# Empirical Evidence on the Labor Market Impacts of U.S. Social Insurance Programs

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# EMPIRICAL EVIDENCE ON THE LABOR MARKET IMPACTS OF U.S. SOCIAL INSURANCE PROGRAMS

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## ABSTRACT

Social insurance programs exist in the United States to help workers maintain their standard of living across different states of the world. Examples include unemployment insurance, which aids workers through the state of being unemployed, and Social Security, which supports workers through the state of retirement. The three essays in this dissertation study how these types of social insurance programs alter the decisions workers make in the labor market. The first and third essays focus on unemployment insurance, where the first essay focuses on how different types of workers make decisions in the presence of unemployment insurance and the third essay studies how all workers respond to changes in the provision of unemployment insurance. The second essay examines how Social Security retirement income influences the decision of late-career workers to participate in the labor market. All three essays emphasize that the willingness of workers to pursue a job in the labor market relies upon the social insurance available to them outside of employment.

Theoretical models of optimal unemployment insurance predict that the job search and savings behavior of unemployed workers will partially be determined by how long a worker expects to remain unemployed. Empirical evidence suggests, however, that workers often underestimate the duration of their unemployment spell. These

biased beliefs about the duration of unemployment among unemployed workers should therefore affect their job search and savings behavior. To date, no reliable data have been used to empirically analyze to what degree biased beliefs would change the behavior of unemployment workers. In the first essay, titled **Biased Beliefs and Job Search: Implications for Optimal Unemployment Insurance**, I use a novel dataset, the Survey of Unemployed Workers in New Jersey, to evaluate how biased beliefs vary across unemployed workers and how they influence the behavior of those workers. I find that overly-optimistic unemployed workers underestimate the duration of their unemployment, leading them to spend 26 percent less time searching for a job each week than those with a pessimistic bias. I also find that overly-optimistic unemployed workers have over $8,500 less saved at any given point during an unemployment spell. These results suggest that unemployed workers with an optimistic bias would benefit from an information "nudge" that encourages increased search effort and could lead to faster reemployment.

The first essay demonstrates how workers respond to the presence of social insurance when they are still focused on rejoining the labor market. That is, it provides evidence on the intensive margin. However, it does not say anything about how it would influence a worker's desire to participate in the labor market at all, on the extensive margin. In the second essay, **Do Late-Career Wages Boost Social Security More for Women than Men?**, Matthew Rutledge and I estimate the incentives for older workers to continue working during their retirement-age years when they could be collecting Social Security. Any worker who delays claiming Social Security receives a larger monthly benefit because of the actuarial adjustment. Some claimants - particularly women, who are more likely to take time out of the labor force early in their careers - can further increase their benefits if the extra years of work raise their career average earnings by displacing lower-earning years. This essay uses the *Health and Retirement Study* (HRS) linked to earnings records to quantify the impact of women's late-career earnings on Social Security benefits relative to men's. The essay finds that the average gain in Social Security retirement benefits from working one additional year raises women's monthly benefits by 8.6 percent, of which

1.6 percent is from late-career earnings. These results suggest that, especially among women, there are additional benefits to delaying claiming and further increasing the retirement age.

Through both of the first two chapters, the parameters outlining the social insurance program were held constant. In reality, the rules of a social insurance program can change over time. Motivated by this possibility, my third chapter, **The Impact of Unemployment Insurance Extensions on Worker Job-Search Behavior**, explores how reservation wages and job search effort respond to extensions of unemployment insurance. Current economic theory predicts that reservation wages should rise following an extension of potential benefit duration, while search effort should fall. Previous papers in this literature focus on the end result, which is that UI extensions result in prolonged unemployment spells. Using the Survey of Unemployed Workers in New Jersey, and the UI benefit extension in the United States in November 2009, this paper identifies the worker behaviors that lead to prolonged unemployment durations. Employing hypothesis testing and event study analysis, this study shows there are lagged, significant increases in reservation wages and decreases in search effort following the benefit extension. The results suggest that an alternative model of job search is needed.

*To Anna Marie*

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> *I think the process, the growing. You go into a winning locker room after all that we do, and all the sacrifices these young men make, and their families make, and the coaching staff, and everybody in the organization, and then you go into a winning locker room where you've actually won a game - I don't think it gets any better than that. And it makes a lot of the things that are not comfortable, more comfortable and more palatable.*

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## TABLE OF CONTENTS

# LIST OF TABLES

**LIST OF FIGURES**

# CHAPTER 1

# BIASED BELIEFS AND JOB SEARCH: IMPLICATIONS FOR OPTIMAL UNEMPLOYMENT INSURANCE

Theoretical models of optimal unemployment insurance (UI) often rely on rational unemployed workers to accurately predict the length of time they will remain jobless. This prediction influences their savings, consumption and job search behavior leading up to and through an unemployment spell. In reality, unemployed workers have been shown to systematically *underestimate* the length of their unemployment. Spinnewijn (2015) incorporates this systematic error into a model of optimal unemployment insurance and derives implications for how it would influence the behavior of unemployed workers. In particular, Spinnewijn shows that unemployed workers who underestimate the length of their unemployment spell will save less and unemployed workers who overestimate their influence over the length of their unemployment spell will spend more time searching for a job. This paper examines whether these theoretical predictions are empirically true and, if so, what the government can do to correct such suboptimal behavior.

Price et al. (2004) show that in a survey of unemployed workers and their spouses, a large portion of the unemployed have an optimistic bias. They find that the actual duration of unemployment is significantly longer than expected for most of their sample, implying that job seekers are overconfident about their chances of finding a job. In particular, the average person took 16 weeks longer than expected to find a job. These results suggest that not only is this optimism widespread, it is quite large. If it is true that this bias warps the decision-making of unemployed workers, social and individual welfare may be improved through adjustments to the UI system.

To evaluate whether unemployed workers with an optimistic bias alter their behavior compared to unbiased workers, data is needed on job search activity and household finances, both of which are not included in the survey collected by Price et al. (2004). I can overcome this limitation using the 2009-2010 Survey of Unemployed Workers in New Jersey (SUWNJ). This data set contains rich information which can be used to test the prediction that biased and unbiased unemployed workers behave differently. Given the richness of the data, I can also determine among which groups of individuals the bias is most prevalent.

In the first part of the paper, I measure the degree of biased beliefs using the SUWNJ. Measurement is complicated by the absence of information about the length of unemployment duration for the majority of survey respondents. I introduce a new technique into the literature on UI, estimating a discrete-time hazard model to determine the job-finding rates for survey respondents. Combining this analysis with subjective survey questions gives me two measures of bias, one about the perceived probability that a job seeker finds employment and a second about how much seekers believe they can influence their probability of finding employment. While the former has previously been measured in Price et al. (2004), I am the first to estimate the latter. I find that among the unemployed in New Jersey, the average job seekers needed 26 more weeks than expected to find a job. This level of optimism is significantly larger than what was observed in previous data, which is likely due to the time period during which the New Jersey survey was conducted, and is most prevalent among younger workers at low unemployment durations.

I use these measures of bias to see if people with biased beliefs behave as predicted by the theoretical model of Spinnewijn (2015). The model predicts that job seekers who are overconfident about the probability they will find employment *save less for unemployment*, while job seekers who are overconfident about their influence over their job finding probability will *search harder*. My results show the savings prediction is true. Unemployed workers with an optimistic bias have an average of \$8,400 less in savings remaining compared to their unbiased counterparts. However, the prediction about search effort is not supported in the data. Rather, I find that overconfidence in the ability to influence job-finding probabilities leads to four fewer hours of job search each week.

Compared to the existing literature, this paper makes three clear contributions. First, it employs a novel dataset in the optimal unemployment insurance literature. The SUWNJ contains a new set of rich information related to the subjective beliefs of unemployed workers. Second, it is the first paper to apply a discrete-time hazard model in the estimation of unemployment hazard rates. Given the censored nature of most non-administrative unemployment data, this is a useful tool for future research. Finally, this is currently the only paper to empirically estimate how the presence of optimism among unemployed workers influences their decision-making. The estimation results imply that policymakers could "nudge" unemployed workers towards the socially optimal outcome by promoting more precautionary savings, slower savings depletion, and more job search effort.

The remainder of the paper proceeds with a review of the literature in Section 1. A framework for how the biases may be related to the behavior of unemployed workers is provided in Section 2, followed by an outline of the data available in the SUWNJ in Section 3. Given some data limitations in the survey, Section 4 describes how the true and perceived job prospects are measured and provides estimates for those measures. The biases among unemployed workers are also presented in that section. Results from the empirical specification follow in Section 5. The final section discusses future extensions.

## 1 Literature Review

While there is some empirical evidence showing the effects of the UI program on search effort, precautionary saving, and reservation wages, this study is the first to examine those effects when agents have biased beliefs. As a result, this paper combines several strands of literature. First and foremost, psychology and economics have a long history of work studying systematic biases in risk perceptions and have found broad evidence of overconfidence. This empirical work is summarized in Moore and

Healy (2008) and has been modeled theoretically by a number of authors.[1] Behavioral economics has adopted some of this work to study how these biases distort behavior, as well as how we can learn agents' true preferences from this distorted behavior Koszegi and Rabin (2007, 2008). In the context of unemployment, Paserman (2008) uses hyperbolic discounting and DellaVigna and Paserman (2005) use impatience to analyze policy interventions. However, the most clear connection is to the theoretical work of Spinnewijn (2015) and empirical work of Mueller et al. (2016) and Ganong and Noel (2017). Both empirical papers look at the persistence of biased beliefs over time, with Ganong and Noel going on to evaluate how consumption patterns for unemployed workers evolve over the duration of unemployment spells.

Estimates of UI hazard rates and the effects of UI on consumer choices have been studied in depth. The seminal work on constructing hazard rates for UI exit was Katz and Meyer (1990), who used data from Pennsylvania and Missouri and found a flat hazard rate over the profile of unemployment duration.[2] In the same set of studies, it was found that the average job searcher without a prospect of being recalled to their previous job searched an average of 12 hours per week Katz and Meyer (1990); Krueger and Mueller (2011).[3] Empirical work linking unemployment insurance and precautionary saving was explored in SIPP data by Engen and Gruber (2001), who found that UI benefits crowd out precautionary saving. Subsequent work focuses on consumption smoothing and intra-household risk as it relates to precautionary saving Ortigueira and Siassi (2013). Studies linking unemployment insurance and reservation wages are scarce, but two were conducted by Feldstein and Poterba (1984) and Fishe (1982). Both articles estimated that higher unemployment benefits raised the reservation wage by as much as 40%.

The literature on sufficient statistic approaches to optimal UI benefit levels, the broad set of models under which Spinnewijn (2015) falls, is summarized by Chetty (2009). Within this theoretical context, many papers have used the above empiri-

[1] See for example Benabou and Tirole (2002), Compte and Postlewaite (2004), Van den Steen (2004), and Brunnermeier and Parker (2005)

[2] See also Krueger and Mueller (2011), Chetty et al. (2007b), Meyer (1990), and Moffitt (1985).

[3] See also Barron and Mellow (1979), Krueger and Mueller (2010) and Wanberg et al. (2012).

cal work as motivation. The original paper, Baily (1978), captured all three ideas of search effort, precautionary saving, and reservation wages in a simple two-period model. Chetty (2008) relaxed some of Baily's assumptions to derive a more general formula for optimal UI benefits. This formula can be estimated in several ways, either through a consumption-based approach Baily (1978), by making use of reservation wages Shimer and Werning (2007), or by breaking down the formula into moral hazard and liquidity effects Chetty (2008). This literature relies on fully informed rational agents. Mullainathan et al. (2012) give a review of reduced form approaches to estimating the influence of non-standard agents on optimal policies. In other insurance markets, many authors have already used behavioral biases as a justification for interventions.[4] There have also been papers that use the variation in risk preferences across agents to discuss the optimal design of insurance and consumer contracts.[5]

## 2 Model

This section discusses the framework for biased beliefs, as presented in Spinnewijn (2015). The model falls into the sufficient statistics literature, which frames the optimal unemployment insurance decision in terms of observables from the data rather than deep structural parameters. This model nests the rational expectations model of Chetty (2008), who extends Baily (1978) to allow for savings and benefits form leisure. However, it rules out general equilibrium effects, distortions due to firm behavior, or congestion externalities in labor supply.[6]

In Spinnewijn's model, all risk-averse agents are employed in the first period and become unemployed in the second period. In the first period, agents decide how much of their after-tax wage $w-\tau$ to save $s \in [0, w]$. Any savings allow the agent to increase second period consumption by $s(1+r)$. In the second period, agents decide how much effort $e$ to put into searching for a job, where $e$ is a utility cost. Given effort $e$, an

[4] See Santos-Pinto (2008), de la Rosa (2011) and Grubb and Osborne (2015).

[5] See for example Sandroni and Squintani (2007), Eliaz and Spiegler (2008), Grubb (2009) and Spinnewijn (2013).

[6] This is likely not true, since experience ratings weigh on employers when they decide to fire employees. As an example, see Hagedorn et al. (2016).

agent finds a job with probability $\pi(e) \in [0, 1]$, which is assumed to be increasing and concave in effort. If the agent finds a job, they earn wage $w$ again and pay tax $\tau$ for unemployment benefits. If the agent is unsuccessful, they collect unemployment benefit $b$. Given an unemployment policy $(b, \tau)$, the agent has expected utility:

$$u(w - \tau - s) + \beta[\pi(e)(u(w - \tau + (1 + r)s) + (1 - \pi(e))u(b + (1 + r)s) - e] \quad (1.1)$$

The discount factor $\beta$ is standard. For notational ease, write:

$$c_0 = w - \tau - s \quad (1.2)$$

$$c_e = w - \tau + (1 + r)s \quad (1.3)$$

$$c_u = b + (1 + r)s \quad (1.4)$$

so that the consumption wedge between being employed and unemployed can be defined as $c_e - c_u = w - \tau - b$.

The agent will choose an effort level $e$ and savings level $s$ to maximize their expected utility, taking as given the unemployment policy $(b, \tau)$.

$$U(b, \tau) = \max_{e,s} u(w - \tau - s) + \beta[\pi(e)(u(w - \tau + (1 + r)s) + (1 - \pi(e))u(b + (1 + r)s) - e] \quad (1.5)$$

First order conditions yield:

$$\pi'(e)[u(c_e) - u(c_u)] - 1 = 0 \quad (1.6)$$

$$-u'(c_0) + \beta(1 + r)\{\pi(e)[u'(c_e) - u'(c_u)] + u'(c_u)\} = 0 \quad (1.7)$$

These are familiar looking first order conditions. The search effort condition (Equation 1.6) equates the marginal cost and benefit of searching for a job in the second period. With the UI benefit $b$ present in the problem, the agent faces a moral hazard problem. Having UI benefits available diminishes the marginal benefit to searching for work. The savings condition (Equation 1.7) equates the marginal utility of consumption in the first and the second periods. An agent would want to save in period one for the chance they could be unemployed in period two, but the presence of UI benefits in period two smooths the consumption in period two between the employed and unemployed states. This reduces the agent's desire to save in period one. Generally speaking, these represent standard intratemporal and intertemporal constraints.

However, agents will behave according to their *perceived* job-finding probability. Define the perceived job-finding probability as $\hat{\pi}(e) \in [0, 1]$, which is also assumed increasing and concave in search effort $e$. There are no other restrictions given here on how the true and perceived job-finding probabilities are related. I define the difference in levels $\hat{\pi}(e) - \pi(e)$ as the baseline bias and the difference in margins $\hat{\pi}'(e) - \pi'(e)$ as the control bias.

**Definition 1** An agent is baseline optimistic (pessimistic) if $\hat{\pi}(e) \geq (\leq)\pi(e)$ for all $e \geq 0$.

**Definition 2** An agent is control optimistic (pessimistic) if $\hat{\pi}'(e) \geq (\leq)\pi'(e)$ for all $e \geq 0$

Note here that the idea of baseline bias is what was measured and observed in Price et al. (2004). A baseline optimistic worker will have a perceived job-finding probability that is greater than their true job-finding probability at every level of search effort. Control bias, however, has not previously been estimated. This paper is the first to provide an estimate of the degree of control bias that prevails among unemployed workers. Control optimistic workers believe that each extra hour of search effort increases their job-finding probability by more than it does in reality.

As a result of this bias, agents will choose an effort level $e$ and savings level $s$ to maximize their *perceived* expected utility taking the policy $(b, \tau)$ as given.

$$\hat{U}(b, \tau) = \max_{e,s} u(w - \tau - s) + \beta[\hat{\pi}(e)(u(w - \tau + (1+r)s) + (1 - \hat{\pi}(e))u(b + (1+r)s) - e] \tag{1.8}$$

First order conditions yield:

$$\hat{\pi}'(e)[u(c_e) - u(c_u)] - 1 = 0 \tag{1.9}$$

$$-u'(c_0) + \beta(1+r)\{\hat{\pi}(e)[u'(c_e) - u'(c_u)] + u'(c_u)\} = 0 \tag{1.10}$$

These are nearly identical to the set of conditions derived when agents are not biased, as differences only occur in the parts of the equation where job-finding probabilities appear. Moving forward, variables associated with the agent's maximization of their perceived expected utility will be denoted with the superscript $b$ and variables associated with the agent's maximization of their true expected utility will be denoted with the superscript $ub$.

The constraints for search behavior, shown in Equations 1.6 and 1.9, will govern the relationship between control bias and an unemployed worker's search effort. For a fixed level of saving, the constraints imply:

$$\pi'(e^{ub}) = \hat{\pi}'(e^b) \tag{1.11}$$

If an agent is control optimistic and the assumptions that both the true and perceived job finding functions are concave in search effort hold, then this condition will only hold if $e^{ub} < e^b$. This implies that a control optimistic agent will exert more search effort, a result coming from overestimating the marginal benefit of more search effort. Control pessimistic agents would result in $e^b < e^{ub}$, implying less search effort exerted. In theory, this would lead to greater search effort, offsetting the hazard problem created by UI benefits. However, the results rest on the assumption that both of the two types of job finding functions are increasing and concave.

Similarly, the constraints 1.7 and 1.10 will determine the relationship between an unemployed worker's savings and their baseline bias. Holding the level of effort constant in these constraints, they jointly imply:

$$\frac{u'(c_0^{ub})}{\pi(e)[u'(c_e^{ub}) - u'(c_u^{ub})] + u'(c_u^{ub})} = \frac{u'(c_0^b)}{\hat{\pi}(e)[u'(c_e^b) - u'(c_u^b)] + u'(c_u^b)} \tag{1.12}$$

where $c_0^{ub} = w - \tau - s^{ub}$, $c_0^b = w - \tau - s^b$, $c_e^{ub} = w - \tau + (1+r)s^{ub}$, $c_u^{ub} = b + (1+r)s^{ub}$, $c_e^b = w - \tau + (1+r)s^b$, and $c_u^b = b + (1+r)s^b$. If an agent is baseline optimistic, this equation will only hold if $s^b < s^{ub}$. This implies that the baseline optimistic agent will save less for an unemployment spell because they underestimate the value of UI and protect themselves less against unemployment risk through savings.[7] Baseline pessimistic agents will save more than their unbiased counterparts. In the unbiased case, the consumption-smoothing benefit of UI payments already reduced precautionary savings for unemployed workers. Baseline optimistic agents will exacerbate this problem by seeing an even smaller benefit to saving for a possible unemployment spell.

## 3 Data

The Survey of Unemployed Workers in New Jersey (SUWNJ) collected information from a subset of the universe of roughly 360,000 individuals receiving UI benefits in the state of New Jersey as of September 28, 2009. Responses were received from 6,025 of the roughly 64,000 surveyed recipients. Each respondent was interviewed every week for up to 24 weeks from October 2009 to April 2010, resulting in a total of over 39,000 weekly interviews. Initial work on the data done by Krueger and Mueller (2011) confirms that, after applying the appropriate survey weights, the survey captured a representative sample despite the high non-response rate.[8] This is shown in Table 1.1. Mean UI benefits were about $400 per week, which amounts to nearly 55% of the respondent's previous weekly earnings. Demographic, education and earnings statistics for the weighted group of respondents are all nearly the same as observed in the universe.

While much of the survey focuses on emotional well-being, there is a significant amount of data on job-search activity and sufficient data on job seeker perceptions and savings. Most importantly, the survey asks for respondents' perceptions about their job-finding prospects (Table 1.2). Beliefs are reported about the probability that job seekers will find a job within the next four weeks, as well as the number of weeks they estimate it will take them to find a job. On average, respondents predicted they would have a 29% chance to find a job within the next four weeks. This

[7] This result relies on the concavity of the utility function and the assumption that $c_e > c_u$.

[8] See Appendix 1.A for more detail on the survey sample.

Table 1.1: Descriptive Statistics for the Universe and Respondents

| | *Unweighted* | | *Weighted* |
|---|---|---|---|
| | *Universe* | *Respondents* | *Respondents* |
| No. of observations | 362,292 | 6,025 | 6,025 |
| Previous earnings during base year (in dollars) | $35,335 | $48,994 | $37,960 |
| Weekly UI benefit | $387 | $442 | $397 |
| Weeks of UI paid by Sep. 28, 2009 | 30.6 | 40.7 | 27.4 |
| Female | 0.454 | 0.521 | 0.472 |
| Race | | | |
| Percent white | 59.5 | 68.0 | 59.8 |
| Percent black | 20.1 | 15.3 | 20.0 |
| Percent Hispanic | 19.1 | 9.1 | 17.8 |
| Education | | | |
| Percent less than high school | 15.6 | 7.0 | 14.1 |
| Percent high school | 43.3 | 26.0 | 45.1 |
| Percent some college | 22.2 | 26.4 | 22.1 |
| Percent college | 19.0 | 40.7 | 18.7 |

*Notes: The universe is the group of individuals receiving UI benefits in New Jersey as of September 28, 2009. From that universe, a sample of nearly 64,000 individuals were surveyed and only a subset of those who were surveyed submitted responses. When sample weights are applied to account for non-response and selection probability, descriptive statistics for the respondents are similar to those for the universe.*

matches the estimate of 20 weeks of unemployment remaining reported in the survey. Predictably, the reported probability of finding a job within the next four weeks is declining in age and unemployment duration.[9] A follow-up question inquires about the length of time to reemployment if the worker searched an extra 7 hours per week.[10] Respondents generally did not seem to think extra search effort would shorten their unemployment spell. This set of survey questions provides the information needed to measure the perceived job-finding rates of job seekers. A potential problem, as outlined in the literature started by Manski (2004), is the possibility that respondents may not be reporting their actual beliefs in the survey.[11] I assume that subject responses are generally accurate, as suggested by Manski (2004), and leave a more detailed exploration of beliefs to future work.

[9] See Appendix 1.D for more detail.

[10] This length of time is only reported for those that thought more search would help their cause, a small subset of those asked. When a respondent reports that they do not think extra search will shorten their unemployment spell, I assume that the weeks to regain employment for this question are the same as in the previous question.

[11] In particular, the elicitations may not reflect the true beliefs of agents due to an anchoring bias, bunching, laziness, or a lack of knowledge. It may also be the case that the beliefs are being swayed by observable decisions, such as leaving the labor force or acceptance of a job offer. See Appendix 1.A for more on this survey data.

Table 1.2: Descriptive Statistics on Job-Finding Prospects

| | Obs. | Mean | Std. Dev. |
|---|---|---|---|
| ***Baseline beliefs*** | | | |
| Job-finding probability within 4 weeks | 5,827 | 0.293 | 0.260 |
| Weeks to regain employment | 5,628 | 20.2 | 26.4 |
| ***Control beliefs*** | | | |
| Weeks to regain employment with extra search | 5,633 | 19.9 | 26.6 |

*Notes: Questions about respondent beliefs were asked once every four weeks. In the section on control beliefs, less than 20% of respondents reported that extra search effort would reduce their unemployment spell. It is assumed that respondents who reported that extra search effort would not shorten their unemployment spell believe the number of weeks it will take for them to regain employment is the same with and without extra search.*

The data provide a rough measure of how long survey respondents have been unemployed, as given by the number of weeks they have claimed UI benefits, but very little indication of what happens to respondents when they leave the survey. I want an estimate of when respondents regain employment, which I infer from the available data by estimating a discrete-time hazard model. However, the unemployment exit rate I estimate contains not only individuals that find a job during the survey, but also those that leave the sample due to survey attrition or leave UI, with or without a job.[12]

Data on savings and search effort are also available in the survey, with the richest set of information being about search effort (Table 1.3). Respondents were asked to keep a time diary as well as to summarize their job search activities over the past week. The result is a set of information about the time spent searching, the methods used, and the quantity of applications filed.[13] Respondents spent an average of 14 hours per week searching for a job, submitting nearly six applications each week. Most search time was spent doing self-directed work, such as directly contacting employers, going to interviews, or filling out applications.

[12]Individuals leaving UI without jobs are most likely to have exhausted their maximum duration of benefits.

[13]There is also information on the number of job offers received, reservation wages, restrictions that job searchers faced, and training programs attended. I will focus on hours spent searching, but results are generally unchanged when using the number of applications submitted. For the hours measure, there were some extreme values - some reported searching over 100 hours per week. To address this, the hours spent searching are windsorized at the 98% level.

Table 1.3: Descriptive Statistics on Job Search

| | Obs. | Mean | Std. Dev. |
|---|---|---|---|
| **Total hours searched in past 7 days** | 35,183 | 14.0 | 18.0 |
| Hours of self-directed search | 35,569 | 10.1 | 15.9 |
| Hours of aided search | 36,215 | 3.5 | 8.7 |
| Applications submitted in past 7 days | 26,946 | 5.9 | 9.4 |

*Notes: Total hours searched and number of applications submitted are reported as summary values by respondents at the end of every seven day period. This can be split into self-direct search and aided search. Self-directed search includes contacting employers directly, contacting friends or relatives, attending job training, placing or answering ads, going to interviews, sending resumes, filling out applications, or looking at ads. Aided search includes contacting public or private employment agencies, contacting university employment centers or checking a union register.*

Savings are reported in an entry survey answered by respondents during their first survey week, indicating a rough amount of money available in a savings account (Table 1.4). Most respondents indicated that they had some savings. Respondents report how much they have in savings in discrete bins, with the majority of respondents who do have savings reporting they have less than $10,000 remaining. Subsequent surveys ask about the availability and sources of emergency funds, homeownership, mortgage and credit card debt, and other measures of liquidity constraints (whether they've sold assets and their sources of funding for different purchases). While not reported here, the data in subsequent surveys could be used to provide a broader picture of the financial constraints facing unemployed workers.

## 4 Bias Measurement

I estimate unemployment hazard rates using a discrete-time hazard model and controlling for worker characteristics and job market conditions. This provides true job-finding probabilities for each survey respondent. I combine these probabilities with the subjective probabilities and self-reported search effort to characterize the degree of bias among workers. As a preview of results, I find that unemployed workers display a large degree of baseline optimism, over-predicting their job-finding probability by an average of nearly 20 percentage points, but they exhibit, on average, no significant control bias.

Table 1.4: Descriptive Statistics on Savings

| Variable | Observations |
|---|---|
| Do you have savings? | **5,943** |
| *Yes* | 3,614 |
| *No* | 2,329 |
| How much savings do you have? | **3,330** |
| *Less than $10,000* | 1,888 |
| *$10,000 to $24,999* | 483 |
| *$25,000 to $49,999* | 312 |
| *$50,000 to $99,999* | 236 |
| *$100,000 or more* | 411 |

*Notes: Questions about savings were only asked in the initial survey for each respondent. If a respondent reported to have savings available, they were asked a follow-up question inquiring how much they had available in savings.*

### 4.1 Hazard Model

In order to assess whether beliefs are systematically biased, I first estimate the true job-finding probabilities of workers. In the literature, the standard approach is to estimate a hazard rate. In hazard models, the technique is meant to measure the rate at which a 'failure' event will occur. The rate is based upon data that identifies which observations are at risk of failing at a given point in time, and is calculated by taking a ratio of the subset of observations that fail compared to the total number of observations at risk for each possible point in time. For work related to unemployment insurance, the 'failure' event is leaving UI. The hazard rate is interpreted as the probability that an individual will leave the UI program at any possible duration of unemployment.

I design my empirical strategy to overcome two limitations of the data. The first is that the survey is not administrative data, and hence there is less certainty about when people leave the UI rolls than in the estimates of Krueger and Mueller (2011). A more nuanced problem is that my variable of interest is not when a worker leaves unemployment insurance, but when they finally leave unemployment. These need not be the same. In many cases, especially during the SUWNJ sample period, workers ran out of UI benefit payments, and by default left the UI program, before finding a job. My main interest is in the behavior of unemployed workers with biased beliefs about their job-finding probability, not their probability of leaving unemployment insurance. In that sense, I estimate a fundamentally different value. I interpret this unemployment hazard rate as the true job-finding probability.

As a first step, I define the concept of 'failure' for this paper. Using a large subset of survey questions, I identify respondents that have met the requirements to exit UI into full-time employment. Starting with information on job acceptances, I restrict the set of newly employed workers to those that accept jobs offering either high enough wages to surpass their UI benefit payment or jobs offering more than 35 hours of work per week. There are often delays between job acceptance and start date, which are also factored into the date of exit from unemployment. On rare occasions, some unemployed workers report accepting two separate full-time offers. In those cases, I assume the worker has re-entered unemployment for a brief period of time in between their two job offers.[14] After factoring in the 'failure' criteria, 86% of respondents leave the survey without reporting to have found a job. The data are severely censored.

The literature that studies UI exit hazard rates typically uses a Kaplan-Meier nonparametric hazard model. These hazard rates are unconditional, and are not estimated controlling for any observable characteristics. An alternative method is the Cox proportional hazards model. This is a semiparametric model that allows for observable characteristics of the respondents to influence the hazard rates. In the Cox hazard model, I can estimate unemployment exit hazard rates that are conditional on respondent's observable characteristics. However, there are still two main drawbacks to the Cox approach. First, the Cox model cannot account for time-varying covariates. Given the time period that the SUWNJ was collected, it is reasonable to assume that the statewide job market conditions varied between October 2009 and April 2010. In addition, the probability of finding a job may vary with each individual's remaining weeks of UI benefits available.[15] Since the stock of UI benefit eligible weeks falls

[14]Alternatively, assuming that the worker only accepts the offer they self-reported as their 'best' offer yields nearly identical results. This is due to the small subsample of respondents for whom this particular problem applies.

[15]This accounting factors in several extensions of unemployment insurance benefit extensions that occurred prior to or during this sample period. In particular, Emergency Unemployment

as a worker remains unemployed, this covariate cannot be incorporated. Second, in order to use the continuous hazard function, the Cox model assumes that the 'failure' events occur in continuous time. This prevents any ties in 'failure' time. However, exits from unemployment are recorded in terms of weeks in the data. At nearly every unemployment duration, there are multiple respondents that exit unemployment and are tied in 'failure' time. While there are methods to modify the Cox estimation, I opt to use an alternative approach.[16]

I estimate a discrete-time hazard model, as developed in Glennon and Nigro (2005). This method has the distinct advantage of resolving both issues associated with the Cox proportional hazard model. In this model, I can control for time-varying labor market conditions, as well as the evolution of UI eligibility over the duration of unemployment. I organize the data into an event history format, ordering the set of observations for each respondent from their earliest to latest survey. This model can then be estimated using a standard logistic regression. I estimate this model controlling for measures of the respondent's education, marital status, number of children, previous income, severance pay, savings, spousal job status, gender, race/ethnicity, remaining UI eligibility, and generally for time-varying New Jersey labor market conditions.[17] Results of the discrete-time hazard model are reported in Figure 1.1.

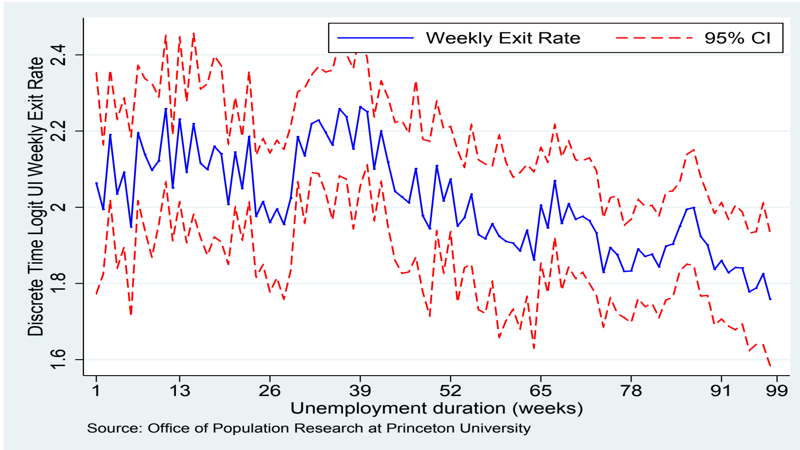
I convert these hazard rates, which I interpret as the true job-finding probability, to make them comparable to the survey. The hazard rates describe the probability the respondent will find a job in the current week, but the survey is concerned with whether a respondent will find a job in the next four weeks. To adjust, I multiply the hazard rates by four. Comparing these probabilities across various respondent characteristics, the job-finding rate is decreasing with respondent age, savings and

Compensation (EUC) was extended in New Jersey in early November 2009, just after the survey had started. This extension prolonged benefits for many survey respondents from a maximum of 72 weeks to 99 weeks. New Jersey also allows for extensions of UI benefits after eligibility has been exhausted through the Additional Benefits during Training (ABT) program. Unemployed workers can receive up to 26 additional weeks of UI benefits if they enroll is an approved training program.

[16] See Appendix 1.C for results from Kaplan-Meier and Cox estimations.

[17] While not executed here, a useful extension would be to account for unobserved heterogeneity among the respondents. There are likely unobservable characteristics specific to each respondent that influences their overall job finding rate, beyond what can be determined using observable characteristics. For an outline of the methodological approach, see Jenkins (2017).

Figure 1.1: Average Discrete-Time Hazard Rates




*Notes: Predicted Logit hazard rates with 95% confidence bands. Hazards are weighted with survey weights for respondents. Standard errors are derived asymptotically and used to calculate 95% confidence bands. Symmetry of confidence bands arises from symmetry of standard errors in linear prediction, which is used to transform the logit standard errors. Discrete-time estimation controls for survey respondent's education, marital status, number of children, previous income, severance pay, savings, spousal job status, gender, race/ethnicity, remaining UI eligibility, and generally for time-varying New Jersey labor market conditions.*

unemployment duration, but is increasing with respondent education.[18]

I estimate the following quadratic regression using ordinary least squares to predict how the true job-finding rate is related to the worker's level of search effort:

$$\pi = \beta_0 + \beta_1 e + \beta_2 e^2 + \varepsilon \tag{1.13}$$

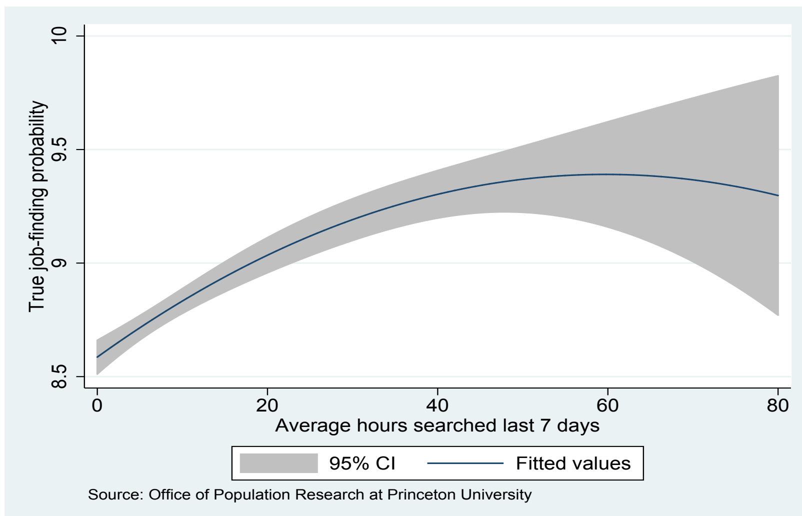
where $\pi$ is the estimated probability that the worker will find a job in the next four weeks and $e$ is the total number of hours spent searching for a job over the previous seven days. Results are listed in Table 1.5. Notably, the results suggest that the true job-finding function is both increasing and concave in search effort. This result confirms the assumption made in the sufficient statistics literature and suggests that the theoretical implication from the section 2 of the paper, that baseline optimistic workers will have less precautionary savings, should hold.

I plot predicted point estimates of this relationship at each level of effort, where effort is the sum of the total hours spent searching for a job over the past seven days.

[18] See Appendix 1.D for more detail.

This is presented in Figure 1.2. The average is roughly 8.5%, meaning respondents have an 8.5% change to be employed in the next four weeks. While not reported in the paper, estimates of the true job-finding probability as a function of different measures of search effort, such as applications submitted within the last seven days, yields qualitatively similar results.[19]

Figure 1.2: True Job-Finding Probability by Search Effort




*Notes: Estimated hazard rates at each level of search effort. Quadratic estimation using ordinary least squares. Standard errors are derived asymptotically and used to calculate 95% confidence bands.*

## 4.2 Perceived Job Finding Function

I construct the perceived job-finding function $\hat{\pi}(e)$ using survey answers about a worker's job-finding probability over the next four weeks.[20] Therefore, the perceived job-finding rate is the reported probability that the worker will find a job in the next four weeks. I estimate the following regression using ordinary least squares to predict

[19]This result passes other robustness checks, as well. Generally, the function remains largely the same if workers who are waiting to start a job are dropped from the sample and workers who claim to have given up searching are dropped from the sample.

[20]This relies upon the assumption that people generally report something close to their true beliefs in the survey. One way to check this would be to use a method to extract true beliefs from subjective survey responses, as outlined in Hendren (2013). This exercise is left for future work, but is outlined in Appendix 1.B.

how the perceived job-finding rate is related to the worker's level of search effort:

$$\hat{\pi} = \beta_0 + \beta_1 e + \beta_2 e^2 + \varepsilon \tag{1.14}$$

where $\hat{\pi}$ is the reported probability that the worker will find a job in the next four weeks and $e$ is the total number of hours spent search for a job over the previous seven days. Results are listed in Table 1.5. Notably, the results show no evidence that the perceived job-finding function is increasing or concave in search effort. This result suggests that the theoretical implication from the section 2 of the paper, that control optimistic workers will exert more search effort, may not hold.

Table 1.5: Estimated Effect of Job Search on Job-Finding Probabilities

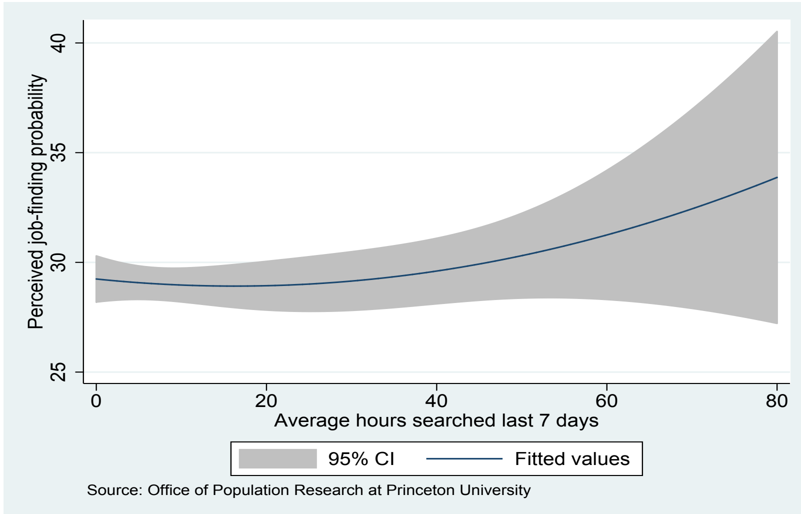
| | $\hat{\pi}(e)$ Perceived Prob. | $\pi(e)$ True Prob. |
|---|---|---|
| Total search hours | -0.040 | 0.027*** |
| | (0.062) | (0.004) |
| (Total search hours)$^2$ | 0.001 | -0.001*** |
| | (0.001) | (0.000) |
| Constant | 29.242*** | 8.586*** |
| | (0.537) | (0.038) |
| | N = 5,529 | N = 13,370 |

*Notes: Regression is ordinary least squares with standard asymptotically derived variance estimator. Standard errors are in parentheses. Coefficients are significant at: 1% (***), 5% (**), 10%(*).*

Again, I plot predicted point estimates of this relationship, where effort is the sum of the total hours spent searching for a job over the past seven days. This is presented in Figure 1.3. The average is roughly 30%, meaning respondents thought that there was a 30% chance that they would be employed in the next four weeks. Robustness checks with different measures of job search effort and sample adjustments also yield qualitatively similar results.[21]

[21] Another robustness check particular to the perceived rate depends upon the survey administration. I can control for the way in which the perceived job-finding probability question is asked on the survey, but this also yields little difference in average perceptions across different search efforts.

Figure 1.3: Perceived Job-Finding Probability by Search Effort




*Notes: Estimated perceived job-finding probabilities at each level of search effort. Quadratic estimation using ordinary least squares. Standard errors are derived asymptotically and used to calculate 95% confidence bands.*

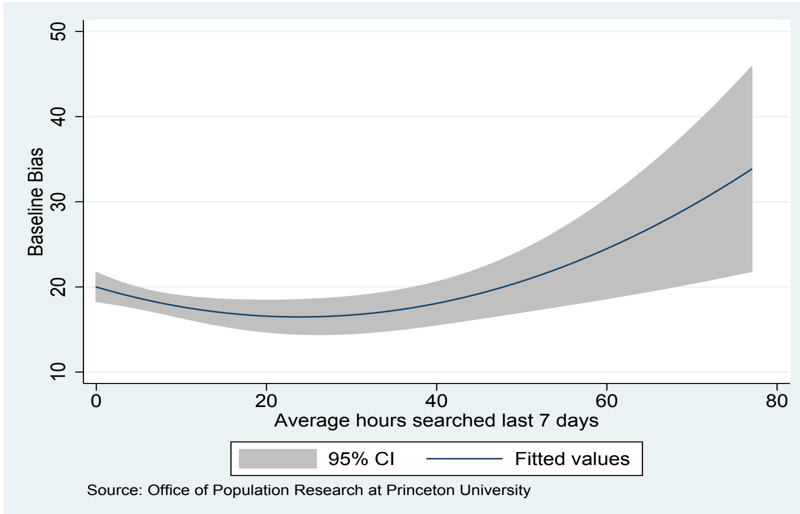
### 4.3 Baseline and Control Bias

I use the perceived job-finding probability estimates $\hat{\pi}(e)$ and the true job-finding probability estimates $\pi(e)$ to calculate the baseline bias $\hat{\pi}(e) - \pi(e)$. I take the difference of the two probabilities for each observation, accounting for the standard errors reported in the discrete-time hazard estimation. Figure 1.4 shows the average baseline bias at different levels of search effort. Given that the perceived job-finding probability is roughly 30% and the true job-finding probability is around 8.5%, there is a consistent optimistic bias over all levels of search effort. This bias falls with age and unemployment duration, suggesting that unemployed workers correct this bias over time.[22] An alternative measurement would be to monotonically transform these probabilities into the remaining unemployment duration.[23] The bias would imply, on average, that it takes 26 more weeks to find a job than expected for unemployed workers. In Price et al. (2004), the authors found that unemployed workers in their sample remained unemployed for 16 weeks longer than expected. However, there

[22] See Appendix 1.D for more detail.

[23] This is likely an upper bound on the number of weeks until employment is found since the hazard rate will decline as the worker remains unemployed for a longer period of time.

Figure 1.4: Baseline Bias by Search Effort




*Notes: Average of baseline bias at each level of search effort. Baseline bias is calculated as the difference between perceived and true job finding probabilities. Standard errors are derived asymptotically and used to calculate 95% confidence bands.*

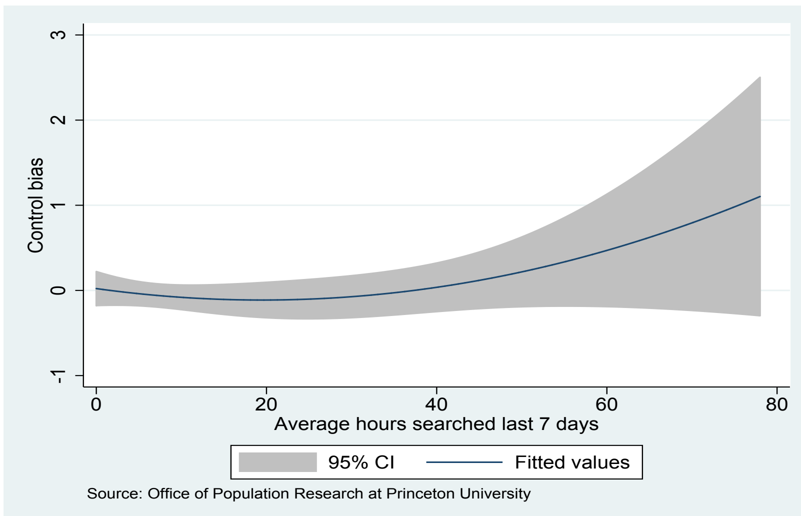
are some clear differences between the two data sets. The SUWNJ sample is 86% censored. This is significantly less than in Price et al. (2004), where roughly 20% of the sample was censored. The two samples were also collected at drastically different time periods. The SUWNJ was collected near the end of the 2009 recession, so the exaggerated bias is likely the result of a shift to lower true job-finding rates. Price et al. was conducted in the late 1990s, concluding well before the recession that began in 2000.

Control bias is more difficult to measure. For the perceived influence, I use the number of weeks that people report they expect it to take to find a job, as well as the number of weeks they would expect it to take to find a job with seven extra hours of search per week.[24] I convert both of these reported numbers of weeks into an implied job-finding probability. These allow me to find an estimate of the change in the perceived job-finding probability when a respondent spends more time searching for a job. For the true influence, I use the estimated job-finding function and compute the difference for each respondent between their job finding probability at their current

[24]Recall, for a large group of the sample, there was no difference between these two values. This suggests that the perceived influence over job finding rates is zero.

level of search effort and an effort level seven hours higher. With those two measures, I can take the difference to get their true level of influence. Then, I compute the control bias by finding $\hat{\pi}'(e) - \pi'(e)$. The average level of control bias at different levels of search effort are reported in Figure 1.5. Respondents generally show no

Figure 1.5: Control Bias in Weeks by Search Effort




*Notes: Average of control bias at each level of search effort. Control bias is calculated as the difference between perceived influence over the job finding probability and true influence over the job finding probability. Standard errors are derived asymptotically and used to calculate 95% confidence bands.*

pattern of control optimism or pessimism over all levels of search effort. This holds true along all respondent characteristics.

## 5 Testing Implications of UI Models

In this section, I formally test predictions of optimal UI models. To briefly preview the results, I find evidence that baseline optimistic workers save less for unemployment spells than baseline pessimistic workers and control optimistic workers spend less time searching for a job than control pessimistic workers. This suggests that policymakers should account for the presence of biased beliefs in estimating the effects of UI benefits on worker incentives.

The model of optimal unemployment insurance with biased beliefs implies that a control optimistic unemployed worker will overestimate the return to extra job search. This overestimation will, when perceived and true job-finding probabilities are increasing and concave in search effort, increase job search, offsetting the moral hazard problem created by the presence of UI benefits. However, during the data analysis, I discovered that the perceived job-finding function was not concave in search effort. Since there is not a clear shape to the perceived job-finding function, there is not a clear prediction for the regression.

I run an ordinary least squares regression to evaluate the relationship between search effort and control bias.

$$effort_i = \beta_0 + \beta_1 \mathbb{I}(CO_i) + X_i'\beta + \varepsilon_i \tag{1.15}$$

The regression controls for respondent characteristics and the survey date in the vector $X_i$. The primary coefficient of interest is $\beta_1$, which describes the relative search effort of a control optimist compared to a control pessimist. Results are reported in Table 1.6. The results indicate that a control optimist searches for a job a significantly smaller amount of time each week than a control pessimist. On average, a control optimist spends nearly four fewer hours searching for a job, which is a 26% decline. I interpret this result as suggesting that when control optimists overestimate the return to extra job search, they view it necessary to complete a smaller total number of search hours to achieve the same level of job-finding probability.[25]

In the theoretical model, it is also implied that a baseline optimistic unemployed worker will underestimate the need to smooth consumption when unemployed. This underestimation will lead to lower precautionary savings, exacerbating the lower savings created through the consumption smoothing benefit of UI payments. Slightly constrained by the data available, I run an interval regression. Interval regression is a generalization of the tobit estimation method used to deal with censored data. In this case, the data I have available only reports a bin into which each respondent's savings falls. Defining the upper and lower bounds of each bin, I can run an interval

[25]This is akin to relative substitution and income effects. While the relative reward for search should encourage more search, the so-called income effect will discourage workers from increasing their search effort because they feel their current effort is producing a larger payoff.

Table 1.6: Regression of Job Search and Savings on Bias Measures

| | Total search hours | Savings |
|---|---|---|
| Unemployment duration | 0.012 | 288.27*** |
| | (0.016) | (43.11) |
| $\mathbb{I}$(Control optimism) | -3.645*** | |
| | (0.968) | |
| Baseline bias | -0.007 | |
| | (0.014) | |
| $\mathbb{I}$(Baseline optimism) | | -8,360.49** |
| | | (3,720.74) |
| Control bias | | -257.31* |
| | | (152.84) |
| | N = 2,071 | N = 2,073 |

*Notes: The search effort regression is estimated by ordinary least squares with asymptotically derived standard errors. The search effort regression also controls for sex, education, marital status, number of children, previous income, severance pay, savings, spousal work status, and survey date. The savings regression is estimated by interval regression with asymptotically derived standard errors. The savings regression includes all regressors used in search effort regression except savings, and adds a measure of search effort. Coefficients are significant at: 1% (***), 5% (**), 10%(*).*

regression with the censored savings data as the dependent variable and the measures of bias and other controls as independent variables.

$$savings\ bins_i = \beta_0 + \beta_1 \mathbb{I}(BO_i) + X_i' \beta + \varepsilon_i \tag{1.16}$$

Again, $\beta_1$ is the primary coefficient of interest, which describes the relative savings behavior of a baseline optimist compared to a baseline pessimist. Results for this interval regression are reported in the second column of Table 1.6. The results indicate that a baseline optimist has a significantly smaller amount of savings remaining compared to a baseline pessimist who has been unemployed the same length of time. On average, a baseline optimist has over $8,500 less remaining in savings when they reach the same unemployment duration as a baseline pessimist. This suggests that the theoretical predictions in the model are correct, and that baseline optimists underestimate their need to smooth consumption when unemployed. There are other measures of financial constraint that indicate baseline optimists are generally less well-equipped to endure a long unemployment spell. For example, baseline optimists also have \$4,000 more in credit card debt compared to baseline pessimists at the same unemployment duration.

## 6 Discussion and Conclusion

This empirical exercise provides measurement of two ways that unemployed workers may be biased about their job finding beliefs. In doing so, I have introduced the discrete-time hazard model into the UI literature. Among unemployed New Jersey workers in 2009-2010, I have shown that a significant underestimation of unemployment duration exists and that this is correlated with less savings during unemployment. I have also shown that, while there is not a systematic over- or underestimation of the influence an unemployed worker can have over their job-finding rate through job search, the subset of unemployed workers who do overestimate the value of extra search tend to spend less time searching for a job.

Several avenues for future research present themselves from this work. One potential extension could be gaining an understanding of *why* unemployed workers harbor these unrealistic expectations. The behavior of biased unemployed workers, in which they seem to put off searching for a job and avoid saving for the possibility of unemployment, appears to be consistent with present bias. Introduced in O'Donoghue and Rabin (1999), unemployed workers with present bias would more heavily weight the payoffs closer to present time. This would include a desire to procrastinate on 'immediate-cost' activities, such as applying to or searching for a job. A similar result has been found by DellaVigna and Paserman (2005) in a paper studying impatient job searchers. New theoretical models that replace the forms of baseline and control bias seen here with an alternative form of discounting could yield a complementary approach to explain the empirical results in this paper.[26]

Optimism among job searchers, which is characterized by less saving and less job search, seems to suggest that unemployed workers could improve their welfare if UI benefits were permanently extended. As unemployment spells endure for longer than job searchers expect, the spell is prolonged by a lack of search effort and potentially

[26] One such approach is using hyperbolic discounting, as in Paserman (2008).

characterized by lower consumption as searchers exhaust their savings. However, recent research by Ganong and Noel (2017) utilizes spending data to show that the path of consumption during unemployment is fairly flat. Even when UI benefit eligibility ends, the drop in consumption is just over 10 percentage points. Ganong and Noel calibrate Spinnewijn's model with overly optimistic job searchers and show that the model would predict a much larger decline in consumption. Even though optimism among job searchers leads to less savings available during unemployment, it appears that spending is maintained through the use of other available assets. Given this result, and other recent work studying UI benefit extensions that have found the provision of benefits for a longer period of time to further reduce job search effort (Hagedorn et al. (2016)), permanent extensions to UI benefits seem to be an ill-advised solution.

A more promising approach may be an information program aimed at making unemployed workers aware of their optimistic bias. This type of intervention could encourage unemployed workers to put forth more search effort during their current unemployment spell and to start establishing better savings habits to prepare for the possibility of a future unemployment spell. Information interventions with institutional support have been shown to have significant impacts on worker behavior. For example, in a new paper by Barr and Turner (2017), the authors disseminated information about the benefits and costs of post-secondary programs that could improve future employment outcomes. The authors found that unemployed workers who receive this information were 40% more likely to enroll in a post-secondary program. Such an information policy about the behavior of optimistic job searchers could help shorten unemployment spells and further smooth consumption of unemployed workers.

# APPENDIX

## 1.A SUWNJ Data

Krueger and Mueller (2011) started with the complete list of 362,292 UI recipients as of September 28, 2009. They ended up obtaining a sample of 63,813 UI recipients which were broken down into 18 strata. Long-term unemployed workers were intentionally oversampled.

Table 1.7: Sample by Number of Weeks of UI Benefits Paid on Current UI Claims as of September 2009

| | With e-mail address | Without e-mail address | Total |
|---|---|---|---|
| zero to 2 weeks | 5,000 | 2,000 | 7,000 |
| 10 to 12 weeks | 5,000 | 2,000 | 7,000 |
| 20 to 22 weeks | 5,000 | 2,000 | 7,000 |
| 30 to 32 weeks | 5,000 | 2,000 | 7,000 |
| 40 to 42 weeks | 5,000 | 2,000 | 7,000 |
| 50 to 53 weeks | 5,000 | 2,000 | 7,000 |
| 60 to 63 weeks | less than 5,000 | 2,000 | less than 7,000 |
| 64 to 71 weeks | nonzero | | nonzero |
| 72 to 77 weeks | less than 10,000 | 4,000 | less than 14,000 |
| more than 77 weeks | nonzero | | nonzero |
| TOTAL | 45,813 | 18,000 | 63,813 |

Entry occurred between October 13, 2009 and November 3, 2009 following invitations (by e-mail or mail) on October 13. Overall, 6,025 workers participated and 5,680 entered within the first 2 weeks (October 13-October 27). Individuals were invited to participate for 12 consecutive weeks, with the weekly interview invitations sent 7 days after the most recent interview. In early January 20, individuals with 60 or more weeks (the last 4 strata) of UI benefits paid at the start of the study were invited for the extended study (additional 12 weeks). Out of 2,022 eligible, 1,148 filled out at least one of those surveys. Response rate (adjusted for mailing errors) was 9.7%, and was higher for the e-mail address group. Respondents completed an average of 4.1 surveys after the initial interview.

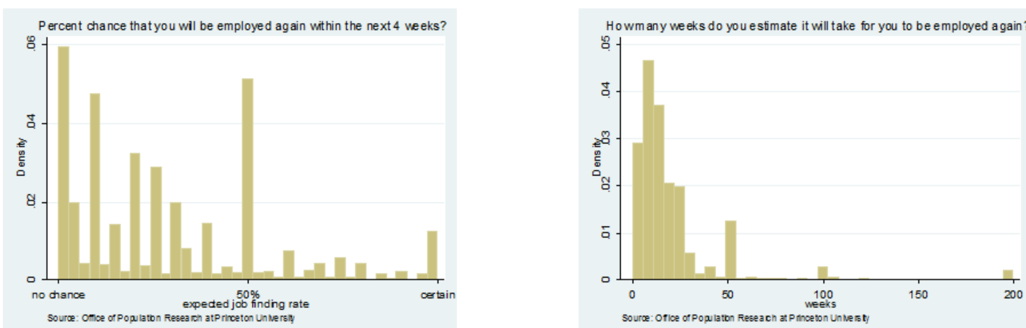
Comparing the sample frame to the respondents shows that the respondents have higher earnings, higher benefit amounts, were more likely female, white, non-Hispanic, and college graduates. Therefore, Krueger and Mueller created survey weights for the initial interview using a logit regression, where the dependent variable is one if the person responded, and zero if the person in the sample frame did not respond. Entry-wave survey weights were computed as the inverse of the predicted value of the logit regression and then rescaled to sum to the total number of individuals in the universe. I use these weights to survey set the data and produce weighted statistics in Table 1.1.

## 1.B Subjective Elicitations

Manski (2004) thinks that people report their true beliefs, but that may not be true in this data. Even if respondents are not be lying, there are several reasons that they may not accurately report their true perceptions. For example, they could be lazy when answering the questions, they could not know how to form a forecast of their perceived job-finding rate, or they could just be bunching near round numbers. It is also possible that they just have some extra information unobservable in the survey data. Looking at the reported job-finding probabilities over the next four weeks and the expected time to find a job, there are some obvious problems. For the job-finding

Figure 1.6: Subjective Elicitations

(a) Perceived Job-Finding Rates (b) Perceived Unemployment Durations




probabilities, there is clear bunching at 0%, 50% and 100%. For the extremes, it could be that people are resigned to leaving the labor force or that they know they'll be accepting a position soon. At the 50% mark, it could be that it is a simply percentage to pick out. There was a period during the survey where the sliding scale for the question started at 50%, so a concern could be that people were framed in their thinking and simply failed to move the slider. However, a quick elimination of those respondents show that does not appear to be the case. Looking at the unemployment durations, the same bunching appears at 50 weeks, 100 weeks and 200 weeks. This appears to be a less severe problem in this question, though.

Hendren (2013) develops a way to use this type of subjective data in evaluating the insurance market, which suffers from an adverse selection problem. To implement this, I would need to introduce a third type of probability - the reported job-finding rate $\bar{\pi}(e)$. This reported rate would be represented by the actual survey responses, which could be interpreted as a noisy measure of the agent's beliefs. In this formulation, the perceived probability $\hat{\pi}(e)$ is private information and needs to be derived from the reported probabilities provided in the survey data.

There is a complication, however, due to the fact that the labor market is different than the insurance market. Hendren addresses the adverse selection problem in the insurance market, which is preventing trade from occurring between high risk people and insurance providers. In the case of unemployment insurance, there is a moral hazard problem that results from private information. The presence of moral hazard does not prevent the trade from occurring, but it prevents some people from leaving UI more quickly. I will need to adjust Hendren for this moral hazard problem, using his same methods for identifying private information.

## 1.C Alternative Hazard Model Estimation

Figure 1.7: Kaplan-Meier Hazard Rates

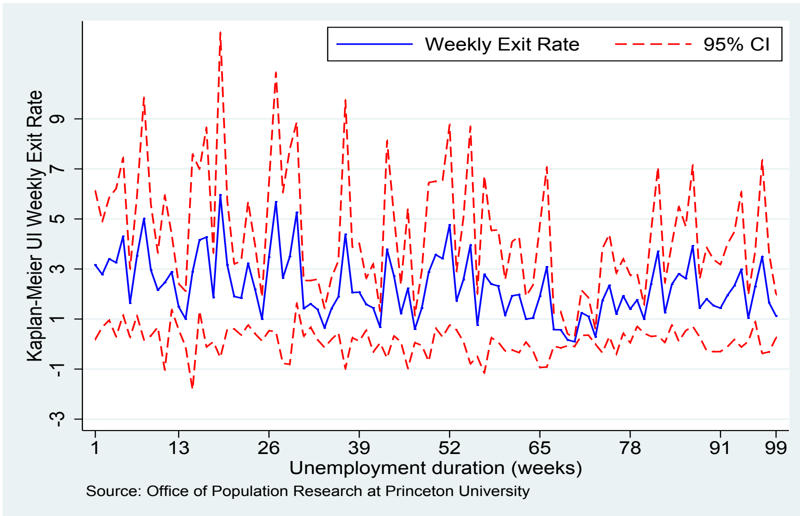
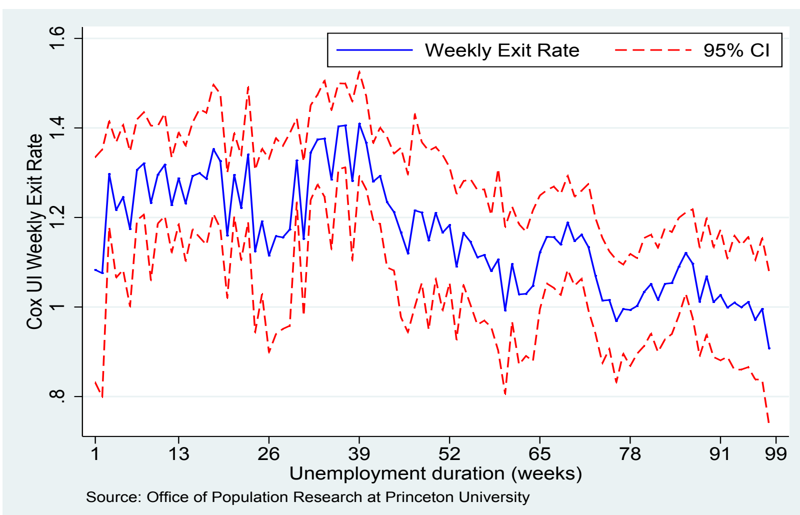



Figure 1.8: Cox Proportional Hazard Rates



## 1.D Job-finding Probabilities and Bias By Characteristic

Table 1.8: Perceived Job-Finding Rate by Characteristic

| | Obs. | Mean | | Obs. | Mean |
|---|---|---|---|---|---|
| **Entire sample** | 5,827 | 29.3 | Marital status | | |
| Age | | | Single | 1,322 | 33.3 |
| 30-34 | 427 | 35.1 | Married | 3,180 | 28.1 |
| 35-39 | 447 | 33.0 | Divorced | 771 | 26.7 |
| 40-44 | 693 | 32.0 | Unemployment duration | | |
| 45-49 | 835 | 29.9 | 10-19 weeks | 366 | 35.3 |
| 50-54 | 1,056 | 28.6 | 20-29 weeks | 462 | 33.8 |
| 55-59 | 1,102 | 26.0 | 30-39 weeks | 449 | 30.3 |
| 60-64 | 773 | 21.1 | 40-49 weeks | 453 | 29.3 |
| Education | | | 50-59 weeks | 393 | 30.5 |
| High school | 821 | 31.2 | 60-69 weeks | 360 | 28.8 |
| Some college | 1,714 | 28.7 | 70-79 weeks | 694 | 26.5 |
| College | 1,837 | 29.5 | 80-89 weeks | 1,306 | 28.1 |
| Graduate school | 914 | 29.1 | 90-99 weeks | 948 | 27.3 |
| 2008 Household income | | | Credit card debt at time of entry survey | | |
| $20,000-$29,999 | 592 | 28.4 | Less than $1,000 | 1,474 | 26.7 |
| $30,000-$39,999 | 538 | 30.4 | $1,000-$2,499 | 677 | 27.6 |
| $40,000-$49,999 | 522 | 27.8 | $2,500-$9,999 | 906 | 29.5 |
| $50,000-$59,999 | 444 | 29.7 | $10,000-$19,999 | 551 | 28.8 |
| $60,000-$69,999 | 372 | 29.6 | $20,000 or more | 544 | 28.2 |
| $70,000-$79,999 | 357 | 26.7 | Savings at time of entry survey | | |
| $80,000-$89,999 | 338 | 28.4 | Less than $10,000 | 1,837 | 30.1 |
| $90,000-$99,999 | 390 | 28.2 | $10,000-$24,999 | 528 | 28.9 |
| $100,000-$149,999 | 903 | 28.9 | $25,000-$49,999 | 361 | 25.8 |
| $150,000+ | 584 | 28.1 | $50,000-$99,999 | 306 | 23.4 |
| Home purchase price | | | $100,000 or more | 558 | 24.9 |
| $100,000-$149,999 | 553 | 27.4 | Sex | | |
| $150,000-$199,999 | 512 | 27.0 | Male | 2,673 | 29.6 |
| $200,000-$299,999 | 729 | 30.7 | Female | 3,154 | 29.1 |
| $300,000-$399,999 | 410 | 27.8 | | | |

Table 1.9: True Job-Finding Rate by Characteristic

| | Obs. | Mean | | Obs. | Mean |
|---|---|---|---|---|---|
| **Entire sample** | 14,084 | 8.8 | Marital status | | |
| Age | | | | | |
| 30-34 | 944 | 10.2 | Married | 13,271 | 8.7 |
| 35-39 | 922 | 10.2 | | | |
| 40-44 | 1,743 | 9.0 | Unemployment duration | | |
| 45-49 | 2,087 | 9.2 | 10-19 weeks | 1,330 | 9.6 |
| 50-54 | 2,732 | 8.5 | 20-29 weeks | 1,558 | 9.2 |
| 55-59 | 3,150 | 8.6 | 30-39 weeks | 1,400 | 9.3 |
| 60-64 | 2,069 | 7.8 | 40-49 weeks | 1,356 | 9.2 |
| Education | | | 50-59 weeks | 1,018 | 8.8 |
| High school | 1,611 | 6.7 | 60-69 weeks | 1,045 | 8.5 |
| Some college | 3,470 | 7.8 | 70-79 weeks | 1,713 | 8.6 |
| College | 4,799 | 8.8 | 80-89 weeks | 2,204 | 8.4 |
| Graduate school | 3,055 | 11.1 | 90-99 weeks | 1,314 | 8.2 |
| 2008 Household income | | | Credit card debt at time of entry survey | | |
| $20,000-$29,999 | 435 | 7.1 | Less than $1,000 | 4,537 | 8.7 |
| $30,000-$39,999 | 669 | 7.4 | $1,000-$2,499 | 2,191 | 8.7 |
| $40,000-$49,999 | 793 | 8.1 | $2,500-$9,999 | 2,461 | 9.0 |
| $50,000-$59,999 | 963 | 8.1 | $10,000-$19,999 | 1,496 | 9.4 |
| $60,000-$69,999 | 878 | 8.2 | $20,000 or more | 1,602 | 9.2 |
| $70,000-$79,999 | 983 | 8.8 | Savings at time of entry survey | | |
| $80,000-$89,999 | 1,244 | 9.2 | Less than $10,000 | 6,129 | 9.8 |
| $90,000-$99,999 | 1,453 | 9.7 | $10,000-$24,999 | 2,312 | 9.4 |
| $100,000-$149,999 | 3,556 | 9.2 | $25,000-$49,999 | 1,661 | 8.6 |
| $150,000+ | 2,892 | 9.2 | $50,000-$99,999 | 1,351 | 7.5 |
| Home purchase price | | | $100,000 or more | 2,631 | 7.0 |
| $100,000-$149,999 | 1,743 | 8.6 | Sex | | |
| $150,000-$199,999 | 1,423 | 8.7 | Male | 7,885 | 9.2 |
| $200,000-$299,999 | 2,457 | 9.4 | Female | 6,199 | 8.3 |
| $300,000-$399,999 | 1,610 | 9.1 | | | |

Table 1.10: Baseline Bias by Characteristic

| | Obs. | Mean | | Obs. | Mean |
|---|---|---|---|---|---|
| **Entire sample** | 2,196 | 18.6 | Marital status | | |
| Age | | | | | |
| 30-34 | 134 | 23.7 | Married | 2,077 | 18.8 |
| 35-39 | 115 | 23.8 | | | |
| 40-44 | 280 | 19.5 | Unemployment duration | | |
| 45-49 | 330 | 23.3 | 10-19 weeks | 145 | 25.4 |
| 50-54 | 420 | 18.4 | 20-29 weeks | 207 | 24.4 |
| 55-59 | 507 | 15.7 | 30-39 weeks | 204 | 21.1 |
| 60-64 | 362 | 13.9 | 40-49 weeks | 202 | 18.1 |
| Education | | | 50-59 weeks | 137 | 18.8 |
| High school | 227 | 17.7 | 60-69 weeks | 123 | 19.0 |
| Some college | 564 | 18.3 | 70-79 weeks | 254 | 15.8 |
| College | 776 | 20.0 | 80-89 weeks | 489 | 16.4 |
| Graduate school | 459 | 16.7 | 90-99 weeks | 311 | 15.4 |
| 2008 Household income | | | Credit card debt at time of entry survey | | |
| $20,000-$29,999 | 70 | 17.9 | Less than $1,000 | 720 | 17.4 |
| $30,000-$39,999 | 108 | 17.5 | $1,000-$2,499 | 346 | 17.1 |
| $40,000-$49,999 | 142 | 20.6 | $2,500-$9,999 | 387 | 20.2 |
| $50,000-$59,999 | 165 | 19.0 | $10,000-$19,999 | 248 | 20.2 |
| $60,000-$69,999 | 135 | 14.3 | $20,000 or more | 242 | 17.8 |
| $70,000-$79,999 | 149 | 15.2 | Savings at time of entry survey | | |
| $80,000-$89,999 | 180 | 16.1 | Less than $10,000 | 955 | 19.1 |
| $90,000-$99,999 | 230 | 18.9 | $10,000-$24,999 | 347 | 20.5 |
| $100,000-$149,999 | 560 | 20.5 | $25,000-$49,999 | 262 | 19.0 |
| $150,000+ | 430 | 18.9 | $50,000-$99,999 | 203 | 13.9 |
| Home purchase price | | | $100,000 or more | 429 | 18.0 |
| $100,000-$149,999 | 270 | 19.6 | Sex | | |
| $150,000-$199,999 | 235 | 19.5 | Male | 1,233 | 18.7 |
| $200,000-$299,999 | 376 | 20.0 | Female | 963 | 18.6 |
| $300,000-$399,999 | 263 | 15.7 | | | |

# CHAPTER 2

# DO LATE-CAREER WAGES BOOST SOCIAL SECURITY MORE FOR WOMEN THAN MEN? (WITH MATTHEW S. RUTLEDGE)

Delaying claiming Social Security benefits as long as possible - from age 62 to 70 - increases benefits by 76 percent for workers born in 1943-1954. This feature is due to the actuarial adjustment, which aims to ensure that the expected present value of lifetime benefits for workers with average mortality varies little by claiming age[1]. But monthly benefits can increase even more if late-career earnings displace zero- or lower-earning years in their careers, thereby raising the average career earnings used to calculate benefits. Women, in particular, stand to gain from longer careers, as late-career earnings are more likely to replace years lost to childrearing and child care.

This study uses *Health and Retirement Study (HRS)* data linked to Social Security earnings records to quantify the extent to which late-career earnings increase workers' benefits, focusing in particular on how women boost their benefits relative to men.

The results indicate that the total gain in Social Security income from delaying claiming all the way to age 70 is 85 percent for the full sample - 76 percent of this gain is from the actuarial adjustment alone (for individuals born in 1943 or later) and 9 percent from the increase in career average earnings. The portion attributable to the increase in the career average earnings is substantial, because the vast majority of individuals have late-career earnings that surpass their earnings earlier in their careers. Women in particular have an opportunity to increase their benefits, because

[1] In part because the actuarial adjustment was based on mortality rates from the early 1960s, delaying claiming - at least past the Early Entitlement Age of 62 - increases the present discounted value of lifetime Social Security benefits for almost all groups (at least at current interest rates), despite well-known differences in life expectancy by socioeconomic status (Shoven and Slavov (2014); Sanzenbacher and Ramos-Mercado (2016)).

nearly one-half of women have at least one year with no earnings among their top 35 years. Women's Social Security benefits rise by 88 percent from delaying retirement until age 70 (for all cohorts combined), compared with 82 percent for men. Even delaying retirement by any one year (on average across ages) increases benefits by 8.6 percent for women, of which 1.6 percent is from replacing low-earning years. These gains in monthly benefits are consistent among women, regardless of marital status, education, and selection into late-career labor market participation.

This paper is structured as follows. The next section explains how late-career earnings factor into the calculation of Social Security benefits and reviews the literature on the extent to which working at older ages increases benefits. A description of the data and an outline of the empirical methodology follows. The subsequent section presents the results, and the final section concludes that working longer helps older individuals - especially women - substantially increase their Social Security income, not just by delaying when they claim but also because late-career work supersedes earlier, low-earning years.

## 1 Background on Social Security Benefits

Social Security retirement benefits are available to individuals who have spent a sufficiently long time contributing payroll taxes into the Social Security system. Workers are entitled to retirement benefits if they have accumulated 40 quarters of coverage - where quarters of coverage accrue for each $1,260 in earnings (in 2016 dollars), up to four per year - and have reached at least age 62.

The value of retirement benefits is based on workers' Average Indexed Monthly Earnings (AIME), which is the average of their highest 35 years of inflation-adjusted wage-indexed earnings (divided by 12). The calculation includes zeros for workers with fewer than 35 years of earnings. Workers with gaps in their careers, therefore, stand to gain substantially from further years of work, as replacing zeros with even fairly small full-time or full-year wages will greatly raise their AIMEs. Even workers whose employment records do not have full years of zero earnings can increase their AIMEs if they have low-earning years because they experienced long spells of nonemployment, earned low hourly wages, or worked few hours per week.

Calculating the actual Social Security benefit requires two more steps. One is converting the AIME to a Primary Insurance Amount (PIA), based on a progressive benefit formula that allows low earners to keep a greater share of their AIME. If late-career earnings increase the AIME by one dollar, the PIA increases by 90 cents for workers with very low career earnings, but only by 15 cents for higher earners. Therefore, the PIA formula reduces the potential return to working longer for workers with higher career earnings.

The other step is the actuarial adjustment, which results in benefits that are less than the PIA when workers claim their benefits before their Full Retirement Age (FRA) and benefits that exceed the PIA when workers claim after the FRA. The amount of the increase from delaying claiming by one extra year varies across birth cohorts because of an increase over time in the FRA - which necessitates different adjustments for early claiming - and because of the gradual actuarial increase in the delayed retirement credit received by those who wait past their FRA. For our sample of individuals born in 1931-1950, the gain from waiting an extra year - without any increase in the PIA - is as small as 4.2 percent and as large as 8.3 percent (Appendix Table 2.6)[2].

Delayed retirement, therefore, has the potential to increase Social Security retirement benefits in two ways. First, claiming later increases Social Security benefits because of the actuarial adjustment. Second, if the worker can earn more than his $35^{th}$-best year to date - and especially if his $35^{th}$-best year had no earnings at all - his AIME will increase, which, in turn increases his PIA and his retirement benefit.

[2]The minimum increase of 4.2 percent is for individuals born in 1931-1932 who postpone claiming from their $69^{th}$ birthdays (at which time they receive 120 percent of their PIA) to their $70^{th}$ birthdays (125 percent of their PIA; (125/120 - 1)*100 = 4.2 percent). The maximum increase is for individuals born in 1937 or earlier who postpone claiming from 62 (80 percent of their PIA) to 63 (86.7 percent of their PIA).

## 2 Literature Review

Despite the obvious potential for increased Social Security benefits from additional years of work, little is known about the impact of late-career earnings replacing the zero- or low-earning years in a workers' career. Most previous studies examining the returns to late-career employment limit their analysis to stylized households with consistent histories of earning near the average wage[3]. One example is Butrica et al. (2008), who examine the potential gain for higher- and lower-earning households. They characterize the potential gain from lower-income workers extending claiming from 62 to 67 as modest but not insubstantial. But none of these studies use actual earnings records that would account for the gaps that individuals often have in their earnings records when they are out of work or experience periods of low earnings. Furthermore, these studies generally do not decompose the gain in retirement benefits into its two components: the actuarial adjustment and the increase in their career average earnings (via a higher PIA).

To our knowledge, the only paper that uses actual workers' earnings records to examine the gains from working an extra year - Reznick et al. (2009) - focuses on Social Security's implicit rate of return. While their analysis reflects the net benefit of working and paying payroll taxes for an additional year, their focus on the marginal rate of return measure does not decompose the gain from working longer into the actuarial adjustment and the replacement of low-earning years. Indeed, their analysis does not address the simple but relevant question of how many years of such earnings are replaced by continued work - and how women, in particular, benefit from delayed retirement[4].

Our paper focuses on the potential gain in retirement income - relative to pre-retirement earnings - for women with gaps in their earnings histories. In contract

[3]See Butrica et al. (2004); Coile et al. (2002); Gokhale et al. (2002); and Koszegi and Rabin (2007).

[4]Reznick et al. (2009) find that most women ages 62-65 in 2005 gain nothing from an extra year of work because they are likely to receive the spousal benefit, or just a little more than the spousal benefit, regardless of their earnings in that year. But they also find that the gains for women grow considerably in later cohorts where spousal benefits are less prevalent.

to the marginal internal rate of return on an additional year's contributions, which measures the relationship between additional taxes and additional benefits, the dollar value of the benefit used here provides a direct measure of the effect of delay on women's well-being in the short run. The paper will present, to our knowledge, the first decomposition of the increase in Social Security income into the actuarial adjustment and the replacement of low-earning years using actual women's earnings histories.

The effect of Social Security on women's retirement has changed greatly over the past few decades. As women approach earnings parity with men, they will come to rely less on spousal benefits (Wu et al. (2013)), and the benefit of delayed retirement is likely only to increase. But little is known about how much Social Security income they are currently leaving on the table. The findings of this study will inform assessments of how delaying retirement has already increased benefits for women, and how increases in their retirement ages will further help secure their household's retirement well-being.

## 3 Data and Methodology

This study uses the 1992-2012 HRS linked to U.S. Social Security Administration's Summary Earnings Records, which capture earnings histories (up to the taxable maximum) for most respondents in the HRS through 2013. Having complete earnings histories allows for counterfactual calculations of what the AIME would have been if respondents had stopped working earlier in their careers.

The sample for this analysis consists of HRS respondents born between 1931 and 1950, who reach age at least age 62 by the end of the HRS sample window and who collect benefits on their own earnings records (i.e., no spousal benefits). Much of the analysis is presented separately by gender. We also present separate analyses for subsamples of women grouped by marital status and education. The marital status analysis splits the sample between 1) women who have been married at least once but never divorced (continually married); and 2) women who have been divorced (even if they subsequently remarried)[5]. Continually married women are more likely to have gaps in their earnings records, because they took time off to raise children or take care of elders, or because of preferences for a single-earner household. The educational analysis splits the sample between women with a high school degree or less and women with some college experience or more.

The aim of this project is to quantify the degree to which women who work longer increase their Social Security benefits, relative to the gains for men. As part of this analysis, we report the proportion of individuals who increased their PIAs by earning more during any year of post-62 employment than their previous $35^{th}$-best year. The analysis also quantifies the proportion of workers who replace zero-earning years with earnings after age 62.

The primary outcome of interest is the increase in Social Security benefits at each age between 63 and the last year of positive earnings, based on actual earnings that year, and how that increase decomposes into PIA increases and gains from the actuarial adjustment for delayed claiming.

One concern arises from self-selection into late-career earnings. It is reasonable to presume that the respondents who continue working do so because they have higher earnings potential. If this is true, it would create an upward bias on the gains from late-career earnings. As a robustness exercise, we impute one extra year of earnings for recently retired non-workers in order to estimate potential benefit increases for a broader population.

In general, finding an adequate instrument to account for selection in a wage regression is difficult. Appendix 2.B outlines one approach to account for selection that exploits how the Bush tax cuts of the early 2000s had varying impacts by marital status. We find this method to be sensitive to the earnings data and time frame used. However, in scenarios where the tax laws serve as a powerful selection instrument, predicted earnings with and without the selection adjustment are nearly identical. We ultimately proceed with earnings predicted using a modified Mincerian earnings

[5] Widows are included in both groups, based on their marital histories.

equation (Mincer (1958)) that includes controls for labor income taxes[6].

A summary of the actual and imputed earnings data are reported in Table 2.1. It shows that predicted earnings for workers of all ages are nearly identical to actual earnings. Men's earnings are slightly underestimated, while women's earnings are slightly overestimated. As expected, non-workers have lower predicted earnings. The subsample of late-career workers ages 62-70 show predicted earnings that are slightly less accurate[7].

Table 2.1: Average Actual and Imputed Labor Earnings

| | All | Men | Women |
|---|---|---|---|
| *Actual earnings* | | | |
| Workers - all ages | \$26,206.71 | \$31,320.80 | \$21,726.71 |
| Workers - ages 62-70 | \$23,921.41 | \$29,033.59 | \$18,849.04 |
| *Predicted earnings - all ages* | | | |
| Workers | \$26,332.27 | \$29,994.34 | \$22,010.82 |
| Non-workers | \$17,610.93 | \$20,482.58 | \$13,993.82 |
| *Predicted earnings - ages 62-70* | | | |
| Workers | \$22,770.68 | \$26,405.61 | \$17,336.62 |
| Non-workers | \$17,106.10 | \$20,046.49 | \$13,317.72 |

*Source: Health and Retirement Study*, 1992-2012 linked to SSA Respondent Cross-Year Summary Earnings File. Tax liabilities calculated using TAXSIM9.

Social Security benefits are calculated at each age between 62 and 70 for every individual. For each year after one's $62^{nd}$ birthday in which an individual had not yet claimed Social Security benefits and had either positive actual or imputed earnings, we calculate the gain in Social Security benefits from working that additional year[8]. To determine how much of the increase in benefits is due to the extra year of earnings, we also calculate the gain in the PIA from the additional year[9]. The remaining

[6] The Mincerian regression derives from the selection model presented in Appendix 2.B. Independently, the literature on earnings functions has justified including labor income taxes as a control by arguing that progressive taxes reduce the return to working. See, for example, Heckman et al. (2008)

[7] We have attempted to correct these earnings, applying the smearing estimator developed in Duan (1983). However, the estimator may not be appropriate in this situation where we have predicted a change in logs rather than just a fitted log value.

[8] Benefits are calculated using the last full year of earnings before each person's birthday. For example, we calculate the benefits for an individual turning 63 in 2005 using earnings data ending in 2004.

[9] When the PIA increases, the gain itself also rises because of the actuarial adjustment, and our

difference in Social Security benefits from one year to the next is attributed to the actuarial adjustment[10]. The gain in benefits is reported separately for each age, and as an unweighted average for all workers at all ages; the average is calculated only for workers with gains in their Social Security benefits and their PIAs.

We then construct the gain in Social Security benefits from delaying both claiming and retirement from age 62 all the way to one's $70^{th}$ birthday. We assume that all workers face the actuarial adjustment of the cohort born in 1943-1950, which gains 76 percent for delaying claiming until age 70. The portion of the gain in benefits attributable to the PIA assumes a worker would get the average gain in the PIA observed among *all* people working at each age[11]. The total increase in the benefit is the increase in the actuarial adjustment plus the increase from late-career earnings.

## 4 Results

Table 2.2 reports the proportion of people working past age 62 whose earnings increase their PIA, and the proportion whose earnings replace a zero-earnings year from earlier in their career. The left panel indicates that, overall, about 91 percent of age-62-plus workers increase their PIAs - that is, their recent earnings are more than the $35^{th}$-highest year already on their record. Adding in the set of non-workers with imputed earnings leads to a steady decline in the fraction of workers increasing their PIAs,

study attributes this compounded gain to the benefit of working an extra year. That is, the PIA at age 63 is:

$$PIA63 = PIA62(1+g)(1+a) = PIA62(1+g+a+ga)$$

where $PIA62$ is the PIA calculated using only earnings before age 62; $g$ is the percent gain in the PIA from working at age 62; and $a$ is the percent gain due to the actuarial adjustment. The calculated gain in the PIA from working an extra year is $(g + ga)$. We attribute the interaction term $ga$ to the gain from working because the PIA would not have increased by $ga$ if the individual did not work (that is, if $g = 0$).

[10] The difference in PIA is taken before the PIA receives a Cost of Living Adjustment (COLA). We do this because our goal is to capture the influence of an extra year of earnings rather than the increase in PIA resulting from an inflation adjustment. The increase in Social Security benefit attributed to the actuarial adjustment includes this COLA adjustment.

[11] An alternative approach would use the average observed increase for workers who actually delay until age 70, but only 47 workers in our sample do so. Instead, we use the average increase in PIA among individuals of each age; the sample decreases with age, but remains substantial into the late 60s. This assumption likely overstates the potential gain given that it is calculated from workers who opted to delay retirement; these workers likely have greater earnings than individuals who opted to retire earlier.

which matches the intuition that these individuals had lower earnings potential and smaller gains from late-career earnings. The share of older workers who replaced a zero in their earnings record falls with age, as expected, but is quite high: about 30 percent of all workers increased their benefit at age 62 by replacing a zero-earning year. This pattern persists when we incorporate imputed earnings for non-workers.

Table 2.2 also indicates that while the vast majority of both men and women are able to increase their PIAs by working past age 62, only women have a substantial amount of zeros to replace. Prior to age 63, only 15 percent of men still have a zero-earning year among their top-35 years, but nearly one-half of all women do, and slightly more women do if they have a high school degree or less. Women who have ever divorced their spouses are slightly less likely to replace a zero-earning year, perhaps because they have had more consistent work histories than women who have been married for most of their working years[12]. Considering the imputed earnings across gender, we find that the share of men increasing their PIAs falls as they increase in age. This supports the idea that men have more complete and higher earnings records.

Table 2.3 reports the one-year percentage gains in monthly Social Security retirement benefits for people who work past age 62, and decomposes the gains into the portion that derives from the PIA increasing and the portion that is due to the change in the actuarial adjustment. The numbers at the bottom of each panel report the share that is due to PIA increases (the second row divided by the third row). We report the calculations for the full sample and separately by gender and for the two subsamples of women defined by marital status or education.

On average, delaying claiming by any one year - not just from age 62 to 63, but from any age to the next - increases benefits by 7 percent (first column) simply through the actuarial adjustment[13]. Working an extra year raises benefits by another

[12]The high share of workers who can increase their benefits with further work is consistent with a report from the U.S. Social Security Administration (2004), which shows that on average men have 6 years of zero earnings after age 22, and women have 13 years. Our calculations show that women are less likely to have zero-earning years to replace at age 62 in more recent cohorts: 56 percent of women born from 1931-1940 had a zero-earning year in their top 35 at age 62, compared with 39 percent of women born from 1941-1950.

[13]The average increase from the actuarial adjustment, 7 percent, is only slightly larger than the

Table 2.2: Share of Workers Who Increase Their PIAs by Working at Ages 62+

| | Full sample | | Men | | Women | | Ever-divorced women | | Women who did not attend college | |
|---|---|---|---|---|---|---|---|---|---|---|
| Age of worker | Increased | Replaced a zero | Increased | Replaced a zero | Increased | Replaced a zero | Increased | Replaced a zero | Increased | Replaced a zero |
| *Using reported earnings records* | | | | | | | | | | |
| 63 | 91.2% | 29.9% | 87.9% | 15.3% | 94.9% | 46.3% | 93.9% | 39.4% | 95.2% | 50.8% |
| 64 | 91.8 | 26.5 | 89.3 | 13.1 | 94.6 | 41.4 | 92.6 | 34.7 | 95.9 | 46.8 |
| 65 | 92.0 | 24.6 | 89.2 | 12.4 | 95.1 | 38.0 | 92.0 | 31.2 | 96.4 | 43.3 |
| 66 | 91.3 | 22.6 | 88.9 | 10.9 | 93.7 | 34.5 | 89.4 | 26.7 | 94.7 | 40.2 |
| 67 | 91.4 | 21.7 | 89.3 | 9.9 | 93.5 | 33.4 | 88.8 | 31.0 | 94.1 | 38.2 |
| 68 | 92.0 | 20.1 | 90.8 | 9.5 | 93.1 | 30.6 | 84.9 | 27.9 | 94.4 | 34.3 |
| 69 | 92.4 | 17.7 | 91.0 | 8.7 | 93.8 | 26.9 | 86.2 | 22.8 | 95.6 | 30.2 |
| 70 | 92.0 | 16.0 | 91.1 | 7.9 | 92.9 | 24.7 | 82.9 | 22.8 | 94.7 | 26.8 |
| *Using imputed earnings without selection adjustment* | | | | | | | | | | |
| 63 | 90.5% | 29.7% | 86.9% | 15.6% | 94.4% | 45.2% | 93.6% | 39.7% | 94.6% | 49.5% |
| 64 | 90.7 | 26.0 | 87.7 | 13.4 | 93.9 | 39.7 | 92.5 | 34.7 | 94.9 | 44.9 |
| 65 | 90.5 | 24.1 | 87.1 | 12.7 | 94.1 | 35.9 | 91.9 | 31.2 | 95.2 | 40.9 |
| 66 | 88.6 | 22.0 | 85.2 | 11.4 | 91.7 | 31.8 | 89.4 | 26.7 | 92.6 | 37.3 |
| 67 | 88.1 | 21.5 | 84.5 | 10.7 | 91.2 | 30.9 | 88.8 | 31.0 | 91.1 | 35.7 |
| 68 | 87.6 | 20.6 | 84.9 | 10.8 | 90.0 | 28.9 | 84.9 | 27.9 | 91.1 | 32.9 |
| 69 | 88.2 | 18.0 | 84.7 | 10.5 | 91.3 | 24.5 | 86.3 | 23.3 | 93.5 | 27.9 |
| 70 | 87.2 | 16.8 | 84.9 | 9.8 | 89.3 | 23.2 | 82.9 | 22.8 | 91.9 | 25.2 |

*Source: Health and Retirement Study*, 1992-2012 linked to SSA Respondent Cross-Year Summary Earnings File.

Table 2.3: One-Year Gain in Social Security Benefits from Working Ages 62+

| | Earnings records | Imputed earnings |
|---|---|---|
| *Full sample* | | |
| From actuarial adjustment | 7.0% | 7.1% |
| From PIA | 1.2 | 1.3 |
| Total increase | 8.2 | 8.4 |
| Share from PIA growth | 14.2 | 15.5 |
| *Men* | | |
| From actuarial adjustment | 7.0% | 7.2% |
| From PIA | 0.8 | 0.9 |
| Total increase | 7.8 | 8.1 |
| Share from PIA growth | 9.9 | 11.1 |
| *Women* | | |
| From actuarial adjustment | 7.0% | 7.1% |
| From PIA | 1.6 | 1.7 |
| Total increase | 8.6 | 8.8 |
| Share from PIA growth | 18.4 | 19.3 |
| *Women, ever divorced* | | |
| From actuarial adjustment | 7.3% | 7.3% |
| From PIA | 1.6 | 1.6 |
| Total increase | 8.9 | 8.9 |
| Share from PIA growth | 18.3 | 18.0 |
| *Women, continually married* | | |
| From actuarial adjustment | 7.1% | 7.1% |
| From PIA | 1.8 | 1.9 |
| Total increase | 9.0 | 9.0 |
| Share from PIA growth | 20.6 | 21.1 |
| *Women, high school degree or less* | | |
| From actuarial adjustment | 6.8% | 7.1% |
| From PIA | 1.2 | 1.3 |
| Total increase | 8.1 | 8.4 |
| Share from PIA growth | 15.1 | 15.5 |
| *Women, some college or more* | | |
| From actuarial adjustment | 7.1% | 7.2% |
| From PIA | 1.2 | 1.3 |
| Total increase | 8.4 | 8.5 |
| Share from PIA growth | 14.5 | 15.3 |

Note: The actuarial adjustment is the one faced by the 1943-1950 birth cohorts. *Source: Health and Retirement Study*, 1992-2012 linked to SSA Respondent Cross-Year Summary Earnings File.

increase from delaying claiming before the FRA (6.67 percent), because most of the extra years worked were at pre-FRA ages.

Table 2.4: Cumulative Gain in Social Security Benefits from Working Ages 62+

| | Earnings records | Imputed earnings |
|---|---|---|
| *Full sample* | | |
| From actuarial adjustment | 76.0% | 76.0% |
| From PIA | 8.9 | 9.4 |
| Total increase | 84.9 | 85.4 |
| Share from PIA growth | 10.5 | 11.0 |
| *Men* | | |
| From actuarial adjustment | 76.0% | 76.0% |
| From PIA | 5.7 | 6.2 |
| Total increase | 81.7 | 82.2 |
| Share from PIA growth | 6.9 | 7.5 |
| *Women* | | |
| From actuarial adjustment | 76.0% | 76.0% |
| From PIA | 12.4 | 12.6 |
| Total increase | 88.4 | 88.6 |
| Share from PIA growth | 14.0 | 14.2 |
| *Women, ever divorced* | | |
| From actuarial adjustment | 76.0% | 76.0% |
| From PIA | 12.0 | 12.0 |
| Total increase | 88.0 | 88.0 |
| Share from PIA growth | 13.6 | 13.6 |
| *Women, continually married* | | |
| From actuarial adjustment | 76.0% | 76.0% |
| From PIA | 13.9 | 14.0 |
| Total increase | 89.9 | 90.0 |
| Share from PIA growth | 15.5 | 15.6 |
| *Women, high school degree or less* | | |
| From actuarial adjustment | 76.0% | 76.0% |
| From PIA | 12.6 | 12.9 |
| Total increase | 88.6 | 88.9 |
| Share from PIA growth | 14.2 | 14.5 |
| *Women, some college or more* | | |
| From actuarial adjustment | 76.0% | 76.0% |
| From PIA | 11.9 | 12.0 |
| Total increase | 87.9 | 88.0 |
| Share from PIA growth | 13.5 | 13.6 |

Note: The actuarial adjustment is the one faced by the 1943-1950 birth cohorts. *Source: Health and Retirement Study*, 1992-2012 linked to SSA Respondent Cross-Year Summary Earnings File.

1.2 percent, because those years frequently replace low- or zero-earning years from earlier in workers' careers. The total increase in benefits from an extra year of work (on average across ages) is 8.2 percent; about one-seventh of this increase is due to increasing the career average earnings, with the remainder due to the actuarial adjustment.

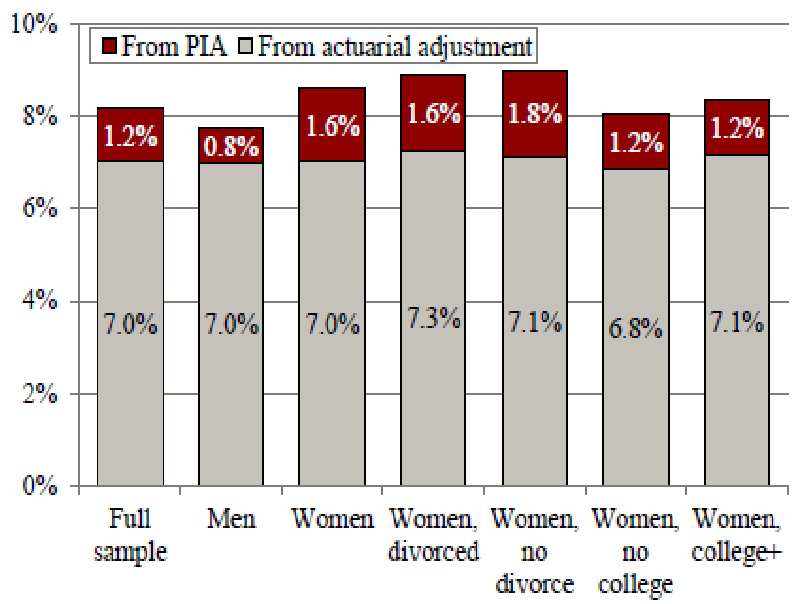
The next two panels in Table 2.3 show that women stand to gain more from raising their PIAs: 1.6 percentage points, compared with 0.8 percentage points for men. This result is sensible given that women have more low-earning years to replace. The total gain to Social Security benefits for women is 8.6 percent, of which about one-fifth derives from the PIA increase; for men, benefits rise by 7.8 percent, of which about one-tenth derives from the PIA increase.

The bottom four panels of Table 2.3 present results separately for women by marital status and education. The actuarial adjustments here differ - not because the actuarial adjustment formulas differ, but because most birth cohorts have different actuarial adjustment rates, and the sub-samples by marital status and education have different shares of each birth cohort. As a result, the actuarial adjustment is slightly larger for women who have ever been divorced than for women who were continually married, probably because the rise in the divorce rate means that more divorced women are in more recent cohorts, where the delayed retirement credit is more generous. Both ever-divorced and continually married women receive substantial increases in their PIAs for one year of late-career earnings: an additional 1.6 percentage points and 1.8 percentage points. The total increase in benefits from delaying retirement by any one year is about 9 percent for both marital groups.

Educational attainment, like divorce, has also increased in later cohorts. Therefore, better-educated women also see a slight advantage in the actuarial adjustment over less-educated women. Late-career earnings increase for both better- and less-educated women by 1.2 percent. But the total increase in benefits from delaying retirement by any one year is 8.1 percent for women with a high school degree or less, and 8.4 percent for women with at least some college experience.

Throughout Table 2.3, there are only negligible differences between outcomes using only actual earnings compared to outcomes incorporating imputed earnings. Gains from the actuarial adjustment rise consistently as workers from more recent birth cohorts are added to the sample.

Figure 2.1: Decomposition of the Increase in Social Security Retirement Benefits from Delaying Retirement by One Year



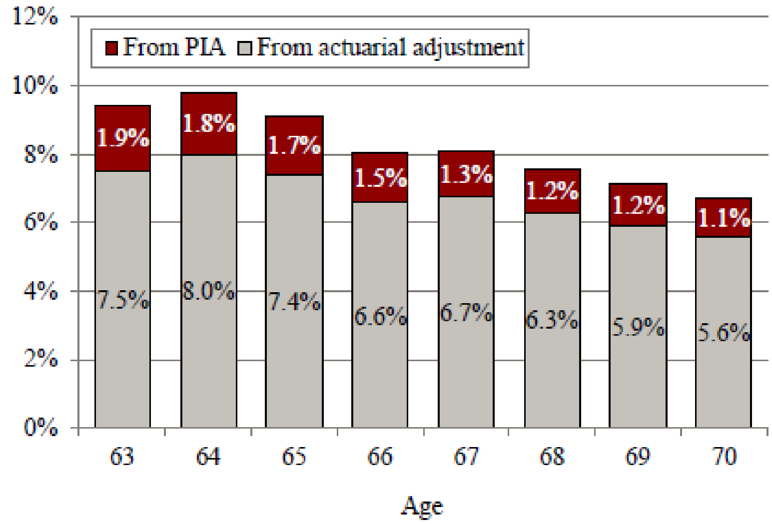

*Source: Health and Retirement Study, 1992-2012 linked to SSA Respondent Cross-Year Summary Earnings File.*

Figure 2.1 displays these results in a graph. The height of each bar is the overall gain in the Social Security benefit from working one more year at some time after age 62. The bottom portion of the bar is the gain attributable to the actuarial adjustment through delayed claiming, and the top portion is attributable to increasing the PIA through delayed retirement. It is clear that the majority of the gain in benefits for each group is from delayed claiming. Men do not substantially increase their career average earnings, and the extra amount attributable to late-career earnings is larger for each group of women.

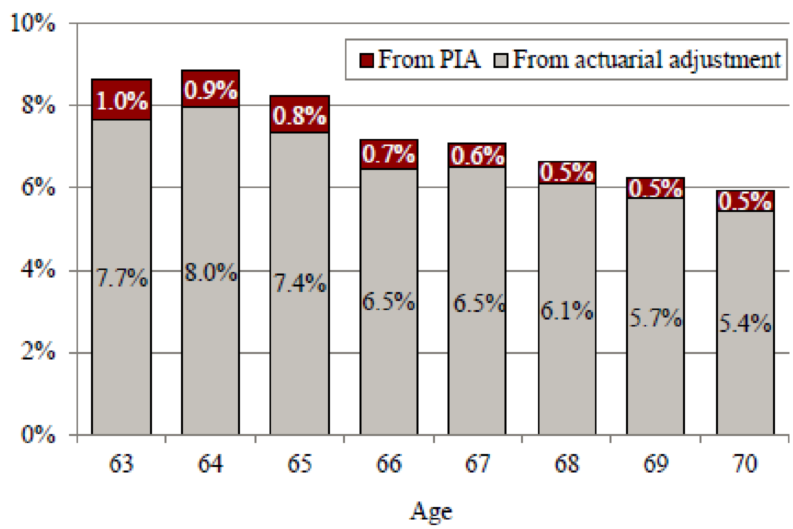
Figure 2.2 examines the increase in retirement benefits at each age for women (Panel A) and men (Panel B) and decomposes this gain into the portions attributable to the actuarial adjustment (bottom) and the PIA increase (top). The boost to Social Security benefits is largest between ages 63-65, at least in part because of the selection

Figure 2.2: Decomposition of the Increase in Social Security Retirement Benefits from Delaying Retirement by One Year, by Age

*a. Women*




*b. Men*




*Source: Health and Retirement Study, 1992-2012 linked to SSA Respondent Cross-Year Summary Earnings File.*

effect: lower earners are more likely to drop out of the labor force closer to age 62, leaving mostly higher earners - who have fewer low-earnings years to replace - working closer to their FRA. After age 65, however, the retirement benefit boost starts to fade; earnings at even older ages are not replacing the lower-earning, early-career years, as evidenced by the shrinking boost coming from changes to the PIA (the top area).

Table 2.5 shows similar patterns when imputed earnings are included.

Table 2.5: Decomposition of the Increase in Social Security Retirement Benefits from Delaying Retirement by One Year, by Age

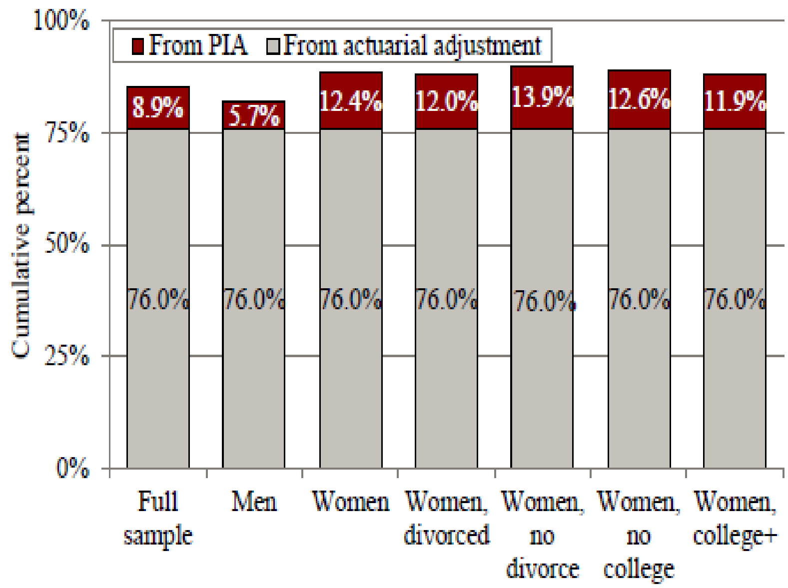
| | Age | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | 63 | 64 | 65 | 66 | 67 | 68 | 69 | 70 |
| *Earnings records* | | | | | | | | |
| **Men** | | | | | | | | |
| From actuarial adj. | 7.7% | 8.0% | 7.4% | 6.5% | 6.5% | 6.1% | 5.7% | 5.4% |
| From PIA | 1.0% | 0.9% | 0.8% | 0.7% | 0.6% | 0.5% | 0.5% | 0.5% |
| **Women** | | | | | | | | |
| From actuarial adj. | 7.5% | 8.0% | 7.4% | 6.6% | 6.7% | 6.3% | 5.9% | 5.6% |
| From PIA | 1.9% | 1.8% | 1.7% | 1.5% | 1.3% | 1.2% | 1.2% | 1.1% |
| *Imputed earnings* | | | | | | | | |
| **Men** | | | | | | | | |
| From actuarial adj. | 7.6% | 8.0% | 7.4% | 6.5% | 6.6% | 6.2% | 5.8% | 5.5% |
| From PIA | 1.0% | 1.0% | 0.9% | 0.8% | 0.6% | 0.6% | 0.6% | 0.6% |
| **Women** | | | | | | | | |
| From actuarial adj. | 7.5% | 8.0% | 7.4% | 6.6% | 6.8% | 6.3% | 5.9% | 5.6% |
| From PIA | 2.0% | 1.8% | 1.8% | 1.5% | 1.3% | 1.2% | 1.2% | 1.1% |

*Source: Health and Retirement Study*, 1992-2012 linked to SSA Respondent Cross-Year Summary Earnings File.

Figure 2.3, and Table 2.4, present the overall gain from delaying claiming and retirement from age 62 all the way to one's $70^{th}$ birthday; the gain is decomposed into the same two portions. The gain from delayed claiming is fixed at 76 percent, the actuarial adjustment for the youngest cohorts in our sample (1943-1950). On top of this increase, late-career earnings push up the PIA, which raises benefits by an additional 8.9 percent, for a total increase of 84.9 percent. Women receive larger increases from raising their PIAs: 12.4 percent, compared with 5.7 percent for men. The extra boost from women's late-career earnings results in a larger overall increase of 88.4 percent, compared with 81.7 percent for men.

As expected, continually married women receive a slightly larger PIA increase (13.9 percent) from delaying retirement to age 70 than divorced women (12 percent); each has an 88-90 percent increase in benefits overall. Less-educated women, also as expected, receive a larger increase from the PIA (12.6 percent) than better-educated women (11.9 percent); the overall gain for both education groups is around 88 percent.

Figure 2.3: Decomposition of the Increase in Social Security Retirement Benefits from Delaying Retirement from Age 62 to 70




*Source: Health and Retirement Study, 1992-2012 linked to SSA Respondent Cross-Year Summary Earnings File.*

## 5 Conclusion

The advantage of delaying one's Social Security retirement benefit claim is well-known: postponing claiming from age 62 until 70 increases monthly benefits by 76 percent even if delayed claimers never work beyond age 62. But claiming and retirement tend to go hand-in-hand, so most older people who do not claim their benefits keep working. Older workers further increase their Social Security benefits by replacing low-earning years from early in their careers, thereby raising their career average earnings based on their top 35 years of earnings.

The results in this study show that the gains in retirement benefits are substantial not just because of the 76 percent bonus for delayed claiming. The overall increase in Social Security benefits of working until 70 is 85 percent among recent cohorts of individuals working after age 62 (and 85 percent across all cohorts), because of the additional 9-percent boost from late-career earnings. Women are able to increase their benefit by a total of 88 percent, because nearly one-half of women have at least one zero-earnings year in their top 35 years of earnings. There are similarly large gains for women who are divorced and continually married, and better- and less-educated. The gains are also appear robust to the selection into late-career labor force participation.

These findings emphasize the effectiveness of delaying one's retirement in shoring up the retirement security of vulnerable workers. Working longer: allows older individuals to postpone drawing down their retirement saving; permits them to save longer or accumulate more pension benefits; makes them more likely to maintain their employer-sponsored health insurance; and may have positive effects on their mental and cognitive health (Meyer and Sullivan (2008)). This study's results suggest that policies aimed at increasing employment at older ages - through reforms to Social Security and Medicare, or through tax credits that reduce the cost of employing older workers - also increase Social Security benefits. That increase arises not only from the delayed retirement credit but also because most workers earn more at the end of their careers.

# APPENDIX

## 2.A Actuarial Adjustments to Social Security Benefits

Table 2.6: Actuarial Adjustments to Social Security Retirement Benefits from Delayed Claiming, by Age and Cohort

| Birth cohort | | Claiming age 62 | 63 | 64 | 65 | 66 | 67 | 68 | 69 | 70 | Implied gain in benefits |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1943+ | Percent of PIA | 75.0 | 80.0 | 86.7 | 93.3 | 100.0 | 108.0 | 116.0 | 124.0 | 132.0 | 76 |
| | Actuarial adj. | | 5.0 | 6.7 | 6.7 | 6.7 | 8.0 | 8.0 | 8.0 | 8.0 | |
| | Y-o-Y increase | | 6.7 | 8.3 | 7.7 | 7.1 | 8.0 | 7.4 | 6.9 | 6.5 | |
| | Cum. increase | | 6.7 | 15.6 | 24.4 | 33.3 | 44.0 | 54.7 | 65.3 | 76.0 | |
| 1942 | Percent of PIA | 75.8 | 81.1 | 87.8 | 94.4 | 101.3 | 108.8 | 116.3 | 123.8 | 131.3 | 73.1 |
| | Actuarial adj. | | 5.3 | 6.7 | 6.7 | 6.8 | 7.5 | 7.5 | 7.5 | 7.5 | |
| | Y-o-Y increase | | 7.0 | 8.2 | 7.6 | 7.2 | 7.4 | 6.9 | 6.5 | 6.1 | |
| | Cum. increase | | 7.0 | 15.8 | 24.5 | 33.5 | 43.4 | 53.3 | 63.2 | 73.1 | |
| 1941 | Percent of PIA | 76.7 | 82.2 | 88.9 | 95.6 | 102.5 | 110.0 | 117.5 | 125.0 | 132.5 | 72.8 |
| | Actuarial adj. | | 5.6 | 6.7 | 6.7 | 6.9 | 7.5 | 7.5 | 7.5 | 7.5 | |
| | Y-o-y increase | | 7.2 | 8.1 | 7.5 | 7.3 | 7.3 | 6.8 | 6.4 | 6.0 | |
| | Cum. increase | | 7.2 | 15.9 | 24.6 | 33.7 | 43.5 | 53.3 | 63.0 | 72.8 | |
| 1940 | Percent of PIA | 77.5 | 83.3 | 90.0 | 96.7 | 103.5 | 110.5 | 117.5 | 124.5 | 131.5 | 69.7 |
| | Actuarial adj. | | 5.8 | 6.7 | 6.7 | 6.8 | 7.0 | 7.0 | 7.0 | 7.0 | |
| | Y-o-Y increase | | 7.5 | 8.0 | 7.4 | 7.1 | 6.8 | 6.3 | 6.0 | 5.6 | |
| | Cum. increase | | 7.5 | 16.1 | 24.7 | 33.5 | 42.6 | 51.6 | 60.6 | 69.7 | |
| 1939 | Percent of PIA | 78.3 | 84.4 | 91.1 | 97.8 | 104.7 | 111.7 | 118.7 | 125.7 | 132.7 | 69.4 |
| | Actuarial adj. | | 6.1 | 6.7 | 6.7 | 6.9 | 7.0 | 7.0 | 7.0 | 7.0 | |
| | Y-o-Y increase | | 7.8 | 7.9 | 7.3 | 7.0 | 6.7 | 6.3 | 5.9 | 5.6 | |
| | Cum. increase | | 7.8 | 16.3 | 24.8 | 33.6 | 42.6 | 51.5 | 60.4 | 69.4 | |
| 1938 | Percent of PIA | 79.2 | 85.6 | 92.2 | 98.9 | 105.4 | 111.9 | 118.4 | 124.9 | 131.4 | 66.0 |
| | Actuarial adj. | | 6.4 | 6.7 | 6.7 | 6.5 | 6.5 | 6.5 | 6.5 | 6.5 | |
| | Y-o-Y increase | | 8.1 | 7.8 | 7.2 | 6.6 | 6.2 | 5.8 | 5.5 | 5.2 | |
| | Cum. increase | | 8.1 | 16.5 | 24.9 | 33.2 | 41.4 | 49.6 | 57.8 | 66.0 | |
| 1937 | Percent of PIA | 80.0 | 86.7 | 93.3 | 100.0 | 106.5 | 113.0 | 119.5 | 126.0 | 132.5 | 65.6 |
| | Actuarial adj. | | 6.7 | 6.7 | 6.7 | 6.5 | 6.5 | 6.5 | 6.5 | 6.5 | |
| | Y-o-Y increase | | 8.3 | 7.7 | 7.1 | 6.5 | 6.1 | 5.8 | 5.4 | 5.2 | |
| | Cum. increase | | 8.3 | 16.7 | 25.0 | 33.1 | 41.3 | 49.4 | 57.5 | 65.6 | |
| 1935-36 | Percent of PIA | 80.0 | 86.7 | 93.3 | 100.0 | 106.0 | 112.0 | 118.0 | 124.0 | 130.0 | 62.5 |
| | Actuarial adj. | | 6.7 | 6.7 | 6.7 | 6.0 | 6.0 | 6.0 | 6.0 | 6.0 | |
| | Y-o-Y increase | | 8.3 | 7.7 | 7.1 | 6.0 | 5.7 | 5.4 | 5.1 | 4.8 | |
| | Cum. increase | | 8.3 | 16.7 | 25.0 | 32.5 | 40.0 | 47.5 | 55.0 | 62.5 | |
| 1933-34 | Percent of PIA | 80.0 | 86.7 | 93.3 | 100.0 | 105.5 | 111.0 | 116.5 | 122.0 | 127.5 | 59.4 |
| | Actuarial adj. | | 6.7 | 6.7 | 6.7 | 5.5 | 5.5 | 5.5 | 5.5 | 5.5 | |
| | Y-o-Y increase | | 8.3 | 7.7 | 7.1 | 5.5 | 5.2 | 5.0 | 4.7 | 4.5 | |
| | Cum. increase | | 8.3 | 16.7 | 25.0 | 31.9 | 38.8 | 45.6 | 52.5 | 59.4 | |
| 1931-32 | Percent of PIA | 80.0 | 86.7 | 93.3 | 100.0 | 105.0 | 110.0 | 115.0 | 120.0 | 125.0 | 56.3 |
| | Actuarial adj. | | 6.7 | 6.7 | 6.7 | 5.0 | 5.0 | 5.0 | 5.0 | 5.0 | |
| | Y-o-Y increase | | 8.3 | 7.7 | 7.1 | 5.0 | 4.8 | 4.5 | 4.3 | 4.2 | |
| | Cum. increase | | 8.3 | 16.7 | 25.0 | 31.3 | 37.5 | 43.8 | 50.0 | 56.3 | |

*Source:* U.S. Social Security Administration.

## 2.B Mincerian Earnings Regression

To get a more complete picture of the Social Security benefit gains from late-career earnings, it is useful to include estimates of the potential gains for late-career non-workers. Calculating the foregone Social Security benefit increases requires forming a prediction of unobserved earnings for non-workers.

Work has been done to impute earnings for non-workers in the labor supply literature. Several cross-sectional studies have simply imputed earnings for non-workers using observable covariates[14]. In these exercises, it is assumed that controlling for observable characteristics renders workers and non-workers identical. Studies predict labor earnings $L$ using a vector of covariates $X$.

$$ln(L_i) = \alpha + X_i\beta + \epsilon_i \tag{2.1}$$

As an extension of this method, other authors have accounted for selection bias by using a Heckman (1979) model. In these cases, an extensive-margin selection equation is estimated in order to derive an inverse Mills ratio to add to equation (2.1). The extensive-margin equation requires an instrument for selection, which is typically the number of preschool-aged children[15]. Alpert and Powell (2012) outline how this instrument poses two problems. First, it may not be exogenous. While the number of preschool-aged children would affect labor force participation, it also could separately affect labor earnings[16]. Second, and perhaps more importantly, this instrument is unlikely to be very strong for the older workers considered in this study.

This paper explores an alternative instrument for selection introduced in Alpert and Powell (2012) and Alpert and Powell (2016). Exploiting changes in tax laws, this method uses the varying impacts of tax laws on married and single workers with

[14]See, for example, Meyer and Rosenbaum (2001), Blau and Kahn (2007), and Gelber and Mitchell (2012).

[15]See, for example, Eissa and Hoynes (2004), Eissa et al. (2008), and Gelber and Mitchell (2012).

[16]Specifically, Alpert and Powell (2012) argue that workers with preschool-aged children might differ from workers without pre-school aged children along unobservable dimensions such as intensive labor supply preferences or productivity.

different levels of initial income[17]. One feature of this method that differs from most of the literature is the use of panel data. In addition to the HRS and Summary Earning Records, this part of the paper utilizes NBER's Taxsim program (Feenberg and Coutts (1993)) to calculate tax rates and tax liabilities.

The underlying theoretical framework in Alpert and Powell (2016) follows from Eissa et al. (2008). Individual workers maximize utility by choosing consumption and labor, subject to a budget constraint. Workers incur a cost from working, and the budget constraint contains labor and non-labor income, as well as a total tax liability.

$$\max_{c,L} U(c, L) - \mathbb{I}(L > 0)q \quad s.t. \quad c = L + y^0 - T[L + y^0] \tag{2.2}$$

where $c$ is consumption, $L$ is labor earnings, $y^0$ is non-labor income, and $y = L + y^0$ is total income. $T[y]$ is total tax liability, which is non-linear in total income, and $q$ is the cost of working. Defining the derivative of $T$ as the marginal tax rate $\tau$, the first-order conditions show that changes in labor income depend upon changes in the marginal net-of-tax rate $(1 - \tau)$ and changes in after-tax total income $y - T[y]$. Therefore, the model imputes changes in labor earnings according to the intensive labor supply equation:

$$\Delta ln(L_{i,t}) = \alpha_t + X'_{it}\delta + \beta^I \Delta ln(1 - \tau_{i,t}) + \theta^I \Delta ln(y_{it} - T_{it}[y_{it}]) + \Delta\epsilon_{i,t} \tag{2.3}$$

where $X$ is a vector of observable characteristics that includes measures of initial income, education, age and marital status. After accounting for after-tax total income, the coefficient $\beta^I$ is interpreted as a compensated elasticity. It should be positive, showing that there is a positive substitution effect and that a higher cost of leisure leads to increased labor earnings through increased work hours.

At the interior solution for the consumer, we can consider their utility from working versus their utility from not working. At the point where this individual is indif-

[17] A more detailed explanation of the variation in tax laws being exploited can be found in Alpert and Powell (2012).

ferent:

$$U(L + y^0 - T[L + y^0], L) - q = U(y^0 - T[y^0], 0) \tag{2.4}$$

Implicitly, the decision to work depends upon the worker's after-tax total income $y - T[y]$, their after-tax non-labor income $y^0 - T[y^0]$, and their labor earnings. Differentiating this indifference expression shows that the worker's labor force participation depends upon changes in the tax schedule. The extensive margin labor force participation decision can be estimated using the regression:

$$\begin{aligned} P(Work_{i,t} = 1) = F(\phi_t + X_{it}'\gamma + \beta^E \Delta ln(y_{it} - T_{it}[y_{it}]) \\ + \theta^E \Delta ln(y_{it}^0 - T[y_{it}^0]) + \rho^E \Delta ln(L_{it}) + \nu_{it}) \end{aligned} \tag{2.5}$$

The coefficient $\beta^E$ is the compensated elasticity on the extensive margin and is expected to be positive. After accounting for after-tax non-labor income and labor earnings, it is capturing the idea that higher after-tax labor earnings should encourage more labor force participation. Higher after-tax non-labor income should discourage work, so $\theta^E$ should be negative[18]. Similarly, after holding constant after-tax total income, $\rho^E$ illustrates the disutility from extra work and should be negative.

There are several identification problems in estimating equations (2.3) and (2.5). Both equations can only be estimated using workers in a given period, since $L_{it}$ is unobserved for non-workers. Also, in equation (2.3), labor earnings determine tax rates and tax liabilities through the tax schedule. Similarly, equation (2.5) includes after-tax non-labor income, which is potentially dependent upon labor force participation. Therefore, standard estimation techniques will provide biased estimates.

We create three tax-related instruments to take advantage of the exogenous shocks to tax variables: measures of the marginal net-of-tax rate, after-tax income, and after-tax labor income. Variation comes from the interaction of the tax legislation changes, initial income levels, and marital status. For each individual, we use the HRS income, asset and demographic variables combined with the earnings records to compute tax liabilities and rates for initial year $t$. Holding all individual variables constant, we

[18] By the same reasoning, using after-tax labor income should result in a positive coefficient.

then calculate tax liabilities and rates using the tax laws from year $t+2$[19]. In order to take advantage of the differential effects that tax changes have on married and single filers, these steps are then repeated for each individual under the assumption every person is single[20]. All tax liabilities and rates are calculated using NBER's TAXSIM. The result is the following set of instruments:

$$\Delta ln(1-\tau) - \Delta ln(1-\bar{\tau}) \tag{2.6}$$

$$\Delta ln(z-T(z)) - \Delta ln(z-\bar{T}(z)) \tag{2.7}$$

$$\Delta ln(L-(T^{w}-T^{nw})) - \Delta ln(L-(\bar{T}^{w}-\bar{T}^{nw})) \tag{2.8}$$

where $\bar{\tau}$ is the marginal tax rate for a single filer and $\bar{T}$ is the tax liability for a single filer. $T^{w}$ and $T^{nw}$ are total tax liabilities for individuals when then have worked and when they have not worked, and they are assumed to be equal for non-workers in the data. Having the instruments defined in differences should reduce the bias that may occur across households that face different tax changes. These instruments also vary independently from one another. Marginal tax rates vary when tax legislation moves a kink point in the nonlinear tax schedule. After-tax income varies from tax changes based on the distance from kink points in the nonlinear tax schedule. After-tax labor income varies based on variation in non-labor income.

The theoretical prediction is that the after-tax labor income instrument in (2.8) is an appropriate instrument for selection into the intensive labor supply equation (2.3). Holding constant the variation in marginal tax rates and total after-tax income, changes in labor tax liability will serve as an exogenous shock to employment. In order for this instrument to be valid, the labor force participation decision must be correlated with labor tax changes.

All estimation is done for the entire sample, as well as separately by gender. A vector of covariates is included in all estimation. Each individual $i$ at time $t$ is placed in a cell based on age, education and marital status. There are 60 cells, coming

[19] We need to use $t+2$ rather than $t+1$ because HRS waves are only collected every other year.
[20] Any spousal earnings are added into other income.

from two marital statuses (married or single), five education categories (less than high school, GED, high school graduate, some college, college graduate), and six age groups (55-60, 61-62, 63-64, 65-67, 68-69, 70+). Indicator variables are also based on spousal age (under 55, 55-60, 61-62, 63-64, 65-67, 68-69, 70+) and education. Finally, there are measures of initial labor income $L_{it}$, spousal labor income and total household income, each interacted with a dummy variable for "married at time $t$" so that initial income has different effects based on marital status. Estimates using self-reported earnings utilize waves of the HRS covering 2000-2014, while results using SSA administrative data only cover 2000-2012.

The first step is to estimate a reduced-form version of equation (2.5). It contains all exogenous variables, including all three instruments:

$$\begin{aligned} P(Work_{i,t+1} = 1) = F(\phi_t + X_{it}'\gamma + \beta_1[\Delta ln(1-\tau) - \Delta ln(1-\bar{\tau})]_{it} \\ + \beta_2[\Delta ln(y - T(y)) - \Delta ln(y - \bar{T}(y))]_{it} \\ + \beta_3[\Delta ln(L - (T^w - T^{nw})) - \Delta ln(L - (\bar{T}^w - \bar{T}^{nw}))]_{it} + \eta_{it}) \end{aligned} \tag{2.9}$$

A probit regression is estimated for (2.9), and predictions from this estimation are then used to compute an inverse Mills ratio to use as an adjustment for selection in the intensive labor supply equation.

Table 2.7 shows the results from the selection equation using self-reported earnings and the SSA earnings file. The first two columns show that the instrument is correlated with employment probabilities for the entire self-reported sample and the subsample of self-reported earnings among men. However, column three shows that this instrument is not very good for the subsample of women, which is this paper's primary demographic of interest. The right panel of the table provides results using the administrative SSA data. Switching to the administrative data yields opposite results. The instrument is negatively correlated with employment probabilities. This result is counter to the expected influence of labor earnings on labor force participation. Further evaluation of this instrument's ability to predict labor force participation showed that the self-reported earnings results depend heavily upon the

Table 2.7: Selection Equation, Reduced Form

| Dependent Variable: | Self-reported | | | SSA earnings | | |
|---|---|---|---|---|---|---|
| I(Employed) | All | Men | Women | All | Men | Women |
| *All ages* | | | | | | |
| $\Delta ln(L-(\bar{T}^w-\bar{T}^{nw}))$ | 1.364*** | 2.154*** | 0.766 | -1.159*** | -0.946* | -1.343*** |
| | (0.386) | (0.586) | (0.512) | (0.353) | (0.496) | (0.514) |
| Observations | 37,933 | 19,298 | 18,634 | 28,260 | 15,323 | 12,937 |
| *Ages 55-74* | | | | | | |
| $\Delta ln(L-(\bar{T}^w-\bar{T}^{nw}))$ | 1.280*** | 2.377*** | 0.323 | -1.542*** | -1.352** | -1.692*** |
| | (0.453) | (0.688) | (0.606) | (0.420) | (0.588) | (0.612) |
| Observations | 26,186 | 14,412 | 11,771 | 20,286 | 11,343 | 8,943 |
| *Ages 62-70* | | | | | | |
| $\Delta ln(L-(\bar{T}^w-\bar{T}^{nw}))$ | 1.582** | 2.716*** | 0.479 | -1.689*** | -2.146*** | -1.123 |
| | (0.667) | (0.910) | (0.993) | (0.603) | (0.826) | (0.934) |
| Observations | 9,839 | 5,734 | 4,103 | 9,375 | 5,475 | 3,899 |

Significance levels: * 10%, ** 5%, *** 1%. Other variables included: predicted change in log of marginal net-of-tax rate; predicted change in log of after-tax income; year dummies; interactions for (age group X education X initial marital status); interactions based on (spousal age group X spousal education). Initial income controls include initial labor income, spousal labor income, and total income by marital status. Standard errors in parentheses are adjusted for clustering at individual level. *Source: Health and Retirement Study*, 2000-2014 linked to SSA Respondent Cross-Year Summary Earnings File. TAXSIM9 used to find estimates for tax liabilities.

HRS waves used in estimation.

Given the inconsistency of these results, we decided to assume workers and non-workers are identical after controlling for observables. This allows us to estimate a Mincerian earnings regression without adjusting for selection. This decision in reinforced by the results in Table 2.8. The best set of results coming from the first stage of the selection estimation used data from 2000-2014 with self-reported earnings. If we carry through the estimation process, the resulting predicted earnings are not much different when accounting for the selection bias. Therefore, it appears selection is not a large source of bias.

Estimating the Mincerian earnings equation, in the form derived in this Appendix, still requires instrumental variables. The marginal tax rate and the tax liability will be mechanically defined by the labor earnings of workers. We estimate equation (2.3) using the instruments created for the marginal net-of-tax rate and after-tax total income. Standard errors are clustered at the individual level.

Table 2.9 shows the results from the first stage of the intensive labor supply

Table 2.8: Average Predicted Labor Income

| | | | | All | Men | Women |
|---|---|---|---|---|---|---|
| Actual average earnings of workers - all ages | | | | \$46,462.90 | \$55,729.23 | \$37,248.74 |
| Actual average earnings of workers - ages 55-74 | | | | \$47,200.37 | \$56,580.25 | \$36,478.73 |
| Actual average earnings of workers - ages 62-70 | | | | \$40,531.02 | \$47,589.91 | \$31,398.60 |
| Selection | All | | Men | | Women | |
| Adjustment: | None | Heckman | None | Heckman | None | Heckman |
| **Predicted Labor Income Using All Ages** | | | | | | |
| *Workers* | \$39,751.60 | \$39,729.85 | \$45,302.45 | \$45,272.40 | \$34,161.35 | \$34,147.96 |
| *Non-Workers* | \$28,156.58 | \$28,125.98 | \$31,905.03 | \$31,877.70 | \$23,921.82 | \$23,887.56 |
| **Predicted Labor Income Using Ages 55-74** | | | | | | |
| *Workers* | \$37,930.12 | \$37,855.51 | \$43,059.52 | \$42,963.83 | \$31,664.57 | \$31,615.69 |
| *Non-Workers* | \$28,089.14 | \$27,986.93 | \$32,268.53 | \$32,163.69 | \$22,944.14 | \$22,845.19 |
| **Predicted Labor Income Using Ages 62-70** | | | | | | |
| *Workers* | \$29,919.14 | \$29,936.34 | \$33,735.20 | \$33,750.53 | \$24,523.88 | \$24,543.73 |
| *Non-Workers* | \$24,344.62 | \$24,377.45 | \$28,058.12 | \$28,088.26 | \$19,279.31 | \$19,315.81 |
| **Predicted Labor Income Using All Ages, by Gender** | | | | | | |
| *Workers* | | | \$47,161.01 | \$47,027.47 | \$31,988.87 | \$32,051.73 |
| *Non-Workers* | | | \$33,341.65 | \$33,147.03 | \$22,358.60 | \$22,474.81 |
| **Predicted Labor Income Using Ages 55-74, by Gender** | | | | | | |
| *Workers* | | | \$44,553.20 | \$44,425.99 | \$29,674.92 | \$29,692.18 |
| *Non-Workers* | | | \$33,475.55 | \$33,248.44 | \$21,659.11 | \$21,676.84 |
| **Predicted Labor Income Using Ages 62-70, by Gender** | | | | | | |
| *Workers* | | | \$35,253.56 | \$35,120.92 | \$23,218.58 | \$23,304.36 |
| *Non-Workers* | | | \$29,247.06 | \$29,021.63 | \$18,701.32 | \$19,230.06 |

*Source: Health and Retirement Study*, 2000-2014 using TAXSIM9 estimates for tax liabilities.

equation when the sample is workers of all ages. The results suggest that there is a strong relationship between the instruments and each endogenous variable.

Table 2.10 shows the results from the second stage of the intensive labor supply equation. The compensated elasticity coefficient on the marginal net-of-tax rate is positive, but insignificant, matching expectations. Using the coefficients from this estimation, we can predict the labor income for workers and non-workers. The predicted changes in earnings are transformed to estimate next-period earnings using:

$$\hat{L}_{i,t+1} = exp(ln(L_{it}) + \underbrace{ln(L_{i,t+1}) - ln(L_{it})}_{Predicted}) \tag{9}$$

Results from this transformation are reported in Table 2.1.

Table 2.9: Intensive Labor Supply Equation, First Stage

| Dependent Variable: | $\Delta$ ln(1-MTR) | $\Delta$ ln(After-Tax Income) |
|---|---|---|
| **All** (N=27,794) | | |
| Predicted $\Delta$ ln(1-MTR) | 0.521*** | 0.001 |
| | (0.016) | (0.001) |
| Predicted $\Delta$ ln(After-Tax Income) | 0.438*** | 1.068*** |
| | (0.038) | (0.014) |
| **Men** (N=13,365) | | |
| Predicted $\Delta$ ln(1-MTR) | 0.545*** | 0.002 |
| | (0.024) | (0.001) |
| Predicted $\Delta$ ln(After-Tax Income) | 0.448*** | 1.120*** |
| | (0.057) | (0.023) |
| **Women** (N=11,429) | | |
| Predicted $\Delta$ ln(1-MTR) | 0.495*** | -0.001 |
| | (0.023) | (0.001) |
| Predicted $\Delta$ ln(After-Tax Income) | 0.495*** | 1.039*** |
| | (0.051) | (0.017) |

Significance levels: * 10%, ** 5%, *** 1%. Other variables included: year dummies; interactions for (age group X education X initial marital status); interactions based on (spousal age group X spousal education). Initial income controls include initial labor income, spousal labor income, and total income by marital status. Standard errors in parentheses are adjusted for clustering at individual level. *Source: Health and Retirement Study*, 2000-2012 linked to SSA Respondent Cross-Year Summary Earnings File. TAXSIM9 used to find estimates for tax liabilities.

Table 2.10: Intensive Labor Supply Equation, 2SLS

| Dependent Variable:<br>$\Delta$ ln(Labor Income) | All | Men | Women |
|---|---|---|---|
| $\Delta$ ln(1-MTR) | 0.213 | 0.321 | 0.131 |
| | (0.162) | (0.232) | (0.225) |
| $\Delta$ ln(After-Tax Income) | -2.595*** | -3.493*** | -1.897*** |
| | (0.734) | (1.092) | (0.967) |
| Observations | 24,794 | 13,365 | 11,429 |

Significance levels: * 10%, ** 5%, *** 1%. Other variables included: year dummies; interactions for (age group X education X initial marital status); interactions based on (spousal age group X spousal education). Initial income controls include initial labor income, spousal labor income, and total income by marital status. Standard errors in parentheses are adjusted for clustering at individual level. *Source: Health and Retirement Study*, 2000-2012 linked to SSA Respondent Cross-Year Summary Earnings File. TAXSIM9 used to find estimates for tax liabilities.

# CHAPTER 3

# THE IMPACT OF UNEMPLOYMENT INSURANCE EXTENSIONS ON WORKER JOB-SEARCH BEHAVIOR

The expected effects of unemployment insurance (UI) have been postulated by economists for decades. UI Benefits provide consumption-smoothing liquidity to unemployed workers at the risk of encouraging those same workers to extend their unemployment spells. In a variety of studies across a variety of settings, economists have established that the presence of UI benefits does, in fact, lead to increases in unemployment duration. However, the exact mechanism by which unemployment persists is rarely addressed. For example, does an unemployed worker interpret an extension of UI benefits as an opportunity to find a better match in the labor market, or as a signal that it is less likely they will find a job at all? This study sheds light on this question.

Theoretical models of UI, starting with Mortensen (1977), frame the decision of unemployed workers around two key choices. First, workers must choose a reservation wage, which is the lowest wage a worker would accept in a job offer to leave unemployment. Second, workers can adjust the amount of time they spend searching for a job. According to this framework, increasing either the amount of weekly UI benefits or the length of time workers receive UI benefits should result in making unemployment more attractive. With a stronger desire to remain unemployed, workers should set higher reservation wages and exert lower search effort. An alternative response from unemployed workers would be interpreting longer or more generous benefits as a signal that they should expect to remain unemployed longer. Viewed as bad news, workers may set lower reservation wages and exert more search effort to increase the chances they find an acceptable job.

I use the Survey of Unemployed Workers in New Jersey (SUWNJ) to address whether workers generally see extended UI benefits as an opportunity to improve their labor market outcome or as a discouraging signal. The SUWNJ has several distinct advantages. First, it is a weekly, longitudinal survey that collects information from the same group of respondents for up to 24 weeks. The sample is completely composed of unemployed workers collecting UI benefits, which is the group of interest. Second, it collects detailed information about the job search activity of unemployed workers and directly asks workers about their reservation wages. Finally, the survey period overlaps with the November 6, 2009 extension of federal UI benefits from 76 to 99 weeks. This change in UI benefit duration can be exploited to determine the behavioral response of unemployed workers.

In order to test these theoretical predictions, I employ two separate approaches. As an initial step, I conduct hypothesis tests to evaluate whether the mean reservation wage rose or the mean search effort fell after the UI extension. Results are reported for paired samples, comparing data from the last survey a respondent completed before the extension to the first survey the same respondent completed after the extension. Due to the weekly nature of the data, tests are also run comparing the cross-sectional means within a equal-sized windows before and after the UI extension. A second approach uses methods developed in the finance event study literature and accounts for the possibility that there is a trend in reservation wages or search effort over the unemployment spell. The method is slightly modified to account for the timing of the SUWNJ data.

The results of this paper suggest that there is an absolute increase in reservation wages following the November 2009 UI extension. This increase takes time to appear in the reported reservation wages for individual workers. Search effort among unemployed workers falls in both absolute and relative terms. That is, average search effort is lower after the UI extension, and it is lower than would be expected along the regular path of search effort over an unemployment spell.

While the SUWNJ has been used by researchers to evaluate the job search behavior and reservation wages of unemployed workers, this is the first study using the dataset to examine how these behaviors respond to an extension of UI benefits. Compared with other studies that evaluate behavioral responses to UI extensions, the SUWNJ has the advantage of providing individual-level results rather than cross-sectional results. In the context of the search effort literature, it is also has the advantage of reporting the impact of UI extensions on total search effort while unemployed rather than just the impact on unemployment duration. To my knowledge, this is also the first paper within the UI literature to apply event study methods to this UI context.

The next section provides a review of the existing literature on the impact of UI benefits on worker behavior. Section 2 outlines a model of job search presented in Mortensen (1977) to highlight the expected empirical results. The data are described in section 3 and the methodological approach to testing the theoretical implications is presented in section 4. All results are presented in section 5 before the conclusion.

## 1 Literature Review

Empirical evidence on how unemployed workers respond to unemployment insurance benefits is scarce. In this paper, the two main behavioral responses are reservation wages and search effort. Measurement of both variables is often difficult, and few data sources are able to provide repeated observations over the duration of unemployment.

Previous work on reservation wages is largely subject to two data limitations. First, most self-reported reservation wages are cross-sectional. As a result, the sample of unemployed workers changes over time. This leads to potentially biased results because the distribution of unemployment durations within the sample is also changing. Second, many surveys do not directly elicit reservation wages from respondents. Rather, much of the empirical evidence on reservation wages relies upon the newly accepted wage reported by previously unemployed workers[1]. The Survey of Unemployed Workers in New Jersey (SUWNJ) resolves both of these data issues by having

[1] In some cases, newly accepted wages may be a preferred measure. There is a large literature on the inability of economic agents to accurately report their true information and expectations. See, for example, Bound and Krueger (1991) for evidence of this problem related to self-reported earnings and Manski (2004) for evidence of this problem related to subjective expectations. When a wage below the *reported* reservation wage is accepted, this may reveal more information about the individual's *true* reservation wage.

respondents complete weekly surveys in which they directly report their reservation wage.

Authors generally focus on estimating an elasticity of reservation wages with respect to UI benefits, as that parameter appears in the Shimer and Werning (2007) formula for optimal UI benefits. In particular, Shimer and Werning showed that worker welfare can be completely summarized in their reservation wage. Larger reservation wage elasticities, in this formulation, suggest that welfare may be improved through higher UI benefits. Intuitively, smaller increases in UI benefits could lead to larger increases in reservation wages, and subsequently higher accepted wages when new job matches are found.

Previous empirical work studying cross-sectional self-reported reservation wages show inconsistent responses to UI benefit extensions. For example, Feldstein and Poterba (1984) find a large elasticity of reservation wages with respect to UI benefits using a supplemental questionnaire from the 1976 Current Population Survey. In contrast, DellaVigna and Paserman (2005) show that unemployed workers who receive and do not receive UI benefits report similar reservation wages in both the Panel Study of Income Dynamics and the National Longitudinal Survey of Youth. More recently, Le Barbanchon et al. (2018) conclude they cannot reject the hypothesis that the elasticity of reservation wages with respect to UI benefit duration is zero using French data.

Newer research has studied accepted wages as a proxy for reservation wages. Results in this set of research are more consistent, showing no significant impact of UI benefits on new wages. These results also appear to be fairly widespread, as Nekoei and Weber (2017) and Chetty et al. (2007a) find evidence in Austria and Schmieder et al. (2013) use German data. Accepted post-employment wages likely create a more reasonable upper bound for reservation wages, but they also confound reservation wages with the potential wage offer distribution. Additionally, these results are often plagued with selection issues because not all workers receive and accept job offers. Both of these issues are overcome using the SUWNJ since it is longitudinal and contains direct measurement of reservation wages.

Adding to this literature are Krueger and Mueller (2016), who also use the SUWNJ. They contribute to the cross-sectional papers that use self-reported reservation wages, confirming a smaller and insignificant elasticity of reservation wages with respect to UI benefits. Krueger and Mueller (2016) also report results of fixed effects estimates of reservation wages on unemployment duration, showing an insignificant change in reservation wages after the November 2009 UI extension. In contrast to Krueger and Mueller (2016), who use regression analysis over the entire survey period to produce their result, this paper uses testing of means and modified event study analysis to study how workers adjust their reservation wages in response to an extension of UI benefits in short windows immediately following the extension announcement.

Regarding the impact of UI benefits on job search effort, much of the research addressing the relationship ultimately reports the relationship between UI benefits and unemployment duration[2]. The general results suggest that UI extensions result in moderately longer unemployment spells, which occur through a channel of decreased search effort around the extension. On average, roughly 10 extra weeks of unemployment benefits are estimated to extend unemployment durations by 1-2 weeks.

More closely related to this study are the worker-level search effort adjustments. Current papers on this matter highlight the pattern of search effort over an unemployment spell. Krueger and Mueller (2011) use the SUWNJ to show that search effort falls over the unemployment spell. The Krueger and Mueller (2011) result is expanded by Potter (2018), who shows that the pattern of search effort can be explained by workers learning about the job arrival process. These papers corroborate evidence in the Current Population Survey and American Time Use Survey, which Shimer (2004) and Mukoyama et al. (2014) show capture a hump-shaped pattern of search effort over the unemployment spell[3]. In all papers, the results indicate that

[2]For example, see Valletta (2014), Rothstein (2011), Mas and Johnston (2018), Schmieder et al. (2012) or Farber and Valletta (2015)

[3]These results are in opposition to those found in DellaVigna et al. (2017), in which Hungarian workers ramp up search just before and just after changes in their UI benefit level. Using reference-dependent utility, they argue that workers search very hard just before a fall in their UI benefit level, and continue to search hard for a period while they adjust to lower levels of consumption.

search effort falls with unsuccessful search, lending credence to the idea of discouraged workers. The paper presented here, however, is the first to report the individual-level search effort response to UI extensions.

## 2 Model

Krueger and Mueller (2016) present a simple model of reservation wages. I modify their model to include endogenous search effort, as in Mortensen (1977). In this simplified model, unemployed workers are assumed to have no savings but can receive a maximum of $T$ weeks of unemployment benefits $b$. Job offers arrive at rate $\lambda * s(t)$, which depends on the amount of search effort, $s(t)$ put forth by the worker when they have $t$ more weeks of UI benfits remaining. Offers have wages $w$ that are drawn from a distribution of wages $F(w)$. With $t$ more weeks of unemployment benefits $b$, the value function for an unemployed worker is:

$$U(t) = \max_{R(t),s(t)} \Bigg[ u(b(t), 1 - s(t)) + \beta \Bigg[ U(t-1) + \lambda * s(t) \int_R (W(x, m=0) - U(t-1)) dF(x) \Bigg] \Bigg] \quad (3.1)$$

where $u(\cdot)$ is the worker's flow utility function, which takes as inputs the worker's income and time devoted to leisure. For unemployed workers who qualify for unemployment benefits, income is the UI benefit and leisure is the fraction of the unit-length period that is not spent searching for a job. The worker discounts by factor $\beta$, and the value of starting a job paying wage $w > R(t)$ is $W(w, m = 0)$ for new workers.

In this value function, $m$ is the number of weeks employed - so new workers are 0 months employed - which influences the value of a job by determining the amount of time until a new unemployment claim can be filed. The longer is the qualification period for new-claim eligibility, the more rigid reservation wages are expected to be over unemployment duration. This is because the value of a *new job* would be significantly lower for longer qualification periods.

An increase in either the benefit $b$ or the benefit period $T$ not only increases the value of being currently unemployed, but also increases the value of being laid off in the future. As an unemployed worker nears the end of her benefit eligibility, the second effect begins to dominate and the value of becoming employed increases. This, as optimality conditions will demonstrate, leads to lower reservation wages and higher search effort.

The value function for an employed worker on a job paying wage $w$ can take two forms, one where the worker has not qualified for UI benefits and one where the worker has qualified. If a worker qualifies for UI benefits after $\bar{m}$ months, then her value function while unqualified ($m < \bar{m}$) but earning wage $w$ is:

$$W(w,m) = \max_{s(t)} \Bigg[ u(w, l_0 - s(t)) + \beta \Bigg[ (1-\delta) W(w, m+1) + \delta U(0) \\ + \lambda * s(t)(1-\delta) \int_w (W(x, m+1) - W(w, m+1)) dF(x) \Bigg] \Bigg] \quad (3.2)$$

and while qualified ($m \geq \bar{m}$) is:

$$W(w,\bar{m}) = \max_{s(t)} \Bigg[ u(w, l_0 - s(t)) + \beta \Bigg[ (1-\delta) W(w, \bar{m}) + \delta U(T) \\ + \lambda * s(t)(1-\delta) \int_w (W(x, \bar{m}) - W(w, \bar{m})) dF(x) \Bigg] \Bigg] \quad (3.3)$$

where the exogenous separation probability is $\delta$ and $l_0$ represents the remaining time outside of work that can be split between leisure and job search.

Equation 3.1 implies the optimal choice of reservation wage for an unemployed worker is defined by:

$$W(R(t), m = 0) = U(t-1) \quad (3.4)$$

Intuitively, an unemployed worker sets her reservation wage such that the value of starting a job at that reservation wage is equivalent to the value of collecting $t-1$ more periods of UI benefits while unemployed. Naturally, as unemployed workers near the exhaustion of UI benefits, the value of remaining unemployed declines and the reservation wage is predicted to decline. If an extension of benefits is granted, the worker has a incentive to delay the decrease in reservation wage. This effect should be particularly prominent for workers nearing the end of their benefit eligibility *prior to* the extension.

Equation 3.1 also implies the optimal choice of search effort for an unemployed worker:

$$\frac{\partial u(b, 1-s)}{\partial(1-s)} = \lambda \int_R (W(x, m=0) - U(t-1))dF(x) \tag{3.5}$$

Here, the unemployed worker searches until the cost of search - the marginal utility of foregone leisure - is equal to the marginal return in indirect utility from search time. The marginal return to search depends critically on the gap between the value of becoming employed and the value of remaining unemployed. Since the value of remaining unemployed falls as the remaining UI eligibility falls, the marginal return to search grows nears exhaustion. Therefore, unemployed workers increase their search effort as they near benefit exhaustion. An extension of benefits pushes UI eligible unemployed workers further from their exhaustion point, which should reduce search effort among unemployed workers. As with the effect of a benefit extension on reservation wages, this effect should be more pronounced for workers who are already near the end of their benefit eligibility or may have exhausted benefits prior to the extension of benefits.

## 3 Data and Descriptive Statistics

This study employs the Survey of Unemployed Workers in New Jersey (SUWNJ). Sampled from the universe of unemployed workers receiving UI benefits as of September 28, 2009 in the state of New Jersey, the SUWNJ contains demographic and labor market information about a group of over 6,000 workers. Weekly surveys were conducted between October 2009 and April 2010 for 12 weeks, with an additional 12 weeks collected from workers who remained unemployed after the initial sample period. In total, there are roughly 39,000 weekly interviews collected. I will restrict the sample to working age respondents (20-65) who have yet to find a full-time job and exclude extreme reservation wage and search effort responses[4]. This reduces the

[4] Specifically, I will exclude any weekly reservation wages above $8,000 or below $100, any hourly reservation wages above $100 or below $5, and anyone who reports spending more than 80 hours

sample to just over 25,600 observations and roughly 4,800 unique respondents.

The majority of reservation wage observations are reported as an annual salary or as an hourly wage. Among the respondents who report reservation wages in the same units throughout the survey, over half reported annual salaries and another 44% reported hourly wages. As shown in Table 3.1, once converted to the equivalent of an annual salary, reservation wages reported as weekly and hourly wages are substantially lower. Across all reservation wage observations, the reported annual-equivalent reservation wage is about $50,000[5]

Table 3.1: Descriptive Statistics for Reported Reservation Wages

| | Mean | Observations | Respondents |
|---|---|---|---|
| *Reported reservation wages* | | | |
| Annual salary | $61,210.03 | 13,618 | 2,229 |
| Monthly salary | $4,801.75 | 252 | 25 |
| Weekly wage | $709.96 | 1,156 | 177 |
| Hourly wage | $16.84 | 10,085 | 1,883 |
| *Converted to annual salary* | | | |
| Annual reported monthly | $57,620.95 | | |
| Annual reported weekly | $36,917.98 | | |
| Annual reported hourly | $35,036.21 | | |
| **All converted to annual salary** | $49,543.86 | 25,111 | 4,314 |

*Notes:* Reservation wages are reported in each survey. Non-annual reservation wages are converted in the following ways: reported monthly salaries are multiplied by 12; reported weekly wages are multiplied by 52; reported hourly wages are multiplied by 2,080 (assuming 40/hour work weeks and 52 weeks worked per year).

Table 3.2 shows the annual-equivalent reservation wages for workers, separated by different worker characteristics. Most of these relationships make intuitive sense. Older workers set higher reservation wages, as do the more highly educated, those with higher 2008 household incomes, and those with higher home purchase prices. Given gender differences in wages, it is also unsurprising to see men with higher reservation

searching in a week. The elimination of surveys completed after a worker has accepted a new job eliminates the largest portion of observations, dropping about 9,700 weekly surveys (about 25% of the original sample). Other large groups of observations are dropped because of the age restriction or the elimination of search effort outliers, about 5% of the original sample each.

[5] In Krueger and Mueller (2016), the authors have access to administrative data. They are able to construct reservation wages as a ratio compared with their previous wage/salary. This study does not have access to administrative data, so lacks data on when respondents leave UI and the size of UI benefits received.

wages. Similarly, given the erosion of human capital that comes with unemployment, the fall of reservation wages with rising unemployment duration is predictable.

Having more in your savings account or less credit card debt is associated with a higher reservation wage. This is likely correlated with 2008 household income, but also fits in with the model presented in this paper. The model predicts that the reservation wage is set equal to the value of remaining unemployed, which is higher when unemployed workers have more savings and more credit to maintain their utility. Those who exert more search effort also have higher reservation wages. Presumably, these individuals feel as if they have more control over the arrival rate of their job offers.

Measures of bias were calculated in Lindner (2018). Comparing those measures to reported reservation wages shows that baseline pessimists, workers who overestimate their unemployment spell, have higher average reservation wages. This is counter to expectations, as we should expect to see optimists feeling more confident about finding a job and therefore maintaining higher reservation wages. It should be noted, however, that the mean reservation wage for optimists is still above the mean for the entire sample, suggesting that those setting higher reservation wages are more likely to respond to the questions about expectations. Conversely, control optimists, workers who overestimate the return to their search effort, have higher average reservation wages. If workers believe that more effort translates into more job offers, it follows that they would have a higher reservation wage.

Search effort is reported in a number of ways. Respondents were asked to keep a time diary as well as to summarize their job search activities over the past week. The result is a set of information about the time spent searching, the methods used, and the quantity of applications filed.[6] Respondents spent an average of 14.7 hours per week searching for a job, submitting nearly 6 applications each week. Most search time was spent doing self-directed work, such as directly contacting employers, going to interviews, or filling out applications.

[6] There is also information on the number of job offers received, restrictions that job searchers faced, and training programs attended. I will focus on hours spent searching.

Table 3.2: Descriptive Statistics for Reported Reservation Wages by Characteristic

| | Mean | Obs. | | Mean | Obs. |
|---|---|---|---|---|---|
| **Entire sample** | $49,543.86 | 25,111 | *Marital status* | | |
| *Age* | | | Single | $41,590.15 | 5,970 |
| 20-24 | $27,118.14 | 815 | Married | $55,038.70 | 13,387 |
| 25-29 | $35,489.29 | 1,800 | Separated | $44,500.02 | 694 |
| 30-34 | $43,910.02 | 2,130 | Divorced | $46,234.10 | 3,321 |
| 35-39 | $47,101.40 | 2,136 | Widowed | $39,558.56 | 542 |
| 40-44 | $49,472.90 | 2,868 | Domestic partnership | $44,444.61 | 1,183 |
| 45-49 | $53,227.42 | 3,532 | *Unemployment duration* | | |
| 50-54 | $53,262.43 | 4,260 | Less than 10 weeks | $51,591.26 | 1,377 |
| 55-59 | $54,245.78 | 4,384 | 10-19 weeks | $54,942.60 | 1,990 |
| 60-64 | $53,163.25 | 3,186 | 20-29 weeks | $55,630.84 | 2,413 |
| *Education* | | | 30-39 weeks | $55,908.58 | 2,403 |
| Some high school | $30,376.27 | 447 | 40-49 weeks | $51,445.55 | 2,494 |
| High school | $34,803.05 | 3,872 | 50-59 weeks | $48,742.60 | 2,126 |
| Some college | $40,903.56 | 7,759 | 60-69 weeks | $47,165.98 | 2,093 |
| College | $55,104.73 | 7,524 | 70-79 weeks | $47,047.71 | 3,313 |
| Some graduate school | $59,358.11 | 1,922 | 80-89 weeks | $45,348.16 | 4,069 |
| Graduate school | $69,672.59 | 3,579 | 90-99 weeks | $44,614.72 | 2,220 |
| *Number of children* | | | 100 or more weeks | $40,860.98 | 613 |
| 0 | $46,848.24 | 7,892 | *Credit card debt at time of entry survey* | | |
| 1 | $47,357.52 | 4,782 | Less than $1,000 | $55,367.45 | 6,407 |
| 2 | $52,885.77 | 7,601 | $1,000-$2,499 | $52,933.49 | 2,800 |
| 3 | $52,926.54 | 2,992 | $2,500-$9,999 | $50,401.01 | 3,756 |
| 4 | $47,693.84 | 854 | $10,000-$19,999 | $54,451.20 | 2,261 |
| 5 | $53,625.95 | 346 | $20,000 or more | $58,530.60 | 2,333 |
| *2008 Household income* | | | *Savings at time of entry survey* | | |
| Less than $10,000 | $27,184.59 | 1,035 | Less than $10,000 | $44,326.69 | 7,959 |
| $10,000-$19,999 | $30,951.75 | 1,644 | $10,000-$24,999 | $54,613.20 | 2,248 |
| $20,000-$29,999 | $35,169.06 | 2,433 | $25,000-$49,999 | $61,501.58 | 1,532 |
| $30,000-$39,999 | $37,578.10 | 2,300 | $50,000-$99,999 | $66,509.36 | 1,344 |
| $40,000-$49,999 | $39,813.30 | 2,329 | $100,000 or more | $77,951.09 | 2,179 |
| $50,000-$59,999 | $42,521.72 | 1,891 | *Spousal work status* | | |
| $60,000-$69,999 | $48,047.67 | 1,588 | Spouse works | $53,700.01 | 10,819 |
| $70,000-$79,999 | $50,542.36 | 1,630 | Spouse does not work | $56,185.46 | 3,604 |
| $80,000-$89,999 | $50,902.57 | 1,555 | *Baseline bias* | | |
| $90,000-$99,999 | $50,436.18 | 1,605 | Baseline optimists | $58,687.92 | 6,963 |
| $100,000-$149,999 | $60,154.52 | 3,863 | Baseline pessimists | $66,016.91 | 343 |
| $150,000+ | $91,002.50 | 2,701 | *Control bias* | | |
| *Home purchase price* | | | Control optimists | $63,374.71 | 3,233 |
| Less than $25,000 | $42,312.21 | 341 | Control pessimists | $56,580,84 | 3,855 |
| $25,000-$49,999 | $39,836.19 | 768 | *Weekly search hours* | | |
| $50,000-$74,999 | $47,479.13 | 899 | Less than 5 hours | $43,685.30 | 8,268 |
| $75,000-$99,999 | $52,415.51 | 1,142 | 5-9 hours | $46,840.85 | 4,723 |
| $100,000-$149,999 | $50,708.46 | 2,291 | 10-14 hours | $50,424.67 | 2,986 |
| $150,000-$199,999 | $52,391.53 | 2,141 | 15-19 hours | $51,645.78 | 2,139 |
| $200,000-$299,999 | $55,739.23 | 3,008 | 20-24 hours | $53,385.59 | 1,647 |
| $300,000-$399,999 | $61,006.92 | 1,713 | 25-29 hours | $57,676.25 | 1,195 |
| $400,000-$499,999 | $71,362.44 | 721 | 30-34 hours | $56,889.66 | 996 |
| $500,000-$599,999 | $80,973.49 | 332 | 35-39 hours | $58,477.97 | 760 |
| $600,000 or more | $93,280.25 | 478 | 40 or more hours | $59,525.93 | 2,397 |
| *Gender* | | | | | |
| Male | $56,965.68 | 11,736 | | | |
| Female | $43,031.54 | 13,375 | | | |

*Notes:* Reservation wages are reported in each survey. Non-annual reservation wages are converted in the following ways: reported monthly salaries are multiplied by 12; reported weekly wages are multiplied by 52; reported hourly wages are multiplied by 2,080 (assuming 40/hour work weeks and 52 weeks worked per year).

Table 3.4 provides results of search effort by demographic characteristic. Hours spent searching follows a hump-shaped pattern over age, suggesting that prime-age

Table 3.3: Descriptive Statistics on Job Search

| | Obs. | Mean |
|---|---|---|
| **Total hours searched in past 7 days** | 25,607 | 14.7 |
| *Hours of self-directed search* | | *12.4* |
| Directly contact employer | 6,755 | 0.8 |
| Contact friends/relatives | 9,349 | 1.2 |
| Attend job training | 1,814 | 0.8 |
| Answering ads | 10,509 | 2.0 |
| Attend interviews | 3,541 | 0.4 |
| Send resumes/applications | 15,408 | 3.3 |
| Look through ads | 16,760 | 3.9 |
| *Hours of aided search* | | *1.2* |
| Employment agency | 6,664 | 0.8 |
| University employment center | 1,563 | 0.1 |
| Union register | 2,200 | 0.3 |
| Applications submitted in past 7 days | 19,378 | 5.9 |

*Notes: Total hours searched and number of applications submitted are reported as summary values by respondents at the end of every seven day period. This can be split into self-directed search and aided search. Self-directed search includes contacting employers directly, contacting friends or relatives, attending job training, placing or answering ads, going to interviews, sending resumes, filling out applications, or looking at ads. Aided search includes contacting public or private employment agencies, contacting university employment centers or checking a union register. The remaining search hours not categorized fell into an 'other' category.*

workers are working harder to regain employment. This is also true for men and respondents whose spouses do not work, potentially increasing the urgency to leave unemployment when needing to support a family as a sole breadwinner. Financial constraints may also press respondents to search more intensively. Those with more credit card debt spend more time searching, as well as those with lower levels of savings.

Higher levels of search effort are correlated with being more highly educated. Other variables correlated with education, such as home purchase price and household income (and, in turn, reservation wages) show the same increases in search effort at higher levels of income.

As described in Shimer (2004) and Mukoyama et al. (2014), search effort initially increases over unemployment duration before falling at longer durations. Workers likely increase search effort as they grow eager to find a job, only to decrease search effort as they become discouraged. Potter (2018) identifies the drop in search effort that occurs through prolonged periods of bad news. There is no clear pattern across the two measures of bias measured in Lindner (2018). The expectation is for search effort to vary over the measure of control bias, which is defined based upon the return to search effort, but the theoretical prediction about whether optimists should search more or less is ambiguous due to competing incentives. The search effort reported here does not shed light on the matter.

Figures 3.1 and 3.2 show the average reported reservation wage and search effort for each day around the November 6, 2009 extension announcement. A local polynomial is fit to the set of reservation wages and search hours reported. Figure 3.1 illustrates a steady increase in average reservation wages following the announcement date. While the graph does not clearly show a discrete jump in average reservation wages, it appears as if there are higher average reservation wages in the periods that follow the extension. Similarly, Figure 3.2 shows steady declines in search hours following the extension of UI benefits.

## 4 Empirical Approach

There were a series of UI benefits extensions during the Great Recession. Beyond the standard, state-funded 26 weeks of unemployment insurance, a federally-funded increase was added in June 2008 as support for workers given the rising unemployment rate and worsening labor market. Emergency Unemployment Compensation 2008 (EUC08) allowed for workers unemployed in May 2006 or after to move through a tiered system of extra unemployment benefits after their state-funded UI was exhausted. As the recession worsened, the EUC08 was modified on several occasions to either provide more benefits or reduce the eligibility requirements.

This paper will exploit the presence of the November 6, 2009 EUC08 extension within the SUWNJ sample period[7]. As reported by Nakajima (2012), this particular

[7] I will use the exact date of the extension in the analysis, but an additional robustness check could utilize other dates. For example, while President Obama signed the extension on November 6, the

Table 3.4: Descriptive Statistics for Reported Search Hours by Characteristic

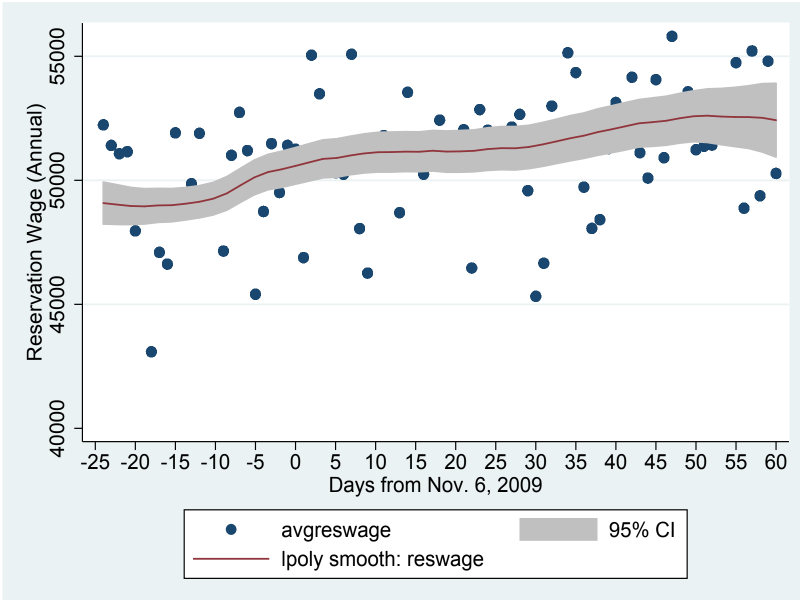
| | Mean | Obs. | | Mean | Obs. |
|---|---|---|---|---|---|
| **Entire sample** | 14.7 | 25,607 | *Marital status* | | |
| *Age* | | | Single | 13.6 | 6,105 |
| 20-24 | 10.4 | 831 | Married | 14.2 | 13,656 |
| 25-29 | 12.4 | 1,841 | Separated | 14.8 | 721 |
| 30-34 | 12.6 | 2,208 | Divorced | 18.3 | 3,369 |
| 35-39 | 13.4 | 2,172 | Widowed | 16.8 | 548 |
| 40-44 | 14.8 | 2,906 | Domestic partnership | 15.4 | 1,190 |
| 45-49 | 16.4 | 3,592 | *Unemployment duration* | | |
| 50-54 | 17.0 | 4,388 | Less than 10 weeks | 14.6 | 1,404 |
| 55-59 | 15.5 | 4,440 | 10-19 weeks | 13.8 | 2,033 |
| 60-64 | 13.4 | 3,229 | 20-29 weeks | 15.2 | 2,475 |
| *Education* | | | 30-39 weeks | 15.4 | 2,446 |
| Some high school | 11.1 | 452 | 40-49 weeks | 15.8 | 2,520 |
| High school | 12.8 | 3,914 | 50-59 weeks | 15.8 | 2,168 |
| Some college | 13.8 | 7,897 | 60-69 weeks | 15.2 | 2,137 |
| College | 15.3 | 7,702 | 70-79 weeks | 15.2 | 3,365 |
| Some graduate school | 15.7 | 1,942 | 80-89 weeks | 14.0 | 4,165 |
| Graduate school | 17.4 | 3,690 | 90-99 weeks | 12.8 | 2,272 |
| *Number of children* | | | 100 or more weeks | 12.0 | 622 |
| 0 | 15.0 | 8,075 | *Credit card debt at time of entry survey* | | |
| 1 | 13.5 | 4,881 | Less than $1,000 | 13.6 | 6,516 |
| 2 | 15.0 | 7,702 | $1,000-$2,499 | 15.1 | 2,834 |
| 3 | 14.5 | 3,054 | $2,500-$9,999 | 15.4 | 3,831 |
| 4 | 14.8 | 875 | $10,000-$19,999 | 16.3 | 2,320 |
| 5 | 18.1 | 356 | $20,000 or more | 16.0 | 2,369 |
| *2008 Household income* | | | *Savings at time of entry survey* | | |
| Less than $10,000 | 11.8 | 1,047 | Less than $10,000 | 14.2 | 8,077 |
| $10,000-$19,999 | 13.5 | 1,675 | $10,000-$24,999 | 15.1 | 2,278 |
| $20,000-$29,999 | 14.6 | 2,482 | $25,000-$49,999 | 15.7 | 1,555 |
| $30,000-$39,999 | 13.8 | 2,341 | $50,000-$99,999 | 14.1 | 1,355 |
| $40,000-$49,999 | 15.8 | 2,360 | $100,000 or more | 14.1 | 2,198 |
| $50,000-$59,999 | 13.9 | 1,922 | *Spousal work status* | | |
| $60,000-$69,999 | 14.1 | 1,616 | Spouse works | 13.7 | 10,993 |
| $70,000-$79,999 | 13.8 | 1,650 | Spouse does not work | 16.0 | 3,698 |
| $80,000-$89,999 | 14.3 | 1,595 | *Baseline bias* | | |
| $90,000-$99,999 | 14.7 | 1,621 | Baseline optimists | 14.4 | 7,000 |
| $100,000-$149,999 | 15.1 | 3,940 | Baseline pessimists | 14.1 | 348 |
| $150,000+ | 17.9 | 2,749 | *Control bias* | | |
| *Home purchase price* | | | Control optimists | 14.5 | 3,252 |
| Less than $25,000 | 11.8 | 341 | Control pessimists | 14.6 | 3,874 |
| $25,000-$49,999 | 11.6 | 782 | *Annual reservation wage* | | |
| $50,000-$74,999 | 12.1 | 903 | Less than $20k | 11.3 | 1,107 |
| $75,000-$99,999 | 12.7 | 1,152 | $20k - $30k | 12.3 | 4,316 |
| $100,000-$149,999 | 14.3 | 2,335 | $30k - $40k | 13.0 | 5,645 |
| $150,000-$199,999 | 14.9 | 2,180 | $40k - $50k | 14.4 | 4,138 |
| $200,000-$299,999 | 14.5 | 3,048 | $50k - $60k | 15.6 | 3,261 |
| $300,000-$399,999 | 15.7 | 1,744 | $60k - $70k | 15.9 | 1,860 |
| $400,000-$499,999 | 15.6 | 737 | $70k - $80k | 16.7 | 1,372 |
| $500,000-$599,999 | 18.5 | 351 | $80k - $90k | 17.7 | 1,090 |
| $600,000 or more | 21.7 | 482 | $90k - $100k | 21.8 | 612 |
| *Gender* | | | $100k - $150k | 20.3 | 1,287 |
| Male | 16.3 | 11,915 | $150k or more | 24.8 | 423 |
| Female | 13.3 | 13,692 | | | |

*Notes:* Search effort hours reported in each survey.

extension was significant. As part of this extension, Tiers 3 and 4 of EUC08 program

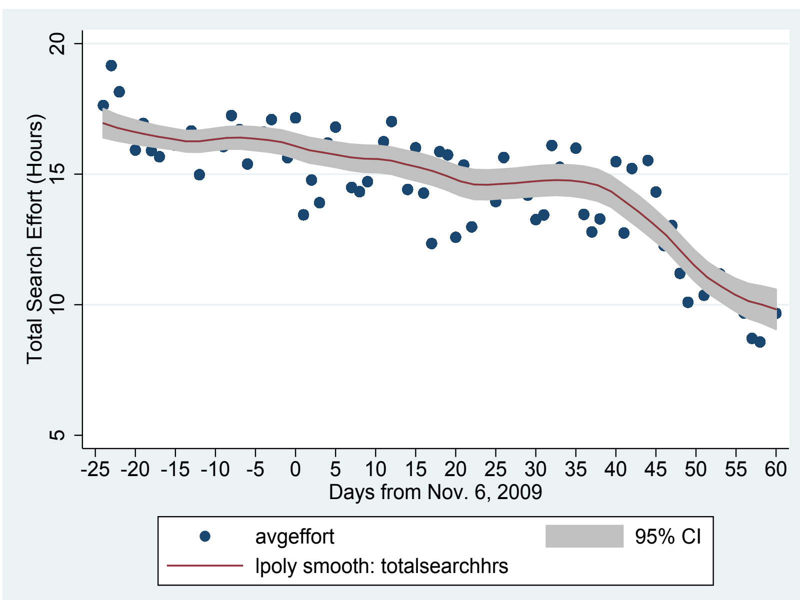
legislation was passed by Congress the previous afternoon. Also, as noted in Krueger and Mueller (2016), the law did not go into effect until November 8. As noted in Rogers (1998), anticipation of UI extensions seems like a larger problem. However, the choice of November 6 is reinforced by the additional shock of a worse-than-expected jobs report that morning.

Figure 3.1: Average Daily Reservation Wages Around UI Extension




*Notes: Red line is a kernel-weighted local polynomial on the set of reservation wages. Time 0 represents the UI benefit extension date of November 6, 2009. Dots are the average reservation wage for each date. Source: Office of Population Research at Princeton University and author's calculations.*

Figure 3.2: Average Daily Search Effort Around UI Extension




*Notes: Red line is a kernel-weighted local polynomial on the set of search effort responses. Time 0 represents the UI benefit extension date of November 6, 2009. Dots are the average search effort for each date. Source: Office of Population Research at Princeton University and author's calculations.*

were enacted. Including a minor modification to Tier 2, this provided a total of 20 more weeks of unemployment benefits[8]. For most workers in New Jersey, this allowed for a total of 99 weeks of unemployment insurance.

The 20-week extension of UI benefits represents a discrete jump in the benefit period $T$. According to the model presented in section 2, this change should lead to a discrete jump in the value function for an unemployed worker and lead to higher reservation wages. Similarly, the longer benefit period and increased value of unemployment reduces the benefit to search and should result in lower search effort.

In order to address the question of whether UI extensions influence the reservation wages set or search effort exerted by workers, this paper employs several techniques that utilize the longitudinal nature of the SUWNJ. In the first part, a set of hypothesis tests are conducted to evaluate whether there are significant changes to the mean reservation wage or search effort following the UI extension date. In addition, since the majority of observations appear after the extension occurs, a modified event study analysis, where the baseline measure for the variable will be estimated using post-event data, is employed.

### 4.1 Means Tests

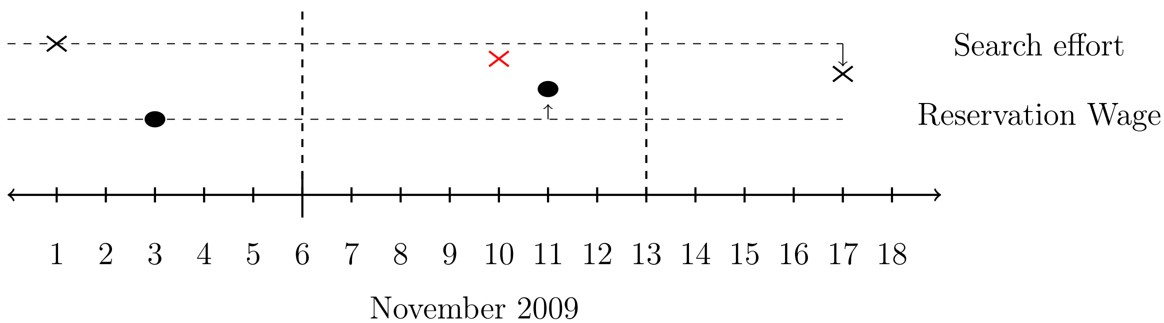
Figures 3.1 and 3.2 illustrate that reservation wages rise and search effort falls near the November 6 extension. These tests provide a formal way to examine whether there are significant absolute changes in these two variables of interest.

In the first set of results, I identify the last survey before the extension and the first survey after the extension completed by each respondent. For reservation wages, as illustrated in Figure 3.3, the post-extension observation can fall anywhere after November 6. This is because the reservation wage is a forward-looking response. On average the last survey before the extension was completed 7.5 days before November 6, while the first survey after the extension was completed within 14.5 days. However, search effort is reported as the number of hours spent looking for a job *over the past 7 days*. Because of this timing issue, the post-estimation search effort observation is

[8] The expansion of benefits was one extra week in Tier 2, 13 in Tier 3, and six in Tier 4

the first survey completed after November 13. This means the first survey completed after the extension occurs over 20 days after November 6, while the last pre-extension survey occurs 7 days before.

Figure 3.3: Illustration of Paired Sample Means Test



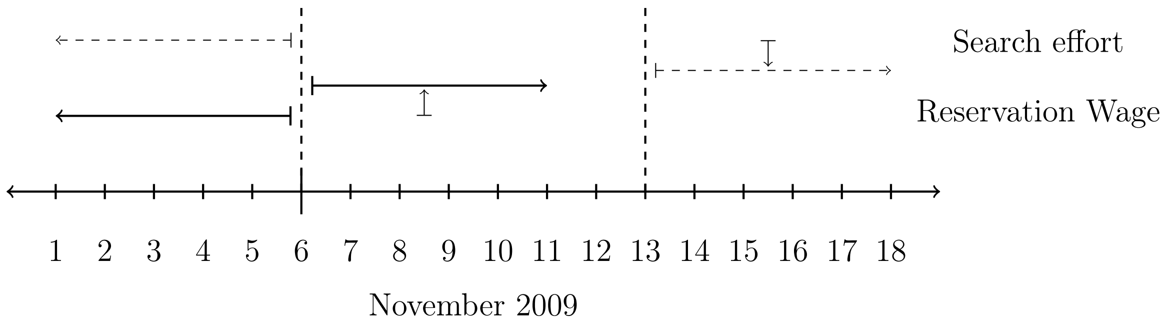

*Notes: The goal is to match the last observation for a respondent before the UI extension with the first observation from the same respondent following the UI extension. Reservation wages are reported in real-time, so there is no need to compare pre- and post-reservation wages with a lag. Search effort is a recall question over the past 7 days, so the post-window is shifted to avoid overlap with the extension date.*

Given the varying and low response rates, as well as the weekly frequency, I also run tests for independent before and after samples when studying windows close to the extension date. By doing this, I am implicitly assuming that the respondents in the pre-event window are not the same as the respondents in the post-event window. Figure 3.4 illustrates the technique, using a 5-day window before and after the extension as an example. The average over all respondents who completed a survey within the pre-extension window is compared with the average over all respondents who completed a survey within the post-extension window. As in the paired exercise, the post-extension window for search effort begins after November 13 when search effort data do not contain the November 6 announcement date. As the event window get larger, the sample size grows.

### 4.2 Event Study Analysis

This exercise is based on the event study analysis presented in Kothari and Warner (2007), which is largely rooted in the corporate finance literature. Typically, event studies are used to analyze the effect of an event such as a stock split on the stock

Figure 3.4: Illustration of Independent Sample Means Test (5-day window)



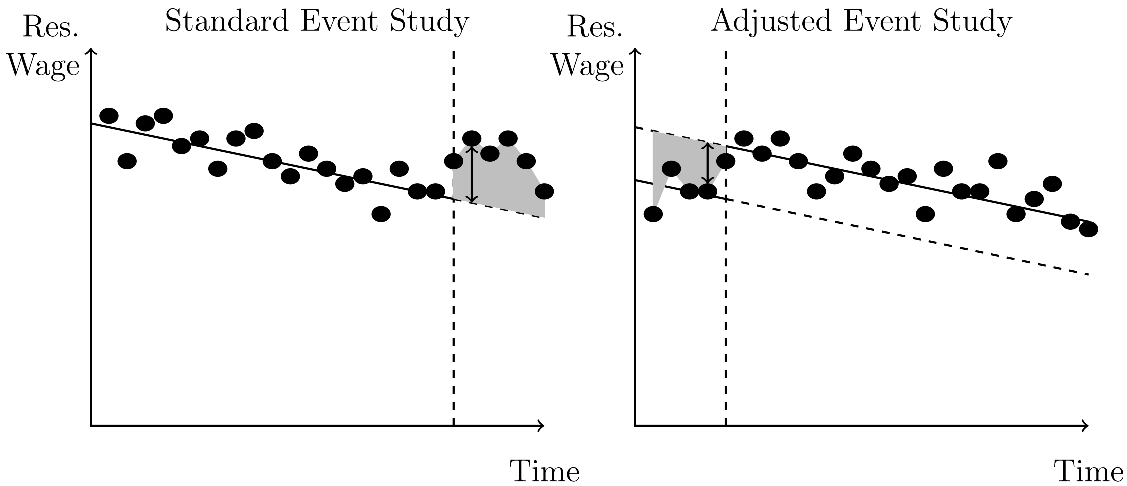

*Notes: Windows are symmetric on either side of the event. The variable or interest is averaged within the window for each window size. Reservation wages are reported in real-time, so windows are immediately before or after November 6. Search effort is a recall question over the past 7 days, so the post-window gets shifted to avoid overlap with the extension date.*

returns for a company. In this case, the UI extension on November 6 serves as the event.

In a standard event study analysis, a large pre-event period is used to estimate some version of a normal trend in the variable of interest for each respondent. Then, for each respondent, it is possible to calculate error terms in the post-event window. These errors are referred to as 'abnormal' variations in the variable of interest, and provide a relative measure of whether reservation wages have increased or search effort has declined. An alternative way to evaluate these outcomes is to sum the errors over a period following the event to see if there are cumulative misses. This is illustrated in Figure 3.5 for reservation wages. The abnormal reservation wage is the vertical line just after the event date, showing the gap between the reported reservation wage and the reservation wage predicted by the pre-event period trend. The shaded gray area shows the cumulative abnormal responses above trend.

However, there are not enough observations in the SUWNJ before November 6 for each respondent to form a pre-event trend. Instead, this paper modifies the approach by estimating a trend in the *post-extension* period. Taking advantage of the longer post-extension sample period, I estimate a trend for reservation wages and search effort after the extension that can be used to evaluate whether the pre-extension outcomes were significantly different from trend. The modified approach is also presented in Figure 3.5.

Figure 3.5: Illustration of Event Study Adjustment




*Notes: In the standard approach, a trend level of the variable of interest is estimated in a pre-event period. Abnormal responses of the variable can be calculated by taking the difference between the normal expectation and the true outcome (vertical line) or by summing all of these errors over a post-event window (shaded gray area). There are not enough pre-event observations in the SUWNJ to find a trend, so this study finds a* ***post-event trend*** *and calculates differences between normal expectations and true outcomes in the* ***pre-event period****.*

After trimming the original sample to 4,813 respondents, only 1,956 report at least three reservation wages and 1,841 report at least three search effort observations over the post-extension period. To form a simple linear estimate relating reservation wages or search effort to unemployment duration for an individual, a meaningful gradient would need at least 10 observations. There are just 385 respondents with at least 10 reservation wage observations and 375 respondents with at least 10 search effort observations in the post-extension period.

One concern is that the group of individuals with ten or more reported reservation wages are systematically different from the rest of the survey sample. Table 3.5 shows the descriptive statistics for survey respondents depending upon how many reservation wage or search effort observations they reported. On average, the group of survey-weighted respondents with enough reservation wage observations for a linear prediction (10 or more) entered the survey 40 weeks further into their unemployment spell. They are more likely to be female, but less likely to be black or Hispanic.

On average, they are also older and more highly educated. The weighted samples with 3 or more reservation wage or search effort observations, besides being more educated, are much closer to the entire weighted sample. Moving forward, given the lack of representativeness among respondents with 10 or more observations in the post-extension window, I perform the analysis using both subsamples.

In completing the analysis, I run respondent-level regressions of reservation wages and search on unemployment duration, without controls. Using the results of individual linear regressions, I form predictions of reservation wages and search effort for each observation. I then create a measure of abnormal reservation wages or search effort by taking the difference between survey response and predicted values. For each group, I also calculate cumulative abnormal differences over different windows - 14, 21 and 24 days[9].

These results of the individual-level trend regressions are summarized in Table 3.6. For the reservation wage responses, there are large masses of respondents that always report the same reservation wage. After removing those respondents, trends for reservation wages among those with three or more responses appear to be relatively flat. Trends slope downward more sharply among respondents with 10 or more reservation wage observations, which matches what would be expected among longer-term unemployed workers. For search effort, there are clear downward trends over unemployment duration. This confirms previous findings in the literature.

## 5 Results

The paired reservation wage test of means relies on over 3,200 respondents. A difference is taken between the first reservation wage reported after the UI extension and the last reservation wage reported before the UI extension. Using the set of differences, a one-sided t-test is conducting to see if the difference in means is positive. Recall, the model in section 2 showed that reservation wages should rise in response to an increase in benefit duration. Results in Table 3.7 show that reservation wages

[9] I could calculate cumulative returns for shorter windows, but I need to calculate a standard deviation over a longer window in order to have enough observations.

Table 3.5: Descriptive Statistics for the Universe and Respondents

| | | All Respondents | | 3+ Res. Wages | | 10+ Res. Wages | | 3+ Effort | | 10+ Effort | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Universe | Raw | Weight | Raw | Weight | Raw | Weight | Raw | Weight | Raw | Weight |
| No. of observations | 362,292 | 4,813 | | 1,956 | | 385 | | 1,841 | | 375 | |
| Weeks of UI benefits paid by September 28, 2009 | 30.6 | 41.1 | 28.1 | 44.9 | 30.3 | 69.4 | 67.9 | 45.6 | 30.6 | 69.6 | 68.8 |
| *Demographic data (percent of total)* | | | | | | | | | | | |
| Female | 45.4 | 53.0 | 48.5 | 53.8 | 49.1 | 56.9 | 54.1 | 53.6 | 47.7 | 56.8 | 53.9 |
| Age in years | | | | | | | | | | | |
| 24 or less | 9.7 | 6.4 | 9.6 | 2.1 | 3.3 | 1.3 | 2.6 | 2.0 | 2.9 | 1.3 | 2.8 |
| 25-34 | 22.5 | 22.0 | 27.1 | 13.7 | 16.2 | 11.7 | 13.7 | 13.1 | 14.2 | 11.7 | 14.8 |
| 35-44 | 22.0 | 22.2 | 23.4 | 20.5 | 22.7 | 15.1 | 13.7 | 19.6 | 21.6 | 14.9 | 13.8 |
| 45-54 | 23.6 | 27.7 | 24.7 | 32.7 | 31.4 | 32.2 | 29.3 | 33.4 | 33.3 | 32.5 | 30.0 |
| 55 or over | 22.1 | 21.7 | 15.1 | 31.0 | 26.3 | 39.7 | 40.5 | 32.0 | 28.0 | 39.5 | 38.6 |
| Black | 20.1 | 17.5 | 23.6 | 13.5 | 18.1 | 9.7 | 14.0 | 12.7 | 16.7 | 9.9 | 14.1 |
| Asian | 4.8 | 5.5 | 4.4 | 4.6 | 4.7 | 4.1 | 2.2 | 4.6 | 4.8 | 10.7 | 2.3 |
| Hispanic | 19.1 | 10.7 | 20.0 | 7.5 | 16.1 | 5.9 | 12.2 | 7.3 | 15.5 | 6.0 | 12.5 |
| Education | | | | | | | | | | | |
| Less than high school | 15.6 | 2.7 | 6.9 | 1.5 | 3.4 | 1.6 | 3.7 | 1.4 | 3.2 | 1.3 | 2.1 |
| High school | 43.4 | 17.4 | 28.6 | 14.5 | 25.1 | 14.0 | 22.5 | 14.2 | 24.9 | 13.9 | 22.2 |
| Some college | 22.2 | 32.3 | 36.1 | 29.7 | 36.0 | 31.7 | 40.6 | 29.8 | 35.6 | 32.0 | 40.5 |
| College | 19.0 | 47.6 | 28.4 | 54.3 | 35.3 | 52.7 | 33.2 | 54.6 | 36.3 | 52.8 | 35.0 |

*Notes:* The universe is the group of individuals receiving UI benefits in New Jersey as of September 28, 2009. From that universe, a sample of nearly 64,000 individuals were surveyed and only a subset of those who were surveyed submitted responses. Sample weights are applied to account for non-response and selection probability. This table compares the statistics among respondents that have varying numbers of responses to survey questions about reservation wages and search effort.

Table 3.6: Summary of Trend Regressions

| | | At least 2 different | | | Significant trend | | |
|---|---|---|---|---|---|---|---|
| | Total | Total | Pos. | Neg. | Total | Pos. | Neg. |
| *3 or more reservation wages reported* | | | | | | | |
| Count | 1,956 | 1,058 | 512 | 546 | 200 | 76 | 124 |
| Percent of total | | | 48.4% | 51.6% | | 38.0% | 62.0% |
| *10 or more reservation wages reported* | | | | | | | |
| Count | 385 | 239 | 107 | 132 | 91 | 30 | 61 |
| Percent of total | | | 44.8% | 55.2% | | 33.0% | 67.0% |
| *3 or more search effort obs. reported* | | | | | | | |
| Count | 1,841 | 1,770 | 627 | 1,143 | 263 | 65 | 198 |
| Percent of total | | | 35.4% | 64.6% | | 24.7% | 75.3% |
| *10 or more search effort obs. reported* | | | | | | | |
| Count | 375 | 367 | 136 | 231 | 111 | 33 | 78 |
| Percent of total | | | 37.1% | 62.9% | | 29.7% | 70.3% |

*Notes:* Normal reservation wages and search effort are predicted using results from a linear regression of reservation wages or search effort on unemployment duration for each individual, where linear regression is separately estimated for the sample of individuals with 3 or more observations or 10 or more observations. Reported are the number and fractions of respondents with positive or negative slope coefficients. A smaller subset is reported for the respondents with slope coefficients that are significantly different from zero.

actually dropped among the paired sample of respondents. This result is driven by the drop among respondents reporting their reservation wage as an annual salary. In fact, the entire sample and the group responding with annual salaries reported statistically significant declines in their reservation wages at the 5 percent level. This suggests that respondents may have taken the UI extension as a signal that labor market conditions were worse than expected.

Table 3.8 reports the results of reservation wage means tests on subsets within the sample. The decline in reservation wages is driven by men. They seem to be college-educated, single and without children. There is also a correlation with financial constraints, as those with higher home purchase prices, moderately high amounts of credit card debt, and lower savings all reduce their reservation wage. The UI extension may lower their threshold for accepting a new job. However, there also appears to be a tipping point, as those with the highest amounts of credit card debt and home prices increase their reservation wages. Presumably, these individuals have so many

Table 3.7: Reservation Wage Means Test: Paired Sample, by Reservation Wage Reporting Method

| | Mean Difference | Observations |
|---|---|---|
| *Reported reservation wages* | | |
| Annual salary | -$1,175** | 1,711 |
| Monthly salary | $1,791.30 | 23 |
| Weekly wage | -$3,882.88* | 125 |
| Hourly wage | $106.47 | 1,319 |
| **All respondents** | -$730.34** | 3,208 |

*Notes:* The event date is taken as November 6, 2009, the extension of UI benefits. Each row shows the mean difference in reservation wages reported by the same respondent in their last survey before and first survey after the extension. One-sided t-tests are computed to test whether the mean reservation wage is different before and after the UI extension. Significance levels: * 10%, ** 5%, *** 1%.

financial obligations that they cannot afford to accept a low paying job.

Interestingly, reservation wages fall for the group of workers who were unemployed between 80-89 weeks upon entering the survey. This group, if they had been fully eligible for all previous UI extensions, would have just exhausted their benefits when the survey started. Even though they were just provided benefits and had the value of remaining unemployed increased dramatically, they lowered their reservation wages. More in line with the theoretical prediction, the group of workers who had been unemployed for over 100 weeks did dramatically increase their reservation wage after being given 20 extra weeks of unemployment benefits.

The search effort means tests of paired respondents are reported in Table 3.9. One-sided t-tests are conducted to test whether the difference in means is negative. Among the 2,539 respondents, the average decline in search effort was one hour per week. This matches the expected outcome from the theoretical model, and is consistent over sex, marital status and unemployment duration. The effect is strongest for more highly educated prime age workers who have working spouses and lower reservation wages.

In the means-test analysis that relies on the assumption of independent samples before and after the UI extension, the event window size can vary. For each window size, a one-sided t-test evaluates whether the mean reservation wage after the UI

Table 3.8: Reservation Wage Means Test: Paired Sample, by Characteristic

| | Mean Diff. | Obs. | | Mean Diff. | Obs. |
|---|---|---|---|---|---|
| **Entire sample** | -$730.34** | 3,208 | *Marital status* | | |
| *Age* | | | Single | -$1,300.05* | 782 |
| 20-24 | $550.43* | 117 | Married | -$711.24 | 1,674 |
| 25-29 | -$563.03 | 256 | Separated | $50.69 | 104 |
| 30-34 | -$338.30 | 297 | Divorced | -$564.60 | 419 |
| 35-39 | -$1,021.63 | 312 | Widowed | $527.08 | 65 |
| 40-44 | -$728.65 | 409 | Domestic partnership | $315.75 | 159 |
| 45-49 | -$276.70 | 466 | *Unemployment duration (entry survey)* | | |
| 50-54 | -$519.32 | 508 | Less than 10 weeks | $1,518.81 | 148 |
| 55-59 | -$1,680.92* | 497 | 10-19 weeks | -$2,193.45* | 314 |
| 60-64 | -$918.40 | 346 | 20-29 weeks | -$689.82 | 340 |
| *Education* | | | 30-39 weeks | -$440.90 | 362 |
| Some high school | -$705.52 | 58 | 40-49 weeks | -$845.10 | 361 |
| High school | -$446.67 | 508 | 50-59 weeks | -$363.80 | 305 |
| Some college | $107.80 | 988 | 60-69 weeks | -$908.10 | 236 |
| College | -$2,773.23*** | 936 | 70-79 weeks | $451.56 | 319 |
| Some graduate school | -$272.46 | 252 | 80-89 weeks | -$2,844.36** | 498 |
| Graduate school | $995.27* | 464 | 90-99 weeks | $782.29 | 256 |
| *Number of children* | | | 100 weeks or more | $3,155.36*** | 69 |
| 0 | -$1,256.26** | 955 | *Credit card debt at time of entry survey* | | |
| 1 | -$749.54 | 605 | Less than $1,000 | -$735.38 | 749 |
| 2 | -$284.02 | 997 | $1,000-$2,499 | -$1,750.88 | 361 |
| 3 | $385.15 | 399 | $2,500-$9,999 | -$888.23 | 510 |
| 4 | $1,252.14 | 117 | $10,000-$19,999 | -$2,692.57* | 294 |
| 5 | -$10,250.23 | 43 | $20,000 or more | $1,061.74* | 299 |
| *2008 Household income* | | | *Savings at time of entry survey* | | |
| Less than $10,000 | -$409.88 | 145 | Less than $10,000 | -$898.19* | 1,012 |
| $10,000-$19,999 | $427.18 | 215 | $10,000-$24,999 | -$163.16 | 294 |
| $20,000-$29,999 | -$759.00 | 325 | $25,000-$49,999 | -$195.63 | 192 |
| $30,000-$39,999 | $907.21 | 286 | $50,000-$99,999 | -$2,968.24 | 148 |
| $40,000-$49,999 | -$2,298.11* | 301 | $100,000 or more | -$1,519.00** | 241 |
| $50,000-$59,999 | -$1,552.85** | 221 | *Spousal work status* | | |
| $60,000-$69,999 | $1,934.34 | 205 | Spouse works | -$785.08 | 1,377 |
| $70,000-$79,999 | -$661.75 | 208 | Spouse does not work | -$222.92 | 436 |
| $80,000-$89,999 | $46.98 | 195 | *Baseline bias* | | |
| $90,000-$99,999 | -$1,812.16 | 204 | Baseline optimists | -$538.94 | 815 |
| $100,000-$149,999 | -$1,662.57* | 491 | Baseline pessimists | $114.42 | 43 |
| $150,000+ | -$1,252.26 | 354 | *Control bias* | | |
| *Home purchase price* | | | Control optimists | -$782.79 | 414 |
| Less than $25,000 | -$20.00 | 38 | Control pessimists | -$343.77 | 419 |
| $25,000-$49,999 | $482.59 | 85 | *Weekly search hours (average)* | | |
| $50,000-$74,999 | $459.21 | 101 | Less than 5 hours | -$728.88 | 906 |
| $75,000-$99,999 | -$929.69 | 130 | 5-9 hours | -$1,342.55 | 705 |
| $100,000-$149,999 | -$213.07 | 270 | 10-14 hours | -$851.81 | 440 |
| $150,000-$199,999 | -$654.72 | 271 | 15-19 hours | -$55.44 | 316 |
| $200,000-$299,999 | -$2.78 | 374 | 20-24 hours | -$106.46 | 221 |
| $300,000-$399,999 | -$3,865.97** | 211 | 25-29 hours | -$811.02* | 167 |
| $400,000-$499,999 | -$2,249.17* | 96 | 30-34 hours | -$1,805.21 | 140 |
| $500,000-$599,999 | -$7,557.33 | 45 | 35-39 hours | $743.33 | 84 |
| $600,000 or more | $5,494.60** | 63 | 40 or more hours | $24.10 | 229 |
| *Gender* | | | | | |
| Male | -$1,557.20*** | 1,477 | | | |
| Female | -$7.74 | 1,731 | | | |

*Notes:* The event date is November 6, 2009, the extension of UI benefits. Each row shows the mean difference in reservation wages reported by respondents in their last survey before and first survey after the extension. T-tests are computed to test whether the mean reservation wage is different before and after the UI extension. Significance levels: * 10%, ** 5%, *** 1%.

Table 3.9: Search Effort Means Test: Paired Sample, by Characteristic

| | Mean Diff. | Obs. | | Mean Diff. | Obs. |
|---|---|---|---|---|---|
| **Entire sample** | -1.00*** | 2,539 | *Marital status* | | |
| *Age* | | | Single | -1.87*** | 608 |
| 20-24 | -1.67 | 77 | Married | -0.87*** | 1,340 |
| 25-29 | -1.88** | 188 | Separated | -1.81 | 77 |
| 30-34 | -1.52** | 231 | Divorced | -0.04 | 336 |
| 35-39 | -2.40*** | 238 | Widowed | -1.89 | 54 |
| 40-44 | -1.49** | 330 | Domestic partnership | 0.07 | 120 |
| 45-49 | -1.12* | 357 | *Unemployment duration (entry survey)* | | |
| 50-54 | 0.22 | 415 | Less than 10 weeks | 1.09 | 82 |
| 55-59 | 0.16 | 413 | 10-19 weeks | -1.20* | 241 |
| 60-64 | -1.37** | 290 | 20-29 weeks | -1.90*** | 267 |
| *Education* | | | 30-39 weeks | -1.35** | 292 |
| Some high school | -3.01* | 50 | 40-49 weeks | -0.52 | 282 |
| High school | -0.62 | 398 | 50-59 weeks | -0.83 | 250 |
| Some college | -1.39*** | 787 | 60-69 weeks | -1.74** | 186 |
| College | -1.16*** | 747 | 70-79 weeks | -0.38 | 258 |
| Some graduate school | 0.79 | 194 | 80-89 weeks | -0.07 | 412 |
| Graduate school | -0.94* | 362 | 90-99 weeks | -2.63*** | 219 |
| *Number of children* | | | 100 weeks or more | -1.10 | 50 |
| 0 | -1.13** | 751 | *Credit card debt at time of entry survey* | | |
| 1 | -0.92** | 489 | Less than $1,000 | -0.92** | 617 |
| 2 | -0.85** | 785 | $1,000-$2,499 | -0.99* | 293 |
| 3 | -2.24*** | 311 | $2,500-$9,999 | -0.76 | 391 |
| 4 | 0.92 | 92 | $10,000-$19,999 | -1.12* | 230 |
| 5 | -2.78 | 35 | $20,000 or more | -1.12 | 223 |
| *2008 Household income* | | | *Savings at time of entry survey* | | |
| Less than $10,000 | -1.35* | 116 | Less than $10,000 | -0.51 | 794 |
| $10,000-$19,999 | -1.47 | 163 | $10,000-$24,999 | -1.11* | 230 |
| $20,000-$29,999 | -0.79 | 252 | $25,000-$49,999 | 0.68 | 157 |
| $30,000-$39,999 | -1.02 | 228 | $50,000-$99,999 | -1.43* | 127 |
| $40,000-$49,999 | -2.08*** | 231 | $100,000 or more | -0.62 | 186 |
| $50,000-$59,999 | -0.89 | 180 | *Spousal work status* | | |
| $60,000-$69,999 | -.41 | 158 | Spouse works | -1.13*** | 1,076 |
| $70,000-$79,999 | -2.17*** | 162 | Spouse does not work | -0.08 | 365 |
| $80,000-$89,999 | -1.13 | 163 | *Baseline bias* | | |
| $90,000-$99,999 | 0.30 | 158 | Baseline optimists | -0.91** | 699 |
| $100,000-$149,999 | -1.59*** | 400 | Baseline pessimists | 0.21 | 37 |
| $150,000+ | 0.21 | 274 | *Control bias* | | |
| *Home purchase price* | | | Control optimists | -0.93** | 357 |
| Less than $25,000 | -1.32 | 28 | Control pessimists | -1.30** | 360 |
| $25,000-$49,999 | -1.36 | 64 | *Reservation Wage (average)* | | |
| $50,000-$74,999 | -0.41 | 90 | Less than $20k | -0.28 | 112 |
| $75,000-$99,999 | -1.52 | 109 | $20k-$30k | -1.39** | 437 |
| $100,000-$149,999 | -0.34 | 225 | $30k-$40k | -0.87* | 560 |
| $150,000-$199,999 | -1.87*** | 221 | $40k-$50k | -0.69 | 402 |
| $200,000-$299,999 | -1.58*** | 292 | $50k-$60k | -1.63** | 318 |
| $300,000-$399,999 | -0.11 | 162 | $60k-$70k | -1.15* | 192 |
| $400,000-$499,999 | 0.36 | 77 | $70k-$80k | -0.18 | 137 |
| $500,000-$599,999 | -4.04** | 32 | $80k-$90k | -0.49 | 95 |
| $600,000 or more | -1.17 | 44 | $90k-$100k | 1.03 | 55 |
| *Gender* | | | $100k-$150k | -0.84 | 131 |
| Male | -1.15*** | 1,154 | $150k or more | -2.76 | 46 |
| Female | -0.87*** | 1,385 | | | |

*Notes:* The event date is November 6, 2009, the extension of UI benefits. Each row shows the mean difference in total search hours reported by the same respondent in their last survey before and first survey after the extension. T-tests are computed to test whether the mean search effort is different before and after the UI extension. Significance levels: * 10%, ** 5%, *** 1%.

extension across respondents within the window is higher than the mean reservation wage among respondents within an equal sized window prior to the extension. Table 3.10 reports the results for the t-tests for windows spanning from 1-24 days. Following a period of adjustment, there is a strong increase in the average reservation wage. Roughly one and a half weeks after the UI extension, reported reservation wages had risen by $1,500-$2,500. Looking at these gains by worker characteristic, the rise is reservation wages is driven by young, unmarried workers who are college educated. As shown in Table 3.11, this is pattern is heavily influenced by the respondents reporting hourly wages.

Table 3.10: Reservation Wage Means Test: Independent Sample

| Days from | Before Nov. 6 | | | After Nov. 6 | | | |
|---|---|---|---|---|---|---|---|
| event date | Mean | Std. Dev. | Obs. | Mean | Std. Dev. | Obs. | Difference |
| 1 | $53,110.72 | 32,986.02 | 293 | $46,647.78 | 27,684.13 | 198 | -$6,462.94*** |
| 2 | $50,329.05 | 33,621.36 | 588 | $50,059.56 | 36,471.37 | 307 | -$269.49 |
| 3 | $50,501.60 | 34,518.29 | 1,189 | $51,004.96 | 36,644.97 | 417 | $503.36 |
| 4 | $50,505.92 | 34,176.48 | 1,355 | $50,712.83 | 33,476.19 | 939 | $206.91 |
| 5 | $50,180.98 | 33,486.65 | 1,493 | $50,627.32 | 33,080.07 | 1,211 | $446.34 |
| 6 | $50,424.85 | 34,839.82 | 1,712 | $50,510.03 | 32,017.12 | 1,490 | $85.18 |
| 7 | $50,898.13 | 35,020.13 | 2,082 | $51,542.08 | 34,510.69 | 1,807 | $643.95 |
| 8 | $50,880.47 | 35,407.31 | 2,386 | $51,289.52 | 34,398.37 | 2,009 | $409.05 |
| 9 | $50,217.82 | 34,362.60 | 2,771 | $50,994.52 | 34,066.78 | 2,116 | $776.71 |
| 10 | $49,958.95 | 34,014.22 | 3,461 | $51,149.98 | 34,019.89 | 2,255 | $1,191.03* |
| 11 | $49,810.51 | 34,215.25 | 3,780 | $51,328.40 | 33,635.55 | 2,682 | $1,517.89** |
| 12 | $49,898.74 | 34,634.51 | 3,932 | $51,289.69 | 33,433.98 | 2,943 | $1,390.95** |
| 13 | $49,861.65 | 34,471.43 | 4,241 | $51,124.60 | 32,968.75 | 3,166 | $1,262.94* |
| 14 | $49,861.90 | 34,209.92 | 4,670 | $51,305.38 | 33,369.79 | 3,450 | $1,443.48** |
| 15 | $50,035.56 | 34,455.22 | 5,072 | $51,429.45 | 33,830.75 | 3,609 | $1,393.89** |
| 16 | $49,761.08 | 34,112.22 | 5,467 | $51,387.02 | 33,862.45 | 3,699 | $1,625.94** |
| 17 | $49,529.13 | 34,077.35 | 6,007 | $51,353.10 | 33,830.44 | 3,805 | $1,823.97*** |
| 18 | $49,073.77 | 33,712.96 | 6,453 | $51,520.24 | 34,051.98 | 4,179 | $2,446.47*** |
| 19 | $49,076.83 | 33,725.35 | 6,708 | $51,446.21 | 33,860.22 | 4,396 | $2,369.37*** |
| 20 | $49,006.50 | 33,751.65 | 7,141 | $51,431.54 | 33,685.48 | 4,498 | $2,425.05*** |
| 21 | $49,104.94 | 33,448.06 | 7,653 | $51,487.52 | 33,794.42 | 4,758 | $2,382.58*** |
| 22 | $49,206.48 | 33,176.62 | 8,014 | $51,319.61 | 33,570.42 | 4,954 | $2,113.14*** |
| 23 | $49,318.99 | 33,242.37 | 8,307 | $51,373.26 | 33,560.03 | 5,085 | $2,054.27*** |
| 24 | $49,407.51 | 33,240.88 | 8,599 | $51,375.56 | 33,466.11 | 5,223 | $1,968.05*** |

*Notes:* The event date is November 6, 2009, the extension of UI benefits. Each row shows the mean reservation wage reported within the specified window before and after November 6. T-tests are computed to test whether the mean reservation wage is different before and after the UI extension, assuming the responses before and after the extension are independent and the two samples have different variances (as evidenced by the different standard deviations). Significance levels: * 10%, ** 5%, *** 1%.

The search effort results in Table 3.12 show outcomes from one-sided t-tests eval-

Table 3.11: Reservation Wage Means Test: Independent Sample, by Reservation Wage Reporting Method

| Days from event date | Full Sample | Reported Reservation Wages Frequency | | | |
|---|---|---|---|---|---|
| | | Annual | Monthly | Weekly | Hourly |
| 1 | -$6,462.94*** | -$8,159.11** | | -$8,710.00** | $1,505.02 |
| 2 | -$269.49 | -$615.46 | -$11,700.00 | -$3,298.45 | $1,523.41 |
| 3 | $503.36 | $1,331.21 | $24,120.00 | -$2,857.94 | $505.05 |
| 4 | $206.91 | -$539.53 | -$2,909.09 | -$2,065.07 | $725.68 |
| 5 | $446.34 | -$31.21 | -$9,243.96 | $391.36 | $510.06 |
| 6 | $85.18 | -$888.49 | -$2,500.00 | $808.22 | $501.88 |
| 7 | $643.95 | $360.42 | -$2,940.00 | -$1,179.29 | $980.82 |
| 8 | $409.05 | -$297.21 | -$1,400.00 | -$415.73 | $1,260.39* |
| 9 | $776.71 | -$160.67 | $1,104.35 | $602.71 | $1,344.53* |
| 10 | $1,191.03* | $434.77 | $4,814.12 | $1,617.87 | $1,464.12** |
| 11 | $1,517.89** | $351.10 | $7,031.66 | $1,419.08 | $1,692.58** |
| 12 | $1,390.95** | $334.27 | -$4,263.16 | $2,274.60 | $1,511.96** |
| 13 | $1,262.94* | $316.61 | -$5,809.76 | $491.75 | $1,284.23** |
| 14 | $1,443.48** | $808.15 | -$6,507.94 | -$267.90 | $1,217.33** |
| 15 | $1,393.89** | $572.19 | -$1,322.22 | $148.33 | $1,376.97** |
| 16 | $1,625.94** | $658.84 | -$893.63 | $101.83 | $1,626.45*** |
| 17 | $1,823.97*** | $863.72 | $90.00 | $1,502.92 | $1,866.88*** |
| 18 | $2,446.47*** | $1,360.70* | $2,167.62 | $1,032.22 | $2,058.77*** |
| 19 | $2,369.37*** | $1,185.29 | -$338.31 | $1,205.04 | $2,021.69*** |
| 20 | $2,425.05*** | $1,347.39* | -$4,997.75 | $923.88 | $2,100.37*** |
| 21 | $2,382.58*** | $1,340.25* | -$4,892.31 | $305.47 | $2,176.34*** |
| 22 | $2,113.14*** | $1,187.61* | -$2,953.85 | -$202.46 | $1,924.03*** |
| 23 | $2,054.27*** | $1,058.26 | -$2,420.02 | -$490.19 | $1,998.61*** |
| 24 | $1,968.05*** | $1,003.20 | -$906.10 | -$385.00 | $1,967.43*** |

*Notes:* The event date is November 6, 2009, the extension of UI benefits. Each row shows the difference in mean reservation wages reported within the specified window before and after November 6. T-tests are computed to test whether the mean reservation wage is different before and after the UI extension, assuming the responses before and after the extension are independent and the two samples have different variances (as evidenced by the different standard deviations). Significance levels: * 10%, ** 5%, *** 1%.

uating whether the mean level of search effort is lower following the UI extension. As in Table 3.10, the results are reported using a variety of event windows. Here, the theoretical prediction is clearly matched, as search effort falls significantly following the UI extension. Among different subsamples, this effect is most prevalent among younger and married cohorts who are college educated. Workers who have been unemployed for shorter periods of time and with more savings reported larger reductions in search effort, as well.

A major finding in Lindner (2018) was that workers who overestimated the marginal return to search effort ultimately spent more time searching for a job. In this hypothesis testing framework, this result is reinforced. Workers who underestimate the marginal return to search effort respond more strongly to the UI extension, perhaps because they started at a higher level of search effort.

Table 3.12: Search Effort Means Test: Independent Sample

| Days from | Before Nov. 6 | | | After Nov. 6 | | | |
|---|---|---|---|---|---|---|---|
| event date | Mean | Std. Dev. | Obs. | Mean | Std. Dev. | Obs. | Difference |
| 1 | 15.63 | 15.44 | 298 | 14.32 | 15.43 | 203 | -1.30 |
| 2 | 15.93 | 15.56 | 604 | 14.46 | 15.41 | 312 | -1.47* |
| 3 | 16.51 | 16.09 | 1,214 | 14.83 | 15.25 | 455 | -1.68** |
| 4 | 16.53 | 16.15 | 1,383 | 15.52 | 15.91 | 886 | -1.01* |
| 5 | 16.49 | 16.19 | 1,521 | 15.86 | 16.19 | 1,150 | -0.63 |
| 6 | 16.35 | 16.04 | 1,743 | 15.83 | 16.26 | 1,375 | -0.52 |
| 7 | 16.41 | 16.07 | 2,116 | 15.59 | 16.03 | 1,660 | -0.83* |
| 8 | 16.52 | 16.07 | 2,425 | 15.63 | 16.07 | 1,820 | -0.89** |
| 9 | 16.46 | 16.07 | 2,818 | 15.56 | 16.04 | 1,911 | -0.89** |
| 10 | 16.47 | 16.05 | 3,514 | 15.39 | 15.94 | 2,020 | -1.09*** |
| 11 | 16.45 | 16.07 | 3,840 | 15.46 | 16.03 | 2,399 | -0.99*** |
| 12 | 16.40 | 16.04 | 3,994 | 15.49 | 16.00 | 2,617 | -0.91** |
| 13 | 16.42 | 16.06 | 4,311 | 15.37 | 15.99 | 2,723 | -1.04*** |
| 14 | 16.40 | 16.12 | 4,745 | 15.37 | 16.07 | 2,986 | -1.03*** |
| 15 | 16.37 | 16.05 | 5,152 | 15.22 | 15.92 | 3,184 | -1.15*** |
| 16 | 16.37 | 16.08 | 5,555 | 15.18 | 15.90 | 3,319 | -1.19*** |
| 17 | 16.31 | 16.01 | 6,100 | 15.15 | 15.87 | 3,464 | -1.16*** |
| 18 | 16.28 | 16.02 | 6,558 | 15.04 | 15.90 | 3,821 | -1.24*** |
| 19 | 16.30 | 16.04 | 6,819 | 15.07 | 15.90 | 4,045 | -1.23*** |
| 20 | 16.28 | 15.99 | 7,261 | 15.06 | 15.91 | 4,203 | -1.23*** |
| 21 | 16.29 | 15.92 | 7,786 | 15.04 | 15.87 | 4,420 | -1.26*** |
| 22 | 16.38 | 15.95 | 8,153 | 15.01 | 15.85 | 4,573 | -1.37*** |
| 23 | 16.47 | 16.04 | 8,450 | 14.98 | 15.82 | 4,657 | -1.50*** |
| 24 | 16.51 | 16.08 | 8,748 | 14.95 | 15.83 | 4,739 | -1.56*** |

*Notes:* The event date is November 6, 2009, the extension of UI benefits. Each row shows the mean search hours reported within the specified window before and after November 6. T-tests are computed to test whether the mean search effort is different before and after the UI extension, assuming the responses before and after the extension are independent and the two samples have different variances (as evidenced by the different standard deviations). Significance levels: * 10%, ** 5%, *** 1%.

In periods over one week removed from the UI extension, the independent sample means tests show that reservation wages and search effort both responded in accordance with economic theory. As a robustness check, the event study analysis will account for the trend in reservation wages and search effort for each individual. Previous research has shown, for example, that search effort generally falls over an unemployment spell. Using the means test establishes that search effort has fallen in absolute terms. Incorporating the event study approach allows an evaluation of whether search effort, in response to the UI extension, has fallen more than expected.

The first step is to look at the individual-level abnormal reservation wages and search effort to see if any particular individuals have responded to the benefit extension. Within the different window lengths, I can find the individual's standard deviation of their abnormal outcomes[10]. Using these standard deviations, I can calculate a t-statistic to test how many individuals have statistically significant abnormal cumulative reservation wages or search effort. This corresponds to the gray areas in Figure 3.5.

The results of this analysis are reported in Table 3.13. The theoretical prediction is that the extension of UI benefits should lead to higher reservation wages, through the increased present discounted value of staying unemployed. Given the structure of this analysis, with the event period occurring *before* the UI extension, this would be **negative** deviations from the post-extension trend. In each size of event window, a larger fraction of workers have negative shocks to their reservation wages than positive shocks when the trend is estimated using 3 observations as a minimum. However, the fraction of those with significantly increased and decreased reservation wages are nearly equal[11]. For search effort, the theoretical prediction is for search hours to fall after the extension. Therefore, positive cumulative abnormal search effort fits with the theoretical prediction. That does not appear to be the case here.

A second way to analyze the event study results is to look at the cross-sectional averages. Table 3.14 reports the average cumulative abnormal reservation wages and search for different event window sizes. As described for individuals, because of the

[10]Here, I only use the 14, 21 and 24 day window lengths because I need enough observations within the window (minimum of 3) to calculate the standard deviation.

[11]This lends credence to an interpretation among some workers that extended UI benefits indicates a much weaker labor market, and therefore they should lower their reservation wage so as to escape unemployment more quickly. It is a form of learning.

Table 3.13: Cumulative Abnormal Outcomes, Within Respondent

| | Window Size | | | | | |
|---|---|---|---|---|---|---|
| | 14 Days | | 21 Days | | 24 Days | |
| | Count | Pct. of Total | Count | Pct. of Total | Count | Pct. of Total |
| *Normal res. wage predicted with 3 obs. or more* | | | | | | |
| Positive abnormal cum. res. wage | 10 | 11.2% | 64 | 10.6% | 69 | 9.2% |
| Negative abnormal cum. res. wage | 12 | 13.5% | 74 | 12.2% | 75 | 10.0% |
| **Total** | **89** | | **606** | | **752** | |
| *Normal res. wage predicted with 10 obs. or more* | | | | | | |
| Positive abnormal cum. res. wage | 3 | 20.0% | 24 | 16.9% | 23 | 13.7% |
| Negative abnormal cum. res. wage | 3 | 20.0% | 22 | 15.5% | 23 | 13.7% |
| **Total** | **15** | | **142** | | **168** | |
| *Normal effort predicted with 3 obs. or more* | | | | | | |
| Positive abnormal cum. effort | 15 | 11.0% | 49 | 5.9% | 58 | 5.9% |
| Negative abnormal cum. effort | 23 | 16.9% | 85 | 10.2% | 93 | 9.4% |
| **Total** | **136** | | **835** | | **985** | |
| *Normal effort predicted with 10 obs. or more* | | | | | | |
| Positive abnormal cum. effort | 3 | 11.1% | 6 | 3.2% | 12 | 5.5% |
| Negative abnormal cum. effort | 6 | 22.2% | 15 | 8.0% | 16 | 7.3% |
| **Total** | **27** | | **188** | | **219** | |

*Notes:* Normal reservation wages are predicted using results from a linear regression of reservation wages on unemployment duration for each individual, where linear regression is separately estimated for the sample of individuals with 3 or more reservation wage observations or 10 or more reservation wage observations. In order to test for significance, each respondent must have 3 or more reservation wage observations within the event window, from either Oct. 14, Oct. 17, or Oct. 24 to Nov. 6.

modified technique, theory predicts negative reservation wage outcomes and positive search effort outcomes. The results show positive average cumulative abnormal reservation wages, counter to the theoretical prediction, but most of them are not significantly different from zero. Search effort results are much stronger among the sample with 10 or more observations, clearly showing a large cumulative investment in search effort prior to the UI extension.

Averaging in the cross-section, a slightly noisier approach is to test if the mean abnormal reservation wage or search effort is significantly different from zero at different dates. The area of interest would be the dates leading up to the UI extension. Figure 3.6 shows the average abnormal reservation wage for different trend estimation samples. Generally, there is no consistent pattern of negative pre-extension reservation wages, as predicted by the model presented in this paper. Search effort results are in Figure 3.7. In the right panel, the mean abnormal search effort prior to the

Table 3.14: Cross-Sectional Cumulative Abnormal Outcomes

| | Event Window Size | | | | |
|---|---|---|---|---|---|
| | 3 Days | 7 Days | 14 Days | 21 Days | 24 Days |
| | Coeff. | Coeff. | Coeff. | Coeff. | Coeff. |
| *Normal res. wage predicted with 3 obs. or more* | | | | | |
| Average value | 622.93 | 742.08 | 1,138.01 | 1,304.66 | 1,196.73 |
| P-value | 0.20 | 0.13 | 0.08 | 0.19 | 0.24 |
| Observations | 1,164 | 1,494 | 1,729 | 1,828 | 1,852 |
| *Normal res. wage predicted with 10 obs. or more* | | | | | |
| Average value | 730.92 | 271.78 | 1,120.83 | 974.98 | 969.99 |
| P-value | 0.61 | 0.83 | 0.50 | 0.58 | 0.58 |
| Observations | 248 | 310 | 350 | 367 | 375 |
| *Normal effort predicted with 3 obs. or more* | | | | | |
| Average value | 0.69 | 0.23 | 0.33 | 0.03 | 0.09 |
| P-value | 0.06 | 0.57 | 0.60 | 0.97 | 0.93 |
| Observations | 1,112 | 1,413 | 1,628 | 1,720 | 1,741 |
| *Normal effort predicted with 10 obs. or more* | | | | | |
| Average value | 2.08 | 1.99 | 3.56 | 4.60 | 4.55 |
| P-value | 0.00 | 0.01 | 0.00 | 0.00 | 0.00 |
| Observations | 248 | 309 | 343 | 360 | 367 |

*Notes:* Normal reservation wages and search effort are predicted using results from a linear regression on unemployment duration for each individual, where linear regressions are separately estimated for the sample of individuals with 3 or more observations or 10 or more observations. All event windows span from between Oct. 14 and Nov. 3 to Nov. 6.

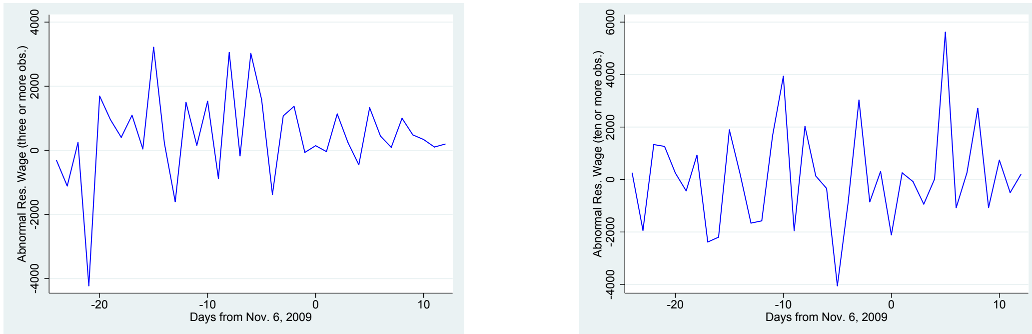
November 6 extension is well above zero for most of the period. This captures the idea that there were drops in search effort after the UI extension beyond what would be expected during an unemployment spell.

## 6 Conclusion

While there is great interest in studying the effects of unemployment insurance on the duration of unemployment, few current papers study the underlying worker behavior in response to the presence of UI. This paper takes a closer look at how UI benefits change the incentive for workers to enjoy leisure or be selective in their labor market outcomes. These both serve as inputs in determining unemployment duration.

This paper utilizes an extension in UI benefit duration to test how the reserva-

Figure 3.6: Abnormal Reservation Wages Around UI Extension

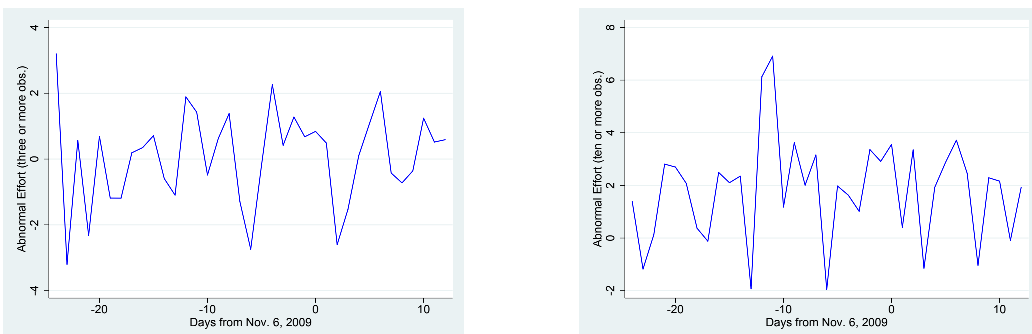



(a) 3 or more observations

(b) 10 or more observations

*Notes: Constructed using the sample of respondents who reported at least 3 or 10 reservation wages in the post-extension survey period. Time 0 represents the UI benefit extension date of November 6, 2009. Each point is the average abnormal reservation wage for each date. Source: Office of Population Research at Princeton University and author's calculations.*

Figure 3.7: Abnormal Search Effort Around UI Extension




(a) 3 or more observations

(b) 10 or more observations

*Notes: Constructed using the sample of respondents who reported at least 3 or 10 search effort observations in the post-extension survey period. Time 0 represents the UI benefit extension date of November 6, 2009. Each point is the average abnormal search effort for each date. Source: Office of Population Research at Princeton University and author's calculations.*

tion wage and search effort of workers change following a lengthening of potential UI benefit duration. Even though economic theory predicts that more benefits should increase reservation wages and decrease search effort, the results presented here suggest that workers are more likely to adjust along the margin of search effort. One possible explanation for these results could be related to the accuracy of the data. Search effort is an easy value to report, as respondents can easily recall what they have already done. Reservation wages, however, require foresight and can be dictated by outside financial obligations. Further work using self-reported reservation wages may be useful in understanding what information is contained in this self-reported value.

It may be the case that reservation wages did not significantly increase within the survey respondents because UI extensions not only change the value of remaining unemployed, but also provide a negative signal about the labor market. This seems especially likely given the survey time period, which occurred during the Great Recession. Offsetting the increase in reservation wages predicted by the consumption-smoothing benefit of extended benefits could be a drop in reservation wages to increase the arrival of job offers following the revelation that the labor market is in worse condition than expected. In this sense, studying UI extensions could have additional value compared with studies using, for example, variation in state-level UI rules.

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