Social Security and Labor Supply of Older Workers and the Disabled

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Abstract

The essays that comprise my dissertation analyze the interactions among old age labor supply, health, and Social Security system in the United States. In the first chapter of my dissertation, I provide estimate of the causal effect of Social Security Disability Insurance (SSDI) application on employment of denied applicants. Using exogenous variations of SSDI application decision, I find that the employment rate is 36 percentage points lower for the denied applicants than that of comparable non-applicants in the short-run. In the second chapter (with Matthew Rutledge and April Wu) we explore the relationship between individuals' expectation on longevity and their plans for retirement in a quasi-experimental setting. The estimates in this paper suggest a large and statistically significant relationship between subjective life expectancy and retirement expectations: an individual who is one standard deviation more optimistic about living to age 75 has a greater probability of planning to work fulltime at 62 and 65 by 10 percent and 21 percent, respectively. In the third chapter of my dissertation (with Norma Coe and Matthew Rutledge) we identify the contribution of Medicare in explaining the retirement behavior of workers. We find individuals without access to retiree health insurance from work are 7.5 percentage points more likely to retire soon after their 65^{th} birthdays and are 5.8 percentage points less likely to delay retirement until the Full Retirement Age (FRA) than those with that insurance. We interpret this finding as evidence that Medicare eligibility persuades more people to retire, because they can begin receiving federal health coverage. The findings of the research in my dissertation provide important insights in making the Social Security system more welfare enhancing for the older workers and the disabled as well as keeping it sustainable in the long-run.

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INTRODUCTION

The primary role of the Social Security is to provide income support in the form of social insurance to workers to ensure their well-being in retirement or in disability. In the process it plays a crucial role in shaping the labor supply of workers who are close to the end of their career. Technological innovations in healthcare that helped people to live longer challenged the sustainability of the Social Security. To adjust to the demographic shifts the policymakers redefined the role of Social Security to make it sustainable in a way that also aims to balance its effects on the labor supply and well-being of workers. The essays that comprise my dissertation analyze the interactions among old age labor supply, health, and Social Security system in the United States. These papers make some important contributions to the literature that analyzes these interactions and also carry important policy implications for the Social Security Administrations as well as for the welfare of older workers and the disabled in developed countries around the world.

In first chapter of my dissertation titled "*The Effect of the Disability Insurance Application Decision on the Employment of Denied Applicants*," I provide estimate of the causal effect of Social Security Disability Insurance (SSDI) application on the labor supply of denied applicants. SSDI affects the labor supply of applicants through its work discouragement and through human capital deterioration regardless of the ultimate acceptance or denial of the claim. The existing literature is primarily focused on estimation of the benefit receipt effect of SSDI using the denied applicants as a comparison group. Instead, this paper provides an estimate of the causal effect of SSDI application on denied applicants using non-applicants as a comparison group. I find that SSDI causes a 36 percentage point reduction in employment for the denied applicants in the short-run. I exploit the differential incentives for SSDI application across birth-cohorts due to the increase in the Full Retirement Age and variance in the SSDI allowance rates across states to exogenously identify the SSDI application decision of denied applicants. The IV estimates suggest that the existing literature does not fully capture the negative labor supply effects of SSDI on the applicants.

The findings of this paper will facilitate policymakers to re-think reforms to reduce the work disincentives while applying and waiting for SSDI determination and to make more resources available to smooth the transition of denied SSDI applicants back into the labor force, especially for older workers close to retirement.

In the second chapter titled "*How Do Subjective Longevity Expectations Influence Retirement Plans?*" (with Matthew S. Rutledge and April Y. Wu) we explore the relationship between individuals' expectation on longevity and their plans for retirement in a quasi-experimental setting. Increasing life expectancy has made working longer both more necessary and more possible, but the relationship between an individual's survival expectations and his planned retirement age is unclear in the existing literature. This study uses an instrumental variables (IV) approach to examine how subjective life expectancy influences planned retirement ages and expectations of working at older ages, and how individuals update those expectations when they receive new information. The estimates in this paper suggest a large and statistically significant relationship between subjective life expectancy and retirement expectations: an individual who is one standard deviation more optimistic about living to age 75 has a greater probability of planning to work fulltime at 62 and 65 by 10 percent to 21 percent, respectively. Respondents who are more optimistic about their survival to age 75 or 85 also expect to work five months longer on average.

We also find that increases over time in subjective life expectancy for a given individual are associated with increases in his planned retirement ages and expectations of working at older ages. Finally, actual retirement behavior also changes with subjective life expectancy, but the relationship is somewhat weaker. The results further our understanding of how survival and retirement expectations are "anchored" to the previous generation's experience and suggest how targeted efforts at increasing knowledge about rising life expectancy may increase the proportion of younger cohorts who decide to work longer.

In the third chapter titled "*Sticky Ages: Why Is Age 65 Still a Retirement Peak?*" (with Norma B. Coe and Matthew S. Rutledge) we identified the contribution of Medicare in explaining the retirement behavior of workers. When Social Security's Full Retirement Age (FRA) increased to age 66 for recent retirees, the peak retirement age increased with it. However, a large share of people continue to claim their Social Security benefits at age 65. This paper explores two potential explanations for the "stickiness" of age 65 as a claiming age: Medicare eligibility and workers' lack of knowledge about their future Social Security benefits. First, we analyze the impact of Medicare eligibility by comparing two groups – one has an FRA of exactly 65; the other, between age 65 and 2 months and age 66. We find that the group with later FRAs who do not have access to retiree health benefits through their employer are more likely to claim Social Security at age 65. We interpret this finding as evidence that Medicare eligibility persuades more people to retire, because they can begin receiving federal health coverage.

Individuals without access to retiree health insurance at work are 7.5 percentage points more likely to retire soon after their 65th birthdays and are 5.8 percentage points less likely to delay retirement until the FRA than those with that insurance. This result fits into extensive research showing that access to health insurance is an important component of the retirement decision. On the question of whether misinformation about Social Security benefits may drive individuals to claim at age 65, we find that some individuals are unable to accurately forecast their retirement benefits. However, our analysis suggests that there is no relationship between this confusion and the age 65 peak for claiming Social Security.

CHAPTER 1

"The Effect of the Disability Insurance Application Decision on the Employment of Denied Applicants[1]"

In pursuit of explaining the persistent decline of labor force participation (LFP) rates among prime-aged individuals in the United States in the last few decades, a substantial body of research has focused on the interactions of social insurance programs and LFP. Much of this literature analyzes the causal effect of benefits receipt of the Social Security Disability Insurance (SSDI). In these studies, denied applicants are of interest mostly as a control group. Bound (1989) pioneers the empirical approach of using the labor supply of the denied SSDI applicants to estimate an upper bound on the potential labor supply of accepted applicants. Although Parsons (1991) raises questions on the validity of this comparison, the approach of Bound (1989) of comparing accepted and rejected SSDI applicants has been widely used to date (see for example, von Wachter et al., 2011). Chen and van der Klaauw (2008), Maestas et al. (2013), French and Song (2014), and Autor et al. (2015) have used this approach, along with various sources of exogenous variation inherent to the SSDI's application and evaluation process, to estimate the causal effect of receiving SSDI benefits on employment and earnings.

Implicit in the analytical approach of comparing for example, the employment of accepted and denied SSDI applicants is the assumption that SSDI affects employment through a single causal pathway: whether the applicant ultimately receives benefits. If the decision to go through the process of application itself affects the applicants - both the awarded and the denied – similarly,

[1] Mashfiqur R. Khan is very grateful to his advisors, Mathis Wagner, Matthew S. Rutledge, Joseph F. Quinn, and Alicia H. Munnell. He also thanks Alexander Strand for providing the aggregated SSDI application and award data. The research reported herein this chapter was performed pursuant to a grant from the U.S. Social Security Administration (SSA) funded as part of the Disability Research Consortium. The opinions and conclusions expressed are solely the author's and do not represent the opinions or policy of SSA or any agency of the Federal Government. The author thanks Mathematica for providing the generous Dissertation Fellowship funded by the SSA and thanks the Center for Retirement Research at Boston College for administrative support in accessing the restricted SSA geographic data. For questions and comments, please send an email to mashfiqur.khan@bc.edu.

then the comparison between these two groups is still valid. However, the negative effect of application on denied applicants is unaccounted for in such analysis, despite the fact that initially denied applicants make up two-thirds of applicants – a total of 1.8 million individuals in 2013 alone. In this paper, I estimate the causal effect of the SSDI application on the labor supply of denied SSDI applicants.

SSDI affects the labor supply of applicants through the application and determination process itself. Denied applicants do not receive benefits, yet still face the same cost of applying as applicants who are ultimately allowed onto the program. Bound (1989, 1991a) and Parsons (1991) discuss three ways the SSDI application process can influence the labor-market activity of denied applicants in the post-application period: 1) they may be out as they plan to reapply; 2) applicants who appeal their initial denial decision may be out of the labor force while awaiting the decision of appeal to make their case stronger; or 3) once the process is over, they may face increased difficulty to get back to work due to human capital deterioration because they were out of the labor force for so long. The first two channels involve employment disincentives while individuals are still in the process of SSDI application, appeal, and reapplications. The third channel suggests that human capital deterioration while staying out of the labor market during this process can cause substantial loss in potential employment after denial.

In this paper, I estimate the causal effect of SSDI on employment of the denied applicants (combining all of these channels) over the short-run. Comparing the labor supply of the eligible non-applicants of SSDI who are similar in observable characteristics to those of the denied SSDI applicants, I examine how the application process hurts the post-application employment of denied applicants. I find that the employment of denied applicants at ages 50-58 is as much as 49 percentage points lower two to three years after the application. The causal effect is somewhat smaller: a 36 percentage point reduction estimated using an instrumental variable approach that exploits the differential incentives for SSDI application across birth-cohorts and states.

An important decision like applying for SSDI involves a set of characteristics unobserved by econometricians, which are correlated to most of the economic decisions that individuals make. Unobservable characteristics like relatively low opportunity costs of applying for SSDI affect labor supply as well. Econometricians need to take into account the endogeneity of decision to apply for SSDI while estimating its effect on labor supply. The data in this paper allow me to exploit the exogenous variation across birth cohorts in the Full Retirement Age (FRA) – the age at which claimants receive the full Social Security retirement benefit (their Primary Insurance Amount, or PIA). The FRA has increased from 65 for individuals born before 1937 to 66 for those born in 1943-1954, effectively lowering their benefit at any claiming age. The lower benefit increases the attractiveness of the SSDI application decision, but should have no direct effect on labor supply before turning age 62.

Moreover, I use data on allowance rates[1] into SSDI to capture differences across individuals in their likelihood of successfully obtaining benefits based on the state in which they live. The Social Security Act and the regulations implementing it set up universal criteria to determine the disability status of someone who applies for SSDI benefits. However, historically the Disability Determination Services (DDS) offices across states in the U.S. have awarded SSDI benefits to applicants at differential rates with significant variation not only across states but also over time within each state (McVicar, 2006; Bound and Burkhauser, 1999; Rupp and Stapleton, 1998; Parsons, 1991a). The DDS office that evaluates an application depends on which state the applicant resides. The DDS allowance rate does not endogenously affect the application decision of denied applicants. However, the allowance rate affects the outcome of the application. In this paper, I exploit the variation of the aggregate allowance rates across location and over time that is exogenous to the labor supply of individuals but influences the outcome of SSDI application.

[1] The allowance rate is the ratio of number of SSDI applicants awarded benefits over the total number of applicants.

Hence, I can use both the FRA and a component of the aggregate allowance rate to instrument for the SSDI application decision of the denied applicants to control for the endogeneity problem that arises while estimating its causal effect on labor supply.

The employment and earnings trajectories of denied applicants before and after the application decision has been well analyzed in the literature, but only in the context of understanding the effects of SSDI on the awarded applicants. Very few papers have focused on the importance of the effects the SSDI application process have on the denied applicants. As the number of denied applicants has been growing over the years, understanding these effects on them is becoming more and more important and relevant for policy considerations. While analyzing the trends in employment and income of SSDI applicants, von Wachter et al. (2011) compared the employment and earnings of the rejected SSDI applicants to non-applicants using matching in observable pre-application characteristics; they find sizeable negative effects on employment rate of the rejected applicants. However, they cannot claim these findings to be causal as unobservable characteristics may affect their results. In a recent paper, Autor et al. (2015) explores the human capital deterioration effects on labor supply of the denied SSDI applicants to find a significant causal adverse effect of waiting time on employment and earnings in the long run. However, they only capture a part of the total negative effects that SSDI application process has on the rejected applicants, because their model misses the effect of work disincentives while applications are pending.

In this paper, I make a unique contribution to the literature by providing the full causal effect of SSDI application process on the labor supply of denied applicants in the short run. To the best of my knowledge this is the first paper to measure the negative causal effect of the SSDI application decision on employment of denied applicants using non-applicants as a control group. Most of the papers in this literature estimating the negative effect of SSDI benefit receipt use administrative disability insurance data to exploit the exogenous institutional variations for identification. This type of administrative data typically does not have information on non-applicants. The data I use in this paper has detailed information on applicants and non-applicants in their 50s and 60s, which allow me to identify a comparison group of non-applicants for the denied SSDI applicants.

The rest of this paper is organized as follows. The next section provides a background of the Old Age, Survivor, and Disability Insurance (OASDI) program and the Social Security Amendments of 1983 to understand the institutional setup relevant for this paper. Section 1.2 provides the description of the data I use in this paper and highlights important sample characteristics. Section 1.3 outlines my identification strategy. Section 1.4 presents the estimates of the labor supply effects of SSDI application decision of denied applicants. This is followed by a sensitivity analysis of the main finding of this paper in section 1.5. I conclude in section 1.6 with a discussion of the policy implications of my findings.

1.1 Background on OASDI program and 1983 amendments

1.1.1 The Disability Program

The SSDI program is part of the Old Age, Survivors, and Disability Insurance (OASDI) program of Social Security Administration and is funded mainly through payroll taxes. It is a social insurance program for disabled workers with eligibility conditioned on previous sufficient employment in jobs covered by Social Security[2]. The SSDI program defines disability as the "inability to engage in substantial gainful activity (SGA) by reason of any medically determinable physical or mental impairment(s) which can be expected to result in death or which has lasted or can be expected to last for a continuous period of not less than 12 months." Activity is considered "substantial" if it involves significant physical and/or mental exertion and it is considered "gainful" if it is performed for pay or profit (although realization of profit is not binding). SSA implements

[2] To qualify for SSDI benefits, an individual must have sufficient employment subject to Social Security contributions. The amount of required labor force attachment depends on the age of disability application. Generally, an individual needs to have worked 10 years, five of which needs to be during the 10 years preceding the year of DI application. Relatively younger workers may qualify with less work experience than the general rule.

this definition by setting an earnings threshold - $1130 per month for non-blind and $1820 per month for blind in 2016 for example, which is adjusted over time - over which individuals are said to be engaging in SGA and are therefore disqualified from participating in the SSDI program.

While SSDI is a federal program with a uniform national standard, the initial disability determinations are made at state SSDI offices, on the basis of medical criteria. Individuals apply for DI benefits at their local field office, which screens out those who are not currently insured or who are engaging in SGA. These are labeled "technical denials" and do not receive further review. The remaining applications are forwarded to a state Disability Determination Services (DDS) office, where cases are assigned to disability examiners for review. The rejected applicants at the DDS level are then entitled to a series of appeals, first to the state SSDI agency, then to the Administrative Law Judge (ALJ), then to an Appeals Council, and finally to the Federal court system.

Autor et al. (2015) present statistics on fraction of SSDI applicants appearing in different stages of determination and how long an individual on average waits in each stage of determination in 2005.[3] Approximately two-thirds of the applicants are denied at the DDS level with determinations made on average in about three months. Just over a quarter of applicants go to the reconsideration stage, which adds another five months on average to the determination period. Just under one-third of applicants take their case to the ALJ level, which adds more than two years to the average total waiting time for determination. Very few applicants who are denied at the ALJ level move their case further into appeal system. However, applicants who do so have to wait more than a year on average to learn about the final outcome of the appeal.

[3] Although this statistics differ from the SSA official statistics, I prefer these for the following reasons: Autor et al. (2015) exclude all technical denial; they drop people who died within two years of initial decision; they also drop people who previously applied for or received SSDI or SSI benefits. These three groups of people are excluded from the analysis in this paper as well, making the statistics provided in Autor et al. (2015) much more relevant for this paper.

Clearly, there is a significant amount of time that the applicants are out of the labor force, which varies across individuals according to their selection onto reconsideration and appeals of initial decision on DI application. In addition to the processing time, as the determination requires that an individual was not engaged in SGA five months prior to application, many applicants were out of the labor force even before filing application for SSDI. The SSDI application can keep an applicant out of the labor force for a significant amount of time, significant enough for deterioration of human capital and labor market attachment. This is particularly important for the eventually denied applicants who then need to return to the labor force, especially for people who are well below the early retirement age set by the Social Security.

1.1.2 Calculation of the OASI and SSDI Benefits

The progressive nature of Social Security benefits calculation makes the SSDI benefits fairly generous compared to OASI benefits. Workers are eligible for OASI benefits after accumulating at least forty quarters in employment subject to Social Security contributions and may collect benefits starting at age 62. Both OASI and SSDI benefits are based on workers' earnings history. The first step for either program is calculating the Average Indexed Monthly Earnings (AIME). For OASI benefits, the AIME is the average of the top 35 years of earnings indexed to the year of age 60 using the Average Wage Index (AWI), divided by 12. For SSDI, the Social Security averages earnings from the year a worker turned 21 to the year that worker became disabled and index the earnings to the year of disability onset. If a disabled worker has over 35 years of indexed earrings, the Social Security only averages 35 highest years of earnings.

The next step to calculate benefits in both programs is determining the Primary Insurance Amount (PIA), based on a progressive benefit formula. The formula for calculating PIA is the same for both OASI and SSDI. The lower income workers receive a higher return on their Social Security taxes than the higher income workers. This is achieved by breaking the AIME into three parts and weighting each part[4]. The main difference between OASI and SSDI benefit calculation is in the actuarial adjustment factors applicable to earlier or later than FRA OASI claiming. However, the FRA does not play any role on the benefit calculation of the disabled workers.

Disability beneficiaries receive their full PIA regardless of the age at which they first receive benefits. OASI beneficiaries, however, have their benefits adjusted to account for their age at claiming relative to their FRA. Retired workers' benefits are exactly equal to their full PIA only if they first claim the benefits at their FRA. Otherwise, OASI benefits are actuarially adjusted, which is designed to yield equal expected lifetime benefits no matter when they are first received. The earliest age for OASI claiming is 62 and it has the largest actuarial adjustment factor associated with it. If retirees choose to receive benefits early, their benefits are adjusted downward by more for each month that the claim was earlier than the FRA.[5] Claiming after the FRA yields larger benefits than the PIA.

Individuals may apply for SSDI benefits up to their FRA, and the fact that SSDI benefits – unlike OASI benefits – are not subject to actuarial reduction makes SSDI application quite valuable to individuals who think they are too unhealthy to keep working and disabled enough to get enrolled into SSDI. Although, the SSDI benefits are fairly generous in replacing the earning of the beneficiaries, however, the applicants need to put in considerable amount of effort to arrange necessary documentations to prove the disability status. In practice, the SSDI application rate falls off quickly after retirement benefits become available at age 62, which suggests that the

[4] The breakpoints in AIME to calculate PIA are adjusted annually based on changes in national average wages. However, the weighting scheme remains the same. For example, for a worker with 62nd birthday in 2016 the PIA is equal to 90 percent of the worker's first $856 of AIME, plus 32 percent of the AIME between $856 and $5,157, plus 15 percent of the remaining AIME.

[5] Benefits are reduced by 5/9 of one percent times the number of months between claiming and the FRA, if claiming was no more than 36 months early; if benefits were claimed more than 36 months early, benefits are reduced by 5/12 of one percent per month up to where the 36-month period begins.

"pecuniary" and "non-pecuniary" costs of disability application substantially exceed the (minimal) costs of claiming retirement benefits (Rutledge, 2012).

1.1.3 The Social Security Amendments of 1983

The Social Security Amendments of 1983 included a number of significant changes to social security, including an increase in the payroll tax rate, an expansion in the number of individuals covered by the program, and an increase in the actuarial adjustment factors beyond the FRA. Perhaps the most significant change of all, which plays an instrumental role in this paper, was a maximum of two-year increase in the full retirement age and a corresponding increase from 20 to 30 percent in the penalty for claiming OASI benefits at the early retirement age of 62.

These reductions in the generosity of Social Security OASI benefits were phased in gradually and occurred in two main stages. Individuals born in 1937 or earlier were unaffected by the change. The full retirement age then increased in two-month increments by subsequent birth cohort until reaching 66 for those born in 1943. For individuals born between 1943 and 1954 (inclusive) the FRA remains at 66 years until again increasing in two-month increments from the 1955 to 1960 cohorts. Along with this change, the fraction of full benefits that individuals could receive at the early retirement age of 62 fell from 80 percent for those born in 1937, to 75 percent for those born between 1943 and 1954, and to 70 percent for those born in 1960 or later.[6] Most importantly, for the purpose of this paper, these amendments do not change the benefits of SSDI across birth-cohorts.

[6] This policy also changed the actuarial adjustment factors beyond the age of 62 from 5/9 of a percentage point per month to 5/12 of a percentage point per month. This converted back to 5/9 of a percentage point 36 months before the full retirement age. Thus a person born in 1943 could receive 75 percent of his or her PIA at the age of 62, 80 percent at the age of 63, 86.67 percent at the age of 64, 93.33 percent at the age of 65, and 100 percent at age 66.

1.2 Data and Sample Characteristics

In this paper I use data from the *Health and Retirement Study* (HRS), a nationally representative longitudinal household survey of older Americans. The original sample of 12,561 comprised individuals who were born 1931-41 or were the spouse of a participant in that birth cohort; individuals born 1942-47, 1948-53, and 1954-59 were added in 1998, 2004, and 2010. Participants are interviewed every two years. This paper uses eleven waves of data from 1992 to 2012 and detailed information on both SSDI applicants and the non-applicants in panel form. The HRS has detailed self-reported information on SSDI application, award, reapplication, and appeal as well as a whole array of information on health, wealth, demographic and socio-economic characteristics, and employment.

I merge the HRS to the Social Security Administration (SSA) administrative geographic identification data in order determinate the state of residence in US of individuals from age 50 and older as long as they are observed in the HRS. This allows me to match the DDS level SSDI allowance rate data to individuals at a given age. The SSA provided me with aggregate data on the number of applications, the number of awards, and the number of denials at the DDS level of determination by gender, age-groups defined as 45-49, 50-54, 55-59, and 60-64, and US state for each year from 1992 to 2013[7]. Each individual in my sample is then matched to the appropriate allowance rate by geographic location, gender, and age.

In this paper I estimate the labor supply model of workers age 50 to 58 two to three years from every integer age. For example, for a worker who is at 50, I estimate the probability of being employed of the worker at 52 or 53. The HRS interviews people every two years. Depending on the month of interview in two consecutive HRS waves an individual can be observed more or less than two years apart, but always less than three years apart between waves.

[7] I do not have access to the Social Security "831 files" data to use in this paper.

I am particularly interested in estimating post-application employment of SSDI denied individuals. The application to SSDI can happen in the year when HRS interview happens or in between two consecutive interviews. If the application was filed during the HRS interview year, then for that individual the post-application employment is observed roughly two years from the application. If the application was filed in a year in between HRS waves, then I observe the labor supply of that individual in the second wave of HRS after the application. I do not take the wave immediately after the application as it makes the post-application labor supply within 1 year of application. I want to observe the employment of the denied applicants after their initial denial for SSDI. Hence, I skip the immediate wave of HRS post-application and instead observe the employment for those individuals around 3 years after the application.

If I had a control group of individuals who are very similar to the people who file an unsuccessful application for SSDI, then I could compare the average labor supply of the two groups at a given age to measure the loss of employment potential of the SSDI denied applicants. However, to interpret the difference to be causal, the treatment and control status need to be assigned randomly. One might create such plausible treatment and control groups in a randomized control experiment, but not in survey data. Using survey data merged with administrative Social Security earnings data, Bound (1989) uses the labor supply of the denied SSDI applicants – who, he argues, is a “natural” control group for the awardees – to measure the potential labor supply of accepted applicants had they not received the benefit. As the denied applicants are healthier and more capable to work than the awarded applicants, he argues the difference in employment is an upper bound of the employment potential of awarded applicants.

In this paper the treatment group is the SSDI denied applicants who go through the SSDI application process with eventually unsuccessful in getting onto the program. Because these unsuccessful applicants were denied due to health reasons, the administration views them as capable of SGA like other non-applicants in the labor force. Thus, all the eligible non-applicants can be thought of as a control group to compare with the treatment group of SSDI applicants denied at the DDS level. However, some individuals with health shocks that may make them eligible for SSDI benefits may not apply for it due to the “hassle cost” and “stigma cost” associated with the social insurance programs (Benıtez-Silva et al., 1999; Haveman et al., 1991).

I identify a subset of all eligible non-applicants for SSDI who I argue to have only slightly higher “hassle cost” and “stigma cost” at a given age than the denied SSDI applicants at that age. Along the line of argument in Bound (1989), I argue that this subset of non-applicants is a “natural” control group for the denied applicants. As the individuals in the control group have slightly higher opportunity costs of application and most likely are in better health than the denied applicants, the labor supply of the control group can be thought of as an upper bound of the employment potential of the denied applicants. Obviously, there are unobserved factors associated with the treatment and control groups, which are likely to be correlated with the labor supply of the individuals. Thus, the difference in labor supply between the denied applicants and the control group cannot be interpreted as the causal effect of the SSDI application decision.

In this paper the control group for the denied SSDI applicants filing application at a given age between 50 and 58 (inclusive) comprises the individuals observed to be non-applicants between the age of 50 and 58 (inclusive), but who later filed SSDI applications for the first time on or after age 60. This set of individuals in the control group have a “hassle cost” and “stigma cost” that is at least a little bit higher in their 50s, but not so high that application is never worthwhile, seeing as they applied in their 60s. This is because workers who eventually apply in their 60s are likely to have their health deteriorating in their 50s. Researchers have shown that the employment and income of SSDI applicants start to decline as many as four to six years before SSDI application (see for example, von Watcher et al., 2011). The rest of the non-applicants who never apply for SSDI at any age must have experienced no significant work-limiting health shocks or may have had adverse health shocks, but they must have a much higher threshold of “hassle cost” and “stigma cost” in their 50s than the denied SSDI applicants around that age (assuming all else the same). This group of people who never applied for SSDI is much less comparable than those who apply in their 60s to the denied applicants at a given age between 50 and 58 when the denied applicants file SSDI application.

The sample in this paper includes all individuals whose labor supply is observed in HRS between age 50 and 61 (inclusive). I observe the non-applicants until they reach their FRA in HRS to define the control group for analysis. The reference ages of analysis are between 50 and 58 (inclusive). Because, I am estimating the employment after two to three years from the reference age and I do not want to observe labor supply at 62 or later, which is the Social Security early retirement age. In this way I am excluding the potential confounding effect of OASI benefits in the analysis.

Table 1.1 presents the economic, health, and demographic characteristics of the denied SSDI applicants, the control group, the other non-applicants who are not in the control group, and SSDI beneficiaries. In the sample, there are 322 individuals who applied to SSDI for the first time at any age of 50 to 58 and did not eventually receive benefits. The control group consists of 347 individuals who filed SSDI application for the first time only in their 60s. The sample includes the denied SSDI applicants only once at the age of their application and observes their employment two to three years from that age. The individuals in the control group are observed multiple times starting from the first time they are interviewed in HRS on or after age 50 and then in two years interval as they grow older. The primary sample of analysis has a total of 1231 observations.

I define the denied SSDI applicants as the treatment group and the control group is defined above. In an ideal scenario, the treatment and the control groups would have very similar characteristics. In Table 1.1, columns 2 and 3 provide the average sample characteristics of the treatment and control groups, respectively. Demographic characteristics show that the treatment group has a slightly higher fraction of female, non-white, school-dropouts and single individuals than the control group. The average age of treatment and control groups are roughly 55 years and there is no statistical difference in the fraction of high school graduates and college educated individuals between the two groups. These two groups have very similar educational profiles. The control group appears to be somewhat healthier than the treatment group. For example, compared to the control group in treatment group faction of individuals reporting poor or fair health is 28 percentage points higher, fraction reporting mobility problems is 25 percentage points higher, fraction with large muscle problems is 17 percentage points higher, and fraction reporting back pain is 16 percentage points higher. A higher fraction of individuals with doctor diagnosed diseases like high blood pressure, stroke, psychiatric problems, and arthritis are in the treatment group than in the control group.

Individuals in the treatment group on average worked 4.6 years less than the control group, are less likely to have retiree health insurance from the employer than the control group, and have similar tenure in their longest jobs. In terms of wealth, the treatment group has a higher fraction of individuals in the bottom quintile and a lower fraction in the top quintile than the control group. Worse health conditions compared to the control group might have induced the treatment group to apply for SSDI; however, I cannot rule out unobserved factors like job loss or intensity of the taste for work as additional causes of application of the denied applicants. I argue that the severity of the health shocks of the individuals in the treatment group, which is unobserved in this paper, is marginally higher than the severity of the shocks of the control group as the rejected applicants are denied benefits at the DDS level for not being disabled enough.

A comparison of the treatment group with the all other never applicants for SSDI and the SSDI benefit recipients reveals that the applicants who apply in their 60s are, as I claimed, a “natural” control group when they are in their 50s for the denied SSDI applicants. In almost all the characteristics the difference between the treatment and the never applicants are much more pronounced than the difference between the treatment and the control group. For instance, the fraction of college educated in treatment group is 21 percentage points lower than the never applicants, whereas, the difference between the treatment and control among the college educated is statistically insignificant. The fraction of school dropouts in treatment group is 19 percentage points higher than the never applicants, but only 7 percentage points higher than the control group.

Among all the health related variables both self-reported and medically diagnosed, the treatment group are much less healthy than the never applicants. The difference in health and wealth between the denied SSDI applicants and the never applicants are much broader than that of between denied applicants and the control group. The never applicants are more educated, more wealthy and much healthy than the control group and even more so than the treatment group. These differences in characteristics explains why the never applicants have much higher threshold of “hassle cost” and “stigma cost” than the control group and never apply for SSDI.

In Table 1.1, column 7 presents the average characteristics of SSDI awardees and column 8 presents the difference between awardees and denied SSDI applicants. It is evident that the awardees are much less healthy than the denied applicants. Observing this difference, Bound (1989) argue that the employment of the denied applicants provide a plausible upper bound of employment of the awarded applicants had they not received the SSDI benefits. The differences in health profile between the treatment and control group used in this literature estimating the benefit receipt effect of SSDI is very similar to the differences in treatment and control group used in this paper to evaluate a treatment effect different from the focus of this literature.

Using the Bound (1989) approach it can be argued that as the control group defined in this paper is healthier than the denied SSDI applicants, the labor supply of the control group is an approximation of the upper bound of employment potential of denied SSDI applicants. The control group is also wealthier than the denied applicants. So, the labor supply of the control group at most be an underestimation of the upper bound of employment potential of denied applicants. However, this difference in labor supply between the treatment and control group cannot be treated as causal as itself, because, there are unobserved differences between the two groups. For example, compared to the denied SSDI applicants there could be much stronger labor market attachment of the individuals in the control group or the severity of the health shocks could be much less for people in the control group. However, an exogenous variation of the SSDI application decision that is uncorrelated with the unobserved characteristics differences between the treatment and the control group would allow estimating the difference in labor supply between the two groups that is causal. The goal of this paper is to find this causal treatment effect of SSDI application decision on employment.

1.3 Identification Strategy

In this paper I estimate the causal effect of SSDI application on post-application employment of denied applicants aged 50 to 58. Let y_i be the measure of outcome, in this paper indicator for working for pay, and t_i be the indicator of denied SSDI application, where $t_i = 1$ if treatment was received and $t_i = 0$ if not. Define $y_i(1)$ as the outcome if the individual i is given treatment and $y_i(0)$ if untreated. It is not possible to observe an individual simultaneously as an applicant for DI and a non-applicant at a given age. What I observe is the following:

$$y_i = t_i y_i(1) + (1 - t_i) y_i(0) \tag{1.1}$$

The strategy in this paper to evaluate the effect of SSDI application denial on employment originated from the evaluation strategy of Bound (1989), where he evaluates the effect of SSDI benefits receipt on employment using the denied applicants as a comparison group. However, in this paper the denied applicants represent the treatment group and I need a comparison group for evaluation of the treatment effect. The comparison group I propose is an observably similar group of individuals who are eligible for SSDI application, but choose not to apply. This allows me to compare the labor supply of the comparison group with those who applied for SSDI, but eventually did not receive benefits, which is primarily for not being disabled enough.

To understand this approach, consider the evaluation of the average treatment effect on the treated $E[y_i(1) - y_i(0)|t_i = 1]$, which in this paper is the average effect on post-application employment of denied SSDI applicants. While I can use the employment of the denied applicants and non-applicants in our sample to estimate $E[y_i(1)|t_i = 1]$ and $E[y_i(0)|t_i = 0]$ respectively, I do not observe the percentage of denied SSDI applicants who would have worked in the absence of SSDI program to estimate $E[y_i(0)|t_i = 1]$. I argue that as the denied SSDI applicants are denied for health reasons, they are not very different from individuals in the control group selected in this paper in terms of significant work limiting health shocks. However, as the individuals in the comparison group are generally healthier and more capable of performing SGA than the denied SSDI applicants, that is $E[y_i(1)|t_i = 1] \leq E[y_i(1)|t_i = 0]$, one could treat the observed labor supply of the individuals in the comparison group as an upper bound on the missing counterfactual. That is, by restricting the sample to the eventual SSDI applicants, I am able to estimate an upper bound of the average treatment effect on the treated. This is because if $E[y_i(1)|t_i = 1] \leq E[y_i(1)|t_i = 0]$ is true, then

$$|E[y_i(1) - y_i(0)|t_i = 1]| \leq | E[y_i(1)|t_i = 1] - E[y_i(0)|t_i = 0] \tag{1.2}$$

Moreover, if the individuals in the comparison group would have worked more as denied SSDI applicants at post-application ages, i.e. $E[y_i(1)|t_i = 0] \geq E[y_i(1)|t_i = 1]$, then the estimate of the right hand side of equation (1.2) would represent an upper bound of the average treatment effect $E[y_i(1) - y_i(0)]$.

The comparison group approach used to identify the upper bound of the employment potential of the denied SSDI applicants rests on the assumption that the only difference between the control group and the denied applicants is that the former are on average in better health and have more capacity to work. Table 1.1 provides evidence supporting this assumption in the sample selected for analysis in this paper. It is evident from Table 1.1 that the two groups are different in other characteristics as well as in health. Individuals in the comparison group are less likely to be school dropouts, female, and non-white and more likely to be married than the denied SSDI applicants. Individuals in the comparison group are on average have more work experience and more likely to have retiree health insurance from employment. I assume all these differences in characteristics would lead to individuals in the comparison group to be more capable of working (all else equal) than the denied SSDI applicants. Therefore, these differences in characteristics would reinforce the argument that the average employment of the comparison group can be considered to be an upper bound of the employment potential of denied SSDI applicants.

There could be differences in other characteristics that could make the individuals in the comparison group less likely to work in post-application years had they applied for SSDI. For example, the comparison group is on average wealthier than the denied SSDI applicants, making them less likely to work as they age. Factors like these may cause potential problems in identification of upper bound using the comparison group approach depending on the relative magnitude and importance of these factors.

Although, the employment comparison between the denied applicants and the appropriate control group gives a good measure of loss of employment potential of the denied applicants due to SSDI application, however, it does not necessarily give a causal estimate. Because, the treatment and control status was not assigned in a random way as it would have had it been a controlled experiment. There are unobservable characteristics – such as applicants having unobserved worse job prospects, lower taste for work, or higher severity of health issues than non-applicants – that may induce individuals to apply for SSDI and those characteristics most likely be correlated to labor supply making the SSDI application decision endogenous in any simple labor supply estimation function. As a result, I need to find an exogenous variation of SSDI application decision so that the estimate of employment difference of the denied SSDI applicants and non-applicants can be argued as causal effect of applying for, and being denied, SSDI benefits.

To identify exogenous variation in the application decision of the denied SSDI applications, I use the variation in FRA across different birth-cohorts brought about by the Social Security Amendments of 1983 and DDS level allowance rate of SSDI applicants for different age groups. The earliest claiming age for OASI benefits is 62. The OASI benefits at 62 depend on FRA along with other characteristics such as income history. The higher is the FRA the higher is the Social Security reduction factor applied to the PIA to reduce the benefits at 62. However, SSDI can be claimed at any age if you are insured for it and if awarded then the benefit amount is equal to the PIA calculated at the disability onset date using a different formula than that of calculating PIA for OASI benefits if SSDI is claimed before 62.

The actuarial reduction factor associated with OASI makes SSDI relatively more generous. This relative generosity provides greater incentives to apply for SSDI if insured than for OASI on or after 62. This incentive is proportionately greater for workers of different birth-cohorts as their FRA grows. Because, the actuarial reduction factors grows in a defined way as FRA grows. The change in FRA creates the relative generosity of SSDI to vary across birth-cohorts identified with the FRA. As a result, the incentive to apply for SSDI is proportionately greater for workers with higher FRA even before age 62.

Duggan et al. (2007), Li and Maestas (2008), and Coe and Haverstick (2010) find that the SSDI application rate is significantly higher for birth cohorts with later FRAs of men and women between the ages of 45 and 64, which suggests that the change in FRA is a sufficiently strong instrument for SSDI application decision in this paper. The crucial identifying assumption to satisfy the exclusion restriction of FRA in labor supply estimation is that the differences in employment of the different cohorts associated with different FRAs are only due to their heterogeneous incentives to apply for SSDI.

These heterogeneous incentives across cohorts for SSDI application are exogenously created by the Social Security Act of 1983 by changing the FRA across cohorts. It is well documented that these changes in OASI benefits across cohorts result into differences in LFP across cohorts identified by the FRA from age 62 onwards (see for example, Behaghel and Blau, 2012; Coe et al., 2013). These changes in OASI benefits, however, do not create any direct incentives to change the LFP before age 62, except through SSDI application, because workers cannot obtain retirement benefits until 62. In this paper I estimate the labor supply before age 62, so that I can exploit this exogenous variation in cohort differences identified with FRA that affect the employment only through the differential incentives to apply for SSDI.

Figure 1.1 presents the unconditional employment over ages 51 to 61, separately for the denied SSDI applicants and the non-applicants, of three different cohorts identified by FRA. For both denied applicants and non-applicants average employment declines with age; the trend for the denied applicants is steeper than for the non-applicants. The gap between the non-applicants and denied applicants shows not only the denied applicants have lower employment rate on average at all ages but also that the employment falls more as denied applicants grow older. It is important to note from Figure 1.1 that the employment of different cohorts of the non-applicants are essentially lying on top of each other, implying that the FRA increase, which only affected later cohorts, had no effect on employment by age for non-applicants.

The employment for the denied applicants across cohorts are also similar, but not as much as for the non-applicants, which is expected as different cohorts have different propensity to apply for SSDI. However, across cohorts there are no strong systematic differences in employment between the denied applicants and the non-applicants. A prominent systematic difference in unconditional employment between the two groups across cohorts would cast doubt on the assumption behind the exclusion restriction that the cohort differences do not affect labor supply directly. The pattern of employment of the denied applicants across cohorts suggests that the indirect effect of cohort differences on unconditional employment is a moderate one. However, this does not raise any concern over using the cohort differences identified with FRA as an instrument for the SSDI application decision of denied applicants. While analyzing employment trends over time researchers have shown educational and demographic composition across cohorts plays a role in explaining the trend (Banerjee and Blau, 2016). The regression model controls for education and demographic characteristics to account for these differences.

In this paper I use aggregate data from the Social Security administration providing information on number of SSDI applicants, awards and denials at the DDS level by gender and age groups for almost all the states in US from 1992 to 2013. I calculate the DDS level allowance rate, which is defined as the number of awards or successful applications as a percentage of number of applications in a given year. Figure 1.2 presents three different US maps by states for men age 50 to 54 for year 1992, 2000, and 2010, each one plots the same five different categories of generosity levels of the SSDI allowance rate. In cross section of states the color composition reveals that there are significant variations in SSDI allowance rates across states. The changing color pattern of the three maps for three different years reflects that there is significant variation of this allowance rate over time within a state. Figure 1.3 presents US maps by states for women aging 50 to 54 for year 1992, 2000, and 2010, each one plots the same five different categories of generosity levels of the SSDI allowance rate similar to Figure 1.2. For women as well the SSDI allowance rate has significant variation across states and over time.

Figure 1.4 presents two sets of scatter plots for men age 50-54 and 55-59 of SSDI allowance rate, number of application per year, number of awards per year, and number of denials per year against calendar year of 1992 to 2013 for selected states. For either age group of men the number of applicants rises over time with a small spike after the 2001 recession and a big spike around the great recession. However, the allowance rate over time does not show any systematic pattern. This pattern of SSDI application and allowance rates suggest that although the application behavior closely follow the overall macroeconomic outlook of the economy, the allowance rate depends on state specific factors other than the macroeconomic conditions. The same conclusion can be drawn from Figure 1.5, which presents the same data for women age 50-54 and 55-59.

Much of the variation in allowance rates across DDS offices can be explained by economic, health, and demographic factors (Gruber and Kubik, 1997; Strand, 2002; Duggan and Imberman, 2009; Coe et al., 2011). Researchers attribute the remaining unexplained variation to factors like interpretation and application of the universal disability determination criterion differently by the DDS offices, variation in administrative efficiency across DDS offices, budgetary consideration of the states, and state level politics and policy making (Strand, 2002; Iyengar and Mastrobuoni, 2014; Coe et al., 2011). Factors like administrative efficiency of DDS, apparent differential interpretation of federal disability determination criterion, and state level politics are exogenous to the labor supply decision of individuals.

In a given cross-section of states the allowance rates reflect a number of factors as argued above, like average health composition of the applicant pool. Within a state the variation over time in allowance rates reflect factors like changing economic environment. As a result this variation in allowance rates as itself is not meaningful in identifying the effect of SSDI application decision on labor supply. Gruber and Kubik (1997) use a dramatic rise in state denial rates in late 1970s, due to federal funding crisis of SSDI, as an exogenous variation of SSDI policy to measure its effect on labor supply.

The data and time period that I explore in this paper do not allow me to exploit such policy shocks to allowance rate like in Gruber and Kubik (1997). Instead I propose a measure to identify state's level of generosity towards awarding SSDI benefits to applicants in a way that is independent of economic, health, and demographic factors. I calculate allowance rate for age groups 45-49, 50-54, 55-59, and 60-64 for each state from 1992 to 2013. For each age group in a given state I compare the allowance rate for a given year to the allowance rate of that age group in the same state in next year. For example, I compare the allowance rate of 45-49 in Massachusetts in 1992 to the allowance rate of 45-49 in Massachusetts in 1993. I define a state to be more generous in a given year only if the allowance rate of all four age groups in the following year is strictly higher than the allowance rate of the corresponding age group for that state in the base year.

This measure of categorizing the states according to its generosity inherently controls for change in application rates over time due to changing economic environment. Demography at the state level is slow to change making it less of a concern as we are considering change over only a year. The reason I choose all four age groups is to control for the unknown average health composition of underlying applicant pool. It would be highly unlikely to have the allowance rates for a given state in a given year to be higher compared to the previous year among all age groups due to health differences. Rather it reflects change in the level of overall generosity of the state in awarding SSDI benefits to applicants.

I assume that people do not choose their state of residence on the basis of allowance rate of the DDS office of that state, which is a fairly reasonable assumption. Then controlling for state level observables I argue that the measure of generosity across states and over time within state exogenously determines the outcome of SSDI application. The more generous the state's DDS office is the lower the probability that an applicant would be denied benefits (all else equal).

The goal of this paper is to estimate a causal model of labor supply using SSDI applicants and eligible non-applicants of age 50 to 58 of the following form:

$$y_i = X_i\beta + \gamma DI_i + \nu_i$$

where y_i is the employment status of individual i measured two to three years after a reference age, X_i denotes observable characteristics at the reference age and changes in time-varying attributes in two to three years after the reference age that may influence labor supply at the point of measurement, $DI_i = 1$ if individual i applied SSDI first time at the reference age and never received benefits, and ν_i is an error term. The causal parameter of interest in this paper is γ which measures of effect of SSDI on employment of the denied applicants two to three years after the decision to apply for SSDI. This parameter represents the average effect of SSDI application on post-denial employment rates over application ages 50 to 58. Inference is hindered if some unobserved factors such as severity of health shock or low opportunity cost of SSDI application due to lack of labor force attachment affect both labor supply and SSDI application decision. Then essentially what I have is the following:

$$y_i = X_i\beta + \gamma DI_i - s_i + \varepsilon_i$$

where s_i represents unobserved factors, which are uncorrelated with any remaining idiosyncratic element ε_i. If $E[s_i|DI_i] \neq 0$, then in the regression of labor supply on observed factors and $\nu_i = -s_i + \varepsilon_i$ with ordinary least squares (OLS) gives a biased estimate of the average treatment effect of γ. In particular, the OLS estimate can be written as $\gamma - \big[E[s_i|DI_i = 1] - E[s_i|DI_i = 0]\big]$. If $\gamma < 0$ and if the unobserved characteristics are positively correlated with the SSDI application decision, then OLS overestimates the magnitude of the coefficient on DI_i and provides an upper bound of the potential labor supply loss of the denied SSDI applicants.

Exogenous variation of the SSDI application decision that is uncorrelated with the unobserved characteristics differences between the denied SSDI applicants and the control group would allow estimating the difference in employment between the two groups that is causal. I provide evidence above to show that birth-cohorts identified with differences in FRA and relative generosity of states in awarding SSDI benefits exogenously determine the SSDI application decision of the denied applicants. Using these set of variables we can instrument the indicator variable of the denied SSDI application in the labor supply equation and estimate the model using two-stage least squares

(2SLS) technique. The first stage of the two stage instrumental variable (IV) model can be written as:

$$DI_i = \lambda X_i + \delta Z_i + \eta_i$$

where Z_i includes the indicators for FRA between 65 and 66, FRA equal to or higher than 66, and FRA equal to 65 the omitted category. Z_i also includes an indicator variable for individuals living and applying for SSDI in the most generous states in terms of awarding the benefit to the SSDI applicants at the age of application. η_i is an idiosyncratic error term. The literature provides evidence on the existence of this first stage (see for example, Dugan et al. 2007; Li and Maestas 2008) and the exclusion restriction assumes that the change in FRA and relative generosity of states in SSDI award rate affect the employment only through the channel of SSDI application decision and its outcome. As I have more instruments than the endogenous variable I can test for the validity of the overidentification restriction while estimating the 2SLS model.

I estimate the upper bound of loss of employment two to three years after a reference age of 50 to 58 of the denied SSDI applicants using OLS and the causal estimate of the loss of employment using 2SLS for three different specifications. The specification (i) includes all the individual specific controls and age fixed effect and national unemployment rate for the US to control for the macroeconomic conditions. Specification (ii) includes all the controls included in (i) and adds unemployment rate of the state of residence of the individuals to capture the local macroeconomic environment. Finally, specification (iii) adds individuals' state of residence fixed effects to all the controls in (ii). Specification (iii) is the preferred specification in this paper.

The individual specific controls include demographic information like race, gender, marital status, and indicator for school dropouts and college education leaving the high school graduates as the omitted category. Individuals' taste for work is measured by three different variables: number of years worked till the reference age; indicator for having at least one job with more than 5 years in tenure; and indicator for having retiree health insurance from employer. Higher degrees polynomial of number of years worked is included in the labor supply equation. Indicators for different wealth quintile are incorporated in the labor supply equation with the first quintile as the omitted option. Previous research finds that care-giving for parents is an important factor in determining the labor supply of older workers. So, I include indicator whether an individual is a care giver for parents or not in the labor supply equation.

A variety of self-reported and medically diagnosed indicator variables for health outcome are included to control for the health status of the workers. Previous research on health and labor supply argues that self-assessed health status has contemporaneous reverse causality in labor supply equation (see for example, Bound, 1991b; Gruber and Kubik, 1997). However, medically diagnosed health outcomes are unlikely to have such problems. I include all medically diagnosed medical condition at the reference age and at the age when the labor supply is measured. I included the self-assessed health status in the reference age, which is well before the labor supply is measured. These self-assessed health status variables are strong predictor of the decision to apply for SSDI (Li and Maestas, 2008).

Researchers have documented a strong U-shaped relationship between the Body Mass Index[8] (BMI) and both self-assessed health status and mortality, controlling for other demographic factors (Kushner, 1993; Gruber and Kubik, 1997). Gruber and Kubik (1997) argue that being underweight for one's height is associated with increased risk of respiratory diseases and being overweight for one's height is associated with increased risk of cardiovascular disease, diabetes, and colon cancer. These diseases are most common among the SSDI applicants (Social Security Administration, various years). Instead of self-assessed health measures at the age when labor supply is measured I use the BMI to control for health status at that age in the labor supply equation. Figure 1.6 presents

[8] BMI = (Body mass in kilograms)/ (Height in meters)^2. Gruber and Kubik (1997) argue that BMI is an objective anthropometric measure of disability status.

the relationship BMI, self-reported poor or fair health, and employment. Considering the non-linear relationship between BMI and employment, I include a quadratic form of BMI at the reference age into the labor supply equation. Gruber and Kubik (1997) define most severe case of disability by BMI less than 20 and greater than 34. Using this definition of disability I also include transition in health indicator by moving in and out of this disability measure from the reference age to the age at which the labor supply is measured.

1.4 Empirical Results

Table 1.2 presents the average employment of the SSDI denied applicants, the control group of non-applicants, the awarded applicants, and the never-applicants. The table provides the average employment at a reference age between 50 and 58 (inclusive); two to three years before the reference age; and two to three years after that same reference age. For the denied SSDI applicants in this paper the reference age is the age of application. Column 4 of this table presents the average unconditional difference in employment between the denied SSDI applicants and the control group. It shows that the unconditional employment of the denied applicants are 18 percentage points lower than a comparison group of non-applicants two to three years before the application. This pattern of employment of the rejected applicants before the application is consistent with the findings of other papers in this literature (see for example, von Wachter et al., 2011).

Table 1.2 also shows that the denied SSDI applicants are on average 56 percentage points less likely to be working during the period of application and are on average 54 percentage points less likely to be working two to three years after the application compared to the comparison group of non-applicants, though this comparison does not control for the observable differences between these groups. The employment of the denied applicants falls significantly during the time of SSDI application and remains at that low level two to three years from that time period.

Table 1.3 presents the regression estimates of the instrumental variables from the first stage of the 2SLS model for labor supply. All the estimates are fairly stable over all three specifications. The estimates in the preferred specification (iii) show that compared to the birth-cohorts with FRA equal to 65, the cohorts with FRA more than 65 and less than 66 are 3 percentage points more likely to be denied SSDI applicants, although the parameter is not statistically significant. Compared to the omitted category, cohorts with FRA equal or higher than 66 are 17 percentage points more like to be denied SSDI applicants and this parameter is significant at 1 percent level. The parameter of the indicator for relatively more generous states in terms SSDI allowance rate over time is -0.02, which has the expected negative sign. However, this parameter is not statistically significant. The first stage is fairly strong with 28 percent of the variation of the indicator of denied SSDI application explained by the model. The results of the weak identification test show that the preferred specification rejects the null that the first stage is weakly identified at 10 percent level.

The main findings of the paper are presented in Table 1.4. The OLS estimates represent the estimates of the upper bound of the effects on employment of SSDI application on denied applicants. Estimates from the preferred specification (iii) shows that the potential loss of employment of the denied SSDI applicants is at most 49 percentage points. This estimate is statistically the same across all three specifications. The IV (2SLS) estimates present the causal point estimate of the effect on employment of the application decision of the denied SSDI applicants. The IV estimates show that the SSDI application decision causes a 36-percentage-point decrease in the employment of the denied SSDI applicants of ages 50-58 two to three years after filing the application. Thus, adjusting for differences in the unobserved characteristics between the denied SSDI applicants and the comparison group through IV has a substantial impact on the estimated labor supply effect of SSDI.

Unobserved factors like lower opportunity cost of SSDI application or severity of health shocks are positively correlated to SSDI application decision. OLS overestimates the causal effect on employment of SSDI application on denied applicants due to these unobservables by 13 percentage points, relative to the IV estimate. As the OLS estimate represents the upper bound of the effect of SSDI application decision of the denied applicants, I argue that unobserved factors such as the severity of health shocks or low labor market opportunities of the denied applicants account for this 13-percentage-point reduction in employment rate for the denied applicants.

Table 1.4 also reports the p-value from the overidentification test. These values are fairly high for all the specifications implying that I cannot reject the null that the overidentification restrictions are valid. Although the overidentification test does not provide any sense of the validity of the instruments in term of the underlying identifying assumptions, failing to reject the null of this test validates the use of more than one instrument for the single endogenous variable in the model.

My estimate of a 36-percentage-point reduction in employment is in line with correlational estimate that other researchers have found in the literature. Using the matching in observable pre-application characteristics, von Wachter et al. (2011) find that in 1990s the employment rate of the denied SSDI applicants of age 45-64 is about 30 percentage points lower than the rate of the non-applicants two to three years after the application. Although they cannot claim this estimate to be causal as their identification strategy cannot isolate the effects of unobserved characteristics, they argue that this is a close approximation of the true effect. The causal estimate I find in this paper for the age group 50-58, which is a subset of the population analyzed by von Wachter and others, falls between their matching estimate and the OLS upper bound estimate that I find of the effect on labor supply for the denied SSDI applicants.

Autor et al. (2015) estimate that the effect of waiting time of denied SSDI applicants on employment reduction in the long-run is around 6 percentage points, which they interpret as the human capital deterioration effect. They do not provide the causal estimate for this human capital effect for the short-run. Although it is expected that the human capital effect is higher in the short-run than in the long-run, without knowing the changing pattern over time, it is hard to extrapolate the human capital effect for the short-run. However, the findings of Autor and others suggest that the total effect of SSDI that I find for denied applicants on employment in the short-run comprise a relatively larger fraction of work disincentive effect than the human capital deterioration effect. Hence, initiatives to reduce the work disincentives of the SSDI determination process would result into a significant first order improvement of labor supply of the denied applicants. The findings of this paper suggest that the existing literature does not fully capture the negative labor supply effects of SSDI on the applicants.

1.5 Sensitivity Analysis

I use self-reported survey data on SSDI application, award, and denial decision in this paper. These self-reported data always have this concern about under-reporting or misreporting the true outcome causing measurement error in the variable of interest. The indicator of denied SSDI application is the key explanatory variable in this paper, which is vulnerable to under reporting by the survey respondents. For example, if someone reports the application decision but fails to report the award decision, then I might incorrectly include that person into the denied SSDI applicant category. This person has larger negative effect on post-application labor supply compared to the rejected applicants due to being less healthy as well as for the negative effects of SSDI benefit receipt. If this misreporting is significant in the sample, then the estimated effect of SSDI application would be biased upward in absolute value. Similarly, if I misspecify a Supplemental Security Income (SSI) applicant as SSDI applicant, then the estimates would be biased upward, because, SSI applicants generally have very low labor market attachment to begin with.

The miss-specification of SSI as SSDI is not a big concern in HRS data. Khan et al. (2016), merging the HRS with the administrative SSA summary earnings data, find that the HRS reporting of SSDI and SSI status can be verified with job history data in HRS. Using the methodology in that paper I am able to isolate the SSI applicant from the analysis of this paper. The other concern of misreporting of SSDI beneficiary as SSDI denied applicant is hard to verify directly without the

SSA disability record files. However, I can replicate the Bound (1989) approach of comparing the SSDI beneficiary and SSDI denied applicants. If the SSDI denied applicants have a misreporting problem in my sample, then I would get an underestimate of the upper bound of labor supply effect of the beneficiaries compared to what other researchers have found.

Column 8 of Table 1.1 presents the differences in characteristics between the SSDI beneficiaries and denied applicants. The groups have no statistically significant differences in demographic, wealth, and taste for work characteristics. However, the denied applicants are healthier than the SSDI beneficiaries both in terms of self-reported health status and medically diagnosed conditions. Bound (1989) also found similar differences in characteristics between two groups using a completely different sample of men in 1970s. Comparing the employment of these two groups Bound found that benefit receipt led to a 29 percent reduction in the employment rate of the beneficiaries. Using the much more recent Survey of Income and Program Participation (SIPP) from the 1990s, Chan and van der Klaauw (2008) replicated the Bound (1989) approach using administrative disability data that comes from SSA's "831 file". They found that the employment rate of awarded SSDI applicants is 19 percentage point lower than the denied applicants in 1990s for a sample that includes both men and women. The reduction in estimate compared to what Bound found is argued to be largely due to the increased generosity of SSDI policy and change in demographic composition of applicants from the 1970s to the 1990s.

Table 1.2 shows that the unconditional employment rate of the awarded SSDI applicants is 22 percent lower than the rate of denied applicants in the sample selected in this paper. Using specification (iii), the OLS estimates in my sample of SSDI beneficiaries and denied applicants show that the benefit receipt reduce employment rate of the awarded applicants by 21 percent[9]. As the replication of the Bound (1989) approach using the sample of denied SSDI applicants used in

[9] The full set of this regression results is not reported in this paper, but is available upon request.

this paper produces a similar estimate of the one that Chan and van der Klaauw (2008) found in their paper using administrative data, I believe that the problem of misreporting is not a concern for the sample of denied SSDI applicants selected in this paper.

To make sure that my findings are not driven by any specific characteristics of the sample, I estimate the preferred specification of the model using both OLS and 2SLS for different subsamples identified for specific considerations. The results of this sensitivity analysis are presented in Table 1.5. During the great recession the SSA observed a big spike in SSDI applications and it was difficult for people to get back in to the labor market if they were out for any reason. Considering that these factors might influence the result, I dropped individuals who applied for SSDI around 2008. The OLS estimate of this subsample is a little higher than the full sample estimate; however, the IV estimates are the same. There are not a lot of individuals in the full sample who are observed during the great recession and the results in this paper clearly are not affected by the outcomes of the great recession.

Considering the demographic differences between the denied SSDI applicants and the comparison group, I create separate subsamples without widows, without singles, and without non-whites. Both the OLS and IV estimates using these three different subsamples are very similar to the estimates of the full sample. However, the IV estimates are not significant in subsamples without the singles and without the non-whites. The singles and the non-whites are 32 percent and 26 percent of the full sample. Thus, it is not surprising that dropping a large proportion of the small full sample to get to those subsamples of married and whites results into the IV estimates being imprecisely estimated. Over all, the sensitivity analysis presented in this section indicates that the findings of this paper are robust.

1.6 Conclusions

In this paper I identify the causal effect of the SSDI application process on the employment at ages 50-58 of denied applicants two to three years after filing the application. For the identification, I use a comparison group of non-applicants of age 50-58 whom I observe in the data eventually applying for SSDI in their 60s. I provide evidence that this group of late applicants represent a natural control group for SSDI denied applicants. Using the variation in FRAs as a result of the Social Security Act of 1983 and a measure of generosity of state SSDI allowance rate that exogenously determine the decision to apply for SSDI of the denied applicants, I find that the application process reduces the employment rate of the denied applicants by 36 percentage points. I interpret this effect as a loss of employment potential of the denied SSDI applicants *caused* by their decision to go through the process of SSDI application without successfully receiving benefits.

Although my estimates represent the short-run effect of SSDI on employment, there is evidence in recent literature that suggests that this effect persists even in the long-run, especially for older workers close to their end of their careers. The magnitude of the effect is expected to decline over time, because, in the long-run, the work disincentive effect disappears as the denied applicants are done with their appeal and reapplications. The only effect that remains in the long-run is through the human capital deterioration channel.

My findings also suggest that unobserved factors like severity of health condition or low labor market opportunities of the denied applicants account for another 13-percentage-point reduction in the employment rate. It is hard to disentangle the contribution of different factors into this 13-percentage-point reduction in employment of denied applicants, but this result suggests that some of the denied applicants are incapable of doing SGA. However, as these people are not disabled enough to receive SSDI, they are left out of the Social Security safety net. Further research is required to think about policy considerations to improve the work capacity and welfare of this group of people.

In United States the denial rate of SSDI applications, combining at all adjudicative levels, has risen from 45 percent in 2000 to 72 percent in 2013. The denial rate for medical reasons at the medical adjudicative level has also risen from 38 percent to 50 percent during this time period. In absolute terms, the number of applicants has doubled during that time period and about 1.8 million applicants were denied SSDI benefits in 2013.[10] More people than ever are applying for disability benefits, removing themselves from the labor market for months or even years, and then many are forced to re-enter when they are denied enrollment. The findings of this paper suggest that the process of SSDI application causes a substantial loss of employment potential to the denied applicants. The magnitude of the effect is so significant that it suggests that policymakers revisit the disability determination process to make it shorter and also make reforms to reduce the work disincentives while applying and waiting for SSDI determination. Moreover, these findings should facilitate policymakers to think about the importance of resources needed for smoothing the transition of denied SSDI applicants back into the labor force, especially for older workers close to retirement.

[10] Annual Statistical Report on the Social Security Disability Insurance Program, 2014.

CHAPTER 2

"How Do Subjective Longevity Expectations Influence Retirement Plans?[†]*"*
(with Matthew S. Rutledge and April Yanyuan Wu)

The rapid increase in life expectancy over the past several decades – remaining life expectancy for the 65-year-old male cohort has increased from 14.7 years in 1980 to 18.7 years in 2012 (U.S. Social Security Administration, 2012) – has changed the calculus behind Americans' retirement decisions. A longer retirement increases the funds needed to support one's lifestyle, but assuming healthy life expectancy has also increased, workers should be better able to continue working (Munnell and Sass, 2008; Munnell et al., 2008).

An extensive literature has documented the ways in which financial and health shocks have affected retirement expectations and the ability of older workers to continue working. But less attention has been paid to how information about the dramatic increase in longevity has been transmitted to individuals approaching retirement, altering their perceptions about their ability, willingness, and need to work at older ages. Using the *Health and Retirement Study* (HRS) and an Instrumental Variables (IV) approach, this study examines how subjective life expectancy influences planned retirement age and expectations of working at older ages, and how individuals update those expectations with new information.

Individuals who expect to live longer are expected to retire later, for at least two reasons. First, a longer life requires greater wealth to finance consumption (Chang, 1991; Kalemli-Ozcan and Weil, 2010). Second, greater longevity is likely associated with better health during one's working years,

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making continued work more feasible. But the literature examining the relationship between subjective longevity and retirement is not yet settled. Hamermesh (1984) finds no consistent evidence that longevity expectations explain the work effort at ages 55-70. Hurd, Smith, and Zissimopoulos (2004) find that only those who hold the lowest self-assessed probability of living to 85 are likely to retire early. O'Donnell et al. (2008) show the opposite: those who are most pessimistic about their longevity are least likely to retire between waves using the English Longitudinal Study of Ageing. Bloom et al. (2006) find that subjective life expectancy (SLE) has little effect on the probability of working at any given time.[11]

Our study builds on this literature in two ways. First, we examine how the *change* in subjective life expectancy alters retirement plans, which the literature has not previously explored. The study emphasizes how receiving new information about one's own mortality induces an individual to reconsider his retirement plan. Second, we compare the relationship between SLE and both *actual* and *expected* retirement behavior. Actual retirement behavior can deviate from plans for retirement when shocks arise: a new diagnosis or an acute medical episode, a job loss, the unexpected death of a spouse, or the need to care for a loved one. Retirement expectations – as expressed in survey questions about the age at which one expects to retire, or the probability one works to a milestone age – better reflect desired labor supply because they are set before these shocks occur. The only prior study to examine expected retirement age is van Solinge and Henkens (2009), for a smaller sample of Dutch workers.

Concerns remain, however, that the correlation between SLE and retirement plans may be driven by a third factor, such as optimism about life in general (and not just longevity). Moreover, the previous literature points out that SLE responses are bunched at focal points, leading to measurement error. We use an IV approach to address these issues, using lessons from the

[11] Delavande et al. (2006), focusing on Social Security claiming rather than labor supply, find that subjective life expectancy is associated with a significant decrease in the probability of claiming at age 62.

burgeoning literature on decision heuristics. Coming to grips with one's own mortality is unpleasant, and centering those expectations may be difficult given the secular trend in mortality. Behavioral economics suggests that in the face of a difficult decision, individuals start with a readily available answer and then "anchor" to that initial answer; that is, their subsequent answers depend on their initial answer (Tversky and Kahneman, 1974). Hurd and McGarry (1995) suggest one's parents' experience serves as this anchor: the longer that parents lived (or are still living), the higher the individual's subjective life expectancy. For this reason, we use parents' current age or age at death instruments for SLE; this approach is also suggested by Bloom et al. (2006).

In our dynamic analysis, parents' current ages or death ages serve the role of new information: when a parent survives to another benchmark age, the child likely increases his perceived chances of survival to that same age, and vice versa if the parent dies earlier; indeed, Hurd and McGarry (2002) report that subjective life expectancy decreases upon a parent's death. We use the variation in parents' survival or death between HRS interviews to explore whether the change in a middle-aged child's life expectancy is correlated with a change in retirement expectations.

The estimates in this paper suggest a large and statistically significant relationship between subjective life expectancy and retirement expectations. Respondents who are one standard deviation more optimistic about their survival to age 75 or 85 are 4 to 7 percentage points – or about 10 percent to 24 percent – more likely to be planning to work full time into their 60s, and they expect to work five months longer on average. To put these estimates in perspective, individuals of the highest tercile of the difference between SLE and OLE expect to work 4 months more than a median person, and 10 months longer than someone in the lowest tercile. These results are fairly consistent across specifications but are somewhat stronger for women. We also find that increases in SLE over time for a given individual are associated with increases in his planned retirement ages and planning to work at ages 62 and 65. Actual retirement behavior also increases with SLE, but the relationship is somewhat weaker, similar to previous studies.

These results further our understanding of the role of information and expectation formation on retirement decision-making. They suggest that further gains in the average retirement age will require not just continued gradual increases in longevity, but increases in longevity expectations. This emphasizes the role of information in communicating the risks of living “too long” relative to one’s retirement savings.

2.1 Data and Methodology

2.1.1 Data and Sample

This project uses the 1992-2010 waves of the HRS to examine the relationship between retirement plans and subjective life expectancy. The HRS is a longitudinal data collection effort begun in 1992 with a cohort of about 10,000 individuals between ages 51 and 61 (i.e., born between 1931 and 1941). Additional cohorts have been enrolled over time so that the survey includes 30,500 individuals in 2010 and can be weighted to be nationally representative of the population over the age of 50. Respondents are interviewed every two years.

For our primary analysis on expectations about retirement and working at older ages, the sample is restricted to individuals age 50 through 61, who are in the labor force and have non-missing values for both retirement and longevity expectations.[12] Figure 2.1 summarizes the sample construction. Other than the age criterion, the exclusion of people from the labor force is the most restrictive criterion, eliminating 21 percent of the age-eligible sample. Retirement plans of those who are temporarily out of the labor force may still be of interest, but their expectations are not collected. Their exclusion may bias our sample toward those with later expected retirement ages, but when we compare the demographic and socioeconomic characteristics of HRS respondents in our sample to those who are excluded, we find mostly statistically insignificant differences, and even the few

[12] Proxy interviewees and Social Security Disability Insurance (SSDI) recipients are excluded from our analysis. We exclude individuals who receive SSDI because they have very limited labor market attachment and are converted to retirement benefits at their FRA automatically, and thus the retirement expectation question is irrelevant.

significant differences are of low magnitude.[13] These comparisons suggest that those who are missing retirement expectations are a random sample of relevant older individuals, at least with respect to observable characteristics.

We also examine the relationship between the SLE and actual retirement behavior, to test the hypothesis that subjectivity life expectancy is more highly correlated with retirement expectations because shocks may interfere with actual retirement. In these analyses, the sample is restricted to those respondents observed both before and after age 62.[14]

2.1.2 Empirical Strategy

When investigating retirement expectations, the study focuses on three outcome variables. The first outcome is based on answers to an HRS question about when the respondent plans to retire; the most common responses are ages 62 and 65, though other ages are also common.[15] We measure the expected retirement age using the respondent's age on the birth month of the year in which he plans to retire.[16] HRS also asks for the respondent to estimate his probability of working full-time at or after 62 (the Early Entitlement Age) and 65; these variables are our second and third outcome measures.

The key independent variable is a measure of longevity expectation. HRS asks each respondent their probability of living to ages 75 and 85.[17] The RAND version of the HRS standardizes these

[13] Sample selection results are available from the authors upon request.
[14] The sample is further refined for each outcome variable. For the expected and actual retirement age regressions, the individual must have an observed retirement date. For the probability of working at age 62 or 65, the individual must be sampled at or after that age.
[15] Approximately 4.5 percent refuse to answer or do not have a plan, while 6.8 percent report that they will never retire. We exclude those who do not plan to retire from the analysis, but include them in the robustness checks by recoding their expected retirement age as age 70. The robustness tests show that this decision does not materially affect our results. We also top-code about 7.7 percent of individuals with retirement ages beyond 70 as retiring at 70; in robustness checks, the results are largely consistent using the non-top-coded expected retirement age.
[16] We also estimate regressions that assume that the individual plans to retire in December of the expected retirement year reported; see robustness checks.
[17] In HRS waves 5-7 the SLE question asks respondents under 62 about their probability of living to age 80 rather than 85. Normalizing the SLE using the OLE from the life table should capture most of the difference between waves asking about age 80 and waves asking about age 85, but we drop these waves from the

probabilities using the actuarial projections of longevity reported in the Vital Statistics life tables, by birth cohort and sex. The resulting measure is the difference between subjective and objective life expectancy (OLE): a value greater than zero indicates the individual has a higher probability than his average peer of living to the given age; a value less than zero indicates a more pessimistic expectation. This standardization accounts for both the differing expectations by age – a 62-year-old is likely to have a more accurate view of his probability of reaching age 75 than a 51-year-old – and the secular trend toward longer lives. Our preferred specification uses the standardized version of each variable (separately), but we also report results that use the SLE by itself.

The concern with both subjective life expectancy and its standardized version is classical measurement error which leads to attenuation bias in the estimation: respondents sometimes report a higher probability of living to 80 than 75, and focal points like 0, 0.5, and 1 dominate the probability values (Hurd and McGarry, 1995; Hurd et al., 1998; Bassett and Lumsdaine, 2001; Bloom et al., 2006). We adopt the instrumental variables (IV) model suggested by Bloom et al. (2006), in which parents' current ages or ages at death as instruments for SLE. Hurd and McGarry (1995) show that SLE is highly correlated with parents' death ages, so the instrument is likely to be strong. In a follow-up study, Hurd and McGarry (2002) also find that the death of a parent of the same sex has a larger impact on SLE than the death of a parent of the opposite sex; our model allows for this difference by controlling for ages of the same-sex and opposite-sex parents separately.[18]

analysis. The sensitivity analysis, however, shows that our results are robust even if we include these 3 waves in the sample.

[18] There is little consensus in the literature on the effects of parents' longevity on their children's mortality. Vandenbroucke et al. (1984) and Van Doorn and Kasl (1998) find no correlation between the number of parents which a middle-aged person still has alive and that person's longevity. On the other hand, Goldberg et al. (1996) find that parental survival to age 75 increases the probability that 50 year olds survive to age 75. A recent paper of Portner and Wong (2013) also finds strong evidence that individuals with longer-lived parents exhibit lower mortality risk using the HRS data, even after controlling for health and behavioral variables of the offspring.

Further, the IV approach helps address endogeneity concerns. If some unobserved factors are correlated with both the SLE measure and with retirement expectations, then endogeneity may arise. For instance, a generally optimistic person may overestimate his life expectancy as well as his working horizon. In such cases, ordinary least squares (OLS) would be biased towards a positive value. The validity of using parents' current ages or ages at death as instruments relies on the fact that each parent's longevity should impact middle-aged childrens' retirement expectations only through the channel of the offsprings' SLE.

The first set of econometric models examine retirement expectations in a static framework: what is the relationship between retirement expectations and SLE? The functional form of the reduced form regression is:

$$RetExp_{it} = \alpha_0 + \alpha_1(SLE_{it} - OLE_{it}) + \gamma X_{it} + \varepsilon_{it} \tag{2.1}$$

where $RetExp$ is the retirement expectation measure and (SLE-OLE) is the difference between subjective life expectancy and objective life expectancy for person *i* in HRS wave *t*. For all outcome variables, the model is linear: OLS for the expected retirement age and the probability of working full-time at or after 62 and 65.[19]

In one model, we estimate the extent to which the evolution of subjective life expectancy over time, particularly upon receiving new information about one's own mortality, induces an individual to reconsider his retirement plan. This "updating" model, which exploits the longitudinal nature of the data set, includes individual fixed effects (FE) in order to capture time-invariant unobservable characteristics that might be correlated with the participation decision. The specification takes the following form:

[19] Given that the mean of the probability of working is 0.39 and thus not close to zero or one, the linear probability model likely does not differ substantially from a probit or logit specification. The expected retirement age is fractional: someone who expects to retire at age 64 and is born in March would be assigned the value of 64.75, as they would be 64 and 9 months in December of that year.

$$RetExp_{it} = \beta_0 + \beta_1(SLE_{it} - OLE_{it}) + \gamma X_{it} + \psi_i + \varepsilon_{it} \quad (2.2)$$

where ψ_i is the unobserved time-invariant individual fixed effect.

To examine the causal relationship between retirement expectations and SLE, we estimate a Two Stage Least Squares (TSLS) model with parents' longevity as instruments. While the individual fixed effect model takes into account time-invariant individual unobservable heterogeneity**,** the IV model has the advantages of accounting for time-varying unobservables and measurement error in SLE.

The first stage estimates the effect of parents' longevity on SLE:

$$\begin{aligned}(SLE_{it} - OLE_{it})& \\ &= \delta_0 + \theta_s Alive_{st} + \theta_o Alive_{ot} \\ &+ \sum_{j=1}^{3} \big[\lambda_{sj} Alive_{st} AgeCat_{jst} \\ &+ \eta_{sj}(1 - Alive_{st}) AgeCat_{jst} \\ &+ \lambda_{oj} Alive_{ot} AgeCat_{jot} \\ &+ \eta_{sj}(1 - Alive_{ot}) AgeCat_{jot}\big] + \xi X_{it} + \nu_{it}\end{aligned} \quad (2.3)$$

and the second stage substitutes $(\widehat{SLE_{it} - OLE_{it}})$ for $(SLE_{it} - OLE_{it})$ in equation (2.1).[20] The first stage uses a set of instruments that includes indicators for whether the same-sex ($Alive_{st}$) and opposite-sex ($Alive_{ot}$) parents are still alive, and separate categorical variables for their ages conditional on either being alive or dead by wave *t*, as well as the vector of exogenous variables X. Whether the IV estimate is bigger or smaller than the OLS estimate is an empirical question. On the one hand, correcting for the positive bias of the OLS due to endogeneity concerns leads to

[20] The fixed effects in an IV model soak up much of the variation and lead to insignificant results, which are not reported here.

smaller IV estimates. On the other hand, the IV estimates may be larger than the OLS estimates due to the presence of classical measurement error (e.g., Hyslop and Imbens, 2001).

The vector X includes a full set of personal and family characteristics that previous studies have found to affect the retirement decision (e.g., Haider and Loughran, 2010). These include basic demographics, such as marital status, sex, race and Hispanic origin, educational attainment, and region of residence, household income and wealth quintiles, and an indicator for the number of children throughout one's fertility history (three or fewer versus four or more). We include indicators for working in a blue-collar industry and being self-employed, having defined benefit or defined contribution pension plans, and the national unemployment rate in the survey year, to capture current working conditions. X also includes an indicator for whether the individual has a financial planning horizon of greater than "the next few years." As risk preference may affect retirement expectations, we include categorical variables for risk tolerance, omitting the most risk averse category. Finally, all models include age dummies and dummies for cohorts grouped together by FRA. The FRA cohort dummies will capture the framing effect of focal retirement ages provided by Social Security program parameters (Behaghel and Blau, 2013).

Because retirement is often related to health insurance coverage (Gruber and Madrian, 1995), we include variables that summarize one's current health coverage: indicators for the source of health insurance (own employer, government, spouse's employer, and other private) and an indicator for the availability of retiree health insurance. X also includes a comprehensive list of health status and health behavior variables: an indicator for reporting fair or poor health, an indicator for reporting any limitations in the Activities of Daily Living module, separate indicators for whether the individual has high blood pressure, diabetes, cancer, lung disease, heart disease, stroke, arthritis, and psychological problems, and variables that capture smoking history and drinking habits.

As couples generally prefer to synchronize their retirements (e.g. Coile, 2004), we include controls for spouse's age, work status, and pre- and post-retirement health insurance coverage. Given the instrument is the presence of living parents and their age, conditional on their mortality status, we also control for ways in which one's parents might directly influence retirement behavior. Over the long run, parents may affect adult children's retirement decisions through their own socioeconomic status and financial knowledge; to proxy for these factors, we control for each parents' educational attainment. In the more immediate run, assisting a parent with lifestyle and health needs decreases the net benefits of working, making early retirement more likely (van Houtven et al., 2013); we therefore include an indicator for any time spent caring for a parent or parent-in-law, and an indicator for having siblings, to reflect the potential to share the caregiving role.

Retirement expectations and their relationships with SLE may differ by gender, because of differing attachment to the labor force. As in Haider and Loughran (2010), we estimate each equation separately for men and women.

2.2 Results

2.2.1 Summary Statistics

Table 2.1 reports the summary statistics (means and standard deviations) for the full sample, using the values from each person's first wave in the sample. The expected retirement age is 63.1. The average probabilities that the respondent gives for working full time are 48 percent at age 62 and 28 percent at age 65. On average, 68 percent expect to live to at least age 75, and 47 percent expect to live until at least age 85. The former expectation is close to the objective life tables, as shown by a mean difference between SLE and OLE of just below zero, -2.25 percentage points. The latter expectation, however, differs from the OLE by slightly more, but in this case is optimistic: the average respondent is 5.22 percentage points more optimistic than his actuarial projection.

Table 2.1 also summarizes the within-individual variation for the outcome variables and subjective life expectancy measures, which is useful for evaluating the magnitude of the fixed effects estimates. We observe an evolution of expectations within individual over time, with a within-individual standard deviation of expected retirement age of 1.6 years, and within-individual standard deviations of 33.71 and 19.21 for the probabilities for working full time at age 62 and 65, respectively. Over time, individuals also update their SLE, as the within-individual variation in the SLE measures is sizeable.

Table 2.1 displays the mean for each instrumental variable, based on the respondents' parents' mortalities. In the full sample, 37 percent of respondents have a living parent of the same sex, and 35 percent of parents of the opposite sex are still living; in each case, the plurality of respondents' parents is between ages 75 and 85. The death ages for parents who have passed away are evenly distributed between ages 66-75 and older than 75.

The other three columns in Table 2.1 report summary statistics for three terciles of the difference between SLE and OLE for age 75, to examine the unconditional relationship between retirement expectations and subjective life expectancy. Longevity expectations vary greatly across these terciles. Only 37 percent of individuals in the least-optimistic tercile expect to live to 75, half of their OLE of about 71 percent (36.68 plus 34.27). In the most optimistic tercile, individuals are about 26 percentage points more optimistic than their OLE of 94.5 percent. The means of the outcome variables by tercile indicate a correlation between SLE and retirement expectations: the expected retirement age and the probability of working full time at or after 62 and 65 all monotonically increase with longevity expectations, though the correlation between expected retirement age and SLE seems fairly small.

2.2.2 Main Results

The first stage results for predicting SLE from parents' age in life and death are reported in Table 2.2. The set of instruments (including exogenous regressors) has an F-test of 15.9 for the age-75 expectation regression (left columns) and 14.0 for the age-85 expectation regression (right columns), rejecting the null of weak instruments. Furthermore, the Hansen J test statistic indicates that the instruments are uncorrelated with the error term in the regression of interest and are therefore appropriately excluded from the second stage.

As expected, the strongest predictors of subjective life expectancy are the indicator of the same-sex parent being currently alive and the indicators for the age at death for the parent of the same sex, while the age of living parents of either gender are mostly insignificant. The first stage indicates that subjective life expectancy is lowest among people whose same-sex parent died between 51 and 65, which almost entirely coincides with our sampling window, suggesting that the parents' experience at the respondents' current age is most relevant to their behavior in the near term. Interestingly, a parent dying at a younger age (50 or less) is associated with a greater probability of living to 75 or 85; respondents may write off these deaths as "flukes" or accidental.

Table 2.3 reports the results from regressions where the expected retirement age is the outcome variable. Two sets of regressions are displayed: the first set of three columns controlling for longevity expectations (SLE – OLE) at age 75, and the second set of three columns controlling at age 85. In each set, the first column reports the results from an OLS model (estimating equation 1); the second column is from an individual fixed effects model (estimating equation 2); and the third column is from an IV model without individual fixed effects (estimating equation 3).

The key variable (top line) in the second stage is the difference between subjective and objective life expectancy to either age 75 or age 85. In both cases, the OLS estimate is positive and highly statistically significant. The FE model is also statistically significant, though the magnitude is roughly 30 to 40 percent of the OLS estimate. The IV model's estimates are of larger magnitude than either the OLS or FE and are also statistically significant. This finding of a larger IV estimate fits the pattern of classical measurement error.

Though standardizing the subjective life expectancy measure with the actuarial expectation reduces concerns about longevity differences by age and secular trends in longevity, the coefficient on this difference can be difficult to interpret. From Table 2.1, the standard deviation of the standardized age-75 expectancy is 27.75, while the age-85 expectancy is nearly 31.75. Therefore, a one-standard-deviation increase in the subjective life expectancy to age 75, relative to the individual's objective longevity, is associated with an increase in the expected retirement age of 0.19 years, or about 2.3 months, according to the OLS estimate, and 0.39 years, or 4.7months, according to the IV estimate. The magnitudes are similar for standardized subjective expectancy of living to 85: a one-standard-deviation increase is associated with retiring between 2.6 and 4.6 months later. According to the FE model, as SLE evolves, individuals update their planned retirement ages: a one-standard-deviation increase in the within-individual subjective life expectancy to age 75, relative to OLE, is associated with an increase in the expected retirement age of 0.03 years, or about 0.4 months.

The summary statistics by tercile also aid in evaluating the magnitude of the estimates. Individuals in the highest tercile of the difference between SLE and OLE are about 25.1 percentage points more optimistic about living to age 75, relative to their actuarial projection, than someone in the middle tercile, and 60.6 percentage points more optimistic than someone in the least-optimistic tercile. Our IV estimates suggest that these highly-optimistic individuals expect to work 4 months more than a person around the median, and 10 months more than those of the most pessimistic tercile.

The relationship between other variables and expected retirement age are largely in line with other studies (Appendix Table AT2.1). Women, Hispanics, blue-collar workers, those with higher incomes and wealth, those with retiree health insurance and DB pensions, and those in worse health expect to retire earlier. A retired spouse, especially if he or she has employer-sponsored health retiree insurance before and after retirement, is also associated with earlier retirement. Whites, the divorced, the higher educated, the uninsured, the self-employed, those with moderate risk tolerance, and those with a spouse who is unemployed or in poor health expect to retire later. These estimated correlations are almost identical in magnitude and significance across specifications.

The IV model estimates imply that, a one-standard-deviation larger difference between subjective and objective life expectancy at age 75 is associated with an increase in the probability of working full time at or after age 62 by 5 percentage points, or about 10 percent of the mean probability of working full time at 62 (48 percent). That same subjective life expectancy is associated with a 6 percentage point, or 21 percent increase over the mean expected probability of working full time at 65 (28 percent). For a specific individual, as his own SLE increases relative to the OLE at age 75 by one-standard-deviation, his expected probability of working full time at or after age 62 increases by 1.6 percentage points, and for working at age 65 by 1.4 percentage points. Overall, a higher SLE leads to a higher expectation of working at older ages.

2.2.3 *Sensitivity Checks*

Because men and women have different attachments to the labor force, and different (though converging) life expectancies, Table 2.4 presents estimates of the relationship between these two variables by gender. OLS, FE and IV estimates are statistically significant for the probability of working full time at 65 for both men and women and are significant for the probability of working full time at 62 for women as well. For each measure, the relationship between the SLE and OLE difference and retirement expectations is stronger for women, though the differences between each estimate are not statistically significant.

One concern with using SLE standardized by the OLE is that this specification might miss differences in SLE alone. For example, a 61-year-old woman and a 50-year-old man might both have SLEs that exactly match their OLEs, so that (SLE – OLE) is zero in both cases. But the 61-year-old woman has a higher probability of living to age 75 than the 50-year-old man, and the expected result is that she will retire later and be more likely to work at ages 62 and 65. The standardized SLE treats these two individuals as equals, missing a potential level of variation. To address this concern, Table 2.5 presents the coefficients for the subjective life expectancy variable without standardization: that is, the probability of living to age 75 or 85, without subtracting objective life expectancy. Each estimate is once again positive, and the statistical significance of each matches the results reported in Table 2.3. The magnitude of the estimates is also similar: a one-standard-deviation greater subjective life expectancy at age 75 is associated with an expected retirement age that is 2.6-4.6 months later, with a 4-5 percentage point higher probability of working full time at 62, and with a -6 percentage point higher probability of working at 65.

Table 2.6 adds two additional robustness checks. First, the above analysis recodes the retirement age to age 70 for any individuals who report a retirement age above 70. Across the board, the results are largely similar to those with top-coding, which indicates that estimations are insensitive to the top-coding strategy (Panel A). Panel B of Table 2.7 adds previously-excluded observations from waves 5 through 7, when the HRS (2000-2004) asked the respondent about their probability of living to age 80 rather than 85. The results for the larger sample are very similar to those reported in Table 2.3.

2.2.4 Actual versus Expected Retirement

The above results consider the relationship between subjective life expectancy and retirement expectations, but previous literature examines the relationship with actual retirement behavior. Table 2.7 presents the results for both actual retirement behavior and expected retirement plans using the specifications and the specialized sample described in the methodology section. When examining the relationship between the SLE and actual retirement behavior, the outcome measures are the actual retirement age and indicators for whether the individual reports working full time at ages 62 or 65. Because each actual retirement measure is unique to each person, when comparing the expectation to actual behavior, we also re-estimate the expected retirement behavior regressions for this limited sample using just one observation per person to be consistent: their last observation before reaching age 62. In the regressions for both actual and expected retirement ages, the explanatory variables are taken from the last pre-62 wave.

We find that the relationships with subjective longevity (SLE – OLE) are mostly positive for both actual and expected retirement, though the standard errors for the estimates in the actual retirement age and actual working full time at or after 62 regressions are large, and thus not statistically significantly.[21] These results fit with our hypothesis that actual retirement behavior is complicated by shocks, whereas retirement expectations better reflect the desire to retire by a certain age.

The only IV estimates that are statistically significant are in the regressions with working full time at age 65 as the outcome variable. Though the subjective longevity coefficient in these regressions is not statistically significantly different from the same coefficient in the working-at-62 regression, this result (taken at face value) suggests an interesting possibility. A plurality of individuals retires at age 62 because Social Security is available starting at that age. These individuals are unlikely to change their behavior in response to either their life expectancy at any given point in time or the evolution of their understanding about that longevity; no matter what they think about their longevity, they will retire at 62 – and would probably want to retire earlier if it was financially feasible.

The people who are more likely to change their retirement expectations, instead, may be those who have a substantial probability of working at 65. These individuals would be sensitive to the possibility that their longer work-lives could result in a retirement that was too short. New information that leads them to think they will live a long time will make working at 65 more

[21] Although the coefficients for actual retirement behavior are not statistically significantly different from zero, we cannot reject the null that the estimates in the actual retirement regressions are statistically significantly different from those from the expected retirement regressions. The failure to reject the null of equivalence between the expected and actual estimates means that our finding is consistent with Benitez-Silva and Dwyer (2005), which finds that retirement expectations and actual retirement behavior are closely linked.

palatable. In that case, an increase in longevity leaves the probability of working at age 62 unchanged, but the probability of working at age 65 increases.

To test for heterogeneity in the response to life expectancy, we examined estimates for those with and without college experience, under the assumption that individuals who had attended college would be more likely to work at older ages and therefore more sensitive to longevity expectations, but the results were not substantially nor statistically significantly different. Given that the working-at-65 and working-at-62 results are not statistically significantly different from each other, further tests for heterogeneity are left for future research.

2.3 Conclusions

The increase in life expectancy is expected to result in older Americans working longer, whether because the associated gains in healthy life expectancy make continued work more feasible or because further resources are needed to afford additional years of consumption. The results of this paper suggest a statistically significant relationship between an individual's subjective life expectancy and his expectations of when he'll retire. As individuals become more optimistic about living to ages 75 or 85 (relative to their actuarial probability of living to those ages), they push out their planned retirement dates and increase their expectations about working to the milestone ages of 62 and 65.

Our IV estimates show that these relationships are fairly substantial: an individual who is one-standard-deviation more optimistic than another about their survival to age 75 is 10 percent more likely to expect to work full-time at 62, and to 21 percent more likely to expect to work full-time at 65. Respondents who are more optimistic about their survival to age 75 or 85 also expect to work five months longer on average. As individuals learn more about their longevity, they are also likely to update their expectations: an increase in the SLE is associated with increases in the expected retirement age and planning to work at ages 62 and 65. Furthermore, we examine the relationship between SLE and actual retirement behaviors and find that SLE also impacts the actual retirement behavior, though to a lesser degree than it impacts retirement expectations. .

These results emphasize the importance of longevity expectations in retirement planning and, ultimately, making the decision to actually retire. In addition, these findings have important implications for modeling future labor force participation. With further health improvements, objective life expectancy continues to increase, but to extend one's working life, subjective life expectancy needs to increase as well. Our results suggest that policy reforms aimed at encouraging longer work lives must effectively target communication on the gains in life expectancy, in particular toward those individuals whose SLE continues to lag OLE, perhaps because this group places heavy weight on the smaller gains in longevity experienced by their parents' generation.

CHAPTER 3

"Sticky Ages: Why is Age 65 Still A Retirement Peak?[‡]"
(with Norma B. Coe and Matthew S. Rutledge)

In order to address an immediate and long-term funding problem, the Social Security Amendments of 1983 gradually increased the Full Retirement Age (FRA), changed the actuarial adjustment for individuals claiming benefits between the early and full retirement ages, and increased the delayed retirement credit. Together, these increase the financial gain to delaying Social Security benefit claims. An extensive literature documents that retirement and claiming decisions are responsive to Social Security incentives (see Krueger and Meyer, 2002 for a review), and recent work has shown that individuals are responding to the incentives laid out in the 1983 Amendments. Kopczuk and Song (2008) and Song and Manchester (2007) use administrative data and find that a new peak in claiming ages has appeared at the higher FRA. However, these papers also show that a large age-65 spike remains.

While it is clear that the increases in the FRA are causing changes at the aggregate level, it is not yet known who in particular has altered his or her behavior. Because the increase in the FRA is akin to a benefit cut, traditional economic theory would predict an increase in the retirement age among younger cohorts, but not an increase that is lock-step with the phase-in of the higher FRA. Behaghel and Blau (2011) use individual-level data to try to estimate what characteristics are correlated with delaying claiming until the FRA, and thus estimate, "who is behavioral?" They find that individuals with higher cognitive abilities are most likely to increase their retirement age,

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and interpret this finding as evidence of reference dependence and loss aversion. However, their focus is purely on who changes their behavior; the focus on this paper is who remains claiming at age-65 even as their FRA changes.

We explore whether two policy-relevant parameters help explain the remaining age-65 spike: understanding of the financial incentives to delay retirement and Medicare eligibility. To understand why age 65 remains "sticky," we estimate hazard models for exiting the labor force, retiring, and claiming Social Security benefits. We then examine whether the accuracy of self-reported Social Security benefit levels are significant determinants of claiming at precisely 65. Access to health insurance may also play a critical role explaining the age 65 peak. Historically, the Social Security FRA has been 65, the exact same age as the Medicare eligibility age, and thus it has been impossible to determine how much of an effect Medicare eligibility has on retirement behavior.

This paper continues as follows. Section 3.1 presents background information. Section 3.2 presents the model. Section 3.3 discusses the data. Section 3.4 presents the results and section 3.5 concludes that one's ability to predict their Social Security benefits seems, for the most part, unrelated to whether or not they claim benefits, retire, or exit the labor market on the 65th birthday or at their full retirement age. However, we find that individuals without access to retiree health insurance are more likely to retire at age 65, and less likely to postpone retirement until their FRA. We interpret these findings as suggestive evidence of a Medicare-eligibility effect on retirement behavior.

3.1 Background

Historically, the Social Security FRA has been 65, but the 1983 Amendments slowly increased the FRA by birth cohort. Individuals born through 1937 have the FRA of 65, while successive birth years have their FRA increase by 2 months for every year. Individuals born between 1943 and

1954 have a FRA of 66.[22] Figure 3.1 illustrates the relationship between claiming age and Social Security benefits by birth cohort. The increase in the FRA reduces the expected present discounted value of Social Security benefits, and if leisure is a normal good, should increase the retirement age. However, the increase in the FRA did not change the slope of the benefit schedule around the FRA, and thus there is no expectation for individuals to increase their retirement age by exactly the 2-months per birth year, as is the FRA increase.

Two other changes in the Social Security program during the period we study may also impact claiming behavior. First, the 1983 reforms increased the delayed retirement credit (DRC) – the increase in benefits one receives if they postpone claiming until after their FRA – as well as increasing the FRA. However, the DRC increases only every other birth-year, and thus does not increase at the same time as the FRA. Second, in 2000, the earnings test was eliminated for individuals working beyond their FRA. This change could increase the claiming hazard right at the FRA since workers would no longer be tempted to delay claiming in order to avoid this tax, but there is no reason to think it would impact the age-65 claiming spike. Fortunately, there is not complete overlap between the cohorts impacted by the earnings test removal and the FRA increases, allowing researchers to identify the effects separately (Song and Manchester, 2008; Behaghel and Blau, 2011).

Much attention has been paid to the effect the increase in the FRA has on retirement timing (Blau and Goodstein, 2010; Kopczuck and Song, 2008; Pingle, 2006; Mastrobuoni, 2009; Song and Manchester, 2008; Behaghel and Blau, 2011). Kopczuk and Song (2008) and Song and Manchester (2008) use aggregate claiming data and find that a new peak in claiming ages has appeared at the higher FRA, but a smaller age-65 claiming spike remains.

[22] The FRA again increases by 2 months for each birth year until it reaches 67 for people born in or after 1960, but these birth cohorts have not yet reached FRA and thus are outside of our analysis.

Behaghel and Blau (2011) use individual-level data from the *Health and Retirement Study* (HRS) and the *Longitudinal Employer-Household Dynamics* (LEHD) to explore how individual characteristics are related to the responsiveness of the retirement decision to the FRA. They find that pension incentives, wealth, and workers with high cognitive skills are more likely to respond to the FRA increase and retire later. However, the focus of their study is to identify who is changing their retirement age, while the emphasis of this study is who is *not.* This paper sets out to test why the age-65 spike remains, and whether it can be explained by Medicare availability or a lack of knowledge about Social Security program.

This paper fits in two strands of the economic literature surrounding retirement behavior. First, it contributes to the relatively small literature that examines the effect of the FRA increase on retirement. Song and Manchester (2008) and Kopczuk and Song (2008) offer thorough examinations of the effects of the changes in the FRA on claiming decisions, but since they use administrative data they cannot determine why the age-65 spike has survived despite the gradual increase of the FRA.

Secondly, it fits in the rather extensive literature that finds that access to health insurance is an important component of the retirement decision. While the size of the impact is debated, the literature is virtually unanimous that access to health insurance as a retiree increases the probability of retirement (see Monk and Munnell, 2009 for a review). The current evidence on the role of Medicare in the retirement decision is derived from simulations based on structural models. Rust and Phelan (1997) conclude that Medicare is important; Blau and Gilleskie (2006, 2008) conclude that it was much less important in the 1990s. Estimates from French and Jones (2011) are in between. However, since the Social Security FRA and Medicare eligibility ages have only recently been decoupled, this paper is the first to estimate the relative importance of Medicare in the claiming decision directly.

3.2 Data

This paper uses the *Health and Retirement Study* (HRS) in order to estimate the effect of the changes in the Social Security program on the Social Security benefits claiming age. We linked the HRS to the Social Security Administration (SSA) administrative earnings history data in order to (1) adequately control for lifetime earnings; and (2) correctly calculate the individual's Average Indexed Monthly Earnings (AIME) and financial incentives to retire.

The HRS is a biennial household survey of individuals over the age of 50 and their spouses. The survey began in 1992 with the 1931-1941 birth cohorts, with additional cohorts added over time. This study uses individuals born between 1931 and 1944, all of whom have reached their FRA by 2010. The treatment cohorts are those born 1938-1944, whose FRA is between 65 and 2 months and 66 years, while the control group are those born before 1938, when the FRA is 65th birthdays. For the primary analysis we select individuals with observable labor market status between the ages of 64 and 66. We drop individuals who report receiving Social Security benefits before age 62, as they likely receive disability benefits and thus are automatically rolled into the retirement program at their FRA. The remaining sample consists of 3,717 individuals among whom 1,608 individuals are in the treatment group and 2,109 individuals are in the control group.

We examine three labor force outcomes between the 64th and 66th birthdays: self-reported labor force exit, self-reported retirement, and Social Security claiming age. Using the detailed job history questions, we construct a monthly employment history for each individual. The self-reported age of labor force exit is defined as the month in which the individual first leaves the labor force for at least 3 months, conditional on having been employed in the previous 3 months. The individual reports the year and month he began receiving Social Security retirement benefits (OASI) directly. Finally, self-reported retirement is defined as the first month the individual reports himself to be completely retired, conditional on being in the labor force in the month before his 64th birthday.

3.2.1 Social Security Knowledge

The HRS has very detailed questions about participants' expected benefits if they claim at their preferred claiming age.[23] In 2002, they added questions about the benefit they would receive if they claim at age 62 or the FRA as well. We use these questions to assess individuals' knowledge about their own benefits by seeing how their expected benefit aligns with their true benefit calculated from the SSA earnings records.[24]

3.2.2 Health Insurance

In order to measure the importance of Medicare eligibility in the retirement decision, we examine current sources of health insurance coverage. If an individual has employer-sponsored health insurance and no retiree health insurance benefits, we hypothesize he is more likely to retire at age 65 when first Medicare-eligible than individuals who have their health insurance from sources that are not tied to their own employment status. Individuals with retiree health insurance coverage should they retire or health insurance from their spouse's employer should be less responsive to the Medicare eligibility age.

3.3 Model

We compare the claiming behavior of those impacted by the 1983 Amendments who have reached their FRA (born 1938-1944, the treatment group) to those born between 1931 and 1937 with the FRA of 65 (the control group), using a hazard model framework that controls for observable characteristics, such as health and lifetime earnings. Following Behaghel and Blau (2011), we

[23] The HRS picks one person within a couple to be the financial respondent. Before 2000, the financial respondent was asked about Social Security benefits for both themselves and their spouse. We assign these expectations to each person in the couple, respectively. After 2000, the financial respondent was only asked about their own Social Security benefit expectations. We assign this one measure to both members of the couple. This attribution suggests that the financial respondent is at least as knowledgeable as the non-financial respondent in terms of Social Security benefits. We test if our results are robust to including only financial respondents in section 5.

[24] See the Appendix for more information on how we calculated the Social Security benefit from the summary earnings records.

estimate different retirement patterns for the treatment and control groups using difference-in-differences methodology:

$$P_{iac} = \alpha FRA_{iac} + \rho X_{iac} + \beta_a + \gamma_c + \varepsilon_{iac} \tag{3.1}$$

where P_{iac} is one of three outcomes of interest for person i born in cohort c currently at age a: claiming Social Security retirement benefits, exiting the labor force, or reporting that he is retired.[25] The unit of observation is the person-month between his 64th birthday and either failure (claiming, exiting, or retiring) or censoring (death, survey attrition, or reaching age 66 or calendar year 2010 without failure).

FRA indicates if individual i has reached the full retirement age on or before age a. X is a set of individual-level controls. We include race, sex, marital status, education, health, health insurance coverage, retiree health insurance coverage, pension coverage, pension type, household wealth, average hourly earnings, and measures of cognitive capability, planning horizon, and risk aversion. β_a and γ_c represent a full set of age (in months) and cohort dummies, respectively. The estimated coefficients on FRA and the age 65 (or 780 months) dummy are the key measures in determining the average responsiveness to the change in financial incentives.

The model also controls for other changes in the Social Security program that may also impact claiming behavior. The 1983 reforms also increased the delayed retirement credit (DRC). However, the DRC increases are cohort-specific, so the effects of this policy change would be absorbed in the birth cohort dummy variables. A second confounding policy change occurred in 2000, when the earnings test was eliminated for individuals working beyond their FRA. This change could increase the claiming hazard right at the FRA since workers would no longer be

[25] The current estimates are from linear probability models, for simplicity. We do not expect these estimates to differ substantially from probit or logit models, as the dependent variable is not close to zero or one in any month.

tempted to delay claiming in order to avoid this tax. Fortunately, there is not complete overlap between the cohorts impacted by the earnings test removal and the FRA increases, so we also include an indicator for individuals in birth cohorts that were not subject to the post-FRA earnings test in some models.

To this main estimating equation, we add interaction terms in order to directly test for different responses to the FRA increase based on access to retiree health insurance. We interact the retiree health insurance indicator with the FRA dummy and each age and birth cohort dummy to allow for the full retirement and claiming patterns to differ by post-retirement health insurance ownership.

We also test whether one's knowledge of Social Security program rules are associated with retirement and claiming patterns. In this set of specifications, we include a categorical variable that summarizes this knowledge at three different ages: 62, FRA, and his expected retirement date. First, for each age, we calculate the difference between the benefit the respondent reports that he expects to receive and the respondent's actual Social Security monthly benefit, as calculated from his earnings. Then, for each of the three ages, we split the sample into five groups: (1) within one standard deviation and positive (slight overestimation, the omitted category from the regression); (2) within one standard deviation and negative (slight underestimation); (3) outside of one standard deviation and positive (large overestimation); (4) outside of one standard deviation and negative (large underestimation); (5) no answer or don't know. We also interact this categorical variable with the FRA dummy and the age and birth cohort dummies.

3.4 Results

Figures 3.2-3.4 illustrate the behavior we find in the HRS: Social Security benefit claiming hazard, exit from employment, and retirement hazards, by birth cohort. Despite the difference in definition of retirement and employment, we find very similar patterns as Behaghel and Blau (2011) by birth cohort. The FRA-claiming spike is the most affected, and moves in lock-step with the increase in the FRA. However, the age-65 spike does not disappear, and remains larger than the age-63 and age-64 spikes.

Table 3.1 presents the means of the characteristics of our sample, measured as of age 64. For the most part, there are few demographic differences between our control (1931-1937) and treatment (1938-1942) cohorts. The younger (treatment) cohorts tend to have slightly higher educational attainment, are wealthier, and are less likely to report bad health. They are also less likely to have a defined benefit pension, and more unsure about their pension coverage. About one-half of the sample receives health insurance through their employer, and one-quarter have a spouse that gets health insurance through their employer. Two-thirds to three-quarters of the sample reports no access to retiree health insurance.

Table 3.2, Panel A presents the estimated impact of the FRA increase on Social Security claiming timing. We present the coefficients on the FRA, the removal of the Social Security Earnings test, age-65 and age-64. Panel B presents these coefficients for the exit from employment. Panel C presents these coefficients for retirement hazard.[26]

Panel A illustrates that our estimated effect of the FRA and the removal of the Social Security test on Social Security claiming behavior are in-line with those reported in Table 3.1 of Behaghel and Blau (2011). We find a 15 percentage point increase in claiming at the FRA, slightly larger than Behaghel and Blau's 13-percentage-point estimate, likely due to slight differences in the samples. Our estimate of the effect of the earnings test – those subject to it are 4.7 percentage points less likely to claim – is also about one percentage point larger than Behaghel and Blau's estimate, as well.

In the model presented in Panel B, we find that, compared to individuals retiring at age 64.5 (the omitted age category), exiting the labor market does not follow the FRA increase, but the spike at

[26] The full results of Model 3 are presented in Appendix Table AT3.1.

age 65 remains: individuals in our sample are 1.3 percentage points more likely to exit the labor force during the month they turn 65. Self-reported retirement does follow the FRA increase, but again, the hazard at age 65 remains larger than at the FRA. Intuitively it makes sense that self-reported retirement is less responsive to the removal of the earnings test above the FRA than the actual exit from the labor force.

Table 3.3 presents the summary statistics concerning Social Security expectations. Since the expectations questions are only asked of individuals who have not yet claimed (and some of the questions were added in 2002), our sample size drops when using this metric of Social Security knowledge. One does see a slight increase in the expected age of retirement between the control cohorts and the treatment cohorts.[27] Expected benefits increase by birth cohort – this could be due to the underlying differences in the cohort characteristics or differences in knowledge about the Social Security program. To determine if it's a difference in underlying characteristics, we compare it to the calculated Social Security benefits. The mean calculated benefit also increases with birth cohort; but the differences between expected benefits and calculated benefits varies by birth cohort. Interestingly, those born 1938-1942 tend to be the most accurate, on average, despite the complexity of their increasing FRA by 2-months per birth year. Since the expectations of benefits of claiming at 62 and 65 were added in 2002, we do not have these measures for the control cohorts, who were already beyond these ages. For the regressions where we use these measures, we are in essence comparing differences between knowledgeable and unknowledgeable individuals with the same birth cohort.

Since we want a measure of Social Security knowledge, we take the difference between the expected and the calculated benefits for a given claiming age by individual. The distributions of these differences are shown in Figure 3.5. Panels A, B, and C present claiming at age 62, the FRA,

[27] It is important to note that these expectations are measured around the age of 64 among those who have not yet claimed Social Security, which is why the mean is relatively high (65-65.6).

and one's preferred retirement age, respectively. Interestingly, people tend to underestimate their Social Security benefits, since the mean of the distribution is negative in all three cases. As one would expect, individuals are much better at predicting benefits for their preferred retirement age than other ages – the distribution in panel C is much more concentrated and has a larger peak around zero. We then categorize the difference into whether benefits are over- or underestimated, and whether the difference is large or small.

What this paper aims to find is whether there are differential effects of Social Security knowledge or health insurance availability on the likelihood of claiming/exiting the labor force/retiring at age 65. To do so, we modify equation (1) to include information on Social Security knowledge (Table 3.4) and retiree health insurance access (Table 3.5), both in levels and interacted with age, FRA, and birth cohort.[28] Panels A, B, and C are the results from the claiming equation, exit from employment, and retirement hazard, respectively, as in Table 3.2.

In Table 3.4, each column presents results from a different equation, with a different interaction term that measures expectations about Social Security benefits at different potential claiming ages. Column 1 measures Social Security knowledge as the difference between one's self-reported benefits if one claimed at 62 and the benefits we calculate from the earnings record for claiming at age 62. Column 2 measures knowledge in the same manner, using benefits that would be claimed at the FRA. Likewise, Column 3 calculates knowledge using the individual's self-reported expected retirement age.

Overall, we find little evidence of a relationship between one's ability to predict their Social Security benefits and the remaining age-65 claiming, retirement, or exiting of the labor force spike. While some of the coefficients are statistically significant – for example, individuals not responding to the question about expected benefits at their self-reported preferred retirement age are less likely

[28] Full results available from the authors upon request.

to claim at their FRA – they are not consistently significant across models and thus are hard to interpret as robust results.

Table 3.5 presents the results interacted with whether the individual has no access to retiree health insurance upon retirement, presumably those who would be more sensitive to the Medicare eligibility-age. We find that access to health insurance may help explain the remaining age-65 spike in the retirement hazard, but not in claiming behavior. Individuals without access to retiree health insurance are 7.5 percentage points more likely to report retiring in the month of their 65th birthdays, the Medicare eligibility age, and 5.8 percentage points less likely to delay retirement until the FRA. To further support the idea that this is due to Medicare eligibility, we also report the interaction terms with age 64, which we hypothesize should be insensitive to retiree health insurance since there is not exogenous change in health insurance access at that age. As hypothesized, there is no relationship with claiming right at age 64 and access to retiree health insurance.

The results are largely robust to alternative specifications. One such alternative concerns the role of the financial respondent, the individual who is asked about their expectations for Social Security benefit amounts for both spouses. It may be the case that the non-financial respondent has different expectations, and we may be incorrectly categorizing the household based on one spouse's ignorance. We limit the estimates to only financial respondents and find the results largely unchanged.[29]

3.5 Conclusions

While we find considerable error in self-reported future Social Security benefit levels, this does not seem to contribute independently to the remaining claiming spike at age 65. However, we do find that individuals whose health insurance coverage is most tied to their employment – namely people

[29] Future versions of the paper will also include analysis by gender.

with no retiree health benefits – do seem to contribute to the age-65 claiming peak. We interpret this finding as evidence for Medicare-eligibility impacting retirement decisions, something that has not been documented before due to the historical link between the Social Security FRA and the Medicare program.

The results of this paper imply that projections of retirement behavior need to account for the extent to which Medicare eligibility affects retirement and claiming. Currently, the FRA is scheduled to increase to 67, but the Medicare-eligibility age is scheduled to remain at 65. Numerous policy experts have suggested extending the FRA even further.[30] The results in this paper suggest that a retirement spike would remain at age 65; indeed, with a larger gap between Medicare eligibility and the FRA, this spike could grow larger relative to the FRA peak, if individuals' health care and health insurance needs supersede the financial incentives to delay retirement.

Other policy experts have expressed interest in increasing the Medicare eligibility age beyond 65 to address budgetary concerns. Such a policy would re-align the health insurance and financial incentives, and could restore the prominent peaks in retirement and claiming at FRA that we observed before 2002. The results of this paper suggest that this policy change would keep older workers in the labor force, and off of Social Security, longer than further FRA extension alone; whether this policy is desirable depends on the relative importance of welfare loss among seniors who would have otherwise retired versus reduced Medicare and Social Security outlays. On the other hand, if health insurance exchanges are implemented as detailed by the Affordable Care Act, concerns about health insurance access need not delay retirement as much as we observe under current conditions.

[30] For example, the Bowles-Simpson commission recommends increasing the FRA to 69.

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Figure 1.1 - Labor Force Participation of Eligible Disability Insurance Denied Applicants and Non-applicants by FRA Cohort for Age 51-61

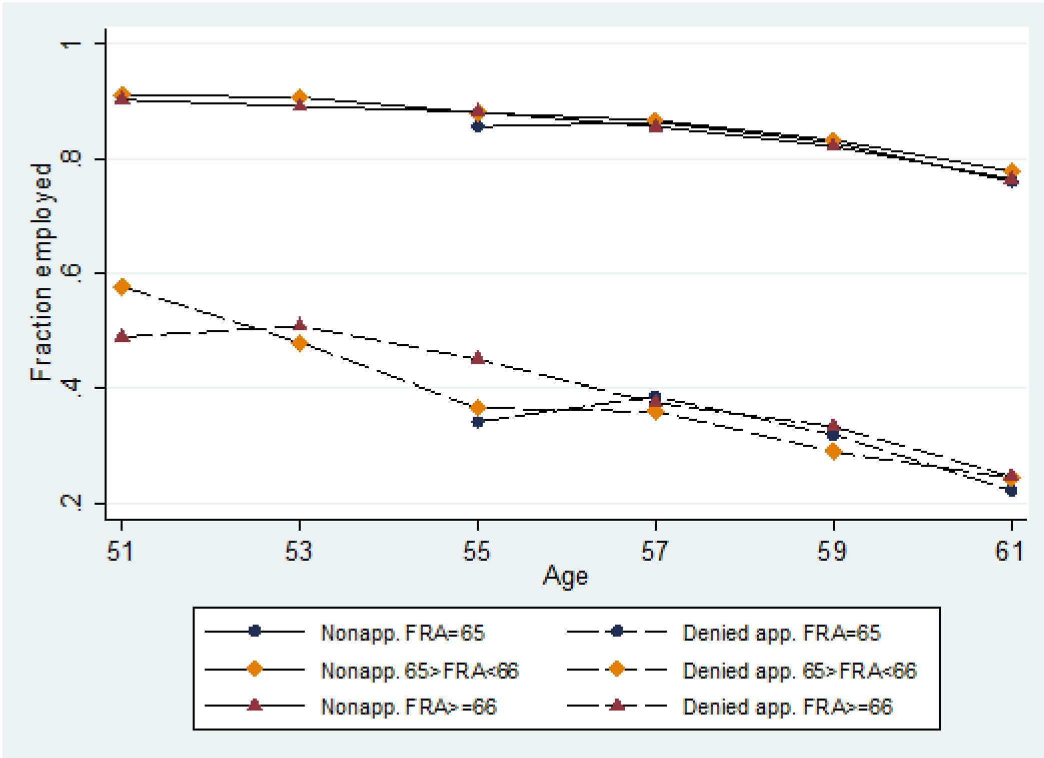


Figure 1.2 - SSDI Allowance Rate for Men Age 50-54

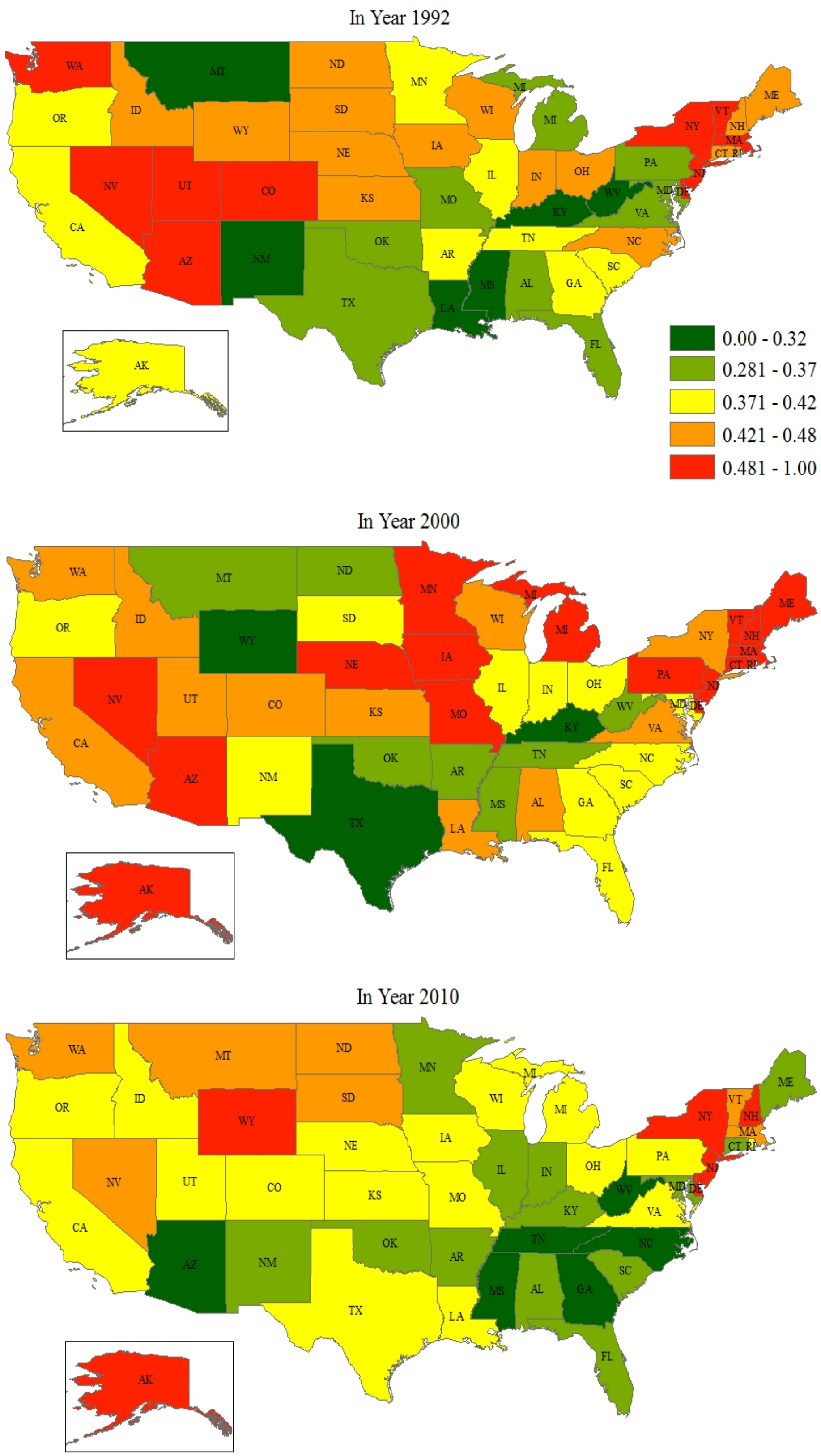


Figure 1.3 - SSDI Allowance Rate for Women Age 50-54

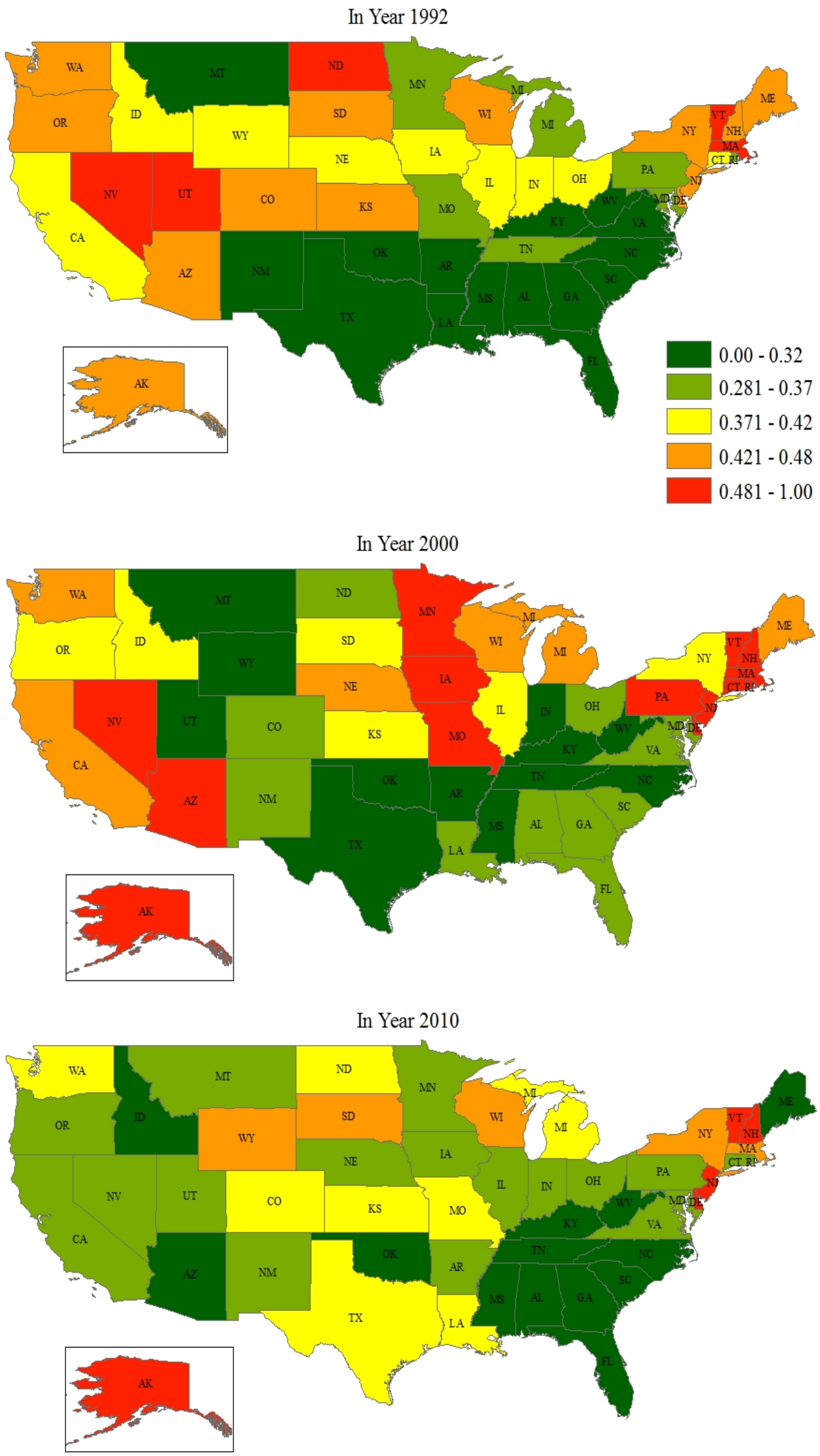
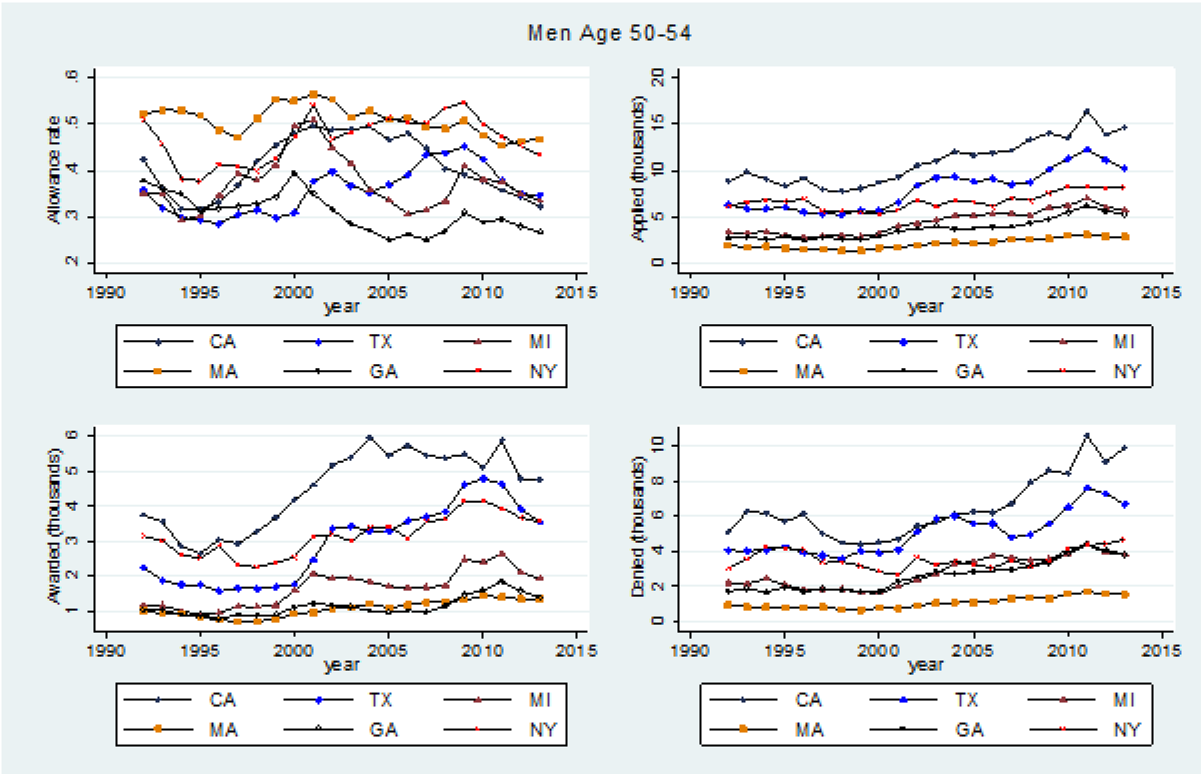


Figure 1.4 - Disability Insurance Application, Award, Denial, and Allowance Rate of Selected States in US for Men 1992 -2013



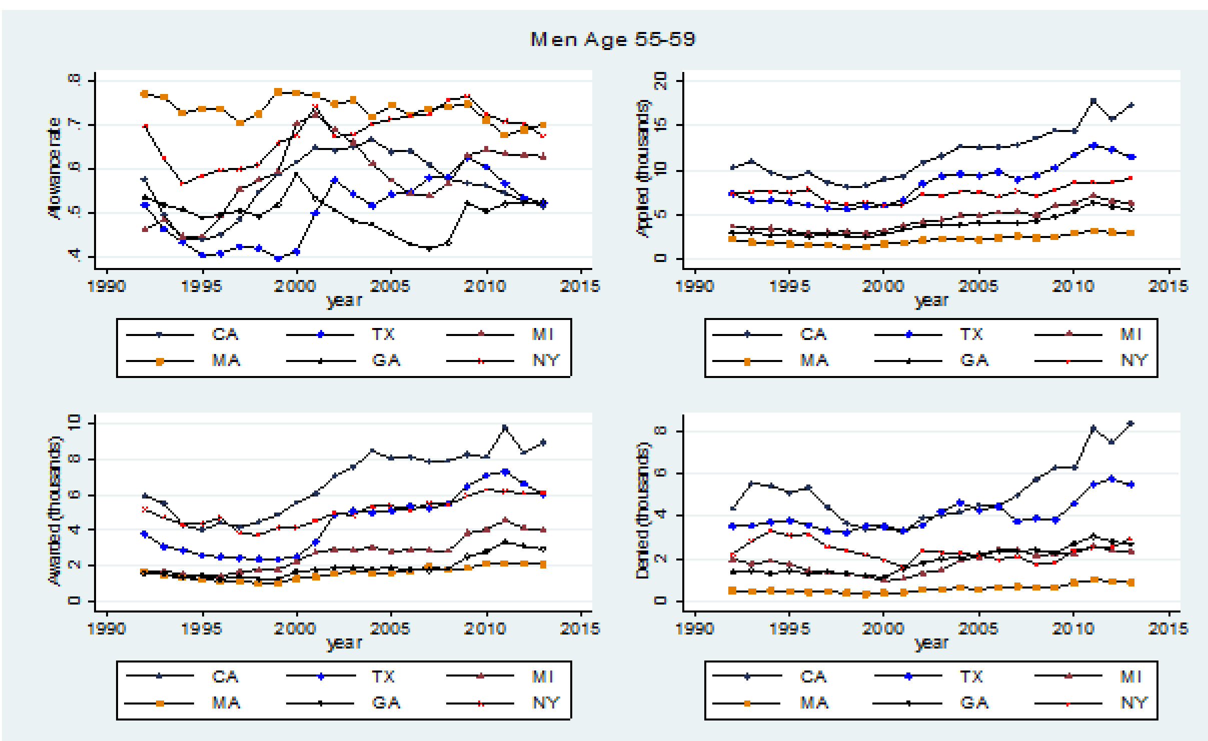


Figure 1.5 - Disability Insurance Application, Award, Denial, and Allowance Rate of Selected States in US for Women 1992 -2013

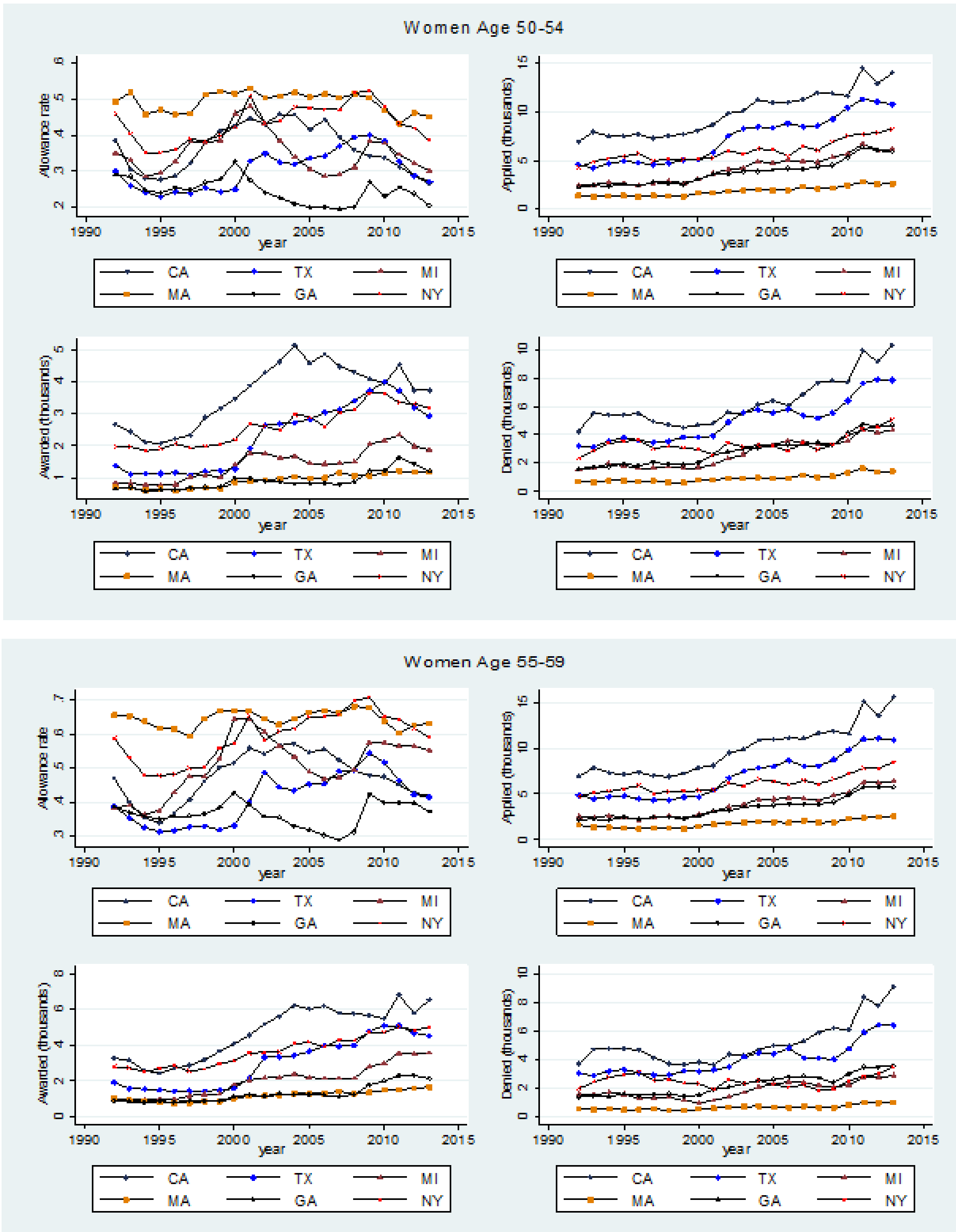


Figure 1.6 – The Relationship Between BMI, Self-reported Poor or Fair Health, and Employment

Figure 2.1 - Sample Selection Criteria

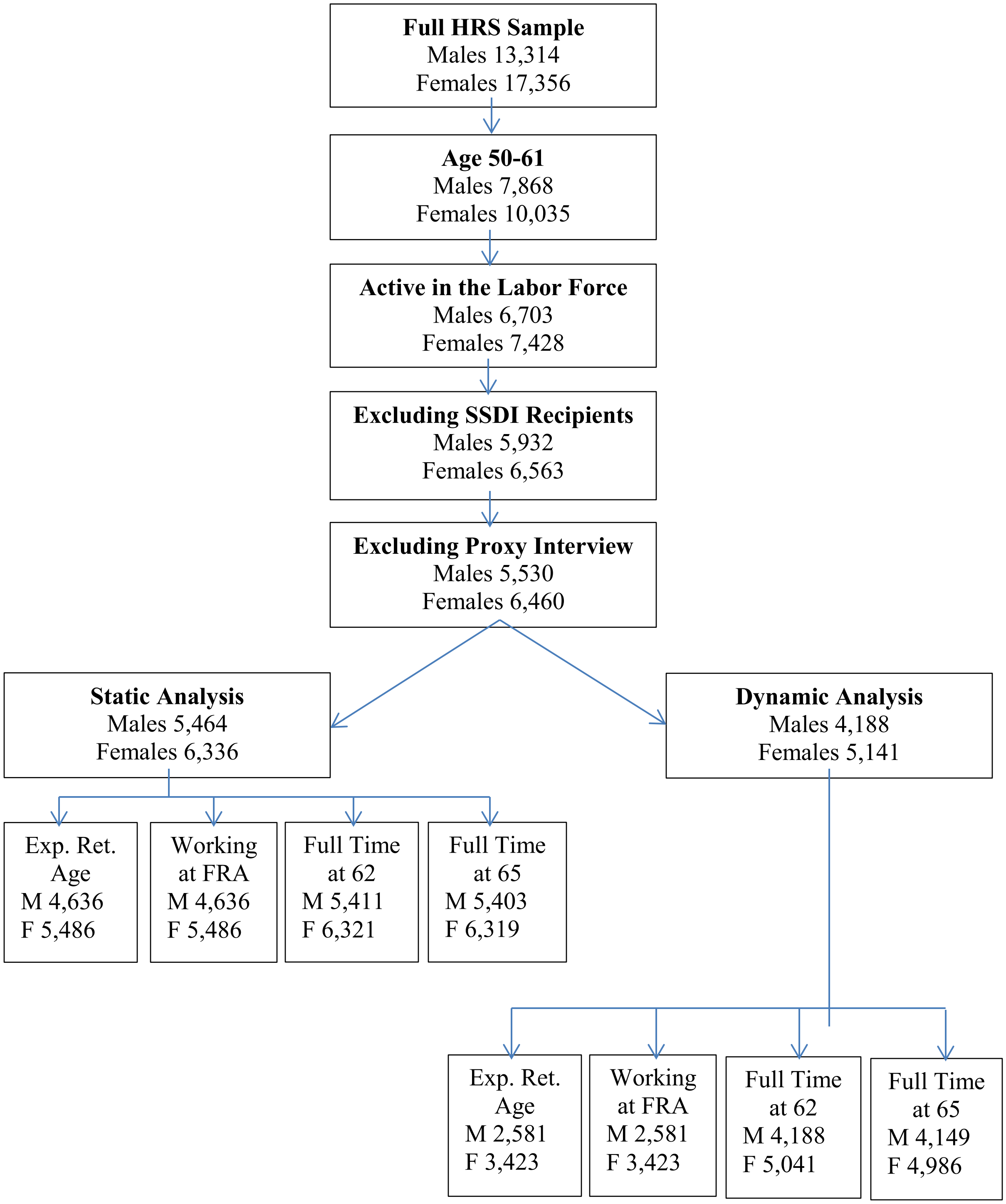


Figure 3.1 - Social Security Retirement Benefits, by Birth Cohort

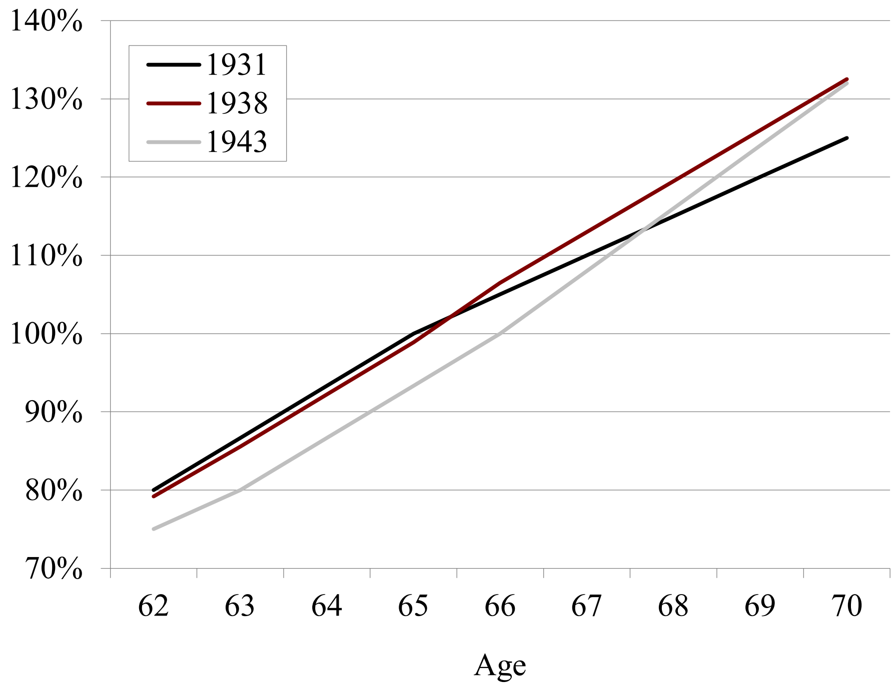
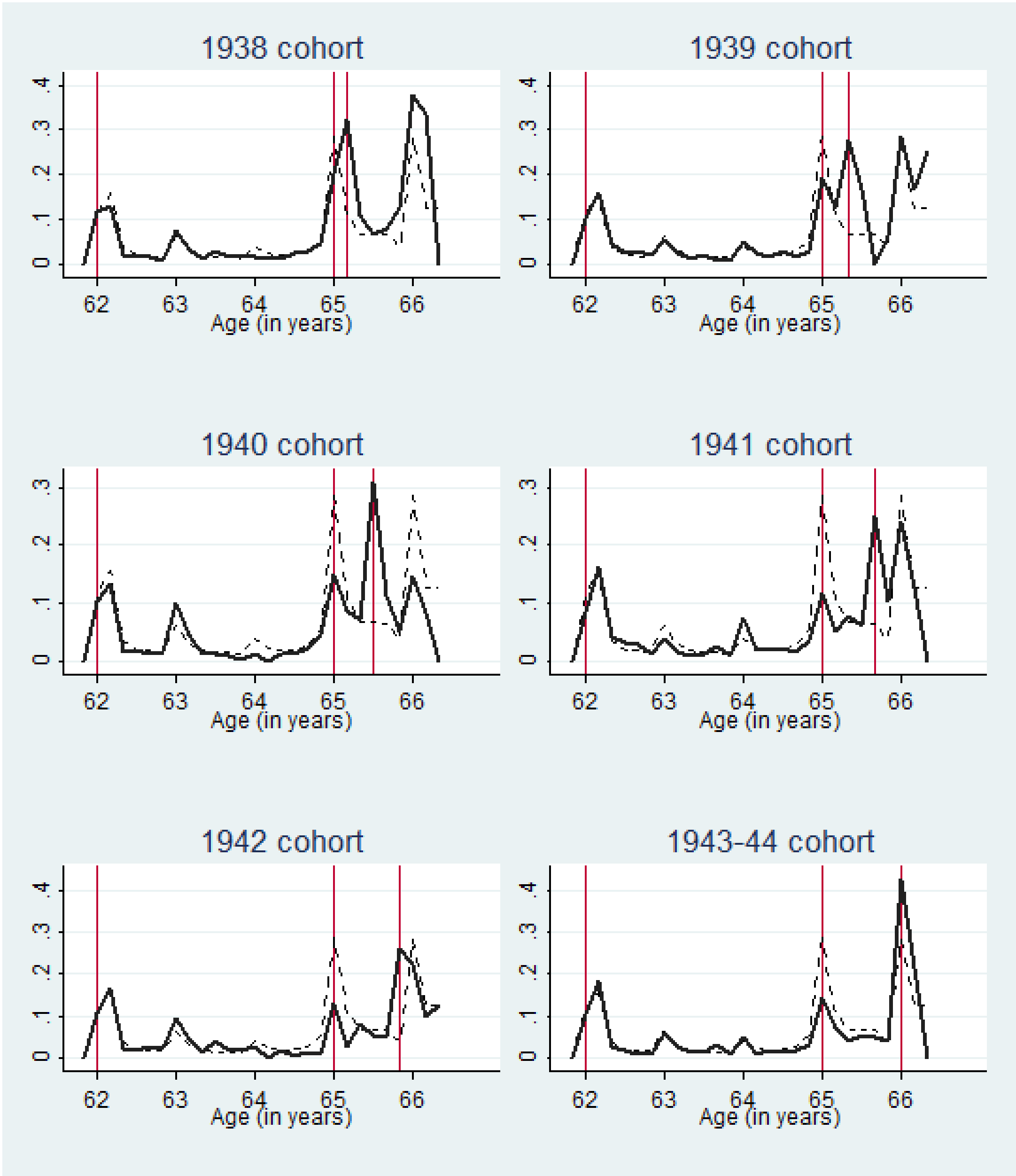
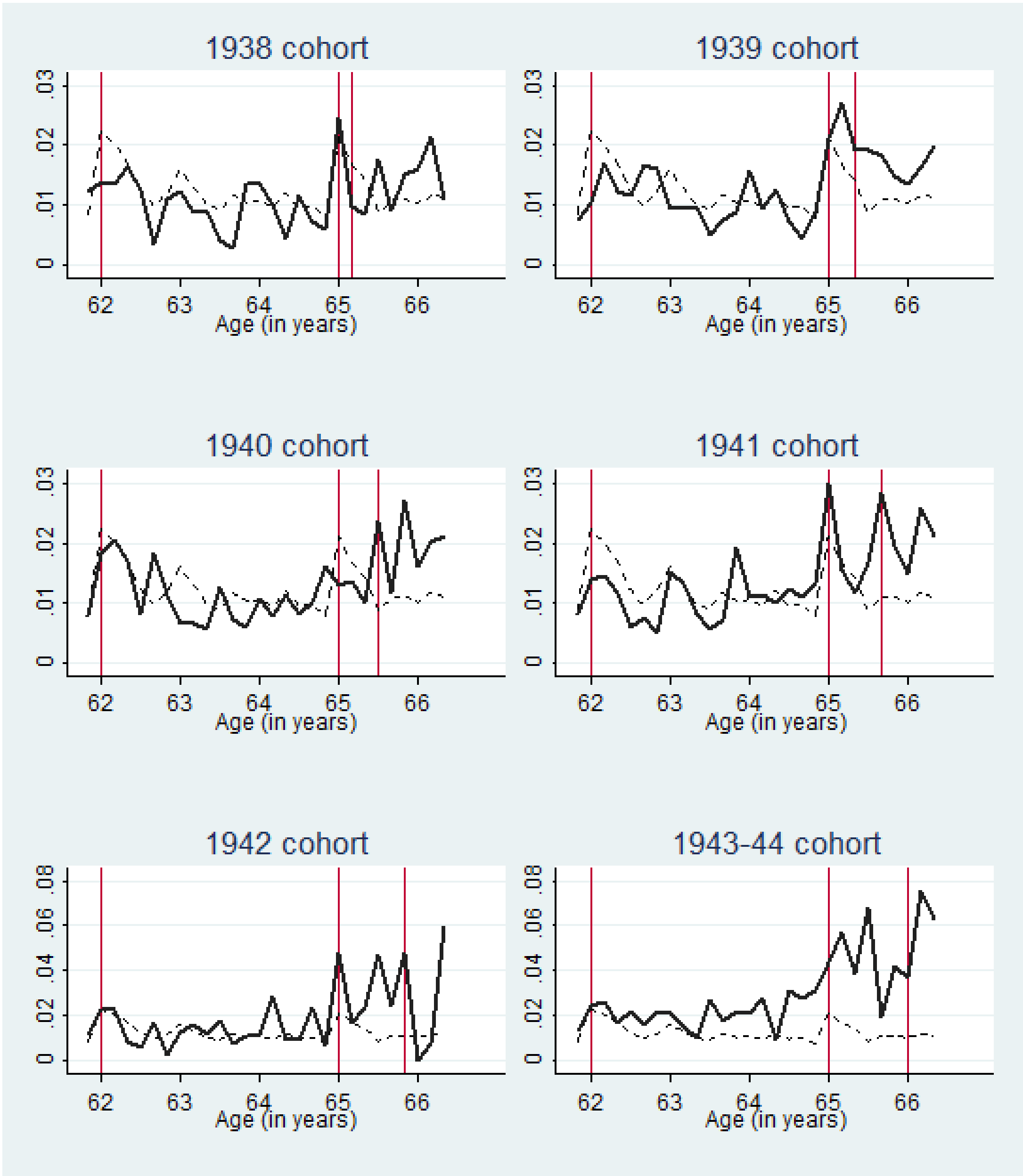


Figure 3.2 - OASI Benefit Claiming Hazard



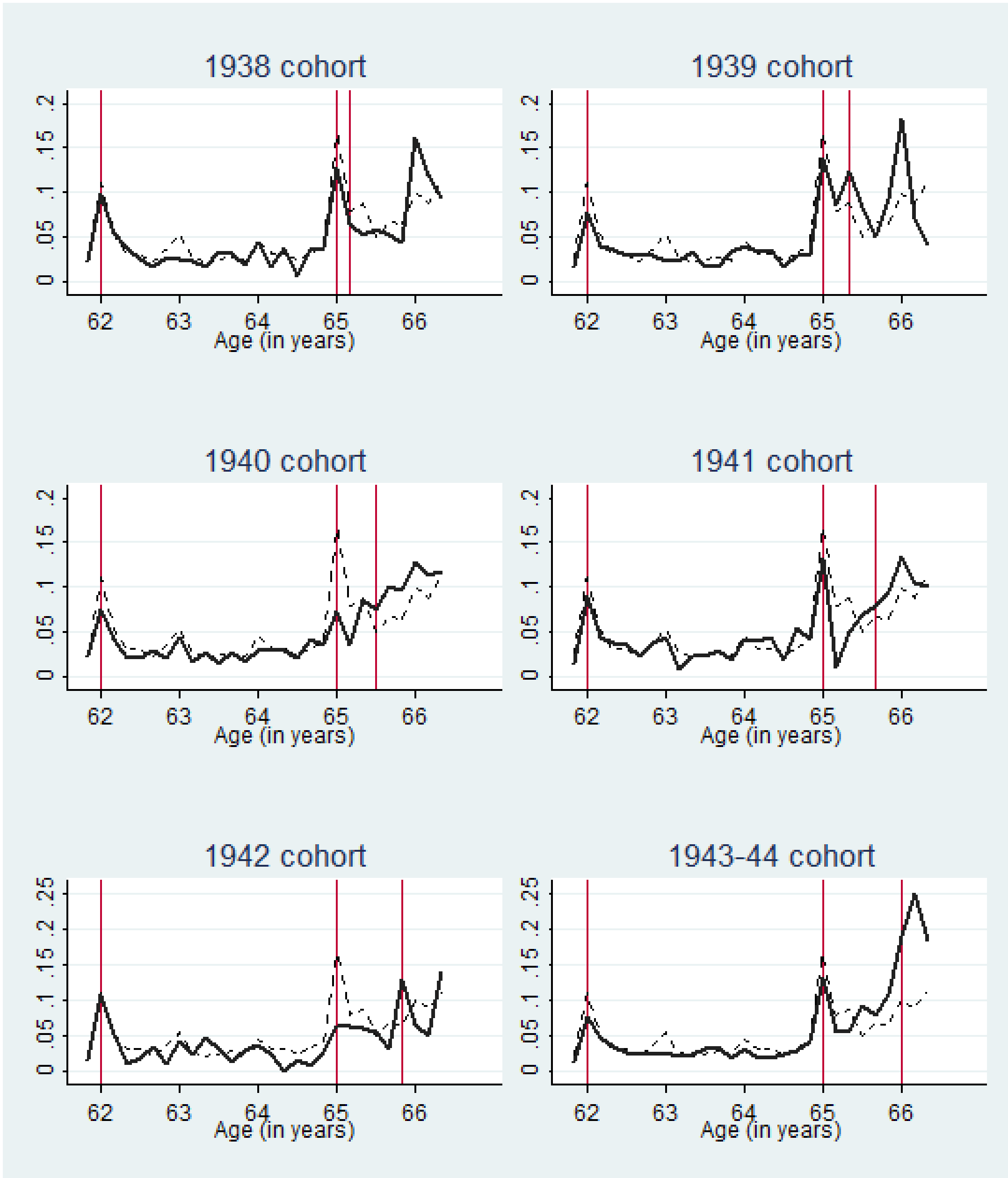
Notes: The graphs show the average monthly claiming hazard rates for cohorts born in between 1931 to 1944. The OASI claiming hazard rate is defined as the probability of claiming at a given age, conditional on not having claimed previously with claiming being an absorbing state. Age is measured at a bimonthly frequency; e.g. age 62 denotes age 744 and 745 in months. In each graph, the dotted line depicts the claiming hazard for workers with Full Retirement Age (FRA) of 65. For each cohort, the vertical lines indicate age 62, age 65, and the FRA.

Figure 3.3 - Hazard of Exit from Employment



Notes: The graphs show the average monthly hazard of exit from employment rates for cohorts born in between 1931 to 1944. The hazard of exit from employment rate is defined as the probability of exit at a given age, conditional on being employed in the previous 3 months and not being employed in the next 3 months. Age is measured at a bimonthly frequency; e.g. age 62 denotes age 744 and 745 in months. In each graph, the dotted line depicts the claiming hazard for workers with Full Retirement Age (FRA) of 65. For each cohort, the vertical lines indicate age 62, age 65, and the FRA.

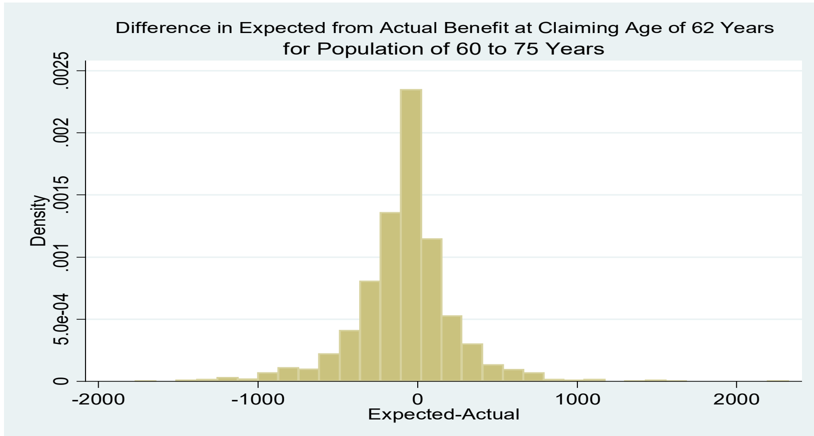
Figure 3.4 - Hazard of Retirement



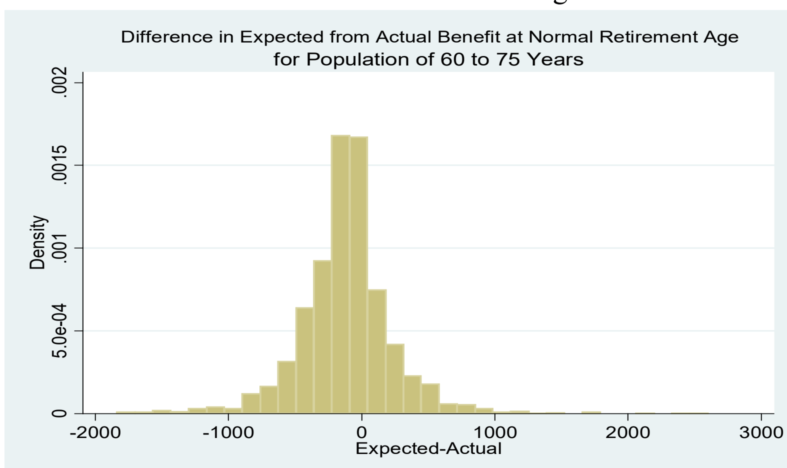
Notes: The graphs show the average monthly hazard of retirement rates for cohorts born in between 1931 to 1944. The hazard of retirement rate is defined as the probability of claiming to be retired at a given age on or after age 62 years, conditional on not having claimed to be retired between then and 62 with claiming being an absorbing state. Age is measured at a bimonthly frequency; e.g. age 62 denotes age 744 and 745 in months. In each graph, the dotted line depicts the claiming hazard for workers with Full Retirement Age (FRA) of 65. For each cohort, the vertical lines indicate age 62, age 65, and the FRA.

Figure 3.5 - The Difference between Expected and Calculated Social Security Benefits, by Claiming Age

Panel A: At age 62



Panel B: At full retirement age



Panel C: At expected retirement age

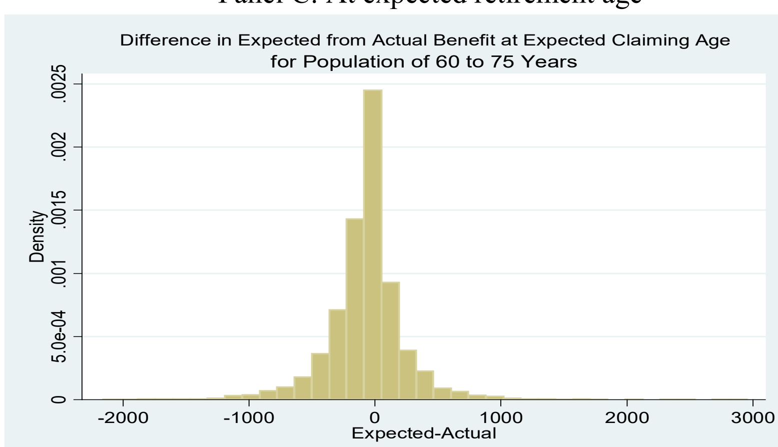


Table 1.1 – Sample Characteristics of Non-applicants, Denied, and Allowed SSDI Applicants and Comparison Between Groups

	Denied applicants T=1	Control group T=0	(T=1) - (T=0)	Never applied N=1	(T=1) - (N=1)	Allowed applicants B=1	(B=1) - T(=1)
Demographics							
Age	54.76	55.26	-0.51***	54.82	-0.06	55	0.24
	(2.28)	(2.21)	(0.15)	(2.34)	(0.13)	(2.20)	(0.16)
Fraction of female	0.57	0.5	0.07*	0.56	0.01	0.56	-0.01
	(0.50)	(0.50)	(0.03)	(0.50)	(0.03)	(0.50)	(0.04)
Fraction of non-white	0.33	0.23	0.10**	0.17	0.15***	0.32	0
	(0.47)	(0.42)	(0.03)	(0.38)	(0.03)	(0.47)	(0.03)
Fraction of school dropouts	0.31	0.24	0.07*	0.12	0.19***	0.28	-0.04
	(0.46)	(0.43)	(0.03)	(0.33)	(0.03)	(0.45)	(0.03)
Fraction of high school educated	0.40	0.38	-0.02	0.35	0.03	0.42	0.04
	(0.49)	(0.49)	(0.03)	(0.48)	(0.03)	(0.49)	(0.04)
Fraction of college educated	0.31	0.36	-0.05	0.52	-0.21***	0.31	0
	(0.46)	(0.48)	(0.03)	(0.50)	(0.03)	(0.46)	(0.03)
Fraction married	0.6	0.71	-0.10***	0.76	-0.16***	0.65	0.05
	(0.49)	(0.46)	(0.03)	(0.43)	(0.03)	(0.48)	(0.04)
Fraction widowed	0.03	0.07	0.04**	0.04	0.03*	0.06	-0.01
	(0.17)	(0.26)	(0.02)	(0.19)	(0.01)	(0.24)	(0.02)
Caregiver for parents	0.11	0.14	-0.03	0.17	-0.06***	0.15	0.04
	(0.32)	(0.35)	(0.02)	(0.38)	(0.02)	(0.36)	(0.02)
Caregiver in next wave	0.16	0.17	0	0.21	-0.05*	0.19	0.03
	(0.37)	(0.37)	(0.02)	(0.41)	(0.02)	(0.39)	(0.03)
Body Mass Index (BMI)	28.87	28.80	0.08	27.80	1.08***	30.10	1.22**
	(5.37)	(6.36)	(0.37)	(5.31)	(0.30)	(7.01)	(0.45)
Fraction with health conditions							
BMI return to normal range	0.03	0.03	0.00	0.03	0.00	0.05	0.02
	(0.17)	(0.18)	(0.01)	(0.16)	(0.01)	(0.22)	(0.01)
BMI move to abnormal range	0.07	0.05	0.02	0.03	0.03*	0.09	0.02
	(0.25)	(0.22)	(0.02)	(0.18)	(0.01)	(0.28)	(0.02)
Self-reported poor/fair health	0.53	0.25	0.28***	0.11	0.42***	0.69	0.16***
	(0.50)	(0.43)	(0.03)	(0.31)	(0.03)	(0.46)	(0.04)
Self-reported mobility problems	0.68	0.43	0.25***	0.28	0.40***	0.81	0.12***
	(0.47)	(0.50)	(0.03)	(0.45)	(0.03)	(0.39)	(0.03)
Self-reported Large muscle problems	0.76	0.59	0.17***	0.42	0.34***	0.83	0.06*
	(0.43)	(0.49)	(0.03)	(0.49)	(0.02)	(0.38)	(0.03)
Self-reported back problem	0.54	0.38	0.16***	0.29	0.26***	0.62	0.08*
	(0.50)	(0.49)	(0.03)	(0.45)	(0.03)	(0.48)	(0.04)
Health limits work previous wave	0.13	0.1	0.04	0.05	0.08***	0.23	0.10***
	(0.34)	(0.30)	(0.02)	(0.22)	(0.02)	(0.42)	(0.03)
High blood pressure (BP)	0.48	0.36	0.12***	0.31	0.17***	0.57	0.08*
	(0.50)	(0.48)	(0.03)	(0.46)	(0.03)	(0.50)	(0.04)
Onset of BP current wave	0.3	0.16	0.13***	0.15	0.15***	0.31	0.01
	(0.46)	(0.37)	(0.03)	(0.35)	(0.03)	(0.46)	(0.03)
Onset of BP next wave	0.07	0.05	0.01	0.04	0.02	0.05	-0.02
	(0.25)	(0.22)	(0.02)	(0.20)	(0.01)	(0.22)	(0.02)

Table 1.1 *continued ...*

Cancer	0.08	0.06	0.02	0.05	0.04*	0.09	0.01
	(0.28)	(0.24)	(0.02)	(0.21)	(0.02)	(0.29)	(0.02)
Onset of cancer current wave	0.06	0.03	0.03	0.02	0.03**	0.06	0
	(0.23)	(0.17)	(0.01)	(0.15)	(0.01)	(0.23)	(0.02)
Onset of cancer next wave	0.02	0.01	0.01	0.01	0.01	0.04	0.02
	(0.15)	(0.12)	(0.01)	(0.10)	(0.01)	(0.20)	(0.01)
Lung disease	0.09	0.08	0.01	0.04	0.05**	0.16	0.07**
	(0.29)	(0.28)	(0.02)	(0.19)	(0.02)	(0.37)	(0.02)
Onset of lung disease current wave	0.07	0.04	0.02	0.02	0.05***	0.11	0.05*
	(0.25)	(0.20)	(0.02)	(0.14)	(0.01)	(0.31)	(0.02)
Onset of lung disease next wave	0.03	0.01	0.01	0.01	0.02*	0.05	0.02
	(0.17)	(0.12)	(0.01)	(0.09)	(0.01)	(0.21)	(0.01)
Heart disease	0.16	0.14	0.02	0.08	0.09***	0.28	0.11***
	(0.37)	(0.35)	(0.02)	(0.26)	(0.02)	(0.45)	(0.03)
Onset of heart disease current wave	0.11	0.07	0.04*	0.04	0.08***	0.17	0.05*
	(0.32)	(0.26)	(0.02)	(0.19)	(0.02)	(0.37)	(0.02)
Onset of heart disease next wave	0.06	0.03	0.03*	0.02	0.04**	0.05	-0.01
	(0.24)	(0.17)	(0.01)	(0.13)	(0.01)	(0.22)	(0.02)
Stroke	0.09	0.02	0.06***	0.01	0.08***	0.07	-0.02
	(0.28)	(0.15)	(0.02)	(0.10)	(0.02)	(0.25)	(0.02)
Onset of stroke current wave	0.06	0.01	0.05***	0.01	0.06***	0.06	0
	(0.24)	(0.11)	(0.01)	(0.08)	(0.01)	(0.23)	(0.02)
Onset of stroke next wave	0.03	0.01	0.02*	0	0.03**	0.04	0.01
	(0.18)	(0.10)	(0.01)	(0.06)	(0.01)	(0.21)	(0.01)
Psychiatric problems	0.24	0.15	0.10***	0.1	0.14***	0.29	0.05
	(0.43)	(0.35)	(0.03)	(0.31)	(0.02)	(0.45)	(0.03)
Onset of psychiatric event current wave	0.16	0.06	0.10***	0.04	0.11***	0.18	0.02
	(0.36)	(0.24)	(0.02)	(0.20)	(0.02)	(0.38)	(0.03)
Onset of psychiatric event next wave	0.07	0.03	0.04*	0.01	0.06***	0.06	-0.01
	(0.26)	(0.18)	(0.02)	(0.12)	(0.01)	(0.24)	(0.02)
Arthritis	0.46	0.4	0.06*	0.3	0.16***	0.57	0.11**
	(0.50)	(0.49)	(0.03)	(0.46)	(0.03)	(0.50)	(0.04)
Onset of arthritis current wave	0.3	0.19	0.10***	0.14	0.16***	0.31	0.02
	(0.46)	(0.39)	(0.03)	(0.34)	(0.03)	(0.46)	(0.03)
Onset of arthritis next wave	0.09	0.08	0	0.05	0.03*	0.08	-0.01
	(0.28)	(0.28)	(0.02)	(0.23)	(0.02)	(0.26)	(0.02)
Diabetes	0.2	0.15	0.05*	0.08	0.12***	0.2	0
	(0.40)	(0.36)	(0.03)	(0.27)	(0.02)	(0.40)	(0.03)
Onset of diabetes current wave	0.13	0.07	0.06**	0.04	0.09***	0.13	0
	(0.34)	(0.25)	(0.02)	(0.19)	(0.02)	(0.34)	(0.02)
Onset of diabetes next wave	0.03	0.03	0	0.02	0.01	0.04	0.01
	(0.17)	(0.17)	(0.01)	(0.14)	(0.01)	(0.19)	(0.01)
Taste for work							
Total years worked till reference age	27.9	32.53	-4.63***	31.5	-3.60***	29.11	1.21
	(10.73)	(9.14)	(0.67)	(8.54)	(0.60)	(10.10)	(0.76)
Fraction of at least one 5-year job tenure	0.87	0.92	-0.06**	0.94	-0.08***	0.88	0.02
	(0.34)	(0.27)	(0.02)	(0.23)	(0.02)	(0.32)	(0.02)
Fraction with retiree health insurance	0.29	0.41	-0.13***	0.43	-0.15***	0.33	0.04
	(0.45)	(0.49)	(0.03)	(0.50)	(0.03)	(0.47)	(0.03)

Table 1.1 *continued ...*

Fraction in wealth quintile							
Lowest	0.35	0.22	0.13***	0.14	0.21***	0.32	-0.02
	(0.48)	(0.41)	(0.03)	(0.34)	(0.03)	(0.47)	(0.03)
Second	0.27	0.24	0.02	0.19	0.07**	0.29	0.03
	(0.44)	(0.43)	(0.03)	(0.40)	(0.02)	(0.46)	(0.03)
Third	0.19	0.23	-0.04	0.21	-0.02	0.19	-0.01
	(0.39)	(0.42)	(0.03)	(0.41)	(0.02)	(0.39)	(0.03)
Fourth	0.11	0.17	-0.05*	0.23	-0.11***	0.13	0.01
	(0.32)	(0.38)	(0.02)	(0.42)	(0.02)	(0.33)	(0.02)
Highest	0.08	0.14	-0.06**	0.23	-0.15***	0.07	-0.01
	(0.27)	(0.35)	(0.02)	(0.42)	(0.02)	(0.25)	(0.02)
Obs.	322	909	1231	21306	21628	453	775
Number of Individuals	322	347	669	8452	8774	453	775

Notes: Standard deviations are in parentheses. For the mean differences the standard errors are in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 1.2 – Employment Rate of Non-applicants, Denied, and Allowed SSDI Applicants

	Denied applicants T=1	Control group T=0	(T=1) - (T=0)	Never applied N=1	(T=1) - (N=1)	Allowed applicants B=1	(B=1) - T(=1)
Labor supply							
Fraction of working in previous wave	0.70	0.88	-0.18***	0.89	-0.19***	0.71	0
	(0.46)	(0.32)	(0.04)	(0.31)	(0.04)	(0.46)	(0.05)
Fraction of working in reference wave	0.31	0.87	-0.56***	0.88	-0.57***	0.34	0.03
	(0.46)	(0.34)	(0.03)	(0.33)	(0.03)	(0.47)	(0.03)
Fraction of working in next wave	0.28	0.82	-0.54***	0.85	-0.57***	0.06	-0.22***
	(0.45)	(0.38)	(0.03)	(0.36)	(0.03)	(0.23)	(0.03)
Obs.	322	909	1231	21306	21628	453	775
Number of individuals	322	347	669	8452	8774	453	775

Notes: Standard deviations are in parentheses. For the mean differences the standard errors are in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 1.3 - First Stage Regressions Using different Specifications

	(i)	(ii)	(iii)
Indicator 65>FRA<66	0.04	0.04	0.03
	(0.05)	(0.04)	(0.05)
Indicator FRA>=66	0.19***	0.19***	0.17***
	(0.05)	(0.05)	(0.05)
More generous state		-0.03	-0.02
		(0.03)	(0.03)
Age fixed effects	Y	Y	Y
State level controls	N	Y	Y
State fixed effects	N	N	Y
Obs.		1231	
R^2	0.25	0.26	0.28
F-statistic of the weak identification test	17.34	11.81	9.84
Critical value for max 10% bias of the weak identification test	19.93	9.08	9.08

Note: Robust standard errors are in parentheses account for clustering at the individual level.
The regressions include demographic, health, and economic controls as described in the paper.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 1.4 - Effect of SSDI Application Decision on Employment

	(i)		(ii)		(iii)	
	OLS	IV	OLS	IV	OLS	IV
Denied SSDI applicant	-0.49***	-0.37**	-0.50***	-0.38**	-0.49***	-0.36*
	(0.03)	(0.19)	(0.03)	(0.19)	(0.03)	(0.20)
Age fixed effects	Y	Y	Y	Y	Y	Y
State level controls	N	N	Y	Y	Y	Y
State fixed effects	N	N	N	N	Y	Y
Obs.			1231			
R^2	0.37	0.37	0.38	0.37	0.41	0.39
F stat.	14.08	7.05	14.51	6.94	11.73	6.47
P-value of the overidentification test		0.78		0.58		0.57

Note: Robust standard errors are in parentheses account for clustering at the individual level. The regressions include demographic, health, and economic controls as described in the paper.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 1.5 - Sensitivity Analysis of the Main Findings of the Paper

	Drop great recession years		Drop widows		Drop singles		Drop non-whites	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Denied SSDI applicant	-0.51***	-0.36*	-0.49***	-0.39**	-0.47***	-0.35	-0.47***	-0.34
	(0.03)	(0.22)	(0.03)	(0.21)	(0.04)	(0.26)	(0.04)	(0.29)
	First Stage of the 2SLS							
Indicator 65>FRA<66		0.03		0.002		-0.02		0.002
		(0.05)		(0.05)		(0.06)		(0.05)
Indicator FRA>=66		0.16***		0.15***		0.12**		0.11**
		(0.05)		(0.05)		(0.07)		(0.06)
More generous state		-0.02		-0.02		-0.01		-0.03
		(0.03)		(0.03)		(0.04)		(0.04)
Obs.	1191		1177		835		912	
P-value from the overidentification test		0.61		0.82		0.89		0.47

Notes: Robust standard errors are in parentheses account for clustering at the individual level. All the regressions are estimated using the specification (iii) described in the paper. Models also include age fixed effects and state fixed effects.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 2.1 - Select Summary Statistics for Static Model Sample

			Tercile of SLE-OLE at 75		
	All	Within Std	Low	Medium	High
Age (years)	54.1		54.3	53.8	54.2
	(3.17)		(3.32)	(2.97)	(3.20)
Expected Retirement Age (years)	63.1		62.9	63.0	63.4
	(3.67)	(1.60)	(3.37)	(3.71)	(3.92)
Expected Pr(Working Full Time at or After 62) (%)	47.80		42.69	48.02	52.84
	(38.44)	(21.99)	(36.83)	(37.04)	(40.60)
Expected Pr(Working Full Time at or After 65) (%)	27.92		23.60	28.31	32.05
	(33.64)	(19.20)	(30.68)	(32.44)	(36.93)
SLE at 75 (%)	67.55		36.68	71.53	94.45
	(27.39)	(15.07)	(18.83)	(11.83)	(8.05)
SLE at 85 (%)	46.99		21.21	49.33	70.47
	(31.18)	(16.68)	(19.81)	(23.78)	(27.19)
SLE-OLE at 75 (%)	-2.25		-34.27	1.23	26.31
	(27.75)	(15.22)	(18.23)	(8.23)	(7.64)
SLE-OLE at 85 (%)	5.22		-22.50	6.64	31.55
	(31.73)	(17.06)	(20.30)	(20.86)	(26.69)
Same-Sex Parent					
Currently Alive	0.37		0.31	0.43	0.39
	(0.48)		(0.46)	(0.49)	(0.49)
Alive and <75	0.09		0.09	0.10	0.08
	(0.29)		(0.28)	(0.30)	(0.27)
Alive and 75-85	0.23		0.19	0.27	0.25
	(0.42)		(0.39)	(0.44)	(0.43)
Alive and 85+	0.05		0.03	0.05	0.06
	(0.21)		(0.18)	(0.22)	(0.23)
Died <50	0.06		0.07	0.05	0.06
	(0.24)		(0.26)	(0.22)	(0.23)
Died 66-75	0.18		0.21	0.16	0.16
	(0.38)		(0.41)	(0.37)	(0.37)
Died 75+	0.21		0.18	0.21	0.24
	(0.40)		(0.38)	(0.41)	(0.43)
Not Known if Alive	0.02		0.02	0.02	0.02
	(0.15)		(0.15)	(0.14)	(0.15)

Table 2.1 - Select Summary Statistics for Static Model Sample (*continued* ...)

		Tercile of SLE-OLE at 75		
	All	Low	Medium	High
Opposite-Sex Parent				
Currently Alive	0.35	0.29	0.36	0.39
	(0.48)	(0.45)	(0.48)	(0.49)
Alive and <75	0.07	0.06	0.07	0.07
	(0.25)	(0.24)	(0.25)	(0.26)
Alive and 75-85	0.23	0.19	0.25	0.25
	(0.42)	(0.39)	(0.43)	(0.44)
Alive and 85+	0.05	0.04	0.04	0.06
	(0.21)	(0.19)	(0.20)	(0.23)
Died <50	0.06	0.07	0.06	0.06
	(0.24)	(0.25)	(0.25)	(0.24)
Died 66-75	0.19	0.22	0.19	0.17
	(0.39)	(0.41)	(0.39)	(0.38)
Died 75+	0.21	0.21	0.20	0.22
	(0.41)	(0.41)	(0.40)	(0.41)
Not Known if Alive	0.02	0.02	0.02	0.02
	(0.15)	(0.14)	(0.14)	(0.15)
Sample Size	7,105	2,284	2,358	2,315

Note: Standard deviations in parentheses.
Source: Authors' calculations from the *Health and Retirement Study* (1992-2010).

Table 2.2 – First Stage Regression Results of Living and Deceased Parents' age on SLE-OLE

	SLE-OLE at 75	SLE-OLE at 85
Same-Sex Parent		
Currently Alive	12.737**	15.142**
	(5.384)	(6.444)
Alive and <75	-3.828	-6.89
	(5.444)	(6.535)
Alive and 75-85	-2.215	-5.34
	(5.366)	(6.427)
Alive and 85+	-1.275	0.985
	(5.413)	(6.493)
Died <50	5.309***	5.585***
	(1.289)	(1.525)
Died 66-75	3.012***	-0.195
	(0.902)	(1.029)
Died 75+	8.742***	6.170***
	(0.836)	(0.976)
N/A	0.018	
	(0.032)	
Opposite-Sex Parent		
Currently Alive	2.808	3.557
	(8.799)	(7.406)
Alive and <75	0.219	-0.821
	(8.865)	(7.521)
Alive and 75-85	0.706	0.317
	(8.802)	(7.397)
Alive and 85+	2.985	4.71
	(8.828)	(7.452)
Died <50	0.873	1.265
	(1.165)	(1.433)
Died 66-75	0.769	-1.247
	(0.835)	(0.996)
Died 75+	3.551***	3.458***
	(0.775)	(0.962)
N	17,775	13,134
Overidentification test p-value	0.210	0.100
F-stat	15.9	14.0

Notes: ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Source: Authors' estimates using *Health and Retirement Study* (1992-2010).

Table 2.3 - Results of Regressions of Retirement Expectations on Subjective Life Expectancy

	SLE-OLE at 75			SLE-OLE at 85		
	OLS	FE	IV	OLS	FE	IV
Expected Retirement Age	0.007***	0.002*	0.014**	0.007***	0.003**	0.012**
	(0.001)	(0.001)	(0.007)	(0.001)	(0.002)	(0.006)
N		17775			13134	
Working Full Time at or After 62	0.151***	0.106***	0.168**	0.123***	0.078***	0.167***
	(0.010)	(0.011)	(0.068)	(0.010)	(0.012)	(0.059)
N		34245			24971	
Working Full Time at or After 65	0.142***	0.082***	0.209***	0.157***	0.097***	0.204***
	(0.009)	(0.009)	(0.060)	(0.009)	(0.011)	(0.050)
N		34169			24921	

Notes: Each row is a separate regression. Each cell contains coefficient and standard error for subjective life expectancy variable (standardized by objective longevity).
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Source: Authors' estimates using *Health and Retirement Study* (1992-2010).

Table 2.4 - Results of Regressions of Retirement Expectations on Subjective Life Expectancy, by Gender

	SLE-OLE at 75			SLE-OLE at 85		
	OLS	FE	IV	OLS	FE	IV
Panel A: Men						
Expected Retirement Age	0.008***	0.005**	0.017	0.007***	0.006**	0.007
	(0.002)	(0.002)	(0.011)	(0.047)	(0.002)	(0.009)
N		7515			5778	
Working Full Time at or After 62	0.150***	0.119***	0.082	0.119***	0.075***	0.058
	(0.015)	(0.015)	(0.102)	(0.015)	(0.018)	(0.095)
N		15323			11434	
Working Full Time at or After 65	0.149***	0.101***	0.185**	0.169***	0.121***	0.162*
	(0.013)	(0.013)	(0.091)	(0.013)	(0.016)	(0.083)
N		15291			11415	
Panel B: Women						
Expected Retirement Age	0.006***	0.000	0.007	0.007***	0.001	0.015*
	(0.001)	(0.002)	(0.009)	(0.001)	(0.002)	(0.008)
N		10227			7323	
Working Full Time at or After 62	0.146***	0.092***	0.231***	0.127***	0.078***	0.247***
	(0.013)	(0.014)	(0.087)	(0.013)	(0.017)	(0.073)
N		18861			13476	
Working Full Time at or After 65	0.132***	0.065***	0.223***	0.148***	0.078***	0.226***
	(0.011)	(0.012)	(0.076)	(0.011)	(0.014)	(0.061)
N		18817			13445	

Note: Each row is a separate regression. Each cell contains coefficient and standard error for subjective life expectancy variable (standardized by objective longevity). ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Source: Authors' estimates using *Health and Retirement Study* (1992-2010).

Table 2.5 - Results of Regressions of Retirement Expectations on Subjective Life Expectancy without Standardization

	SLE at 75			SLE at 85		
	OLS	FE	IV	OLS	FE	IV
Expected Retirement Age	0.008***	0.002*	0.014**	0.007***	0.003**	0.012**
	(0.001)	(0.001)	(0.007)	(0.001)	(0.002)	(0.006)
N		17775			13134	
Working Full Time at or After 62	0.154***	0.105***	0.169**	0.125***	0.078***	0.168***
	(0.010)	(0.011)	(0.068)	(0.010)	(0.012)	(0.059)
N		34245			24971	
Working Full Time at or After 65	0.144***	0.082***	0.210***	0.159***	0.097***	0.205***
	(0.009)	(0.009)	(0.060)	(0.009)	(0.011)	(0.050)
N		34169			24921	

Note: Each row is a separate regression. Each cell contains coefficient and standard error for subjective life expectancy variable (without standardization by objective longevity). ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Source: Authors' estimates using *Health and Retirement Study* (1992-2010).

Table 2.6 - Results of Regressions of Retirement Expectations on Subjective Life Expectancy: Robustness Checks

	SLE-OLE at 75			SLE-OLE at 85		
	OLS	FE	IV	OLS	FE	IV
Panel A: Without max 70 restriction						
Expected Retirement Age	0.010***	0.002	0.019**	0.011***	0.004**	0.017**
	(0.001)	(0.001)	(0.008)	(0.001)	(0.002)	(0.007)
N		17775			13134	
Panel B: Without Dropping Waves 5-7						
Expected Retirement Age				0.007***	0.001	0.012**
				(0.001)	(0.001)	(0.006)
N					17654	
Working Full Time at or After 62				0.121***	0.078***	0.151***
				(0.009)	(0.010)	(0.054)
N					33969	
Working Full Time at or After 65				0.151***	0.096***	0.188***
				(0.008)	(0.008)	(0.047)
N					33905	

Note: Each row is a separate regression. Each cell contains coefficient and standard error for subjective life expectancy variable (standardized by objective longevity). ***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Source: Authors' estimates using *Health and Retirement Study* (1992-2010).

Table 2.7 - Regression Results for Expected vs. Actual Retirement Decisions on Subjective Life Expectancy

	SLE-OLE at 75				SLE-OLE at 85			
	Expected		Actual		Expected		Actual	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Retirement Age	0.004**	0.031**	0.001	0.005	0.002	0.015*	0.003	0.002
	(0.002)	(0.013)	(0.003)	(0.014)	(0.002)	(0.009)	(0.002)	(0.010)
N	2590				2571			
Working Full Time at or After 62	0.158***	0.227*	0.018	0.069	0.125***	0.221**	0.045**	0.120
	(0.019)	(0.116)	(0.023)	(0.142)	(0.017)	(0.094)	(0.020)	(0.114)
N	6290				6232			
Working Full Time at or After 65	0.163***	0.270**	0.075***	0.249*	0.162***	0.271***	0.076***	0.219*
	(0.017)	(0.106)	(0.022)	(0.136)	(0.016)	(0.089)	(0.020)	(0.112)
N	5152				5110			

Note: Each cell contains coefficient and standard error for subjective life expectancy variable (standardized by objective longevity).
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.
Source: Authors' estimates using *Health and Retirement Study* (1992-2010).

Table 3.1 - Summary Statistics

	All cohorts	1938-42 Cohorts	1943-44 Cohorts	
Variable	Mean	Mean	Mean	Mean
Earnings test removed cohort	0.67	0.42	1.00	1.00
Race				
African American	0.14	0.14	0.14	0.13
Hispanic	0.07	0.07	0.08	0.06
Female	0.50	0.48	0.52	0.55
Married	0.71	0.72	0.71	0.72
Education (in years)	12.72	12.48	12.97	13.48
Fair or poor health	0.18	0.18	0.17	0.15
Pension on current job	0.12	0.13	0.10	0.11
Pension on current job - don't know	0.78	0.75	0.80	0.83
DB pension on current job	0.14	0.14	0.13	0.10
DB pension on current job - don't know	0.01	0.01	0.02	0.04
Household wealth ($100,000)	4.96	3.86	6.22	7.34
Wage (hourly)	16.02	14.48	18.08	17.84
Wage - don't know	0.11	0.09	0.13	0.10
High TICS score	0.22	0.32	0.09	0.11
High memory score	0.60	0.56	0.64	0.66
Salaried	0.19	0.17	0.22	0.21
Salaried - unknown	0.43	0.46	0.38	0.38
Self-employed	0.17	0.15	0.19	0.18
Self-employed - unknown	0.26	0.31	0.19	0.20
Health insurance				
Employer	0.52	0.49	0.56	0.55
Government	0.23	0.25	0.19	0.20
Other	0.04	0.03	0.06	0.09
Spouse's employer	0.17	0.16	0.18	0.18
Spouse HI from spouse's employer	0.27	0.26	0.29	0.26
No retiree health insurance	0.69	0.74	0.62	0.60
Retiree health insurance - unknown	0.16	0.21	0.11	0.09
Average indexed monthly earnings at 64 (lifetime earnings measure)	2,252	1,900	2,576	2,805
Observations	3,717	2,109	1,363	245

Table 3.2 - Baseline Results of Social Security Program and Labor Market Outcomes

	(1)	(2)	(3)
Panel A: Claiming Social Security			
FRA dummy	0.1498***	0.1488***	0.1488***
	(0.017)	(0.017)	(0.017)
Month when turns 65	0.1509***	0.1480**	0.1480**
	(0.014)	(0.014)	(0.014)
Month when turns 64	0.0123***	0.0162***	0.0162***
	(0.006)	(0.006)	(0.006)
Earnings test removal cohorts			0.0473***
			(0.010)
Panel B: Exit from the labor force hazard			
FRA dummy	0.0052	0.003	0.003
	(0.007)	(0.007)	(0.007)
Month when turns 65	0.0108	0.0130*	0.0130*
	(0.007)	(0.007)	(0.007)
Month when turns 64	-0.0035	-0.0041	-0.0041
	(0.004)	(0.004)	(0.004)
Earnings test removal cohorts			0.0297***
			(0.010)
Panel C: Retirement hazard			
FRA dummy	0.0256***	0.0238**	0.0238**
	(0.010)	(0.010)	(0.010)
Month when turns 65	0.0657***	0.0662***	0.0662***
	(0.010)	(0.010)	(0.010)
Month when turns 64	0.0152***	0.0152***	0.0152***
	(0.006)	(0.006)	(0.006)
Earnings test removal cohorts			0.0036
			(0.005)
Observations	30,214	30,141	30,141

Standard errors in parentheses

Notes: a) Model 1 has cohort and age fixed effects only; model 2 adds the individual controls; model 3 adds Earnings Test Removal indicator model 2.

b) Controls in models (2) & (3): race, sex, marital status, education, health, health insurance coverage, retiree health insurance coverage, pension coverage, pension type, household wealth, average hourly earnings, and measures of cognitive capability, planning horizon, and risk aversion.

c) Sample: HRS waves 1996-2010, cohorts born in 1931-44.

d) Age range included in the regression is 64-66.

e) The models were estimated by OLS with standard errors clustered by individual.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 3.3 - Descriptive Statistics of Social Security Knowledge

	All cohorts			1931-37 Cohorts			1938-42 Cohorts			1943-44 Cohorts		
Variable	Obs	Mean	Std. dev.	Obs	Mean	Std. dev.	Obs	Mean	Std. dev.	Obs	Mean	Std. dev.
Expected age of retirement	1,626	65.1	1.7	593	65.1	1.8	854	65.1	1.6	179	66.6	1.7
Expected SS benefit at expected retirement age	1,540	1,022	490.5	764	866	387.2	642	1,160	516.2	134	1,251	595.6
Calculated benefits at expected retirement age	1,231	1,164	476.4	408	1,019	380.1	685	1,214	496.6	138	1,346	518.8
Expected SS benefit at 62[1]	277	871	432.6	0	--	--	222	853	427.0	55	946	450.8
Calculated benefits at age 62	2,484	781	329.8	1,236	679	268.4	1,060	874	352.2	188	932	353.2
Expected SS benefit at FRA[1]	455	1,191	517.1	0	--	--	391	1,190	513.6	64	1,203	542.0
Calculated benefits at FRA	2,611	1,036	435.0	1,341	889	335.8	1,082	1,169	468.1	188	1,317	479.3

[1] Expected Social Security benefits at age 62 and the FRA were added to the HRS in 2002, and only asked of people who have not yet reached those ages. Thus we do not know the expectations of the earlier cohort.

Table 3.4 - Sensitivity to Social Security Knowledge

	(1)	(2)	(3)
	Age 62	FRA	Self-reported retirement age
Panel A: Claiming Social Security			
FRA dummy * slight underestimation	0.0162	-0.1142	-0.0556
	(0.086)	(0.096)	(0.055)
FRA* large overestimation	-0.1696	-0.1238	0.0233
	(0.119)	(0.139)	(0.101)
FRA* large underestimation	-0.0162	-0.0393	-0.0508
	(0.117)	(0.117)	(0.068)
FRA * no response	0.0378	-0.0977	-0.1445***
	(0.084)	(0.101)	(0.053)
Age 65 * slight underestimation	0.0518	0.0646	0.0259
	(0.050)	(0.050)	(0.039)
Age 65 * large overestimation	0.1333	0.0049	-0.0344
	(0.094)	(0.101)	(0.071)
Age 65 * large underestimation	0.0947	0.0114	-0.0337
	(0.071)	(0.064)	(0.049)
Age 65 * no response	0.0571	0.0128	0.0503
	(0.046)	(0.055)	(0.040)
Age 64 * slight underestimation	-0.0189	0.0248	0.0157
	(0.031)	(0.028)	(0.016)
Age 64 * large overestimation	-0.0236	0.0089	-0.0238
	(0.023)	(0.067)	(0.035)
Age 64 * large underestimation	0.0001	0.0036	-0.0055
	(0.032)	(0.035)	(0.018)
Age 64 * no response	0.005	0.0217	0.0162
	(0.028)	(0.031)	(0.015)
Panel B: Exit from the labor force hazard			
FRA dummy * slight underestimation	0.0083	-0.0712	-0.0026
	(0.054)	(0.048)	(0.022)
FRA * large overestimation	-0.0672	-0.0864	-0.0398
	(0.080)	(0.057)	(0.025)
FRA * large underestimation	-0.0438	-0.067	-0.0393*
	(0.049)	(0.064)	(0.022)
FRA * no response	0.0311	-0.0126	-0.0073
	(0.054)	(0.052)	(0.021)
Age 65 * slight underestimation	0	-0.0014	-0.0155
	(0.042)	(0.041)	(0.022)
Age 65 * large overestimation	-0.0381	-0.0995	-0.0691
	(0.034)	(0.089)	(0.044)
Age 65 * large underestimation	-0.0262	0.0097	0.0019
	(0.036)	(0.045)	(0.026)
Age 65 * no response	-0.0138	-0.0494	-0.0215
	(0.040)	(0.046)	(0.023)
Age 64 * slight underestimation	-0.0048	-0.0181	-0.0021
	(0.031)	(0.031)	(0.014)
Age 64 * large overestimation	-0.0286	-0.1348	-0.0362
	(0.032)	(0.091)	(0.049)

Table 3.4 - Sensitivity to Social Security Knowledge (*continued* ...)

	(1)	(2)	(3)
	Age 62	FRA	Self-reported retirement age
Age 64 * large underestimation	-0.0085	-0.013	0.0018
	(0.028)	(0.030)	(0.016)
Age 64 * no response	0.0075	0.0049	-0.0043
	(0.028)	(0.033)	(0.015)
Panel C: Retirement hazard			
FRA dummy * slight underestimation	-0.075	-0.0431	0.014
	(0.067)	(0.060)	(0.030)
FRA * large overestimation	-0.0978*	0.0529	0.0682
	(0.059)	(0.132)	(0.073)
FRA * large underestimation	-0.1434**	-0.0197	0.0535
	(0.064)	(0.080)	(0.047)
FRA * no response	-0.0752	-0.03	-0.0354
	(0.065)	(0.064)	(0.030)
Age 65 * slight underestimation	-0.0099	0.0772	0.0013
	(0.042)	(0.048)	(0.029)
Age 65 * large overestimation	0.2014	0.0022	0.0402
	(0.159)	(0.044)	(0.055)
Age 65 * large underestimation	0.0736	0.0849	-0.0803*
	(0.074)	(0.069)	(0.045)
Age 65 * no response	0.0172	0.0287	0.0426
	(0.040)	(0.054)	(0.031)
Age 64 * slight underestimation	0.0234	0.0737*	0.0166
	(0.023)	(0.038)	(0.018)
Age 64 * large overestimation	0.0814	0.0502	0.0161
	(0.086)	(0.089)	(0.027)
Age 64 * large underestimation	0.0118	-0.0075	-0.0035
	(0.008)	(0.042)	(0.031)
Age 64 * no response	0.03	0.0573	0.0068
	(0.019)	(0.043)	(0.017)
Observations			

Standard errors in parentheses

Notes: a) All models include all controls as model 3 in Table 2.

b) Sample: HRS waves 1996-2010, cohorts born in 1931-44.

c) Age range included in the regression is 64-66.

d) The models were estimated by OLS with clustered standard errors at the individual.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table 3.5 - Sensitivity to Retiree Health Insurance

	(1)
Panel A: Claiming Social Security	
FRA dummy * no retiree health insurance	-0.0453
	(0.047)
Age 65 * no retiree health insurance	-0.0189
	(0.033)
Age 64 * no retiree health insurance	-0.0085
	(0.013)
Panel B: Exit from the labor force hazard	
FRA dummy * no retiree health insurance	0.0131
	(0.020)
Age 65 * no retiree health insurance	0.0163
	(0.019)
Age 64 * no retiree health insurance	0.005
	(0.010)
Panel C: Retirement hazard	
FRA dummy * no retiree health insurance	-0.0575**
	(0.028)
Age 65 * no retiree health insurance	0.0748***
	(0.023)
Age 64 * no retiree health insurance	-0.0058
	(0.013)
Observations	

Standard errors in parentheses

Notes: a) All models include all controls as model 3 in Table 2.

b) Sample: HRS waves 1996-2010, cohorts born in 1931-44.

c) Age range included in the regression is 64-66.

d) The models were estimated by OLS with clustered standard errors at the individual.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

APPENDIX

A3.1 Social Security Benefit Calculations

The Social Security benefit calculation is a three step process. First, a worker's previous earnings are restated in terms of today's wages by indexing past earnings to wage growth. Second, earnings for the highest 35 years are then averaged and divided by 12 to calculate Average Indexed Monthly Earnings (AIME). Finally, the Primary Insurance Amount (PIA) is the sum of three separate percentages that are applied to portions of the AIME (SSA 2012). The bend-points are indexed for wage growth, and thus depend on the year in which a person reaches age 62. Specifically, for workers first becoming eligible for benefits in 2013, their PIA is the sum of:

- 90 percent of the worker's first $791 of AIME, plus
- 32 percent of AIME between $791 and $4,768, plus
- 15 percent of any AIME in excess of $4,768.

This PIA is continually recalculated as long as the individual remains employed; and is indexed to prices from age 62. The benefit actually paid depends on when the worker claims. A retiree is entitled to a benefit equal to the PIA at the Full Retirement Age (which was 65 for the first cohort under consideration, 66 for the last cohort). A worker may choose to retire as early as age 62, with reduced benefits. In contrast, if a worker elects to delay receipt of benefits to an age as late as 70, the eventual benefits are permanently increased for each year of delay.

The administrative data provides Social Security earnings histories back to 1951 for the approximately 70 percent of the sample that has given permission to link. Previous work suggests that giving permission to link is essentially random (Kapteyn et al. 2006). Thus we follow Gustman and Steinmeier (2001) and estimate earnings histories based on survey data on previous jobs and wages, using the estimated returns to tenure from Anderson et al. (1999).

To estimate Social Security benefits in retirement, it is necessary to project earnings beyond the year at which the individual last gave permission to match to the administrative data. Following Gustman and Steinmeier (2001), we make the following assumptions about wage growth. For individuals with self-reported earnings in the survey, the assumption is that their real earnings observed in the last reported year persist until their expected retirement date. For those who do not report self-reported earnings, zeros are projected for future years. Future benefits are calculated at three claiming ages to match the expectations questions in the HRS: age 62, the FRA, and the individual's self-reported retirement age. We then put it in current-year dollars to match the year of the expectation questions using the CPI.

Appendix Table AT2.1 Full Regression Results of Expected Retirement Age

	OLS	FE	IV	OLS	FE	IV
SLE-OLE at 75	0.007***	0.002*	0.014**			
	(0.001)	(0.001)	(0.007)			
SLE-OLE at 85				0.007***	0.003**	0.012**
				(0.001)	(0.002)	(0.006)
Low risk tolerant	0.069		0.075	0.075		0.08
	(0.093)		(0.093)	(0.096)		(0.095)
Moderate risk tolerant	0.332***		0.338***	0.292**		0.297***
	(0.106)		(0.106)	(0.113)		(0.113)
High risk tolerant	0.073		0.071	0.032		0.025
	(0.104)		(0.104)	(0.110)		(0.110)
Married	-0.157	-0.259	-0.155	-0.01	-0.033	-0.005
	(0.169)	(0.290)	(0.167)	(0.182)	(0.373)	(0.181)
Divorced	0.323*	-0.118	0.301	0.29	-0.09	0.281
	(0.185)	(0.310)	(0.185)	(0.191)	(0.357)	(0.191)
Widowed	-0.033	-0.307	-0.043	-0.157	0.237	-0.158
	(0.207)	(0.374)	(0.206)	(0.213)	(0.437)	(0.213)
Female	-0.348***		-0.306***	-0.300***		-0.264***
	(0.081)		(0.091)	(0.085)		(0.095)
White	0.512***		0.529***	0.506***		0.551***
	(0.088)		(0.090)	(0.094)		(0.109)
Hispanic	-0.239*		-0.196	-0.258**		-0.233*
	(0.132)		(0.137)	(0.130)		(0.133)
<4 Children	0.098	0.116	0.098	0.217	0.455	0.21
	(0.135)	(0.287)	(0.134)	(0.139)	(0.305)	(0.139)
4+ Children	0.04	-0.125	0.036	0.2	0.222	0.196
	(0.140)	(0.304)	(0.140)	(0.146)	(0.347)	(0.146)
Has Siblings	-0.133	0.022	-0.139	-0.11	0.064	-0.113
	(0.100)	(0.279)	(0.100)	(0.101)	(0.327)	(0.101)
Northeast	-0.001	-1.400*	0.005	0.145	-1.715**	0.149
	(0.091)	(0.736)	(0.091)	(0.096)	(0.763)	(0.096)
Midwest	-0.066	-0.902**	-0.062	-0.025	-0.681	-0.023
	(0.079)	(0.449)	(0.079)	(0.083)	(0.452)	(0.083)
West	0.138	-1.468**	0.121	0.180*	-1.580*	0.169
	(0.099)	(0.586)	(0.100)	(0.104)	(0.891)	(0.104)
Less than HS	-0.113		-0.113	-0.117		-0.117
	(0.100)		(0.100)	(0.104)		(0.103)
Some College	0.347***		0.333***	0.286***		0.270***
	(0.083)		(0.084)	(0.089)		(0.090)
College or More	0.663***		0.651***	0.674***		0.659***
	(0.095)		(0.096)	(0.100)		(0.101)
Employer HI	-0.042	-0.055	-0.046	-0.007	-0.028	-0.007
	(0.093)	(0.112)	(0.092)	(0.103)	(0.142)	(0.102)
Government HI	-0.285**	0.149	-0.300**	-0.176	0.229	-0.193
	(0.137)	(0.206)	(0.137)	(0.149)	(0.259)	(0.149)
Other HI	-0.094	-0.041	-0.092	-0.1	-0.054	-0.094
	(0.102)	(0.112)	(0.102)	(0.112)	(0.133)	(0.112)

Appendix Table AT2.1 (*continued ...*)

	OLS	FE	IV	OLS	FE	IV
Spouse on respondent's ESI	0.167*	0.039	0.170*	0.131	-0.001	0.136
	(0.091)	(0.101)	(0.091)	(0.100)	(0.129)	(0.100)
Retiree HI	-0.687***	-0.111	-0.695***	-0.523***	-0.029	-0.530***
	(0.081)	(0.091)	(0.081)	(0.088)	(0.115)	(0.089)
Blue Collar	-0.308***	0.25	-0.294***	-0.272***	0.625**	-0.267***
	(0.077)	(0.218)	(0.079)	(0.082)	(0.298)	(0.082)
Self Employed	1.106***	0.957***	1.091***	1.049***	1.012***	1.032***
	(0.115)	(0.274)	(0.115)	(0.125)	(0.370)	(0.126)
Ever Have DB	-0.963***	-0.245***	-0.964***	-0.880***	-0.330***	-0.878***
	(0.062)	(0.071)	(0.062)	(0.068)	(0.091)	(0.068)
Ever Have DC	0.103*	-0.177***	0.091	0.078	-0.161*	0.071
	(0.058)	(0.065)	(0.059)	(0.065)	(0.083)	(0.065)
U Rate	0.120***	0.075***	0.121***	0.125***	0.090***	0.123***
	(0.018)	(0.017)	(0.018)	(0.019)	(0.019)	(0.019)
Long Financial Planning	0.247***	0.039	0.234***	0.250**	0.169	0.242**
	(0.084)	(0.092)	(0.085)	(0.101)	(0.131)	(0.101)
Lowest HH Income	-0.001	0.331**	0.01	0.07	0.423**	0.076
	(0.116)	(0.140)	(0.116)	(0.128)	(0.178)	(0.128)
2nd HH Inc Quintile	0.196**	0.204**	0.197**	0.223**	0.210*	0.224**
	(0.083)	(0.093)	(0.083)	(0.092)	(0.127)	(0.092)
4th HH Inc Quintile	-0.317***	-0.258***	-0.320***	-0.315***	-0.313***	-0.317***
	(0.075)	(0.081)	(0.075)	(0.084)	(0.098)	(0.084)
Highest HH Income	-0.640***	-0.414***	-0.654***	-0.562***	-0.348***	-0.570***
	(0.092)	(0.104)	(0.092)	(0.101)	(0.126)	(0.101)
Lowest Wealth	0.870***	0.267**	0.856***	0.845***	0.207	0.828***
	(0.103)	(0.131)	(0.104)	(0.113)	(0.166)	(0.115)
2nd Wealth Quintile	0.455***	0.152*	0.451***	0.370***	0.099	0.370***
	(0.081)	(0.091)	(0.081)	(0.091)	(0.119)	(0.090)
4th Wealth Quintile	-0.276***	-0.063	-0.282***	-0.283***	-0.099	-0.283***
	(0.076)	(0.084)	(0.076)	(0.085)	(0.108)	(0.085)
Highest Wealth	-0.626***	-0.144	-0.635***	-0.613***	-0.206	-0.615***
	(0.095)	(0.118)	(0.095)	(0.103)	(0.148)	(0.102)
Fair or Poor Health	-0.332***	-0.299***	-0.245*	-0.395***	-0.407***	-0.332***
	(0.088)	(0.102)	(0.125)	(0.099)	(0.131)	(0.124)
Any ADLs	-0.208***	-0.016	-0.190***	-0.206***	0.021	-0.188***
	(0.059)	(0.064)	(0.061)	(0.065)	(0.079)	(0.067)
Never Smoked	-0.027	-0.72	-0.017	0.047	1.208***	0.051
	(0.073)	(1.594)	(0.074)	(0.076)	(0.408)	(0.076)
Smoke Now	-0.141*	-0.255*	-0.098	-0.126	-0.19	-0.091
	(0.083)	(0.145)	(0.095)	(0.088)	(0.180)	(0.099)

Appendix Table AT2.1 (*continued ...*)

	OLS	FE	IV	OLS	FE	IV
Doesn't Drink	0.047	-0.01	0.057	0.022	-0.016	0.028
	(0.065)	(0.085)	(0.065)	(0.071)	(0.116)	(0.071)
Drink Heavily	-0.095	0.062	-0.093	-0.072	0.042	-0.067
	(0.089)	(0.110)	(0.089)	(0.094)	(0.129)	(0.094)
High Blood Pressure	0.039	0.143	0.056	0.036	0.113	0.051
	(0.063)	(0.089)	(0.064)	(0.067)	(0.099)	(0.069)
Diabetes	0.039	0.073	0.052	0.03	0.103	0.049
	(0.102)	(0.157)	(0.102)	(0.110)	(0.188)	(0.112)
Cancer	0.038	0.144	0.068	0.081	0.092	0.103
	(0.141)	(0.198)	(0.144)	(0.150)	(0.251)	(0.152)
Lung Condition	0.262*	0.14	0.289**	0.339**	0.203	0.359**
	(0.145)	(0.202)	(0.146)	(0.147)	(0.226)	(0.148)
Heart Condition	-0.053	0.043	-0.028	-0.059	-0.098	-0.033
	(0.105)	(0.153)	(0.108)	(0.110)	(0.184)	(0.114)
Stroke	0.039	-0.362	0.05	-0.084	-0.174	-0.075
	(0.263)	(0.279)	(0.265)	(0.277)	(0.384)	(0.276)
Psychiatric Condition	0.178*	-0.176	0.181*	0.189*	-0.107	0.193*
	(0.104)	(0.165)	(0.104)	(0.115)	(0.214)	(0.114)
Arthritis	-0.110*	-0.01	-0.109*	-0.112*	0.023	-0.110*
	(0.063)	(0.080)	(0.063)	(0.067)	(0.091)	(0.067)
Spouse <50	0.581***	0.251*	0.571***	0.510***	0.085	0.499***
	(0.109)	(0.141)	(0.109)	(0.116)	(0.171)	(0.116)
Spouse 62+	0.345***	0.137	0.346***	0.323***	0.182	0.320***
	(0.091)	(0.118)	(0.091)	(0.106)	(0.159)	(0.105)
Spouse Fair/Poor Health	0.168*	0.126	0.180**	0.175*	0.073	0.179*
	(0.090)	(0.102)	(0.091)	(0.098)	(0.134)	(0.098)
Spouse Working	0.341***	0.131	0.343***	0.284***	0.148	0.284***
	(0.091)	(0.115)	(0.091)	(0.098)	(0.145)	(0.098)
Spouse Unemployed	0.649***	0.337	0.662***	0.634**	0.359	0.652***
	(0.224)	(0.240)	(0.224)	(0.249)	(0.286)	(0.249)
Spous Disabled	0.228	0.173	0.21	0.102	0.321	0.1
	(0.222)	(0.215)	(0.223)	(0.249)	(0.276)	(0.248)
Spouse Retired	-0.447***	0.162	-0.445***	-0.435***	0.307**	-0.431***
	(0.097)	(0.117)	(0.097)	(0.109)	(0.153)	(0.109)
Respondent on Spouse's ESI	-0.332***	-0.143	-0.340***	-0.262**	-0.213	-0.262**
	(0.103)	(0.120)	(0.104)	(0.116)	(0.156)	(0.116)
Spouse has ESI	0.068	0.051	0.085	0.046	-0.09	0.056
	(0.095)	(0.112)	(0.097)	(0.106)	(0.149)	(0.106)
Spouse has RHI	-0.182**	0.092	-0.185**	-0.295***	0.085	-0.298***
	(0.089)	(0.096)	(0.089)	(0.097)	(0.123)	(0.097)

Table AT2.1 (*continued ...*)

	OLS	FE	IV	OLS	FE	IV
Spouse RHI N/A			0.05	0.033		0.03
			(0.120)	(0.120)		(0.120)
Mom's Schooling 8 and Up	0.191*		0.191*	0.262**		0.272***
	(0.101)		(0.100)	(0.103)		(0.103)
Mom's Schooling N/A	0.15		0.152	0.356**		0.367**
	(0.161)		(0.161)	(0.164)		(0.165)
Dad's Schooling 8 and Up	0.142		0.142	0.049		0.048
	(0.094)		(0.094)	(0.098)		(0.098)
Dad's Schooling N/A	0.054		0.051	-0.067		-0.071
	(0.137)		(0.137)	(0.142)		(0.142)
Caregiving	-0.142*	0.106	-0.152*	-0.106	0.124	-0.109
	(0.078)	(0.077)	(0.078)	(0.087)	(0.098)	(0.087)
First Stage						
Same-Sex Parent						
Currently Alive			12.737**			15.142**
			(5.384)			(6.444)
Alive and <75			-3.828			-6.89
			(5.444)			(6.535)
Alive and 75-85			-2.215			-5.34
			(5.366)			(6.427)
Alive and 85+			-1.275			0.985
			(5.413)			(6.493)
Died <50			5.309***			5.585***
			(1.289)			(1.525)
Died 66-75			3.012***			-0.195
			(0.902)			(1.029)
Died 75+			8.742***			6.170***
			(0.836)			(0.976)
Opposite-Sex Parent						
Currently Alive			2.808			3.557
			(8.799)			(7.406)
Alive and <75			0.219			-0.821
			(8.865)			(7.521)
Alive and 75-85			0.706			0.317
			(8.802)			(7.397)
Alive and 85+			2.985			4.71
			(8.828)			(7.452)
Died <50			0.873			1.265
			(1.165)			(1.433)
Died 66-75			0.769			-1.247
			(0.835)			(0.996)
Died 75+			3.551***			3.458***
			(0.775)			(0.962)
N	17,775	17,775	17,775	13,134	13,134	13,134
adj. R-sq	0.185			0.190		
Overidentification test p-value			0.210			0.100
F-stat			15.9			14.0

Appendix Table AT3.1 - Full Baseline Results of Social Security Program and Labor Market Outcomes

	(1)		(2)		(3)	
	Claim Social Security		Exit the labor force		Retire	
	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error
FRA indicator	**0.1488**	**(0.0165)**	**0.0030**	**(0.0065)**	**0.0238**	**(0.0095)**
Race						
African American	-0.0122	(0.0045)	-0.0088	(0.0022)	-0.0028	(0.0029)
Hispanic	-0.0142	(0.0053)	0.0027	(0.0034)	0.0046	(0.0050)
Female	0.0027	(0.0030)	-0.0002	(0.0018)	-0.0007	(0.0021)
Married	0.0078	(0.0034)	-0.0043	(0.0020)	0.0007	(0.0024)
Education (in years)	-0.0015	(0.0006)	-0.0002	(0.0003)	-0.0004	(0.0004)
Fair or poor health	-0.0060	(0.0042)	0.0099	(0.0024)	-0.0007	(0.0029)
Pension on current job	0.0199	(0.0087)	0.0046	(0.0043)	0.0131	(0.0062)
Pension on current job –don't know	-0.0046	(0.0072)	-0.0137	(0.0033)	-0.0080	(0.0045)
DB pension on current job	-0.0021	(0.0033)	0.0048	(0.0020)	0.0040	(0.0024)
DB pension on current job - don't know	-0.0081	(0.0081)	0.0014	(0.0067)	-0.0015	(0.0066)
Household wealth	-0.0001	(0.0000)	0.0000	(0.0000)	0.0000	(0.0000)
Wage	-0.0035	(0.0015)	-0.0015	(0.0015)	0.0026	(0.0028)
Wage - don't know	0.0018	(0.0042)	-0.0010	(0.0022)	0.0012	(0.0031)
High numeracy	0.0035	(0.0041)	0.0004	(0.0026)	0.0050	(0.0030)
Numeracy - unknown	-0.0203	(0.0080)	0.0001	(0.0035)	0.0093	(0.0044)
High TICS score	0.0021	(0.0032)	0.0024	(0.0019)	0.0033	(0.0022)
High memory score	-0.0011	(0.0030)	-0.0019	(0.0018)	0.0011	(0.0021)
Long financial planning horizon	-0.0043	(0.0040)	0.0020	(0.0024)	0.0024	(0.0028)
Financial planning horizon - missing	-0.0123	(0.0066)	-0.0006	(0.0039)	-0.0022	(0.0043)
Risk averse	0.0015	(0.0043)	0.0003	(0.0028)	-0.0003	(0.0033)
Rick aversion - missing	0.0068	(0.0054)	0.0099	(0.0033)	0.0055	(0.0036)
Salaried	-0.0062	(0.0032)	0.0000	(0.0018)	-0.0083	(0.0022)
Salaried - unknown	-0.0594	(0.0321)	0.0055	(0.0167)	0.0516	(0.0240)
Self-employed	0.0507	(0.0323)	-0.0022	(0.0167)	-0.0514	(0.0243)

Appendix Table AT3.1 - Full Baseline Results of Social Security Program and Labor Market Outcomes (*continued* ...)

	(1)		(2)		(3)	
	Claim Social Security		Exit the labor force		Retire	
	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error
Self-employed - unknown	0.0679	(0.0324)	0.0352	(0.0170)	-0.0034	(0.0238)
Health insurance						
Employer	0.0042	(0.0108)	0.0112	(0.0066)	-0.0041	(0.0089)
Government	-0.0001	(0.0073)	-0.0069	(0.0047)	-0.0089	(0.0053)
Other	-0.0056	(0.0132)	-0.0179	(0.0085)	-0.0246	(0.0111)
Spouse's employer	0.0024	(0.0126)	0.0161	(0.0090)	0.0024	(0.0106)
Spouse HI from spouse's employer	-0.0110	(0.0064)	-0.0018	(0.0043)	0.0012	(0.0051)
Retiree health insurance	-0.0101	(0.0118)	0.0065	(0.0073)	-0.0026	(0.0096)
Retiree health insurance - unknown	-0.0006	(0.0041)	0.0028	(0.0027)	-0.0013	(0.0032)
ETR_cohort	**0.0473**	**(0.0104)**	**0.0297**	**(0.0095)**	**-0.0036**	**(0.0049)**
Age (in months)						
768 (64 years)	**0.0162**	**(0.0057)**	**-0.0041**	**(0.0043)**	**0.0152**	**(0.0057)**
769	0.0242	(0.0060)	0.0086	(0.0049)	0.0040	(0.0052)
770	-0.0016	(0.0048)	0.0000	(0.0045)	0.0040	(0.0052)
771	-0.0044	(0.0047)	0.0028	(0.0047)	-0.0014	(0.0049)
772	-0.0065	(0.0046)	0.0005	(0.0045)	0.0008	(0.0051)
773	-0.0020	(0.0049)	0.0024	(0.0047)	0.0011	(0.0051)
774 (omitted group)						
775	-0.0064	(0.0047)	0.0013	(0.0046)	-0.0095	(0.0043)
776	-0.0033	(0.0049)	0.0020	(0.0047)	-0.0023	(0.0049)
777	0.0033	(0.0054)	-0.0002	(0.0046)	0.0052	(0.0055)
778	0.0152	(0.0061)	0.0012	(0.0047)	0.0026	(0.0054)
779	0.0236	(0.0066)	-0.0035	(0.0044)	0.0082	(0.0057)
780 (65 years)	**0.1480**	**(0.0138)**	**0.0130**	**(0.0071)**	**0.0662**	**(0.0101)**
781	0.2103	(0.0132)	0.0410	(0.0065)	0.0329	(0.0074)
782	0.0718	(0.0111)	0.0119	(0.0054)	0.0046	(0.0060)

Appendix Table AT3.1 - Full Baseline Results of Social Security Program and Labor Market Outcomes (*continued ...*)

	(1)		(2)		(3)	
	Claim Social Security		Exit the labor force		Retire	
	Coef.	Std. error	Coef.	Std. error	Coef.	Std. error
783	0.0940	(0.0120)	0.0177	(0.0056)	0.0227	(0.0070)
784	0.0457	(0.0105)	0.0129	(0.0055)	0.0138	(0.0067)
785	0.0706	(0.0118)	0.0073	(0.0052)	0.0234	(0.0072)
786	0.0643	(0.0125)	0.0154	(0.0057)	0.0028	(0.0060)
787	0.0635	(0.0125)	0.0065	(0.0052)	0.0179	(0.0071)
788	0.0378	(0.0123)	0.0116	(0.0056)	0.0014	(0.0061)
789	0.0627	(0.0134)	0.0073	(0.0053)	0.0149	(0.0070)
790	0.0336	(0.0123)	0.0128	(0.0057)	0.0184	(0.0073)
791	0.0409	(0.0127)	0.0149	(0.0058)	0.0051	(0.0062)
792 (66 years)	0.2021	(0.0223)	-0.0001	(0.0049)	0.0249	(0.0080)
Birth cohort						
1931 (omitted)						
1932	0.0246	(0.0101)	-0.0051	(0.0057)	-0.0028	(0.0057)
1933	0.0261	(0.0104)	-0.0165	(0.0056)	(omitted)	
1934	0.0321	(0.0102)	-0.0111	(0.0055)	-0.0032	(0.0046)
1935	(omitted)		-0.0459	(0.0079)	(omitted)	
1936	0.0023	(0.0071)	-0.0424	(0.0074)	0.0038	(0.0045)
1937	-0.0107	(0.0086)	-0.0370	(0.0073)	0.0058	(0.0052)
1938	-0.0236	(0.0105)	-0.0341	(0.0071)	0.0127	(0.0063)
1939	-0.0250	(0.0111)	-0.0285	(0.0072)	0.0148	(0.0065)
1940	-0.0434	(0.0112)	-0.0340	(0.0075)	0.0132	(0.0068)
1941	-0.0422	(0.0110)	-0.0267	(0.0076)	0.0122	(0.0067)
1942	-0.0558	(0.0107)	-0.0101	(0.0081)	0.0194	(0.0072)
1943	-0.0546	(0.0111)	(omitted)		0.0247	(0.0079)
1944	-0.0379	(0.0119)	0.0148	(0.0128)	0.0333	(0.0088)
Constant	0.0316	(0.0197)	0.0165	(0.0116)	0.0154	(0.0133)

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