The Effect of Unconditional Cash Transfers on the Labor Supply of Workers with Disabilities*

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Abstract

We provide novel estimates of income effects for workers with permanent partial disabilities. Exploiting a 2005 reform to workers' compensation benefits in Oregon, we use administrative data on claims and employment to identify income effects by relating differences in benefits to differences in labor supply between observationally identical people before and after the reform. We find that increasing benefits by $1,000 decreases the probability of work by 0.19 percentage points, persisting at least three years after claim closure, and beneficiaries spend 67 percent of the additional income on leisure. The findings are consistent with a rapidly increasing disutility of work.

JEL codes: J14, J22, H51

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1. Introduction

Income effects are an important input into the optimal design of public tax and transfer programs (Baily 1972, Chetty 2008). While the substitution effect reveals the extent to which changes in the shadow price of leisure distort workers' labor supply, the income effect reflects a non-distortionary response as workers re-optimize their leisure-consumption bundle in response to a change in non-labor income. As a result, income and substitution effects have very different implications for public policy: substitution effects indicate the deadweight loss associated with the program design, while income effects are welfare-enhancing. Income effects are thus a critical component to evaluate the efficiency of income support programs, including Social Security Disability Insurance (SSDI) and Workers' Compensation. These large social insurance programs insure against lost earnings due to health impairments that limit active labor force participation.

Several convincing studies provide estimates of income effects for general populations —that is, largely healthy populations—in the U.S. (e.g., Imbens et al. 2001, Golosov et al. 2021) and Europe (e.g., Cesarini et al. 2017, Picchio et al. 2018). But tradeoffs between leisure and consumption depend on individuals' preferences, and specifically on their disutility of work, meaning that income effects likely vary across different populations in important ways. In particular, disutility of work may vary with an individual's underlying health. Estimating income effects among working-age people with significant health problems has proven challenging, largely due to the strong work disincentives inherent in the social insurance programs serving this population. While a large empirical literature examines the extent to which disability insurance benefits reduce labor supply among beneficiaries with remaining work capacity (e.g., Bound 1989, Chen and van der Klaauw 2008, von Wachter et al. 2011, Maestas, Mullen, Strand

2013, French and Song 2014), in nearly all of these studies the estimated labor supply response reflects a combination of income and substitution effects. Even studies that explicitly seek to isolate the income effect of disability benefits using local changes in benefit levels suffer from the fact that the beneficiaries affected by these changes still face broader programmatic work disincentives (Marie and Vall Castello 2012, Autor et al. 2016, Gelber, Moore and Strand 2017).

In this paper, we provide to our knowledge the first estimates of income effects for a population of workers with permanent disabilities unencumbered by programmatic work disincentives. We take advantage of a 2005 reform to the permanent partial disability (PPD) benefit formula for workers' compensation in Oregon. A key feature of this setting is that PPD benefits are calculated and paid at the end of the workers' compensation claim without any contingencies on future work. Benefits are one-time awards and are either provided in one lump-sum or in payments spread out over three to four months. In other words, the PPD benefit provides a one-time increase in income to the beneficiary without distorting incentives to work, allowing us to identify the income effect of benefits in a population of workers with permanent partial disabilities. By contrast, federal disability programs such as the SSDI program provide an annuitized benefit that increases non-labor income but is only provided so long as the beneficiary does not return to work at substantial levels, leading to an inherent substitution effect the closer the worker's desired earnings level is to the substantial gainful activity threshold.

In 2005, Oregon implemented a major reform that changed the calculation of PPD benefits for all new beneficiaries. Importantly, although all beneficiaries were affected by the change in benefits, they were affected in different ways depending on their characteristics and their injury. We exploit variation in the size and direction of the change in benefits to identify the causal effect of an unconditional cash transfer on PPD beneficiaries' subsequent labor supply.[1] Using comprehensive administrative data on workers' compensation claims, disability ratings, and employment records in Oregon, we implement a two-stage least squares approach. We instrument for benefit values with a rich set of formula inputs measured consistently before and after reform for all claims, interacted with indicators for policy regime.

We find that a $1,000 increase in the PPD benefit amount leads to a 0.188 percentage point (0.26%) decrease in the probability of work, corresponding to a labor supply elasticity of -0.023. This effect is persistent through at least the first three years after the end of a workers' compensation claim (corresponding to, on average, four years after the onset of disability), well after the vast majority of any applicants pursuing SSDI benefits would have received a determination (Autor et al., 2015). This suggests a fairly permanent labor supply response that is unlikely to reflect strategic behavior while pursuing benefits from other programs. The same $1,000 increase in PPD benefits leads to a reduction in annual hours of 2.36 (0.21%) and a reduction in annual earnings of $38.56 (0.19%). Considering the fact that we identify a persistent labor supply response to a one-time change in income, these effects are large. A one-time $1,000 increase is equivalent to a $57 per year increase in an annuitized benefit.[2] Thus, we estimate that PPD beneficiaries spend nearly 67% of their additional income on increased leisure time.

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[1] Similar approaches have been used to study the regional effects of the federal minimum wage (Card 1992), the impact of student aid on college enrollment (Nielsen et al. 2010), and the effect of disability insurance benefit generosity on labor supply in Austria (Mullen and Staubli 2016).

[2] We assume a discount factor of 2.4% and an average onset age of 43 to calculate the equivalent amount of an annuity that beneficiaries would receive until the full retirement age of 66. Despite the fact that annuities insure against the risk of outliving one's assets, several studies have documented that individuals tend to prefer lump sum pension settlements to annuities (e.g., Hurd et al, 1998; Brown, 2001; Butler and Teppa, 2007). Thus, our conversion may overestimate the annuity value of the lump sum payment and therefore underestimate the equivalent annuitized income effect.

The estimated income effects vary by different measures of impairment severity. Beneficiaries whose disabilities are more likely to interfere with their ability to work at their pre-injury job are more sensitive to the benefit level than those whose disabilities are less likely to interfere with their ability to return to their pre-injury job. At the same time, the probability of returning to work is similar among beneficiaries with higher and lower medical expenditures. Those with higher medical expenditures arguably have more severe health impairments, but these impairments may not always inhibit subsequent work. Together, these findings suggest that variation in income effects is not driven by differences in impairment severity per se but instead by differences in how the impairment specifically affects one's ability to work.

Our paper contributes to a growing body of work focused on estimating the causal effect of non-labor income on labor supply in various populations. Several papers identify income effects in general populations by exploiting unexpected lottery winnings (Imbens et al. 2001, Cesarini et al. 2017, Picchio et al. 2018, Golosov et al. 2021) or unique cash transfer policies (Jacob and Ludwig 2012, Feinberg and Kuehn 2018).[3] Studies from European settings tend to find small or negligible labor supply responses, especially on the extensive margin. On the other hand, studies from American settings tend to find larger income effects, suggesting that differences in population characteristics could matter for the size of income effects. For example, Cesarini et al. (2017) estimate the effect of receiving one million Swedish krona on the probability of employment is -2.015 percentage points; in U.S. dollars, the effect of a $1,000 increase in non-

[3] Jacob and Ludwig (2012) exploit a housing voucher lottery to estimate a negative effect of housing vouchers on labor supply. Feinberg and Kuhn (2018) estimate a negative labor supply response to year-to-year variation in the Alaska Permanent Fund (APF) dividend. Jones and Marinescu (2021) also analyze the effect of the APF on labor supply, but focus on the *macroeconomic* effects of the APF; they find no significant employment effect on the extensive margin and a moderate *increase* in the share of workers in part-time employment, suggesting some reduction in full time work on the intensive margin.

labor income on employment is -0.018 percentage points. Golosov et al. (2021) estimate the same effect at -0.037 percentage points in the U.S.

Focusing on individuals with disabilities, two noteworthy studies by Marie and Vall Castello (2012) and Gelber, Moore and Strand (2017) exploit kinks or nonlinearities in formulae for disability benefits to estimate the effect of the level of monthly disability benefits (received until the full retirement age) on the labor supply of public disability insurance beneficiaries in Spain and the U.S., respectively. Relatedly, Autor et al. (2016) examine the effect of the 2001 Agent Orange decision that expanded eligibility of U.S. veterans with type 2 diabetes who served in the theater for Disability Compensation (DC) benefits on labor supply. If we rescale estimates from these studies to represent the effect of a one-time change in benefits, Marie and Vall Castello (2012) yields an employment effect of -0.023 percentage points per $1,000 increase in the present discounted value (PDV) of disability benefits for Spanish PPD beneficiaries (around age 55), Gelber, Moore and Strand (2017) yields an employment effect of -0.099 percentage points per $1,000 increase in the PDV of SSDI benefits, and Autor et al. (2016) yields an employment effect of -0.159 percentage points per $1,000 increase in the PDV of DC benefits. Comparing these estimated income effects to estimates from general populations, it is clear that individuals with disabilities tend to be more sensitive to non-labor income. At the same time, income effect estimates from studies of disability beneficiaries from the Spanish PPD, SSDI and DC programs may be under-estimates, as these individuals would still be subject to broader institutional work disincentives built into the disability program design.[4]

[4] For example, benefits in the Spanish DI program studied by Marie and Vall Castello (2012) are suspended if beneficiaries start working. Similarly, all workers receiving any benefits on either side of the primary insurance amount kink in SSDI examined by Gelber, Moore and Strand (2017) are still subject to the "substantial gainful activity" limit in order to receive benefits. Autor et al. (2016) report that 15% of DC beneficiaries are deemed "Individually Unemployable" and concede that for these veterans the estimated labor supply effect likely encompasses both income and substitution effects.

Our estimate of a 0.188 percentage point reduction in the probability of work per $1,000 increase in non-labor income is therefore an order of magnitude higher than estimates from the general population and the same order of magnitude as estimates from studies of workers with disabilities in the U.S. One potential explanation for this pattern is that the different income effect estimates reflect different preferences over work that are affected by health. Employing a simple static utility maximization framework, we derive an expression for the income effect showing that that the sensitivity of one's labor supply to non-labor income is a function of the wage (i.e., the shadow price of leisure) and the relative curvature of the utility function with respect to labor supply vs. consumption. Intuitively, the more steeply one's utility falls with labor supplied, the more an individual will find it attractive to use an increase in non-labor income to purchase additional leisure. Our estimate is consistent with a rapidly increasing disutility of work and corroborates prior research findings that workers with permanent partial work-related impairments continue to experience high rates of pain after returning to work (Sears et al. 2021).

The insight that income effects depend on the shape of the disutility-of-work function—specifically, the interactions between health and working conditions—may at least partially explain why prior estimates of income effects tend to be larger in American settings that in European settings. It has long been known that Americans tend to have worse health than Europeans, and these health differences are not an artifact of survey measurement (Banks et al. 2006). Recent comparisons between American and European working conditions using harmonized surveys show that American workers tend to work longer hours, face greater cognitive, physical and social job demands, and have greater exposure to posture-related, ambient, and biological/chemical risks (Eurofound and ILO 2019).

Moreover, recent evidence suggesting American cohorts are experiencing more pain and worse health outcomes than prior cohorts (see, e.g., Case, Deaton and Stone 2020) may have important implications for the evolving efficiency of social insurance programs in the U.S. such as SSDI. At the same time, continued improvements in working conditions, such as physical demands, over time (e.g., Gordon 2016) may temper the effects of worsening health on the velocity of the marginal disutility of work.

Finally, our paper adds an important data point to our understanding of the labor supply of individuals with permanent partial disabilities. In our setting, PPD benefits are awarded to individuals with average whole-body impairment ratings ranging between 5 and 10 percent. Individuals with partial disabilities are an important group to consider for several reasons. First, because their impairments are less severe, they may retain a higher work capacity than those deemed permanently totally disabled (and therefore eligible for programs such as SSDI). As a result, workers with partial disabilities may be more responsive to policy changes than those with more severe disabilities. Second, this group is likely to grow in the near future, particularly as the population ages and as individuals with long-COVID join those suffering from post-viral syndrome (Institute of Medicine 2007, Briggs and Vassall 2021, Brown and O'Brien 2021, NIHR 2022). Third, despite their growing importance, individuals with partial disabilities are generally excluded from the federal disability insurance in the U.S.[5] Several policy proposals advocate that partial benefits should be incorporated into future reforms to the disability insurance system in the U.S. (e.g., SSAB 2006, Mitra 2009, Fichtner and Seligman 2016,

[5] By contrast, individuals with partial disabilities are generally eligible for federal disability insurance benefits in other developed countries.

Maestas 2019). A better understanding of income effects for the population of workers with permanent partial disabilities is essential to inform the optimal design of any such future reforms.

The remainder of the paper proceeds as follows. Section 2 provides institutional background on PPD benefits in Oregon and explains how the 2005 reform affected the calculation of PPD benefits. Section 3 describes the data. Section 4 explains our empirical strategy and the necessary assumptions for causal identification. Section 5 presents results. In Section 6, we derive an expression for the income effect as a function of the wage and preferences for work and consumption using a simple static utility maximation framework, and using this result, put our estimate into context with estimates from the prior literature. Finally, Section 7 concludes.

2. Institutional Background

In Oregon, when a worker files a workers' compensation claim, she first receives temporary total disability (TTD) benefits equal to two-thirds of wages (subject to a minimum and maximum) after a three-day waiting period from the date of injury to cover missed work time due to the injury.[6] The worker may receive temporary benefits as long as a doctor verifies that she is currently unable to work and her condition has not yet stabilized. Eventually, the worker is deemed to have reached "maximum medical improvement" (MMI), the point where no further recovery is expected. At this stage, if there is any residual incapacity due to the injury or illness, the worker is assessed for permanent disability benefits and the claim is closed. On average, it takes just under a year for a claim to resolve, from date of injury to date of claim closure.

[6] See https://www.oregonlaws.org/ors/656.210 for the exact details of how TTD payments are calculated. Workers' compensation beneficiaries are also immediately eligible for health insurance, which covers any medical expenses associated with the workplace injury.

The most common type of permanent disability benefit, and the focus in this paper, is the permanent partial disability (PPD) benefit.[7] If awarded, PPD benefits are provided to the worker at the time of claim closure, regardless of the workers' subsequent work activity.[8] Conditional on receiving PPD benefits, the average time from injury date to claim closure date is just over one year, and 95 percent of workers reach MMI within three years.

In Oregon, PPD beneficiaries' injuries are rated along two dimensions: *impairment severity*, or the extent to which the injury results in impairment in functioning, and (if eligible) *work disability*, or the extent to which the injury might prevent future work. The work disability rating takes into account the worker's age, education, the specific vocational preparation required to perform the pre-injury job, and the relationship between the claimant's base functional capacity (before the injury) and residual functional capacity (after the injury).[9]

In 2003, Oregon passed Senate Bill 757 (SB 757), which introduced a significant change to the PPD benefit formula effective for injuries occurring on or after January 1, 2005. This reform changed how the impairment and work disability percentages appear in the benefit formula, but did *not* change the way these percentages are assessed.[10] Below we explain how PPD benefits were calculated before and after the 2005 reform, and how the reform affected different types of

[7] In 2018, approximately 17 percent of all indemnity claims in Oregon were for PPD awards. The other main type of permanent benefit is permanent total disability, which accounted for 2 percent of all indemnity claims in 2018. See https://www.oregon.gov/dcbs/reports/compensation/indemnity/Pages/index.aspx.

[8] Awards totaling less than $6,000 are provided in a lump sum at claim closure. By default, larger awards are provided in monthly installments at a rate similar to the temporary benefit rate, although workers with larger awards may opt to receive their PPD benefit as a lump sum. We do not observe whether the beneficiary received a lump sum or monthly payment in our data.

[9] Eligibility for a work disability rating is conditional on the worker being assessed as unable to return to the job held at the time of injury. The exact formula for calculating work disability rating can be found under Rule 436-035-0012 of the Disability Rating Standards Oregon Administrative Rules published by the Oregon Department of Consumer Business Services Workers' Compensation Division.

[10] Note that, before 2005, impairment and work disability ratings were expressed as degrees, which we convert into percentages by dividing by 320 (the maximum number of degrees) and multiplying by 100. After 2005, both ratings were expressed as percentages. For simplicity, we refer to the impairment and work disability ratings as percentages when we describe the benefit formulas before and after the reform.

beneficiaries based on their characteristics (specifically, injury type, impairment severity, pre-injury weekly wage, and characteristics associated with work disability eligibility/rating such as age and occupation).

2.1 PPD Benefit Calculation Before the Reform

Prior to 2005, the PPD benefit formula depended on whether the injury involved particular body parts that were listed on a pre-existing schedule ("scheduled injuries") or not ("unscheduled injuries"). For example, injuries to the hand or foot, or hearing loss, were scheduled injuries; unscheduled injuries included conditions such as back pain, shoulder pain, and mental conditions. For scheduled injuries, the extent of impairment was determined relative to the injured body part, whereas for unscheduled injuries, the extent of impairment was determined relative to the whole body. However, both ratings can easily be converted to percentages of the whole body.[11] In addition to being rated for impairment severity, individuals with unscheduled injuries were also potentially eligible for a work disability rating (described above); those with scheduled injuries were ineligible for work disability.

Specifically, the benefit formula prior to the 2005 reform was as follows:

$$PPD_{iT}^{Pre} = (S_i = 1) * \left(BEN_T^S * p_i^p\right) + (S_i = 0) * (BEN_{1T}^U * \min\{p_i^{P+W}, 20\} + BEN_{2T}^U * min\{max\{p_i^{P+W} - 20,0\}, 50 - 20\} + BEN_{3T}^U * max\{p_i^{P+W} - 50,0\}), \qquad (1)$$

where PPD_{iT}^{Pre} denotes the pre-reform PPD benefit awarded to worker *i* for an injury occurring in year *T*, which is a function of whether the injury is scheduled ($S_i = 1$) or unscheduled ($S_i = 0$). Let p_i^P denote the individual's impairment rating expressed as percentage of the person (0-

[11] Rule 436-035-0008 of the Disability Rating Standards Oregon Administrative Rules published by the Oregon Department of Consumer Business Services Workers' Compensation Division explains how impairment ratings for body parts can be converted to impairment ratings for the whole person by dividing the specified degrees for the body part(s) by 320 (the maximum number of degrees for the whole body).

100) and let $p_i^{P+W} = p_i^P + W_i * p_i^W$ denote the sum of the impairment rating p_i^P and, if eligible for work disability, $W_i = 1$, the work disability rating p_i^W.[12] For scheduled impairments, the benefit increased linearly in the impairment rating p_i^P with slope BEN_T^S. For unscheduled impairments, the benefit was a convex kinked function increasing in p_i^{P+W} with kink points at 20 and 50 percent of the whole person. The dollars per percentage (BEN_T^S, BEN_{1T}^U, BEN_{2T}^U, and BEN_{3T}^U) were updated every two years on January 1 in even-numbered years.[13]

2.2 PPD Benefit Calculation After the Reform

In 2005, SB 757 introduced a new rating procedure and benefit calculation to be applied to all PPD cases, eliminating the distinction between scheduled and unscheduled injuries. After the change, all impairments were rated relative to the whole person. Additionally, all claimants, regardless of injury type, are potentially eligible for work disability (if deemed unable to return to the pre-injury job). Finally, if eligible for work disability, the benefit now depends on an additional factor: the individual's pre-injury weekly wage. Specifically, the benefit formula after the 2005 reform is as follows:

$$PPD_{iT}^{Post} = p_i^P * SAWW_T + W_i * p_i^{P+W} * 1.5 * w_{iT}. \tag{2}$$

where PPD_{iT}^{Post} indicates the benefit post-reform, $SAWW_T$ is the state average weekly wage in the year of injury, and w_{iT} is the individual's pre-injury weekly wage (subject to a minimum of 50% and maximum of 133% of $SAWW_T$).[14]

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[12] Though technically the work disability rating is a function of several factors including age and occupation, the rating procedure is the same before and after the reform, and we observe p_i^{P+W} in our data consistently throughout the sample period.

[13] In 2004, these amounts were, respectively: $1,788.80 (scheduled) and $588.80, $1,027.20, and $2,393.60 (unscheduled). See Oregon Workers' Compensation Division Bulletin 111 for details of the PPD benefit formula by injury date from before 1992 until the present: https://wcd.oregon.gov/Bulletins/bul_111.pdf (accessed 10/2/23). We convert dollars per degree to dollars per percentage by scaling by 320/100.

[14] On January 1, 2005, the state average weekly wage was $688.56 and was updated every July 1 thereafter.

In the next subsection, we describe how the reform affected PPD benefits—increasing them for some, and decreasing them for others—based on individual characteristics such as injury type, impairment severity, and pre-injury weekly wage. In the following section, we explain how we leverage this variation in the effect of the reform on PPD benefits by individual characteristics to identify the causal effect of PPD benefits on labor supply.

2.3 Variation in the Effect of the Reform on PPD Benefits By Individual Characteristics

Table 1 illustrates the effect of the 2005 reform on PPD benefits for select cases. For each case, we assume the beneficiary has a pre-injury weekly wage of $600. In the first case, we assume that an individual receives an injury to their arm (a scheduled injury) resulting in an impairment rating of 12% of the person; furthermore, we assume that the individual is not eligible for work disability after 2005. Applying the scheduled benefit formula for 2004, the individual would receive $21,466 in PPD benefits if the injury occurred in 2004; however, this same individual would receive only $8,263 if the injury occurred in 2005, *with no other differences in individual or injury characteristics*. Thus, a change only in the timing of the injury results in a $13,203 decrease in PPD benefits received by an otherwise identical individual.

The second case changes the location of the injury, from the arm (scheduled injury) to the back (unscheduled injury), leaving all other features (impairment rating, work disability rating, etc.) the same as in the first case. Applying the 2004 benefit formula for an unscheduled injury, this individual would receive $7,066 in PPD benefits; because the 2005 reform erased the difference between scheduled and unscheduled injuries, the individual would receive $8,263 in PPD benefits if the injury occurred in 2005—the same benefit amount as an individual with a scheduled injury (case 1). In this second case, changing the timing of the injury from before to after 2005 results in a $1,197 *increase* in PPD benefits.

The next two cases illustrate the effect of eligibility for a work disability award on PPD benefits before and after the reform, for scheduled vs. unscheduled injuries. In the third case, we again consider an individual with an arm (scheduled) injury with an impairment rating of 12% but now we assume they would be eligible for a work disability award after 2005—recall that before 2005, individuals with scheduled injuries were ineligible for work disability. Before 2005, this individual would receive a PPD award of $21,466, the same as someone without a work disability rating (case 1); after 2005, however, the individual would receive $24,463 in PPD benefits—a $2,997 increase. An individual with a back (unscheduled) injury, who is otherwise identical to the individual in case 3, would receive an even larger--$13,865—difference in PPD benefits depending on the timing of the injury (case 4).

Finally, the last two rows of Table 1 illustrate how the balance of impairment vs. work disability rating affects the difference in PPD benefits before and after the reform for individuals with unscheduled injuries. Note that the two individuals in cases 4 and 5 have the same PPD benefit level before 2005 because their impairment and work disability ratings both add up to the same number, 18%. However, after 2005, the two ratings enter the PPD benefit formula separately (rather than as a sum). As a result, the two individuals receive different benefit amounts after the reform—in this case, the individual with the lower impairment rating and higher work disability rating (case 4) receives $4,132 more than the individual with the higher impairment rating and lower work disability rating (case 5) after 2005 (i.e., $24,463-$20,331).

Figure 1 illustrates the effect of the 2005 reform on PPD benefits more generally. We use the benefit formula to calculate the difference between the benefit before and after 2005 for each possible impairment rating, separately for injuries which, based on the body part, would have been classified as scheduled (Panel A) and unscheduled (Panel B) prior to the reform.[15] Because benefits vary with multiple parameters we plot three cases: 1) no work disability rating (blue solid line); 2) a 10% work disability rating and pre-injury wage of $400 (the 25th percentile in our sample) (orange dashed line); and 3) a 10% work disability rating and pre-injury wage of $800 (the 75th percentile in our sample) (grey dash-dot line). Both the sign and magnitude of the hypothetical change in benefit due to the reform vary enormously, ranging from -$75,000 (in very rare cases) to upwards of $45,000 depending on the combination of the type of injury, impairment rating, potential work disability rating and the claimant's pre-injury wage. Prior to the reform, individuals with scheduled injuries tended to receive higher PPD benefits than those with unscheduled injuries, especially if the unscheduled injury was not rated for work disability; however, the reform equalized benefits for those with scheduled and unscheduled injuries, with the presence of work disability (newly available to those with scheduled injuries), along with pre-injury wage, now being the primary driver of differences in benefit levels.

3. Data Sources

Detailed data on injury types, disability ratings, and worker characteristics are essential to examine the effect of this policy change. We use several administrative datasets from the Workers' Compensation Division of the Oregon Department of Business and Consumer Services (DBCS) and the Oregon Employment Department (OED). DCBS provided claim-level data for all closed claims for workers' compensation indemnity benefits between 1987 and 2012. The database includes information about total indemnity and medical payments made on the claim, and key dates including date of injury, first and last dates of total temporary disability (TTD) payments, and claim closure date. Worker characteristics included in the database are date of

[15] Figure A1 plots the benefit levels corresponding to the differences shown in Figure 1.

birth, gender, pre-injury weekly wage, industry and occupation. DCBS also provided information on total permanent partial disability (PPD) awards, injured body parts, and award type (i.e., impairment, work disability). Additional information about return to work at the time of claim closure is provided for the subset of claims with injury years between 2001 and 2012. The dataset also includes impairment ratings for injury years 1999-2012.

DCBS worked with OED to match PPD awards in these years to quarterly wage records in the state Unemployment Insurance (UI) database starting in the third quarter of 1999. We obtained employment data through the fourth quarter of 2013. DCBS and OED matched records using worker Social Security Numbers and excluded outlier records in the wage database as well as observations with inconsistent and incomplete data. OED and DCBS achieved a 97 percent worker match rate between the UI database and workers' compensation claims records. The UI database includes quarterly data on total earnings, hours and an anonymized employer ID.

Together, these data sources give us a detailed account of claimant demographic and injury characteristics, PPD rating and other formula inputs used to calculate PPD benefits before and after the reform. Additionally, we have complete employment information before and after injury for closed PPD claims in Oregon between 2001 and 2013. Because PPD claims can take years to develop and ultimately close, we apply a constant maturity screen to all injury years in our analysis and include claims that were closed within two years of the date of injury. This screen addresses concerns that slow-developing claims in later injury years might not have closed at the time of the match to the wage records and would be disproportionately excluded from the dataset. We restrict our analysis sample to injuries occurring between 2001 and 2009 (that closed by 2010) to observe post-claim labor supply for up to three years after the claim closes (by 2013, the last year for which we have employment data). In our sample, claims last on average approximately one year, meaning we observe labor supply outcomes up to four years after the onset of disability. After these restrictions, our total sample size is approximately 34,000.[16]

Column 1 of Table 2 presents descriptive statistics for all observations in our sample. Sixty three percent of claimants are older than age 40 and 72 percent are men. The average pre-injury weekly wage is $625 in 2005 dollars ($967 in 2023 dollars). Total medical expenditures averaged $12,677 in 2005 dollars ($19,649 in 2023 dollars). Temporary total disability (TTD) benefits were paid for 50 days before claim closure, on average. Slightly more than 80 percent of claimants in the database were released to work by a doctor at the time of claim closure, and just over two-thirds had returned to work prior to claim closure. Over half of claims occur in one of four main occupation categories: transportation (18%), production (17%), construction (12%), and maintenance (7%). Approximately one-third of all claims result from fractures and breaks, and one-third from muscle strains or sprains. Trauma and unexpected injuries account for 15 percent of cases, followed by wounds, cuts and burns (8 percent), with other injuries making up 9 percent of claims.

4. Empirical Strategy

4.1. Instrumental Variables Research Design

The goal of our paper is to estimate the causal effect of PPD benefits on labor supply outcomes of beneficiaries. From a glance at Figure 1, a natural estimation strategy appears. Observationally identical individuals—those with the exact same injury, demographic characteristics and pre-injury job—received in some cases substantially different PPD benefit amounts based solely on whether the individual was injured before or after January 1, 2005. As a

[16] Applying a constant maturity screen of three years (restricting the sample size to 38,000) allows us to observe labor supply up to two years after claim closure and yields similar results (available upon request).

result, we would expect to see a much larger effect of the reform on the labor supply of those individuals whose benefit levels were dramatically changed by the reform.

Figure 2 illustrates this intuition graphically by plotting the relationship between the difference in benefits and difference in outcomes between the pre- and post-reform periods. To construct preliminary estimates of the differences, we compare matched dyads of pre- and post-2005 claims using a nearest neighbor matching algorithm based a comprehensive set of observable characteristics.[17] Within each of the resulting matched pairs, we calculate the difference between the benefits and outcomes, respectively, for claims before 2005 and for claims after 2005. Figure 2 shows that matched pairs with larger absolute differences in benefits have larger absolute differences in labor supply, earnings and hours, and there is a negative relationship between PPD benefits and each outcome. Because PPD benefits are calculated at the time of claim closure and are not affected by post-closure labor supply, the change in labor supply associated with a change in PPD benefit level can be interpreted as an income effect, without an accompanying change in the shadow price of leisure.

Formally, in order to exploit the wide variation in the effect of the 2005 reform on PPD benefit levels, we implement an instrumental variables approach. Specifically, we estimate a two-stage model of the following form:

$$E[PPD_{iT}] = POST_{iT} * Z_i * \psi + Z_i \, \gamma + \lambda_T. \qquad (3)$$

$$E[y_{it}] = \phi PPD_{iT} + Z_i \, \beta + \delta_T. \qquad (4)$$

In the first stage, we predict the benefit PPD_{iT} for worker i who was injured in year T using information about a comprehensive set of observable demographic and case characteristics Z_i

[17] Specifically, we match on: age, pre-injury wage, medical expenditures, TTD duration, gender, body part of injury, return/release to work, impairment rating for scheduled injuries, and the sum of impairment and work disability ratings for unscheduled injuries.

(described in detail below), interactions between Z_i and an indicator for claims that occurred after 2005 ($POST_{iT}$* Z_i), and injury year fixed effects λ_T.[18] We use the benefit formulas shown in equations (1) and (2) to determine which variables and interactions belong in equation (3), as well as the appropriate functional forms (e.g., splines in the sum of impairment rating and work disability rating for unscheduled injuries to account for the convex kinked benefit function prior to the reform). In the second stage, we regress labor supply outcome y_{it} in post-closure year t (i.e., t years after the claim was closed) on predicted benefits from equation (3), along with controls for observable characteristics and injury year fixed effects. The coefficient ϕ represents the causal effect of an increase in PPD benefits (unconditional income) on labor supply.[19]

A limitation of administrative claims data, both generally and in our setting, is that, while it includes rich information on demographic and case characteristics, it only captures the specific formula inputs—or combination of inputs—required to calculate benefits in the contemporaneous policy regime and not in alternative regimes. In particular, prior to 2005, work disability ratings (if any) were not reported separately from impairment ratings for unscheduled claims since only the sum of the ratings was needed to calculate benefits. Moreover, since scheduled claims were ineligible for work disability awards prior to 2005, we do not know which of these claims would have been eligible for work disability, or what the rating would have been. Finally, due to the change in focus from ratings based on individual body parts to those based on

[18] More precisely, we condition on and interact observable characteristics with a *series* of "policy regime" fixed effects based on injury year to account for different benefit schedules based on changing factors within regime (i.e., scheduled and unscheduled degrees per dollar, state average weekly wage).
[19] As pointed out by Callaway et al. (2021), if ϕ is heterogeneous, then identification based on a dose-response relationship implicitly assumes the "dose" is randomly assigned with respect to the potential treatment effect (ϕ). In our setting, since the dose is determined by a complex combination of factors (specifically, impairment type, impairment rating, eligibility for work disability, work disability rating, and pre-injury wage; see Figure 1), the treatment effect is unlikely to systematically vary with the dose.

the whole person, there are some differences in the way specific body parts are recorded in the database before and after the reform.[20]

We account for these asymmetries by using in our analysis only those observable variables that are available and consistently recorded for *all* claims in the data set—both pre- and post-2005. Specifically, we include in Z_i the impairment rating interacted with an indicator for scheduled injuries and the *sum* of the impairment and work disability ratings interacted with an indicator for unscheduled injuries. To address the issue of missing (non-separate) work disability eligibility and rating in the pre-reform period, we include in Z_i a comprehensive set of demographic and case characteristics available in our data that predict work disability eligibility and rating, including as whether the claimant returned or was released to work by a physician prior to claim closure, age, gender, injury type, medical expenditures, TTD duration and occupation categories.[21] We use this set of characteristics instead of the actual work disability even in cases where we observe the work disability rating to ensure consistency in our prediction across all claims. Finally, to account for differences in injury records across regimes, we aggregate individual injuries into broader body systems (e.g., combining injuries to the hand and fingers). We derive the specification for the first stage equation (3) from the known PPD benefit

[20] For example, suppose a worker burned her hand. Prior to the reform, each finger would receive a separate scheduled rating based on the extent of the burn to that finger, and the multiple injuries would be combined to obtain a total scheduled award. After the shift to assessment of impairment for the whole person, the distinction between hand and finger no longer mattered, so the same burn would more likely be reported simply as an injury to the hand.

[21] As discussed in Section 2, workers are eligible for work disability if they are deemed unable to return to their pre-injury job. Conditional on eligibility, the work disability rating is a function of the worker's age, education, the specific vocational preparation required to perform the pre-injury job, and the relationship between the claimant's base functional capacity (before the injury) and residual functional capacity (after the injury). Our data set does not include education or claimants' base and residual functional capacity, but it does include a rich set of demographic, injury and occupation characteristics that are correlated with work disability eligibility and rating observed in post-2005 claims (see Tables A1 and A2).

schedules in equations (1) and (2), replacing inconsistently observed variables with their consistently observed predictors.[22]

Figure 3 plots the first stage: the observed PPD benefit against the predicted benefit estimated from equation (3), separately for claims before 2005 (Panel a) and after 2005 (Panel b). The predicted benefit lines up exactly with the actual benefit for claims before 2005, as indicated by the fact that all data points fall on the 45 degree line in the chart; this is because all formula inputs required to calculate the pre-2005 benefit are observed for all claims. There is more noise in the predicted benefit for post-2005 claims due to the fact that our controls do not perfectly predict work disability, but the trend still tracks the 45 degree line closely.[23] Overall, the first stage regression has an F-statistic of 233.5 and an R-squared of 0.94, indicating that the instruments collectively are strongly predictive of the actual benefit.

4.2. Identifying Assumptions and Potential Threats to Identification

The key identifying assumption is that, conditional on Z_i, variation in observed benefits (driven by the policy change, $POST_{iT}* Z_i$) is uncorrelated with unobserved determinants of labor supply (e.g., pre-injury labor force attachment or injury severity). Practically, this assumption may be violated if there are systematic shifts in unobserved claim characteristics before and after the reform, whether by chance or due to benefit-maximizing manipulation by beneficiaries or raters. Note that shifts in characteristics across regimes are only problematic if they are correlated with unobserved labor supply determinants.

[22] Note that if we could perfectly predict benefits in both regimes then the first stage is perfectly identified and two-stage least squares will break down; in that case, a control function strategy can be employed to identify the causal effect of benefits on labor supply (see e.g., Mullen and Staubli, 2016).
[23] Figure A2 shows that when we use all available formula inputs in the post-2005 regime only, including the actual work disability rating, we predict actual post-2005 benefits with 100% accuracy.

To investigate whether pre- and post-reform beneficiaries are likely to be similar in terms of their unobservable characteristics, we investigate whether *observable* characteristics are balanced before and after the reform. Table 2 compares observable characteristics of claims in our sample with injury dates before and after 2005.[24] We calculate the difference between post- and pre-2005 claims and present p-values from tests of statistical significance for these differences. Several of the differences are statistically significantly different from zero. Workers after 2005 are more likely to be older than 40 (62 vs. 64 percent), are less likely to be men (73 vs. 71 percent), received TTD benefits for more time (51 vs. 49 days), are more likely to have been released to work prior to claim closure (81 vs. 79 percent), and are more likely to have returned to work prior to claim closure (68 vs. 67 percent). There are also differences in occupations, perhaps reflecting broader macro changes in the economy over the decade. Most of the differences are small in magnitude and the directions do not point to systematic differences in unobserved labor determinants. One difference of note is the fact that the share of injuries categorized as strains and sprains is 11 percentage points less after the reform.

Table 3 compares the distribution of broad body system injuries and the associated PPD ratings before and after 2005, separately for scheduled and unscheduled injuries to account for the comparability issues discussed in Section 4.1. Specifically, for scheduled injuries we show the average impairment rating as a percentage of the person before and after the reform, and for unscheduled injuries we show the average combined impairment and work disability ratings. As shown in Panel A, although there is a statistically significant increase (1.7 percentage points) in the share of claims for scheduled injuries after the reform, the average overall impairment

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[24] Recall from above that, to address the issue of missing (non-separate) work disability eligibility and rating in the pre-reform period, we include in Z_i a comprehensive set of demographic and case characteristics available in our data that predict work disability eligibility and rating. As a result, we cannot regress the observable characteristics in Table 2 on predicted benefits before and after the reform as a specification test.

percentage among scheduled injuries is balance before and after the reform at approximately 5 percent. Similarly, there are a few statistically significant differences in the distribution of injury types for scheduled injuries, but there is only one statistically significant difference in impairment percentage conditional on injury type (for leg/hip injuries) and the differences in ratings are uniformly small. Panel B shows some statistical differences in the distribution of injury types for unscheduled injuries, but importantly there are no statistically significant differences in combined impairment and work disability rating conditional on injury type among unscheduled injuries. The consistency of the impairment and work disability percentages before and after the reform is reassuring in that, although there appear to be some shifts in the composition of injuries over time, there are no systematic shifts in ratings that could be indicative of either changes in rater behavior or differences in the unobserved severity of injuries before and after the reform.

To address potential manipulation in timing by claimants, we first note that the policy regime is determined by the injury date, and it can often take a year or longer for workers' compensation claims to reach the point of maximum medical improvement, when claims are rated for PPD. In the early stages of the claim, workers are unlikely to be able to anticipate the extent of eventual permanent impairment or work restriction. Furthermore, few workers are familiar with the details of the program before experiencing an injury and even fewer were likely aware of the details of SB757 to the extent they could anticipate the implications for their own benefits (Rennane and Cherney 2019). As a result, it is unlikely that workers could strategically manipulate the timing of their injury in order to qualify for a more generous benefit. Overall award rates, claim durations, and the share of claims that ultimately received PPD benefits are all similar before and after 2005, although the frequency of claims and the share of claims receiving PPD trend downward trend during the 2000s (DCBS 2015).

Even if claimants do not strategically delay or expedite their claims around 2005, it is still possible that claimants with higher expected benefits could be more likely to file a claim. In this case, we might expect to observe bunching of more claims from those who could expect to gain from the reform ("winners") and fewer claims from those who could expect to lose from the reform ("losers") after 2005. Because we only observe a claimant's actual benefit and not their benefit in the alternative regime, we test this empirically by using equation (3) to obtain a consistent measure of predicted benefits for all claims before and after the reform using actual ratings from our data, but hypothetically assigning all claims an injury date in 2004 and then again in 2005. Then, we divide the sample into expected winners and losers based on whether the difference between the expected benefit in 2005 and 2004 was positive or negative. Figure 4 presents bin-scatter plots by injury month of the number of claimants, separately for expected winners and losers. In both cases, we find no evidence of a discontinuous change or bunching in claiming behavior before vs. after the reform, and if anything we find there were more claims from expected losers and fewer claims from expected winners after the reform.[25] We formally tested whether there was a discontinuous increase in the number of claimants from the winning group and/or decrease in the number of claimants from the losing group after the reform, by

[25] Similarly, Figure A3 presents bin-scatter plots by injury month for key benefit formula inputs. As shown in the first three panels, the trends in the worker age at injury, pre-injury weekly wage, and share of claims that had returned to work at the time of claim closure are relatively flat and smooth, slowly increasing over time. The last three panels of Figure A3 show the trend in the share of claims with injuries that would be classified as scheduled before 2005 trends smoothly through 2005. The trends in the impairment and work disability ratings increase and fall over time, but there are no significant shifts around 2005. Figure A4 shows bin-scatter plots for total claim volume over time and we also do not see bunching; however because the benefit change resulted in winners and losers the implications of bunching in the entire sample are unclear; Figure 4 provides a more direct test of bunching as discussed above.

regressing claim counts on a post-2005 indicator variable, a flexible polynomial in month of injury and an interaction between the post-2005 indicator and polynomial in month of injury. The coefficients on the post-2005 indicators were not statistically significant, providing no evidence of bunching.

Taken together, we find some evidence of small, idiosyncratic differences in the observable characteristics of claims before and after the reform, but no evidence of strategic manipulation either by potential claimants or examiners. We conclude that, conditional on observed characteristics, claims are likely to be comparable to one another before and after the 2005 reform in terms of unobserved severity and other characteristics likely to affect labor supply. As a result, we attribute any observed variation in labor supply (conditional on observable characteristics) to variation in PPD benefit levels resulting from the 2005 reform.

5. Results

5.1 Main Results

Figure 5 presents an event study of the effect of PPD benefits on labor supply over time, examining the estimated effect during the eight quarters before injury, the quarter of claim closure, and the twelve quarters after closure.[26] Panel (a) shows the effect of PPD benefits on (unconditional) weekly earnings, panel (b) shows the effect on (unconditional) weekly hours worked, and panel (c) shows the effect on the probability of employment, for each quarter. Each point estimate on the graphs represents the coefficient on the PPD benefit, scaled by $1,000, from equation (4) where we instrument for PPD benefit using interactions of the post-reform period with observable case characteristics. The eight quarters prior to injury serve as a placebo test since, conditional on case characteristics, the benefit should not be correlated with labor

[26] We omit the quarters occurring between the injury and claim closure (when beneficiaries are not working).

supply before the worker is injured. The effect is very close to zero and not statistically significant in any quarter prior to injury.[27]

We interpret the estimates in the quarters after claim closure as estimates of the causal effects of PPD benefits on labor supply over time. For all three outcomes, the point estimate is largest in the second quarter after claim closure, and the point estimates decline slightly in magnitude over the subsequent ten quarters. In general, however, the effect is quite stable – similar in magnitude and statistical significance for each of our three labor supply outcomes. Because claims close on average approximately one year after injury, these results reflect persistent reductions in labor supply nearly four years after the onset of disability.

Table 4 shows the IV coefficients from models estimating the impact of PPD benefits on labor supply outcomes in the third year after claim closure. Column 1 shows the effect of PPD benefits on the probability of employment in the third year after claim closure, and columns 2 and 3 show the effect of PPD benefits on (unconditional) hours worked and earnings, respectively, during the third year after closure. Across all outcomes, increasing the value of the PPD benefit has a statistically significant negative effect on labor supply, indicating the presence of an income effect. Column 1 shows that increasing the PPD benefit by $1,000 reduces the probability of returning to work by 0.188 percentage points. Using equation (4), we predict that the share of individuals who would return to work at the average PPD benefit (approximately $8,900) is 73 percent of beneficiaries. Compared to this average, the estimated effect of a $1,000 increase in benefits reflects a change of approximately 0.3 percent, and yields an elasticity of -0.023 when scaled by the relative change in benefits (11.2%). Columns 2 and 3 of Table 3 show

[27] The quarter of claim closure can be viewed as a "partially treated" quarter, since claims are closed at varying points during the quarter.

that a \$1,000 increase in the PPD benefit results in a reduction in hours worked by approximately 2.5 hours per year and a reduction in annual earnings of \$39. These results translate into slightly smaller elasticities as the effect on employment (between -0.019 and -0.017, respectively).[28]

5.2 Heterogeneity

Next, we explore the extent to which income effects are heterogeneous along two measures of claim severity: total medical expenditures associated with the injury, and the likelihood of receiving a work disability award. We predict the probability of receiving work disability using the worker characteristics that serve as our controls for work disability in the main regression (shown in Tables A1 and A2), as discussed in Section 4. While medical expenditures indicate the severity of the health condition without regard to its specific effect on work capacity, the likelihood of receiving a work disability award results from the *intersection* of the health condition with the individual's occupational demands. Because the work disability award is determined on the basis of individual functioning and occupational requirements, the probability of receiving a work disability award in addition to an impairment award likely serves as a better proxy for the disutility of work. We present heterogeneity estimates for both measures to understand the impact of the health condition itself, versus its interaction with occupational demands, on responsiveness to PPD benefits. Table 5 presents IV estimates of equation (4) for the subset claims that fall in the lowest and highest quartiles of total medical expenditures in Panel A, and the lowest and highest quartiles of predicted probability of work disability in Panel B.

[28] Table A3 presents results for log hours and earnings, conditional on employment. The estimated elasticities are slightly smaller but still largely consistent with estimates using unconditional hours and earnings.

Panel A shows mixed findings for the comparison of workers with low vs. high medical expenditures. On the one hand, the level effect of PPD benefits on employment is larger for claims with the lowest medical expenditures than for those with the highest medical expenditures: a $1,000 increase in PPD benefits yields a decrease in the probability of working of 0.5 percentage points for those with low medical expenditures, compared to 0.16 percentage points for those with high medical expenditures, in the third year after closure. As a percentage of baseline employment levels, the magnitude of the effect is approximately twice as large for those with low medical expenditures (-0.67%) vs. those with high medical expenditures (-0.24%). However, since $1,000 represents a much larger percent increase in average PPD benefits for those with low medical expenditures (22% vs. 6%), the elasticities are about the same magnitude for those with low vs. high medical expenses. At the same time, the effects of PPD benefits on hours and earnings, both in levels and elasticities, are larger in magnitude for those with *low* medical expenditures compared to those with high medical expenditures.[29]

Panel B of Table 5 presents results by the lowest and highest quartiles of the estimated probability of receiving a work disability award. For those in the highest quartile of work disability propensity, we find that a $1,000 increase in PPD benefits leads to 0.21 percentage point decline in the probability of working, a 2.3 hour reduction in hours worked, and a $40 reduction in earnings during the third year after claim closure. The baseline labor supply outcomes are low for workers with the highest likelihood of receiving a work disability award, so these point estimates result in elasticities of 0.06 for employment, 0.054 for hours, and 0.059 for earnings, respectively. The effect sizes are slightly larger in magnitude for claims in the lowest

[29] Indeed, the results on hours and earnings are not statistically significant for workers in the highest quartile of claim severity. We find similar results for log hours and earnings (see Table A4).

quartile of work disability propensity, though the effects on hours and earnings are only marginally statistically significant. Because baseline employment levels are higher among those less likely to have a work disability award, the elasticities are smaller for those in the lowest quartile of work disability.

Overall, we find that workers with higher medical claims are no more sensitive to benefit levels than workers with lower medical claims, yet workers with a higher likelihood of qualifying for work disability *are* more sensitive to benefit levels than those with a lower likelihood of qualifying for work disability. Because work disability is likely a better proxy for disutility of work, these findings are consistent with those with higher disutility of work having a larger income effect.

6. Discussion and Comparison with Other Literature

These results show that the receipt of a sizeable, unconditional cash payment reduces labor supply, providing evidence of an income effect. While the absolute magnitude of these point estimates is small, these estimates reflect a persistent change in extensive margin labor supply in response to a relatively small change in income. As a result, these point estimates reflect a large response to a relatively small benefit. Indeed, assuming a discount factor of 2.4% and that the average PPD beneficiary will work for an additional 23 (=66-43) years, we calculate that a one-time $1,000 increase is equivalent to a $57 per year increase in a hypothetical annuitized benefit. Since a $1,000 increase in benefits decreases average annual earnings by $38.56, we estimate that PPD beneficiaries spend nearly two-thirds of the additional income arising from the 2005 reform on increased leisure time. Moreover, we find that the effect of PPD benefits on labor supply is concentrated among individuals with a greater likelihood that their injury specifically impaired their ability to work.

To put our results in context, consider the following simple static utility maximization problem:

$$\text{Max } u(c, l)$$

$$s.t. \;\; c = y + wl,$$

where *c* is consumption, *l* is labor supply, *y* is non-labor income, and *w* is the wage. Assume diminishing marginal utility of consumption and increasing marginal disutility of work ($u_c > 0$, $u_{cc} < 0$, $u_l < 0$, $u_{ll} > 0$), and for simplicity assume utility is separable in consumption and labor supply ($u_{cl} = 0$). Differentiating the first order condition with respect to *y*, substituting in $w = -u_l/u_c$, and rearranging terms, we obtain the following expression for the income effect:

$$\frac{\partial l}{\partial y} = \frac{-1}{w\left(1 - \frac{1}{w}\frac{u_{ll}/u_l}{u_{cc}/u_c}\right)}.$$

From the expression, we can see that the sensitivity of one's labor supply to non-labor income is a function of the wage and the relative *curvature* of the utility function with respect to labor supply vs. consumption. Note that if disutility of work is linear in hours worked (i.e., the marginal disutility of labor is constant), then $u_{ll} = 0$ and $\frac{\partial l}{\partial y} = -\frac{1}{w}$; in that case the income effect varies inversely only with the wage (the shadow price of leisure). However, if the marginal disutility of work is increasing in hours worked, then reducing one's labor supply in response to an exogenous change in income becomes even more attractive. Intuitively, the more steeply one's utility falls with labor supplied, the more an individual will find it attractive to use an increase in non-labor income to purchase additional leisure.[30]

The insight that income effects depend on the shape of the disutility-of-work function may at least partially explain why estimates of income effects in the literature vary by order of magnitude. Table 6 presents the settings and income effect estimates from seven prior studies, along with this paper, rescaled to represent the effect of a one-time $1,000 increase in non-labor income on the probability of employment, and ordered from smallest to largest in magnitude.[31] For studies estimating income effects in lottery settings, we simply rescale the published estimates of employment effects to represent a the effect of a $1,000 increase.[32] For studies estimating income effects in social insurance settings (e.g., cases where benefits are received as an annuity rather than one-time), we calculate the average duration of benefit receipt by subtracting the average age of initial benefit receipt from the full retirement age in that setting and, assuming a 2.4% discount factor, convert changes in annuitized benefits to their equivalent of a one-time $1,000 increase in non-labor income.[33] Finally, for the housing voucher study in

[30] If workers are credit constrained, receipt of non-labor income could also have significant impacts on labor supply by relaxing the constraint in addition to the impacts through the disutility channel. Unfortunately our data do not contain information on savings which would enable us to test this hypothesis, and we are underpowered to use the change in default from lump sum at $6,000 as an alternative test.

[31] We exclude studies that do not report employment effects (focusing instead on hour or earnings). We also exclude a recent paper by Artmann et al. (2023) examining the labor supply effects of a 2014 German reform that increased *future* pension wealth for mothers by on average €3830 (4.4%) per child born before 1992; they find a statistically significant and sizable effect of the reform on earnings but not employment, i.e., operating solely through the intensive margin channel.

[32] E.g., for Cesarini et al. (2017), we take -2.015 from Table 4, use the conversion 1 million SEK = $110,000 from the paper, and divide -2.015/110=-0.018.

[33] E.g., Gelber, Moore and Strand (2017) report that their preferred estimate of the effect of a $1,000 increase in DI benefits on employment is -1.3 percentage points (Table 3). Assuming this increase applies to benefits for 16 years (=66-50, the average age from Table 1), this translates to a one-time increase of $13,157. The scaled effect is therefore -1.3/13.157=-0.099. Giupponi (2019) reports that the effect of a €1,000 increase in annual survivors' benefits on labor force participation is -4.4 percentage points (Table 6, column 1, row 1), however she also reports that the change in benefits due to the reform is equivalent to approximately €100,000 in lifetime benefits. We use a Euro-to-Dollar conversion of 1.3 given time frame in the paper, and obtain a scaled effect of -4.4/13000 = -0.034.

Jacob and Ludwig (2012), we take the annual cash value of the voucher reported in the paper and assume that recipients consume the subsidy until age 66.[34]

Table 6 reveals two interesting patterns. First, the three studies with the smallest income effects are all studies from European settings. This may reflect both Europeans' better average health and working conditions than their American counterparts (Banks et al. 2006, Eurofound and ILO 2019). Second, income effects estimated in general populations tend to be smaller than those estimated in populations with disabilities, even though prior estimates from studies of disability beneficiaries are likely underestimates given the institutional work disincentives present in these settings. The estimate from our paper, from a setting without such disincentives, is the largest estimate in Table 6. Taken together, the estimates from the literature are generally consistent with a hypothesis that income effects reflect differences in underlying disutility of work across populations.

7. Conclusion

Despite their importance for understanding individual behavior and designing public policy, income effects have been difficult to identify empirically, particularly for populations with disabilities. This paper provides to our knowledge the first estimates of income effects for workers with permanent partial disabilities who do not face institutional work disincentives. We take advantage of a 2005 reform to the permanent partial disability (PPD) benefits formula for workers' compensation in Oregon to estimate the causal effect of non-labor income on labor supply outcomes. We identify the income effect based on the dose-response relationship between differences in benefits and labor supply between observationally identical people whose injuries

[34] Jacob and Ludwig (2012) report the annual cash value of the voucher as $8,160-$3,735=$4,425 (bottom of pg. 281), the average age of the head of household as 32 years (Table 1) and the IV estimate of the effect of the housing voucher on employment of household heads as -0.036 (Table 3).

occurred before and after the reform. Using comprehensive administrative data on workers' compensation claims, disability ratings, and employment records in Oregon, we implement a two-stage least squares approach where we instrument for PPD benefits with a rich set of formula inputs measured consistently before and after reform for all claims, interacted with indicators for policy regime.

This analysis yields large and persistent income effects for this population. We find that a \$1,000 increase in the PPD benefit amount leads to a 0.19 percentage point (0.26%) decrease in the probability of work, corresponding to a labor supply elasticity of -0.023. This effect is persistent through at least the first three years after the end of a workers' compensation claim (on average, four years after injury onset), suggesting a fairly permanent labor supply response. The same \$1,000 increase in PPD benefits leads to a reduction in annual hours of 2.36 (0.21%) and a reduction in annual earnings of \$38.56 (0.19%). Considering the fact that we identify a persistent labor supply response to a one-time change in income, these effects are large. We estimate that PPD beneficiaries spend two-thirds of the value of their additional PPD benefits on increased leisure time. Furthermore, we find evidence that heterogeneity in income effects is driven not by differences in impairment severity per se but largely by differences in how one's impairment specifically affects one's ability to work.

To put our results in context, we derive an expression for the income effect as a function of the wage and preferences for work and consumption using a simple static utility maximation framework. Using this framework, we compare our results with other estimates of income effects in the prior literature for a variety of populations in the U.S. and Europe, and for healthy and disabled populations. We find that our estimate is significantly larger than estimates for healthy populations, and of a similar magnitude to estimates of disabled populations in the U.S. Still, our estimate is slightly larger than other estimates of disabled populations, likely resulting from the absence of broader work disincentives inherent in other disability programs.

Put together, these findings are consistent with a rapidly increasing disutility of work in a population of American workers with permanent partial disabilities. Furthermore, these findings are consistent with a hypothesis that income effects reflect differences in underlying disutility of work across populations. The fact that income effects increase with the speed at which the disutility of work increases has important implications for policy and the interpretation of labor supply responses to disability programs estimated in prior literature. In particular, it implies that a significant portion of the labor supply reductions following receipt of disability benefits could in fact be due to workers making optimal choices that enhance their utility, rather than a distortionary disincentive response.

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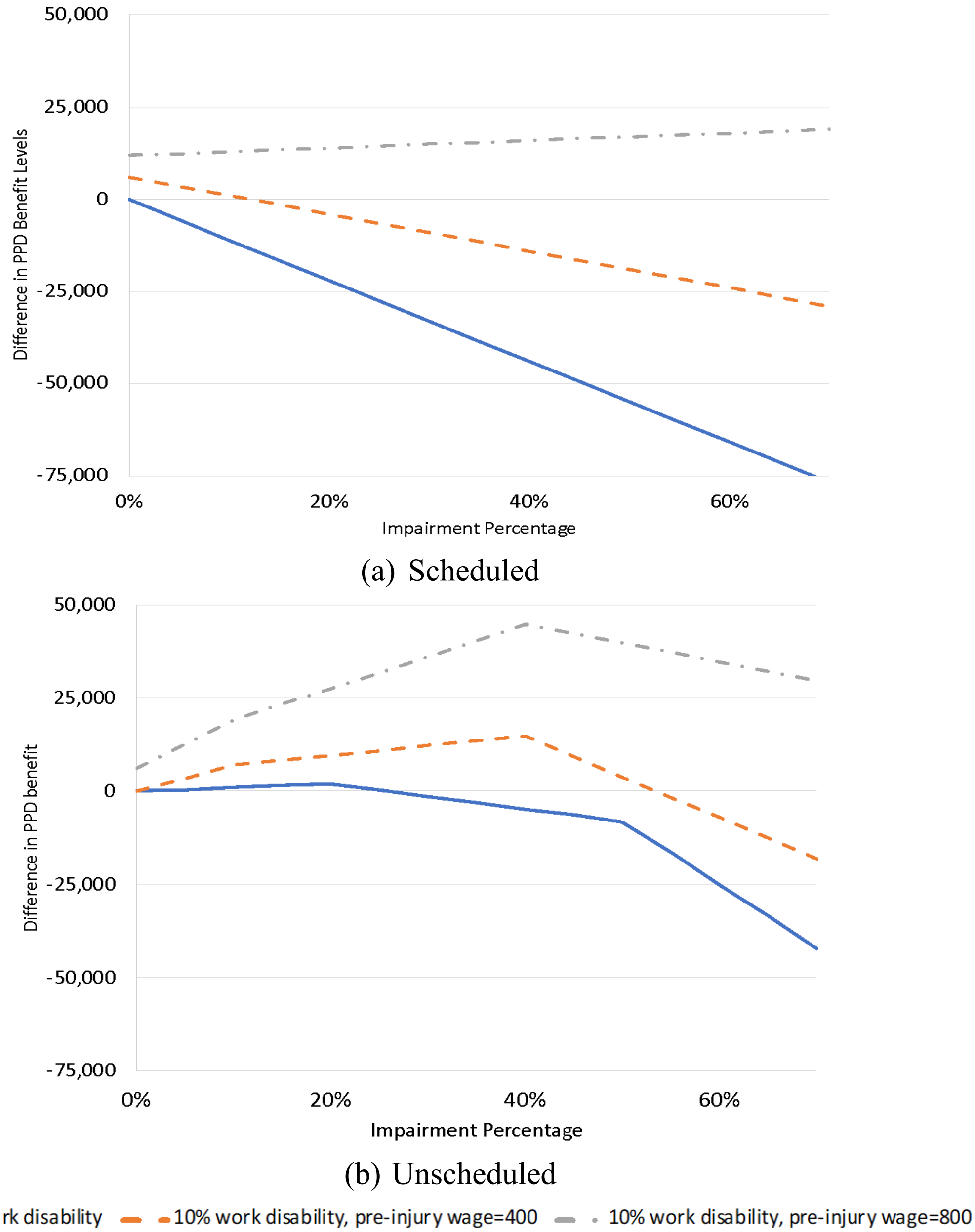
Figure 1: Difference in PPD Benefit Levels by Impairment Rating (Percent of Person), Before and After Reform

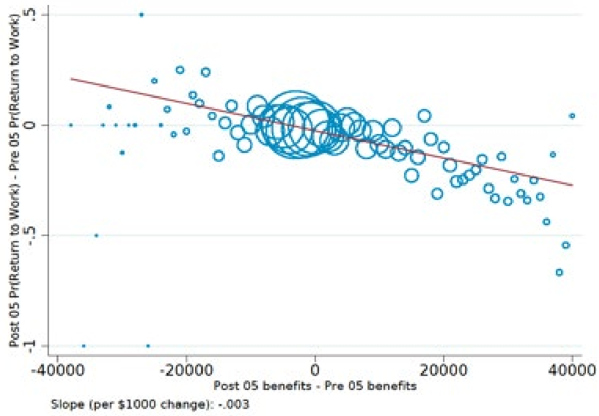


(a) Scheduled

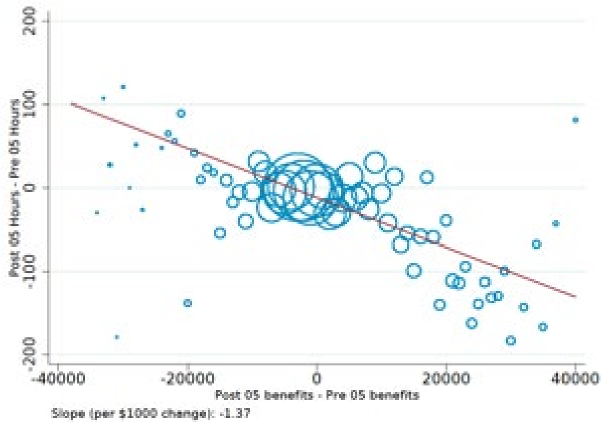
(b) Unscheduled

Notes: Author calculations based on Oregon PPD benefit formulas as described in the Oregon Disability Rating Standards, 2015. To obtain these estimates, we calculated the benefit under the old (pre-2005) and new (post-2005) benefit formulas at each possible impairment percentage. These benefit values are shown in Figure A1. We subtracted the new value from the old value to plot the difference shown above. (In practice we do not observe impairment percentages above 60% so have truncated the figure to a relevant range) In addition to variation in the impairment percentage (shown along the x-axis), benefits depend on the work disability percentage in both regimes and also depend the workers' pre-injury wage in the post-2005 regime. We therefore repeat the calculation for three hypothetical scenarios which vary these parameters: a worker with no work disability (solid blue line); a worker with 10% work disability and pre-injury weekly wage of $400 (dashed orange line); and a worker with 10% work disability and pre-injury weekly wage of $800 (dashed gray line). Because the formulas prior to 2005 varied depending on whether the injury was classified as scheduled or unscheduled, we plot the hypothetical difference separately using the pre-2005 scheduled formula in (a); and the pre-2005 unscheduled formula in (b).

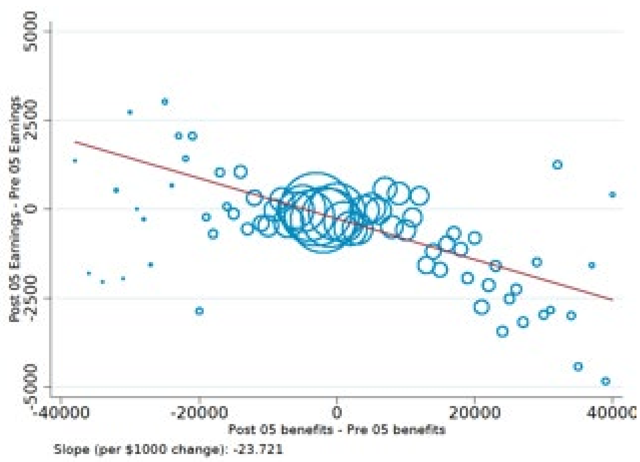
Figure 2: Reduced Form: Change in Labor Supply vs. Hypothetical Change in Benefits



Pr(Return to Work)



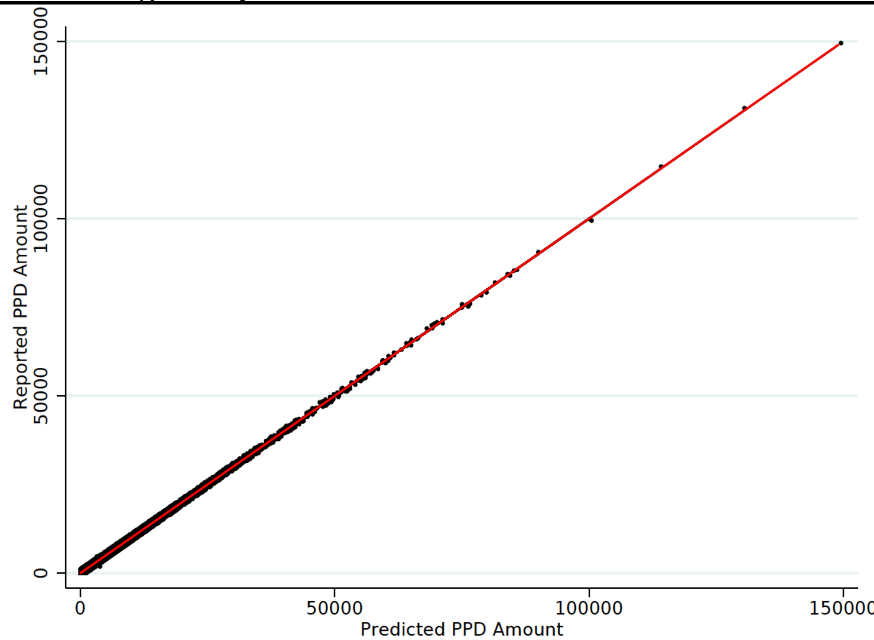
Hours



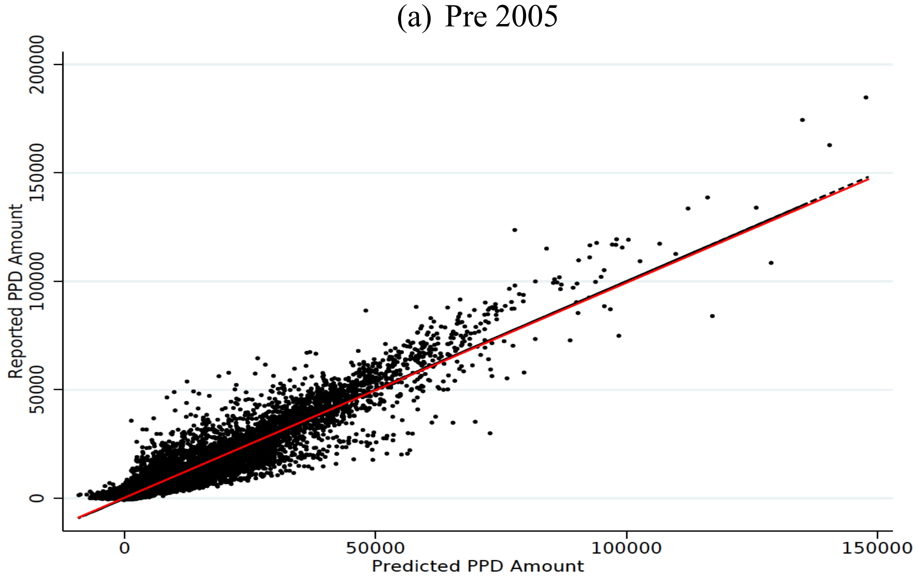
Earnings

Notes: Based on data from the Oregon Department of Business and Consumer Services and Oregon Employment Department, 2001-2010. The difference in benefits and outcomes is calculated based on comparing observed benefits between similar post-2005 and pre-2005 claims. We matched post-2005 claims to pre-2005 claims based on a nearest neighbor match based on a series of observable characteristics including age, pre-injury wage, medical expenditures, TTD duration, gender, body part of injury, return/release to work, and impairment ratings for scheduled injuries, and impairment + work disability ratings for unscheduled injuries. The difference in benefits is collapsed to $1,000 cells for the sake of presentation in the figure above.

Figure 3: First Stage – Reported vs. Predicted PPD Benefit from Formula Inputs



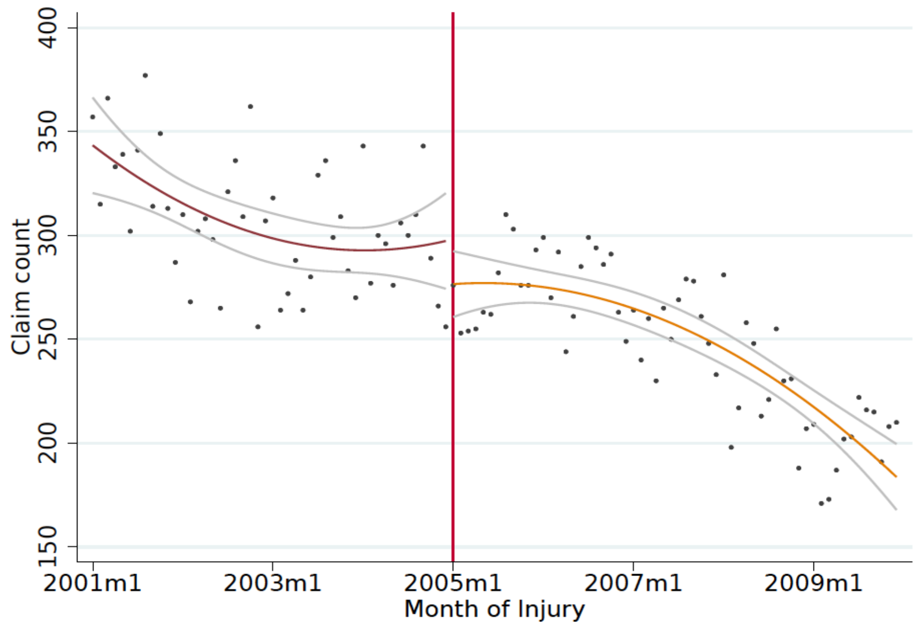
(a) Pre 2005



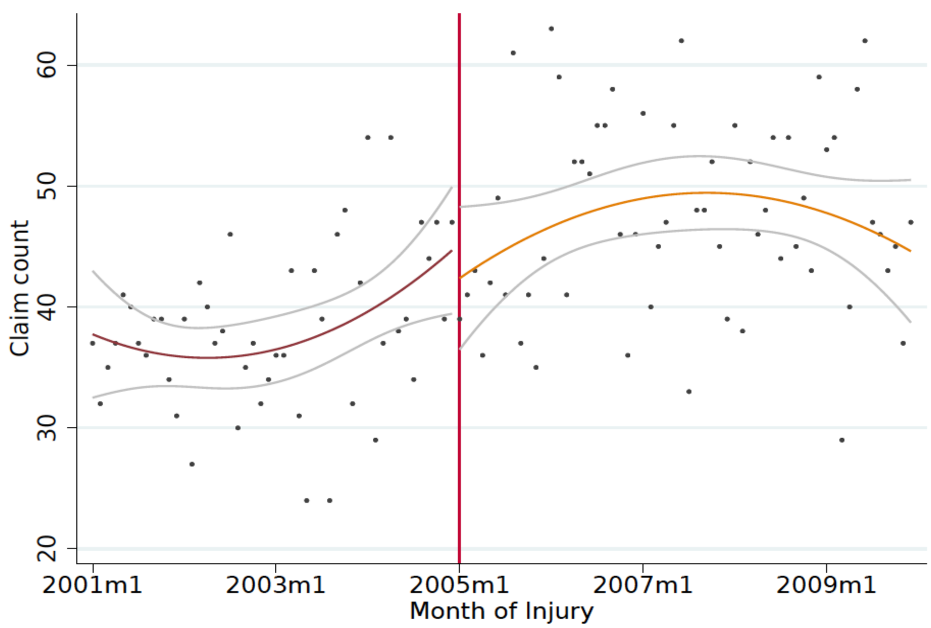
(b) Post 2005

Notes: Based on data from the Oregon Department of Business and Consumer Services, 2001-2009. Figures plot actual benefits as reported in the administrative data (y-axis) against our regression predicting benefits based only on formula inputs that are consistently observed in the data for claims occurring before and after the policy change. These inputs include impairment percentage (interacted with an indicator for scheduled injuries); the sum of impairment and work disability percentages (interacted with an indicator for unscheduled injuries); and a set of observable characteristics that are used to predict work disability including age, gender, medical expenditures, duration of temporary disability benefits, occupation and injury characteristics including nature and body part of injury. All of these inputs are subsequently interacted with year indicators due to the fact that the inputs are used differently in calculating benefits before and after 2005. Figure shows the regression perfectly predicts benefits prior to 2005 because we perfectly observe all formula inputs for the pre-2005 benefit formulas for all claims; there is some random noise in the post-2005 prediction because we do not observe work disability percentages consistently for all claims and therefore include the prediction, rather than actual percentage.

Figure 4: Claim Frequency by Expected Change in Benefit



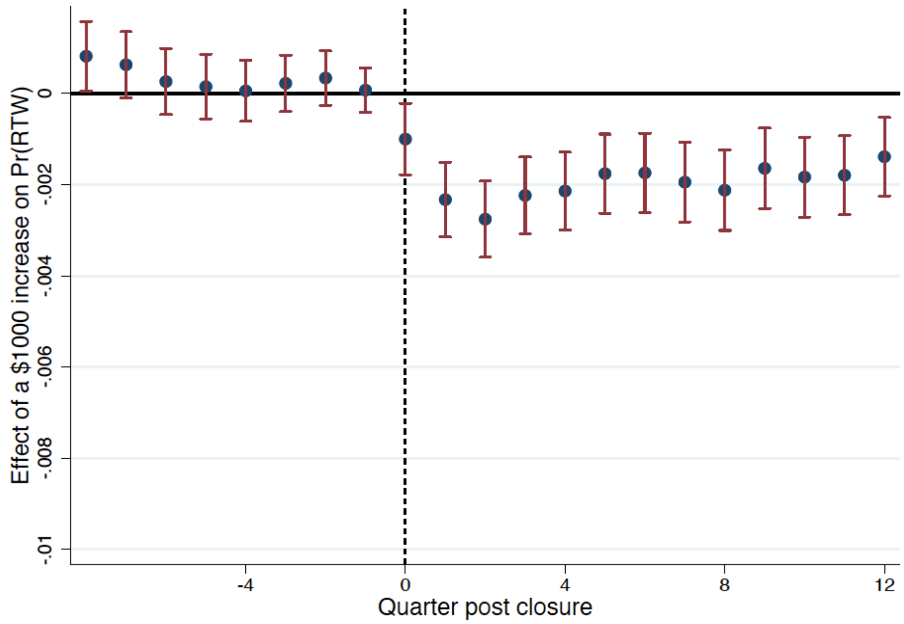
(a) Claims with a greater expected benefit in 2005



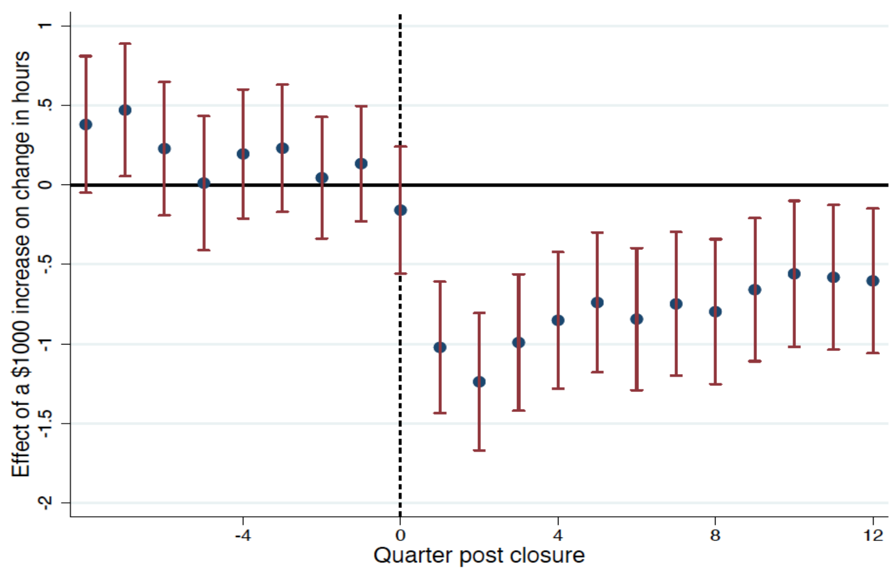
(b) Claims with a smaller expected benefit in 2005

Notes: Based on data from the Oregon Department of Business and Consumer Services, 2001-2009. Each plot shows the count of claims in each month that would be expected to win or lose after the reform based on an estimate of hypothetical benefits. We calculate hypothetical expected benefits for each claim using actual formula inputs in the administrative data, but assume all claims occurred in 2004 and 2005. We then calculate the difference between these two hypothetical benefits to determine which claims would receive larger benefits after the reform compared to their hypothetical benefit prior to the reform, and vice versa. Claims where the expected benefit is larger in 2005 are plotted in (a); claims where the 2005 expected benefit is smaller are plotted in (b). Colored lines plot the trend and gray lines show 95 percent confidence intervals.

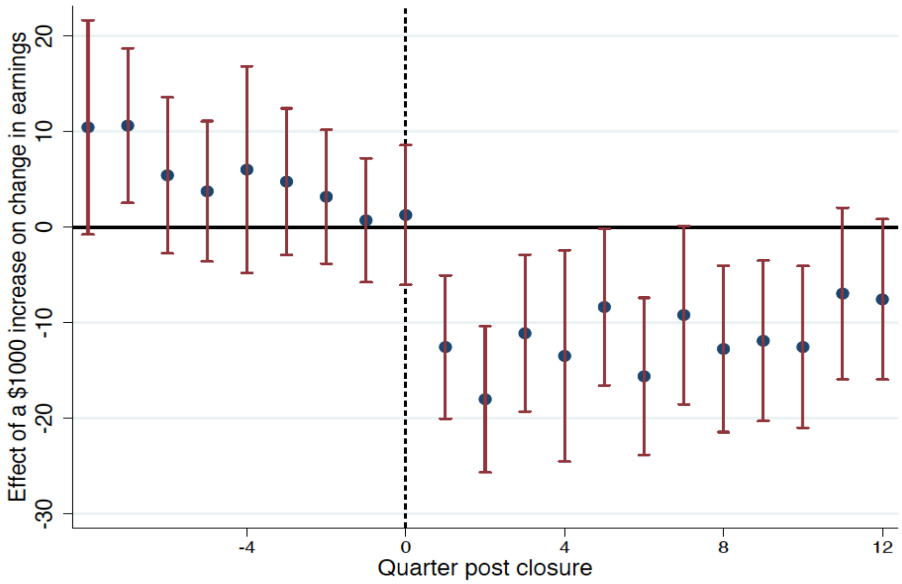
Figure 5: Trends in Estimated Effects over Time



(a) Pr(Return to Work)



(b) Hours



(c) Earnings

Notes: Based on data from the Oregon Department of Business and Consumer Services and Oregon Employment Department, 2001-2010. Each point on the graph is the coefficient from a separate regression from Equation 4 regressing the outcome of interest listed in the figure header for a different quarter before or after claim closure on predicted benefits and formula inputs including impairment percentage (interacted with an indicator for scheduled injuries); the sum of impairment and work disability percentages (interacted with an indicator for unscheduled injuries); and a set of observable characteristics that are used to predict work disability including age, gender, medical expenditures, duration of temporary disability benefits, occupation and injury characteristics including nature and body part of injury. 95 percent confidence intervals shown in the red bars.

Table 1: Effect of SB 757 on Benefits, Select Example Cases

Case	Injury	Impairment	Work Disability	2004 Benefit	2005 Benefit	Difference
1	Arm (S)	12%	0%	$21,466	$8,263	-$13,203
2	Back (U)	12%	0%	$7,066	$8,263	$1,197
3	Arm (S)	12%	6%	$21,466	$24,463	$2,997
4	Back (U)	12%	6%	$10,598	$24,463	$13,865
5	Back (U)	6%	12%	$10,598	$20,331	$9,733

Notes: Table shows what the PPD benefit would be before and after the reform for five example injuries. Benefits calculated by applying the formulas explained in the text from the Disability Rating Standards-Oregon Administrative Rules outlined in Bulletin 111 by the Oregon Department of Consumer Business Services Workers' Compensation Division. In this table, the impairment percentage reflects the percentage relative to the whole person which, for scheduled injuries, was derived by dividing the specified degrees for the body part(s) by 320 (the maximum number of degrees for the whole body). Weekly wage was assumed to be $600 when calculating the post-2005 benefit amounts.

Table 2: Claim Demographic Characteristics Pre- and Post- Reform

	All	Pre 2005	Post 2005	Difference	P-value
	Claimant Characteristics				
Age > 40	0.63	0.62	0.64	0.02	0.00
% male	0.72	0.73	0.71	-0.01	0.00
Pre-injury weekly wage ($2005)	625	627	622	-5	0.09
Medical expenditures ($2005)	12,677	12,573	12,777	204	0.19
TTD days	50	51	49	-2	0.02
% released to work at claim closure	0.81	0.79	0.83	0.04	0.00
% returned to work at claim closure	0.68	0.67	0.68	0.01	0.00
	Pre-injury occupation				
Transportation	0.18	0.18	0.18	-0.01	0.20
Production	0.17	0.20	0.15	-0.06	0.00
Construction	0.12	0.12	0.13	0.01	0.01
Maintenance	0.07	0.07	0.08	0.01	0.00
Other Occupation	0.45	0.43	0.47	0.04	0.00
	Injury Characteristics				
Fracture/break	0.34	0.34	0.34	0.00	0.49
Strain/sprain	0.33	0.39	0.28	-0.11	0.00
Trauma/unexpected	0.15	0.11	0.19	0.08	0.00
Wounds, cuts, burns	0.08	0.10	0.07	-0.02	0.00
Other	0.09	0.07	0.12	0.05	0.00
Observations	33,778	16,520	17,258		-

Notes: Data from Oregon Department of Consumer and Business Services, 2001-2009. P-values test the equivalence of the means among claims occurring before and after 2005.

Table 3: Formula Inputs and Body Codes, Pre and Post 2005 Reform

	Scheduled Injuries							
	Percentage of All Injuries				Impairment Percentage			
	Pre 2005	Post 2005	Difference	P-value	Pre 2005	Post 2005	Difference	P-value
Overall	62.0	63.6	1.7	0.00	4.6	4.7	0.1	0.38
Arm/Shoulder	30.2	31.1	0.9	0.07	9.2	9.2	-0.1	0.66
Leg/Hip	25.8	25.0	-0.8	0.10	4.1	4.4	0.2	0.01
Hand/Finger	19.2	19.2	0.1	0.88	4.1	4.2	0.1	0.25
Toes/Foot	7.7	6.8	-0.9	0.00	4.3	4.3	0.0	0.78
Ear	0.5	0.5	0.0	0.96	12.7	12.5	-0.2	0.92
Eye	0.1	0.1	0.0	0.73	7.3	8.1	0.8	0.80
	Unscheduled Injuries							
	Percentage of All Injuries				Impairment + Work Disability Percentage			
	Pre 2005	Post 2005	Difference	P-value	Pre 2005	Post 2005	Difference	P-value
Overall	38.0	36.4	-1.7	0.00	12.9	12.5	-0.3	0.09
Low back	13.9	11.1	-2.8	0.00	14.5	14.4	-0.1	0.85
Neck	4.3	3.4	-0.9	0.00	13.1	13.4	0.3	0.58
Back - multiple	2.1	2.0	-0.1	0.51	11.1	10.6	-0.6	0.47
Other body systems	0.8	0.6	-0.1	0.10	12.9	13.5	0.6	0.70
Brain	0.3	0.6	0.2	0.00	21.8	20.7	-1.1	0.72

Notes: Data from Oregon Department of Consumer and Business Services, 2001-2009. P-values test the equivalence of means among claims before and after 2005. Some arm, shoulder, leg and hip injuries are classified as scheduled and unscheduled prior to 2005, we combine them and list under the scheduled category for simplicity in this table. Total percent of all injuries summed across categories exceeds 100 percent prior to 2005 due to claims with multiple injuries.

Table 4: IV Estimates of the Effect of PPD Benefit on Labor Supply in Third Year Post-Closure: Main Results

	(1) Return to Work	(2) Hours	(3) Earnings
PPD Benefit/1000	-0.00188***	-2.36***	-38.56**
	(0.0004)	(0.86)	(16.20)
Predicted Y-mean at a	0.726	1127	20714
Pct change in Y-mean	-0.26%	-0.21%	-0.19%
$1000 change as pct c	11.2%	11.2%	11.2%
Elasticity	-0.023	-0.019	-0.017
Observations	33,778	33,778	33,778
R-squared	0.10	0.15	0.36
First stage F-statistic	233.5	233.5	233.5

Notes: Data from Oregon Department of Consumer and Business Services and Oregon Employment Department, 2001-2009. Table shows IV coefficients on PPD benefits from the second stage of the specification described in Equation 4. Additional controls in the second stage regression include the following: for scheduled injuries, impairment rating and case characteristics, respectively, interacted with pre-injury wage; and for unscheduled injuries, the sum of impairment and work disability rating interacted with pre-injury wage, and uninteracted case characteristics. For case characteristics, we include interactions between variables that are strong predictors of work disability eligibility and those that are strong predictors of work disability ratings. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: IV Estimates of the Effect of PPD Benefit on Labor Supply in Third Year Post-Closure: Heterogeneity

	Return to Work		Hours		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
	Lowest Quartile	Highest quartile	Lowest Quartile	Highest quartile	Lowest Quartile	Highest quartile
Panel A: Top and Bottom Quartiles by Claim Medical Expenditures						
PPD Benefit/1000	-0.0050***	-0.0016**	-8.63***	-1.16	-180.37***	-9.66
	(0.0012)	(0.0007)	(2.35)	(1.25)	(40.53)	(23.80)
Observations	8,534	8,312	8,534	8,312	8,534	8,312
R-squared	0.1180	0.1579	0.17	0.20	0.39	0.39
FS fstat	2470	185.1	2470	185.1	2470	185.1
Ymean Pre-05	0.743	0.678	1162	1019	20564	18608
Average benefit in	4641	16355	4641	16355	4641	16355
% change in Y-mea	-0.67%	-0.24%	-0.74%	-0.11%	-0.88%	-0.05%
1000 as % change i	22%	6%	22%	6%	22%	6%
Elasticity	-0.031	-0.039	-0.034	-0.019	-0.041	-0.008

Notes: Data from Oregon Department of Consumer and Business Services and Oregon Employment Department, 2001-2009. Table shows IV coefficients on PPD benefits from the second stage of the specification described in Equation 4 run on separate regressions for the top and bottom quartiles of the variables listed in the panel headers. Additional controls in the second stage regression include the following: for scheduled injuries, impairment rating and case characteristics, respectively, interacted with pre-injury wage; and for unscheduled injuries, the sum of impairment and work disability rating interacted with pre-injury wage, uninteracted case characteristics, and injury year FE. For case characteristics, we include interactions between variables that are strong predictors of work disability eligibility and those that are strong predictors of work disability ratings. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5, con't: IV Estimates of the Effect of PPD Benefit on Labor Supply in Third Year Post-Closure: Heterogeneity

	Return to Work		Hours		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
	Lowest Quartile	Highest quartile	Lowest Quartile	Highest quartile	Lowest Quartile	Highest quartile
Panel B: Top and Bottom Quartiles by Work Disability Propensity						
PPD Benefit/1000	-0.00292***	-0.00210***	-4.61*	-2.29**	-92.83*	-39.76**
	(0.00109)	(0.00022)	(2.47)	(1.14)	(48.24)	(19.59)
Observations	10,188	7,662	10,188	7,662	10,188	7,662
R-squared	0.06	0.14	0.12	0.16	0.37	0.31
FS fstat	25458	71.40	25458	71.40	25458	71.40
Ymean Pre-05	0.808	0.592	1331	822	25942	13150
Average benefit in	5149	19541	5149	19541	5149	19541
% change in Y-mea	-0.36%	-0.35%	-0.35%	-0.28%	-0.36%	-0.30%
1000 as % change i	19%	5%	19%	5%	19%	5%
Elasticity	-0.019	-0.069	-0.018	-0.054	-0.018	-0.059

Notes: Data from Oregon Department of Consumer and Business Services and Oregon Employment Department, 2001-2009. Table shows IV coefficients on PPD benefits from the second stage of the specification described in Equation 4 run on separate regressions for the top and bottom quartiles of the variables listed in the panel headers. Additional controls in the second stage regression include the following: for scheduled injuries, impairment rating and case characteristics, respectively, interacted with pre-injury wage; and for unscheduled injuries, the sum of impairment and work disability rating interacted with pre-injury wage, uninteracted case characteristics, and injury year FE. For case characteristics, we include interactions between variables that are strong predictors of work disability eligibility and those that are strong predictors of work disability ratings. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Summary of Prior Studies on Effect of a One-Time Increase in Non-Labor Income on Labor Supply

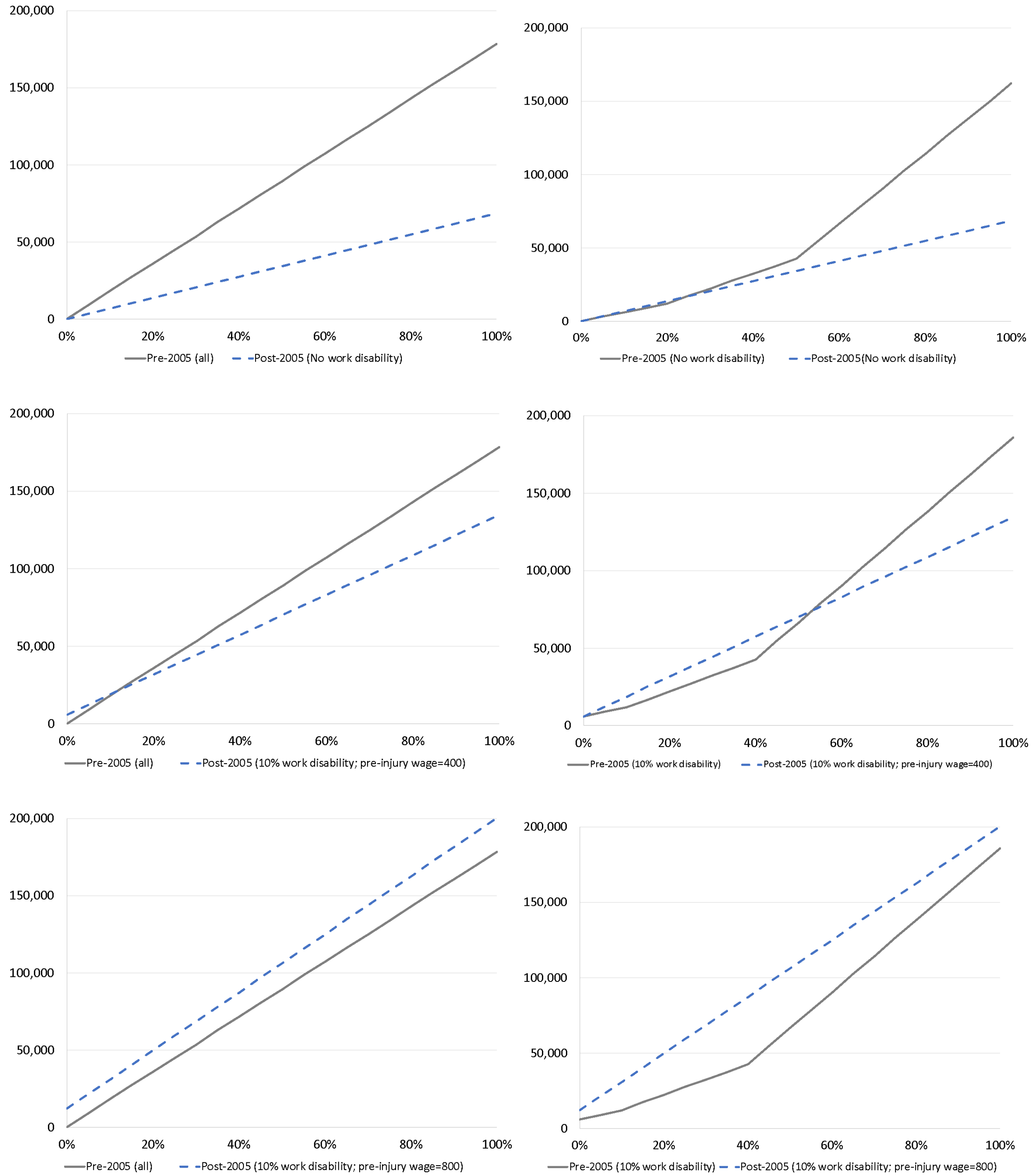
Study	Population	Effect of $1,000 on Employment
Cesarini et al. (2017)	Swedish lottery players	-0.018
Marie and Vall Castello (2012)	Spanish DI PPD beneficiaries at age 55	-0.023✢
Giupponi (2019)	Italian survivors' insurance beneficiaries	-0.034
Jacob and Ludwig (2012)	Chicago housing voucher recipients	-0.035✢
Golosov et al. (2021)	U.S. lottery players	-0.037
Gelber, Moore and Strand (2017)	U.S. SSDI beneficiaries	-0.099✢
Autor et al. (2016)	U.S. VA DC beneficiaries with type 2 diabetes	-0.159✢
Mullen and Rennane (2023) (this study)	Oregon WC PPD beneficiaries	-0.188

Notes: DC=disability compensation, DI=disability insurance, PPD=permanent partial disability, WC=workers' compensation. Income effects are scaled to represent the percentage point change in employment resulting from a one-time $1,000 change in non-labor income.

✢To convert increase in annual DI benefit/cash value of housing voucher to a one-time payment, we assume a discount rate of 2.4%.

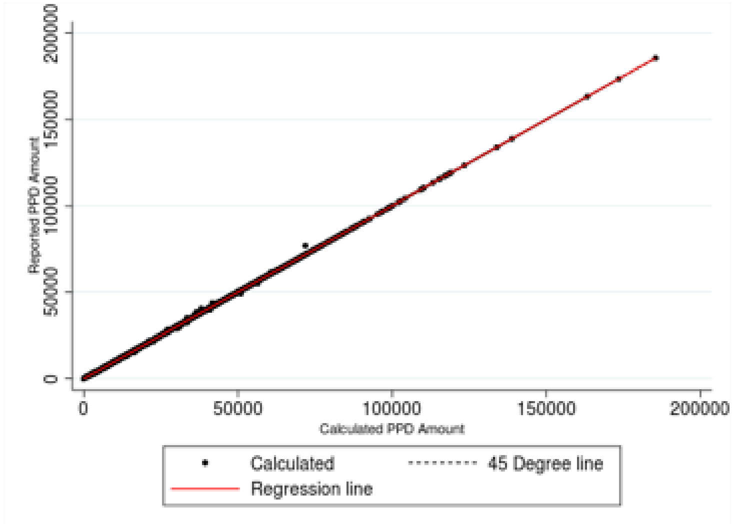
Figure A1: PPD Benefit Levels by Impairment Rating (Percent of Person), Before and After Reform

Scheduled | Unscheduled



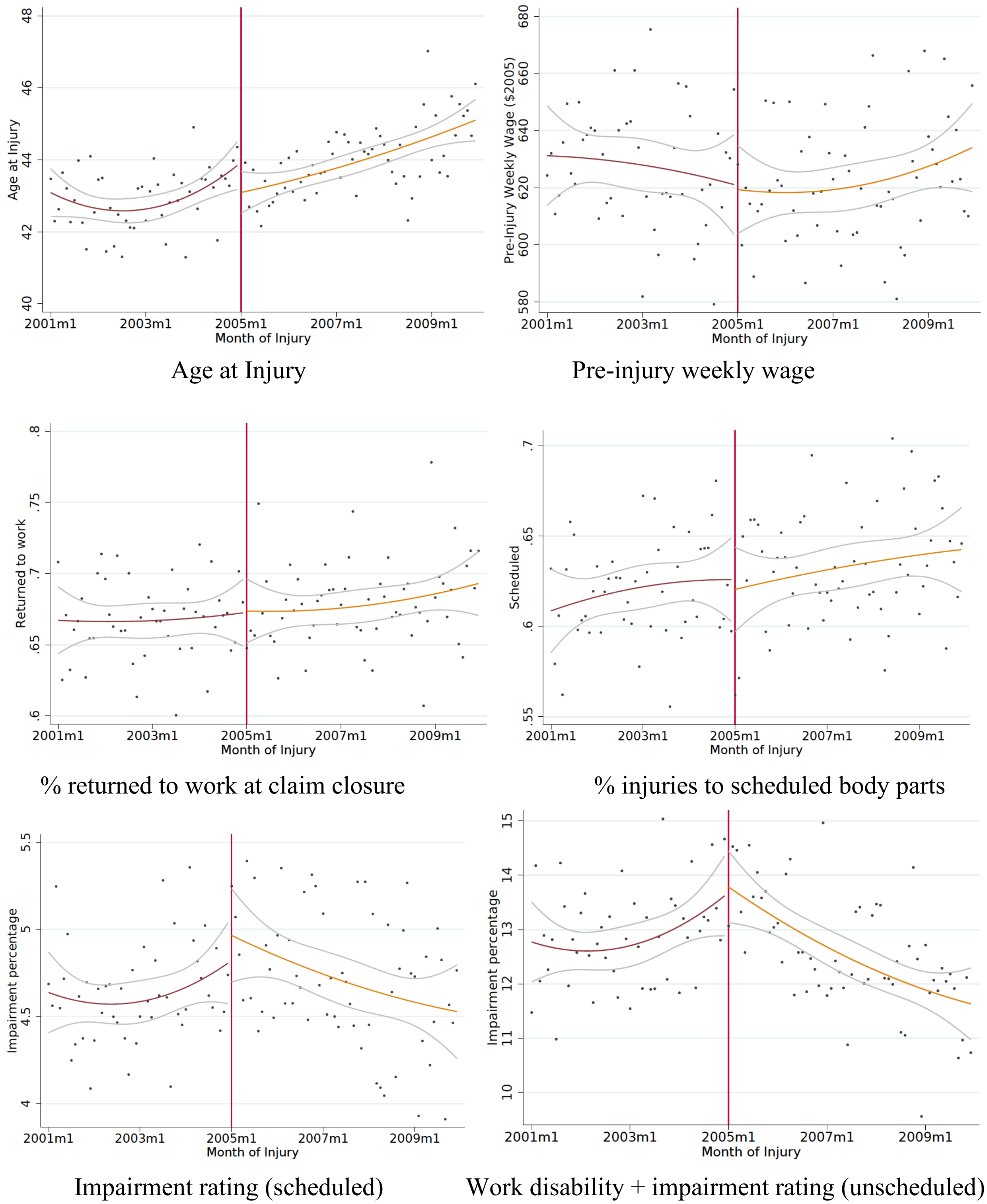
Notes: Author calculations based on Oregon PPD benefit formulas as described in the Oregon Disability Rating Standards, 2015. To obtain these estimates, we calculated the benefit under the old (pre-2005) and new (post-2005) benefit formulas at each possible impairment percentage. Because the formulas prior to 2005 varied depending on whether the injury was classified as scheduled or unscheduled, we calculate the benefits separately using the pre-2005 scheduled formula (left column) and pre-2005 unscheduled formula. (right column). Each subplot shows a benefit calculation using the pre-2005 formula (gray line) and a post-2005 formula (blue dashed line) assuming a specific set of inputs specified in the figure. Each row shows a separate set of inputs related to work disability: no work disability (top row); 10% work disability and pre-injury weekly wage of $400 (middle row); and 10% work disability and pre-injury weekly wage of $800 (bottom row). Prior to 2005, the formula for scheduled injuries only depended on the impairment percentage so the gray line remains the same in each plot in the left column.

Figure A2: Calculated vs. Reported Actual Benefit (post-2005 claims)



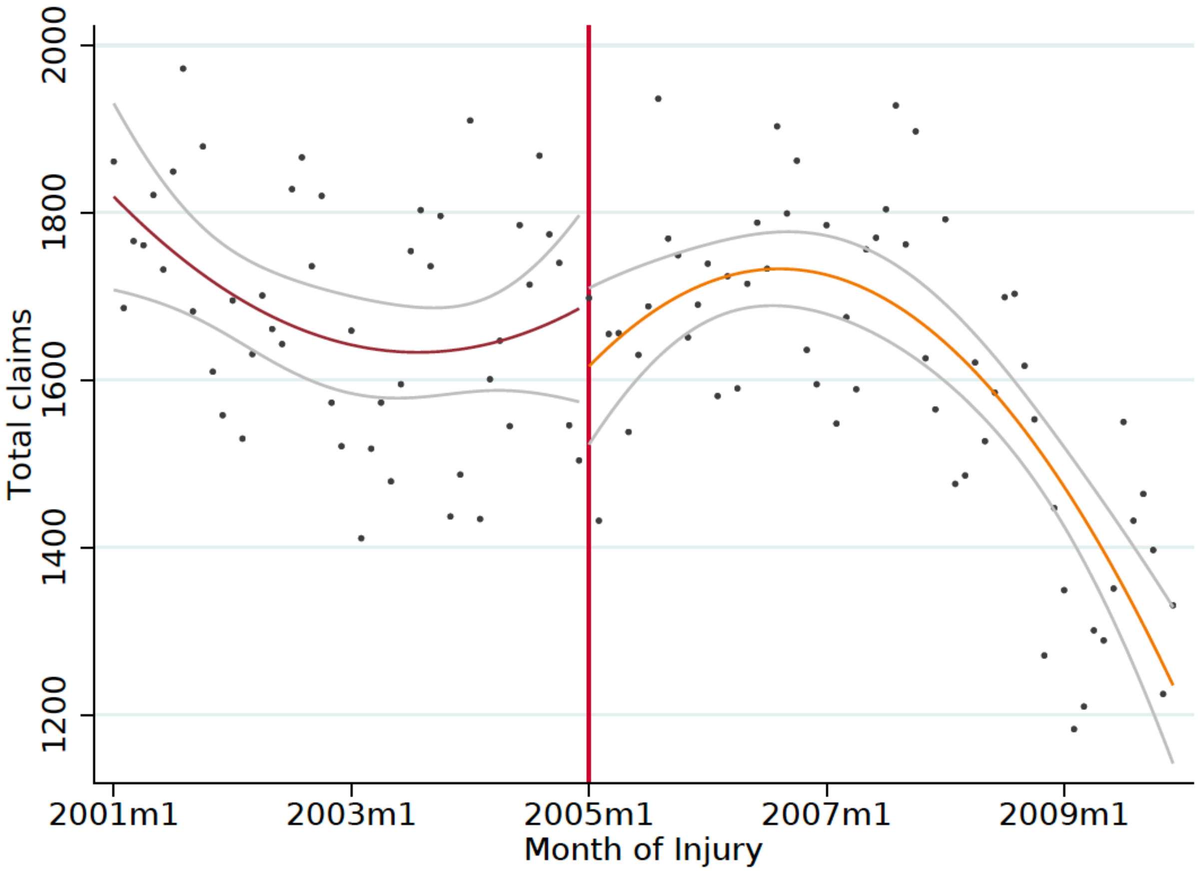
Notes: Based on data from the Oregon Department of Business and Consumer Services, 2001-2009. Figures plot actual benefits as reported in the administrative data (y-axis) against our regression predicting benefits only for the post-2005 claims. Because we are only using post-2005 claims in this plot, we include all actual formula inputs, including those not observed in claims prior to 2005 (and thus not included in our main specification). This figure demonstrates that our model perfectly predicts workers' benefits when using all actual inputs.

Figure A3: Trends in Formula Inputs by Month of Injury



Notes: Data from the Oregon Department of Business and Consumer Services, 2001-2009. Each figure shows a bin-scatter of the value of each demographic characteristic shown in the title, averaging within each month of injury. Colored lines plot the trend and gray lines show 95 percent confidence intervals.

Figure A4: Trends in Overall Claim Volume by Month of Injury



Notes: Data from the Oregon Department of Business and Consumer Services, 2001-2009. Figure shows a total count of each month of injury. Colored lines plot the trend and gray lines show 95 percent confidence intervals.

Appendix Table 1: Predictors of Work Disability Eligibility

	(1)	(2)
Return to work at closure	-0.05643***	-0.04669***
	(0.00343)	(0.00349)
Released to work at closure	-0.75006***	-0.73078***
	(0.00422)	(0.00432)
TTD Duration Deciles (omit lowest decile)		
2.decile		-0.00194
		(0.00558)
3.decile		-0.00194
		(0.00550)
4.decile		0.00040
		(0.00554)
5.decile		-0.00221
		(0.00564)
6.decile		0.00305
		(0.00547)
7.decile		-0.00113
		(0.00563)
8.decile		0.00051
		(0.00559)
9.decile		0.02463***
		(0.00567)
10.decile		0.08139***
		(0.00591)
Constant	0.79582***	0.76281***
	(0.00303)	(0.00524)
Observations	17,708	17,708
R-squared	0.76296	0.76724
Ymean	0.139	0.139

Notes: Data from Oregon Department of Consumer and Business Services, 2005-2009. Shows coefficients from a regression of an indicator for work disability on the coavariates shown in the table. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix Table 2: Predictors of Work Disability Percentage

	(1)	(2)
Age > 40	2.31712***	2.34591***
	(0.26867)	(0.26869)
Male	-0.62294*	-0.62695*
	(0.33857)	(0.33856)
Traumas	-0.09635	-0.09679
	(0.42596)	(0.42574)
Fractures/breaks	-0.40429	-0.40451
	(0.37957)	(0.37908)
Sprain/Strain	-0.40171	-0.38545
	(0.35074)	(0.35041)
Wound/cut/burn	0.32579	0.38880
	(0.86842)	(0.86865)
Medical Expenditures (1000s, $2012)	0.02282***	0.02426***
	(0.00565)	(0.00557)
Body System - First Impairment		
Upper extremities	4.75924**	4.69140**
	(2.23031)	(2.23182)
Shoulder	4.29921*	4.20087*
	(2.22617)	(2.22854)
Lower extremities	2.81814	2.71364
	(2.23219)	(2.23400)
Back	4.57500**	4.46761**
	(2.22912)	(2.23140)
Head	3.37395	3.27592
	(2.27080)	(2.27418)
Internal	1.81588	1.67214
	(2.56010)	(2.56202)
Body System - Second Impairment		
Upper extremities	0.49918	0.27294
	(1.68173)	(1.68183)
Shoulder	0.64547	0.13703
	(3.20004)	(3.19975)
Lower extremities	-2.78514	-2.88275
	(2.20419)	(2.20907)
Back	-1.46339	-1.72418
	(2.78300)	(2.78418)
Head	1.23533	1.08198
	(3.11802)	(3.11823)

Appendix Table 2, con't: Predictors of Work Disability Percentage

Occupation Categories		
o1	-2.27531**	-2.36884***
	(0.89679)	(0.89684)
o2	1.09509	1.44229
	(2.88275)	(2.89069)
o3	-5.72322	-4.56897
	(6.22401)	(6.25363)
o4	-5.23890***	-5.49161***
	(1.71708)	(1.71925)
o5	-2.67549	-2.46779
	(2.11828)	(2.11927)
o6	-3.19170	-3.37762
	(2.57447)	(2.58626)
o8	-3.36928**	-3.29179**
	(1.36404)	(1.36806)
o9	-3.73413*	-3.72192*
	(2.11376)	(2.11331)
o10	-4.62987***	-4.74264***
	(0.88103)	(0.88158)
o11	0.10659	0.00947
	(0.77925)	(0.77998)
o12	-1.97553	-2.06165
	(1.27528)	(1.27584)
o13	-0.43186	-0.35467
	(0.81530)	(0.81576)
o14	1.39181**	1.29943*
	(0.69905)	(0.69842)
o15	-3.27586**	-3.48725**
	(1.53709)	(1.53798)
o16	-1.03692	-0.92808
	(0.71916)	(0.72077)
o17	-2.23562***	-2.26443***
	(0.82379)	(0.82325)
o18	4.88242***	4.87630***
	(0.76656)	(0.76672)
o19	-2.10029***	-2.19312***
	(0.54591)	(0.54631)
o20	-2.90767***	-2.88286***
	(0.63635)	(0.63569)
o21	-0.25521	-0.36505
	(0.55766)	(0.55728)
o22	0.86509*	0.78985
	(0.52408)	(0.52448)

Appendix Table 2, con't: Predictors of Work Disability Percentage

	TTD Duration (omit lowest decile)	
TTD Duration/10	0.04946***	
	(0.01040)	
2.decile		-1.49857
		(1.13110)
3.decile		0.01252
		(1.13748)
4.decile		-1.22008
		(1.07133)
5.decile		-0.35588
		(1.05632)
6.decile		1.07060
		(1.01141)
7.decile		-0.85983
		(1.00227)
8.decile		0.54336
		(0.95193)
9.decile		0.49282
		(0.91380)
10.decile		1.24965
		(0.90217)
Constant	5.73968**	6.23821***
	(2.25540)	(2.41407)
Observations	2,445	2,445
R-squared	0.14627	0.15156
Ymean	11.44	11.44

Notes: Data from Oregon Department of Consumer and Business Services, 2005-2009. Shows coefficients from a regression of an indicator for work disability on the coavariates shown in the table. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix Table 3: IV Estimates of the Effect of PPD Benefit on Labor Supply in the Third Year Post-Closure: Log Specification

	(1) Log Hours	(2) Log Earnings
PPD Benefit/1000	-0.0001	-0.0010
	(0.0012)	(0.0013)
Predicted Y-mean at average benefit	7.1	9.9
Pct change in Y-mean	-0.01%	-0.10%
$1000 change as pct of average benefit	11.2%	11.2%
Elasticity	-0.001	-0.009
Observations	24,316	24,306
R-squared	0.1013	0.2506
First stage F-statistic	318.3	475.9

Notes: Data from Oregon Department of Consumer and Business Services and Oregon Employment Department, 2001-2009. Table shows IV coefficients on PPD benefits from the second stage of the specification described in Equation 4. Additional controls in the second stage regression include the following: for scheduled injuries, impairment rating and case characteristics, respectively, interacted with pre-injury wage; and for unscheduled injuries, the sum of impairment and work disability rating interacted with pre-injury wage, and uninteracted case characteristics. For case characteristics, we include interactions between variables that are strong predictors of work disability eligibility and those that are strong predictors of work disability ratings. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 4: IV Estimates of the Effect of PPD Benefit on Labor Supply in Third Year Post-Closure: Heterogeneity, Log Specification

	Log Hours		Log Earnings	
	(1)	(2)	(3)	(4)
	Lowest Quartile	Highest quartile	Lowest Quartile	Highest quartile
Panel A: Top and Bottom Quartiles by Claim Medical Expenditures				
PPD Benefit/1000	-0.00195	0.00086	-0.00177	-0.00053
	(0.00320)	(0.00176)	(0.00333)	(0.00206)
Observations	6,295	5,585	6,328	5,537
R-squared	0.15	0.17	0.29	0.30
FS fstat	175186	183.6	79734	182
Ymean Pre-05	7.1	7.1	9.8	9.8
Average benefit in quartile	4641	16355	4641	16355
% change in Y-mean	0.19%	0.09%	0.18%	0.05%
1000 as % change in benefit	22%	6%	22%	6%
Elasticity	0.009	0.014	0.008	0.009

Notes: Data from Oregon Department of Consumer and Business Services and Oregon Employment Department, 2001-2009. Table shows IV coefficients on PPD benefits from the second stage of the specification described in Equation 4 run on separate regressions for the top and bottom quartiles of the variables listed in the panel headers. Additional controls in the second stage regression include the following: for scheduled injuries, impairment rating and case characteristics, respectively, interacted with pre-injury wage; and for unscheduled injuries, the sum of impairment and work disability rating interacted with pre-injury wage, uninteracted case characteristics, and injury year FE. For case characteristics, we include interactions between variables that are strong predictors of work disability eligibility and those that are strong predictors of work disability ratings. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 4, con't: IV Estimates of the Effect of PPD Benefit on Labor Supply in Third Year Post-Closure: Heterogeneity, Log Specification

	Log Hours		Log Earnings	
	(1)	(2)	(3)	(4)
	Lowest Quartile	Highest quartile	Lowest Quartile	Highest quartile
Panel B: Top and Bottom Quartiles by Work Disability Propensity				
PPD Benefit/1000	0.00126	-0.00149	0.00212	-0.00339
	(0.00233)	(0.00201)	(0.00246)	(0.00231)
Observations	8,166	4,489	8,159	4,479
R-squared	0.12	0.15	0.27	0.24
FS fstat	24888	88.88	25956	118.1
Ymean Pre-05	7.2	6.9	10.1	9.5
Average benefit in quartile	5149	19541	5149	19541
% change in Y-mean	-0.13%	0.15%	-0.21%	0.34%
1000 as % change in benefit	19%	5%	19%	5%
Elasticity	-0.006	0.029	-0.011	0.066

Notes: Data from Oregon Department of Consumer and Business Services and Oregon Employment Department, 2001-2009. Table shows IV coefficients on PPD benefits from the second stage of the specification described in Equation 4 run on separate regressions for the top and bottom quartiles of the variables listed in the panel headers. Additional controls in the second stage regression include the following: for scheduled injuries, impairment rating and case characteristics, respectively, interacted with pre-injury wage; and for unscheduled injuries, the sum of impairment and work disability rating interacted with pre-injury wage, uninteracted case characteristics, and injury year FE. For case characteristics, we include interactions between variables that are strong predictors of work disability eligibility and those that are strong predictors of work disability ratings. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1