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The Effect of an Increase in Worker's Compensation Benefits on the Duration and Frequency of Benefit Receipt

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Abstract

In this paper, we present new estimates of the effect of changes in workers' compensation benefits on benefit duration and application frequency. Using administrative data for the state of California, we estimate the duration and frequency effects resulting from two increases in temporary disability benefits occurring during the mid 1990s. We employ a quasi-experimental research design that exploits the fact that these increases affected the weekly benefits of high-wage earners while not affecting the benefits of low-wage earners. The design also makes use of the fact that benefits are tied to the benefits schedule as of the date of injury. We find consistent increases in the duration of benefit receipt among injured workers whose benefits were affected by the schedule changes. In addition, we find some evidence indicating that the likelihood of filing for benefits conditional on being injured is responsive to the benefits level. Finally, we assess whether the marginal claims that generate the frequency response to the benefits increase involve less sever injuries (as indicate by the duration of benefit receipt) than claims that are not on the margin between applying and not applying for benefits. We find evidence confirming this suspicion and assess the degree to which this non-random selection into the pool of claims receiving benefits biases the duration-elasticity estimates.

1. Introduction

Over the past decade, legislative efforts to reform workers' compensation and improve benefits for injured workers have stalemated over the net cost of the legislative package. A major component of disagreement over cost centers around the behavioral responses of workers to changes in state program characteristics. Assessing the costs of program changes requires precise estimates of such responses. This task, however, is made difficult by the fact that changes in a single parameter are likely to induce behavioral responses along multiple dimensions.

For example, several researchers have estimated the effect of the level of indemnity payments that workers receive in the event of an injury on the length of time that workers collect benefits (Butler and Worral 1985, Worrall and Butler 1985; Krueger 1991; Meyer, Viscusi, and Durbin 1995). Theoretically, higher benefits would increase the duration of benefit receipt since higher benefits alleviate some of the pressure to return to one's job. These studies generally find that increases in benefits cause significant, yet modest, increases in duration. A parallel line of research assesses whether higher benefits increase the likelihood that a claim is filed (Butler 1983, Ruser 1985, and Krueger 1990). Claim frequency may increase with the level of payment for several reasons. Higher benefits may induce workers who experience relatively minor injuries (and who would otherwise continue to work) to submit a claim. Alternatively, higher benefits may increase the propensity to report incidents holding injury severity constant. There are few studies that present estimates of duration and frequency responses to benefits increases within the same research design and sample analyzed.

In this study, we present new estimates of the effect of changes in workers' compensation benefits on benefit duration and application frequency. Using administrative data for the state of California collected by the California Workers' Compensation Institute, we estimate the duration and frequency effects resulting from two increases in temporary disability (TD) benefits occurring during the mid 1990s. We employ a quasi-experimental research design that exploits the fact that these increases affected the weekly benefits of high-wage earners while not affecting the benefits of relatively low-wage earners. This aspect of the increase coupled with the fact that benefits are determined by the state benefit schedule as of the date of injury create natural treatment and control groups of workers that can be used to estimate duration and frequency responses to a benefits change.

We use this quasi-experimental research strategy to explore three issues. First, in an analysis of claims receiving temporary disability (TD) benefits, we estimate the change in the average duration of benefit receipt caused by the two increases in benefit levels. We present separate estimates for each of the two benefits increases. We then use these estimates to calculate duration-benefits elasticities, defined as the percent change in mean duration caused by a percent increase in benefits. For TD claims overall, we find duration-benefits elasticities between 0.25 and 0.35. The estimated duration responses are comparable in magnitude across the two benefits increases.

Next, we apply the same quasi-experimental design to test for a frequency effect of a benefits increase. To do so, we make use of the large number of claims where no indemnity payments are received and where the only costs incurred are medical expenses. Using the larger sample of medical-only claims and claims receiving temporary disability payments, we assess whether there is an increase in the propensity to collect benefits relative to filing a medical-only claim for injured workers affected by the benefits increase. The results of this exercise are mixed with strong evidence of frequency effects arising from a 1994 increase in benefits but no evidence of frequency effects arising from a benefits increase one year later. For the 1994 benefits increase, our estimates indicate a frequency-benefits elasticity of approximately 0.5.

Finally, we use the larger sample of medical-only claims and claims receiving TD benefits to assess whether the marginal claims that generate the frequency response are less severe on average (as indicated by the duration of benefit receipt) than those claims that would have filed for TD benefits regardless. Using standard adjustment for sample selection, we assess whether there is a positive association between claim duration and the likelihood of filing for benefits. Moreover, we assess the degree to which such sample selection biases downwards estimates of the duration response. We find strong evidence that the marginal claims driving the frequency effects of a benefits increase involve less severe injuries and collect benefits for fewer days when compared with claims that would apply for benefits regardless. Adjusting for sample selection substantially increases the estimate of the duration response of injured workers who are not the margin between applying and not applying for benefits. Among this group of injured workers, the selection-corrected duration models increase the duration-benefit elasticity estimate from 0.3 to 0.8.

2. Modeling the Incentive Effects of Workers' Compensation

State workers' compensation systems are the principal programs through which employees are insured against workplace injuries. Coverage is nearly universal with the majority of workers covered by one of the 50 independently-regulated state systems and a small percentage of workers covered by a federal system. For the most part, employers are mandated to purchase insurance for their employees to cover both the medical costs associated with a workplace injury, indemnity benefits for work-time lost due to the injury, and indemnity benefits for any permanent injury. Indemnity benefits are determined according to a state formula that depends on weekly earnings at the time of injury. For nearly all workers, benefits are less than weekly wages. Larger employers are permitted to self-insure. Unlike state unemployment insurance systems where a minimum amount of work time is necessary for eligibility, workers become eligible for workers' compensation benefits as soon as they begin to work (see Krueger 1990, Krueger and Meyer 2001 for detailed discussions of state workers' compensation systems).

For non-fatal injuries, there are three broadly-defined claim types. First, there are claims where only the medical expenses of the injury are submitted. In California during the mid 1990s, these claims account for nearly half of the cases in any given year. Second, there are claims involving payments of medical expenses and TD indemnity payments. TD benefits are collected while absent from work due to a temporary-disabling injury or to working at reduced wages. These benefits are an increasing function of weekly earnings and are capped at some maximum level. Third, for injuries resulting in a permanent disability, injured workers are eligible to receive PD payments. These claims usually receive TD benefits until the injury fails to heal further before receiving a PD settlement.[1] In many states, PD payments are set equal to TD benefits for workers that are totally disabled and are set at a fraction of TD payments for worker with permanent-partial disabilities. In California, PD benefits are determined according to a separate schedule and depend on a physician's rating of the permanent disability (See California Commission on Health and Safety and Workers' Compensation 2000a, 2000b, 2000c).

Like all public programs that pay cash benefits, the payment schedule for workers' compensation benefits and changes thereto create a complex system of incentives for employers and injured workers that are likely to impact programmatic outcomes and overall costs. Here, we will focus on the potential incentives that are likely to influence the behavior of injured workers.[2] An increase in TD benefit payments is likely to elicit a behavioral response among both workers who would have received TD benefits in the absence of the benefits increase and workers who would not have filed for benefits under the lower benefits schedule. For the first group of workers, a simple model of benefit duration can be used to explore this behavioral response.

For injured workers with a given level of injury severity, define t as the number of days that have passed since the injury date and the function $h(t)$ as the monetary value that the worker places on being able to recuperate at home. Assume that this function is decreasing in the number of days that have passed since the date of injury (i.e., $h'(t) < 0$). The worker will remain out of work and collect benefits as long as the monetary value of recuperation time exceeds the difference between daily wages, W, and daily benefits, B. A formal statement of the decision rule is that the worker collects an additional day of benefits as long as $h(t) \geq W\text{-}B$ and goes back to work otherwise.[3] The ultimate duration of a given spell, t^*, is thus determined by the equation

[1] A very small fraction of claims receive PD benefits without first receiving TD benefits.
[2] Krueger (1990) presents an interesting discussion of some of the incentive effects faced by employers –e.g., the effect of incomplete experience rating in some states and the impact of self-insuring.

[3] We are implicitly assuming that wages exceed benefits. In practice this is true for nearly all recipients with the

(1) $$h(t^*) = W - B$$

which thus implies

(2) $$t^* = h^{-1}(W - B),$$

where $h^{-1}(.)$ is the inverse of the function $h(.)$. The change in duration caused by a change in benefits is found by taking the derivative of equation (2), and is given by $\partial t^*/\partial B = -h'^{-1}(W\text{-}B)$. Since the derivative of *h(.)* is negative, the derivative of its inverse is negative. Thus, the derivative of t^* with respect to *B* is positive. Hence, for workers who would receive benefits regardless of the increase, an increase in benefit levels should increase the duration of benefit receipt.

To theoretically analyze the behavioral responses driving frequency effects, we need a model of application behavior for individuals on the margin between applying and not applying for indemnity benefits. Here, we will elaborate on the model presented in Krueger (1990). Suppose that the latent severity of a workplace injury is described by the variable y^*, where higher values indicate more serious injuries. Among the population of workers injured during a given time period, y^* has a cumulative density function given by *F(.)*. A worker who is injured on the job must decide whether to go through the process of applying for disability benefits. In practice, an injured worker may not apply for many reasons, such as transaction costs or imperfect information regarding benefit eligibility. Here, we will focus on the fact that benefits are set at a fraction of weekly wages (–i.e., W >B, the assumption that was made implicitly above), and that some workers will deem the utility loss from working while injured to be less than the utility loss from foregone earnings.

To model application behavior, we must provide a more general form of the function providing the monetary value of healing at home to incorporate the effect of injury severity. We can do so by adding the argument y^* to the function *h(.)* to get $h(y^*,t)$. Since individuals

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exception of the small sample of low-wage claims where wages equal benefits.

deciding whether to apply for benefits compare the monetary value of recuperating to the implicit earnings loss at the time of the injury, the relevant value for deciding whether to apply is given by *h(y*,0).* For notational simplicity, we will express the function *h(y*,0)* as *g(y*).* Assume that $g'(.) > 0$. This is equivalent to assuming that the more serious the injury, the greater value the worker will place on being able to take time off from work. A worker applies for disability benefits if the monetary value of taking time off exceeds the difference between weekly wages and benefits, or $g(y^*) > W - B$. Otherwise, the injured worker continues to work and heals (or doesn't) on the job.

Given the distribution of injury severity among the population of injured workers, the proportion of injuries resulting in an indemnity payment is given by

(3)
$$P[g(y^*) > W - B] = P[y^* > g^{-1}(W - B)] \\ = 1 - F[g^{-1}(W - B)].$$

It is easy to show that this proportion increases with the benefit rate.[4] Higher benefits will render collecting benefits and healing at home more attractive to a larger proportion of injured workers. This proportional effect is the frequency effect estimated in previous research. One can also show that the expected value of the severity of claims conditional on filing a claim decreases with the benefit level, or

(4)
$$\frac{\partial E[y^* | y^* > g^{-1}(W - B)]}{\partial B} < 0.$$

Equation (4) follows from the fact that increasing benefits lowers the severity of the marginal injury, and hence the conditional expectation is calculated incorporating a larger portion of the left tail of the injury-severity distribution.

This model of application behavior can be used to illustrate the potential sample selection

[4]This can be seen by differentiating the expression with respect to B. This partial derivative is equal to $g^{-1'} f[g^{-1}(W-B)]$, where *f(.)* is the probability distribution function corresponding to *F(.)*. All of the terms of this expression are positive.

bias to previous estimates of duration effects that results from ignoring frequency effects that are non-random with respect to injury severity. Suppose that benefit duration, t^*, for a filed claim is linearly related to the severity of the injury, the benefits received, wages, and a mean-zero random error term reflecting idiosyncratic factors.[5] For a given injury, duration is determined by the equation

(5) $$t^*_i = \alpha + \beta y_i^* + \gamma B_i + \phi W_i + \varepsilon_i ,$$

where i indexes individual claims, α, β, γ, and ϕ are parameters, and ε_i is the error term. The true duration effect representing the behavioral response of workers to an increase in benefits is given by the parameter γ. For a given level of benefits, the expected value of duration for observed spells is found by taking expectations of equation (3), or

(6) $$E(t^*|B) = \alpha + \beta E[y^*|y^* > g^{-1}(W-B)] + \gamma B.$$

This expectation depends on the conditional expectation of injury severity due to the fact that we only observe a claim if the monetary value of utility derived from staying home is higher than the difference between average weekly wages and benefits. Taking the derivative of equation (6) with respect to benefit levels gives

(7) $$\frac{\partial E(t^*|B)}{\partial B} = \beta \frac{\partial E[y^*|y^* > g^{-1}(W-B)]}{\partial B} + \gamma.$$

This partial derivative provides a decomposition of the empirical relationship that one would observe between benefit levels and mean duration if one were to exogenously increase B. The

[5]Linearity in benefits, injury severity, and wages would result from a functional specification of *h(.,.)* that is linear in t and y^*. An additional restriction that would arise in this case is that the coefficient on benefits should be the negative of the coefficient on wages. The added random error term can be thought of as a component accounting for non-systematic variation in t^*.

total effect consists of two components. The second term on the right hand side of (7) represents the direct behavioral response to the increase in benefits of injured workers who would have applied for benefits in the absence of an increase. The first term provides the indirect effect of a change in B on mean duration via the change in the composition of observed claims. Since increases in benefit levels reduce the conditional expectation of injury severity, this first term is negative.

Previous research has been unable to model the selection process since the analysis has been confined to using samples of claims where indemnity payments are received. In other words, the complete partial derivative, $\partial E(t^*|B)/\partial B$ is used to estimate the direct behavioral response, γ. If the sample selects according to the model specified above, such estimates are biased downwards by the selection term. Accounting for sample selection bias requires that the researcher observe the larger population (or a sample thereof) of potential claimants out of which observed claims emerge. Below, we outline one strategy for constructing this larger population based on a set of claims that are already filed in most state workers' compensation systems.

3. Empirical Strategy and Description of the Data

The empirical strategy that we pursue uses a quasi-experimental research design to investigate the three issues modeled in the previous section. First, we analyze the effect of benefits increases on average duration by analyzing a sample of claims filed in California that all received TD indemnity payments. Next, we assess whether increases in benefits cause increases in the proportion of all claims that enter the system that collect indemnity payments (i.e., the frequency effect). For these latter estimates, we expand the sample to include claims that are filed and that only incur medical expenses. Finally, we combine the two components of the analysis to adjust the quasi-experimental estimates of the duration effect for sample-selection bias.

The California Natural Experiment

To clarify the basic identification strategy, a brief discussion of the determination of TD benefit rates in California is needed. A worker injured on the job who, as a result, must take time off work receives total temporary disability benefits. In many states, benefits are set equal to two-thirds of weekly earnings at the time of injury with benefits capped at a maximum level. Injured workers for whom two-thirds of weekly earnings exceed the maximum receive the maximum. The ratio of benefits to earnings for workers with very low earnings may be higher than two-thirds. The majority of workers, however, receive benefits equal to two-thirds or less. Benefits stop when the injured employee returns to work.

Figure 1 graphically displays the TD benefit schedules for California for three time periods: (1) January 1991 to June 1994, (2) July 1994 to June 1995, and (3) July 1995 to June 1996. For the earlier time periods, injured workers with weekly earnings of $126 or less receive benefits equal to earnings, while workers earning between $126 and $189 receive the fixed benefits of $126. Injured workers earning $189 to $504 receive benefits equal to two-thirds of weekly earnings, while injured workers earning in excess of $504 receive two-thirds of $504 in weekly benefits (maximum benefits of $336 per week). The change in the benefit schedule effective July 1, 1994 increased the maximum benefit level from $336 to $406 per week (a 21 percent increase in the weekly benefit maximum). This change increased the benefit rates for workers injured after the date of the increase who earn $505 or more, with workers earning in excess of $609 experiencing the full $70 increase in the maximum. The change in the benefit schedule effective July 1, 1995 increased the maximum benefit levels from $406 to $448 (an increase of approximately 10 percent). This increased benefits for workers with weekly earnings of $609 or greater, with workers earning $672 or more receiving the full $42 increase in weekly benefits.

There are two aspects of these increases in the TD benefits maximum that, in conjunction, create an opportunity to evaluate behavioral responses to changes in benefit rates. First, benefit levels are tied to the date of injury. Hence, a worker who is injured on June 30, 1994 may face a different benefit schedule than that which he would have faced had the injury occurred one day later. Second, the benefits of injured workers with average weekly earnings that placed their benefits below the pre-increase maximum were not affected by the change in benefits schedule. Hence, each change permits the segmentation of injured workers into natural treatment and control groups.

To clarify the quasi-experimental design, we will explicitly outline the experiment created by the 1994 increase. Here, we focus on the time period ending on June 30, 1995. The natural experiment is specified as follows. Define the pre-change sample of injuries as those with injury dates prior to July 1, 1994 and the sample of post-change injuries as those with injury date after June 30, 1994 and define high-wage workers as those with earnings in excess of $504 and low wage workers as those with weekly earnings of $504 or less. The pre-post change in average duration for high wage workers is given by the equation

(8) $$Diff_{high-wage} = t*^{post-change}_{high-wage} - t*^{pre-change}_{high-wage},$$

where $t*^{i}_{j}$ is average duration for workers with wages j (high-wage, low-wage) who are injured during time period i (post-change, pre-change).

The difference in equation (8) will be driven by the incentive effects of the benefits increase and by whatever other changes (in the economy, or otherwise) affect mean duration. If we assume that all other factors affecting duration impact the low earnings group in the same manner as they impact the higher-earnings group, the experience of the low earnings workers can be used to net out the impact of all other unobservable factors. Defining a similar pre-post change in average duration for low wage workers, the difference-in-difference estimator is given by the equation

(9) $$DD = Diff_{high-wage} - Diff_{low-wage}.$$

To regression-adjust this estimate for observable covariates, define the dummy variable $Treat_i$ as equal to one if the injured worker's weekly wages are in the range affected by the benefits increase and the dummy variable $After_i$ as equal to one if the injury occurs during the post-change period. The adjusted difference-in-difference estimate comes from the regression equation

$$t^*_i = \beta_o + \beta_1 After_i + \beta_2 Treat_i + \beta_3 Treat_i * After_i + \delta X_i + \varepsilon_i, \quad (10)$$

where β_0 through β_3 and the vector δ are regression coefficients and ε_i is an error term. The coefficient β_3 gives the extent to which the pre-post increase change in duration for workers effected by the increase exceeds the change for workers un-effected by the increase, holding constant the variables in *X*.

We apply this quasi-experimental design to the two benefits increases illustrated in Figure 1. To analyze the experiment created by the first benefits increase, we restrict our sample to claims with injury dates occurring between July 1, 1993 and June 30 1995 (one full year before and one full year after the increase). The second benefits increase is analyzed with a sample of claims restricted to the time period between July 1, 1994 and June 30, 1996. We first estimate unadjusted and regression adjusted difference-in-difference duration effects using the sample of claims that is restricted to those with positive TD payments. We then apply this design using the full sample of medical-only and TD claims to an outcome variable indicating whether TD benefits are received. Since this is the identification strategy employed by Krueger (1991) and Meyer, Viscusi, and Durbin (1995) using alternative states and time periods, the first set of results provide duration effect estimates comparable to those presented in previous studies. The second set of results adds new information, in that the frequency effects are estimated using the same methodological design and data set that are used to estimate the duration effects.

For both the duration and frequency effect models we present estimates for all claims receiving TD payments as well as separate estimate for claims that receive TD payment only and claims that ultimately receive both TD and PD payments. The first sub-set of claims account for the majority of the TD caseload in California (approximately 65 percent). Durations for these claims, however, are relatively short. TD/PD claims, while the minority of claims receiving TD benefits, are considerably longer than claims that receive TD benefits only (see the means in Tables 2 and 3 below). We present separate estimate for several reasons. First, given the likely large difference in severity between the two types of claims, one might expect different response elasticities. Second, previous research on duration elasticities has focused almost exclusively on TD claims that do not receive PD payments. Hence, providing separate estimates by sub-sample facilitates comparisons between the results presented below and those from previous studies.

Adjusting for sample selection

Changes in benefit rates are likely to impact the composition of the pool of claims receiving TD benefits. If an increase in benefits lowers the injury severity of the marginal claim, the average severity of injuries will decrease with increases in mean benefits. The quasi-experimental research strategy outlined above is designed to identify exogenous variation in benefit levels. Identifying exogenous variation, however, does not address the changing of the pool of claims receiving indemnity payments, since such variation itself impacts the selection of the sample.

We use our complete sample of medical-only and TD claims to explore this sample-selection issue. First, we investigate whether the increased relative propensity to receive indemnity payments varies across injuries defined by the cause of the injury (COI), the nature of the injury (NOI), and the body part injured (BPI). Identifying the injury types with large frequency effects permits a comparison of the TD-duration distribution for injuries exhibiting frequency effects to the similar distribution for injuries that do not. If there is a selection bias, duration effects should be smaller for injury types with large frequency effects.

The results of this descriptive analysis are then used to specify a standard sample-selection model a la Heckman (1979). Specifically, define a dummy variable $Type_i$ as equal to one if the injury has COI, NOI, and BPI codes that exhibit statistically significant increases in the likelihood that workers effected by the benefits increase receive TD benefits. Let y^* be the latent unobserved severity of the injury which is determined according to the equation

$$y^* = \beta_0 + \beta_1 Treat_i + \beta_2 After_i + \beta_3 Treat_i * After_i + \beta_4 Type_i + \beta_5 Type_i * After_i + \beta_6 Type_i * Treat_i + \beta_7 Type_i * Treat_i * After_i + \delta X_i + \eta_i, \quad (11)$$

where β_0 through β_7 and the vector δ are parameters, and η_i is an error term with a standard normal distribution. We specify this model as the underlying process determining whether or not TD benefits are collected. Assume that we observe a claim receiving TD benefits when $y^* > 0$. Then the outcome variable indicating receipt of TD benefits can be modeled using the probit estimator.

This first stage selection equation is then used to adjust the difference-in-difference estimate of the benefit-duration effect. The equation modeling the duration of benefit receipt is given by equation (10) above. The expectation of duration conditional on observing TD benefits being paid is given by

$$\begin{aligned} E(t^*_i | Benefits) &= \beta_o + \beta_1 After_i + \beta_2 Treat_i + \beta_3 Treat_i * After_i + \\ &\quad \beta_4 Type_i + \beta_5 Type_i * After_i + \beta_6 Type_i * Treat_i + \delta X_i + E(\varepsilon | Benefits), \\ &= \beta_o + \beta_1 After_i + \beta_2 Treat_i + \beta_3 Treat_i * After_i + \\ &\quad \beta_4 Type_i + \beta_5 Type_i * After_i + \beta_6 Type_i * Treat_i + \delta X_i + \sigma_{\varepsilon\eta} \frac{\phi(\gamma' Z_i)}{\Phi(\gamma' Z_i)}, \end{aligned} \quad (12)$$

where $\sigma_{\varepsilon\eta}$ is the covariance between the error terms from the duration and selection equations, ϕ is the standard normal probability distribution function, Φ is the cumulative normal distribution function, and $\gamma' Z_i$ is substituted for the right hand side of the selection equation (11). The exclusion restriction identifying the selection correction is the omission of the triple interaction term between the variables "Type," "Treat," and "After" from the duration equation. Hence, the model is identified on the differential *relative responsiveness* (injured workers with high frequency effect injuries relative to injured workers with low frequency effect injuries) of the filing behavior of treatment group workers to the change in benefits.

We first estimate the probit model in equation (11) using the entire sample of medical-only and TD claims. We then use the parameter estimates from this model to construct the inverse-mills ratio for each observation receiving TD benefit payments. Finally, we restrict the sample to claims receiving TD payments and estimate equation (12) adding the estimated inverse-mills ratio to the specification. Since the model in equation (12) is heteroscedastic, we calculate the Huber-White standard errors for the purposes of inference.

The predictions for the selection-corrected results are clear. To the extent that a benefits increase causes a frequency effect for less severe injuries, including the inverse mills ratio in the specification should increase the difference-in-difference estimate of the benefits-duration effect (given by the coefficient β_3). In addition, the underlying model specifies that more serious injuries should be more likely to collect TD benefits. If severity and duration are positively related, then the covariance between the error terms from the duration and selection equations should be positive. We test this proposition by analyzing the coefficient on the constructed inverse-mills ratio.

Description of the data

We use administrative data provided by the California Workers' Compensation Institute (CWCI). The CWCI master file contains information on approximately 70 percent of the claims filed by California employees who work at firms that purchase workers' compensation insurance. The data set does not include the claims of employees working at large firms that self insure. The Institute provided us with an 80 percent random sample of their file. Hence, our sample covers approximately half of the market where insurance is purchased.

The data provided to us by CWCI came in the form of two files: (1) a summary claims file containing information on the workers average weekly wage, date of injury, nature, cause, and body part of injury, and other summary information, and (2) a transaction level file providing information on each payment made within a claim, the type of payment transaction (TD, PD, or medical expense), and the time period covered by the payment. We use the transaction-level file to calculate the duration of each TD claim in the following manner. For each TD transaction payment, we use the from and through dates of the time period covered to calculate the number of days of TD receipt covered by the transaction. We then sort the transaction files by claim identification codes and sum the days variable within claims. This method requires that we restrict the data set to claims with complete information on from and through dates for each of the transactions.[6]

A final set of restrictions that we place on the sample concern the average weekly wage variable used to assign workers to treatment and control groups. There are several observations with extremely low wages ($0.01 for example) as well as observations with extremely high wages ($900,000 per week). Since these are clearly errors, we restrict the sample to claims with average weekly wages between $65 and $2000. The first value is slightly lower than 1st percentile of the weekly-wage distribution while the second value is slightly higher than the 99th percentile. In addition, we eliminate claims (both medical-only and claims receiving TD payments) with missing information on weekly wages. We drop all claims with TD benefit duration that exceeds three years since these observations are in the extreme right tail of the duration distribution and are likely to be erroneous. Finally, for the 1995 experiment, we top-code all duration spells in excess of 900 days to 900 days. This restriction is imposed due to the fact that for a sub-sample of the data we do not observe payments beyond the end of 1998. The common 900-day top-code is imposed to ensure that all claims with injury dates occurring during the two-year period surrounding the second benefits increase are observed over time periods of comparable length. Fortunately, the top-code chosen exceeds the value at the 99th percentile of the duration distribution. For the two-year period surrounding the 1994 increase, there are

[6]An alternative method of calculating duration would make use of the variable in the summary claims file giving the total TD benefits paid. Combined with information on the worker's weekly earnings and the state benefit schedule corresponding to the date of injury, one could impute days of receipt by dividing total benefits by the imputed weekly benefit rate, and then multiplying by seven. One problem with this imputation, however, is that if the weekly wage is measured with error, this will introduce non-random measurement error in the construction of treatment and control groups that will be directly related to the constructed dependent variable. For example, if recorded wages are erroneously high, the constructed duration variable will understate the true duration. Moreover, a large positive error in the wage variable may lead to an observation being erroneously placed in the treatment group. Hence, measurement error will induce a negative relationship between being in the treatment group and benefit duration.

Analysis of the data revealed several observations where wage information was clearly incorrect. Erroneous wage information was inferred from the fact that for several observations, imputed weekly benefits did not match the weekly benefit payments observed in individual transactions. For this reason, we choose to avoid imputation altogether and construct the duration dependent variable from the individual transactions.

254,148 claims, 123,925 of which receive some TD payments. For the two-year period surrounding the 1995 increase, there are 272,889 claims, 122,727 of which receive some TD payments. Over the total time period analyzed our final sample includes 423,326 claims with 218,165 receiving some TD indemnity benefits.[7]

To illustrate the source of the quasi-experimental variation in benefits, Table 1 presents comparisons of average weekly benefits before and after the benefits increases by wage categories. Panel A focuses on the one-year periods before and after the 1994 benefits increase while Panel B focuses on the one-year periods before and after the 1995 benefits increase. In Panel A, the first row presents figures for all TD claims. The second row presents means for workers earnings less than $504 per week. The next three rows present means for three alternative treatment groups: workers earnings $505 or more, workers earnings $505 to $609 per week, and workers earning $610 or more. The first group includes all workers experiencing a change in benefits, the second group includes workers with earnings that would place their benefits between the old and new maximums, while the third group corresponds to workers who experience the full increases in the benefits maximum. These three treatment groups correspond to the alternative treatment groups that we use below. For the 1994 experiment, the control group is always those workers earnings $504 or less. As can be seen, average benefits did not increase for workers in the control group (there is a trivial increase in average benefits of $0.73 which is likely due to small before-after differences in the composition of low-wage injured workers). For workers with earning exceeding $504, average weekly benefits increase by $59 (an 18 percent increase). Injured workers with earnings between $504 and $609 experienced an average increase of $37 (an 11 percent increase), while workers earnings in excess of $609 received the full $70 increase (a 21 percent change).

In Panel B, workers are stratified by earnings into four groups comparable to the stratification used for the period surrounding the 1994 increase. Workers earnings less than $609

[7]To be sure, the sum of the sub-samples used for the two experiments does not add up to the total sample size due to the overlapping period July 1, 1994 to June 30, 1995.

now constitute the control group while workers earnings in excess of $609 are the first treatment group. Workers with earnings between $609 and $672 have earnings that place their benefits levels between the old and new maximums while workers with earnings in excess of $672 experience the full increase in benefits. Again, there is no (or trivial) change in benefits levels for workers in the control group. For all workers in the high-wage range (in excess of $609), benefits increase by 9.3 percent ($38). For workers in the intermediate range, benefits increase by only 5.4 percent (or $22 on average), while workers earnings in excess of $673 receive the full 10 percent increase ($42).

Hence, Table 1 demonstrates the clear differences in treatment across the distributions of weekly wages and by the date of injury that result from the benefits increases. The table also illustrates that the second increase was considerably smaller (proportionally and absolutely) than the first.

4. Empirical Results

A. Difference-in-difference duration effects

We begin by analyzing the duration effects for the sample of claims restricted to those claims receiving some TD benefits. Table 2 presents average duration of TD receipt in days by time period and by average weekly wages at the time of injury. The table provides calculations for all claims receiving TD benefits (the first four columns), calculations for claims that receive TD benefits but no PD benefits (the middle four columns), and calculations for claims receiving TD and PD benefits (the final four columns). For each group of claims, the first column present means for all claims, the second column presents means for those workers injured prior to the benefits increase, the third column presents means for those injured after the increase, while the fourth column presents the before-after changes in these averages. The first row for each set of calculations provides means for all claims while the next four rows provides means by the wage categories used in Table 1.

Beginning with the tabulations for all claims receiving TD benefits, there are clear patterns across the four designated wage groups that indicate a benefits-duration response. For those injured workers with earnings of less than $505, mean duration declined by 2.23 days from 104.7 to 102.4. In contrast, average duration increases by 2.9 days for workers earning $505 plus, by 7.74 days for workers earning between $505 and $609, and by approximately 0.64 days for workers earnings in excess of $609.

The difference-in-difference estimator subtracts the before-after change for the control group from those of each of the treatment groups. These estimates are presented in the final three rows of the table. For all workers in the wage range affected by the benefits increase (greater than $504), duration increases by 5.1 days relative to workers who were not affected (earnings $504 or less). This relative change is significant at the 2 percent level of confidence. For workers with weekly earnings placing the benefits change in the intermediate range, relative duration increase by approximately 10 days (significant at the one percent level of confidence). Finally, for workers with earnings in excess of $610, relative duration increases by 2.9 days. This change however, is not statistically significant. Hence, for all claims, there is a statistically significant relative increase in duration among workers in the effected wage range. The relative increase appears to be largest for workers with the lowest earnings within the group of workers with wages in the effected range.

The results for claims receiving TD payment only and claims receiving TD and PD payments are qualitatively similar. For claims receiving TD payment only, there is a statistically significant increase in duration of 2.2 days for high-wage ($505+) relative to low-wage ($504 or less) workers. For workers in the intermediate wage range, there is no significant increase in relative duration, while for workers in the highest earnings range, relative duration increases by 2.5 days (significant at the 2 percent level of confidence).

For TD/PD claims, we also see statistically significant relative increases in the average duration of TD receipt among workers in the effected wage range. For all workers with earnings in excess of $504, average duration increases by 7.7 days relative to low-wage workers. This relative increase is significant at the 8 percent level of confidence. For workers earnings between $505 and $609, relative duration increases by 22 days and is statistically significant at the one percent level of confidence. For workers in the highest wage category, there is no measurable relative increase in benefit receipt duration.

Table 3 presents comparable tabulations for the time periods surrounding the 1995 TD benefits increase and for the wage categories employed in Panel B of Table 1. Again, separate tabulations are presented for all claims, claims receiving TD benefits only, and TD/PD claims. The results for all claims yield no evidence of a relative increase in average duration among workers in the effected wage range. For workers in the control group (earning $609 or less), average duration increase by 8.34 days, while for workers earning in excess of $609, average duration increases by 9.46 days. Combined, these two figures indicate a relative increase in duration of 1.11 days among high-wage workers, a change that is statistically insignificant. Moreover, there is no significant relative increase for workers in the intermediate or high wage ranges.

The tabulations for TD-only claims yield results that contrast those for the sample overall. Among this group of claims average duration increases by 4.6 days for the control group of workers and 8 days for the first treatment group of workers, yielding a difference-in-difference estimate of 3.8 days that is statistically significant at the one percent level of confidence. Moreover, similar to the results from the 1994 experiment, there is no measurable relative increase among workers in the intermediate wage range yet a sizable and statistically significant increase among workers in the high wage range. For workers earning $673 or more, average duration increases by 4.1 days relative to workers earning less than $609. This estimate is also significant at the one percent level of confidence. For the sample of claims receiving both TD and PD payments, only one of the difference-in-difference estimates is positive and significant. For workers with wages between $609 and $672, average duration increases by approximately 19 days relative to the change in the average for low-wage workers in the control group. This relative change is significant at the 5 percent level of confidence. Note, this relatively large effect for workers in the intermediate wage range (for this sub-sample of claims) was also evident in the 1994 results.

Comparing the results in Tables 2 and 3, there are several similarities and one glaring difference. The major difference concerns the fact that we observe significant duration effects for all claims receiving TD benefit payments in response to the 1994 increase but not the 1995 increase. The similarities are found in the comparable duration responses for both experiments among claims that receive TD payments only and TD/PD claims. For the former, there are clear and statistically significant duration effects, for all effected workers overall and especially for high-wage workers. For the latter, there are significant effects that are most pronounced for workers in the intermediate wage range whose benefits post-increase fall between the old and the new maximum.

The differences for the results using the overall sample may be driven by several factors. For one, the 1995 benefits increase was proportionally smaller (10 percent) than that for the 1994 change (21 percent). Hence, if the response elasticitiies are comparable in both time periods, one would expect smaller responses to the 1995 experiment that may be more difficult to statistically detect. This explanation however, conflicts with the significant duration responses that we find when the sample is split into TD-only claims and claims receiving both TD and PD benefits. An alternative explanation may be that changes in the internal composition of claims occurring within the time/wage cells created to evaluate the experiment are confounding the duration response for the sample overall but not for the sub-samples stratifies by whether PD benefits are ultimately received. This would occur if the proportion receiving PD payments (an indicator of injury severity) changes in a non-neutral fashion across wage categories.

To address this possibility and to more generally assess whether the duration estimates are sensitive to statistical adjustment for observable covariates, Table 4 presents difference-in-difference estimates from regression specifications that control for various aspects of the injured worker and the injury. We calculate these estimates using the duration regression given in equation (10). The figures in Table 4 are the coefficient estimates on the interaction term between a dummy indicating an injured worker in the effected wage range and a dummy indicating that the injury occurred after the benefits increase. Again, this gives the extent to which the pre-post change in average duration for the treatment group exceeds the comparable change for the control group after adjusting for the variables included in the specification. Similar to the structure of Tables 2 and 3, we present three comparisons: (1) all affected workers relative to all low-wage unaffected workers, (2) workers in the intermediate wage range relative to all low-wage unaffected workers, and (3) workers whose wages place them above the benefits maximums in both periods relative to all low-wage workers. For each comparison, we present estimates from three specifications: (1) the simple specification with no other covariates (comparable to the figures in Tables 2 and 3), (2) the base specification plus controls for average weekly wages, gender, age, and age squared, and (3) specification (2) plus 71 COI dummies, 53 NOI dummies, 55 BPI dummies, and 80 dummies indicating the injured worker's two-digit industry code.

For the regression models using the sample of all claims receiving TD benefits, we include the additional control variable indicating a claim receiving TD benefits only in models (2) and (3). This additional control statistically adjusts the difference-in-difference estimates for change in the composition of claims along this dimension. Panel A presents results for the 1994 benefits increase while Panel B presents results for the 1995 increase.

Beginning with the results in Panel A, holding constant the variables in Models (2) and (3) does not appreciably affect the duration-effect estimates. For all TD claims and comparison (1), the point estimate drops from a relative increase of 5.1 days in model (1) to a relative increase of 4.2 days in model (3). Similarly, there are slight drops in the duration effect estimate for comparisons (2) and (3) when control variables are held constant. For claims receiving TD benefits only, statistically adjusting for the variables in models (2) and (3) actually increases the point estimates of the difference-in-difference duration effects. Similarly, regression-adjusting does not alter the estimates of the duration responses among claims receiving both TD and PD benefits. In Panel A, there is not a single case where statistically adjusting for observable covariates knocks out a statistically significant duration effect. Hence, the duration effects observed in response to the 1994 increase are quite robust.

Turning to the results in Panel B, the results for all claims receiving TD benefits indicate that regression-adjusting matters. For example, in comparison (1) we see that holding constant the variables in Models (2) and (3) increases the difference-in-difference estimate of the duration effect from 1.11 in Model (1), to 4.03 in Model (2) and 3.47 in Model (3). Moreover, while the unadjusted duration effect estimate is not statistically significant, the estimates from Models (2) and (3) are significant at the 5 percent and 10 percent levels of confidence, respectively. Similar patterns are observed for comparison (2), while for comparison (3) both the unadjusted and regression-adjusted difference-in-difference effects are statistically insignificant. Concerning the results for the sample of TD-only and TD/PD claims, the duration effect estimates from the multi-variate regressions are quite similar to the unadjusted duration effect estimates presented in Tables 2 and 3. Again, estimates that are statistically significant without regression adjusting are generally significant after adjustment.

To facilitate comparisons between the results from the 1994 increase and the results from the 1995 increase (as well as to the results from previous research), it is helpful to convert the duration effect estimates in Table 4 into duration-benefits elasticities. We compute these elasticities in the following manner. First, we use the coefficient estimates from the regression models estimated in Table 4 to predict what average duration would have been among workers affected by the increase had the increase not gone into effect. Next, we calculate the percentage increase in duration by dividing the difference-in-difference duration estimate by this counterfactual average duration. Finally, we divide this percentage increase in duration by the relevant percentage increase in benefits listed in Table 1. This final ratio provides our estimate of the duration-benefit elasticity.

Table 5 presents the results from this exercise. For each of the difference-in-difference estimates presented in Table 4, Table 5 presents the corresponding elasticity estimate. Comparisons of the results in Panels A and B indicate proportionally larger duration responses to the 1995 benefits increase than the 1994 benefits increase. For example, for the total sample the overall elasticity estimate from the most complete regression specification (comparison (1) model (3)) is 0.25 for the 1994 experiment and 0.35 for the 1995 experiment. Similar results are observed for comparisons (2) and (3) using the full sample of claims. Similarly, among claims receiving TD benefits only, the duration-benefits elasticity estimates from comparison (1) range from 0.38 to 0.45 for the 1994 experiment and 0.88 to 0.98 for the 1995 experiment. Similar patterns are observed for comparison (3) for this sub-sample and comparison (2) for the sub-sample of claims receiving both TD and PD benefits.

Despite these differences, the elasticities for the overall samples are quite similar across experiments. Moreover, the relative size of the elasticities across comparisons and sub-samples are similar within the 1994 and 1995 experiments. An additional useful exercise is to compare the elasticity estimates presented here to those from previous research. Meyer, Viscusi, and Durbin (1995) find elasticity estimates between 0.30 and 0.62 using changes in the benefits maximum occurring in Kentucky and Michigan in the early 1980s. Using data for Minnesota, Krueger (1991) find considerably larger elasticity estimates, ranging from 1.22 to 3.68. Both of these studies analyze duration responses among samples of claims where only TD benefits are received. Our range of estimates for the TD-only sample lie between 0.37 and 0.98. Hence, our estimates are within the range of estimates from previous studies. We are closer in magnitude, however, to the estimate presented by Meyer et. al. Unfortunately, we are unaware of quasi-experimental estimates of duration effects for all TD claims combined.

B. Using the natural experiment to test for a frequency effect

Here we turn to estimates of the effect of a benefits increase on the likelihood that a reported injury (injuries where at least a medical-only claim is filed) collects TD indemnity payments. Our empirical strategy is to apply the difference-in-difference estimator used to assess duration to an outcome dummy variable that is equal to one if the injured worker collects benefits. While in the previous section, we analyzed the sub-sample of data constrained to those injuries receiving benefits, here we use the full sample of medical-only and TD claims.

Table 6 presents difference-in-difference estimates for the TD-receipt outcome variable.

The comparison groups and model specifications are equivalent to those used to estimate the duration effects in Table 4 and are detailed in the notes to the table. Beginning with the 1994 results using all claims, there is considerable evidence of statistically significant frequency effects in several of the models presented. In comparison (1) (using all workers earning more than $504 as the treatment group), the unadjusted estimate indicates that the proportion of claims that collect TD indemnity benefits increases by 3.8 percentage points among workers affected by the increase. Controlling for personal characteristics and wages in model (2) does not alter this estimate. Finally, adding complete sets of dummy variables for COI, NOI, BPI, and industry causes a slight decline in the estimated frequency effect. In model (3), the proportion of claims receiving TD benefits increases by 3.3 percentage points. In all three models for comparison (1), the difference-in-difference estimate of the frequency effect is statistically significant at the one percent level of confidence.

The difference-in-difference estimates for comparison (2) indicate no frequency effect. For all models the point estimates are near zero with fairly small standard errors. This result is sensible considering that workers with weekly earnings of $505 to $609 experience the smallest increase in weekly benefits. Comparison (3) yields the largest effects. The three difference-in-difference estimates indicate that the increase in benefits caused a 4.5 to 5.3 percentage point increase in the percent of high wage workers collecting TD indemnity benefits. Again, adjusting for differences in age, gender, the worker's industry, and the characteristics of the injury do not cause notable changes in the difference-in-difference estimate. All three estimates are statistically significant at the one percent level of confidence.

Table 6 also presents estimates of the frequency effects for the sample restricted to medical-only and TD-only claims and the sample restricted to medical-only and TD/PD claims. These results for 1994 are presented in the final six columns of Panel A. The patterns for both sub-samples are nearly identical to those observed for the overall sample of claims. For comparison (1), there are significant difference-in-difference estimates of the frequency effects that range from 3 to 3.5 percentage points. Models estimated for comparison (2) (that uses workers in the intermediate earnings range of $505 to $609 as the treatment group) fail to find statistically significant frequency effects. Finally, in all model estimates for comparison (3), there are statistically significant frequency effects ranging from approximately 4 to 4.8 percentage points.

The results for the 1995 benefits increase presented in Panel B contrast with the findings for the 1994 increase. Starting with comparison (1) for all claims, the difference-in-difference estimates for all three models are near zero and statistically insignificant. In addition, with the exception of the estimate from model (2) comparison (3) (statistically significant at the 10 percent level), there is not a single positive and significant estimate for the remaining six comparisons. For the sub-sample restricted to medical-only and TD-only claims, there are several small and marginally significant frequency effect estimates in the comparison using all workers earnings more than $504 as the treatment group. The point estimates for this group range from 0.6 to 1 percentage points. The point estimates for this sub-sample using the highest wage earners as the treatment group are also positive and significant, though small. For the sub-sample using medical-only and TD/PD claims, there is not a single positive and statistically significant estimate of the frequency effect.

The differences in frequency effect results between the 1994 and 1995 experiments may be driven by a number of factors. One possibility is that the proportional responsiveness of workers to both increases is the same but that the relatively smaller 1995 increase induced an absolute frequency effect that is too small relative to the standard error of the estimate to statistically detect. One way to assess this possibility would be to calculate the frequency elasticity observed for the 1994 experiment and use this elasticity to predict what we would have observed in 1995. One would then compare this hypothetical response to the size of the standard errors for the 1995 estimates to assess whether the expected response is statistically detectable. For the comparison (1) models using all claims in 1994, the frequency effect estimates represent an approximately 9 percent increase in claims collecting TD benefits. Given that benefits increased for effected workers overall by approximately 18 percent, this change indicates a frequency-benefits elasticity of 0.5. Applying this elasticity estimate to 1995, the overall increase in benefits of 9.3 percent would have induced an increase in the proportion of claims receiving TD benefits of 4.7 percent. For the models in comparison (1), a 4.7 percent increase corresponds to increases of approximately 2 percentage points among effected workers. Given that the standard errors of the points estimates in this comparison for 1995 range between 0.004 and 0.005, an increase in claims receiving benefits of 2 percentage points would be easily detected (the expected effect is at least 4 times the standard error in each instance). Hence, the patterns in Table 6 strongly indicate that the proportionally lower response observed for 1995 is not driven by measurement problems but by a truly smaller frequency effect than that which is observed for 1994.

With this in mind, we must look elsewhere for an explanation. An alternative explanation may come from the model of application behavior presented above. Specifically, suppose that the distribution of injury severity is unimodal and that the marginal injury between filing and not filing for TD benefits is in the left tail of the distribution. For purposes of illustration, assume that the function giving the monetary value of being able to recuperate at home takes the simple form $g = \alpha_0 + \alpha_1 y^*$, where y^* is the severity of the injury α_0 and α_1 are parameters that are both greater than zero. Recall, we assume that y^* is distributed according to the cumulative distribution function *F(.)* and that the marginal injury will be that injury where the value of recuperating at home is just equal to the difference between benefits and wages. Using our explicit functional form, this marginal injury is defined by the condition

$$\alpha_0 + \alpha_1 y^* = W - B.$$

Solving this expression explicitly for the injury on the margin between filing and not filing for benefits gives

$$y^* = \frac{\alpha_o}{\alpha_1} + \frac{W - B}{\alpha_1}.$$

Figure 2 shows the case where y^* is normally distributed and where the marginal injury is in the left tail of the severity distribution. y^*_0 shows the marginal injury prior to a benefits increase. In this figure, all injuries to the right of y^*_0 apply for TD benefits while all injuries to the left do not. The area under the distribution to the right of the marginal injury provides the proportion of claims that collect benefits while the area under the distribution to the left gives the proportion of claims that do not collect benefits. An increase in benefits of \$10 shifts the severity of the marginal injury to the left by $-10/\alpha_1$ (as calculated by the equation above). This shift corresponds to an absolute frequency effect equal to the area under the distribution between the old (y^*_0) and new (y^*_1) marginal injuries (given by A1 in the figure). A further \$10 increase will cause a similar-size shift in the marginal injury (to y^*_2) but will elicit a smaller absolute frequency response (given by the area A2). Hence, if the marginal injury is in the left tail of the distribution of injury severity, one would expect subsequent benefits increases to elicit consecutively smaller frequency responses.

To be sure, while the above explanation is consistent with the patterns observed across statistical experiments, a number of conditions must be met for this explanation to be true. To start, marginal injuries must select into the population of claims receiving temporary disability benefits in the manner specified by the model. There is little evidence of this being the case, though we will present some tests of such sample selection below. In addition, the marginal injury must be in the left tail of the severity distribution. If the margin is in the right tail of the distribution, the frequency elasticity might actually increase with subsequent benefits changes until the margin passes over the distribution's mode. Moreover, the differences in estimated responses between the 1995 and 1994 experiments are quite large. While the direction of the change is consistent with the theoretical argument presented above, the magnitude of the change indicates that this theoretical explanation should be viewed with caution. The differential responses across the two experiments suggest that for the purposes of forecasting the costs of future or proposed benefits increases, the estimates from the 1994 experiment should be viewed as upper bound. More research is clearly needed on this question.

In the same manner that we calculated duration-benefits elasticities for the duration effects, we can use the difference-in-difference estimates in Table 6 to calculate frequency-benefits elasticities. To do so, we first calculate the counterfactual proportion of claims among treatment group workers that would have collected indemnity payments had benefits not increase using the parameters from the regression models used to estimate the unadjusted and adjusted difference-in-difference effects in Table 6. Next, we use the difference-in-difference estimate to calculate the percentage increase in the proportion receiving TD benefits. The ratio of this increase to the percentage increase in benefits provides the elasticity estimate.

Table 7 presents elasticity estimates corresponding to each difference-in-difference frequency effect presented in Table 6. Beginning with the 1994 results for the sample overall, elasticity estimates for comparison (1) are quite stable across models and are centered around 0.5. The response of workers in the intermediate wage range is considerably smaller (approximately 0.2) while the elasticity estimates for the highest earning workers is somewhat larger (ranging from 0.59 to 0.70). For the estimates using the TD-only sub sample and the TD/PD sub sample, the elasticities are slightly higher, although the patterns across comparisons are similar to the patterns observed for the complete sample. The results for 1995 indicate much smaller responses as has already been discussed. Few of these elasticity estimates are based on statistically significant difference-in-difference estimates and several are negative.

Again, we can compare these elasticity estimates to those from previous research using alternative estimation strategies. Krueger (1990) provides a review of studies using aggregate data to test for changes in benefits levels on the total number of claims receiving TD benefits. Elasticity estimates from these studies (most of which use data for the 1970s and 1980s) range from 0.06 to 0.78, with a cluster of estimates around 0.30. In the same study, Krueger provides the first microdata estimates of frequency effects and the implicit benefits elasticities. Using longitudinal data constructed by matching consecutive samples from the March Current Population Surveys, Krueger estimates frequency elasticities ranging from 0.46 to 0.67. Hence, our 1994 estimates are quite close to Krueger's CPS estimates while the results for 1995 are within the range of estimates from research using aggregate data.

C. Sample-selection and the duration effect estimates

The results presented thus far demonstrate two patterns. First, among injured workers whose benefits were affected by the 1994 and 1995 increases, the average duration of benefit receipt increased significantly. Second, among affected workers, the likelihood of collecting indemnity benefits rather than filing a claim for medical expenses only also increased in response to the 1994 increase but not the 1995 increase. The 1994 findings indicate that the post-increase sample of injured workers may differ in composition from the pre-increase sample of workers due to the observed frequency effects. In addition, the frequency results for 1995 are consistent with a selection model where marginal claims come from the lower tail of the injury-severity distribution.

The frequency results raise several questions for our duration elasticity estimates and for the use of previous frequency estimates for future cost projections. First, if the duration distribution for the marginal claims differs from the duration distribution for the intra-marginal claims (where the margin of reference is that dividing claims that collect indemnity payments from claims that do not), our duration estimates would suffer from systematic sample-selection bias. Furthermore, if the marginal claims entail less severe injuries on average, duration effect estimates that do not account for sample selection are biased downwards. In addition, to the extent that marginal claims come from the lower tail of a severity distribution, consecutive benefits increases will yield smaller frequency responses. This follows mechanically from the shape of the statistical distribution assumed.

These possibilities hinge on the selection process of marginal claims into the pool of claims receiving indemnity benefits. In this section, we combine the two parts of our analysis to explore this issue. We begin by using variables describing the details of the injury to characterize marginal claims for indemnity benefits that respond to the increase in benefit levels. Specifically, we use detailed information on the COI (71 categories), the NOI (53 categories), and the BPI (55 categories), to assess which types of injuries exhibit positive frequency effects and which do not. For each of these three variables, we estimate separate unadjusted difference-in-difference frequency estimates for each possible category. For claims falling into COI, NOI, and BPI categories exhibiting statistically significant (at the 5 percent level) and positive frequency effects, the variable “Type” is coded to one. For other claims, this variable is coded to zero. Claims with the variable “Type” coded to one can be thought of as claims that are more likely to be on the margin between applying and not applying for benefits.

Since we do not observe significant frequency effects for the 1995 experiment, in this section we focus on the effects of the 1994 benefits increase. Table 8 shows the average days of benefit receipt for claims stratified by the value of the variable “Type”. To avoid any incentive effect on duration, we use the sample of injuries constrained to those with injury dates prior to the July 1, 1994 benefits increase. The first row presents figures for all claims, the second row presents figures for TD-only claims, while the final row presents figures for claims receiving both TD and PD benefits.

In all comparisons, mean duration is lower for frequency-effect claims than for non-frequency effect claims. For all claims, the difference is 4.5 days. For TD-only claims and TD/PD claims these differences are 6.9 and 5.9 days. The first two differences are statistically significant at the one percent level of confidence while the final difference (for the TD/PD claims) is not significant. Hence, the patterns in Table 8 clearly indicate that marginal claims (as defined by the coding process discussed above) have shorter durations on average and are likely to involve less severe injuries than non-marginal claims.

With this information in hand, we can now specify a sample-selection model using the heterogeneity in the frequency effect as an identifying exclusion restriction. We use the variable “Type” interacted with the treatment group dummy, the post-benefits increase dummy, and the triple interaction term between type, treatment group, and post increase in a probit equation modeling the process by which injuries select into the pool of injuries that collect indemnity benefits. Appendix Table A1 presents estimation results for three probit models, all of which define workers with weekly earnings exceeding $504 as the treatment group. The first equation uses the entire sample of claims, the second equation uses the medical only claims and the TD-only claims, while the final equation uses the medical only claims and the TD/PD claims. The models also include controls for gender, age, average weekly wages, and dummies indicating that these variables have missing values.[8]

For all three models, the *Type* variable exerts statistically significant effects on the selection process. Perhaps the most important coefficients are those on the triple-interaction term. The coefficient on this term indicates whether the increase among these types of injuries relative to non-frequency effect injuries among workers affected by the benefits increase exceeds the comparable relative effect among injured workers not affected by the benefits increase.[9] For all models, the coefficient on this term is statistically significant and positive.

We use these probit coefficient estimates to construct the inverse-mills ratio and to estimate the selection-correction model given in equation (12). Table 9 presents the results of this exercise for the full sample claims receiving TD benefits. The table presents the results from three regression models. The first regression is the basic model not correcting for sample selection. To ensure that the classification of claims by frequency type in the probit regression is not driving any of the correction results, we include the variable Type by itself and interacted with the post-increase and treatment dummies in this base model and in the two selection-corrected models. The second model adds the inverse mills ratio to the specification and hence, corrects for sample selection. The third model uses an alternative method of correcting for sample selection.[10] For model three, we first predict the probabilities of selecting into the sample for each claims receiving benefits using the probit coefficients in appendix Table A1. We then add this predicted probability, its square, and its cube to the base specification in model (1). We calculate robust standard errors for all models to account for the heteroscedasticity of the two-stage estimators.

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[8]We attempted to add 80 industry dummies to the model, but had difficulty with convergence. However, the results above indicate that controlling for the industry of the injured worker has no impact on the frequency effect estimates and the uncorrected duration effect estimates.

[9]This is a triple difference frequency effect estimate.

[10]The inverse-mills ratio specification constrains the functional form of the relationship between duration and the probability of selecting into the sample.

The results from regression (1) indicate a duration difference-in-difference estimate of 5.2 days. This is quite close to the estimates presented in Table 4 (although the specification differs slightly). Model (2) adds the inverse mills ratio to the specification. Adjusting for sample selection leads to a sizable increase in the duration effect estimate. The coefficient on the interaction term between the post increase dummy and the treatment dummy increases to 13.3 days, more than doubling. In addition, the coefficient on the inverse mills ratio is positive and statistically significant at the one percent level of confidence. Recall, the coefficient on this variable provides an estimate of the covariance between the residuals from the probit selection model and the duration equation. Hence the positive coefficient indicates that the unobservables that increase the duration of benefit receipt and the unobservables that increase the likelihood of claiming benefits are positively correlated. This result combined with the increase in the difference-in-difference estimate indicates that the marginal claims do indeed select from the bottom of the injury severity distribution.

Finally, regression (3) enters the probability of selection, its square, and its cube to correct for selection bias rather than the inverse mills ratio. The result from an F-test of the joint significance of the selection probabilities is presented at the bottom of the table. Again, adding the selection correction terms to the specification causes a large increase in the difference-in-difference estimate of the duration effect (relative to regression (1)). The duration effect estimate in model (3) increases to 16 days from 5.2 in model (1). The selection terms are jointly significant at the one percent level of confidence.

Table 10 presents comparable results for the TD-only sample and the TD/PD claims. To conserve space, we only report the coefficient on the interaction term between post-increase and treatment, the coefficient on the inverse mills ratio, and the coefficients on the three selection probabilities. For each sub-sample we present the results from three model specifications comparable to those in Table 9. Beginning with the results for the TD-only sample, adding the selection correction term does not cause an increase in the duration effect estimate. Moreover, the inverse mills ratio variable is statistically insignificant, indicating no evidence of selection bias among this group of claims. Similar results are found in the model adding the selection probability variables. For claims receiving TD and PD benefits, correcting for sample selection does impact the difference-in-difference duration estimates. Adding the inverse mills ratio in column (5) increases the duration effect estimate from 8.2 to 21.9 days. The inverse mills ratio is significant at the 5 percent level of confidence and has a positive coefficient as expected. Finally, the selection correction model that directly controls for the selection probabilities yields comparable results to the model with the inverse-mills correction variable.

The selection-corrected results presented in Tables 9 and 10 can be used to calculate alternative elasticity estimates that are corrected for changes in sample composition induced by the benefits change. Here we will focus on the first two specifications in Table 9 (the uncorrected and Heckman selection models) and the corresponding regressions in Table 10. For all claims, the uncorrected model in Table 9 indicates a duration response elasticity of 0.29 in response to the 1994 benefits increase. The selection-corrected model on the other hand, yields a duration-benefits elasticity of 0.82. Hence, the duration response to a change in benefits of those workers who would apply for indemnity benefits regardless of the change is considerably larger than the uncorrected estimates presented in Tables 4 and 5 suggest. For workers receiving TD and PD benefits, correcting for sample selection yields a benefit-duration elasticity of 0.62 compared with an uncorrected elasticity of 0.21. Since there was no evidence of selection among claims receiving TD benefit only, we did not calculate an alternative elasticity for this group.

5. Conclusion

The findings of this paper are several. Using two natural experiment for the state of California, we find duration responses to increases in workers' compensation temporary disability benefit increase that are comparable across experiments and comparable to those estimated in previous research. Specifically, we show that the average duration of benefit receipt increased significantly for workers experiencing an increase in indemnity payment relative to injured workers who do not experience the increase. Concerning frequency effects, we find that workers affected by the legislative change in 1994 were significantly more likely to collect indemnity payments conditional on being injured and at least filing for the costs of medical expenses. We did not find similar patterns for the second experiment. For those models where we find significant effects, the size of the frequency-benefits elasticity are comparable in magnitude to those from previous studies. On both margins, the response is somewhat inelastic, with an duration-benefits elasticity estimate of 0.3 and a frequency-benefit elasticity estimate of 0.5.

In addition, we find considerable evidence that the frequency effects observed for the 1994 experiment are driven by a non-random selection process by which workers with relatively less severe injuries respond to increases in benefits by applying for benefits when they otherwise would not. This selection bias dampens the duration-benefits elasticity estimate for injured workers who do not respond along the frequency margin and who would apply for benefits regardless of the whether the benefits increases that we observe in the data had occurred. Accounting for selection bias for this group of worker substantially increase the duration-benefits elasticity. Our corrected estimates yield benefits-duration elasticity estimates of approximately 0.8, compared with the uncorrected elasticity estimate of 0.3. While these are still inelastic responses, they are somewhat higher than the elasticity estimates found here and in past research that do not correct for sample selection.

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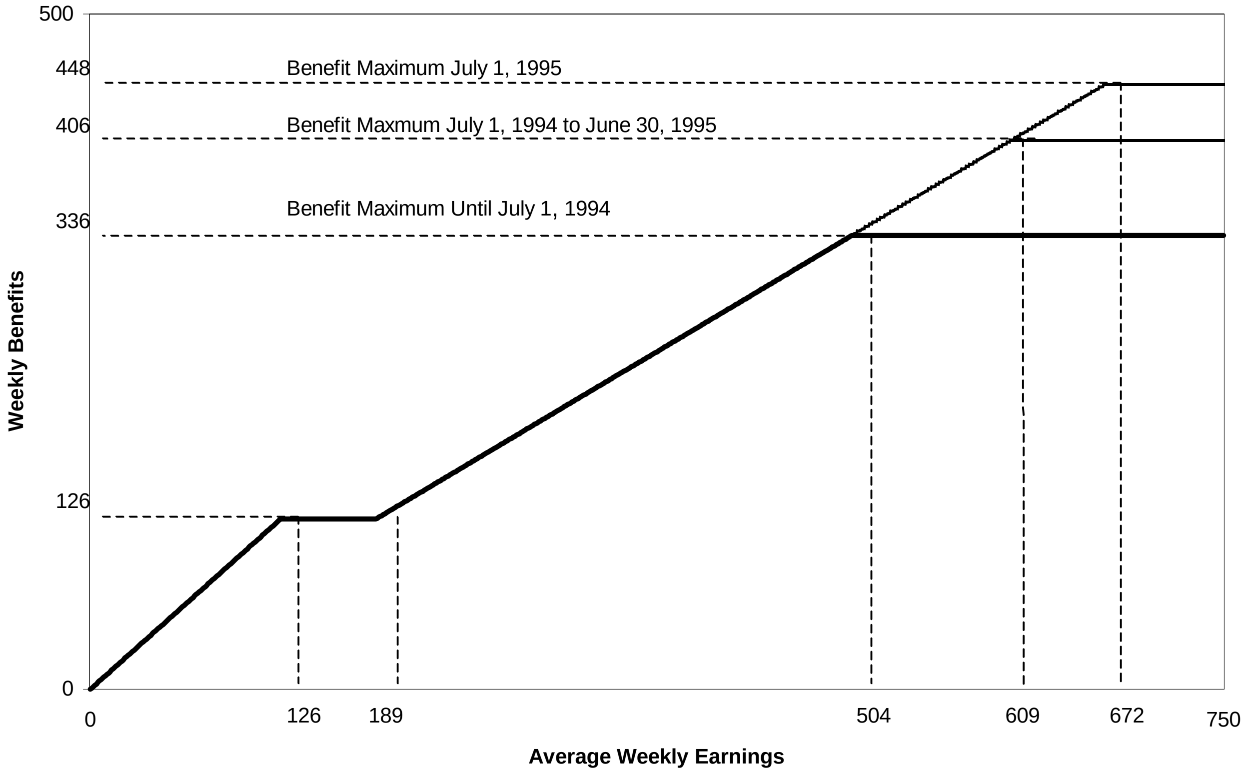
Figure 1



500
448
406
336
126
0
Benefit Maximum July 1, 1995
Benefit Maxmum July 1, 1994 to June 30, 1995
Benefit Maximum Until July 1, 1994
Weekly Benefits
0
126
189
504
609
672
750
Average Weekly Earnings

Figure 2

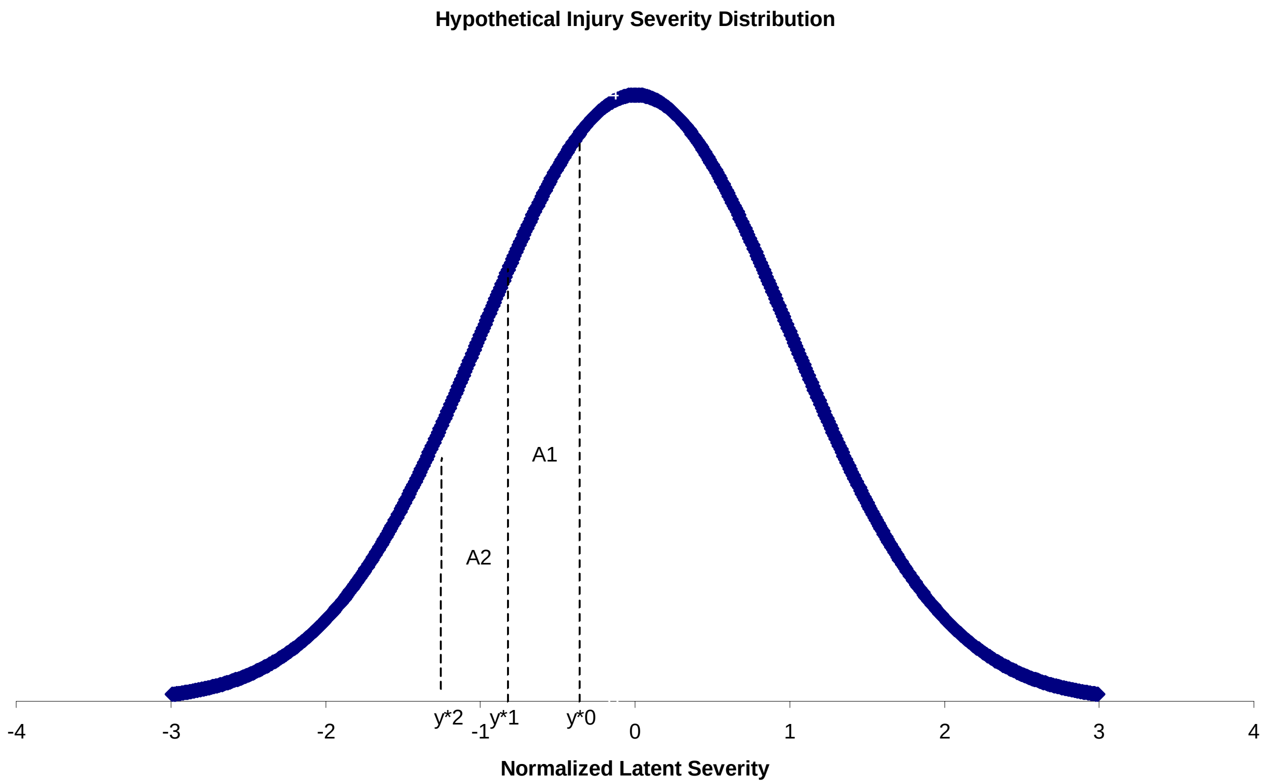

Hypothetical Injury Severity Distribution
A1
A2
y*2
y*1
y*0
-4
-3
-2
-1
0
1
2
3
4
Normalized Latent Severity

Table 1
Average Weekly Benefits for TD Claims by Time-Periods Bracketing the 1994 and 1995 Benefits Increase and Average Weekly Wage Categories

Panel A: Categories Bracketing the 1994 Benefits Increase

	All Claims	Injured Prior Benefit Increase	Injured After Benefit Increase	After-Before	% Change in Average Benefits
All Claims	249.46 (0.25)	238.57 (0.31)	259.85 (0.39)	21.28 (0.51)	8.91 %
Wages					
(1) Less than $505	198.81 (0.21)	198.44 (0.30)	199.17 (0.29)	0.73 (0.42)	0.36 %
(2) $505 plus	367.10 (0.16)	336.00 (0.00)	395.10 (0.13)	59.10 (0.14)	17.59 %
(3) $505 to $609	354.11 (0.19)	336.00 (0.00)	372.63 (0.24)	36.63 (0.24)	10.90 %
(4) $610 plus	374.04 (0.20)	336.00 (0.00)	406.00 (0.00)	70.00 (0.00)	20.83 %

Panel B: Categories Bracketing the 1995 Benefits Increase

	All Claims	Injured Prior Benefit Increase	Injured After Benefit Increase	After-Before	% Change in Average Benefits
All Claims	265.31 (0.30)	259.85 (0.39)	271.28 (0.45)	11.43 (0.59)	4.40 %
Wages					
(1) Less than $609	221.95 (0.25)	221.35 (0.35)	222.64 (0.36)	1.29 (0.50)	0.58 %
(2) $609 plus	424.51 (0.11)	406.00 (0.00)	443.74 (0.08)	37.74 (0.08)	9.30 %
(3) $609 to $672	416.15 (0.16)	406.00 (0.00)	427.92 (0.22)	21.92 (0.20)	5.40 %
(4) $673 plus	426.94 (0.14)	406.00 (0.00)	448.00 (0.00)	42.00 (0.00)	10.34 %

Standard errors are in parentheses.

Table 2
Mean Duration of Benefit Receipt by Time-Periods Bracketing the 1994 Benefits Increase and Average Weekly Wage Categories

	All Temporary Disability Claims				**Claims Receiving Temporary Disability Benefits Only**				**Claims Receiving Temporary and Permanent Disability Benefits**			
	All Claims	Injured Before Increase	Injured After Increase	After-Before	All Claims	Injured Before Increase	Injured After Increase	After-Before	All Claims	Injured Before Increase	Injured After Increase	After-Before
All Claims	105.36	105.71	105.03	-0.68	32.53	32.90	32.18	-0.72	224.71	223.23	226.14	2.91 (2.04)
	(0.49)	(0.69)	(0.68)	(0.97)	(0.21)	(0.29)	(0.29)	(0.42)	(1.02)	(1.45)	(1.44)	
Wages												
(1) <$505	103.57	104.70	102.47	-2.23	31.73	32.43	31.07	-1.36	224.73	224.41	225.05	0.64 (2.41)
	(0.57)	(0.81)	(0.79)	(1.14)	(0.23)	(0.33)	(0.32)	(0.46)	(1.21)	(1.71)	(1.71)	8.31 (3.83)
(2) $505 plus	109.86	108.32	111.24	2.91	34.61	34.18	35.00	0.81	224.66	220.31	228.62	22.47
	(0.16)	(1.34)	(1.32)	(1.89)	(0.44)	(0.61)	(0.62)	(0.87)	(1.92)	(2.72)	(2.69)	(6.51)
(3) $505 to $609	113.51	109.61	117.5	7.74	34.56	34.50	34.62	0.12	232.14	220.90	243.37	1.34 (4.74)
	(1.63)	(2.21)	(2.41)	(3.26)	(0.25)	(1.02)	(1.07)	(1.48)	(3.26)	(4.38)	(4.82)	
(4) $610 plus	107.95	107.60	108.24	0.64	34.64	34.00	35.18	1.18	220.69	219.98	221.31	
	(1.15)	(1.69)	(1.58)	(2.31)	(0.54)	(0.76)	(0.76)	(1.08)	(2.36)	(3.45)	(3.25)	
Duration Effect	-	-	-	5.14	-	-	-	2.18	-	-	-	7.67 (4.47)
(2) - (1)				(2.16)				(0.93)				
Duration Effect	-	-	-	9.97	-	-	-	1.48	-	-	-	21.84
(3) - (1)				(3.30)				(1.39)				(6.78)
Duration Effect	-	-	-	2.87	-	-	-	2.54	-	-	-	0.70 (5.23)
(4) - (1)				(2.52)				(1.07)				

Standard errors are in parentheses.

Table 3
Mean Duration of Benefit Receipt by Time-Periods Bracketing the 1995 Benefits Increase and Average Weekly Wage Categories

	All Temporary Disability Claims				**Claims Receiving Temporary Disability Benefits Only**				**Claims Receiving Temporary and Permanent Disability Benefits**			
	All Claims	Injured Before Increase	Injured After Increase	After-Before	All Claims	Injured Before Increase	Injured After Increase	After-Before	All Claims	Injured Before Increase	Injured After Increase	After-Before
All Claims	108.47	104.33	112.96	8.63	34.56	32.14	37.25	5.10	227.68	224.34	231.17	6.83
	(0.49)	(0.67)	(0.73)	(0.99)	(0.24)	(0.29)	(0.40)	(0.48)	(1.01)	(1.41)	(1.44)	(2.01)
Wages												
(1) <$609	107.57	103.61	111.95	8.34	33.45	31.45	35.71	4.62	228.56	225.70	231.59	5.89
	(0.55)	(0.74)	(0.81)	(1.09)	(0.26)	(0.30)	(0.42)	(0.51)	(1.13)	(1.58)	(1.62)	(2.25)
(2) $609 plus	112.00	107.30	116.76	9.46	39.05	35.08	43.10	8.02	224.34	219.08	229.64	10.56
	(1.12)	(1.54)	(1.63)	(2.24)	(0.63)	(0.75)	(1.03)	(1.26)	(2.24)	(3.14)	(3.20)	(4.49)
(3) $609 to $672	118.36	112.55	124.46	8.34	36.89	33.55	40.35	6.80	241.82	229.97	254.59	24.61
	(0.16)	(3.46)	(3.80)	(1.09)	(1.24)	(1.52)	(1.98)	(2.48)	(5.09)	(6.89)	(7.53)	(10.19)
(4) $673 plus	110.38	103.61	111.95	8.88	39.63	35.49	43.83	8.34	219.56	215.97	223.10	7.14
	(1.24)	(0.74)	(0.81)	(2.49)	(0.73)	(0.85)	(1.19)	(1.45)	(2.49)	(3.51)	(3.52)	(4.98)
Duration Effect	-	-	-	1.11	-	-	-	3.76	-	-	-	4.66
(2) - (1)				(2.45)				(1.20)				(4.95)
Duration Effect	-	-	-	3.57	-	-	-	2.54	-	-	-	18.72
(3) - (1)				(4.86)				(2.31)				(9.78)
Duration Effect	-	-	-	0.54	-	-	-	4.07	-	-	-	1.24
(4) - (1)				(2.69)				(1.32)				(5.45)

Standard errors are in parentheses.

Table 4
Regression-Adjusted Double-Difference Estimates of the Effect of the Increase in Temporary Disability Benefits on the Duration of Benefit Receipt

Panel A: Natural Experiment Using the July 1994 Benefits Increase

	All Temporary Disability Claims*			Claims Receiving Temporary Disability Benefits Only			Claims Receiving Temporary and Permanent Disability Benefits		
	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)
Model (1)	5.14 (2.16)	9.97 (3.30)	2.87 (2.52)	2.18 (0.93)	1.48 (1.38)	2.54 (1.07)	7.67 (4.47)	21.84 (6.78)	0.70 (5.23)
Model (2)	4.43 (1.80)	9.84 (2.74)	1.88 (2.10)	2.13 (0.92)	1.58 (1.38)	2.47 (1.06)	8.18 (4.46)	22.56 (6.75)	1.19 (5.22)
Model (3)	4.18 (1.79)	9.47 (2.72)	1.56 (2.09)	2.43 (0.89)	1.76 (1.34)	2.67 (1.04)	7.97 (4.42)	21.13 (6.69)	1.59 (5.17)

Panel B: Natural Experiment Using the July 1995 Benefits Increase

	All Temporary Disability Claims*			Claims Receiving Temporary Disability Benefits Only			Claims Receiving Temporary and Permanent Disability Benefits		
	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)
Model (1)	1.11 (2.45)	3.57 (4.86)	0.54 (2.69)	3.76 (1.21)	2.54 (2.32)	4.07 (1.33)	4.66 (4.96)	18.72 (9.79)	1.24 (5.45)
Model (2)	4.03 (2.05)	8.59 (4.04)	2.89 (2.25)	3.59 (1.20)	2.34 (2.30)	3.91 (1.32)	4.57 (4.95)	17.87 (9.75)	1.27 (5.44)
Model (3)	3.47 (2.03)	8.29 (4.01)	2.22 (2.23)	3.14 (1.18)	2.51 (2.25)	3.26 (1.30)	4.18 (4.88)	15.61 (9.64)	1.67 (5.37)

Standard errors are in parentheses. The double-difference estimates are the coefficient on the interaction term between a dummy variable indicating a post-benefit increase observation and a dummy variable indicating a worker in the wage range affected by the increase. For the 1994 experiment, comparison (1) defines workers with weekly wages in excess of $504 as the treatment group and workers with $504 or less in weekly earnings as the control group, comparison (2) defines workers with earnings of $505 to $609 as the treatment group and workers with $504 or less in weekly earnings as the control group, and comparison (3) defines workers with earnings that are $610 or more as the treatment group and workers with $504 or less as the control group. For the 1995 experiment, comparison (1) defines workers earnings in excess of $609 as the treatment group and workers earning less than $609 as the control group, comparison (2) uses the same control group and defines workers earnings between $609 and $672 as the control group, while comparison (3) uses the same control group and defines workers earning in excess of $672 as the treatment group. Model one includes a dummy for a post-increase observation, a dummy indicating an observation in the treatment group, and an interaction between these two dummy variables. The double-difference estimates from model one correspond to the unadjusted double-difference estimates in Tables 1 through 4. Model (2) adds to the variables in model (1) controls for age and age-squared, a dummy variable indicating a male injured worker, and dummy variables indicating missing values for age and gender (less than one percent for each variable). Model (3) adds 71 dummy variables indicating the cause of the injury, 53 dummy variable indicating the nature of the injury, 55 dummy variables indicating the body part that was injured, and 80 dummy variables indicating the injured worker's two-digit industry of employment.

*. Duration effect models using the combined sample of claims (receiving TD benefits only plus those receiving TD and PD benefits) include an additional control variable indicating whether the claims is a TD-only claim in models (2) and (3).

Table 5
Duration/Benefits Elasticity Estimates Based on the Unadjusted and Regression-Adjusted Difference-in-Difference Duration Effects

Panel A: Natural Experiment Using the July 1994 Benefits Increase

	All Temporary Disability Claims			Claims Receiving Temporary Disability Benefits Only			Claims Receiving Temporary and Permanent Disability Benefits		
	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)
Model (1)	0.276**	0.855***	0.131	0.379**	0.416	0.376**	0.196*	0.897***	0.015
Model (2)	0.247**	0.894***	0.088	0.382**	0.459	0.373**	0.214*	0.960***	0.024
Model (3)	0.248**	0.876***	0.078	0.451***	0.534	0.429***	0.210*	0.908***	0.037

Panel B: Natural Experiment Using the July 1995 Benefits Increase

	All Temporary Disability Claims			Claims Receiving Temporary Disability Benefits Only			Claims Receiving Temporary and Permanent Disability Benefits		
	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)
Model (1)	0.101	0.529	0.044	0.979***	1.183	0.944***	0.220	1.450*	0.053
Model (2)	0.398**	1.421**	0.260	0.974***	1.138	0.915***	0.222	1.427*	0.059
Model (3)	0.345*	1.395**	0.203	0.880***	1.127	0.812**	0.204	1.252	0.076

The elasticity estimates are based on the difference-in-difference estimates presented in Table 4 and the benefits increases presented in Table 1. Base durations used to calculate the percent change in mean duration are calculated from the regression models used to estimate the difference-in-difference effects. The base year duration is calculated as the predicted value of the counterfactual benefits duration for effected workers had benefit levels not increase holding all control variables in the regression constant at the sample averages.

*. Calculated with a difference-in-difference estimate that is significant at the 10 percent level of confidence.

**. Calculated with a difference-in-difference estimate that is significant at the 5 percent level of confidence.

***. Calculated with a difference-in-difference estimates that is significant at the 1 percent level of confidence.

Table 6
Regression-Adjusted Double-Difference Estimates of the Effect of the Increase in Temporary Disability Benefits on the Likelihood of Filing for TD Benefits

Panel A: Natural Experiment Using the July 1994 Benefits Increase

	All Claims			Medical-Only and TD-Only claims			Medical Only and TD/PD Claims		
	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)
Model (1)	0.038 (0.004)	0.009 (0.006)	0.053 (0.005)	0.035 (0.004)	0.008 (0.007)	0.049 (0.004)	0.032 (0.004)	0.006 (0.007)	0.045 (0.005)
Model (2)	0.037 (0.004)	0.009 (0.007)	0.052 (0.005)	0.035 (0.004)	0.008 (0.007)	0.048 (0.004)	0.030 (0.005)	0.006 (0.007)	0.041 (0.005)
Model (3)	0.033 (0.004)	0.009 (0.006)	0.045 (0.005)	0.032 (0.004)	0.009 (0.007)	0.043 (0.005)	0.027 (0.004)	0.008 (0.006)	0.036 (0.005)

Panel B: Natural Experiment Using the July 1995 Benefits Increase

	All Claims			Medical-Only and TD-Only claims			Medical Only and TD/PD Claims		
	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)
Model (1)	0.004(0.004)	-0.013(0.009)	0.008 (0.005)	0.009 (0.004)	-0.002(0.010)	0.012 (0.005)	-0.005(0.005)	-0.024(0.010)	0.000(0.005)
Model (2)	0.005(0.005)	-0.012(0.010)	0.009 (0.005)	0.010 (0.005)	-0.001(0.010)	0.013 (0.005)	-0.004(0.005)	-0.024(0.010)	0.001(0.005)
Model (3)	-0.001(0.004)	-0.019(0.009)	0.004 (0.005)	0.006 (0.005)	-0.007(0.009)	0.009 (0.005)	-0.008(0.004)	-0.032(0.009)	-0.002(0.005)

Standard errors are in parentheses. The double-difference estimates are the coefficient on the interaction term between a dummy variable indicating a post-benefit increase observation and a dummy variable indicating a worker in the wage range affected by the increase. For the 1994 experiment, comparison (1) defines workers with weekly wages in excess of $504 as the treatment group and workers with $504 or less in weekly earnings as the control group, comparison (2) defines workers with earnings of $505 to $609 as the treatment group and workers with $504 or less in weekly earnings as the control group, and comparison (3) defines workers with earnings that are $610 or more as the treatment group and workers with $504 or less as the control group. For the 1995 experiment, comparison (1) defines workers earnings in excess of $609 as the treatment group and workers earning less than $609 as the control group, comparison (2) uses the same control group and defines workers earnings between $609 and $672 as the control group, while comparison (3) uses the same control group and defines workers earning in excess of $672 as the treatment group. Model one includes a dummy for a post-increase observation, a dummy indicating an observation in the treatment group, and an interaction between these two dummy variables. The double-difference estimates from model one correspond to the unadjusted double-difference estimates in Tables 1 through 4. Model (2) adds to the variables in model (1) controls for age and age-squared, a dummy variable indicating a male injured worker, and dummy variables indicating missing values for age and gender (less than one percent for each variable). Model (3) adds 71 dummy variables indicating the cause of the injury, 53 dummy variable indicating the nature of the injury, 55 dummy variables indicating the body part that was injured, and 80 dummy variables indicating the injured worker's two-digit industry of employment.

Table 7
Frequency-Benefits Elasticity Estimates Based on the Unadjusted and Regression-Adjusted Difference-in-Difference Frequency Effects

Panel A: Natural Experiment Using the July 1994 Benefits Increase

	All Claims			Medical-Only and TD-Only Claims			Medical-Only and TD/PD claims		
	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)
Model (1)	0.535***	0.186	0.664***	0.697***	0.244	0.866***	0.897***	0.248	1.158***
Model (2)	0.557***	0.201	0.700***	0.721***	0.254	0.903***	0.994***	0.291	1.136***
Model (3)	0.479***	0.199	0.585***	0.639***	0.269	0.772***	0.875***	0.396	1.127***

Panel B: Natural Experiment Using the July 1995 Benefits Increase

	All Temporary Disability Claims			Claims Receiving Temporary Disability Benefits Only			Claims Receiving Temporary and Permanent Disability Benefits		
	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)	Comparison (1)	Comparison (2)	Comparison (3)
Model (1)	0.099	-0.529	0.191	0.320*	-0.123*	0.383**	-0.208	-1.674**	0.009
Model (2)	0.124	-0.532	0.221*	0.360**	-0.059	0.418**	-0.204	-1.944**	0.055
Model (3)	-0.017	-0.800**	0.098	0.204	-0.395	0.292*	-0.432*	-2.573***	-0.114

The elasticity estimates are based on the difference-in-difference estimates presented in Table 6 and the benefits increases presented in Table 1. Base frequencies used to calculate the percent change in the propensity to file are calculated from the regression models used to estimate the difference-in-difference effects. The base year duration is calculated as the predicted value of the counterfactual filing frequency for effected workers had benefit levels not increase holding all control variables in the regression constant at the sample averages.

*. Calculated with a difference-in-difference estimate that is significant at the 10 percent level of confidence.

**. Calculated with a difference-in-difference estimate that is significant at the 5 percent level of confidence.

***. Calculated with a difference-in-difference estimates that is significant at the 1 percent level of confidence.

Table 8
Average Duration of Temporary Disability Benefit Receipt for Injuries Occurring During the Year Prior to the 1994 Benefits Increase by Whether there is a Statistically Significant Increase in the Relative Likelihood of Filing for Temporary Disability Indemnity Benefits Among Affected Injured Workers

	Categories with increased propensity to received TD benefits “Type=1"	Categories with no increased propensity to received TD benefits “Type=0"	Difference
All Claims	102.37 (1.33)	106.88 (0.82)	-4.51 (1.58)
TD-Only Claims	27.59 (0.48)	34.57 (0.48)	-6.98 (0.69)
Claims Receiving TD and PD Benefits	217.96 (4.34)	223.88 (1.54)	-5.92 (4.61)

Standard errors are in parentheses. Average durations are for the sample stratified by whether the claims have cause-of-injury, nature-of-injury, and body-part-injured codes that all exhibit statistically significant increases in the propensity to file.

Table 9
Difference-in-Difference Estimates of the Duration-Benefits Effect With and Without a Sample-Selection Correction: All Claims Receiving TD Benefits Around the 1994 Benefits Increase

	(1)	(2)	(3)
Post Increase	-1.972	-18.001	-23.973
	(1.253)	(6.798)	(7.891)
Treatment	-3.807	-6.208	-8.036
	(2.240)	(2.451)	(2.645)
Post Increase*Treatment	5.215	13.254	16.814
	(2.181)	(4.066)	(4.758)
Wage	0.000	-0.028	-0.037
	(0.003)	(0.012)	(0.014)
Male	-24.286	-12.748	-8.539
	(1.198)	(4.942)	(5.767)
Gender Missing	71.966	132.005	157.654
	(8.716)	(27.076)	(33.618)
Age	6.575	9.471	10.518
	(0.190)	(1.218)	(1.425)
Age^2	-0.062	-0.087	-0.096
	(0.002)	(0.011)	(0.013)
Age Missing	95.317	136.277	152.766
	(4.686)	(17.714)	(20.868)
Type	-0.734	30.871	45.863
	(1.682)	(13.349)	(16.609)
Type*Post Increase	-4.155	-4.094	-5.860
	(2.171)	(2.170)	(2.567)
Type*Treatment	-9.299	-4.699	-2.707
	(2.430)	(3.036)	(3.272)
Inverse Mills	-	135.275	-
		(55.972)	
Predicted Prob.[a]	-	-	-100.172
			(363.632)
$(\text{Predicted Prob.})^2$	-	-	-401.936
			(638.470)
$(\text{Predicted Prob.})^3$	-	-	255.674
			(409.349)
R^2	0.025	0.025	0.025
F-statistic[b] (P-value)	-	-	3.938
			(0.008)
N	123,925	123,925	123,925

Standard errors are in parentheses. All regression include a constant term. Workers earnings in excess of $504 are classified as the treatment group.
a. The predicted probability variable is the predicted probability of selecting into the sample calculated from the first stage probit model. The probit results are presented in Appendix Table A1.
b. This row provides the test-statistic and p-value from an F-test of the joint significance of the predicted probability, the predicted probability squared, and the predicted probability cubed.

Table 10
Difference-in-Difference Estimates of the Duration-Benefits Effect With and Without a Sample-Selection Correction: TD Only and TD and PD Claims

	Claims Receiving TD Benefits Only			Claims Receiving TD and PD Benefits		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Increase*	2.354	1.889	0.993	8.209	21.949	17.365
Treatment	(0.981)	(1.580)	(1.557)	(4.533)	(7.276)	(7.981)
Inverse Mills	-	-7.371	-	-	194.775	-
		(16.151)			(79.541)	
Predicted Prob.[a]	-	-	-260.379	-	-	-623.837
			(175.567)			(442.989)
(Predicted Prob.)^2	-	-	653.402	-	-	644.658
			(418.762)			(663.493)
(Predicted Prob.)^3	-	-	-471.566	-	-	-241.891
			(328.448			(500.638)
R^2	0.008	0.008	0.008	0.006	0.006	0.006
F-statistic[b]	-	-	3.465	-	-	2.517
(P-value)			(0.015)			(0.057)
N	76,962	76,96	76,962	46,963	46,963	46,963

Standard errors are in parentheses. All regression include a constant term and all of the variables listed in the specifications in Table 9.
a. The predicted probability variable is the predicted probability of selecting into the sample calculated from the first stage probit model. The probit results are presented in Appendix Table A1.
b. This row provides the test-statistic and p-value from an F-test of the joint significance of the predicted probability, the predicted probability squared, and the predicted probability cubed.

Appendix Table A1
Probit Regression Models of Claim Frequencies Used to Construct the Inverse Mills Selection-Correction Terms

	All Claims	Medical-Only and TD-Only Claims	Medical Only and TD/PD Claims
Constant	-0.750	-0.611	-2.085
	(0.021)	(0.023)	(0.030)
Treatment	-0.017	0.004	-0.033
	(0.012)	(0.013)	(0.014)
After	-0.175	-0.168	-0.184
	(0.007)	(0.007)	(0.008)
Treatment*After	0.068	0.051	0.077
	(0.012)	(0.014)	(0.015)
Wage	-0.0003	-0.0003	-0.0003
	(0.00001)	(0.00004)	(0.00002)
Type	0.414	0.425	0.393
	(0.011)	(0.013)	(0.020)
Type*After	-0.057	-0.068	-0.134
	(0.014)	(0.017)	(0.027)
Type*Treatment	-0.011	-0.028	-0.183
	(0.021)	(0.024)	(0.039)
Type*Treatment*After	0.120	0.195	0.215
	(0.028)	(0.033)	(0.053)
Male	0.134	0.211	0.022
	(0.006)	(0.007)	(0.007)
Gender Missing	0.791	0.426	1.108
	(0.043)	(0.057)	(0.048)
Age	0.033	0.011	0.072
	(0.001)	(0.001)	(0.002)
Age Missing	0.482	0.175	0.902
	(0.031)	(0.033)	(0.048)
Age^2	-0.0003 (0.00001)	-0.00009	-0.0007
		(0.00001)	(0.00002)
N	254,108	207,145	177,146

Standard errors are in parentheses.