

## ABSTRACT

Title of dissertation:       ESSAYS ON THE EFFECTS OF  
                                      SOCIAL INSURANCE FOR DISABILITY

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This dissertation examines how social insurance, family support and work capacity enhance individuals' economic well-being following significant health and income shocks.

I first examine the extent to which the liquidity-enhancing effects of Worker's Compensation (WC) benefits outweigh the moral hazard costs. Analyzing administrative data from Oregon, I estimate a hazard model exploiting variation in the timing and size of a retroactive lump-sum WC payment to decompose the elasticity of claim duration with respect to benefits into the elasticity with respect to an increase in cash on hand, and a decrease in the opportunity cost of missing work. I find that the liquidity effect accounts for 60 to 65 percent of the increase in claim duration among lower-wage workers, but less than half of the increase for higher earners. Using the framework from Chetty (2008), I conclude that the insurance value of WC exceeds the distortionary cost, and increasing the benefit level could increase social welfare.

Next, I investigate how government-provided disability insurance (DI) interacts with private transfers to disabled individuals from their grown children. Using the Health and Retirement Study, I estimate a fixed effects, difference in differences regression to compare transfers between DI recipients and two control groups: rejected applicants and a reweighted sample of disabled non-applicants. I find that DI reduces the probability of receiving a transfer by no more than 3 percentage points, or 10 percent. Additional analysis reveals that DI could increase the probability of receiving a transfer in cases where children had limited prior information about the disability, suggesting that DI could send a welfare-improving information signal.

Finally, Zachary Morris and I examine how a functional assessment could complement medical evaluations in determining eligibility for disability benefits and in targeting return to work interventions. We analyze claimants' self-reported functional capacity in a survey of current DI beneficiaries to estimate the share of disability claimants able to do work-related activity. We estimate that 13 percent of current DI beneficiaries are capable of work-related activity. Furthermore, other characteristics of these higher-functioning beneficiaries are positively correlated with employment, making them an appropriate target for return to work interventions.

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DISABILITY

by

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## Dedication

For Mom and Dad, who continue to teach me what matters most;

and for Brian, who is always by my side.

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## Chapter 1: Introduction

Disability is a multi-faceted health and income shock. Disability can render an individual unable to earn income, and often creates uncertainty about whether the individual will be able to return to the work that he or she used to do. In light of this large shock and uncertainty, disabled individuals rely on a patchwork of assistance from state and federal government programs, savings, family and friends, and their own, if limited, capacity to work. While these channels of support provide important assistance, they also interact with each other. The incentives imbedded in these support mechanisms could lead to unintended consequences. As with any public program, the optimal design of public assistance for the disabled must weigh the social benefits against the social costs, balancing improved beneficiary outcomes against costly changes in behavior, the costs of screening, and potential spillovers on other programs and other agents.

This dissertation analyzes the incentives in different sources of support for the disabled, and discusses potential interactions between them. Each chapter emphasizes a separate theoretical consideration in the design of a social insurance program. The first chapter examines the extent to which the liquidity-enhancing benefits of social insurance outweigh the moral hazard costs in the context of Workers' Com-

pensation (WC), a state-based program for short-term on-the-job disabilities. The second chapter examines the extent to which Social Security Disability Insurance (DI) crowds out informal assistance from the family, and examines whether transfers provide the family with additional information about the disability. The final chapter assesses the targeting efficiency of the current disability determination process and proposes a new screening mechanism to identify claimants who could benefit from return to work interventions. Each chapter provides perspective on the financial consequences of disability, and examines how public benefits enhance individuals' economic well-being following the shock of a significant negative health event.

While social insurance for disability provides claimants with needed income that allows them to smooth their consumption, the benefits also create deadweight loss by distorting claimants' incentives to work. A rich economic literature on the labor supply effects of disability benefits confirms that receipt of benefits reduces work activity (e.g., [Autor and Duggan 2003](#); [Bound 1989](#); [Chen and van der Klaauw 2008](#); [French and Song 2014](#); [Gruber 2000](#); [Maestas \*et al.\* 2013](#); [Von Wachter \*et al.\* 2011](#)). However, the financial consequences of disability have been less emphasized in most of this research. In recent years, approximately \$140 billion has been spent annually on DI benefits ([OASDI Board of Trustees 2015](#)), and approximately \$60 billion has been spent annually on WC benefits ([National Academy of Social Insurance 2014](#)). Still, [Meyer and Mok \(2013\)](#) finds that individuals with a chronic disability suffer a 24 percent decline in consumption ten years after the onset of a chronic disability, and that public programs and family support only partially offset

this shock. [Bronchetti \(2012\)](#) also finds that consumption could fall by as much as 30 percent for individuals who have experienced a workplace disability. The findings in these papers suggest that disability benefits could provide important insurance value. As is well established in the public economics literature, the optimal level of benefits depends not only on the social costs arising from the disincentives to work, but also on this insurance value of benefits ([Baily 1978](#); [Chetty 2006, 2008](#)).

In the first chapter, I examine this tradeoff in the context of the WC program in Oregon. Analyzing administrative claims data, I estimate a discrete proportional hazard model exploiting variation in the timing and size of a retroactive lump-sum WC payment to decompose the elasticity of claim duration with respect to benefits into two main channels: an increase in cash on hand (a liquidity effect) and a decrease in the opportunity cost of an absence work (a moral hazard effect). Typically, social insurance benefits provide claimants with cash on hand, but at the same time, they lower the claimant's net wage and reduce the incentive to return to work. However, a payment that is made regardless of when claimants return to work, such as with the retroactive payment in WC, separately identifies the liquidity effect. Under the assumption that claimants have maximized their private welfare, the elasticity of claim duration with respect to liquidity and moral hazard are sufficient statistics to determine the effect of a local change in benefits on social welfare ([Chetty 2008](#)).

The retroactive payment is a common feature at the beginning of the WC claim in nearly all states. In Oregon, claimants are paid a small lump sum if their claim lasts longer than two weeks. This means that claimants first have an incentive

to extend their claim, and, conditional on remaining out of work for the first two weeks, later receive additional cash regardless of when they return to work. If WC claimants extend their claims after receiving the retroactive payment, this implies that the additional income affords them more time to recover, moving them closer to the claim duration they would choose in a world without liquidity constraints that force them to return to work prematurely. I take advantage of the specific timing of the retroactive payment to isolate this liquidity effect.

With the hazard model, I examine changes in the rate of exit from WC before and after eligibility for the retroactive payment to decompose the elasticity of claim duration with respect benefits into the elasticity with respect to a change in moral hazard and liquidity. Among claimants with pre-injury wages below the median wage in Oregon (i.e., claimants earning less than \$700 per week), I find that the liquidity effect accounts for 60 to 65 percent of the increase in claim duration. By contrast, I find that the liquidity effect accounts for less than half of the increase in claim duration for higher-wage workers. These estimates suggest that WC plays an important role in relaxing liquidity constraints for all WC claimants. However, higher-wage workers may have alternative forms of insurance (e.g., savings) to help smooth their consumption during temporary spells away from work, resulting in a smaller liquidity effect.

By observing how the retroactive payment affects behavior during the first few weeks of the WC claim, I demonstrate that claimants are sensitive to changes in their income even after short spells away from work. This sensitivity is additional evidence that liquidity constraints could be an important consideration for

the population of temporarily disabled workers. Applying my liquidity and moral hazard elasticities to the optimal benefit formula from [Chetty \(2008\)](#), I conclude that increasing benefits could increase overall social welfare, particularly for lower-wage workers. Under the key assumption that private utility is at the optimum, the local change in benefits does not have a first-order effect on other inputs that are endogenous to policy changes. While the estimated elasticities are informative for marginal welfare effects, the results cannot be extrapolated beyond local policy changes due to this assumption.

The first chapter uses a revealed preference approach to analyze the value of public benefits for temporary disabilities. The presence of a liquidity effect indicates that public insurance enhances claimants' ability to smooth their consumption beyond what they could achieve on their own ([Chetty 2008](#)). The insurance value of benefits thus depends on claimants' ability to self-insure after a negative event, whether through personal savings, private insurance, or informal assistance from family and friends. The insurance value of permanent disability benefits relies on claimants' ability to self-insure over a longer time horizon. In the second chapter I provide a deeper analysis of one of these self-insurance mechanisms: assistance from the family. I examine interactions between DI and informal transfers from the family and ask whether public insurance crowds out informal private insurance for the permanently disabled.

The degree of crowd out describes the extent to which family support covers the income shock, and characterizes public insurance's role in increasing overall coverage. Typically, crowd out implies that public insurance is a less efficient way to insure the

population due to the cost of raising public funds, and the optimal level of benefits is lower with crowd out than without crowd out (Chetty and Saez 2010; Gruber 2013). However, families also experience the shock of a disability, meaning the disability could result in economic consequences for family members as well as the disabled individual. As a result, spillovers to the family are an important consideration when analyzing the effects of DI on social welfare. A small body of research examines the effect of unemployment insurance on monetary family transfers (Schoeni 2002), and other research examines interactions between government-provided insurance and in-kind transfers from the family (e.g., Engelhardt *et al.* 2005; Orsini 2010; Stabile *et al.* 2006). However, no research has examined the casual relationship between either type of transfer and DI. Disability is unique in that it results in a health and an income shock. As a result, families could serve as a substitute or complement for both types of support.

Using a fixed effects, difference in differences research design, I examine transfers from grown children to their disabled, aging parents. I use panel data from the Health and Retirement Study (HRS), which allows me to control for time-invariant factors affecting transfers between families. In order to identify the effect of DI on transfers, I compare monetary and in-kind transfers before and after the onset of the disability for DI recipients and two control groups: rejected applicants and disabled individuals who do not apply for DI. I find that while the probability of receiving a monetary transfer increases slowly after the onset of the disability and peaks around the time of DI receipt, the probability of receiving an in-kind transfer increases sharply following the onset of the disability and persists following DI

receipt. After including time-varying controls and an individual-level fixed effect, the confidence intervals on my estimates allow me to reject that DI reduces the probability of receiving a transfer by more than 3 percentage points. I additionally find that DI could increase the probability of receiving a transfer by up to 5-7 percentage points. These estimates, combined with estimates on the intensive margin, suggest that crowd out of family transfers in response to DI is lower than crowd out in response to other social insurance programs.

Additionally, I find that receipt of DI significantly increases the probability of a transfer to claimants with less observable disabilities such as arthritis or back pain. In these cases, the family likely had incomplete information about the disability prior to DI receipt. With perfect ex-ante information about the disability and likelihood of receiving DI, families could perfectly anticipate the disabled individual's need and would not change their transfer decisions when the individual receives DI. However, DI could help solve the problem of imperfect information by signaling the severity of the disability. As a result, families may adjust their transfer behavior after learning about DI receipt. The family's response to this information could imply a higher optimal level of benefits compared to a world where the family has perfect information about the disability.

Of course, disabilities are not perfectly observable to the government, either. In practice, the government relies on a lengthy process to determine who is categorically eligible for benefits. On one hand, the "tag" of a disability allows the government to transfer a larger benefit to a smaller group of eligible individuals ([Akerlof 1978](#)). On the other hand, any screening evaluation to determine eligibility will inevitably lead

either to admitting claimants who do not meet the eligibility requirement, excluding claimants who truly are eligible for the benefit, or both (Diamond and Sheshinski 1995, Kleven and Kopczuk 2011). Ultimately, the success of a social insurance program for disability relies on minimizing these types of errors.

There is also considerable ambiguity in disability application decisions, meaning there is scope to improve the screening process. In recent years, over 30 percent of applicants have been initially rejected from benefits, but later accepted after an appeal process that often lasts several years (Benitez-Silva *et al.* 1999; Office of the Inspector General 2008; Social Security Administration 2015). Application reviewers also have varying propensities to accept applicants onto DI, and many applicants are on the margin of being accepted at the initial application stage. Maestas *et al.* (2013) finds that approximately 23 percent of applications could have had a different initial outcome had they been assigned to a different reviewer during the first round of review. Furthermore, French and Song (2014) documents considerable variability in administrative law judge decisions at the appeal stage. In an audit study of the accuracy of the disability decision, Benitez-Silva *et al.* (2006) analyzes self-reported disability status in the HRS and estimates that approximately 20 percent of accepted disability applicants should have been denied, and 60 percent of denied disability applicants should have been accepted.

In the third chapter, Zachary Morris and I analyze the targeting efficiency of the determination process for DI and Supplemental Security Income (SSI; together, SSD) benefits. The Social Security Administration (SSA) classifies an individual as disabled if they are “unable to engage in any substantial gainful activity (SGA)

because of a medically-determinable physical or mental impairment(s) that is expected to result in death or to last for a continuous period of at least 12 months,” ([Social Security Administration 2015](#)). The verification process to receive disability is thus premised on two major assumptions: (a) that to be disabled means to be completely unable to work, and (b) that inability to work can be determined medically. We study claimants’ functioning based on self-reported survey data to provide a new perspective on these criteria. We analyze the extent to which the current “tag” of disability results in benefits going to claimants who retain capacity for work. We also discuss how an analysis of functional capacity could target return to work interventions to claimants who may be able to transition back into the labor force.

In order to identify work capacity, we analyze self-reported data on functioning from survey questions in the National Beneficiary Survey (NBS), a nationally representative survey of SSD beneficiaries in the United States. We match questions in the NBS to questions used in a functional assessment in the United Kingdom that evaluates functional capacity to target return to work interventions. We estimate that 13 percent of US beneficiaries would be classified as capable of work-related activity based on the UK target threshold. At the time of the survey, this group, whom we call the “higher-functioning” group, is more than twice as likely to be working (at levels below the SGA threshold) as lower-functioning DI beneficiaries. Higher-functioning beneficiaries are also younger and have more education, on average. These characteristics suggest that this subgroup of claimants likely has a

higher potential to work than the average beneficiary and may be an appropriate target group for return to work interventions.

This dissertation focuses on several sub-populations of the disabled, and as a result, the findings in one chapter do not necessarily generalize to the subpopulations analyzed in other chapters. For example, claimants who receive WC are typically younger and more likely to experience physical impairments and partial impairments than the population of DI beneficiaries. Additionally, the majority of WC claimants are typically absent from work for several weeks, while DI beneficiaries stop working permanently. DI and WC claimants thus respond to changes in benefits on different margins: while most WC claimants make a decision about whether to begin or extend a temporary stay out of work, most DI claimants make a decision about whether to exit the labor force completely.

The permanent nature of the shock to DI beneficiaries also suggests that DI could provide even larger consumption smoothing gains than WC benefits, although this should be verified with future research. Additionally, a minority of WC claimants do face permanent impairments. While permanently disabled WC claimants do not respond to the payment I analyze here, I plan to analyze these claimants' responsiveness to a change in permanent WC benefits in future work. This analysis will be more informative about how changes in benefits could affect the decisions faced by the DI population.

Higher-functioning DI beneficiaries, who are the focus of the final chapter, likely fall somewhere in between these two extremes. While they participate in the permanent DI program, they tend to be younger, better educated, and more likely

to be currently working, characteristics that suggest they could increase their participation in the labor force. This relatively under-studied group of DI beneficiaries could provide important lessons about the desired structure of disability benefits.

DI participation has also been growing among younger adults who enter the program with more marginal, non-life threatening disabilities and continue to receive benefits throughout adulthood (Ben-Shalom and Stapleton 2015). This growing group of beneficiaries has led to an increased policy discussion acknowledging that return-to-work initiatives or a partial disability benefit could stem this growth (Autor and Duggan 2010; Burkhauser *et al.* 2014; Liebman and Smalligan 2013). Information on claimant functioning could identify the beneficiaries who would benefit most from any proposed interventions.

Furthermore, many disabilities evolve over time, posing further challenges to characterizing claimants as “disabled” or “not”. For example, Moore (2015) analyzes a policy change that removed claimants with a primary diagnosis of a drug or alcohol addiction from DI, and finds that claimants who had received benefits for 2-3 years had higher rates of later employment than other claimants who were on benefits for shorter or longer periods before being removed from the program. This suggests that temporary receipt of DI could have a rehabilitative effect for these claimants. Von Wachter *et al.* (2011) and Mann *et al.* (2015) also demonstrate that there is a wide spectrum of work capacity within the DI beneficiary population. The interest and potential capacity for work among current beneficiaries provides further suggestion that it could be socially beneficial to introduce a temporary or partial benefit for some subset of disability claimants. Since WC is one of few existing

programs that provide assistance for short-term disabilities, it is a fruitful setting to analyze claimants' sensitivity to changes in temporary benefits.

In the chapters that follow, I elaborate on these theoretical and empirical considerations. Each chapter draws upon broader themes that are important in designing a social insurance program. It also analyzes the ways that the shock of a disability spreads beyond the individual, and seeks to account for these interactions in an analysis of the social welfare consequences of disability and disability benefits. At the same time, these papers provide a detailed view of the current circumstances of families and individuals who experience a disability in the United States. The group of individuals who fall under the label of "disabled" is in fact quite heterogeneous, which highlights both the challenges and the opportunities in designing a better public support system.

## Chapter 2: Buying Time: The Insurance Value and Distortionary Effects of Workers' Compensation

### 2.1 Introduction

Social insurance programs are designed to provide protection for individuals against losses in consumption owing to some unanticipated negative shock, such as unemployment, disability onset, or injury on the job. If individuals cannot fully insure against an unexpected health or income shock through private insurance or other alternatives, public social insurance programs provide claimants with needed cash (liquidity) during a time when they cannot earn a wage. However, the payments from such a program also lower the opportunity cost of missing work, and thereby have a distortionary “moral hazard” effect. As is well-recognized in the public finance literature, the optimal design of social insurance depends critically on balancing the welfare gains of providing additional liquidity against the welfare costs of unintended distortions in claimant behavior.

There is a growing body of research estimating the benefits of social insurance programs, in particular for the unemployment insurance (UI) program. These studies consistently find evidence that UI provides considerable insurance value to un-

employed workers (e.g., [Card \*et al.\* 2007](#); [Chetty 2008](#); [Gruber 1997](#); [LaLumia 2013](#); [Schmieder \*et al.\* 2012](#)).<sup>1</sup> [Bronchetti \(2012\)](#) investigates the consumption smoothing benefits of the Workers Compensation program for older workers. Taking advantage of within-state variation in benefit levels, Bronchetti estimates that a 10 percent increase in benefits would offset approximately 3-5 percent of the consumption loss following an on-the-job injury.

My study builds on these literatures with an examination of the liquidity-enhancing benefits and moral hazard costs in the context of Worker's Compensation (WC). Analyzing administrative data from Oregon, I estimate a discrete proportional hazard model exploiting variation in the timing and size of a retroactive lump-sum WC payment to decompose the elasticity of claim duration with respect to benefits into two components: the elasticity with respect to an increase in cash on hand (a liquidity effect) and a decrease in the opportunity cost of missing work (a moral hazard effect). Typically, UI or WC benefits provide claimants with cash on hand that allows them to stay out of work while maintaining a particular level of consumption. At the same time, they effectively lower the claimant's net wage, distorting the decision to return to work. However, a payment that is made regardless of when claimants return to work, such as with the retroactive payment in WC, separately identifies the liquidity effect. [Chetty \(2008\)](#) outlines this approach in the context of UI. If WC claimants extend their claims after receiving the retroactive payment, this implies that the additional income affords them more time to recover,

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<sup>1</sup>This complements a set of studies investigating the distortionary labor supply effects of the unemployment insurance program (see [Krueger and Meyer \(2002\)](#) for a review of that literature.) Those studies tend to find that higher levels of UI benefits lead to longer unemployment duration, but it is debated as to whether that increased duration is socially costly or beneficial.

and allows them to move closer to the claim duration they would choose in a world without liquidity constraints that force them to return to work prematurely. This is the approach I take to separately identify these two effects in the context of WC.

The WC program provides approximately \$60 billion annually to insure workers against the health and income shock of an illness or injury on the job ([National Academy of Social Insurance 2014](#)). Since the majority of WC claims occur in physical jobs, WC benefits could be essential in affording claimants sufficient recovery time to return to work successfully. On the other hand, injuries are often difficult to observe, and claimants typically return to the same job they had prior to their injury, so there is little uncertainty about future employment prospects. These factors could increase moral hazard costs relative to UI, or imply less need for the liquidity that WC provides. Many states have recently started reducing benefits and making it more difficult to qualify for WC, in order to lower costs ([Grabell and Berkes 2015](#)). However, there is little empirical evidence about the relative magnitude of the insurance value and distortionary costs to determine the welfare consequences of these reforms ([Meyer 2002](#)).

In order to disentangle these two effects, I take advantage of a small retroactive lump-sum payment to WC claimants in Oregon that separates the liquidity and moral hazard effects. As I explain in detail below, WC claimants are paid a small lump sum (equal to 25 percent of their weekly wage, on average) if their claim lasts longer than two weeks. This means that claimants first have an incentive to extend their claim, and later receive additional cash regardless of when they return to work. I estimate a discrete proportional hazard model and examine changes in the rate of

exit from WC before and after eligibility for the retroactive payment to decompose the elasticity of claim duration with respect benefits into the elasticity with respect to a change in moral hazard and liquidity. Among claimants with pre-injury wages below the median wage in Oregon (i.e., claimants earning less than \$700 per week), I obtain moral hazard and liquidity elasticities of .17 and .26, respectively, indicating that the liquidity effect is approximately 1.5 times as large as the moral hazard effect. In contrast, for high wage workers, I estimate a moral hazard and liquidity elasticities of approximately .22 and .19, respectively. These estimates suggest that WC plays an important role in providing cash on hand for all WC claimants, but higher-wage workers may be more likely to have alternative forms of insurance (e.g., savings) that help them smooth their consumption during temporary spells away from work, leading to a smaller liquidity effect.

By observing how the retroactive payment affects behavior during the first few weeks of the WC claim, I demonstrate that claimants are sensitive to changes in their income even after short spells away from work. This sensitivity is additional evidence that WC relaxes claimant liquidity constraints, affording claimants more time to recover from an injury or illness. Longer recoveries could additionally improve workers' long-term health, reduce the probability of re-injury on the job, or may increase adjustment costs when a worker returns. I carry out an additional analysis to explore this possibility using linked claims and wage data that I obtained from the state of Oregon. In general, the results do not provide strong evidence that claim length significantly affects post-injury outcomes for those claimants whose return to work decisions are influenced by the retroactive payment.

In the setting I examine, WC claimants face a three-consecutive day waiting period after their injury before they receive any cash benefits. If the injury lasts longer than two weeks, claimants are retroactively paid a lump sum equal to the benefits they would have received during the waiting period, effectively increasing their second bi-weekly WC check by 10 percent, on average. The retroactive payment only reimburses benefits for scheduled work days during the waiting period, meaning that identical claimants injured on different days of the week will have different sized retroactive payments. Under the assumption that injuries occur randomly across different days of the week and that existing levels of cash on hand are uncorrelated with the date of injury, this variation in the size of the claimant's retroactive payment identifies the liquidity and moral hazard effects. I assess the validity of these assumptions and find that the frequency and distribution of observable characteristics of claims in my sample are balanced across the date of injury. Additionally, I find that my baseline results are comparable to results for a subgroup of claimants who are most likely to have similar levels of cash on hand, regardless of their date of injury.

I obtained access to an original administrative dataset of WC claims from the Oregon Department of Business and Consumer Services for this study. The database contains rich information on cash benefit claims over more than twenty years and also includes detailed worker and injury characteristics that provide valuable information about other factors that would affect claim length. Additionally, I worked with the Department of Business and Consumer Services and the Oregon Employment Department to obtain a file of matched claims data to employment data. I use

these records to examine the effects of longer claims on post-injury outcomes. I supplement this administrative data with data from the National Compensation Survey, the Survey on Occupational Illness and Injury and the Current Employment Statistics Survey. I use additional statistics from these surveys in combination with my estimates of liquidity and moral hazard to analyze the welfare effects of a change in WC benefits, and to test my identifying assumptions.

I use the variation in the retroactive payment in Oregon to analyze how WC affects claimant behavior and well-being. The findings in this paper offer additional evidence that social insurance provides lower-income claimants with insurance value, relaxing their liquidity constraints. Under the assumption that claimants have maximized their private welfare, the elasticity of claim duration with respect to liquidity and moral hazard are sufficient statistics to determine the effect of a local change in social insurance benefits on social welfare (Chetty 2008, 2009). Applying my liquidity and moral hazard elasticities to the optimal benefit formula from Chetty (2008), I conclude that increasing benefits could increase overall social welfare, particularly for lower-wage workers.

## 2.2 Identification and Data

### 2.2.1 Identification Strategy

In order to separate the liquidity and moral hazard channels, I take advantage of a common feature of WC payments in all states that separates these effects. First, workers face a waiting period at the beginning of their WC claim. Benefits

are withheld for the first few days of the claim, and if the claim's duration exceeds a certain length, claimants are reimbursed for the withheld benefits in a lump sum. All state WC programs have a waiting period at the beginning of the claim, and in 46 states, claimants with claims exceeding a certain duration can receive a retroactive payment for this waiting period. The length of the waiting period and duration of the claim before claimants are eligible to receive the retroactive payment both vary across states (Tambe 2012).<sup>2</sup>

In Oregon, the setting for my analysis, workers have a three-consecutive day waiting period before they receive cash benefits. If the injury lasts longer than two weeks, they become eligible for a retroactive payment equal to the benefits they would have received during the waiting period.<sup>3</sup> WC checks are paid every two weeks relative to the injury date, and eligible claimants will receive the retroactive payment (RP) in their second WC check regardless of when they return to work. As a result, if claimants with larger RPs differentially lengthen their WC spells after they are eligible for the RP, this can be attributed solely to the effect of receiving additional income after a negative shock: the liquidity effect. Since claimants are not eligible for the RP during the first two weeks, any response to a change in the RP during the first two weeks of the claim can be attributed to the increased *incentive*

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<sup>2</sup> See [Information Technology and Research Section \(2012\)](#) for details on the general structure of WC payments in Oregon.

<sup>3</sup> Workers also are eligible for the retroactive payment if they are admitted to the hospital, regardless of how long their claim lasts. Unfortunately, the Oregon Worker's Compensation Division does not maintain data on hospitalizations; however, as long as hospitalizations are orthogonal to the date of injury, potential hospitalizations should not bias my analysis. Conversations with staff in the Oregon Worker's Compensation Division confirm that hospitalizations during the first two weeks of WC claims are infrequent. While statistics on the share of claimants admitted to the hospital are not available, inpatient hospital services only account for approximately 13 percent of total medical costs ([Information Technology and Research Section 2012](#)).

to lengthen claims in order to satisfy the eligibility condition for the RP. If workers cannot borrow against the future benefit, the response during the first two weeks represents a moral hazard effect (Chetty and Finkelstein 2013; Shavell and Weiss 1979).

I take advantage of variation in the RP to identify these two effects. As noted above, the waiting period in Oregon is three *consecutive* days from the beginning of the claim, including holidays, weekends, and unscheduled work days. Since the RP only reimburses benefits for scheduled work days during the waiting period, the date of the injury creates variation in the size of this one-time unconditional payment. As an example, consider a typical worker with a Monday to Friday work schedule. Figure 2.1 shows that for workers injured on a Friday, only one of the waiting period days occurs on a day he was scheduled to work, and the other two waiting period days fall on the weekend. As a result, the worker only has one day of benefits withheld and reimbursed as a lump sum in the RP. However, an identical worker injured on Wednesday or earlier would receive an RP equal to three times his daily benefit, since the entire waiting period falls during the workweek. Under the assumption that injuries occur randomly across different days of the week and that existing levels of cash on hand are uncorrelated with the date of injury, I use this variation in the size of the retroactive payment to estimate liquidity and moral hazard effects.

On average, eligible claimants receive \$100 to \$300 in a lump sum due to the RP. For comparison, the average WC claimant in my sample earns approximately \$650 per week, meaning the RP ranges between 15 and 45 percent of gross weekly

earnings. While the absolute value of this payment is small, it provides claimants with a lump sum that is large relative to their typical income stream, precisely at a point in time when they face reduced income due to their injury. In other words, the cash on hand effects could be substantial. The RP is most likely to affect claimants with some degree of liquidity constraint who are on the margin of staying out of work, rather than claimants with extremely severe or minor injuries. I examine heterogeneity in the effect of the RP across injury type and income level to test these hypotheses.

Since the date of the injury is the main source of variation in the size of the RP, I address several concerns that the results could be driven by other unobservable characteristics that are correlated with the day of the week. First of all, research has documented that a higher frequency of WC claims are filed on Mondays, suggesting the date of injury is not entirely random ([Card and McCall 1996](#)). I conduct my main analysis on claims occurring in the second half of the week, where the frequency and distribution of observable characteristics of claims is balanced. Secondly, variation in the day of the week of the injury could affect the size of the worker's final pre-injury paycheck, which could also affect consumption and claim duration decisions. I estimate liquidity and moral hazard effects on a subsample of workers whose final paycheck is less likely to be affected by the date of the injury and find a similar pattern of results as in my main estimates. I also reweight claims in my sample to address the fact that I estimate the liquidity effect on the select sample of claimants who remain out of work at least two weeks, and my results are broadly robust to

this correction. Finally, I find that the results are also robust to employers' use of return to work interventions.

## 2.2.2 Data and Summary Statistics

I analyze a rich administrative dataset from the Oregon Department of Consumer and Business Services, Worker's Compensation Division (ORWC) which contains information on closed claims for which cash benefits were paid between roughly 1974 and 2013 ([Oregon Department of Consumer and Business Services 2015](#)). The dataset includes detailed information needed to determine the length of the claim, including the date of injury, date of first and last timeloss payments, total workdays for which timeloss benefits were paid, and the number of days typically worked per week. It also contains information about the worker's pre-injury wage, total amount of timeloss payments, total amount of medical payments, age, gender, occupation and industry. Injury information is categorized with ICD-9 codes and includes the nature of the injury, the event causing the injury, and the body part(s) affected. I impute a worker's potential RP using the date of injury, the number of days worked per week, and the worker's pre-injury wage.

Additionally, the database contains several measures of post-injury outcomes for claims occurring after 1999. ORWC matched these more recent claims to closure reports containing information about the worker's employment immediately following their claim, including whether the worker was released to return to work, whether the worker returned to the same employer and/or the same job, and whether the

worker required modifications to his work activities. The data also includes a count of the number of times the claim was re-opened due to an aggravation of the injury. Finally, together with the Oregon Employment Department, ORWC matched claims to quarterly earnings records from 1999-2013, allowing me to observe changes in hours and wages before and after the injuries occurring within this time frame ([Oregon Employment Department 2015](#)). For all injuries occurring after 1999, I observe wages at least 2 quarters before, and 4 quarters after the event.

I make several restrictions to derive the sample used for this analysis. Because the RP likely will not affect claim decisions for workers with extremely severe injuries, I exclude workers receiving permanent benefits. I restrict my sample to years where the database contains the complete record of claims: between 1987 and 2012. I also restrict the sample to claims lasting at most one year and to cases where the claimant stopped working immediately after the injury. In order to impute the RP, I restrict the sample to injuries occurring on weekdays and to claimants reporting a five-day workweek. [Table 2.1](#) provides a complete list of all sample restrictions, and the appendix provides more information about the criteria used in making these restrictions. As shown in [appendix table 2.12](#), individuals excluded from the sample are older and have slightly higher wages. Additionally, the excluded observations also are more likely to have suffered severe injuries, such as fractures, and less likely to have suffered minor injuries like cuts or burns. These restrictions predominantly exclude claimants who are unlikely to be responsive to the RP.

[Table 2.2](#) shows the observable characteristics of claimants in the sample across days of the week. Over 70 percent of the sample is male, and the average age of

claimants is 36. Table 2.2 also shows that 60 percent of all injuries are muscle strains or sprains, approximately 10 percent are bone breaks or fractures, and an additional 20-24 percent of injuries are wounds (cuts or burns). The remaining share of injuries are traumatic injuries or other occupational illnesses and diseases (approximately 5 percent for each category). Nearly 65 percent of claimants worked in one of five industries prior to their injury: agriculture, construction, trade, transportation, or manufacturing. The mean weekly wage ranges between \$720-\$740; the median weekly wage ranges from \$630-\$650 in 2012 dollars.<sup>4</sup> On average, WC claimants earn a lower wage than the typical worker in Oregon: the median weekly wage in Oregon is approximately \$700 (Peniston 2014).

As a first test of my identifying assumption, I examine whether WC claimants are similar across different days of the week. First of all, figure 2.2 confirms that injuries, particularly among claims lasting less than two weeks, are more frequent on Monday and Tuesday. Additionally, table 2.2 shows that injuries occurring on Monday and Tuesday are slightly more likely to occur in the morning, and have a shorter average duration than claims on other days of the week. Relative to the second half of the week, a higher frequency of Monday and Tuesday injuries are muscle strains. Indeed, the p-values in column (6) confirm that although the differences are small in magnitude, observable characteristics of Monday claims are significantly different from Wednesday claims. These differences in the observable characteristics at the beginning of the week are consistent with the “Monday effect”

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<sup>4</sup> I inflate all monetary variables to 2012 dollars using the nominal growth rate in Oregon’s state average weekly wage.

documented in the literature ([Card and McCall 1996](#); [Hansen 2014](#); [Ruser 1998](#); [Smith 1989](#)). The Monday effect could occur if workers try to receive benefits for non-work related injuries occurring over the weekend or if workers are more careless on Mondays, perhaps due to fatigue. If workers are more likely to report false claims on Monday, these claims are likely less severe and would result in the shorter claims observed at the beginning of the week. The higher frequency of claims and shorter duration of injuries on Tuesday could result from this effect spilling over to Tuesdays after several Monday holidays throughout the year, or due to workers taking long weekends ([Smith 1989](#)).

As a result, I restrict the analysis to injuries occurring on Wednesday, Thursday and Friday, where the frequency of injuries is relatively stable. Because the weekend creates variation in the size of the RP, this restriction still allows me to identify claimant responses to the RP. The p-values in column (7) demonstrate that overall, the observable characteristics of workers are balanced between Wednesday and Thursday. Additionally, the composition of injuries and industries is similar between Wednesday, Thursday, and Friday. However, the p-values in column (8) show that there are significant differences in the weekly wage and medical costs for Friday injuries. I control for these observable differences in the analysis and test whether observable or unobservable differences in characteristics of Friday injuries affect the results by restricting the analysis to Wednesday and Thursday injuries in a robustness check. As shown in table [2.13](#), the estimates with this restriction are qualitatively similar.

Figure 2.3a shows the distribution of claim length in my sample of claims. The measure of duration is the number of workdays for which benefits were paid, so five days represents one work week. These figures reveal two important facts about the distribution of claims. First, there is a long and thin right tail to the distribution of claims: approximately 92 percent of claim durations in my sample are less than 40 work days, and 96 percent of durations are less than 60 work days. Additionally, figure 2.3a demonstrates a spike in the frequency of exits at five-day intervals (corresponding to work weeks). As shown in figure 2.7, this pattern is consistent across injuries on each day of the week, suggesting that the pattern is due to the weeks since the claim began, rather than the day of the week.

### 2.3 Distinguishing Liquidity from Moral Hazard

To show how liquidity and moral hazard can be separated conceptually, I draw upon frameworks for the optimal design of benefits from Chetty (2006, 2008) and Diamond and Sheshinski (1995) as well as a dynamic decision-making model from Manoli and Weber (2011), which describes how workers respond to the option value of receiving a future payment. Consider a WC claimant injured at the beginning of period  $t = 1$  who must decide whether or not to return to work during periods  $t \in \{1, 2, \dots, T\}$ , where each period represents a two-week interval since the injury. For each period in which the claimant remains out of work, he will receive a WC benefit  $b_t$ . If he returns to work in period  $t$ , he will earn a net wage  $w_t$ , but will experience disutility from working, measured by  $\alpha_t$ . This disutility of work

$\alpha_t$  represents a combination of the claimant's preference for leisure over work, as well as any additional disutility associated with working after an injury. Because workers are uncertain about how long their recovery will take, disutility of work is determined by  $\alpha_t = \delta\alpha_{t-1} + \epsilon_t$ , where  $\epsilon_t \sim F_t(\sigma_t)$  represents unexpected variation in the recovery process. Additionally, the worker has cash on hand  $A_t$ , and must decide how much to save for the next period,  $s_t \geq L$ , where  $L$  could be negative if the claimants is able to borrow.

At the beginning of period 1, the worker must decide whether to stay out of work or return to work in the current period, and must also consider the fact that remaining out of work during period 1 maintains the option to receive the RP during period 2. The claimant's value function of returning to work in period 1 can be written as

$$V_1 = \max_{s_1 \geq L} v(A_1 - s_1 + w_1) - \alpha_1 + \beta V_2(A_2),$$

where  $v(A_1 - s_1 + w_t) = v(c_1^e)$ , with  $v'(c_1^e) > 0, v''(c_1^e) < 0$ . If the claimant decides to return to work in period 1, he does not receive the RP, and I assume he remains at work in all subsequent periods.<sup>5</sup> The claimant's value function of choosing WC during period 1 can be written as:

$$U_1 = \max_{s_1 \geq L} u(A_1 - s_1 + b_1) + \beta J_2(A_2, RP),$$

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<sup>5</sup>Future versions of this framework could relax this assumption. Realistically, the claimant could face a risk of being injured again in the future, and this risk could be correlated with the length of his recovery time.

where  $u(A_1 - s_1 + b_1) = u(c_1^n)$  is also concave, and  $J_2(A_2, RP)$  represents the expected value of the claimant's decision in the next period:

$$J_2(A_2, RP) = E[\max\{U_2(A_2, RP), V_2(A_2, RP)\}].$$

If the worker chooses WC during period 1, he receives the RP during period 2, regardless of his work decision. This leads to the following value functions in period 2:

$$V_2 = \max_{s_2 \geq L} v(A_2 - s_2 + w_2 + RP) - \alpha_2 + \beta V_2(A_3) \quad (2.1)$$

$$U_2 = \max_{s_2 \geq L} u(A_2 - s_2 + b_2 + RP) + \beta J_2(A_3) \quad (2.2)$$

In each period  $t \in \{1, 2\}$ , the claimant has a reservation disutility level  $\alpha_t^*$  at which he is indifferent between returning to work or receiving WC for another period:

$$\alpha_t^* = v_t(c_t^e) - u_t(c_t^n) + \beta E[OV_t]. \quad (2.3)$$

The claimant will choose to work if his realized disutility of work is lower than his reservation disutility level,  $\alpha_t < \alpha_t^*$ . Note that  $E[V_{t+1}(A_{t+1}) - J_{t+1}(A_{t+1}, RP)] = E[OV_t]$  represents the claimant's expected option value associated with deciding whether or not to work. The RP increases the expected option value of staying out of work during period 1.

With this framework, the hazard rate of returning to work during period  $t$  can be represented as the probability that a worker's disutility is below his reservation disutility during period  $t$  (Manoli and Weber 2011):

$$h_t = Pr(\alpha_t < \alpha_t^*). \quad (2.4)$$

Factors that increase  $\alpha_t^*$  indicate an increase in the worker's reservation disutility, shortening claims. Similarly, factors that decrease  $\alpha_t^*$  lower the threshold disutility level and lengthen claims. Empirically, I estimate changes in the hazard rate, or probability of return to work. Changes in the hazard rate translate to changes in  $\alpha_t^*$  scaled by the probability density function of  $\alpha_t^*$ . By examining how changes in each of the parameters in  $\Omega_t = \{b_t, w_t, A_t, RP\}$  affect the duration of claims, I examine how these parameters influence the claimants' decision to return to work and, as a result, how changes in the parameters affect claimants' utility in different states of the world.

First, consider the effect of a one-time change in the WC benefit level in any period:

$$\frac{\partial \alpha_t^*}{\partial b_t} = \frac{\partial U_t}{\partial b_t} = -u'(c_t^n) < 0.$$

Increasing  $b_t$  increases utility while on WC, but does not affect utility while working. Given this result, an increase in  $b_t$  decreases the hazard of leaving WC and lengthens claims. This prediction has been confirmed in previous work finding that more generous WC benefits lead to longer claims (e.g., Butler and Worrall 1985;

Krueger 1990; Meyer *et al.* 1995; Neuhauser and Raphael 2004). On the other hand, increasing the wage during any one period yields:

$$\frac{\partial \alpha_t^*}{\partial w_t} = \frac{\partial V_t}{\partial w_t} = v'(c_t^e) > 0.$$

Here, a change in  $w_t$  only increases utility if the claimant returns to work. Since the opportunity cost of missing work is increasing in the wage, this implies that increasing the wage will increase the rate at which claimants return to work.

Now, consider the effect of a change in the level of cash on hand during any one period:

$$\frac{\partial \alpha_t^*}{\partial A_t} = v'(c_t^e) - u'(c_t^n) \leq 0.$$

In this case, the change in cash on hand affects utility in *both* the working and non-working state. The sign of  $\frac{\partial \alpha_t^*}{\partial A_t}$  depends on how  $A_t$  affects utility when individuals are working, relative to when they are not. If workers are able to maintain their desired consumption level when out of work, then their marginal utility of consumption will be the same in each state of the world, such that  $v'(c_t^e) = u'(c_t^n)$  and  $\frac{\partial \alpha_t^*}{\partial A_t} = 0$  (Chetty 2008). However, since  $b_t < w_t$ , claimants may lower their consumption while on WC if they cannot completely offset the gap in income with savings, or if they have precautionary savings motives. If workers reduce their consumption such that  $v'(c_t^e) < u'(c_t^n)$ , then  $\frac{\partial \alpha_t^*}{\partial A_t} < 0$ , indicating that additional cash on hand is *more valuable* to individuals when they are not working. In this case, an increase in  $A_t$  allows workers to move closer to their desired consumption level while out of

work. Since they are now able to consume more while out of work, their reservation disutility falls.

As shown in [Chetty \(2008\)](#),  $\frac{\partial \alpha_t^*}{\partial b_t}$  can be decomposed into the response to change in the level of cash on hand and a change in the wage:

$$\begin{aligned} \frac{\partial \alpha_t^*}{\partial b_t} &= [v'(c_t^e) - u'(c_t^n)] - v'(c_t^e) \\ &= \frac{\partial \alpha_t^*}{\partial A_t} - \frac{\partial \alpha_t^*}{\partial w_t} \end{aligned} \quad (2.5)$$

Hence, an increase in benefits could increase the reservation disutility level and lengthen claims through two distinct channels: by relaxing liquidity constraints and by reducing the opportunity cost of missing work. Importantly, while the first term captures the extent to which claimants value the additional income while out of work, the second term reflects the extent to which claimants respond to the change in incentive to work. The ratio of  $\frac{\partial h_t}{\partial A_t}$  and  $\frac{\partial h_t}{\partial w_t}$ , which are estimated in data, yields the ratio of  $\frac{\partial \alpha_t^*}{\partial A_t}$  and  $\frac{\partial \alpha_t^*}{\partial w_t}$ , informing the relative size of these two channels.

To see how the RP helps to identify these effects, consider comparative statics on RP during period 1 and period 2. Because workers who stay out of work during period 1 maintain the option of receiving the RP, the payment effectively lowers the opportunity cost of missing work during period 1. For these workers, the RP changes the expected value of utility during period 2,  $J_2$ :

$$\frac{\partial \alpha_1^*}{\partial RP} = \beta E \left[ \frac{\partial OV_1}{\partial RP} \right] = \begin{cases} -\beta \frac{\partial V_2}{\partial RP} : V_2 > U_2 \\ -\beta \frac{\partial U_2}{\partial RP} : V_2 \leq U_2 \end{cases} < 0 \quad (2.6)$$

Since both  $-\beta \frac{\partial V_2}{\partial RP} < 0$  and  $-\beta \frac{\partial U_2}{\partial RP} < 0$ , increasing the RP always lowers the reservation disutility level during period 1. Since workers do not receive the income from the RP until period 2, the response to the RP during period 1 is solely due to the increased option value of receiving the RP during period 2.

Once the worker is eligible for the RP, equations 2.1 and 2.2 show that the RP increases his utility during period 2 regardless of the decision to work, and has an identical effect on  $\alpha_2^*$  as a change in  $A_2$ :<sup>6</sup>

$$\frac{\partial \alpha_2^*}{\partial RP} = v'(c_2^e) - u'(c_2^e) \leq 0.$$

The separation between the time when claimants face the change in their *opportunity cost* and the time when claimants actually receive the *payment* allow me to distinguish the response to receiving additional cash from the response to a change in the incentive to return to work. If the response to the option value in period 1 is small relative to the response of receiving the non-distortionary payment during period 2, this implies that workers primarily lengthen claims in response to income that offsets the gap in their consumption: the liquidity effect. On the other hand, if the response during period 1 is larger than the response during period 2, this suggests that claimants primarily respond to the change in incentives: the moral hazard effect.<sup>7</sup>

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<sup>6</sup>In practice, workers will not receive the RP at the beginning of period 2. However, since the value of the RP is guaranteed upon reaching period 2, the effect of the RP could also be interpreted as decreasing the borrowing constraint  $L$  during period 2. Conceptually, this one-time decrease in  $L$  has the same effect on utility during period 2 as an increase in  $A_2$ . If workers instead wait until they receive the payment at the end of period 2, the RP will relax liquidity constraints during period 3.

<sup>7</sup>If workers have some ability to borrow and have a strong expectation that their claim will last long enough to receive the RP, they could choose to “spend” the RP prior to the two week mark.

## 2.4 Empirical Analyses

As a first assessment of how the RP affects claim length, table 2.3 shows the estimated coefficients from a linear regression of the log of the total number of workdays in the claim on the log retroactive payment, controlling for the claimant's pre-injury wage, injury, occupation, gender, age, and total medical costs in the claim. The results show that claim length does respond to the RP: a 1 percent increase in the RP lengthens claim durations by approximately .02 percent overall, and .03 percent among claims lasting longer than two weeks. On average, increasing the RP by one day of benefits represents a 50 percent increase in the RP. Based on the estimated coefficients, a 50 percent increase in the RP translates to an increase in duration of approximately half a day, or 3 percent relative to an average duration of approximately 14-15 days.

Additionally, the RP has a small negative effect on claims lasting less than two weeks, which could result from the offsetting effect of the smaller paycheck during period 1, or due to compositional changes from workers who extend their claims to two weeks or longer to claim the RP. Column (4) shows that the RP does not significantly affect claims lasting longer than eight weeks. This finding is reasonable, as a claimants with long claims likely have more severe injuries, and are unlikely to be influenced by a small change in the structure of their payment during the first four weeks of their claim. While these estimates demonstrate that the RP has an effect on claim length, the effect of the RP on total claim length does not inform

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If this occurs, the response during period 1 could be an over-estimate of moral hazard, and an under-estimate of the overall liquidity-moral hazard ratio. See section 2.4 for more details.

whether workers respond to the distortionary effects in the RP, or if the lump-sum payment relaxes their liquidity constraints after two weeks. In the sections below, I analyze claimants' responses before and after the payment of the RP to decompose these two possible channels for how the RP leads to longer claims.

I decompose the elasticity of claim duration with respect to benefits into the liquidity and moral hazard effects in two ways. First, I estimate a discrete proportional hazard model to determine the extent to which the RP affects claim duration at different points in time. Then, I obtain an alternative estimate of the moral hazard effect by estimating excess bunching around the eligibility threshold (i.e., claims lasting two weeks) for the RP. Finally, to examine whether the liquidity and moral hazard effects have consequences after claimants return to work, I investigate the effects of longer claims on post-injury outcomes using the RP as an instrument for claim length.

### 2.4.1 Hazard Analysis

First, I estimate the following discrete proportional hazard model:

$$h_{it} = 1 - \exp[-\exp(\sum_{k=1}^t \theta_k \ln(RP_i) \gamma_k + X'_i \beta + \gamma_k)] \quad (2.7)$$

where  $h_{it}$  represents the hazard rate: the probability of individual  $i$  leaving WC during period  $t$ , conditional on *not* leaving WC prior to period  $t$ . In this model, each  $t$  represents two week periods over the course of the claim. I control for duration dependence over time with indicators for duration representing every two week

period in the claim, represented by the  $\gamma_k$  terms. Then, I interact these indicators with  $\ln(RP_i)$ , allowing the effect of the RP to vary over the duration of the claim. I adjust the hazard rate for time-invariant individual observable characteristics in  $X_i'\beta$ . Most importantly, I control for the claimants' pre-injury wage and weekly WC benefit. Conditional on the claimant's pre-injury wage, the variation in the RP comes from exogenous variation in the date of injury as explained in section 2.2. I also control for gender, age, and total WC-paid medical costs. I include a parsimonious set of indicators for broad injury categories, key occupation groups and for claims occurring after 2002, a year when the maximum benefit increased and several other WC policy changes occurred in Oregon. Because of the spikes in the frequency of claim exits shown in figure 2.3a, I also include an indicator for durations in multiples of five.

I estimate the discrete proportional hazard model with the complementary log-log function shown above, which allows me to observe how observable characteristics affect the probability of exit during grouped time intervals (Allison 1982; Jenkins 2005; Meyer 1990), in this case, two-week intervals. I obtain the coefficients in equation 2.7 using maximum likelihood estimation in Stata. While this specification does not identify the underlying baseline hazard rate, it relies on fewer assumptions than a fully parametric specification with little loss in efficiency (Meyer 1986, 1990). I censor claims exceeding 60 workdays (12 weeks), since accurate estimation of the long right tail of the distribution would require parametric assumptions about the baseline hazard rate (Meyer 1990). Less than 5 percent of claims in my sample

exceed 60 workdays and, in practice, this restriction does not affect the coefficients appreciably.<sup>8</sup>

The main assumption in proportional hazard estimation is that observable characteristics and the baseline hazard are multiplicative: the effect of an observable characteristic  $X_i$  scales the baseline hazard rate by  $X_i'\beta$ . For example, if  $X_i = \{0, 1\}$  is a dummy variable indicating whether or not a worker is female, the corresponding coefficient identifies the proportional difference in the hazard rate for women, relative to the hazard rate for men. Similarly, the time-varying coefficients on the interacted  $\ln(RP)$  terms scale the hazard rate during each period  $t$  (Jenkins 2005; Kalbfleish and Prentice 2002). Because I include the log of the RP in this specification, the  $\theta_t$  coefficients represent the elasticity of the hazard rate with respect to the RP during period  $t$ . For example, a coefficient of -0.04 would indicate that claimants with a 100 percent higher RP have 4 percent lower hazard rate, or qualitatively, that claimants with larger RPs have longer claims.

Liquidity effects are likely to be most important for workers who have low levels of wealth prior to their injury. As a proxy for wealth, I additionally interact the coefficients for the RP with an indicator for whether workers earning above and below the median wage in Oregon, which is approximately \$700 per week (Peniston 2014). I also interact the coefficients with indicators for Oregon wage quartiles to examine in more detail how liquidity effects vary across the income distribution. If claimants with lower wages are more sensitive to small changes in their payment, i.e., if they are more liquidity constrained, then there should be a larger effect of

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<sup>8</sup>Censoring claims above 40 and 100 days yield similar results.

the RP during period 2 for claimants with lower earnings. In practice, the baseline hazard rate varies for individuals with different pre-injury earnings. I performed a test of the proportionality and can reject the hypothesis that wages affect the hazard rate proportionally. Failing to account for this variation in the baseline hazard leads to a mis-specification of the model. As a result, I interact the model for different wage groups to allow more flexibility in the baseline hazard rate for claimants with different pre-injury earnings.

Table 2.4 and figure 2.5 show the coefficients on  $\ln(RP)$  from equation 2.7. Table 2.4 shows the coefficients interacted with an indicator for claimants earning above or below Oregon's median wage, and figure 2.5 shows the coefficients interacted with wage quartiles. Columns (1) and (2) in table 2.4 show that conditional on the other covariates, a 1 percent increase in the RP reduces the hazard of leaving WC during the first two weeks of the claim by .041 percent for workers above the median wage, and 0.031 percent for workers below the median wage. For reference, a 50 percent increase in the RP increases the payment from 2 days of benefits to 3 days of benefits, on average. A change of this size leads to a 2 and 1.5 percent decrease in the probability that a worker's WC claim will end during the first two weeks for workers above and below the median wage, respectively.

All workers respond to the option value to recoup the benefits withheld during the first two weeks, and also significantly lengthen their claims in response to receiving the unconditional payment. For low wage workers, a 1 percent increase in the RP additionally reduces the hazard of leaving WC during the *second* two weeks by 0.042 percent. Conditional on having a claim lasting at least two weeks, these

estimates imply that a 50 percent increase in the RP leads to an approximate 2 percent decrease in the probability of exit for a low-wage worker. Claimants earning more than the median wage are slightly less responsive, but still significantly reduce their rate of exit from WC in response to the liquidity effect: a 1 percent increase in the RP significantly reduces the hazard of leaving WC by 0.03 percent among this higher earning group.

To put these results in context, a 50 percent increase in the RP amounts to an 8 percent increase in the bi-weekly benefit, on average. Similarly, a 50 percent increase in the RP implies that the claimant would give up a payment equal to an additional 6 percent of the average bi-weekly wage if he returns to work during period 1 and gives up the option to receive the RP. Scaling the coefficients in table 2.4 by these amounts, I obtain a liquidity elasticity of approximately .26 and a moral hazard elasticity of approximately .17 for low-wage workers. Based on equation 2.5, this implies an overall elasticity of approximately .43, and shows the liquidity effect amounts to approximately two-thirds of the total response to a change in benefits for low wage workers. For claimants above the median wage, the moral hazard elasticity is approximately .22, and the liquidity elasticity is approximately .19.

Figure 2.5 generalizes this trend, displaying the coefficients from equation 2.7 interacted with wage quartiles, instead of above and below the median wage. The point estimates for the moral hazard effect show a larger response as incomes increase, and the coefficients for the liquidity effect show a smaller liquidity response as incomes increase. The larger responses to liquidity for the lower two quartiles of the income distribution in figure 2.5a are again consistent with the RP playing more

of an insurance role for lower earners, who may be less likely to have other sources of income to smooth their consumption after an on-the-job injury. By contrast, the smaller moral hazard effects for lower earners in figure 2.5b could reflect the fact that lower earners are not as well informed of the incentives in WC benefits. Additionally, lower earners may be less able to extend their claim in order to benefit from the RP if they are more likely to face binding liquidity constraints during the first two weeks of their claim that force them to return to work more quickly. Section 2.5 elaborates on these potential selection effects.

These results add to a broader literature finding that individuals are sensitive even to small lump-sum payments, and this sensitivity suggests that workers could face liquidity constraints (Soueles *et al.* 2006). While previous research finds that workers who have experienced injuries on the job reduce their consumption relative to when they are employed (Bronchetti 2012), existing research does not provide information about *when* the decline in consumption occurs following the injury. The significant liquidity effect in table 2.4 suggests that claimants reduce their consumption even after fairly short spells away from work. Workers could reduce their consumption right after an on the job injury due to immediately binding liquidity constraints, or to increase their precautionary savings to hedge against the risk of facing a binding liquidity constraint later in their claim (Carroll and Kimball 2008; Chetty 2005). The fact that this payment affects the duration of fairly short claims suggests that timely changes in income can significantly affect injured workers' welfare, in particular for those with low incomes and presumably, low assets.

In general, the decision to leave WC is not completely determined by the claimant; doctors also play an important role in determining the length of a claim. A claimant's doctor must initially certify that a claimant cannot work for a certain period of time, and workers must revisit the doctor in order to be granted additional time away from work. Workers facing fairly minor injuries should be less likely to have a doctor certify that their injury warrants two weeks away from work, and workers facing severe injuries will likely remain out of work longer than two weeks, regardless of how large their RP might be. However, workers with less obvious recovery times may be able to adjust their claim length in response to the RP. In order to test this hypothesis, table 2.5 presents estimates from equation 2.7 where the coefficients are additionally interacted with broad injury categories.

Columns (2) and (3) of this table demonstrate that claimants with fractures and sprains have the largest response to the RP. Importantly, these injuries typically have more variable recovery times, and the claimant likely has more discretion about when to return to work. Additionally, the average claim duration for fractures and sprains is 3-4 weeks, meaning that these claimants are making their decision about when to return to work during the period in which they receive the RP. By contrast, column (4) shows the results for cuts and burns, injuries which typically have the shortest recovery times and are the least likely to last longer than two weeks. These injuries have smaller responses to the RP: claimants earning less than the median wage, who might be most sensitive to the option value in the RP, significantly lengthen their claims during the first two weeks, but not during the period when they receive the RP. However, higher earners with cuts or burns do not significantly

respond to the effects of the RP during any period. Claimants with traumatic injuries also have a significantly smaller response to the RP both above and below the median wage.<sup>9</sup>

## 2.4.2 Excess Bunching

The estimates from the proportional hazard model are based on the assumption of a semi-parametric functional form for the hazard rate, and allow me to identify the relative effect of the RP on the rate at which people end their WC spells. I provide further evidence about the magnitude of the moral hazard effect using a different estimation procedure that does not rely on these parametric assumptions. If claimants respond to the incentive stay out of work until they are eligible for the RP but do not use the additional income to further extend their claim past two weeks, this would lead to a large share of claims ending exactly at the point where workers become eligible for the RP. Indeed, figure 2.3a exhibits a spike in claim exits at exactly two weeks. Additionally, because these claimants do not extend their claim beyond two weeks, it indicates that claimants are able to reach their optimal claim length without the non-distortionary payment from the RP.

I estimate the amount of excess mass in the distribution of claim exits at the two week threshold as an alternative estimate of moral hazard. The main assumption in estimating excess bunching is that the distribution of claim length would be

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<sup>9</sup> The most severe traumas typically lead to permanent benefits, and are excluded from the sample.

smooth without the discrete change in the payment after two weeks. However, figure 2.3a shows spikes in the frequency of claim exit every 5 workdays, indicating a seasonal pattern in exits of WC after each week of the claim. As a result, I estimate a counterfactual distribution of claims that allows for a pattern of seasonality, but smooths the spike at two weeks, similar to what might exist in a world where workers do not have incentives to lengthen claims due to the option value of receiving the RP. I draw upon methodologies in Saez (2010) and Manoli and Weber (2011) to estimate excess bunching. In particular, I estimate the following regression:

$$n_d = \sum_{t=1}^5 f(d) * \mathbb{I}[d \in \{10(t-1), 10 * t\}] + \beta S_d + \epsilon_d \quad (2.8)$$

where  $n_d$  is the number of claims ending after  $d$  days of benefits,  $f(d)$  is a fourth-degree polynomial, interacted with an indicator for each 10-day duration interval.<sup>10</sup> Additionally,  $S_d$  is an indicator for exits occurring at any interval of 5 days. Finally, I interact this equation with indicators for each day of the week included in the main analysis. Using this regression, I predict a counterfactual count of claims on each day. Then, I calculate the number of claims ending at exactly 10 workdays under the original and counterfactual distribution, and attribute the difference between these two shares as excess bunching due to the option value incentive of the RP. I estimate the excess mass as a fraction of two intervals: a fraction of total claims ending during the second week, and as a fraction of all claims ending during the first two weeks.

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<sup>10</sup>The results are robust to interacting the polynomial with 9 or 11 day intervals instead of 10 day intervals.

Figure 2.3b compares the actual density of claim exit with the estimated counterfactual density of claim exit. Comparing the two densities suggests the spikes in the distribution are driven in part by seasonality in claim length. Still, there is a small amount of excess bunching around the two week mark, when claimants would become eligible for the RP. Additionally, figures 2.4a and 2.4b show that the excess bunching appears to be larger for claimants above the median wage.

Claimants who leave WC prior to the two week mark “give up” the option of receiving the RP, which is equal to approximately 13 percent of the claimant’s pre-injury bi-weekly wage. In estimating excess bunching in earnings with respect to a change in taxes, Saez (2001, 2010) show that when the change in the tax rate is small, any excess bunching is a function of the compensated elasticity, a pure substitution effect. Chetty (2006) shows that liquidity and moral hazard effects can also be represented as a Slutsky decomposition of income and substitution effects in a static model, where the income effect corresponds to the liquidity effect and the substitution effect corresponds to moral hazard. Under the assumption that the effective “tax” of 13 percent represents a small change, the estimate of excess bunching can thus be interpreted as the moral hazard effect in a static model framework. I obtain this elasticity for an alternative estimate of moral hazard by scaling the estimate of excess bunching in the following equation (Saez 2010):

$$e = \frac{dn/n}{dr/(1-r)}, \tag{2.9}$$

where  $dn$  is the estimate of excess mass at day 10,  $n$  is the time interval for claim exit: either the second week or the first two weeks; and  $dr$  represents the 13 percent of “tax” that claimants incur by leaving WC prior to two weeks. I estimate the excess mass and the elasticities separately for workers earning above and below the median wage in addition to estimating these statistics for the overall sample. For each estimation, I bootstrap the estimation of excess bunching and the elasticity to obtain standard errors.

Table 2.6 shows the estimates of excess bunching and elasticities. Panel A reports the estimates calculated over the second week; panel B reports the estimates calculated over the first two weeks. Column 1 reports the estimated excess bunching, and column 2 scales this calculation by the average change in the share of wages “given up” by returning to work prior to eligibility for the RP. I estimate that the option value of the RP leads to approximately 3.5 (1) percent more claims ending on day 10, rather than some other day during the second (first two) weeks. The estimate of excess bunching is larger for workers above the median wage: I estimate excess bunching of approximately 4.7 (1.3) percent for workers earning above the median wage, compared to 2.6 (0.6) percent for workers below the median wage.

Overall, these estimates are broadly consistent with the hazard estimates in section 2.4.1. For example, recall that a 50 percent increase in the RP leads to a 1.5 percent decline in the probability that a low-wage worker’s claim will end during the first two weeks of the claim. Approximately 56 percent of low-wage workers have claims ending during the first two weeks (excluding the 10th workday), and 1.5 percent of this share is 0.8 percent - similar to the estimate of excess bunching

for low wage workers reported in Panel B, column 1. Approximately 54 percent of high-wage workers end their claims before day 10, and 2 percent of this total is 1.08 - similar to the estimate of excess bunching for high wage workers in Panel B of 1.2.

Once scaling these excess bunching estimates by the change in the “tax”  $dr/(1-r)$ , I obtain alternative estimates of the substitution elasticity, the moral hazard effect (Chetty 2006; Saez 2010). As a result, column 2 in panel A shows that the elasticity of claim duration with respect to a change in option value RP is approximately .14 for claimants above the median wage, and .07 for claimants below the median wage. These elasticities are slightly smaller than the elasticities derived from the proportional hazard model, but have overlapping confidence intervals.

<sup>11</sup> In general, the evidence of excess bunching provides visual and non-parametric evidence of the moral hazard effect, again suggesting that the moral hazard effect is fairly small, in particular for low-wage claimants.

### 2.4.3 Effects on Return to Work Outcomes

Longer claims could also affect outcomes once claimants return to work. On one hand, if the liquidity effect affords workers to more time to recover, this could lead a better match with the employer upon return, potentially increasing earnings relative to what the claimant would have earned if he had returned to work earlier (Boden *et al.* 2001). On the other hand, employers may have a harder time re-integrating employees into the workforce, or may penalize their workers for their

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<sup>11</sup>If the moral hazard estimates from the proportional hazard model include an income effect due to claimants “spending” the RP in advance of qualifying for it, this could explain why the moral hazard elasticity from the proportional hazard model is larger than the elasticity calculated with excess bunching.

longer absence. Higher adjustment costs could lead to lower wages or fewer working hours once a claimant returns to his job (Butler *et al.* 1995). Since the duration of a claim is endogenous to injury severity, it is difficult to determine the effect of claim duration on these outcomes. After demonstrating that the RP lengthens claims, I use the retroactive payment as an instrument for the duration of a claim and estimate the following instrumental variables (IV) regression:

$$\begin{aligned}
 y_{it} &= \alpha + \gamma d_i + X'_{it}\beta + \epsilon_{it} \\
 d_i &= \theta + \phi RP_i + X'_{it}\delta + \nu_{it}
 \end{aligned}
 \tag{2.10}$$

I examine the effect of longer claims on return to work outcomes by estimating equation 2.10 with two-stage least squares (2SLS).  $d_i$  measures the duration of the claim. I examine several outcome variables in  $y_{it}$  including the change in average hours worked per quarter and the change in the average hourly wage, where I take the average over the quarter before and after the injury, omitting the quarter(s) including the date of injury and the last day for which benefits were paid.<sup>12</sup> Additionally, I estimate the effect of a longer duration on the probability that the claimant returns to the same work as before and the probability that the claimant returns to modified work after the injury.

Panel C in table 2.7 gives the first stage coefficients: a 100 percent increase in the RP lengthens claims by approximately 0.7 days in the overall sample, and 1 day

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<sup>12</sup> The state of Oregon Employment Department collects information on quarterly earnings and quarterly hours in the Unemployment Insurance database. I calculate the claimant's hourly wage by dividing total quarterly earnings by total hours worked in the quarter.

among claims lasting longer than two weeks. While this result is highly significant, the magnitude of the change induced by the RP is small, but not surprising given the size of the variation in the RP, and the size of the payment itself. Panels A and B in table 2.7 show the IV and reduced form coefficients on the change in hours and wages between the quarters before and after the injury. Columns (1) and (2) do not provide evidence that longer claims significantly affect the probability of a claimant returning to the same job, or requiring modifications on their work activities after their injury. Similarly, the coefficients in column (3) do not provide evidence that an increase in claim length significantly affects the hourly wage earned one quarter after the injury, relative to the hourly wage earned one quarter prior to the injury. If liquidity and moral hazard have conflicting effects on post-injury outcomes, this could explain the lack of a result. On the other hand, these negligible effects are consistent with research finding that liquidity effects in unemployment insurance do not significantly improve subsequent job matches (Card *et al.* 2007).

Column (4) suggest that longer claims could reduce the number of hours worked after the injury. The instrumental variables estimate implies that increasing claim length by one day leads to a 10 hour decrease in hours worked during the first quarter after an injury. However, this result is only marginally significant at the 10 percent level, and represents a very small relative change in total quarterly hours worked. In sum, these results do not provide strong evidence that the changes in claim length induced by the RP affect post-injury outcomes. As shown in the first stage, the RP typically extends claims by approximately 1 day. This marginal extension in claim length likely does not appreciably change the worker's circumstances

when they return to work. Longer claim extensions may have a larger impact for claimants with significantly longer durations and more severe injuries. However, this population’s decision about when return to work would not be influenced by the RP.

## 2.5 Robustness Checks

### 2.5.1 Variation in Cash on Hand

My empirical strategy exploits variation in the RP generated by the day of the week of the injury to identify the liquidity and moral hazard effects. However, the day of the week also creates variation in the size of the worker’s last pre-injury paycheck: workers who would receive larger RPs also earn fewer days of wages during the week of their injury. Approximately 85 percent of workers in Oregon receive their final paycheck during the first two weeks of their claim, meaning workers with larger RPs have less cash on hand during period 1.<sup>13</sup> Consider a revised version of equation 2.3 to understand the implications of this fact:

$$\alpha_1^* = v(A_1 - s_1 + w_1) - u(A_1(d) - s_1 + b_1) + \beta[V_2(A_2) - J_2(A_2, RP(d))].$$

Assume that  $d$  is increasing in the number of waiting period days on which benefits are withheld, increasing the  $RP$  in period 2 and decreasing  $A_1$ . Then, the effect of variation in the date of the injury is as follows:

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<sup>13</sup>Based on special calculations from Burgess (2014), approximately 71 percent of workers are paid at least twice a month, and one-half of the remaining 28 percent of workers paid monthly would receive their monthly check during any given two-week period.

$$\frac{\partial h_1}{\partial d} = -u'(c_1^n) \frac{\partial A_1}{\partial d} + \beta \frac{\partial OV_1}{\partial d} = -u'(c_1^n) \frac{\partial A_1}{\partial d} - \begin{cases} \beta \frac{\partial V_2}{\partial d} : V_2 > U_2 \\ \beta \frac{\partial U_2}{\partial d} : V_2 \leq U_2 \end{cases} \quad (2.11)$$

The second term in equation 2.11 is the same as in equation 2.6, implying that increasing the  $RP$  decreases  $h_1$ . But the first term is positive and potentially increases  $h_1$ , since  $\frac{\partial A_1}{\partial d}$  is negative. Ultimately, whether  $h_1$  rises or falls during period 1 will depend on which one of these two effects dominates. If workers have a large amount of cash on hand, then they are better able to smooth their consumption and  $d$  likely only has a small effect on  $A_1$ , making the first term small. As a result, the incentive in option value will dominate for workers with a high ability to smooth.

However, if workers have limited cash on hand or have a precautionary savings motive,  $d$  could have a relatively large effect on  $A_1$  and they will reduce consumption while on WC. If  $u'(c_1^u) \frac{\partial A_1}{\partial d} < \beta \frac{\partial OV}{\partial d}$ , then the option value will dominate, and workers will lengthen their claims. On the other hand, if marginal utility is sufficiently large, any small change in  $c_1^n$  will result in  $u'(c_1^u) \frac{\partial A_1}{\partial d} > \beta \frac{\partial OV}{\partial d}$ , and the reduction in the benefit will increase  $\alpha_1^*$ , shortening claims. On average, this effect could attenuate the moral hazard response to the option value during period 1. Additionally, since the workers who would be *most* sensitive to receiving the RP are more likely to leave the sample *prior* to their RP eligibility, using variation in the day of the week to identify the response to the RP could lead to a lower-bound estimate of the liquidity effect. On the other hand, if claimants deplete their cash on hand to smooth through

the smaller paycheck during period 1, they could be more sensitive to receiving the RP during period 2.

I use access to sick leave as a proxy to test how sensitive workers are to a change in the size of their final pre-injury paycheck. Because sick leave is managed by the employer and WC payments are managed separately by the insurer, a worker may use sick leave during the waiting period without affecting their eligibility for the RP. However, using sick leave during the waiting period equalizes the size of the final paycheck for workers who are injured on different days of the week. If a smaller final paycheck leads workers who are sensitive to small variations in income, to “select out” of receiving benefits and return to work more quickly, workers without sick days could be less sensitive to the RP. On the other hand, the lack of sick leave may lead claimants to deplete their existing cash on hand during period 1, making them more eager to hold on to become eligible for the RP, and sensitive to receiving it during period 2. I examine whether the results vary with access to sick leave to test for these potential biases.

I obtain national estimates of the share of workers in each industry who have sick leave from the 2010 National Compensation Survey. I adjust the industry-specific estimates by the total share of workers in the West region who have sick leave based on data from the 1999 Employee Benefits Survey ([U.S. Bureau of Labor Statistics, 1999, 2010](#)).<sup>14</sup> Table 2.14 shows there is considerable variance in the share of workers per industry who have sick leave. While only 24 percent of workers in

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<sup>14</sup>Unfortunately neither state-specific estimates nor industry-specific estimates of the prevalence of sick leave were available prior to 1999.

the food and accommodation industry have sick leave, over 77 percent of workers in utilities have sick leave. Based on the composition of industries in my sample, I approximate that 48 percent of the total sample has access to sick leave. I divide the sample into high and low sick day prevalence categories depending on whether at least 50 percent of workers in the industry have access to sick leave, the median industry share in my sample.

Admittedly, workers with and without sick leave could be different along many other characteristics. Tables 2.15a and 2.15b show that workers without sick leave are younger, more likely to be male, earn a slightly lower wage, and have longer WC spells. While I control for these observable differences, workers without sick days may also claim WC for more severe injuries, if they are willing to “tough out” fairly minor injuries to avoid missing wages that would not be replaced by sick leave. On the other hand, workers without sick leave could claim WC for minor injuries since they don’t have an alternative way to cover their wages during missed work time. While claimants with a low likelihood of sick leave are more likely to have cuts or burns, which are typically less severe injuries, they are also more likely to have fractures, which are typically more severe injuries. On average, the difference in total medical costs across groups, another measure of injury severity, is small. For low-wage workers, there is no significant difference in medical costs; for workers above the median wage, the difference less than \$200, but is statistically significant.

Table 2.8 reports the RP coefficients for workers above and below the median wage in industries with a high and low prevalence of sick days, respectively. The results show that claimants who are less likely to have access to sick leave are

more responsive to the incentives in the RP. As shown in column 1 of Panel B, the coefficient during the first two weeks for claimants unlikely to have sick leave is larger than in the overall sample of claimants earning less than the median wage: increasing the RP by 1 percent decreases the probability of exit during the first two weeks by 0.037 percent. By contrast, the coefficient during period 1 is slightly smaller for low-wage claimants who do have access to sick leave: increasing the RP by 1 percent decreases the probability of exit during the first two weeks by approximately 0.028 percent. The liquidity effect is also larger for claimants who likely do not have access to sick leave: increasing the RP by 1 percent further reduces the probability of exit by 0.053 percent during the second two weeks for these claimants, compared to 0.038 percent for claimants with access to sick leave. If claimants without sick leave need to deplete more of their cash on hand during the first two weeks, this would lead to a larger liquidity effect. The larger response during the first two weeks could reflect the fact that these claimants are more motivated to reclaim the RP, or could reflect selection that claimants with sick leave are less liquidity constrained, even when comparing against other claimants with similar earnings.

The response to the RP in Panel A displays a similar pattern for earners above the median wage: claimants unlikely to have sick leave have a larger moral hazard and liquidity response. In fact, claimants with access to sick leave do not have a significant liquidity effect at all. Indeed, Panel A shows that increasing the RP by 1 percent reduces the hazard of leaving WC during the first two weeks by approximately 0.051 percent for claimants unlikely to have sick leave, and 0.034 percent for claimants likely to have sick leave. Additionally, the coefficient on the

RP during period 2 indicates that a 1 percent increase in the RP significantly reduces the rate of leaving WC by 0.051 percent for claimants above the median wage who do not have access to sick leave. While these higher-wage claimants are able to “hold on” during period 1 to become eligible for the RP, this significant result after the first two weeks again could suggest that the lower paycheck depletes these claimants’ cash on hand, increasing their sensitivity to later receiving the RP. The stronger response for claimants without access to sick leave could also reflect the fact that claimants with sick leave may claim WC for more severe injuries, making them less responsive to the RP overall.

In general, these findings suggest that claimants without sick leave are more sensitive to the RP, perhaps due to the fact that they must deplete more of their assets to smooth consumption prior to receiving the RP. However, the confidence intervals between the coefficients in table 2.8 and table 2.4 overlap. As a result, I cannot reject the hypothesis that the trends in these sub-samples and the overall sample of low-wage workers are the same.

## 2.5.2 Changes in the Composition of Claimants

An ideal experiment would use changes in benefits for the entire population of beneficiaries to estimate liquidity and moral hazard effects. In this analysis, however, the liquidity estimate is based on claims that last longer than two weeks. If these claimants are less sensitive to small fluctuations in their benefits, either due to the severity of their injury or a better ability to smooth income, this select sample

of claimants could have a lower elasticity with respect to liquidity than the average claimant in the overall population. To examine the extent to which this affects my estimates, I reweight the sample of claimants who have claims less than and greater than two weeks to reflect the overall distribution of claims in the sample.

First, I estimate a propensity score of the probability of remaining out of work at least two weeks on a set of observable covariates including age, gender, pre-injury wage, industry and occupation. I determine which linear and quadratic covariates should be included in the propensity score using the stepwise regression procedure outlined in [Imbens \(2014\)](#). Then, I reweight the sample using the estimated propensity scores to minimize the difference between the sample of claims longer and shorter than two weeks so that the distribution of each group is similar to the overall distribution of claims ([Nichols 2008](#)). [Figure 2.6a](#) shows the distribution of propensity scores for the overall sample, the sample of claims less than 10 days and the sample of claims greater than or equal to 10 days. After reweighting the claims, the distribution of propensity scores is better matched across the three groups, as shown in [figure 2.6b](#).

[Table 2.9](#) provides coefficients from [equation 2.7](#) with the reweighted sample. As expected, the coefficients on the liquidity effect during period 2 are slightly larger for claimants below the median wage on the reweighted sample compared to the baseline estimates, suggesting that selection in the sample of claimants could attenuate the baseline estimates of the liquidity effect. Additionally, the coefficient on the first period is smaller in the reweighted sample. The coefficients are virtually identical for claimants above the median wage in this sample. These estimates

suggest that the moral hazard effect is slightly larger, and the liquidity effect slightly smaller, without accounting for this selection. As a result, the baseline estimates could yield a lower-bound estimate of the liquidity to moral hazard ratio for both claimants above and below the median wage. <sup>15</sup>

### 2.5.3 Effects of Employer Incentives

WC is unique from other forms of social insurance because the claimant maintains a relationship with his employer. Employers face several costs associated with injuries on the job: the cost of WC insurance premiums, the costs of improving the safety of the workplace, and direct and indirect costs associated with an accident, including productivity losses and repair costs. Employers seeking to minimize these costs may encourage workers to return to work more quickly (Bronchetti and McInerney 2015; Krueger and Burton 1990; McInerney 2010).<sup>16</sup> This could mitigate the overall elasticity of duration with respect to benefits in equation 2.5. However, if this incentive is correlated with the day of the week, employer incentives could introduce bias in my estimation of the liquidity and moral hazard effects. Since a larger RP would have a larger impact on premium costs, employers may have a greater incentive to encourage workers with the largest potential RPs to return to work before their eligibility for the RP. If true, employer incentives could bias my

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<sup>15</sup> I also have tested an alternative frailty hazard model which corrects the hazard model for unobservable differences among claimants that could lead some claimants to systematically leave more quickly than others (Kalbfleish and Prentice 2002). The estimates have the same pattern as correcting the sample for observable differences with the reweighting technique outlined above.

<sup>16</sup>Employers may also encourage workers to take a longer absence to ensure a complete recovery. This employer response would depend on the severity of the injury, which table 2.2 shows is broadly balanced across different days of the week. As a result, this incentive does not bias my estimates. See the appendix for more information about the how the role of the employer affects this analysis.

estimate of moral hazard during period 1 towards zero. Employers may also seek to mitigate the increase in claim duration after the RP is received. However, one additional day of benefits has the same effect on premium costs regardless of the date of injury. As a result, after workers have earned the RP, employer incentives could lead to a smaller elasticity, but likely do not introduce bias. Admittedly, while an increase in claim duration increases premium costs, the change in total costs is likely small relative to the costs incurred from an additional injury on the job, and employers likely devote more time to reducing accident costs along other margins.<sup>17</sup>

I use information on Oregon's Employer at Injury Program (EAIP) to empirically examine how employer incentives might affect the response to the RP. The EAIP subsidizes wages for injured workers who return to the same employer, but require modifications to their work activities. If the employer finds transitional work for the injured worker, it receives a subsidy of 45 percent of the injured worker's wages for the first two months after their return to work, and receives additional subsidies for accommodation equipment.<sup>18</sup> Since the EAIP makes it more affordable to accommodate injured workers, it may facilitate an employer's ability to reduce claim length, offsetting the response to the RP. As a result, I split the sample for claimants whose employers have and have not used the EAIP to examine the extent to which employer activity could offset the incentive to lengthen claims during the first two weeks.

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<sup>17</sup>Employers could also reduce the frequency of injury by enhancing safety features. Since most safety features are designed to reduce the frequency of injury, rather than the severity ([Occupational Safety and Health Administration 2012](#)), employer's control over safety features likely has a larger effect on the extensive margin of injuries, rather than the intensive margin that would determine eligibility for the RP.

<sup>18</sup>For more details on EAIP, see <http://www.cbs.state.or.us/wcd/rdrs/rau/eaip/eaip.html>.

Table 2.10 shows the results for high and low wage workers separately based on whether the employer has ever taken advantage of the EAIP. Column 3 in table 2.10 shows that claimants whose employers have not used the EAIP have the same pattern of results above and below the median wage as in the overall sample. On the other hand, higher-wage claimants whose employers have used the EAIP are slightly less sensitive to the RP during the first two weeks of the claim, and do not have a significant liquidity effect after the first two weeks. While this could be due to employer behavior that works to mitigate the additional cost of the RP, it also could result if workers who use the EAIP have more severe injuries. However, table 2.16 does not show a clear pattern indicating differences in severity depending on employer's use of the EAIP.

For claimants earning less than the median wage, the response to the RP does not change appreciably even for employers who use the EAIP. Since the RP is a function of the wage, lower earners who receive the RP would pose less of a cost, leading the employer to focus their efforts on mitigating claim length among higher earners. Additionally, if the mitigated response among higher earners was due to selection in the severity of injuries among employers using the RP, this would likely be present in the results for both high and lower earners. Hence, these results provide suggestive evidence that employer incentives could offset the response to the option value during the first two weeks of the claim, but employer incentives broadly do not appear to have large impacts on the effect of the RP.

## 2.6 Implications for Optimal Benefits

Importantly, the change in benefits does not have a first-order effect on other inputs that are endogenous to policy changes under the assumption that agents have already maximized their expected private utility. As a result, the estimated elasticities are informative about marginal welfare effects, but cannot be extrapolated beyond local policy changes (Chetty 2008).

The ultimate goal of estimating liquidity and moral hazard effects is to determine how a local change in benefits could affect social welfare. Under the assumption that agents have maximized their private welfare, the optimal benefit level is determined by the first order condition on the social planner's problem (Baily 1978; Chetty 2008):

$$\frac{dW_t}{db_t} = \frac{(1 - \sigma_t)}{\sigma_t} \left( \underbrace{\frac{u'(c_t^n) - v'(c_t^e)}{v'(c_t^e)}}_{(1)} - \underbrace{\frac{\epsilon_{1-d_t,b}}{\sigma_t}}_{(2)} \right). \quad (2.12)$$

The optimal benefit level depends on (1) the relative difference in marginal utilities of consumption in the working and non-working state; and (2) the elasticity of the probability of not working with respect to benefits. The benefit level maximizes social welfare when equation 2.12 equals zero. While extensive research in WC has yielded estimates of (2), only one paper has attempted to estimate (1) for WC. Bronchetti (2012) uses within-state variation in WC benefits over time to estimate how WC affects consumption following a workplace injury. Under plausible levels of risk aversion and assumptions about the utility function, she combines her estimates

on the effects of WC on consumption to a variant of equation 2.12 and obtains a range of possible optimal replacement rates for WC between 0.1 to 0.6. However, Chetty (2008) shows that the ratio of liquidity to moral hazard effects is a sufficient statistic for (1), without requiring additional assumptions about consumption or utility.

Importantly, the local change in benefits does not have a first-order effect on other inputs that are endogenous to policy changes if agents have already maximized their expected private utility. Under this key assumption, the ratio of liquidity to moral hazard elasticities estimated from changes in the current benefit level informs whether a local change in benefits would increase or decrease social welfare. If equation 2.12 yields a positive number when applying (1) the liquidity to moral hazard ratio estimated around current benefit levels, this indicates that increasing the benefit level will increase overall social welfare. Similarly, if the equation yields a negative number, this indicates that the current benefit level is too high: decreasing the benefit level would increase social welfare. While the estimated elasticities are informative about marginal welfare effects, the results cannot be extrapolated beyond local policy changes because the optimal benefits formula relies on the assumption that private utility is at the optimum (Baily 1978; Chetty 2008).

Furthermore, by taking advantage of the separation of the liquidity and moral hazard responses to the RP, I estimate the liquidity and moral hazard effect that occur at specific points in time during a claimant's absence from work. Hence, applying my estimated liquidity to moral hazard ratio to equation 2.12 requires assuming that workers' elasticity with respect to lump sum payments is the same

across all points in time, and that the elasticity is constant for payments of all sizes. Approximately 80 percent of claimants in my sample exit WC during the first four weeks. As a result, these estimates are based on responses during a time frame when most claimants make a decision about when to return to work.

Additionally, equation 2.12 represents the first order condition from the social planner's problem, assuming that individuals pay for the benefit through a lump-sum tax. In the case of WC, the government mandates that firms provide benefits, rather than providing them directly. Under the assumption that employees value the benefit at its full cost, the costs of providing WC will be fully passed through to employees, lowering wages by the full cost (Summers 1989). As a result, the conclusions about optimal benefits in this case hold under the assumption that workers bear the full cost of WC premiums. Research on the incidence of WC premiums finds that the majority of costs are indeed fully passed through to the employee, suggesting that this is a reasonable assumption (Dorsey and Walzer 1983; Fortin and Lanoie 2000; Krueger and Gruber 1990).<sup>19</sup>

Scaling the baseline estimates from table 2.4 by the percentage change in income due to the RP, I approximate that a liquidity elasticity of approximately .26 and .19 for low and high-wage claimants, and moral hazard elasticities of .17 and .22 for low and high wage claimants, respectively. The sum of the two effects as the overall elasticity of the probability of not working with respect to benefits during the first four weeks. I apply these estimates to equation 2.12 to determine the effects

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<sup>19</sup>See the appendix for additional details on this assumption.

of WC on social welfare.<sup>20</sup> I use the Survey of Occupational Illness and Injury to obtain estimates of  $(1 - \sigma_t)$ , the incidence rate of workplace injury (U.S. Bureau of Labor Statistics 2013). As the rate of workplace injury has declined over time, I present results based on two incidence rates: the incidence rate in 2013, and the average incidence rate between 1994 and 2013, to approximate the incidence over the same time frame for the data used in estimating the liquidity and moral hazard effects.

Table 2.11 shows the application of equation 2.12 for individuals above and below the median wage based on both incidence rates. While the magnitude of  $\frac{dW}{db}$  is small, the signs of the equation inform whether a marginal increase in benefits would increase or decrease overall social welfare.  $\frac{dW}{db}$  is scaled such that the magnitude of the equation can be interpreted as the monetary value of a change in benefits. In other words, column (4) of panel A indicates that increasing WC benefits by \$1 would increase lower earner's utility by approximately 2 cents per week, or \$1 per year. On the other hand, increasing weekly WC benefits by \$1 would increase individuals' utility above the median wage by approximately 50 cents per year.<sup>21</sup>

These approximations indicate small welfare gains to increasing benefits; however, they do imply that the optimal benefit level is higher than the current level for all workers to varying degrees. Additionally, given that the liquidity effect could be

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<sup>20</sup>As noted in Bronchetti (2012), the elasticity of the probability of not working with respect to benefits is the same as the elasticity of claim duration with respect to benefits if benefits do not influence the frequency of claims. Bronchetti and McInerney (2011) find very small elasticities of the frequency of claims with respect to benefit levels once they flexibly control for pre-injury wages, suggesting that the elasticity of duration with respect to benefits is a reasonable approximation for the overall elasticity in equation 2.12.

<sup>21</sup>For comparison, Chetty (2008) finds that increasing UI benefits by \$1 per week would increase an individual's utility by approximately 4 cents per week, or \$2 per year.

under-estimated due to the selection effects, this estimate likely represents a lower-bound on the welfare gains from increasing the WC benefit level. Panel B shows the welfare estimates based on the reweighted coefficients in table 2.9, which suggest slightly larger welfare gains, in particular for claimants below the median wage.

To understand how estimates from WC claims in Oregon might compare with estimates based on claims in other states, table 2.17 compares current WC benefit parameters in Oregon with the average parameter across all other states. While a few states have slightly larger (75-80 percent) or smaller (60 percent) replacement rate, the two-thirds replacement rate in Oregon is quite standard. The minimum benefit level in Oregon, \$50 or 90 percent of the workers' average weekly wage (whichever is higher), is low compared to an average of approximately \$150 across all other states.<sup>22</sup> On the other hand, Oregon's maximum benefit is much more generous than the average across other states - approximately \$1120, compared to approximately \$830 across other states (Tambe 2012). In practice, very few people in the claims data reach the maximum benefit level. Finally, the median weekly wage in Oregon is slightly larger, but fairly close to the median wage across all other states.

Additionally, table 2.18 shows that workers in Oregon are similar across demographic characteristics and savings habits, using data from the Survey on Income and Program Participation. Oregonians are slightly more likely to have a checking or interest accruing savings account, suggesting that liquidity constraints could be a smaller concern in Oregon than in other states. They are also more likely to owe

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<sup>22</sup>All dollar values in 2012 dollars.

debt, meaning they could be less constrained in their borrowing as well. Both of these facts mean that the liquidity effect could be smaller in Oregon than in other states, but this hypothesis should be verified with additional research.

While these characteristics suggest that Oregon's WC system and the characteristics of the Oregonian population is broadly similar to other states, the differing minimum and maximum benefits in other states could lead to different conclusions about the welfare impacts of a change in the current benefit level in other states. In particular, the benefits paid to lower-wage workers could be closer to optimal in places that have more generous minimum benefits.

## 2.7 Conclusion

Despite the large expenditures on social insurance in the United States, relatively little is known about the social welfare effects of many social insurance programs. In particular, little is known about the magnitude of the positive and negative welfare consequences of WC despite the growing policy discussion about reforming WC benefits. I observe how claimants adjust the duration of their WC claims in response to variation in a retroactive payment, allowing me to isolate the liquidity and moral hazard effects for WC. I find that the liquidity effect accounts for 60 to 65 percent of the increase in claim duration among lower-wage workers, and approximately 45 percent of the increase for high wage workers. These results are primarily driven by injuries that have variable recovery times, where claimants' decision to return to work could be influenced by small fluctuations in WC payments.

Under the assumption that the elasticity of duration is constant over the size and frequency of the payment, I apply these estimates to the optimal benefit formula outlined in [Chetty \(2008\)](#). The results suggest that the increasing the benefit level would increase social welfare, particularly for lower wage workers. Variation in pre-injury paychecks may lead some workers to be more sensitive to the RP, potentially introducing a bias in my estimates. However, when I restrict the sample to workers who likely can use sick days to make up the difference in their final paycheck, the confidence intervals overlap with the baseline estimates, suggesting that this potential bias is small. While changes in the composition of claimants in the sample during the first and second two weeks of the claim could also bias the liquidity to moral hazard ratio, reweighted estimates suggest that bias due to this selection is again likely not very large and, if anything, imply my baseline estimates could present a lower-bound on the potential welfare gains associated with increasing the benefit level.

This analysis also provides evidence that WC claimants respond to small payments ([Soueles \*et al.\* 2006](#)). Additionally, my results demonstrate that low-wage workers are sensitive to fluctuations in income even at the beginning of their WC spell, either due to an immediately binding liquidity constraint, or precautionary savings to prevent a constraint from binding in the future. Both of these findings provide evidence that liquidity constraints are an important consideration for the population of workers at risk of an on-the-job injury.

My analysis of post-injury outcomes does not suggest that small increases in claim duration have a significant effect on the probability of returning to the same

work, or on post-injury wages. The analysis does find that longer claims lead to fewer hours worked after an injury; however, the reduction in hours is quite small. The reduction in hours could reflect a positive or negative consequence of longer claims on the post-injury job match. However, even without a substantial effect on post-injury outcomes, an increase in duration due to a liquidity effect itself implies that WC benefits provide insurance value to injured workers, and as a result, this provides evidence that WC benefits relax claimants' liquidity constraints during recovery from an on-the-job injury. Future work could look more in depth at return to work outcomes. A better understanding of whether an increase in claim duration is beneficial or costly to workers could provide information about the welfare effects of WC once a worker returns to the labor force.

Table 2.1: Sample Selection Criteria

Restriction	N
Adults >18	614,217
No PPD/TPD	367,249
Five-day workweeks	309,363
Weekday injuries	274,222
Continuous WC spell	170,657
Wed-Fri injuries	96,694

Notes: Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Main sample restrictions exclude: claimants under age 18; claimants who received permanent disability payments (who are unlikely to respond to the RP) or temporary partial disability payments (likely ineligible or the RP); claimants who worked more than five days per week or were injured on the weekend, Monday or Tuesday (to improve accuracy of RP calculation and avoid the Monday effect). Claimants are also excluded if they did not leave work right after the injury, or if they returned to work intermittently during their WC claim.

Table 2.2: Claimant Characteristics by Day of the Week of Injury

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Mon	Tue	Wed	Thu	Fri	Pvalue - MW	Pvalue - WTh	Pvalue - WThF
Male	0.74	0.73	0.71	0.72	0.71	0.00	0.00	0.00
Age	36.0	35.87	35.91	36.10	36.08	0.25	0.04	0.07
Wage and benefit information								
Weekly wage	743.37	732.39	725.41	728.50	717.14	0.00	0.26	0.00
WC days paid	13.53	13.59	14.79	14.16	13.89	0.00	0.00	0.00
Ret pmt	291.59	287.22	281.80	189.54	93.85	0.00	0.00	0.00
Daily benefit	97.20	95.74	94.82	95.24	93.85	0.00	0.21	0.00
Medical cost	2,103.86	2,122.38	2,211.46	2,203.74	2,176.54	0.00	0.83	0.61
Afternoon	0.46	0.48	0.50	0.50	0.51	0.00	0.74	0.14
Injury type								
Trauma	0.04	0.04	0.04	0.04	0.04	0.87	0.18	0.21
Fracture	0.09	0.09	0.10	0.10	0.10	0.00	0.68	0.90
Strain	0.63	0.60	0.61	0.59	0.59	0.00	0.00	0.00
Wound	0.21	0.23	0.22	0.23	0.24	0.00	0.00	0.00
Other	0.03	0.04	0.04	0.04	0.04	0.00	0.99	0.96
Industry								
Agriculture	0.06	0.06	0.06	0.06	0.06	0.08	0.74	0.42
Construction	0.13	0.12	0.11	0.12	0.11	0.00	0.20	0.23
Manufacturing	0.21	0.20	0.20	0.20	0.20	0.00	0.23	0.30
Trade	0.16	0.17	0.17	0.17	0.18	0.01	0.18	0.02
Transportation	0.10	0.10	0.10	0.10	0.09	0.97	0.75	0.06
Other	0.34	0.36	0.37	0.36	0.37	0.00	0.64	0.22
Observations	38,517	35,446	31,898	32,704	32,092			

Notes: Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Sample includes claims that lasted at most one year. All dollar values in 2012 dollars. Medical costs reflect total medical expenditures during the WC spell. Injuries are recorded in the data with ICD-9 codes and are grouped here into five broad categories. Industry is recorded with six-digit NAICS codes in the data and grouped here into six broad categories. P-values test the equality of means across different days of the week: column (6) shows the p-values on a test of equality between Monday and Wednesday, column (7) shows the p-values on a test of equality between Wednesday and Thursday, and column (8) shows the p-values on a test of equality between Wednesday, Thursday and Friday.

Table 2.3: OLS Estimates of the Effect of the Retroactive Payment on Claim Duration

Dependent variable: $\ln(\text{duration})$	(1) All	(2) < two weeks	(3) $\geq$ two weeks	(4) $\geq$ eight weeks
Log (RP)	0.020* (0.008)	-0.015* (0.007)	0.029** (0.007)	-0.001 (0.009)
Observations	96,605	56,656	39,949	8,171

Notes: Standard errors, clustered at the claimant level, in parenthesis. +  $p < 0.1$ , \*  $p < 0.05$ , \*\* $p < 0.01$ . Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Duration is measured by the number of workdays for which benefits were paid. Each column contains coefficients from a separate regression. Column (1) includes all claims in the sample; column (2) limits the sample to claims lasting less than two weeks (i.e., claims ineligible for the retroactive payment); column (3) limits the sample to claims lasting at least two weeks (i.e., claims eligible for the retroactive payment); and column (4) limits the sample to claims lasting at least eight weeks (i.e., claims with durations unlikely to be responsive to the retroactive payment). Sample includes claims for injuries occurring on a Wednesday, Thursday or Friday, lasting at most one year. Regression includes controls for gender, age, pre-injury wage, total medical costs, and year fixed effects.

Table 2.4: Proportional Hazard Estimates of the Effect of the Retroactive Payment on the Probability of Exit from WC at Varying Points During the Claim

	(1) Above median wage	(2) Below median wage
Weeks 1-2	-0.041** (0.009)	-0.031** (0.010)
Weeks 3-4	-0.030* (0.014)	-0.042** (0.015)
Weeks 5-6	-0.021 (0.020)	-0.043+ (0.023)
Weeks 7-8	-0.011 (0.026)	-0.038 (0.030)
Observations	92,735	

Notes: Standard errors, clustered at the claimant level, in parenthesis. +  $p < 0.1$ , \*  $p < 0.05$ , \*\* $p < 0.01$ . Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Columns contain interacted coefficients from a single regression. Column (1) shows the coefficients on the  $\ln(\text{RP})$  interacted with an indicator for claimants earning more than the Oregon median wage (\$700) prior to their injury; column (2) shows the coefficients on the RP interacted with an indicator for claimants earning less than the Oregon median wage prior to their injury. Sample includes claims for injuries occurring on a Wednesday, Thursday or Friday that lasted at most one year. Duration is censored at 60 workdays. All dollar values in 2012 dollars and represented in logs. Regression also includes controls for the claimant's weekly benefit, wage, total medical costs, age, gender, an indicator for claims occurring after 2002, an indicator for experiencing a trauma, fracture, muscle sprain, or cut/burn, indicators for participating in a health support or transportation occupation, a spline in total duration with knots every two weeks, and an indicator for five-day multiples in duration to control for weekly spikes.

Table 2.5: Proportional Hazard Estimates of the Effect of the Retroactive Payment on the Probability of Exit from WC at Varying Points During the Claim, by Injury Type

	(1) Trauma	(2) Fracture	(3) Sprain	(4) Wound	(5) Other
Panel A: Claimants earning above median wage					
Weeks 1-2	-0.027* (0.011)	-0.125** (0.010)	-0.049** (0.009)	-0.000 (0.009)	-0.032** (0.011)
Weeks 3-4	-0.026 (0.017)	-0.106** (0.015)	-0.031* (0.014)	0.015 (0.014)	-0.043* (0.017)
Weeks 5-6	-0.021 (0.025)	-0.064** (0.021)	-0.021 (0.020)	0.022 (0.021)	-0.023 (0.025)
Weeks 7-8	0.014 (0.032)	-0.026 (0.027)	-0.018 (0.027)	0.041 (0.028)	-0.046 (0.044)
Panel B: Claimants earning below median wage					
Weeks 1-2	-0.023* (0.011)	-0.123** (0.011)	-0.049** (0.010)	0.030** (0.010)	-0.019 (0.011)
Weeks 3-4	-0.033+ (0.018)	-0.131** (0.017)	-0.047** (0.019)	0.019 (0.016)	-0.045* (0.019)
Weeks 5-6	-0.026 (0.028)	-0.109** (0.024)	-0.042+ (0.023)	0.007 (0.024)	-0.038 (0.029)
Weeks 7-8	-0.049 (0.038)	-0.040 (0.032)	-0.047 (0.031)	0.021 (0.032)	-0.055 (0.039)
Observations	92,735				

Notes: Standard errors clustered at the claimant level in parenthesis. +  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ . Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Columns contain interacted coefficients from a single regression. Each column shows the coefficients of the interaction of the  $\ln(\text{RP})$  with the injury type listed in the column header. Panels A and B show the coefficients where the  $\ln(\text{RP})$  was interacted with indicators for whether the claimant's pre-injury wage was above or below the median wage in Oregon (\$700), respectively. Includes claims for injuries occurring on a Wednesday, Thursday or Friday that lasted at most one year. Duration is censored at 60 workdays. All dollar values in 2012 dollars and represented in logs. Regression also includes controls for the claimant's weekly benefit, wage, total medical costs, age, gender, an indicator for claims occurring after 2002, an indicator for experiencing a trauma, fracture, muscle sprain, or cut/burn, indicators for participating in a health support or transportation occupation, a spline in total duration with knots every two weeks, and an indicator for five-day multiples in duration to control for weekly spikes.

Table 2.6: Excess Bunching and the Elasticity of Claim Exit at the Threshold for Retroactive Payment Eligibility (two weeks)

	(1) Excess mass of claims at threshold	(2) Elasticity of claim exit at the threshold
Panel A: Claims ending during the second week		
All	0.035 (0.020)	0.101 (0.057)
Below Median	0.026 (0.020)	0.073 (0.055)
Above Median	0.047 (0.031)	0.136 (0.089)
Panel B: Claims ending during the first two weeks		
All	0.009 (0.009)	0.061 (0.061)
Below Median	0.006 (0.006)	0.083 (0.033)
Above Median	0.013 (0.013)	0.044 (0.046)

Notes: Bootstrapped standard errors reported in parenthesis. Data from Oregon Department of Consumer and Business Services, includes WC claims from 1987-2012. Column (1) shows the excess mass in the distribution of claim durations exactly at the two week threshold (i.e., the point of eligibility for the RP). To estimate the excess mass I predict a counterfactual count of claims on each day. Then, I calculate the share of claims ending exactly after two weeks under the original and counterfactual distribution, and attribute the difference between these two shares as excess bunching due to the incentive of the RP. Column (2) scales the estimate of excess mass by the relative gain in benefits due to the RP to obtain the elasticity of claim exit at the two week threshold. Panel A shows the estimate of excess mass as a fraction of all claims ending during week 2, and Panel B shows the estimates of excess mass and elasticity as a fraction of all claims ending during the first two weeks.

Table 2.7: Instrumental Variables Regressions of Claim Duration on Post-Injury Labor Force Outcomes

Dependent var:	(1) Same work = 1	(2) Modified work = 1	(3) Chg-log wage	(4) Chg- hours
Panel A: IV Coefficients				
WC days	0.001 (-0.006 - 0.017)	0.005 (-0.008 - 0.001)	-0.011 (-0.033 - 0.011)	-10.703 + (-23.500 - 2.095)
Panel B: RF Coefficients				
Log RP	0.001 (-0.005 - 0.017)	0.005 (-0.007 - 0.001)	-0.008 (-0.022 - 0.007)	-7.070 + (-13.572 - -0.568)
Mean of dep var	0.869	0.016	0.018	-13.81
Panel C: First stage				
Dependent var: duration	(1) All	(2) < two weeks	(3) ≥ two weeks	(4) ≥ eight weeks
Log RP	0.726** (0.249)	0.008 (0.037)	1.050* (0.521)	0.716 (1.498)
Mean of dep var	14.3	3.67	31.3	69.9
Obs	38,069	40,901	38,538	41,121

Notes: Standard errors, clustered at the claimant level, in parenthesis. +  $p < 0.1$ , \*  $p < 0.05$ , \*\* $p < 0.01$ . Data from Oregon Department of Consumer and Business Services and the Oregon Employment Department, WC claims from 1999-2012. Sample includes claims for injuries occurring on a Wednesday, Thursday or Friday that lasted at most one year. All dollar values in 2012 dollars and represented in logs. Regression includes controls for gender, age, pre-injury wage, total medical costs, and year fixed effects. In panels A and B, column (1) shows the effects of a longer claim on whether the claimant returns to the same work he did prior to the injury, column (2) shows the effect on whether the claimant needed modifications to his activities, column (3) shows the change in the log wage in the quarter worked before and after the injury, and column (4) shows the change in hours worked in the quarter before and after the injury. In panel C, column (1) includes all claims in the sample; column (2) limits the sample to claims lasting less than two weeks (i.e., claims ineligible for the retroactive payment); column (3) limits the sample to claims lasting at least two weeks (i.e., claims eligible for the retroactive payment); and column (4) limits the sample to claims lasting at least eight weeks (i.e., claims with durations unlikely to be responsive to the retroactive payment). Duration in panel C is measured by the number of workdays for which benefits were paid. F-statistic from the first stage is 8.76.

Table 2.8: Proportional Hazard Model Estimates of the Effect of the Retroactive Payment on the Probability of Exit from WC at Varying Points During the Claim, by Prevalence of Sick Days in Worker Industry

	(1) Baseline	(2) No sickdays	(3) Sickdays
Panel A: Claimants earning above median wage			
Weeks 1-2	-0.041** (0.009)	-0.051** (0.009)	-0.034** (0.009)
Weeks 3-4	-0.030* (0.014)	-0.051** (0.014)	-0.021 (0.014)
Weeks 5-6	-0.021 (0.020)	-0.044* (0.020)	-0.011 (0.020)
Weeks 7-8	-0.011 (0.026)	-0.016 (0.027)	-0.001 (0.027)
Panel B: Claimants earning below median wage			
Weeks 1-2	-0.031** (0.010)	-0.037** (0.010)	-0.028** (0.010)
Weeks 3-4	-0.042** (0.015)	-0.053** (0.015)	-0.038* (0.015)
Weeks 5-6	-0.043+ (0.023)	-0.057* (0.023)	-0.039+ (0.023)
Weeks 7-8	-0.038 (0.030)	-0.032 (0.031)	-0.038 (0.030)
Observations	92,735	92,735	

Notes: Standard errors, clustered at the claimant level, in parenthesis. +  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ . Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012, the 1999 Employee Benefits Survey, and the 2010 National Compensation Survey. Column (1) contains the coefficients from the baseline regression, where the  $\ln(\text{RP})$  was interacted with indicators for whether the claimant's pre-injury wage was above or below the median wage in Oregon. Columns (2) and (3) show the interacted coefficients from another regression, where the  $\ln(\text{RP})$  was also interacted with an indicator for whether or not the claimant worked in an industry where less than 50% of workers have access to paid sick leave. Panels A and B show the coefficients where the  $\ln(\text{RP})$  was interacted with indicators for whether the claimant's pre-injury wage was above or below the median wage in Oregon (\$700), respectively. Sample includes claims for injuries occurring on a Wednesday, Thursday or Friday that lasted at most one year. Duration is censored at 60 workdays. All dollar values in 2012 dollars and represented in logs. Regression also includes controls for the claimant's weekly benefit, wage, total medical costs, age, gender, an indicator for claims occurring after 2002, an indicator for experiencing a trauma, fracture, muscle sprain, or cut/burn, indicators for participating in a health support or transportation occupation,

Table 2.9: Proportional Hazard Model Estimates of the Effect of the Retroactive Payment on the Probability of Exit from WC at Varying Points During the Claim, Reweighted

	(1) Above median wage	(2) Below median wage
Weeks 1-2	-0.029** (0.010)	-0.022* (0.010)
Weeks 3-4	-0.053** (0.014)	-0.053** (0.015)
Weeks 5-6	-0.034+ (0.020)	-0.043+ (0.023)
Weeks 7-8	-0.029 (0.027)	-0.042 (0.031)
Observations	92,735	

Notes: Standard errors, clustered at the claimant level, in parenthesis. +  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ . Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Each column contains the coefficients from a separate regression. Sample includes claims for injuries occurring on a Wednesday, Thursday or Friday that lasted at most one year. Column (1) shows the coefficients on the  $\ln(\text{RP})$  interacted with an indicator for claimants earning more than the Oregon median wage (\$700) prior to their injury; column (2) shows the coefficients on the RP interacted with an indicator for claimants earning less than the Oregon median wage prior to their injury. The sample of claims is reweighted using the predicted probability of the claim lasting longer than two weeks to minimize the distance between the distribution of claims less than two weeks and greater than two weeks, in order to mirror the overall distribution of claims. All dollar values in 2012 dollars and represented in logs. Duration is censored at 60 workdays. Regression also includes controls for the claimant's weekly benefit, wage, total medical costs, age, gender, an indicator for claims occurring after 2002, an indicator for experiencing a trauma, fracture, muscle sprain, or cut/burn, indicators for participating in a health support or transportation occupation, a spline in total duration with knots every two weeks, and an indicator for five-day multiples in duration to control for weekly spikes.

Table 2.10: Proportional Hazard Model Estimates of the Effect of the Retroactive Payment on the Probability of Exit from WC at Varying Points During the Claim, by Employer Use of Return to Work Incentives

	(1) Baseline	(2) EAIP	(3) No EAIP
Panel A: Claimants earning above median wage			
Weeks 1-2	-0.041** (0.009)	-0.035** (0.009)	-0.047** (0.009)
Weeks 3-4	-0.030* (0.014)	-0.022 (0.014)	-0.039** (0.014)
Weeks 5-6	-0.021 (0.020)	-0.010 (0.020)	-0.030 (0.020)
Weeks 7-8	-0.011 (0.026)	0.002 (0.027)	-0.020 (0.027)
Panel B: Claimants earning below median wage			
Weeks 1-2	-0.031** (0.010)	-0.026** (0.010)	-0.036** (0.010)
Weeks 3-4	-0.042** (0.015)	-0.040** (0.015)	-0.044** (0.015)
Weeks 5-6	-0.043+ (0.023)	-0.044+ (0.023)	-0.044+ (0.023)
Weeks 7-8	-0.038 (0.030)	-0.045 (0.031)	-0.033 (0.031)
Observations	92,735		92,735

Notes: Standard errors, clustered at the claimant level, in parenthesis. +  $p < 0.1$ , \*  $p < 0.05$ , \*\* $p < 0.01$ . Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Sample includes claims for injuries occurring on a Wednesday, Thursday or Friday, lasting at most one year. Column (1) contains the coefficients from the baseline regression, where the  $\ln(\text{RP})$  was interacted with indicators for whether the claimant's pre-injury wage was above or below the median wage in Oregon. Columns (2) and (3) show the interacted coefficients from another regression, where the  $\ln(\text{RP})$  was also interacted with an indicator for whether or not the claimant's employer had ever used the Employer at Injury Program (EAIP). Panels A and B show the coefficients where the  $\ln(\text{RP})$  was interacted with indicators for whether the claimant's pre-injury wage was above or below the median wage in Oregon (\$700), respectively. All dollar values in 2012 dollars and represented in logs. Duration is censored at 60 workdays. Regression also includes controls for the claimant's weekly benefit, wage, total medical costs, age, gender, an indicator for claims occurring after 2002, an indicator for experiencing a trauma, fracture, muscle sprain, or cut/burn, indicators for participating in a health support or transportation occupation, a spline in total duration with knots every two weeks, and an indicator for five-day multiples in

Table 2.11: Welfare Effects of WC Benefits

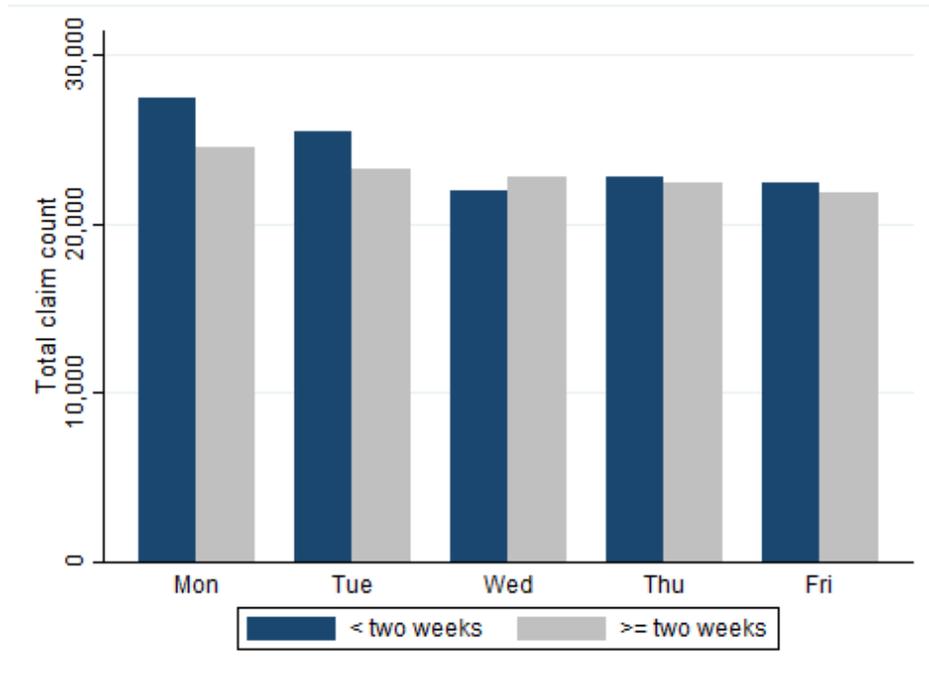
	(1)	(2)	(3)	(4)
	Injury Incidence	Liquidity	Moral Hazard	Welfare Change
	$(1 - \sigma)$	$\frac{\partial h_t}{\partial A_t}$	$\frac{\partial h_t}{\partial w_t}$	$\frac{dW}{db}$
Panel A - Baseline estimates				
Below median wage				
2013	1%	-.26	-.17	.012
	–	(0.08)	(0.05)	(0.08)
Avg 1994 - 2013	1.6%	-.26	-.17	.018
	–	(0.08)	(0.05)	(0.01)
Above median wage				
2013	1%	-.19	-.22	.005
	–	(0.07)	(0.06)	(0.04)
Avg 1994 - 2013	1.6%	-.19	-.22	.007
	–	(0.07)	(0.06)	(0.06)
Panel B - reweighted estimates				
Below median wage				
2013	1%	-.32	-.12	.025
	–	(0.12)	(0.07)	(0.18)
Avg 1994 - 2013	1.6%	-.32	-.12	.036
	–	(0.12)	(0.07)	(0.26)
Above median wage				
2013	1%	-.32	-.16	.017
	–	(0.14)	(0.07)	(0.04)
Avg 1994 - 2013	1.6%	-.32	-.16	.025
	–	(0.14)	(0.07)	(0.06)

Notes: Bootstrapped standard errors reported in parenthesis. Data from Oregon Department of Consumer and Business Services, 1987-2012, and the Survey of Occupational Injuries and Illnesses. Column (1) contains the incidence rate of workplace injury for the relevant time frame as documented by the Survey of Occupational Injuries and Illnesses. Column (2) contains the liquidity elasticities - scaling the coefficients on the  $\ln(\text{RP})$  during Weeks 3-4 from the proportional hazard model by the percentage change in benefits due to the RP. Similarly, column (3) contains the moral hazard elasticities, scaling the coefficients on the  $\ln(\text{RP})$  during weeks 1-2 of the proportional hazard model by the equivalent percentage change in the bi-weekly wage induced by the RP. Under the assumption that the elasticities with respect to liquidity and moral hazard are constant over time, column (4) applies these estimates to equation 2.12. The value in column (4) represents the monetary value of a change in welfare in response to a \$1 change in benefits. Panel A represents the welfare calculations using the baseline estimates, and panel B represents the welfare calculations using the reweighted estimates.

Figure 2.1: Variation in Retroactive Payment by Day of the Week

	SUN	MON	TUE	WED	THU	FRI	SAT
				X	X	X	
Injury occurs on Wed		\$	\$	\$	\$	\$	
		\$	\$XXX	\$	\$	\$	
					X	X	X
Injury occurs on Thu		\$	\$	\$	\$	\$	
		\$	\$	\$XX	\$	\$	
						X	X
Injury occurs on Fri	X	\$	\$	\$	\$	\$	
		\$	\$	\$	\$X	\$	

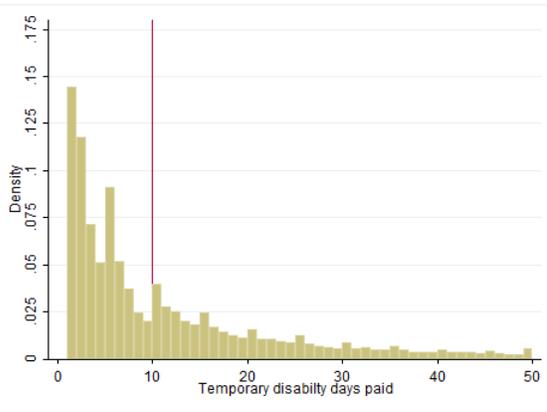
Figure 2.2: Frequency of WC Claims by Day of the Week of Injury



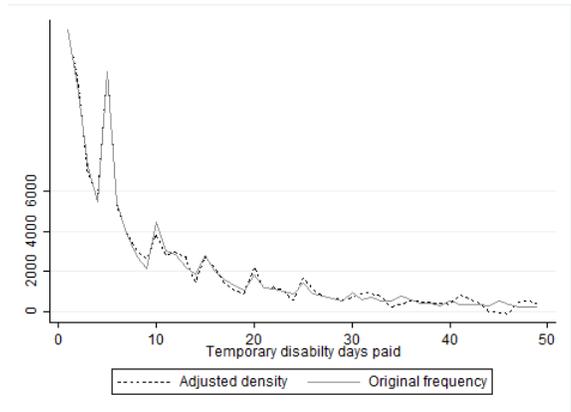
Notes: Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Sample includes claims occurring on a Wednesday, Thursday or Friday that lasted at most one year. The dark bars on the left show the frequency of claims lasting less than two weeks by week date of injury; the light bars on the right show the frequency of claims lasting at least two weeks by week date of injury.

Figure 2.3: Distribution of WC Claim Duration

(a) Actual distribution of WC claim duration



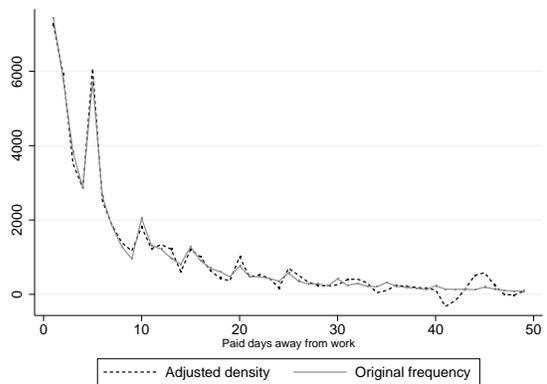
(b) Actual vs counterfactual distribution



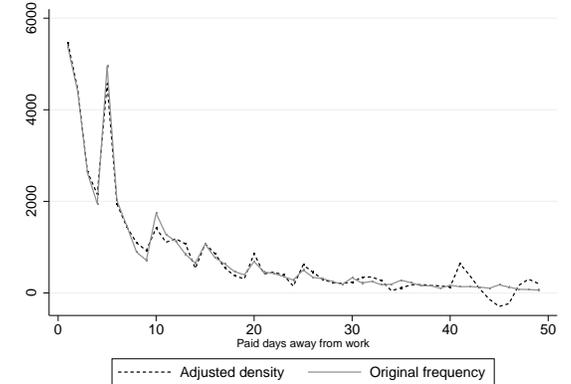
Notes: Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Sample includes claims occurring on a Wednesday, Thursday or Friday that lasted at most one year. Counterfactual distribution is predicted from a regression of the total count of claims ending per each duration on a flexible polynomial interacted for each ten day interval of claim length. In each panel, the x-axis represents the duration of claims, measured by the number of workdays for which benefits were paid. Because the sample is limited to claimants working five days per week, 10 days corresponds to two weeks.

Figure 2.4: Actual vs Counterfactual Distribution of WC Claim Duration, by Median Wage

(a) Claimants earning below Oregon median wage

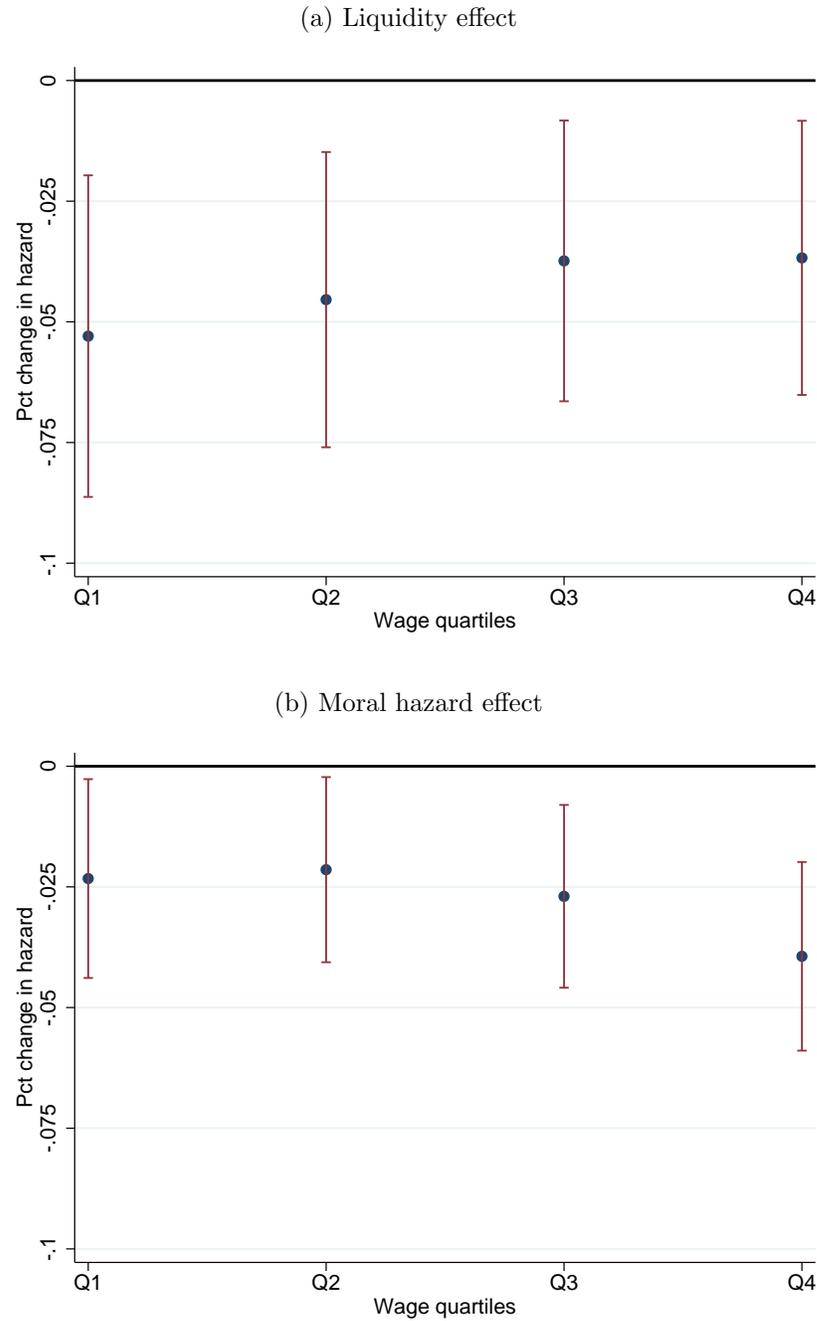


(b) Claimants earning above Oregon median wage



Notes: Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Sample includes claims occurring on a Wednesday, Thursday or Friday that lasted at most one year. Counterfactual distribution is predicted from a regression of the total count of claims ending per each duration on a flexible polynomial interacted for each ten day interval of claim length. In each panel, the x-axis represents the duration of claims, measured by the number of workdays for which benefits were paid. Because the sample is limited to claimants working five days per week, 10 days corresponds to two weeks.

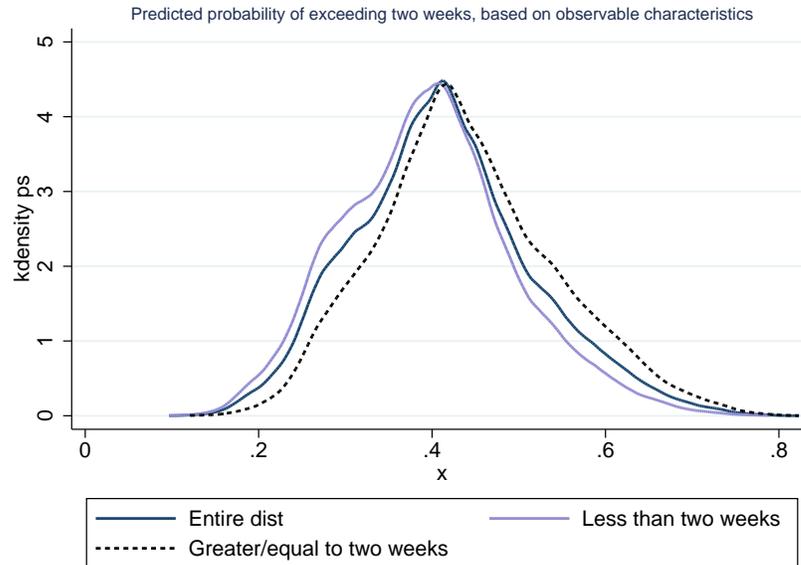
Figure 2.5: Proportional Hazard Coefficients of the Liquidity and Moral Hazard Effects, by Wage Quartile



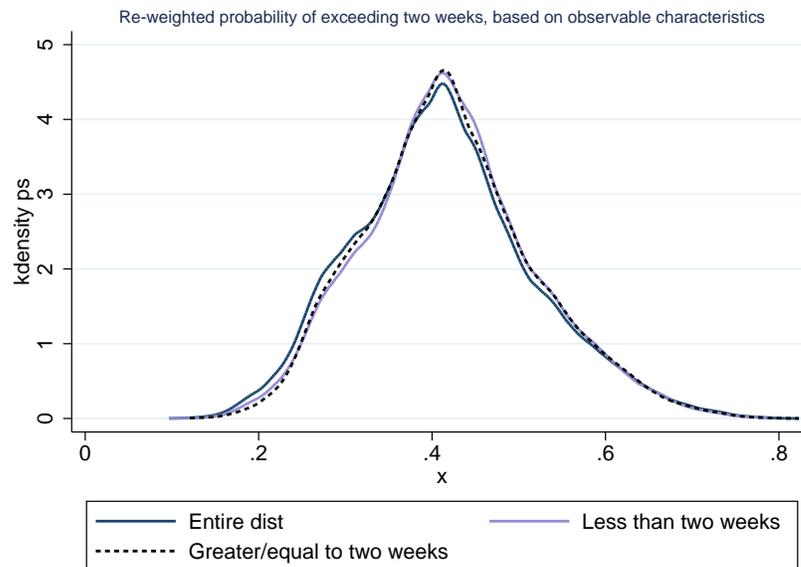
Notes: Bars indicate 95% confidence intervals. Dashed vertical line indicates the two week threshold for RP eligibility. Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Sample includes claims occurring on a Wednesday, Thursday or Friday that lasted at most one year. Duration censored at 60 workdays. Regression also includes controls for the claimant's weekly benefit, wage, total medical costs, age, gender, an indicator for claims occurring after 2002, an indicator for experiencing a trauma, fracture, muscle sprain, or cut/burn, indicators for participating in a health support or transportation occupation, a spline in total duration with knots every two weeks, and an indicator for five-day multiples in duration to control for weekly spikes.

Figure 2.6: Distribution of Propensity Scores for the Probability of a Claim Lasting At Least Two Weeks

(a) Propensity score, baseline



(b) Propensity score, reweighted



Notes: Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Sample includes claims occurring on a Wednesday, Thursday or Friday that lasted at most one year. Panel (a) shows the distribution of predicted probability that the claim lasts longer than two weeks for the entire distribution, claims lasting less than and longer than two weeks, respectively. Panel (b) shows the distribution of the predicted probability or claims lasting less than and longer than two weeks, respectively, reweighted to minimize the distance between the overall distribution and the distribution of the subsample, using estimated propensity score weights.

## 2.8 Appendix

### 2.8.1 Sample Selection

In order to construct the sample of claims used in analysis, I make the following restrictions to the dataset:

1. Exclude individuals under age 18: these workers comprise less than one percent of the total sample, and are likely to have unusual work schedules and possibly other sources of income from their parents, and so do not represent the typical WC claimant.
2. Exclude the bottom and top 0.5% of the wage distribution: most of these cases represent extreme outliers.
3. Exclude claims prior to 1987: the dataset does not contain the full set of claims in years prior to 1987, so they are dropped from the analysis. These early claims represent approximately 5% of the original sample.
4. Exclude claims lasting more than one year: The distribution of claims has a long right tail. Claims lasting more than one year are likely so severe that they would not be influenced by the retroactive payment: the retroactive payment comprises less than 1 percent of the total WC payments these beneficiaries receive during their claim. Power calculations suggest that I am unable to detect a change in duration in response to a payment comprising less than 1 percent of the total WC payments and so these claims are dropped.

5. Exclude claims where claimants did not stop working immediately: I exclude claims where the date of first payment occurs more than one week after the date of injury or claims where the date the employer was informed of the injury occurs more than one week after the date of the injury. These claims could represent injuries that occur more gradually over time, and workers could have time to adjust to the injury and plan their exit from work.
6. Exclude weekends, Monday and Tuesday injuries: Due to the Monday effect and the fact that claimants who work on the weekend likely do not have typical work schedules, I exclude all of these claims from the analysis.

## 2.8.2 Optimal Benefits Formula with the Firm

Equation 2.12 represents the first order condition from the social planner's problem, assuming that individuals pay for the benefit through a lump-sum tax. In the case of WC, the government mandates that firms provide benefits, rather than providing them directly. Under the assumption that employees value the benefit at its full cost, the costs of providing WC will be fully passed through to employees, lowering wages by the full cost (Summers 1989). In this case, the optimal benefits formula in equation 2.12 is identical when firms pay directly for benefits, rather than workers. Research on the incidence of WC finds that the majority of costs are indeed passed through to the employee, suggesting that this is a reasonable assumption (Dorsey and Walzer 1983; Fortin and Lanoie 2000; Krueger and Gruber 1990). For more detail, consider the static social welfare model from Chetty 2008, where the

social planner chooses the benefit level  $b$  to maximize the worker's expected utility with following equation:

$$\begin{aligned} \max_b W(b) &= [1 - s(b)]u(A + b) + s(b)v(A + w - \tau) - \phi(s(b)) \\ \text{s.t. } b[1 - s(b)] &= s(b)\tau \end{aligned} \tag{2.13}$$

In this equation,  $s$  percent of individuals work, receive a wage  $w$ , and pay a lump-sum tax  $\tau$  to finance benefits.  $1 - s$  percent of individuals do not work and receive the WC benefit  $b$ . Note that

$$\frac{d\tau}{db} = \frac{1 - s}{s} - \frac{b}{s^2} \frac{ds}{db}. \tag{2.14}$$

Equation 2.14 there are two effects of an increase in benefits on taxes: the first term shows that the tax increases in proportion to the share of individuals that receive the benefit, relative to the share that pays for it. However, the second term represents the fact that the share of people who are working declines when benefits increase. Since there are now fewer workers to pay the taxes needed to finance benefits, taxes must increase more than they would if there were no adjustment in the duration or incidence of claims. Using this equation, the first order condition for 2.13 is equal to:

$$\frac{dW}{db} = (1 - s)[u'(c^n) - v'(c^e)] + v'(c^e) \frac{b}{s} \frac{ds}{db} = 0. \tag{2.15}$$

Now, consider the social planner's problem with a constraint of maintaining firm profits above a given level  $c$ , rather than a balanced budget constraint:

$$\begin{aligned} \max_b W(b) &= [1 - s(b)]u(A + b) + s(b)v(A + w - \tau) - \phi(s(b)) \\ \text{s.t. } F(s) - w \cdot s - b(1 - s) &\geq C \end{aligned} \tag{2.16}$$

In this case,  $s$  percent of individuals work, produce  $F(s)$  for the firm and earn  $w$ . Firms also pay a premium cost  $b$  based on the share of claimants who do not work. Here, the premium is assumed to be equal to the benefit workers receive, reflecting a case of perfect experience rating. See [National Council on Compensation Insurance 2014](#); [Ruser 1985](#) for detailed explanations of experience rating in WC. Assume the firm's profit condition in 2.16 holds with equality, and consider the effect of a change in benefits on wages:

$$\frac{dw}{db} = -\frac{(1 - s)}{s} + \frac{F'(s)s - F(s) + b + C}{s^2} \frac{ds}{db}. \tag{2.17}$$

Here, a change in benefits affects the net wage via a mechanical effect equal to the share of employees who now receive the larger benefit, as well as by an additional amount due to fact that higher benefits reduce the share of employees who work, and this affects productivity and firm costs above and beyond the mechanical effect. Combining equation 2.17 and equation 2.16 yields the following equation:

$$\frac{dW}{db} = (1 - s)[u'(c_n) - v'(c_e)] + v'(c_e) \left[ \frac{F'(s)s - F(s) + b + C}{s^2} \frac{ds}{db} \right]. \quad (2.18)$$

Under the additional assumption that workers are paid their marginal product (i.e.,  $F'(s) = w$ ), this reduces to equation 2.15:

$$\frac{dW}{db} = (1 - s)[u'(c_n) - v'(c_e)] + v'(c_e) \left[ \frac{b}{s} \frac{ds}{db} \right].$$

Just as in the standard problem, the worker decides whether to stay out or return to work by choosing between his benefit level and his net wage. If employers are able to shift the cost of higher benefits onto the employee, they essentially lower the worker's net wage in the same way as would an increase in  $\tau$ , driving a larger wedge between the market wage and the net return to work.

If firms do not pass the full amount of benefits through to employee wages, higher firm costs will lead to a lower equilibrium level of employment (Summers 1989). Since WC provides insurance for workers who are injured on the job, the welfare consequences of non-employment that is not a direct result of a disability would not be incorporated in the benefit formula. However, as mentioned above, the best estimates of the incidence of WC find that the majority of WC costs are passed through to workers, suggesting that full pass-through is a reasonable assumption.

If  $F'(s) > w$ , then equation 2.18 will not reduce to equation 2.15. As a result, the optimal benefit level will also depend on the effects on firm profits. Reductions

in firm profits that are not passed through to wages would increase the cost of social insurance, and an optimal benefit formula that does not incorporate this effect will likely overstate the optimal benefit level. Alternatively, if  $F'(s) < w$ , then the optimal benefit level could be understated.

Table 2.12: Characteristics of Claimants in Sample Compared to Excluded Observations

	Monday		Tuesday		Wednesday		Thursday		Friday	
	Sample	Other	Sample	Other	Sample	Other	Sample	Other	Sample	Other
Male	0.74	0.67	0.73	0.66	0.72	0.66	0.72	0.66	0.71	0.66
Age	36.73	39.78	36.69	39.91	36.81	39.86	36.91	39.80	36.94	39.90
Wage and benefit information										
Weekly wage	740.47	779.96	730.14	778.08	725.89	778.38	728.01	774.94	719.12	770.70
WC days paid	20.18	95.20	20.46	96.80	22.01	96.14	21.03	94.70	20.89	95.81
Ret pmt	290.91	305.17	286.84	304.40	282.45	301.78	189.66	201.36	94.23	100.49
Daily benefit	96.97	101.72	95.61	101.47	95.05	101.51	95.31	101.11	94.23	100.49
Medical cost	3,216.33	12,123.76	3,270.93	12,541.87	3,447.86	12,319.75	3,352.20	12,060.50	3,372.65	12,307.94
Afternoon	0.46	0.46	0.48	0.48	0.50	0.50	0.50	0.51	0.50	0.53
Injury type										
Trauma	0.05	0.05	0.05	0.06	0.05	0.05	0.05	0.05	0.05	0.05
Fracture	0.12	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13	0.13
Strain	0.59	0.65	0.57	0.64	0.57	0.64	0.56	0.64	0.56	0.64
Wound	0.20	0.09	0.22	0.09	0.21	0.09	0.22	0.10	0.22	0.10
Other	0.03	0.08	0.04	0.08	0.04	0.08	0.04	0.08	0.04	0.08
Industry										
Agriculture	0.06	0.05	0.06	0.05	0.06	0.06	0.06	0.05	0.06	0.06
Construction	0.13	0.12	0.13	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Manufacturing	0.21	0.22	0.20	0.22	0.20	0.22	0.20	0.22	0.19	0.22
Trade	0.16	0.16	0.17	0.16	0.17	0.16	0.16	0.16	0.17	0.17
Transportation	0.10	0.09	0.10	0.09	0.10	0.08	0.09	0.08	0.09	0.08
Other	0.34	0.35	0.35	0.36	0.36	0.36	0.36	0.36	0.37	0.36
Observations	52,560	47,996	49,185	44,542	45,180	44,290	45,717	44,520	44,748	42,705

Notes: Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Main sample restrictions exclude weekend claims, claims opened more than once and lasted more than one year. All dollar values in 2012 dollars.

Table 2.13: Baseline Estimates Excluding Friday Injuries

	(1) Above median wage	(2) Below median wage
Weeks 1-2	-0.062* (0.021)	-0.054* (0.022)
Weeks 3-4	-0.041 (0.028)	-0.050+ (0.030)
Weeks 5-6	-0.049 (0.039)	-0.069 (0.043)
Weeks 7-8	-0.072 (0.050)	0.066 (0.057)
Observations	64,602	

Notes: Standard errors, clustered at the claimant level, in parenthesis. +  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ . Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Columns contain interacted coefficients from a single regression. Column (1) shows the coefficients on the  $\ln(\text{RP})$  interacted with an indicator for claimants earning more than the Oregon median wage (\$700) prior to their injury; column (2) shows the coefficients on the RP interacted with an indicator for claimants earning less than the Oregon median wage prior to their injury. Sample includes claims for injuries occurring on a Wednesday or Thursday that lasted at most one year. Duration is censored at 60 workdays. All dollar values in 2012 dollars and represented in logs. Regression also includes controls for the claimant's weekly benefit, wage, total medical costs, age, gender, an indicator for claims occurring after 2002, an indicator for experiencing a trauma, fracture, muscle sprain, or cut/burn, indicators for participating in a health support or transportation occupation, a spline in total duration with knots every two weeks, and an indicator for five-day multiples in duration to control for weekly spikes.

Table 2.14: Frequency of Sick Days by Industry

Industry	Share of workers in industry	Share of industry with sick leave
Agriculture	0.06	0.30
Mining	0.006	0.51
Utilities	0.01	0.77
Construction	0.11	0.30
Manufacturing	0.19	0.51
Wholesale trade	0.05	0.66
Retail trade	0.12	0.43
Transportation/warehousing	0.10	0.60
Information	0.01	0.74
Finance and insurance	0.01	0.76
Real estate	0.01	0.67
Professional/technical	0.01	0.61
Management	0.002	0.74
Waste management	0.07	0.33

Table 2.15: Observable Characteristics by Sick Day Prevalence

(a) Below median wage				(b) Above median wage			
	Low sickday	High sickday	Pvalue	Low sickday	High sickday	Pvalue	
Male	0.65	0.56	0.00	0.91	0.78	0.00	
Age	32.72	35.25	0.00	36.98	40.56	0.00	
Weekly wage	438.45	481.38	0.00	1,004.64	1,013.52	0.00	
Median wage	0.00	0.00		1.00	1.00		
WC days paid	16.70	15.81	0.00	17.14	15.40	0.00	
Retroactive payment	115.46	128.33	0.00	258.04	263.82	0.00	
Daily benefit	58.47	64.20	0.00	129.58	132.16	0.00	
Medical cost	2,278.79	2,297.04	0.67	2,553.61	2,388.61	0.00	
Wed	0.33	0.33	0.18	0.33	0.33	0.90	
Thu	0.33	0.34	0.00	0.34	0.34	0.19	
Fri	0.34	0.32	0.00	0.33	0.33	0.23	
Afternoon	0.54	0.51	0.00	0.44	0.49	0.00	
Trauma	0.04	0.04	0.72	0.04	0.04	0.77	
Fracture	0.10	0.09	0.00	0.13	0.10	0.00	
Strain	0.58	0.62	0.00	0.55	0.64	0.00	
Wound	0.26	0.22	0.00	0.24	0.18	0.00	
Other	0.04	0.04	0.04	0.04	0.04	0.62	
Agriculture	0.09	0.00	0.00	0.19	0.00	0.00	
Construction	0.14	0.01	0.00	0.41	0.02	0.00	
Manufacturing	0.00	0.35	0.00	0.00	0.34	0.00	
Trade	0.30	0.09	0.00	0.21	0.10	0.00	
Transportation	0.00	0.12	0.00	0.00	0.22	0.00	
Other	0.47	0.44	0.00	0.19	0.32	0.00	
Observations	32,999	29,168		19,133	34,527		

Notes: Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Claims are included in the high sick day prevalence category if at least 50 percent of workers in the claimant's industry has access to paid sick leave. sick day prevalence estimates obtained using the National Compensation Survey, 2010 and Employee Benefits Survey, 1999.

Table 2.16: Observable Characteristics by EAIP Participation

(a) Below median wage				(b) Above median wage			
	Never EAIP	Used EAIP	Pvalue	Never EAIP	Used EAIP	Pvalue	
Male	0.63	0.56	0.00	0.88	0.77	0.00	
Age	32.99	34.28	0.00	38.01	40.00	0.00	
Weekly wage	455.24	469.50	0.00	1,001.56	1,031.90	0.00	
Median wage	0.00	0.00		1.00	1.00		
WC days paid	14.37	14.16	0.30	14.78	13.63	0.00	
Ret pmt	120.30	124.43	0.00	256.77	267.84	0.00	
Daily benefit	60.72	62.61	0.00	129.38	133.95	0.00	
Medical cost	2,077.60	2,168.59	0.08	2,356.56	2,220.84	0.00	
Wed	0.33	0.33	0.97	0.33	0.33	0.28	
Thu	0.33	0.34	0.20	0.34	0.35	0.12	
Fri	0.34	0.33	0.21	0.33	0.32	0.01	
Afternoon	0.53	0.52	0.30	0.46	0.49	0.00	
Trauma	0.04	0.04	0.61	0.04	0.04	0.26	
Fracture	0.09	0.08	0.00	0.12	0.10	0.00	
Strain	0.57	0.63	0.00	0.57	0.64	0.00	
Wound	0.26	0.22	0.00	0.23	0.18	0.00	
Other	0.04	0.04	0.21	0.04	0.04	0.41	
Agriculture	0.05	0.03	0.00	0.10	0.03	0.00	
Construction	0.10	0.03	0.00	0.21	0.09	0.00	
Manufacturing	0.16	0.18	0.00	0.22	0.23	0.13	
Trade	0.19	0.23	0.00	0.14	0.14	0.11	
Transportation	0.05	0.07	0.00	0.12	0.17	0.00	
Other	0.45	0.46	0.04	0.20	0.35	0.00	
Observations	33,463	17,317		25,186	20,728		

Notes: Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Claims are separated depending on whether or not the employer ever used the Employer at Injury Program (EAIP), a program that subsidizes employer efforts to induce employees to return to work after a WC claim.

Table 2.17: WC Benefit Parameters in Oregon vs All Other States, 2012

	(1) Oregon	(2) All other states
Replacement rate	0.67	0.68
Minimum weekly benefit	50	151
Maximum weekly benefit	1120.55	832.08
Median hourly wage	17.14	16.64
Median weekly wage	685.60	665.61

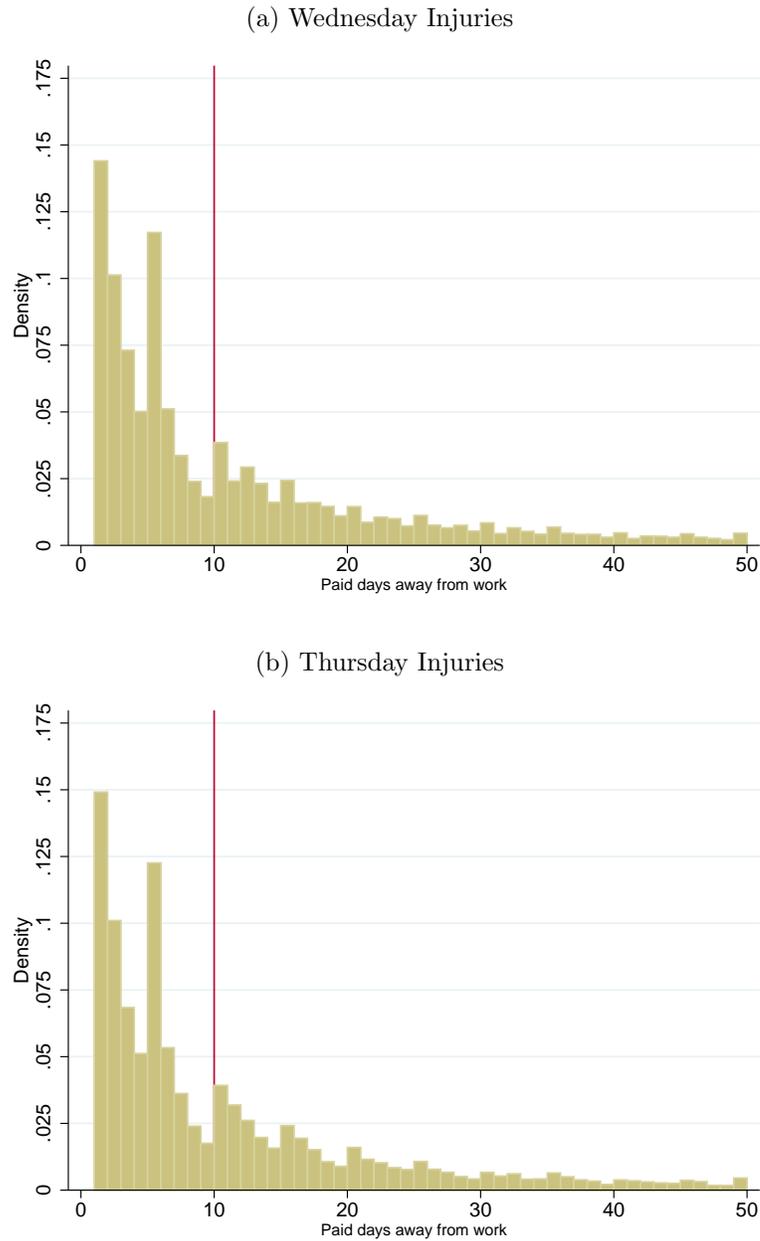
Notes: Data from the Worker's Compensation Research Institute, 2012, and the Occupational Employment Statistics, 2012. The all other states column represents an average of the values from all states excluding Oregon. All dollar values in 2012 dollars.

Table 2.18: Demographic and Savings Habits in Oregon vs All Other States, 2009

	Other states	Oregon	P-value
Demographics			
Female	0.51	0.52	0.33
Nonwhite	0.20	0.14	0.00
Married	0.40	0.40	0.97
Ed < HS	0.34	0.30	0.01
Ed - HS	0.20	0.21	0.41
Ed- some college	0.26	0.28	0.04
Ed - BA+	0.20	0.20	0.89
Work-limiting disability	0.04	0.04	0.15
Monthly earnings	1,516.27	1,410.79	0.22
Benefit receipt			
On WC	0.00	0.00	0.37
Rec noncash ben	0.33	0.33	0.81
Rec cash ben	0.08	0.08	0.53
Debt and Savings			
Total debt owed	4,306.98	3,659.21	0.13
Non-interest checking acct value	242.23	302.07	0.05
Interest acct value	5,558.53	6,412.43	0.12
Have any debt	0.34	0.39	0.00
Have non-int check acct	0.18	0.25	0.00
Have an interest acct	0.43	0.50	0.00
Observations	90,177	1,042	

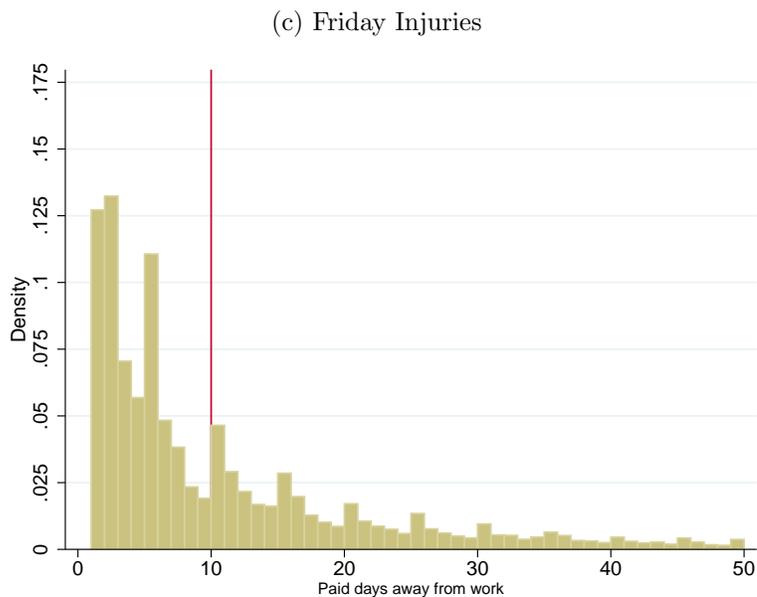
Notes: Data from the 2008 Survey on Income and Program Participation, wave 4 ([U.S. Census Bureau 2015](#)). The “all other states” column represents an average of the values from all states excluding Oregon. All dollar values in 2012 dollars. Statistics calculated with SIPP respondent weights.

Figure 2.7: Distribution of WC Claim Duration, by Day of the Week of Injury



Notes: Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Sample includes claims occurring on a Wednesday, Thursday or Friday that lasted at most one year. The x-axis represents the duration of claims, measured by the number of workdays for which benefits were paid. Because the sample is limited to claimants working five days per week, 10 days corresponds to two weeks.

Figure 2.7: Distribution of WC Claim Duration, by Day of the Week of Injury (continued)



Notes: Data from Oregon Department of Consumer and Business Services, WC claims from 1987-2012. Sample includes claims occurring on a Wednesday, Thursday or Friday that lasted at most one year. The x-axis represents the duration of claims, measured by the number of workdays for which benefits were paid. Because the sample is limited to claimants working five days per week, 10 days corresponds to two weeks.

## Chapter 3: A Double Safety Net? Interactions between Government and Family Assistance for the Disabled

### 3.1 Introduction

Disability is a large health and income shock which can significantly lower an individual's income and consumption over a long period of time ([Meyer and Mok 2013](#)). Disabled individuals often draw upon a patchwork of support systems, including Social Security Disability Insurance (DI), to respond to this large shock. While approximately one-third percent of the working population has access to private disability insurance through their employer ([Autor \*et al.\* 2014](#)), there are few other private sources of long-term support for disability. The family, however, is uniquely positioned to support the disabled. Family members can provide more personal support, and might be able to provide assistance quickly, for example, by paying for a prescription before a benefit check arrives. Family assistance also might provide complementary support, for example, by assisting the disabled in managing their finances and navigating the complicated bureaucratic disability system. Together, these casual forms of assistance make up a significant insurance network: recent estimates approximate that the monetary value of informal care for aging

and disabled adults ranges between \$150 and \$450 billion ([Arno \*et al.\* 1999](#); [Chari \*et al.\* 2015](#); [Feinberg \*et al.\* 2011](#); [O’Shaughnessy 2013](#)).

In this paper, I examine how DI affects the level of assistance provided by the family. I study this question empirically using a fixed effects, difference in differences research design with panel data from the Health and Retirement Study (HRS) on both family transfers and disability insurance. I examine transfers from grown children to their disabled, aging parents, and control for time-invariant factors affecting transfers between families. I find that while the probability of receiving a monetary transfer increases slowly after the onset of the disability and peaks around the time of DI receipt, the probability of receiving an in-kind transfer increases sharply following the onset of the disability and persists following DI receipt.

In order to identify the effect of DI on transfers, I compare monetary and in-kind transfers before and after the onset of the disability for DI recipients and two control groups: rejected applicants and disabled individuals who do not apply for DI. After including time-varying controls and an individual-level fixed effect, the confidence intervals on my estimates allow me to reject that DI reduces the probability of receiving a transfer by more than 3 percentage points, and find that DI could increase the probability of receiving a transfer by up to 5-7 percentage points. These findings suggest that crowd out of family transfers in response to DI is lower than crowd out of family transfers in response to other social insurance programs. Additionally, receipt of DI significantly increases the probability of a transfer to individuals with less observable disabilities such as arthritis or back pain,

cases where the family likely had incomplete information about the disability prior to DI receipt. As a result, DI could send a welfare-improving information signal.

The extent of crowd out characterizes DI's role in increasing insurance coverage against the health and income risks of a disability. In an extreme case of perfect crowd out, public DI simply replaces existing private insurance networks and does not change the overall level of insurance coverage (Gruber 2013). Since DI is paid for with tax dollars that generate deadweight loss, this indicates that provision of DI could be less efficient than existing private insurance. However, in the other extreme, if there are no private insurance alternatives, then increasing the provision of public insurance increases the overall insurance rate commensurately, indicating that public insurance plays an irreplaceable role in insuring the population against the negative shock of disability.

An extensive literature examines whether public insurance crowds out formal private insurance, and concludes that some degree of crowd out exists (e.g., Cutler and Gruber, 1996; Duggan and Kearney, 2007; Gruber and Simon, 2008; Schoeni, 2002). However, when the private insurer is a family member, this mitigates some of the efficiency costs of crowd-out. If DI crowds out family transfers following receipt of DI, this alleviates the family member's cost of providing the transfer, while leaving the disabled individual equally well off. In this case, DI does replace existing private insurance networks, but in so doing, reduces costs on the family. On the other hand, the transfers could be relatively easy for the family member to provide, but could significantly improve the disabled individual's well-being. If the gain to the disabled individual is greater than the cost to the family, private transfers would increase

the family's overall welfare. When the cost of transfers to the family is relatively low, shifting to public insurance would only have a small effect on family member's well-being.

Relatively little work examines the interaction between family transfers and insurance empirically.<sup>1</sup> [Schoeni \(2002\)](#) studies the interaction between unemployment insurance (UI) and family transfers using the Panel Study of Income Dynamics (PSID) and exploiting variation in the maximum level of unemployment insurance (UI) across states. He estimates that one dollar of UI crowds out 24-40 cents of transfers from the extended family. In the case of a disability, however, families could provide in-kind assistance in addition to monetary assistance. [McGarry and Schoeni \(1995\)](#) provide descriptive evidence that children are more likely to provide financial and in-kind assistance to parents with lower incomes and who are in poor health, but do not examine explicit changes in transfers at the time of the onset of a disability or interactions with public benefits.

A related literature examines interactions between social insurance and in-kind transfers to older adults. [Engelhardt \*et al.\* \(2005\)](#) takes advantage of the Social Security "notch" to determine how a change in the generosity of Social Security benefits affects elderly living arrangements, and finds that decreasing benefits increases the share of elderly who live with family or others. Using variation across states over time, [Orsini \(2010\)](#) build on [McKnight \(2006\)](#) and finds that decreasing the generosity of the Medicare reimbursement policy resulted in a small but significant increase

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<sup>1</sup>A related literature examines how public assistance affects spousal labor supply, and finds that more generous public benefits reduce spousal labor supply both in context of unemployment insurance and disability insurance (e.g., [Autor \*et al.\* 2015](#); [Coile 2004](#); [Cullen and Gruber 2000](#)).

in the proportion of elderly living with someone besides their spouse. Other work finds suggestive evidence of substitution between long-term home care in Canada and in-kind transfers of time (Stabile *et al.* 2006). This research leads to a natural question of how public assistance for the disabled, a younger population, but one with significant health impairments, interacts with in-kind transfers and living arrangements.

Disability is also a unique setting to study the role of the family due to the frequent challenges of accurately observing a disability and understanding how the disability will evolve over time. With perfect information about the disability and likelihood of receiving DI, families could anticipate the disabled individual's need for assistance over time. However, under the assumption of imperfect information, families may change their transfer decisions if they learn new information about the severity of the injury when the individual begins receiving DI.

Throughout this paper, I elaborate on these potential interactions between the behavior of disabled individuals, their families, and the government. As the disability rolls increase, it is increasingly important for researchers and policymakers to understand the sources of support that the disabled use to smooth consumption following this large negative shock. Understanding how disabled individuals and their families respond to the benefits and incentives of DI provides information about the program's importance in the safety net for the disabled, and the various ways this program interacts with the family.

## 3.2 Conceptual Framework

In order to examine the implications of potential crowd out of family assistance in response to receipt of DI, I draw upon a framework from [Chetty and Saez \(2010\)](#), which examines how endogenous private insurance affects the level of optimal public benefits. In this paper, I focus on one particular private market: insurance provided by the family. If families had perfect information about the disability, they could choose the optimal level of transfers ex-ante, and government insurance would not change the family's response. However, this assumption may not be true in all cases, in particular for extended families who do not share a household. While the degree of asymmetric information is likely lower between family members than in other private markets, disabled individuals and their families still could have different information about the disability. Families may not appreciate the severity of the disability, particularly with mental disabilities or chronic pain conditions that occur gradually. Prior to the application decision, individuals and their families are also uncertain about whether or not the individual will receive DI.

Chetty and Saez show that the level of public insurance that maximizes social welfare is represented by the following equation<sup>2</sup>:

$$\frac{dW}{db} = (1 - e)(1 - r)u'(c_H) \left[ \underbrace{\frac{u'(c_L) - u'(c_H)}{u'(c_H)}}_{(A)} - \underbrace{\frac{\epsilon_{1-e,b}}{e} \cdot \frac{1 + \frac{b_p}{b}}{1 - r}}_{(B)} \right] \quad (3.1)$$

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<sup>2</sup>See the appendix for more detailed discussion of [Chetty and Saez \(2010\)](#).

The first term in brackets (A) represents the extent to which increasing public insurance benefits would increase claimants' ability to smooth their consumption, and the second term (B) indicates the extent to which increasing public insurance benefits will decrease the probability that the claimant will work ( $1 - e$ ). While (B) reflects a combination of a welfare gain to workers and the moral hazard costs of insurance, the (A) provides a measure of the relative value of an additional dollar when an individual is employed compared to when the individual is out of work (disabled). Together, the relative magnitude of these two effects determines where the current benefit level is relative to the optimal benefit level. At the optimum,  $\frac{dW}{db} = 0$  and the two terms are equal.

The degree of crowd out is represented by  $r = \frac{-db_p}{db}$ , the extent to which private benefits ( $b_p$ ) change as public benefits change. In a world without private insurance (e.g. [Chetty 2008](#)),  $r = 0$  and  $\frac{b_p}{b}(b) = 0$ . In this setting, estimates of (A) and (B) are sufficient statistics to determine the optimal level of benefits. However, in a world with private insurance, the crowd-out parameter ( $1 - r$ ) is an additional statistic needed to determine the optimal level of benefits. Because crowd out amplifies the overall elasticity in (B) but does not directly affect (A), this implies the level of public benefits should be lower when crowd out exists.

In this framework, the initial level of private insurance is based on expectations about the risk of facing a negative shock. If individuals perfectly anticipate their disability risk, and private insurers have perfect information to set the benefit level optimally, there would be no first-order effect of a change in public benefits on private benefits. However, incomplete information about health or the likelihood of receiving

benefits could lead private insurers to make errors in setting the private benefit level  $b_p(b)$ . As a result, private benefits could adjust to changes in the provision of government benefits, affecting the overall welfare gain of providing public insurance.

### 3.2.1 Crowd Out and the Family

The representation of the family's utility and budget constraint has important implications for how family transfers affect optimal benefits (Chetty and Saez 2010; Di Tella and MacCulloch 2002). While a unitary household model predicts significant crowd out when the family has altruistic motives (Becker 1974), if families have other incentives to provide transfers, such as warm glow or exchange motives, crowd out could be less than complete, or not occur at all (Andreoni 1990; Cox 1987). Furthermore, a body of research rejects that family utility can be accurately described by one utility function. Research has come to this conclusion both within households (Browning and Chiappori 1998) and among extended family in different households (Altonji *et al.* 1992; Choi *et al.* 2015). Under the assumption that extended families share one budget constraint, transfers from the extended family are simply a form of self-insurance, and role of the family would be captured in (A), simply increasing the disabled individual's ability to smooth his consumption rather than in the crowd out parameter  $r$ . In this paper, I assume that the beneficiary and her family have separate utility functions and separate budget constraints.

To see how these assumptions affects the optimal level of benefits, I extend the Chetty and Saez model to include two groups of agents: disabled individuals

and their families. For simplicity, I assume that each disabled individual has one family member whose utility is represented by  $u_F(c)$ . While the disabled individual pays government taxes when she is in good health and receives benefits when she is in poor health, I assume her family member never becomes disabled and always pays taxes. Under the assumption that families are uncertain about the severity of the disability and probability of receiving DI, the equation determining the optimal level of benefits is:

$$\frac{dW}{db} = (1 - e)\theta \cdot \left( \underbrace{\left[ \frac{((1 - r)u'(c_L) + r \cdot u'_F(c_L)) - \frac{\theta}{(1+e)}}{\theta}}_{(A)} \right]}_{- \epsilon_{1-e,b} \left[ \underbrace{\frac{2}{(1+e)^2} + \frac{u_F(c_H) - u_F(c_L)}{\theta \cdot b}}_{(A)} \right]} \right), \quad (3.2)$$

where

$$\theta = e(u'(c_H) + u'_F(c_H)) + (1 - e) \cdot u'_F(c_L) \quad (3.3)$$

Broadly, the first term in brackets still represents the consumption smoothing benefit of DI, and the second term in brackets represents the overall elasticity of work with respect to a change in  $b$ . However, unlike in equation (3.1), here the crowd out parameter weighs the two agents' marginal utilities. If there is no crowd out ( $r = 0$ ), the family provides the same level of transfers with or without public insurance, leaving their utility unaffected, and the level government transfers only affects the

recipient's utility. Alternatively, if  $r = 1$ , the effect public transfers spills over completely to the family, causing them to reduce the transfer  $b_p(b)$ , and increasing the family's consumption. In this case, the disabled individual will maintain the same income level with or without public insurance. Finally, the term  $\frac{u_F(c_H) - u_F(c_L)}{\theta} \cdot \frac{1}{b}$  reflects the fact that the disabled's reduction in work effort also affects the family.

If the family has incomplete information prior to DI receipt, they may update their transfer behavior when the individual receives DI.<sup>3</sup> On one hand, families may reduce transfers now that the individual receives income from DI. As in the standard crowd out case, this response would indicate some inefficiency in the government providing transfers that the family would have been able to provide. However, in the case of the family, the inefficiency of public insurance is offset in part by the fact the family no longer needs to provide transfers, and can consume those resources instead. In other words, some of the benefits of DI spill over to the family. On the other hand, DI may signal that the disability is more severe than the family anticipated and could actually *increase* transfers. If DI increases transfers, the weighting term  $(1 - r) > 1$ , indicates that the DI recipient receives *higher* transfers from the family with DI than without DI. While families incur the cost of providing additional transfers, the weighted sum of utilities in (A) indicates that the increase in transfers to the disabled could increase overall social welfare if the disabled individual's marginal utility of consumption is higher than the marginal utility of the family member.

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<sup>3</sup>If the individual and her family systematically overestimate her health, then benefits based on this biased expectation will be too low. On the other hand, if they systematically underestimate her health, then benefits based on this expectation will be too high (Spinnewijn 2015). While many researchers have hypothesized that individuals may have biased beliefs or biased reports of their health, the extent and direction of this bias is unclear. See Benitez-Silva *et al.* (2003) for a review of this literature.

In my empirical analysis, I estimate the crowd out parameter  $r$  to determine how government transfers affect family transfers. Secondly, I estimate the effect on monetary and in-kind transfers separately to study different margins through which families may respond.<sup>4</sup> I use panel data over time to distinguish a family's response to the onset of disability from the response to DI receipt. Then, to test whether DI sends a separate information signal to the family, I compare transfers to recipients with more and less observable disabilities. If families of individuals with less observable disabilities increase transfers following receipt of DI, then DI could be sending an information signal about the severity of the disability that could improve the disabled individual's welfare. However, if the response to DI is the same for more and less observable disabilities, then the family's response is more likely due to substitution or complementarities between public and family assistance.

### 3.3 Empirical Approach

DI benefits are a function of prior earnings and there is little variation in the size of benefits across places or over time. As a result, researchers increasingly use administrative data and exploit either random assignment in the DI application process (e.g., [Autor \*et al.\* 2015](#); [French and Song 2014](#); [Maestas \*et al.\* 2013](#)), or examine large policy changes (e.g., [Deshpande 2015](#); [Kostol and Mogstad 2014](#); [Moore 2015](#)) to identify causal effects. However, links between family members in U.S. administrative data are limited, and more importantly, administrative records

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<sup>4</sup>See the appendix for an expansion of the framework that incorporates the family's choice between giving in-kind and monetary transfers.

do not contain information about informal support. As a result, I use HRS survey data, which contains detailed information about DI participation and interactions between family members for this analysis. I take advantage of the HRS panel, which allows me to examine changes in transfers within families over time, and use detailed information on health and income in order to control for time-varying factors that likely affect participation in DI and family transfers. Additionally, I compare the treatment group, DI beneficiaries, with two control groups to address additional concerns about selection into DI participation. I use a fixed effects, difference in differences model to identify the impact of DI on family transfers:

$$Y_{it} = \alpha + \beta_1 H_{it} + \beta_2 H_{it} * D_{it} + X_{it} \delta + \alpha_i + \gamma_t + \epsilon_{it}. \quad (3.4)$$

$Y_{it}$  is an indicator for whether or not individual  $i$  receives a certain transfer type in year  $t$ ;  $H_{it}$  indicates whether or not individual  $i$  experiences a disability that limited her ability to work in wave  $t$ ; and  $D_{it}$  indicates whether or not individual  $i$  receives DI in wave  $t$ . The panel structure of the HRS allows me use an individual-level fixed effect  $\alpha_i$  to control for time-invariant characteristics that affect the family's propensity to provide transfers. Characteristics such as gender, family size and unobservable factors, such as the quality of the individual's relationship with her family, may all affect the level of transfers, but likely do not vary over time. I control for other observable factors that do change over time, such as marital status, assets, and earnings, in the vector  $X_{it}$ , and account for trends over time with  $\gamma_t$ . To adjust

for differential sampling rates due to the HRS sampling scheme, I use respondent level weights from the HRS in the regression analysis.

In order to distinguish a change in transfers due to DI from a change in transfers due to the disability itself, I take advantage of the lag between the onset of the disability and the time when the individual applies for and receives DI. On average, HRS respondents report applying for DI approximately 2.4 years after the first report of a health condition limiting work. The parameter  $\beta_1$  captures the family's response to the onset of the disability. The parameter on the interaction term between health and receipt of DI,  $\beta_2$ , represents how the probability of receiving a transfer from the family changes in response to DI. A rejection of the null hypothesis  $\beta_2 = 0$  would suggest that DI affects the probability of receiving a transfer. Furthermore, a rejection of the null hypothesis in favor of the alternative hypothesis  $\beta_2 < 0$ , this suggests that DI reduces the probability of receiving a family transfer. In order to identify this parameter, I use difference in differences to compare transfers between recipients and two control groups: rejected applicants and a sample of disabled individuals who do not apply for DI. In each of these control samples, the main identifying assumption is that there are no time-varying, unobservable factors affecting the probability of a transfer that are correlated with receipt of DI. In other words, I assume that the sample of recipients would have had similar trends in transfers as each control group if they did not receive DI. I use rich information included in the survey to control for many observable factors in order to compare DI beneficiaries to two counterfactual groups of disabled individuals.

If individuals who receive DI are in worse health than individuals who do not, they might experience diverging trends in transfers due to their worsening health. In order to compare the effect of DI on individuals with similar degrees of disability, I also include specific measures of health in  $X_{it}$ . However, all health measures in the HRS are self-reported and could be measured with error. Indeed, there is a body of research showing that even self-reports of specific, verifiable conditions could be measured with considerable error (Baker *et al.* 2004). However, some work studying the HRS specifically suggests that health measures in the HRS may not be severely biased. Benitez-Silva *et al.* (2004) examines the potential bias in the HRS variable indicating whether the respondent's health limits their ability to work, and the authors are unable to reject a hypothesis that this measure is unbiased.<sup>5</sup> If respondents who exaggerate their poor health are also more likely to receive DI, perhaps as ex-post justification for receiving benefits, then including health controls could attenuate the interaction term on DI. If health is endogenous to DI, then controlling for health could further attenuate the effect of DI on transfers.<sup>6</sup> As a result, I present the main regression results with and without specific measures of health status in a given wave, including whether the respondent had issues with mobility and the total number of doctor visits since the last interview. The results excluding and including health controls likely present an upper and lower bound for the effect of DI on family transfers, respectively.

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<sup>5</sup>While this strengthens the validity of the HRS health measures, I conduct the analysis using a variety of health measures in robustness checks to examine how sensitive the results are to the choice of controls for health status.

<sup>6</sup>For example, Moore (2015) demonstrates that DI could provide time for rehabilitation for certain types of disabilities.

### 3.3.1 Control Groups and Selection

I first compare DI beneficiaries to individuals who apply for DI during the survey and are rejected. While both groups experience the onset of a disability and participate in the DI system, acceptance into DI is not random. If rejected applicants are more able to recover from their impairment and as a result, require less help from their families over time, selection into DI receipt could understate any crowd out, or overstate any crowd-in. Additionally, rejection itself could be an alternative form of treatment. If rejection from DI sends a signal that the individual is not severely disabled, families may reduce their transfers. This alternative treatment would again understate any estimate of crowd out, or over-estimate crowd-in.

As a result, I also compare DI beneficiaries to individuals who report chronic disabilities in the HRS but do not apply for DI. Since these individuals do not apply for DI, there are no concerns of the control group experiencing any treatment from being rejected from DI. However, this sample is likely different in other ways that led them not to apply for DI: most importantly, they could also have less severe disabilities, or could have more financial resources. To address these concerns, I reweight the sample of non-applicants by an estimated propensity score of applying for and receiving DI in order to compare DI beneficiaries to a sample of individuals who have a similar distribution of observable characteristics. Combining DI beneficiaries with a sample of disabled non-applicants, I estimate a propensity score for being in the treatment group using the stepwise-regression procedure outlined in [Imbens](#)

(2014).<sup>7</sup> I include measures of health status, income and assets in the estimation of the propensity score, along with other demographic characteristics including gender, age and marital status. Figure 3.1a shows that the distribution of propensity scores for DI beneficiaries and disabled, non-applicants satisfy the common support condition over the entire range of propensity scores. I reweight the health sample to reflect the distribution of propensity scores in the recipient sample, as shown in Figure 3.1b.<sup>8</sup>

While this control group avoids the problems of selection into DI receipt, it presents an alternative concern of selection into DI application. If disabled individuals do not apply for DI because they expect to receive assistance from their family in the future, this selection problem could overestimate any reduction in transfers in response to DI receipt. However, the summary statistics presented in tables 3.1 and 3.2 show evidence that beneficiaries in fact are more likely to receive transfers than non-applicants at the baseline. Additionally, tables 3.10 and 3.11 show the results of a logistic regression predicting the probability of being included in the treatment sample and the probability of ever applying for DI, respectively. Table 3.11 shows that while all transfer types are positively correlated with applying for DI, this correlation is not significantly different from zero even after controlling for health conditions.

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<sup>7</sup>See the appendix for more details on the propensity score estimation process.

<sup>8</sup>I rescale the propensity score weights using the HRS respondent weights to preserve the proportion of respondents in treatment and control groups, as suggested in Nichols (2008).

### 3.4 Data

I use the HRS study transfers from grown children to their parents, HRS respondents.<sup>9</sup> The HRS is a longitudinal panel survey of adults over age 50 in the United States. Since 1992, the survey has tracked a representative sample of individuals every two years, adding new panels to the survey as people age. This survey contains detailed information on health, disability, family structure, transfers to and from children, and information on application, receipt and appeal of DI.<sup>10</sup> Additionally, the panel structure of the HRS allows me to observe individuals before and after they applied for DI and provides the application date and date of DI receipt.<sup>11</sup>

In the HRS, over 60 percent of disabled respondents report receiving some type of assistance from their children in at least one wave of the survey, and approximately 20 percent of respondents report receiving a transfer from their children in the first wave. Among disabled respondents who report receiving assistance from their children, the average size of a monetary transfer is approximately \$1,500 over two years, and average amount of time spend providing in-kind assistance is approximately 17 hours per month. While the monetary support from children is small

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<sup>9</sup>The HRS (Health and Retirement Study) is sponsored by the National Institute on Aging (grant number NIA U01AG009740) and is conducted by the University of Michigan. The RAND HRS Data file is an easy to use longitudinal data set based on the HRS data. It was developed at RAND with funding from the National Institute on Aging and the Social Security Administration. I use the Rand HRS Data Versions M and B (RAND 2012, 2013) for the majority of the analysis. In some cases I supplement with additional data from the HRS core data (Health and Retirement Study 2013).

<sup>10</sup>Prior to 2000, the HRS asked about DI and SSI together. To maintain consistency throughout the entire panel, I do not distinguish between the two programs in the analysis.

<sup>11</sup>For a complete explanation of how the disability application process works, see Chen and van der Klaauw (2008).

relative to the cash assistance provided by DI, children provide important in-kind assistance to their disabled parents.

I select a sample of individuals who apply for DI in the middle of the survey in order to have information before and after applicants receive notification about DI (the applicant sample). The survey contains 5,334 respondents who ever report applying for DI; out of this sample I observe 1,004 who receive their first DI check during the panel, and an additional 653 who applied to DI in the middle of the survey, but were rejected. The remaining DI applicants either applied before the survey, or in the last wave that they were interviewed. Because the majority of questions ask about transfers from a child, the receiving and rejected samples are limited to individuals with children. Approximately 90 percent of DI applicants in the survey have children. In constructing the control group of disabled non-applicants, I identify an individual as disabled if she reports a health condition limiting work in at least two waves in the survey, and only include individuals who never report applying for SSDI and who are below the full retirement age (the health sample).<sup>12</sup> There are 2,261 non-applicants who meet these criteria.

I consider monetary transfers, in-kind transfers and shared living arrangements as dependent variables. The monetary transfer variable indicates whether or not the individual received a monetary transfer from their child since their last interview.

The indicator for receipt of in-kind assistance from a child includes whether the

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<sup>12</sup>Results are robust to selecting the sample based on individuals who report having a health condition limiting work in more than two waves, and selecting the sample based on the first report of other disabilities. The sample restriction of excluding individuals who ever apply for DI yields more conservative estimates (in terms of significance, but of a similar magnitude) on crowd out than a sample based only on individuals who report a work-limiting health condition in at least two waves. See the appendix section on propensity score estimation for more details on this sample.

respondent receives assistance with activities of daily living (ADLs, e.g., bathing, dressing), instrumental activities of daily living (IADLs, e.g., grocery shopping, money management, making phone calls), or assistance with finances. I also consider two measures of living transfers: one, simply if the respondent reports that they moved in with their child since the last interview, and secondly, if the respondent reports that this move was mainly for her benefit, rather than for her child's benefit. In addition to the data on disability application status and transfer status, I also construct variables indicating marital status, health, family structure, age, and employment from the survey. See the appendix for more information on the construction of the control and treatment groups, weighting and construction of additional covariates for the analysis.

### 3.4.1 Summary Statistics

In order to compare how individuals in different samples evolve over time, tables 3.1 and 3.2 compare the beneficiary sample at first and last interview to rejected applicants and disabled non-applicants. Table 3.1 shows that at their first interview, beneficiaries and rejected applicants are similar along dimensions of labor force participation, marital status, and a number of health criteria. In addition, they have similar numbers of children (or children in-law), and receive transfers at similar rates. The majority of these characteristics remain similar at the time of the last interview. At the last interview, rejected applicants are more likely to be in the labor force, less likely to receive public health insurance and are slightly health-

ier. However, the two groups are equally likely to report several health conditions including back problems, mental health conditions, and diabetes. At their last interview, rejected applicants are less likely to receive in-kind transfers from their family, although the differences are not statistically significant. Table 3.2 displays similar trends between recipients and the disabled non-applicant control group. Notably, non-applicants have fewer mobility problems, fewer diagnoses of mental health conditions, and are less likely to be hospitalized. Perhaps due to their better health, they are less likely to receive any type of transfer.

These summary statistics reveal several important points. First, the data does not show evidence that transfer recipients select out of DI application: disabled non-applicants receive transfers at similar rates as rejected applicants at the baseline. Secondly, the two control groups are observationally similar not only to the treatment group, but also to one another. Because of these similarities, the health control group could provide evidence about the extent of bias due to selection into DI receipt, or the effect of rejection. Finally, both control groups appear to be in slightly better health than the treatment group. Without controlling for health status, the interaction on DI and disability could reflect the differences in severity and need between the treatment and control groups. As a result, I present results with and without health controls, presenting an upper and lower bound of the possible crowd out effect.

### 3.4.2 Trends in Health, Income and Transfers

Figures 3.2a and 3.2b demonstrate the size of the health and income shock around the onset of disability. In each graph, the light grey vertical line indicates the onset of the disability, the dark red vertical line indicates the average number of years after the onset of the disability before DI application. Figure 3.2a shows that respondent earnings decline around the onset of the disability. While all three groups experience a decline in their income, DI beneficiaries experience the sharpest decline, losing over half of their pre-disability earnings. Rejected DI applicants experience approximately a 50 percent decline in earnings around the time of disability onset, and non-applicants experience the most gradual decline in their income. While figure 3.4b shows that income from Unemployment Insurance, Worker's Compensation and other government transfers, increase around the onset of the disability, these increases do not offset the decline in earnings. Additionally, figure 3.4a spousal earnings experience a slow decline following the onset of disability, perhaps because spouses work less in order to spend more time caring for their disabled husband or wife.

As a result of the decline in respondent earnings and, to a lesser extent, spousal earnings, figure 3.4c shows that total household income declines by 40 percent and 20 percent for DI beneficiaries and rejected applicants following the onset of a disability, respectively. This magnitude of this decline is similar other research: Meyer and Mok (2013) finds that individuals experience a 35 percent decline in after-tax income following the onset of a chronic disability using the PSID. The smaller decline in

total household incomes for non-applicants suggests that they draw upon savings or assets following the onset of the disability, or that they maintain some capacity to work if the disability onsets gradually. I control for total assets and earnings in the regressions to address these concerns.

At the same time that income declines, figure 3.2b shows that DI beneficiaries and both control groups experience a significant decline in their health. Figure 3.2b shows the total number of poor health conditions reported increases from approximately 3 conditions prior to the disability to 6 at the time the respondent first reports having a health condition that limits their work. While the shock is again slightly smaller for disabled non-applicants, all groups experience a similar trend in disability severity. In the years following the onset of the disability, the number of poor health conditions slowly increases in parallel for all three groups, in contrast to the sharp increase around the time of the onset of a work-limiting condition. Together, figures 3.2a and 3.2b confirm that disability presents a significant shock to all households in the treatment and control samples for this analysis.

Figures 3.3a - 3.3c display the trends in transfers around the time of disability onset for both control groups. Figure 3.3a graphs the share of DI recipients and the two control groups who receive a monetary transfer from their children in the years before and after they receive DI. This figure does not show evidence of a large change in monetary transfers before or after the onset of disability for the treatment or either control group.<sup>13</sup>

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<sup>13</sup>Previous versions of these graphs show the timing based on the year of application rather than the onset of disability, and show that monetary transfers to DI recipients peak at the time of DI application.

Figure 3.3b shows a clear break in the share of respondents who receive an in-kind transfers for all three groups, increasing at the time of the disability onset. Transfers continue on an upward trend in the years following onset for recipients and increase sharply again around the time of DI receipt. The trend for disabled non-applicants and rejected applicants, while noisy, displays a similar pattern. While the share of recipients receiving a transfer peaks after receipt of DI, the share remains significantly higher than the period before disability onset, a contrast with the trend for monetary transfers. Figure 3.3c shows that the share of respondents who live with a child declines at the onset of disability and does not change significantly around the time of DI application. This could result from individuals moving into assisted living or being hospitalized around the onset of disability.

### 3.5 Regression Analysis

Next, I build upon these descriptive trends in the regression analysis where I control for individual fixed effects, wave fixed effects and time-varying characteristics including health status and marital status. In each of the regression tables, the first column shows the results of an ordinary least squares (OLS) regression, column (2) includes the individual-level fixed effects, column (3) includes individual fixed effects, survey wave controls, and time-varying controls including marital status, the number of living children, and measures of assets and earnings, and column (4) incorporates health covariates including whether the respondent has issues with mobility, and the number of doctor visits since the last interview. Comparing columns (3) and

(4) demonstrate the effect of including controls for health status in the regression. In each table, panel (a) shows the results using the applicant sample, and the panel (b) shows the results using the health sample.

### 3.5.1 Main Results

Table 3.3 shows the results of equation 3.4 with monetary transfers as the dependent variable. Panel (a) shows that DI applicants are approximately 2-3 percentage points more likely to receive a monetary transfer after the onset of a disability. Since approximately 25-28 percent of disabled respondents ever report receiving a monetary transfer, this represents an increase of approximately 10 percent. While the interaction terms are all positive, none of them are significant. The health sample, shown in panel (b), does not provide evidence that the probability of receiving a monetary transfer does not significantly change following the onset of a disability. The coefficients on the interaction between disability status and DI receipt are positive in all columns in both control samples, although the coefficients are smaller in the health sample. While none of these coefficients are significantly different from zero, the confidence intervals imply that the probability of receiving a transfer falls by no more than approximately 1.5 percentage points.

Table 3.4 repeats the exercise in Table 3.3 with a dependent variable of in-kind transfers rather than monetary transfers. In both panels, respondents are again significantly more likely to receive a transfer after the onset of the disability, consistent with Figure 3.3b. After conditioning on several indicators of health, all

disabled individuals in the sample are approximately 3-5 percentage points more likely to receive an in-kind transfer from their children following the onset of their disability. Additionally, the interaction term is positive and significant in panel (a), indicating that DI could increase in-kind transfers by an additional 5 percentage points. While the interaction coefficient in the health sample is no longer significant after controlling for time-varying observable characteristics and health conditions, the coefficients remain positive and the confidence intervals overlap between the two samples. This suggests that the two control groups could provide bounds on the size of the family's response to receipt of DI. The coefficients with a dependent variable indicating whether not the respondent moved in with a child, shown in table 3.12 in the appendix, are closer to zero, and all are insignificant after controlling for wave fixed effects and time-varying covariates.

Finally, table 3.5 shows the results when pooling all transfer types. The dependent variable in this regression is an indicator equal to one if the respondent receives any type of transfer in a given survey wave. Once again, both the main term and the interaction term are positive and significant in the applicant sample: even after controlling for time-varying characteristics and health conditions, receipt of DI increases the probability of receiving any transfer from children by 7-8 percentage points. As in table 3.4, the coefficients in the health sample are no longer significant after including time-varying controls in the regression.

Table 3.6 shows the regression results on the intensive margin of transfers. To examine the intensive margin, I use measures of the dollar amount of monetary transfers received since the last interview, and the number of hours and days per

month on which children helped their parents. While the results on monetary transfers are not significant in either sample, both samples show that children increased assistance to their parents by about 1.5 days per month after the onset of a disability. Since the unconditional mean of days on which respondents in the health and applicant samples received help from their children is approximately 2 days, this represents an 75 percent increase. This is accompanied by an increase of approximately 5-11 additional hours per month of care provided by children, relative to an unconditional mean of 8.5 hours per month, again a sizeable increase of approximately 67 percent. Panel (a) shows that receipt of DI led children to provide an additional 1.3 days of assistance, although DI does not lead to a corresponding increase in the hours of care. The confidence intervals in panel (a) imply that DI reduces the amount of monetary transfers by no more than \$200, or 14 percent relative to a median transfer amount of \$1500. Similarly, the confidence intervals imply that DI reduces the number of hours of care provided by no more than 3 hours per week. The health sample does not provide evidence that DI significantly increases the number of days or hours of care.

In all regressions, the direction, magnitude and confidence intervals of the coefficients across the two control groups are broadly consistent across the two groups. However, the difference in significance of the results between the applicant sample and the health sample suggest that while DI leads to an increase in transfers to DI beneficiaries relative to rejected applicants, there is no significant change in transfers for DI beneficiaries relative to disabled non-applicants. These results are robust to trimming extreme values for the propensity score of DI participation, and to

using propensity score weights for the rejected applicant sample, rather than using HRS respondent weights. Given the balance in observable characteristics between the treatment group and rejected applicants, propensity score weights do not affect the results in the applicant sample. This suggests that the difference in transfers between accepted and rejected applicants could be due to something other than differences in observable characteristics, and possibly indicate that DI rejection also provides the family with information about the disability.

Including controls for health status do not change the interpretation of results of the effect of DI on transfers dramatically, although they do reduce the magnitude and significance of the interaction term. The health measures control for the severity of the health condition; however, they also could exert a downward bias on the estimates if individuals exaggerate their health status to justify the receipt of DI or the receipt of transfers. Regardless, neither the rejected applicant control group nor the propensity-score reweighted sample yields a strong crowd out result: at most, the results suggest that DI does not crowd out the probability of a transfer by more than 3 percentage points, and could in some cases lead to an increase in transfers from the family.

By construction, I observe the receiving sample in the years surrounding the onset of the disability. Because I examine the transition before and after the onset of disability, I exclude respondents who have been managing their disability for a longer period of time. In these cases, respondents and their children may have already adjusted behavior in response to the disability. If there is a lag between the time of the shock and the financial response to the shock, then focusing on this

sample could yield a lower-bound estimate of the ultimate response of the family.<sup>14</sup> These results are broadly consistent with other results in the literature finding that families respond by providing transfers following a significant health shock (Coile 2004; Faldon and Nielsen 2015; McGarry and Schoeni 1995). While some studies have found little to no effect of long term care programs on informal care giving (Stabile *et al.* 2006), a number of other studies find evidence of some degree of crowd out of family transfers, in particular co-residence decisions (Engelhardt *et al.* 2005; Orsini 2010). Based on the confidence intervals in this analysis, I conclude that crowd out of family assistance in response to DI is significantly smaller than the existing estimates of crowd out of family assistance in response to other social insurance programs, such as unemployment insurance. The lack of crowd out in monetary transfers also departs from other literature finding an effect of social insurance on family transfers (Cullen and Gruber 2000; Schoeni 2002).

### 3.5.2 Observability of Health Condition

The results in table 3.4 and 3.5 suggest that DI could increase transfers from the family. There are several reasons why DI could increase family transfers. First of all, family assistance could complement DI, if the disabled individual now needs help navigating the DI system. Additionally, DI could send a signal about the severity of the condition if children do not have perfect information about the disability. I perform several tests order to further investigate the hypothesis that DI sends a

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<sup>14</sup>Previous versions of this draft included a specification including all DI recipients rather than those with a transition during the survey, and results are broadly consistent with the results presented here.

signal about health. First, I separate the sample based on types of care that would be observable to children: whether or not the respondent has been hospitalized, and whether or not the respondent ever receives home care. Secondly, I estimate equation 3.4 but change  $H_{it}$  to indicate the onset of a particular health condition, rather than the first report of a health condition that limits work. I run this regression examining how the frequency of transfers evolves relative to the onset of several specific health conditions including the first report of back pain, arthritis, and diabetes, which could be less observable to children. Then, I examine how the frequency of transfers evolves relative to the timing of a stroke, the first report of hospitalization, and first report of home care, three events which could be more easily observed.<sup>15</sup>

Table 3.7 shows the results separating the sample by whether or not the respondent was hospitalized or whether or not the respondent received home care, two types of care that would be easily observed by children. Panels (a) and (b) show the results for whether or not respondents ever report being hospitalized for the rejected applicant and propensity score samples, respectively; and panels (c) and (d) show the results for whether or not respondents report receiving home care for the two samples. The dependent variable in each of the four panels is whether or not the respondent received any transfer. In each of the four panels, column 1 shows the coefficients from the regression estimated on the share of the sample that did not have a hospitalization or home care, and column 2 reflects the results for the share of the sample that did have hospitalization or home care.

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<sup>15</sup>Consistent with the observation by McGarry (1998) above, events like a hospitalization or accident (and any assistance resulting from the hospitalization/accident) could also be easier for a parent to recall on a survey than other chronic conditions.

The coefficient on the onset of the health condition is insignificant or even negative for individuals who did not receive observable care across all four panels. By contrast, the interaction term is positive and significant for individuals that did not receive observable care in all specifications, except in panel (b). Additionally, the interaction terms are larger than in the overall sample: even in the health sample, these results indicate that DI increases the probability of a transfer by 9 and 4 percentage points for individuals who were not hospitalized or did not receive home care, respectively. By contrast, the majority of the coefficients reflecting the onset of the health condition are positive and significant for respondents who did receive observable care, and only panel (a) shows that transfers increase significantly in response to receipt of DI among individuals who received observable care.

Table 3.8 shows the results when equation 3.4 is estimated relative to the onset of a specific health condition, rather than the respondent's first report of a health condition limiting work. In both panels, columns (1)-(3) show the results of a regression on an indicator for receiving any kind of transfer relative to the respondent's first report of arthritis, back pain and diabetes, all conditions that might be difficult for the child to observe. Column (4) shows the results relative to the respondent's report of a stroke, and columns (5) and (6) show the results relative to the respondent's first report of a hospitalization or home care (rather than ever reporting that they received home care or were hospitalized). In the rejected applicant sample, the indicator for the onset of the health condition is positive and significant for the more observable events, and insignificant for the less observable events. By contrast, the coefficients on the interaction term are positive

and significant for the less observable events, and generally insignificant for the more observable events. However, consistent with the baseline results, the results on the health sample in panel (b) are broadly insignificant.

I also separate the sample by whether or not a child lives with the parents. Presumably, children who live with their parents are better able to observe the severity of the condition from the onset, and DI would send less of an information signal for these families. Table 3.13 in the appendix shows that transfers both after the onset of the disability and after DI receipt respond more significantly to transfers when the child lives within 10 miles of the disabled respondent.

### 3.5.3 Spousal Response

A child's decision to assist her disabled parents also depends on whether the parent has a spouse who can also help with care. Children might be more likely to assist parents who do not have a spouse in the household who can help with daily living activities, or who could increase work activity following the disability to make up for lost earnings. Table 3.13 in the appendix shows that the increase in transfers after receipt of DI is larger when the respondent is not married, although the confidence intervals between the coefficients for the married and non-married samples overlap. As a further investigation of this relationship, I estimate equation 3.4 with a dependent variable of the number of hours worked by the spouse, the spouse's annual earnings, and an indicator for whether or not the spouse is working in a given survey wave.

The results, shown in table 3.14, do not provide evidence that the onset of a disability leads spouses to exit the labor force. Furthermore, neither the health nor the applicant sample provides evidence that receipt of DI significantly affects spousal labor supply on either the extensive or intensive margins. The lack of a spousal response could result from spouses needing to balance the need to provide care for their spouse and to compensate for their disabled spouse's lost earnings.<sup>16</sup> Children could step in and provide necessary in-kind care to their disabled parent, allowing the non-disabled parent to remain in the labor force and maintain household income

### 3.5.4 Robustness Checks

Table 3.9 displays various robustness checks, all using a dependent variable indicating receipt of any transfer from a child. Since one of the main selection concerns is that DI beneficiaries might be in worse health, column (1) uses different measures of health to control for the severity of the disability. In the main specification, I include measures for the number of times the respondent visited the doctor since the last interview, and also a measure of whether the respondent had issues with mobility. In the robustness check, I include indicators for whether or not the respondent has issues with ADLs or IADLs, whether or not the respondent had a specific diagnosis (e.g., cancer), the individual's self report of being in poor health

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<sup>16</sup>While Autor *et al.* (2015) finds that the spouse's labor force participation responds significantly to receipt of DI in Norway, differences in the labor markets between Norway and the United States could explain the different findings.

in addition to the measures of mobility and doctor's visits. The size, direction and significance of the coefficients are comparable to the main results in table 3.5.

Because the identification comes from the change in DI recipient status, individuals who stop receiving DI could affect the results. In the HRS, 988 people report an end date for DI during the survey wave, 222 of which are included in the receiving sample.<sup>17</sup> Column (2) in table 3.9 demonstrates that the results are quite similar when excluding individuals who exit DI. One main reason that individuals might no longer report receiving DI is they transition to receiving retirement benefits once they reach the retirement age. While this will not affect their benefits significantly, it could affect work decisions, or children's view of the disability. In order to separate retirement factors from disability factors, I also limit the sample to waves where respondents were below the full retirement age.<sup>18</sup> Similar to Column (2), Column (3) shows that the result are statistically indistinguishable when limiting the sample to individuals who are not eligible for retirement benefits for individuals in the rejected applicant sample, and are now significant in the health sample as well.

Additionally, the main sample of DI recipients includes individuals who received DI after appealing the initial decision. Applicants who are initially denied could demonstrate a higher capacity for independent living or may not demonstrate a strong work history (Von Wachter *et al.* 2011). Column (4) excludes individu-

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<sup>17</sup>The share of respondents reporting a termination of benefits in this sample is roughly in line with the overall percentage of DI applicants ending per year due either to death, retirement, or a medical disqualification (Moore 2015). Between 40 and 60 percent of the respondents in the sample who report an end to their DI benefits in any given wave were age 65 or older, suggesting that a substantial fraction of those reporting an end to DI are actually transitioning to OASI.

<sup>18</sup>The full retirement age in my sample ranges between 65 and 66 years.

als who were granted benefits after an appeal. This restriction does not affect the results substantially either.

Finally, column (5) tests whether the results are sensitive to the use of survey weights or propensity score weights, depending on the sample. If there are large differences between the results in the health sample with and without the propensity score weights, this could suggest that the control sample may not be an accurate comparison to the treatment sample, because the two groups differ greatly along observable characteristics (Imbens 2014). Similarly, if the results change dramatically when excluding the survey weights, it suggests that the composition of the sample might be excessively sensitive to changes in the composition of respondents over time. While the main disability coefficient in both the applicant and health sample is larger and significant when weights are excluded, it remains within the confidence interval of the coefficients in the main estimates. Additionally, the interaction term in the rejected applicant sample is not changed dramatically when excluding the weights. However, the interaction term in the health sample is now larger and significant without the weights, suggesting that individuals with less severe disabilities could be over-represented in the health control group.

In general, the results in table 3.9 show that the results are robust to potential transitions out of DI, and that the treatment and control samples likely have substantial overlap along observable characteristics. Even with these more conservative estimates, however, the coefficients remain positive, and I can reject that the probability of a transfer declines by more than 2-3 percentage points. Table 3.15 in the appendix shows that other robustness checks also are consistent with the main

results. These robustness checks include accounting for a potential mechanical effect of transfers not being reported in the first wave of the survey, and inclusion of an indicator for whether an individual receives unemployment insurance, worker's compensation payments, or other government transfers.

### 3.6 Discussion

Disability affects an individual's livelihood, future earnings and savings, and could also affect her family's livelihood and income. Because the costs of an individual's disability spill over to her family, the optimal level of DI benefits should account for interactions with this source of private insurance. In this paper I demonstrate that families offer their disabled relatives both monetary and in-kind assistance. The probability of receiving monetary and in-kind transfers increases following the onset of a disability, and this probability remain significantly higher throughout the duration of the disability regardless of DI status, in particular for in-kind transfers. This indicates that grown children provide insurance to their parents following the onset of a disability, and there is scope for interactions between this private source of support and public assistance provided by DI.

While my results do not completely rule out the possibility that families reduce their transfers in response to DI, any reduction in transfers is likely small. I reject the hypothesis that DI decreases the probability of receiving a transfer by more than 3 percentage points. Given that approximately 28 percent of disabled respondents receiving a monetary transfer in any given wave of the survey and 34

percent of disabled respondent report ever receiving an in-kind transfer, I can reject that the probability of receiving a transfer after DI receipt declines by more than approximately 10 percent. These estimates, combined with estimates on the intensive margin, suggest that crowd out of family transfers in response to DI is lower than crowd out in response to other social insurance programs such as UI. The small magnitude of this response is more consistent with findings in the literature on crowd out of living transfers in response to long term care insurance. Additionally, I find suggestive evidence that families could increase transfers, particularly in-kind transfers, by up to 5-7 percentage points. This could reflect the fact that DI sends an information signal to the family about the need for assistance.

In support of this hypothesis, I find that families are more likely to provide assistance around the onset of the disability when the disability is likely easier to observe. For example, transfers increase at the onset of the disability when the respondent receives observable types of outside help. By contrast, I find the probability of a transfer increases following receipt of DI in cases where the onset of the disability was more gradual, and is likely more difficult to observe. In these cases, DI could send a signal about the severity of the disability, and provide information that the parent will likely require assistance over a long period of time. These results suggest that DI provides families with a way to verify the severity of the disability and update their transfers accordingly.

Given the magnitude of the family's response, interactions with private family insurance likely do not change the optimal level of DI benefits substantially. However, the results confirm that families help smooth the shock of disability onset and

provide suggestive evidence that DI could increase family transfers. This suggests that interactions with family insurance could complement the benefits of receiving DI. Assuming that the marginal utility gain of increased transfers is larger for disabled individuals than the marginal utility fall due to providing more transfers, this could suggest that increasing benefits would improve overall welfare. Future work should continue to examine the family's role in insuring against the costs of disability and investigate more direct methods to mitigate the costs of disability on the entire family.

Table 3.1: Recipients and Rejected Applicants at First and Last Interview

	First interview			Last interview			Pvalue-DD
	Recipients	Rejected	Pvalue	Recipients	Rejected	Pvalue	
Age	52.11	52.14	0.93	63.93	63.48	0.25	0.21
In LF	0.73	0.74	0.40	0.09	0.22	0.00	0.02
Married	0.74	0.69	0.04	0.58	0.51	0.01	0.63
Spouse who works	0.48	0.47	0.64	0.40	0.43	0.39	0.30
Number of kids	3.40	3.53	0.20	3.41	3.47	0.55	0.61
Kids within 10 mi	1.11	1.00	0.13	0.70	0.62	0.07	0.80
Medicare	0.02	0.04	0.01	0.77	0.49	0.00	0.00
Medicaid	0.05	0.06	0.86	0.30	0.17	0.00	0.00
Long term care ins	0.04	0.03	0.14	0.06	0.04	0.06	0.73
Health limits work	0.39	0.32	0.01	0.91	0.85	0.00	0.89
Gross motor activities	0.27	0.23	0.06	0.58	0.47	0.00	0.15
Fine motor activities	0.15	0.12	0.04	0.31	0.28	0.24	0.80
Mobility	0.52	0.52	0.92	0.81	0.71	0.00	0.03
Large muscle activities	0.62	0.61	0.62	0.83	0.79	0.02	0.43
Back problems	0.48	0.41	0.01	0.54	0.54	0.95	0.12
Cancer	0.06	0.05	0.51	0.18	0.14	0.05	0.20
Heart	0.13	0.10	0.02	0.35	0.27	0.00	0.14
Mental health	0.20	0.24	0.09	0.38	0.37	0.88	0.20
High blood pressure	0.48	0.44	0.11	0.68	0.68	0.92	0.32
Diabetes	0.20	0.19	0.54	0.34	0.36	0.51	0.31
Lung problems	0.11	0.10	0.28	0.24	0.19	0.01	0.21
Stroke	0.04	0.05	0.35	0.13	0.11	0.19	0.12
Arthritis	0.49	0.41	0.00	0.71	0.64	0.00	0.79
ADL problems	0.17	0.15	0.36	0.39	0.32	0.01	0.21
IADL problems	0.10	0.10	0.59	0.18	0.20	0.30	0.40
Hospitalized	0.27	0.23	0.09	0.46	0.36	0.00	0.19
Any transfer	0.31	0.29	0.54	0.35	0.30	0.03	0.44
Monetary xfer	0.07	0.06	0.58	0.08	0.09	0.76	0.66
Inkind xfer	0.07	0.06	0.18	0.19	0.16	0.06	0.50
Live with child	0.28	0.27	0.75	0.14	0.12	0.32	0.82
<i>N</i>	1,004	1,004	653	653	0.00	0.00	0.00

Notes: Compares means at first and last interview across the treatment group and the disabled non-applicant control group. Treatment group began receiving DI during the HRS survey, disabled non-applicants report a work limiting health condition in at least two waves and do not apply for DI. Statistics calculated using propensity score weights. P-values test the equality of means cross the two groups at the time of first interview, last interview and whether the difference in difference is significantly different from zero.

Table 3.2: Recipients and Disabled Non-applicants at First and Last Interview

	First interview			Last interview			Pvalue-DD
	Recipients	Non-applicant	Pvalue	Recipients	Non-applicant	Pvalue	
Age	53.32	52.94	0.26	66.06	66.92	0.03	0.00
In LF	0.69	0.69	0.80	0.07	0.16	0.00	0.00
Married	0.73	0.75	0.24	0.54	0.57	0.30	0.96
Spouse who works	0.45	0.48	0.26	0.35	0.34	0.86	0.87
Number of kids	3.67	3.73	0.72	3.68	3.71	0.86	0.66
Kids within 10 mi	1.21	1.22	0.84	0.81	0.96	0.27	0.17
Medicare	0.04	0.04	0.93	0.80	0.73	0.00	0.01
Medicaid	0.08	0.04	0.00	0.32	0.20	0.00	0.00
Long term care ins	0.02	0.01	0.02	0.06	0.07	0.18	0.03
Health limits work	0.39	0.43	0.15	0.88	0.86	0.16	0.03
Gross motor activities	0.31	0.28	0.14	0.62	0.57	0.06	0.76
Fine motor activities	0.16	0.13	0.32	0.36	0.38	0.47	0.14
Mobility	0.58	0.51	0.01	0.84	0.76	0.00	0.91
Large muscle activities	0.67	0.65	0.27	0.85	0.83	0.18	0.93
Back problems	0.46	0.43	0.37	0.54	0.51	0.24	0.79
Cancer	0.08	0.05	0.01	0.20	0.17	0.08	0.76
Heart	0.14	0.08	0.00	0.42	0.37	0.06	0.88
Mental health	0.23	0.22	0.66	0.39	0.31	0.01	0.01
High blood pressure	0.51	0.51	0.89	0.71	0.69	0.35	0.41
Diabetes	0.22	0.21	0.77	0.38	0.39	0.83	0.50
Lung problems	0.15	0.13	0.33	0.27	0.21	0.01	0.08
Stroke	0.05	0.03	0.03	0.16	0.12	0.05	0.42
Arthritis	0.55	0.52	0.24	0.75	0.70	0.06	0.57
ADL problems	0.20	0.16	0.07	0.43	0.45	0.30	0.04
IADL problems	0.13	0.11	0.40	0.20	0.19	0.88	0.55
Hospitalized	0.30	0.21	0.00	0.51	0.42	0.00	0.89
Any transfer	0.22	0.17	0.00	0.36	0.31	0.04	0.92
Monetary xfer	0.09	0.10	0.44	0.08	0.08	0.85	0.53
Inkind xfer	0.12	0.05	0.00	0.24	0.18	0.00	0.65
Live with child	0.20	0.16	0.03	0.13	0.12	0.63	0.31
<i>N</i>	1,004	1,004	2,261	2,261	0.00	0.00	0.00

Notes: Compares means at first and last interview across the treatment group and the rejected applicant control group. Treatment group began receiving DI during the HRS survey, rejected control group was rejected during the survey. Statistics calculated using HRS respondent weights. P-values test the equality of means cross the two groups at the time of first interview, last interview and whether the difference in difference is significantly different from zero.

Table 3.3: Regression Results, Monetary Transfers

	(a) Applicant sample				(b) Health sample			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Disabled	0.019+ (0.011)	0.028* (0.013)	0.028+ (0.015)	0.027+ (0.016)	0.015 (0.012)	0.015 (0.014)	0.001 (0.019)	-0.003 (0.021)
Disabled * DI	0.002 (0.011)	0.022 (0.017)	0.027 (0.018)	0.024 (0.018)	0.008 (0.010)	0.016 (0.014)	0.017 (0.016)	0.014 (0.017)
Observations	8,850	8,850	8,850	8,850	20,195	20,195	20,195	20,195
Ind FE	NO	YES	YES	YES	NO	YES	YES	YES
Wave FE	NO	NO	YES	YES	NO	NO	YES	YES
Health	NO	NO	NO	YES	NO	NO	NO	YES
Number of ind	1,617	1,617	1,617	1,617	3,242	3,242	3,242	3,242

Notes: Robust standard errors in parenthesis, clustered at the household level. + p<0.1, \* p<0.05, \*\*p<0.01. Each column indicates a separate regression. Sample is limited to respondents who have at least one child and are in either the recipient treatment group, or the rejected applicant or disability control samples, respectively. The dependent variable is an indicator for receiving a monetary transfer. The applicant sample in panel (a) includes claimants who begin receiving DI during their time in the HRS, and claimants who are rejected from DI during their time in the HRS. The health sample in panel (b) compares claimants who begin receiving DI during their time in the HRS with claimants who do not apply or DI, but experience the onset of a disability during their time in the HRS. The indicator "disabled" indicates waves in which the respondent reported a work-limiting health condition. "Disabled\*DI" indicates waves in which the respondent reported being disabled and receiving DI. Column (1) runs an OLS regression of receipt of a monetary transfer on these two indicators; column (2) includes an individual-level fixed effect; column (3) includes an individual-level and survey wave fixed effects; column (4) includes time-varying controls, including measures of health status, marital status, assets, and number of children.

Table 3.4: Regression Results, In-kind Transfers

	(a) Applicant sample				(b) Health sample			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Disabled	0.102** (0.010)	0.059** (0.014)	0.050** (0.016)	0.034* (0.016)	0.094** (0.011)	0.104** (0.016)	0.049** (0.017)	0.041* (0.016)
Disabled * DI	0.033* (0.014)	0.056** (0.021)	0.059** (0.022)	0.053* (0.022)	0.066** (0.014)	0.047** (0.016)	0.019 (0.017)	0.015 (0.017)
Observations	8,850	8,850	8,850	8,850	20,195	20,195	20,195	20,195
Ind FE	NO	YES	YES	YES	NO	YES	YES	YES
Wave FE	NO	NO	YES	YES	NO	NO	YES	YES
Health	NO	NO	NO	YES	NO	NO	NO	YES
Number of ind	1,617	1,617	1,617	1,617	3,242	3,242	3,242	3,242

Notes: Robust standard errors in parenthesis, clustered at the household level. +  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ . Each column indicates a separate regression. Sample is limited to respondents who have at least one child and are in either the recipient treatment group, or the rejected applicant or disability control samples, respectively. The dependent variable is an indicator for receiving an in-kind transfer. The applicant sample in panel (a) includes claimants who begin receiving DI during their time in the HRS, and claimants who are rejected from DI during their time in the HRS. The health sample in panel (b) compares claimants who begin receiving DI during their time in the HRS with claimants who do not apply or DI, but experience the onset of a disability during their time in the HRS. The indicator "disabled" indicates waves in which the respondent reported a work-limiting health condition. "Disabled\*DI" indicates waves in which the respondent reported being disabled and receiving DI. Column (1) runs an OLS regression of receipt of a monetary transfer on these two indicators; column (2) includes an individual-level fixed effect; column (3) includes an individual-level and survey wave fixed effects; column (4) includes time-varying controls, including measures of health status, marital status, assets, and number of children.

Table 3.5: Regression Results, Any Transfer

	(a) Applicant sample				(b) Health sample			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Disabled	0.096** (0.024)	0.059* (0.025)	0.040 (0.025)	0.029 (0.025)	0.127** (0.019)	0.136** (0.023)	0.037 (0.023)	0.034 (0.023)
Disabled * DI	0.026 (0.020)	0.096** (0.027)	0.083** (0.027)	0.075** (0.027)	0.054** (0.020)	0.055** (0.021)	0.004 (0.023)	-0.000 (0.023)
Observations	8,850	8,850	8,850	8,850	20,195	20,195	20,195	20,195
Ind FE	NO	YES	YES	YES	NO	YES	YES	YES
Wave FE	NO	NO	YES	YES	NO	NO	YES	YES
Health	NO	NO	NO	YES	NO	NO	NO	YES
Number of ind	1,617	1,617	1,617	1,617	3,242	3,242	3,242	3,242

Notes: Robust standard errors in parenthesis, clustered at the household level. +  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ . Each column indicates a separate regression. Sample is limited to respondents who have at least one child and are in either the recipient treatment group, or the rejected applicant or disability control samples, respectively. The dependent variable is an indicator for receiving any transfer. The applicant sample in panel (a) includes claimants who begin receiving DI during their time in the HRS, and claimants who are rejected from DI during their time in the HRS. The health sample in panel (b) compares claimants who begin receiving DI during their time in the HRS with claimants who do not apply or DI, but experience the onset of a disability during their time in the HRS. The indicator "disabled" indicates waves in which the respondent reported a work-limiting health condition. "Disabled\*DI" indicates waves in which the respondent reported being disabled and receiving DI. Column (1) runs an OLS regression of receipt of a monetary transfer on these two indicators; column (2) includes an individual-level fixed effect; column (3) includes an individual-level and survey wave fixed effects; column (4) includes time-varying controls, including measures of health status, marital status, assets, and number of children.

Table 3.6: Regression Results, Intensive Margin

		(a) Applicant sample			(b) Health sample		
		(1)	(2)	(3)	(1)	(2)	(3)
		Dollars	Days	Hours	Dollars	Days	Hours
Disabled		66.204 (167.068)	1.450** (0.406)	5.946** (2.240)	-677.971 (480.429)	1.574** (0.554)	10.895* (5.365)
Disabled * DI		-15.101 (96.002)	1.356+ (0.699)	2.585 (2.876)	-220.658 (309.577)	0.470 (0.517)	-0.209 (3.479)
Observations		8,850	8,729	8,727	20,195	19,416	19,414
R-squared		0.017	0.024	0.013	0.243	0.036	0.029
Number of ind		1,617	1,610	1,610	3,242	3,196	3,196
Mean		248.5	2.650	10.10	225.6	1.350	4.760

Notes: Robust standard errors in parenthesis, clustered at the household level. +  $p < 0.1$ , \*  $p < 0.05$ , \*\* $p < 0.01$ . Each column indicates a separate regression. Sample is limited to respondents who have at least one child and are in either the recipient treatment group, or the rejected applicant or disability control samples, respectively. Each column estimates equation 3.4 with a measure of transfers along the intensive margin. The dependent variable in columns (1), (2) and (3) is the dollar value of transfer, the number of hours and the number of days spent caring for the parent, respectively. The applicant sample in panel (a) includes claimants who begin receiving DI during their time in the HRS, and claimants who are rejected from DI during their time in the HRS. The health sample in panel (b) compares claimants who begin receiving DI during their time in the HRS with claimants who do not apply or DI, but experience the onset of a disability during their time in the HRS. The indicator "disabled" indicates waves in which the respondent reported a work-limiting health condition. "Disabled\*DI" indicates waves in which the respondent reported being disabled and receiving DI. Each regression includes individual and survey-wave fixed effects and time-varying controls, including measures of health status, marital status, assets, and number of children.

Table 3.7: Regression Results by Observable Events

(a) Hospitalization - applicant sample			(b) Hospitalization - health sample		
	(1)	(2)		(1)	(2)
	No hosp	Hosp		No hosp	Hosp
Disabled	-0.042 (0.056)	0.047+ (0.028)	Disabled	-0.066 (0.041)	0.061* (0.025)
Disabled * DI	0.188** (0.071)	0.051+ (0.029)	Disabled * DI	0.116+ (0.060)	-0.016 (0.024)
Observations	1,051	7,799	Observations	3,128	17,067
R-squared	0.101	0.040	R-squared	0.051	0.093
Number of ind	200	1,417	Number of ind	513	2,729

(c) Home care - applicant sample			(d) Home care - health sample		
	(1)	(2)		(1)	(2)
	No home care	Home care		No home care	Home care
Disabled	0.019 (0.030)	0.048 (0.043)	Disabled	0.005 (0.026)	0.094* (0.042)
Disabled * DI	0.096** (0.032)	0.032 (0.052)	Disabled * DI	0.043+ (0.025)	-0.062 (0.039)
Observations	5,147	3,703	Observations	12,557	7,638
R-squared	0.053	0.041	R-squared	0.048	0.134
Number of ind	959	658	Number of ind	2,025	1,217

Notes: Robust standard errors in parenthesis, clustered at the household level.  $**p < 0.01$ ,  $*p < 0.05$ ,  $+p < 0.1$ . Dependent variable in each regression is an indicator for receiving any transfer. Statistics calculated HRS respondent weights and propensity score weights, respectively. Panels (a) and (b) examine how a hospitalization affects receipt of transfers from grown children for the applicant and health samples, respectively. Panels (c) and (d) examine how home care affects the receipt of transfers from grown children for the applicant and health samples, respectively. The applicant sample includes claimants who begin receiving DI during their time in the HRS, and claimants who are rejected from DI during their time in the HRS. The health sample compares claimants who begin receiving DI during their time in the HRS with claimants who do not apply or DI, but experience the onset of a disability during their time in the HRS. The indicator "disabled" indicates waves in which the respondent reported a work-limiting health condition. "Disabled\*DI" indicates waves in which the respondent reported being disabled and receiving DI. In each panel, column (1) estimates equation 3.4 on the share of claimants who did not experience the type of observable care (a hospitalization or home care), and column (2) estimates equation 3.4 on the share of claimants who did receive observable care. Each regression an individual-level and survey wave fixed effects and time-varying controls, including measures of health status, marital status, assets, and number of children.

Table 3.8: Regression Results by Onset of Specific Health Conditions

(a) Applicant sample						
	(1)	(2)	(3)	(4)	(5)	(6)
	Arthritis	Back pain	Diabetes	Stroke	Hospitalization	Home care
Disabled	-0.023 (0.028)	0.013 (0.028)	-0.038 (0.031)	0.173** (0.059)	0.009 (0.024)	0.071+ (0.041)
Disabled * DI	0.061* (0.027)	0.074** (0.028)	0.101** (0.033)	0.043 (0.060)	0.029 (0.026)	-0.019 (0.047)
Observations	7,710	7,287	4,147	1,886	7,808	3,704
R-squared	0.034	0.032	0.043	0.088	0.036	0.042
Number of ind	1,352	1,273	758	358	1,417	658
(b) Health sample						
	(1)	(2)	(3)	(4)	(5)	(6)
	Arthritis	Back pain	Diabetes	Stroke	Hospitalization	Home care
Disabled	0.015 (0.026)	0.054 (0.039)	-0.029 (0.032)	0.046 (0.068)	-0.005 (0.025)	0.029 (0.031)
Disabled * DI	-0.002 (0.023)	-0.023 (0.025)	0.004 (0.028)	-0.020 (0.059)	-0.021 (0.022)	-0.062+ (0.034)
Observations	17,708	16,512	8,083	3,039	17,067	7,638
R-squared	0.082	0.092	0.111	0.151	0.091	0.131
Number of ind	2,786	2,592	1,314	513	2,729	1,217

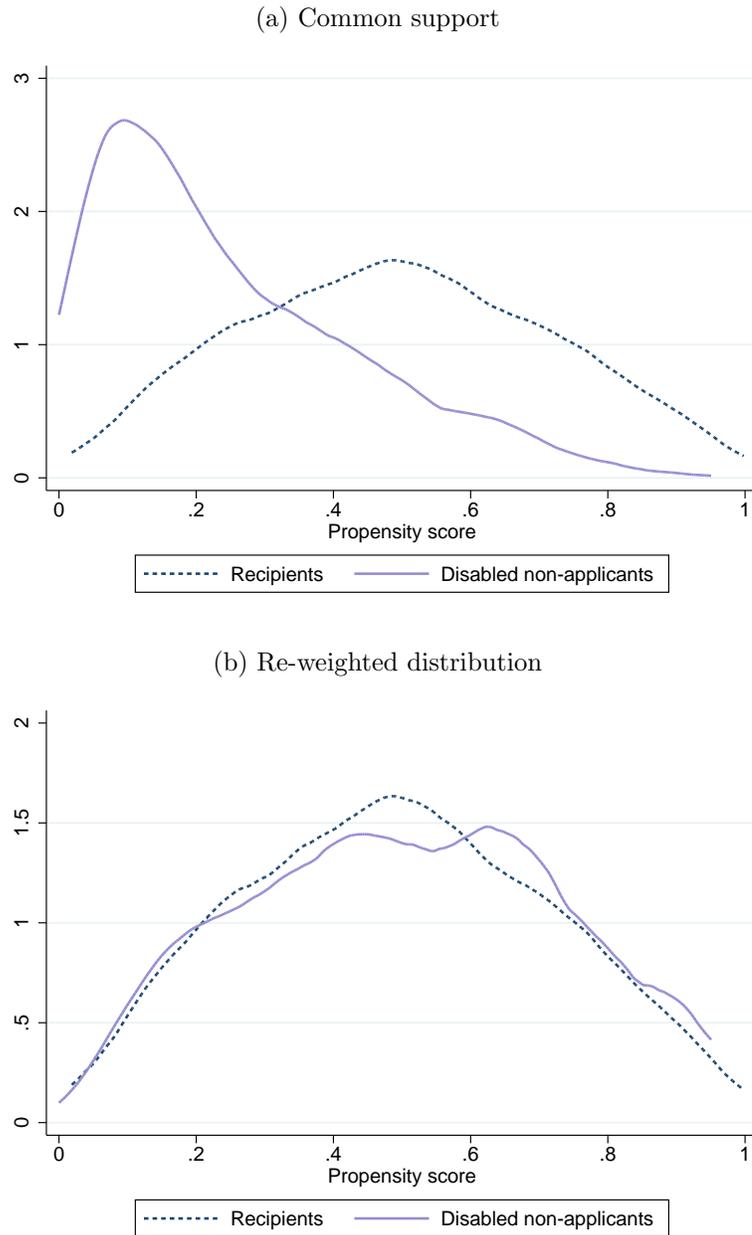
Notes: Robust standard errors in parenthesis, clustered at the household level.  $**p < 0.01$ ,  $*p < 0.05$ ,  $+p < 0.1$ . Dependent variable in each regression is an indicator for receiving any transfer. Statistics calculated HRS respondent weights and propensity score weights, respectively. Each column estimates equation 3.4 on the subset of the population reporting a particular condition or use of care. The applicant sample in panel (a) includes claimants who begin receiving DI during their time in the HRS, and claimants who are rejected from DI during their time in the HRS. The health sample in panel (b) compares claimants who begin receiving DI during their time in the HRS with claimants who do not apply or DI, but experience the onset of a disability during their time in the HRS. The indicator "disabled" indicates waves in which the respondent reported a work-limiting health condition. "Disabled\*DI" indicates waves in which the respondent reported being disabled and receiving DI. Each regression includes individual and survey-wave fixed effects and time-varying controls, including measures of health status, marital status, assets, and number of children.

Table 3.9: Robustness Checks

(a) Applicant sample					
	(1)	(2)	(3)	(4)	(5)
	Health controls	End DI = 0	Under 65	No appeals	No weights
Disabled	0.001 (0.027)	0.042 (0.027)	0.025 (0.028)	0.030 (0.027)	0.042* (0.021)
Disabled * DI	0.061* (0.027)	0.086** (0.031)	0.082** (0.031)	0.068* (0.031)	0.050* (0.021)
Observations	8,850	7,319	5,959	6,629	8,850
R-squared	0.062	0.043	0.041	0.050	0.035
Number of ind	1,617	1,381	1,517	1,214	1,617
(b) Health sample					
	(1)	(2)	(3)	(4)	(5)
	Health controls	End DI = 0	Under 65	No appeals	No weights
Disabled	-0.009 (0.021)	0.023 (0.020)	0.033 (0.024)	0.023 (0.020)	0.004 (0.009)
Disabled * DI	0.001 (0.020)	0.017 (0.023)	0.039 (0.024)+	-0.001 (0.022)	0.056 (0.016)**
Observations	22,683	21,029	13,737	21,133	22,683
R-squared	0.098	0.081	0.053	0.081	0.046
Number of ind	3,261	3,039	3,221	3,012	3,261

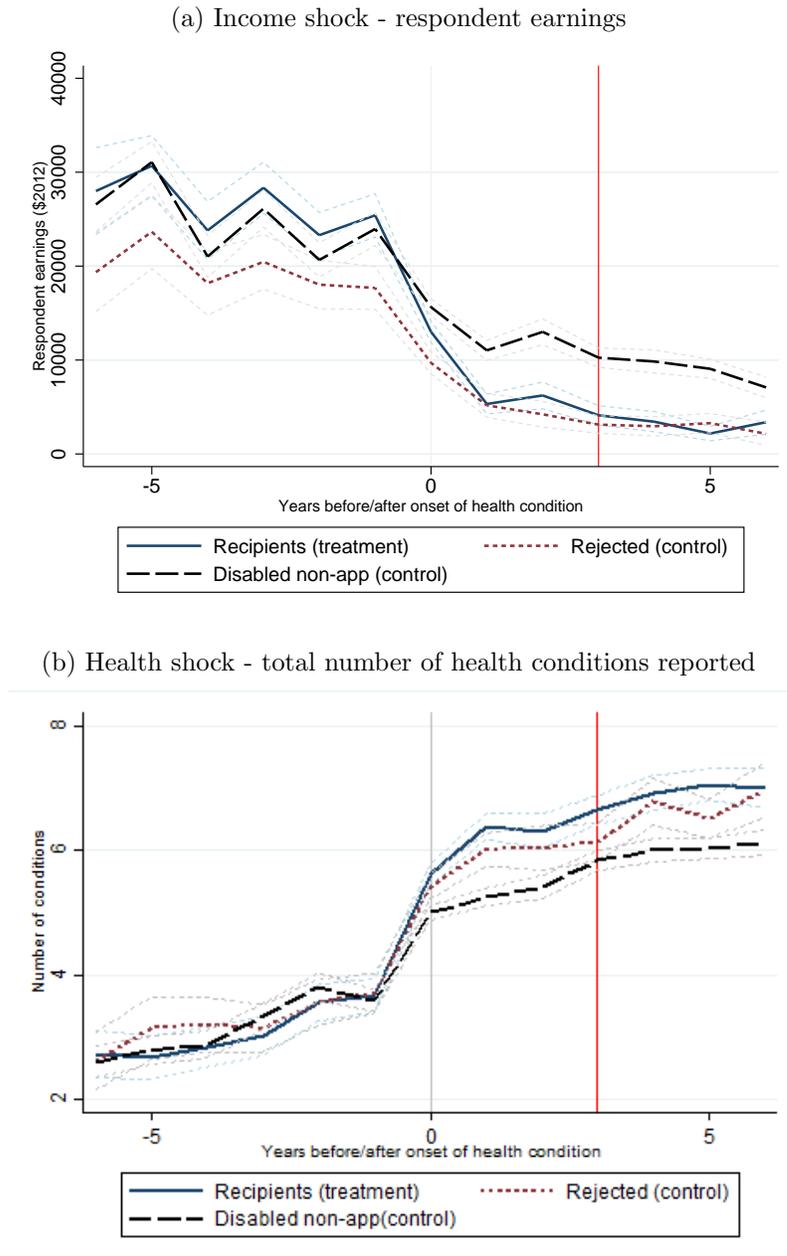
Notes: Robust standard errors in parenthesis, clustered at the household level. \*\*  $p < 0.01$ , \*  $p < 0.05$ , +  $p < 0.1$ . Dependent variable in each regression is an indicator for receiving an in-kind transfer. Statistics calculated HRS respondent weights and propensity score weights, respectively. Each column estimates equation 3.4 under various robustness checks. Column (1) changes the health controls used in the regression to include issues with ADLs or IADLs, specific medical diagnoses, self-report of poor health, and number of doctor's visits. Column (2) excludes respondents who leave DI for any reason. Column (3) excludes claimants once they exceed the full retirement age to exclude transitions from DI to OASI. Column (4) excludes DI claimants who were admitted after an appeal. Column (5) estimates equation 3.4 without weights. The applicant sample in panel (a) includes claimants who begin receiving DI during their time in the HRS, and claimants who are rejected from DI during their time in the HRS. The health sample in panel (b) compares claimants who begin receiving DI during their time in the HRS with claimants who do not apply or DI, but experience the onset of a disability during their time in the HRS. The indicator "disabled" indicates waves in which the respondent reported a work-limiting health condition. "Disabled\*DI" indicates waves in which the respondent reported being disabled and receiving DI. Each regression includes individual and survey-wave fixed effects and time-varying controls, including measures of health status, marital status, assets, and number of children.

Figure 3.1: Distribution of Propensity Scores of Participation in DI



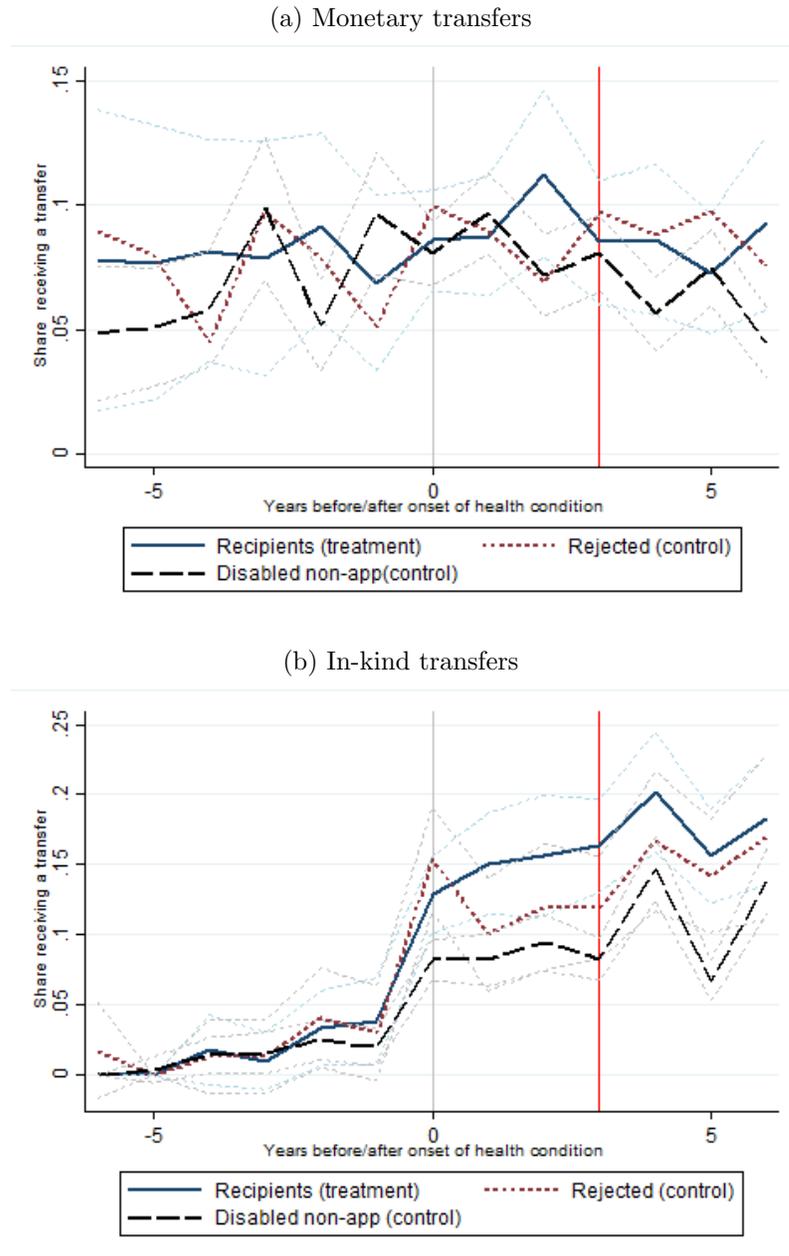
Notes: Data from the Health and Retirement Study, 1992-2010. Each figure reflects the distribution of propensity scores reflecting the predicted probability of participating in DI, based on a linear probability model on observable characteristics including age, health status, earnings and assets. The exact variables included in the propensity score analysis were chosen using the stepwise regression procedure explained in Imbens (2014). The distribution for the DI recipient sample and the health sample are shown in the regression. In panel (a) the propensity scores are shown as predicted from the regression. In panel (b), claimants in the health sample are re-weighted by the inverse of their propensity score. Propensity scores are rescaled by HRS respondent weights in panel (b).

Figure 3.2: Trends in Health and Income Before and After Disability Onset



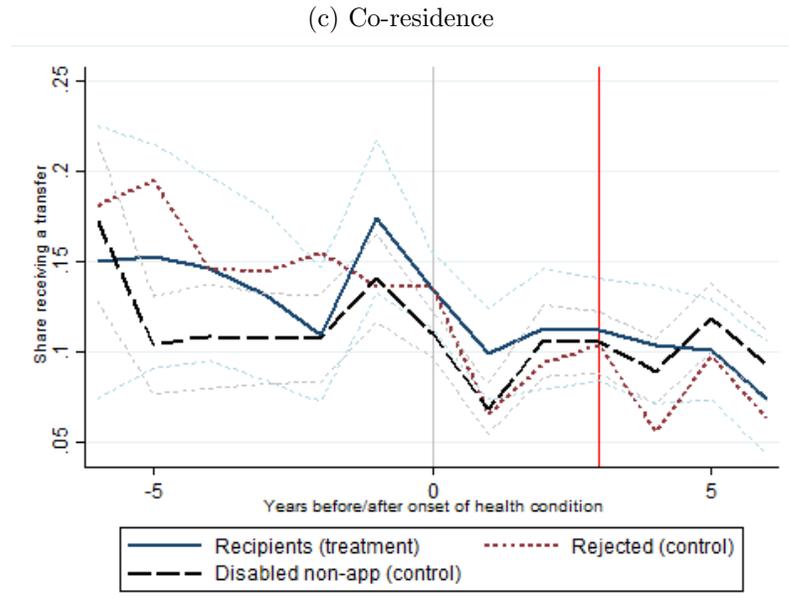
Notes: Data from the Health and Retirement Study, 1992-2010. The lines describe trends in earnings and the number of health conditions reported before and after the first report of a work-limiting health condition in the survey for the treatment group (recipients) and both control groups (rejected applicants and disabled non-app). The light dashed lines indicate 95-percent confidence intervals. The vertical dashed line indicates the onset of a work-limiting health condition, and the solid line indicates the average time of DI receipt relative to the onset of the disability (approximately 2.4 years after the first report of a disability).

Figure 3.3: Trends in Transfers from Children



Notes: Data from the Health and Retirement Study, 1992-2010. The lines describe trends in earnings and the number of health conditions reported before and after the first report of a work-limiting health condition in the survey for the treatment group (recipients) and both control groups (rejected applicants and disabled non-app). The light dashed lines indicate 95-percent confidence intervals. The vertical dashed line indicates the onset of a work-limiting health condition, and the solid line indicates the average time of DI receipt relative to the onset of the disability (approximately 2.4 years after the first report of a work-limiting disability).

Figure 3.3: Trends in Transfers from Children (continued)



Notes: Data from the Health and Retirement Study, 1992-2010. The lines describe trends in earnings and the number of health conditions reported before and after the first report of a work-limiting health condition in the survey for the treatment group (recipients) and both control groups (rejected applicants and disabled non-app). The light dashed lines indicate 95-percent confidence intervals. The vertical dashed line indicates the onset of a work-limiting health condition, and the solid line indicates the average time of DI receipt relative to the onset of the disability (approximately 2.4 years after the first report of a work-limiting disability).

## 3.7 Appendix

### 3.7.1 Data Appendix

I use the Rand HRS Data Version M ([RAND 2013](#)) and Rand HRS Family Data Version B ([RAND 2012](#)), and supplement additional questions from the main HRS for cases where RAND has not included the question in their streamlined datasets. To date, the HRS covers five panels: the original HRS sample, comprised of individuals born between 1931 and 1941; the Asset and Health Dynamics sample

(AHEAD) sample, individuals born before 1923; children of the depression (CODA), individuals born between 1923-1931; the war babies sample (WB), individuals born between 1942-1947, and finally, the early baby boomers (EBB), born between 1948-1953.<sup>19</sup> In the event that a sampled household has one individual in the target age group and a younger or older spouse, information was collected about both household members, meaning there are a select group of individuals below age 50 in the survey. To date, the combined panels yield a total sample size of approximately 36,000 individuals. Below, I explain my adjustments and definitions, as well as some of the important definitions and adjustments in the RAND files themselves. More information on how RAND compiles the datasets is available from [Chien \*et al.\* \(2013, 2012\)](#).

## Disability Episodes Status

The RAND HRS files include detailed information on up to 10 disability “episodes”, which correspond to separate applications. Each episode contains information on the dates of application, receipt, and re-application as well as the application status. In order to observe respondents before and after they begin receiving disability, the receiving sample focuses on the subset of applicants whose first disability check occurred after their first interview and before their last interview. I determine when the applicant received their first disability check based on the date on which they report the first receipt of DI. I cross-walk cases where applicants report a date of first receipt with the application status for that episode. If the

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<sup>19</sup>The baseline for panel 6, or the middle baby boomers (MBB), began in 2010.

applicant reports receiving DI but the application status does not indicate that the recipient has received DI (e.g., it says the application was rejected), I exclude this observation from the receiving sample. I interpret the applicant as receiving DI if the status is coded as (a) receiving benefits; (b) new episode receiving (used when there is not a clear end to a prior episode, but the current episode indicates receipt); or (c) stopped receiving benefits (indicating that the date of first receipt is valid, but the benefits later were terminated - I address respondents with terminated benefits in the robustness checks).

Similarly, when I look at a comparable sample of rejected applicants, I focus on the subset of applicants whose first application to DI occurred after their first interview and before their last interview, and who I never observe receiving DI. This means that they do not report a date of first receipt, and the application status does not indicate that they ever received DI. Note that not all application statuses in this control group necessarily indicate rejection; some of them still reflect a “not awarded” status, which the RAND data documentation notes could include some applications that are not yet resolved ([Chien \*et al.\* 2013](#)). However, because I observe these respondents for multiple waves and do not observe them receiving DI in any of these waves, I make the assumption that if the status is never updated, then this is a rejection. The average wait time for accepted applicants in my sample is between six months and one and a half years. Because I observe applicants every two years, and I require having a post-application or post-receipt wave in order to be included in the sample, I assume the disability application would have been resolved by the next interview.

In a few cases, respondents report receiving DI before they report applying for DI. These people are excluded from the sample. If a respondent has an unsuccessful application prior to the survey, and has a second application that occurs during the survey, I use this second application to determine my sample. There are 93 respondents included in the control sample due to this secondary application, but these respondents are excluded in a robustness check. In sum, this results in sample sizes of 1,004 for the receiving sample (44 people were excluded based on inconsistencies in their dates), and an additional 656 people whose applications occurred during the survey, but never receive DI.

## Transfers from Children

All transfer questions, including the monetary value of the transfer, are asked of the respondent. They are asked in each wave, and then the respondent is asked follow up, detailed questions about the helper in a separate module. The helper, or transferor, is never interviewed directly. Because all transfer questions ask about transfers from children, if a respondent does not have children, step children, or grandchildren in any wave of the survey, they are excluded from the sample. In 144 cases, the respondent does not report any children, but does report receiving transfers from children. In all of these cases the indicator for having received a transfer is replaced to zero. Because they do not have children, they are not included in the sample.

**Monetary transfers:** While the survey is amended after the first wave to allow monetary transfers of all values to be reported, they first ask if they received any monetary transfers \$500 or higher. Then in a follow up question, they ask about the value of any transfers (including those below \$500). In my analysis I count the respondent as receiving a monetary transfer even if she receives a transfer less than \$500. In practice, this is relatively rare: only approximately 10 percent of respondents who report a value for their transfer report a value less than \$500.

**Waves with transfer questions:** Respondents are asked whether or not they have moved in with a child in all waves of the survey. HRS asks about monetary transfers in waves 2-10, and asks about in-kind transfers in waves 3-10. Particular in-kind transfer questions are only asked in a subset of these waves: help with chores is only asked in waves 3-6, and help with health costs is only asked in waves 5-10. These types of assistance are excluded from the main in-kind transfer measure in order to avoid a mechanical effect on transfers as respondents progress through the waves, but are included in the alternative definitions of transfers. Because I do not observe certain transfers in all waves, I flag respondents whose only pre or post DI wave did not record certain transfers.

**Frequency of transfers:** I measure the frequency of transfers in three ways:

1. Count number of waves where an individual received a transfer separately for each transfer type, and then a combined measure of any transfer type
2. Use HRS variables for number of days or hours provided in the last month, although these measures are noisy and often reported

3. Count the maximum number of children in the family who ever provided a transfer to the parents

**Defining a living transfer:** The main HRS files contain a question asking whether or not the respondent has moved in with one of their children since the last interview, and if yes, who benefits the most from the move - the respondent, the child, or both. In the broadest measure of living transfers, I include everyone who moved in with a child. In the most conservative, I only count if the move benefited respondent the most. In an in-between measure, I count moves that helped both respondent and child (so omitted category is that it only helped the child).

## Health Status

**ADLs and IADLs:** I use RAND's summary variables of all the individual ADL and IADL categories. The data documentation notes that there a separate measure of ADLs developed by Wallace & Herzog 1991 leads to a higher incidence problems with ADLs compared to RAND's ADLs ([Chien \*et al.\* 2013](#)). The main drawback of using RAND's summary over the Wallace & Herzog measure is that RAND does not include a measure of ADLs in wave 1, because the questions about ADLs were distinctly different in that wave. An alternative approach would be to use the Wallace & Herzog measure, which is available in all waves, and acknowledge that it may overstate the incidence of issues with ADLs and IADLs.

**Chronic Health Conditions:** I indicate a respondent as having a chronic issue with a certain health condition if the respondent indicates having an issue with

the condition in all subsequent waves following the first wave in which the problem was noted. As an additional measure of the long term nature of the disability, I sum the total number of waves in which the individual reports having an issue with the condition, regardless of whether or not this occurs in consecutive waves.

## Income Variables

Note that according to the RAND data appendices, all income variables were asked about the previous calendar year. As a result, I adjust all income and wealth variables for inflation based on the year prior to the interview year.

**Social Security Retirement and Disability Income:** Because SSDI automatically transfers to OASI at full retirement age, the respondent could be confused about which program she participates in. Indeed, a handful of respondents report receiving DI after age 66, and retirement before age 62. In order to deal with this, I do the following:

1. If (a) respondent reports receiving disability, (b) their retirement income is reported as zero, and (c) they are over the full retirement age (65 for respondents born before 1943; 66 for respondents born in/after 1943), then I recode their disability income to be retirement income.
2. If (a) the respondent reports receiving retirement; (b) their disability income is zero; (c) they are below age 62 (early retirement age) and (d) they report receiving DI based on the DI episode variables, then I recode this income to

be disability, not retirement. Otherwise, I leave it as is - this income could be due to a spouse's retirement, widower benefits, etc.

3. If respondent is between 62 and 66, they could be receiving either disability or retirement. I leave it as listed.

Additionally, there are a few people who report income from DI, but based on their DI episode variables they either are not receiving DI at that time (data indicates their DI should have stopped), or all the episode variables are missing, which I take to mean they never receive it. Since disability episode variables are asked of every respondent, while disability income is only asked of the financial respondent in the household, I assume the episode variables reflect the truth and recode these disability income values to be zero.

### 3.7.2 Weighting

The inclusion of new cohorts in the HRS beginning in 1998 led to a revision of the weights in wave 4. Previously, spouses who had been interviewed, but who did not fall in the target age-ranges of the original to HRS cohorts (HRS and AHEAD) were assigned a weight of zero, as the weights were designed to be representative of the population within the given birth cohorts of the survey. However, once the additional WBB and CODA panels were added, these spouses could fall into the specific birth cohorts of the new panels. As a result, the weights were amended so that all respondents (whether or not they were in the birth cohort for which their household was sampled) be weighted to represent the entire population of adults

in the United States born before 1948 ([University of Michigan 2013](#)). Additionally, when the EBB cohort was added in wave 7, the weights are updated to represent all individuals born before 1953 ([Chien \*et al.\* 2013](#)). HRS weights in follow up waves adjust for “wave specific non-response among those who participated at baseline” ([Ofstedal \*et al.\* 2011](#)).

My sample selection includes individuals from any HRS cohort as long as they meet the criteria explained in the paper, regardless of their birth cohort. As a result, I use the weights from wave 7, the first wave for which all five cohorts could appear in the sample. Approximately 80 percent of my sample participates in wave 7, one of the highest proportions of any wave, and the highest proportion of the waves that include all five cohorts. However, because the earlier cohorts have aged significantly by that time the AHEAD and CODA cohorts are less likely to participate in the survey in wave 7 relative to later cohorts. Note that while weights are designed to be representative for a given wave of the survey, I pool across all waves. As a result, using the wave 7 weights without any adjustment to include individuals who are in my sample, but would not be included in wave 7 weight could lead to biased estimates. If it is true that older people could be more likely to receive transfers from their children, this could lead to a downward bias on my estimate of the effect of DI on transfers.

As a result, I adjust the weights in wave 7 to include these omitted individuals. I employ a technique consistent with the technique used in the PSID to adjust weights for temporary non-response ([Gouskova \*et al.\* 2008](#)). First, I group individuals in each wave into cells based on the following observable characteristics: birth

year, gender, race, and whether or not they have problems with any ADLs (a rough measure of health status). Then, I calculate the proportion of individuals in each of these cells who participate in the survey in wave 7. Then, I multiply the weights of respondents who do participate in that wave by the inverse of this proportion. For example, if 20 percent of individuals who are male, born between 1931 and 1941, white, and do not have any ADL issues are in wave 7, then I multiply the weight for each individual in this cell who is in the survey in wave 7 by 5.

### 3.7.3 Stepwise Regression Procedure for Propensity Score Estimation

I use methods from [Imbens \(2014\)](#) to estimate the propensity score of being in the treatment group. I estimate the propensity score with the following steps:

1. **Determining the relevant population:** While I could reweight the entire HRS population to match the observable characteristics of my treatment group, this would result in many individuals with extremely different observable characteristics being included in the sample, even if they enter in the sample with very low weights. Hence as a first step to identify a relevant population, I take the entire HRS population and limit it to anyone who reports a having a health condition that limits work in at least two waves of the survey. The results are robust to other broad determinations of disability including individuals who report a chronic health condition that limits work, individuals reporting a chronic mobility condition, individuals reporting a mobility con-

dition in at least two waves of the survey, and individuals with a self-report of poor health in at least two waves. Because DI applications require individuals to demonstrate that an individual has a condition limiting work, this broad definition is maintains the spirit of the DI criteria, even if the individuals do not apply for DI. Furthermore, [Benitez-Silva \*et al.\* \(2004\)](#) finds little bias in this question in the HRS, making it a suitable criteria on which to condition my sample.

- 2. Estimate the propensity score:** I use the stepwise regression method for estimating the propensity score described in [Imbens \(2014\)](#). This requires first determining several guaranteed linear covariates to include in the regression and selecting a series of linear and quadratic covariates to be considered for inclusion in the regression. The guaranteed controls I use are birth year (to determine the HRS cohort), age at first interview, total number of waves reporting problems with ADLs, IADLs, or mobility. The potential linear controls include gender, race, number of waves reporting a diagnosed condition, number of waves reporting poor health, years of education, marital status at first interview, spousal labor force participation at first interview, and number of children at first interview, and respondent assets and earnings at baseline. Potential quadratic covariates include the interaction of guaranteed and potential linear covariates, as well as age squared and education squared. I run the regression with and without each of these potential covariates and use a likelihood ratio test to determine whether the covariate should be included in

final regression. [Imbens \(2014\)](#) suggests using critical values around 2 for the likelihood ratio test, but notes that this choice is somewhat arbitrary. I use a critical value of 3 in my analysis, but the results do not change when using critical values of either 1.5 or 4.5. I only consider time-invariant covariates in estimating the propensity score to avoid selecting on characteristics that may change over the course of the panel.

- 3. Construct the propensity score weight:** Once I have predicted the propensity score for each observation in my sample, I construct the weight  $\frac{\hat{\lambda}}{1-\hat{\lambda}}$ , where  $\hat{\lambda}$  is the estimated propensity score. Following [Nichols \(2008\)](#), I weight the control group by this ratio, and weight the treatment group by the weighted fraction of respondents in the treatment group, in order to preserve the relative representation of the two groups in my sample.

#### 3.7.4 Chetty and Saez 2010

The theoretical framework for this paper builds upon [Chetty and Saez \(2010\)](#), which examines how endogenous private insurance changes the determination of optimal benefits. In order to understand how this model can be adapted to include family insurance, I first explain the Chetty and Saez model in the context of disability. Consider a world with agents who have varying degrees of health, represented by the distribution  $f(n)$ , where higher levels of  $n$  indicate better health. Individuals choose either a high or low work intensity and earn high or low earnings in return for their work,  $z \in \{z_H, z_L\}$ . Without loss of generality, I normalize  $z_L = 0$ ,

assuming the low work intensity represents exiting the labor force. The increasing, convex function  $h\left(\frac{z}{n}\right)$  demonstrates that the cost of work increases as health declines. The following separable function shows that utility increases in consumption and decreases with the cost of working:

$$U(c, z|n) = u(c) - h\left(\frac{z}{n}\right) \quad (3.5)$$

Individuals choose whether or not to work by comparing the utility benefit from working with the cost of work. This yields a threshold health level  $n^*$  where individuals are exactly indifferent between working and not working. If an agent's health is above the threshold, she will work, earn  $z_H$ , pay government taxes  $\tau$  and pay a premium on private disability insurance  $\tau_p$ .<sup>20</sup> If the agent is below the threshold, she earns nothing and receives benefits from the government and the private insurer,  $b$  and  $b_p$ , respectively. The share of individuals above this threshold is represented by

$$e = 1 - F(n^*) = \int_{n^*}^{\infty} dFn. \quad (3.6)$$

The government chooses the level of  $b$  that maximizes social welfare (incorporating the public and private budget constraints):

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<sup>20</sup>According to [Autor \*et al.\* \(2014\)](#), approximately one-third of workers in the United States are covered by private disability insurance. Employers pay the premium in the majority of these cases, although workers likely still bear some of the incidence of the premium. For exposition, I maintain consistency with the general version of Chetty & Saez's model and assume all agents have private disability insurance. Relaxing this assumption could provide another adaptation of the model to investigate in future work.

$$\max_b W = e \left[ u(z_H - \frac{(1-e)}{e}(b_p(b) + b)) - h\left(\frac{z}{n}\right) \right] + (1-e) \cdot u(b_p(b) + b). \quad (3.7)$$

Importantly, the level of  $b_p(b)$  is endogenous: private insurers take individuals' expectations of their own health and the probability of benefit receipt into account when setting private benefits. As a result,  $b_p(b)$  may not be set optimally if individuals or their families do not accurately anticipate their draw of  $n$ . However, individuals have already optimally chosen their earnings capacity  $z$  for a given draw of  $n$  and  $b$ .

### Effect of Monetary and In-kind Transfers on the Family's Budget

In reality, families could respond to disability not only through monetary transfers  $b_p$ , but also through trade-offs between work, leisure and in-kind transfers of time,  $t_p(b)$ . Here, I disaggregate the family's outside income as  $z_F = y + w(T - l - t_p(b))$ , where  $T$  is the total time endowment,  $l$  is the amount of time spent on leisure,  $w$  is the wage, and  $y$  is unearned income. This results in a new interpretation of the family's budget constraint:

$$c = y + w \cdot (T - l - t_p(b)) - \frac{1-e}{1+e}b - b_p(b) \quad (3.8)$$

Since the opportunity cost of any time spent assisting the disabled is the wage  $w$ , time transfers increase the budget set of the disabled individual in the same way

a monetary transfer of  $w \cdot t_p$  would increase the budget set.<sup>21</sup> Letting  $\frac{-dt_p}{db} = s$  leads to the new first order condition:

$$\begin{aligned} \frac{dW}{db} = & (1 - e)\theta \cdot \left( \left[ \frac{((1 - r - w \cdot s)u'(c_L) + (r + w \cdot s) \cdot u'_F(c_L)) - \frac{\theta}{(1+e)}}{\theta} \right] \right. \\ & \left. - \epsilon_{1-e,b} \left[ \frac{2}{(1+e)^2} + \frac{u_F(c_H) - u_F(c_L)}{\theta \cdot b} \right] \right) \end{aligned} \quad (3.9)$$

Now, the change in utility reflects the tradeoff between both types of crowd out: monetary transfers and in-kind transfers. Families could respond to DI by increasing monetary transfers, in-kind transfers, or both. Incorporating both transfer types yields a more complete picture of the family's potential share of the disability's burden and demonstrates the channels of any potential spillovers.

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<sup>21</sup>I assume that the family can perfectly substitute between monetary transfers and in-kind transfers at the rate  $b_p = w \cdot t_p$ . However, if the family member faces additional constraints on either her time, say through a required number of hours at work, or on the level of monetary transfers she can provide, then this assumption may not hold. I plan to expand upon this analysis in future work.

Table 3.10: Determinants of Being Included in the Treatment Group

Dependent variable = 1 if treatment group	
Age	-0.027 (0.021)
ADLs	0.074 (0.030)*
IADLs	-0.031 (0.023)
Mobility issues	-0.098 (0.014)**
Nonwhite	0.141 (0.034)**
Specific diagnosis	0.099 (0.048)*
Self-report of poor health	-0.028 (0.069)
Years of education	-0.009 (0.005)+
Spouse works	-0.144 (0.241)
Number of children	0.022 (0.007)**
Financial wealth	-0.001 (0.002)
Non-housing wealth	0.001 (0.001)
Pre-disability earnings	0.007 (0.004)*
Attrit from sample	0.013 (0.071)
Die during survey	0.288 (0.323)
Age squared	0.000 (0.000)*
Age*Diagnosis	-0.002 (0.001)*
Age*Self-report	-0.001 (0.001)
Age*Spouse works	0.002 (0.004)
Age*Financial wealth	0.000 (0.000)
Age*Non-housing wealth	-0.000

	(0.000)+
Age*earnings	-0.000
	(0.000)+
Age*Die during survey	-0.003
	(0.006)
ADL*Female	-0.003
	(0.007)
ADL*Nonwhite	-0.016
	(0.011)
ADL*Specific diagnosis	-0.000
	(0.002)
ADL*Self report	-0.001
	(0.003)
ADL*Education	-0.001
	(0.002)
ADL*Children	-0.005
	(0.002)*
IADL*Education	0.004
	(0.002)*
IADL*Die during survey	-0.042
	(0.014)**
Mobility*Diagnosis	-0.001
	(0.002)
Mobility*Self report	0.018
	(0.002)**
Mobility*Ever attrit	-0.002
	(0.011)

Notes: Standard errors in parenthesis.  $**p < 0.01$ ,  $*p < 0.05$ ,  $+p < 0.1$  Table shows marginal effects of a logistic regression with participation in the treatment group as the dependent variable. Sample includes all members of the treatment group and the health control group. Regression estimated with propensity score weights.

Table 3.11: Determinants of Applying for DI

	(1) Transfers	(2) Controls only
Monetary	0.029 (0.007)**	-0.004 (0.006)
In-Kind	0.120 (0.006)**	0.014 (0.006)*
Co-residence	0.007 (0.006)	-0.011 (0.005)*
Age		0.017 (0.004)**
ADL		0.053 (0.006)**
IADL		0.010 (0.005)*
Mobility		0.005 (0.002)*
Nonwhite		0.039 (0.008)**
Specific Diagnoses		0.007 (0.007)
Self-report of poor health		0.087 (0.009)**
Years of education		0.001 (0.001)
Spouse works		-0.049 (0.037)
Number of children		-0.000 (0.001)
Financial wealth		0.000 (0.000)
Non-housing wealth		0.000 (0.000)
Pre-disability earnings		0.001 (0.001)
Attrit from sample		0.011 (0.014)
Die during survey		0.164 (0.042)**
Age squared		-0.000 (0.000)**
Age*Specific Diagnosis		-0.000

		(0.000)+
Age*Self report	-0.001	(0.000)**
Age*Spouse works		0.000 (0.001)
Age*Financial wealth		-0.000 (0.000)
Age*Non-housing wealth		-0.000 (0.000)
Age*Earnings		-0.000 (0.000)*
Age*Die during survey		-0.002 (0.001)**
ADL*Female		-0.007 (0.002)**
ADL*Nonwhite		-0.000 (0.003)
ADL*Specific diagnosis		-0.002 (0.001)**
ADL*Self report		-0.002 (0.001)**
ADL*Education		-0.000 (0.000)
ADL*Number of children		-0.000 (0.000)
IADL*Education		-0.000 (0.000)
IADL*Die during survey		-0.015 (0.003)**
Mobility*Diagnosis		-0.000 (0.000)
Mobility*Self report		-0.003 (0.000)**
Mobility*Ever attrit		-0.004 (0.003)

Notes: Standard errors in parenthesis. \*\*  $p < 0.01$ , \*  $p < 0.05$ , +  $p < 0.1$  Table shows marginal effects of a logistic regression with ever applying for DI as the dependent variable. Sample includes all members of the treatment group and the health control group. Regression estimated with propensity score weights. Columns (1) and (2) compare the effect of transfers on the probability of receiving a transfer with and without controls for other observable characteristics including health status.

Table 3.12: Regression Results, Shared Living Arrangements

(a) Applicant sample				
	(1)	(2)	(3)	(4)
Disabled	0.018 (0.021)	0.005 (0.019)	-0.021 (0.018)	-0.023 (0.017)
Disabled * DI	-0.004 (0.016)	0.044** (0.017)	0.019 (0.018)	0.017 (0.018)
Observations	8,850	8,850	8,850	8,850
R-squared	0.000	0.005	0.029	0.032
Ind FE	NO	YES	YES	YES
Wave FE	NO	NO	YES	YES
Health	NO	NO	NO	YES
Number of ind	1,617	1,617	1,617	1,617
(b) Health sample				
	(1)	(2)	(3)	(4)
Disabled	0.051** (0.018)	0.051* (0.023)	-0.008 (0.024)	-0.010 (0.022)
Disabled * DI	-0.000 (0.017)	0.020 (0.016)	-0.011 (0.020)	-0.015 (0.020)
Observations	20,195	20,195	20,195	20,195
R-squared	0.003	0.005	0.039	0.061
Ind FE	NO	YES	YES	YES
Wave FE	NO	NO	YES	YES
Health	NO	NO	NO	YES
Number of ind	3,242	3,242	3,242	3,242

Notes: Robust standard errors in parenthesis, clustered at the household level. +  $p < 0.1$ , \*  $p < 0.05$ , \*\* $p < 0.01$ . Each column indicates a separate regression. Sample is limited to respondents who have at least one child and are in either the recipient treatment group, or the rejected applicant or disability control samples, respectively. The dependent variable is an indicator for the disabled individual entering a shared living arrangement. The applicant sample in panel (a) includes claimants who begin receiving DI during their time in the HRS, and claimants who are rejected from DI during their time in the HRS. The health sample in panel (b) compares claimants who begin receiving DI during their time in the HRS with claimants who do not apply or DI, but experience the onset of a disability during their time in the HRS. The indicator "disabled" indicates waves in which the respondent reported a work-limiting health condition. "Disabled\*DI" indicates waves in which the respondent reported being disabled and receiving DI. Column (1) runs an OLS regression of receipt of a monetary transfer on these two indicators; column (2) includes an individual-level fixed effect; column (3) includes an individual-level and survey wave fixed effects; column (4) includes time-varying controls, including measures of health status, marital status, assets, and number of children.

Table 3.13: Regression Results, by Proximity of Children and Disabled Individual's Marital Status

(a) Child lives within 10 miles - applicant sample			(b) Child lives within 10 miles - health sample		
	(1) Not in 10 miles	(2) In 10 miles		(1) Not in 10 miles	(2) In 10 miles
Disabled	-0.019 (0.061)	0.045+ (0.027)	Disabled	0.019 (0.033)	0.037 (0.028)
Disabled * DI	0.054 (0.053)	0.076* (0.031)	Disabled * DI	-0.033 (0.043)	0.004 (0.026)
Observations	1,657	7,193	Observations	4,768	15,427
R-squared	0.058	0.046	R-squared	0.169	0.070
Number of ind	364	1,253	Number of ind	829	2,413

(c) Marital status at first interview - applicant sample			(d) Marital status at first interview- health sample		
	(1) Not married	(2) Married		(1) Not married	(2) Married
Disabled	0.049 (0.058)	0.023 (0.027)	Disabled	0.032 (0.049)	0.037 (0.025)
Disabled * DI	0.111+ (0.059)	0.059* (0.030)	Disabled * DI	0.010 (0.052)	-0.005 (0.025)
Observations	2,307	6,543	Observations	4,224	15,971
R-squared	0.066	0.047	R-squared	0.066	0.093
Number of ind	438	1,179	Number of ind	720	2,522

Notes: Robust standard errors in parenthesis, clustered at the household level.  $**p < 0.01$ ,  $*p < 0.05$ ,  $+p < 0.1$ . Dependent variable in each regression is an indicator for receiving any transfer. Statistics calculated HRS respondent weights and propensity score weights, respectively. Panels (a) and (b) examine whether having a child that lives within 10 miles the affects receipt of transfers from grown children for the applicant and health samples, respectively. Panels (c) and (d) examine how the disabled applicant's marital status affects the receipt of transfers from grown children for the applicant and health samples, respectively. The applicant sample includes claimants who begin receiving DI during their time in the HRS, and claimants who are rejected from DI during their time in the HRS. The health sample compares claimants who begin receiving DI during their time in the HRS with claimants who do not apply or DI, but experience the onset of a disability during their time in the HRS. The indicator "disabled" indicates waves in which the respondent reported a work-limiting health condition. "Disabled\*DI" indicates waves in which the respondent reported being disabled and receiving DI. In each panel, column (1) estimates equation 3.4 on the share of claimants who did not experience the type of observable care (a hospitalization or home care), and column (2) estimates equation 3.4 on the share of claimants who did receive observable care. Each regression an individual-level and survey wave fixed effects and time-varying controls, including measures of health status, marital status, assets, and number of children.

Table 3.14: Regression Results, Spousal Labor Force Activity

(a) Applicant sample			
	(1)	(2)	(3)
	Earnings	Hours worked	Working = 1
Disabled	0.551 (1.386)	-0.545 (2.230)	0.019 (0.029)
Disabled * DI	-1.441 (1.439)	-0.610 (2.256)	-0.035 (0.032)
Observations	5,496	2,995	5,353
R-squared	0.131	0.024	0.137
Number of ind	1,172	899	1,147
Mean	20.82	61.12	.59
(b) Health sample			
	(1)	(2)	(3)
	Earnings	Hours worked	Working = 1
Disabled	2.177 (1.407)	-1.323 (2.311)	0.018 (0.030)
Disabled * DI	-1.341 (1.362)	-3.828 (2.590)	-0.050 (0.031)
Observations	13,928	7,793	13,639
R-squared	0.120	0.018	0.121
Number of ind	2,528	2,005	2,494
Mean	21.78	59.05	.56

Notes: Robust standard errors in parenthesis, clustered at the household level. +  $p < 0.1$ , \*  $p < 0.05$ , \*\* $p < 0.01$ . Each column indicates a separate regression. Sample is limited to respondents who have at least one child and are in either the recipient treatment group, or the rejected applicant or disability control samples, respectively. The dependent variables in column (1), (2) and (3) are the disabled individual's spouse's total earnings, the number of hours worked by the disabled individual's spouse, and an indicator for whether the spouse works, respectively. The applicant sample in panel (a) includes claimants who begin receiving DI during their time in the HRS, and claimants who are rejected from DI during their time in the HRS. The health sample in panel (b) compares claimants who begin receiving DI during their time in the HRS with claimants who do not apply or DI, but experience the onset of a disability during their time in the HRS. The indicator "disabled" indicates waves in which the respondent reported a work-limiting health condition. "Disabled\*DI" indicates waves in which the respondent reported being disabled and receiving DI. All regressions include individual-level and survey wave fixed effects and time-varying controls, including measures of health status, marital status, assets, and number of children.

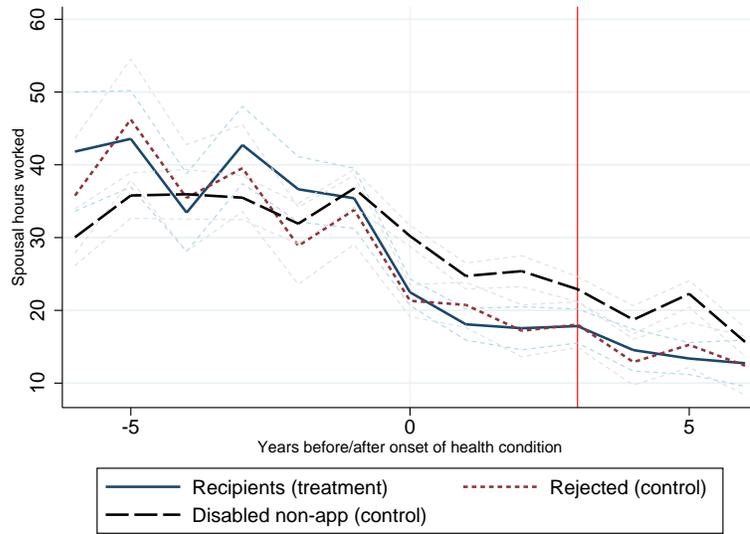
Table 3.15: Additional Robustness Checks

(a) Applicant sample			
	(1)	(2)	(3)
	No weights	No weights	No weights
Disabled	0.031 (0.025)	0.031 (0.025)	0.031 (0.025)
Disabled * DI	0.075** (0.027)	0.074** (0.027)	0.074** (0.027)
Observations	7,563	8,850	8,850
R-squared	0.042	0.039	0.039
Number of ind	1,377	1,617	1,617
(b) Health sample			
	(1)	(2)	(3)
	No weights	No weights	No weights
Disabled	0.034 (0.023)	0.034 (0.023)	0.034 (0.023)
Disabled * DI	0.007 (0.022)	0.006 (0.022)	0.006 (0.022)
Observations	19,467	20,195	20,195
R-squared	0.076	0.076	0.076
Number of ind	3,109	3,242	3,242

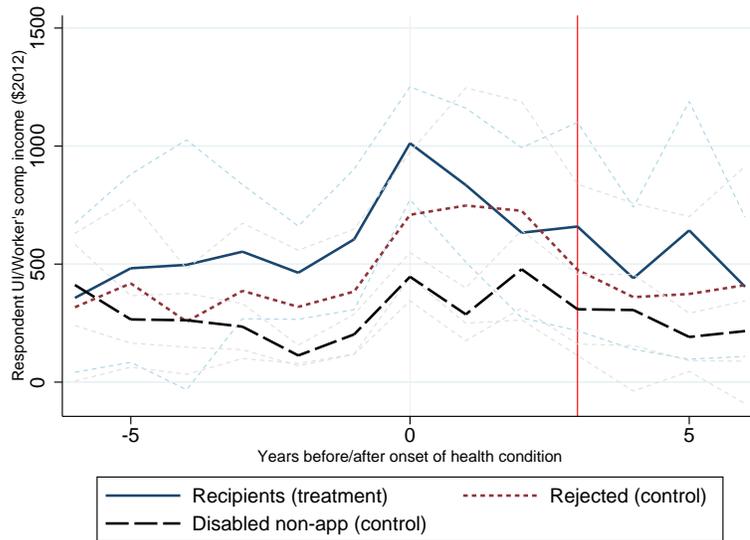
Notes: Robust standard errors in parenthesis, clustered at the household level.  $**p < 0.01$ ,  $*p < 0.05$ ,  $+p < 0.1$ . Dependent variable in each regression is an indicator for receiving an in-kind transfer. Statistics calculated HRS respondent weights and propensity score weights, respectively. Each column estimates equation 3.4 under various robustness checks. Column (1) limits the sample to claimants with more than one wave prior to the onset of their disability. Column (2) controls for other social insurance transfers including unemployment insurance and workers' compensation. Column (3) controls for all other government transfers. The applicant sample in panel (a) includes claimants who begin receiving DI during their time in the HRS, and claimants who are rejected from DI during their time in the HRS. The health sample in panel (b) compares claimants who begin receiving DI during their time in the HRS with claimants who do not apply or DI, but experience the onset of a disability during their time in the HRS. The indicator "disabled" indicates waves in which the respondent reported a work-limiting health condition. "Disabled\*DI" indicates waves in which the respondent reported being disabled and receiving DI. Each regression includes individual and survey-wave fixed effects and time-varying controls, including measures of health status, marital status, assets, and number of children.

Figure 3.4: Additional Trends in Health and Income

(a) Income shock - spousal hours worked

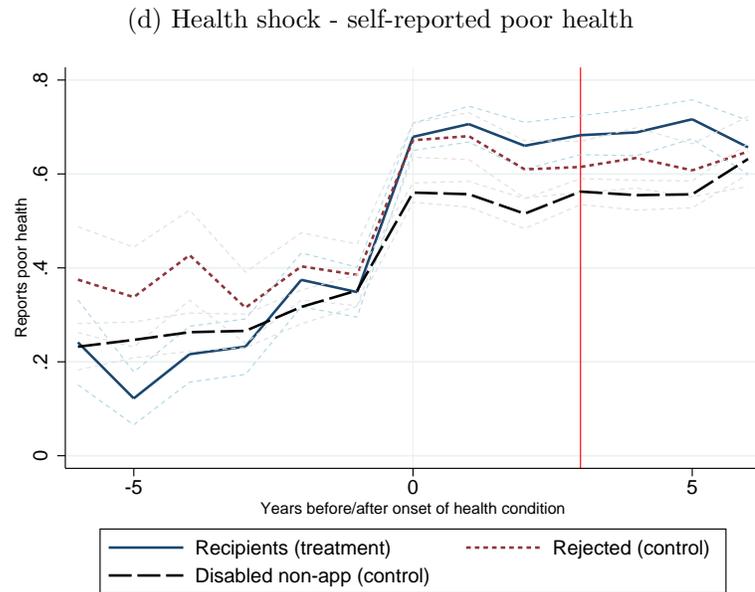
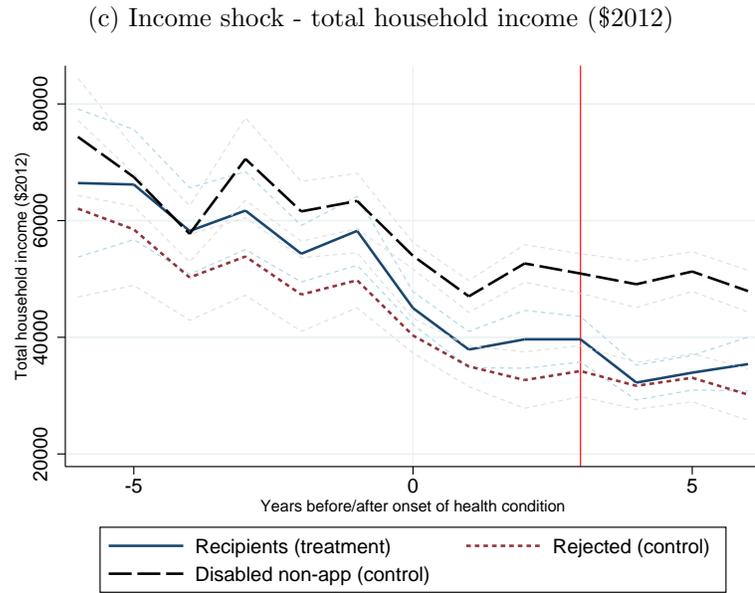


(b) Income shock - non-SSA government transfers (\$2012)



Notes: Data from the Health and Retirement Study, 1992-2010. The lines describe trends in income and health conditions reported before and after the first report of a work-limiting health condition in the survey for the treatment group (recipients) and both control groups (rejected applicants and disabled non-app). The light dashed lines indicate 95-percent confidence intervals. The vertical dashed line indicates the onset of a work-limiting health condition, and the solid line indicates the average time of DI receipt relative to the onset of the disability (approximately 2 and a half years after the first report of a disability).

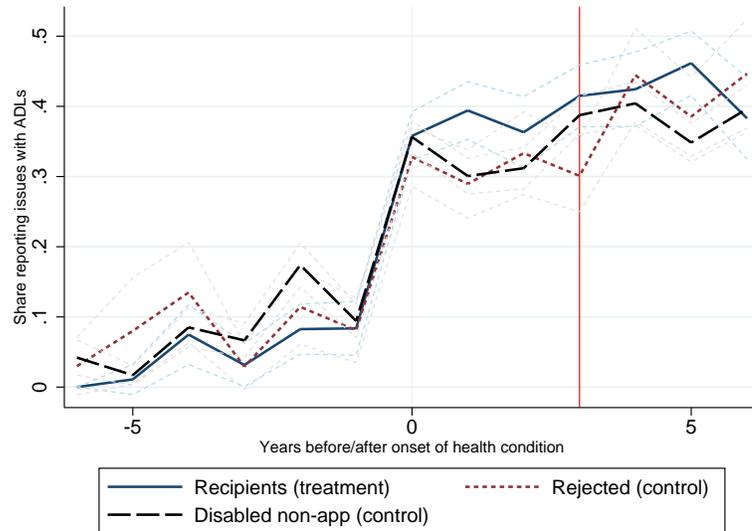
Figure 3.4: Additional Trends in Health and Income (continued)



Notes: Data from the Health and Retirement Study, 1992-2010. The lines describe trends in income and health conditions reported before and after the first report of a work-limiting health condition in the survey for the treatment group (recipients) and both control groups (rejected applicants and disabled non-app). The light dashed lines indicate 95-percent confidence intervals. The vertical dashed line indicates the onset of a work-limiting health condition, and the solid line indicates the average time of DI receipt relative to the onset of the disability (approximately 2 and a half years after the first report of a disability).

Figure 3.4: Additional Trends in Health and Income (continued)

(e) Health shock - reports issues with ADLs



Notes: Data from the Health and Retirement Study, 1992-2010. The lines describe trends in income and health conditions reported before and after the first report of a work-limiting health condition in the survey for the treatment group (recipients) and both control groups (rejected applicants and disabled non-app). The light dashed lines indicate 95-percent confidence intervals. The vertical dashed line indicates the onset of a work-limiting health condition, and the solid line indicates the average time of DI receipt relative to the onset of the disability (approximately 2 and a half years after the first report of a disability).

## Chapter 4: Targeting Efficiency in Disability Insurance: Considering a Functional Assessment

### 4.1 Introduction

Many social insurance programs rely on a categorical eligibility requirement - for example, age, employment status, or family structure - to target benefits more effectively to the intended population. Disability status is used as a way to target social insurance benefits to claimants who have an impairment that affects their ability to work. On one hand, the “tag” of a disability allows the government to transfer a larger benefit to eligible individuals than a universal program could afford ([Akerlof 1978](#)). On the other hand, any screening evaluation to determine who should be tagged will inevitably lead either to admitting claimants who don’t meet the eligibility requirement, excluding claimants who truly are eligible for the benefit, or both ([Diamond and Sheshinski 1995](#); [Kleven and Kopczuk 2011](#)). Ultimately, the success of a screening mechanism for social insurance depends on its ability to minimize these types of errors.

In this paper, Zachary Morris and I analyze the targeting efficiency of the disability assessment used in the determination process for Social Security Disabil-

ity Insurance (DI) and Supplemental Security Income (SSI; together, SSD) benefits. The Social Security Administration (SSA) classifies an individual as disabled if they are “unable to engage in any substantial gainful activity (SGA) because of a medically-determinable physical or mental impairment(s) that is expected to result in death or to last for a continuous period of at least 12 months,” ([Social Security Administration 2015](#)). The verification process to receive disability is thus premised on two major assumptions: (a) that disability implies a complete inability to work, and (b) that inability to work can be determined medically. We study claimants’ functioning based on self-reported survey data to provide a new perspective on these criteria. First, we analyze the extent to which the current “tag” of disability results in claimants receiving benefits when they retain some capacity for work. Secondly, we discuss how a functional assessment could be used to more efficiently target return to work interventions to claimants who may be able to transition back into the labor force.

Many disabilities evolve over time, and the changing nature of disability raises the question of whether it is optimal to characterize claimants based on a binary system of “disabled” or “not”. [Moore \(2015\)](#) finds that claimants with a primary diagnosis of a drug or alcohol addiction who were removed from DI after receiving benefits for 2-3 years had higher rates of later employment than claimants who were on benefits for shorter or longer periods before being removed from the program, suggesting that temporary receipt of DI could have a rehabilitative effect for some claimants. Additionally, [Livermore \(2011\)](#) analyzes a representative sample of SSD beneficiaries and estimates that 40 percent of SSD beneficiaries have work-oriented

goals and expectations. [Von Wachter \*et al.\* \(2011\)](#) and [Mann \*et al.\* \(2015\)](#) demonstrate that there is a wide spectrum of work capacity within the DI beneficiary population, and that younger beneficiaries and beneficiaries with low-mortality impairments such as back pain or mental conditions, likely retain some capacity for work. Other research has found similar trends outside of the United States: [Kostol and Mogstad \(2014\)](#) find that increased financial incentives to return to work significantly increase labor force participation and earnings among younger disability beneficiaries in Norway. The interest and potential capacity for work among current beneficiaries suggests that it could be socially beneficial to introduce a temporary or partial benefit for some subset of disability claimants.

In the current system, SSA evaluates disability applications using a five-step determination process, where reviewers could reach a decision at any step if the relevant criteria are met.<sup>1</sup> Functional information is analyzed in a Residual Functional Capacity (RFC) assessment when a decision is not made during the first three stages. Empirical estimates find that approximately 50 percent of applicants are decided in the last two stages of the determination process, where the RFC is considered ([Hu \*et al.\* 2001](#); [Social Security Administration 2015](#)). The information collected in the RFC could be used to identify higher-functioning beneficiaries who could be targeted for additional employment and rehabilitation supports, in addition to determining eligibility.

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<sup>1</sup>See [Chen and van der Klaauw \(2008\)](#) and [Lahiri \*et al.\* \(1995\)](#) for detailed explanations of the five-stage determination process

In order to identify work capacity, we analyze self-reported data on functioning based on survey questions in the National Beneficiary Survey (NBS), a nationally representative survey of SSD beneficiaries in the United States. We match questions in the NBS to questions used in a functional assessment in the United Kingdom that evaluates disability claimants in order to target return to work interventions. Our analyses find that approximately 13 percent of US beneficiaries would be classified as capable of work-related activity based on the UK target threshold. At the time of the survey, this group, whom we call the “higher-functioning” group, is more than twice as likely to be working (at levels below the SGA threshold) as lower-functioning DI beneficiaries. Higher-functioning beneficiaries are also younger and have higher levels of education, on average. These characteristics suggest that this subgroup of claimants is likely to have a higher potential to work than the average beneficiary and may be an ideal target group for return to work interventions.

Using functional information to target a group for return to work interventions also speaks to broader policy concerns surrounding SSD benefits. There has been steady growth in participation in disability programs over the past twenty-five years, and as a result, increased concerns about the sustainability of disability programs in the U.S. ([Autor and Duggan 2006](#)). Demographic trends and increases in women’s labor force participation account for a large portion of the trend ([Liebman 2015](#)). However, participation has also been growing among younger adults who enter the program with more marginal, non-life threatening disabilities and continue to receive benefits throughout adulthood ([Ben-Shalom and Stapleton 2015](#)). This growing group of beneficiaries has led to an increased policy discussion acknowl-

edging that return-to-work initiatives or a partial disability benefit could stem this growth (Autor and Duggan 2010; Burkhauser *et al.* 2014; Liebman and Smalligan 2013). However, the question remains as to how to identify the beneficiaries who could benefit most from any proposed interventions.

There is also considerable ambiguity in the disability application decision, meaning there is scope to improve the screening process. In recent years, over 30 percent of awardees have been initially rejected from benefits, but later accepted after a lengthy appeal process (Benitez-Silva *et al.* 1999; Social Security Administration 2015). Depending on the final stage of appeal, the appeal process often lasts several years (Office of the Inspector General 2008). Additionally, application reviewers have varying propensities to accept applicants onto DI, and a considerable share of applicants are on the margin of being accepted at the initial stage: Maestas *et al.* (2013) estimates that approximately 23 percent of applications could have had a different outcome had they initially been assigned to a different reviewer. Furthermore, French and Song (2014) also finds considerable variability in administrative law judge decisions. In an audit study of the accuracy of the disability decision, Benitez-Silva *et al.* (2006) uses self-reported disability status data from the Health and Retirement Study to assess the accuracy of SSD benefit decisions and finds that approximately 20 percent of accepted SSD applicants should have been denied, and 60 percent of denied SSD applicants should have been accepted.

Several papers have conceptualized these challenges in designing a screening system for public benefits. In one of the seminal discussions of targeting efficiency, Akerlof (1978) highlights the benefits of using a categorical requirement, or tag, in

determining eligibility or a public program. While the tag allows the government to provide a higher benefit to a smaller group of people, if the tag is mutable, individuals have an incentive to feign eligibility for the program. Akerlof also notes that if the program administrator cannot perfectly observe the tag, there is the possibility that some eligible claimants will be excluded from the program. Our current analysis of functioning addresses concerns about the mutability of the disability tag, and could be adapted to address concerns about entry to SSD programs in future research.

[Diamond and Sheshinski \(1995\)](#) expands on this research in a model of optimal disability benefits. The authors begin with the question of whether a separate disability system is socially beneficial, or whether it would be optimal to provide disabled workers with a standard welfare benefit. They find that even in the case where disability is observed with some error, it is still optimal to target individuals for a separate disability benefit. This result will be true as long as the probability of being truly disabled increases with the severity of the observed disability, even if the observed disability is an imperfect measure of an individual's capacity to work. Another result of their model is that disability benefits will be larger in systems with a smaller population of severely disabled beneficiaries, or in more discriminating systems with better measures of true disability status.

Better measures of disability often introduce more complexity and cost into the application process, but [Kleven and Kopczuk \(2011\)](#) outline a model demonstrating that in many cases, it is optimal to introduce high complexity into a screening process for public benefits, even if it means that this could lead to incomplete take-

up of the program. Ultimately, they show that the optimal level of complexity must make a tradeoff between incidence of type I and type II errors.

Each of these themes is a central concern in the SSD determination process. Applicants have an incentive to feign the “tag”, a severe disability, in order to qualify for benefits. Additionally, the government receives an imperfect measure of disability, and claimants must undergo a complicated application process to be considered for eligibility. As we elaborate in the sections that follow, a functioning evaluation could reduce false tagging and increase the strength of the disability signal. In our setting, the increased burden of collecting functioning information is fairly minimal, given the fact that SSA already collects functioning information in the RFC during the fourth stage of the initial review process. As a result, evaluating claimant’s functioning could work within the bounds of the current system to use existing information more efficiently.

## 4.2 Classification Method and Data

We analyze data in the NBS for this analysis, and use functioning criteria introduced as part of a recent reform to the disability system in the United Kingdom as a benchmark for assessing the functioning status of US beneficiaries. The NBS has so far collected four cross-sectional national surveys of SSD beneficiaries in 2004, 2005, 2006, and 2010, with additional survey rounds planned in the future ([Social Security Administration 2010](#)). The survey collects a wealth of information on SSD beneficiaries, including data on their health, human capital, employment

behaviors, awareness of services, and barriers to work.<sup>2</sup> For this analysis, we pool all respondents from the first four waves of the survey.

The disability determination processes in the United States and the United Kingdom were very similar before a new process was introduced in the United Kingdom in 2008. In 2008, the UK program was replaced with the Employment and Support Allowance (ESA) program. Initial eligibility for the ESA is determined based on a broad functional assessment. Once a claimant is allowed ESA, the next part of the assessment considers the claimant's capability for work-related activity. An assessment of sixteen activities determines if an ESA-eligible claimant is capable of any work-related activity. If at least one of the sixteen descriptors is satisfied, the claimant is placed in the Support Group. Those in the Support Group receive benefits indefinitely with no work conditions attached. If none of these descriptors are met, the claimant is placed in the Work-Related Activity Group. Those in the Work-Related Activity Group receive benefits for fifty-two weeks and are required to attend work-focused interviews and undertake work-related activities, such as training or condition management programs. For more information on the UK system, see [Morris \(2015\)](#). We match the functioning questions used to determine the Work Related Activity Group in the United Kingdom to similar survey questions in the NBS.

We were able to closely match twelve out of the sixteen UK descriptors with questions available in the NBS.<sup>3</sup> In Table 4.1, we compare the functioning ques-

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<sup>2</sup> Public use survey files for the NBS can be found at <http://www.ssa.gov/disabilityresearch/nbs.html>

<sup>3</sup> Because the descriptors in the UK are generally more specific than the matched questions in the NBS, we assume this increases the odds of low-functioning categorization and provides us

tions in the NBS with the descriptors used to determine the Work Related Activity Group. We identified beneficiaries as “higher-functioning” and “lower-functioning” depending on whether the beneficiary answered affirmatively to at least one of these twelve functioning questions. Claimants who did not respond affirmatively to any of these questions are identified as “higher-functioning.”

Approximately 12.7 percent of the weighted SSD beneficiary population is categorized as higher-functioning. Table 4.2 shows the share of lower-functioning beneficiaries who responded that they experience difficulty with each of the indicators listed above. Approximately 12 percent of lower-functioning beneficiaries are classified as lower-functioning based only on a physical health condition, 38 are classified as lower-functioning based only on mental conditions, and the remaining 50 percent qualify as lower-functioning based on both physical and mental conditions. Among the physical conditions, 51 percent of lower-functioning beneficiaries report being unable to walk a quarter mile, and 20 percent of lower-functioning beneficiaries are unable to move between seated positions. Only a minority of lower-functioning beneficiaries experience other physical challenges such as being unable to have their speech understood, issues with manual dexterity, or being unable to eat independently.

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with a more conservative estimate of higher-functioning beneficiaries. The four missing descriptors are highlighted in grey in Table 1. For three of the missing descriptors, there is an NBS question covering a related and less severe functioning criterion. We assume that anyone who would be classified as low-functioning based on one of these three missing questions would also be classified as low-functioning based on one of the existing NBS functioning questions. The one exception is the descriptor on “loss of control of bowel movement,” which does not have a related question in the NBS.

By contrast, mental limitations are more prevalent. Approximately 65 and 68 percent of lower-functioning beneficiaries report having trouble concentrating and coping with stress, respectively. Additionally, 31 percent of beneficiaries report social problems, and 46 percent report that emotional problems prevent them from working. Because the questions concerning mental functioning are fairly general, we perform several tests to determine how sensitive the classification system is to these mental classifications. We re-classify individuals to be higher-functioning if they qualify as lower-functioning by responding “yes” only to one of the most common mental conditions. In general, this reclassification does not alter the size or composition of the higher- and lower-functioning beneficiary groups substantially. The results of these sensitivity tests are available in tables 4.8 - 4.11 in the appendix.

### 4.3 Characteristics and Employment Behaviors of Higher-functioning SSD Beneficiaries

Table 4.3 describes demographic characteristics for higher- and lower-functioning SSD beneficiaries. On average, higher-functioning beneficiaries are significantly younger: 28 percent of higher-functioning beneficiaries are age 40 or younger compared to only 21 percent of lower-functioning beneficiaries. Additionally, they are slightly better educated: 72 percent of higher-functioning beneficiaries have a high school degree or higher, compared to 65 percent of lower-functioning beneficiaries. Higher-functioning beneficiaries have significantly higher household income levels and are less likely to rely on other government assistance. However, similar shares

of higher- and lower-functioning beneficiaries receive benefits from SSI rather than DI, and high and lower-functioning beneficiaries receive similar sized monthly benefits.

Approximately 18 percent of higher-functioning beneficiaries report that they are currently working, significantly higher than the 7 percent of lower-functioning beneficiaries who report currently working. However, few beneficiaries report being aware of job services within or outside of SSA, and higher- and lower-functioning beneficiaries appear equally likely to be aware of these services. While higher-functioning beneficiaries are more likely to have used employment services or job training, only 10-12 percent of beneficiaries report ever having used these services.

Between 10 and 20 percent of beneficiaries report being aware of most SSA services, and there are few differences in awareness of these services between higher- and lower-functioning beneficiaries. Beneficiaries are most aware of the Trial Work Period (TWP), which allows a DI beneficiary to test his or her ability to work while still being considered disabled, and the Ticket to Work program (TTW), which provides free access to employment and rehabilitation assistance.<sup>4</sup> Between 34 and 39 percent of DI beneficiaries report that they have heard of the TWP, and approximately 28 percent of all beneficiaries report that they have heard of TTW. These statistics reveal that higher-functioning beneficiaries view themselves as being more able to work, and have a higher interest in working than lower-functioning beneficiaries. Yet, despite the increased interest in work, very few higher-functioning

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<sup>4</sup>For more information on the Trial Work Period and Ticket to Work, see <http://www.ssa.gov/oact/cola/twp.html> and <http://www.chooseworkttw.net/>.

beneficiaries report being aware of, or ever using, services available to assist them in returning to work.

Within the higher-functioning group, there is also considerable heterogeneity in work activity across age categories. Figures 4.1a - 4.1d show that the youngest higher-functioning beneficiaries, those ages 18-25 and 26-40, are approximately 10-15 percentage points more likely to be working than lower-functioning beneficiaries in their age group. This gap narrows for older beneficiaries, but even the oldest higher-functioning beneficiaries, those over age 55, are approximately 5 percentage points more likely to be working than lower-functioning beneficiaries over 55.

Younger beneficiaries are also more likely to avail themselves of general employment services, job training, and to be enrolled in school. However, consistent with the averages reported in table 4.3, there is not a significant difference in use of these services between higher- and lower-functioning beneficiaries. These figures show that higher-functioning claimants are more likely to be working across all ages, despite the fact that they use employment services at similar rates as lower-functioning beneficiaries. Additionally, only a minority of both higher- and lower-functioning beneficiaries report using existing return to work services.

Figure 4.2a shows that higher-functioning beneficiaries with musculoskeletal or sensory impairments have the highest probability of work. At least 25 percent of higher-functioning beneficiaries in both impairment categories report that they are currently working. The gap in employment between higher- and lower-functioning beneficiaries is also largest for these impairments. Figures 4.2b - 4.2c show less of a pattern in use of employment services and job training by impairment type: while

beneficiaries with musculoskeletal and sensory impairments use these services most frequently, beneficiaries with mental impairments also use these services, but are much less likely to be working. This could suggest that existing employment services are not effective at assisting individuals with mental impairments in returning to work.

#### 4.3.1 Comparison of Functioning and Medical Status

Table 4.4 shows that higher-functioning beneficiaries also report being in better medical health. For example, that only 18 percent of higher-functioning beneficiaries self-reported being in poor or very poor health compared with 47 percent of lower-functioning beneficiaries. Additionally, 69 and 24 percent of higher-functioning beneficiaries report taking medication for physical or mental conditions, compared to 82 and 51 percent for lower-functioning beneficiaries, respectively. Higher-functioning beneficiaries also have significantly higher Mental Component Summary (MCS) and Physical Component Summary (PCS) scores. These measures compile responses to eight mental health and eight physical health related questions, respectively. Higher scores indicate better health: a score of 51 corresponds approximately to the 50th percentile for the general population. See [Ware \*et al.\* \(2001\)](#) and [Livermore \(2011\)](#) for a detailed description of these measures. The average PCS and MCS scores for higher-functioning beneficiaries are 51 and 59, respectively, indicating these claimants' health is comparable to the average population.

Despite the fact that on average, higher-functioning claimants are in better medical health, there is still considerable variance in the medical status of higher-functioning beneficiaries. Figure 4.3 shows the distribution of MCS and PCS scores for high and lower-functioning respondents. The PCS score has a common support for higher- and lower-functioning respondents over the entire range of possible scores, and the MCS score has common support over the majority of the range. If the information gathered from the functional questions captured the same information about claimants' health as the MCS and PCS scores, there would be no overlap in the distribution of MCS and PCS scores for higher- and lower-functioning claimants. The common support in the range of scores for higher- and lower-functioning beneficiaries suggests that the functional criteria provide information about the beneficiaries that would not be determined using only medical information.

While the majority of questions relate directly to medical health, a minority of questions included in the MCS and PCS contain information on functioning, which could explain some of the overlap in the distributions. However, we constructed an alternative index of medical health based on the other medical questions listed in Table 4.4 and continue to see overlap in the distribution of higher- and lower-functioning claimants. The results for this index are shown in figure 4.5 in the appendix.

Additionally, the correlation between a claimant's functioning classification and specific medical criteria is low. Table 4.7 in the appendix shows the correlation between our overall indicator for higher-functioning status and each of the medical criteria included in table 4.4. While there is significant correlation between the

higher-functioning indicator and each of these health conditions, the correlations are fairly low: the majority of correlations range between 0.15 and 0.2 in absolute value, with MCS score having the strongest correlation of -0.37.

#### 4.4 Analysis of Services Correlated with the Likelihood of Working

In addition to examining several characteristics of higher- and lower-functioning beneficiaries separately, we estimate a linear probability model to examine which characteristics are most correlated with work, and how the relationship varies by functioning status. We estimate the following linear probability model:

$$Y_{it} = \alpha + X_{it}\beta + X_{it} * HF\delta + \gamma_t + \epsilon_{it} \quad (4.1)$$

Where  $Y_{it}$  is an indicator equal to 1 if the respondent reports that he is currently working. We include demographic characteristics and benefit information about the respondent in  $X_{it}$ , including age categories, marital status, race, education, and income. In some specifications, we also include variables describing work-related variables, controlling for whether the respondent has used any employment services, job training, or is currently enrolled in school. Then, we interact these variables with an indicator for whether the beneficiary is classified as higher-functioning ( $HF$ ), to test whether any characteristics have a differential impact on the probability of work depending on functioning status. We control for survey wave fixed-effects in  $\gamma_t$ . We also consider other dependent variables including whether the

respondent has ever used general employment services, job training, or is enrolled in school.

Table 4.5 shows key coefficients from the linear probability model. The full regression table is included in the appendix. The results from this model show that conditional on other factors including impairment type, education, benefit size, family composition and awareness of SSA services, the age gradient displayed in figure 4.1a is still quite apparent in the regression coefficients. Compared to the omitted category of beneficiaries over age 55, all other beneficiaries are significantly more likely to be working. The youngest lower-functioning beneficiaries, ages 18-25, are approximately 10 percentage points more likely to be working than the oldest beneficiaries, and even lower-functioning beneficiaries aged 41-55 are approximately 2 percentage points more likely to be working than beneficiaries over age 55. The interaction terms demonstrate an even higher likelihood of work among higher functioning beneficiaries: the probability of work increases by approximately 7-9 percentage points for higher-functioning beneficiaries in all age categories.

The coefficients on impairment type also reflect the trends in the figures: beneficiaries with musculoskeletal and sensory impairments are significantly more likely to be working than beneficiaries who did not report a primary impairment, and the probability of work increases by 13-15 percentage points for higher-functioning beneficiaries in these impairment groups. Other characteristics are correlated with the probability of work: for example, being white, having at least a high school degree, and being unmarried all increase the probability of work. However, the interaction terms for these characteristics on the probability of work are not significant, suggest-

ing that the effect of these characteristics does not vary by functioning status. The effect of most SSA services on the probability of work also did not vary significantly by functioning status. However, higher-functioning beneficiaries who have ever used the TWP are nearly 19 percentage points more likely to be currently working.

Columns (2) and (3) in table 4.5 show the results of the linear probability model using other dependent variables indicating whether claimants have ever used general employment services or job training. While younger beneficiaries are significantly more likely to participate in each of these activities, participation does not vary significantly by functioning status, again consistent with the figures discussed in section 4.2. Awareness of TTW and the TWP were also significantly correlated with use of non SSA employment services or job training, but in general, there is no significant interaction of this awareness by functioning status.

These descriptive results confirm that age is one of the strongest predictors of work status. Furthermore, separating young beneficiaries by functioning status focuses even more on a group of claimants with significant work potential. However, the majority of SSA services do not have a differential effect on employment, and higher-functioning beneficiaries appear no more likely to use general employment services than lower-functioning beneficiaries. Overall employment rates are low: even among higher-functioning beneficiaries, approximately 18 percent of beneficiaries report currently working. While higher-functioning beneficiaries demonstrate a higher level of work capacity, currently available services may not provide effective assistance to help them return to work. Increased awareness of available services, or a more targeted set of services could even further increase the work capacity of these

claimants. Analyzing the causal effect of these programs, particularly on youth, is an important area for future research.

## 4.5 Analyzing Predictive Power of Functioning Criteria

Finally, to analyze how successful functioning criteria are at predicting work activity, figures 4.4a-4.4d contrast the rate of type 1 and type 2 errors that would occur under models to determine a claimant's work capacity. In order to construct these plots, we first predict propensity scores from several different linear probability models. In each model, we vary the explanatory variables included to compare the success of different variables in accurately predicting whether or not a claimant is currently working. In the first model, we only include demographic characteristics; in the second model, we add information on broad impairment types; the third model does not include impairment types but does include the indicator for whether the claimant is classified as higher-functioning; the fourth model includes indicators for all twelve of the questions used to determine functioning status. We also compare models (1) and (4) specifically for claimants under age 40, and beneficiaries who have received benefits for less than the median length of eight years. Table 4.13 in the appendix includes a full list of the variables included in each model.

Then, we consider each value of the propensity score (in intervals of 0.005) as a potential threshold for determining whether or not a claimant should be classified as having work capacity. Each point on the curve represents a given propensity score  $x$ , and assumes that any claimant with a propensity score greater than or

equal to  $x$  is working. Then, this predicted outcome is compared to the claimant's self-reported work status in the survey.<sup>5</sup> In each plot, the share of claimants who report that they do work, but are not assessed to be work-capable based on the propensity score threshold is plotted as the share of type-I errors on the y-axis. The share of claimants who do not report working, but are classified as working by the threshold propensity score  $x$  are plotted as the type-II error rate on the x-axis. Table 4.6 summarizes the exercise for each point on the curve.

The curve represents the tradeoff between the two error types. For example, the left upper corner point (0,1) represents the scenario where the model predicts that no one can work, but in reality everyone is working. In this scenario, it is impossible to have a type-II error, but every case is a type-I error. Similarly, the right bottom corner (1,0) represents a case where in reality no one can work, but the model predicts that everyone can work. Here, every case is a type-II error. The more the line bends towards the origin (the point which represents no error), the more accurate the model's predictions are. The line  $y = 1 - x$  would represent a model with no predictive power: cases would essentially be classified randomly. These plots are the inverse of receiver operating characteristic (ROC) curves that compare and contrast the "specificity" and "sensitivity" of a diagnostic tool in determining the correct binary outcome (e.g., [Bamber 1975](#); [Ben-Shalom and Stapleton 2016](#)). Using this criteria, we compare the predictive power under different models - importantly,

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<sup>5</sup>It is important to note that claimants could under-report their work activity on the survey if they are worried that reporting work could affect their benefit status. We discuss potential implications of this fact below.

comparing models that include our higher-functioning classifiers with those that do not - by assessing which model minimizes the area under this curve.

Figures 4.4a-4.4d compare the curves from different models. Figure 4.4a compares the model that only includes demographic information with the model that includes demographic information as well as the indicator for higher-functioning status (model (1) vs model (3)); figure 4.4b compares the predictive power of impairment information compared to all the functioning criteria (models (2) and (4)); figure 4.4c compares the indicator for high-functioning status and all functioning categories (models (3) and (4)); and figure 4.4d compares baseline demographic information with all functioning categories (models (1) and (4)) specifically for beneficiaries under age 40.<sup>6</sup>

In each model, the propensity score accurately predicts a significant share of claimants' work status: the probability that a claimant is incorrectly classified is 32 percent under the baseline model (1) including only demographic information. Adding either impairment groups or the indicator for high functioning status (models (2) or (3)) only lower the error rate by approximately 1-2 percentage points, and the difference is not statistically significant.

However, including each of the functioning questions individually significantly improves the classification of the model, reducing the error rate to 25 percent. Furthermore, the relative improvement in accuracy is even greater for younger beneficiaries. The baseline demographic model has an error rate of 37 percent when restricted to beneficiaries under age 40, and the model including impairment information has

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<sup>6</sup>Other comparisons are available upon request.

an error rate of 34 percent.<sup>7</sup> By contrast, the higher-functioning criteria improves the error rate to 27 percent for these younger beneficiaries, and the difference is statistically significant.

In an additional comparison, we restrict the analysis to claimants who have been receiving benefits for less than 8 years, which is the median length of benefit receipt in the survey. For these claimants who have not been on benefits as long, the functioning criteria also perform significantly better than the demographic criteria or the impairment criteria. The error rate based on only demographic criteria is approximately 38 percent, compared to 29 percent when the functioning indicators from model 4 are included. Similarly, the error rate when including the impairment indicators, but no functioning criteria, is approximately 32 percent.

Notably, many respondents could be hesitant to respond that they are currently working on a survey about their disability benefits, when they know that working could disqualify them from receiving benefits. Additionally, the ideal time to measure a claimant's functioning and work capacity would be at the time they are admitted to the program, rather than once they have settled in to beneficiary status after several years. These factors both reduce the probability that a claimant will work even if he still maintains capacity to work. As a result, this exercise could represent a lower bound on the share of type-I errors predicted by the model, and could over-represent the share of type-II errors. The overall effect of under reporting of work status on the accuracy of the prediction is, therefore, ambiguous. Future research could apply the same methodology using different measures of work status,

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<sup>7</sup>Figure available upon request.

and could compare claimants who are and are not on disability programs to better examine the effectiveness of these criteria.

## 4.6 Discussion

This paper demonstrates that analyzing claimant functioning can provide a new, potentially more accurate, perspective on the work capacity of disability claimants. Consistent with existing literature, we find that a considerable share of current beneficiaries are currently working, and among young beneficiaries with musculoskeletal and sensory impairments, current employment rates have been as high as 25-30 percent in recent years. More importantly, we find significant heterogeneity within these groups based on functioning status: young, higher-functioning beneficiaries are between 7 and 9 percentage points more likely to be working than young, lower-functioning beneficiaries. Higher-functioning beneficiaries with musculoskeletal or sensory impairments are between 12 and 15 percentage points more likely to be working than lower-functioning beneficiaries in these impairment groups. Additionally, our analysis demonstrates that information on functioning increases the accuracy of predicting which claimants are likely to be currently working. In some models, criteria on functioning performed better than basic information about the claimant's impairment. As a result, functioning criteria, which are already collected as part of the disability determination process, could be used to determine which claimants would be most likely to be successful in return to work interventions.

Table 4.1: Matching the UK Assessment to Functioning Questions in the National Beneficiary Survey

UK Descriptor	Matching NBS Functioning Question
Mobilising unaided by another person or with or without a walking stick, manual wheelchair or other aid	Able to walk a quarter of a mile without assistance at all?
Cannot move between one seated position without assistance	Needs the help of another person in order to get into and out of bed or a chair?
Cannot raise arm	Able to reach over head at all?
Cannot pick up and move a .5 litre carton full of liquid	Able to use hands and fingers to grasp and handle at all?
Cannot press a button; turn the pages	Able to use hands and fingers to grasp and handle at all?
Convey a simple message, such as the presence of a hazard	Able to have speech understood at all?
Understanding communication	Able to hear what is said in normal conversation at all?
Loss of control of bowel movement	None that match
Cannot learn how to complete a simple task	Has a lot of trouble concentrating long enough to finish everyday tasks?
Reduced awareness of everyday hazard to the point require supervision	None that match
Cannot reliably complete 2 sequential personal actions	None that match
Cannot cope with change to the extent that day to day life interrupted	Has a lot of trouble coping with day-to-day stresses?
Cannot cope with social engagement	Has a lot of trouble getting along with other people and making or keeping friendships?
Has on a daily basis uncontrollable episodes of aggressive behaviour	During the past 4 weeks, how much did personal or emotional problems keep beneficiary from doing his/her usual work, school or other daily activities?
Cannot convey food or drink to the mouth	Needs the help of another person in order to eat?
Cannot chew or swallow food or drink	None that match

Table 4.2: Share of Claimants Answering Affirmatively to Indicators for Lower-functioning Status

Share reporting “yes” to each condition:	higher-functioning	lower-functioning
Physical Criteria		
Difficulty having speech understood	0	0.04
Cannot walk a quarter mile	0	0.51
Problems with manual dexterity	0	0.05
Cannot lift hand over head	0	0.11
Cannot move between seated positions	0	0.20
Need help to eat	0	0.05
Mental Criteria		
Difficulty understanding communication	0	0.05
Trouble concentrating	0	0.65
Trouble coping with stress/change	0	0.68
Trouble getting along with others	0	0.31
Emotional problems keep you from work	0	0.46
Summary Measures		
LF based on physical condition only	0	0.12
LF based on mental condition only	0	0.38
LF based on both physical and mental	0	0.50
Observations (unweighted)	2,261	13,929
As a % of total beneficiary pop (unweighted)	13.97%	86.03%
As a % of total beneficiary pop (weighted)	12.70%	87.30%

Notes: Data from all four rounds of the National Beneficiary Survey. Claimants are classified as “lower-functioning” if they answer affirmatively to any of the criteria listed in the above table. Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

Table 4.3: Baseline Characteristics by Functioning Status

	Higher-functioning		Lower-functioning		Pvalue
	Mean	N	Mean	N	
Age 18-25	0.08	2261	0.05	13929	0
Age 26-40	0.2	2261	0.16	13929	0
Age 41-55	0.33	2261	0.39	13929	0
Age 56+	0.4	2261	0.39	13929	0.56
Married	0.31	2261	0.31	13929	0.91
No HS degree	0.28	2261	0.35	13929	0
HS degree	0.42	2261	0.37	13929	0
Nonwhite	0.31	2261	0.29	13929	0.32
HH size	2.35	2251	2.34	13868	0.89
Live with non-family member	0.35	2261	0.36	13929	0.23
Income					
Below FPL	0.45	2261	0.49	13929	0
Below 150% FPL	0.62	2261	0.68	13929	0
Below 200% FPL	0.74	2261	0.79	13929	0
Below 300% FPL	0.85	2261	0.89	13929	0
SSA Benefits					
SSI only	0.29	2261	0.29	13929	0.76
SSDI only	0.56	2261	0.56	13929	0.93
Concurrent	0.15	2261	0.14	13929	0.8
Total monthly SS benefit	814.05	2261	819.41	13929	0.55
Years of eligibility	9.43	2261	9.59	13927	0.29
Job Training Use Since Disability					
Employment services	0.12	2233	0.1	13739	0
Job training	0.1	2248	0.09	13819	0.06
Currently in school	0.1	2253	0.09	13859	0.11
Familiarity with SSA Services - SSI Beneficiaries					
Earned income exclusion	0.12	1067	0.13	6448	0.49
Continued Medicaid eligibility	0.16	1072	0.15	6462	0.34
Student income exclusion (ages<25)	0.1	321	0.08	1505	0.4
Familiarity with SSA Services - SSDI Beneficiaries					
Heard - Trial Work Period	0.34	1010	0.39	6111	0.01
Used - Trial Work Period	0.25	372	0.21	2390	0.11
Extended Medicare eligibility	0.17	1013	0.19	6064	0.11
Familiarity with SSA Services - All Beneficiaries					
Heard - Expedited Reinstatement	0.13	2231	0.14	13678	0.56
Used - Expedited Reinstatement	0.11	313	0.09	1868	0.21
Heard - Ticket to Work	0.28	2226	0.27	13655	0.5

Notes: Data from all four rounds of the National Beneficiary Survey. Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey. P-values shown from a test of equality of means between higher- and lower-functioning groups for the descriptor in each row.

Table 4.4: Reported Medical Conditions by Functioning Status

	Higher-functioning		Lower-functioning		Pvalue
	Mean	N	Mean	N	
Medication for physical	0.69	2245	0.82	13824	0
Medication for mental	0.24	2241	0.51	13804	0
Treated for a health condition within past 4wk	0.16	2250	0.36	13798	0
Health declined over past year	0.17	2261	0.45	13929	0
Poor health over past 4 weeks	0.18	2261	0.47	13929	0
Moderate/severe pain	0.41	2261	0.7	13929	0
Little/no energy	0.27	2261	0.57	13929	0
Overweight	0.69	2261	0.71	13929	0.01
PCS score	51.77	2261	43.24	13929	0
MCS score	59.84	2261	46.42	13929	0

Notes: Data from all four rounds of the National Beneficiary Survey. Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey. P-values shown from a test of equality of means between higher- and lower-functioning groups for the descriptor in each row.

Table 4.5: Key Coefficients from Linear Probability Model

Dependent variable	(1) Currently working	(2) Used employment services	(3) Used job training
Age Groups			
Ages 18-25	0.095** (0.011)	0.107** (0.015)	0.097** (0.014)
Ages 18-25 - HF	0.086* (0.041)	0.035 (0.042)	0.058 (0.037)
Ages 26-40	0.077** (0.009)	0.066** (0.012)	0.055** (0.011)
Ages 26-40 - HF	0.074* (0.032)	0.044 (0.035)	0.068* (0.028)
Ages 41-55	0.019** (0.007)	0.025* (0.010)	0.027** (0.010)
Ages 41-55 - HF	0.072* (0.035)	0.012 (0.030)	0.044 (0.029)
Impairment Type			
Mental Illness	0.017* (0.008)	0.036** (0.011)	0.038** (0.011)
Mental Illness - HF	0.041 (0.049)	0.059 (0.053)	-0.013 (0.045)
Musculoskeletal	0.067** (0.012)	0.045** (0.014)	0.004 (0.013)
Musculoskeletal - HF	0.148* (0.067)	0.052 (0.047)	0.026 (0.042)
Intellectual disability	-0.009 (0.008)	-0.012 (0.011)	-0.002 (0.010)
Intellectual disability - HF	0.092* (0.040)	-0.031 (0.020)	0.021 (0.026)
Sensory impairment	0.016 (0.020)	0.029 (0.029)	0.050+ (0.028)
Sensory impairment - HF	0.128* (0.051)	0.100+ (0.056)	-0.029 (0.039)
Awareness of SSA Services			
Expedited reinstatement	0.025*	0.002	0.014

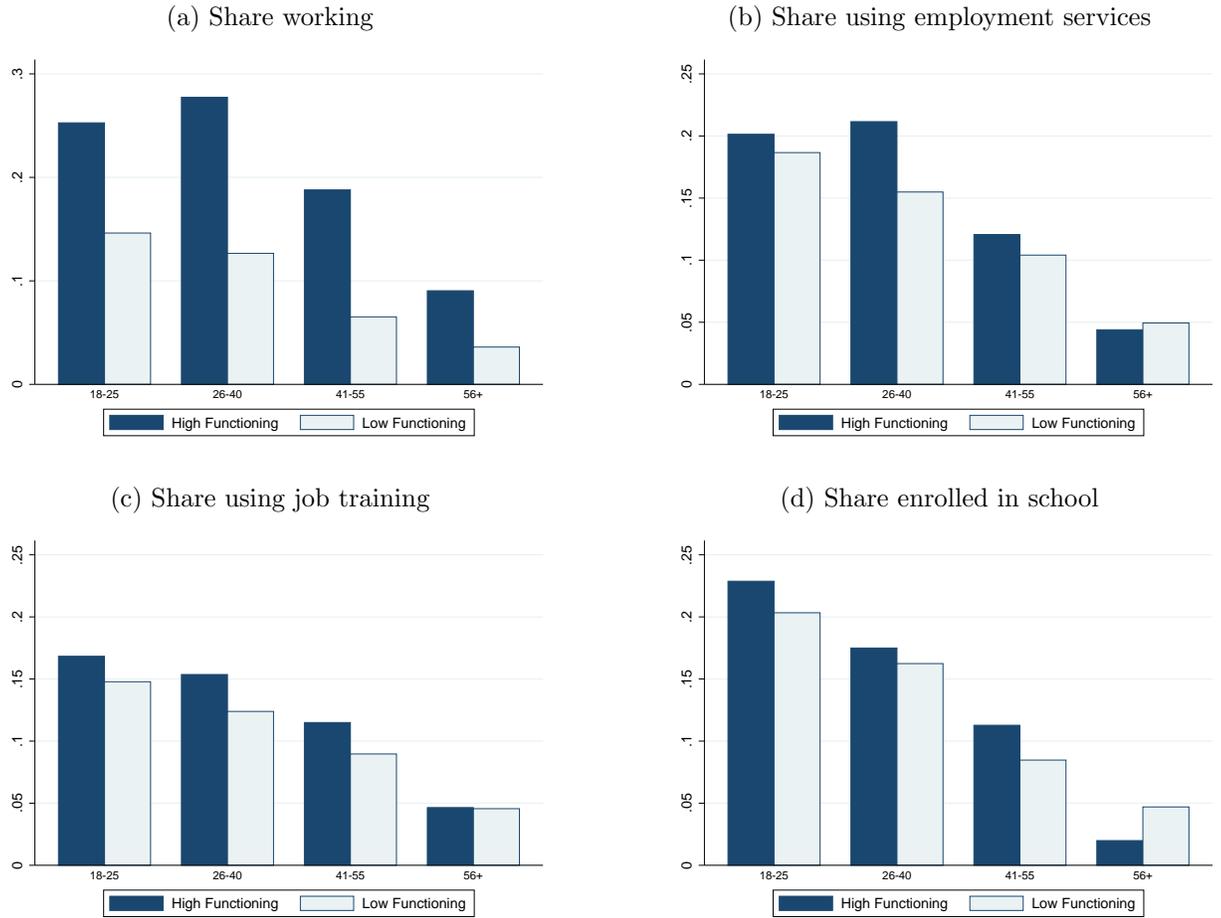
	(0.012)	(0.016)	(0.014)
Expedited reinstatement - hF	0.097	0.007	0.024
	(0.063)	(0.043)	(0.035)
Ticket to Work	0.003	0.048**	0.037**
	(0.007)	(0.010)	(0.010)
Ticket to Work - HF	-0.048	0.008	0.012
	(0.033)	(0.029)	(0.028)
Trial Work Period (used)	0.097**	0.189**	0.122**
	(0.023)	(0.034)	(0.029)
Trial Work Period (used) - HF	0.187**	-0.209**	-0.014
	(0.072)	(0.062)	(0.060)
Observations	11,883	11,883	11,883
R-squared	0.101	0.116	0.067
Mean of dependent variable	0.122	0.137	0.113

Notes: Robust standard errors in parentheses. \*\*  $p < 0.01$ , \*  $p < 0.05$ , +  $p < 0.1$  Table coefficients show the correlation between each descriptor, and the interaction of the descriptor with an indicator for being classified as higher-functioning, on the outcome listed in the column heading. Regression also controls for other demographic characteristics, income, Social Security Benefit size, and survey-wave fixed effects. The results of all of these coefficients are shown in appendix table 4.12. Data from all four rounds of the National Beneficiary Survey. Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

Table 4.6: Assessment for Classifying Claimants Based on Propensity Score Thresholds

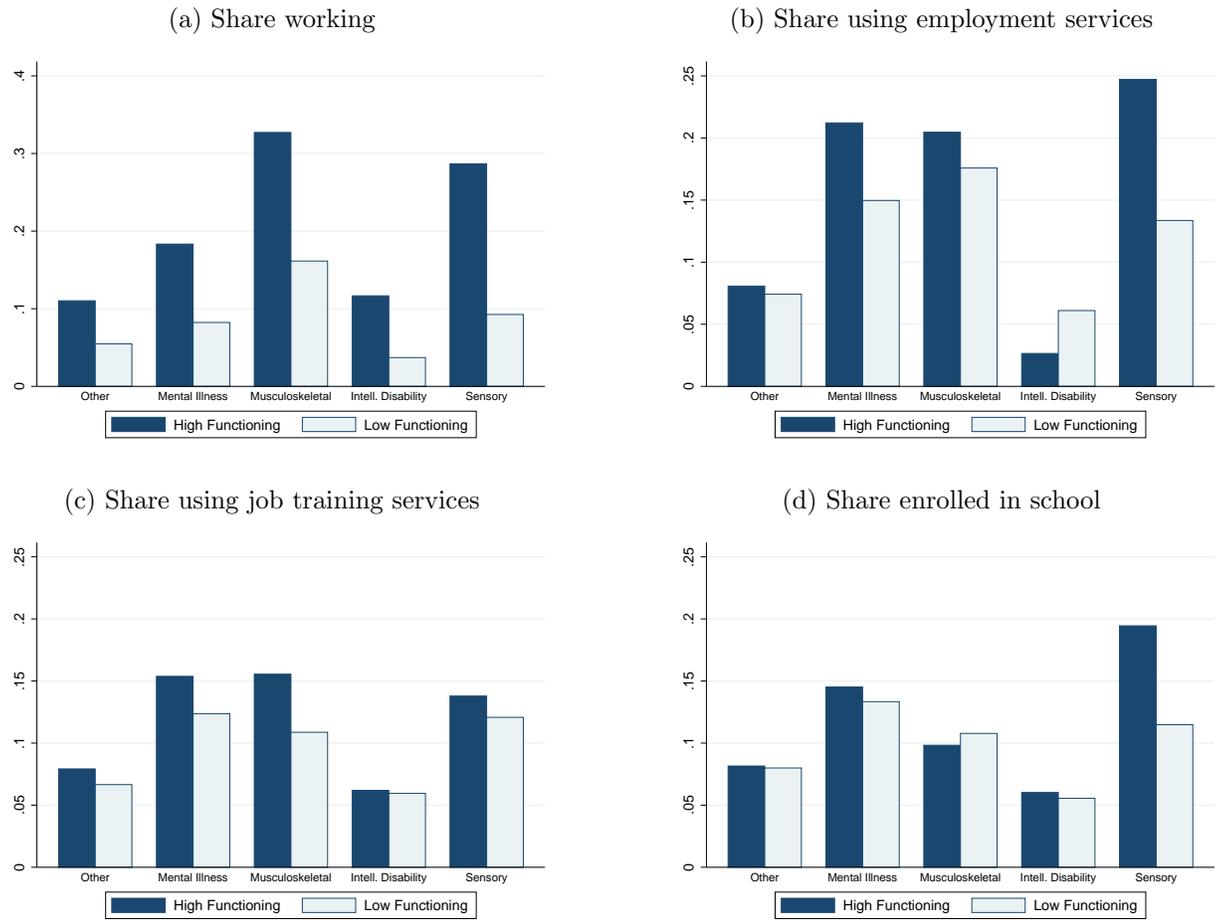
		Does the claimant report that they are currently working?	
		Yes	No
Is the respondent's propensity score $\geq x$ ?	Yes	Yes	Type II error - x-axis
	No	Type I error - y-axis	No

Figure 4.1: Share of Higher- and Lower-functioning Beneficiaries Reporting Work and Job Training Use, by Age Categories



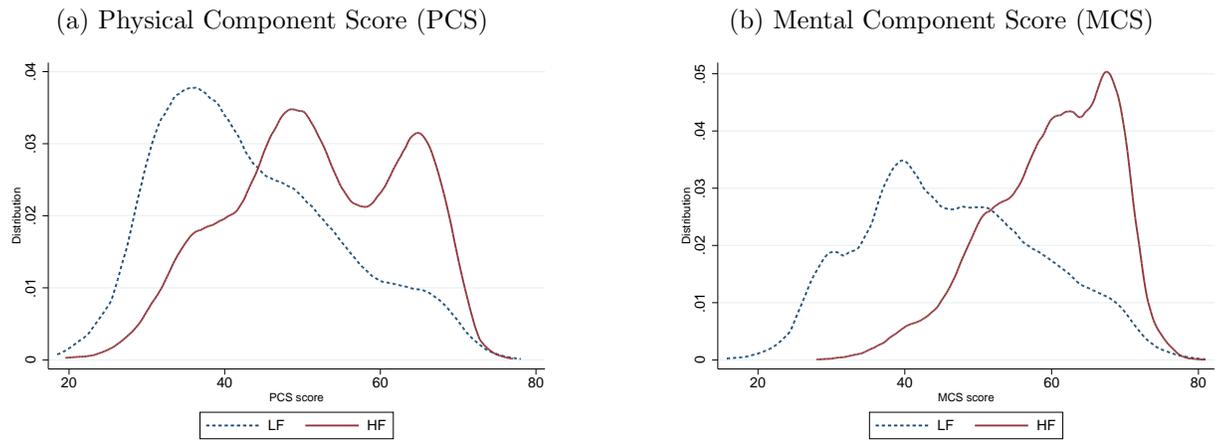
Notes: Data from all four rounds of the National Beneficiary Survey. Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

Figure 4.2: Share of Higher- and Lower-functioning Beneficiaries Reporting Work and Job Training Use, by Impairment Type



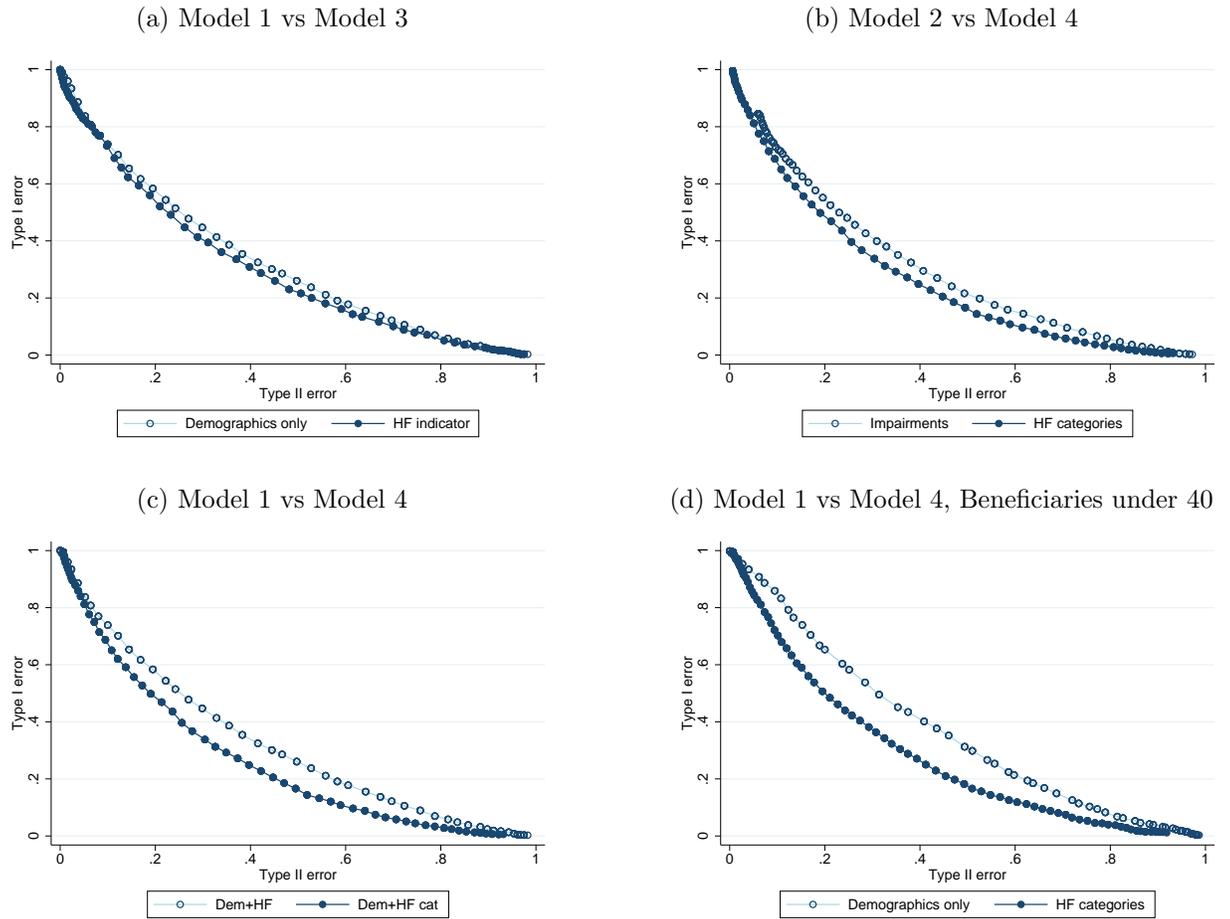
Notes: Data from all four rounds of the National Beneficiary Survey (2004, 2005, 2005, 2010). Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

Figure 4.3: Distribution of Mental and Physical Component Scores, by Functioning Status



Notes: Displays the distribution of MCS and PCS scores for higher- and lower-functioning beneficiaries. MCS and PCS scores are a summary index of a claimant's mental and physical health, with higher scores representing better health. The median MCS and PCS score in the entire population is 51. Data from all four rounds of the National Beneficiary Survey. Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

Figure 4.4: Tradeoff Between Type-I and Type-II Errors Under Different Models



Notes: These curves compare the likelihood of type-1 and type-2 misclassification errors under propensity scores predicted from different models. Each point on the curve represents a given propensity score  $x$ . The y-axis plots the share of claimants who would be misclassified as being unable to work based on their predicted propensity score from the model when they are actually working. The x-axis plots the share of claimants who would be misclassified as being able to work based on their predicted propensity score when they are actually unable to work. The variables included in the propensity score prediction for each model are listed in table 4.13. Smaller areas under the curve represent more accurate predictions. Area under the curve for model 1 (Demographics only): .32 (s.e. .01) Area under the curve for model 2 (Demographics and impairment indicators): .31 (s.e. .01) Area under the curve for model 3 (Demographics and higher-functioning indicator): .3 (s.e. .01) Area under the curve for model 4 (Demographics and functioning category indicators) .25 (s.e. .01) Area under the curve for model 1, under age 40: .37 (s.e. .01) Area under the curve for model 4, under 40: .27 (s.e. .01)

## 4.7 Appendix

Table 4.7: Correlation of Higher-functioning Indicator and Reported Medical Conditions

	Higher-functioning
Low-functioning	1.000
MCS score	0.374
PCS score	0.236
Moderate/severe pain	- 0.197
No energy	-0.205
Medication for physical	- 0.104
Medication for mental	-0.185
Treated for a health condition within past 4wk	-0.164
Health declined over past year	- 0.182
Poor health over past 4 weeks	-0.191

Notes: Represents correlation between indicator for higher-functioning status and listed medical conditions. All correlations are significantly different from zero. Data from all four rounds of the National Beneficiary Survey. Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

Table 4.8: Sensitivity - Classify Claimants with Trouble Concentrating to Higher-functioning

	Higher-functioning		Lower-functioning		Pvalue
	Mean	N	Mean	N	
Age 18-25	0.09	3021	0.05	13169	0
Age 26-40	0.2	3021	0.16	13169	0
Age 41-55	0.34	3021	0.39	13169	0
Age 56+	0.38	3021	0.39	13169	0.09
Married	0.29	3021	0.32	13169	0.01
No HS degree	0.3	3021	0.35	13169	0
HS degree	0.42	3021	0.37	13169	0
Nonwhite	0.31	3021	0.29	13169	0.04
HH size	2.34	3009	2.34	13110	0.94
Live with non-family member	0.35	3021	0.36	13169	0.34
Income					
Below FPL	0.46	3021	0.49	13169	0.01
Below 150% FPL	0.63	3021	0.68	13169	0
Below 200% FPL	0.74	3021	0.79	13169	0
Below 300% FPL	0.85	3021	0.89	13169	0
SSA Benefits					
SSI only	0.31	3021	0.29	13169	0.01
SSDI only	0.54	3021	0.57	13169	0.01
Concurrent	0.15	3021	0.14	13169	0.83
Total monthly SS benefit	803.1	3021	821.82	13169	0.02
Years of eligibility	9.72	3021	9.54	13167	0.16
Job Training Use Since Disability					
Employment services	0.12	2982	0.1	12990	0
Job training	0.1	3005	0.08	13062	0
Currently in school	0.11	3013	0.09	13099	0
Familiarity with SSA Services - SSI Beneficiaries					
Earned income exclusion	0.14	1456	0.13	6059	0.28
Continued Medicaid eligibility	0.17	1463	0.15	6071	0.05
Student income exclusion (ages<25)	0.09	474	0.09	1352	0.85
Familiarity with SSA Services - SSDI Beneficiaries					
Heard - Trial Work Period	0.35	1303	0.39	5818	0
Used - Trial Work Period	0.24	472	0.21	2290	0.16
Extended Medicare eligibility	0.17	1303	0.19	5774	0.15
Familiarity with SSA Services - All Beneficiaries					
Heard - Expedited Reinstatement	0.13	2977	0.14	12932	0.22
Used - Expedited Reinstatement	0.12	401	0.08	1780	0.03
Heard - Ticket to Work	0.28	2969	0.27	12912	0.44

Notes: Data from all four rounds of the National Beneficiary Survey. Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

Table 4.9: Sensitivity - Classify Claimants with Trouble Coping with Stress to Higher-functioning

	Higher-functioning		Lower-functioning		Pvalue
	Mean	N	Mean	N	
Age 18-25	0.08	2906	0.05	13284	0
Age 26-40	0.21	2906	0.16	13284	0
Age 41-55	0.35	2906	0.39	13284	0
Age 56+	0.37	2906	0.39	13284	0.01
Married	0.31	2906	0.31	13284	0.55
No HS degree	0.28	2906	0.35	13284	0
HS degree	0.43	2906	0.37	13284	0
Nonwhite	0.29	2906	0.3	13284	0.85
HH size	2.37	2892	2.34	13227	0.33
Live with non-family member	0.35	2906	0.36	13284	0.35
Income					
Below FPL	0.46	2906	0.49	13284	0
Below 150% FPL	0.63	2906	0.68	13284	0
Below 200% FPL	0.75	2906	0.79	13284	0
Below 300% FPL	0.86	2906	0.89	13284	0
SSA Benefits					
SSI only	0.29	2906	0.29	13284	0.92
SSDI only	0.55	2906	0.56	13284	0.41
Concurrent	0.15	2906	0.14	13284	0.3
Total monthly SS benefit	805.83	2906	821.16	13284	0.06
Years of eligibility	9.58	2906	9.57	13282	0.92
Job Training Use Since Disability					
Employment services	0.12	2872	0.1	13100	0
Job training	0.1	2891	0.08	13176	0
Currently in school	0.1	2897	0.09	13215	0.07
Familiarity with SSA Services - SSI Beneficiaries					
Earned income exclusion	0.13	1374	0.13	6141	0.79
Continued Medicaid eligibility	0.17	1379	0.15	6155	0.03
Student income exclusion (ages<25)	0.09	387	0.08	1439	0.55
Familiarity with SSA Services - SSDI Beneficiaries					
Heard - Trial Work Period	0.36	1294	0.39	5827	0.1
Used - Trial Work Period	0.25	491	0.21	2271	0.06
Extended Medicare eligibility	0.18	1294	0.19	5783	0.36
Familiarity with SSA Services - All Beneficiaries					
Heard - Expedited Reinstatement	0.13	2868	0.14	13041	0.24
Used - Expedited Reinstatement	0.11	402	0.09	1779	0.19
Heard - Ticket to Work	0.27	2864	0.27	13017	0.54

Notes: Data from all four rounds of the National Beneficiary Survey. Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

Table 4.10: Sensitivity - Classify Claimants with Trouble Coping with Stress and Concentrating to Higher-functioning

	Higher-functioning		Lower-functioning		Pvalue
	Mean	N	Mean	N	
Age 18-25	0.09	4869	0.05	11321	0
Age 26-40	0.21	4869	0.15	11321	0
Age 41-55	0.37	4869	0.39	11321	0.01
Age 56+	0.33	4869	0.41	11321	0
Married	0.28	4869	0.32	11321	0
No HS degree	0.3	4869	0.35	11321	0
HS degree	0.41	4869	0.36	11321	0
Nonwhite	0.3	4869	0.3	11321	0.77
HH size	2.38	4845	2.33	11274	0.03
Live with non-family member	0.35	4810	0.35	11152	0.94
Income					
Below FPL	0.47	4869	0.49	11321	0.13
Below 150% FPL	0.65	4869	0.68	11321	0
Below 200% FPL	0.76	4869	0.79	11321	0
Below 300% FPL	0.86	4869	0.89	11321	0
SSA Benefits					
SSI only	0.32	4869	0.28	11321	0
SSDI only	0.53	4869	0.57	11321	0
Concurrent	0.15	4869	0.14	11321	0.1
Total monthly SS benefit	790.89	4869	828.25	11321	0
Years of eligibility	9.82	4869	9.48	11319	0
Job Training Use Since Disability					
Employment services	0.12	4798	0.1	11174	0
Job training	0.11	4834	0.08	11233	0
Currently in school	0.11	4851	0.08	11261	0
Familiarity with SSA Services - SSI Beneficiaries					
Earned income exclusion	0.14	2374	0.13	5141	0.08
Continued Medicaid eligibility	0.17	2380	0.14	5154	0.01
Student income exclusion (ages<25)	0.08	725	0.09	1101	0.71
Familiarity with SSA Services - SSDI Beneficiaries					
Heard - Trial Work Period	0.37	2084	0.39	5037	0.12
Used - Trial Work Period	0.24	788	0.21	1974	0.13
Extended Medicare eligibility	0.18	2073	0.19	5004	0.32
Familiarity with SSA Services - All Beneficiaries					
Heard - Expedited Reinstatement	0.13	4795	0.14	11114	0.24
Used - Expedited Reinstatement	0.12	650	0.08	1531	0
Heard - Ticket to Work	0.28	4780	0.27	11101	0.02

Notes: Data from all four rounds of the National Beneficiary Survey. Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

Table 4.11: Sensitivity - Classify Claimants who Cannot Walk a Quarter Mile to Higher-functioning

	Higher-functioning		Lower-functioning		Pvalue
	Mean	N	Mean	N	
Age 18-25	0.06	3036	0.06	13154	0.75
Age 26-40	0.16	3036	0.17	13154	0.23
Age 41-55	0.32	3036	0.4	13154	0
Age 56+	0.46	3036	0.37	13154	0
Married	0.35	3036	0.3	13154	0
No HS degree	0.27	3036	0.35	13154	0
HS degree	0.4	3036	0.37	13154	0
Nonwhite	0.29	3036	0.3	13154	0.33
HH size	2.29	3024	2.35	13095	0.03
Live with non-family member	0.28	3002	0.37	12960	0
Income					
Below FPL	0.42	3036	0.5	13154	0
Below 150% FPL	0.6	3036	0.69	13154	0
Below 200% FPL	0.71	3036	0.8	13154	0
Below 300% FPL	0.84	3036	0.89	13154	0
SSA Benefits					
SSI only	0.26	3036	0.3	13154	0
SSDI only	0.61	3036	0.55	13154	0
Concurrent	0.13	3036	0.15	13154	0.01
Total monthly SS benefit	844.97	3036	812.43	13154	0
Years of eligibility	9.27	3036	9.64	13152	0
Job Training Use Since Disability					
Employment services	0.12	3004	0.1	12968	0
Job training	0.09	3019	0.09	13048	0.9
Currently in school	0.09	3026	0.09	13086	0.44
Familiarity with SSA Services - SSI Beneficiaries					
Earned income exclusion	0.15	1348	0.13	6167	0.01
Continued Medicaid eligibility	0.18	1357	0.15	6177	0.01
Student income exclusion (ages<25)	0.1	372	0.08	1454	0.43
Familiarity with SSA Services - SSDI Beneficiaries					
Heard - Trial Work Period	0.38	1414	0.38	5707	0.96
Used - Trial Work Period	0.23	559	0.21	2203	0.37
Extended Medicare eligibility	0.18	1415	0.19	5662	0.48
Familiarity with SSA Services - All Beneficiaries					
Heard - Expedited Reinstatement	0.15	2996	0.14	12913	0.02
Used - Expedited Reinstatement	0.09	450	0.09	1731	1
Heard - Ticket to Work	0.29	2986	0.27	12895	0.01

Notes: Data from all four rounds of the National Beneficiary Survey. Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

Table 4.12: All Coefficients from Linear Probability Model

Dependent variable	(1)	(2)	(3)
	Currently working	Used employment services	Used job training
Age Groups			
Ages 18-25	0.095** (0.011)	0.107** (0.015)	0.097** (0.014)
Ages 18-25 - HF	0.086* (0.041)	0.035 (0.042)	0.058 (0.037)
Ages 26-40	0.077** (0.009)	0.066** (0.012)	0.055** (0.011)
Ages 26-40 - HF	0.074* (0.032)	0.044 (0.035)	0.068* (0.028)
Ages 41-55	0.019** (0.007)	0.025* (0.010)	0.027** (0.010)
Ages 41-55 - HF	0.072* (0.035)	0.012 (0.030)	0.044 (0.029)
Impairment Type			
Mental Illness	0.017* (0.008)	0.036** (0.011)	0.038** (0.011)
Mental Illness - HF	0.041 (0.049)	0.059 (0.053)	-0.013 (0.045)
Musculoskeletal	0.067** (0.012)	0.045** (0.014)	0.004 (0.013)
Musculoskeletal - HF	0.148* (0.067)	0.052 (0.047)	0.026 (0.042)
Intellectual disability	-0.009 (0.008)	-0.012 (0.011)	-0.002 (0.010)
Intellectual disability - HF	0.092* (0.040)	-0.031 (0.020)	0.021 (0.026)
Sensory impairment	0.016 (0.020)	0.029 (0.029)	0.050+ (0.028)
Sensory impairment - HF	0.128* (0.051)	0.100+ (0.056)	-0.029 (0.039)
Other demographics			
Married	-0.009 (0.008)	-0.023* (0.009)	-0.030** (0.008)
Married - HF	-0.039 (0.037)	0.012 (0.019)	0.042* (0.019)
No HS Degree	-0.018* (0.008)	-0.055** (0.011)	-0.055** (0.010)
No HS Degree - HF	-0.004 (0.036)	-0.046+ (0.028)	-0.034 (0.028)

HS Degree	-0.005 (0.008)	-0.021+ (0.012)	-0.038** (0.010)
HS Degree - HF	0.017 (0.035)	-0.032 (0.027)	-0.014 (0.027)
Nonwhite	-0.014* (0.006)	-0.016* (0.008)	0.004 (0.008)
Nonwhite - HF	-0.037+ (0.022)	-0.019 (0.018)	-0.003 (0.022)
Household size	-0.004* (0.002)	-0.008** (0.003)	-0.000 (0.003)
Household size - HF	0.012 (0.009)	0.005 (0.006)	-0.013* (0.006)
Number of children	-0.006+ (0.003)	-0.002 (0.005)	-0.003 (0.005)
Number of children - HF	-0.007 (0.012)	-0.020+ (0.012)	-0.008 (0.011)
Income			
Income <=FPL	-0.020** (0.007)	-0.013 (0.012)	-0.005 (0.011)
Income <=FPL - HF	-0.003 (0.036)	-0.058 (0.036)	-0.007 (0.029)
Income<=150% FPL	-0.022 (0.014)	0.009 (0.017)	0.006 (0.015)
Income<=150% FPL - HF	-0.030 (0.045)	0.005 (0.040)	-0.041 (0.035)
Income<=200% FPL	0.000 (0.015)	-0.013 (0.016)	-0.017 (0.013)
Income<=200% FPL- HF	-0.000 (0.049)	0.087** (0.033)	0.066* (0.030)
SS Benefit	-0.008** (0.001)	-0.004** (0.001)	-0.002 (0.001)
SS Benefit - HF	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Years of Eligibility	0.001 (0.001)	0.003** (0.001)	0.003** (0.001)
Years of Eligibility - HF	0.002 (0.002)	-0.001 (0.002)	-0.000 (0.002)
SSI Beneficiary	-0.050** (0.008)	-0.004 (0.012)	-0.011 (0.011)
SSI Beneficiary - HF	-0.046 (0.031)	-0.047+ (0.028)	0.002 (0.025)
Concurrent Beneficiary	-0.036** (0.008)	0.031* (0.015)	0.013 (0.014)
Concurrent Beneficiary - HF	0.048	-0.029	-0.001

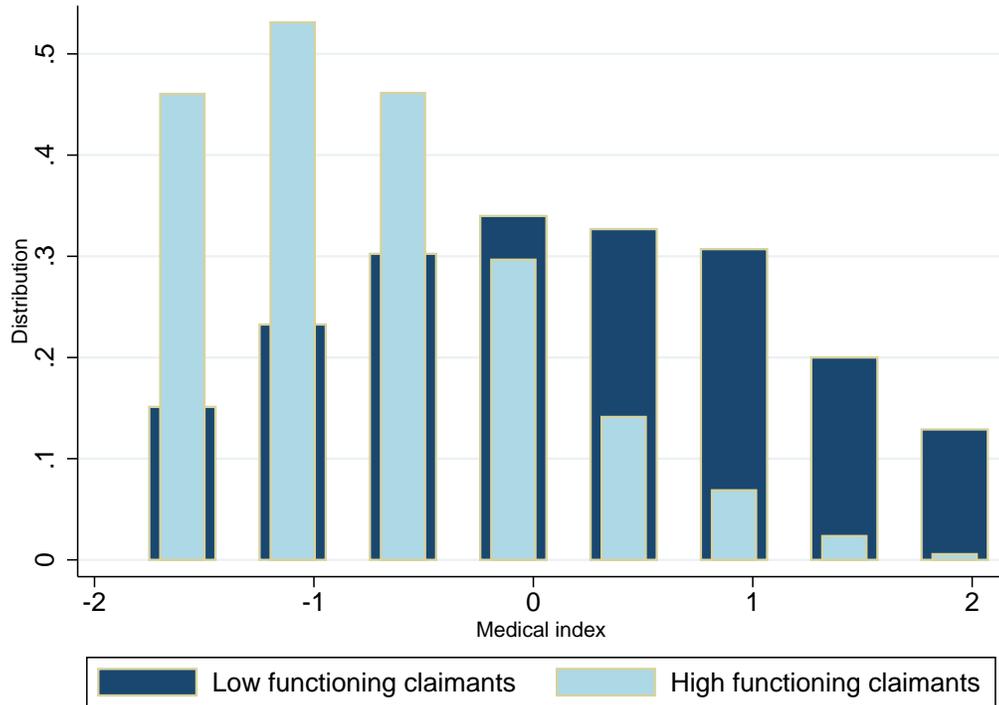
	(0.052)	(0.040)	(0.041)
Awareness of SSA Services			
Ever heard of impairment work expenses	0.022 (0.016)	0.066** (0.024)	0.022 (0.019)
Impairment work expenses - HF	-0.044 (0.064)	0.056 (0.063)	-0.013 (0.042)
Ever heard of expedited reinstatement	0.025* (0.012)	0.002 (0.016)	0.014 (0.014)
Ever heard of expedited reinstatement - HF	0.097 (0.063)	0.007 (0.043)	0.024 (0.035)
Ever heard of benefit counseling	-0.012 (0.010)	0.014 (0.015)	-0.005 (0.014)
Ever heard of benefit counseling - HF	0.062 (0.063)	0.003 (0.052)	-0.018 (0.039)
Ever heard of Ticket to Work	0.003 (0.007)	0.048** (0.010)	0.037** (0.010)
Ever heard of Ticket to Work - HF	-0.048 (0.033)	0.008 (0.029)	0.012 (0.028)
Ever used Trial Work Period	0.097** (0.023)	0.189** (0.034)	0.122** (0.029)
Ever used Trial Work Period - HF	0.187** (0.072)	-0.209** (0.062)	-0.014 (0.060)
Ever used benefit counseling	0.125* (0.051)	0.275** (0.070)	0.103+ (0.062)
Ever used benefit counseling - HF	-0.218+ (0.125)	0.063 (0.178)	-0.039 (0.104)
Ever used expedited reinstatement	-0.018 (0.031)	0.055 (0.051)	-0.014 (0.036)
Ever used expedited reinstatement - HF	-0.056 (0.119)	0.107 (0.133)	0.049 (0.097)
Survey waves			
Wave 2 - 2005	0.002 (0.007)	0.002 (0.008)	0.009 (0.007)
Wave 3 - 2006	0.004 (0.008)	0.008 (0.010)	0.010 (0.009)
Wave 4 - 2010	-0.004 (0.008)	0.004 (0.010)	0.004 (0.009)
Constant	0.145** (0.019)	0.096** (0.020)	0.055** (0.018)
Observations	11,883	11,883	11,883
R-squared	0.101	0.116	0.067
Mean	0.122	0.137	0.113

Notes: Robust standard errors in parentheses. \*\* p<0.01, \* p<0.05, + p<0.1. Displays coefficients showing the correlation between each descriptor, and the interaction of the descriptor with an indicator for being classified as higher-functioning, on the outcome listed in the column heading. Data from all four rounds of the National Beneficiary Survey (2004, 2005, 2005, 2010). Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

Table 4.13: Variables Included in Analysis of Predictive Power

Model 1	Model 2	Model 3	Model 4
Age categories Marital status Education Race Household size Poverty level	Age categories Marital status Education Race Household size Poverty level Indicators for impairments: mental disability, intellectual disability, sensory impairment, musculoskeletal disability	Age categories Marital status Education Race Household size Poverty level Indicator for high functioning	Age categories Marital status Education Race Household size Poverty level Indicator for high functioning  Indicators for separate function- ing questions (see Table 4.2 )

Figure 4.5: Distribution of Aggregated Medical Index, by Functioning Status



Notes: Index created by summing the total number of medical conditions reported by the respondent. All potential medical conditions in the index are listed in table 4.4. Higher values indicate that claimants suffer from more medical conditions. Index standardized to mean 0, standard deviation 1 for all beneficiaries. Data from all four rounds of the National Beneficiary Survey (2004, 2005, 2005, 2010). Statistics calculated with respondent weights, rescaled to maintain the proportion of respondents in each wave of the survey.

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