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

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Abstract

There is considerable controversy over whether much of the spending on health care in the United States delivers enough value to justify the cost. We contribute to this literature by studying the causal relationship between medical care spending and labor outcomes, exploiting a policy which directly impacted medical spending for reasons unrelated to health and using a unique data set which includes medical spending and labor earnings. Our focus on labor outcomes is motivated by its potential usefulness as a measure of health, the importance of understanding the relationship between health and labor productivity, and the policy interest in improving labor outcomes for the population that we study - injured workers. We exploit the 2003-2004 California workers' compensation reforms which reduced medical care spending for injured workers with a disproportionate effect on workers suffering lower back injuries. We link administrative data on workers' compensation claims to earnings and test the effect of the reforms on labor force outcomes for workers who experienced the biggest drop in medical care costs. Adjusting for the severity of injury and selection into workers' compensation, we find that workers with low back injuries experienced a 7.3% greater decline in medical care after the reforms, and that this led to an 8.3% drop in post-injury earnings relative to other injured workers. These results suggest jointly that medical care spending can impact health and that health affects labor outcomes.

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1 Introduction

There is considerable controversy over the value of the marginal dollar spent on medical care. Policy debates over health care often focus on the idea that there is a nontrivial 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 promote value in health care through the elimination of care with benefits that are considered low relative to the costs. Despite the widespread concern about rising medical costs, we have relatively little evidence about the impact of health care spending on important health and productivity outcomes.

Given the concern with rising medical spending in the United States, the lack of research on the impacts of this spending on labor productivity is striking. Health is an important component of an individual's and an economy's productivity, yet there is limited evidence concerning whether and how medical care affects labor outcomes. When evaluating the cost-effectiveness of additional medical spending, additional work capacity and labor earnings are obvious potential benefits that should be considered. However, the economic literature rarely evaluates the relationship between health care generosity and labor productivity.

Estimating the causal relationship between medical spending on health or labor outcomes is confounded by several issues. Individuals receiving more care tend to be less healthy. Previous work has attempted to address this by comparing population-level averages of medical spending and health, finding little relationship between health outcomes and medical expenditures across countries or regions.[1] Furthermore, it is difficult to quantify health in a useful manner 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,[2] but these measures almost certainly fail to meaningfully capture the full relationship between health and labor productivity.

Our paper deals with both of these issues by exploiting a natural experiment to estimate the causal relationship between health care and labor force outcomes. We es-

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[1] See Phelps [2000] for a discussion of this literature.

[2] 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.

timate whether an exogenous decrease in medical care expenditures for individuals with work-related injuries harms their future labor outcomes. Our experiment comes from the 2003-2004 California workers' compensation reforms, which introduced utilization review and other mechanisms designed to reduce spending on medical care for injured workers. We exploit the fact that these reductions were not uniform across different types of injury, which provides exogenous variation in the generosity of medical care treatment based on the type of condition that requires treatment. 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 affected specific types of injuries.

There are several advantages to studying medical care in workers' compensation. Individuals in need of medical care are potentially subject to policy changes which impact medical treatment for reasons unrelated to health or labor productivity. Furthermore, injured workers are an important policy concern on their own. Work-related injuries have large effects on the welfare and income of households. Moreover, while the growth in per capita medical costs in the United States is well-known, medical costs per injured worker in workers' compensation programs has outpaced it, implying that study of the effectiveness of medical care in this context is especially policy-relevant. Finally, workers' compensation programs are significant government programs and important medical care providers in the United States, but the medical component of these programs has been understudied in the economics literature. Workers' compensation programs in the United States in aggregate were a $57.5 billion program in 2010 (Sengupta et al. [2012]), including $28.1 billion in medical care costs.

We estimate the relationship between medical spending and labor outcomes using linked administrative data combining workers' compensation claims with earnings. The workers' compensation claims were obtained from the Workers' Compensation Insurance Rating Bureau (WCIRB) of California, which records information on workers' compensation claims by insured firms in California, for workers injured from 2000 and 2006. To obtain earnings data, we linked these claims to unemployment insurance records from the Employment Development Department (EDD). These data include information on the earnings of uninjured workers at the same (pre-injury) firm as the injured workers, matched based on pre-injury earnings and tenure. Thus, we can observe the drop in earnings associated with the worker's injury relative to workers with similar pre-injury earnings capabilities, and we can test whether the magnitude of this earnings drop is related to differential changes in

medical care resulting from the reforms.

Our preferred estimates suggest 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% decrease in post-injury earnings. These percentages translate to a \$767 reduction in medical spending decreasing post-injury earnings by \$428 per quarter (or \$2,567 in the first 6 quarters post-injury). This large response is due partially to injured workers delaying their return to work when medical spending is less generous. We are also able to study the impact of the medical care reforms on the timing of the effect and find that returning to work is most affected in the first 6 months after the injury, though we find persistent effects throughout our time period. Furthermore, we provide evidence that the differential shocks to medical care generosity in our analysis were not correlated with differential shocks to other types of workers' compensation benefits.

Our results are robust to concerns about differential selection into the workers' compensation system and we model these potential selection mechanisms explicitly. We also account for the possibility that the incentives to report injuries changed disproportionately for some injury types over our time period due to the reforms or due to secular trends. This 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 specifically for this type of selection. If we ignore selection, however, the primary conclusions of this paper would remain the same.

Our work intersects two important literatures in the health and labor economics fields. First, we provide evidence that additional medical spending improves health. The health literature has found mixed evidence concerning the effectiveness of medical spending. We study a reform that reduced medical care generosity overall, and did so disproportionately for workers with low back injuries. Our results suggest that the workers who experienced the biggest decline in medical care generosity also experienced worse post-injury labor market outcomes.

Second, our findings contribute to a limited labor literature on the effects of health on labor productivity. Because labor productivity may impact health through a variety of channels, it is difficult to isolate the causal effect of a change in health on labor outcomes. 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

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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). Under the assumption that medical care affects health, we have a useful experiment for observing how exogenous shifts in health can impact labor productivity. Our results suggest that health is an important component of labor productivity and that health investments in injured workers can increase earnings capacity and affect labor supply behavior.

In the next section we briefly discuss the prior literature on the relationship between medical care and health and labor productivity. We also provide an overview of the California workers' compensation system and describe the reforms that provide our exogenous variation in medical spending. Section 3 discusses the data. We introduce the empirical strategy in section 4 and the results in section 5. Section 6 concludes.

2 Background

2.1 Medical care, health, and productivity

A vast literature finds that the United States has relatively high medical spending without better outcomes (see Garber and Skinner [2008]). Fisher et al. [2003] finds that regions with higher Medicare spending do not have better health outcomes or more satisfaction with care. Baicker and Chandra [2004] find that higher spending areas tend to use less effective types of care. Fisher et al. [2009] track different trends in geographic medical expenditures and finds that faster growth in medical costs is not associated with better health outcomes.

However, recent evidence has begun to question just how little effect medical spending really has. Chandra and Staiger [2007] find that regional specialization driven by productivity spillovers mute the aggregate relationship between spending and outcomes. Romley et al. [2011] find 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 finds significant effects on mortality. Doyle [2011] uses geographic variation in medical spending in Florida for patients that were visiting Florida to circumvent concerns that individuals living in high

spending areas are different. Doyle et al. [2012] use ambulance referral patterns to predict the medical costs associated with a patient, finding that higher costs are associated with lower mortality. We should note that while some of the better-identified papers account for patient selection (eg., sicker patients selecting into places with more expensive care), they rarely account for the possible non-causal relationship between quality and spending at the provider level. A patient may "randomly" be treated at a high spending hospital, but the spending at the hospital is not random and it is difficult to determine whether the additional spending drives the better health outcomes or some unobserved factor leads to both better outcomes and additional spending.

Understanding the role and importance of health in explaining labor outcomes has also interested economists despite the empirical difficulties associated with it. Thomas and Strauss [1997] study the relationship of health and wages in Brazil, using individual and community characteristics as instruments for health. Schultz and Tansel [1997] uses geographic variation in food prices and health services in Ghana and Côte d'Ivoire to instrument health status. Jäckle and Himmler [2010] uses data from Germany to estimate a simultaneous equations models which separately identifies health and wages. Our paper contributes to this literature by using a policy change which affects medical care and, potentially, health. While labor outcomes likely have some independent effect on health, we utilize variation that impacts health for reasons unrelated to labor productivity, labor supply preferences, human capital, etc. Furthermore, labor income is an important outcome on its own. A work injury represents a potentially large income shock to the household and understanding means of reducing this loss is a concern of policymakers.

It is generally accepted that poor health will lower labor force participation, but there is relatively little evidence on the effectiveness of medical care in improving productivity. While most of the literature focuses on mortality or subjective measures of health, Garthwaite [2012] studies the effect of Cox-2 inhibitors on labor force participation and labor earnings for ages 55-75. This study finds that pharmaceutical innovations can improve labor outcomes for an older population.

Overall, it is difficult to find variation in medical spending that is plausibly unrelated to health status at the individual- or geographic-level. Even most instrumental variable strategies tend to rely on assumptions that the geographic location of the person at the time of care or adverse health event is exogenous. Our paper exploits a policy change which differentially affected medical care spending based on the type of injury. It is rare in the

literature to find such a policy change which directly altered the care afforded to a person. This is a major advantage of studying a workers' compensation program since the care received by injured workers is potentially subject to legislative reforms and spending cuts. This allows us to study the impact of variation in medical expenditures for reasons unrelated to health status or individual choices that are potentially related to health.

2.2 Workers' compensation reform in California

Work-related injuries, illnesses and fatalities have long been an economically important factor affecting the country's labor force and health. In 2011, there were 3.8 million reported nonfatal occupational injuries and illnesses, including 1.2 million which resulted in days away from work (Bureau of Labor Statistics [2012]). In the United States, state workers' compensation systems provide the primary source of compensation for the lost wages and medical expenses associated with workplace injuries.

While we may often think of workers' compensation as a singular government program, it actually refers to a series of state laws 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. And traditionally the bar that workers have had to meet to have an injury determined work-related has been fairly low, with only a small connection to work generally sufficient to ensure compensation.

While states vary considerably in the extent to which they provide compensation for earnings losses, all states require employers to provide full compensation for necessary medical expenditures with almost no cost-sharing. Workers' compensation in the U.S. developed in the early 1900's as a compromise that eliminated costly and uncertain lawsuits by 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 avoided cost-sharing as a method to constrain utilization.

While the rapid growth of per capita medical expenditures in the United States has been well-documented, medical costs associated with workers' compensation have outpaced even this fast growth rate. Using data on health care expenditures from the National Health Expenditure Accounts (NHEA) published by the Center for Medicare and Medicaid Services (CMS), we graph the growth in per capita health care expenditures for the United States. Alongside this trend, we show the growth in workers' compensation medical costs per injury using data published in Sengupta et al. [2012]. Figure 1 normalizes both numbers to 100 in year 1995.

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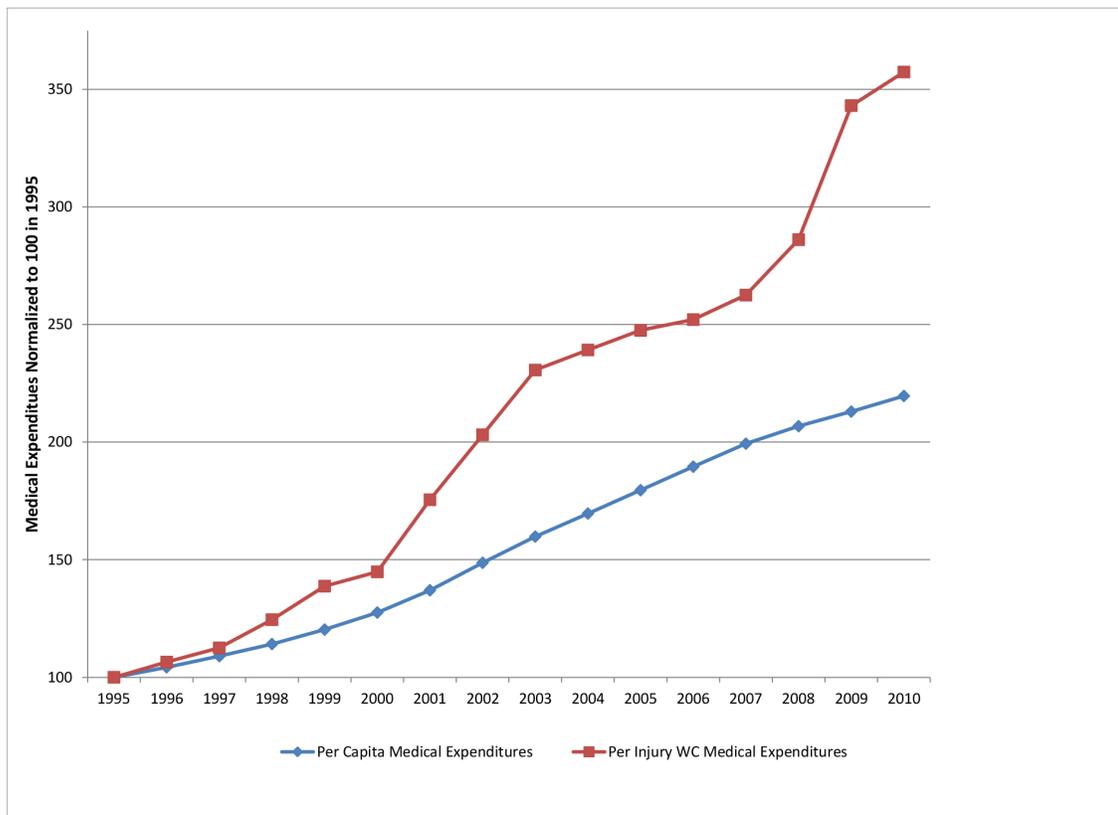

Figure 1 shows that medical costs in workers' compensation have been rising at

a rate even faster than overall per capita health expenditures. We observe especially fast growth in workers' compensation relative to per capita health costs beginning in 2000. The growth rate of the workers' compensation leveled off significantly from 2004-2007, partially due to reforms adopted in California and elsewhere aimed at curbing costs.

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 injuries covered by workers' compensation typically result in significantly higher medical expenditures than comparable injuries experienced by individuals who receive treatment through private insurance (Baker and Krueger [1995], Durbin et al. [1996], Johnson et al. [1993], Johnson et al. [1996]). This has motivated the adoption of utilization review and other mechanisms to contain the growth of medical expenditures in workers' compensation cases.

Workers' compensation reform has been a particularly important and prominent issue in California. The California workers' compensation is the largest in the country. In 2011, there were 464,100 work-related reported injuries, about 12.2% of the national total (Bureau of Labor Statistics [2012]). California represents an even larger share of the nation's workers' compensation costs, accounting for $9.4 billion or 16% of the national total (Sengupta et al. [2012]). Of these benefits, $5.1 billion were attributable to medical care, about 18% of the national total.

In the early 2000's, costs in the California workers' compensation had risen to extreme levels. Between 1995 and 2002, the insurance premiums charged to employers to provide workers' compensation coverage had increased from $5.5 billion to $14.7 billion (Division of Workers Compensation [2003]). At its peak in 2003, employers in California were paying $6.29 per $100 of payroll (Commission on Health and Safety and Workers' Compensation [2006]). Excessive payments and unpredictable costs caused many insurance companies to liquidate or withdraw from the workers' compensation market. Medical costs were thought to be a major cause of rising premiums in California before the reforms. Medical costs per injured worker were more than twice as large as the national average in 2002 (calculations made using Williams et al. [2004]). 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

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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 were effective in reducing medical costs. Utilization of chiropractic care and physical therapy declined by more than 50% (Swedlow [2005a]). Estimates suggest that the medical costs for insurers fell approximately 24% from 2003 to 2007 (Workers Compensation Insurance Rating Bureau of California [2010]).

Importantly for our purposes, the effect of these reforms on medical care spending was not uniform. The reforms disproportionately impacted back injuries through its limits on chiropractic care and physical therapy. This differential effect has been noted elsewhere (see Swedlow [2005b]). We can also observe this difference in our data. We report changes in medical spending by injury type from the pre-reform period (2000-2002) to the post-reform period (2004-2006). Table 1 shows that lower back injuries experienced a larger drop in medical spending than the other injury types. These differential effects on medical expenditures across injury types is key to our identification strategy, outlined below.

Table 1: Mean Medical Expenditures by Injury Type Before and After Reforms

	Pre-Reforms	Post-Reforms	% Change	% Change Relative to Lower Back
Shoulder	$11,560	$8,763	-24.2%	4.4%
Knee	$11,306	$8,553	-24.3%	4.3%
Hand/Wrist	$8,942	$6,847	-23.4%	5.2%
Total (Excluding Lower Back)	$10,084	$7,738	-23.3%	5.4%
Lower Back	$10,510	$7,501	-28.6%	

Source: 2000-2006 Injuries from the California WCIRB. 2003 injuries excluded from calculations. Pre-reform refers to 2000-2002; post-reform refers to 2004-2006.

3 Data

We have 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 provided data using the Uniform Statistical Reporting Plan (USRP) which 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.

These data have been matched 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 our sample. We use the pre-injury information to control for individual-level labor productivity. Specifically, we control for earnings in the 6 quarters before the injury. The post-injury earnings data allow us to study the effect of the reform on post-injury outcomes. Some of our analysis will look at the timing of the effect since we have earnings by quarter.

These data also include matched workers for each injured worker. Up to 5 workers employed at the same firm were selected based on earnings in the year immediately before the injured worker's injury. The control workers include workers with earnings within 15 percent of the log standard deviation of the earnings of all injured workers. Tenure at the firm was also a factor in selecting control workers. In our results section, we will show some graphs of earnings ratios - the earnings of the injured workers divided by the earnings of the control workers. Before the injury, these ratios are centered around one. In the quarters immediately before the injury, this relationship is mechanical. However, there is little reason to expect the ratio to be different from one even before the time period used to match the workers and, in fact, we find that the ratios even up to 12 quarters before the injury are centered around one.

A limitation of the WCIRB data is that they only cover workers injured at insured firms. We do not believe that this is problematic given that our source of variation is based on injury type and firms cannot self-insure only certain types of injuries while remained insured through an outside company for other injuries. Our specifications include industry-time fixed effects and these should account for firm-level entry and exit decisions. We also

account for selection on this dimension more directly in three additional ways. First, our primary specification will model selection by injury type and account for differential selection. Although this selection adjustment is not specifically motivated by firm-level selection, it should still account for differential selection whether it occurs at the individual- or firm-level. Second, we show that our results are robust to using a "balanced sample," which we define as selecting on firms that appear in our data both pre- and post-reform. This result suggests that the industry-time effects, in fact, are adequate to account for firm entry and exit. Finally, we note that small firms are always insured through outside companies and we create an additional "selection instrument" based on this insight. We allow our selection adjustment to also depend on this term and find that our results are robust to modeling firm-level selection more explicitly.

The data also only provide information on the total medical expenditures in the first 18 months post-injury. We do not know the timing of these expenditures. In our analysis, we will estimate the differential effect of the reforms on medical spending and on labor outcomes. We do not use instrumental variables because we believe that the reforms may have impacted medical care through channels other than total spending, such as the timing of care. Instead, we interpret all results as showing that the reforms differentially affected medical care for lower back injuries and this medical care was less costly. And, the reforms - through medical care changes - affected labor outcomes.

4   Empirical Strategy

We exploit the differential effect that the California reforms had on the medical care generosity for different types of injuries. It is rare in the economics literature to exploit a shock which directly impacts the medical treatment afforded for specific conditions. The California reforms reduced medical care generosity for all injured workers but disproportionately affected lower back injuries. We compare the outcomes for workers with lower back injuries to workers with other common work injuries: shoulder, knee, and hand/wrist injuries. We choose these injuries because they are common and had similar per-injury medical costs and pre-injury earnings before the reforms.

We exclude 2003 for most of our analysis. Some of the reforms took effect in 2003 but many were not in effect until 2004. Consequently, injuries in 2003 are only partially treated. We drop 2003 though we will show that this decision has little effect on our final

conclusions.

We model earnings as

$$w_{ijkqt} = \exp\left(\alpha_{jk} + \gamma_{jt} + \phi_{jq} + \beta M_{ijkt} + \mu_i + \nu_{kt}\right) \eta_{ijkqt} \tag{1}$$

where w_{ijkt} is a measure of post-injury earnings for individual i working (pre-injury) in industry j with injury type k injured at quarter q in year t. M represents medical care generosity. We include fixed effects for each industry-injury type, industry-year, and industry-quarter. Time is based on the time of injury since the regulations governing medical care generosity depend on when the injury took place. We believe that the industry-time interactions are especially important to account for the effects of the other aspects of the reforms. The reforms affected not only medical care generosity but also earning replacement rates, which also likely change post-injury labor supply behavior. These interaction terms should account for common trends in these behaviors and net out the other effects of the reforms, helping us to isolate the differential impact of the changes in medical care generosity. We will also directly test the assumption that these time fixed effects are accounting for the other impacts of the reforms. We present results at the end which suggest that the other facets of the reforms are not biasing our results.

Medical expenditures are likely endogenous to post-injury earnings. People with more severe injuries will, on average, receive more expensive medical care and are less likely to return to work, implying that $E\left[\eta_{ijkt}|M_{ijkt}\right] \neq 1$. Instead, we study the differential effect that the reforms had on back injuries for (1) medical expenditures and (2) post-injury labor outcomes. While expenditures themselves are likely related non-causally to post-injury outcomes, the differential effect of the reform should not be since they are not dependent on individual-specific factors such as injury severity. We represent these outcomes by y and estimate the specification:

$$y_{ijkqt} = \exp\left(\alpha_{jk} + \gamma_{jt} + \phi_{jq} + \theta\left[\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_k\right)\right] + \mu_i + \nu_{kt}\right) \eta_{ijkqt} \tag{2}$$

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] discuss the limitations of a log-linear equation and the advantages of using an exponential form. We

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estimate equation (2) using nonlinear least squares. In practice, the first-order conditions are the same as those of a Poisson pseudo-maximum likelihood (PPML) estimator. While typically used for count data, this estimator has nice properties even with a continuous outcome variable. As long as the conditional mean of the outcome is specified correctly, this estimation technique will provide consistent estimates. While it is frequently argued that Poisson regression requires additional assumptions, Silva and Tenreyro [2006] illustrate that it actually requires fewer assumptions than OLS estimation of its log-linear counterpart by placing less structure on the error term, allowing both multiplicative and additive error terms. Furthermore, our data contain a non-trivial number of people with no post-injury earnings. These workers make estimation of a log-linear model difficult since ln(0) is undefined. Equation (2) is appropriate even when the outcome variable is sometimes equal to 0. Let X represent all covariates (including interaction terms) in equation (2). We assume

$$E[y_{ijkqt}|X_{ijkt}] = \exp\left(\alpha_{jk} + \gamma_{jt} + \phi_{jq} + \theta\left[\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_k\right)\right] + \mu_i + \nu_{kt}\right). \quad (3)$$

We could simply proceed with a Poisson regression of post-injury earnings on X. This specification is equivalent to a straightforward difference-in-differences strategy, widely-employed in applied work. Given the richness of our data, however, we can address two remaining concerns directly. We have explicitly modeled these concerns in (2) with μ_i and ν_{kt}. In our data, we only observe reported injuries. The composition of reported injuries may be changing and these changes may be related (secularly or causally) to the reforms. This would imply that even if the change in medical care generosity has no effect on post-injury outcomes, we may still observe differential changes in labor outcomes. Note that many of these concerns are alleviated through the inclusion of industry-year fixed effects. Workers in some industries, possibly with a disproportionate number of lower back injuries, may experience changes in injury reporting. The industry-year fixed effects will account for these common trends and make comparisons within that industry in the post-reform period. We show results using this variation. However, our data allow us to account for the possibility that reporting trends may be more systematic than this example. Consequently, we consider two additional possible sources of systematic bias.

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4.1 Selection Concern: Worker Productivity Differences, μ_i

First, we are worried that the reforms will impact the underlying skill or labor productivity of the population receiving workers' compensation. The industry-time fixed effects should reduce concerns of this selection issue as they account for common within-industry incentives to report injuries and other common shocks. However, the differential effect of the reforms on medical care expenditures for lower back injuries may disproportionately affect the type of people that report their injuries and that we subsequently observe in our data set. If the change in medical care generosity impacted the reporting behavior of high earnings and low earnings workers (within the same industry) differentially by injury type, then our shock to medical care is correlated with μ_i. To account for this possibility, we model

$$\mu_i \equiv g(\text{Post-Injury Wages of Control Workers}_i, \text{Pre-Injury Wages of Injured Workers}_i) \equiv g(L_i). \tag{4}$$

Our data contain a wealth of information on the wages of similar workers (where similarity is determined by 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 up to 6 quarters 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 observed workers changes over time, our rich earnings data should capture these changes. We use L_i for the worker's earnings potential and will show results using different sets of the earnings control variables. In most specifications, we include all pre-injury earnings (Q1-Q6) or pre-injury earnings for Q2-Q6.[3] We prefer the latter because there is some evidence that injured workers may have experienced an earnings drop in the quarter immediately before they reported their injury. Our results are robust to the inclusion of the Q1 earnings, but we primarily show results controlling only for Q2-Q6. In fact, we should note that controlling for pre-injury earnings seems to have little effect on our estimates once the post-injury wages of control workers are included.

In practice, we include a term for the wages of the matched control workers and a term for the pre-injury wage measure. In each case, we use the log of the wage term when the wages are positive and set this term equal to 0 if the wage variable is 0. We include a separate dummy variable equal to 1 if the wage variable is equal to 0.

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[3]We have up to 12 quarters of pre-injury earnings for the years 2002-2006. We find similar results using this sample and controlling for all 12 quarters of pre-injury earnings.

4.2 Selection Concern: Injury Severity, ν_{kt}

We are also concerned that $E\left[\exp(\nu_{kt})|X_{ijkt}, \text{Report Injury}_{ijkt}\right] \neq 1$. Again, note that overall or industry-specific common shocks to the distribution of injuries that are reported are accounted for flexibly through the inclusion of industry-time fixed effects. Our concern is that even after controlling for industry-time fixed effects and changes in workers' earnings potential, the reported injuries may differentially change in severity over time. For example, if the incentives to report minor back injuries change relative to the incentives to report minor arm injuries, then the exogenous covariates predict changes in observed injury severity. Thus, we may not only observe different workers in terms of pre-injury labor productivity, we may observe workers with different injury severity. These workers may experience a drop in earnings potential that cannot be captured by their pre-injury labor earnings or the labor earnings of their control workers. Note that there is a mechanical selection effect inherent in our data because our data only includes claims with total benefits in excess of $2,000. Again, this is not necessarily problematic, but we leverage our rich data to further reduce the possibility of systematic bias. Before describing our strategy to account for this possibility, we show changes in reporting behavior over time in Figure 2.

Figure 2 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 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, it does not appear that the reforms had an important effect. While this graph is encouraging, there is also some evidence of possible secular trends. They do not appear systematic, but we will attempt to account for them in our analysis.

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Figure 2: Changes in Composition over Time
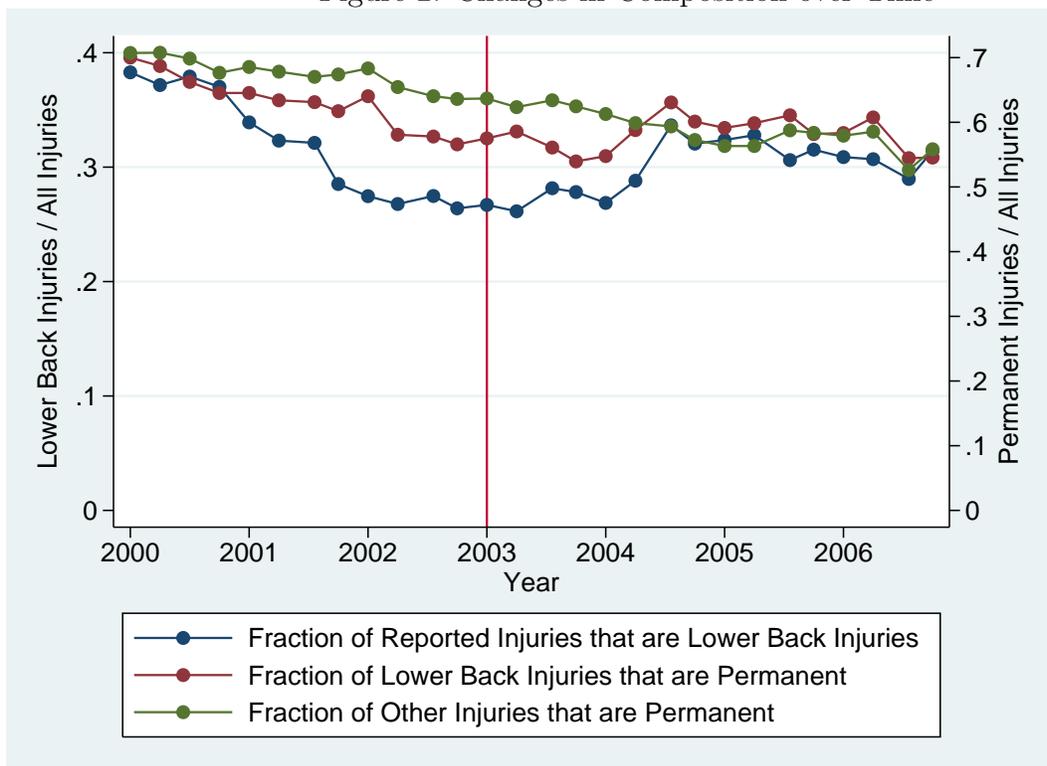
Graph represents total number of lower back injuries divided by the total number of injuries (lower back, shoulder, knee, hand/wrist) in each quarter. It also includes the fraction of back injuries and fraction of other injuries which are permanent.

The motivation for our strategy to account for differential changes in injury severity over time is that the most serious injuries are more likely to always be reported (and incurred), regardless of the workers' compensation environment. Our ideal experiment is to find injuries with the same severity in the pre- and post-reform periods. Some of these injuries, due to the reforms, experience a large drop in medical care generosity relative to other injuries. Since the severity of the injuries has not changed, we can assume that any outcome changes are due to the changes in medical care policy. While we observe measures of injury severity for the injuries in our data, it is possible that the reforms affected these ratings so we do not attempt to "match" pre- and post-reform injuries based on the severity ratings. Instead, we control for systematic changes in ν_{kt} through a selection adjustment.

Though we do not observe a consistent measure of severity, we can imagine selecting on industries which incur a disproportionate number of severe injuries for a specific injury type in the pre-reform era. We could focus our analysis on industries with a large number of

very severe injuries. Since these injuries are likely to always be reported, severity selection is less likely to be an issue in these industries.

We operationalize this experiment by predicting the differential change in the number of reported injuries using our exogenous variables X and a separate shock to selection, employing the idea that initial severity should predict changes in the number of reported injuries. We then include the predicted number of reported injuries in the final specification. Variation in this term accounts for differences in injury severity selection.

In a typical selection model framework (Newey [2009]), the idea is to compare observations with similar probabilities of selection into the sample. These observations likely have the same bias due to selection so the comparison differences out the selection bias. Proper identification requires an additional "selection instrument" which affects selection without otherwise impacting the outcome variable. We create a "selection instrument" $h_{jkt} = \mathbf{1}(\text{Post}_t) \times (\text{Pre-Reform Injury Severity Measure}_{jk})$. 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

$$\ln R_{jkqt} = \tilde{\alpha}_{jk} + \tilde{\gamma}_{jt} + \tilde{\phi}_{jq} + \tilde{\theta}\left[\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_k\right)\right] + \eta h_{jkt} + \upsilon \equiv W'_{jkqt}\delta + \upsilon \qquad (5)$$

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 from the shock to medical care generosity. We estimate equation (5) using OLS. 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.[4] 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 medical care instrument. Note that it is important that the selection instrument impacts the number of reported injuries above and beyond the $\left[\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_j\right)\right]$ variable. Furthermore, note that the independent effects of the pre-reform injury severity variable are accounted for non-parametrically through the inclusion of the industry-injury type interactions.

[4]We prefer OLS estimation because we want to include the predicted value of $\ln R$ in our main specification, not R.

Estimating equation 5 is equivalent to estimating the probability of reporting of an injury. Assume that N, the "true" number of injuries (reported and unreported) can be modeled as

$$\ln N_{jkqt} = \breve{\alpha}_{jk} + \breve{\gamma}_{jt} + \breve{\phi}_{jq} + \breve{\theta}\left[\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_k\right)\right] + \breve{\eta} h_{jkt} + \breve{v}. \tag{6}$$

In words, the number of real injuries is simply of function of industry-injury type interactions and secular industry-time trends. We can even allow the number of injuries to be impacted by the reforms themselves and initial severity. If P is the probability of reporting a work-injury, then $\ln P_{jkqt} = \ln R_{jkqt} - \ln N_{jkqt}$. Estimating equation (5), then, is sufficient for our purposes. We can arrive at an exogenous shock to the number of injuries reported, which is equivalent to controlling for changes in the probability of reporting.

Intuitively, we are estimating which industry-injury 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 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.

4.3 Final Specification

Our final specification is:

$$y_{ijkqt} = \exp\left(\alpha_{jk} + \gamma_{jt} + \phi_{jq} + \theta\left[\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_j\right)\right] + f(L_i) + \psi(W'_{jkt}\hat{\delta})\right)\eta_{ijkqt} \tag{7}$$

Because of the use of a two-part model, we bootstrap our standard errors using a clustered (by industry-injury type) non-parametric bootstrap. We report 95% confidence intervals using the 2.5% and 97.5% values estimated in the bootstrap. For this reason, the results with the selection adjustment may not have symmetric confidence intervals. There are 83 separate industry-injury type combinations in our data.

We use $100 \times (\exp(\theta) - 1)$ for all calculations to interpret the differential effect of the reforms on lower back injuries. Let y^0 be the outcome variable (earnings) if lower back injuries did not experience a differential effect due to the reforms ($\theta = 0$) and let y^{reforms}

be the effect if the reforms did have a differential effect. Then,

$$\frac{y^{\text{reforms}} - y^0}{y^0} = \exp(\theta) - 1$$

When the outcome is a dummy variable for whether or not the individual has returned to work, we estimate

$$P(\text{Work}_{ijkqt} = 1) = \Phi\left(\alpha_{jk} + \gamma_{jt} + \phi_{jq} + \theta\left[\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_j\right)\right] + f(L_i) + \psi(W'_{jkt}\hat{\delta})\right) \tag{8}$$

We report the impact of $\text{Post}_t \times \mathbf{1}\left(\text{Back Injury}_j\right)$ on the probability of working. We compute the marginal effects using the method discussed in Puhani [2012] to report this probability shift.[5] 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. We report the mean of this probability increase in our data. For inference, we use a nonparametric clustered bootstrap.

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[5]Puhani [2012] argues that Ai and Norton [2003] does not provide the parameter of interest.

5 Results

5.1 Graphical Evidence
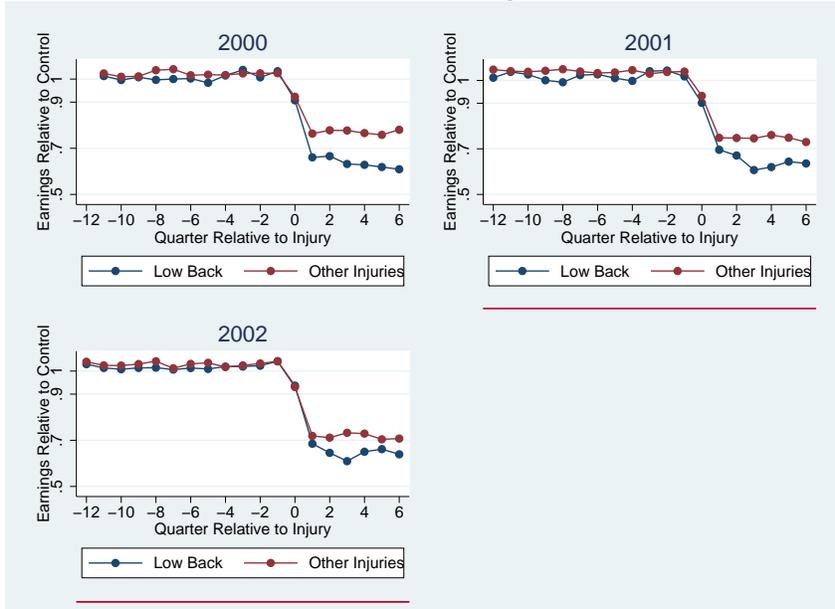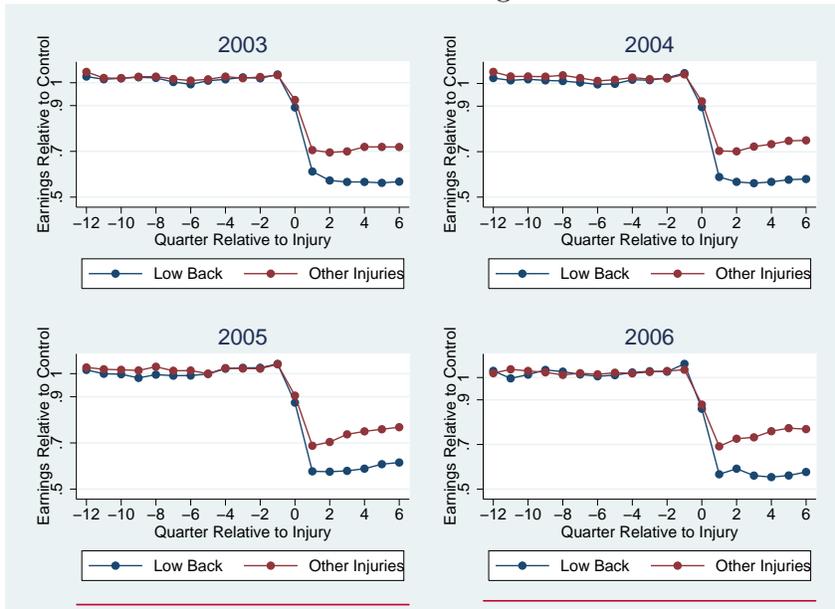
Figure 3: Pre-Reforms

Figure 4: Post-Reforms

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

We can use our data to 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. As we explained in the Data section, the control workers are selected based (partially) on the similarities of their earnings to the injured worker in the year prior to the injury. Consequently, the ratio is close to one in the pre-injury quarters immediately prior to the injury for mechanical reasons. 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 does suggest that the injured and uninjured workers were not on separate trends prior to the injury.[6]

Figure 3 shows these graphs for each of the pre-reform years in our data.[7] After a lower back injury, injured workers' earnings drop to 64% in 2000, 65% in 2001, and 65% in 2002 of the earnings of their control workers.[8] 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.

Figure 4 includes the same graphs for the post-reform period. In our analysis below, we exclude the 2003 data since some of the reforms were enacted for 2003 but some were not. We include 2003 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. This is a large decrease relative to the pre-reform period. The other injuries did not experience such a decrease - 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.

Another important lesson from Figures 3 and 4 is that the injured workers appear to experience an earnings loss beginning in the quarter prior to the recorded injury. This could simply result from a lag between the time of the injury and the time it is reported and recorded. In our analysis, we control for pre-injury earnings. Our preferred results will exclude the earnings in the quarter prior to the injury since these may be affected by the injury. This choice has no effect on our results.

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[6]Note that different trends would not be problematic given our empirical strategy if those trends remained constant after the reforms, but it is comforting that no trends appear to exist.

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

[8]These numbers are simply the average across all 6 post-injury quarters.

5.2 Regression Analysis

Table 2 presents summary statistics for our data. We present these statistics separately for lower back injuries and the other 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 suggests the use of the exponential in our main specification.

In Table 3, we report the relationship between medical expenditures and post-injury earnings. As expected, people with more medical care consumption have worse post-injury outcomes. Since we do not observe perfect measures of injury severity, it is likely that this relationship is due to the additional demand for care associated with worse injuries. People with worse injuries are also less likely to return to work or earn as much. This pattern of results suggests the need to study the effects of a policy change which impacted people for reasons unrelated - conditional on covariates - to injury severity. We study the 2003-2004 reforms and look at the differential impact that these reforms had on workers experiencing back injuries.

In many of our specifications, we 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 industry-injury type combination. All of our results are robust to the measure of severity used in this step. However, in the analysis that follows, we report results using one specific measure of severity. Our goal is to use injuries that were likely to be reported regardless of incentives in the workers' compensation program. Using permanent injuries, excluding the least severe permanent injuries, should be adequate for our purposes. We used the severity rating in 2000 to classify an injury in the top 75% of permanent injuries. Thus, our selection instrument is equal to

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

We will also check the robustness of our results by using all permanent injuries in the selection instrument. We estimate equation (5) using the selection instrument. We present the results in Table 4 for both our primary selection instrument and the instrument using all permanent injuries. The first column presents the results that will be used 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 $\times$ quarter of injury $\times$ year of injury interactions. The results are consistent with 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 2 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. Furthermore, Table 4 provides some evidence that lower back injuries did incur a differential change in reporting correlated with the reforms.

Before studying the effect on labor outcomes, we examine whether the reforms did, in fact, affect back injuries more than other injuries in terms of medical spending. Table 5 is equivalent to a "first stage" regression to test whether our policy intervention impacted medical spending. 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) uses pre-injury earnings and 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. 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 very consistent across specifications, inclusion of different controls, and use of the selection adjustment. A coefficient of -0.076 implies that back injuries experienced about a 7.3% decline in medical care expenditures relative to all other injuries.

Table 6 presents the main results of this paper. We find consistent evidence that people with back injuries experienced relatively large drops in post-injury outcomes after the reforms. We also find some evidence of selection issues. Column (1) includes no control for labor productivity or selection. We find a statistically significant estimate of -0.103. In Column (2), we add the earnings of the matched control workers to the specification and estimate an effect of -0.070, still statistically significant at the 1% level. We estimate an effect

of -0.062 (shown in Column (3)) when using pre-injury earnings instead of the earnings of the control workers. Column (4) excludes the quarter immediately prior to the injury, but 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 absolute value) when this selection adjustment is made. Our concern was that the least severe lower back injuries may disappear from our data after the reforms since the return to reporting such an injury is less beneficial due to the less generous medical care or, more mechanically, because those observations fall below the $2,000 of total costs threshold in our 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 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 meaningful effect on the estimates. However, the estimated effect is robust to the inclusion of further labor productivity controls. The selection adjustment also seems to affect the estimates. Combining Tables 5 and 6, we can infer that the 7.3% decline in medical care expenditures was associated with an 8.3% drop in earnings.

We also study the effect on returning to work. We define returning to work as having positive labor earnings in the first 18 months after the worker's injury. Table 7 reports the 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 of back injuries by 1.5 percentage points, relative to their probability of working had the reforms not had a differential effect on back injuries. This estimate is only significant at the 10% level.

We can also look at the timing of the effects. In Table 8, we present results of the differential effect of the reforms on earnings by quarter. All results include the earnings of the control workers in that period, the pre-injury earnings (Q2-Q6) for the worker, and the selection adjustment. Again, we find consistent evidence of an effect. All of the estimates are negative and significant, though the effect at the last quarter in our data is only significant

at the 10% level. In our data, we do not have information about the timing of the receipt of care. However, we refer to Swedlow [2005b] which used data from the Industry Claims Information System, a special data set with medical utilization at 3, 6, and 9 months post-injury. We discuss this paper more in Section 5.3. Here, we note that even at 3 months, lower back injuries experience reduced utilization post-reform relative to all other injuries according to data in Swedlow [2005b].[9] Consequently, a response even at the first quarter post-injury is likely.

Table 9 presents probit regression results for working in each specific post-injury quarter. As in Table 7, we find effects on this dimension, suggesting that generous medical care increases the probability of returning to work. In Table 7, we looked at whether the individual had returned to work at all in the first 6 quarters after the injury. That is not a cumulative measure that increases if the worker returns sooner (within the first 18 months) so the Table 7 and 9 results are reporting different - though complementary - evidence. The Table 9 results show that the largest effect is in quarter 2, and the effect steadily decreases after that period.

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 the central policy variable of interest - does a reduction in total medical costs harm labor outcomes? Changes in prices and utilization could both potentially impact health outcomes. We think that the differential effect of the policies on medical costs for lower back injuries should primarily be a utilization effect. The pricing changes were more uniformly applied, while the utilization caps were more targeted towards care used by workers with lower back injuries. Thus, the differential effect would represent a change in utilization. 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 earlier. Swedlow [2005b] uses medical utilization data for closed claims in the pre- and post-reform periods to evaluate changes in utilization. The paper reports utilization for 7 different categories. Utilization is reported by year. 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 reported in

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[9]Calculations made by authors.

Swedlow [2005b]. Using the data for all injuries and the relative frequency of the injuries reported in Swedlow [2005b], 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 very similar to the differential effect that we estimate on total expenditures, suggesting that our estimates do represent a change in utilization.

5.4   Robustness Checks

Table 11 presents a series of robustness checks for three outcomes - medical expenditures, earnings, and working. 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. This variable was shown in Table 4 to predict selection. Column (3) shows that the results are consistent with our previous findings.

In Table 12, we include 2003 as a pre-reform year. We have previously excluded 2003 since it is partially treated. 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. Overall, though, we find that our results are relatively robust to the inclusion of 2003 as part of the pre-reform era.

We previously discussed the benefits of using nonlinear least squares in our context. Earnings are highly-skewed and there is a benefit to allowing the reforms to have a proportional effect to initial earnings. In Table 13, we replicate our results using ordinary least squares and find similar evidence.

Next, when discussing the data, we mentioned that the 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 exit our data for only specific injuries so there should not be any systematic bias. Furthermore, we previously discussed that the 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. Our strategy assumes that changes are due to individual-level changes, but the strategy still account for differential changes regardless of the source.

To verify that our empirical strategy does account for firm-level selection, we implement two different strategies. In the top half of Table 14, we report results using a "balanced sample." We select on firms that have at least one injury in each year of our data. Note that this leads to a small sample and noisy estimates. Despite the noise, the estimated coefficients are generally the same magnitude as our main results. These results suggest that firm-level selection is not driving our results.

Our second strategy is to directly account for firm-level selection. We note that small firms should almost always be included in our data as only large firms tend to self-insure. Consequently, we create a variable which uses the initial composition of firm size in each industry-injury type combination. We create

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

We estimate our selection equation using both h_{jkt} and d_{jkt} as selection instruments. We find that both are statistically significantly related to the number of injuries. After the reforms, more firms began to self-insure, but these were primarily large firms. The bottom half of Table 14 reports our results using this additional selection instrument to help adjust for selection concerns. We find similar estimates as before.

5.5   Other Aspects of the Reforms

Our analysis assumes that the California reforms differentially affected 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 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 back injuries were more or less affected by other facets of the reforms. The most important and salient part of workers' compensation benefits is the indemnity benefits. The formula used to calculate indemnity benefits was changed by the reforms. We calculate replacement rates for each worker in our data and then estimate equation (7) with the replacement rate as the outcome variable. Table 15 presents these results in Column (1). While indemnity benefit generosity was changed by the reforms, there is no evidence that lower back injuries were differentially affected. If anything, the results suggest that workers with back injuries received less generous benefits, implying that they should return to work earlier than workers with other injuries.

We may also be 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.

The reforms also affected generosity of vocational rehabilitation vouchers. There is little evidence that these vouchers had any 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 lower back injuries did not appear to benefit more or less than other injuries due to the reforms on dimensions other than medical care.

Finally, we can account for these other dimensions more explicitly. Other facets of the reform were triggered by the severity ratings, including changes in the benefit formula and generosity of vocational rehabilitation vouchers. We coded up categories based on all the severity rating thresholds in the California workers' compensation program after the reforms and interacted these categories with year dummies.[10] We included these interactions in our main specification. The results are reported in the bottom half of Table 15. The results are noisier due to the inclusion of a large number of additional controls, but the estimates are generally supportive of our earlier findings.

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[10]To clarify, we interacted these categories for all year dummies even when those thresholds were not active.

5.6 Discussion

We find consistent evidence that the 2003-2004 California workers' compensation reforms reduced medical spending disproportionately for back injuries. Our results suggest that workers with lower back injuries make less money after the reforms and are less likely to return-to-work. The results imply that medical care spending for 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 drop in post-injury earnings. We estimated that the reforms reduced the probability of returning to work in the first 18 quarters by 1.5 percentage points.

6 Conclusion

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. We use the differential effect of the 2003-2004 California workers' compensation reforms on the medical spending for lower back injuries. We find that lower back injuries experienced a 7.3% decline in medical spending after the reforms relative to other injuries. Consequently, workers incurring lower back injuries experienced an 8.3% decline in post-injury earnings and a reduction in the probability of returning to work. 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 suggest 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 this medical spending is efficient.

7 Tables

Table 2: Summary Statistics

	Lower Back Injuries		Other Injuries	
	Mean	SE	Mean	SE
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	

Table 3: 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 4: 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%. 95% Confidence Intervals in brackets adjusted for clustering at industry-injury type level. "Severe Permanent Injuries" refer to the top 75% of permanent injuries.

Table 5: 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, -0.045]	[-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 6: 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***	-0.062***	-0.063***	-0.064***	-0.065***
	[-0.133, -0.073]	[-0.101, -0.038]	[-0.094, -0.031]	[-0.092, -0.034]	[-0.098, -0.030]	[-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***	-0.086***	-0.087***
	[-0.161, -0.049]	[-0.169, -0.022]	[-0.162, -0.017]	[-0.146, -0.020]	[-0.165, -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 7: Differential Impact of Reforms on Probability of Working Post-Injury

Dependent Variable	1(Work within 18 months of Injury)			
Post x Back Injury	-0.015***	-0.011***	-0.011***	-0.015*
	[-0.022, -0.008]	[-0.018, -0.005]	[-0.017, -0.004]	[-0.036, 0.002]
Control Wages	No	Yes	Yes	Yes
Pre-Injury Wages	No	No	Q2-Q6	Q2-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., Q2-Q6 implies earnings for quarters 2 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 8: 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**	-0.074**	-0.105**	-0.099**	-0.064*
	[-0.188, -0.032]	[-0.157, -0.022]	[-0.121, -0.000]	[-0.194, -0.008]	[-0.248, -0.003]	[-0.171, 0.020]
Control Wages	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Injury Wages	Q2-Q6	Q2-Q6	Q2-Q6	Q2-Q6	Q2-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 in the same period. Pre-Injury Wages included represented by quarters (eg., Q2-Q6 implies earnings for quarters 2 to 6 pre-injury are included as a covariate).

Table 9: Differential Impact of Reforms on Probability of Working in Each Quarter

Dependent Variable:	Post-Injury Earnings					
Post-Injury Quarter:	Q1	Q2	Q3	Q4	Q5	Q6
Post x Back Injury	-0.013*	-0.037**	-0.033***	-0.029**	-0.022**	-0.016*
	[-0.029, 0.003]	[-0.065, -0.012]	[-0.050, -0.012]	[-0.050, -0.004]	[-0.047, -0.006]	[-0.047, 0.002]
Control Wages	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Injury Wages	Q2-Q6	Q2-Q6	Q2-Q6	Q2-Q6	Q2-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 in the same period. Pre-Injury Wages included represented by quarters (eg., Q2-Q6 implies earnings for quarters 2 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 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

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

Table 11: Robustness Checks: Richer Controls and Different Selection Instrument

	(1)	(2)	(3)
Dependent Variable:	**Medical Expenditures**		
Post x Back Injury	-0.076***	-0.078***	-0.073**
	[-0.116, -0.045]	[-0.118, -0.051]	[-0.110, -0.043]
Dependent Variable:	**Post-Injury Earnings**		
Post x Back Injury	-0.082***	-0.096***	-0.083***
	[-0.224, -0.013]	[-0.173, -0.056]	[-0.164, -0.016]
Dependent Variable:	**1(Work within 18 months of injury)**		
Post x Back Injury	-0.016*	-0.017***	-0.015**
	[-0.032, 0.000]	[-0.032, -0.009]	[-0.024, -0.002]
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q2-Q6	Q2-Q6	Q2-Q6
Control Wages x Injury Type	Yes	No	No
Pre-Injury Wages x Injury Type	Q2-Q6	No	No
Selection Adjustment	Severe Permanent Injuries	Severe Permanent Injuries	Permanent Injuries
Industry x Quarter of Injury	Yes	Yes	Yes
Industry x Year of Injury	Yes	No	Yes
Industry x Quarter of Injury x Year of Injury	No	Yes	No
Industry x Injury Type	Yes	No	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., Q2-Q6 implies earnings for quarters 2 to 6 pre-injury are included as a covariate). "Severe Permanent Injuries" refers to top 75% of permanent injuries.

Table 12: Including 2003 as a Pre-Reform Year

	(1)	(2)	(3)
Dependent Variable	Medical Expenditures	Post-Injury Earnings	1(Work)
Post x Back Injury	-0.090***	-0.057*	-0.016**
	[-0.123, -0.058]	[-0.114, 0.009]	[-0.025, -0.003]
Year 2003	Included	Included	Included
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q2-Q6	Q2-Q6	Q2-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).

Table 13: OLS Estimates		
Dependent Variable	Medical Expenditures	Post-Injury Earnings
Post x Back Injury	-905.09***	-2041.79*
	[-1369.22, -332.27]	[-3359.31, 150.23]
N	158,065	158,065
Control Wages	Yes	Yes
Pre-Injury Wages	Q2-Q6	Q2-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., Q2-Q6 implies earnings for quarters 2 to 6 pre-injury are included as a covariate). Coefficient units are dollars.

Table 14: Firm-Level Selection

	Balanced Sample		
	(1)	(2)	(3)
Dependent Variable	Medical Expenditures	Post-Injury Earnings	1(Work)
Post x Back Injury	-0.078	-0.069	-0.021*
	[-0.264, 0.105]	[-0.199, 0.075]	[-0.070, 0.025]
N	16,064	16,064	16,064
Sample	Balanced	Balanced	Balanced
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q2-Q6	Q2-Q6	Q2-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
	Include Firm-Level Selection Term		
	(4)	(5)	(6)
Dependent Variable	Medical Expenditures	Post-Injury Earnings	1(Work)
Post x Back Injury	-0.072***	-0.082***	-0.015**
	[-0.115, -0.033]	[-0.149, -0.016]	[-0.027, -0.002]
N	158,065	158,065	158,065
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q2-Q6	Q2-Q6	Q2-Q6
Selection Adjustment	Injury + Firm	Injury + Firm	Injury + Firm
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., Q2-Q6 implies earnings for quarters 2 to 6 pre-injury are included as a covariate). "Balanced Sample" selects on firms that have at least one injury in the data in each year. "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 in the industry x injury type.

Table 15: Other Dimensions of Reforms

	(1)	(2)	(3)
Dependent Variable	Replacement Rate	Defense Costs	1(Vocational Rehabilition)
Post x Back Injury	-0.034	-0.006	0.006
	[-0.278, 0.195]	[-0.378, 0.392]	[-0.026, 0.034]
N	158,065	158,065	158,065
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q2-Q6	Q2-Q6	Q2-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**
	[-0.116, 0.026]	[-0.186, -0.018]	[-0.033, -0.004]
N	158,065	158,065	158,065
Control Wages	Yes	Yes	Yes
Pre-Injury Wages	Q2-Q6	Q2-Q6	Q2-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.

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