Medical Care Spending and Labor Market Outcomes: Evidence from Workers' Compensation Reforms*

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Abstract

Injuries sustained at work represent large income and welfare losses to households and there is a significant policy interest in reducing these burdens. Workers' compensation program is a large government program which provides monetary and medical benefits to injured workers. Despite the potential importance of medical care in improving the health and labor productivity of injured workers, little research has addressed the relationship between medical care provided through workers' compensation and post-injury labor outcomes. This paper exploits the 2003-2004 California workers' compensation reforms which reduced medical care spending for injured workers with a disproportionate effect on workers suffering low back injuries. We study the differential impact of this reduction in medical care generosity on post-injury outcomes, using administrative data which includes claim-level medical costs, pre- and post-injury labor earnings, and earnings information for matched (uninjured) workers at the same pre-injury firm. Our focus on labor outcomes is motivated by the importance of understanding the relationship between health and labor productivity more broadly and by the policy interest in mechanisms to improve the labor outcomes of injured workers. Adjusting for injury severity and selection into workers' compensation, we find that workers with lower back injuries experienced a 7.3% greater decline in medical care after the reforms, and that this led to an 8.3% reduction in post-injury earnings relative to other injured workers. We estimate that this earnings decline is due both to an increase in injury duration and to lower earnings conditional on working.

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JEL classification: I12, I13, J24, J28, J38

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1 Introduction

Workers' compensation provides the primary source of compensation for the lost wages and medical expenses associated with workplace injuries in the United States. In 2011, there were 3.8 million reported nonfatal occupational injuries and illnesses, 1.2 million of which resulted in days away from work (Bureau of Labor Statistics [2012]). In aggregate, workers' compensation was a $60.2 billion program in 2011 (Sengupta, Baldwin, and Reno [2013]), which is equivalent in size to the Earned Income Tax Credit (EITC).[1] Almost half of these costs, $29.9 billion, were medical payments. While there is a significant policy interest in increasing the labor productivity of injured workers and policy changes have direct influence on the medical care provision for injured workers, little economic research has addressed the role of medical care in improving post-injury labor outcomes.

The rapid growth in per capita medical costs in the United States is well-documented, but medical costs per injured worker in workers' compensation programs have far outpaced overall health spending growth. Figure 1 shows this relationship using data on per capita health care expenditures from the National Health Expenditure Accounts (NHEA) and data on workers' compensation medical costs per injury from the National Academy of Social Insurance (NASI),[2] normalizing both trends to 100 in year 1995. Figure 1 shows that medical spending in workers' compensation rose over 60% faster than overall per capita health expenditures from 1995 to 2010. We observe especially fast growth in workers' compensation relative to per capita health costs around 2000, the beginning of the time period that this paper will study. The growth rate of the workers' compensation leveled off significantly from 2004-2007, partially due to reforms adopted in California and elsewhere that were aimed at curbing costs.

This paper studies the 2003-2004 California workers' compensation reforms which dramatically reduced medical care spending for injured workers. California has the largest workers' compensation program in the United States and high per worker costs relative to other states. With 464,100 work-related reported injuries in 2011 (Bureau of Labor Statistics [2012]), California accounts for 12% of the country's work-related reported injuries, yet it accounts for 17% of national workers' compensation costs ($10.5 billion) and 20% of national workers' compensation medical costs ($5.9 billion) (Sengupta, Baldwin, and Reno [2013]). Workers' compensation costs were especially high in California prior to the 2003-

[1] The EITC cost the federal government $62.9 billion in 2011 (IRS [2013]).

[2] See Sengupta, Baldwin, and Reno [2013].

2004 reforms. Immediately before the reforms, employers were paying $6.29 per $100 of payroll (Workers' Compensation Insurance Rating Bureau of California [2013a]) for workers' compensation insurance with a significant portion of this cost due to medical care spending. Medical costs per injured worker were more than twice as large as the national average in 2002 (calculations made using Williams, Reno, and John F. Burton [2004]). In response to these high costs, California implemented a series of reforms to curb rising spending through such measures as utilization review and limits on certain types of medical care.

These reforms represent a natural experiment to estimate the causal relationship between health care and labor market outcomes, such as earnings and return to work. We estimate whether an exogenous decrease in medical care expenditures for individuals with work-related injuries harms their future labor outcomes. While the reforms affected several dimensions of workers' compensation such as indemnity benefits and vocational rehabilitation, these components were applied uniformly to all injuries while the medical care reforms disproportionately targeted medical treatments associated with low back injuries. This differential impact on medical care spending was predictable given pre-reform utilization of these treatments by injury type.

We use linked administrative data combining workers' compensation claims with earnings records. The workers' compensation claims were obtained from the Workers' Compensation Insurance Rating Bureau (WCIRB) of California, which records detailed information on workers' compensation claims by insured firms in California, for workers injured between 2000 and 2006. We linked these claims to unemployment insurance records from the Employment Development Department (EDD) to obtain pre- and post-injury earnings information. Furthermore, we matched each injured worker with up to 5 non-injured workers at the same firm based on pre-injury earnings and tenure to obtain earnings information for a set of "control workers." This matching allows us to observe the drop in earnings associated with the worker's injury relative to workers with similar pre-injury earnings capabilities, and we can test how the magnitude of this earnings drop is related to differential changes in medical care resulting from the workers' compensation reforms.

The lack of evidence about the effectiveness of workers' compensation medical care likely reflects the difficulties of isolating a causal impact given that individuals receiving more care tend to be less healthy. This issue parallels the identification problems found more broadly in the literature studying the returns to additional medical care spending. Previous work on the effectiveness of medical care spending has attempted to address these concerns by comparing population-level averages of medical spending and health, finding little relationship between health outcomes and medical expenditures across countries or regions (see Garber and Skinner [2008] and Skinner [2012] for extensive discussions). More recent work has used natural experiment-type approaches to isolate differences in medical care spending from health status (e.g., Doyle [2011], Doyle et al. [2014]). Furthermore, this literature has struggled to quantify health for assessing the effectiveness of medical care. Past work often focuses on comparatively crude measures of health, most notably mortality or subjective valuations, to measure the effectiveness of additional spending. The usefulness of these metrics in explaining a wide spectrum of health is debatable.[3]

Although the economic literature has long noted the possibly important relationship between health and labor outcomes (Currie and Madrian [1999]), there is little work on the potential of medical care to improve labor productivity, as highlighted by Hirth et al. [2003]. Health is a critical component of an individual's and an economy's productivity, yet there is limited evidence concerning whether and how medical care affects labor outcomes (see Garthwaite [2012]). When evaluating the cost-effectiveness of additional medical spending, researchers and policymakers should include additional work capacity and labor earnings as potential benefits. We have a useful experiment for observing how exogenous shifts in health can impact labor productivity.

We estimate that the injuries most affected by the reforms - lower back injuries - experienced a 7.3% reduction in medical expenditures relative to other injuries and that workers incurring lower back injuries consequently experienced an 8.3% relative decrease in post-injury earnings. These percentages translate to a $767 reduction in medical spending and a reduction in post-injury earnings of $2,567 over the first 18 months post-injury. This large response is due partially to injured workers delaying their return to work when medical spending is less generous, though we find evidence of a substantial earnings drop even conditional on injury duration. Furthermore, we provide evidence that the differential shocks to medical care generosity were not correlated with changes to other types of workers' compensation benefits.

Our results are robust to concerns about differential selection into the workers' compensation system. We account for the possibility that the incentives to report injuries changed disproportionately for some individuals and injury types over our time period. This

[3]Mortality is the most extreme manifestation of poor health which is potentially uncorrelated with critical health improvements at lower levels of the health distribution. Subjective measures tend to be noisy proxies for health with biases that may be systematically related to medical spending.

selection issue is potentially problematic if the reporting of less severe injuries is more elastic to medical care generosity and differs by type of injury. We model and account explicitly for these possible selection mechanisms.

While our experiment and outcomes are specific to the workers' compensation context, this context is especially policy-relevant. The California workers' compensation reforms were major reforms to the largest workers' compensation program in the United States and many states have since enacted similar reforms. There is a significant policy interest in understanding mechanisms to improve the labor outcomes of injured workers, and the literature has generally overlooked medical care generosity as a means of improving labor productivity. Furthermore, studying medical care in workers' compensation has several advantages in the broader literature on the effectiveness of medical care spending. Policy debates over health care often focus on the idea that there is a sizeable amount of "low-value" medical care. The rationing of medical treatment through utilization review and other mechanisms of managed care is often promoted as a means to improve value in health care through the elimination of inefficient medical care. These debates are persistent in the workers' compensation arena and more broadly. The workers' compensation context provides a useful laboratory to study a type of medical care reform which is commonly proposed as a means of controlling costs.

More generally, it is rare for policy to have such a direct impact on the medical care treatment that an individual receives. We view this paper as making several contributions. First, we provide some of the first economic evidence about the role of medical care generosity on the post-injury outcomes of injured workers. Second, our work provides evidence about the causal impact of medical care spending on health. Third, a small literature has highlighted the role of health in labor outcomes but struggled with identification issues given that labor outcomes may also directly affect health. This paper uses a policy shock to medical care generosity and studies its labor consequences with health as the implicit channel. Overall, our results suggest jointly that health is an important component of labor productivity and that health investments in injured workers can increase earnings capacity.

The next section provides an overview of the California workers' compensation system and describes the reforms that provide our exogenous variation in medical spending. We also discuss the prior literature on the relationship between medical care and health and labor productivity. Section 3 discusses the data. We introduce the empirical strategy in

Section 4 and the results in Section 5. Section 6 concludes.

2 Background

While workers' compensation programs are significant government programs and important medical care providers in the United States, the medical component of these programs has been understudied. A small literature studies the effects of the generosity of wage replacement in workers' compensation on incidence and injury duration (Butler and Worrall [1985], Krueger [1990], Ruser [1991], Meyer, Viscusi, and Durbin [1995], Bolduc, Fortin, Labrecque, and Lanoie [2002], Neuhauser and Raphael [2004]). Gruber and Krueger [1991] studies the wage incidence of workers' compensation employer costs. The economics literature has largely ignored the medical side of workers' compensation, yielding little evidence about whether reductions in medical care care generosity are associated with poorer post-injury outcomes.

2.1 Workers' compensation reform in California

Workers' compensation in the United States covers 125.8 million workers at a cost to employers of $77.1 billion, or $1.27 per $100 of payroll (Sengupta, Baldwin, and Reno [2013]). Workers' compensation is a series of state programs requiring employers to provide workers with certain predefined benefits when they suffer an injury or illness as a result of their work. Currently all 50 states and the federal government have workers' compensation laws in place, though it is optional in Texas. Almost all workers are eligible for compensation if they are injured, with only a few types of workers exempt from coverage. Additionally, virtually all workplace injuries are eligible for compensation. Workers' compensation provides no-fault coverage for workplace injuries, with a few exemptions for certain kinds of worker misconduct such as substance abuse, meaning that workplace causality is the determining factor for eligibility. Traditionally, the bar that workers have had to meet to have an injury deemed as work-related has been fairly low, with only a small connection to work generally sufficient to ensure compensation.

While compensation generosity for earnings losses varies considerably across states, all states require employers to provide full compensation for necessary medical expenditures with almost no employee cost-sharing. To eliminate costly and uncertain lawsuits, workers' compensation in the United States developed in the early 1900's as a generous system offering income benefits and reimbursement of nearly all medical costs regardless of fault (Fishback and Kantor [1998]). As a result, medical care in workers' compensation has historically excluded cost-sharing as a method to constrain utilization.

The cost of medical care in workers' compensation cases would not necessarily be a public policy concern if it represented efficient utilization of care. The empirical evidence suggests, however, that differences in medical expenditures between workers' compensation and other settings cannot be explained by differences in the nature or severity of injury. Several studies have shown that comparable injuries lead to significantly higher medical expenditures when workers' compensation pays for the treatments rather than private insurance (Baker and Krueger [1995], Durbin et al. [1996], Johnson et al. [1993, 1996]). States have consequently adopted utilization review and other mechanisms to contain the growth of medical expenditures in workers' compensation cases.

In the early 2000's, costs in the California workers' compensation program had risen dramatically. Between 1995 and 2002, the insurance premiums charged to employers to provide workers' compensation coverage increased from $5.5 billion to $14.7 billion (Division of Workers Compensation [2003]). Excessive payments and unpredictable costs caused many insurance companies to liquidate or withdraw from the workers' compensation market. Twenty-six insurance companies became insolvent in California between 2000 and 2003 (The California Commission on Health and Safety and Workers' Compensation [2008]). A report of the California workers' compensation market in 2003 states, "One of the main cost drivers we found was the extreme pressure from medical costs" (Hays Companies [2003]). For claims involving lost work time, the average insurer medical payment increased from $9,041 in 1993 to $25,560 in 2002 (Commission on Health and Safety and Workers' Compensation [2006]). In response to these rising costs, California adopted dramatic reforms to the delivery of medical care in workers' compensation beginning in 2003.

California Senate Bill 228 (SB 228) and Senate Bill 899 (SB 899) made several revisions to the provision of medical care related to workplace injuries. SB 228 implemented utilization review and adopted the American College of Occupational and Environmental Medicine (ACOEM) Occupational Medical Practice Guidelines as a set of treatment guidelines. SB 899 strengthened the use of these guidelines, giving insurers the ability to reject or delay care that did not conform to the recommendations of the guidelines. SB 228 also targeted utilization directly by placing limits on certain types of care. Physical therapy and chiropractic visits were each limited to 24 visits per injury. SB 899 limited occupational therapy to 24 visits as well. Finally, the reforms tied the fee schedule in workers' compensation to Medicare, lowering reimbursement rates. All of these factors reduced medical costs associated with an injury, through changes in prices and utilization. These reforms reduced medical costs almost immediately. Swedlow [2008] reports reductions of 60.2% in the number of physical therapy visits and 68.0% reductions in the number of chiropractic manipulations visits in the first quarter of 2005 relative to 2002. Estimates suggest that the medical costs for insurers fell by approximately 24% from 2003 to 2007 (Workers' Compensation Insurance Rating Bureau of California [2010]).

Importantly, for the purposes of studying the effect of the medical care reforms, the effects of these reforms on medical care spending were not uniform. The reforms disproportionately impacted low back injuries through limits on chiropractic care and physical therapy. This differential effect has been noted elsewhere and was predictable given the additional utilization of these services for low back injuries in the pre-reform period (see Swedlow [2005]). We can also observe these differences in our data. Figure 2 shows the ratio of mean medical expenditires for low back injuries relative to other injuries in our data (shoulder, knee, and hand/wrist). These injuries initially have almost the same mean medical care spending. The spending on low back injuries relative to other injuries rises slightly before the reforms and then drops substantially after the reforms. A sharp decline begins in 2004 when caps on chiropractic, occupational therapy, and physical therapy visits took effect. In the post-reform period, low back injuries have lower mean medical expenditures than the other injury types. These differential effects on medical expenditures across injury types will be key to our identification strategy, outlined below.

In the medical literature, research has found a relationship between physical therapy and injury duration for back injuries (Zigenfus et al. [2000]). A randomized trial for injured workers with work-related back injuries found that those receiving higher intensity care returned to work faster (Loisel et al. [1997]). Figure 2 also provides graphical evidence of an earnings effect and previews the main result of this paper. The ratio of post-injury earnings closely tracks the medical spending ratio. After the reforms, the post-injury earnings of those with lower back injuries drops relative to the post-injury earnings of workers with other types of injuries.

2.2 Medical care, health, and productivity

Our paper is further motivated by the difficulties in estimating the effectiveness of medical care more broadly. A vast literature finds that the United States has relatively high medical spending without better outcomes (see Garber and Skinner [2008] for a discussion). Fisher et al. [2003] finds that regions in the United States with higher Medicare spending do not have better health outcomes or higher satisfaction with care. Baicker and Chandra [2004] shows that higher spending areas tend to use less effective types of care. Fisher, Bynum, and Skinner [2009] tracks different trends in geographic medical expenditures and reports that faster growth in medical costs is not associated with better health outcomes. Recent work provides evidence that differences in patient health cannot entirely explain geographic variation in medical spending. Finkelstein, Gentzkow, and Williams [2014] studies the change in costs associated with patients who move to higher (or lower) spending areas and finds that geographic spending predicts changes in individual costs.

Recent evidence questions the ineffectiveness of the marginal dollar of care in the United States. Chandra and Staiger [2007] finds that regional specialization driven by productivity spillovers mutes the aggregate relationship between spending and outcomes. Romley, Jena, and Goldman [2011] shows that inpatient mortality is lower at hospitals with higher spending. Luce et al. [2006] and Cutler et al. [2006] find large returns to additional medical care spending. Cutler [2007] uses variation in distance to a hospital capable of providing revascularization to estimate the causal effect of revascularization after a heart attack and reports significant effects on mortality. Doyle [2011] uses geographic variation in medical spending in Florida for patients that were visiting the state to circumvent concerns that there are underlying differences in the health of individuals living in high spending areas. Doyle et al. [2014] uses ambulance referral patterns to predict patient costs, finding that higher costs are associated with lower mortality.

Even if patients are effectively randomly-assigned to a higher spending/utilization provider, this higher spending may be correlated with unobservable factors at the provider-level. For example, less efficient hospitals may spend more (or less), and randomization at the patient-level does not address this issue. Consequently, it is difficult to determine whether the additional spending affects health outcomes or whether unobserved factors lead to both different outcomes and additional spending.

Understanding the role and importance of health in explaining labor outcomes has also interested economists despite the empirical challenges in isolating a causal relationship. As Currie and Madrian [1999] summarize the literature, "[A]lthough many studies attempt to go beyond ordinary least squares in order to deal with measurement error and the endogeneity of health, it is difficult to find compelling sources of identification. The majority of these studies rely on arbitrary exclusion restrictions, and estimates of some quantities appear to be quite sensitive to the identification assumptions" (p., 3320).

It is generally accepted that poor health will lower labor force participation, but the effectiveness of medical care in improving productivity remains understudied. Most of the literature focuses on mortality or subjective measures of health. However, Garthwaite [2012] studies the effect of the removal of Cox-2 inhibitors from the market on labor force participation and labor earnings for ages 55-75. This study concludes that pharmaceutical innovations can improve labor outcomes for an older population. Our paper contributes to the literature studying the effect of health on labor outcomes by exploiting a plausibly exogenous policy shock which affects medical care and, subsequently, health.

Overall, it is difficult to find variation in medical spending that is plausibly unrelated to health status at the individual- or geographic-level. It is rare in the literature to use a legislative policy change which directly altered the provision of medical care. Studying medical care in workers' compensation, which is subject to legislative reforms, allows us to examine the impact of variation in medical expenditures for reasons unrelated to health status or individual choices that are potentially related to health.

3 Data

We use data on workers' compensation claimants from the Workers' Compensation Insurance Rating Bureau (WCIRB). WCIRB collects information from licensed workers' compensation insurers in California for the purposes of calculating recommended premium rates. The WCIRB data includes information on the date of injury, indemnity benefits, defense costs, medical costs, type of injury, and severity of the injury. The data include all permanent disability and temporary disability claims with total costs of $2,000 or more for 2000-2006.

We matched these data to earnings data from the base-wage file maintained by the California Employment Development Department (EDD). All employers covered by unemployment insurance are required to report quarterly earnings to the EDD. This matching gives us pre-injury and post-injury earnings information for all individuals in our sample. The pre-injury earnings information will be useful to control for individual-level labor productivity. The post-injury earnings data allow us to study the effect of the reforms on post-injury labor outcomes.

We also matched each injured worker in our data to up to 5 co-workers at the same (pre-injury) firm. These control workers were matched based on earnings in the year prior to the injured worker's injury and tenure at the firm. The control workers are useful because they, on average, have the same earnings as the injured worker, which we will show graphically below. The control workers help account for secular earnings trends which may vary by industry and differential changes in the pre-injury labor productivity of those filing for workers' compensation. Our data are explained in greater detail in Seabury et al. [2011].

A limitation of the WCIRB data is that the data only include workers injured at insured firms, excluding self-insured firms. This should not be problematic given that our source of variation is based on injury type, and firms cannot self-insure only certain types of injuries while remaining insured through an outside company for other injuries. Our specifications include industry-year fixed effects and these should account for firm-level entry and exit decisions. Furthermore, there is little evidence that the reforms themselves impacted the decision to self-insure. Between 2000 and 2006, the self-insurance market share was relatively consistent, ranging from 25.4% to 26.8% with little evidence of systematic trends (Division of Workers Compensation [2014]).[4]

We also account for selection on this dimension more directly in our empirical analysis and we show that our results are similar whether we model firm-level selection or not. Finally, we use data from the Disability Evaluation Unit (DEU), which include both insured and self-insured employers. The DEU is a state agency which collects information from medical reports to produce a disability severity rating for the workers' compensation program. These data were also linked to earnings data for both the injured workers and matched workers at the same firms. The main disadvantages of the DEU data are that the data do not provide information about medical costs, only include permanently disabled workers, and do not specifically categorize low back injuries. Instead, the DEU data include the cate-

[4] In the year prior to the reforms, the self-insurance rate was 26.4% while the rate was 26.5% in the year after the reforms were fully-enacted.

gory "neck, spine, or pelvis injury."[5] For these reasons, we focus our analysis on the WCIRB data. However, we show that our results are consistent with the DEU data, providing further evidence that the exclusion of self-insured firms is not biasing our results.

Table 1 presents summary statistics for the linked WCIRB data. We present these statistics separately for lower back injuries and other injuries – shoulder, knee, and hand/wrist injuries. Note that permanent lower back injuries tend to receive higher severity ratings than other permanent injuries, but a smaller fraction of lower back injuries are considered permanent. Earnings and medical expenditures are both highly-skewed, which will motivate the functional form of our main specification.

4 Empirical Strategy

We exploit the differential effect of the 2003-2004 California reforms on the medical care generosity for different types of injuries. It is rare in the economics literature to use legislative policy variation that directly impacts medical treatment for specific conditions. The California reforms reduced medical care generosity for all injured workers but disproportionately affected low back injuries. We compare the outcomes for workers with lower back injuries (19.0% of injuries[6]) to workers with other common work injuries: shoulder (7.6%), knee (12.4%), and hand/wrist (22.1%) injuries. These are the most common injury types in our data.[7] We chose these injuries because they are common and because they have similar per-injury medical costs and pre-injury earnings in the pre-reform period as shown in the bottom half of Table 1.

We exclude 2003 for most of our analysis because some of the reforms took effect in 2003 but many were not in effect until 2004. This decision has little effect on our final conclusions, as we will note below.

[5] Our assumption will be that this DEU category is primarily low back injuries, though we might expect to estimate an attenuated effect. Neck injuries are relatively rare in the WCIRB data while low back injuries in the WCIRB data include spine and pelvis injuries. These categories should have substantial overlap.

[6] These percentages exclude injuries not classified by a specific body part in the data (most of these are general trauma injuries), mental stress injuries, and mental disorders.

[7] One exception is a broader classification labeled "other lower extremity" injuries, which are 12.1% of all injuries. Our concern with the broadness of this category is that some of these injuries may also be disproportionately affected by the reforms so we exclude them from the analysis. Empirically, inclusion of this category has little impact on the final results, but we adhere to our original decision and exclude these injuries from our main analysis.

Injury severity affects both post-injury labor earnings and medical care consumption; thus, the relationship between labor outcomes and medical spending does not provide evidence about the causal impact of changes in medical care generosity. Instead, we study the differential impact of the reforms on lower back injuries for both of these outcomes. We estimate the specification:

$$y_{ijkqt} = \exp\left(\alpha_{jk} + \gamma_{jt} + \phi_{jq} + \theta\left[\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_k\right)\right] + \mu_i + \nu_{kt}\right)\eta_{ijkqt} \qquad (1)$$

where y_{ijkt} is a measure of post-injury earnings or medical expenditures for individual i working (pre-injury) in industry j with injury type k injured at calendar quarter q in year t. We include fixed effects for each industry-injury type, industry-year, and industry-calendar quarter. Time is based on the time of injury since the regulations governing medical care generosity depend on when the injury took place. Post_t is a variable equal to one in years 2004 and later. The reforms affected not only medical care generosity but also wage replacement rates, which potentially change post-injury labor supply behavior. The industry-year interaction terms account for common trends in these behaviors and net out effects of the reforms that are common across injuries, allowing us to isolate the differential impact of the changes in medical care generosity. We present results at the end which provide evidence that the other facets of the reforms are not biasing our results, implying that these industry-year interactions adequately account for other aspects of the reforms. The industry-time interactions also control for industry-specific economic trends during our time period.

We model our outcomes as an exponential of the variables of interest instead of using a log-linear form frequently used in applied work. Silva and Tenreyro [2006] discusses the limitations of a log-linear equation and the advantages of using an exponential form. We estimate equation (1) using Poisson regression. While it is frequently claimed that Poisson regression requires additional assumptions, Silva and Tenreyro [2006] illustrates that it actually requires fewer assumptions than OLS estimation of its log-linear counterpart because it places less structure on the error term, allowing both multiplicative and additive error terms. Furthermore, our data contain a non-trivial number of people with zero post-injury earnings so a log-linear specification would be inappropriate.

4.1 Identification Concerns

Our approach is a straightforward difference-in-differences strategy, exploiting a large policy shock while controlling for the uniform effects of the reforms and the fixed differences across injury types. However, given the richness of our data, we can explicitly account for two possible threats to our identification strategy. Our concerns stem from the nature of our experiment and data – we only observe reported injuries. We have explicitly modeled our concerns in equation (1) with μ_i and ν_{kt}, which we discuss in detail below. These terms represent possible changes in the types of individuals reporting injuries or possible changes in the distribution of injury severity. We should highlight that our industry-year interactions account for many of these potential issues, controlling for common trends in reporting on many dimensions. However, the richness of our data allows us to account for the possibility that reporting trends may be more systematic.

4.1.1 Worker Productivity Differences (μ_i)

One identification threat is that the reforms could be differentially correlated with changes in the underlying skill or labor productivity of the population reporting injuries to workers' compensation. By reducing generosity, the reforms may impact the type of individuals that report injuries. If this reporting effect change is not uniform within industry (across injury types), then $\text{Post}_t \times \mathbf{1}(\text{Back Injury}_k)$ is correlated with μ_i. We model μ_i as a function of the pre-injury wages of the injured worker and the post-injury wages of the control workers:

$$\begin{aligned}\mu_i &\equiv g(L_i) \equiv g(\text{Post-Injury Wages of Control Workers}_i, \text{Pre-Injury Wages of Injured Workers}_i) \\ &= \kappa_1 \ln(\text{Post-Injury Wages of Control Workers}_i) + \kappa_2 \ln(\text{Pre-Injury Wages of Injured Workers}_i).\end{aligned}$$

Our data contain a wealth of information on the earnings of similar workers (where similarity is based on firm, tenure at the firm, and pre-injury earnings) in the quarters after the worker's injury. Furthermore, we also have the pre-injury labor earnings by quarter before the injury for each person in our sample. Both sets of variables should provide sufficient information on the worker's earnings potential. If the earnings potential of the workers claiming workers' compensation changes over time, our rich earnings data should capture these changes. We denote the worker's earnings potential by L_i and will show results using different sets of the earnings control variables. In most specifications, we include pre-injury earnings for the 6 quarters preceding the injury (Q1-Q6).[8]

4.1.2 Injury Severity (ν_{kt})

Another possible concern is that the distribution of ν_{kt} may change by injury type over time. While we flexibly account for overall and industry-specific common shocks to the distribution of reported injuries through the inclusion of industry-time fixed effects, and for changes in workers' earnings potential (L_i), the reported injuries may differentially change in severity over time. If the incentives to report minor back injuries change relative to the incentives to report minor shoulder injuries, then $\text{Post}_t \times \mathbf{1}(\text{Back Injury}_k)$ is correlated with changes in the severity of reported injuries.

We show changes in reported injuries over time in Figure 3. Figure 3 graphs three relationships by quarter of injury. First, it shows the trend in the fraction of all reported injuries that are lower back injuries. It does not appear that the reforms directly affected this proportion. We also graph a measure of severity for low back injuries and, separately, all other injuries to see if the reforms differentially affected the types of injuries that were reported. Our measure of severity here is the fraction of injuries designated permanent injuries. Again, the data do not suggest that the reforms had an important effect. While this graph suggests that different changes in injury severity are not problematic, we still formally adjust for this possibility.

For illustrative purposes, let us assume that the reforms induced injured workers in a specific industry to not report the least severe low back injuries, but the reporting behavior for all other injuries did not change. In this case, we would observe a relative reduction in the number of low back injuries reported in the data for that industry and that reduction may be associated with outcome changes. In a separate industry, all low back injuries are severe injuries and are reported in each period. There is no reporting effect so we do not observe a reduction in the number of reported low back injuries. Consequently, we can separately identify the effects of differential reporting (occurring in the first industry) on observed outcomes from the causal impact (observed in both industries) of the policy.

We relate changes in the total number of injuries by industry and injury type to our outcomes, separately identifying the effect of changes in the total number of reported injuries

[8]For a small number of observations, the control workers' wages are equal to 0. We set ln(Post-Injury Wages of Control Workers$_i$) equal to 0 for these observations and include a separate dummy variable equal to 1 if and only if the control workers' wages equal 0.

from the independent (differential) effects of the reforms. There are many reasons that the number of reported injuries may change, including reasons that are correlated with changes in the labor productivity or injury severity for a given industry and injury type. Instead of using the actual number of reported injuries, we use the predicted number of reported injuries based on a factor that is uncorrelated with changes in medical expenditures or post-injury earnings. This approach resembles a selection model framework (see Newey [2009]).

During our sample period, the number of workers' compensation claims steadily decreased, not only in California but nationally (see Chart 1 in Workers' Compensation Insurance Rating Bureau of California [2013b]). We predict that industries with a large fraction of severe injuries should see smaller reductions in the total number of claims. Workers likely always report the most serious injuries, regardless of the incentives in the workers' compensation environment. We do not observe a consistent measure of severity,[9] but we can imagine selecting on industries which incur a disproportionate number of severe injuries for a specific injury type in the pre-reform era. In an extreme case, we could select on industries which only incur very severe industries. Differential reporting should not affect these industries so estimation of the causal effect is straightforward. While we do not observe industries with only severe industries, we do observe industries with different initial distributions of injury severity, predicting differential changes in the number of reported injuries.

We create a "selection instrument" $h_{jkt} = \text{Post}_t \times (\text{Pre-Reform Injury Severity Measure}_{jk})$. We should see a differential change in the number of injuries reported post-reform based on the initial severity distribution. This is testable and, in fact, we will show that our measure significantly predicts the number of injuries in each period. We model the number of reported injuries R as[10]

$$\ln R_{jkqt} = \tilde{\alpha}_{jk} + \tilde{\gamma}_{jt} + \tilde{\phi}_{jq} + \tilde{\theta}\left[\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_k\right)\right] + \eta h_{jkt} + \upsilon \equiv W'_{jkqt}\delta + \upsilon \qquad (2)$$

where h is excluded from equation (1). We model $E[\exp(\nu_{kt})|X_{ijkt}, \text{Report Injury}_{ijkt}] = \exp\left[\lambda(W'_{jkt}\delta)\right]$. The selection bias is a function of the number of reported injuries. Since we have an independent shock to the selection equation, we can separately identify this selection

[9] Alternative, we could "match" injuries based on severity in the pre- and post-reform era. We do not pursue this strategy because the reforms may have impacted the severity ratings themselves.

[10] We cannot distinguish between changes in the probability of reporting an incurred injury and changes in the probability of incurring an injury. For our purposes, however, this does not matter. Either of these factors could drive selection, but the selection correction should account for the effects of differential changes in the number of injuries for a given industry and injury type regardless of the source.

from the shock to medical care generosity. We estimate equation (2) using OLS,[11] and we use $\lambda(W'_{jkt}\delta) \equiv \psi W'_{jkt}\delta$. By including this term, we account for differential shocks to injury severity correlated with our $\text{Post}_t \times \mathbf{1}\,(\text{Back Injury}_k)$ interaction. The industry-injury type interactions in equation (1) account non-parametrically for the independent effects of the pre-reform injury severity measure.

Intuitively, we are estimating which industry-injury type cells should experience the smallest changes in the number of reported injuries. Then, we include the predicted number of injuries in the main specification to estimate the independent impact of changes in the number of reported injuries on the outcome variables. By separately identifying this term, we account for possible systematic biases resulting from selection and obtain consistent estimates of the relationship between our outcomes and $\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_j\right)$.

There is a mechanical selection effect inherent in our data because our data only include claims with total benefits in excess of $2,000. This threshold may differentially affect low back injuries since the reforms disproportionately reduced medical costs of low back injuries. Our selection adjustment should account for the potential differential "disappearance" of these less severe injuries in the post-reform period.

4.2 Final Specification

Accounting for our selection concerns, our final specification is

$$y_{ijkqt} = \exp\left(\alpha_{jk} + \gamma_{jt} + \phi_{jq} + \theta\left[\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_j\right)\right] + g(L_i) + \psi(W'_{jkt}\hat{\delta})\right)\eta_{ijkqt}, \quad (3)$$

where we assume that the conditional mean of η_{ijkqt} is 1. Because of the use of a two-part model, we bootstrap our standard errors using a clustered (by industry-injury type)[12] non-parametric bootstrap. We report 95% confidence intervals using the 2.5% and 97.5% percentile values estimated in the bootstrap. For this reason, the results with the selection adjustment do not have symmetric confidence intervals.

When the outcome is a binary variable for whether or not the individual has returned

[11] Though not shown, we have also used an exponential specification in this step and estimated with a Poisson regression. The results are not meaningfully different. We prefer OLS estimation because we include the predicted value of $\ln R$ in our main specification, not R.

[12] There are 84 separate industry-injury type combinations in our data.

to work, we estimate

$$P(\text{Work}_{ijkqt} = 1) = \Phi\left(\alpha_{jk} + \gamma_{jt} + \phi_{jq} + \theta\left[\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_j\right)\right] + g(L_i) + \psi(W'_{jkt}\hat{\delta})\right) \tag{4}$$

We report the impact of $\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_j\right)$ on the probability of working. We compute average marginal effects using the method discussed in Puhani [2012] to report this probability shift.[13] We calculate the change in probability due to the interaction variable for each observation in our data where this interaction is equal to 1. More specifically, we calculate the probability of working for each of these observations when the observation is equal to 1 and then subtract off the probability of working when we set this interaction term to 0. For inference, we use a nonparametric clustered bootstrap.

5 Results

5.1 Graphical Evidence

Figure 4 shows the trends in post-injury earnings by injury type, disaggregating the earnings effect shown in Figure 2 by injury type. Given the level differences in post-injury outcomes between low back injuries and the other injury types, we normalize post-injury earnings by the mean earnings in 2002, the year prior to the reforms. We find little evidence of pre-existing trends for low back injuries. Knee and hand/wrist injuries may have been trending downward before the reforms, which would work against finding a negative effect for low back injuries. While workers with low back injuries experience a decline in earnings at the same time that the reforms are implemented, the other injury types actually experience earnings increases beginning in 2004.

We can also utilize the earnings information of the control workers. We graph the ratio of earnings for the injured worker to the earnings for the control workers for each quarter relative to the time of the injury. Reflecting the selection criteria for the control workers, the ratio is close to one in the four pre-injury quarters prior to the injury. We have pre-injury earnings prior to that period, however, and find that the ratio is centered around one during that earlier period as well. This is not necessarily surprising, though it

[13] Puhani [2012] argues that the technique proposed in Ai and Norton [2003] does not provide the parameter of interest.

does affirm the value of the control group as a set of workers on a similar earnings trajectory prior to the injury.[14]

Figure 5a shows these graphs for each of the pre-reform years in our data.[15] After a lower back injury, injured workers' earnings drop to 64% in 2000 of the earnings of their uninjured peers in the control group, 65% in 2001, and 65% in 2002.[16] The earnings drop due to other injuries is smaller. Other injuries result in earnings of 77% in 2000, 75% in 2001, and 72% in 2002 relative to the control workers' earnings. Even in the pre-reform period, lower back injuries appear to result in worse post-injury labor outcomes.

Figure 5b includes the same graphs for the post-reform period. As noted above, our main analysis excludes 2003 since it is only a partially "treated" year, but we include it here for the sake of completeness. Lower back injuries resulted in earnings of 57% in 2003, 57% in 2004, 59% in 2005, 57% in 2006 relative to the control workers' earnings. These are large decreases relative to the pre-reform period. Workers with other types of injuries did not experience such decreases - 71% in 2003, 73% in 2004, 73% in 2005, 74% in 2006 of the control workers' earnings. Despite our caution at including 2003 as a post-reform year, the graphs seem to suggest that there was a large differential effect and this large effect began in 2003.

5.2 Regression Analysis

Table 2 reports the relationship between medical expenditures and post-injury earnings. Even conditional on fixed effects and our rich measures of labor productivity, people with more medical care consumption have worse post-injury outcomes. We estimate that a 10% increase in medical care spending is associated with a 2% reduction in labor earnings. This relationship likely reflects the additional demand for care associated with worse injuries. This pattern of results should highlight the benefits of studying a policy change which impacted workers for reasons unrelated - conditional on covariates - to injury severity.

Many of our specifications include a selection adjustment term. We construct a "selection instrument" which is a measure of severity in the first year of our data for each

[14]Different trends between injured and control workers would not be problematic given our empirical strategy if those trends remained constant after the reforms.

[15]Note that we do not have pre-injury earnings 12 quarters prior to the injury for 2000.

[16]These numbers are the averages across all 6 post-injury quarters.

industry-injury type combination. We use the severity rating in 2000 to classify an injury in the top 75% of permanent injuries. Thus, our selection instrument is

$$h_{jkt} \equiv \left(\frac{\text{Number of injuries in 2000 that are in the top 75\% of all permanent injuries}_{jk}}{\text{Number of temporary and permanent injuries in 2000}_{jk}} \right) \times \text{Post}_t.$$

We estimate equation (2) using the selection instrument. We present the results in Table 3. The first column presents the results we will use to predict our selection adjustment variable for most of the results shown below. We also show that the significance of our selection instrument is robust to the inclusion of industry × quarter of injury × year of injury interactions. The results support the idea that industry-injury types with more severe injuries are less likely to "lose" observations in the post-reform period. Consequently, our selection equation is identified and we can include a prediction of the log of the number of reported injuries for each industry-injury type. Our selection adjustment will refer to this prediction. In the last two columns, we use all permanent injuries in the initial period to predict changes in the number of injuries and find that this instrument is also a statistically significant predictor. We use this selection instrument in a robustness check below. Furthermore, Table 3 provides some evidence that lower back injuries did incur a differential change in reporting correlated with the reforms, suggesting that accounting for this selection is potentially important.

Before studying the effect on labor outcomes, we examine whether the reforms affected medical care spending for low back injuries more than other injuries. In Table 4, we present several results, varying the inclusion of controls for the labor earnings of the matched control workers, controls for pre-injury labor earnings of the injured workers, and the selection adjustment. Columns (1)-(6) do not include the selection adjustment term. We start without any controls for labor productivity and estimate an effect of -0.075, significant at the 1% level. Adding the earnings of the control workers in Column (2) has little effect. Column (3) conditions on pre-injury earnings, which also has little effect on the estimate. Column (4) only uses earnings for two to six quarters before the injury as controls and we obtain a similar estimate. We exclude earnings in the quarter immediately prior to reporting an injury to test for the possibility that workers may delay reporting an injury, which would imply that earnings immediately prior to the injury report actually partially reflect the impact of the injury.[17] Earnings do not dip in the quarter prior to the injury in Figures 5a

[17] Such reporting delay would not complicate our empirical strategy if it did not change differentially across injuries after the reforms.

and 5b, which suggests that no such delays occur, and the similarity between the results in Column (3) and Column (4) provide evidence that our results are not driven by the inclusion or exclusion of the earnings in the quarter prior to the injury. Columns (5) and (6) use both the control workers' and pre-injury earnings and, again, we find almost no change in the estimate.

Columns (7)-(12) replicate the first 6 columns but include the selection adjustment. The selection adjustment term has almost no effect on the final estimate. Overall, the results are consistent across specifications, inclusion of different controls, and use of the selection adjustment. A coefficient of -0.076 implies that low back injuries experienced a 7.3% decline in medical care expenditures relative to all other injuries.

Table 5 presents the main results of this paper. We find consistent evidence that workers with low back injuries experienced relatively large drops in post-injury earnings after the reforms. Column (1) includes no control for labor productivity or selection. We find a statistically significant estimate of -0.103, which translates to a 9.8% decrease in earnings. In Column (2), we condition on the log of earnings of the matched control workers to the specification and estimate an effect of -0.070 (6.7% reduction), still statistically significant at the 1% level. We estimate an effect of -0.062 (shown in Column (3)) when using the log of pre-injury earnings instead of the earnings of the control workers. Column (4) also controls for pre-injury earnings but excludes the quarter immediately prior to the injury. This exclusion has little effect on the estimate. Columns (5) and (6) use both the control workers' and pre-injury earnings. The results are similar to the results obtained from controlling for only one set of these labor productivity controls.

Columns (7)-(12) include the selection adjustment term. The results are consistently larger in magnitude when this selection adjustment is made. We make this adjustment because some injuries may not be reported after the reforms since the return to reporting such an injury is less beneficial or, more mechanically, because those observations fall below the $2,000 total cost threshold in the WCIRB data. We find some evidence that there is a differential reporting effect since when we account for selection, we estimate a larger impact. Our preferred estimate is presented in Column (11). The estimate of -0.087 suggests that workers experiencing lower back injuries lost 8.3% of post-injury earnings after the reforms relative to workers with other injuries.

Overall, the inclusion of some measure of labor productivity appears to have a modest effect on the estimates. However, the estimated effect is robust to the inclusion of further labor productivity controls. The selection adjustment also has a small effect on the estimates. Combining Tables 4 and 5, we can infer that the 7.3% decline in medical care expenditures was associated with an 8.3% drop in earnings.

A common outcome in analysis of workers' compensation policy is injury duration (time post-injury before re-entering employment) or return to work. We define returning to work as having positive labor earnings in the first 18 months after the worker's injury and report results for this outcome in Table 6. We present average marginal effects from probit regressions. Here, we find evidence that exogenous declines in medical care generosity are associated with reductions in the probability of returning to work. We find little evidence that adjusting for selection or labor productivity matters on this dimension. Our preferred estimate suggests that the reforms reduced the return-to-work (in the first 6 quarters) probability for low back injuries by 1.5 percentage points, relative to their probability of working had the reforms not had a differential effect on low back injuries. This estimate is significant at the 5% level. However, this effect is small relative to the fraction of workers that eventually return to work (0.87 for lower back injuries).

We can also study the timing of the effects for the labor outcomes. Table 7 presents results of the differential effect of the reforms on earnings by quarter. All results include the log of earnings of the control workers in that period, the log of pre-injury earnings for the worker, and the selection adjustment. Again, we find consistent evidence of an effect. All of the estimates are negative and, except for the last quarter in our data, significant at the 10% level. We find that the largest effects – with magnitudes of 9.3% and 10.1%, respectively – occur in quarters 4 and 5 post-injury.

Our data only contains total medical spending over the first 18 months post-injury and lacks information about the timing of the receipt of care. However, Swedlow [2005] used data from the Industry Claims Information System, a data set with medical utilization at 3, 6, and 9 months post-injury and shows that even at 3 months, lower back injuries experienced reduced utilization post-reform relative to all other injuries.[18] Our estimate of a response even at the first quarter post-injury is consistent with this finding.

Next, we study the differential impact of the reforms on injury duration. Table 8 presents average marginal effects where the outcome variable is whether the worker has worked at all in that quarter or any prior quarter since the injury. As in Table 6, we find effects on this dimension, suggesting that reductions in medical care generosity decrease the

[18] Authors' calculation.

probability of returning to work at each point in time. We estimate negative effects for each quarter, finding the largest effects in quarters 2-4. These results suggest that the medical care generosity reductions increase injury duration.

While we find evidence of effects on injury duration, these estimates alone do not provide information about whether these "extensive margin" effects drive the earnings reduction reported in Table 5. Less generous medical care delays when individuals return to work, reducing total post-injury earnings. In addition, there is an intensive margin – reduced medical care generosity may impact the types of jobs that individuals can take, the number of hours that they work (conditional on working), their productivity on the job, etc. In our data, we are unable to distinguish between these intensive margin components since we only observe earnings. But we pursue a decomposition of the main effect into the extensive margin and the intensive margin. We perform this decomposition in two ways. First, we examine the relative importance of returning to work by 6 quarters post-injury (corresponding to Table 6). Second, we examine the relative importance of injury duration.

Our goal is to replicate the Column (11) estimate in Table 5 while shutting down the intensive margin. Using the pre-reform (2000-2002) data only, we model post-injury earnings for injured workers that return to work at some point in the first 18 months pre-injury. We estimate

$$E_{ijkqt} = \exp\left(\alpha_{jk} + \gamma_{jt} + \phi_{jq} + g(L_i)\right) \eta_{ijkqt}$$

where E represent total (6 quarters) post-injury earnings. Using the estimated parameters, we predict earnings for the entire 2000-2006 sample, $\hat{E}_{ijkqt}$. In the post-period, this prediction varies based only by injury type, industry, calendar quarter, and the labor productivity measures (pre-injury earnings and the post-injury earnings of the control workers). We replace each individual's earnings with $\tilde{E}_{ijkqt}$ which is defined by

$$\tilde{E}_{ijkqt} = \begin{cases} 0 \quad \text{if} \quad E_{ijkqt} = 0 \\ \hat{E}_{ijkqt} \quad \text{if} \quad E_{ijkqt} > 0 \end{cases} \tag{5}$$

In words, we only replace the earnings of individuals that returned to work within the first 6 quarters post-injury. This counterfactual earnings variable eliminates intensive margin effects and varies (conditional on covariates) only based on whether the individuals works or not within the first 6 quarters post-injury. Using this variable as our outcome, we re-estimate our main specification and present the results in Column (1) of Table 9. The estimate implies that the extensive margin effect (estimate in Table 6) explains a 1.3% earnings reduction, about 15% of the total earnings effect.

Next, we pursue a similar exercise but focus on injury duration. Using the pre-reform (2000-2002) data only, we estimate post-injury earnings by quarter relative to injury for injured workers that worked during that quarter. We estimate

$$E^s_{ijkqt} = \exp\left(\alpha_{jk} + \gamma_{jt} + \phi_{jq} + g(L_i)\right)\eta_{ijkqt}$$

where E^s represent post-injury earnings in quarter s (where quarter is measured relative to time of injury) for quarters 1-6. Using the estimated parameters, we predict earnings for the entire 2000-2006 sample, $\hat{E}^s_{ijkqt}$. As before, we create

$$\tilde{E}^s_{ijkqt} = \begin{cases} 0 \quad \text{if} \quad E^s_{ijkqt} = 0 \\ \hat{E}^s_{ijkqt} \quad \text{if} \quad E^s_{ijkqt} > 0 \end{cases} \tag{6}$$

Finally, we create $\tilde{\tilde{E}}_{ijkqt} = \sum_{s=1}^{6} \tilde{E}^s_{ijkqt}$. This variable varies (conditionally) due only to differential duration of not working, eliminating the variation resulting from earning more or less (conditional on working) due to the reforms. We re-estimate our main specification with this outcome variable and present the result in Column (2) of Table 9. We estimate an effect of -0.044, which is about 51% of the overall estimated effect. Note that this approach obscures duration differences which occur within a quarter. However, it over-weights small duration differences that straddle the end of a quarter and the beginning of another. On average, these two errors should cancel out. Given this possible caveat, we are estimating a relatively large role for the intensive margin. While the workers' compensation literature has frequently focused on injury duration, this result suggests that there are important effects occurring beyond that simple metric.

5.3 Prices vs. Utilization

The reforms changed both the pricing and utilization of medical care. Our focus has been on total medical expenditures since we think that this is a central policy variable of interest and because changes in prices and utilization could both potentially impact health outcomes. Given the details of the reforms, however, we believe that the differential effect of the policies on medical costs for low back injuries should primarily be a utilization effect. The pricing changes were more uniformly applied, while the utilization caps targeted care disproportionately used by workers with low back injuries. Unfortunately, our data do not allow us to study this issue further. However, research by the California Workers' Compensation Institute used the Industry Claims Information System (ICIS) briefly discussed above. Swedlow [2005] publishes medical utilization data for claims in the pre- and post-reform periods to evaluate changes in utilization. The paper reports utilization for 7 different categories – Evaluation & Management, Physical Therapy, Surgery (excluding injections), Chiropractic Manipulation, Medicine Section Services, Radiology, and Injection. These data are reported for all injuries and then specifically for lower back injuries. The top half of Table 10 includes the data for lower back injuries. Using the data for all injuries and the relative frequency of the injuries, we calculate the utilization for all other injuries (excluding lower back injuries) and list these numbers in the bottom half of Table 10.

Using these data, we calculate a differential utilization effect of $[\ln(60.1) - \ln(101.5)] - [\ln(58.9) - \ln(93.0)] = -0.067$. Note that this is similar to the differential effect that we estimate on total expenditures, suggesting that our estimates primarily represent a change in utilization.

5.4 Robustness Tests

This section explores the robustness of our results to the inclusion of richer controls, changes to the analysis sample, functional form assumptions, and different selection instruments. In the Appendix, we perform a placebo test and find evidence that our estimated main effect is unlikely to occur randomly. We begin by presenting results with richer controls.

5.4.1 Inclusion of Different Controls

Table 11 presents a series of robustness checks for three outcomes - medical expenditures, earnings, and return to work. Column (1) reports results when the labor productivity control variables are allowed to have differential effects by type of injury. We interact both the control worker earnings and the pre-injury earnings by indicators for each injury type. The results suggest that this added flexibility has little effect on our estimates. In Column (2), we replace the Industry x Quarter of Injury and Industry x Year of Injury interactions with Industry x Quarter of Injury x Year of Injury interactions. We find slightly stronger effects on all dimensions using these interactions. Finally, Column (3) reports results using our alternate selection instrument. Instead of using severe permanent injuries, we use all permanent injuries in 2000 interacted with the post-2003 dummy as our selection instrument. Table 3 showed that this variable predicts selection. The results in Column (3) of Table 11 are consistent with our previous findings.

5.4.2 Changes to Analysis Sample

We also explore the robustness of our results to changes in the analysis sample. Table 12 includes 2003 as a pre-reform year in the top section. Including 2003 as a pre-reform year is a conservative choice and should attenuate our effects. We do find smaller estimates of the impact on earnings. We actually estimate a larger effect for medical expenditures, though we cannot reject our previous estimates. Overall, we find that our results are relatively robust to the inclusion of 2003 as part of the pre-reform era.

We also originally selected shoulder, knee, and hand/wrist injuries in our data as appropriate controls for lower back injuries. These injuries are the most common and the workers had similar pre-injury earnings in the pre-reform period. Furthermore, many injury types in our data, such as mental illnesses, are likely less comparable to lower back injuries. In the bottom section of Table 12, we present results which also include upper back, neck, "other lower extremity," and head injuries as controls. Despite the possibility that these injuries differ significantly from lower back injuries, the estimates are generally consistent with the main results of this paper. The medical expenditure results are larger while the earnings and return-to-work estimates are slightly smaller. We cannot reject our main results.

5.4.3 Linear Estimation

We previously discussed the benefits of using nonlinear least squares in our context given that earnings and medical expenditures are highly-skewed. Poisson regression allows for additional flexibility in the error term while log-linear regression requires the error term to only be multiplicative in earnings and medical spending. We replicate our medical expenditure results using a log-linear specification and present the estimates in the first column of Table 13. We estimate even larger effects on medical spending when using a log-linear specification.

Given that a nontrivial fraction of our sample have no post-injury earnings, we use the inverse hyperbolic sine (IHS) transformation for earnings, which is often used when the outcome is skewed and contains 0's. The literature discusses this transformation (Burbidge, Magee, and Robb [1988], MacKinnon and Magee [1990], Pence [2006]) and it is used in applied work (see Gelber [2011], Carboni [2012], Barcellos and Jacobson [2013] for examples). The second column of Table 13 presents our results using ordinary least squares. Our estimates are relatively consistent to our main results though, again, we estimate a larger magnitude.

5.4.4 Inclusion of Self-Insured Firms

As noted, the WCRIB data only include insured firms. We do not believe that this poses a problem for our empirical strategy as the industry-time fixed effects likely account for any firm-level entry and exit decisions that are related to the reforms. Firms cannot cover different injury types through different sources of insurance so there should not be any systematic bias. Furthermore, our selection adjustment term - while not designed specifically with firm-level selection concerns in mind - should account for systematic selection even if driven by firm-level decisions. The selection adjustment accounts for differential changes in the number of injuries in our data by injury type, regardless of the source of those changes.

Alternatively, we can explicitly adjust for firm-level selection. Given that only large firms tend to self-insure, smaller firms are less subject to any possible selection bias resulting from differential self-insurance rates. We create a variable that uses the initial composition of firm size in each industry-injury type combination:

$$d_{jkt} \equiv \left(\frac{\text{Number of injuries in 2000 that are in firms with 500 or fewer employees}_{jk}}{\text{Number of injuries in 2000}_{jk}} \right) \times \text{Post}_t.$$

The motivation for this selection instrument parallels the motivation for our primary selection instrument – we should observe a smaller reduction in claims in industries (and injury types) with a high fraction of small firms. We estimate our selection equation using both h_{jkt} and d_{jkt} as selection instruments. We find that both instruments statistically predict the number of injuries. The top half of Table 14 reports our results using this additional selection instrument to help adjust for selection concerns. We find similar estimates as before.

In the bottom half of Table 14, we present results using the DEU data, which have the advantage of including injuries at self-insured firms. We cannot present results on medical expenditures since the DEU data do not include this information. Furthermore, the DEU data only contain permanent injuries and our selection adjustment is primarily identified from variation in the fraction of all injuries which are permanent. Consequently, adjusting for selection is more difficult in the DEU data and our confidence intervals are much larger due to the difficulties in separately identifying selection from the other variables. However, the point estimates are consistent with the main estimates of the paper, suggesting that the exclusion of self-insured firms in the WCIRB is not biasing our results.

5.5 Other Components of the Reforms

Our analysis assumes that the California reforms differentially affected low back injuries through medical care generosity changes only. The reforms made many other changes to the workers' compensation program which we account for by comparing the outcome changes of lower back injuries to outcome changes of other injuries. While the medical care reforms did target care that was disproportionately used for low back injuries, the other aspects of the reform should not have had a differential effect based on injury type. We can study this more explicitly, however, and test whether low back injuries were disproportionately affected by other facets of the reforms, starting with the change in the formula for indemnity benefits. We calculate replacement rates for each worker in our data and then estimate equation (3) with the replacement rate as the outcome variable. Table 15 presents these results in Column (1). There is no evidence that lower back injuries were differentially affected. If anything, the results suggest that workers with low back injuries received less generous benefits, implying that they would return to work earlier than workers with other types of injuries.

We are also concerned that the reforms altered the incentives to contest judgments or make settlements. Our data include total defense costs and we use this measure as a proxy for court-driven changes in behavior. Again, we find no evidence that these factors can explain our results, shown in Column (2).

The reforms also affected generosity of vocational rehabilitation vouchers. There is evidence that these vouchers had little impact on returning to work (Seabury et al. [2011]). However, we can test whether the reforms had a differential effect on use of vocational rehabilitation. In Column (3), we report our estimate, suggesting little relationship. Overall, we conclude the our workers with lower back injuries did not appear to benefit more or less than workers with other types of injuries due to the reforms on dimensions other than medical care.

Finally, we can account for these other dimensions more explicitly when estimating the differential impact on our primary outcomes. The severity ratings trigger different levels of benefits such as changes in the benefit formula and generosity of vocational rehabilitation vouchers. For example, vocational rehabilitation voucher amounts in the post-reform period increased by severity rating category: $< 15\%$, 15-25%, 26-49%, 50-99%, and 100%. Other benefits had different thresholds. We coded up categories based on *all* severity rating thresholds in the California workers' compensation program after the reforms and interacted these categories with year dummies. We included these interactions in our main specification. We expect these controls to capture some of the effect that we are hoping to estimate and attenuate our results since the reforms may have affected the severity ratings and because within-year severity ratings may be correlated with injury type. The bottom half of Table 15 reports the results. The results are noisier due to the inclusion of a large number of additional controls, but are generally supportive of our main findings.

6 Discussion and Conclusion

We find consistent evidence that the 2003-2004 California workers' compensation reforms reduced medical spending disproportionately for low back injuries. The results imply that medical care spending for low back injuries decreased by 7.3% more than spending on other injuries. Evaluating at the mean of medical spending on lower back injuries in the pre-reform period ($10,510), this amounts to a reduction in medical costs of $767. We also find that earnings associated with back injuries decreased by 8.3% relative to other injuries. This decrease amounts to a $2,567 average drop in post-injury earnings during the first 6 quarters after the injury. We estimate that about half of this earnings drop reflects a delay in returning to work, while half occurs on the intensive margin through lower earnings conditional on working. This latter effect implies that injured workers are working fewer hours, are less productive, or are working at lower-paying jobs.

While the reforms reduced medical care spending, we also estimate whether costs per claim were reduced. Given the finding that injured workers remain out of the labor force for a longer period of time, it is possible that they are receiving more indemnity benefits and that these additional benefits are reducing the per-claim cost-savings. We estimate our main specification (equation (3)) with the outcome equal to total medical costs plus indemnity benefits in the first 18 months. We estimate a coefficient of -0.008 (with a p-value of 0.663), implying that total costs per claim were not statistically significantly reduced. Thus, while medical costs were reduced, injured workers remained out of the labor force longer and accepted indemnity benefits for more time and, overall, total per-claim costs remained about the same. This per-claim calculation does not account for the potential that the reforms reduced the total number of claims, which would reduce total costs.

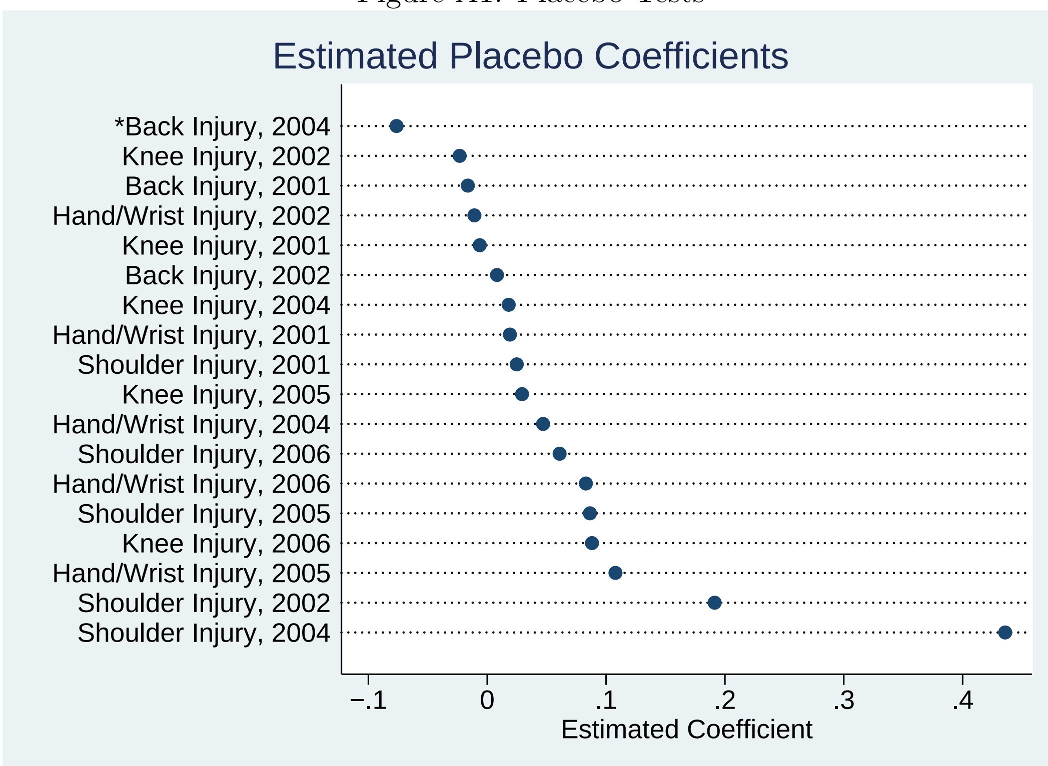
This paper provides evidence of the value of medical care and its impact on labor outcomes. Injured workers experience large and long-lasting drops in labor income, but we find strong evidence that medical care generosity can reduce this income shock and return workers to work more quickly. Our results are robust to the inclusion of rich labor productivity variables and adjustments for selection into our sample.

These results jointly suggest that marginal medical care spending is productive in terms of promoting the health of injured workers and that health is an important component of labor productivity. Our estimates imply that a $1 increase in medical care spending is associated with an increase in labor earnings that far surpasses $1. Even without accounting for changes in quality of life and other non-monetary factors, the results provide evidence that the reforms eliminated efficient medical care.

Appendix: Placebo Tests

We replicate our results while assigning the treatment to different start years and injury types. We vary the year that the reforms took place between 2001 and 2006 and also vary which injury type is "treated." We perform this placebo test for each combination of years and injury type for shoulder, knee, and hand/wrist injuries. We also perform this test for each pre-period year for lower back injuries. Figure A1 presents the results. The top estimate is this paper's main estimate (Column (11) of Table 5). The estimates plotted below are the estimated placebo effects. The placebo estimates are all larger than the effect estimated for $\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_j\right)$, suggesting that the main estimate is unlikely to reflect random variation.[19]

Figure A1: Placebo Tests



Notes: Each row designates the injury type and (first) time period that are considered "treated" in the placebo test. "*Back Injury, 2004" is the true effect estimated in the paper.

[19]Despite the large positive coefficients estimated for some of the shoulder injury placebo tests, the inclusion of shoulder injuries is not driving our main estimates. Excluding shoulder injuries yields point estimate for medical expenditures and post-injury wages (replications of Column (11) in Tables 4 and 5) of -0.082 and -0.085, respectively.

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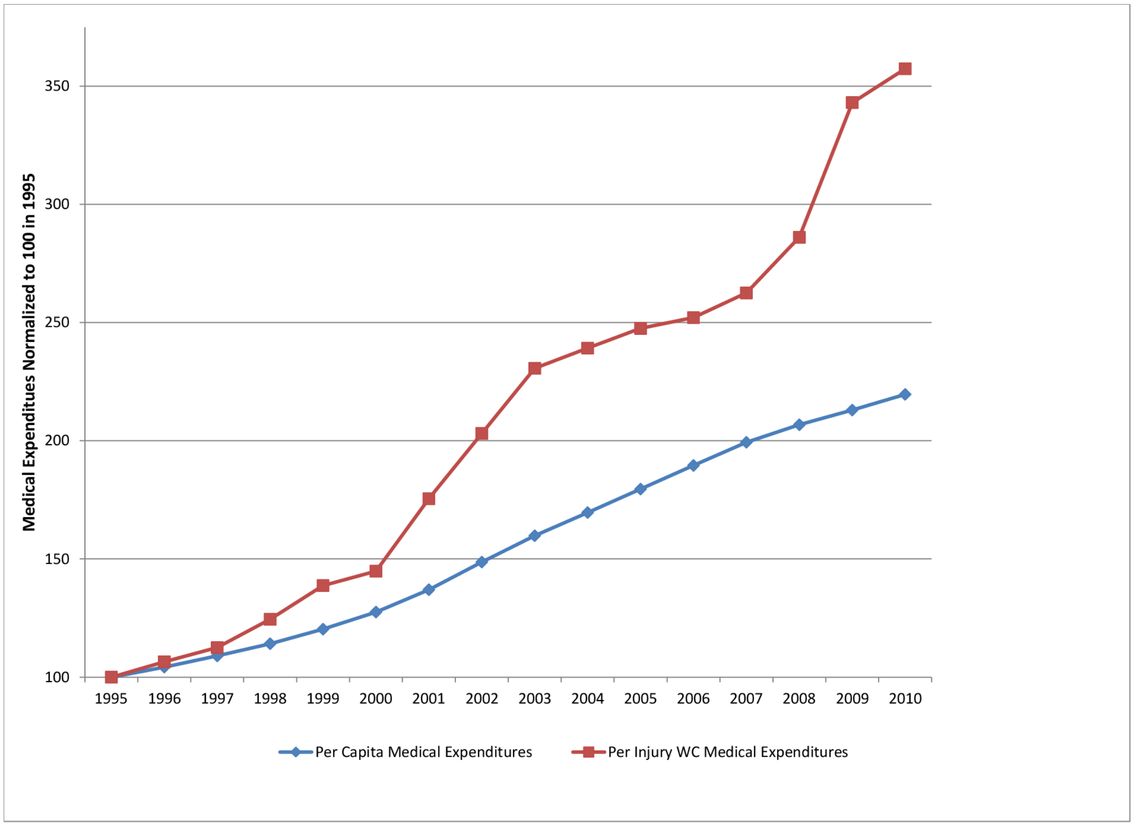
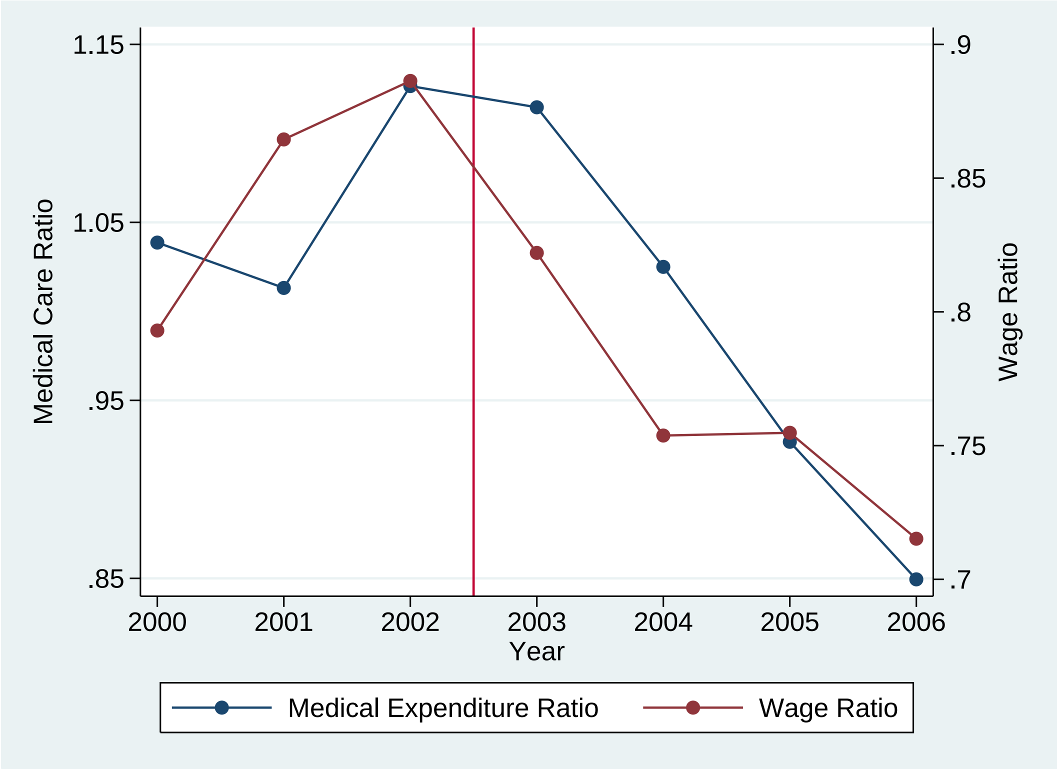
Figures

Figure 1: Growth in National Health Care Expenditures and Workers' Compensation Medical Costs



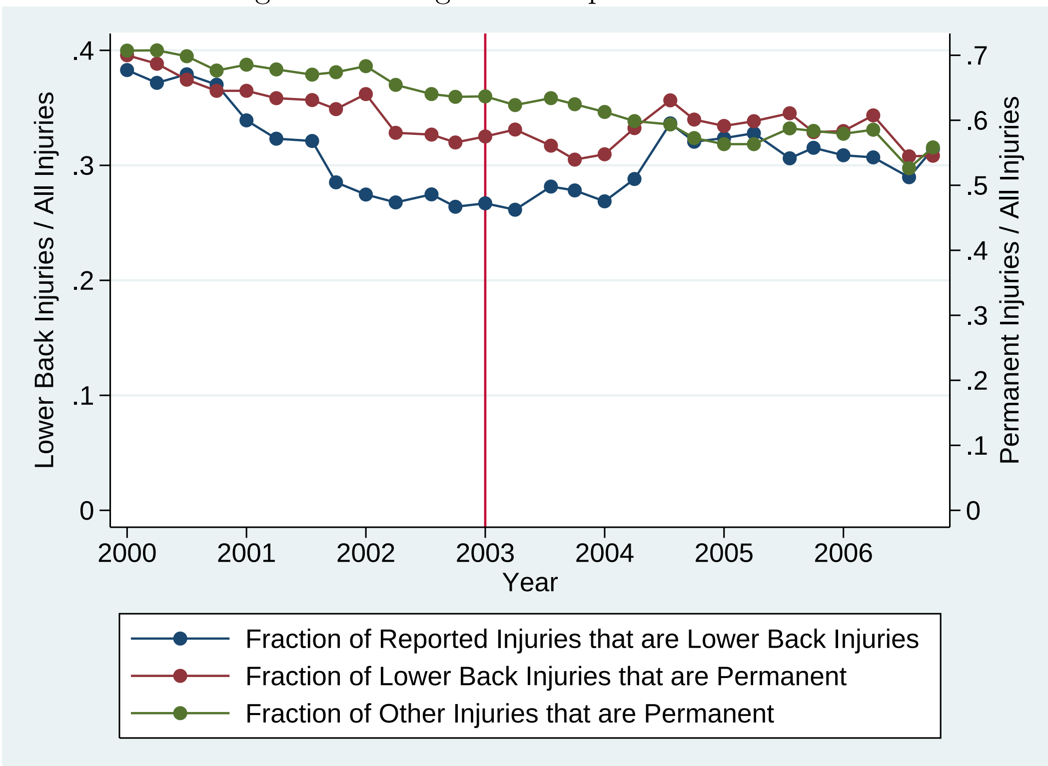
Notes: Per Capita Medical Expenditures are derived from the National Health Expenditure Accounts, published by CMS. Per Injury Workers' Compensation Medical Expenditures are derived from the National Academy of Social Insurance, published in Sengupta, Baldwin, and Reno [2013]. We normalize all values to 100 in 1995.

Figure 2: Medical Expenditures and Post-Injury Earnings of Low Back Injuries Relative to Other Injuries



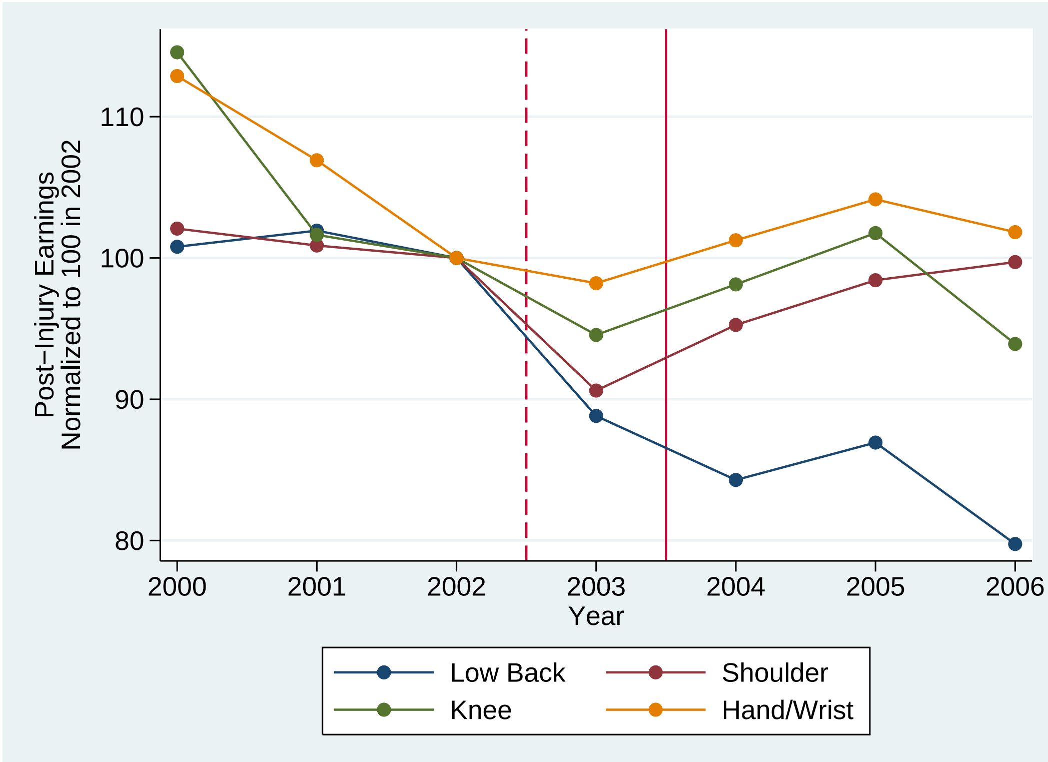
Notes: Other injuries are shoulder, knee, hand/wrist injuries. Ratios calculated using WCIRB data. Medical care ratio is the mean medical spending on low back injuries relative to other injuries. The wage ratio is the post-injury earnings of low back injuries relative to other injuries.

Figure 3: Changes in Composition over Time



Notes: This figure represents total number of lower back injuries divided by the total number of injuries (lower back, shoulder, knee, hand/wrist) in each quarter. It also includes the fraction of back injuries and fraction of other injuries which are permanent as measures of injury severity.

Figure 4: Post-Injury Earnings



Notes: Each point represents the average post-injury earnings for a worker injured in that year. Each injury type is normalized to 100 based on the mean in 2002, the year immediately prior to the reforms.

Figure 5a: Pre-Reforms

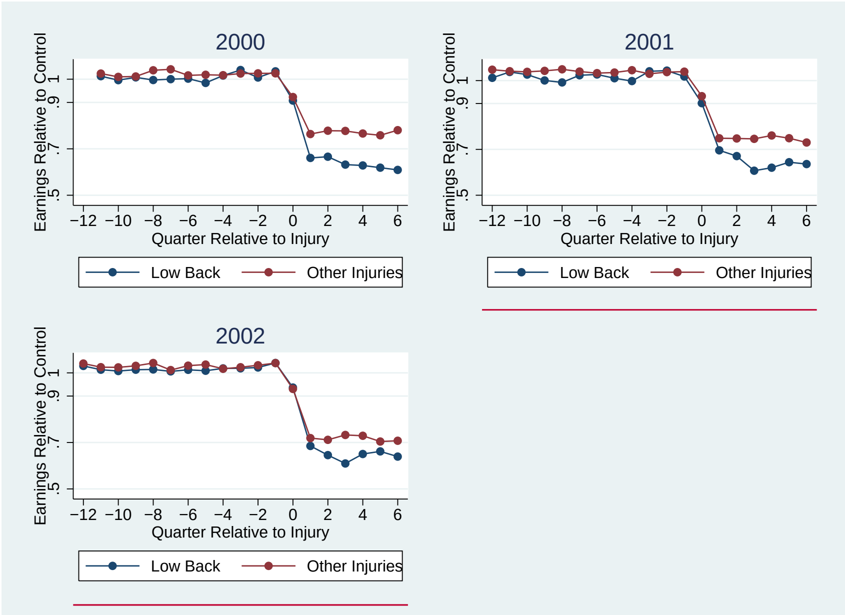
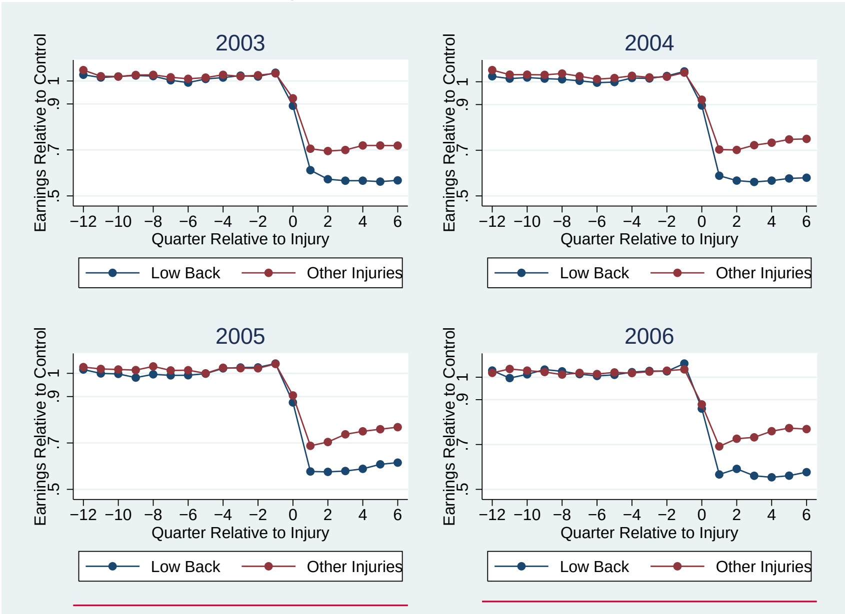


Figure 5b: Post-Reforms



Notes: Earnings expressed as earnings in quarter divided by earnings in quarter of matched control workers.

Tables

Table 1: Summary Statistics

	Low Back Injuries		Other Injuries	
	Mean	Standard Dev.	Mean	Standard Dev.
Medical Expenditures	$9,544.73	$14,434.37	$9,298.15	$10,984.29
Pre-Injury Earnings (6 quarters)	$46,023.22	$53,131.57	$47,967.25	$89,068.98
Post-Injury Earnings (6 quarters)	$29,345.27	$99,658.95	$35,871.86	$86,623.27
Fraction Working within First 6 Quarters	0.87	0.34	0.91	0.28
Fraction of Injuries that are Permanent	0.62	0.48	0.65	0.48
Severity Ranking (for Permanent Injuries)	19.9	14.5	16.3	13.0
N	50,342		107,723	
	Pre-Reform (2000-2002)			
	Mean	Standard Dev.	Mean	Standard Dev.
Medical Expenditures	$10,510.36	$15,576.72	$10,084.45	$11,993.39
Pre-Injury Earnings (6 quarters)	$47,404.67	$60,127.56	$48,875.60	$105,957.00
Post-Injury Earnings (6 quarters)	$30,934.19	$118,617.60	$36,494.67	$102,843.40

Notes: WCIRB data for 2000-2006 injuries. "Other Injuries" are shoulder, knee, and hand/wrist injuries.

Table 2: Relationship Between Medical Expenditures and Post-Injury Earnings

Dependent Variable:	Post-Injury Earnings		
ln(Medical Expenditures)	-0.029***	-0.022***	-0.021***
	[-0.034, -0.024]	[-0.026, -0.018]	[-0.026, -0.017]
Control Wages	No	Yes	Yes
Pre-Injury Wages	No	No	Q1-Q6
Industry x Quarter of Injury	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes
N	158,065	158,065	158,065

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets adjusted for clustering at industry-injury type level. Control Wages refer to the earnings of the control workers for the individual. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies that the first 6 quarters of pre-injury earnings are included as a covariate).

Table 3: Selection Equation Estimates

Dependent Variable:	ln(Number of Reported Injuries)			
Post x Back Injury	-0.084**	-0.089**	-0.030	-0.035
	(0.038)	(0.037)	(0.039)	(0.038)
Post x Initial Severity	1.310***	1.270***	1.613***	1.605***
	(0.309)	(0.312)	(0.229)	(0.225)
Severity Measure	Severe Permanent Injuries	Severe Permanent Injuries	All Permanent Injuries	All Permanent Injuries
Industry x Quarter of Injury	Yes	No	Yes	No
Industry x Year of Injury	Yes	No	Yes	No
Industry x Injury Type	Yes	Yes	Yes	Yes
Industry x Quarter of Injury x Year of Injury	No	Yes	No	Yes
N	158,065	158,065	158,065	158,065

***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in brackets adjusted for clustering at industry-injury type level. “Severe Permanent Injuries” refer to the top 75% of permanent injuries.

Table 4: Differential Impact of Reforms on Medical Expenditures

Dependent Variable:	Medical Expenditures					
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Back Injury	-0.075***	-0.076***	-0.077***	-0.077***	-0.077***	-0.077***
	[-0.095, -0.056]	[-0.096, -0.057]	[-0.096, -0.057]	[-0.096, -0.057]	[-0.096, -0.057]	[-0.096, -0.057]
N	158,065	158,065	158,065	158,065	158,065	158,065
Control Wages	No	Yes	No	No	Yes	Yes
Pre-Injury Wages	No	No	Q1-Q6	Q2-Q6	Q1-Q6	Q2-Q6
Selection Adjustment	No	No	No	No	No	No
Industry x Quarter of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes	Yes	Yes	Yes
	(7)	(8)	(9)	(10)	(11)	(12)
Post x Back Injury	-0.075***	-0.076***	-0.076***	-0.076***	-0.076***	-0.076***
	[-0.114, -0.044]	[-0.115, -0.044]	[-0.115, -0.044]	[-0.115, -0.045]	[-0.115, -0.044]	[-0.115, -0.045]
N	158,065	158,065	158,065	158,065	158,065	158,065
Control Wages	No	Yes	No	No	Yes	Yes
Pre-Injury Wages	No	No	Q1-Q6	Q2-Q6	Q1-Q6	Q2-Q6
Selection Adjustment	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Quarter of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes	Yes	Yes	Yes

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets adjusted for clustering at industry-injury type level. When "Selection Adjustment" included, confidence intervals generated by clustered bootstrap. Control Wages refer to the earnings of the control workers for the individual. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies that the first 6 quarters of pre-injury earnings are included as a covariate).

Table 5: Differential Impact of Reforms on Post-Injury Earnings

Dependent Variable:	Post-Injury Earnings					
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Back Injury	-0.103***	-0.070***	-0.063***	-0.063***	-0.065***	-0.065***
	[-0.133, -0.073]	[-0.101, -0.038]	[-0.094, -0.033]	[-0.092, -0.034]	[-0.098, -0.031]	[-0.098, -0.032]
N	158,065	158,065	158,065	158,065	158,065	158,065
Control Wages	No	Yes	No	No	Yes	Yes
Pre-Injury Wages	No	No	Q1-Q6	Q2-Q6	Q1-Q6	Q2-Q6
Selection Adjustment	No	No	No	No	No	No
Industry x Quarter of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes	Yes	Yes	Yes
	(7)	(8)	(9)	(10)	(11)	(12)
Post x Back Injury	-0.115***	-0.091***	-0.084***	-0.082***	-0.087***	-0.087***
	[-0.161, -0.049]	[-0.169, -0.022]	[-0.159, -0.019]	[-0.146, -0.020]	[-0.167, -0.018]	[-0.164, -0.020]
N	158,065	158,065	158,065	158,065	158,065	158,065
Control Wages	No	Yes	No	No	Yes	Yes
Pre-Injury Wages	No	No	Q1-Q6	Q2-Q6	Q1-Q6	Q2-Q6
Selection Adjustment	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Quarter of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes	Yes	Yes	Yes

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets adjusted for clustering at industry-injury type level. When "Selection Adjustment" included, confidence intervals generated by clustered bootstrap. Control Wages refer to the earnings of the control workers for the individual. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies that the first 6 quarters of pre-injury earnings are included as a covariate).

Table 6: Differential Impact of Reforms on Probability of Working Post-Injury

Dependent Variable	1(Work within 18 months of Injury)			
Post x Back Injury	-0.017***	-0.014***	-0.014***	-0.015**
	[-0.025, -0.009]	[-0.021, -0.006]	[-0.021, -0.006]	[-0.030, -0.002]
Control Wages	No	Yes	Yes	Yes
Pre-Injury Wages	No	No	Q1-Q6	Q1-Q6
Selection Adjustment	No	No	No	Yes
Industry x Quarter of Injury	Yes	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes	Yes

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets adjusted for clustering at industry-injury type level. When "Selection Adjustment" included, confidence intervals generated by clustered bootstrap. Control Wages refer to the earnings of the control workers for the individual. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies earnings for quarters 1 to 6 pre-injury are included as a covariate). Marginal effects reported by estimating the change in probability for entire sample of post-reform back injuries and calculating the mean probability change.

Table 7: Differential Impact of Reforms on Post-Injury Earnings by Quarter

Dependent Variable:	Post-Injury Earnings					
Post-Injury Quarter:	Q1	Q2	Q3	Q4	Q5	Q6
Post x Back Injury	-0.078***	-0.088**	-0.075*	-0.106**	-0.098**	-0.064
	[-0.178, -0.029]	[-0.156, -0.021]	[-0.124, 0.001]	[-0.195, -0.006]	[-0.248, -0.001]	[-0.169, 0.019]
Control Wages	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Injury Wages	Q1-Q6	Q1-Q6	Q1-Q6	Q1-Q6	Q1-Q6	Q1-Q6
Selection Adjustment	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Quarter of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes	Yes	Yes	Yes

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets adjusted for clustering at industry-injury type level. When "Selection Adjustment" included, confidence intervals generated by clustered bootstrap. Control Wages refer to the earnings of the control workers for the individual in the same period. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies earnings for quarters 1 to 6 pre-injury are included as a covariate).

Table 8: Differential Impact of Reforms on Injury Duration

Dependent Variable:	1(Work in or before Quarter)					
Post-Injury Quarter:	Q1	Q2	Q3	Q4	Q5	Q6
Post x Back Injury	-0.013*	-0.020**	-0.020**	-0.019**	-0.016**	-0.015**
	[-0.029, 0.005]	[-0.038, -0.004]	[-0.034, -0.004]	[-0.034, -0.002]	[-0.029, -0.001]	[-0.030, -0.002]
Control Wages	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Injury Wages	Q1-Q6	Q1-Q6	Q1-Q6	Q1-Q6	Q1-Q6	Q1-Q6
Selection Adjustment	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Quarter of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes	Yes	Yes	Yes

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets adjusted for clustering at industry-injury type level. When "Selection Adjustment" included, confidence intervals generated by clustered bootstrap. Control Wages refer to the earnings of the control workers for the individual in the same period. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies earnings for quarters 1 to 6 pre-injury are included as a covariate). Marginal effects reported by estimating the change in probability for entire sample of post-reform back injuries and calculating the mean probability change.

Table 9: Extensive Margin Effects Only

Dependent Variable:	Counterfactual Post-Injury Earnings	
	(1)	(2)
Post x Back Injury	-0.013*	-0.044**
	[-0.029, 0.002]	[-0.086, -0.006]
N	158,065	158,065
Control Wages	Yes	Yes
Pre-Injury Wages	Q1-Q6	Q1-Q6
Selection Adjustment	Yes	Yes
Industry x Quarter of Injury	Yes	Yes
Industry x Year of Injury	Yes	Yes
Industry x Injury Type	Yes	Yes
Earnings Replacement	Entire Post-Injury Period	By Quarter

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets adjusted for clustering at industry-injury type level. When "Selection Adjustment" included, confidence intervals generated by clustered bootstrap. Control Wages refer to the earnings of the control workers for the individual in the same period. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies earnings for quarters 1 to 6 pre-injury are included as a covariate).

The outcome variable in Column (1) varies based on injury type, industry, calendar quarter, control wages, pre-injury wages, and whether the worker has any earnings in the post-injury period. Given the controls included, the outcome variable varies conditionally based only on whether the worker has any earnings in the post-injury period.

The outcome variable in Column (2) varies based on injury type, industry, calendar quarter, control wages, pre-injury wages, and which post-injury quarters that the worker earned positive earnings. Given the controls included, the outcome variable varies conditionally based only on which post-injury quarters that the worker earned positive earnings.

Table 10: Utilization of Services by Injury Type at 9 Months Post-Injury

Lower Back Injuries	2002	2004
Evaluation & Management	11.8	10.8
Physical Therapy	30.0	14.9
Surgery (excluding injections)	5.8	4.3
Chiropractic Manipulation	38.6	17.7
Medicine Section Services	5.1	4.0
Radiology	4.6	4.1
Injection	5.6	4.3
Total	101.5	60.1
All Other Injuries	2002	2004
Evaluation & Management	10.6	9.6
Physical Therapy	27.0	15.4
Surgery (excluding injections)	4.7	4.1
Chiropractic Manipulation	36.7	17.3
Medicine Section Services	4.6	4.2
Radiology	4.7	4.3
Injection	4.8	3.9
Total	93.0	58.9

Notes: Original data found in Swedlow [2005]. Average number of visits per claim at 9 months from the date of injury listed.

Table 11: Richer Controls and Different Selection Instrument

	(1)	(2)	(3)
Dependent Variable:		**Medical Expenditures**	
Post x Back Injury	-0.076***	-0.077***	-0.073**
	[-0.116, -0.045]	[-0.117, -0.045]	[-0.115, -0.038]
Dependent Variable:		**Post-Injury Earnings**	
Post x Back Injury	-0.082***	-0.091***	-0.084***
	[-0.222, -0.010]	[-0.243, -0.017]	[-0.197, -0.017]
Dependent Variable:		**1(Work within 18 months of injury)**	
Post x Back Injury	-0.016*	-0.016**	-0.015***
	[-0.032, 0.000]	[-0.032, -0.001]	[-0.026, -0.002]
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q1-Q6	Q1-Q6	Q1-Q6
Control Wages x Injury Type	Yes	No	No
Pre-Injury Wages x Injury Type	Q1-Q6	No	No
Selection Adjustment	Severe Permanent Injuries	Severe Permanent Injuries	Permanent Injuries
Industry x Quarter of Injury	Yes	No	Yes
Industry x Year of Injury	Yes	No	Yes
Industry x Injury Type	Yes	Yes	Yes
Industry x Quarter of Injury x Year of Injury	No	Yes	No

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets adjusted for clustering at industry-injury type level. When "Selection Adjustment" included, confidence intervals generated by clustered bootstrap. Control Wages refer to the earnings of the control workers for the individual. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies earnings for quarters 1 to 6 pre-injury are included as a covariate). "Severe Permanent Injuries" refers to top 75% of permanent injuries.

Table 12: Changes to Analysis Sample

	Including 2003 as Non-Reform Year		
	(1)	(2)	(3)
Dependent Variable	Medical Expenditures	Post-Injury Earnings	1(Work)
Post x Back Injury	-0.090***	-0.056*	-0.016**
	[-0.127, -0.059]	[-0.142, 0.010]	[-0.033, -0.002]
Year 2003	Included	Included	Included
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q1-Q6	Q1-Q6	Q1-Q6
Selection Adjustment	Yes	Yes	Yes
Industry x Quarter of Injury	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes
	Including Other Injury Types		
	(4)	(5)	(6)
Dependent Variable	Medical Expenditures	Post-Injury Earnings	1(Work)
Post x Back Injury	-0.134***	-0.075**	-0.009
	[-0.228, -0.065]	[-0.126, -0.014]	[-0.020, 0.003]
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q1-Q6	Q1-Q6	Q1-Q6
Selection Adjustment	Yes	Yes	Yes
Industry x Quarter of Injury	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets generated by clustered bootstrap. Control Wages refer to the earnings of the control workers for the individual. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies that the first 6 quarters of pre-injury earnings are included as a covariate). Other injury types (columns 4-6) include upper back, neck, "other lower extremity," and head injuries.

Table 13: OLS Estimates

Dependent Variable	log(Medical Expenditures)	IHS(Post-Injury Earnings)
Post x Back Injury	-0.120***	-0.149*
	[-0.155, -0.088]	[-0.294, 0.011]
N	158,065	158,065
Control Wages	Yes	Yes
Pre-Injury Wages	Q1-Q6	Q1-Q6
Selection Adjustment	Yes	Yes
Industry x Quarter of Injury	Yes	Yes
Industry x Year of Injury	Yes	Yes
Industry x Injury Type	Yes	Yes

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets generated by clustered bootstrap. Control Wages refer to the earnings of the control workers for the individual. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies earnings for quarters 1 to 6 pre-injury are included as a covariate). IHS is the inverse hyperbolic sine transformation.

Table 14: Firm-Level Selection

	Include Firm-Level Selection Term		
	(1)	(2)	(3)
Dependent Variable	Medical Expenditures	Post-Injury Earnings	1(Work)
Post x Back Injury	-0.072***	-0.083***	-0.016**
	[-0.114, -0.032]	[-0.150, -0.017]	[-0.027, -0.002]
N	158,065	158,065	158,065
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q1-Q6	Q1-Q6	Q1-Q6
Selection Adjustment	Injury + Firm	Injury + Firm	Injury + Firm
Industry x Quarter of Injury	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes
	DEU Data Results		
		(4)	(5)
Dependent Variable		Post-Injury Earnings	1(Work)
Post x Back Injury		-0.089	-0.018
		[-0.174, 0.123]	[-0.066, 0.037]
N		98,290	98,290
Control Wages		Yes	Yes
Pre-Injury Wages		Q1-Q6	Q1-Q6
Selection Adjustment		Yes	Yes
Industry x Quarter of Injury		Yes	Yes
Industry x Year of Injury		Yes	Yes
Industry x Injury Type		Yes	Yes

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets generated by clustered bootstrap. Control Wages refer to the earnings of the control workers for the individual. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies earnings for quarters 1 to 6 pre-injury are included as a covariate). "Firm-Level Selection Term" implies that the selection adjustment term is a prediction using both initial severity of injuries in that industry x injury type and initial firm size distribution in the industry x injury type. The DEU data do not provide information about medical expenditures.

Table 15: Other Dimensions of Reforms

	(1)	(2)	(3)
Dependent Variable	**Replacement Rate**	**Defense Costs**	**1(Vocational Rehabilitation)**
Post x Back Injury	-0.034	-0.005	0.006
	[-0.280, 0.196]	[-0.378, 0.399]	[-0.026, 0.034]
N	158,065	158,065	158,065
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q1-Q6	Q1-Q6	Q1-Q6
Selection Adjustment	Yes	Yes	Yes
Industry x Quarter of Injury	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes

	(4)	(5)	(6)
Dependent Variable	**Medical Expenditures**	**Post-Injury Earnings**	**1(Work)**
Post x Back Injury	-0.044	-0.101***	-0.017**
	[-0.126, 0.032]	[-0.247, -0.022]	[-0.033, -0.004]
N	158,065	158,065	158,065
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q1-Q6	Q1-Q6	Q1-Q6
Selection Adjustment	Yes	Yes	Yes
Severity Rating x Year Controls	Yes	Yes	Yes
Industry x Quarter of Injury	Yes	Yes	Yes
Industry x Year of Injury	Yes	Yes	Yes
Industry x Injury Type	Yes	Yes	Yes

***Significance 1%, ** Significance 5%, * Significance 10%. 95% Confidence Intervals in brackets generated by clustered bootstrap. Control Wages refer to the earnings of the control workers for the individual. Pre-Injury Wages included represented by quarters (eg., Q1-Q6 implies that the first 6 quarters of pre-injury earnings are included as a covariate). "Severity Rating" refers to a series of indicators variables based on severity ratings. These indicators are interacted with year dummies.