The Effect of Unconditional Cash Transfers on the Return to Work of Permanently Disabled Workers*

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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 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.

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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.

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. After 2005, the reform increased benefits for 24 percent of beneficiaries and decreased benefits for 47 percent of beneficiaries, relative to what an observationally identical claimant would have gotten before 2005. We identify the income effect based on the dose-response relationship between changes in these benefits and workers' subsequent labor supply. 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.[1] Thus, we estimate that PPD beneficiaries spend nearly 67% of their additional income on increased leisure time.

The estimated income effects vary substantially 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

[1] 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.

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).[2] 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-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.

[2] 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.

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.[3]

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

[3] 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.

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 2021). Third, despite their growing importance, individuals with partial disabilities are generally excluded from the federal disability insurance in the U.S.[4] 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, 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. Section 3 describes are the data. Section 4 explains our empirical

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

strategy. 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.[5] 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.

The most common type of permanent disability benefit, and the focus in this paper, is the permanent partial disability (PPD) benefit.[6] If awarded, PPD benefits are provided to the worker at the time of claim closure, regardless of the workers' subsequent work activity. 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.[7] Conditional on

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[5] 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.

[6] 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.

[7] Over the analysis period for our paper, between 50 and 60 percent of PPD awards were less than $6,000. Based on estimates from a large state insurer, between 15 and 20 percent of awards above $6,000 request to

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 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. Prior to 2005, the PPD award to depended on two main factors: 1) whether the injury involved particular body parts that were listed on a pre-existing schedule ("scheduled injuries") or not ("unscheduled injuries");[8] and 2) the severity of the resulting impairment in functioning, rated as a percent of the person or whole body.[9] For unscheduled injuries, the benefit amount depended on two additional factors: 1) whether the worker was deemed unable to return to the job held at the time of injury; and 2) conditional on being deemed unable to return to one's pre-injury job, one's rating of *work disability*, defined as the extent to which the injury might prevent *future* work, taking 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). Workers with scheduled injuries were ineligible for work disability awards prior to 2005.

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

$$PPD_{iT}^{Pre} = \begin{cases} p_i^p * BEN_T^S, & S_i = 1, \\ f_U(p_i^P + W_i * p_i^W), & S_i = 0, \end{cases} \tag{1}$$

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receive a lump sum. Mullen and Rennane (2020) analyze the impact of receiving lump sum vs. monthly payments using the $6,000 threshold and find no evidence that default payment frequency affects labor supply.

[8] 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.

[9] Scheduled and unscheduled injuries had different rating procedures. 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. We convert impairment ratings for scheduled injuries to the percent of the person by dividing the specified degrees for the body part(s) by 320 (the maximum number of degrees for the whole body).

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 ($S_i = 1$). 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 f_U increasing in 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.

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 claimants, regardless of injury type, are eligible for work disability if deemed unable to return to the pre-injury job. Additionally, if deemed unable to return to one's pre-injury job, 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:

$$PPD_{iT}^{Post} = p_i^P * 100 * SAWW_T + W_i * (p_i^P + W_i * p_i^W) * 150 * w_{iT}. \qquad (2)$$

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.

Figure 1 illustrates the effect of the 2005 reform on PPD benefits as a function of whole-body impairment rating, separately for scheduled (Panel A) and unscheduled (Panel B) injuries, for each of 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).[10] Both the sign and magnitude of the hypothetical change in benefit due to the

[10] Figure A1 shows the corresponding PPD benefit *levels* before and after the reform for each of the six cases shown in Figure 1.

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 and Descriptive Statistics

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 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.[11]

[11] 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).

3.1 Observed Formula Inputs Before and After the Reform

A limitation of the administrative claims data set is that, while it includes rich information on formula inputs, it only captures the specific inputs—or combination of inputs—required to calculate PPD benefits in the contemporaneous policy regime and not in the alternative regime. 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. Since we do not separately observe the work disability rating for pre-2005 claims, we are unable to calculate hypothetical post-2005 benefits to assess the exact difference in their benefit levels under the two policy regimes for pre-2005 claims as we would be able to for post-2005 claims.[12] We account for this asymmetry by using only observable characteristics that are available for *all* claims in the data set—both pre- and post-2005—in our analysis.

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,

[12] The existence of separate variables for impairment rating and work disability rating recorded after 2005 does enable us to calculate hypothetical pre-2005 benefits for all post-2005 claims.

injury and occupation characteristics that are correlated with work disability eligibility and rating observed in post-2005 claims.[13]

Finally, due to the change in focus from ratings based on individual body parts to those based on the whole person, there are some differences in the way specific body parts are recorded in the database before and after the reform. 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. In other words, identical injuries with the same level of severity may be attributed to different body parts before and after 2005. To account for this difference, we aggregate injuries into broader body systems (e.g., combining injuries to the hand and fingers).

3.2 Descriptive Statistics and Trends in Claim Characteristics Before and After the Reform

Table 1 compares observable characteristics of claims in our sample with injury dates before and after 2005. To examine whether there was a discontinuous change in claim characteristics coinciding with the reform, we regressed claimant characteristics (separately) on a post-2005 indicator variable, a flexible polynomial in month of injury and an interaction between the two for claims in 2004 and 2005. We present p-values from tests of statistical significance of the post-2005 indicators in Table 1.

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[13] Table A1 shows that indicators for whether the claimant was "released" to work by a physician and whether the claimant returned to work *prior* to claim closure are strong predictors of whether or not someone is eligible for work disability at claim closure; the applicant's age, gender, occupation, injury type, medical expenditures, and TTD duration are strong predictors of the work disability rating.

The average claimant is in their early 40s, with slightly more than 60 percent of claimants older than age 40. Approximately 70 percent of claimants are men. The average pre-injury weekly wage is approximately \$630 in \$2005 (\$955 in \$2022). Approximately 80 percent of claimants in the database were released to work by a doctor at the time of claim closure, and two-thirds percent of claimants had returned to work prior to claim closure. Total medical expenditures range between \$12,000 and \$13,000 in \$2005 (approximately \$18,000-\$19,000 in \$2022). Temporary total disability benefits are paid for approximately 50 days on average (median 28 days) before claim closure, and average claim duration is just under one year.

Over half of claims occur in one of four main occupation categories: production, transportation, construction, and maintenance. The share of claims from production occupations declined from 20 percent before 2005 to 14 percent afterwards, the result of a secular downward trend in claims from this occupation (Figure 2). At the same time, the share of claims from occupations other than the main four occupations gradually increased between 2001 and 2009. However, there are no discrete breaks in the trend in occupations before and after the policy change in 2005.

Approximately 30-40 percent of all claims result from muscle strains or sprains, although the share of claims resulting from sprains declines over time. Fractures and breaks account for another third of cases, followed by trauma and unexpected injuries (11-19 percent), wounds, cuts and burns (7-10 percent) and other injuries (7-12 percent). The share of claims in injuries that would have been scheduled based on body part(s) injured is 62-63 percent. Finally, there is a statistically significant decline in the share of claims with multiple injuries, falling from nine percent of claims before 2005 to just one percent of claims after 2005. As discussed in Section 3.1, this decline does not reflect a significant change in the composition of injuries, but only a shift in the way similar injuries are recorded before and after the policy change. As a result, we aggregate injuries in the same broad body system in our analysis.

Table 2 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 3.1. 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.[14] As shown in Panel A, the average overall impairment percentage remains steady over the entire analysis period at approximately 5 percent of the whole person for scheduled injuries. The share of claims occurring due to hand/finger injuries and difference in the impairment rating for hand/finger injuries is statistically significant before and after 2005, although the difference in rating in practice is small (0.1 percentage point). Panel B shows similar trends for unscheduled injuries. Overall, the average impairment plus work disability percentage is constant around 12-13 percent. The only statistically significant differences in the composition of unscheduled injuries occurs in neck injuries, which represent approximately 4 percent of the sample.[15] There are no statistically significant differences in the average combined impairment and work disability rating by body system.

Figure 2 presents bin-scatter plots by injury month for key benefit formula inputs which we are able to observe consistently over the analysis period. The trends in the worker age at injury, share of claims with injuries that would be classified as scheduled under the old benefit formula

[14] Some body system groups (specifically, arm/shoulder and leg/hip) combine body parts that were both scheduled and unscheduled before the reform. For these groups, we present the frequency and average impairment or combined rating for the subset of scheduled and unscheduled injuries within these groups. injuries.

[15] As discussed below, Figure 2 shows that the trends injuries to these two body systems are smooth over time.

and share of claims that had returned to work at the time of claim closure are relatively flat and smooth, slowly increasing over time. Although the p-value for the trend in weekly wage is marginally statistically significant in 2005, the trend is flat. The trend in the impairment and work disability ratings increases and falls over the sample period, but there are no significant level shifts around 2005.

4. Empirical Strategy

Our empirical approach takes advantage of the significant and varied changes in benefits under SB 757 to estimate the income effect associated with disability benefits. 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.

In both policy regimes, PPD benefits are functions of observable factors—specifically, impairment type (scheduled or unscheduled), impairment severity, work disability eligibility, work disability rating (if eligible) and pre-injury wage. However, these factors are likely independently related to labor supply and therefore an Ordinary Least Squares (OLS) regression of labor supply on PPD benefits controlling for formula inputs will identify the causal effect of the benefit only if strict functional form assumptions are met (for example, in the case where the benefit value is a non-linear function of formula inputs). Instead, we identify the causal effect of PPD benefits on labor supply using a dose-response relationship relating the difference in benefit level to the difference in labor supply for observationally identical individuals whose injuries occurred across policy regimes. 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).

As demonstrated in Figures 3 and 4, SB 757 changed the value of benefits for all workers with permanent impairments. Figure 3 presents the difference between the actual PPD benefit for post-2005 claims and the hypothetical pre-2005 PPD benefit for scheduled (Panel a) and unscheduled (Panel b) injuries as a function of impairment rating.[16] (Figure 3 is analogous to the empirical analogue of the illustrative scenarios shown in Figure 1). Figure 4 shows the combined cumulative distribution of this difference for all post-2005 claims. As can be seen from the figures, 24 percent of post-2005 claimants received larger benefits than they would have prior to the reform and 47 percent—mostly those whose injuries would have been considered scheduled before 2005—received smaller benefits. The mean decrease was $2,918 (median $1,976) and the average increase was $4,085 (median $1,344).

To exploit the wide variation in the effect of the 2005 reform on PPD benefit levels, we implement an instrumental variables approach where we first predict each worker's benefit based on a comprehensive set of observable formula inputs and case characteristics that are comparable across policy regimes interacted with the policy regime, and then regress return to work outcomes on the predicted benefit. We estimate a two-stage model of the following form:

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

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

In the first stage, we predict the benefit PPD_{iT} for worker i who was injured in year T using information about observable characteristics Z_i (described in detail below), interactions between

[16] Recall we can calculate exact hypothetical benefits under the alternative regime for post-2005 claims but not for pre-2005 claims.

Z_i and an indicator for claims that occurred after 2005 ($POST_{iT}* Z_i$), and injury year fixed effects λ_T.[17] In the second stage, we regress return to work 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 return to work.

To illustrate how our approach identifies the causal effect of benefits on labor supply, consider a case with only two types of people, classified by a single binary indicator variable Z_i, and two periods, where $T=0$ denotes the pre-reform period and $T=1$ the post-reform period. Normalize $\lambda_0=0$. From equation (3), we see the expected "dose," or *difference* in benefits for two *individuals who only differ in their injury date and are otherwise identical*, is λ_1 if $Z_i =0$, and $\psi+\lambda_1$ if $Z_i=1$.[18] From equation (4), we see the expected "response," or difference in labor supply, is $\phi\, \lambda_1+ \delta_1$ if $Z_i =0$, and $\phi(\psi+\lambda_1)+ \delta_1$ if $Z_i =1$. Taking differences across the two types of individuals, the difference in dose is ψ and the difference in response is $\phi\psi$. The ratio of the differences is ϕ.[19]

The key identifying assumption is that, conditional on Z_i, variation in observed benefits is driven by the policy change ($POST_{iT}* Z_i$) and is uncorrelated with unobserved determinants of labor supply. Practically, this translates into two assumptions that must be met. First, there are no

[17] 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).

[18] If $Z_i =0$, expected pre-reform benefits are λ_1 and post-reform benefits are 0 (since we normalize $\lambda_0 = 0$); the difference is therefore λ_1. Similarly, if $Z_i =1$, expected pre-reform benefits are $\gamma + \lambda_1$ and post-reform benefits are $\psi + \gamma$; the difference is $\psi+\lambda_1$.

[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.

shifts in unobserved claim characteristics before and after the reform. Second, formula inputs and observable characteristics Z_i are measured the same in both policy regimes. We address each of these two assumptions in turn.

To address the first assumption, as described in Section 2, 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. As shown in Figure A2, there are no discrete changes around 2005 in the frequency of claims overall or the share of claims that settle. Moreover, as discussed in Section 3.2, there are no discrete changes in observable claim characteristics around 2005.

Second, as discussed in Section 3.1, there are differences in how work disability eligibility, impairment and work disability ratings, and injury types are recorded before and after the reform. We address this issue by restricting Z_i to include only those formula inputs and case characteristics that are comparable across policy regimes (specifically, impairment rating interacted with an indicator for scheduled injuries, the *sum* of the impairment and work disability ratings interacted with an indicator for unscheduled injuries, and controls for broad body system groups rather than individual body parts). To address the issue of missing work disability eligibility and rating in the pre-reform period, we include in Z_i a comprehensive set of case characteristics that predict work disability eligibility and the work disability rating, including as whether the claimant returned or was released to work at claim closure, age, gender, injury type, medical expenditures, TTD duration and occupation categories.[20] 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. We derive the specification for the first stage equation (3) from the known PPD benefit schedules in equations (1) and (2), replacing inconsistently observed variables with their consistently observed predictors.

Figure 5 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.[21] 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.

5. Results

5.1 Graphical Evidence

Before estimating our two-stage model, we examine graphically the reduced form relationship between the difference in benefits and difference in outcomes between the pre- and

[20] Recall that the work disability rating is determined conditional on eligibility (e.g., whether or not a worker is released and returned to work. See Table A2 for the full set of predictors of the work disability percentage.
[21] Figure A3 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.

post-reform periods in Figure 6. To construct estimates of the differences, we compare matched dyads of pre- and post-2005 claims using a nearest neighbor matching algorithm based the following observable characteristics: 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. 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 6 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, suggestive of an income effect.

5.2 Main Results

To formally examine the magnitude and statistical significance of this relationship, we estimate the two-stage IV model in equations (3) and (4). Table 3 shows the IV coefficients on PPD benefits, scaled by $1,000, for each of our main outcomes from equation (4). Column 1 shows the effect of PPD benefits on return to work, or the probability of employment in the second year after claim closure, and columns 2 and 3 show the effect of PPD benefits on (unconditional) hours worked and earnings, respectively, during the second 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 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 return to work (between -0.019 and -0.017, respectively).[22]

Figure 7 presents an event study of the income effect over time, examining the estimated effect during the four quarters before injury, the quarter of claim closure, and the twelve quarters after closure.[23] Each point estimate on the graphs represents the coefficient on the scaled PPD benefit from a separate regression where the dependent variable is the labor supply response in the quarter of interest. The four quarters prior to injury serve as a placebo test since the benefit should not affect labor supply before the worker is injured. The effect is very close to zero and not statistically significant in any quarter prior to injury. Next, the quarter of claim closure can be viewed as a "partially treated" quarter, since claims are closed at varying points during the quarter. The effect in the quarter of closure is statistically significant for return to work, but smaller in magnitude than in the subsequent quarters. For all three outcomes, the point estimate is largest in the second quarter after claim closure, after which point 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.

[22] 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.
[23] We omit the quarters occurring between the injury and claim closure when beneficiaries are not working.

5.3 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 Table A1), 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 4 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.

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 return to work 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.[24]

Panel B of Table 4 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 return to work, 0.054 for hours, and 0.059 for earnings, respectively. The effect sizes are slightly larger in magnitude for claims in the lowest 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

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[24] 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 A5).

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 does indeed reduce 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.

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 5 presents the settings and income effect estimates from six 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.[25] 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.[26] For studies estimating income effects in disability insurance settings (e.g., cases where benefits are received

[25] We exclude studies that do not report employment effects (focusing instead on hour or earnings).
[26] 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.

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.[27] Finally, for the housing voucher study in 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.[28]

Table 5 reveals two interesting patterns. First, the two studies with the smallest income effects are both 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 5. 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

[27] 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.

[28] 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).

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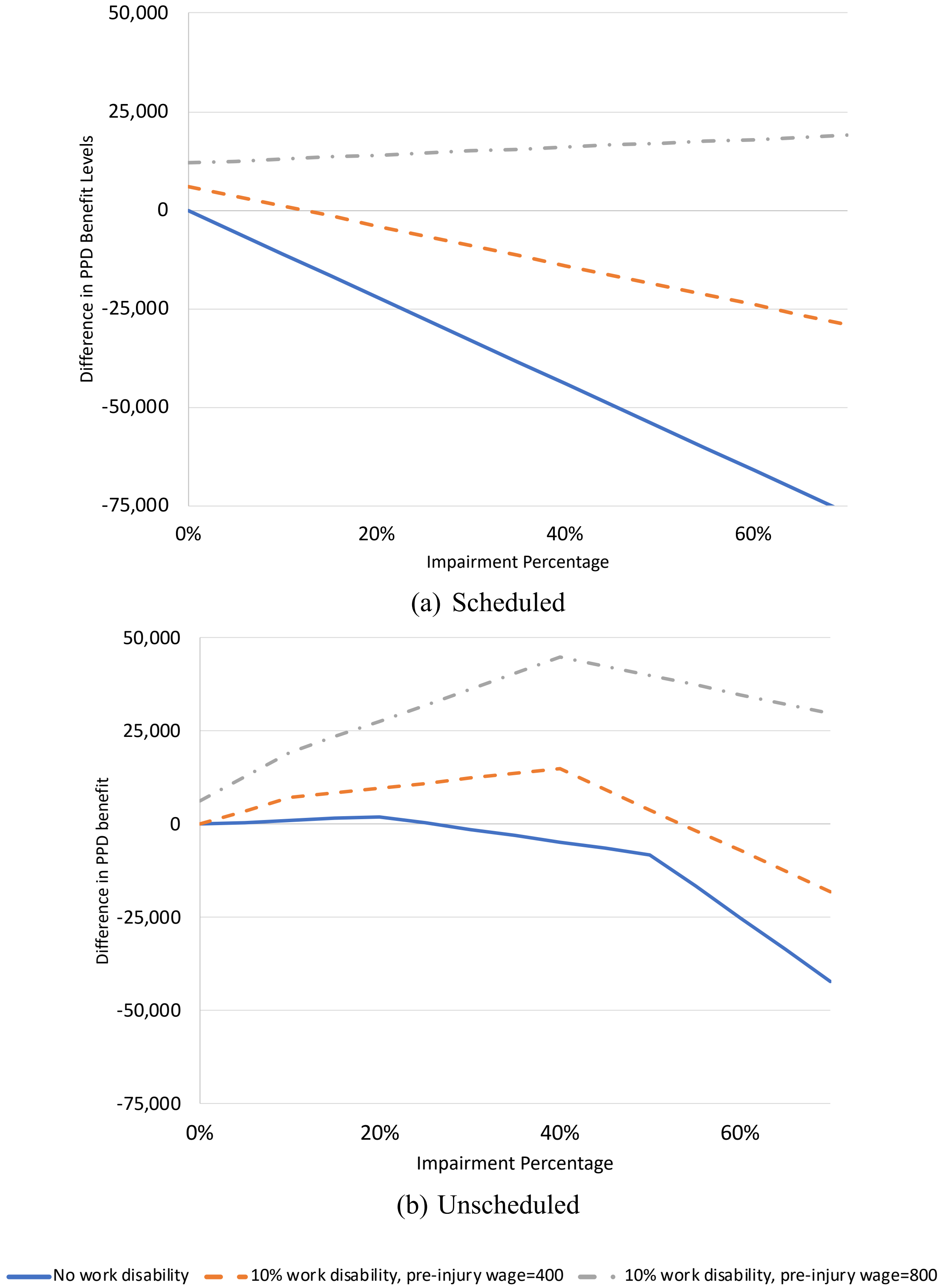
Figure 1: Difference in PPD Benefit Levels by Impairment Rating (Percent of Person), Before and After Reform

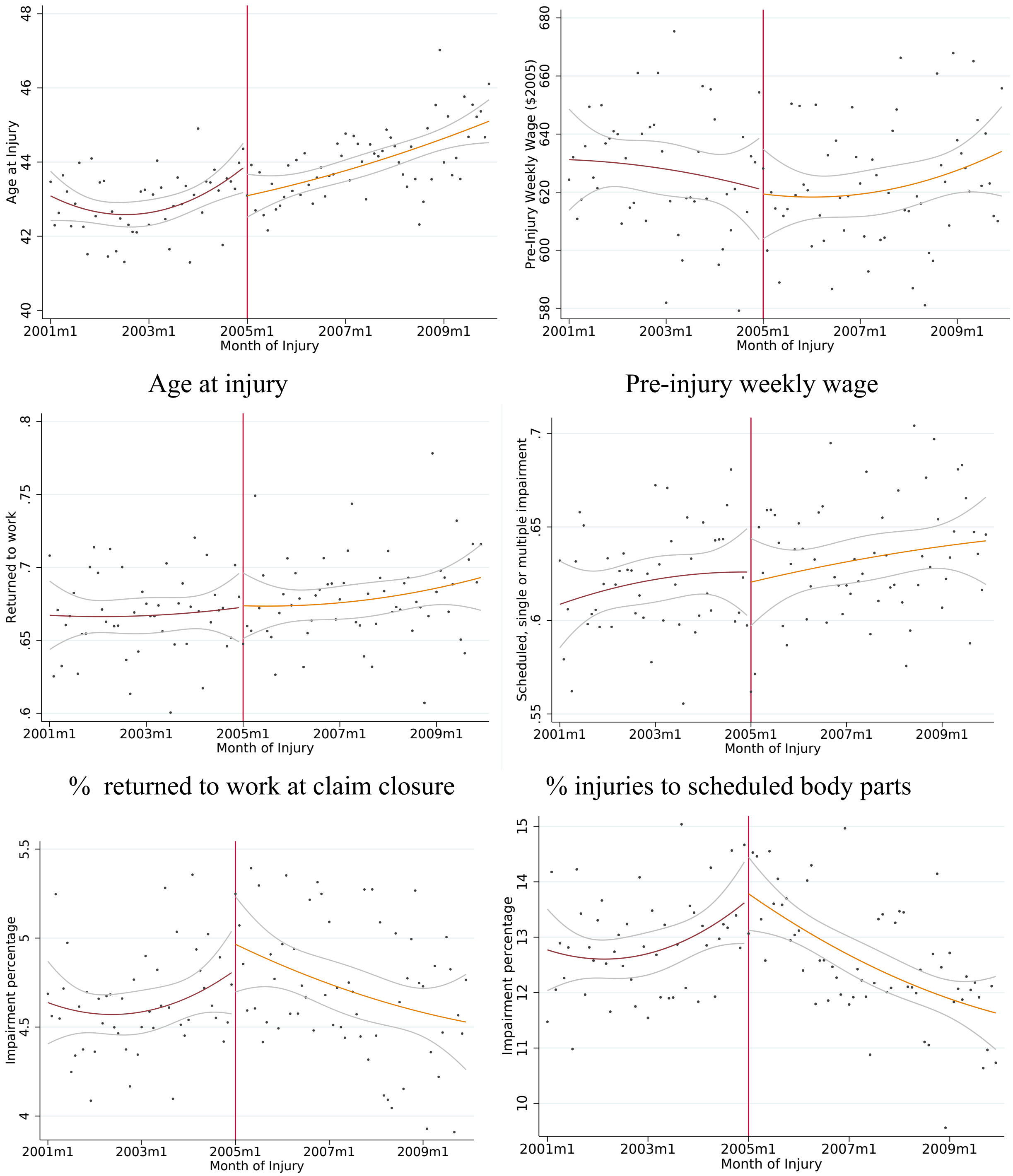


(a) Scheduled

(b) Unscheduled

Notes: Author calculations based on Oregon PPD benefit formulas as described in the Oregon Disability Rating Standards, 2015.

Figure 2: Trends in Formula Inputs by Injury Year



Age at injury

Pre-injury weekly wage

% returned to work at claim closure

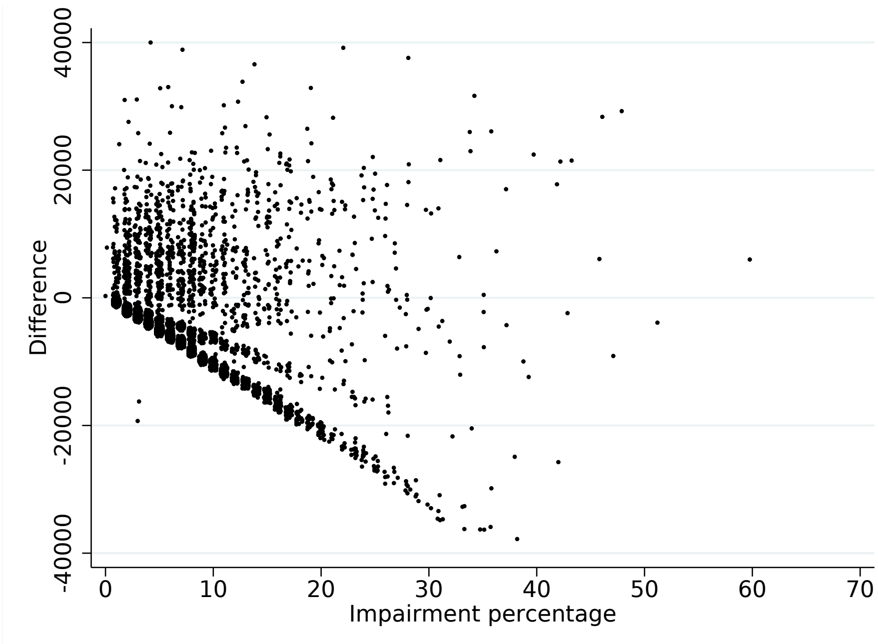
% injuries to scheduled body parts

Impairment rating (scheduled)

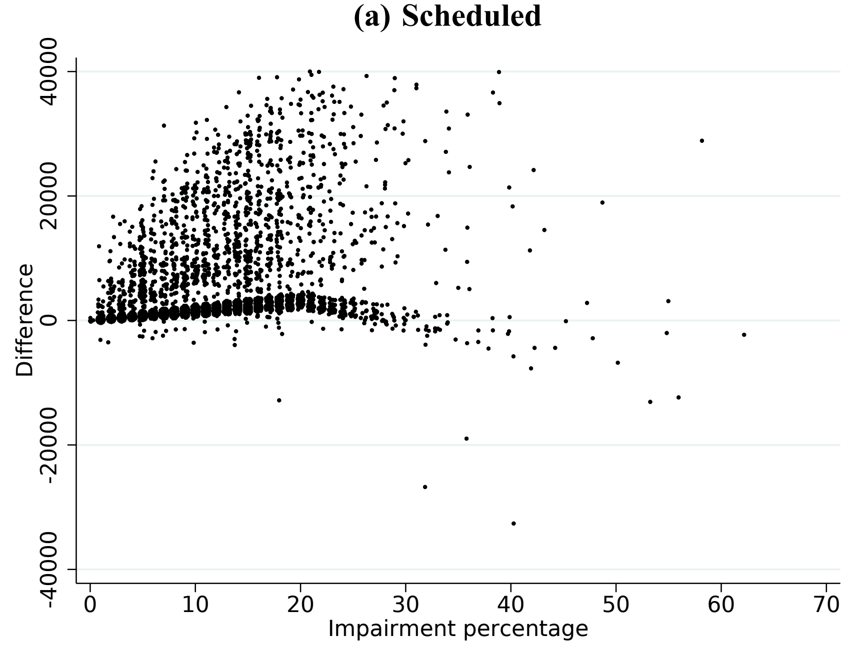
Work disability + impairment rating (unscheduled)

Notes: Data from the Oregon Department of Business and Consumer Services, 2001-2009. Figures show trends in key benefit formula inputs.

Figure 3: Difference Between Actual Benefit After 2005 and Hypothetical benefit Before 2005
(*Post-2005 Claims Only*)



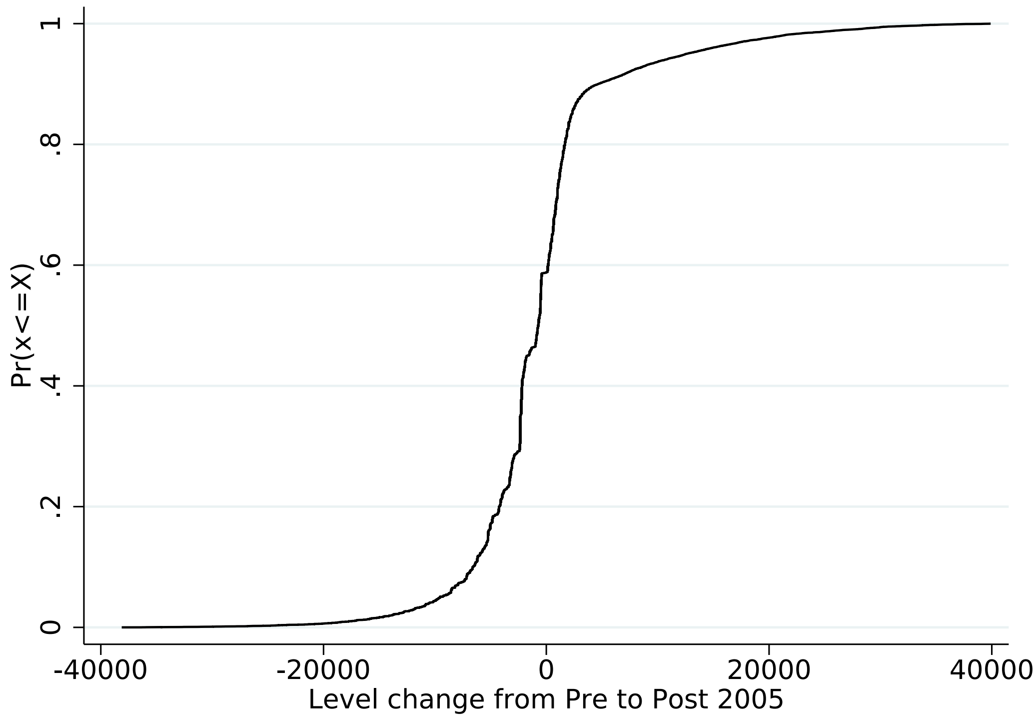
(a) Scheduled



(b) Unscheduled

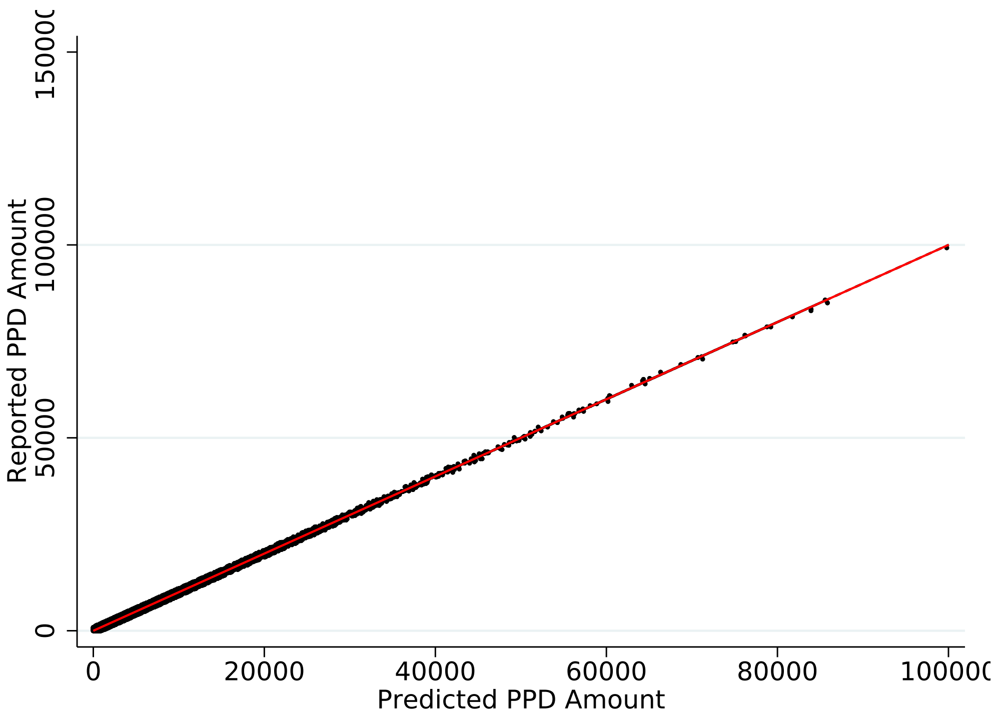
Notes: Based on data from the Oregon Department of Business and Consumer Services and author calculations. Hypothetical benefit before 2005 calculated using Oregon benefit formulas as described in in the Oregon Disability Rating Standards, 2015.

<u>Figure 4: Distribution of Change in Benefits from Hypothetical Pre-2005 Benefit to Actual Post-2005 Benefit (*Post-2005 Claims Only*)</u>

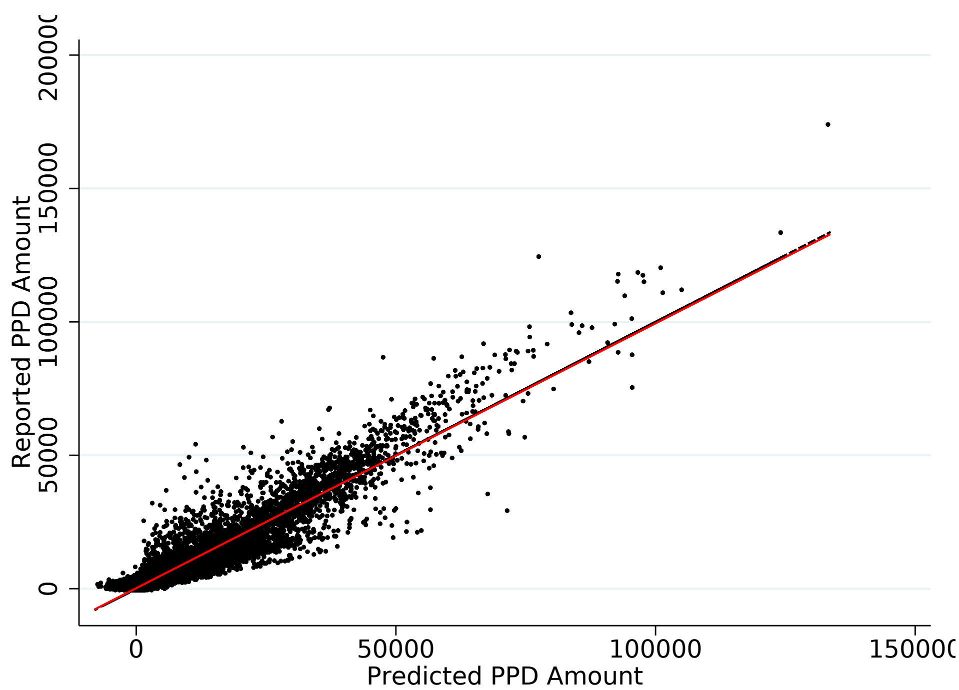


Notes: Based on data from the Oregon Department of Business and Consumer Services and author calculations. Hypothetical benefit before 2005 calculated using Oregon benefit formulas as described in the Oregon Disability Rating Standards, 2015.

Figure 5: First Stage – Reported vs. Predicted PPD Benefit from Formula Inputs



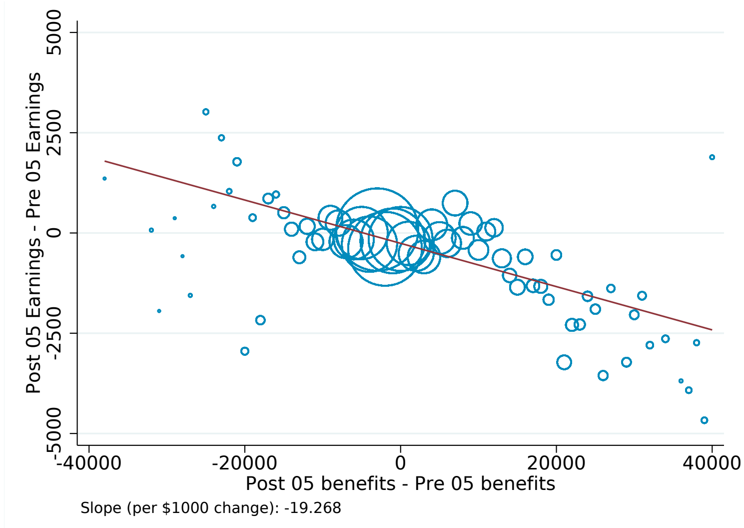
(a) Pre 2005



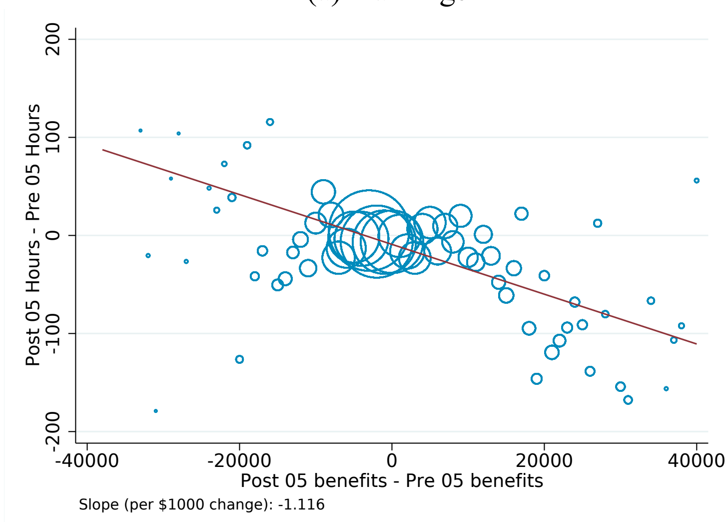
(b) Post 2005

Notes: Based on data from the Oregon Department of Business and Consumer Services, 2001-2009.

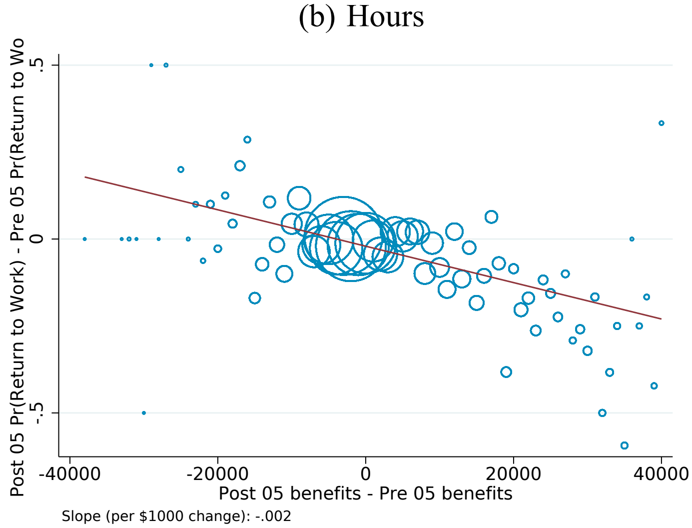
Figure 6: Reduced Form: Change in Labor Supply vs. Hypothetical Change in Benefits



(a) Earnings



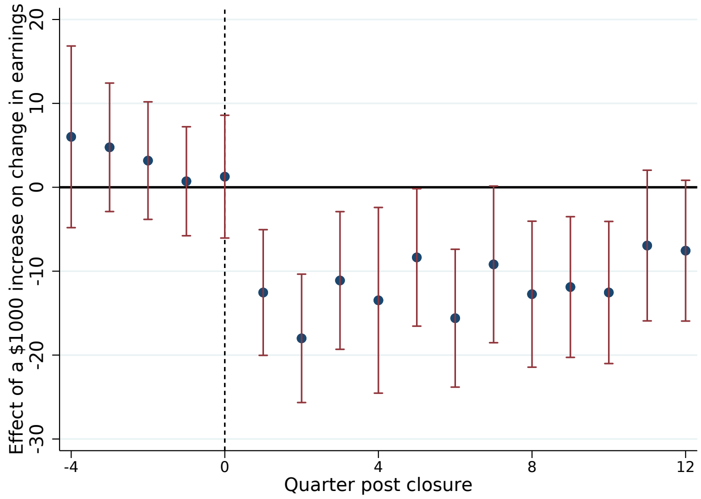
(b) Hours



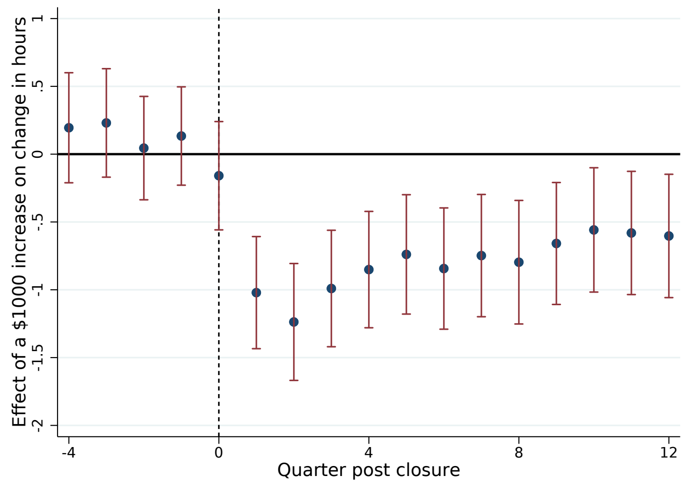
(c) Pr(Return to Work)

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.

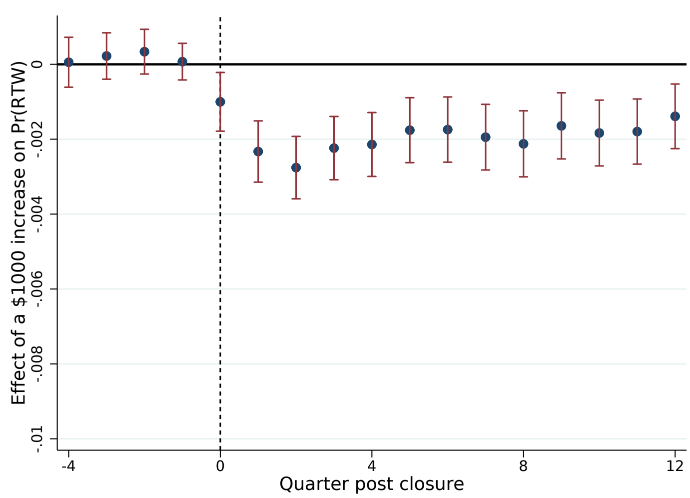
Figure 7: Trends in Estimated Effects over Time



Earnings



Hours



Pr(Return to Work)

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. 95 percent confidence intervals shown in the red bars.

Table 1: Claim Demographic Characteristics Pre- and Post- Reform

	Pre 2005	Post 2005	P-value
Claimant Characteristics			
Age	42.9	43.9	0.46
Age > 40	0.62	0.64	0.57
% male	0.73	0.71	0.15
Pre-injury weekly wage ($2005)	627	623	0.09
Medical expenditures ($2005)	12,365	13,519	0.80
TTD days	51.1	50.4	0.22
% returned to work at claim closure	0.67	0.68	0.97
% released to work at claim closure	0.79	0.83	0.21
Claim Duration (years)	0.91	0.91	0.48
Pre-injury occupation			
Production	0.20	0.14	0.58
Transportation	0.18	0.17	0.82
Construction	0.12	0.13	0.27
Maintenance	0.07	0.08	0.83
Other Occupation	0.43	0.47	0.68
Injury Characteristics			
Strain/sprain	0.39	0.28	0.12
Fracture/break	0.34	0.34	0.32
Trauma/unexpected	0.11	0.19	0.78
Wounds, cuts, burns	0.10	0.07	0.76
Other	0.07	0.12	0.79
Scheduled injuries	0.62	0.63	0.68
Multiple injuries	0.08	0.01	0.00
Observations	16,537	17,708	-

Notes: Data from Oregon Department of Consumer and Business Services, 2001-2009. We regressed the variable in the row on an indicator for whether or not the claim occurred before 2005, a flexible polynomial in month of injury and an interaction between the two for claims in 2004 and 2005. The p-value shown is the p-value from the coefficient on the post-2005 indicator.

Table 2: Formula Inputs and Body Codes, Pre and Post 2005 Reform

	Scheduled Injuries					
	Percentage of All Injuries			Impairment Percentage		
	Pre 2005	Post 2005	P-value	Pre 2005	Post 2005	P-value
Overall	62.0	63.2	0.68	4.6	4.7	0.06
Leg/Hip	25.7	24.6	0.48	4.1	4.2	0.55
Hand/Finger	19.2	18.9	0.03	4.1	4.2	0.05
Arm/Shoulder	15.1	12.7	0.47	6.4	6.4	0.53
Toes/Foot	7.7	6.9	0.49	4.3	4.3	0.72
Ear	0.5	0.5	0.19	12.7	12.4	0.16
Eye	0.1	0.1	0.41	7.3	8.1	0.80
	Unscheduled Injuries					
	Percentage of All Injuries			Impairment + Work Disability		
	Pre 2005	Post 2005	P-value	Pre 2005	Post 2005	P-value
Overall	38.0	36.8	0.68	12.9	12.6	0.82
Arm/Shoulder	15.1	18.8	0.29	11.6	11.1	0.25
Low back	13.9	11.1	0.79	14.5	14.5	0.90
Neck	4.3	3.4	0.07	13.1	13.7	0.05
Back - multiple	2.1	2.0	0.27	11.1	10.7	0.45
Other body systems	0.7	0.6	0.21	12.9	13.2	0.82
Brain	0.4	0.6	0.24	21.5	20.5	0.81
Leg/Hip	0.1	0.3	0.99	11.4	19.9	0.80

Notes: Data from Oregon Department of Consumer and Business Services, 2001-2009. We regressed the variable in the row on an indicator for whether or not the claim occurred before 2005, a flexible polynomial in month of injury and an interaction between the two for claims in 2004 and 2005. The p-value shown is the p-value from the coefficient on the post-2005 indicator. Some arm, shoulder, leg and hip injuries are classified as scheduled and unscheduled prior to 2005, so we show the share of injuries within this category classified as scheduled or unscheduled, respectively, in each panel. Total percent of all injuries summed across categories exceeds 100 percent prior to 2005 due to claims with multiple injuries.

Table 3: 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 average benefit	0.726	1127	20714
Pct change in Y-mean	-0.26%	-0.21%	-0.19%
$1000 change as pct of average benefit	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 4: 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 quartile	4641	16355	4641	16355	4641	16355
% change in Y-mean	-0.67%	-0.24%	-0.74%	-0.11%	-0.88%	-0.05%
1000 as % change in benefit	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 4, 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 quartile	5149	19541	5149	19541	5149	19541
% change in Y-mean	-0.36%	-0.35%	-0.35%	-0.28%	-0.36%	-0.30%
1000 as % change in benefit	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 5: 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✢
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 (2021) (this study)	Oregon WC PPD beneficiaries	-0.193

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%.