explaining their ideas to the public. Unfortunately, Wiggam said, “there is no science of propaganda. You and your assocaites [sic] find out many facts of social evolution, but in making them effective we can only proceed by mutual council and I might say ‘gentleman’s agreements.’” The problem, as he saw it, was that “the scientist is a bit likely to be somewhat too strict and literal in his statements to reach the public at all and the journalist too little regardful of fact.”⁵⁰ If the scientist, too devoted to literally

⁵⁰ Albert E. Wiggam to Davenport, 4 September 1922; Wiggam to Davenport, 11 September 1922; Wiggam to Davenport, 20 September 1922; all in Davenport papers. Davenport’s correspondence file with Wiggam also contains a newspaper clipping for the advertisement, which was sent to newspaper editors by the George Matthew Adams Service. For the Carnegie Institution, see Jane

correct and true assertions of scientific fact, did not describe to the public his findings, what good was the knowledge? What was needed was a journalist with a background in scientific training to bridge the two poles. Wiggam flattered himself that he could perform this task.

Although the two patched up this misunderstanding, Davenport's position and reaction are intriguing. He nearly endangered his beloved facilities by failing to police the boundaries of science adequately and to ensure that his reputation, and the reputation of his patrons, was not damaged in any way by shoddy scientific work. But he was conflicted by the desire – the necessity – of informing the public on the work that the Station for Experimental Evolution and the Eugenic Record Office were carrying on, a task that he himself had consistently been reluctant to perform. For Davenport, the preaching and teaching of scientific theories went hand in hand, and if the preaching sometimes outpaced the theory, it was a calculated risk that he was often willing to take. Wiggam was suitable for the job; he was an effective advocate. But the decision to not restrict the claims of eugenicists to their literal and factual basis (which was really nothing), proved fatal to his precious eugenic movement.

“One of the most useful citizens in the United States”

The complications that Davenport experienced trying to manage and police the boundaries of scientific knowledge can also be seen in his associations

Maienschein, Marie Glitz, Garland Allen, eds., *The Department of Embryology* vol. 5 of *The Centennial History of the Carnegie Institution of Washington* (Cambridge: Cambridge University Press, 2004).

with Madison Grant. Grant embodied many of the correlative methodological approaches to scientific investigation, and like Wiggam, relied on his social connections to legitimize his “scientific” credentials. The increasing influence that Grant wielded over the direction of the nativist movement in the 1920s, and his adherence to classic racialist orthodoxy that was no longer scientifically tenable, led professional academics to confront Grant’s mischievous misrepresentations of the scientific basis of race. Though rarely mentioning heredity and genetics in his writings, the New York patrician emerged as one of the most vocal, strident, and effective nativists during the early twentieth-century push for immigration restriction. Lacking any professional scientific training, or any genuine grasp of the complexities of heredity and genetics, Grant nonetheless became one of the most prominent names of the immigration restriction movement. His connections with scientists like Davenport and Henry Fairfield Osborn, policy-makers like Congressman Albert Johnson, and institutions like the New York Zoological Society ultimately made him a well-connected advocate. As restrictionists began to secure favorable legislation, and shifted tactics to barring immigrants based upon their “racial” identity, backers of legitimate scientific knowledge were forced into a showdown with the movement. For much of the elite public, this showdown between methods and content occurred in 1925 in the pages of *The Nation* and *The Forum* magazines.

Grant and Davenport had an association going back to 1908 when Grant received some of Davenport’s writings.⁵¹ Their association was strengthened in

1914 when a Philadelphia doctor, J. Madison Taylor, forwarded to Davenport a letter that Grant had sent to Taylor. Grant wrote the physician questioning the validity of some of Davenport's claims in *Heredity in Relation to Eugenics*. Davenport responded directly to Grant, in an attempt to clear up some of the confusion about eugenics. Davenport expressed confidence in the veracity of eugenic claims that he made in his book. In the note's certainty that like produced like, that biology was destiny and that body forms reflected the unchanging racial typology of an individual were the seeds for the future of Davenport's eugenics movement. And Jennings, Pearl, Boas and Hrdlicka would later challenge the certainty with which Davenport conveyed these alleged truths to Grant.

In response to Grant's criticisms to Taylor, Davenport made some curious confessions. He admitted to Grant that "I know full well the book is not as good as it might have been, as I could have made it, if I had been willing to spend more time." As he addressed Grant's criticisms on the racial mixing described in the book, the biologist said that as far as he was concerned, all humanity came from ancestors with dark skin, and it was white skin that was, evolutionarily speaking, highly abnormal. To suggest that whiteness was abnormal was a politically charged statement, but it was true—and the consequences of a scientifically true statement, Davenport insisted, "are the last things in the mind of a scientific writer." Scientific inquiry had to proceed, in Davenport's mind, regardless of the consequences. But for someone like Grant, who believed vehemently in the

⁵¹ The essays were Davenport's "Heredity of Eye Color in Man," "Heredity of Hair Form in Man," and "The Determination of Dominance in Mendelian Inheritance." H. J. Shorter (Secretary to Madison Grant) to Charles Davenport, 28 August 1908, Davenport papers.

supremacy of the white race, and who thought the Nordic branch of that race was the pinnacle of all human development, Davenport's statement should have been horrifying. Surprisingly, Grant's reply was actually tame. White skin is certainly abnormal, the New Yorker agreed, "but I doubt very much whether we ever had any direct negro blood in our ancestry." In fact, this would be impossible for Grant to believe; he was writing at this time *The Passing of the Great Race*, in which he declared that any mixture of a white and a non-white resulted in a non-white. The implication of his statement was that even over the course of thousands of years the taint of "negro" blood could never be effaced.⁵²

This frank discussion of an evolutionary hypothesis brought Grant and Davenport closer together. This closeness became key for validating Grant's scientific credentials. When *The Passing* was published, Grant had an advance copy sent to Long Island for Davenport to read. Davenport quickly sent his approval of the work, which, as was so often the case, meant a great deal to a layman. Expressing his appreciation, Grant wrote back, "I must say that it has really given me a great deal of pleasure because I have been greatly disappointed in the failure of American biologists to support me, as they all seem to be either afraid of Boas or else impregnated with Socialism."⁵³ In Grant's mind, a scientist's opposition or criticism of his work did not stem from the dubious

⁵² Madison Grant to J. Madison Taylor, 27 February 1912; Davenport to Grant, 1 July 1914; Grant to Davenport 6 July 1914, all "Grant, Madison" folder, Davenport papers. Grant, *The Passing of the Great Race*.

⁵³ Davenport to Grant, 10 February 1917; Grant to Davenport, 16 February 1917, Davenport papers. Redbaiting would become a common tactic for Grant in the late 1910s and 1920s; he repeated the assertion in April. See Grant to Davenport, 2 April 1917, Davenport papers.

scientific arguments he made in his polemical writings. Grant believed that the criticism came from personal animus, intimidation, or political radicalism. He felt that his arguments were scientifically accurate, and Davenport's affirmation merely reinforced that. As he prepared the second edition of the book in 1917, he wrote to Davenport, who was then serving in the Surgeon General's Sanitary Corps, for constructive criticism of the first edition. "As we are all working for the same causes, I want to make this second edition fool-proof, so that it can be used and quoted without hesitation by those of us, who have the courage to remain [restrictionists]." What Grant feared was that American biologists would oppose and criticize the new edition, because they were Socialists or they "appear to be of an ethnic strain, which precludes the admission of the existence of any stock or race gifted with superior attributes."⁵⁴ Grant remained convinced in the correctness of his scientific arguments. Only injured race pride or un-American political ideologies could explain hostility to his work. He shared Davenport's self-certainty and self-righteousness too.

These shared sentiments, and their common lackadaisical approach to accurate scientific knowledge, ultimately made the two men close allies in the movement to protect the Anglo-Saxon race in America. Classic racialism served as cement for their bonds of friendship and collaboration throughout the 1920s. When Grant needed to recover from a persistent and bothersome inflammation in his knees in 1923, Davenport reached out to the Director of the Battle Creek Sanitarium—with whom Davenport had become acquainted through the Race

⁵⁴ Grant to Davenport, 16 February 1917, Davenport papers. Grant somehow avoided service in the war effort, despite having no spouse or family to support.

Betterment Foundation—to introduce Grant before he arrived. “In receiving Mr. Madison Grant you will receive one of the most useful citizens of the United States,” Davenport informed John Harvey Kellogg. “A lawyer by vocation he is a traveler, hunter, explorer by avocation but, best of all, he has been instrumental in getting thru the legislature some of the most useful legislation.” Grant was a prominent conservationist, and the Director of the Station for Experimental Evolution elaborated on Grant’s success in securing reservations for western bison, securing protected status for western lands including the California Red Woods, and in New York, establishing the Bronx Park, the Bronx River Park, and the New York Zoological Society’s Zoological Park and Aquarium. But the “useful legislation” Grant was instrumental in passing was the immigration restriction laws of the early 1920s. The New York lawyer was an important resource for fanning the flames of Anglo-Saxon racial insecurity. Davenport pointed out these common interests between Grant and Kellogg. Not only was Grant “largely instrumental” in securing the immigration legislation, he was also the Treasurer of the Second International Eugenics Congress that was held in New York City in 1921. With an endorsement like this to recommend him, Grant received the greatest hospitality that Kellogg could offer.⁵⁵

When others heard of Grant’s ailments, however, they were not so sympathetic. Kellogg extended a similar recuperative invitation to Ales Hrdlicka in October 1923—while Grant was still in Battle Creek—but Hrdlicka declined.

⁵⁵ Davenport to John Harvey Kellogg, 17 August 1923, Davenport papers. See also Spiro, “Patrician Racist.”

Kellogg and Hrdlicka knew each other moderately well, and Kellogg was one of the early financial backers to Hrdlicka's *American Journal of Physical Anthropology* in the late 1910s. But there was no way that the D.C.-based anthropologist would consider being in the same facility as Grant: "Madison Grant ought to be afflicted with everlasting rheumatism of all his writing organs, for he has done a great deal of mischief with his 'Nordicism,'" he wrote in declining the invitation.⁵⁶ Hrdlicka refused to associate with such a disagreeable person. Not only was Grant creating mischief with his classic racist "Nordicism," but by invoking totally inaccurate science to justify his racial prejudice, he was having real-world impacts on the lives of Europeans trying to emigrate to America. Grant slandered the people of southern and eastern Europe – people from his own background whom Hrdlicka knew well – and dressed up his slanders in the garb of scientific objectivity. His rheumatoid afflictions, in Hrdlicka's mind, were just retribution.

Boas's opinion of Grant was no better. For highly respected professional anthropologists like Boas and Hrdlicka, Grant's work and the influence it had was tremendously frustrating. It compelled them to try to demolish the claims that issued forth from "the Charlatan Grant," as Boas called him in private, in published essays, book reviews, and even, as Hrdlicka's note to Kellogg shows, in private correspondence. As Grant's work became increasingly influential to the

⁵⁶ John Harvey Kellogg to Ales Hrdlicka, 30 October 1923; Hrdlicka to Kellogg, 6 November 1923, both box 37, Hrdlicka papers. For notes on Kellogg's role in helping to fund the *American Journal of Physical Anthropology*, see Kellogg to Hrdlicka 1 April and 5 April 1918, box 37, Hrdlicka Papers.

public, and as he published more and more of his inaccurate scientific musings, the two anthropologists tried any method they could to discredit him.⁵⁷

But they faced a significant dilemma in trying to correct the inaccurate claims that non-scientists like Wiggam and Grant were making. *The New Republic* offered Boas one of the first public forums to critique Grant's work when *The Passing of the Great Race* was first published in 1916. Boas's review was severely critical, although he tried to maintain an amicable and professional tone. He opened the review by explaining that citizens of New York, of whom Boas was one, owed Grant a great debt for his work at the Zoological Garden and the American Museum of Natural History. But this debt, and Grant's scientific associations, must not cloud readers of *Passing* from recognizing the serious errors at the heart of the book. The Columbia anthropologist placed Grant's work squarely in the camp of reactionary, white-supremacist amateurs like Arthur Comte de Gobineau and Houston Stewart Chamberlain, describing Grant's book as "a Cassandric prophecy of all the ills that will befall us on account of the increase of dark-eyed types." Boas then proceeded to dismiss every argument Grant tried to make in the book, demonstrating that Grant's understanding of the way heredity functioned was extremely faulty. Citing Hrdlicka's and his own work on the malleability of human forms, and the influence of the environment on physical stature, Boas denied Grant a hereditary basis for his arguments of pure, unchanging types. In several paragraphs, Boas pointed to arguments that Grant had fashioned out of thin air. Concluding his dismissive review, Boas noted that

⁵⁷ "The Charlatan Grant" is from a letter of Boas to Hrdlicka, 18 March 1919, box 14, Hrdlicka Papers, NAA.

the kind of “race aristocracy of which Mr. Grant is dreaming is unreal.”⁵⁸ He and Hrdlicka hoped this critical review in a popular magazine like *The New Republic* would suppress the popularity of the book and caution readers of the significant uncertainty in anthropological and racial sciences. They did this not out of malice, but because Grant’s work was scientifically wrong. They did not succeed.

When *The Passing of the Great Race* was issued in a second edition Hrdlicka noted that “in some respects is even worse than the first,” and offered Boas his *American Journal of Physical Anthropology* as a forum to try again to combat the erroneous arguments of the “Charlatan Grant.” Although he judged *Science* the best place for a review, as it would reach a wider audience, Hrdlicka earnestly wanted Boas to write another for any journal. “I do not remember having ever seen a book either more pretentious or more biased,” he lamented. Hrdlicka also saw a greater danger in not trying to demonstrate the erroneous scientific claims in the book, since “unless promptly shown exactly what it is, [the book] may be used to influence men in important positions who are now trying to get all possible data on the European nationalities in the way of preparation for eventual [peace] negotiations.” Not only were Grant’s classic racist ideas threatening the hopes of European emigrants and cultivating racial prejudice, but *Passing* could also be used as support for making racial divisions and invidious distinctions during the peace negotiations. He feared the book could even be used as leverage to establish a separate committee on “Race” in the Peace Council, that

⁵⁸ Franz Boas, “Inventing a Great Race,” review of *The Passing of the Great Race* by Madison Grant, *The New Republic* v. 9, n. 115 (January 13, 1917), 305-08.

could, like the Dillingham Commission had tried, devise a universal statement of racial differences and a hierarchy of superior races. Grant might be made manager of such a committee. Considering Grant's views on the Anglo-Saxon race, this could not be allowed to happen.⁵⁹

Boas was not able to review the new edition for *Science*; Frederick Adams Woods secured the privilege, and he gave it a rather positive review, explaining "there is a mass of carefully furnished statistical research on the problem of human heredity which tends to support the whole theory of race as against environment."⁶⁰ This was a highly contentious claim, and precisely the type that Hrdlicka and Boas were trying to prevent. The mischief that would come of the book – now reviewed favorably in a widely-read scientific journal – bothered Boas, but he had reviewed each edition, and did not know what else to do. The use of the book to influence the public was hard to combat "when scientists who have a reputation [Woods was a geneticist at the Massachusetts Institute of Technology] will lend themselves to support views of this sort." Woods was, in fact, sympathetic to Grant's views; he was on the Advisory Council of the American Eugenics Society, and his own work, like the *Heredity of Monarchs*,

⁵⁹ Hrdlicka's handwritten note at the bottom of this letter said that the possibility of a "Race" committee on the Peace Council was confidential. Hrdlicka to Boas, 6 May 1918, *Boas Film*.

⁶⁰ Frederick Adams Woods, review of *The Passing of the Great Race*, by Madison Grant, *Science* n.s. v. 48, n. 1243 (October 25, 1919), 419-20.

was equally scientifically dubious. If Woods's understanding of heredity was so faulty, he certainly could not be expected to point out Grant's inconsistencies.⁶¹

Having previously been unsuccessful at dissuading the public from lending credence to Grant's writings, Boas leapt at the chance to pen the first entry when *The Nation* dedicated a series of columns on "The Nordic Myth" in 1925. He chose for his topic "What is a Race?" and suggested that, although racial antagonisms obviously existed between groups, the pressing question – instead of "which race is better?" – was where did these antagonisms come from? Children's behavior, the anthropologist pointed out, suggested that race consciousness was learned or socialized. The reality of race, Boas wrote, was incredibly complex, and the terminology used by immigration officials had only increased the confusion because "they designate people speaking different languages and of different political association as races without any regard to their biological characteristics." Heredity, Boas cautioned, could be spoken of only in genotypic lines in immediate families. To speak of general racial characteristics, whether anatomical, mental, or physiological across a broad group, was arbitrary, misleading, and incorrect.⁶² Yet this was precisely what classic racialism proposed.

The succeeding entries in "The Nordic Myth" series reinforced this notion that racial groups, as restrictionists, nativists and classic racialists used them, were

⁶¹ Boas to Hrdlicka, 23 February 1918, box 14, Hrdlicka Papers, NAA. For Woods's position on the Advisory Council, see Barry Mehler, "A History of the American Eugenics Society, 1821-1940" (Ph.D. Diss., University of Illinois at Urbana-Champaign, 1988), Appendix.

⁶² Franz Boas, "What is a Race?" *The Nation* v. 120, n. 3108 (28 January 1925), 89-91.

non-existent. There were no pure racial groups, there were no particular hereditary “gifts” of specific to racial groups, and the scientific underpinnings of such ideas were completely unfounded. When Konrad Bercovici wrote in mid-March that only Nordics declared their superiority over all other races, he spoke a fundamental, if unspoken, truth. No other group claimed the intellectual, moral, political and racial supremacy that Nordics claimed for themselves. The entire foundations of religion and science, including Christianity, math, and geometry, Bercovici wrote, were actually laid down by non-Nordics. Yet Nordics not only asserted their superiority, they grounded that superiority in scientific theories, which they deployed to legitimize their dominance.⁶³

In addition to criticizing Grant’s classic racialism in *The New Republic*, Boas also used an offer from *The Forum* in 1925 to confront Grant directly on his scientific inaccuracies. In September, Grant had published “America for the Americans” in *The Forum*, explaining that the swarm of immigrants that had been invading the United States threatened to “submerge the native population” and reduce American standards of living to those in China. America, Grant argued, had a duty to protect itself first and foremost, and the sentimental objections of

⁶³ Konrad Bercovici, “You Nordics!” *The Nation* v. 120, n. 3115 (18 March, 1925), 288-90. Bercovici’s was the fourth in the series; Melville Herskovits, “Brains and the Immigrant” *The Nation* v. 120, n. 3110 (11 February 1925), 139-41; Edward Sapir, “Let Race Alone” *The Nation* v. 120, n. 3112 (25 February 1925), 211-13; Hendrik Willem van Loon, “Our Nordic Myth-Makers” v. 120, n. 3117 (1 April 1925); Alexander Goldenweiser, “Can There Be a ‘Human Race’?” *The Nation* v. 120, n. 3120 (22 April 1925). Interestingly, Bercovici’s book *On New Shores* (New York: Century, 1925) appears on Madison Grant’s “List of Authoritative Works on Immigration” that he submitted to the House Committee on Immigration in 1929. Bercovici had emigrated to the U.S. in 1916 from Rumania, and wrote several books about the immigrant experience in the United States. “List of Authoritative Works on Immigration, prepared by Madison Grant, 1929” in RG 233 [cite is box 66, HR 70A – D. 10, fold. HR 6465-HR 55094]; Bercovici’s biographical information is from *Merriam-Webster’s Biographical Dictionary* (Springfield, MA: Merriam-Webster, 1995).

pro-immigration forces, that the United States needed the labor and energy of immigrants, were un-American. The only immigration that should be admitted, the *New Yorker* insisted, was Nordic or Anglo-Saxon immigration. “Our institutions are Anglo-Saxon and can only be maintained by Anglo-Saxons and by other Nordic peoples in sympathy with our culture.”⁶⁴ The new immigrants, for Grant, were fundamentally disruptive and detrimental to this culture, and should therefore be excluded.

Boas’s response the following month was unyielding in its hostility to Grant’s understanding of race and his classic racialist science. Again lumping Grant with other European white supremacists like Gobineau, Chamberlain, Vacher de Lapouge and Hans Gunther, men who would ultimately provide Nazis with scientific legitimacy for the Holocaust, Boas declared this group responsible for “the growth in intensity of feeling and the increasing lack of scientific judgment” that had accompanied the increasing racial antagonisms over the previous decades. These men spoke not in objective, scientific ways aimed at advancing knowledge, but instead were “carried away by the ardent wish to establish the superiority of the ‘Nordic.’” Boas repeatedly argued that there were no pure racial types, and that one simple physical gaze at a person was not sufficient to determine their racial background. There was no exact understanding of the genetic or anthropological aspects of racial identity, and to presume so, and

⁶⁴ Madison Grant, “America for the Americans” *The Forum* v. 74, n. 3 (September 1925), 346-55.
Chapter 6

to insist that coincidence or correlation was evidence of a scientific fact, was intellectually dishonest and scientifically wrong.⁶⁵

By connecting Grant's work to "the increasing lack of scientific judgment," Boas was publicly stating that the scientific validity of claims made by Grant was dubious, and that Grant's assertions were not to be believed. By also linking Grant to noted white supremacists, Boas raised questions as to Grant's prejudice and bias and his ability to be objective, a key requisite for the accurate evaluation of scientific claims. When immigration restrictionists and nativists used scientific knowledge to justify their positions, scientists were willing to confront them publicly and defend scientific truths.

Unfortunately, if there was, as Wiggam described, "no science of propaganda," there was also no science of combating it. As popularizers and amateurs spoke of scientific truths with increasing authority in the 1920s in service of political agendas and racial prejudice, the inability of professional academics and scientists to counter these erroneous assertions left the public under the impression that like did indeed produce like. The difficulty resided in both the complexity of genetics, heredity, biology and embryology, as well as in the cautiousness of researchers to declare exactly what these new fields proved. A consensus among scientists was gradually coalescing around Morgan's chromosome theory of heredity, which historian Stephen Brush suggests was near universal among genetic researchers in 1925. But the compelling scientific

⁶⁵ Franz Boas, "This Nordic Nonsense" *The Forum* v. 74, n. 4 (October 1925) 502-11.

explanations that supported the theory were very complex.⁶⁶ In 1922, Pearl wrote in “Trends in Modern Biology” that “ontogeny does not repeat phylogeny with anything like the degree of fidelity which would be required if it were to be the means of unraveling the tangled thread of evolutionary progress.” So the goal of eugenics was simply not possible, despite the certitude with which writers like Wiggam, Grant and others insisted that it was, and that like would produce like.⁶⁷ But demonstrating or predicting how offspring *would* develop was impossible, nor was it easy to explain in easily understandable terms, the complex genetic concepts of inheritance and chromosomes. Lacking an easy language to demonstrate scientific truths of genetics and evolution, or the unlikely hereditary origin for intelligence, or the basis of anthropological races, the public’s understanding of an organism’s development, whether shaped by their religious beliefs or their racial or cultural anxieties, remained driven by the work of propagandists, charlatans and popularizers.

⁶⁶ Brush, “How Theories became Knowledge.” The main compelling explanations for acceptance of Morgan’s theory were sex-linked inheritance, a testable, tangible mechanism, the pairs of chromosomes and their linkage groups, non-disjunction during early cellular division, the predictive capability for crossing-over frequencies, and the ability to map individual genes. 516-18.

⁶⁷ Raymond Pearl, “Trends in Modern Biology” *Science* n.s. v. 56, n. 1456 (24 November 1922), 581-92; 586.

Chapter 7: The Battle for National Origins

In a speech delivered to the annual convention of the Daughters of the American Revolution in April 1924, after the House of Representatives passed H.R. 7995, Representative Albert Johnson of Washington explained his attitude toward immigration. “We are all the children of immigrants,” he lectured to the assembled ladies. He explained that the new immigration bill that Congress had just passed “makes it clear that it makes no difference whether our ancestors came on the *Mayflower* or whether they missed the first boat.” Now that significant restriction had been achieved, the Washington state Republican told the D.A.R. audience that the next step was a process of Americanization to ensure that the few aliens arriving in the country understood and adopted the Anglo-Saxon values of the United States. Playing to his audience’s sensibilities, he described the 1924 Immigration Act as “America’s second Declaration of Independence.”¹ And just as the original Declaration of Independence inaugurated a protracted struggle, so did the implementation of the “National-Origins” provision of the 1924 act.

The struggle over national origins was not, like the first Declaration, a statement of political separation from Great Britain. On the contrary, it was, in the minds of its supporters, a declaration that the United States would remain an Anglo-Saxon nation. It declared independence from a belief that all men were created equal. The 1924 Immigration Act stated, in Charles Davenport’s words

¹ Pamphlet of Albert Johnson’s speech to the Daughters of the American Revolution’s annual congress, 18 April 1924, box 2, John B. Trevor Papers, Bentley Historical Library, University of Michigan, Ann Arbor, MI [hereafter Trevor papers]. It also appears in the *Congressional Record*, House of Representatives, 18 April 1924, 6687, after the House passed the first version of H. R. 7995.

from a decade earlier, that men were born and remained unequal in their powers, and the inferior European races, those of Alpine and Mediterranean stock, would be excluded from entering.² Yet the debate that raged in the five years before national origins went into effect threw into dramatic relief the basis of the racial nativism that provided key intellectual support to immigration restriction. By 1925, the professional scientific community had exposed the complex of classic racist beliefs that underpinned racial nativism to be wrong. There was no basis in genetics, embryology, or biology for the claims that classic racials – whether they were eugenicists, restrictionists or politicians – made. How, then, were the proponents of national origins able to win the battle for its implementation, when the scientific assumptions at the base of it were discredited? In large part, the implementation of national origins represented a failure of the scientific community to overcome or address the uncertainties inherent in their researches. The “new biology,” with its complex experimental methods and cautious declarations of biological and genetic knowledge, was unable to counter the repeated, forceful, and compelling arguments that like produced like. In the final analysis, the simplicity of classic racist arguments of immutable biological and anthropological racial types, combined with the powerful and well-connected advocates that flattened out and elided scientific complexities, forestalled a widespread understanding of the revolution in genetic and biological sciences.

To understand fully the nature of the clash over national origins, it is necessary to examine its intent. Restrictionists had long been attuned to

² Davenport makes the statement in *Heredity in Relation to Eugenics*, iv.

monitoring their rhetoric and arguments to mask the social, class and racial prejudice at the core of their long campaign to restrict what they believed was inferior immigration to the United States. Their essential goal was always to ensure that undesirable immigrants were prevented from entering the country, although the basis of evaluating them as “undesirable” was fluid and changing. Initially Asians were excluded because they were non-white; quickly after, the inability of immigrants to read and assimilate became the rationale for excluding certain groups, but this proved to be, in historian John Higham’s words, “a fairly coarse sieve.”³ In 1921 and 1924, “racial” identity became the focus for evaluating desirability, eventually to be supplanted by determining the racial composition of the entire American citizenry, and apportioning quotas according to that breakdown. But this later method was highly complicated, of questionable feasibility, and technically and scientifically untenable. Yet, American restrictionists were not deterred, and continued to insist that it was in fact simple, feasible, and scientifically accurate. One of the men instrumental in demonstrating this practicality was Captain John B. Trevor.

On the last day of 1923, when Congress was in recess, Trevor wrote to his old acquaintance, Representative Albert Johnson, about the pending immigration bill’s quota basis. Trevor praised the bill’s “great merit” in the racial selection of immigration by using the 1890 census as opposed to the 1920 enumeration, which would have allowed the entry of a larger number of immigrants from southeastern

³ Higham, *Strangers in the Land*, 308. Higham also writes “Despite the [House] committee’s actual determination to discriminate against southeastern Europe, it could not admit to any but equitable intentions.” 319.

Europe. “It this way it secures, in our European Immigration,” he wrote, “the racial selection which is necessary to preserve the American people from submersion through excessive additions of widely divergent races.”⁴ Trevor was intrigued by a different element of the bill. Section 11 of Senator Reed’s bill contained a very promising method that Trevor regarded as far superior to a quota based on any census. He quickly became one of its most ardent civilian champions. The premise behind national origins, this mysterious provision, was simple: the descendants of the “original” Americans should be given primary consideration in any immigration quota, since they were the real Americans by definition. Their racial stock and composition should be preserved above other alien interests. The census manuscripts from 1790, the first enumeration of the American population, would provide a precise and exact picture of the racial basis of the original American population. By calculating the nativity of these Americans one could determine how many of the current American population derived from that original racial stock, and national quotas for immigration could then be ascertained based on the proportionate contribution to the “American stock” that each European country had made at the origin of the United States.

Statistical data and analysis were essential to this determination. Trevor analyzed the feasibility for calculating national origins in the spring of 1924, and he submitted a report to the House Committee on Immigration and Naturalization titled, “Preliminary Study of Population.” During the Congressional debate, some members had alleged that the shift from 1910 to the 1890 census in the 1924 bill

⁴ John B. Trevor to Albert Johnson, 31 December 1923, box 2, Trevor papers.

was intentionally done to discriminate against immigrants from southern and eastern Europe, a charge that Trevor tried to deflect. He explained, “the late [immigrant] arrivals are in all fairness not entitled to special privilege over those who have arrived at an earlier date and thereby contributed more to the advancement of the Nation...” Trevor’s and Senator David Reed’s plan argued that the most equitable solution was to examine the racial composition of the original population of the United States, which, although it would only “approximate” the truth, would be of “infinite value in demonstrating the falsity of the charges [of discrimination] made by those whose interest and sympathies lie abroad rather than in the country of their adoption.”⁵ The implication in Trevor’s opening statement was clear: only a true American would see how beneficial the national origins provision was; non-native “Americans,” whose loyalty to their adopted country he felt was suspect, would complain about the injustices the new quota would inflict on the countries of their birth. Trevor favored national origins because it was more scientific. It was based on statistical analysis of data from the original enumeration of the American population, rather than an arbitrarily chosen percentage of an arbitrarily selected census. It utilized

⁵ John B. Trevor, “Preliminary Study of Population,” Appendix A, House Report no. 350, “Restriction of Immigration,” 68th Congress, 1st Session, 24 March 1924, 26-7. The statistical tables that Trevor published in “Preliminary Study of Population” demonstrated this effect. 91% of the population in the 1790 census was derived from Great Britain and Ireland, 5.64% from Germany. These two races combined to produce nearly 70 million total offspring resident in the United States based on the census of 1920. Italy, by contrast, had contributed only 3.6 million persons to the American population based on the 1920 census. But the quotas allotted to the three countries based on the 1921 Immigration Act (3% of 1910) were, respectively, 77,342; 67,607; 42,057 (p. 30). Appendix B of the same document listed the number of immigrants to the United States and emigrants from the United States by “Race or people,” and maintained the distinction between “Italian (north)” and “Italian (south).” 36. The “credit” for who thought of the provision is unclear. See Ngai, *Impossible Subjects*, 22.

anthropological and biological categories of race to calibrate the content of the “American” people rather than a haphazard numerical quota that had no genuinely defensible explanation. National origins was precise and objective, and it struck Trevor and others as far superior to the seemingly random alternatives Congress was considering. This was exactly the point opponents of restriction were making – restrictionists were selecting quota percentages and censuses intentionally to cut down on what they deemed to be “undesirable” immigration, rather than on any sound scientific or practical basis. Trevor’s and Reed’s plan circumvented these arguments altogether. If Trevor could prove that this scientific method could be calculated accurately, anti-restrictionists would have no viable or legitimate basis for opposing it.

Unfortunately, demonstrating how national origins could be determined objectively and precisely was a major challenge. Calculating precisely what the national origins of the American population were was difficult because there was no actual data to calculate this in the original census. Instead of being able to use the 1790 manuscript returns, Trevor’s primary sources of data for his study were a volume the Census Department had published in 1909 titled *A Century of Population Growth*, and the returns of the 1920 census. Trevor tried to deduce from these two sources how many current residents of the United States derived from the original population in 1790. Through some complex – and specious – computations, he calculated that just over 47 million white native-born Americans descended from the 1790 population. Northern and western European nations contributed 75.4% of the population of the United States as it had developed since

its founding. This meant, in Trevor's mind, that any quota that did not maintain this proportion in the volume of immigration admitted to the country would have the effect of changing the fundamental racial character and composition of the United States.⁶

These calculations enabled Trevor to determine the contribution of the several European races to the American population since its founding. His task, however, was not to determine what the actual National Origins quotas would be; that highly contentious analysis would not be complete until 1929 and was assigned to a special Quota Board. Rather, he wanted to statistically demonstrate that, if there was any trace of discrimination in the numerical quotas from 1910 or 1890, it in fact discriminated against the native-born population. The historian Mae Ngai explains Trevor's intent succinctly: "These quotas [from 1910 or 1890] were based on the number of foreign born in the population, leaving 'native stock' Americans out of the equation." Trevor argued that the entire American population should be factored into devising immigrant quotas.⁷ To base quotas on the proportion of immigrants resident in the United States (regardless of which census was chosen as the basis) put the descendants of "real" Americans at a gross disadvantage, for it disregarded their existence in the population of the United States.

⁶ Northern and western Europe contributed 75.4%, southern and eastern Europe 13.2%, Africans and Indians and all others contributed 11.4%. John B. Trevor, "An Analysis of the American Immigration Act of 1924" *International Conciliation* no. 202 (September 1924), 375-450, 387-88. Population derived from 1790 on 424-25; proportion of population from 388-89. *International Conciliation* was published by the Carnegie Endowment for International Peace. See also the computations in Bureau of the Census, *A Century of Population Growth*.

⁷ Ngai, *Impossible Subjects*, 22.

This is not to suggest that nativists stopped documenting the danger of the “new” immigration. Before Congress reconvened from its winter break in early 1924, Trevor told Johnson of a meeting he had recently had with Lothrop Stoddard that had impressed on Trevor the importance of tightening restrictions against the new immigration. Trevor related that Stoddard had told him of Syrians, Greeks and “one hundred million [other] people” who were eager to flee Europe and the Middle East if the United States relaxed its restrictions on immigration. Although Stoddard painted a vivid picture of “their filth and disease,” Trevor recommended that Johnson should prepare Stoddard before he testified before the Committee on Immigration and Naturalization to keep Stoddard from using “specific evidence in support of his general view of the situation.”⁸ Although Trevor did not say why it would be necessary to keep Stoddard from using “specific evidence,” it is possible that he recognized that Stoddard’s classic racialism may not have the sway of the presumably better, more “scientific” justification that national origins provided. Stoddard also had no professional scientific training from which he could testify to the racial qualities and inferiority of these impending immigrants that might also raise objections from opponents of restriction.⁹

⁸ Trevor to Johnson, 2 January 1924, box 2, Trevor Papers.

⁹ Stoddard recognized that his lack of training as a biologist or scientist may undercut his credentials as an advocate for restriction (Lothrop Stoddard to Charles B. Davenport, 17 May 1922, Davenport Papers), although this did not stop him from insisting that “Whether it be figs, thistles or humans, like always tends to produce like.” Lothrop Stoddard, “Worthwhile Americans” *Saturday Evening Post* v. 197, n. 29 (17 January 1924), 23, 146, 149-50; 23. Stoddard is briefly discussed in Barbara Solomon, *Ancestors and Immigrants*; John Higham, *Strangers in the Land*; and Matthew Pratt Guterl, *The Color of Race in America: 1900-1940* (Cambridge:

For Trevor, the best “specific evidence” was, of course, statistical and demographic data. Walker had used them to great effect to really begin the restrictionist impulse and illustrate the dangers of unrestricted immigration. It provided a sense of objectivity, since numbers exactly represented a specific thing; one million foreign-born immigrants represented one million non-Anglo-Saxons living in the United States. In February 1924, Trevor demonstrated to Johnson the intrinsic inferiority and undesirability of Italian immigrants by using a report released by the New York State Commission of Prisons, which reported that 80% of inmates in New York prisons were foreign-born, and of those 45% were Italian. Completely disregarding Jennings’s critique of Laughlin’s studies on criminality and inferiority for the House Committee, Trevor asserted that the evidence from the report confirmed Laughlin’s study, and could also “be an answer for the exponents of the Italian viewpoint in Congress—Mr. LaGuardia in particular.”¹⁰ Trevor also rehearsed his own testimony for Johnson’s committee with James Horace Patten, the Immigration Restriction League’s lobbyist. “In regard to the attitude I had better take as a witness,” Trevor told Johnson, he had decided that he would make “a cold presentation of the material,” relying heavily on statistical analysis to demonstrate the necessity of further restriction.¹¹ Sticking to statistical data would, the Army captain hoped, prevent any discussion of discrimination.

Harvard University Press, 2001); see also Gene C. Fant, “Stoddard, Lothrop” *American National Biography*.

¹⁰ Trevor to Johnson, 14 February 1924, box 2, Trevor Papers.

¹¹ Trevor to Johnson, 11 March 1924, *ibid*.

The impact of the 1924 bill on Japanese immigration was another complicating issue for restrictionists. The Immigration Act of 1924 breached the “Gentlemen’s Agreement” between President Theodore Roosevelt and Japan because the bill explicitly excluded any immigrant who was ineligible for American citizenship, which the Chinese and Japanese were. Some groups, like the National Committee for Constructive Immigration Legislation, supported a more flexible immigration policy towards Japan that might not universally exclude them from citizenship.¹² Sidney Gulick, as Secretary of this organization, often discussed Asian immigration with Immigration Restriction League members, hoping to encourage them to change their opinions on Japanese restriction. They did not. League members wanted a racially pure, Anglo-Saxon country.¹³ As Prescott Hall had told Senator Lodge when the first quota laws were being considered in the late 1910s, “I think we shall decide to stand with Australia and Canada for a white man’s country, and turn down Gulick’s request for cooperation, and stand for our own bill....If we are to have a war with Japan, as I

¹² See Ngai, *Impossible Subjects*, especially chapter 1.

¹³ National Committee for Constructive Immigration Legislation, “Our Immigration and Naturalization Laws” (n.d.) pamphlet in box 3, Lee papers; Charles Davenport to Sidney Gulick, 30 October 1917; Gulick to Davenport, 10 July 1918; Davenport to Gulick, 15 July 1918; Davenport to Gulick, 7 July 1919; Davenport to Gulick, 2 February 1920; Davenport to Gulick, 9 February 1920; Davenport to Gulick, 21 June 1920, all in Davenport papers. In his note of 2 February, Davenport explicitly laid out his attitude toward Asian immigration: “I am [not] in favor of the limitation of immigration on the grounds of percentage[,] instead of the sole and only ground of racial and family suitability for citizenship.” Gulick’s letter to Davenport of 10 July 1918 was on the letterhead of the League for Constructive Immigration Legislation – precursor to the National Committee for Constructive Immigration Legislation [NCCIL] – and included among its members Boas’s old contact Jeremiah Jenks, George Kennan, and W. W. Husband. For IRL contacts with Gulick and the NCCIL, see Prescott Hall to Joseph Lee, 11 March 1917, box 1, Lee papers; E. A. Ross to Sidney Gulick, 5 December 1918, box 2, IRL papers; E. A. Ross to Prescott Hall, 6 May 1917; Ross to Hall, 5 December 1918, both box 3, IRL papers.

believe, there's no use sacrificing a principle for a temporary soothing of the feelings. What [Japan] wants is free [sic] immigration."¹⁴ Despite the diplomatic difficulties this aspect of the bill raised, it was less important than ensuring that the *white* American population conformed to the racial proportions at its founding.

Maintaining his focus on the task of rhetorically shifting the perceived discrimination in immigration legislation to the native-born American population, in August 1925 Trevor submitted a letter to the editor of *The New York Times*. Trevor denied accusations of discrimination against southern and eastern European immigrants, specifically Italians and Jews, whose “partisans...shout from the housetops that restriction of immigration is tantamount to prejudice against nations and their religious beliefs.” In fact, he explained, “the law is based upon broad general principles with justice to all. It was conceived for the protection of our institutions and the development of American solidarity.” By this, Trevor meant the racial homogeneity of Anglo-Saxon America. He noted that because national origins took into account the contribution of every national population group in Europe, no one was left out. Had tens of thousands of Piedmontese, Florentines, Venetians and Genoese migrated to North America during the colonial period, Trevor pointed out, their representation in the national origins quotas would have been much higher. But they did not. Instead, mostly Britons made the journey across the Atlantic to North America. And, Trevor felt,

¹⁴ Hall to Senator Lodge, May 15, 1919, box 2, IRL papers.

the contributions of this population group to the development of American politics, the economy, and culture should be recognized and protected.¹⁵ Anglo-Saxons had founded the North American settlements, carved out an existence in the wilderness, and defended themselves from Native Americans; Anglo-Saxons of British descent broke from their homeland and declared independence. There were no Romans or Neapolitans who contributed to these mighty struggles, a fact that Trevor claimed could be proven by using the 1790 census to analyze the surnames and determine the national – and thus racial – stock of the nation.

Although Trevor did cooperate with other sympathetic organizations in the five years between Congressional passage of the national origins provision and its implementation, most of his energies were devoted to defending national origins from critics who questioned its assumptions and practicality and tried to repeal the provision.¹⁶ In April 1926 Trevor drafted “A Restudy of the National Origins Plan,” to ensure a “just and accurate appreciation” of the clauses in the 1924 Act “in the light of experience and mature consideration.” The dangers of unrestricted immigration had stalked the American population for decades, he wrote, and “America’s second Declaration of Independence,” a phrase borrowed from Johnson’s D.A.R. speech, established a precise and equitable solution to the immigration problem. Trevor also had to discredit the several other alternatives to

¹⁵ John B. Trevor, “Immigration Quotas” *The New York Times* 14 August 1925, 12.

¹⁶ Trevor had a strained relationship with the Boston-based Immigration Restriction League because of Patten’s opposition to national origins.

computing national origins that had been proposed in the intervening two years before the superiority of the national origins plan could be demonstrated.¹⁷

In these intervening years, Trevor did not dwell on the “racial” aspect of the quotas or national origins plan, but emphasized the inability of new immigrants to participate in a democratic form of government, thus highlighting their undesirability in a more tactful manner. The fundamental shortcoming of the 1924 immigration law – which established a quota based on 2% of the foreign-born in the 1890 census – was that it failed to account for the inability of certain groups of immigrants to “conform to the basic composition of the population as a whole, either in race or political ideals....” The “Preliminary Study” he wrote for Johnson’s Immigration Committee had demonstrated just what that “basic composition” of the population was. The new immigrants that came from countries of Europe that had little experience in self-government were unable to appreciate the responsibilities of a democracy. Thus, large numbers of these immigrants would present a serious threat to the proper functioning of American government. Furthermore, the tendency of immigrants to “colonize and intermarry” within their own racial group meant that even their children, born on the free soil of the United States, absorbed the traditions and ideals of their parents, and not of their country of birth.¹⁸ Any quota based on a census after 1880 would have the effect of favoring races not accustomed to democracy, and

¹⁷ John B. Trevor, “A Restudy of the National Origins Plan for the Selection of Immigrants and Alternative Methods Recently Suggested,” 19 April 1926, box 2, Trevor Papers.

¹⁸ *Ibid.*, 2.

any quota based on the average of immigrants arriving between 1890 and 1920 – which had also been suggested as a method of computation – would have the same effect. Consideration needed to be given, Trevor felt, to “the vast majority of the American people to whom not only the restrictive features [of the 1924 bill], but the inherent justice of the national origins plan in reality appeals.”¹⁹ And as he had already shown, the vast majority of the American people descended from four million people of Anglo-Saxon stock.

Trevor and Johnson maintained a very close friendship throughout the struggle to implement the national origins quotas that enabled them to coordinate strategies to ensure that restrictive immigration policies were enacted.²⁰ In October 1926, when a representative from the Pennsylvania League of Women Voters asked Johnson to recommend “some college professor who is an authority on the biological side of immigration” to address their Annual Convention in Philadelphia, the congressman turned to Trevor for suggestions. Johnson’s preference was for “the prof[essor] at Princeton who studied the intelligence tests [Robert M. Yerkes],” but Trevor’s inclination was to endorse Henry Pratt Fairchild, from Yale University, who had just published *The Melting Pot Mistake*.

¹⁹ *Ibid.*, 6.

²⁰ When Johnson ran for reelection in 1928, Trevor was remarkably active in helping Johnson win a bruising Republican primary against attorney Homer Bone. He also personally vouched for Johnson’s creditworthiness to the Hotel Breslin in Manhattan. Correspondence regarding Johnson’s primary campaign can be found in box 2, Trevor papers, and Trevor’s 30 October reply to the Hotel Breslin Credit Department’s letter of 29 October. Johnson had requested the establishment of credit at the hotel to facilitate his cashing personal checks, and the hotel asked the New Yorker if he had any information relevant to Johnson’s responsibility. Trevor called Johnson “an intimate friend of long standing.” Trevor to Johnson, 1 November 1928; Credit Department of the Hotel Breslin to John B. Trevor, 29 October 1928; Trevor to Hotel Breslin Credit Department to Trevor, 30 October 1928, all in Trevor Papers.

Fairchild's book argued that immigration must be restricted because the alien races did not assimilate over time, but maintained their "exclusively hereditary" attributes in the "germ plasm," which he insisted contained the basic elements of development that "are never changed." Fairchild did make a curious admission about Italian immigration, however, that recognized the long-standing division of the peninsula into two separate groups, one of which was desirable, and one of which was not: "If the Italian immigration to the United States had come from northern Italy its racial effect on the American population would have been very slight."²¹ Trevor replied directly to the Women Voters' representative, and recommended they select Harry Laughlin to address their annual meeting in Pennsylvania, but he gave the organization's chairwoman less than three weeks to secure Laughlin's participation. But while Laughlin was suitable to appear before the assembly of Pennsylvania women, Trevor worried about the prominent role the eugenicist was playing for Johnson's Immigration Committee because some of Laughlin's work, as Jennings had already demonstrated, was of dubious scientific value. One of the pieces of what Trevor regarded as Laughlin's dubious research was the latter's study of the ancestry of delegates to the Constitutional Convention in 1788. Trevor understood not only political delicacies, but also complexities and nuances in scientific research, and he cautioned Johnson against

²¹ Lucy K. Miller to Albert Johnson, 12 October 1926; Trevor to Johnson, 1 November 1928, Trevor papers. Henry Pratt Fairchild, *The Melting Pot Mistake* (Boston: Little, Brown, and Co., 1926). Hereditary from 27; germ plasm from 113; Italian immigration from 109.

using data that Laughlin had given Johnson, which had been called into question when Trevor's own associates had done further research.²²

John Trevor was not the only academic to weigh in with support for the new methods of restriction. Roy Garis, a Political Science and Economics professor at Vanderbilt University, also commented on potential "scientific" methods of restriction. In 1922 Garis had stated that the emergency quota act of 1921 was a proper response to the "unfortunates of Europe" who had survived the war but hoped to escape Europe for the United States. This perceived rush of people for the United States created the necessity of quick restrictive action, but had left "little or no time for an intelligent, historical, and scientific study of the [immigration] question." The Tennessean hewed to the orthodoxy that the physical and cultural differences of the "new" immigrants made them a menace to American institutions, and he encouraged making the temporary law permanent, and basing it on the 1890 census rather than 1910 or 1920.²³

Garis preferred the old, familiar quota policy to national origins. After passage of the 1924 Immigration Act, Garis published two articles in popular magazines further supporting the new policy of dramatically limiting restriction, and the use of the 1890 census as a base for computing the quotas. In August,

²² Trevor to Mrs. John O. [Lucy K.] Miller, 26 October 1926, box 2, Trevor papers. Trevor informed Mrs. Miller that Laughlin had "a comprehensive knowledge on the subject from a biological standpoint and has made extensive researches in Europe on the immigration question also from a practical standpoint." Trevor's recommendation of Laughlin may have been disappointing to the Lucy Miller, since he had no professional scientific training on the "biological side of immigration," nor any academic appointment; but he was the "Expert Eugenics Agent" of the House Committee on Immigration and Naturalization. Trevor's concerns of Laughlin's work from Trevor to Johnson 1 November 1928, box 2, Trevor papers.

²³ Roy L. Garis, "The Immigration Problem: A Practical American Solution" *Scribner's Magazine* v. 72 (19 September 1922), 364-67; 364.

Garis praised the “scientific manner” in which the new bill operated, and like Trevor dismissed the criticisms of the 1924 bill as intentionally discriminatory. The “*new* immigration,” which the bill was designed to curtail, constituted “*the* immigration problem of to-day.” Unable to easily assimilate the alien arrivals from southern and eastern Europe, in selecting the 1890 census Garis argued that Congress solved the immigration problem because it excluded significant numbers of these “new” immigrants. He replicated Trevor’s argument that if any discrimination in fact existed, it was against the original American stock, but Garis flatly dismissed all claims of discrimination: “Immigration is a domestic question to be decided in the interests of the American people and not in the interests of any other people or nation.” Charges of discrimination, in Garis’s mind, were irrelevant. “If we decided to be so arbitrary,” he wrote, “we would be within our rights to decide that no immigrant should be admitted unless he was six feet two inches tall and had red hair.” Yet Garis concluded his analysis of the 1924 act by sharply criticizing the national origins clause of the bill as “a mere estimation.”²⁴ He did not feel that it could be reasonably calculated with any precision.

Garis also wrote about the importance of the “selective” aspect of the 1924 law by arguing that restriction and selection of immigration had a long history. He described the attitudes of colonial leaders who had protested the dumping of

²⁴ Roy L. Garis, “How the New Immigration Law Works” *Scribner’s Magazine* v. 76 (August 1924), 183-88; 185 for “*new*” immigration [emphasis in original], 186 for immigration interests. The other article was Roy L. Garis, “America’s Immigration Policy” *North American Review* v. 220, n. 824 (September 1924), 63-77; “a mere estimation” from “How the New Immigration Law Works,” 187.

“human chaff” on the shores of the fledgling nation, although Garis had no doubt that “the worst felons were promptly hanged.” But environmental influences tripped Garis up: he suggested that the lower stratum of immigrants who arrived in colonial North America, once under “the regenerative stimulus of opportunity...[were] reformed, and...rose to places of honor and distinction.” He lifted this exact phrase from Commons’s book *The Old World in the New*, and like Commons, did not question why the same process could not occur for the current class of immigrants. One may assume, however, that the old immigrants excelled in the salubrious environs of the United States because of their superior racial ancestry, “Anglo-Saxon-Germanic in blood and Protestant in religion,” while the new immigrants, as Garis, Fairchild, Commons and many others had pointed out, were racially inferior and predominantly Jewish or Catholic.²⁵

His comments reflect an underlying tension among nativist and restrictionist groups regarding quotas. Statistics broke down, neatly and concisely, demographic information of the American population. The “national origins” of the American population was, by contrast, exceedingly difficult to compute. Garis told his readers that national origins “at best is a mere estimation,” and was faulty in its “lack of definiteness.” The census returns from 1890, which the current law was based on, were “scientific and automatically selective as well as numerically

²⁵ Garis, “America’s Immigration Policy,” 67 for hanging felons; 73 for regenerative stimulus; 74 for Catholicism. Commons’s division of Europe into northwestern and southeastern halves – the former racially, culturally, and intellectually superior to the latter – was designed to separate Protestant Europe and Catholic Europe—Scandinavia, Britain, Germany and France from Russia, Austria-Hungary, Italy and Turkey. France, in the extreme west of such a geographical binary, was of course Catholic. See Commons, *Races and Immigrants*, chapters 2, 4.

restrictive.”²⁶ This method, by definition was superior. It was criticisms like these, along with Congressional members who tried to repeal national origins – rationalized largely on similar claims that Garis was making – that Trevor worked to defuse.

This lack of precision in the national origins model of restriction troubled other scientists and academics. A memorial addressed to President Coolidge and members of Congress in January 1927 encouraged the permanent adoption of the 1890 quota as the method of restriction instead of national origins. Seven faculty members of Princeton’s biology department signed the memorial, as well as League member Robert DeCourcy Ward (a professor of climatology at Harvard) and two other Harvard professors, four professors from the University of Wisconsin (including Commons), Joseph Lee, League Treasurer Richards M. Bradley, E. A. Ross, Madison Grant, Henry Fairfield Osborn, Robert Yerkes, Ellsworth Huntington, Irving Fisher and Henry Pratt Fairchild, the President of the University of Michigan (Clarence Cook Little), Charles Davenport and Harry Laughlin, and the former governor of the Commonwealth of Massachusetts, Eugene N. Foss.²⁷ Advocates of restriction worried, as Garis pointed out, that the methodological complexity – opponents would describe it as impossibility – of national origins made it an inferior method to that based on the census. It also shows a significant divergence among restrictionist organizations over how best to achieve their goals.

²⁶ Garis, “How the New Immigration Law Works,” 187.

²⁷ Memorial to the President, the Senate, and the House of Representatives, 4 January 1927, box 2, Lee papers.

The Immigration Restriction League's Congressional lobbyist in fact opposed national origins. Patten recognized the widespread opposition from some Congressmen whose constituents were driving a movement to repeal national origins. The controversial provision had the effect of unifying opposition from *all* immigrant communities in America. The quota acts were directed specifically against immigrant groups from southern and eastern Europe, but did not really disturb the volume of immigration from Germany, Ireland or Scandinavia. National origins, as it computed an almost exclusively British or Anglo-Saxon quota, threatened some of these northern countries' quotas as well. Patten suggested that the IRL support the repeal of national origins in order to at least protect the 1890 quota of 2%. This, Patten felt, embodied the intent and goal of the League's members, and he feared that supporting national origins could threaten the whole restrictionist structure they had been carefully building over the previous decades. The National Immigration Restriction Conference, an organization based in New York and headed by John Trevor, was in the process of unifying restrictionist organizations to push hard for national origins. Trevor found Patten's opposition to the provision unacceptable. On May 30, 1928, the Immigration Restriction League had a meeting at the Union Club in Boston, where the League's leaders crystallized the following agenda: to continue to support immigration restriction and add the exclusion of Mexican immigrants, ensure adequate government appropriations for regulatory agencies, a quota reduction to 1%, improved legal machinery to facilitate alien deportation, and a tentative endorsement of National Origins. The conference also yielded "the

unanimous opinion that it is not better to fight for National Origins in those states or districts in which to do so would injure or alienate friends of restriction in the House or Senate.”²⁸ The spurious and dubious calculations that national origins required made it unlikely to be implemented and the IRL’s leadership tried not to overreach and imperil restriction by supporting what they regarded as an unworkable scheme.

The method of restricting immigration to the United States by national origins was so doubtful, so questionable and so impractical, that it split the fragile coalition of groups and individuals in favor of restriction. The members of the IRL, who had founded their organization in 1894 to achieve restriction, found themselves unable to support the new method. Patten actively opposed it, and his position in Washington D.C. was influential. The League’s leaders found the principle unworkable, but Trevor was not going to back down. His organization continued to fight for the National Origins provision, and made attempts to undermine Patten’s credibility with his Congressional contacts, while it simultaneously made overtures to some of the patriotic societies that Patten represented as a lobbyist, suggesting that he was not trying as hard as he could to preserve America’s racial integrity. Patten’s sources of funding somehow became an issue, and he nearly dragged the Immigration Restriction League into a Congressional inquiry on lobbying. When Lee asked Ward in October 1929 what was going on with Patten, Ward was blunt: “I have lost all my use for Patten, and

²⁸ The principles outlined in this paragraph are from an unsigned sheet titled “Conference With Patten”, dated May 31, 1928, box 2, Lee papers and an unsigned typewritten document (on the back of “Community Service” letterhead) dated December 1, 1930, box 2, Lee papers.

most of my interest in him....[He] is doing us very little good.” Ward described how Trevor’s organization, which operated as a federation of restrictionist groups, rather than *one* group as the Immigration Restriction League did, had become “‘it’ almost more than we are in Washington. [And] Patten refuses to work with them.” Lee and Ward were both concerned about how this was affecting the goal of restriction and preserving the movement’s successes, and Ward felt that if the divisions between the restrictionist organizations persisted, “we might as well throw up the sponge.”²⁹ The League had no intention of giving up even if the national origins provision went into effect. They continued to fight for enforcement and against repeal of restriction into the 1930s. But that important members of the League’s leadership had serious reservations about the feasibility of national origins suggests how unworkable the scheme seemed to many otherwise ardent champions of immigration restriction.

The Importance of Blood

The drama and tension over the implementation of national origins was not limited to groups and organizations that supported restriction. The most important arena for conflict was the United States Congress, which had the ultimate decision of which method of restriction would be implemented. In the

²⁹ Marion Snow [Lee’s private secretary; later his second wife] memo to Lee, May 13, 1930; Lee to Ward, October 4, 1929; Ward to Lee, October 5, 1929, all box 2, Lee papers. The situation was resolved by the League refusing Patten’s resignation in January 1930, and opting to give Patten very explicit instructions to cooperate and “play nice” with the restrictionist organizations [February 4, 1930], and Ward resigning [September 15, 1930] to work with the Committee on Selective Immigration of the American Eugenics Society. See: Patten to Ward, January 25, 1930; Ward, “Votes Passed by the Exec. Com. I.R.L., Jan. 6, 1930,” February 4, 1930; Lee to Patten, May 7, 1930; Ward to the Members of the Executive Committee, September 15, 1930, all box 2, Lee papers.

first session of the 69th Congress in 1926, New York Representative Samuel Dickstein spoke out against the national origins provision of the 1924 law during a debate over its enactment. His comments on the floor of the House in late June 1926 not only reflected the tension between restrictionists and anti-restrictionists, but exposed emerging divisions among the Congressional restrictionists and anti-restrictionists, over the most viable and effective method to enact or repeal restriction. Dickstein was clear that he did support a degree of restriction, which he felt would aid the successful assimilation of immigrant groups, but was adamantly opposed to the drastic restrictions of the quotas and national origins. He denounced the initial shift in the quota basis from 1910 to 1890, which he believed was intentionally designed to admit only “more Nordics, more people with blue eyes, blond hair, and long stature.” He furnished statistics on immigration from both census bases that showed that the effect of the shift to the 1890 census was to severely limit the number of southeastern European immigrants who would be eligible to land on American shores. The 3% quota based on the 1910 census had permitted the entry of 42,057 Italians, 77,432 British and Irish, and 67,609 Germans annually. The quota based on the 1890 census for the same immigrant groups was 3,485 Italians, 62,574 British and Irish, and 51,227 Germans (the Irish Free State was separated from the quota of Great Britain/North Ireland in the 1924 bill). Of the total number of immigrants allowed under the 1890 quota, Dickstein pointed out that 86% were reserved for the “so-called Nordics.”³⁰ The New Yorker saw that this policy would exclude many

³⁰ *Congressional Record*, House of Representatives, 29 June 1926, 12280.

people who would be valuable and useful additions to the United States population simply because of their geographical, and thus, racial origin.

Dickstein denounced the principle of national origins as completely arbitrary, a policy supported by paid agents of the Carnegie organization who had secured appointments as unpaid officials of the United States government – a clear reference to Harry H. Laughlin – and a total fiction. The “alleged statistics” to compute national origins, he told Members, “do not exist in fact...” But in this opposition, Dickstein was supported by an unlikely ally: Albert Johnson. When the House’s chair of the Immigration and Naturalization Committee printed an extension of his remarks on national origins in *The Congressional Record*, he began laying the groundwork for retreat on the issue. He explained that the great difficulties of implementing, and deep suspicions of the feasibility and practicality of the policy may make it necessary to repeal it in favor of the 2% quota of the 1890 census. Although Dickstein’s proposed amendment to repeal national origins did not make it out of Johnson’s committee, the bill that had been reported out by the House and Senate conferees had conceded that the computation of national origins might be impossible, in which case clause (e) of Section 11 would remain as the current quota for restriction, which was 2% of the census of 1890.³¹

When the 1924 Immigration Bill was passed, the section pertaining to national origins mandated that the new quotas be declared and implemented by

³¹ *Ibid.*, 12299; see also House Report no. 716, 68th Congress, 1st Session, “Immigration of Aliens into the United States,” 12 May 1924, Conference Report to accompany H. R. 7995, submitted by Johnson.

1927. With the calculations – or even the possibility of successfully performing the calculations – so contested, Congress created extensions of the deadline by passing new bills and resolutions granting more time for the implementation. These extensions, in turn, generated a variety of bills on national origins. Throughout the first session of the 69th Congress, amendments to repeal the national origins provision continually sprang up, although the justifications for repeal varied. House Democrat John Douglass from Boston railed against the abdication of Congressional prerogative that national origins would require, and demanded repeal of the provision on that basis. Douglass pointed out that the establishment of the Quota Board, which was explained in Section 11 of the bill, left the policy of immigration restriction in the hands of government bureaucrats, not in the hands of Congress where it should be. The people’s representatives were renouncing their Constitutional duty, and for that reason alone, Douglass argued, the provision should be repealed. On top of this egregious abdication of legislative prerogative, the representative of the “City Upon a Hill” derided the calculation of national origins as “indefinite, uncertain, and theoretical....” Lest any uncertainty remain as to his position, Douglass declared, “national origins is a huge joke.” Nonetheless, most of the bills introduced in the first session of the 69th Congress were concerned with mild changes to quota and non-quota exemptions under the 1890 census, and of the 55 bills introduced in both chambers relating to immigration, only one – Dickstein’s resolution to amend certain non-quota categories – became law. Two additional bills were reported out of committee and passed one chamber, but no action was taken by the second.

Most of the bills – 22 in the Senate, and 33 in the House – languished in committee.³²

During the second session of the 69th Congress, however, action on national origins took on a sense of urgency. Convening on December 6, members had 88 days before the Congress expired, and as written in the 1924 law, the national origins provision would automatically go into effect on July 1, 1927. Confusion already reigned when the session opened. Three days before the end of the first session, on June 29, 1926, Albert Johnson had discussed his grave doubts about the feasibility of national origins. Two tables entered into the *Congressional Record* gave two separate and different national quotas for Europeans under the origins basis. One table was submitted by the provision's sponsor, Senator Reed, and another the result of Captain Trevor's analysis of the 1920 census. Although most national origins quotas were roughly equivalent, the differences between the two suggested to thoughtful Congressional members the inherent difficulty of computing the quotas. Reed's plan proposed the following quotas: Germany: 22,081, "Great Britain and Ireland": 91,111, Italy: 5,878, Poland: 4,509, Russia: 4,002, and Sweden: 3,707. Trevor's estimates yielded slightly different allowances: Germany: 20,028, Great Britain and North Ireland: 85,135, the Irish

³² *Congressional Record*, 69th Congress, 1st Session, Bill Index. The two other bills were Senator Royal Copeland's resolution to amend a subdivision of section 4, which dealt with non-quota exemptions (S.J.Res. 82) Robert Bacon's bill, H.R. 6238 passed the House, was amended and reported out by the Senate Committee on Immigration, but was not voted on in the full Senate. H.R. 6238 eventually passed by the Senate; it was concerned with providing non-quota entry to American-born women married to aliens who had lost their citizenship under the Cable law.

Free State: 8,330, Italy: 5,716, Poland: 4,535, Russia: 4,002, and Sweden: 3,072.³³

These discrepancies begged the question that, if the national origins method was superior because it was more scientifically and statistically accurate, why was there any divergence at all?

The problems the divergence of these two national origins quotas presented caused the relatively orderly division of pro/anti-restrictionist sentiment in Congress to fragment. Restrictionists split into various camps: those who favored the implementation of national origins without delay on July 1, 1927, some that favored postponing the implementation of national origins to ensure an accurate and compelling analysis of the population, and others who favored repealing the provision because of its dubious statistical accuracy in favor of the much simpler quota basis of 2% of the census of 1890. Pro-immigration forces likewise splintered into several camps: those that demanded the repeal of national origins, those that favored postponing (every year if need be) its implementation, some who favored the 2% quota based on the 1890 census, which would allow a higher number of immigrants to enter, and a group that advocated compromise by using an average of immigration from 1880 to 1920. Also, the gaping hole left by the omission of the western hemisphere from any of the clauses of the 1924

³³ *Congressional Record*, House of Representatives, 29 June 1926, 12300. Reed's table significantly demonstrated that in actuality, the battle over restriction had already been won. Along with listing the quota on the basis of national origins, it also listed the quotas under the two previous regimes, 3% of the 1910 census from 1921, and 2% of 1890 from the 1924 law. Under the 1921 bill, the immigration from the above countries was: Germany: 67,607, Great Britain and Ireland: 77,342, Italy: 42,057, Poland: 30,979, Russia: 24,405, and Sweden: 20,042. The bill annually admitted a total of 357,801 immigrants. For the 2% quota of 1890, the numbers were significantly different – as was intended: Germany: 50,129, Great Britain and Ireland: 62,458, Italy: 3,889, Poland: 8,872, Russia: 1,792, Sweden: 9,561. Under the national origins regime, immigration from Italy and Russia would actually *increase*.

Immigration Act raised concerns for many members of Congress, who decried the unregulated immigration from Mexico and Canada as completely undercutting the basic principle of restriction.³⁴ In the 1930s, the IRL's chief focus would in fact be trying to extend the restrictionist provisions of the law to Mexico and Canada.

Senate Joint Resolution 152, introduced by Senator Hiram Johnson of California after the end of winter recess in early 1927 proposed to postpone for one year the implementation of national origins. The six-member Quota Board, directed under the 1924 law to oversee the computation of the quotas had submitted a preliminary report to the Secretaries of Commerce, State, and Labor in January that was forwarded to Congress and declared that "any racial classification based mainly upon names involves a considerable element of uncertainty...."³⁵ Three days later another message arrived, this one declaring the serious reservations of the three Secretaries, who wished "to state that in our opinion the statistical and historical information available raises grave doubts as to the whole value of these computations as a basis for the purposes intended." They closed memorandum ominously, declaring that "We therefore cannot assume responsibility for such conclusions under these circumstances."³⁶ With the

³⁴ Canada was problematic because of the fear that immigrants could transit through Canada and then enter into the United States outside of close supervision by the American government.

³⁵ Senate Document n. 190, 69th Congress, 2nd Session, "National Origin Provision of the Immigration Act of 1924" 7 January 1927. Since there was no information regarding ancestry or place of birth in the 1790 census, the quota for national origins was to be computed by examining the surnames of residents to derive their European origins, and then calculating, over 130 years later, the geographic (and thus racial) origins of the original population of the United States. This is discussed also in Ngai, *Impossible Subjects* and Ngai, "The Architecture of Race."

³⁶ Senate Document n. 193, 69th Congress, 2nd Session, "National Origin Provision of the Immigration Act of 1924" 10 January 1927. Senator Reed of Pennsylvania would later claim that

inability of the Quota Board to confidently support the national origins quotas, the provision's implementation seemed in serious doubt.

Joseph Hill, a veteran of the U.S. Census Bureau, chaired the Quota Board, but despite his long tenure with the Census, and two reports that he had submitted for the Dillingham Commission, calculating the quotas for national origins was, in Mae Ngai's words, "arguably the most difficult challenge of Hill's career." Congress twice rejected his results before finally approving them. He had at his disposal a very limited amount of data from which to extrapolate the racial composition of the United States at its founding. It had to begin by, Ngai explains, conceptualizing the fundamental categories that comprised the national origins quota system, and it used interchangeably "national origin," "native stock," and "nationality," which did little to resolve confusion.³⁷ But as the "Dictionary of Races or Peoples" showed, as it employed the contributions of the Italian anthropologists, nationality did not necessarily denote "race." This is why the Dillingham Commission distinguished between north Italian and south Italian.³⁸ Several immigrant groups were also arriving in the U.S. in the 1920s who came from nations that did not exist in 1790 – Italy, Germany, Ireland, Poland and Yugoslavia to name just five. The Quota Board designated as "immigrant stock"

the two documents had been tampered with and altered before they were submitted to Congress to highlight the difficulties perceived in national origins.

³⁷ Ngai, *Impossible Subjects*. Hill's background and quota rejections from 25; work for the Dillingham commission from 31; the conceptual formulations from 26.

³⁸ United States Immigration Commission, "Dictionary of Races or Peoples," 81-84. The Commission's dividing line for the two "races" of Italians was preposterously arbitrary, and made Columbus, Dante, Michelangelo and Mazzini, despite Joseph Lee's understanding, southern Italians. The line was effectively the Po River basin, which included only the *compartamenti* of Piedmont, Lombardy, Venetia and Emilia-Romagna. 81.

any person who came into the United States after 1790, and classic racialist ideas infused their entire work.

The Quota Board took national origin as a discrete, quantifiable unit, and as such treated it like a unit character in terms of its heredity. They presumed it followed precise Mendelian inheritance ratios, that the national identity and racial characteristics were fixed, immutable, and unchanging. Ngai explains the Board's assumption that "even if nationalities combined through intermarriage, they did not mix but remained discrete, unalloyed parts in descendants that could be tallied as fractional equivalents." This conception of race by blood quantum, she notes, had its precedent in the "one-drop" rule that had been used to describe blacks in the United States, and created the ridiculous proposition that a person with three English grandparents and one German grandparent was the numerical equivalent of their ancestors: three-quarters of an English person and one-quarter of a German person. This reduced human beings, as Hill elaborated, to "a unit of measure rather than a distinct person."³⁹ Supporters of national origins operated under the assumption that characteristics did not "blend" or average, as Boas, Hrdlicka, and other anthropologists suggested they did. They did not incorporate biological or genetic definitions of race, which looked at mechanisms of heredity that inherited two sets of chromosomes, one from each parent, and suggested that organisms adapted to changes in environmental conditions and changes during the course of their development. In short, they approached national or racial identity

³⁹ Ngai, *Impossible Subjects*, quote on 33; see 25-37, from which this section draws extensively for the work of the Quota Board; Joseph Hill, "The Problem of Determining the National Origin of the American People" quoted in Ngai, *Impossible Subjects*, 33.

in the same way that Galton approached talent and genius or described the stick traveling down the stream: through generations, unbroken and unchanged. The Quota Board's members subscribed to the quintessential biological determinism of classic racialism in order to calculate national origins.

The difficulties in calculating national origins were legion, and the differences of political attitudes toward the provision were too. In February 1927 Senator Hiram Johnson told his colleagues that it would be foolish to base the quotas on inadequate data, and recommended an additional year to refine the computations. Democratic Senator James Reed of Missouri – who opposed national origins – voiced his support for the resolution to postpone the activation of the national origins quotas, although he made clear his desire to repeal the whole scheme. He sensed, correctly, that the ideological premise behind national origins was a desire to maintain the Anglo-Saxon purity of blood in the American population and was motivated by racial antipathy. On the floor of the Senate chamber, the Missouri senator ridiculed the notion of computing the blood of the old native stock. The endless potential for “crosses of the blood” in North America meant that determining the blood of the population almost 140 years ago remained “an impossible task,” and national origins was an “absurd and ridiculous scheme.”⁴⁰ Immediately, Republican Senator David Reed from Pennsylvania – the originator of the national origins basis of restriction – rose to defend his policy, and the repealers and supporters locked horns over proving the statistical accuracy of the national origins quotas.

⁴⁰ *Congressional Record*, Senate, 1 February 1927, 2678.

Defending national origins was complicated by the simple fact that the 1790 census did not provide the necessary data, so supporters had to explain how this difficulty would be surmounted. Or they did what David Reed did: they changed the subject. The Republican Reed, in his defense of the calculations from the 1790 census, used Trevor's defense of national origins – that neither the numerical quota set in 1921 nor that established in 1924 ever took into consideration native-born Americans in the computation of immigration quotas. Reed argued, “we, who were born in this country, had at least as much right to be reflected in the quota as had recently arrived immigrants.” One wonders if the Pennsylvania senator knew that, by Hill's calculations, if his family had arrived after 1790, he would have been considered in the immigrant quota.⁴¹ Some supporters argued that in the surnames listed in the Census manuscript, the original census revealed the true core of the American population. But when Reed insisted that the best basis for quotas was the racial origins in the 1790 census, Senator Royal Copeland pressed him on the enumeration's accuracy, and Reed had to concede that it was unreliable.⁴²

⁴¹ As far as can be ascertained, Reed's family was in fact a very old and established Pennsylvania family. The Senator's father, James Hay Reed, was a business associate of Andrew Carnegie and Philander Knox, and a director of United States Steel. James Hay, born in 1853, was the son of a physician, Joseph A. Reed, about whom little information can be found. But there was a Joseph Reed from Pennsylvania who was an early leader in the newly independent United States, and could perhaps be an ancestor to Dr. Joseph A. Reed. Biographical data for Senator David Aiken Reed from *The New York Times*, 11 February 1953, 29; Bruce Clayton, “Reed, David Aiken” *American National Biography*; Ari Hoogenboom, “David Aiken Reed” *Dictionary of American Biography, Supplement 5: 1951-1955*; on his father James Hay Reed, see Asher Isaacs, “James Hay Reed” *Dictionary of American Biography*; on Joseph Reed from the American Revolution, see G. S. Rowe, “Reed, Joseph” *American National Biography*.

⁴² *Ibid.*, 2679-85.

But it was not so much that the census was unreliable as that national origins was scientific folly, a fruitless endeavor to follow something that did not exist and was not true. That did not deter its supporters, however. Reed later laid out the exact reasons that he supported national origins as a method to restrict immigration. It seemed, to the Pennsylvania senator, that everyone was complaining about national origins – German immigrants, Scandinavian immigrants, Irish and Italian immigrants. The national origins provision, he insisted, was the only method of restricting immigration that would recognize the blood composition of the American population. “If some recently arrived alien who is not yet naturalized has a right to have his blood reflected in the quotas of our immigration,” he opined, “I say that we whose ancestors made this country and fought for it in every war have at least an equal right with that recently arrived, unnaturalized immigrant: and that is all that national origins does...”⁴³ In this narrative, and in this justification, lay the bare essence of the immigration restriction movement going back to at least 1894. The racial composition of the American population, as the racial nativists imagined it, had to be protected. This was the significant shift that Higham points out in the types of nativism that circulated in the United States after 1860, and it was predicated upon a faulty, erroneous understanding of biological and genetic principles of science. But that did not matter. What was important was to use a seemingly objective, non-discriminatory mechanism to achieve that goal of protecting the Anglo-Saxon population of the United States.

⁴³ *Congressional Record*, Senate, 6 December 1928, 137.

The principal source to compute the National Origins quotas was the Census Bureau's 1909 publication *A Century of Population Growth*. It computed the racial origin of the population, based on surnames recorded in the 1790 Census, and then used subsequent censuses to document the statistical patterns of demographic growth of that original population to arrive at the composition of the "blood" of the American population after 100 years. In Congressional testimony, however, supporters of the provision conveniently passed over the author's admitted limitations of the study. S. N. D. North, one of the supervisors of the 1900 census, forwarded the manuscript to Secretary of Labor and Commerce Charles Nagel highlighting the uncertainty behind the Census Bureau's computations. He pointed out that even those states that had manuscript returns from the 1790 census were of dubious value, as the returns were prepared by "underpaid assistant marshals." Further, five of the original thirteen enumerated states had had their schedules destroyed during the course of the War of 1812. Thus, at its core, the document to which the most ardent restrictionists would pin their strategy was, by its own admission, of highly suspect statistical value.⁴⁴

It was the compilation of surnames from the 1790 census that made *A Century of Population Growth* the essential source for national origins. Because of the haphazard way in which the original returns were filed by the marshals, and because several states' returns had already been destroyed, the prime purpose of the Census Bureau's monograph was to preserve the information from the census

⁴⁴ United States Department of Commerce and Labor, Bureau of the Census, *A Century of Population Growth: From the 1st Census of the United States to the 12th, 1790-1900* (Washington, D.C.: Government Printing Office, 1909), i.

of 1790. Extant returns listed 27,337 surnames in the manuscript returns of the original census, many of them the same. *A Century of Population Growth* grouped the names into broad classes based on the derivation of the familial surname: household and domestic items, nations and places, human characteristics, games, religion, music, literature, property, nature, ocean and maritime themes, war, death, violence, time, and “Unusual and ludicrous combinations of common nouns and of Christian names and surnames.” Thus surnames like Supple, Humble, Gaudy, Sullen, Compass, Hornbuckle, Beersticker, Cathole, Clampit, and Getstrap were alleged to reflect the national origin and racial identity of family lines in the United States. From this system of classification, the report drew the conclusion that at the beginning of Constitutional government in the United States, just 800 surnames (“practically all of which were of English or British origin,” the report asserted) contributed wholly one-third of the original population of the United States. The other 26,500 names that appeared less frequently in the manuscript returns demonstrated national and racial origins that led to the conclusion that 87% of the American population was British in origin.⁴⁵

This classification of surnames, the Census Bureau claimed, enabled a “reasonably accurate analysis of the nationality of the population,” but it conceded that the classification was only “an indication of blood, or what may be termed nationality strain, since it takes no account of the actual place of birth or

⁴⁵ Bureau of the Census, *A Century of Population Growth*, 111 for classes; 112-113 for surname examples, 115 for 800 surnames. Some of the “Striking or Ludicrous Combinations” were indeed amusing: Peter Wentup, Sarah Simperts, Wanton Bump, Boston Frog, Jedediah Brickhouse, Pleasant Basket, Hannah Cheese, Over Jordan, among many others. See p. 113; also Ngai, *Impossible Subjects*, 32.

parentage of the individual.” It emphasized this last point again: “it can not be regarded as possessing the least value from the standpoint of modern classification by place of birth.... [But it] possesses great value as indicating the proportions contributed by the different nationalities, to the population at the time the First Census was taken.”⁴⁶ This was the fundamental objection repealers had, and it is telling that the Bureau’s own document conceded its impracticality.

Critics and opponents of the national origins provision seized on this impracticality as a reason to oppose the new method. When Hill testified before the Senate Committee on Immigration, one member challenged the accuracy and value of the 1790 census, since it enumerated very few categories and was done under rudimentary statistical theories by untrained men. None of the marshals had any statistical training, and most of the analysts of the data lacked it as well.⁴⁷ Common sense suggested to Senator Royal Copeland that greater technological sophistication would make more recent censuses more accurate and useful measures of the demographic condition of the country. The 1790 census, in fact, enumerated only six categories. Hill disagreed with Copeland, and ignored the warnings of North that enumerators in 1790 faced “serious difficulties,” the least of which was the accumulation of data by the underpaid marshals. Hill argued that because the population in 1790 was more permanently settled, because the marshals were familiar with the population they counted, and because of the simplicity of the categories of enumeration, that the 1790 census was superior to

⁴⁶ Bureau of the Census, *A Century of Population Growth*, 116.

⁴⁷ See Cohen, *A Calculating People*; Anderson, *The American Census*.

more recent enumerations.⁴⁸ Hill's position was directly contradicted by the Census Bureau however, which declared that ill-defined town and county boundaries, the lack of statistical training for enumerators and their meager salaries, the basic unwillingness of people to supply information, and the distance between "isolated households of pioneers" made returns for the early censuses problematic. Furthermore, the Bureau noted that there was a total lack of uniformity in the way the marshals submitted their returns; some carefully organized and alphabetized the surnames of populations in each area, but others merely compiled them and submitted them with no organization.⁴⁹ These objections did not sway Hill, who continued to insist that national origins based upon data from *A Century of Population Growth* was the most accurate and fair method of determining immigration quotas.

Ultimately, *A Century of Population Growth* amounted to an endorsement – albeit a qualified one – of General Walker's "replacement thesis." Like many of the other dubious sources employed to justify restriction, though, the book itself was confused regarding Walker's theory. Noting that the former Census Superintendent's theory "has been opposed by many statisticians of prominence," the book went on to break down the relative population contributions of the original settler stock and the later immigrant stock, but the chapter dealing with

⁴⁸ Hill's testimony before the Senate Committee in which he claimed that the accuracy of the 1790 census was superior to more recent enumerations was reprinted in the *Congressional Record* by Minnesota Senator Henrik Shipstead. *Congressional Record*, Senate, 6 December 1928, 144-47. See also Texas Representative John Calvin Box in the House, *Congressional Record*, House of Representatives, 3 March 1927, on 5810-11.

⁴⁹ Bureau of the Census, *A Century of Population Growth*, 45, 50.

the “White and Negro Population” quickly confused readers regarding the certainty of the demographic contribution of the original stock. The population growth it computed was specifically limited to the white population – it was never contemplated to assess the contribution of African slaves to the American population for the quotas. Additionally, four generations of immigrants, who had begun arriving in significant numbers in the 1830s, had added to the United States’ population before immigration statistics were efficiently kept, making it “impossible to determine accurately the number of persons in the United States who were directly descended from the population enumerated at the beginning of the nineteenth century.”⁵⁰ But this was the entire premise behind National Origins. Although it pointed out that Walker’s theory had significant critics, the census manuscript also endorsed it. Out of the 66,809,196 whites counted in the 1900 census, it utilized Walker’s computations to assert that the original settler population had contributed 35 million of that total, and the foreign stock nearly 32 million. “It would be difficult,” the Bureau’s book read, “to suggest more vividly the great fecundity during the nineteenth century of the white population inhabiting the United States in 1800.”⁵¹ The theoretical agreement with the

⁵⁰ *Ibid.*, 86.

⁵¹ *Ibid.*, 89-90. See also Maurice Davie, “Immigration and the Declining Birthrate” *Scientific Monthly* v. 19, n. 1 (July 1924), 68-76. Davie, a professor at Yale, wrote that Walker’s theory was “unsound,” since nearly every country in history had experienced a leveling off of its birthrate when it reached a level of mature physical and economic development (72). He also pointed out that the hue and cry over the decline of the native birthrate was always a reference to the decline of the fertility of the upper-classes (75). Davie was not pro-immigration, but rather said that Walker’s theory was unsound because it was based mostly on anecdotal evidence, rather than a statistical analysis of the differential birthrate between the native-born and foreign-born (76). Anecdotal evidence, from Galton to Lombroso to Lodge, was important evidence for the suspicious assertions of classic racialists.

General's "replacement thesis" is clear, although it stopped short of endorsing Walker's implication that "new" immigrants arriving after 1880 were replacing this original stock.

A Century of Population Growth similarly claimed a spectacular fecundity for the British people that enabled them to not only repopulate the British Isles (from 9 million in 1712 to 16.2 million in 1801, to 41 million in 1900), but to furnish surplus population for the extensive settlement of "other nations" as well. Marveling at the robust growth of the English population, the report concluded that "in magnitude there appears to be no parallel in history for this population achievement of the British race from 1700 to 1900." The ability to replenish the British Islands and populate North America and Australia was regarded as a testament to the superior fertility of the British race, and tacitly affirmed Walker's thesis that it was not the inability to reproduce that limited the growth of the English stock of the American population, but as Walker said, the appearance of foreigners themselves.⁵²

Other than compiling a list of funny names in the manuscript returns of the first census, *A Century of Population Growth* was effectively useless as a source to compute national origins. Had there been any careful examination of its arguments, two glaring and fundamental problems would have immediately emerged. First, the presumption that nationality was some indicator of racial composition – the conflation of nationality with race – was simply, based on governmental practice, wrong. If nationality equaled race, why did the Bureau of

⁵² Bureau of the Census, *A Century of Population Growth*, 91.

Immigration, the Dillingham Commission, the Immigration Restriction League and many others consistently distinguish between “Northern Italian” and “Southern Italian”? Secondly, the book itself admitted, “it can not be regarded as possessing the least value” to determine geographical origins of the American population in 1790. Nonetheless, despite these two fundamental, inescapable problems, the book was a central tool to construct quotas that were equally highly suspect and dubious.

Into this mix, Representative Albert Johnson voiced an additional doubt on national origins to his House Committee on Immigration and Naturalization, to which the Senate Joint Resolution to postpone the measure had been referred. Johnson substituted an amendment to repeal the provision altogether instead of postponing it for a year. His reasons for the substitution were a telling indictment on the inadequacies of national origins. Here was a man who consistently worked for immigration restriction yet opposed the provision because it was highly unreliable. Instead, Johnson was a numerical restrictionist, and he believed that national origins was too complicated to create policy. The 2% quota was “easily explained and easily understood by all.”⁵³ It required no convoluted statistical computations, no investigation into the remote past of the old families of the nation. The complexity of national origins was, for Albert Johnson, an inadequate basis for legislating.

⁵³ House Report n. 2029, 69th Congress, 2nd Session, “Repeal of ‘National Origins’ Provisions of Immigration Act of 1924” 9 February 1924, 2.

Johnson was unable to shake his doubts about national origins. The Congressman from Washington state articulated his position on national origins before it went into effect in a series of letters to author Edward R. Lewis of Chicago. Lewis followed immigration matters very closely, and he frequently corresponded with Johnson in 1928 while Lewis was preparing his book *America: Nation or Confusion*, published in the same year.⁵⁴ Lewis argued stridently for racial purity in the United States, suggesting that the loss of racial homogeneity led to the decay and degeneration of nations.⁵⁵ As Lewis and Johnson discussed the feasibility and practicality of national origins, the Congressman persisted in his doubts. It was not that he was opposed to the principle of national origins, he told Lewis. But the “opinion prevails among many of the restrictionist members of the [House Immigration and Naturalization] Committee that to carry National Origins into effect is to add the opposition of organizations representing Northwestern European immigration to the continued assaults to the Southeastern European people, thus increasing a force that will continue to hammer away and

⁵⁴ Edward R. Lewis, *America: Nation or Confusion: A Study of Our Immigration Problems* (New York: Harper Brothers, 1928). In the book, Lewis argued that the increasing heterogeneity of American racial stocks was weakening the nation, and that to protect the United States only immigrants of the original native stock – the intent of both the national origins provision and the 2% quota – should be admitted as immigrants. Race mixing, as Lewis called it, did not result in the creation of breeds of supermen, where each succeeding generation took the best elements of parental inheritance, but instead “may entail jarring misfits...” He singled out the population of South Italian where “Italians, Greeks, Saracens, negroes, Normans, Spaniards and Phoenicians have mixed with results so conglomerate and so unhappy that they have cursed those regions and their peoples to this day.” (130) But he also claimed, *contra* Garis, that with the original colonial stock there was “nothing to indicate decadence...despite all the sneers passed upon it by the immigrants.” (111) Lewis was also a member of the Immigration Restriction League.

⁵⁵ *Ibid.*, 188.

ultimately break down restriction.”⁵⁶ This was a key concern for many elected officials with restrictionist tendencies. As they had consistently argued, curtailing undesirable immigration was the goal of all these restrictive bills, and national origin’s greatest advantage – which Trevor tried to demonstrate, that it was “scientific” and not “discriminatory” – was also its greatest liability, in that it made no distinction between immigrant groups who arrived after 1790. And because Scandinavian and German immigrants had not settled the country in numbers like the Anglo-Saxon British, they too would receive very small quotas. Despite the ethnological classification of these groups as kindred racial stocks, they would not, and could not, be admitted. This placed politicians in a very difficult position, and was one of the key reasons that many ardent Congressional restrictionists opposed National Origins in favor of the numerical quotas.⁵⁷

The political situation in Congress was very complicated, but there was little time to openly debate the relative merits of each method. When Johnson’s amendment was introduced into the House, he quickly realized that repeal of national origins would be impossible in such a short time. The divergent views within both chambers on national origins made any informed debate unrealistic in the brief time remaining in the session. When the bill was recommitted to the Immigration and Naturalization Committee of the House, Johnson restored the original language of the Senate resolution to postpone it, since the national origins

⁵⁶ Albert Johnson to Edward Lewis, 27 January 1927, Record Group 233, HR 70A – F 14.4, box 492, NARA.

⁵⁷ See for instance the petitions and telegrams sent to the Senate Committee on Immigration in 1929, organized in Record Group 46, box 172, Sen. 71-A – J 32, NARA.

quotas would go into effect without any action in Congress to delay it. But he held firm in his belief that national origins was too complicated. When the Committee reintroduced S.J. Res. 152 to the House with a new report, it persisted in the belief that the preliminary national origins quotas were “unsatisfactory,” and that such a “momentous undertaking” as computing immigration quotas, “involving as it does the life currents of millions of human beings” must be “easily recognizable for their integrity.”⁵⁸ The resolution thus endorsed the Senate’s position to postpone its implementation for additional study while it opposed the principle. But with the opposition of the Chair of the House Committee on Immigration and Naturalization, it seemed that the days of national origins – even though it was only postponed – were numbered.

Anti-restrictionists in the House were put in a difficult position when the bill to postpone the implementation of national origins was brought up on the floor, further dividing the political coalitions over immigration. Favoring repeal of the provision, they were deprived of any alternative other than to support postponing the implementation of the new quota for one year. There was no option to repeal. But postponing the provision would strengthen the hands of supporters by providing, theoretically, a more precise and accurate computation, shielding the provision against criticisms of unfairness and inaccuracy. Supporters of national origins, however, recognized that in the terms of the 1924 Immigration Act, the provision would go into effect unless decisive action was taken; thus they

⁵⁸ House Report n. 2260, 69th Congress, 2nd Session, “Postponement of ‘National Origins’ Provisions of the Immigration Act of 1924” 26 February 1927, 2.

tried to filibuster the proposed bill. Opponents of national origins who, like Johnson, favored numerical restriction instead, pushed the bill forward.⁵⁹ When Colorado Republican William Vaile, an ardent restrictionist, rose to support the implementation of national origins, he explicitly articulated the goal of the contentious provision. Deploring the limited debate the House had organized for the motion to postpone national origin's enactment, Vaile pressed for extended debate on "a question which goes to the blood of the Nation for generations to come..." The new immigration "was changing the proportions of American blood in a degree which was undesirable," and the purpose of national origins was simply "to try to continue the blood of the United States in its present mixture without changing its proportions, and, of course, endeavoring to protect its quality."⁶⁰ The bogeyman of decadent, undesirable blood in the American population that was given sanction in the massive Dillingham Commission reports, continued to haunt the minds of legislators fifteen years later, attesting to a deep-seated belief in classic racialism.

Although the resolution to postpone national origins passed the House on March 3 by a healthy margin (234 in favor, 111 opposed, 85 not voting), it did not close off debate.⁶¹ Knud Wefald, a Norwegian immigrant elected to the 68th and

⁵⁹ *Congressional Record*, House of Representatives, 3 March 1927, 5643.

⁶⁰ *Ibid.*, 5643-44.

⁶¹ The recorded votes for the House on S.J.Res. 152 reflect the strange bedfellows that national origins created in the lower house: anti-restrictionists Jacobstein, Sabath, Dickstein, LaGuardia, Wefald, Rogers, and Celler allied with committed restrictionists like Johnson, Burdick and Foss in an attempt to preserve the numerical policy of restriction. *Congressional Record*, House of Representatives, 3 March 1927, 5650.

69th Congresses to represent Minnesota on the Farmer-Labor ticket, took the floor and assailed the underlying premise of protecting the blood and racial purity of the United States that national origins, as Vaile had so explicitly stated, was designed to do. Wefald argued that Scandinavians were just as capable as the British to preserve the institutions of the American government, and the quotas, which drastically cut down immigration from Scandinavian countries, were unfair to a group that in the classic racialist schema was racially equivalent to Anglo-Saxons.⁶² This was what Johnson feared.

Wefald's comments after the postponement of national origins highlighted one of the essential problems that opponents of restriction faced. "The father of the national-origins proposal in 1924," he explained, "was one Capt. John B. Trevor, of New York City, a moneyed man of leisure with strong anti-German sympathies."⁶³ Like Grant and Stoddard, Trevor was a "moneyed man of leisure,"

⁶² Immigration from Norway under the various quotas had been dramatically restricted. 12,202 were allowed annually under the first quota provision based on 1910, which was slashed almost in half to 6,453 under the 1890 basis, to 2,433 under the national origins regime. See table printed in *Congressional Record*, House of Representatives, 29 June 1926, 2282. Massachusetts Democrat John Douglass called national origins "an outrageous act of injustice" and an "eccentric experiment" and Illinois Democrat Adolph Sabath decried the way that "professional restrictionists" in Congress adopted the "McLauhlin [sic] report as an official committee report, which I believe is contemptible and unjustifiable." *Congressional Record*, House of Representatives, 3 March 1927, Douglass on 5848; Sabath 5850. In the "Dictionary of Races or Peoples" the Norwegians, Danes, Swedes and Icelanders were grouped together under "Scandinavian," which were described as the native Teutonic race of Europe, and along with the Germans were the most familiar of the "old" immigration. (119) The report also noted: "Nor is it necessary to remind the student of ethnology that the Scandinavian is considered to be the purest type of one of the three great races of Europe as divided from a physical point of view; that is, of the 'Northern' or 'Teutonic' race" as opposed to the Alpine or Mediterranean races." The Norse element in the American population was so well known that it needed no description, but emphasized that they make ideal farmers, Americanize with great alacrity, have the lowest illiteracy in Europe, and are almost universally Protestant. (120). United States Immigration Commission, "Dictionary of Races or People."

⁶³ *Congressional Record*, House of Representatives, 3 March 1927, 5661.

and like them had no professional scientific training. His commitment to the classic racist orthodoxy, and the restriction of immigrants that he deemed to be inferior, were the only real qualifications he had. He was able to gather support for this complicated scheme of immigration restriction with no professional academic position or scientific credentials. Wefald's description of him as a man of leisure speaks to a much larger issue of the role that amateurs and non-scientists were able to play in the fluid field of scientific inquiry. In Kuhn's scheme for understanding revolutions in scientific knowledge, the two communities devoted to competing scientific methods suffered from what he described as a problem of "local incommensurability": the two camps used lexicons that in many ways were untranslatable or had significantly different meanings.⁶⁴ So for the classic racist community, which did not have the specialized training of appropriate scientific communities, "blood" and nation stood as an equivalent of race, as an immutable, permanent determiner of behavior, development, quality, and thus, desirability. But for the opposing camp, "blood" had come to mean very little in terms of race, and chromosomes and environment, which had a coequal role in determining behavior, development, and quality, were of paramount importance. It was ultimately the wider familiarity of the older lexicon in the public's mind, that audience that men like Grant,

⁶⁴ The notion of a "lexicon" or "local incommensurability" was a refinement of an idea set forth in *The Structure of Scientific Revolutions*, wherein the "revolution" of a new system or method – what he also termed a "gestalt switch" – is generally resolved in favor of the new paradigm. See Kuhn, *The Road Since Structure*, chapter 2 (esp. 37); Kuhn, *Scientific Revolutions*, esp. chapters 10 and 12.

Stoddard, Trevor, Wiggam and many others aimed for, that gave the classic racist camp the ability to influence the political process.

National origins and even the policies of numerical restriction could be easily explained and comprehended by using the old and familiar “lexicon”: southeastern Europeans were racially different from the old native-born American stock, this difference was in their blood, this difference made them undesirable, and this undesirability justified their exclusion. A contrary explanation might be that southern and eastern Europeans were different in physical appearance, cultural values and characteristics, and historical development from the old-stock Americans, but this difference was attributable to various factors like education, economic and industrial development of their country of origin, opportunity, and heredity. “Racial” difference, to people familiar with tenets of classic racialism, was just much easier to understand.

This is in some ways an inversion of Kuhn’s description of how “revolutionary” science displaces “normal” science. A new scientific paradigm will offer greater explanatory power, particularly in its ability to address and solve the problems and issues that the older method cannot – as Kuhn describes it, “the persistent failures of the puzzles of normal science to come out as they should. Failure of existing rules is the prelude to a search for new ones.” Yet he describes that converts to the new paradigm may also shift methods based on arguments, “rarely made entirely explicit, that appeal to the individual’s sense of the appropriate or the aesthetic—the new theory is said to be ‘neater,’ ‘more suitable,’ or ‘simpler’ than the old.” While the general application of this aesthetic neatness

is an important component for understanding the resolution of scientific revolutions (Kuhn's frequent allusion to the Copernican revolution is an excellent example), in *this instance* the opposite seems to be the case. The simplicity of "like produces like" was one of the reasons that the lay community maintained its belief in the old theories of inheritance that the new experimentalist methods had demonstrated to inaccurate.⁶⁵

The success of Trevor's activities, and how his influence extended into professional scientific communities, can be observed in an article IRL member Robert DeCourcy Ward published in the *Journal of Heredity*, in which the Harvard climatology professor explained the difference between the numerical quotas and the national origins quotas. Under national origins, the quotas were no longer to be based, "as now, upon the *foreign born*, those composing the 'alien colonies' and alien 'blocs' now in this country, *ignoring the native-born*, but are to be divided among the different nationalities according to the national origins of our population as a whole."⁶⁶ Ward did not in the slightest hint about the difficulties the provision was facing in Congress, or that many legislators felt that the method was too spurious. The tone of his article was that national origins was not only eminently fairer than the numerical quotas, but that there was unanimity

⁶⁵ Kuhn, *Scientific Revolutions*, failures from 68; aesthetic appeal from 155. Mae Ngai states that "A more honest inquiry into the matter by the Quota Board into the matter might have concluded that determining the national origins of the American people was theoretically suspect and methodologically impossible. But relentless lobbying by the restrictionists and the pedigree of the quotas' authors overcame all obstacles and doubts." Ngai, *Impossible Subjects*, 35.

⁶⁶ Robert DeCourcy Ward, "The Third year of the New Immigration Law" in box 3, Lee Papers, n.p. (5). This was an offprint of Ward's article from the *Journal of Heredity*, v. 18, n. 1 (January 1927). Emphasis in original.

in support of the feasibility of determining the stock of the total population. As often happened with restrictionists in trying to convince the public to support their view, Ward elided many of the complexities and nuances of the politics and the science involved.

Congress twice rejected the national origins quotas, as members examined the methodology and assumptions that Hill and the Quota Board operated under and found errors of computation or logic. In 1928 the American Council of Learned Societies appointed two experts to analyze the proposed quotas – Howard Barker, a professional genealogist and Marcus Hansen, a historian – and who found that the quota for England in the first two estimates was too high because of the improper Anglicization of names by the Census Bureau’s marshals who conducted the 1790 census.⁶⁷ Each time they had to be recalculated, a different numerical quota was returned. Herbert Hoover campaigned in 1928 on a platform that included repeal of both the national origins provision and the immigration quotas based on the 2% quota from the 1890 census that was then in effect. When Hill and the Quota Board submitted a third report to the Senate Committee on Immigration in February 1929, Hill insisted, “The present computations are as near as we can get on this matter of determining the national

⁶⁷ Senate Doc. n. 65, 70th Congress, 1st Session, Message from the President, “Immigration Quotas on the Basis of National Origin,” 27 February 1928, 4; Ngai, *Impossible Subjects*, 32-3. The Quota Board submitted three different quotas in three different years; but Trevor also submitted two different sets of quotas outside the Quota Board’s submissions, in his “Study of the Population of the United States” and “Restudy of the Plan of National Origins.”

origins, practically.”⁶⁸ And yet Representative Johnson maintained his opposition, issuing from his committee a statement that “calculations based on census returns running from 1930 back to 1790...are impossible of explanation to the lay American, much less to the alien and the foreign born.”⁶⁹ Despite the House Committee’s objection, advocates of the quotas insisted that they were as accurate as they would get, and the Senate Committee finally approved the Quota Board’s computations, which Hoover then signed into law, despite his professed opposition, on March 22, 1929.⁷⁰

National origins limped into effect in the final days of the 70th Congress when another resolution to postpone came before the House. But by this time, it was too late. Representative Box of Texas, who in the first session had adamantly insisted on the enactment of national origins, rose before the vote to demand the “yays” and “nays” so that the Representatives’ constituents would know their vote in favor of postponing the clause yet again. When the vote in the House was held on the last weekend of the session, opponents of national origins again carried the day, passing the resolution to postpone national origins until 1930 by a vote of 192 to 152 (83 not voting).⁷¹ But because of parliamentary procedures, for supporters of national origins, the battle was over; they had won. Action on the

⁶⁸ Hearings before the Senate Committee on Immigration, 70th Congress, 2nd Session, “National Origins Provision of the Immigration Law” 4 February 1929 (Washington, D.C.: Government Printing Office, 1929), 16; Ngai, 34-35 for Hoover’s position and the final Quota Board report.

⁶⁹ House Report n. 2811, 70th Congress, 2nd Session, “Postponement of National-Origins Provision of the Immigration Act of 1924” 2 March 1929, 2.

⁷⁰ Senate Document n. 259, 70th Congress, 2nd Session, “Immigration Quotas on the Basis of National Origin” Message from the President, Calendar 27 February 1929.

⁷¹ *Congressional Record*, House of Representatives, 3 March 1929, 5202.

amendment had been sufficiently delayed; the amendment had passed the House on a Sunday, and it could not be properly considered by Senate rules, before the end of the session. When Senator Hiram Bingham (R-CT) rose to introduce a motion to consider the House Joint Resolution on the following Monday, Senator David Reed, knowing full well the fight over national origins was over, rose to challenge him. Reed pointed out Senate Rule XIV which stipulated that all bills received from the House must be read three times on three different days. Reed informed his Senate colleagues that if the opponents of national origins tried to hustle through the resolution, he would dispute the point of order. And that was it. No action having been taken, the quotas would go into effect on the day appointed in the previous bill delaying its enactment. With a whimper, not a bang, when the Senate adjourned *sine die* that afternoon, repealers and numerical restrictionists were beaten. With no action taken by Congress, on July 1, 1929, the quotas announced by President Hoover in March went into effect.⁷²

The myriad scientific criticisms of the impracticality and inherent methodological flaws of computing national origins had had no effect in the age of the Scopes trial. The perception in the public and Congress of the immutability of race, described and reinforced for decades by classic racialists, proved to be too powerful for legitimate scientific criticism to overturn. Even the conviction of John Scopes reinforced the perception of immutable and unchanging races, whether they were created by God or descended from apes. If evolution were true,

⁷² *Congressional Record*, Senate, 4 March 1929, 5222; Senate Doc. 259, 70th Congress, 2nd Session, "Immigration Quotas on the Basis of National Origin" Message from the President, 25 February (Calendar 27 February), 1929.

the scientific arguments made in favor of it in Dayton posited a geologic time-scale, over millennia, which suggested a certain timelessness and permanence of racial qualities. When visual evidence confirmed this – eye color, head shape, hair color, stature, skin color – in the world around them, the public’s understanding that like produced like became more concrete and unshakeable. This can also partially be attributed to the complexity of experimental genetic and biological science. The historians Diane Paul and Hamish Spencer make this point in describing why earlier professional critiques of Charles Davenport’s work at Cold Spring Harbor were unsuccessful. They note, “even the soundest scientific arguments will fail to convince in the absence of a suitable culture in which the argument can take root.”⁷³ The absence of such a “suitable culture” in the United States in the late 1920s can be seen in the 1927 Supreme Court decision to allow the sterilization of Carrie Buck (an inmate at a Virginia institute for feeble-minded, who was allegedly the second of three generations of feeble-minded females). The Court’s decision was another illustration of the simplicity of classic racialism trumping a more accurate scientific paradigm and in the credence given to mental testing.⁷⁴

While professional scientists like Franz Boas, Ales Hrdlicka and Herbert Spencer Jennings could point out the more egregious inaccuracies in the claims made by classic racialists and their nativist associates, their ability to clearly explain how genes and chromosomes actually worked in organisms was itself

⁷³ Hamish Spencer and Diane Paul, “The failure of a scientific critique.” 452

⁷⁴ See Stephen Jay Gould, *The Flamingo’s Smile*, chapt. 20; Paul Lombardo, “Facing Carrie Buck” *Hastings Center Report* v. 33, n. 2 (March-April 2003), 14-17.

highly problematic. Even when Walter Lippmann tried to reach large audiences in *The New Republic* with accessible and understandable critiques of mental testing in the mid-1920s, he was unsuccessful. Essentially, all these critics of classic racialism could say with certainty was that inheritance and heredity were far more complex than its adherents argued, and that predicting the development of human organisms was incredibly difficult due to the interaction of many different factors – the environment, biparental inheritance, complex Mendelian ratios, various chromosomal recombination, and other factors. Racialists were able to counter these confusing claims with an elegantly simple declaration that like produces like. In the hands of skilled and passionate writers like Grant, Stoddard, Wiggam and others, this proved to be remarkably alluring, not only for the public but policy makers as well. The complexity of genetic and biological science could be almost paralyzing – by conceding that knowledge of heredity was imperfect, it left no suitable ground on which to devise policy. Insisting that Carrie Buck or Deborah Kallikak were inferior products of inferior parents, who would certainly perpetuate that inferiority if allowed to reproduce, not only tapped into social anxieties, but also joined a very long and well-established chain that began with Lombroso and Galton.

Considering the intellectual history of racial nativism alongside immigration restriction shows that restriction succeeded not only because of its persistent and well-connected advocates, but also because professional scientists were ineffective at opposing the inaccurate “biological” theories of race that supported restriction. It also complicates the Kuhnian thesis of revolutionary

paradigm shifts in scientific knowledge. For every Morgan, Boas or Jennings, who carefully and cautiously advanced scientific knowledge, there were many more Wiggams, Grants, Stoddards, Laughlins and Goddards. It must be remembered that it was precisely the instability of scientific knowledge in the early twentieth century that created space for the moneyed “men of leisure,” popularizers and partisan advocates, to gain a public forum. Despite Pearl’s and Jennings’s articles in popular magazines, the dominant perception into the 1930s of fixed, immutable and irrepressible hereditary characters could not be combated with any great effectiveness.

Chapter 8: The Futility of Criticism

With the continued calls for immigration restriction on the basis of race gaining strength in the 1920s, Raymond Pearl's uneasiness toward the claims of classic racialists increased. Writing to Robert Yerkes in late 1922, he complained of Charles Davenport's recent statements on racial crosses and mixing and what Davenport believed to be inherently unstable and inferior offspring that resulted from such unions. These early investigations culminated in Davenport's book *Race Crossing in Jamaica*, which was published in 1929, and the assumptions that underlay the book were manifest throughout most of the Cold Spring Harbor director's work. Davenport's work "rests upon very little except a priori reasoning," Pearl told Yerkes, citing his own work on chickens at the Maine Agricultural Station in the 1910s to demonstrate the dangers of deductive approaches to science. Davenport's thoughts on racial crossing were that unstable and weak offspring resulted from widely divergent racial mixing. Race crossing ultimately led to race enervation. As Pearl had found, unions of disparate crosses in poultry were not plagued by dramatic physiological imbalances because these were almost always naturally eliminated before birth. Yerkes replied that he too had often had the feeling that Davenport "was expressing wishes rather than facts."¹ Scientific experimentation had given the lie to simplistic claims of heredity and inheritance, and the important influence of biological determinism on government policy in the 1920s was for Pearl, as for many others, highly

¹ Raymond Pearl to Robert Yerkes, 29 November 1922; Yerkes to Pearl, 29 November 1922, box 39, Yerkes papers.

troubling. As the decade wore on, and the claims of the superiority of certain races continued to resonate with the public, Pearl and these critics would be moved to action. Particular spurs for them were the dubious theories that Harry H. Laughlin was proposing to Congress as it attempted to devise American immigration policy.

This community of criticism, which included Jennings, Boas and Hrdlicka, among others, emerged partly out of the cosmopolitan outlook and perspective the men obtained in their early training, and cultivated throughout their careers. Pearl's skepticism was rooted in his experimental training, learned partly from Jennings and Reighard at the University of Michigan. His two teachers impressed upon him the importance of careful experimental work, which generated data from which cautious conclusions could be researched. His research at Orono exemplified this approach. Jennings's work at Naples became a critical nexus of his hesitancy toward declaring universal biological laws. Boas, whose 1911 book *The Mind of Primitive Man* became one of the foundational texts for modern cultural anthropology and probably exceeded the influence of his Dillingham Commission report from the same year, was driven by the same research agenda, namely that the relationship between elements and wholes had to be understood in relation to each other and their surroundings. The historian Vernon Williams describes *The Mind of Primitive Man* as "complete in its refutation of crude racial determinist thinking, complete in its indictment of crude racial prejudice." Hrdlicka also utilized this method of investigation among his work on Native American tribes in North and Central America. As he explained to the

Anthropology Section of the American Association for the Advancement of Science in December 1919, anthropology was the “science of human variation, both in man himself and in his activities,” and as such could only be understood by “careful observation and consideration.” As a curator of the physical anthropology collections at the Smithsonian, Hrdlicka also had a special responsibility to ensure that the data of the federal institution be available and accessible to investigators among the population. Pearl, Jennings and Boas worked in academic institutions continuing their experimental work, while teaching future students about the methods of modern scientific investigation. It was not the institutional setting that made these men critics of classic racialism in the 1920s, but was instead a methodological critique. And when sweeping, false claims were made in the 1920s, based very loosely and sloppily on the methods they worked so hard to master and practice, they acted to counter and dispute them.²

One of the principal reasons these cosmopolitan critics were not successful was the complexity of their work. While Laughlin, Wiggam, Grant, Stoddard, Davenport and others flattened out the genuine complexity of scientific theories

² “Elements” and “wholes” in Boas’s work taken from Stocking, *The Shaping of American Anthropology*, 4-5; Williams from *Rethinking Race*, 22; Hrdlicka from Ales Hrdlicka, “On the Relations of Anthropology and Psychology” *Science* n.s. v. 51, n. 1313 (27 February 1920), 199-201; 201. On Jennings and Pearl, see Mezzano, “Progressive Origins of Eugenics Critics”; Barkan, “Reevaluating Progressive Eugenics”; idem., *The Retreat of Scientific Racism*; Judith Johns Schloegel, “Intimate Biology: Herbert Spencer Jennings, Tracy Sonneborn, and the Career of American Protozoan Genetics” (Indiana University: Ph.D. Diss., 2006). On Hrdlicka’s work on native American groups, see Ales Hrdlicka, “Physiological and Medical Observations among the Indians of the Southwestern United States and Northern Mexico” *Bulletins of the Smithsonian Institution*, Bulletin n. 34 (Washington, D.C.: Smithsonian Institution, Bureau of American Ethnology, 1908); idem., “Physician Anthropology of the Lenape or Delawares” *Bulletins of the Smithsonian Institution*, Bulletin n. 62 (Washington, D.C.: Smithsonian Institution, Bureau of American Ethnology, 1916).

of heredity and biological development, these men were forced, by training and temperament, to see vastly different conclusions. Pearl's longtime friend, the British epidemiologist and medical statistician Major Greenwood, was one of the chief sounding boards for Pearl's frustrations in the twenties of his inability to adequately defend scientific knowledge from misrepresentation. Both men had extensive experience working in statistics with – or more accurately, under – the great Karl Pearson, both rankled at the production of shoddy scientific work by untrained amateurs, and both had hearty, sparkling wits.³ When Pearl traveled to England, he never failed to look up his friend in London, where they complained, caroused, and pontificated over both frivolous and weighty issues. But their attitudes toward scientific investigation remained consistent: there were proper standards of experimentation and evidence that simply had to be fulfilled.⁴

³ Greenwood became, after an interview with Karl Pearson in 1900, what Lancelot Hogben described as “a convert to the new cult of biometry, a self-dedicated pupil of the great man and anointed evangelist of the gospel of number.” He was also, Hogben noted, by neither choice nor inclination an experimentalist, but due to the fortuitous intervention of Leonard Hill remedied his narrow approach to statistical work in medicine. In 1919 Greenwood became a Member of the Royal College of Physicians, a Fellow of the same organization in 1924, the first Professor of Epidemiology and Medical Statistics at the London School of Hygiene, a Fellow of the Royal Society in 1928, and later, after a brief sojourn as a lecturer at the Johns Hopkins University with his old friend, was made an honorary member of the American Statistical Association. Lancelot Hogben, “Major Greenwood. 1880-1949” *Obituary Notices of Fellows of the Royal Society* v. 7, n. 19 (November 1950), 138-54; 139 for Pearson, 141 for Hill, 142 for academic credentials. See also P. L. McKinlay, “Major Greenwood: 1880-1949” *Biometrika* v. 38, n. 1-2, (June 1951), 1-3, which considers Greenwood's role as a pioneer in the application of statistical ideas and methods to medical investigation as his greatest legacy. Greenwood also had experience in working with Italian scientists on statistical matters; in late November 1923, having recently returned a international conference on statistics in Paris, Greenwood expressed the hope that Niceforo – “who is really a decent figure juggler” – would facilitate international work on correlation tables. Greenwood to Pearl, 18 November 1923, Pearl papers.

⁴ A trip to England in the summer of late 1924 allowed the two friends an opportunity to catch up and, for Pearl, to escape Prohibition and visit “East Anglian towns with commendable pubs.” It was, however, a working trip, as Greenwood arranged for Pearl to address the London School of Economics on “The Curve of Population Growth.” When Greenwood facetiously suggested Sir Arthur Newsholme be allowed to chair Pearl's talk, the American replied that he would be delighted, “especially if I might be permitted to knock him down and step on his face at the end of

In September 1921 Pearl communicated his increasing skepticism of the scientific foundations of the eugenics movement to Greenwood after he returned from attending the Second International Congress of Eugenics, which was held in New York. Pearl and Jennings both delivered scientific papers at the six-day conference, and both advised exercising caution about the potential of the science to increase the quality of the human species. Pearl's paper, "Some Eugenic Aspects of the Problem of Population," exhibited the mathematical and statistical approach he took to matters of inheritance, as well as the importance of experimentally derived data. Eugenics was concerned with the type of human individuals that would inherit the earth, and it assumed that man could shape that type theoretically. This was certainly not practically possible, and even the theoretical possibility, he told the audience, was highly suspect.⁵ Pearl explained to Greenwood that he had avoided as much of the conference as possible – preferring instead "breaking the prohibition law with scientific friends in our rooms at the hotel" – because the eugenic part of the program was "deadly dull," and sounded too much like the fantasy of Alice in Wonderland, although Pearl noted that the papers concerning genetics were rather good. The pragmatic and experimental approach he took to his scientific work was sorely needed in the

the lecture." Pearl and Greenwood disliked Newsholme because the erstwhile Chief Medical Officer of England lacked a sophisticated grasp of statistics, which for his post both men felt he should have had. Greenwood to Pearl, 27 February 1924, for the trip's origins and "commendable pubs"; on Newsholme, see Greenwood to Pearl, 19 April 1924; Pearl to Greenwood, 2 May 1924, all in Pearl Papers, APS. Pearl's paper was also delivered at the American Philosophical Society in Philadelphia, which published his paper as "The Curve of Population Growth" in *Proceedings of the American Philosophical Society*, v. 63, n. 1 (1924), 10-17.

⁵ Raymond Pearl, "Some Eugenic Aspects of the Problem of Population," in *Eugenics in Race and State: Scientific Papers of the Second International Congress of Eugenics* (Baltimore: Williams and Wilkins Co., 1923), v. 2, 212-214; 214. In his talk Pearl laid emphasis on "theoretical."

field of eugenics, which he felt had gradually become divorced from scientific reality.⁶ This divorce from scientific reality was one of the ironies of the increasingly successful restrictionist movement in that it achieved its greatest impact when it was clearly wrong. When Greenwood received a copy of Charles Davenport and Albert Love's statistical book *Army Anthropology*, he complained that Davenport and his "fellow conspirators" produced a volume worse than its English equivalent because it was much longer, although he joked that this made it much more useful as a footstool or paper weight. Pearl replied that Davenport had made attempts to involve him in the statistical computations, but that he had avoided it as "it has always seemed merely a bad tabulation of bad statistics and nothing more."⁷ Pearl had little regard for Davenport's work in general, but his compilation of the Army data was particularly bad.

The paper that Jennings delivered to the International Congress was similarly skeptical of the field, and mapped out the path of criticism toward eugenics that he would take in the coming years. The essential assumption of eugenics was that "like produced like," but Jennings pointed out that man and all higher animals have an elemental contribution of two inheritance strains. Jennings

⁶ Pearl to Greenwood, 29 September 1921, Pearl Papers. Pearl also asked his friend about rumors he had heard at the Congress that Pearson may step down as head of the Galton Lab and that Greenwood would be chosen as the most logical replacement; Greenwood replied that the Galton Professor in Eugenics should be interested in eugenics and not medical statistics, and that the Pearsonian and Leonard Darwinian model of eugenics held no interest for him, as he considered it "merely after dinner tosh." Pearl to Greenwood, 29 September 1921; Greenwood to Pearl 23 November 1921, Pearl Papers.

⁷ Greenwood to Pearl, 21 October 1921; Pearl to Greenwood 26 October 1921, both Pearl Papers. Charles B. Davenport and Albert G. Love, *Army Anthropology: Based on Observations made on Draft Recruits, 1917-1918, and on Veterans at Demobilization, 1919* (Washington, D.C.: Government Printing Office, 1921).

discovered that in unicellular organisms, although most offspring had essentially the same genes, this did not preclude wide variety, and occasionally great diversity, in the offspring even in these simple organisms. The popular impression of eugenics and genetics was that human characteristics behaved as they did in unicellular organisms, and were predetermined in the germ plasm received at birth, which determined all facets of later development and made them resemble the parental characteristics. But Jennings, with his experimental background, insisted that such assumptions were not valid in simple-celled protozoa, and “I venture to say that there is no ground for asserting it to be true in man.”⁸ A great deal of the insights into genetics and heredity came from simple organisms for a variety of reasons – the environment was much easier to control, shorter gestation and reproduction periods enabled long ancestral lines to develop quickly, and experimental variables and manipulation could be used on the simple specimens in a way that they obviously could not on human beings. Thus, scientists with backgrounds in zoology like Pearl and Jennings, saw that the *a priori* assumptions at the core of eugenics did not hold true for small organisms – so how could they be presumed to be true in far more complex organisms like man?

Pearl’s critiques of research in genetics and population patterns extended to serious scientists as well. Shortly before St. Patrick’s Day in 1923, Pearl criticized the noted geneticist August Weismann in a letter to Greenwood. Pearl

⁸ Herbert Spencer Jennings, “Inheritance in Unicellular Organisms,” in *Eugenics Genetics and the Family: Scientific Papers of the Second International Congress of Eugenics held at the American Museum of Natural History, New York City, New York, 22-28 September 1921* (Baltimore: Williams and Wilkins Co., 1923), v. 1, 59-64; 60.

felt that Weismann's greatest contribution as a naturalist was to perceive, through the observation of simple-celled organisms, the mechanism of heredity that he laid out in *The Germ Plasm* in 1893.⁹ But when Weismann "left nature and began to explore his inner consciousness," Pearl pointed out to Greenwood, "he seems to have become pretty stupid. The trouble with the whole natural selection game as it flourished in the 80's and 90's was that, as we now know, it was purely a priori reasoning which had nothing whatever really to do with the actual facts of nature."¹⁰ This was the core of Pearl's growing apprehensiveness to eugenics. Advocates put forth theories based on assumptions that had been experimentally invalidated, that were not based on actual facts of nature. The signal triumph of T. H. Morgan and Mendelian genetics was to make the faulty *a priori* assumptions perfectly clear and to expose them as, quite simply, wrong. The problem, however, was that this was clear only to a very small number of scientists. By and large the American public still had complete confidence in the eugenic implications of natural selection, and by extension, to the immutability of race that underpinned the restrictionist movement.

As the strong influence of Laughlin on the United States Congress and immigration policy became clearer in the mid-1920s, both Pearl and Jennings published critiques of the sloppy statistical data the eugenicist was furnishing the Congressional committees, in particular Laughlin's computation of the

⁹ Carlson, *Mendel's Legacy*; August Weismann, *The Germ Plasm: A Theory of Heredity* W. Newton Parker and Harriet Rönnfeldt, trans. (Bristol, U.K.: Thoemmes Press, 2003 [orig. 1892].

¹⁰ Raymond Pearl to Major Greenwood, 16 March 1923, Pearl papers.

dependency of immigrant stocks. Laughlin's data samples that he used as the basis for his testimony were highly flawed, but no one on the Committee possessed the scientific training to point that out. They may not have understood the ways Laughlin was misrepresenting heredity and biology and flattening out the complexities of scientific knowledge of heredity and genetics. Or, like Johnson, they have not have cared at all about the accuracy of the testimony Laughlin provided, but merely wanted the ERO director to provide them with information to justify their pre-determined policy provisions. Pearl and Jennings tried to provide professional, credible scientific information. Laughlin did not. But significantly, it was only Laughlin who had any effect on Congress and the public.

In June 1923, Jennings received a letter from the editorial board of *The Survey* asking for a contribution to the magazine that would examine Laughlin's "Analysis of America's Modern Melting Pot" that was prepared for the House Committee on Immigration and Naturalization, and published by the Government Printing Office in early 1923. "This report contains some astonishing generalizations," the board's representative Bruno Lasker noted, "and not being expert on this subject we do not feel sure whether the evidence bears them out or whether other important considerations have been neglected...." Lasker and his colleagues felt constrained and confused by the conflicting evidence about heredity and genetics from biologists and sociologists, but they recognized the weight that Laughlin's work carried in favor of "further restrictive legislation." The eugenicist's report would be "difficult to combat if true, and...will in the

future be thrown at us every time we pleaded for a liberal treatment of the immigration question.” Would Jennings not consider doing the public a service, and as a biologist study the matter in “a scientific, unbiased manner”? Lasker assured Jennings that *The Survey* would be able to help the general public gain a better understanding of the complexities of heredity and inheritance if the Hopkins professor would be willing to write the article.¹¹

Jennings not only drafted an article for publication in *The Survey*; when the school year opened he had his seminar students go over Laughlin’s data as well as a didactic exercise of how statistical data could be misrepresented. Geddes Smith, the new managing editor of the weekly publication, welcomed the revisions that Jennings then made to his article, and lined it up for publication in a mid-December issue. The article, Smith felt, “leaves the hasty advocates of restrictive legislation very little ground to stand on.”¹² A note of encouragement to Jennings from Pearl around the same time echoed Smith’s fears of “the reactionary group” that had the ear of Congressmen and were using Laughlin’s data as a support for greater restrictions on immigration.¹³

Although he was given an audience before the House Immigration and Naturalization Committee during the debate over the 1924 Immigration Act, Jennings was squeezed in at the end of the day’s session, and was only able to

¹¹ Bruno Lasker to Herbert Spencer Jennings, 12 June 1923, Jennings papers.

¹² Jennings to the Editor of *The Survey*, 24 October, 1923; Geddes Smith to Jennings, 26 November 1923, both Jennings papers. Jennings’s discusses his classroom in the letter of 24 October.

¹³ Pearl to Jennings, 24 November 1923, Jennings papers.

speak briefly to Johnson and the other members.¹⁴ Unable to adequately point out problems with Laughlin's testimony to the committee members in detail, Jennings used his article for *The Survey* to demonstrate Laughlin's misleading and inaccurate data before the final vote was taken in the whole House. It was true, as Laughlin had testified, that the environmental pressures that immigrants faced were greater than those of the native born, because immigrants often lacked an established social network to provide for economic and social stability. But Jennings demonstrated a glaring problem in Laughlin's data sample: it was drawn only from public institutions, which would naturally deal almost exclusively with the poorer classes, which recent, unestablished immigrants tended to be. It should not be surprising that immigrants would have a higher representation in governmental institutions. Jennings asked, though, if statistics from expensive private institutions might not show a reversal of the proportions of institutionalization of the native-born and the foreign-born. The only logical conclusion that could be drawn from Laughlin's incomplete data sample was that "it cannot be held that the occurrence of defects is actually so much greater in the foreign-born as the face of the present statistics seem to show." For Jennings, recent immigrants were not inferior in their inherited qualities, and he felt confident that a thorough study would show that it was the difficulties of their

¹⁴ See "Restriction of Immigration: Hearings before the Committee on Immigration and Naturalization, House of Representatives" 68th Congress, 1st Session, December 26, 27, 31, 1923, January 2-5, 7, 8, 10, 19, 1924 (Washington, D.C.: Government Printing Office, 1924), Statement of Herbert Spencer Jennings, 510-13.

physical surroundings that limited the ability of the immigrants to successfully adapt and develop.¹⁵

Jennings continued his critique of Laughlin's work in the new year. In the March 14, 1924 issue of *Science*, he again sought to undermine the importance Congress was attaching to Laughlin's statistical testimony. The Hopkins professor showed that the evidence that Laughlin provided to support his assertion that new immigrants were contributing an excess proportion to custodial institutions was wrong. Furthermore, Jennings pointed out that the shift in the census basis from 1910 to 1890 to compute immigration quotas, which Congress was then considering and which supporters argued would reduce the number of "institutional socially inadequates," would not have that effect at all. Though the races represented in custodial institutions would change – from southern to northern Europeans – the 1890 census would still yield essentially the same total number of defectives.¹⁶ This represented an essential point in Jennings's general arguments against classic racialism. The immigrants represented in the 1890 census would generate a similar number of defectives – although he explained that the type of defects would be slightly different – because the stresses of assimilation and environment were the same, regardless of the race of

¹⁵ Herbert Spencer Jennings, "'Undesirable Aliens': A Biologist's Examination of the Evidence Before Congress" *The Survey* v. 51 (15 December 1923), 309-12, 364; quote from 311.

¹⁶ Herbert Spencer Jennings, "Proportions of Defectives from the Northwest and Southeast of Europe" *Science* n.s. v. 59, n. 1524 (March 14, 1924), 256-7.

immigrants.¹⁷ It was not simply the racial character of the immigrants that made them defective and undesirable – it was also the environmental context.

Jennings's great contribution to the classic racialist critique in the 1920s came in the early fall of 1924, when the popular magazine *The Scientific Monthly* published his article "Heredity and Environment." The article showed the culmination of Jennings's researches in the biological development of organisms, and he later expanded this brief essay into book form.¹⁸ The public's "grotesquely inadequate and misleading" understanding of Mendelism, which was "still presented in the conventional biological gospels" and implied a "trivial role of the environment" compelled Jennings to write the lengthy article to correct these misperceptions. All organisms, he announced were subject to the limitations of what they were made of – their heredity – and the material conditions surrounding their development – their environment. He insisted that the development of all

¹⁷ For example, according to Jennings's computations, the number of insane inmates would increase 5.7%, dependent paupers would increase 58.1%, and epileptics would increase 3.0%, while the number of criminals would decrease 42.4%, and the feeble-minded population in custodial institutions would decrease 20.9%. *Ibid.*, 256-7.

¹⁸ Jennings published two books on the subject: *Prometheus, or Biology and the Advancement of Man* (1925) and *The Biological Basis of Human Nature* (1930). Both books were favorably reviewed. E. B. Reuter, a sociology professor at the University of Iowa, noted that the importance of *Prometheus* was "out of all proportion to its [small] size. Jennings shows the inadequacy of the genetic conceptions at the basis of the eugenic programs and other schemes of racial improvement." E. B. Reuter, review of *Prometheus, or Biology and the Advancement of Man* by Herbert Spencer Jennings in *The American Journal of Sociology* v. 31, n. 5 (March 1926), 692; E. B. Reuter, review of *The Biological Basis of Human Nature* by Herbert Spencer Jennings in *The American Journal of Sociology* v. 36, n. 3 (November 1930), 498; R. L. Duffus, "Modern Biological Science and the Future of the Race" review of *The Biological Basis of Human Nature* by Herbert Spencer Jennings in *The New York Times* 13 April 1930, 66. Duffus explained that a careful and close reading by the "intelligent layman" would help him "emerge well-armed against the fatalists, charlatans and fakers who of recent years have been twisting biological fact to bolster up a pseudo-scientific toryism."

organisms was equally shaped by these two factors.¹⁹ But as Jennings explained this process of development, one of the great problems within the new paradigmatic model became clear: the process was complex and difficult to demonstrate. In contrast, the declaration that “like produces like,” as Laughlin, Davenport, Grant and other classic racialists frequently proclaimed, held an alluring simplicity.

The basis of eugenics was wrong, and inheritance was not a physiological principle based on simple Mendelian ratios, Jennings explained. Instead, inheritance was directed by the “arrangement of the packets of chemicals” in cells and their methods of distribution within cells. In many instances of reproduction, “nothing resembling Mendelian inheritance” occurred. The vast majority of organisms have “biparental inheritance,” with every germ cell receiving two different groups of packets, which made accurately predicting development “more hazardous in this field than is sometimes represented.”²⁰ Not only was the organism’s environment important in shaping its development, the hereditary contribution of both familial lines was a significant factor as well. Accurate predictions of inheritance and development within this complex perspective was, therefore, nearly impossible. But in the face of the simplistic determinism of “like produces like,” an understanding of the vast complexities of genetic inheritance was difficult to achieve, let alone effectively communicate to a lay population.

¹⁹ Herbert Spencer Jennings, “Heredity and Environment” *The Scientific Monthly*, v. 19, n. 3 (September 1924), 225-38; 225.

²⁰ *Ibid.*, 227.

Through his laboratory work, Jennings aimed to make these complex rules of inheritance more comprehensible to both scientific and lay communities. His article explained that confidence in asserting the relative influence of environment and heredity could only be obtained by studying the ways embryos changed under microscopic examination, or by following the example of Thomas Hunt Morgan and interchanging the composition of the chemical packets and noting the results. It was the misinterpretation of Morgan's early work that led to the inaccurate doctrine of the "all-might of inheritance [that] is still proclaimed by the popularizers of biological science." The theory of "unit characters," that each characteristic of the organism was contained in a particular place, had been obliterated by scientific progress in genetics. Experimental evidence of the genetic complexity of eye color in fruit flies – the interaction of at least 50 genes, Jennings explained, produced the eye color – undermined the certainty with which eugenicists claimed heredity worked. Jennings insisted that "Neither eye color nor tallness nor feeble-mindedness, nor any other characteristic, is a unit character in any such sense. There is indeed no such thing as a 'unit character,' and it would be a step in advance if that expression should disappear."²¹ He continued, writing that, "Heredity is not the simple, hard-and-fast thing that old-fashioned Mendelism represented it [to be]."²² And Jennings pointed out the implications for this radical complexity of heredity beyond the laboratory as well.

²¹ *Ibid.*, 228.

²² *Ibid.*, 230. See also Carlson, *Mendel's Legacy*; Allen, *Morgan*.

Discussions of immigration problems showed this inaccurate oversimplicity in practice, he noted. The proportion of defective and diseased persons in the immigrant stream, Americans were told, was a result of unchangeable and inherited qualities that inferior immigrants would pass on to future generations. “There is no warrant in the science of genetics for such a statement,” Jennings confidently explained. A “great throwing about of false biology” had misled much of the public to fear the genetic makeup of the “new” immigrants, rather than try to understand how the environment and social factors also directed human development.²³

Jennings’s article elicited a great deal of support from the scientific community. H. F. Perkins, a professor in the Zoology Department at the University of Vermont wrote to Jennings that he found the article “a startling piece of criticism.” Perkins felt that Jennings had weakened the foundation of eugenics by exposing some of the experimental shortcomings of the field: “these [are] questions that now seem to me to be too radical to accept without a great deal of careful thought and considerably more evidence than I have seen brought forward yet.”²⁴ Davenport wrote from Cold Spring Harbor to offer congratulations on the paper, which he felt “quite opens up a new Epoch.”²⁵ Even Albert Wiggam – who told Jennings “yours is the best and most clean-cut discussion yet written

²³ *Ibid.*, 237.

²⁴ H. F. Perkins to Jennings, 22 October 1924, Jennings Papers.

²⁵ Davenport to Jennings, 30 October 1924, Jennings Papers.

[on problems in eugenics], especially from the genetical and embryological standpoint” – found the article instructive.²⁶

The mis-statements of biological knowledge made in support of classic racialism continued to irk Jennings, despite the positive reception of *The Scientific Monthly* article. In *Prometheus, or Biology and the Advancement of Man* (1925), he set out to demolish those “grossly misleading and inadequate” representations of Mendelism that were propounded in the “conventional biological gospels,” and was a more detailed empirical attack on classic racialism, driven by the same fundamental belief that heredity and environment were equally important in human development.²⁷ Jennings argued that the concept of “unit characters” was the single weakest scientific claim that the racialists advanced. With origins in Lamarck’s theory of the inheritance of acquired characteristics, its simple argument that development stemmed from the immutable germ plasm in each cell could no longer be held to be true.²⁸

²⁶ Wiggam to Jennings, 18 October 1924, Jennings Papers. See also Elazar Barkan, “Reevaluating Progressive Eugenics: Herbert Spencer Jennings and the 1924 Immigration Legislation” *Journal of the History of Biology* v. 24, n. 1 (Spring 1991), 91-112 in which Barkan describes Jennings’s activities as follows: “In the biological jungle of racism and xenophobia, with no other biologist speaking out to counter the racist propaganda advanced in the name of biology, Jennings’s testimony to Congress showed civic commitment and made him the standard-bearer of biological egalitarianism.” 98. Also important are Bentley Glass, “Geneticists Embattled: Their Stand Against Rampant Eugenics and Racism in America During the 1920s and 1930s” *Proceedings of the American Philosophical Society* v. 139 (1986), 130-54; Elazar Barkan, *The Retreat of Scientific Racism: Changing Concepts of Race in Britain and the United States Between the World Wars* (Cambridge: Cambridge University Press, 1992).

²⁷ Herbert Spencer Jennings, *Prometheus, or Biology and the Advancement of Man* (New York: E. P. Dutton and Co., 1925), 3; quotes from Jennings, “Heredity and Environment” 262.

²⁸ Jennings, *Prometheus*, 29 emphasis original. See also Paul, *Controlling Human Heredity: 1865 to the Present* (New Jersey: Humanities Press, 1995) and Hamilton Cravens, *The Triumph of Evolution: American Scientists and the Heredity-Environment Controversy, 1900-1941* (Philadelphia: University of Pennsylvania Press, 1978) for descriptions of the role of Lamarckianism in the development of evolutionary theory and eugenics.

But Jennings could not undermine the alluring simplicity of classic racialism. When he described the growth and development of organisms from “genes [that] are simply chemicals that enter in a great number of complex reactions, the final upshot of which is to produce the completed body,” and how “*the way [the chemicals] interact and what they produce depends on the conditions,*” he presented an argument far more scientifically accurate than one based unit characters, but one much more intellectually complex. In Jennings’s system of inheritance, chemical packets inherited from two parents – each with their own particular hereditary lineage – interacted specifically in relation to the environmental circumstances of development. “In these senses,” he explained, “all characteristics are hereditary and all are environmental, but no characteristic is exclusively hereditary or exclusively environmental.”²⁹ But how exactly one would document the impact of the environment on the development of organisms was challenging. As Boas had learned in his study on the descendants of immigrants for the Dillingham Commission, the causal link in changes of body form was difficult to pinpoint; Jennings found the problem persisting into the 1920s. The reality was, the Hopkins biologist felt, was that heredity and environment were coequal factors in determining the development of all organisms. How to communicate and disseminate this knowledge effectively, was a very different matter.

²⁹ Jennings, *Prometheus*, reactions from 29, emphasis original; all characters hereditary and environmental from 48.

Two articles written in 1926 illustrate Jennings's increasing frustration with the misconceptions of genetics and the biological sciences. Shortly after the publication of the Austrian researcher Paul Kammerer's book on midwife toads, which purported to document Lamarckian evolution, Jennings wrote another popular article to caution readers about the limitations of scientific knowledge, and, more importantly, to consider the credibility of authors and researchers in the field. Kammerer had published his controversial book supporting the inheritance of environmentally acquired characteristics in 1924. When the English scientist William Bateson confronted Kammerer regarding the evidence provided by the last-remaining physical specimen that the Austrian claimed demonstrated the inheritance of the character, an investigation led to charges of specimen tampering that Kammerer denied. Then Kammerer committed suicide in September 1926.³⁰ Jennings addressed this issue of "acquired characteristics" in an essay in *The Forum*, in which he insisted on recognizing and accepting the limitations of modern genetic knowledge. Research by Morgan on fruit flies, and R. A. Emerson on variations in corn revealed, he explained, hundreds of cases of environmental change compelling species variation. This suggested that perhaps there was some slight element of Lamarckianism in heredity, and an overall malleability in the

³⁰ Paul Kammerer, *The Inheritance of Acquired Characteristics* trans. A. Paul Maerker-Branded (New York: Boni and Liveright, 1924); see also Arthur Koestler, *The Case of the Midwife Toad* (London: Hutchinson Press, 1971); Sander Gilboff, "'Protoplasm...is the soft wax in our hands': Paul Kammerer and the Art of Biological Transformation" *Endeavor* v. 29, n. 4 (December 2006), 162-67; idem., "The Case of Paul Kammerer: Evolution and Experimentation in the Early Twentieth Century" *Journal of the History of Biology* v. 39, n. 3 (September 2006), 525-63.

genetic structure of organisms.³¹ The mechanism of transmission, however, was a mystery, and that required more knowledge, which could only be gained experimentally. Jennings also cautioned “ninety-nine per cent of the parent’s acquirements are not inherited by his descendents.”³² But near the end of article, he again trumpeted the achievements of experimental breeding, which showed “that a given stock, subjected for many generations to a changed environment, may become hereditarily altered, in structures, functions, or habits.”³³ Although this was essentially the argument Kammerer had been making, and amounted to an endorsement of what historian Sander Gliboff calls “old-school Darwinism,” no one still had any concrete proof of how the variations were transmitted.³⁴ But Jennings went on to describe the absolutely imperative element of proper scientific study: the credibility and reliability of the investigator.

This was the crux of much of Jennings’s critiques of the scientific work of biological and eugenic popularizers. Kammerer’s work was experimentally and methodologically highly problematic – in fact, one could consider Kammerer an Austrian version of Davenport, except that even Davenport critiqued the quality of Kammerer’s work and gossiped that British academics called the Austrian a

³¹ Herbert Spencer Jennings, “The Inheritance of Acquired Characters” *The Forum* v. 76 (November 1926), 702-11; also Carlson, *Mendel’s Legacy*. Jennings’s other article was from a speech he gave that will be discussed below.

³² Jennings, “The Inheritance of Acquired Characters” 708, 707. Jennings’s use of the singular male pronoun is curious considering the efforts he made to explain the limitations of “biparental inheritance.”

³³ *Ibid.*, 709.

³⁴ Gliboff, “The Case of Paul Kammerer,” 538.

“charlatan.”³⁵ Jennings urged that professional and reliable scientists try to confirm Kammerer’s work so that it could be “corroborated or refuted as soon as possible. If it is correct, it deserves the Nobel prize.”³⁶ The implicit point was that only careful, experimental study by trained and creditable professional scientific investigators could gather the clear, definite, and generally valid answers that nature declined to give. What the answers *were* was just as important to Jennings as *how* they were obtained. Jennings made this clear in a lengthy address in June 1926 to the crowd gathered for the dedication of the Whitman Laboratory of Experimental Zoology at the University of Chicago.

Jennings’s career arc intersected with a major revolution in scientific knowledge. Early in his academic career, Jennings told the crowd, zoological work was by and large descriptive – there was no real manipulation of specimens. From those qualitative descriptions far-reaching conclusions were drawn. Quickly, however, the trend changed to experimental work that nearly every zoologist, himself included, embraced. The point of this trend was that to establish truth claims, a scientist must have empirical, experimental and

³⁵ Charles B. Davenport to Irving Fisher, 6 December 1923. Regarding Kammerer’s work, Davenport pointed out that he had never met the Austrian scientist, although he had observed some of Kammerer’s experiments in 1909 and 1922. Davenport felt that “The reputation of the gentleman among geneticists in this country is not at all what could be desired. I mean that they feel that he has never presented proper evidence for his conclusions and one does not get the evidence even when he goes to Kammerer’s own laboratory. ... other Englishman regard him almost in the light of a charlatan.” In a letter eleven days later, Davenport curiously told Fisher that “The whole matter of charlatans in science is one of the most difficult to understand that there is. I have got glimpses of its meaning, however, in the course of thirty years of connection with scientists.” Davenport to Fisher, 17 December 1923, both Davenport papers. The irony, of course, is that many of the same criticisms were levied at Davenport. See, e.g. Boas to Hrdlicka, 18 March 1919, Hrdlicka Papers; Jennings to “Jessica” [Mary Louise Burridge], 10 February 1895; Jennings to “Jessica” 24 February 1895, both in Jennings Papers.

³⁶ Jennings, “Inheritance of Acquired Characters,” 711.

quantitative proof. For Jennings it was critical that “We must be able to say: Such and such things happen under such and such conditions, and if you don’t believe it you may supply the conditions, you may try it for yourself, and you will find it to be true.”³⁷ The ability to reproduce experimental results and verify conclusions was one of the essential principles of the scientific method, as it was the only way that scientists could be confident in the accuracy of scientific knowledge. In this context, the reputation and conscientiousness of the researcher in laboratory or experimental work was crucial. Often, Jennings explained, incautious scientists – though he did not name them perhaps he had in mind Kammerer, Davenport, Laughlin and Goddard – were beguiled by coincidental associations and connections that could be attributed to several causes, such as selective elimination and selective persistence that occur on a genetic level, but which gave rise to “the teleological fantasies of biology.”³⁸ The inability to replicate experimental results in equivalent controlled circumstances would seriously undermine the confidence to be placed in the experiments, precisely what had happened to Kammerer’s toads.

In addition to the printed critiques and public lectures he gave throughout 1925 and 1926, Jennings tried to combat these biological misunderstandings and press the importance of knowledge gained under experimental conditions. In

³⁷ Herbert Spencer Jennings, “Biology and Experimentation” *Science* n.s. v. 64, n. 1648 (July 30, 1926), 97-105; address given 4 June 1926. Quote from 98. Experimentalism could also generate contrary results, showing that general principles that may seem obvious were wrong.

³⁸ *Ibid.*, 104. Selective persistence may include, for example, Kammerer’s midwife toad’s nuptial pads, but the genetic basis of the transmission was not proven. The “teleological fantasies” were the nexus of both the American eugenics movement and the Nazi’s “Final Solution” that mankind could be scientifically purified to something approaching perfection.

mid-March 1926 Jennings returned to the University of Pennsylvania, where he had spent a few years teaching early in his career, to give what he called “An Unpopular Talk.” The lengthy address demonstrated the frustration that Jennings felt over public statements of scientific knowledge by non-academics. He lectured the crowd that the way that science and biology was “proclaimed in the headlines” to advocate eugenics and Nordic supremacy was a grave problem that scientists had to take more seriously. In particular, Jennings singled out Albert Wiggam’s book, *The New Decalogue of Science* as a typical example of “what is being received as the latest and final conclusions of biology.”³⁹ The book claimed, Jennings said, that mankind was degenerating, that heredity alone and not the environment determined development in man, that human differences were inherent in germ cells, that social class was a function of nature and inheritance, and that the melting pot was an untenable, biologically wrong fantasy; in short, Wiggam laid out the orthodox classic racist position. Wiggam used the “supposed present state of the knowledge of genetics” to settle matters of state policy on immigration, criminology, children’s education, hygiene and social services, and marriage.⁴⁰

It was pure fantasy. Jennings became “a bit disturbed,” he explained, when he looked at the “radical doctrines served up to us in the name of biological

³⁹ Herbert Spencer Jennings, Lecture n. 37, “Some Unsettled and Unsettling Questions in Biological Science: An Unpopular Talk” unpublished mss., delivered at the University of Pennsylvania, 11 March 1926, Jennings Papers, “Lectures,” APS, 13, 15. See also Wiggam, *The Fruit of the Family Tree* and *The New Decalogue of Science*, discussed in chapter 6.

⁴⁰ *Ibid.*, 17-18, 20.

science.”⁴¹ He offered a brief history of biological science through his own experience to show how inaccurate were the claims made by Wiggam and his ilk. The generation after Darwin – convinced of the correctness of the evolutionary hypothesis – tried to work out the details of evolution by studying developments and changes in groups of animals over geologic time. Around 1890, the same time that Jennings embarked on his professional academic career at the University of Michigan, the evolutionary hypothesis began to waver as investigators disputed the varying courses of evolution for any particular group of organisms.

For Jennings, when he began his studies in zoology, such a realization was “very distressing,” as it made him realize that “science progresses like the rat in the maze, by trial and error.”⁴² The only solution was to base biological knowledge on careful experimental study. Biology must be studied in a way in which theories can be proven or disproven; “We must find out things that we can show people: we must be able to say to doubters—you try it yourself and you will see that it’s true.”⁴³ This was the fatal flaw in the work presented by Wiggam, Davenport and Laughlin and others. There was no experimental basis of their knowledge that could be tested and verified. Their knowledge claims were based on assumptions – Jennings told how they were “very largely determined by their

⁴¹ *Ibid.*, 26.

⁴² *Ibid.*, 31-32. This is exactly the impetus toward radical changes in scientific paradigms that Kuhn explains: “Discovery [of new paradigms] commences with the awareness of anomaly, i.e. with the recognition that nature somehow was violated the paradigm-induced expectations that govern normal science. It then continues with a more or less extended exploration of the area of anomaly. And it closes only when the paradigm theory has been adjusted so that the anomalous has become the expected.” Kuhn, *Scientific Revolutions*, 52-3.

⁴³ Jennings, “Some Unsettled and Unsettling Questions,” 37. Emphasis original.

general views in certain matters that may be called philosophical” – that were scientifically and empirically untenable.⁴⁴ As he stood over his laboratory tables conducting experiments on his little Neapolitan paramecia at the Naples Zoological Station around the turn of the century, Jennings came to realize that even simple-celled organisms behaved and developed in complex ways, just as dogs, cats, turtles and squirrels did. Jennings concluded the “Unpopular Talk” by advocating “a healthy skepticism as to the finality of the immediate doctrines just at present prevailing in any science.”⁴⁵ That dramatic changes in government policy were being contemplated based on these truth claims troubled Jennings tremendously. He urged his audience to take the truth claims of scientific knowledge with a grain of salt.

Jennings felt that the way that supporters of restriction and eugenics used false biology to rationalize the changes in government policy and their own social prejudices had to be addressed. As a teacher, he could not let stand this sloppy and careless work. Classic racialists and their influence over the public’s and politicians’ understanding of heredity had to be corrected. For a practicing researcher, the declarations of Laughlin and Wiggam on the state of genetics was fundamentally wrong. They were not scientists and had no authority or expertise to speak on it. At the end of 1926, as Jennings prepared to retire from the chairmanship of the Zoological Section of the American Association for the Advancement of Science, he addressed his colleagues and again emphasized the

⁴⁴ *Ibid.*, 53.

⁴⁵ *Ibid.*, 67.

importance of experimentalism as the basis for all scientific knowledge. Since he was speaking to a learned audience, Jennings framed his examples in more esoteric terms than his Philadelphia address from the spring. But the underlying message was the same: observation and experiment “are the primary and the final methods of science, never to be laid aside. They are *the* methods for learning of the universe.”⁴⁶ Deductive reasoning could not prove theories of evolution, and by extension of eugenics and heredity. “When the reasoned conclusion conflicts with experiment, it is the reasoned conclusion that must give way.” Jennings explained how “Herbert Spencer’s tragedy of the deduction killed by an observed fact is as typical and necessary an event in science as is the death of the unfit in the evolution of organisms.”⁴⁷

Pearl shared the frustration of his friend and former teacher, and put to paper for public consumption many of the issues he had raised privately to Greenwood and Jennings. In November 1927 Pearl published an article that took on the idea of the racial hierarchies that lay at the core of nativist support for immigration restriction, and strongly denied the accuracy of Galton’s work in pedigree analysis and eugenics. Pearl explained that the Danish geneticist Wilhelm Johannsen had, like Mendel, used beans to test for a vector of inheritance, and like Pearl, had found great difficulty in accurately predicting the characters of progeny. Referencing his own Orono chickens and the literature on

⁴⁶ Herbert Spencer Jennings, “Diverse Doctrines of Evolution, Their Relation to the Practice of Science and of Life,” *Science*, n.s. v. 65, n. 1672 (14 January 1927), 19-25; 21, emphasis in original. The address was given on December 28, 1926.

⁴⁷ *Ibid.*, 21.

inheritance in animal breeds, Pearl asserted that “there was no evidence whatever that these [superior] breeds had been produced by the method of gradually accumulating small superior bodily variations by continued selection.”⁴⁸ The impossibility of accurately predicting the characteristics of offspring totally undermined the feasibility of eugenics. Pearl noted the methodological and empirical flaws in the research behind eugenic claims, and described “the ill-advised zeal with which some of its more ardent devotees have assigned such complex and heterogeneous phenomena as poverty, insanity, crime, prostitution, cancer, etc.” that was bringing the movement into “disrepute.”⁴⁹ This heterogeneity was becoming, for the professional scientific community, increasingly significant in tempering expectations of eugenics. The simplistic claims of the classic racist position that “like produces like” – Supreme Court Justice Oliver Wendell Holmes Jr. had only that May issued his famous opinion that “three generations of imbeciles are enough” in *Buck v. Bell* – were grossly inaccurate.⁵⁰

By publishing his criticisms in his friend H. L. Mencken’s *American Mercury*, Pearl hoped to reach a large audience and remedy the misconceptions of eugenics, which he described as “a mingled mess of ill-grounded and uncritical sociology, economics, anthropology, and politics...solemnly put forth as science,

⁴⁸ Raymond Pearl, “The Biology of Superiority” *The American Mercury* vol. 12, n. 47 (November 1927), 257-66; 259.

⁴⁹ Pearl, “The Biology of Superiority,” 260.

⁵⁰ On Carrie Buck, see Stephen Jay Gould, *The Flamingo’s Smile: Reflections in Natural History* (NY: W W Norton, 1985); idem., *Mismeasure of Man*; Paul Lombardo, “Facing Carrie Buck” *The Hastings Center Report* v. 33, n. 2 (March-April, 2003), 14-17.

and unfortunately accepted as such by the general public.”⁵¹ This was the essential problem. Not only were the scientific theories underpinning eugenics untenable in light of developments in genetics, they were also used to support and legitimize social prejudice. Pearl regarded “superiority” as a relative term, not a scientific one – it always seemed to mean “a. ‘*My* kind of people,’ or b. ‘People whom *I* happen to like.’” This was nonsense. “The Italians,” he noted, “are proud of themselves, of their history, and of their ancestry, noble in its achievements; but the new existing immigration law of the United States attests that they are an undesirable, and therefore by implication, inferior race.” This was a “profound fallacy” that science had shown to be wholly incorrect. Pearl noted that correspondence between Galton and Darwin in 1872 and 1873 showed that even Darwin was profoundly unsettled by determining who was superior, and Thomas Morgan’s *Evolution and Genetics* (1925) similarly cautioned against deciding the genetic superiority or inferiority of whole races.⁵² But although science had exposed this “profound fallacy,” it retained great purchase in the public mind.

Pearl closed his article with an important warning: “Certainly modern genetics gives no support to the view that the somatic characteristics of the offspring can be predicted from a knowledge of the somatic characters of the parents. In preaching as they do, that like produces like, and that therefore superior people will have superior children, and inferior people will have inferior

⁵¹ Pearl, “The Biology of Superiority”, 260.

⁵² *Ibid.*, 261; Morgan quoted on 262.

children, the orthodox eugenicists are going contrary to the best established facts of genetical science, and are, in the long run, doing their cause harm.”⁵³ Yet maddeningly to professional biologists, this did not matter. As the Congressional debates over national origins and immigration quotas showed, the theories of orthodox eugenicists held much more sway in the public imagination.

The difficulty of navigating between the outdated, scientifically untenable classic racialism and the ambiguities and complexities of modern genetic and biological science can be seen in Edwin Conklin’s troubles in the late 1920s. In March 1928, Conklin began talking privately with Pearl about the over-emphasis being placed on inheritance by lay writers. Conklin was unable to take a decisive stand on the hereditary aspects of personality. The Princeton professor felt that mental capacity was indeed fixed by heredity, and the rediscovery of the Mendelian ratios of inheritance that were unknown to Galton seemed to him to reaffirm the Galtonian theory of the inheritance of genius. Certainly, Conklin conceded, “great men have come from unknown parents and unknown ones from great parents,” but he attributed that to random combinations of hereditary factors rather than the impact of the environment.⁵⁴ Still, in a second note to Pearl sent shortly after the first, Conklin also affirmed the importance of environment, and

⁵³ *Ibid.*, 266.

⁵⁴ Conklin to Pearl, 27 March 1928, box 18, Conklin papers, Series II. The obvious question here was could not the eminent offspring of eminent persons also be attributed to the exact same random combinations?

in his professional relationships tried to remind his colleagues to consider environmental influences as an integral aspect of development.⁵⁵

Unable to sort out in his mind clearly how inheritance worked, and what were the mechanisms and consequences of inheritance, Conklin felt it necessary to chasten some of his associates on the limitations of scientific knowledge of heredity. His speech to the small audience at the Galton Society in May 1928 was rather dramatic in its frankness. Certain critics of eugenics could be ignored, he felt, as their objections tended to the sentimental or philosophical implications of the manipulation of human evolution. But, Conklin said, the criticism of “leading students of heredity” had to be taken seriously. William Bateson, Cold Spring Harbor’s own W. E. Castle, Thomas Hunt Morgan, Jennings and Pearl were all, in some form or another, critiquing the principles or programs of eugenics in the 1920s. Conklin quickly glossed over the issues raised by Bateson, Castle and Morgan. It was Jennings and Pearl that worried him most because of their professional status and their contacts with many of the Galton Society’s members.⁵⁶

⁵⁵ Conklin to Pearl, 29 March 1928, box 18, Conklin papers. In this note, Conklin said that Galton’s theory of ancestral inheritance and Mendel’s law were thoroughly compatible, the former explaining the average results, the latter the individual peculiarities. He also wrote that among organisms man alone was affected by the physical, social, and intellectual environments that were crucial for development, and these were an obvious vector to absorb the experience of former generations.

⁵⁶ Edwin G. Conklin, “Some Recent Criticisms of Eugenics”, *The Eugenical News* v. 13, n. 5 (May 1928), 1, 61-66. The English eugenicist William Bateson read a paper to the Eugenical Society of Great Britain that accepted the ideological principles of eugenics, but cautioned advocates of sterilization because the role that heredity and environment played in human development had not been decisively determined. Castle’s entry in the thirteenth edition of *The Encyclopedia Britannica* on eugenics was tentative in describing the viability of the self-direction of human evolution, and Morgan’s second edition of *Evolution and Genetics* added a chapter on “Human Inheritance” that was critical of simplistic eugenic claims. See also Glass, “Geneticists

The personal and professional relationships Conklin had with Jennings and Pearl impressed upon him the importance of modesty in making scientific claims, and the significance of their reservations about eugenics. Conklin's own respect for Jennings was evident in the seriousness with which the Princeton geneticist approached the criticism that Jennings was making in public. Conklin told the Galton Society audience that *Prometheus* "severely criticizes" the eugenics program on several levels, particularly pointing out that complexity in inheritance superceded the simplistic Mendelism many eugenicists held to be true; that the biparental inheritance of all individuals made impossible the successful prediction of individual characteristics; and that the coupling of unregulated immigration with American racial degradation was simply incorrect.⁵⁷ Pearl's work was also demonstrating effectively that the principles of eugenics were increasingly anachronistic in the face of scientific research. It would behoove members of the Galton Society to be more cautious in endorsing practical applications of eugenic principles, Conklin said, in light of these serious and justifiable criticisms.⁵⁸

Later in the year, Conklin's doubts again surfaced when he was approached by Frederick Osborn, the son of the director of the New York Museum of Natural History, Henry Fairfield Osborn, about becoming involved in the eugenics movement. The younger Osborn showed a caution and reflection that

Embattled"; Barkan, "Jennings and Progressive Eugenics"; Barkan, *The Retreat of Scientific Racism*; Mezzano, "Progressive Origins of Eugenics Critics."

⁵⁷ Conklin, "Some Recent Criticisms," 62.

⁵⁸ *Ibid.*, 64.

was often absent in his father's writings and lectures, and felt that Conklin's advice, since he specialized in genetic inheritance, would help guide him. Eugenics research, as Frederick saw it, cleaved into two fields, popular education or propaganda, and the stimulation of genetic research. The slow progress made in research in human heredity limited – or should limit – the public education aspect, Frederick felt, and he asked if Conklin thought it advisable to become involved with a committee of the Eugenics Research Association that had as members Davenport, Laughlin, Dr. Clark Wissler, Grant and Clarence Campbell?⁵⁹

Conklin's response typifies the difficulty facing researchers in human heredity as they struggled to adapt to the new experimentalist paradigm. Lower organisms, whose brief generational lines facilitated research in physical inheritance, were not helpful in illuminating distinctively human traits like intellect, social instincts and behavior. Statistical analyses of inheritance, like those performed by Pearson and his students, were useful in determining patterns of inheritance in large populations, but were inadequate to the task of determining the specific vectors of inheritance, or how traits were inherited. Regarding American research on inheritance, Conklin was moderately critical. Davenport's institution was performing some excellent work in statistical and detailed analytical studies in human heredity, but the Cold Spring Harbor researcher "has been too ready to decide whether a certain trait is a Mendelian dominant or recessive on the basis of too small a number of instances." Davenport's conception of Mendelian inheritance was problematic because many of the traits

⁵⁹ Frederick Osborn to Edwin G. Conklin, 23 September 1928, box 17, Conklin papers.

that he treated as simple Mendelian characters were, in reality, due to the interplay of many different factors. Davenport's "over-enthusiasm" for Mendelism had led to "severe criticism" from professional researchers – the point he made to the Galton Society in May – although Conklin felt that his research, in the main, was "sound and very useful." But Davenport sometimes went too far, as in his book on thalassophilia, or in his insistence of hereditary criminality. Though he considered Davenport to be a "real scientist," Conklin felt that "his judgment is sometimes at fault" and his positions were sometimes "contrary to human experience, and common sense."⁶⁰

Conklin told Frederick that Laughlin, similarly, was a careful statistical worker whose service to the House Committee on Immigration was "most valuable." Yet Conklin pointed out the difficulty in drawing sound conclusions from purely statistical data, which led to the sharp criticism – like that from Jennings – of Laughlin's arguments on the worth of different racial stocks in the country. On the whole, Conklin regarded the Expert Eugenics Agent as a real

⁶⁰ Conklin to Osborn, 24 October 1928, box 17, Conklin papers. Davenport's book *Naval Officers* suggested that a specific hereditary disposition inherited from sea-faring ancestors – thalassophilia, or love of the sea – was the primary factor in creating effective and successful naval officers. One could safely assume here that Conklin had in mind *Naval Officers* as one instance where Davenport's judgment ran against common sense. Davenport's position on criminality can be found in an undated lecture, "The Factors of Heredity and Environment in Criminality" Davenport Papers, Lectures. He noted that "the wide spread existence of crime enforces the lessons of eugenics. We are breeding too many people with feeble inhibitions and without proper social instincts; persons who have a tendency toward periodic outbreaks of temper, with a tendency to assaults; persons who are liable to periodic bad behavior, including those associated with the epileptic state; persons who are introverts, selfish and self-seeking. True progress will be made only when we understand how those with criminalistic makeup are bred and try to prevent such breeding by segregation or sterilization. If we permit them to be bred, then we must apply such special treatment as will prevent their behavior from disorganizing society." 10-11. This was exactly as Lombroso had detailed, that criminal tendencies were a biological imperative. See chapter 1.

scientist as well, whose work was reliable, and his conclusions sound, “though possibly somewhat warped in judgment by preconceived notions.” Wissler’s expertise in ethnography was beyond Conklin’s ken, so he felt unable to express any opinion on his particular contributions to eugenics research. Madison Grant, however, was another matter entirely.⁶¹

Conklin’s criticism of Grant cut to the core of the objections that Pearl and Jennings had been making. “Madison Grant is not strictly speaking a scientist,” Conklin stated, “and many scientists have taken a shot at his book, ‘The Passing of the Great Race.’” The book, he continued, was laced with “certain prejudices which any strictly scientific man should avoid,” and professional scientists disputed many of his statements and conclusions. He told the young Osborn “while I regard him as a fine gentleman of high ideals, I do not think that he is a scientist or that his conclusions can be relied upon as being thoroughly sound.”⁶² But why was Grant’s “prejudice” different from Laughlin’s “preconceived notions”? Conklin offered no explanation, other than pointing out Grant’s

⁶¹ Conklin to Osborn, 24 October 1928; see also Haskell, *Objectivity is Not Neutrality*.

⁶² Conklin to Osborn, 24 October 1928, Conklin papers, box 17. An undated pamphlet in the papers of geographer Ellsworth Huntington encapsulates the attitudes and opinions of eugenics of a number of researchers. H. H. Goddard noted that anyone who looked about and saw “the mass of incompetents living in poverty, disease, filth, and squalor,” and who understood the hereditary basis of these characteristics could not fail to support eugenics. Edward M. East, a genetics professor affiliated with the Bussey Institution at Harvard University expressed sadness that although so much attention was given to improving animal breeds, very little was given to the improvement of the human race, while William Castle at Harvard endorsed the abstract ideal of eugenics while insisting the basis of practical knowledge was very unstable. Hrdlicka echoed this position. Madison Grant and Harry Laughlin both affirmed the importance of applying eugenic principles to society, Laughlin characterizing “the conscious self-direction of human evolution” as both “a science for fundamental research and a technical art for practical use.” Californian Paul Popenoe, whose book *Applied Eugenics* became the standard textbook in college courses on eugenics and who advocated public education as the necessary next step in advancing the eugenics movement was also given space to express his thoughts about the field. “What I Think About Eugenics” n.d. Ellsworth Huntington Papers, Subject File: Group I, Series IV, box 31, Special Collections, Yale University Library, New Haven, CT.

“gentlemanly” status. Laughlin, at least, had a position at a research institution, if the Eugenics Record Office could be so considered.

This is a significant instance of the hardening division between amateur, gentlemanly science and professional laboratory experimentalism. The historian Steven Shapin has examined this trend in seventeenth-century England, where the social status, trustworthiness, and credibility of researchers was imperative for assessing their truth claims and scientific testimony. His observation on the shifting scientific culture in England could be, in many ways, applied to the growing distinctions in this period between lay-writers and professional scientists: “a characteristic mark of the English natural-philosophical enterprise was its vigilant protection of the factual domain combined with injunctions to speak modestly, diffidently, and doubtingly about the domain of the theoretical.”⁶³ Clearly, writers like Grant, Stoddard, Wiggam, and many others in this period did not speak modestly or doubtingly about the theoretical potential of the infant scientific of genetics; the published works of Jennings, Boas, Hrdlicka and Pearl all express this tentativeness about the basis of their scientific truth claims. Shapin concludes that premodern society, because it was intimate and managed its affairs in essential face-to-face encounters, was able to carefully assess the veracity of the speakers on scientific and intellectual matters. The modern system of inquiry, however, is unable to do that, and therefore knowledge must be guaranteed and affirmed by reference to the *expertise* of the writer, not to their virtue.⁶⁴ Shapin’s

⁶³ Stephen Shapin, *A Social History of Truth*, 125.

thesis is quite perceptive, and like Kuhn's work on radical paradigmatic shifts in the structures of scientific knowledge, there are more nuances at deeper levels. That Conklin felt the need to caution Frederick Osborn about the arguments Grant made in racial anthropology by emphasizing his "gentlemanly" social position, which implicitly described him as a *non-expert*, demonstrates that Shapin's thesis continues to resonate into the modern period.

In closing his reply to the younger Osborn, Conklin felt that the presence of one or two academic scientists on the committee Frederick was considering joining would be much preferred to its composition at present. Particularly useful would be students of heredity in lower forms of life, whose research and experimental expertise, Conklin advised, would provide a critical point of view of eugenics, and would thus help the work of the Eugenics Research Association avoid being classified as "mere propaganda." Conklin believed that the field had great potential and tremendous opportunities, so he did not want to dissuade Osborn from becoming involved in the movement. "But above all things," he wrote, "I should wish that the truth and nothing but the truth should be the aim of the Association and of its committees, and for this reason I should be glad to see some more critical geneticists associated with the Committee."⁶⁵

But even this critical pursuit of scientific truth was complex; it was not solely a division between amateurs and professionals. Distinguished anthropologists also battled over the way scientific information was obtained, as

⁶⁴ *Ibid.*, 410-12.

⁶⁵ Conklin to Osborn, 24 October 1928, Conklin papers, box 17.

an example from the mid-1920s shows. Roland Dixon, the curator of Harvard University's Peabody Museum, sparred with Ales Hrdlicka in early 1925 over the use of anthropological measurements that Hrdlicka controlled out of the Smithsonian. In 1923 Dixon had published *The Racial History of Man*, in which he attempted, like Ripley before him, to identify and classify the various races inhabiting the earth, but with greater scientific precision than the Harvard economist did in *The Races of Man*. The book was an experiment in method, and Dixon compiled data sets of measurements of breadth and height of human crania along with nasal indices, the most universally taken measurements in anthropometric studies. Dixon noted that physical anthropologists with backgrounds in biology and biometry had slightly different methods of measurement, but cranial measurements along length-breadth, length-height and nasal index were generally universally orthodox. These measurements generated twenty-seven possible combinations of racial measurements utilizing these three basic indices.⁶⁶

Dixon's claims became problematic in the associations he drew from these twenty-seven combinations, which he perceived to be characteristic of specific racial groups. Louis Sullivan, who reviewed *The Racial History of Man* for *American Anthropologist*, pointed out the extreme caution that should be used in categorizing population groups by physical measurement. Sullivan, a physical

⁶⁶ Roland Dixon, *The Racial History of Man* (New York: Charles Scribner's Sons, 1923). Physical anthropologists with backgrounds in biology typically analyzed the measurements of individuals to moderate statistical series of averages, standard deviations, and correlation coefficients to improve their accuracy, whereas biometric anthropologists took the statistical measurements as the essential base, with no mediation from individual analysis.

anthropologist trained at Columbia, singled out Dixon's assumption of the immutability of body form – on which his schema rested – as highly problematic.⁶⁷ Sergi had proposed a schema like this, based on a permanent skeletal form that create two great racial groups; Boas had undermined it with his study of immigrant children. Sullivan pointed out that the Peabody curator “argues that the three characters he has chosen, are immutable and that others which disturb his schema are superficial and malleable. While it is conceivable and probable that some characters change rapidly and others remain stable for a long time it is probably true that these have varied from time to time and from group to group so that no generalization in a group so diverse as man is possible.”⁶⁸ The implication of this was that no solid associations of population could be imagined as “permanent.” This would also have clear implications for the Congressional debates on immigration restriction occurring at the same time, as it undermined their entire premise of fixed races of immigrants.

Sullivan's experience researching in the Pacific islands suggested other serious problems with Dixon's hypothesis, in terms of the homogeneity of

⁶⁷ Louis R. Sullivan, review of *The Racial History of Man* by Roland Dixon, *American Anthropologist* n.s. v. 25, n. 3 (July-September 1923), 406-12. Sullivan's biography can be found in “Sullivan, Louis R.” *Dictionary of American Biography* ed. Dumas Malone (New York: Charles Scribner's Sons, 1936), v. 18, 197-98; E. A. Hooton, “Louis Robert Sullivan” *American Anthropologist* n.s. v. 27, n. 2 (April 1925), 357-58; Charles B. Davenport, William K. Gregory, “Minute on the Death of Louis R. Sullivan” *Science* n.s. v. 62, n. 1617 (December 25, 1925), 83.

⁶⁸ Sullivan, review of *The Racial History of Man*, 409. Dixon's nomenclature bothered Sullivan, because had he used structural symbols to designate population groups – “BOP” to signify a measurement of brachycephalic depth, orthocephalic length, platyrrhine nasal index – rather than geographical designations that Dixon chose to use (e.g. Caspian, Mediterranean, Proto-Negroid, Proto-Australian, Ural, Palea-Alpine, Mongoloid), Sullivan felt Dixon could have avoided stirring racial sensibilities. See 411.

population groups, and Dixon's decision not to use structural symbols to designate the groups he derived from combinations of the three measurements. While attempting a racial history of all population groups and his novel approach to associations between them were, to Sullivan, admirable attempts that generated "a truer picture of racial history...than any previous worker," he was unable to give a wholly positive review to *Racial History*. Yet in comparison to the reviews penned by Boas and Hrdlicka, Sullivan was incredibly generous.⁶⁹

Certainly Boas was qualified to discuss elements of physical anthropology and racial types after his important researches for the Dillingham Commission. He pointed out that Dixon's measurements were based on classical morphological measurements, three of the head and two of the nose, which required the belief that these measurements had remained stable since Paleolithic times. Boas used his own measurements that documented the plasticity of body form of descendants of immigrants to demolish Dixon's assumptions. The second hypothesis of *Racial History*, Boas explained, required acceptance that all his measurements could be grouped by the extreme poles of his three measurements, so that there were only eight possible fundamental races that represented the eight possible combinations of the three measurements. No biological evidence had been provided to support this assertion, and nothing to contradict or overturn

⁶⁹ *Ibid.*, 411-12. Another review in *The Geographical Journal* similarly was appreciative of the difficulty that Dixon had tried to overcome, but also cautioned uncritical acceptance of Dixon's theories. A. C. H., review of *The Racial History of Man* by Roland Dixon, in *The Geographical Journal*, v. 62, n. 3 (September 1923), 228-229.

Boas's own researches had been provided.⁷⁰ Yet Dixon maintained that body types of his eight racial groups were fixed and stable.

Boas pointed out, in sharply critical language, that when Dixon addressed German claims for racial preeminence he flatly contradicted himself. Dixon had posited not only that physical form was stable, but that certain racial groups were essentially "pure" races, and this, in turn, explained the notable achievements of national or ethnic groups derived from these races. It was, essentially, an expression of Galton's theory of ancestral inheritance writ large. Boas, who had emigrated from Germany to the United States in 1887, would have none of it. He explained that "when it suits the author's emotional attitude he changes his argument completely and indulges in flings at the assumed claim of racial preeminence on the part of the Germans—an attitude which hardly helps make convincing a treatise that attempts to be scientific."⁷¹

Hrdlicka was similarly unimpressed with Dixon's foray into anthropological and racial typologies. His review in *The American Historical Review* was critical of *The Racial History of Man*, although he took pains to avoid being overly negative out of professional respect for Dixon's work in linguistics and ethnology. But when it came to physical anthropology of racial types,

⁷⁰ Franz Boas, review of *The Racial History of Man* by Roland Dixon, in *Science*, n.s., v. 57, n. 1481 (May 18, 1923), 587-90; 587. On the genetic level, Boas also found fault with Dixon's racial typologies. The measurements that Dixon based his racial groupings upon should have been analyzed in the Mendelian structure of dominant and recessive genes, which would have shown that, in accord with most professional scientific thinking inheritance tended to reversion of parental type. But Dixon assumed a crossing or blending pattern of inheritance, which enabled for an averaging of type, generating wide varieties of crossings. In addition, echoing Davenport's work on sailors, and extending back to Galton, Dixon saw mental achievements as proving the heredity of intellectual ability, which Boas the cultural anthropologist strongly opposed. 588.

⁷¹ *Ibid.*, 589-90; Dixon, *The Racial History of Man*.

Hrdlicka could not help feeling that the Harvard man was out of his league. “In this latter field,” Hrdlicka noted, “he has the very great disadvantage of a lack of personal observations and therefore [was] obliged to depend largely on the records of others,” just as William Ripley was in *The Races of Europe*. (This problem in Dixon’s work would inspire a hostile exchange between Hrdlicka and Dixon in the ensuing years.) His reliance on second-hand measurements, certainly no foundation to build sweeping generalizations of racial typologies, was a fundamental, almost untenable weakness of *Racial History*.⁷² Though he took no joy in exposing Dixon’s work as scientifically sloppy and erroneous, Hrdlicka could not let the major problems in the work go unaddressed.

First, there was the obvious problem in utilizing second-hand evidence, of accuracy of measurement, which, along with problematic and hasty generalizations reflected the wider critique by the professional community of work done by Laughlin, Grant and Wiggam. As many in the professional American anthropological community had learned in the wake of the U.S. Army’s mental and physical measurements of soldiers during World War I, uniform measurements were exceedingly difficult to obtain outside of tightly and highly controlled experimental conditions.⁷³ Using a small group of features or measurements as a basis of generalization increased the problematic nature of creating and organizing racial communities. Hrdlicka stated that professional

⁷² Ales Hrdlicka, review of *The Racial History of Man* by Roland Dixon, in *The American Historical Review*, v. 28, n. 4 (July 1923), 723-26; 723.

⁷³ As an example, see Charles B. Davenport and Albert Love, *The Medical Department of the United States Army in the World War: Vol. XV, Statistics, Part I: Army Anthropology* (Washington, D.C.: Government Printing Office, 1921).

anthropologists had all largely consigned the “fetishistic old-time value” of the cephalic index from the nineteenth-century, height indices of the skull and the nasal index to the dustbin of history. He explained that his own work on Native Americans had proven to him that even in isolated population groups under normal circumstances all characters, including cranial and nasal indices, possessed a wide range of variation.⁷⁴

Ultimately, Dixon’s project was relatively worthless as an anthropological text in Hrdlicka’s mind. For not only was the metrical basis of the generalizations highly problematic, but Dixon made no serious attempt to explain how his eight “‘fundamental’ types have originated, or how it comes that the cephalic, the unstable height-length, and the very changeable nasal index are preserved or reappear intact, regardless of admixture, time, or change of environment.”⁷⁵ As Hrdlicka would soon learn, however, it was very difficult to provide solid measurements to base racial typologies on; Dixon would have his revenge on Hrdlicka, when the latter published a catalog from the Smithsonian Institution’s collections of human crania.

Great hopes had been placed on the Smithsonian’s publication as early as 1922, when Dixon posted a notice to the readers of *Science* that the Smithsonian’s forthcoming efforts to publish measurements from its extensive holdings – a project that was being guided by Hrdlicka – should ideally be followed by similar projects from other organizations with the goal of furthering progress in American

⁷⁴ Hrdlicka, review of *The Racial History of Man*, 723.

⁷⁵ *Ibid.*, 724.

physical anthropology.⁷⁶ When the formidably titled *Catalogue of Human Crania in the United States National Museum Collections: The Eskimo, Alaska and Related Indians, Northeastern Asiatics* was issued by the Government Printing Office in *Proceedings of the United States National Museum* in 1924, Louis Sullivan secured the review in American Anthropological Association's journal. Like Dixon, Sullivan expressed great enthusiasm for the project and hoped that other institutions would follow the National Museum's lead in publishing the measurements of human crania in their collections. But Sullivan felt compelled to include an "adverse comment," unable to let pass serious flaws in the "notable contribution by one of the foremost anthropologists of the world." Sullivan regarded the published measurements as extraordinarily inadequate, which dramatically undercut the usefulness of the catalog.⁷⁷

Sullivan explained that previous publications of similar comparative cranial measurements were flawed because of the dearth or peculiarity of measurements provided. This included such notable classic craniometric work by Virchow, Quatrefages, Davis, Morton, Otis, and others. These men, however, had an excuse in that they published their studies and catalogs when craniometry was in its infancy in the mid- to late-nineteenth century. Hrdlicka had no such excuse.

⁷⁶ Roland Dixon, "Measurements of Human Crania" *Science* n.s. v. 56, n. 1450 (October 19, 1922), 418.

⁷⁷ Louis Sullivan, review of *Catalogue of Human Crania in the United States National Museum Collections: The Eskimo, Alaska and Related Indians, Northeastern Asiatics* by Ales Hrdlicka, in *American Anthropologist* n.s. v. 26, n. 4 (Oct – Dec 1924), 534-36; 534; Ales Hrdlicka, "Catalogue of Human Crania in the United States National Museum Collections: The Eskimo, Alaska and Related Indians, Northeastern Asiatics" *Proceedings of the United States National Museum* (Washington, D.C.: Government Printing Office, 1924) v. 63, article 12, 1-51.

Sullivan pointed out that at the instant of its publication in 1924, the Smithsonian catalog was already out of date. Despite the International Agreement of Monaco in 1906 that called for a standard of thirty-two measurements of human crania, Hrdlicka's catalog gave only eleven. It omitted the maximum and minimum frontal diameters, measurements of maxillo-alveolar length and breadth, and all arcs of mandibular measurements. For a scholar able to examine the material personally, the absence of such measurements would not be problematic, but the entire point of the publication was to provide students who were unable to examine the crania first-hand proper evidence and exact measurements. "Were this an ordinary publication, the omissions noted above would pass unnoticed but this is a catalogue of the collections in the United States National Museum," Sullivan wrote, "and the lack of conformity to international standards is a more serious offense than it would be for a private institution to publish an individualistic catalogue."⁷⁸ Hrdlicka's work was being held to a much higher standard because of the nature of the Smithsonian's collection: it was a depository of physical specimens to be available to all scholars. The paucity of measurements given severely hampered the effectiveness of the National Museum's mission. But Sullivan's intent was not to dismiss the usefulness of the catalog; he was critical because he had hope that in future catalogs of the museum's collections this would be remedied.⁷⁹

⁷⁸ Sullivan, review of *Catalogue of Human Crania*, 535.

⁷⁹ *Ibid.*, 535.

This moderate rebuke spurred Hrdlicka to find other authors to write more favorable reviews. When he approached Dixon about helping, however, Dixon exacted his revenge for Hrdlicka's harsh review of *The Racial History of Man*. Refusing to defend the Smithsonian's *Catalogue*, Dixon emphasized in a letter to Hrdlicka that "I believe, and I think most others will also, that Sullivan's criticisms are wholly justified and might well have been even more forcibly put. You have a good deal to answer for." It was "rather a case of poetic justice," Dixon suggested, "that you, who so disingenuously criticized me for failing to utilize measurements which you knew were either unavailable or non-existent, should now be rightly blamed for withholding data from students which are actually in your hands." The measurements that Hrdlicka provided were, for the scientific community, exceedingly meager data samples that severely limited the utility of the publication. Hrdlicka, Dixon felt, provided measurements designed to undercut the popular prejudices in the public mind regarding the racial characteristics of immigrants rather than assist serious professional study, an act he regarded as "inexcusable."⁸⁰ Dixon mocked Hrdlicka's acid review of *The Racial History of Man* and asked what help Hrdlicka could expect from him, "whose only claim to distinction in the field, lies in having written what you yourself have declared was an 'appalling' book, and one which was nothing short

⁸⁰ See first, Ales Hrdlicka to Roland Dixon, 27 January 1925; Dixon to Hrdlicka, 18 February 1925, both box 22, Ales Hrdlicka papers. Dixon argued this because Hrdlicka described some of the measurements as the "unstable height-length" and "very changeable nasal index" to illustrate the fluidity of physical type and thus undercut the evidentiary basis of nativist arguments of immutable and inferior racial types. Much more useful, for Dixon and other students, would have been radio-humeral differentials and the form of teeth.

of a ‘disaster.’”⁸¹ This schism between Dixon and Hrdlicka should serve as a reminder that, as methods and techniques in physical anthropology were being refined and debated, professional scholars also fell into diverse camps. Thus caution should be exercised in positing monolithic groups of “professional scientists” in opposition to “gentlemanly” researchers, as professional scientists were fragmented as well.

The tone of Hrdlicka’s reply to Dixon was equal parts cordial and angry, and suggests latent tension between the two institutional leaders. Hrdlicka told Dixon, “you are an inordinately ungrateful and greedy lot, all of you. Also, most unreasonable.” But he felt that the continued publication of the *Catalogue* was important, and asked again for Dixon’s help. Hrdlicka had a “good deal of well-rooted respect” for Dixon’s work – “even though you may have temporarily lost the high road in one direction” – and Dixon’s commitment to the advance of anthropological knowledge could help salvage professional opinion to the project.⁸²

But Dixon remained unmoved. Because he was a curator of a national museum, Dixon insisted that Hrdlicka had certain responsibilities to the scientific public. “You are a trustee for scientists everywhere. You have no right to follow your individual opinions in what you shall publish. It is your duty to afford to others the most complete information possible, when you publish an official

⁸¹ Dixon to Hrdlicka, 18 February 1925, Hrdlicka Papers.

⁸² Hrdlicka to Dixon, 21 February 1925, Hrdlicka Papers.

catalog of the national collections,” he wrote. Pointing out that Hrdlicka himself would never confine a professional discussion of crania to “the miserably few measurements” in the *Catalogue*, Dixon used a personal example of how significant the lack of data was for professionals: a student of Harvard anthropologist E. A. Hooton was attempting some significant work on comparative anthropology, and had anticipated using the National Museum’s collection, but now could not since the published data did not contain useful measurements.⁸³ Hrdlicka was retarding analysis of the data of which he was the caretaker.

Ultimately, some of Dixon’s criticisms must have rung true to Hrdlicka, for in April he published a note in *American Anthropologist* defending the National Museum’s *Catalogue*. He insisted the data set conformed the international standards established by the Monaco Agreement, which he noted that he had helped devise, but pointed out that the standards were for measurement of crania, not publication. The larger problem for the *Catalogue* was the extremely high cost of publishing statistical data sets as opposed to text, which necessarily limited the amount of information that could be published. Hrdlicka made clear that he understood that one of his jobs was to make available, for scientific research, the data of the collections, but had to operate within the financial constraints of the Smithsonian’s publication funds. He took his professional responsibility seriously, but also felt that as a professional anthropologist, he had the additional responsibility to act when anthropology was

⁸³ Dixon to Hrdlicka, 26 February 1925, Hrdlicka Papers.

being manipulated and misrepresented by non-scientists for political or social ends.⁸⁴

This sentiment was reflected in April 1929, when Hrdlicka delivered a lecture to a small group in New York City on the anthropological ramifications of immigration. Just as Boas had in a 1925 article titled “What is a Race?” the Bohemian immigrant took a public stand against immigration restriction, and the classical racialism that rationalized it. Although his talk was too late to affect the Congressional debate on restriction, or for that matter, to impact the national origins quotas, the biological and anthropological aspects of immigration needed to be addressed. The question of immigration revolved around culture as well as heredity, a fact that members of the professional scientific community – such as Jennings, Pearl, and Boas – accepted, but one that was being severely distorted by speakers, writers and what he called “agitators.” Hrdlicka argued that to posit distinct, fixed races, with specific mental, physical, and emotional characteristics that were hereditary, was wrong. Elevating some population groups above others as possessing superior qualities was mostly a function of “superficial impressions” mixed with cultural ignorance and prejudice. “Do not believe the alarmists, the Grants or Stoddards, who tell you that the American people through mixture with immigrants are in danger of degeneration,” he told the audience. “No nation of white people in the world is or has ever been racially pure. All are

⁸⁴ Ales Hrdlicka, “Catalogue of Human Crania in the U.S. National Museum” *American Anthropologist* n.s. v. 27 (April 1925), 339-40.

mixed.”⁸⁵ But this was too late – the alarmists *had* already carried the day with their classical racialist arguments.

There was a strange appropriateness to speaking out against the influence of Grant, Laughlin and other biological determinists well after the fact. The futility of criticism lay in the nature of scientific “speciation,” as Kuhn might have put it. The language of the modern scientific community was becoming increasingly esoteric; the “restrictionist language and epistemology” had remained the same for decades. The critics could say Mendelian heredity was wrong, but could not say what was right. Opponents of immigration, who absorbed the racialism of correlation, anecdote and heredity, could argue that Mendelism *was* right, and there were plenty of examples in the real world of hereditary characteristics. That may no longer have been an acceptable method of scientific investigation, but the public was much more familiar with the older lexicon of ontogeny recapitulating phylogeny, and like producing like. Until the Crick-Watson model of DNA was outlined in 1953, modern biology and genetics would lack the certain answers of the pre-modern method, and resistance would, in a way, be futile.

⁸⁵ Ales Hrdlicka, “Conference on Immigration Policy” delivered 10 April 1929, Town Hall Club, New York City, box 19, Hrdlicka papers. The “Grants and Stoddards” from p. 4, ignorance and prejudice from p. 2. See also Boas, “What is a Race?” *The Nation* v. 120, n. 3108 (28 January 1925), 89-91 [covered in another chapt.4]

Conclusion: “Eternal vigilance is the price of safety”

In 1930, Herbert Spencer Jennings published a summary of the state of biological knowledge as it concerned the human organism, an assessment based on his decades in the fields of biology and zoology. As they grew from single cells, human beings manifested a remarkable diversity as they developed into individuals, he explained. The hereditary inheritance from maternal and parental lines combined with “the changes in the outer environment” to shape and direct a human’s development. The environmental changes themselves encompassed a wide variety of elements, from nutrition and sanitation to education and socialization.¹ Jennings remarked upon the dramatic changes in the scientific knowledge of heredity and inheritance that had occurred in the previous thirty years, and emphasized the extraordinary role that the tiny fruit fly had played in advancing the basic knowledge of genes and chromosomes. One thing that had not changed, however, was the role of experimentalism in advancing scientific knowledge. Jennings was as sure of this as he had been in 1897, when he traveled to Naples to conduct research on his single-celled paramecia.

In Jennings’s mind, there simply was no other way to *know* about science. Throughout his rather lengthy book, *The Biological Basis of Human Nature*, he made repeated references to the central role that experimentation played in confirming and demonstrating scientific truths. “Positive and inescapable experimental evidence proves...” hereditary theories, he noted at one point in the

¹ Herbert Spencer Jennings, *The Biological Basis of Human Nature* (New York: W. W. Norton, 1930), xviii for “outer environment,” 7 for dual inheritance and 76-101 for inheritance, 201 for nutrition and environment and 122-138 for environmental role.

book, “objective determination by experimentation” explains what is scientifically possible, he noted in another.² “Science is the organization of *experience*,” Jennings insisted, and “what shall go into it is entirely a matter of experience.” Without experimental and laboratory research to confirm scientific hypotheses of heredity and inheritance, there simply was, in Jennings’s mind, no experience from which to evaluate scientific claims.³ And still, despite these advances in understanding how heredity and inheritance worked, the professor conceded that there was still much that the scientist did not know. Experimental and laboratory work had to continue, with verified results and careful research contributing, brick by brick, to the larger edifice of scientific knowledge; Jennings believed that science could proceed in no other way. Decades before, he had described the fundamental question that scientists must seek to answer: “when an animal is put into new conditions, it begins to change to suit these conditions....And the question is, what makes it change that way?”⁴ Thirty years of teaching students the essential importance of this method, and thirty years spent examining changes in simple-celled organisms, had never shaken his belief and conviction that the only way to answer that question, to know “what makes it change that way?” was through experiment, observation, careful hypothesizing, and verification by the professional community.

² *Ibid.*, 73; 134.

³ *Ibid.*, 372, emphasis original.

⁴ Jennings to Mary Louise Burridge, 1 March 1896, Jennings papers, emphasis original.

But if Jennings and his academic and scientific colleagues understood that this was the only permissible approach to developing scientific knowledge, in the 1930s he continued to confront the hasty, incorrect, and sloppy generalizations that flowed from the classic racist approach to biological knowledge, where anecdote, inference, association and correlation all served as the basis of proof that “like produces like.” And thirty years on, Jennings continued to combat these inaccuracies. If science was a human adventure, as he suggested in the book, some of the adventurers had no regard for caution, safety, rationality or reality. A litany of biological fallacies stemmed from the over-zealous adventurers, the “middlemen, near-biologists—the popular writers that have undertaken to ‘sell’ biology to the world.”⁵ These popularizers – propagandists like Wiggam, whom he named specifically – were responsible for circulating “a lot of fallacies, or half fallacies, or quarter fallacies...of biological principles applicable to human affairs.” These fallacies were particularly prominent, Jennings wrote, in the explanations of the scientific study of heredity.⁶ Some of these fallacies included the theory of unicity, the immutability of inherited characters in the face of environmental pressures, that like produced like, and that superior individuals would produce superior children. In matters of heredity and development, any assertion based on non-experimental judgment was likely to be wrong. Perceiving heredity to be some specific, quantifiable *thing*, which was the underlying

⁵ Jennings, *Biological Basis of Human Nature*, 187 for adventure; middlemen from 204.

⁶ *Ibid.*, Wiggam mentioned on 192; quote from 204.

assumption of immigration restriction, eugenics, and indeed the whole edifice of classic racialism – all these were wrong. Man, Jennings wrote, had only two methods to solve problems: rationality and empiricism. Anything else was basically speculation.⁷ Still, despite this clear and unequivocal explanation of the proper methods of conducting scientific research, these fallacies, half-fallacies and quarter-fallacies persisted.

The defense of the process of scientific inquiry also came, as before, from anthropologists. In his Presidential address to the American Association for the Advancement of Science on June 15, 1931, Franz Boas reiterated the inherent skepticism with which scientists should speak about “race.” As he had in his criticism of Grant’s classic racialism in 1925, Boas insisted that racial groups shared body and “perhaps mental characteristics,” but that “in a strict sense we can not speak of absolutely valid hereditary racial traits.” So much of the immigration restriction movement’s rhetoric had assumed that physical type reflected racial identity, but Boas dismissed this idea as “a rash undertaking.” If one examined family lines, as he had with the descendants of immigrants for the Dillingham Commission, it was clear that even within families there was great physical differentiation.⁸ Boas cautiously reminded his professional audience that there were in fact common characteristics among population groups, but inferences from this premise that anatomical type determined behavior or that

⁷ *Ibid.*, fallacies listed on 206-218; rationality and empiricism from 204-205.

⁸ Franz Boas, “Race and Progress” *Science* n.s. v. 74, n. 1905 (3 July 1931), 1-8; from 2. For his 1925 articles, see Boas, “What is a Race?” *The Nation*; idem., “This Nordic Nonsense” *The Forum*.

body form was absolute and unchanging were simply unfounded. Just as Jennings insisted, Boas emphasized the impact of physical and cultural environmental influences on human development. “Ethnological evidence,” he said, “is all in favor of the assumption that hereditary racial traits are unimportant as compared to cultural conditions.” The present state of knowledge, the distinguished anthropologist noted, “justifies us in saying, that while individuals differ, biological differences between races are small.”⁹ But Boas, like Jennings, Morgan, Hrdlicka, Pearl and most other professional practitioners remained vexed by the basic question: *why* do individuals differ? And to this, they still had no answers.

They could suggest, as Boas and Jennings did, that environmental influences, whether cultural or physical, shaped and changed development. Despite advances, however, they could not demonstrate it. They could not, as Jennings had insisted in his 1926 lecture in Philadelphia, “find out things that we can show people...”¹⁰ They could be confident in the method that would yield these results, but could not say with certainty how human beings developed, why they were different, what the basis of that difference was (if it was hereditary or environmental), or how practical assimilation was. And when it came to devising political policy, certainty – or at least the appearance of it – was essential.

⁹ Boas, “Race and Progress,” 6; 5.

¹⁰ Jennings, “Some Unsettled and Unsettling Questions,” 37, emphasis original.

Restrictionists provided that certainty, and continued in the 1930s to use their perceived certainty to defend restriction against repeal. In 1930 Madison Grant published *The Alien in Our Midst*, a collection of essays from advocates of restriction that demonstrated the continued activism of the Immigration Restriction League and like-minded individuals. The book included selections from the founding fathers – John Adams, Benjamin Franklin, Alexander Hamilton, Thomas Jefferson, and John Jay among others – that purported to demonstrate that the opposition to immigration ranged back to the creation of the American republic.¹¹ The book continued to make the standard assertions of immigrant inferiority from the ideology of classic racialism. Albert Johnson, still serving as the chairman of the House Committee of Immigration and Naturalization, explained the importance of maintaining and defending the gains of restriction. “How shall the Republic endure,” he asked, “if there be steady deterioration of standards by ever-recurring new forces of infection, arriving in the land?” Davenport contributed a brief piece emphasizing the importance of eugenic and hereditary quality of the few permitted aliens at a time when the Carnegie Institution of Washington was re-evaluating its financial support of his facilities at Cold Spring Harbor.¹² Other contributions from Richards Bradley,

¹¹ Grant and Davison, eds. *The Alien in Our Midst*. The selections from “Words of the Founders” also included Fisher Ames, Christopher Gadsen, Patrick Henry, George Washington, James Madison, Gouverneur Morris and Rufus King. The frontispiece contained the following quote from Jefferson: “The mobs of great cities add just so much to the support of pure government, as sores do to the strength of the human body. It is the manners and spirit of a people which preserve a republic in vigor. A degeneracy in these is a canker which soon eats to the heart of its laws and constitution.”

Henry Pratt Fairchild, Francis Kinnicutt, Harry Laughlin, Edward Lewis, Henry Fairfield Osborn, Kenneth Roberts, Lothrop Stoddard and Robert DeCourcy Ward all demonstrated the continuing commitment to restriction by American nativists. Simply because National Origins went into effect in 1929 did not mean that restrictionist organizations like the IRL folded up and went home. They continued their activities.¹³

Contrary to the representation of immigration restriction in the historiography, the IRL remained active well into the 1930s, even after the onset of the economic depression crippled the flow of European migrants. Having achieved significant legislative restriction, the Immigration Restriction League did not disband in triumph. They concentrated their efforts on defending the immigration laws, and

¹² Johnson quoted on p. 10. Davenport's essay was "As the Twig's Bent," in Grant and Davison, eds., *The Alien in Our Midst*, 49-53. John C. Merriam directed the CIW to re-evaluate, in particular, the work of the Eugenics Record office in February 1929, but he established a committee that would, because of its membership, recommend continuing support of the institution. Among the members were Laughlin (who was the superintendent of the ERO), Davenport, Carl C. Brigham (the Princeton professor of psychology who had written *A Study of American Intelligence* in 1923 that is discussed in chapter 5), Edward Thorndike and Clark Wissler (both of whom were affiliated with the restrictionist movement) and L. C. Dunn, the only member who was critical and skeptical of eugenics. Still, the committee pointed out three shortcomings of the Record Office: the techniques for properly assessing the heredity of individual traits was lacking, making the compilation of pedigree studies effectively subjective and thus useless, and the breadth of information it gathered on characteristics provided no basis for comparison. The committee's work is detailed in Allen, "The Eugenics Record Office," 250-251. A second committee was convened in 1935 (a year after Davenport retired) that contained a larger number of critics that was a factor in the CIW ultimately cutting off its funding. See *ibid.*, 251-252. The ERO was ultimately shut down in 1940. See also Kevles, *In the Name of Eugenics*.

¹³ See Richard Bradley, "The Immigration Question"; Henry Pratt Fairchild, "Immigration and National Unity"; Roy Garis, "A Brief Review of Immigration Legislation"; Francis Kinnicutt, "Stopping the Gaps"; Harry H. Laughlin, "The Control of Trends of the Racial Composition of the American People"; Edward Lewis, "European Prejudices and American Politics"; Henry Fairfield Osborn, "Shall We Maintain Washington's Ideal of Americanism?"; Paul Popenoe, "The Immigration Tide"; Kenneth L. Roberts, "Mexican Immigration in the Southwest"; Lothrop Stoddard, "The Permanent Menace from Europe"; Robert DeCourcy Ward, "Fallacies of the Melting Pot Idea and America's Traditional Immigration Policy" all in Grant and Davison, eds. *The Alien in our Midst*.

by extension, maintaining the purity of the Anglo-Saxon race. With the laws on the books, League members realized by 1933 and 1934, that there could be concerted attempts to repeal the legislative provisions with a new party in control of Congress and the White House. The turning of the political tides to the Democratic Party—particularly Franklin Roosevelt’s appointment of Frances Perkins to head the Department of Labor—distressed the Boston nativist immensely. “The attitude of the administration and of Congress,” one member worried, “constitutes the most serious threat to immigration restriction in over twenty-five years.” Additionally, the possibility of a large-scale exodus of Jews from Hitler’s Germany was particularly frightening to League members.¹⁴

In the five years after the National Origins quotas went into effect, the League continued defending and trying to expand restrictive policies. They restricted membership to New England in an effort to control the member’s activities better—to unite them under one tent. Annual reports of the League’s Executive Committee in the early 1930s reflect the continued worries of the leadership. In 1930, Patten represented the IRL before Congress one again, advocating the total suspension of immigration, because of the economic collapse. “Every immigrant alien who comes to this country,” he told the Senate Committee on Immigration, “must necessarily affect the labor market, either as a competitor for a job, or in being dependent on some breadwinner who is a competitor for a job in our labor market.” Patten argued that if the United States was concerned about the welfare

¹⁴ Richards Bradley to Members of the League, 29 March 1934; Bradley to John Moors, 3 August 1934; Bradley to George Nutter, 23 March 1933, all box 2, Lee papers.

of its native-born population, the senators would prohibit all immigration.¹⁵ The American Eugenics Society sent a petition to Congress to endorse the prohibition on immigration, with Robert DeCourcy Ward and John B. Trevor signing in support as members.¹⁶ The League leadership's commitment to restriction was so great that Ward began grooming his son Henry for a leadership position in the organization. The Executive Committee's annual report from 1930 laid out the League's agenda for the coming years. They advocated for the extension of the quota system to the countries of the western hemisphere, and in addition to temporary suspension they wanted all of the national origins quotas reduced by an additional fifty percent, and the registration of all incoming immigrants with improved provisions to deport immigrants who became public charges.¹⁷

The importance of continued vigilance for the IRL was clear in 1931, when its Annual Report noted that an old nemesis, Samuel Dickstein from New York, assumed the chair of the House Immigration and Naturalization Committee.

¹⁵ Statement of James Horace Patten, "Suspension for Two Years of General Immigration into the United States" *Hearings before the United States Senate Committee on Immigration* 71st Congress, 3rd Session, 16 December 1930, 34.

¹⁶ American Eugenics Society, "Statement in support of the Reed Bill (S.J. Res. 207) by the President and the Chairman of the Committee on Selective Immigration" 24 December 1930, in box 3, Lee papers. The petition singled out Ward, the "president of the Immigration Restriction League of Boston, which organization he founded in the '90s and is the pioneer in this country." 2. Henry Fairchild was the AES president, Madison Grant was the chairman of the Committee on Selective Immigration, and other committee members included Laughlin and Roy Garis.

¹⁷ Immigration Restriction League, "Annual Report of the Executive Committee of the Year 1930," box 3, Lee papers. Western hemisphere concerns on 3; legislative program from 4. The Executive Committee in 1930 consisted of Robert DeCourcy Ward president, Joseph Lee chairman, A. Lawrence Lowell and C. C. Little as vice-presidents, along with Richards Bradley (treasurer), Henry DeCourcy Ward (secretary) among others. It also noted that Grant's book *The Alien in our Midst* was "an important publication in favor of restriction." 4.

Bradley, the League's treasurer, used this potential threat to solicit money from members to ensure that the League would have the resources to defend their gains and preserve restriction.¹⁸ Lee wrote to Ward in January before the new session of Congress opened, to explain in greater detail how the Boston philanthropist regarded the temporary suspension bill. Lee feared that any temporary period of suspension of less than five years might endanger long-term, permanent restriction (including within the western hemisphere). A temporary bill would lapse, whereas a long-term, permanent bill would have to be repealed, which Lee felt would give restrictionists some leverage in terms of gaining concessions in the fight for repeal. "In other words," he carefully explained, "a short suspension law—say for two years—might merely serve to lose us our present opportunity for the sake of a short, temporary gain."¹⁹ Despite the loss in November 1931 of one of the original founders of the League, with the death of Robert DeCourcy

¹⁸ Immigration Restriction League, "Annual Report of the Executive Committee for the Year 1931" box 3, Lee papers. Dickstein was not only an old opponent from the battles in the 1920s over restriction, he was responsible for establishing the House committee that would eventually evolve into the House Un-American Activities Committee (established under the Dies-Dickstein Resolution) in 1934 – Massachusetts Representative John McCormack was the initial chair under the "Dickstein Resolution," (H.R. 198); he also made concerted efforts in the late 1930s to mortgage the future quotas of Jewish immigrants to enable them to escape Nazi Germany. There has also been information found in Soviet Union archives suggesting that he acted as a spy for the Soviet Union in the late 1930s, passing along information about pro-fascist and anti-Communist organizations in the United States to the NKVD. For the chairmanship of the House Committee on Immigration and Naturalization from the 72nd to the 79th Congresses (1931-1945), see his entry in the United States Congress, *Biographical Dictionary of American Congress, 1774-1961* House Doc. 442, 85th Congress, 2nd Session (Washington, D.C.: Government Printing Office, 1961); for his involvement on HUAC, see Bertram Gross, *Friendly Fascism: The New Face of Power* (New York: M. Evans Co., 1980), 87-88; for Jewish quotas see David Kennedy, *Freedom from Fear: The American People in Depression and War, 1929-1945* (New York: Oxford University Press, 2005), 416-417; Soviet spying in Allen Weinstein, Alexander Vassiliev, *The Haunted Wood: Soviet Espionage in America, the Stalin Era* (New York: Random House, 1999), 140-50.

¹⁹ Lee to Ward, 13 January 1931, box 2, Lee papers.

Ward, the IRL continued to protect their restriction gains.²⁰ After the elections of 1932 when the Democrats made tremendous strides – and a Democratic president won the election – the Executive Committee warned its members that they faced a great peril. Many of the new Congressional members had not taken a stand on immigration or restriction, and the League was worried that their policy gains may be undermined or repealed by this new unknown quantity. Much of the public mistakenly believed, the annual report from 1932 suggested, that the Depression had taken care of the immigration issue. This was not the case and there was work still to be done. The report closed warning, “Eternal vigilance is the price of safety.”²¹

With the Roosevelt administration in power in 1933, the League’s leaders warned their members “The attitude of the administration and [C]ongress constitutes the most serious threat to immigration restriction in over twenty-five years.” A piecemeal assault on the carefully constructed edifice of restriction, Bradley explained, threatened to undo the present immigration law, and the League must continue to plug holes in the “immigration dike,” and protect the

²⁰ Robert T. Jackson wrote a sympathetic obituary for Ward in *The Scientific Monthly* in which he noticed that just like his friend and co-founder of the IRL, Prescott Hall, Ward suffered from neurasthenic tendencies. Jackson noted that Ward had “a sensitive, nervous, highstrung temperament...” Robert Tracy Jackson, “Robert DeCourcy Ward” *The Scientific Monthly* v. 34, n. 2 (February 1932), 192-94; 194. Of Hall, his sister Lucyle Irby Hall described her brother as a perpetual insomniac, and a “frail little hothouse plant [who] was never allowed to romp, to climb, to be reckless, as other boys were.” She described his life as “a courageous battle against ill-health.” His Brookline physician noted that in Hall “was merged the analytical mind in a supersensitive body.” Hall, *Immigration and Other Interests*, 119 from his sister; physician on xviii.

²¹ Immigration Restriction League, “Annual Report of the Executive Committee for the Year 1932,” 4.

gains already made. Bradley also solicited more money from members, because Lee had to reduce his annual contribution of \$12,000 by about one-third.²² And the financial contribution was vital since, as Bradley noted to Patten – who was still lobbying for the League in Washington, D.C. – many of the new House and Senate members “have always been more or less blind to the real remedy for low living standards and are tied up with those who at heart are opposed to restriction.”²³ In 1933 the League remained true to some of their longstanding, fundamental assumptions of the quality of immigration that stretched back to Walker. These new immigrants had much lower standards of living, and would do tremendous damage not only to the racial homogeneity of the nation, but to the standards and living conditions of native-born Americans. Immigration, even during the Great Depression, remained the same threat it was almost forty years before.

League members also continued to exploit their Congressional contacts as well, sending Patten to testify at public hearings before the House Immigration and Naturalization Committee in May 1934. Trevor was also there to testify, and

²² Richards M. Bradley, “Suggested draft of letter to be sent to the members of the Immigration Restriction League with the annual report for the year ending Dec. 31, 1933” box 2, Lee papers. When the official final annual report was sent around, Bradley included a cover letter with these warnings, along with a statement that, “Most people think that the immigration question was settled in 1924. As you know, the 1924 Immigration Act did not stop all openings in the immigration dike, and made no attempt to touch the immigration flow from the western hemisphere. We must bring to the public attention the limitations of the present law, and the attempts being made to break it down.” Richards Bradley to Members of the Immigration Restriction League, 29 March 1934, box 2, Lee papers. Cutbacks from Lee to Bradley, 20 March 1933, box 2, Lee papers.

²³ Bradley to Patten, 22 August 1933, box 2, Lee papers.

Bradley submitted a written report to the House Committee that urged Congress not to expand the discretionary power of the Secretary of Labor to land immigrants who would otherwise be inadmissible. Perkins oversaw an investigatory commission on the practices at Ellis Island.²⁴ In August Bradley asked Lee to suggest people that the League could send to testify before the House Committee who would oppose the recommendations of the Perkins Committee who the committee would not “suspect of personal or racial prejudice.”²⁵ The leadership of the League knew what worked, and they stuck to it.

But by 1934, the leadership of the League was in declining health. Hall had died in 1922, Ward in 1931, and in 1934 Bradley and John Moors (one of the earliest members) were 73, Lee was 72, Madison Grant was 69, and even Patten was nearing 60. Lee and Grant died in 1937, Patten in 1940, and Bradley in 1943 (Moors lived to his 92nd birthday, passing away in 1953). This is what stopped the Immigration Restriction League. They simply died. The long, sustained and tireless campaign for immigration restriction speaks to the earnest and sincere

²⁴ House Committee on Immigration and Naturalization Hearings, 73rd Congress, 2nd session, “Amendments to the Immigration Laws as Recommended by the Department of Labor and the Secretary’s Ellis Island Committee” 8-10 May 1934, in box 2, Lee papers. Trevor’s testimony was in favor of increasing restriction and the limiting the Secretary of Labor to use discretion to land immigrants in favor of reuniting families, or to override deportation orders to maintain families in the United States. Trevor suggested, in effect, that immigrants could still be reunited if they were all returned back to Europe. He also strenuously objected to the influence that Secretary Perkins’s Ellis Island Committee wielded, since it “contains a large number of well known and eminent persons who, to my knowledge, have never had anything to do with building up the national policy of immigration restriction.” It also contained a “substantial group of antirestrictionists.” 99.

²⁵ Bradley to Lee, 17 August 1934, box 3, Lee papers.

dedication of this small band of men.²⁶ In 1905, Lee had asked Charles Francis Adams to join with the League to protect the Anglo-Saxon character to the United States. Adams declined, and in his reply explained, “the question is too big and too intricate for me to meddle with. I have gone on the retired list, and a generation other than that to which I belong must in future, if possible, save the country from the ‘eternal bow-wows.’ My generation, for better or worse, has done its work.” In 1934, as this new band of men, now very old, regarded the future of immigration restriction, there was a similar sense. They had little energy left to continue fighting. In August, Bradley wrote to Moors to pass on some information about Perkins and Dickstein and their efforts to weaken restriction. Bradley asked him if Moors and Lee might speak out publicly against the “queer alliance between the sentimental idealist, the racial specialist, and the cheap labor exploiter, [who were] all in favor of this line of bills.”²⁷ At the end of the month, Moors sent a note to Lee, describing the “lively correspondence” he was having with Bradley on the subject. Though he was not hopeless that the policies would

²⁶ For biographical information on the League’s leaders, see for Lee: Neva R. Deardoff, “Joseph Lee” *Dictionary of American Biography*, supplement 2 (New York: Charles Scribner’s Sons, 1958), 374-5; *The National Cyclopaedia of American Biography* v. 33 (New York: James T. White and Co., 1947), 66-7; *Biographical Dictionary of Social Welfare in America* Walter Trattner, ed. (New York: Greenwood Press, 1986). On Hall, *Who Was Who in America* (Chicago: Marquis Who’s Who, 1963), v. 1; and Prescott F. Hall, *Immigration and Other Interests of Prescott Farnsworth Hall*, compiled by Mrs. Prescott Hall (New York: Knickerbocker Press, 1922). For Ward, see *The National Cyclopaedia of American Biography*, v. 24; *Who Was Who in America*, v. 1; *Dictionary of American Biography*; and Robert Tracy Jackson, “Robert DeCourcy Ward” *The Scientific Monthly* (February 1932), 192-94. For Patten, consult his brief entry in *Who Was Who in America*, v. 1. Bradley’s biographical information in *The National Cyclopaedia of American Biography* v. 45 (New York: James T. White and Co., 1962); Moors is in *ibid.*, v. 41. Madison Grant’s entry in the *Dictionary of American Biography* supplement 2, was written by Henry Fairfield Osborn.

²⁷ Bradley to John F. Moors, 3 August 1934, box 2, Lee papers.

survive, Moors seemed deflated: “We three are now old men. There is now no Robert Ward or Prescott Hall to take hold as they once did. The public mind is practically asleep on the subject, and the attention of social workers is turned much more toward individuals.” Moors despaired that it “would be a tremendous job to start propaganda necessary to excite people on the dangerous legislation now threatened when the whole world is so stirred by other and more pressing problems. I’ll keep on thinking what, if anything, we three old gentlemen can do, but it is certainly a hard task.”²⁸

It was ultimately old age and attrition that stopped the leadership of the IRL. Since its founding in 1894, the members of the League worked tirelessly to limit the admission of inferior immigrants to the United States. Their wealth and social position gave them important political and social connections that they exploited, as their long affiliation with Senator Lodge (who died in 1924) and Albert Johnson shows. They used these connections constantly, maintaining a professional lobbyist in the capital until the very end. Their education linked them with an ambitious groups of thinkers who provided the evidence and authority for the urgent need of restriction. Sometimes these ties would supply them with cold statistical data and analysis of general demographic trends. Other associations yielded more hysterical alarmism about the “passing of the great race” or studies of feeble-minded families, and some, like that between Davenport and Laughlin, would find kindred spirits pushing boundaries and methods of natural sciences in

²⁸ Moors to Lee, 31 August 1934, box 2, Lee papers.

an era when the standards and content of scientific knowledge were changing. They reached out to any organization, institution, friend or philosophy to provide useful tools to achieve restriction.

In the final analysis, the most important tool that “racial nativists” used to curb immigration to the United States was their own specific episteme, which has been called here “classic racialism.” This broadly defined set of beliefs wove together certain trains of mental and biological determinism, an uncritical acceptance of Mendelian inheritance ratios, a certainty that the germ plasm – the basic element, the blood matter by which ontogeny recapitulated phylogeny – was unchanging and immutable. Within this set of beliefs, physical form reflected racial type distinctly, and from racial type could be gleaned mental characteristics. Like would always produce like. This xenophobia was powerful and persuasive because it was simple and comprehensible, because its racist basis seemingly had the weight of empirical proof behind it. Logically, classic racialism provided a compelling and accessible narrative that biology was destiny, and its proof was in the anecdote, correlation and pedigree. The IRL elites remained so active because they *believed* that southern and eastern European immigrants were hereditarily inferior because the weight of evidence seemed to suggest so. They earnestly took it upon themselves to protect the racial heritage left them by the Anglo-Saxon forebears that populated the country. They used any method they could – literacy tests, pedigree analysis, numerical limits and mental tests – to preserve this traditional and conservative image of the United States.

However complicated this classic racist ideology might be, in terms of the modern scientific disciplines that were emerging at the same time, this belief set, this particular restrictionist epistemology, was factually incorrect. The eugenics movement and its allied restrictionist organizations that believed that biology was destiny were wrong. Zoologists, biologists, anthropologists and geneticists all used a very different standard of conducting scientific investigation and theoretical speculations. Modern science was conducted in specialized and professionalized environments, whether in the remote hills of Orono, Maine, over sterile lab tables surrounded by giant, pleasing aquaria in Naples, or at Columbia University surrounded by masses of human bodies in America's preeminent city. Peer review, experimental replicability and professional credentials became additional foundations of this emergent trans-Atlantic cosmopolitanism that pushed the evolution of the modern scientific revolution. But as this gradual accretion of knowledge continued, and as the classic natural scientific fields of description and deduction split from modern laboratory and inductive methods, the intelligibility and certainty of modern methods dissolved. Until the discovery of DNA, modern science was unable to provide "hard and fast answers," as Jennings had once described, for the workings of biological inheritance. What made classic racialism so appealing was that it harbored none of this uncertainty. Giant intellectual pioneers like Galton and Lombroso had provided the basis of intellectual arguments in favor of this traditional and conservative ideology, and that provided, for its believers, all the scientific authority they needed. Common

sense and the basic obvious reality of physical familial similarity provided compelling evidence that their beliefs were scientifically accurate.

This divergence presented an essential dilemma when it came to enacting policies to regulate immigration. One camp presented a narrative of immutable heredity, dangerously illustrated by the Kallikaks. The offspring of “Old Horror” would always manifest the degenerate characteristics passed on from the nameless feeble-minded mother. This group could point to immigrant communities or working class districts in large towns and point out certain obvious physical dissimilarities among groups of people. Classic racialism was persuasive because it had the support of a superficial reality. The opposing camp, agreeing in principle that there were differences between groups, could not explain what the basis of that difference might be. The racialists assumed the basis was race. The modernists assumed nothing. Governmental policy could not be based on nothing, and with its professed certainties, classic racialism helped support government action.

Ultimately, this reveals much about American society in the early 1900s. As American influence and power expanded, there was an associated connection within a larger trans-Atlantic community. Ironically, this increasingly regular connection provided a recoil from more traditional and conservative Americans. Conservatives were a sophisticated and active minority that came to influence not only immigration restriction, but environmental conservation, large-scale construction of playground and recreational facilities, and intelligence assessments and testing. These were not particularly simple-minded folks that

tried to preserve the traditional values of the United States; they were well-educated, economically comfortable and socially anxious. They believed what they said. They believed immigration was a grave danger. That they actively raised this alarm unceasingly for over forty years is itself notable. That it took the cataclysm of the second World War to shake the public's confidence in the simplistic associations of racialism shows how difficult the process of "speciation" is for scientific revolutions. And that their basic policy of immigration restriction remained intact for almost fifty years (until 1965), demonstrates how effective class racialism was for arousing "public action to the necessity of a further exclusion of elements undesirable for citizenship or injurious to our national character."²⁹

²⁹ From the Immigration Restriction League, "Constitution of the Immigration Restriction League" box 3, Lee papers.

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