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The Labor Supply Consequences of Employment-Limiting Social Insurance Benefits: New Tests for Income Effects

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The Labor Supply Consequences of Employment-Limiting Social Insurance Benefits: New Tests for Income Effects*

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Abstract

Studies of moral hazard in employment-limiting social insurance programs such as Unemployment Insurance or Workers Compensation have demonstrated that higher benefits discourage work, emphasizing the price distortion inherent in benefit provision. Utilizing administrative data linking Workers' Compensation claim records to wage records from an Unemployment Insurance payroll tax database, I explore a different explanation and implement tests for "income effects" that exploit the fact that claimants no longer experience a distorted price of non-employment after an employment-limiting benefit ends. A pair of legislative changes to a Workers' Compensation benefit rate show little or no evidence of income effects and moderate evidence of income effects, respectively.

KEYWORDS: workers' compensation, labor supply, social insurance

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1. Introduction

One of the most widely confirmed results in the empirical literature on employment-limiting social insurance benefits such as Workers Compensation (WC) and Unemployment Insurance is that when benefit rates are higher, claimants spend longer receiving benefits. As Autor and Duggan (2007) and Chetty (2008) note, this effect is usually attributed to the fact that these programs distort the relative prices of consumption and non-employment: such programs replace a fraction of lost earnings only while a claimant is not working, so work is implicitly taxed. The alternative explanation, that a claimant's non-employment increases due to an expansion of the set of feasible consumption and non-employment choices, can be discounted because the total amount of benefits paid is small relative to a claimant's remaining lifetime income. In other words, the effect of these social insurance programs on employment is attributed to a "substitution effect" rather than an "income effect."

None of the previous research on the incentive effects of the employment-limiting WC Temporary Disability (TD) benefit distinguishes between substitution and income effects. Studies such as Krueger (1990), Meyer, Viscusi and Durbin (1995) and Neuhauser and Raphael (2004) use state-maintained administrative databases which track benefit payments, and so these studies measure labor supply changes through a limited measure of unemployment: the duration of TD benefit receipt. These studies confirm that TD durations are increasing in the benefit rate, but do not measure the extent to which this is caused by price distortion.

In this study, I propose new tests for income effects of employment-limiting social insurance benefits and apply these tests to TD. As Card, Chetty and Weber (2007b) have noted, claimants who exhaust an employment-limiting social insurance benefit do not always return to work immediately. In a theoretical model that follows Moffitt (1986) and Meyer (1990), I consider the effect of an increase in a TD benefit rate and derive two tests for income effects on labor supply, exploiting the fact that claimants who exhaust available benefits no longer face a distorted price of non-employment. First, the responsiveness of the duration of total non-employment to the benefit rate should exceed that of duration of benefit receipt only when income effects are substantial. Second, the frequency with which claimants enter into uncompensated non-employment should increase in the benefit rate only when income effects are substantial.

Data requirements for the proposed tests differ from those of tests for income effects that have been used to analyze other social insurance programs. Card, Chetty and Weber (2007a) show that workers who receive a larger lump-sum severance payment upon displacement spend longer unemployed, and Chetty (2008) demonstrates that Unemployment Insurance participants with higher net assets spend longer unemployed. Unlike this previous research on employment-limiting social insurance benefits, the proposed tests do not require data on assets or severance payments. The tests proposed in this study also differ from the test for income effects employed by Autor and Duggan (2007), who analyze an expansion of a cash benefit for disabled veterans that is not employment-limiting, and test for income effects by measuring labor force participation rates. In the tests I propose for employment-limiting social insurance programs, data on labor force participation must be supplemented with data on benefit receipt.

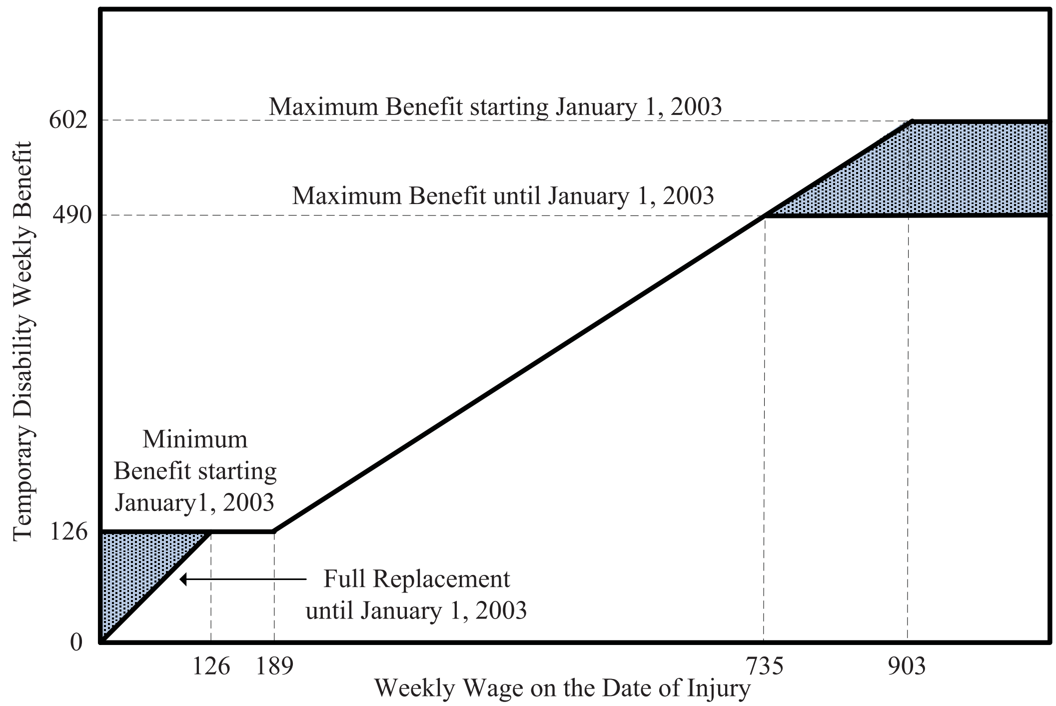
I provide a demonstration of these tests, appending quarterly wage records from an Unemployment Insurance payroll tax database to a WC benefits tracking database. I consider the experience of 330,984 California TD claimants who were injured in years 2001 to 2004. Estimation exploits a legislative change that raised the maximum TD benefit rate and instituted a minimum benefit rate. For each change in benefits, claimants in certain wage categories are subject to an increase in benefits, while others' benefit levels are unaffected, forming natural control and treatment groups. I consider two changes, one of which affected claimants injured on or after January 1, 2003 and another that affected claimants injured on or after January 1, 2004. I estimate difference-in-differences models to explain the labor supply trends of WC claimants in affected and unaffected portions of the wage distribution between 2002 and 2003, and 2003 and 2004, respectively. Available data also allow me to compare claimants in affected and unaffected wage categories between years 2001 and 2002, when there was no change in benefits, in order to assess the robustness of the empirical models.

The duration of TD benefit receipt responds somewhat more strongly to the 2002-2003 benefit increase, while total labor force participation and entry into uncompensated non-employment respond much more strongly to the 2003-2004 increase. This leads to the result that, in both tests, evidence of income effects is stronger for the 2003-2004 increase than the 2002-2003 increase, in which the effects are not statistically significant at conventional levels, and sometimes of the opposite sign of the predicted direction.

In interpreting these results, it is important to note that the legislative event under consideration coincided with a decline in the frequency and duration of TD claims. This leads to the phenomenon that each increase in benefits is followed by an increase in the labor supply of the control group while that of the treatment group remains relatively constant. Thus, this study differs from previous analyses of legislative changes that do not display such trends.

This paper proceeds as follows. I outline WC insurance in California, focusing on TD benefits and the legislative and regulatory changes that took place from 2002 to 2005. I then present a theoretical model and discuss how its implications permit tests for income effects. Afterwards, I describe the data, specify empirical models and present estimation results. A brief conclusion follows.

Figure 1: California TD Maximum and Minimum Benefit Levels 1996-2003



2. Workers Compensation Insurance in California

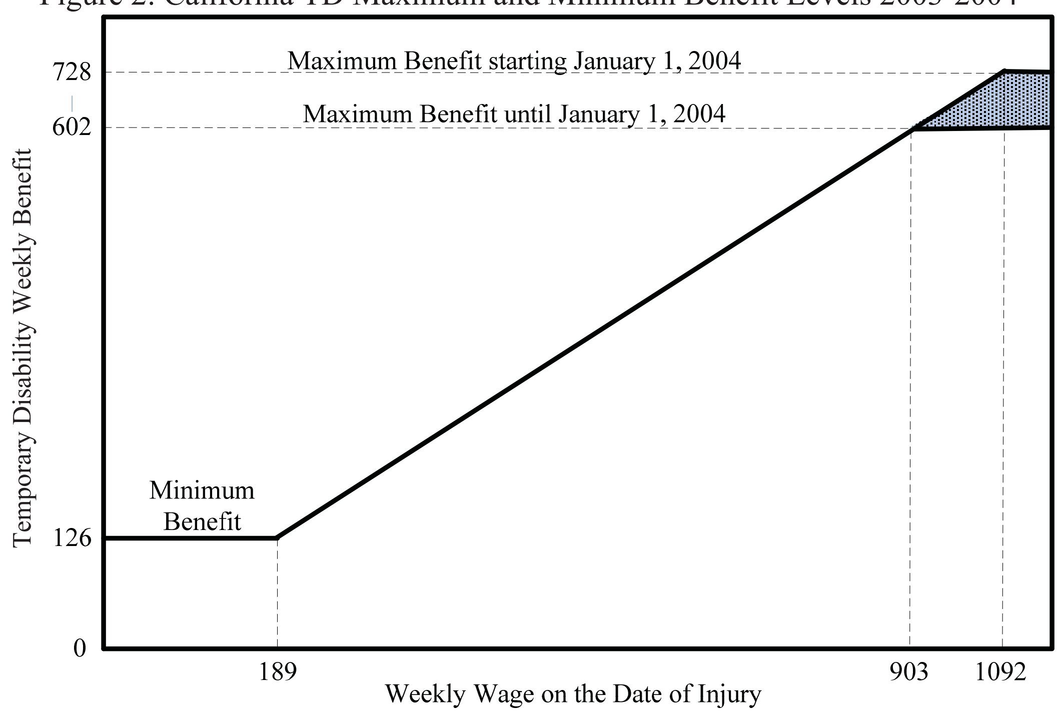
<u>Changes to Temporary Disability Weekly Rates</u>

Insurers in California are required to provide Temporary Disability (TD) benefits to WC claimants (employees injured at work) whenever the treating physician recommends absence from work as part of a recovery program. After a waiting period of three days, the claimant receives a series of bi-weekly checks that replace two-thirds of the claimant's weekly wage, subject to a minimum and a maximum. Periodically, the claimant returns to a physician for re-evaluation. If the physician determines that the scope for improvement has been exhausted, benefits cease even if the claimant does not return to work. The claimant normally may choose to return to work regardless of qualification for TD, in which case the benefit is suspended.

In this study, I consider two changes to the California TD benefit structure: the first, which applied to workplace injuries that occurred on or after

January 1, 2003, instituted a minimum TD benefit and raised the maximum benefit. The second, which applied to injuries that occurred on or after January 1, 2004, raised the maximum benefit again. Figure 1 shows the changes in maximum and minimum weekly TD benefits for claimants injured before and after January 1, 2003. Benefits for claimants injured prior to 2003 equaled the weekly wage up to $126 per week; claimants who earned between $126 and $189 per week received $126 per week. Those earning in excess of $189 per week received two-thirds of their weekly wage, except those earning $735 or more per week, who received the maximum benefit of $490. However, claimants injured on or after January 1, 2003, who earned below $189 per week received the minimum benefit of $126, those who earned between $735 and $903 now received two-thirds of their weekly wage as their TD benefit, and claimants who earned in excess of $903 received the maximum benefit of $602.

Figure 2: California TD Maximum and Minimum Benefit Levels 2003-2004



The TD benefits for claimants injured before and after January 1, 2004 are shown in Figure 2. For 2004 injuries, those earning between $189 and $1092 weekly received two-thirds of their weekly wage, and those earning in excess of $1092 received the maximum benefit of $728. Only claimants earning more than $903 per week were affected by this change. Overall, the two changes increased benefits for 35% of claimants and reduced the fraction of those receiving the maximum benefit from 30% in 2002 to 13% in 2004.

Broader Context

The changes to the TD benefit were enacted into law on February 15, 2002, as part of California Assembly Bill 749, which also increased compensation for Permanent Disability benefits.[1] On May 2, 2002, the California Department of Insurance responded to the law by increasing its statutory baseline insurance rates by 10%. Over the next two months, insurers submitted to the Department of Insurance (2008b) the new rates that they would charge employers in new or renewed contracts, and the overwhelming majority of insurers raised rates by 10%. Because insurance contracts are set once per year, and approved increases are only partially applied to premiums prior to a contract's renewal, insurers were unable to pass on the actuarial assessment of the full costs of this legislation to employers until mid-year 2003.[2]

In response to this new law, employers and insurers successfully advocated additional legislation that would lower WC costs.[3] One cost-saving measure, enacted in 2003, ensured that claimants injured on or after January 1, 2004 received Vocational Rehabilitation benefits, and claimants injured on or after January 1, 2003, could receive an otherwise prohibited cash settlement instead of normal benefits. Another law, passed on April 19, 2004, instituted a maximum of two years of receipt of TD benefits for nearly all types of injuries that occurred on or after that date. All WC claims for which Permanent Disability qualification had not been established by January 1, 2005, were subject to a new, more restrictive evaluation method that substantially reduced Permanent Disability benefits. In 2004, legislative and regulatory changes limited the scope of the primary treating physician to determine the treatment of WC claimants, irrespective of the injury date.

These additional reforms lowered the value of total compensable WC benefits, and increased the percent of total benefits allocated to TD, as described in detail in an actuarial analysis by the California Department of Insurance

[1] Permanent Disability benefits compensate workers for injuries from which they will never fully recover. Benefit receipt immediately occurs after TD benefits end in a subsequent sequence of bi-weekly payments. Although benefits were later linked to labor supply, any Permanent Disability benefits that apply to claimants considered in this study are not employment limiting.

[2] A referee suggested that there may be additional responses by insurers to Assembly Bill 749, and it is certainly possible that insurers may have taken expected legislative changes into consideration when submitting rate increases, and also when deciding to underwrite WC insurance.

[3] At the time of the law's enactment, the motivations of lawmakers and stakeholders were documented in California newspapers including the *San Francisco Chronicle*, for example Chronicle Sacramento Bureau (2002) and Raine (2002).

(2008a). Its analysis shows that the total effect of the legislative and regulatory changes increased TD benefits as a share of total non-medical WC benefits from 32.4% to 46.9%, and this is largely due to the change in the manner in which physicians rate Permanent Disability.

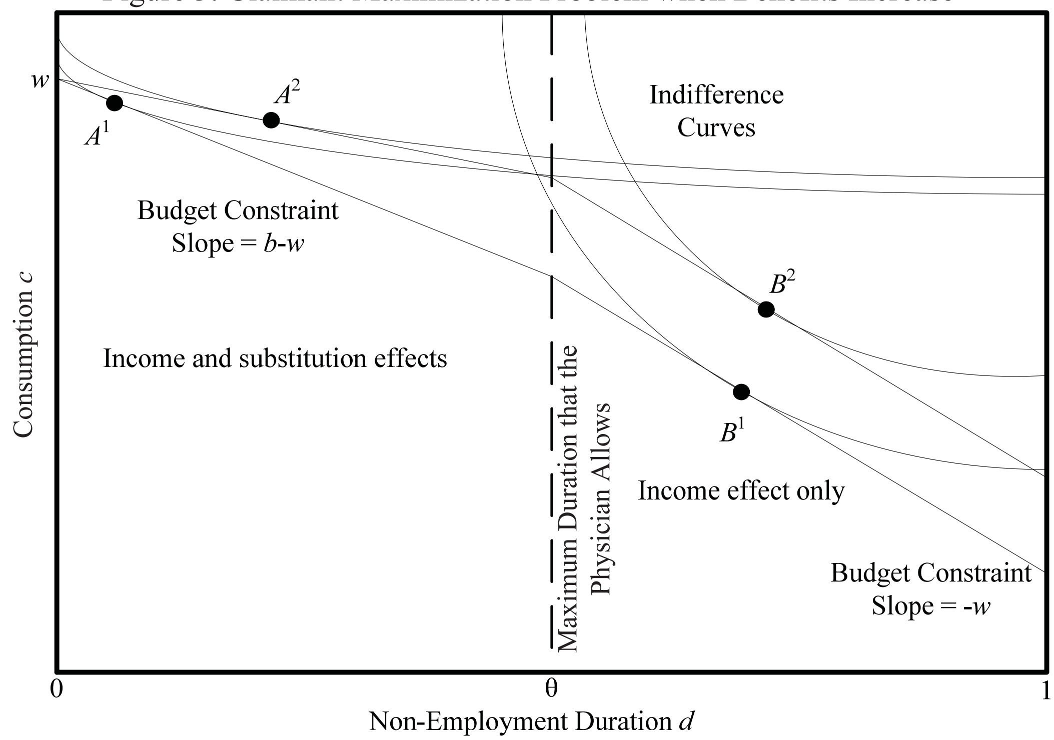
In the estimation that follows, a strong identifying assumption is that any labor supply responses induced by these other legislative and regulatory changes are similar for claimants whose positions in the wage distribution made them affected or unaffected by the increase in TD benefits. Changes in the Permanent Disability amount received, the end of the Vocational Rehabilitation program, the revisions to medical treatment guidelines could affect labor supply, although the magnitude of any such effects is difficult to estimate because most studies on the incentive effects of WC insurance focus on TD benefits. Nevertheless, it may help readers to note that the maximum Permanent Disability weekly benefit rates imposed uniform changes for those who earned \$375 or more per week, while changes to the Permanent Disability rating schedule, the Vocational Rehabilitation program, and medical treatment guidelines applied to all claimants without explicit provisions for different positions of the wage distribution. I note here that the overall result of these changes seems to have been a decrease in the value of total WC benefits that was not generally tied to earnings levels, and I return to the plausibility of the assumption of uniform (if any) labor supply responses to the other legislative and regulatory changes in the Conclusion section below.

3. Theoretical Framework

This paper considers the problem of how a TD claimant weighs the wages lost through non-employment against the recovery or leisure value of continued non-employment. The model I present shares many of the essential features of the Unemployment Insurance problem studied by Meyer (1990), in which a claimant receives a benefit that is revoked upon re-entry into the labor market, and I consider the implications of an increase in benefits such as that which took place in California as described above. Because benefits cease when a claimant returns to work, the benefit structure creates an implicit tax on labor supply, inducing a substitution effect. In what follows, I exploit the fact no substitution effects apply to the decision to work after benefits are exhausted, so any increase in uncompensated non-employment from higher benefits is attributable to income effects alone.

Consider a claimant who has just experienced a workplace injury and must choose a duration $d \in [0,1]$ of non-employment, $d = 0$ denoting immediate return to work, and $d = 1$ never returning to work. Upon labor market re-entry, a claimant earns a wage of w, making total labor income $(1-d)w$. The claimant receives a benefit b for the duration of compensated non-employment. A doctor imposes some upper bound θ on the duration that the claimant can claim benefits. All income is spent on a consumption good c which is expressed as

Figure 3: Claimant Maximization Problem when Benefits Increase



$$c = 1\{d < \theta\}(db + (1-d)w) + 1\{d \geq \theta\}(\theta b + (1-d)w). \tag{1}$$

The claimant maximizes utility $u(c,d)$ subject to this constraint. This utility maximization problem is shown in Figure 3. Note that the budget constraint has two different slopes: in the region in which the claimant's TD duration is less than θ, the marginal effect of continued non-employment on income is $b-w$, and after θ it is $-w$. Prior to θ, the claimant's additional income obtained through return to work is $w-b$ rather than w, so labor supply is implicitly taxed at the rate b/w. It can be helpful, following Moffitt (1986), to rewrite this optimization subject to a kinked piecewise-linear budget constraint as a system of three conditional demand functions:

$$\begin{aligned} d &= g(w-b, w) && \text{if } d < \theta \\ d &= \theta && \text{if } d = \theta \end{aligned} \tag{2}$$

$$d = g(w, w + b\theta) \qquad \text{if} \;\; d > \theta$$

where $g(p, m)$ is a demand function with arguments a marginal cost of non-employment p and an expenditure measure m.

Consider the impact of an increase in the benefit rate b for a claimant who initially would not exhaust benefits, that is, $d < \theta$. A corresponding initial decision is shown in Figure 3 as point A^1. Suppose that after the increase the claimant would still not exhaust TD benefits, shown as point A^2. There are two distinct channels for this effect. The first is an income effect: the presence of the increased benefit expands the claimant's budget constraint. The second is a substitution effect: the increase in the benefits affects the relative price of consumption and non-employment. This can be derived from the conditional demand functions shown above,

$$\frac{\partial g(w-b, w)}{\partial b} = \frac{\partial h(w-b, u)}{\partial b} + \frac{\partial g(w-b, w)}{\partial w} d \tag{3}$$

where $h(p, u)$ is the Hicksian demand function of the price of continued non-employment p and utility u, and the response of the duration of non-employment d to the benefit level b is a function of a combination of an income effect $\frac{\partial g(w-b, w)}{\partial w} d$ and a substitution effect $\frac{\partial h(w-b, u)}{\partial b}$.

However, if a claimant would initially maximize utility by exhausting TD benefits and taking additional time off of work $d > \theta$, then there is only one mechanism for an increase in non-employment: an income effect. The budget constraint has expanded, but the relative price of consumption and non-employment has not been affected. This is presented as a movement from point B^1 to B^2 in Figure 3. Now, the marginal effect of an increase in benefits on non-employment can be written as

$$\frac{\partial g(w, w + b\theta)}{\partial b} \tag{4}$$

and, because the marginal cost of continued non-employment w is not a function of the benefit rate b, a change in benefits only affects the budget constraint and is hence a pure income effect. This analysis provides a testable implication for labor supply when benefits increase and income effects are substantial. If the increase in the duration of total non-employment substantially exceeds the increase in the duration of benefit receipt, then $\frac{\partial g(w, w+b\theta)}{\partial b} > 0$ for at least some claimants, indicating income effects.

Similarly, any movement from $d \leq \theta$ to $d > \theta$ would imply that labor supply shifts at least in part due to an income effect. In this case, a claimant who would, if injured prior to the increase, not exhaust benefits, would, if injured after the increase, enter a period of uncompensated non-employment. In other words, a resultant period of uncompensated non-employment when benefits increase from b^1 to b^2 indicates that a claimant shifts from $\theta \leq g(w, w+b^1\theta)$ to $\theta > g(w, w+b^2\theta)$. Note that the inputs to the demand function only differ through the budget constraint $w+(w-b)\theta$. These arguments provide a second implication for labor supply when benefits increase and income effects are substantial. If the frequency of entry into uncompensated non-employment increases in the benefit rate, then $\frac{\partial g(w, w+b\theta)}{\partial b} > 0$ for at least some claimants, indicating income effects.

In summary, a standard social insurance model implies that the implicit tax on labor supply, which, as discussed by Autor and Duggan (2007), leads to substitution effects, will only apply to TD claimants prior to the cessation of benefits. This reasoning leads to two labor supply responses that will only occur if income effects are substantial. First, the responsiveness of the duration of total non-employment to the TD benefit rate will exceed that of the duration of benefit receipt. Second, the frequency with which claimants enter into uncompensated non-employment will increase in the TD benefit rate. Note that neither of these empirical measures requires data on whether a particular claimant has exhausted a given benefit (which is generally unavailable). Each responsiveness measure can be identified by the estimation of an average treatment effect.

4. The Data

I implement these tests for income effects using data from administrative databases owned by the State of California. I consider injuries in the State's WC tracking database, the Workers' Compensation Information System, which covers all workplace injuries. The data include the type of injury that occurred, including its cause and the part of body affected, the injured worker's age, gender and place of residence; and information about the injured worker's employment, including the injured worker's wage on the date of injury, the date of hire by the at-injury firm, and the insurance classification code applicable to the injured worker, as well as subsequent reports that include detailed information on the date and amount of benefit payments made pursuant to a WC claim.

These workplace injury records are supplemented[4] with quarterly earnings data from the California Base Wage File, which records the Unemployment Insurance taxable quarterly earnings of employees in California and, according to the Applied Research Unit (2002) of the Labor Market Information Division of the Employment Development Department, covers approximately 97% of the workforce. These records were appended with the likelihood that an employee is in any of four race and ethnicity categories: Hispanic, Asian Non-Hispanic, Black Non-Hispanic, and White Non-Hispanic, based on employee last name.

From these databases, I construct four labor supply measures. These measures are the duration of the TD benefit period, total earnings in quarters that include and follow the workplace injury, an earnings-adjusted sum of quarters including and following the quarter of injury in which an injured worker participates in the labor market, and an indicator variable for whether the injured worker has any earnings in the quarter in which TD benefits cease. Due to data constraints, I consider the maximum time horizon that could be applied consistently to all injured workers: the quarter of injury and ten subsequent quarters, or 143 weeks.[5]

The TD duration measure is defined as follows, using the available series of TD benefit summary updates. The start date for the duration is the earliest start date reported for TD benefits. The end date for the duration is the earliest reported end date on which the latest amount of the benefit was reported. I then define duration in weeks as the difference between the two dates, which I top-code at 143 weeks:

$$Weeks\_TD = \min\left\{\frac{EndDate - StartDate + 1}{7}, 143\right\}. \tag{5}$$

I define a second labor force participation measure to be the sum of the earnings for the quarter of injury plus ten subsequent quarters:

$$Earnings = \sum_{t=0}^{10} E_t \tag{6}$$

where E_t is the injured worker's total earnings t quarters after the injury.

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[4] A matching routine linked observations when the following conditions are satisfied: the Soundex code for the last name record is the same in each database and the Social Security Number for the records are different by at most a Levenshtein distance of two, using the SOUNDEX and COMPLEV functions in Base SAS, respectively.

[5] Estimation using other (shorter) time horizons yields results that are similar to those that follow.

Because the earnings measure is a indirect measure of employment duration, I define a third labor force participation measure based on both earnings and the number of quarters in which an injured worker works in the labor market in the ten quarters following injury, hereafter called “weeks LFP” for brevity. Considering the quarter of and the ten quarters that follow the workplace injury, I count as a full quarter worked any quarter including or following the quarter of injury in which earnings are at least equal to earnings in the quarter before that of injury. I count as a fractional quarter worked those quarters after the injury in which earnings constitute a fraction of earnings in the quarter immediately preceding the injury, and multiply the resulting number by 13 in order to approximate the number of weeks employed. In summary,

$$Weeks\_LFP = \sum_{t=0}^{10}\left[1\{E_{-1} \leq E_t\} + 1\{E_{-1} > E_t\}\frac{E_t}{E_{-1}}\right]*13\,. \tag{7}$$

Note that this measure of labor supply is coarser than the earnings measure defined above, in that it is less sensitive to changes in the number of hours employed or small numbers of continuous days or weeks spent in some form of non-employment. As reported below, the weeks LFP measure indicates a somewhat weaker relationship between benefit rates and labor supply than the total earnings measure. This difference may be caused by the relative coarseness of weeks LFP, which induces measurement error, leading to attenuation bias.

As a fourth labor supply measure, I consider whether claimants had earnings in the quarter in which TD benefits end. If there are no earnings in the quarter in which benefits expire, a claimant fails to return to work immediately, and enters a period of uncompensated non-employment. The re-entry variable takes a value of one when the employee has no earnings in the quarter benefits expire (i.e., fails to return to work immediately) and the value zero otherwise.

The primary independent variables are constructed using data on the date of injury and weekly earnings. The variable *After* takes the value one if the injured worker’s date of injury occurs on or after the date of the benefit increase and the value zero if it occurs before it. The variable *Treatment* takes the value of one if the injured worker was in a wage category affected by the increase and the value zero otherwise. For the 2002-2003 increase, employees are included in the treatment group when they earn \$126 or less, or when they earn \$735 or more, weekly. For the 2003-2004 increase, those earning \$903 or more weekly are included in the treatment group, and all others are in the control group. To increase the precision of my estimates, I also estimate narrower ranges of the wage distribution. I consider those who earned from \$500 to \$1100 when considering the 2002-2003 benefit increase. Likewise, I consider a narrower range around the maximum, $900 to $1275 when estimating the effect of the 2003-2004 benefit increase. When estimating the relationship between the wage categories and labor supply between 2001 and 2002, when benefit rates did not change, the wage categories are defined according to the wage categories from the referenced benefit increase. In such estimates, 2002 is naturally chosen to be the subsequent year, or the year of the non-change, and 2001 is its preceding year.

To account for observable heterogeneity, I consider a large number of control variables. With the exception of the race and ethnicity control variables as described above, all variables are derived from the initial report of a workplace injury to the Workers Compensation Information System. I include indicator variables for the insurance occupation classification code, the cause of injury, the nature of injury, the part of body, as well as the county in which the injured worker lived at the time of the injury. I also include as control variables the injured worker's age, tenure at the firm of injury, and gender, plus indicator variables that show when any of these data are missing.

I consider the labor market experience of a subset of the 633,002 TD claimants injured from 2001 to 2004. I exclude seven large insurers that top-coded earnings. Only the roughly 80% of claimants with quarterly earnings information are considered in this analysis. I do not consider the approximately 7% of injuries classified as either cumulative or due to an occupational disease, or had no physical injury, as defined by the International Association of Industrial Accident Boards and Commissions (2002), because these injuries are difficult to associate with a particular date of injury. Consultation with WC attorneys suggested that police and firemen may have some discretion concerning the compensable date of injury, so I exclude injury reports with corresponding insurance classification codes. I further restrict the sample on the basis of the weekly wage, eliminating wages that are below $75 per week or above $1275 per week. This eliminates approximately the lowest 2.5% of observations and highest 5% of observations ranked by weekly wage, respectively. To accommodate the baseline earnings requirement of the adjusted quarterly labor supply variable, I only include claimants who worked in the quarter of and the two quarters immediately preceding the quarter of injury. These sample selection criteria identify a sub-sample of 330,894 observations.

5. Estimation Method

I identify the effect of changing the weekly benefit rate by comparing affected and unaffected wage categories in the years preceding and following changes in the weekly benefit rate. Each of the three duration measures is estimated in a log-linear model, while non-employment in the quarter benefits end is estimated as a linear probability model. Specifically, I estimate difference in differences models of the form

$$L_i = \gamma_{0,i} + \gamma_{A,i} * After + \gamma_{T,i} * Treatment + \gamma_{AT,i} * After * Treatment + X\beta_i + \varepsilon_i \,. \quad (8)$$

In equation (8), L_i is the labor force participation measure i under consideration: log(*Weeks_TD*), log(*Earnings*), log(*Weeks_LFP*), and *Non_Employment_ When_Benefits_End*. *After* and *Treatment* are indicator variables as defined above, with respective marginal effects $\gamma_{A,i}$ and $\gamma_{T,i}$. $\gamma_{0,i}$ is the intercept, and ε_i is an error term. X is a vector of control variables, with marginal effects according to the vector β_i. Estimates that set $X\beta_i = 0$ (i.e., no control variables) are labeled "No Controls," while those that do not are labeled "All Controls." The parameter of interest for each labor supply parameter i is $\gamma_{AT,i}$, the marginal effect of the interaction of *After* and *Treatment*, which is interpreted as the marginal effect of a benefit increase. Robust standard errors are clustered on the month in which the injury occurs.

In order to recover the elasticity of each labor force participation measure i with respect to the benefit level, I compute

$$\frac{\gamma_{AT,i}}{\dfrac{\overline{B_{Treatment=1,After=1}} - \overline{B_{Treatment=1,After=0}}}{\overline{B_{Treatment=1,After=0}}} - \dfrac{\overline{B_{Treatment=0,After=0}} - \overline{B_{Treatment=0,After=1}}}{\overline{B_{Treatment=0,After=0}}}}, \quad (9)$$

where $\overline{B_{Treatment=,After=}}$ is the average TD benefit rate that applies to a particular *Treatment* by *After* group.

In the first test for income effects that I propose, I test the hypothesis that the change in the duration of uncompensated non-employment is equal to zero, in other words

$$\gamma_{AT,j} * \overline{L_{Weeks\_LFP,Treatment=1}} + \gamma_{AT,Weeks\_TD} * \overline{L_{Weeks\_TD,Treatment=1}} = 0 \quad (10)$$

where $\overline{L_{Weeks\_LFP,Treatment=1}}$ is the average number of weeks LFP for the treatment group, and $\overline{L_{Weeks\_TD,Treatment=1}}$ is the average number of weeks of TD benefit receipt for the treatment group, for two labor force participation measures j: total earnings and weeks LFP. This test assesses whether the responsiveness of labor force participation to the TD benefit rate is accounted for fully by that of the duration of benefit receipt. The weeks LFP measure is used to calculate the average duration of employment because it is more directly a duration measure.

In contrast to the duration measure-based tests described above, the estimation of equation (8) for whether the claimant had any earnings in the quarter benefits cease does not require analogous calculations. Now, the test for income effects is simply the hypothesis test from the linear probability model

$$\gamma_{AT,Non\_Employment\_When\_Benefits\_End} = 0\,. \tag{11}$$

Table 1: Descriptive Statistics

	2002-2003 Benefit Increase			
	Broad Sample		Narrow Sample	
	Control $127-734	*Treatment* $75-126 & $735-1275	*Control* $500-734	*Treatment* $735-1100
Sample Size Base Year	61,210	24,786	22,566	20,175
Sample Size End Year	59,183	24,847	22,840	19,419
Mean Benefit Rate Base Year	295.9	479.1	405.8	490.0
	(0.4)	(0.4)	(0.3)	(0.0)
Mean Benefit Rate End Year	299.6	560.3	405.9	569.9
	(0.4)	(0.6)	(0.3)	(0.4)
Percent Change	*1.2*	*16.9*	*0.0*	*16.3*
	(0.2)	*(0.2)*	*(0.1)*	*(0.1)*
Mean Weeks TD Base Year	37.2	33.6	37.6	34.0
	(0.2)	(0.3)	(0.3)	(0.2)
Mean Weeks TD End Year	32.9	31.2	32.4	31.7
	(0.2)	(0.3)	(0.3)	(0.2)
Percent Change	*-11.6*	*-7.0*	*-13.6*	*-6.6*
	(0.6)	*(1.1)*	*(1.0)*	*(1.3)*
Mean Earnings Base Year	41,319	98,657	58,319	94,887
	(144)	(391)	(266)	(403)
Mean Earnings End Year	44,025	101,458	61,817	96,492
	(154)	(399)	(274)	(414)
Percent Change	*6.5*	*2.8*	*6.0*	*1.7*
	(0.5)	*(0.6)*	*(0.7)*	*(0.6)*
Mean Weeks LFP Base Year	77.6	94.8	85.8	94.6
	(0.2)	(0.3)	(0.3)	(0.3)
Mean Weeks LFP End Year	79.8	95.9	88.4	95.6
	(0.2)	(0.3)	(0.3)	(0.3)
Percent Change	*2.8*	*1.2*	*3.0*	*1.1*
	(0.4)	*(0.4)*	*(0.5)*	*(0.5)*

Standard errors in parentheses.

Table 1: Descriptive Statistics (continued)

	2003-2004 Benefit Increase			
	Broad Sample		Narrow Sample	
	Control $75-902	*Treatment* $903-1275	*Control* $700-902	*Treatment* $903-1275
Sample Size Base Year	70,381	13,649	13,256	13,649
Sample Size End Year	59,998	12,018	11,830	12,018
Mean Benefit Rate Base Year	333.0	602.0	527.5	602.0
	(0.5)	(0.0)	(0.3)	(0.0)
Mean Benefit Rate End Year	334.4	690.3	526.5	690.3
	(0.5)	(0.4)	(0.4)	(0.4)
Percent Change	*0.4*	*14.7*	*-0.2*	*14.7*
	(0.2)	*(0.1)*	*(0.1)*	*(0.1)*
Mean Weeks TD Base Year	32.9	29.8	33.0	29.8
	(0.2)	(0.4)	(0.4)	(0.4)
Mean Weeks TD End Year	27.6	26.9	28.0	26.9
	(0.2)	(0.3)	(0.3)	(0.3)
Percent Change	*-16.0*	*-9.7*	*-15.3*	*-9.7*
	(0.6)	*(1.6)*	*(1.4)*	*(1.6)*
Mean Earnings Base Year	49,807	118,760	81,889	118,760
	(159)	(563)	(431)	(563)
Mean Earnings End Year	53,458	119,110	86,411	119,110
	(178)	(604)	(462)	(604)
Percent Change	*7.3*	*0.3*	*5.5*	*0.3*
	(0.5)	*(0.7)*	*(0.8)*	*(0.7)*
Mean Weeks LFP Base Year	81.7	99.2	92.8	99.2
	(0.2)	(0.4)	(0.4)	(0.4)
Mean Weeks LFP End Year	84.9	99.5	95.4	99.5
	(0.2)	(0.4)	(0.4)	(0.4)
Percent Change	*3.9*	*0.3*	*2.8*	*0.3*
	(0.3)	*(0.5)*	*(0.6)*	*(0.5)*

Standard errors in parentheses.

Because the non-employment variable takes the value one if there are no reported earnings in the quarter in which TD benefits end and zero otherwise, in the context of the theoretical section above the sign of this parameter is expected to be positive if income effects are substantial.

6. Results

Descriptive Statistics

I begin by considering the sample sizes and population means of the samples for each of the increases under consideration. Descriptive statistics are listed in Table 1. The sample size decreases from the first year to the second in each two-year interval, which is consistent with the decrease in the incidence rate of lost-time injuries in California that the U.S. Bureau of Labor Statistics (2008) reports for this period. Overall, claimants in affected wage categories exhibit substantial increases in their scheduled benefits defined as a function of the weekly wage: the relative increase in weekly benefits is 15% to 17% from 2002 to 2003, and 14% to 15% from 2003 to 2004.

Table 2: Difference-in-Differences Log-Linear Regression Estimates

	2002-2003 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Log Weeks TD	0.100 *** (0.028)	0.118 *** (0.029)	0.126 *** (0.035)	0.144 *** (0.034)
Log Earnings	-0.048 *** (0.017)	-0.050 *** (0.016)	-0.056 *** (0.016)	-0.051 *** (0.014)
Log Weeks LFP	-0.024 * (0.014)	-0.028 ** (0.014)	-0.035 ** (0.014)	-0.032 ** (0.013)
	2003-2004 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Log Weeks TD	0.078 ** (0.030)	0.105 *** (0.028)	0.046 (0.038)	0.079 ** (0.036)
Log Earnings	-0.093 *** (0.019)	-0.073 *** (0.018)	-0.065 *** (0.015)	-0.045 *** (0.014)
Log Weeks LFP	-0.056 *** (0.014)	-0.046 *** (0.013)	-0.040 *** (0.012)	-0.030 ** (0.011)

Robust standard errors clustered on quarter of injury are in parentheses. *, ** and *** represent that the estimate is significant at the 10%, 5% and 1% level, respectively.

TD durations decline substantially from 2002 to 2004; however, the average decline in the duration of TD benefit receipt is greater in magnitude for the control group compared to the treatment group for each benefit increase. The measures total earnings and weeks LFP both indicate a greater rise in labor force participation for the control groups than for the respective treatment groups, and the magnitude of the proportionate changes is larger using the total earnings measure.

Estimates from Multiple Duration Measures

I next consider the effect of experiencing a benefit increase on the three different labor supply duration measures: the duration of TD benefit receipt, total earnings, and weeks LFP. Specifically, I calculate the parameter estimates for the interaction between the *After* and *Treatment* variables in the difference-in-differences estimation model specified in equation (8), with and without inclusion of the control variables listed above, and present the results in Table 2. The parameter estimates indicate that labor supply exhibits a relative decrease for those whose benefits increased. Those claimants in affected wage categories exhibit a relative increase in the duration of TD benefit receipt of 10% to 15% from 2002 to 2003 and 4% to 11% from 2003 to 2004. Similarly, earnings and the weeks LFP decline for the treatment group relative to the control group: earnings decline 4% to 6% and weeks LFP by 2% to 4% between 2002 and 2003; between 2003 and 2004 earnings decline by 4% to 10% and weeks LFP by 3% to 6%. Most estimates are statistically significant at the 1% or 5% level. Note that the labor supply measures respond differently to the two benefit increases. The duration of benefit receipt responds much more strongly to the 2002-2003 increase than the 2003-2004 increase, while the opposite is true for the labor supply measures derived from quarterly earnings records.

In order to compare the results of the regression analysis above to other estimates of the effect of a change in benefits on TD benefit receipt durations, following equation (9), I divide the parameter estimates as listed in Table 2 by the net increase in the benefit rate for the control group relative to the treatment group as listed in Table 1, and present the resulting elasticity estimates in Table 3. The elasticity of duration with respect to the benefit level is 0.3 to 0.9, with larger elasticities for the 2002 to 2003 benefit increase. These estimates fall in the middle to the high end of the range of empirical estimates: Krueger and Meyer (2002) report that the elasticity of lost time with respect to the benefit level tends to cluster around 0.2 to 0.3, but in some studies exceeds 0.7, and that studies that employ individual microdata tend to have higher elasticities. Neuhauser and Raphael (2004), who use similar data and consider TD benefit increases in the mid 1990s in California, report elasticity estimates of 0.1 to 0.4 for empirical specifications most similar to what I define as the broad sample, whereas the elasticity estimates for the broad sample in Table 3 are clearly higher and range from 0.5 to 0.8. I similarly calculate elasticity estimates for the labor force participation measures derived from quarterly earnings records. The elasticity of earnings with respect to benefits is -0.2 to -0.7, and the elasticity of weeks LFP is -0.1 to -0.4, also shown in Table 3. Elasticity estimates for these two labor supply measures are stronger for the 2003-2004 benefit increase than the 2002-2003 benefit increase.

Table 3: Elasticity Estimates (Calculation)

	2002-2003 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Log Weeks TD	0.639 ***	0.751 ***	0.771 ***	0.882 ***
	(0.181)	(0.182)	(0.216)	(0.209)
Log Earnings	-0.286 ***	-0.283 ***	-0.333 ***	-0.328 ***
	(0.100)	(0.089)	(0.098)	(0.089)
Log Weeks LFP	-0.149 *	-0.151 **	-0.210 **	-0.179 **
	(0.088)	(0.073)	(0.084)	(0.070)
	2003-2004 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Log Weeks TD	0.546 ***	0.741 ***	0.311	0.532 **
	(0.211)	(0.198)	(0.254)	(0.244)
Log Earnings	-0.623 ***	-0.587 ***	-0.419 ***	-0.389 ***
	(0.128)	(0.145)	(0.094)	(0.116)
Log Weeks LFP	-0.386 *	-0.332 **	-0.257 ***	-0.233 ***
	(0.097)	(0.095)	(0.076)	(0.090)

Standard errors in parentheses. *, ** and *** represent that the estimate is significant at the 10%, 5% and 1% level, respectively.

Table 4: Log-Linear Regression Falsification Test on 2001-2002 Injuries

	2002-2003 Benefit Increase Wage Categories			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Log Weeks TD	-0.006	-0.013	0.013	0.008
	(0.025)	(0.025)	(0.033)	(0.032)
Log Earnings	0.013	0.005	0.000	0.006
	(0.018)	(0.016)	(0.019)	(0.017)
Log Weeks LFP	0.012	0.012	0.001	0.008
	(0.016)	(0.015)	(0.016)	(0.014)
	2003-2004 Benefit Increase Wage Categories			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Log Weeks TD	0.018	0.012	0.039	0.042
	(0.023)	(0.026)	(0.029)	(0.036)
Log Earnings	0.004	-0.002	0.024	0.015
	(0.022)	(0.020)	(0.021)	(0.020)
Log Weeks LFP	0.015	0.013	0.016	0.011
	(0.020)	(0.019)	(0.017)	(0.017)

Robust standard errors clustered on quarter of injury are in parentheses. *, ** and *** represent that the estimate is significant at the 10%, 5% and 1% level, respectively.

Table 5: Durations Employed, Compensated by TD, and Remainder, in Weeks

	2002-2003 Benefit Increase			
	Broad Sample		Narrow Sample	
	Control	*Treatment*	*Control*	*Treatment*
	$127-734	$75-$126 & $735-1275	$500-734	$735-1100
Weeks LFP	78.7	95.3	87.1	95.1
	(0.2)	(0.3)	(0.3)	(0.3)
TD Benefit Receipt	35.1	32.4	35.0	32.9
	(0.2)	(0.3)	(0.3)	(0.3)
Other Non-Employment	29.2	15.3	20.9	15.0
	(0.3)	(0.4)	(0.4)	(0.4)
	2002-2003 Benefit Increase			
	Broad Sample		Narrow Sample	
	Control	*Treatment*	*Control*	*Treatment*
	$75-902	$903-1275	$700-902	$903-1275
Weeks LFP	83.2	99.3	94.0	99.3
	(0.2)	(0.4)	(0.4)	(0.4)
TD Benefit Receipt	30.5	28.5	30.6	28.5
	(0.2)	(0.3)	(0.4)	(0.3)
Other Non-Employment	29.3	15.2	18.3	15.2
	(0.2)	(0.5)	(0.5)	(0.5)

Standard errors in parentheses. Other non-employment is defined as 143 minus the number of weeks of LFP and TD Benefit Receipt.

Available data allow similar evaluation of the years 2001-2002, when benefits did not change and, therefore, the labor supply measures are not expected to diverge between the different wage categories. Defining *After* as equal to one when an injury occurs in 2002 and zero when it occurs in 2001, and defining the *Treatment* variable according to the wage categories used to estimate the impact of the two respective benefit increases, I estimate equation (8) and report the results in Table 4. Point estimates are small and not statistically significant, indicating that the labor supply measures did not behave differently for the wage categories during these two years when no benefit increase took place.

I next consider the time that claimants spend in three different states: receiving TD benefits, working, and neither receiving TD benefits nor working. I calculate the number of weeks of non-employment as 143 less weeks LFP and weeks of TD benefit receipt. Weeks LFP, weeks of TD benefit receipt, and weeks of other non-employment are shown in Table 5. On average, claimants spend more than half of the these 143 weeks employed. Of the remainder, more time is spent receiving TD than not.

Table 6: Effect of TD Reform on Three Durations, in Weeks
Treatment Effect Using Earnings (Calculation)

	2003-2004 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Weeks LFP	-4.604 ***	-4.784 ***	-5.298 ***	-4.883 ***
	(1.606)	(1.504)	(1.557)	(1.330)
Weeks TD	3.250 ***	3.821 ***	4.127 ***	4.720 ***
	(0.923)	(0.924)	(1.157)	(1.119)
Other Non-Employment	1.353	0.963	1.171	0.163
	(1.853)	(1.765)	(1.940)	(1.738)
	2003-2004 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Weeks LFP	-9.273 ***	-7.261 ***	-6.438 ***	-4.497 ***
	(1.898)	(1.793)	(1.449)	(1.342)
Weeks TD	2.215 ***	3.002 ***	1.315	2.250 **
	(0.856)	(0.803)	(1.072)	(1.033)
Other Non-Employment	7.058 ***	4.529 **	5.123 ***	2.247
	(2.083)	(1.964)	(1.803)	(1.694)

Standard errors in parentheses. The change in other non-employment is the negative of the sum of the change in weeks in employment and compensated non-employment. *, ** and *** represent that the estimate which the statistic is derived is significant at the 10%, 5% and 1% level, respectively.

Finally, I implement the first test for income effects by performing the calculation in equation (10). I multiply the parameter estimates in Table 2 for the effect of the benefit increase on the duration of TD benefit receipt by the number of weeks the treatment group spends receiving TD benefits in Table 5, and, similarly, the effect of the reform on both employment measures in Table 2 by the number of weeks LFP in Table 5. Note that the change in the number of weeks of TD receipt associated with the benefit increase plus that of weeks of other non-employment must sum to the negative of that of weeks LFP.

Table 7: Effect of TD Reform on Three Durations, in Weeks
Treatment Effect Using Weeks LFP (Calculation)

	2002-2003 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Weeks LFP	-2.320 *	-2.666 **	-3.282 **	-3.087 ***
	(1.364)	(1.297)	(1.315)	(1.205)
Weeks TD	3.250 ***	3.821 ***	4.127 ***	4.720 ***
	(0.923)	(0.924)	(1.157)	(1.119)
Other Non-Employment	-0.931	-1.155	-0.845	-1.633
	(1.647)	(1.592)	(1.751)	(1.644)
	2003-2004 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Weeks LFP	-5.552 ***	-4.580 **	-3.942 ***	-2.951 ***
	(1.403)	(1.308)	(1.173)	(1.139)
Weeks TD	2.215 ***	3.002 ***	1.315	2.250 **
	(0.856)	(0.803)	(1.072)	(1.033)
Other Non-Employment	3.337 **	1.578	2.626 *	0.700
	(1.644)	(1.535)	(1.589)	(1.537)

Standard errors in parentheses. The change in other non-employment is the negative of the sum of the change in weeks in employment and compensated non-employment. *, ** and *** represent that the estimate which the statistic is derived is significant at the 10%, 5% and 1% level, respectively.

The results of these calculations are listed in Tables 6 and 7. The hypothesis test listed in equation (10) is assessed in the rows labeled "Other Non-Employment", and intermediate calculations for responsiveness of the number of weeks LFP and weeks TD to the benefit increases are also listed. Although estimates of the 2002-2003 reform using the measure of weeks derived from quarters of participation show that the number of weeks of uncompensated non-employment decrease, all other estimates show weeks of uncompensated non-employment increase. The magnitude of the change in other non-employment is larger when measuring the change in labor supply using earnings rather than weeks LFP. As discussed above, this may result from different levels of attenuation bias: weeks LFP is a coarser measure of labor supply than total earnings and therefore may measure labor supply with somewhat more measurement error. The calculations for the 2003-2004 increase suggest that the change in the duration of TD benefit receipt only captures part of the effect of the TD benefit rate on the number of weeks spent not working after injury, providing evidence of an income effect of WC on labor supply. However, the 2002-2003 benefit increase does not provide such evidence.

Benefit Expiration and Labor Market Re-Entry

I now present the results of the second test for income effects. Specifically, I estimate the effect of the benefit level on WC claimants' probability of having no earnings in the quarter in which TD benefits end. Recall that having no earnings in the quarter in which benefits expire indicates that a claimant fails to return to work immediately, and enters a period of uncompensated non-employment. An increase in the fraction of claimants who enter a period of uncompensated non-employment when benefits increase is evidence of income effects, as argued above. As reported in Table 8, less than 30% of claimants have no reported wages in the quarter that TD benefits end, and as little as 15% of affected wage earners have no such wages reported. This evidence is consistent with the previous finding, in Card, Chetty and Weber (2007b), that the rate of exit from a social insurance program upon benefit exhaustion is larger than the exit rate from non-employment. It is noteworthy that labor force participation rates in the quarter benefits cease are increasing in income, which indicates that claimants with higher income at the time of injury are less likely to enter into a spell of uncompensated non-employment, the best available signal that benefits are exhausted.

Table 8: Fraction Not Working When Benefits End

	2002-2003 Benefit Increase			
	Broad Sample		Narrow Sample	
	Control	*Treatment*	*Control*	*Treatment*
	$127-734	$75-$126 & $735-1275	$500-734	$735-1100
Mean Not Working Base Year	0.296	0.184	0.248	0.185
	(0.002)	(0.002)	(0.003)	(0.003)
Mean Not Working End Year	0.280	0.175	0.227	0.179
	(0.002)	(0.002)	(0.003)	(0.003)
Percent Change	*-5.6*	*-4.7*	*-8.5*	*3.2*
	(0.9)	*(1.8)*	*(1.5)*	*(2.1)*
	2003-2004 Benefit Increase			
	Broad Sample		Narrow Sample	
	Control	*Treatment*	*Control*	*Treatment*
	$75-902	$903-1275	$700-902	$903-1275
Mean Not Working Base Year	0.268	0.151	0.200	0.151
	(0.002)	(0.003)	(0.003)	(0.003)
Mean Not Working End Year	0.237	0.152	0.177	0.152
	(0.002)	(0.003)	(0.004)	(0.003)
Percent Change	*-11.5*	*0.7*	*-11.8*	*0.7*
	(0.9)	*(3.0)*	*(2.3)*	*(3.0)*

Standard errors in parentheses.

I estimate difference-in-differences linear probability models of the effect of being in a wage category affected by the benefit increase on the frequency of having zero earnings in the quarter that TD benefits expire, according to equation (8) above. The results of this estimation are presented in Table 9. The 2002-2003 benefit increase for the broad sample indicates an increase non-employment upon benefit exhaustion of less than 1%, although this is not statistically different from zero. The narrow sample, in contrast, indicates an increase of 1% to 2% that is statistically different form zero at the 5% level. The 2003-2004 increase is associated with a 1% to 3% increase, and the estimates are more clearly different from zero. Corresponding elasticity estimates are listed in Table 10 for each of the two increases, and the ranges are 0.1 to 0.4 or the 2002-2003 benefit increase and 0.7 to 1 in the 2003-2004 benefit increase. Following equation (11), these estimates demonstrate that an increase in benefits decreases the probability of returning to work immediately, providing evidence of an income effect. As in the previous test, any evidence of income effects is stronger for the 2003-2004 increase than the 2002-2003 increase.

Table 9: Linear Probability Model Difference-in-Differences Regression

	2002-2003 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Non-Employment When Benefits End	0.004 (0.004)	0.006 (0.005)	0.012 ** (0.006)	0.011 ** (0.005)
	2003-2004 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Non-Employment When Benefits End	0.021 *** (0.004)	0.019 *** (0.004)	0.017 *** (0.006)	0.016 ** (0.006)

Robust standard errors clustered on quarter of injury are in parentheses. *, ** and *** represent that the estimate is significant at the 10%, 5% and 1% level, respectively.

Table 10: Elasticity of Non-Participation when Benefits End (Calculation)

	2002-2003 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Non-Employment When Benefits End	0.145 (0.158)	0.195 (0.162)	0.392 ** (0.195)	0.375 ** (0.182)
	2003-2004 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Non-Employment When Benefits End	0.997 *** (0.202)	0.867 *** (0.193)	0.774 *** (0.261)	0.715 ** (0.287)

Standard errors in parentheses. *, ** and *** represent that the estimate is significant at the 10%, 5% and 1% level, respectively.

Table 11: Linear Probability Model Falsification Test on 2001-2002 Injuries

	2002-2003 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Non-Employment When Benefits End	-0.010 ** (0.004)	-0.008 ** (0.004)	-0.004 (0.006)	-0.004 (0.005)
	2003-2004 Benefit Increase			
	Broad Sample		Narrow Sample	
Dependent Variable	*No Controls*	*All Controls*	*No Controls*	*All Controls*
Non-Employment When Benefits End	-0.006 (0.006)	-0.004 (0.005)	0.000 (0.006)	0.001 (0.006)

Standard errors in parentheses. *, ** and *** represent that the estimate is significant at the 10%, 5% and 1% level, respectively.

Estimates from a replication of this estimation procedure on injuries that occurred during 2001 and 2002 are presented in Table 11. These results indicate that immediate return-to-work was changing differentially between 2001 and 2002 for control and treatment groups for the 2002-2003 reform as defined by the broad category but not narrow wage category, and the 2003-2004 reform shows no differential behavior for the two sets of wage categories.

7. Conclusions

In this study, I analyze a legislative reform and use newly assembled data to test for an income effect from the receipt of WC TD benefits. A standard social insurance model suggests two implications for labor supply under income effects. The first implication is that TD benefits should have a greater effect on the number of weeks of non-employment than on duration of benefit receipt. The second implication is that increases in benefits should increase the fraction of claimants who fail to return to work immediately when benefits cease.

Two increases in California's TD benefit rate are associated with different labor supply trends. Program participation responds somewhat more strongly to the 2002-2003 benefit increase, while total labor force participation and the entry into uncompensated non-employment respond much more strongly to the 2003-2004 increase. The result is that both implementations evidence of an income effect are stronger for the 2003-2004 increase than the 2002-2003 increase, in which the effects are not statistically significant at conventional levels, and even sometimes of the opposite sign of the predicted effect. This may be due to the fact that other legislative and regulatory reforms caused the total value of benefits to decline for those injured at later dates, raising the share of TD benefits as a share of total benefits, and causing the "income effect" to be larger for later injuries. Another difference between the two benefit increases, that the 2003-2004 increase affected a higher portion of the income distribution, is itself not likely to lead to stronger income effects because individuals with higher incomes tend to have higher levels of net assets and, as shown by Chetty (2008), those with higher levels of assets tend to be less responsive to the Unemployment Insurance benefit rate.

It should be noted that the durations of TD claims decline throughout the time period under consideration, and so the increase in benefits for the treatment group is associated with increases in labor supply for the unaffected wage categories relative to wage categories that experienced a benefit increase. This phenomenon is certainly related to the legislative, regulatory and other responses to the benefit increases under consideration in this study, which substantially reduced the value of WC benefits for later injuries. These other legislative and regulatory changes were not explicitly modeled in this analysis, and readers should note that the conclusions in this paper rely on a strong identification assumption: that these other mechanisms would similarly affect the labor supply measures of individuals in the wage categories affected and unaffected by the two considered changes in TD benefit rates. Of the other contemporaneous and subsequent changes described above, changes to Permanent Disability weekly benefit rates and rating schedule, the end of the Vocational Rehabilitation program and the new medical treatment guidelines are, in my opinion, the most likely to affect labor supply, and changes in these benefits were largely independent of a claimant's wages. A plausible mechanism by which these changes would differentially affect labor supply is through lower income individuals losing more WC benefits as a percent of annual or life-cycle income, which, especially if income effects are large, would cause labor supply to increase disproportionately for lower income claimants. Indeed, the labor supply of the lower income claimants increases. However, if the lowest income respondents are more responsive to these other changes, then the labor supply response for the samples defined by narrow ranges of the earnings distribution should be less responsive to the benefits than those defined by the broader ranges. Returning to Tables 3 and 10, labor supply responses are stronger for the narrower range for the 2002-2003 benefit increase, but slightly weaker for the narrower range for the 2003-2004 increase. Overall, the estimates do not seem especially sensitive to the range of the wage distribution used for estimation, suggesting that other contemporaneous legislative and regulatory changes do not differentially affect the labor supply of those who earned different wages.

While the formal tests for income effects that I propose yield mixed results in the datasets I analyze, the presence of an income effect has a strong policy implication in the context of optimal social insurance theory. When claimants are not credit constrained, substitution effects should dominate the income effects, suggesting that the optimal rate of wage replacement is very low. A conspicuous income effect, however, indicates that claimants are constrained from borrowing against lifetime income to finance post-injury non-employment, justifying a higher rate of wage replacement.

The optimal rate of TD wage replacement is an open question. At the time of this writing, thirty-seven states have a two-thirds rate of wage replacement, and all other states have a replacement rate that is approximately two-thirds. As Burton (2004) and Thomason, Schmidle and Burton (2000) report, the proximate historical reason for this replacement rate is that it was one of the recommendations issued by the National Commission on State Workmen's Compensation Laws in 1972, which states were facing pressure to implement or else be subject to national standards. The recommendation was based upon assumptions about the intent of the WC program, and explicitly eschewed the use of numeric formulas for replacement rate determination. While two-thirds replacement is high in comparison with other wage insurance programs, tests such as the one I propose may be useful to assess whether this replacement rate can be justified as optimal social insurance.

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