THE EFFECT OF WAGE INSURANCE ON LABOR SUPPLY:

A TEST FOR INCOME EFFECTS

by

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Abstract

Studies of moral hazard in wage insurance programs such as Unemployment Insurance (UI) or Workers Compensation (WC) have demonstrated that higher benefits discourage work, emphasizing the price distortion inherent in benefit provision. Utilizing administrative data linking WC claim records to wage records from a UI payroll tax database, I find that the effect of WC benefits on the duration of benefit receipt cannot fully account for the effect of these benefits on post-injury unemployment. This indicates that a significant fraction of the effect of WC benefits on employment is due to an income effect rather than a price distortion.

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Introduction

One of the most widely confirmed results of the empirical literature on wage insurance programs such as Workers Compensation (WC) and Unemployment Insurance (UI) is that when benefits are higher, claimants spend longer receiving benefits. As Chetty (2008) notes, this effect has traditionally been attributed to the fact that wage insurance distorts the relative prices of consumption and non-employment. The alternative explanation, that claimants increase time spent in unemployment due to an expansion of their budget constraint can be discounted because wage insurance benefits are small relative to a claimant's remaining lifetime income. In other words, the effect of wage insurance on employment is due to a "substitution effect" rather than an "income effect."

There is no research on the incentive effects of WC wage insurance, the Temporary Disability (TD) program, which can distinguish between substitution and income effects. Studies such as Krueger (1990), Meyer, Viscusi and Durbin (1995) and Neuhasuer and Raphael (2004) use administrative databases designed to track the payment of benefits, and so these studies measure labor supply changes through a limited measure of unemployment: the duration of the receipt of TD benefits. These studies establish that TD durations are increasing in the benefit rate, but do not measure the extent to which this is caused by a price distortion.

I propose a new test for an income effect of wage replacement by measuring the responsiveness of post-program unemployment to the benefit level. As Card, Chetty and Weber (2007a) note for UI, wage insurance claimants who exhaust their benefits usually do not return to work

immediately upon exiting the program. In this study, I exploit the fact that claimants who exhaust available benefits no longer face a distorted benefit from working. If post-program unemployment responds to the TD benefit level, this is evidence of an income effect.

This test has different data requirements from tests for income effects that have been used to analyze the UI program. Card, Chetty and Weber (2007b) show that workers who receive a larger lump-sum severance payment upon displacement spend longer unemployed, and Chetty (2008) additionally demonstrates that individuals with higher net assets spend longer unemployed. Unlike this previous research, the following wage insurance income effect test does not require data on assets or severance payments: it can be applied whenever there is available data on program participation and total employment.

I implement this new test by appending quarterly wage records from an UI 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. A series of increases to the maximum TD benefit in California provides variation in the TD benefit rate. For each change in benefits, workers in certain wage categories are subject to an increase in benefits, while others' benefit levels are unaffected, forming natural control and treatment groups. Then, I estimate difference-in-differences models in which I compare the labor supply of WC claimants in the control and treatment groups. Estimates of the effect of the benefit level on the duration of benefit receipt capture only a portion of the total effect on labor force participation, indicating that TD benefits induce an income effect.

This study also demonstrates substantial firm and insurer responses to changes in WC benefits. The average duration of TD receipt is stable until the announcement of the legislative reform, after which it immediately declines. The dominant pattern that follows each increase in benefits is an increase in the labor supply of the control group. The most plausible explanation of this pattern is that firms made greater effort to return workers to work and improve their experience rating, and insurers tightened the provision of benefits in order to limit losses, leading to an overall decline in TD durations. This result highlights employer and insurance provider responses to social insurance systems as an important direction for further work.

This paper proceeds as follows. I outline WC insurance, focusing on TD benefits and the market for WC benefits. I then discuss a legislative reform and discuss the test for income effects of TD. I then discuss the data, present the trends in the variables under consideration and present detailed estimation results. A brief conclusion follows.

Workers Compensation Insurance

Each year, according to the U.S. Bureau of Labor Statistics (2007), one in twenty workers employed in the United States is injured at work. In order to ensure that workers receive timely and appropriate care and compensation for these injuries, nearly all states require firms to provide WC insurance for all employees. Institutions that provide WC insurance have a legal requirement to provide a minimum level of benefits for workers who are injured at work. These benefits include health care, employment and training services, and cash transfers. Cash transfers include TD benefits, which compensate time lost from work, Permanent Disability (PD)

benefits, which compensate injuries from which the claimant will never fully recover, and Vocational Rehabilitation (VR) benefits, which provide employment services.

Temporary Disability

When an injured worker claims these benefits, the physician makes a formal diagnosis and formulates a detailed plan for treating the injury. Absence from work is an explicit part of a program of recovery for more severe injuries. In most cases, treating physicians, in consultation with the injured worker, employer and insurer make an assessment of the scope for injured workers to improve their health status and avoid re-injury by not working.

The injured worker can receive compensation for time lost from work while recovering, a TD benefit. After a waiting period of one day to one week, depending on the jurisdiction, the injured worker receives bi-weekly checks that replace a fraction of his income from time lost from work. The amount of these checks is typically two-thirds of the injured worker's usual weekly wage on the date of injury, subject to a minimum and a maximum. Periodically, the injured worker returns to a physician for re-evaluation. If the physician determines that the scope for improvement has been exhausted, benefits cease even if the injured worker does not return to work. The injured worker normally may choose to return to work regardless of qualification for TD, in which case the benefit is suspended.

Insurance Market

Although a small number of states act as the sole provider of WC insurance, most states have a competitive market that may include a competitive insurer owned by the state government. Many states give firms the option of acting as their own insurance provider. The state sets the level of benefits for WC insurance and, in states that do not have a monopoly, insurers compete by managing costs such as medical treatments and avoiding excess benefits for time lost from work. The level of benefits mandated by state legislatures is the primary factor determining the cost of WC insurance. Costs vary widely among states: in a low cost state like Massachusetts, WC benefits disbursed amount to less than one-half of one percent of payroll; in a high cost state like California, disbursed benefits amount to more than two percent of payroll.[2] Insurers do not compete on the basis of the WC benefits they provide. The overall market for disability insurance is dominated by government-mandated benefit programs, with payments for non-mandated disability insurance making up less than ten billion dollars of more than one hundred billion dollars of disbursements.[3]

The insurer determines the price for WC insurance for a particular firm based on the expected costs of providing the government-mandated benefits. Two factors are considered: the fraction of the firm's workers that are in occupations with a high risk of workplace injury, and the history of

[2] Sengupta, Reno and Burton (2008).

[3] Author's calculation for 2005. The Social Security Administration distributed more than $70 billion in disability insurance, workers compensation programs disbursed $25 billion. Five states and one commonwealth disburse several billion dollars in mandated non-occupational TD benefits. See the California Employment Development Department (2008), the Council for Disability Awareness (2008), and Sengupta, Reno and Burton (2008).

the firm's WC benefits paid. Overall, firms pay approximately 1.6% of payroll[4] to finance a national total of $51 billion in WC benefits.[5] These costs can vary substantially between different industries, reaching as high as 40% of payroll for some occupations in some states. As indicated in Donovan and Neuhauser (2007), firms sometimes under-report the number of workers in high-risk occupations to lower their insurance premiums.

Legislative Reform

Changes to Temporary Disability Weekly Rates

In 2002, the California legislature increased the maximum weekly benefit for TD and instituted a minimum weekly benefit in the context of a larger period of reform of labor market regulations. I consider two changes to the maximum: an increase of 23% on January 1, 2003 and a second increase of 21% on January 1, 2004. Figure 1 shows the changes in maximum and minimum weekly TD benefits for workers injured before and after January 1, 2003, when the first change in benefits was implemented. Benefits for workers injured from 1996 to 2002 equaled the weekly wage for wages up to $126 per week; injured workers earning between $126 and $189 per week received $126 per week. Those earning in excess of $189 per week received two-thirds of their weekly wages, and all TD claimants earning $735 or more per week received the maximum benefit of $490. However, workers injured on or after January 1, 2003, who earned below $189 per week received $126, and those who earned between $189 and $903 received

[4] U.S. Bureau of Labor Statistics (2008a).

[5] Sengupta, Reno and Burton (2008).

two-thirds of their weekly wage as their TD benefit. Injured workers who earned in excess of $903 received the maximum benefit of $602. Note that the benefits for workers earning between $126 and $735 weekly were not affected by the change in benefit schedules.

The legislation provided for an additional change in maximum TD benefits for workers injured on or after January 1, 2004, as shown in Figure 2. Workers 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 workers earning more than $903 per week were affected by this change. Overall, the legislative reform overall increased benefits received by 35% of claimants and reduced the fraction of those receiving the maximum benefit from 30% in 2002 to 13% in 2004.

Other Legislative Changes

WC reform occurred in the context of other institutional reforms which dramatically affected costs faced by firms and insurers. California entered a period of overall reform of its labor market regulations in 2000, with a revision to its schedule for UI benefits and an increase in its minimum wage. From 2002 to 2004, the California legislature passed a series of laws concerning the provision of WC benefits. It increased compensation for PD in a manner that was uniform for most earnings levels. No worker injured on or after January 1, 2004, received VR benefits, and workers injured on or after January 1, 2003, were permitted to receive a cash settlement for the value of these benefits. Because the VR program had the mechanical effect of keeping workers out of the formal labor market, the program's conclusion resulted in an increase

in labor force participation. A maximum of two years of receipt of TD benefits was instituted for nearly all types of injuries for workers injured on or after April 19, 2004. All WC claims for which PD qualification had not been established by January 1, 2005, were subject to a new, more restrictive evaluation method that reduced the amount of the cash transfer that PD claimants were eligible to receive. In 2004, several legislative and regulatory changes limited the scope of the primary treating physician to determine the treatment of the injured workers.

Theoretical Framework

This paper considers the problem of how an injured worker weighs the wages lost through non-employment against the recovery or leisure value of continued non-employment. This problem shares many of the essential features of the UI problem studied by Meyer (1990). A simple model illustrates how measuring both program participation and total labor supply permits identification of an income effect.

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

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

The worker maximizes utility $u(c,d)$ subject to this constraint. This utility maximization problem is shown in Figure 3. Note that this budget constraint has two different slopes: in the region in which the worker's TD duration is less than θ, the marginal effect of continued non-employment on income is $b - w$, and after θ it is $-w$.

Consider the impact of an increase in the benefit rate b for a worker who is initially not exhausting benefits, that is, $d < \theta$ and return to work is immediate. A corresponding initial decision is shown in Figure 3 as point A^1. Suppose that after the reform the worker would not exhaust all available 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, making him wealthier. The second is a substitution effect: the increase in the benefits affects the relative price of consumption and non-employment.

However, if an injured worker initially maximizes utility by exhausting TD benefits and taking additional time off of work $d > \theta$, then there is only 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 for this worker. In Figure 3, the budget constraint change shifts his decision from point B^1 to point B^2.

If a claimant's increase in duration of non-employment as a response to a change in the benefit level is much larger overall than that of the duration of benefit receipt alone, this indicates, in the

context of the model, an income effect: the marginal benefit of work being unchanged, claimants are choosing to self-finance uncompensated non-employment.

The Data

Sources

I implement the test for income effects using data from administrative databases owned by the State of California. I consider claimants in the State's WC tracking database, the Workers' Compensation Information System (WCIS), a database constructed according to the standards set by the International Association of Accident Boards and Commissions (IAIABC), Release 1 (2002). All institutions that provide WC benefits, both external insurance providers and self-insured employers, are required to report every workplace injury with the potential to be the basis for a WC claim. The initial report specifies the type of injury that occurred, including its cause and the part of body affected. It also includes the injured worker's demographic characteristics, including age, gender and place of residence; and information about the injured worker's employment, including the at-injury employer, the injured worker's wage on the date of injury, and the date of hire by the at-injury firm. The initial report also contains the insurance classification code applicable to the injured worker, as specified by the California Workers' Compensation Insurance Rating Bureau (WCIRB). This is an occupation category that private insurers are required to use to price WC insurance. Subsequent reports include detailed information on the date and amount of benefit payments.

These workplace injury records are supplemented with data from the California Employment Development Department's (EDD) Base Wage File (BWF), a database that records the quarterly earnings of workers in California covered by UI or state disability insurance, which, according to the Applied Research Unit (2002) of the Labor Market Information Division of EDD, covers approximately 97% of the workforce. This database contains the employee's quarterly earnings, and identifying information, which is reported by employers at the end of each quarter. On the basis of each employee's last name, I append to each record of the BWF measures that indicate 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. The BWF earnings and implied race and ethnicity are appended to a WCIS record 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.

Dependent Variables

From these databases, I construct three labor supply measures. These measures are the duration of the TD benefit period, total earnings in quarters that include and follow the workplace injury, and an earnings-weighted sum of quarters including or following the quarter of injury in which an injured worker participates in the labor market. Due to data constraints, I consider a time horizon of 11 quarters: the quarter of injury and ten subsequent quarters.

The benefit duration measure is defined as follows. I define a time interval for the receipt of benefits based on the periodic updates provided by insurers to the WCIS. 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:

$$Duration = \min\left\{\frac{EndDate - StartDate + 1}{7}, 143\right\}.$$

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

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

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

Because the earnings measure does not directly capture 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 ten quarters that follow the workplace injury, I count as a full quarter worked any quarter including or following the injury in which earnings are at least equal to earnings in the quarter before 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,

$$Weeks\ LFP = \sum_{t=0}^{10}\left[1\{E_{-1} \le E_t\} + 1\{E_{-1} > E_t\}\frac{E_t}{E_{-1}}\right] * 13,$$

where E_t are earnings t quarters after the workplace injury, and E_{-1} are earnings in the quarter immediately preceding injury.

Other Variables

The primary independent variables are constructed using WCIS data on the date of injury and claimant weekly earnings. An indicator variable specifies whether the claimant was injured before or after the new benefit rate was implemented. Another specifies whether or not the claimant was in an affected wage category. I also consider the interaction of these two variables, which indicates being in the affected wage category after the new benefit rate took effect.

To account for observable heterogeneity in my estimation of labor supply, 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 WCIS. I include WCIS indicator variables for the WCIRB occupation classification code, the cause of injury, the nature of injury, the part of body, as well as the California 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.

Sample Selection

I consider the labor market experience of TD claimants injured from 2001 to 2004 for the entire WCIS database, excluding seven large insurers that top-coded earnings. Only WCIS claimants with wage information from the BWF are considered in this analysis. I do not consider injuries classified as either cumulative or due to an occupational disease, or had no physical injury, as defined by the IAIABC (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 WCIRB classification codes. I further restrict the sample on the basis of the weekly wage, eliminating wages that are below $75 per week and above $1275 per week. This eliminates approximately the lowest 2.5% of observations and highest 5% of observations ranked by weekly wage. To ensure that the earnings adjusted quarterly labor supply has an accurate baseline earnings parameter, I only include workers who the BWF records as working in the quarter of and the two quarters immediately preceding the quarter of injury. Labor supply responses are then estimated on the resulting sample of 330,894 observations.

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 stipulated in the legislative reform. Specifically, I estimate difference in differences models of the form

$$\log(LS) = \gamma_0 + \gamma_A * After + \gamma_T * Treatment + \gamma_{AT} * After * Treatment + X\beta,$$

where *LS* is the labor supply dependent variable under consideration, *After* is an indicator variable that is equal to one if the observation is in the period after the increase in weekly rates and zero otherwise, *Treatment* is an indicator variable that is equal to one if the observation has a weekly wage that is affected by the legislative reform and zero otherwise, and *X* is a matrix of control variables. The marginal effects are, respectively, γ_A, γ_T and β. γ_0 is the intercept. The parameter of interest in the measurement of labor supply responses to benefits is the effect of the interaction between *After* and *Treatment*, γ_{AT}. This is the effect on the worker's labor supply of having benefits raised by this particular reform. I calculate robust standard errors clustered on the month in which the injury occurs.

I define control and treatment groups and measure the difference in labor supply between the year prior to and the year of each reform. For the 2002-2003 reform, workers are included in the treatment group when they earn less than or $126 or more than or $735 weekly. For the 2003-2004 reform, those earning or in excess of $903 weekly are included in the treatment group. To increase the precision of my estimates, I estimate narrower ranges of the wage distribution. I consider workers who earned from $500 to $1100 when considering the change in the maximum benefit from 2002 to 2003. Likewise, I consider a narrower range around the maximum, $900 to $1275, when estimating the effect of the change in weekly benefits between 2003 and 2004.

Results

Descriptive Statistics

I begin by considering the sample sizes and population means of the broad and narrow samples in each of the reforms 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. This is consistent with the decrease in the incidence rate of lost-time injuries in California that the U.S. Bureau of Labor Statistics (2008b) reports for this period. Overall, claimants in affected wage categories experienced substantial increases in their scheduled benefits defined as a function of the weekly wage: the average weekly benefit increased by 16.9% from 2002 to 2003, and by 14.7% from 2003 to 2004.

Durations decline substantially from 2002 to 2004; however, the average decline in the duration of benefit receipt is greater in magnitude for the control group compared to the treatment group in each wage category. Labor supply as measured by total earnings and the sum of adjusted quarters worked also indicate a greater rise in labor force participation in the control groups relative to the respective treatment groups.

Time Trends

Quarterly time trends indicate persistent shifts in average labor force participation measures immediately following legislative reforms. Figures 4-6 show quarterly time trends for the

control and treatment groups in the narrow samples defined for the 2002 to 2003 reform period. Forecast values for each measure are calculated using parameter estimates from a regression of pre-reform labor supply on all control variables and applied to all injuries from 2001 to 2004. The sample for these parameter estimates includes all injured during 2001 and 2002.

After the first quarter of 2002, later injury is associated with a shorter log duration, as shown in Figure 4. The average log duration for the control group is higher than or approximately equal to the treatment group until January 1, 2003, when average log duration is higher for the treatment group. Figures 5 and 6 plot the time trends of the post-injury log earnings and log weeks of labor force participation. These two measures are stable until the first quarter of 2003, when the labor supply increases for the control group but not the treatment group. Forecast lines indicate no change in labor supply based on observable characteristics. These trends are consistent with higher benefits discouraging work. They are surprising in that they appear to indicate a strong response to the legislative reform for the control group but not for the treatment group. Similar labor supply responses for control and treatment groups follow the second reform.

Insurer and Firm Responses

That the changes in benefit levels affect labor supply for the control group rather than the treatment group may be explained by the response of insurers and firms to the legislative reform. 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). In Figure 4, the duration of benefit

receipt is stable until the first quarter of 2002, after which it declines. In that quarter, on February 15, 2002, the reform was enacted despite opposition from the California Chamber of Commerce. On May 2, 2002, The WCIRB, which sets the statutory baseline insurance rates, increased rates by 10% in a special revision in response to the legislative reform. Over the next two months, insurers submitted to the Department of Insurance (2008) the new premiums that they would charge firms in new or renewed contracts, and the overwhelming majority of these were increases of 10%. Due to the nature of the market for WC insurance, insurers were unable to pass on the full costs of this reform to firms until mid-year 2003. Both firms and insurers faced additional costs as a result of this reform, and most likely made changes to their practices to manage costs, which may have included faster return-to-work of employees as a way of reducing a firm's cost history, as well as more frequent and rigorous monitoring WC claimant continued scope for medical improvement through time off work.

Publicly, employers and insurers advocated legislative reform that would lower program costs. A series of legislation and regulatory reforms limited the discretion of physicians, therapists and counselors in efforts to increase the efficiency of the provision of benefits. A decline in the duration of benefit receipt resulted, and WC premiums consequently were lower in 2004.

Difference-in-Differences Estimates

I next consider the effect of experiencing a benefit increase on the three different labor supply measures. Specifically, I calculate parameter estimates for the variable that indicates being in the treatment group after the reform from the log-linear difference-in-differences estimation model

described above, and list the results in Table 2. Estimates are calculated with and without inclusion of the control variables listed above. The parameter estimates of the effect of the treatment are usually positive and significant at the 1% level after regression adjustment. The treatment increased the duration of TD benefit receipt by 10% to 14% between 2002 and 2003 and 8% to 10% between 2003 and 2004. Regressions of earnings and the weeks of labor force participation indicate that labor supply in the treatment group declined relative to the control group. Post-injury earnings decline 5% and weeks by 3% between 2002 and 2003; between 2003 and 2004 earnings decline by 5% to 10% and weeks by 3% to 6%.

To compare these effects with other studies of the effect of WC insurance on labor supply, I calculate elasticity estimates by dividing 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 shown in Table 1, and list these in Table 3. The elasticity of duration with respect to the benefit level is 0.4 to 0.9. This range is somewhat higher than other estimates: Meyer, Viscusi and Durbin (1995) and Neuhauser and Raphael (2004) report elasticity estimates of approximately 0.27 to 0.62 and 0.25 to 0.9, respectively. Estimates measuring labor supply using quarterly earnings reported in Table 3 also demonstrates large responses: the elasticity of earnings with respect to benefits is -0.3 to -0.6, and the elasticity of weeks is -0.1 to -0.4.

Models of treatment defined as being in either affected wage category from 2001 to 2002 indicate that labor supply trends did not diverge between these two years, as shown in Table 4. This falsification test provides strong evidence that higher TD benefits discourage work.

Labor Supply Response Calculations

I next utilize the previous results to test for income effects of wage insurance. I calculate the number of weeks that TD claimants spend in non-employment without TD benefits, or "other non-employment" for brevity, 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 post-injury period employed. Of the remainder, slightly more is spent receiving TD than not.

I then calculate the effect of the change in TD benefits on other non-employment implied by the previous difference in differences models. I multiply the parameter estimates for the effect of the reform on TD benefit receipt by the number of weeks the treatment group spends receiving TD benefits, and, similarly, the effect of the reform on employment by the number of weeks LFP. The effect of the change in TD benefits on other non-employment is derived directly from the identity that the effect of the reform on weeks of TD receipt, weeks other non-employment, and weeks LFP must sum to zero.

The results of this exercise are listed in Tables 6 and 7. 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 decreased, all other estimates show weeks of uncompensated non-employment increase. The magnitude of the change in other non-employment is larger when measuring the effect on labor supply using earnings rather than weeks LFP. This may result from different levels of attenuation bias: the estimated number of

weeks LFP is a coarser measure than total earnings. These calculations indicate 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 lost from work after injury, providing evidence of an income effect of WC on labor supply.

Benefit Expiration and Labor Market Re-Entry

In addition to a calculation-based assessment of the relative magnitude of the responses of duration and total labor supply responses to WC benefits, I estimate a second model that provides direct evidence of an income effect. 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. The lack of earnings in the quarter in which benefits expire indicates that a worker fails to return to work immediately, and enters a period of uncompensated non-employment. 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 high wage earners have no such wages reported.

I estimate difference-in-differences linear probability models of the effect of being in the treatment group on the probability of having zero earnings in the quarter that TD benefits expire. The results of this estimation are presented in Table 9. Identification using the increase in benefits on January 1, 2003 suggests a 0.5% to 1% decrease in return to work. The increase on January 1, 2004, suggests a 1.5% to 2% decrease in return to work, and the estimates are more clearly different from zero. Calculated elasticity estimates are listed in Table 10 for each of the two reforms, and the ranges are 0.2 to 0.4 in the former year and 0.7 to 1 in the latter. A

falsification test on injuries that occurred between 2001 and 2002, presented in Table 11, suggests that the probability of returning to work is not caused by the wage categories alone. These estimates demonstrate that an increase in benefits decreases the probability of returning to work immediately, providing evidence of an income effect.

Conclusions

In this study, I analyze a legislative reform and use newly assembled data to demonstrate the existence of an income effect from the receipt of WC TD benefits. A standard model of social insurance suggests two implications of any income effect. The first is a greater effect of TD benefits on the number of weeks of non-employment than on duration of benefit receipt. The second is an effect of benefits on the likelihood that workers return to work immediately when benefits cease. Estimating difference-in-differences models, I show that both consequences exist, suggesting some income effects from the presence of TD.

The presence of an income effect has a strong policy implication in the context of optimal social insurance theory. In a simple static wage insurance model in which workers are not credit constrained, the substitution effect dominates the income effect, suggesting that the optimal rate of wage replacement is very low. A conspicuous income effect, however, indicates that workers are constrained from borrowing against lifetime income to finance post-injury non-employment, justifying a higher rate of wage replacement.

The optimal rate of WC 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 Thomson, 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 compared to other wage insurance programs, this study provides an initial indication that some of this high wage replacement rate may be justified as pure wage insurance.

This study also highlights that higher WC benefits induce responses not only from workers, but also firms and insurers. The decline in duration of WC benefit receipt immediately following the announcement of legislative reforms indicates that insurers and firms took steps to control the costs of the mandated increase in benefits. Surprisingly, the immediate consequence of an increase in benefits for the treatment group is an increase in labor supply for the control group. These results imply that the higher weekly rate of wage replacement for certain workers was financed in part by firm and insurer measures to control costs.

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

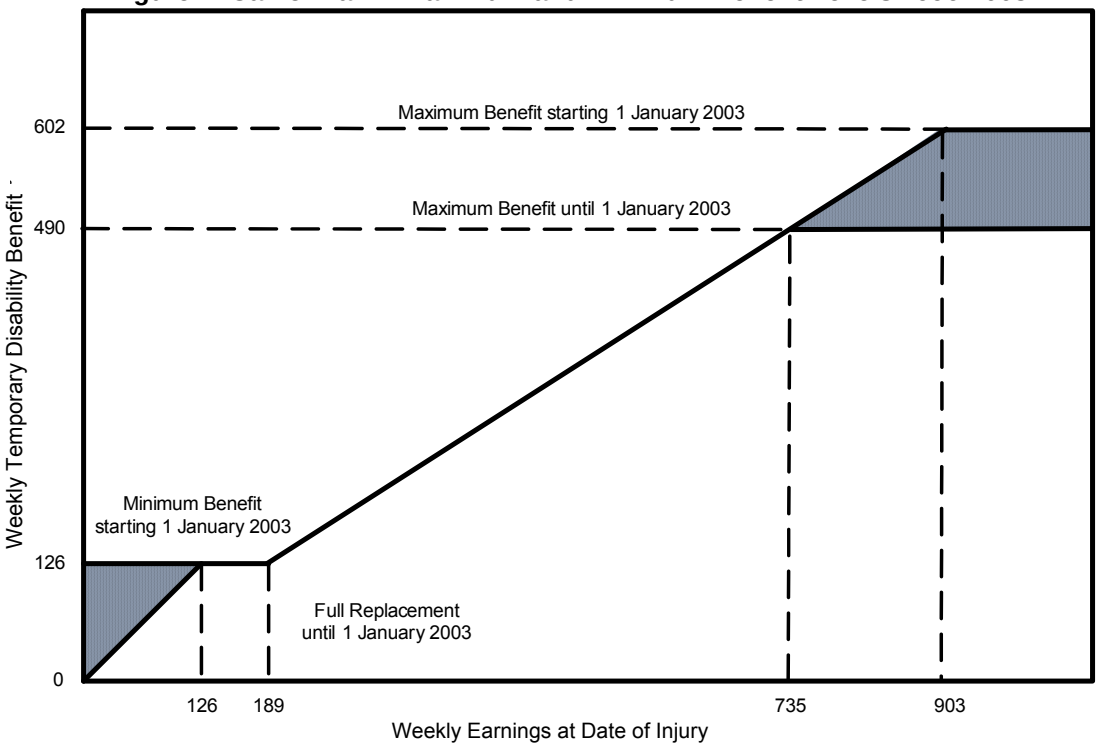


Figure 2: California TD Maximum and Minimum Benefit Levels 2003-2004

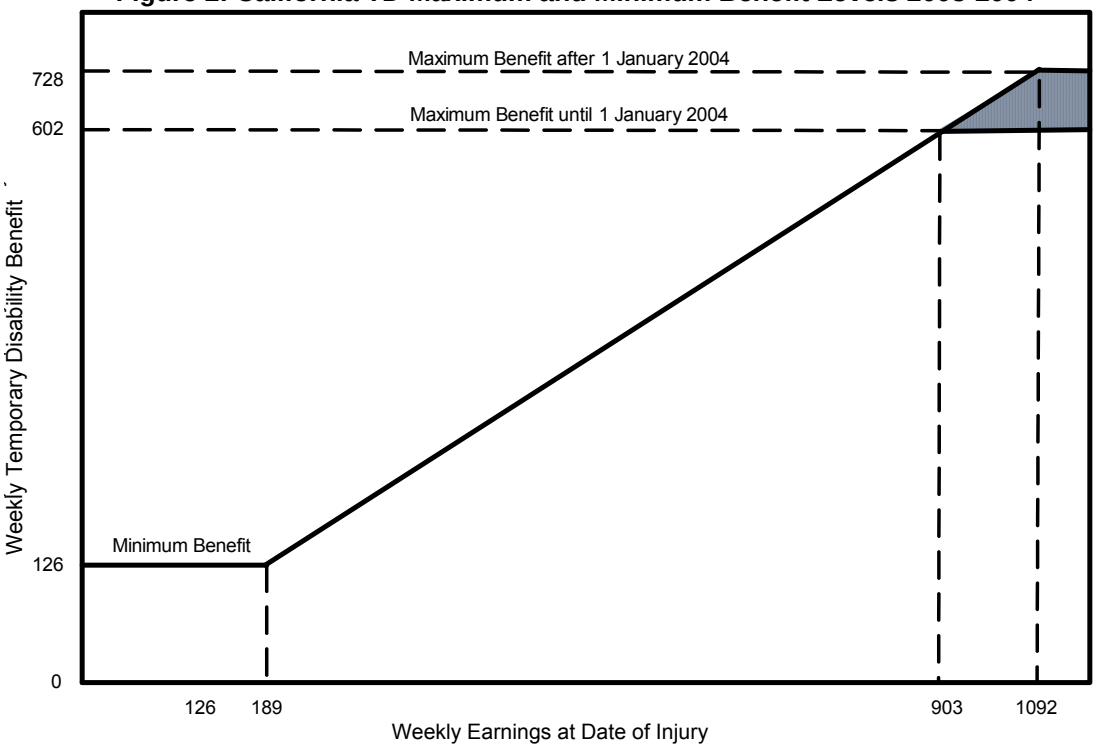


Figure 3: Injured Worker Maximization Problem when Benefits Increase

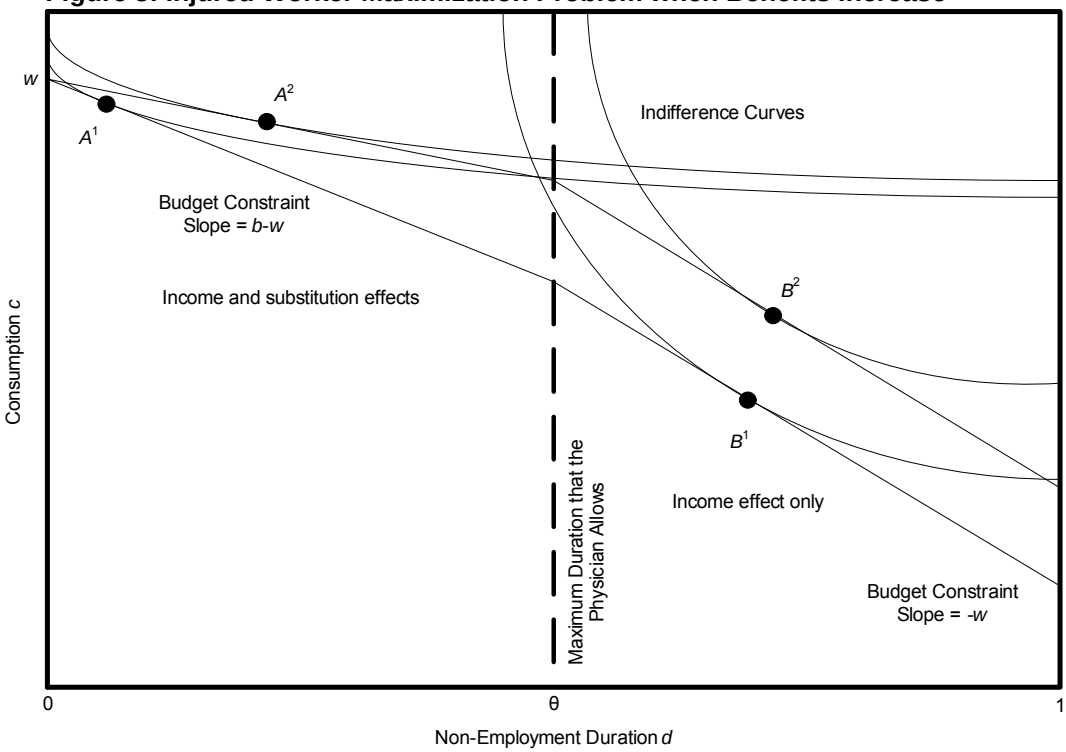
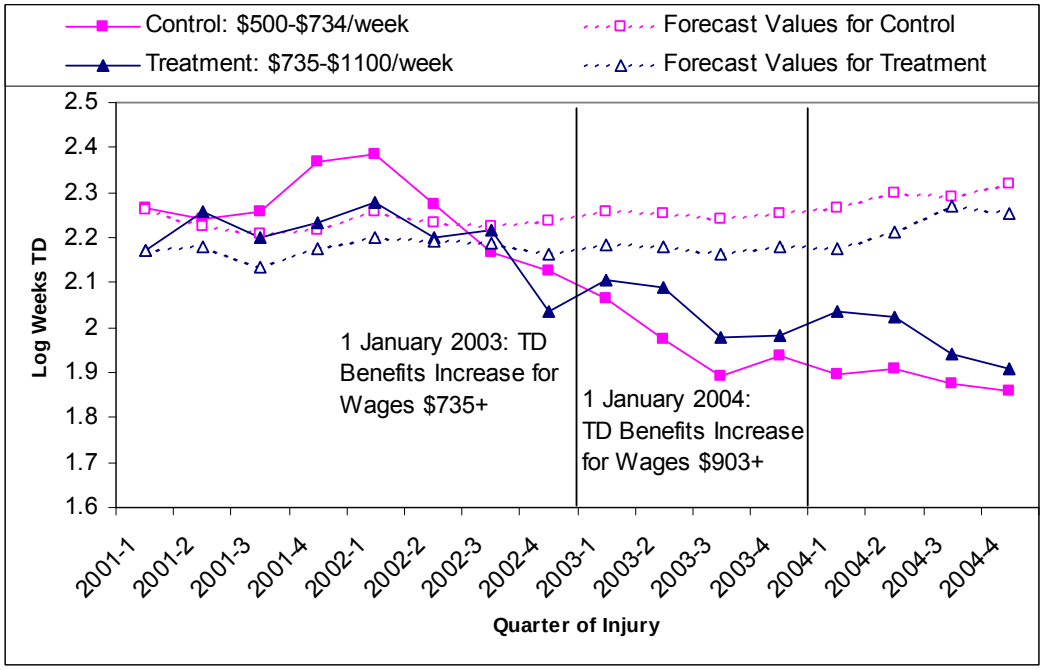
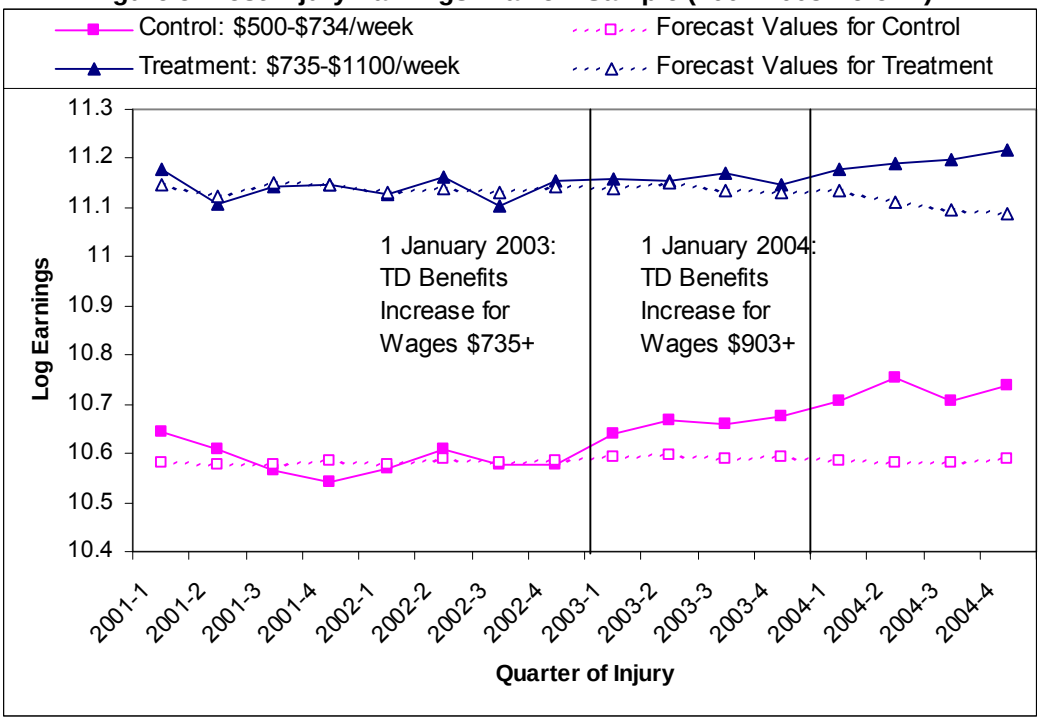


Figure 4: TD Duration: Narrow Sample (2002-2003 Reform)



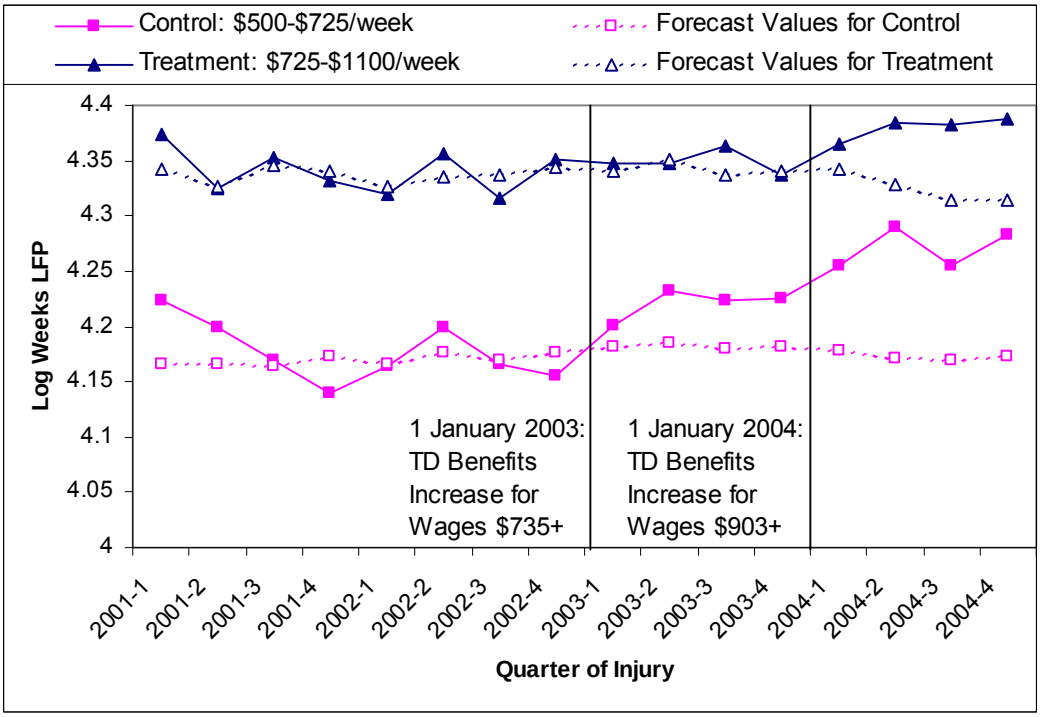
Note: The quarterly average of log duration of TD benefits payments, top-coded at 143 weeks. Fitted values are calculated using parameter estimates from regressions of log TD duration on all observable characteristics, for claimants injured in 2001 and 2002, separately by wage category.

Figure 5: Post-Injury Earnings: Narrow Sample (2002-2003 Reform)



Note: The quarterly average of log total labor market earnings in the quarter of injury and the ten quarters that follow. Fitted values are calculated using parameter estimates from regressions of log earnings on all observable characteristics, for claimants injured in 2001 and 2002, separately by wage category.

Figure 6: Post-Injury Weeks: Narrow Sample (2002-2003 Reform)



Note: The quarterly average log weeks LFP in the quarter of injury and the ten quarters that follow. See text for formula. Fitted values are calculated using parameter estimates from regressions of log sum of adjusted quarters on all observable characteristics, for claimants injured in 2001 and 2002, separately by wage category.

Table 1: Descriptive Statistics

	2002-2003 Reform				2003-2004 Reform			
	Broad Sample $75-$1,275/week		Narrow Sample $500-$1,100/week		Broad Sample $75-$1,275/week		Narrow Sample $700-$1,275/week	
	Controls $130-734	Treatments $75-126 & $735-1275	Controls $500-734	Treatments $735-1100	Controls $75-902	Treatments $903-1275	Controls $700-902	Treatments $903-1275
Sample Size Base Year	61,210	24,786	22,566	20,175	70,381	13,649	13,256	13,649
Sample Size End Year	59,183	24,847	22,840	19,419	59,998	12,018	11,830	12,018
Mean Benefit Rate Base Year	295.9	479.1	405.8	490.0	333.0	602.0	527.5	602.0
	(0.4)	(0.4)	(0.3)	(0.0)	(0.5)	(0.0)	(0.3)	(0.0)
Mean Benefit Rate End Year	299.6	560.3	405.9	569.9	334.4	690.3	526.5	690.3
	(0.4)	(0.6)	(0.3)	(0.3)	(0.5)	(0.4)	(0.4)	(0.4)
Percent Change	*1.2*	*16.9*	*0.0*	*16.3*	*0.4*	*14.7*	*-0.2*	*14.7*
	(0.2)	*(0.2)*	*(0.1)*	*(0.1)*	*(0.2)*	*(0.1)*	*(0.1)*	*(0.1)*
Mean Weeks TD Base Year	37.2	33.6	37.6	34.0	32.9	29.8	33.0	29.8
	(0.2)	(0.3)	(0.3)	(0.3)	(0.2)	(0.4)	(0.4)	(0.4)
Mean Weeks TD End Year	32.9	31.2	32.4	31.7	27.6	26.9	28.0	26.9
	(0.2)	(0.3)	(0.3)	(0.3)	(0.2)	(0.3)	(0.3)	(0.3)
Percent Change	*-11.6*	*-7.0*	*-13.6*	*-6.6*	*-16.0*	*-9.7*	*-15.3*	*-9.7*
	(0.6)	*(1.1)*	*(1.0)*	*(1.3)*	*(0.6)*	*(1.6)*	*(1.4)*	*(1.6)*
Mean Earnings Base Year	41319	98657	58319	94887	49807	118760	81889	118760
	144	391	266	403	159	563	431	563
Mean Earnings End Year	44025	101458	61817	96492	53458	119110	86411	119110
	154	399	274	414	178	604	462	604
Percent Change	*6.5*	*2.8*	*6.0*	*1.7*	*7.3*	*0.3*	*5.5*	*0.3*
	(0.5)	*(0.6)*	*(0.7)*	*(0.6)*	*(0.5)*	*(0.7)*	*(0.8)*	*(0.7)*
Mean Weeks LFP Base Year	77.6	94.8	85.8	94.6	81.7	99.2	92.8	99.2
	(0.2)	(0.3)	(0.3)	(0.3)	(0.2)	(0.4)	(0.4)	(0.4)
Mean Weeks LFP End Year	79.8	95.9	88.4	95.6	84.9	99.5	95.4	99.5
	(0.2)	(0.3)	(0.3)	(0.3)	(0.2)	(0.4)	(0.4)	(0.4)
Percent Change	*2.8*	*1.2*	*3.0*	*1.1*	*3.9*	*0.3*	*2.8*	*0.3*
	(0.4)	*(0.4)*	*(0.5)*	*(0.5)*	*(0.3)*	*(0.5)*	*(0.6)*	*(0.5)*

Note: Standard errors in parentheses.

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

	2002-2003 Reform				2003-2004 Reform			
	Broad Sample $75-$1,275/week		Narrow Sample $500-$1,100/week		Broad Sample $75-$1,275/week		Narrow Sample $700-$1,275/week	
Dependent Variable	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted
Log Weeks TD	0.100 ***	0.118 ***	0.126 ***	0.144 ***	0.078 **	0.105 ***	0.046	0.079 **
	(0.028)	(0.029)	(0.035)	(0.034)	(0.030)	(0.028)	(0.038)	(0.036)
Log Earnings	-0.048 ***	-0.050 ***	-0.056 ***	-0.051 ***	-0.093 ***	-0.073 ***	-0.065 ***	-0.045 ***
	(0.017)	(0.016)	(0.016)	(0.014)	(0.019)	(0.018)	(0.015)	(0.014)
Log Weeks LFP	-0.024 *	-0.028 **	-0.035 **	-0.032 **	-0.056 ***	-0.046 ***	-0.040 ***	-0.030 **
	(0.014)	(0.014)	(0.014)	(0.013)	(0.014)	(0.013)	(0.012)	(0.011)

Note: 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 3: Elasticity Estimates (Calculation)

	2002-2003 Reform				2003-2004 Reform			
	Broad Sample $75-$1,275/week		Narrow Sample $500-$1,100/week		Broad Sample $75-$1,275/week		Narrow Sample $700-$1,275/week	
Dependent Variable	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted
Log Weeks TD	0.639 ***	0.751 ***	0.771 ***	0.882 ***	0.546 ***	0.741 ***	0.311	0.532 **
	(0.181)	(0.182)	(0.216)	(0.209)	(0.211)	(0.198)	(0.254)	(0.244)
Log Earnings	-0.286 ***	-0.283 ***	-0.333 ***	-0.328 ***	-0.623 ***	-0.587 ***	-0.419 ***	-0.389 ***
	(0.100)	(0.089)	(0.098)	(0.089)	(0.128)	(0.145)	(0.094)	(0.116)
Log Weeks LFP	-0.149 *	-0.151 **	-0.210 **	-0.179 **	-0.386 ***	-0.332 ***	-0.257 ***	-0.233 ***
	(0.088)	(0.073)	(0.084)	(0.070)	(0.097)	(0.095)	(0.076)	(0.090)

Note: Standard errors in parentheses. Calculations by divide the parameter estimate from the log-linear regression in Table 2 by the net increase in the weekly benefit rate from Table 1. *, ** 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 Wage Categories				2003-2004 Wage Categories			
	Broad Sample $75-$1,275/week		Narrow Sample $500-$1,100/week		Broad Sample $75-$1,275/week		Narrow Sample $700-$1,275/week	
	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted
Log Weeks TD	-0.006	-0.013	0.013	0.008	0.018	0.012	0.039	0.042
	(0.025)	(0.025)	(0.033)	(0.032)	(0.023)	(0.026)	(0.029)	(0.036)
Log Earnings	0.013	0.005	0.000	0.006	0.004	-0.002	0.024	0.015
	(0.018)	(0.016)	(0.019)	(0.017)	(0.022)	(0.020)	(0.021)	(0.020)
Log Weeks LFP	0.012	0.012	0.001	0.008	0.015	0.013	0.016	0.011
	(0.016)	(0.015)	(0.016)	(0.014)	(0.020)	(0.019)	(0.017)	(0.017)

Note: 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: Three Durations: LFP, Compensated by TD, and Other Non-Employment, in Weeks

	2002-2003 Reform				2003-2004 Reform			
	Broad Sample		Narrow Sample		Broad Sample		Narrow Sample	
	$75-$1,275/week		$500-$1,100/week		$75-$1,275/week		$700-$1,275/week	
	Controls	Treatments	Controls	Treatments	Controls	Treatments	Controls	Treatments
	$130-734	$75-126 & $735-1275	$500-734	$735-1100	$75-902	$903-1275	$700-902	$903-1275
Weeks LFP	78.7	95.3	87.1	95.1	83.2	99.3	94.0	99.3
	(0.2)	(0.3)	(0.3)	(0.3)	(0.2)	(0.4)	(0.4)	(0.4)
TD Benefit Receipt	35.1	32.4	35.0	32.9	30.5	28.5	30.6	28.5
	(0.2)	(0.3)	(0.3)	(0.3)	(0.2)	(0.3)	(0.4)	(0.3)
Other Non-Employment	29.2	15.3	20.9	15.0	29.3	15.2	18.3	15.2
	(0.3)	(0.4)	(0.4)	(0.4)	(0.2)	(0.5)	(0.5)	(0.5)

Note: Standard errors in parentheses. Other non-employment is defined as 143 minus LFP and TD Benefit Receipt.

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

	2002-2003 Reform				2003-2004 Reform			
	Broad Sample		Narrow Sample		Broad Sample		Narrow Sample	
	$75-$1,275/week		$500-$1,100/week		$75-$1,275/week		$700-$1,275/week	
Dependent Variable	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted
Weeks LFP	-4.604 ***	-4.784 ***	-5.298 ***	-4.883 ***	-9.273 ***	-7.261 ***	-6.438 ***	-4.497 ***
	(1.606)	(1.504)	(1.557)	(1.330)	(1.898)	(1.793)	(1.449)	(1.342)
Weeks TD	3.250 ***	3.821 ***	4.127 ***	4.720 ***	2.215 ***	3.002 ***	1.315	2.250 **
	(0.923)	(0.924)	(1.157)	(1.119)	(0.856)	(0.803)	(1.072)	(1.033)
Other Non-Employment	1.353	0.963	1.171	0.163	7.058 ***	4.259 **	5.123 ***	2.247
	(1.853)	(1.765)	(1.940)	(1.738)	(2.083)	(1.964)	(1.803)	(1.694)

Note: Standard errors in parentheses. The change in weeks LFP is the point estimates for models that use earnings as a dependent variable as reported in Table 2 multiplied by the average duration of weeks employment for the treatment group from Table 5. The change in weeks of TD benefit receipt is the point estimate from regressions that use the duration of TD benefit receipt as a dependent variable as reported in Table 2 multiplied by the average duration TD for the treatment group from Table 1. The chage 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.

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

	2002-2003 Reform				2003-2004 Reform			
	Broad Sample		Narrow Sample		Broad Sample		Narrow Sample	
	$75-$1,275/week		$500-$1,100/week		$75-$1,275/week		$700-$1,275/week	
Dependent Variable	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted
Weeks LFP	-2.320 *	-2.666 **	-3.282 **	-3.087 **	-5.552 ***	-4.580 ***	-3.942 ***	-2.951 **
	(1.364)	(1.297)	(1.315)	(1.205)	(1.403)	(1.308)	(1.173)	(1.139)
Weeks TD	3.250 ***	3.821 ***	4.127 ***	4.720 ***	2.215 ***	3.002 ***	1.315	2.250 **
	(0.923)	(0.924)	(1.157)	(1.119)	(0.856)	(0.803)	(1.072)	(1.033)
Other Non-Employment	-0.931	-1.155	-0.845	-1.633	3.337 **	1.578	2.626 *	-0.700
	(1.647)	(1.592)	(1.751)	(1.644)	(1.644)	(1.535)	(1.589)	(1.537)

Note: Standard errors in parentheses. The change in weeks LFP is the point estimates for models using weeks LFP as a dependent variable as reported in Table 2 multiplied by the average duration of weeks employment for the treatment group from Table 5. The change in weeks of TD benefit receipt is the point estimate from regressions that use the duration of TD benefit receipt as a dependent variable as reported in Table 2 multiplied by the average duration TD for the treatment group from Table 1. The chage 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.

Table 8: Not Working When Benefits End

	2002-2003 Reform				2003-2004 Reform			
	Broad Sample $75-$1,275/week		Narrow Sample $500-$1,100/week		Broad Sample $75-$1,275/week		Narrow Sample $700-$1,275/week	
	Controls $130-734	Treatments $75-126 & $735-1275	Controls $500-734	Treatments $735-1100	Controls $75-902	Treatments $903-1275	Controls $700-902	Treatments $903-1275
Mean Not Working Base Year	0.296	0.184	0.248	0.185	0.268	0.151	0.200	0.151
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Mean Not Working End Year	0.280	0.175	0.227	0.179	0.237	0.152	0.177	0.152
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)
Percent Change	*-5.6*	*-4.7*	*-8.5*	*-3.2*	*-11.5*	*0.7*	*-11.8*	*0.7*
	(0.9)	*(1.8)*	*(1.5)*	*(2.1)*	*(0.9)*	*(3.0)*	*(2.3)*	*(3.0)*

Note: Standard errors in parentheses.

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

	2002-2003 Reform				2003-2004 Reform			
	Broad Sample $75-$1,275/week		Narrow Sample $500-$1,100/week		Broad Sample $75-$1,275/week		Narrow Sample $700-$1,275/week	
Dependent Variable	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted
Non-Employment	0.004	0.006	0.012 **	0.011 **	0.021 ***	0.019 ***	0.017 ***	0.016 **
When Benefits End	(0.004)	(0.005)	(0.006)	(0.005)	(0.004)	(0.004)	(0.006)	(0.006)

Note: 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 Reform				2003-2004 Reform			
	Broad Sample $75-$1,275/week		Narrow Sample $500-$1,100/week		Broad Sample $75-$1,275/week		Narrow Sample $700-$1,275/week	
Dependent Variable	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted
Non-Employment	0.145	0.195	0.392 **	0.375 **	0.997 ***	0.867 ***	0.774 ***	0.715 **
When Benefits End	(0.158)	(0.162)	(0.195)	(0.182)	(0.202)	(0.193)	(0.261)	(0.287)

Note: Standard errors in parentheses. Calculations divide the log-linear point estimate in Table 7 by the net increase in the weekly benefit rate from Table 1. *, ** 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 Reform				2003-2004 Reform			
	Broad Sample $75-$1,275/week		Narrow Sample $500-$1,100/week		Broad Sample $75-$1,275/week		Narrow Sample $700-$1,275/week	
Dependent Variable	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted	Unadjusted	Regression Adjusted
Non-Employment	-0.010 **	-0.008 **	-0.004	-0.004	-0.006	-0.004	0.000	0.001
When Benefits End	(0.004)	(0.004)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)

Note: 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.