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

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Abstract

Medical care represents an important component of workers' compensation benefits with the potential to improve health and post-injury labor outcomes, but little is known about the relationship between medical care spending and the labor outcomes of injured workers. We exploit the 2003–2004 California workers' compensation reforms which reduced medical spending disproportionately for workers incurring low back injuries. We link administrative claims data to earnings records for injured workers and their uninjured co-workers. We find that workers with low back injuries experienced a 7.6% post-reform decline in medical care, and an 8.1% drop in post-injury earnings relative to other injured workers.

Keywords

Medical Care Effectiveness; Health; Labor Productivity; Workers' Compensation

JEL classification

I12; I13; J24; J28; J38

1 Introduction

Workers' compensation provides the primary source of insurance for the lost wages and medical expenses associated with workplace injuries in the United States, shielding workers from bearing the full burden of such injuries. In total, workers' compensation is a \$62.3 billion program (Baldwin and McLaren (2016)), equivalent in size to the Earned Income Tax Credit (EITC).¹ The medical care side of workers' compensation has always been a central

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¹All costs are for 2014. The EITC cost the federal government \$66.7 billion in 2014 (IRS (2016)).

component of the program and now accounts for the majority of the program's costs. Many states have struggled with the rising medical costs associated with workers' compensation, motivating the implementation of reforms throughout the country to restrict medical care. While medical care provided through workers' compensation is an important source of social insurance for workers, little economic research has addressed its role in improving post-injury labor outcomes, and there is sparse evidence about the relationship between medical care spending and labor productivity more broadly (Hirth et al. (2003)).

While the rise in health spending in the United States is well-documented, medical spending in workers' compensation rose over 60% faster than overall per capita health expenditures from 1996 to 2010, as shown in Figure 1. The dramatic rise in workers' compensation medical costs has acted as an impetus for large-scale state reforms which attempt to curb medical care utilization. In California, the subject of this paper's analysis, employer workers' compensation costs per \$100 of payroll increased to an unprecedented degree from \$2.69 in 2000 to \$6.29 in 2003 (Workers' Compensation Insurance Rating Bureau of California (2013)), by far the highest rate in the country and motivating major reforms targeting medical costs in the state. The importance of this rise can also be observed in Figure 1, which demonstrates that national trends over this time period are sensitive to the inclusion of the 2000–2003 California workers' compensation crisis. The skyrocketing costs and subsequent reforms in California preceded similar policy responses in other states. In reaction to a recent wave of these reforms, a Department of Labor report “sounds an alarm” that workers' compensation systems are providing increasingly inadequate support for injured workers (United States Department of Labor (2016)).

Workers' compensation plays an important role in the United States safety net. Many jobs carry significant risk – in 2015, there were 3.7 million reported nonfatal occupational injuries and illnesses (Bureau of Labor Statistics (2016)) – and workers' compensation shifts some of this risk away from the worker. Relative to many other social insurance programs, however, workers' compensation has received comparatively less attention from economists. 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); Hyatt (2011)). The medical side of workers' compensation, on the other hand, has been generally ignored by the economic literature.

Social insurance programs often have to balance the benefits of risk protection with distortionary incentives. With health care coverage, such concerns center on the overconsumption of medical care similar to distortions induced by public and private health insurance more generally. In the workers' compensation context, the program has traditionally leaned heavily towards the “risk protection” side of this tradeoff for the medical care component. 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. Consequently, it is not surprising that medical costs have been a primary driver of recent precipitous increases in workers' compensation

costs in many states. While cash benefits provide the core consumption smoothing gains of social insurance, medical care generosity within workers' compensation also may contribute consumption smoothing benefits through the improvement of post-injury labor outcomes while also providing non-monetary health benefits.

The lack of evidence about the effectiveness of workers' compensation medical care reflects the difficulties of isolating causal impacts given that workers receiving more care tend to be less healthy. This issue parallels the identification problems found in the literature studying the returns to additional medical care spending. There is considerable controversy over the return to the marginal dollar spent on medical care, and there is little work on the scope for medical care to improve labor productivity.

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 injured worker costs relative to other states. With 470,600 work-related reported injuries in 2015 (Bureau of Labor Statistics (2016)), California accounts for 11% of the country's work-related reported injuries, yet it accounts for over 19% of national workers' compensation costs (\$12.1 billion) and over 22% of national workers' compensation medical costs (\$7.0 billion) (Baldwin and McLaren (2016)). Relative to other states, workers' compensation costs were especially high in California prior to the 2003–2004 reforms, primarily due to medical costs per injured worker, which were more than twice as large as the national average in 2002.²

The reforms represent a natural experiment to estimate the causal relationship between medical care and labor market outcomes. We study whether an exogenous decrease in medical care expenditures for individuals with work-related injuries harms future labor outcomes. While the reforms affected several dimensions of workers' compensation, such as indemnity benefits, these components were applied uniformly while the medical care reforms disproportionately targeted medical treatments associated with low back injuries.

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, each injured worker was matched with up to 5 non-injured workers at the same firm based on pre-injury earnings (in the four quarters before the injury) and tenure. We collected earnings information for these "control workers" as well.

We estimate that the injuries most affected by the reforms - low back injuries - experienced a 7.8% reduction in medical expenditures relative to other injuries and that workers incurring low back injuries consequently experienced a relative decrease in post-injury earnings of 8.1%. We do not estimate similar earnings reductions for the control workers or for pre-

²Calculations made by authors using data in Williams, Reno and John F. Burton (2004).

injury earnings. 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. The differential shocks to medical care generosity across injury types were not correlated with changes to other workers' compensation benefits, and we find no evidence that the reforms differentially affected the composition of injured workers. We also study administrative workers' compensation data from Oregon, which did not implement major workers' compensation reforms during this time period, and do not observe similar effects.

This paper provides important evidence about the role and value of medical care in affecting post-injury outcomes, exploiting the most substantial reforms to the largest workers' compensation program in the United States. It contributes to the literature on the effectiveness of medical care as well as the literature on the causal relationship between health and labor outcomes by studying the labor consequences of a policy shock to medical care generosity with health as the implicit channel. Overall, our results suggest jointly that medical care affects health and health investments increase earnings capacity. The next section provides an overview of the California workers' compensation system and describes the reforms that provide our 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 present the results in Section 5. Section 6 concludes.

2 Background

2.1 Workers' Compensation

Workers' compensation is the oldest form of social insurance in the United States and the majority of states have programs dating back more than 100 years (Barth (2004); Fishback and Kantor (1998)). Workers' compensation is a series of state programs – the only major social insurance program with no federal oversight or minimum national standards – 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.³ In 2014, state programs covered 132.7 million workers at a cost to employers of \$91.8 billion (Baldwin and McLaren (2016)).⁴ 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.

³Since 2014, firms in Oklahoma may opt-out of the traditional workers' compensation system as long as they adopt an alternative plan with comparable coverage.

⁴Employer costs include premiums plus any payments paid by the employer before reaching the deductible. For self-insured firms, employer costs are the total costs paid.

Firms purchase workers' compensation coverage from a private insurer or state fund, though many large employers choose to self-insure. In 2014, 23 states had a state fund, including California. The State Compensation Insurance Fund is a competitive state fund in California which offers insurance to firms. While the primary program benefits are wage replacement and medical care, workers' compensation often includes additional forms of assistance such as job retraining to foster occupation switches. Cash benefits vary across states in both magnitude and duration and typically depend on the type of claim.

There are three broad types of workers' compensation claims. The vast majority of claims are medical-only which tend to be small in terms of costs. Temporary disability claims pay benefits until the worker is able to return to work (as determined by a physician) or a maximum duration is met (e.g., 104 weeks in California).⁵ Typically, temporary benefits are set at two-thirds of the worker's pre-injury weekly earnings (at the employer in which the injury occurred), subject to minimum and maximum payment levels, and are untaxed. For example, the maximum weekly benefit in California was \$728 in 2004 while the minimum benefit was \$126. Injured workers who do not improve may be transferred to permanent disability. Permanent disability claims involve workers incurring injuries that result in permanent impairments affecting the ability to work. State workers' compensation systems vary widely in how partial permanent disability benefits are determined (see Burton (2005) for details). In California, the amount and duration of cash benefits for permanent disability claims depend on the severity rating ("impairment level")⁶ of the injury as determined by a "qualified medical evaluator." Permanent total disability benefits are paid at the temporary disability rate in California while partial permanent disability benefits follow a separate schedule of minimums and maximums.

The focus of this paper is the medical side. Workers' compensation covers 100% of all medical costs associated with the at-work injury. The medical component of workers' compensation has become an increasingly important and costly factor. The cost of medical care in workers' compensation cases would not necessarily be a public policy concern if it represented efficient utilization of care. However, an older literature found that comparable (on observables) 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)).

The policy tradeoff in this context is the promise of providing appropriate medical care coverage to workers if injured (i.e., insurance) versus the potential for the over-utilization of care. Constraining utilization directly offers one possible solution to this tradeoff but often requires the reliance on general guidelines, potentially removing the ability of patients and physicians to individualize care appropriate for the injury. The rationing of medical treatment through utilization review and other mechanisms of managed care is often promoted, more generally, as a means to improve value in health care through the elimination of care with benefits considered low relative to costs, but the consequences of

⁵Unless a physician states that the worker can return to their job without restrictions, firms are not required to offer injured workers their former jobs or modified work. Firms are bound by the federal American Disabilities Act (ADA) to accommodate workers with disabilities unless this accommodation would result in an undue burden on the firm.

⁶Injuries with a rating of 100% are *total* while injuries with ratings between 1% and 99.75% are *partial*.

constraining medical care utilization for injured workers are not well-understood. In the workers' compensation context, a reduction in medical care may also diminish consumption smoothing benefits if, as we study in this paper, medical care improves post-injury earnings.

2.2 Interactions with Social Security Disability Insurance

Workers' compensation and Social Security Disability Insurance (SSDI) together provide the primary social safety net for workers with disabilities,⁷ targeting different but overlapping populations. The determination of injuries that qualify for benefits varies across the two systems, though there is a subset of injuries that are eligible for both programs. Workers with disabilities expected to last at least 12 months may apply for SSDI benefits. In general, workers' compensation and SSDI operate separately though there is a federally-mandated offset of benefits such that injured workers cannot receive total payments between the two programs exceeding 80% of their pre-injury average earnings. While previous work has demonstrated a link between workplace injuries and later SSDI benefit receipt (O'Leary et al. (2012)), McInerney and Simon (2012) finds no statistical evidence that changes in workers' compensation generosity (in terms of cash benefits) affect applications or enrollment in Social Security Disability Insurance (SSDI).⁸

There are two possible mechanisms through which the SSDI program could affect observed outcomes associated with workers' compensation. First, one requirement for SSDI enrollment is that the individual does not engage in "substantial gainful activity" (SGA) defined as monthly earnings of at least \$700 in 2000 (increasing to \$860 by the end of our time period).⁹ Applicants must reduce their earnings to below SGA for 5 months or until a decision on their application is made (whichever is longer). SGA restrictions could influence the post-injury labor supply decisions of injured workers applying for SSDI benefits. Those that end up receiving SSDI benefits may also respond to benefit receipt through an income effect and because of restrictions on monthly earnings.¹⁰ If medical care affects the probability of applying for and receiving SSDI benefits, then the SSDI program is a possible mechanism that explains our results. In our data, we do not see any evidence of injured workers targeting the SGA thresholds and we estimate strong behavioral responses for a wide range of the earnings distribution, including parts of the distribution that are well above SGA restrictions.

Second, the SSDI program grew rapidly during our sample period and may have affected the pool of workers that eventually incur an at-work injury and enroll in workers' compensation.¹¹ We test for systematic changes in the composition of our injuries, whether driven by the SSDI system, secular trends, or the workers' compensation reforms themselves. We show that, on a rich set of characteristics, there is no statistical evidence of a differential change

⁷Supplemental Security Income (SSI) is also targeted to individuals with disabilities but typically not to workers. SSI benefits are primarily available to low-income people with little work history.

⁸The labor supply consequences of SSDI are studied in Maestas et al. (2013) and French and Song (2014).

⁹The amounts provided are for the non-blind.

¹⁰SSDI beneficiaries may earn up to a monthly threshold while receiving benefits (\$530 in 2000). Furthermore, for a "trial work period," defined as any nine months over a 60 month period, they may surpass this threshold.

¹¹Between 2000 and 2006 the percentage of SSDI beneficiaries categorized with "musculoskeletal system and connective tissue" disabilities grew from 22.9% to 25.9% (Table 21 of Social Security Administration (2014)) in the United States and from 21.4% to 23.8% in California.

for low back injuries correlated with the timing of the reforms. For example, we cannot reject that the pre-injury earnings of workers suffering low back injuries remained constant pre- and post-reforms.

2.3 Interactions with private health insurance

Workers' compensation covers all medical expenses but recent state reforms implementing utilization review permit denial of coverage for unapproved treatments. There are no restrictions for injured workers to seek additional treatment on their own and pay for this medical care out-of-pocket or through private or other types of public insurance. We are unaware of any empirical analysis studying the effects of workers' compensation medical care generosity on private health insurance claims. There is mixed evidence concerning the effects of private health insurance expansions on workers' compensation expenditures.¹² In Section 5.6.2, we study a large data set of private medical claims and find no evidence of a response in private claims to reductions in medical care generosity in workers' compensation.

2.4 The California Workers' Compensation Experience

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)). An evaluation 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)). Medical costs were higher than other states "primarily resulting from higher utilization, not prices" (Telles et al. (2004)). In response, California adopted dramatic reforms to the delivery of medical care in workers' compensation.

The California medical care reforms are especially important for two reasons. First, they represent major reforms to the largest workers' compensation program in the United States. Second, high and rising workers' compensation medical costs have motivated several states to adopt similar medical care reforms (e.g., Indiana HB1320 in 2013; Delaware HB175 in 2013) or begin the process of debating such reforms (e.g., New Hampshire governor issued an Executive Order creating the Commission to Recommend Reforms to Reduce Workers' Compensation Medical Costs¹³). To highlight a few examples: Utah enacted new treatment guidelines in 2006; Texas in 2007; Kansas in 2008; New York in 2010; New Mexico in 2013; Tennessee in 2016 (HB194).¹⁴ As long as rising medical costs put upward pressure on workers' compensation costs, states will continue to push for increased cost control.

¹² Armour et al. (2016) exploit the Affordable Care Act's dependent coverage expansion to estimate that a 10 percentage point reduction in uninsurance (in California, Florida, New Jersey, and New York) leads to a 6–9% drop in the number of workers' compensation bills. However, Dillender (2015) finds the opposite relationship in Texas (where workers' compensation is not mandated).

¹³ <http://www.governor.nh.gov/media/news/2014/pr-2014-09-04-workers-compensation-commission.htm> accessed December 22, 2014.

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, passed in 2003, implemented utilization review and adopted the American College of Occupational and Environmental Medicine (ACOEM) Occupational Medical Practice Guidelines. SB 899, passed in 2004, 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 limiting both physical therapy and chiropractic visits to 24 visits per injury. Before the reforms, physical therapy and chiropractic manipulation accounted for 37% of outpatient medical payments paid by California's workers' compensation program (Swedlow (2005b)). SB 899 also limited occupational therapy to 24 visits. In addition, the reforms also tied the fee schedule in workers' compensation to Medicare. Swedlow (2005a) reports 45.1% reductions in the number of physical therapy visits and 55.8% reductions in the number of chiropractic manipulation visits in 2004 relative to 2002.

The medical care reforms were implemented in 2004. The caps on chiropractic care, physical therapy, and occupational therapy applied only to injuries incurred on January 1, 2004 or after. The ACOEM guidelines took effect in April 2004 and applied immediately to all injuries, including care for injuries incurred before 2004. The ACOEM guidelines generally recommended amounts of care well below the caps. For low back injuries, the guidelines recommended 1–2 physical therapy visits and a maximum of 12 chiropractic manipulations (Swedlow et al. (2005)). This would imply that the caps were, generally, not binding once the guidelines took effect shortly after. We consider these two components of the reforms together as a major shock to medical care spending to study the scope for workers' compensation medical care policy to affect labor outcomes and the role of medical care as part of the workers' compensation protection against the full consequences of a workplace injury.

Importantly, for the purposes of isolating the effects of the medical care reforms, these reforms did not have uniform impacts across all injury types. All injuries were affected, but the medical reforms disproportionately impacted low back injuries. 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 (2005b)).¹⁴ Industry reports at the time predicting the effect of the utilization review component by itself (ignoring the caps) often focused on the impending reductions of care for low back injuries specifically (e.g., Harris and Swedlow (2004)). The ACOEM guidelines received poor evaluations from physicians for the care provided for several injury types, but these evaluations gave especially poor marks for the quantity of care established for low back injuries by the guidelines (Nuckols et al. (2005)) and even recommended the adoption of a separate set of guidelines specifically for low back injuries.¹⁵ Studying pre-reform claims

¹⁴Information found at <http://le.utah.gov/2006/bills/static/HB0150.html>; See Rule 137.100 at <https://www.tdi.texas.gov/WC/dm/index.html>; <http://www.dol.ks.gov/WorkComp/odg.aspx>; <http://www.workerscomp.state.nm.us/research/bulletins/summer2013.pdf>

¹⁵Utilization of chiropractic care and physical therapy was high for many injury types pre-reform but higher, on average, for low back injuries.

¹⁶Quantity of physical therapy and chiropractic care under the ACOEM guidelines for lower back injuries were rated "validity uncertain" and "not comprehensive," respectively. Quantity of care for shoulder and hand/wrist injuries received more positive ratings.

data, Swedlow et al. (2005) reports that 79.1% of workers with low back injuries received more physical therapy than would be recommended by the ACOEM guidelines while 48.7% received more chiropractic care than recommended in just the first 30 days post-injury.

We observe these differential changes in our data. Figure 2 shows the ratio of mean medical expenditures by quarter of injury for low back injuries to the mean expenditures for other injuries (shoulder, knee, and hand/wrist).¹⁷ We shade the time period in which injuries were affected by the *implementation* of the reforms since both our medical spending and earnings measures refer to the six quarters post-injury and the treatment guidelines affected injuries incurred before implementation.¹⁸ Low back injuries and other injuries initially have similar medical care spending. The spending on low back injuries relative to other injuries increases slightly in the pre-reform years and then drops substantially as the reforms are implemented.

Figure 2 also provides graphical evidence of effects on labor outcomes, previewing the main results of this paper. We graph the ratio of mean post-injury earnings for workers with low back injuries relative to the earnings of the other injury types. The relative labor earnings of low back injuries closely parallels the relative generosity of medical care (correlation=0.75). Before the reforms, both ratios are increasing. By the time that the reforms have been fully-implemented, the medical care and post-injury earnings of low back injuries relative to other injury types are substantially lower. We also observe a strong correlation between the two trends within the implementation region as injuries become increasingly exposed to the reforms. We observe an especially large relative drop when injured workers are first fully-exposed to the medical reforms in the second quarter of 2004. The lower earnings persist throughout the post-reform period.

2.5 Literature on medical care, health, and productivity

A vast literature finds that the United States has relatively higher medical spending without better health outcomes (see Garber and Skinner (2008); Fisher et al. (2003); Baicker and Chandra (2004); Fisher et al. (2009)). However, recent evidence questions the ineffectiveness of the marginal dollar of care in the United States (Chandra and Staiger (2007); Romley et al. (2011); Luce et al. (2006); Cutler et al. (2006)), including work exploiting natural experiments (e.g., Doyle (2011); Doyle et al. (2015)).

Understanding the role and importance of health in explaining labor outcomes has also interested economists though the empirical challenges in isolating a causal relationship have limited the amount of research in this area.¹⁹ One exception is Garthwaite (2012) which studies the effect of the removal of Cox-2 inhibitors from the market on labor force participation and labor earnings for individuals ages 55–75. This study concludes that pharmaceutical innovations can improve labor outcomes for an older population. Our paper

¹⁷Because both outcomes in Figure 2 have high variances, we provide the same graph by year of injury in Figure A1 since the outcomes are less noisy at the more aggregated level. Figure A2 presents the same figure for return-to-work.

¹⁸We mark the final quarter of 2002 as part of the implementation region, though only a fraction of injuries incurred during this quarter were potentially affected by the guidelines within 18 months of the injury.

¹⁹As 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).

contributes to the literature studying the effect of health on labor outcomes by exploiting a plausibly exogenous policy shock to health through changes in medical care.

The medical literature provides some evidence concerning the labor effects of the types of care affected by the California reforms. Research has found a relationship between use of physical therapy and injury duration (Zigenfus et al. (2000)). In a randomized trial for injured workers with work-related back injuries, those receiving higher intensity care returned to work faster (Loisel et al. (1997)).²⁰

3 Data

We use administrative data on workers' compensation claimants from the WCIRB, which collects information from licensed workers' compensation insurers in California for the purposes of calculating recommended premium rates. The WCIRB data include 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. Medical costs refer to incurred costs in the first 18 months after injury.

We matched these claims to earnings data from the base-wage file maintained by the California Employment Development Department (EDD).²¹ All employers covered by unemployment insurance are required to report quarterly earnings to the EDD. This matching gives us earnings for our sample for at least six quarters both before and after each injury. More information about this merge is included in Section A.1.

For each injured worker, we selected up to five co-workers at the same (pre-injury) firm.²² 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 show graphically below. The control worker match is explained in greater detail in Section A.2.

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. We will also show results from models with firm-year interactions and the estimates are similar.

For complementary evidence, 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. The main disadvantages of the DEU data are that the data do not

²⁰Lambeck et al. (2010) randomized use of integrated care and also finds especially large effects on time before returning to work. Other research has quantified the labor outcomes of those with back injuries and the association of returning to work with use of workers' compensation (e.g., Andersson (1999); Gallagher et al. (1995)). Other studies have estimated associations with workers' compensation coverage using non-labor outcomes (see Harris et al. (2005) for a review).

²¹We were able to merge earnings data between 1998 and the second quarter of 2008 with our claims data.

²²6.6% of our sample only has one match, 5.1% has two matches, 4.0% has three matches, 3.4% has four matches, and 81.0% has five matches. Our results when we use the control workers' labor outcomes are robust to using only workers with five matches.

provide information about medical costs, only include permanently disabled workers, and do not specifically categorize low back injuries.²³ For these reasons, we focus on the WCIRB data. However, our results are consistent across both data sets, providing further evidence that the exclusion of self-insured firms is not biasing our results.²⁴

Table A1 presents summary statistics for the linked WCIRB data. We present these statistics separately for low back injuries and other injuries – shoulder, knee, and hand/wrist injuries. Low back injuries and other injuries have nearly equal probabilities of being permanent injuries and the severity rating distributions are similar. The cumulative distribution functions for earnings are presented in Figure A3 by injury type.

In Figure A4, we show the ratio of the labor earnings of injured workers to the labor earnings of their control workers by quarter relative to injury. A ratio of 1 implies that that the injured worker and control workers have, on average, equal post-injury earnings. After a low back injury, injured workers' earnings drop to 64% of the earnings of their uninjured peers in the control group before the reforms.²⁵ For other types of injuries, earnings drop to 74%. After the reforms, the earnings reduction for these other types of injuries remains about the same (73%), but it decreases substantially for low back injuries to under 58%.²⁶

Even before the reforms, we observe lower post-injury earnings for low back injuries which motivates modeling outcomes as proportional effects. Moreover, we will study pre-injury earnings and the earnings of the control workers, which are much larger than the post-injury earnings of injured workers. Estimating proportional effects is likely more appropriate given these differences.

4 Empirical Strategy

We exploit the differential effect of the 2003–2004 California reforms on the medical care generosity for different types of injuries to implement a difference-in-differences empirical design. The California reforms reduced medical care generosity for all injured workers but the reforms disproportionately affected low back injuries. We compare the outcomes for workers with low back injuries (31.9% of our sample) to workers with other common work injuries: shoulder (12.2%), knee (20.2%), and hand/wrist (35.7%) injuries. These injuries are the most common in our data²⁷ and have similar medical costs and pre-injury earnings in the pre-reform period.²⁸ We exclude 2003 for most of our analysis because this is a partially treated year.²⁹

²³The DEU data include the category “neck, spine, or pelvis injury.” 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.

²⁴The consistency of the results likely reflects the lack of responsiveness on the self-insurance dimension to the reforms. 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)). 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.

²⁵These numbers reflect the averages across all 6 post-injury quarters.

²⁶Figure A5 shows the same ratios using employment as the outcome.

²⁷One exception is a broader classification labeled “other lower extremity” 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 it. Empirically, inclusion of this category has little impact on the final results. Excluding mental illness injuries, low back injuries are 19.0% of *all* injuries in the data; shoulder injuries are 7.6%; knee injuries are 12.4%; hand/wrist injuries are 22.1%. “Other lower extremity” injuries are 12.1%.

We study the differential impact of the reforms on low back injuries on both medical care spending and post-injury labor earnings, implementing a difference-in-differences design. The identifying assumption is that low back injuries, in the absence of the 2003–2004 medical care reforms, would have experienced similar outcome changes as other injury groups. We will test this assumption by studying pre-reform trends through event study analyses. We will also test whether the composition of injuries is changing over time and whether the reforms differentially impacted the generosity of other workers' compensation benefits. We previously discussed reasons motivating the estimation of proportional, instead of level, effects. We model earnings and medical expenditures in the following manner:

$$y_{ijkqt} = \exp(\alpha_{jk} + \gamma_{jt} + \phi_{qt} + \theta[\text{Post}_t \times \mathbf{1}(\text{Back Injury}_k)]) \mu_{ijkqt} \quad (1)$$

where y_{ijkqt} 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 . Our primary labor outcome measure will be total earnings in the six quarters post-injury.³⁰ The above specification includes fixed effects for each industry-injury type, industry-year, and quarter of injury, though we will also show results with different sets of interactions. Time is based on the time of injury. Post_t is an indicator variable equal to one in years 2004 and later while $\mathbf{1}(\text{Back Injury}_k)$ is equal to one for low back injuries (zero otherwise). The industry-year interaction terms account for common industry trends in the outcomes 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. θ is the parameter of interest and represents the differential effect of the reforms for lower back injuries. We will also show event studies in which we permit the differential relationship of low back injuries with the outcomes to vary across time periods.

We model our outcomes as an exponential function because both post-injury earnings and medical expenditures are highly-skewed and it is common to assume they change in percentage terms, though we will also provide linear estimates. We avoid using a log-linear form frequently used in applied work and estimate equation (1) using Poisson regression for several reasons. First, we are interested in the conditional expectation of post-injury earnings (and medical costs), not the conditional expectation of the log of earnings. By Jensen's inequality, we know that $\ln E[y/X] \geq E[\ln y/X]$, a point often made in the health econometrics literature (e.g., Manning and Mullahy (2001); Manning et al. (2005)). Second, Santos Silva and Tenreiro (2006) discusses the limitations of a log-linear specification ("only under very specific conditions on the error term is the log linear representation of the

²⁸Prior to 2003, average medical spending was \$10,510.36 for lower back injuries and \$10,084.45 for the other injuries (in 2006 dollars). Average earnings in the 18 months before the injury were \$47,404.67 for lower back injuries and \$48,875.60 for the other injuries.

²⁹Our measures of medical costs and post-injury earnings refer to the first 6 quarters after the injury. Utilization review was implemented in April 2004 but applied to injuries incurred before that date. Consequently, some injuries suffered in the last quarter of 2002 also experienced a decrease in medical care at the end of the period that we observe them, though we would expect this decrease to be relatively small given the reduced time for the reforms to have an effect. Excluding the last quarter of 2002 does not affect our results in a meaningful manner.

³⁰We have at least 6 quarters of post-injury earnings for each worker so to create a common labor earnings metric for our entire sample, we are limited to the first six quarters. We will also show results by quarter and extend beyond the six quarter threshold for the observations in which such data are available. The medical care spending variable also refers to the first six quarters post-injury.

constant-elasticity model useful as a device to estimate the parameters of interest”) and the advantages of using an exponential form.³¹ Furthermore, our data contain a non-trivial number of people with zero post-injury earnings so a log-linear specification would be inappropriate.³² We adjust all standard errors for clustering at the industry level. For labor force participation, we estimate linear probability models but will also provide marginal effect estimates from probit models.

5 Results

In Figure 2, we showed that there were differential changes in medical expenditures across injury types due to the reforms. We also plotted the differential changes in labor earnings, observing reductions in earnings for low back injuries relative to other outcomes. There is little evidence of confounding pre-existing trends which supports the use of our difference-in-differences design.³³

Our difference-in-differences design is motivated by the likelihood that the relationship between medical care spending and post-injury labor market outcomes is confounded by unobservables. We report estimates of this conditional relationship in Table A2, finding that people with more medical care consumption have worse post-injury outcomes, highlighting the benefits of exploiting a policy change.

5.1 Difference-in-Differences Estimates

We first provide OLS difference-in-differences estimates in Table 1 for medical expenditures, the log of medical expenditures, post-injury earnings, and the probability of working. We estimate that workers with low back injuries incurred a relative decrease in medical care spending of \$731 (in 2006 dollars), a 7.8% reduction relative to the mean. In Panel B, we use the log of medical expenditures as the outcome and estimate larger reductions. In Panel C, we estimate a \$3072 reduction in earnings within the first 18 months after the injury, a 9.1% reduction relative to mean earnings. Finally, we estimate that workers with low back injuries experienced a 2.2 percentage point reduction in the probability of returning to work, relative to other injured workers. All estimates are statistically significant at the 1% level. Replacing the year fixed effects with quarter-of-injury fixed effects (Column 2) does not change the results in a meaningful manner. In Column (3), we interact the injury type indicators with industry fixed effects and we include industry-year interactions in addition to the quarter fixed effects. We estimate that workers with low back injuries received \$711 less in medical spending due to the reforms (7.6% reduction relative to mean). As before, the implied effects are larger when we use the log of medical expenditures. We

³¹While it is frequently claimed that Poisson regression requires additional assumptions, Santos Silva and Tenreiro (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 (in the outcome) error terms. It is commonly thought that Poisson regression assumes that the mean is equal to the variance. While this is a feature of the Poisson distribution, it is not enforced in Poisson regression.

³²10.1% of the main analysis sample has post-injury earnings equal to 0. Related estimators such as negative binomial estimators require additional assumptions and are less robust than Poisson regression. Poisson regression only requires correct specification of the conditional mean while negative binomial models (and other similar models) also require specification of the variance. See Nichols (2010) or Chapter 18 of Wooldridge (2010) for more details.

³³Fitting pre-reform trends for both variables, we estimate slightly positive slopes, though neither slope is statistically different from zero.

estimate a \$2665 reduction in post-injury earnings (7.9% reduction relative to mean) and a 1.4 percentage point reduction in returning to work. Column (4) interacts the quarter fixed effects with industry fixed effects. The results are robust to these more flexible controls.

We include event studies for each of these outcomes in Figure A6, conditioning on quarter fixed effects, industry-year interactions, and industry-injury type interactions (paralleling Column 3 of Table 1), normalizing the relationship between low back injuries and the outcomes to zero in quarter 3 of 2002. As in Figure 2, we shade the “implementation region” in which injured workers become increasingly exposed to the reforms. We observe relative reductions in medical care beginning as the reforms take effect and continuing as injuries become more impacted by the reforms. We observe a similar trend for earnings. For the probability of returning to work, we observe a lagged effect relative to the other outcomes since most workers injured in 2003 had already returned to work before the reforms affected their medical care, reducing the scope for affecting this outcome until late in the implementation period. Overall, the event studies are consistent with the conclusions from Figure 2 that there is little evidence that pre-existing trends can explain the results.

Next, we estimate our preferred specification, shown in equation (1), and present the estimates in Table 2.³⁴ We estimate that low back injuries experienced a 7.9% reduction in medical expenditures relative to other injury types, similar to the effects implied by Table 1. Reporting marginal effects (using Puhani (2012)), this estimate implies a \$612 drop in medical costs. The estimated relative effect for post-injury earnings is -0.122 which corresponds to an earnings reduction of \$3359, similar to the OLS estimate. When focusing on the extensive margin (Panel C), we provide marginal effect estimates from probit regression and find that the reforms led to a 2.0 percentage point reduction in the probability of working for low back injuries relative to other injuries. The estimates for all three outcomes are similar when we use quarter-of-injury fixed effects instead of year fixed effects (Column 2). In Column 3, we condition on quarter fixed effects, industry-injury type interactions, and industry-year interactions. The medical care spending estimate is -0.076 , implying a \$590 decrease. The post-injury earnings estimate is -0.102 , a \$2801 relative decrease. We also estimate that the reforms led to a 1.4 percentage point decrease in returning to work for low back injuries. The estimates for all three outcomes are similar when we include industry-quarter interactions in Column 4.

We present event study equivalents in Figure 3. For both medical care and post-injury earnings, there is a slight increasing trend before the reforms, followed by reductions as injuries become more exposed to the reforms, similar to the patterns observed in Figure 2. As before, we observe that the reduction in the probability of returning to work is delayed relative to these other outcomes. While both earnings and medical care spending are increasing before the reforms, we do not suspect that our results reflect simple mean reversion. First, the upward trends before the reforms are gradual and represent large numbers of injured workers, and are inconsistent with a large transitory shock. Second, after

³⁴We also implemented a J test, introduced in Davidson and MacKinnon (1981), in which the null hypothesis is that the linear specification is the correct model. The test places “weights” on the two different specifications where α is the weight on the exponential specification, $1 - \alpha$ is the weight on the linear specification, and the null hypothesis is represented by $\alpha = 0$. We estimate $\alpha = 0.97$ and reject the null.

the reforms, these outcomes are both below the point estimates from even the earliest parts of the sample, implying that these movements cannot be simply explained by mean reversion.

5.1.1 Balance Tests—Our identifying assumption is that the post-injury labor outcomes of workers with low back injuries would have changed in a manner similar to the outcomes of other injured workers in the absence of the relative change in medical care generosity due to the reforms. We test this assumption in several ways. In this section, we consider whether the composition of injuries was systematically changing over our sample period, due to the reforms themselves or for other reasons. One possible threat to identification is that the reforms altered the incentives to claim workers' compensation benefits and did so systematically.

First, we examine observable changes in low back injuries after the reforms. Given that we control for many of these factors in our analyses, it would not be problematic to find a relationship. However, the lack of evidence of selection on observables suggests that we should not expect selection on unobservables. We first estimate the relationship between working in a specific industry and $\text{Post}_t \times \mathbf{1}(\text{Back Injury}_k)$, conditioning on quarter-of-injury fixed effects and injury type fixed effects. We estimate this specification for each industry; the parameter estimates are shown graphically in Figure A7. There are no statistically significant (at the 10% level) estimates, strongly suggesting that there were not industry composition changes by injury type coinciding with the reforms.

Second, our data contain the administrative injury severity rating for each worker and rich measures of labor productivity. We test for differential changes in these measures and present the results in Table A3; event studies for these measures are presented in Figure A8. In Column 1 of Table A3, we study the disability rating.³⁵ The relationship between the disability rating and the interaction term of interest is not statistically different from zero and is small relative to the scale of the disability measure which ranges between 0 and 100%.³⁶ The estimate of -0.085 is equivalent to 0.006 standard deviations of the outcome variable. In Column 2, we limit the analysis sample to before 2005 since the disability rating system changed in 2005. The estimate remains small, corresponding to a change of 0.005 standard deviations. The event study in Figure A8(a) also shows only modest changes throughout the sample period. These results strongly suggest that the severity of the injuries was not differentially changing in a manner that would confound our analysis.

In Column 3 of Table A3, we replicate our main results but use workers' pre-injury earnings for the 6 quarters before the injury as the outcome variable. While we find a negative relationship, this estimate is not statistically significant and it is small in magnitude relative to the estimated causal effect on post-injury earnings. Column 4 uses the earnings of the control workers as the outcome. Again, the estimate is not statistically significant and small

³⁵The reported estimates for disability ratings are generated by OLS regressions since the outcome variable is not skewed. Poisson regression generates similar conclusions.

³⁶Ratings are only given for permanent injuries. We assign scores of 0 to temporary injuries since the ratings are designed to reflect the severity of permanently disabling conditions and, thus, 0 is an appropriate score for injuries deemed temporary. The Table A3, Column 1 results are robust to dropping the temporary injuries entirely.

relative to the effect estimated for the injured workers. These results suggest that the main results of this paper are not driven by systematic sample composition changes as the underlying labor productivity of the sample appears to remain relatively constant. The event studies for these outcomes are shown in Figures A8(b) and (c). To compare magnitudes more directly to the estimated effects for the post-injury earnings of the injured workers, Figure A9 overlays the event studies for pre-injury earnings and the control worker earnings with the corresponding post-injury earning estimates. There is a clear separation as the reforms are implemented.

While our data contain rich information about injury severity and labor productivity, we do not have more basic demographic information about the injured workers. However, the DEU data contain information on age and job tenure.³⁷ We provide OLS estimates for these two variables in Columns 5 and 6, respectively. Again, we find no statistically significant relationships and the estimates are close to zero. The estimates correspond to a reduction of 0.022 standard deviations (age) and an increase of 0.013 standard deviations (tenure). The event studies (Figures A8(d) and (e)) also suggest that these variables did not change systematically with the reforms.

We will consider the relative prevalence of low back injuries over time in Section 5.4. Overall, we find little evidence that the composition of injuries was changing over time, consistent with recent work which has found that the number of workers' compensation claims is not particularly responsive to policy-driven changes in benefits (Bronchetti and McInerney (2012)), though this prior work studies cash benefits. We further test for selection in the next section.

5.1.2 Robustness Tests—In Table 3, we consider alternative analysis samples and models to further test the sensitivity of our results. In Column 1, we include 2003 as a pre-reform year. Including 2003 as a pre-reform year is a conservative choice and should attenuate our effects. We find smaller estimates for the impact on earnings,³⁸ though we estimate a larger effect for medical expenditures (we cannot reject equality with our previous estimates).

We originally selected shoulder, knee, and hand/wrist injuries in our data as appropriate comparison groups for low back injuries. In Column 2 of Table 3, we present results which also include upper back, neck, “other lower extremity,” and “other upper extremity” injuries as comparison groups. The estimates are generally consistent with the main results of this paper.

In the previous section, we examined possible composition changes. We test for selection effects further here. In Column 3 of Table 3, we select on workers suffering “traumatic”

³⁷Job tenure is defined as tenure at the firm in which the injury occurred in quarters. This variable is topcoded at 12 quarters and available for 2001–2006. While we provide linear estimates here, we have also studied nonlinear relationships which are more robust to topcoding concerns, such as the relationship of Post $\times$ Back with having tenure less than 1 quarter, 2 quarters, and so on. We find no such relationships.

³⁸Our primary concern with including 2003 as a pre-reform year is that utilization review impacted the medical care that these injuries received beginning in April 2004. Note, however, that this potential reduction in medical care would not affect post-injury outcomes in the quarter immediately after the injury (Q1) for any injury incurred in 2003. In fact, we do *not* find attenuated results when we use Q1 earnings as the outcome. When we exclude 2003, the estimate is -0.110 . When we include 2003, the estimate is -0.105 .

injuries. An injury is designated as traumatic if it is traceable to a specific accident incurred at-work and cannot be classified as a cumulative injury. Workers' compensation claiming should be less responsive to the workers' compensation policy environment for those injuries. The estimates in Column 3 are similar to the main estimates of the paper; the earnings result is larger in magnitude.

Our data include each person's administrative disability rating so we can explicitly control for differential changes in injury severity over our sample period. We create 631 indicators for every observed value of the disability rating interacted with year dummies. The medical spending result decreases in magnitude while the earnings estimate remains about the same.

Next, we consider the possibility of firm-level selection since our data do not include self-insured firms. Firms cannot self-insure only certain injury types so self-insurance is unlikely to be correlated with our source of variation. We condition on firm-year interactions and present the estimates in Column 5.³⁹ The estimates are robust to the inclusion of firm-year interactions, suggesting that firm-level selection concerns are not driving the results.⁴⁰

The final column in Table 3 presents estimates of a model in which we explicitly account for selection concerns. We use a "selection instrument" to predict changes in the number of injuries (by industry and injury type) and then separately identify the effect of changes in the number of injuries from the causal effect of the reforms. This procedure is described further in Section A.3. The results presented in Column 6 are similar to the main results. The estimate for post-injury earnings increases in magnitude when we model selection explicitly.

Finally, our assumption is that the reforms differentially affected low back injuries only through changes in medical care generosity. We address other factors associated with workers' compensation in Section A.4. We find no evidence that workers with low back injuries experienced a disproportionate change in replacement rates, legal costs, or job training (see Table A4).

5.1.3 Difference-in-Difference-in-Differences Estimates—While we cannot statistically reject that the earnings of the matched control workers or the pre-injury earnings of the injury workers did not differentially change due to the reforms, we can use this additional information to implement separate triple-differences designs. First, we use the earnings of the control workers, denoting the two groups as "injured" and "control." We estimate specifications which include time-injury type interactions (where the control workers are assigned the same injury type as the injured worker that they are matched to), accounting for changes in the composition of injuries over time. We present the parameter estimate associated with $\text{Post}_t \times \mathbf{1}(\text{Back Injury}_k) \times \mathbf{1}(\text{Injured}_i)$ in Table A5. In Panel A, we estimate that low back injuries incurred an 8.1% to 8.8% earnings reduction (\$2199 to \$2377). Our most flexible specification includes quarter-industry-group interactions, injury type-group interactions, and quarter-industry-injury type interactions. We estimate an 8.1% earnings reduction using this model.

³⁹The reduced sample size results from the exclusion of firm-year combinations with only one injury.

⁴⁰Moreover, the DEU data include self-insured firms, and we estimate an earnings effect of -0.084 (standard error=0.029) for post-injury earnings in these data.

In Panel B, we implement a separate triple-difference design using pre-injury wages to generate the additional difference. The two groups are “pre-injury” and “post-injury,” and we report the estimates for the $\text{Post}_t \times \mathbf{1}(\text{Back Injury}_k) \times \mathbf{1}(\text{Post-Injury}_j)$ term.⁴¹ The estimates are similar to those shown in Panel A.

Finally, as a placebo test, we repeat the triple-differences design in Panel A but use pre-injury earnings for both the injured and control workers. The reforms should not have had any effect on earnings before the injury. We estimate small and statistically insignificant effects for this outcome.

Table A6 provides OLS triple-difference estimates for post-injury earnings. We consider OLS less appropriate here given that the control worker earnings and injured worker earnings have such different baselines. However, we still estimate large and statistically significant reductions for post-injury earnings. When using pre-injury earnings as a placebo test, we estimate small and statistically insignificant increases.

We include event studies in Figure A10. The top two figures provide the estimates for post-injury earnings and show a steady decline in earnings throughout the implementation region. As a placebo test, we implement the same event study specification using pre-injury earnings for both the injured and control workers. The reforms had little effect on this outcomes, as shown in Figures A10(c) and (d). In Figure A11, we suppress the confidence intervals and show only the post-injury earnings estimates and the placebo pre-injury earnings estimates. The lines diverge throughout the implementation period.

5.1.4 Discussion of Main Results—Our results suggest that the reduction in medical care had large and detrimental effects on post-injury outcomes. Prior to the reforms and in the absence of cost-sharing, over-utilization of medical care was likely. Workers value insurance but generous coverage promotes overuse. Consider workers who value consumption (equal to wages and cash benefits) and health (h) and incur injuries exogenously with probability p . If injured, the individual has health $h^i(m)$, which is a function of medical care spending m such that $\frac{\partial h^i(0)}{\partial m} > 0$ and $\frac{\partial^2 h^i(m)}{\partial m^2} \leq 0$, and post-injury earnings are a function of health: $w(h^i(m))$. Upon injury, the worker receives cash benefits b and solves $\max_m U(b + w(h^i(m)), h^i(m))$ since there is no cost-sharing. The worker will consume medical care until $\frac{\partial h^i(m)}{\partial m} = 0$.

Let us now define the optimal amount of medical care as the amount that the worker would commit to *ex-ante* if she were paying the expected cost of the medical care (pm) regardless of injury.⁴² Given exogenous wages (w_0) and health (h^0) in the uninjured state, the worker solves

⁴¹We do not repeat this exercise for the probability of working since all workers in our sample worked before the injury.

⁴²This payment can be considered a reduction in the wage.

$$\max_m (1-p)U[w_0 - pm, h^n] + pU[b + w(h^i(m)) - pm, h^i(m)]. \quad (2)$$

The first order condition requires $\frac{\partial h^i(m)}{\partial m} > 0$,⁴³ implying that the amount of care consumed in the injured state is sub-optimal for the worker. The worker would prefer purchasing less medical care but cannot commit to it. In principle, policy can solve the commitment problem above by restricting workers to consume less care when injured through caps and guidelines. However, it may be difficult to target this care appropriately or select the correct amount of care.

Our primary interest in this paper is understanding $\frac{\partial w(h^i(m))}{\partial h} \frac{\partial h^i(m)}{\partial m}$, which provides evidence about whether policy eliminated marginal or effective care. Small values of $\frac{\partial w(h^i(m))}{\partial h} \frac{\partial h^i(m)}{\partial m}$ are consistent with policy restrictions improving welfare while large values suggest otherwise. Using the dollar amount reductions estimated in Table 2, Column (3) for medical care and Table A5, Column (3) for earnings, we estimate that each additional dollar of medical spending in this context is associated with an additional \$3.73 in earnings, strong evidence that $\frac{\partial w(h^i(m))}{\partial h} \frac{\partial h^i(m)}{\partial m}$ is positive and large. The magnitudes of the estimates suggest that the medical care provided post-reforms is below the optimal level since each dollar of care increases post-injury earnings by more than \$1. In the absence of strong consumption-health complementarities, this tradeoff is sub-optimal.

While our estimated labor effects may seem large, they are plausible when compared to effects found in the medical literature. Zigenfus et al. (2000) finds that small differences in treatment are associated with 60% reductions in time away from work which is much larger than the effects that we estimate. Loisel et al. (1997) estimates injury duration hazard ratios over 2.2 resulting from differences in treatment intensity in a randomized controlled trial, larger than the implied 1.03 that our analysis suggests (injury duration is discussed below). While the experiments found in the medical literature may be hard to compare to our context, they illustrate that the medical evidence suggests significant scope for medical care to affect post-injury labor outcomes.

5.2 Timing of Effect

We study the timing of the effect in an event study framework, using the control workers to implement the triple-differences specification estimated in Section 5.1.3. We do not observe the timing of medical care spending in the WCIRB data, but Swedlow (2005b) reports that even at 3 months, lower back injuries experienced relative reductions in medical utilization

⁴³The first order condition is $pU_c^i \left[\frac{\partial w(h^i(m))}{\partial h} \frac{\partial h^i(m)}{\partial m} \right] + (1-p)U_c^n = U_h^i \frac{\partial h^i(m)}{\partial m}$, implying that $\frac{\partial h^i(m)}{\partial m} = 0$ is not a solution. A

solution exists if $\frac{\partial w(h^i(m))}{\partial h} > 0$ and $\frac{\partial h^i(m)}{\partial m} > 0$ (a solution may exist if both are negative, which we rule out by assumption).

post-reform. The treatment guidelines impacted utilization even in the first post-injury quarter; moreover, restrictions on utilization encouraged people to delay care and consume visits at a slower rate.

We present the estimates graphically in Figure 4a. We have previously defined post-injury earnings as total earnings in the 6 quarter after the injury. We have up to 12 quarters before injury and 16 quarters after injury, depending on the time of injury.⁴⁴ We show a broader set of results with the caveat that prior to 8 quarters before the injury and 6 quarters after the injury, the sample composition is changing.⁴⁵

There are no statistically significant effects of the reforms prior to injury.⁴⁶ There is also little evidence of an “Ashenfelter dip” in the quarter prior to the injury.⁴⁷ We observe a small effect in the quarter in which the injury occurred (partially-treated) and then larger effects starting in the subsequent quarter. The estimates are negative throughout the entire post-injury period and statistically significant for the six quarters post-injury, ranging from -0.056 to -0.113 . There is some evidence that the effect attenuates over time, though we still observe statistically significant estimates 14 quarters after injury.

Figure 4b provides the extensive margin results (estimated using OLS). For the extensive margin, we find that the effect is largest in magnitude in the second quarter after injury and then monotonically decreases in magnitude until quarter 9 post-injury. The estimate is statistically significant from zero for all quarters between 1 and 8 after the injury.⁴⁸

5.3 Distribution of Earnings

Understanding the distributional effects is also informative as policy implications are often different if the effect is isolated to the bottom of the earnings distribution relative to an effect disproportionately influencing the top of the distribution. Moreover, distribution effects provide evidence of mechanisms driving our mean estimates. In this section, we estimate the differential effect of the reforms on the earnings cumulative distribution function (CDF). We estimate a series of linear probability models with outcome equal to $\mathbf{1}$ (Earnings $< s$), where s is the earnings threshold.⁴⁹ We present the results in Figure A12.

At the very bottom of the distribution, the parameter estimate is 0.014 (our extensive margin estimate) and subsequently increases. The estimate peaks at 0.039 for threshold \$15,000, implying that after the reforms, workers with low back injuries had a 3.9 percentage point increase in earning less than \$15,000 (in the 6 quarters post-injury). Such large percentage

⁴⁴Injuries incurred earlier in the sample have shorter pre-periods and longer post-periods; injuries incurred later in the sample have longer pre-periods and shorter post-periods.

⁴⁵We show results up to 14 quarters after injury given that there are relatively small samples in the post-reform period for quarters 15 and 16.

⁴⁶This lack of a relationship is (partially) mechanical in the 4 quarters prior to the injury since we matched the control workers to the injured workers based on earnings during this time period. However, it is reassuring that there were no differences before that period, suggesting that the workers were also on similar earnings trajectories before the injury.

⁴⁷An “Ashenfelter dip” for this specification would be a *differential* forward-looking effect because of the reduced medical care generosity. More generally, we do not find any evidence of changes in labor supply in the quarter before the injury (see Figure A4).

⁴⁸The estimate is mechanically equal to zero in the quarter of the injury since all of the injured workers and control workers were working in that period.

⁴⁹Recent examples of estimating CDFs in this manner include Cabral and Mahoney (2014) and Hoynes and Patel (2015).

point increases cannot be explained by extensive margin effects as the extensive margin effect is less than half the size of the largest parameter estimates shifting the CDF.

We estimate statistically significant effects up to \$102,000, implying important effects on the CDF for large parts of the earnings distribution. Given that we observe effects even at high values of labor earnings and there is little evidence of a sharp discontinuity in the parameter estimates, it is unlikely that our labor outcomes can be explained by the rules governing SSDI applications.⁵⁰ In Figure A13, we present estimates where the outcome is equal to 1 if the worker reached a certain fraction of their pre-injury earnings (or the post-injury earnings of their control workers). The effect sizes peak at around 50–70% of prior earnings.

5.4 Do we observe similar effects in Oregon?

We obtained access to administrative workers' compensation claims from the Oregon Department of Consumer and Business Services, Workers' Compensation Division. These data include information about type of injury and medical care costs for all resolved disabling claims.⁵¹ We linked these data to quarterly earnings records and constructed post-injury earnings, measured as 6 quarters relative to the time of injury.

Oregon did not have any major workers' compensation medical care reforms over this time period so we can test whether we observe similar differential patterns in an untreated state.⁵² We first study trends in the injury types for both California and Oregon over time in an event study framework. The outcome is the number of injuries for a given industry, quarter, and injury type. We condition on industry-quarter and quarter-injury type interactions. These trends are presented in the Figure A14. The rate of claims for low back injuries decreases steadily in California before the reforms. We observe the same trend in Oregon during this time period.⁵³ In general, the trends are similar over the full sample period. The fraction of low back injuries in California does increase in the latter part of 2004 with some evidence of a similar increase in Oregon. Our concern about injury composition changes is that the reduction in medical care generosity would lead to workers suffering low back injuries to be *less* likely to claim workers' compensation (in a manner that is systematically related to earnings). The delayed response of the relative increase in low back injuries would be consistent with workers learning about the new reforms over time but, again, we would expect a disproportionate reduction in low back claims. Instead, we observe an increase. Moreover, note that the dramatic decrease in earnings shown in Figures 2 and 3 pre-dates this composition shift, which is further evidence that it is not driving the results.

⁵⁰A similar analysis using only earnings in the first quarter post-injury confirms this conclusion and offers a more precise test since we are more confident that the SGA threshold is binding during this time period. Using only the first quarter post-injury, the estimates are larger above the SGA threshold (which we mark at \$2,580) all the way up to \$9,600, suggesting substantial movements in earnings that cannot be explained by SSDI.

⁵¹Rennane (2015) studies the distortionary effects of cash benefits in Oregon workers' compensation system analyzing these same data.

⁵²There was a change in the award structure for permanent partial disability claims in 2005 (due to SB 757), but there is no evidence that this disproportionately affected workers with back injuries.

⁵³We suspect that these trends reflect changes in the incidence of back injuries. Using data from the Bureau of Labor Statistics' Survey of Occupational Injuries and Illnesses (SOII), we calculate that the fraction of injuries resulting in at least one missed day of work which were back injuries in California decreased between 2000 and 2006. The SOII incidence rates are independent of workers' compensation claiming behavior so this is consistent with a broader change in the rate of back injuries.

Finally, we replicate our main analyses using the Oregon data and present the results in Table A7. The first column includes the analysis of the Oregon data for our three main outcomes. The point estimates are small and not statistically distinguishable from zero across all outcomes. We also include triple-difference estimates using pre-injury earnings (equivalent to Panel C of Table A5). The estimated relationship with earnings is equal to -0.002 , compared to -0.072 for California. These results help rule out that our findings are driven by trends occurring outside of the workers' compensation system (such as trends driven by SSDI enrollment) or secular changes in the wage penalty for back injuries.

5.5 Mechanisms

5.5.1 Intensive vs. Extensive Margin Effects—While we find evidence of effects on return-to-work, these estimates alone do not provide information about whether these “extensive margin” effects drive the total earnings reduction, though the CDF estimates in Section 5.3 suggest that they do not. 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 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. Instead, we pursue a decomposition of the main effect into the extensive margin and the intensive margin. Since permanent disability payments are paid regardless of work status, there is significant scope for intensive margin effects in this context relative to social insurance programs which only pay people who not working or tax earnings at a high rate.

Using the pre-reform (2000–2002) data only, we calculate the mean post-injury earnings for injured workers that return to work at some point in the first 18 months pre-injury for each industry-injury type. We substitute these mean earnings (by industry-injury type) for everyone in the sample that returns to work (0 otherwise). This counterfactual earnings variable eliminates intensive margin responses and varies (conditional on industry-injury type) only due to whether the individual works or not within the first 6 quarters post-injury. Using this variable as our outcome, we re-estimate our specification and present the results in Column 2 of Table 4 (Column 1 repeats our main effect). The estimate implies that the extensive margin explains a 1.6% earnings reduction, about 16% of the total effect.

Next, we pursue a similar exercise but focus on injury duration (see Figure A15 for injury duration results⁵⁴). Using the pre-reform (2000–2002) data only, we calculate the mean post-injury earnings by quarter relative to injury for injured workers who worked during that quarter for each industry-injury type combination. Each person is assigned the mean earnings for that quarter if they worked in that quarter, zero otherwise. The outcome variable is the sum of predicted earnings over all 6 post-injury quarters. This variable varies (conditionally) due only to different durations of not working, eliminating within-quarter variation in earnings (conditional on working in that quarter). We re-estimate our main specification with this outcome variable and present the result in Column 3. We estimate an

⁵⁴Note that injury duration is a different measure than working in a quarter.

effect of -0.042 , which is about 41% of the overall estimated effect. This estimate, however, includes some extensive margin information because workers may vary in terms of when they begin working within a quarter. For Column 4, we set all earnings in an individual's first quarter of working to 0, shutting down variation because of differences in within-quarter timing.⁵⁵ The estimate rises again, but we are still finding that the extensive margin explains slightly less than 50% of the overall effect.

5.5.2 Changing Firms—A potentially important dimension of the intensive margin is workers switching jobs, possibly to less intensive (and lower-paying) jobs. In our data, we observe total wages earned at the firm in which the injury was incurred and total wages earned at all other firms. These outcomes are not mutually exclusive. We estimate our model using earnings at the same firm and, separately, earnings at other firms. The results are presented in Panel A of Table 5. We find that the “own firm” effect dominates, implying that the labor productivity loss is greater when the measure is confined to the firm in which the injury occurred. In fact, despite an estimated relative decrease in the overall probability of working, we find evidence that workers with low back injuries experienced a relative *increase* in the probability of working at a different firm. The earnings effect when limited to wages earned at other firms is negative but smaller than the overall effect, consistent with the increased likelihood to change firms as well as the overall negative effect on earnings. These results are consistent with additional prevalence of health limitations which require (or at least make it more desirable for) workers to switch jobs.

We study the propensity to change firms in more detail in Panel B. We estimate a large negative effect on the probability of working at the same firm in which the injury occurred but not working anywhere else. The other three options are estimated to have increased in probability. The largest increase is for the probability of not working anywhere (our extensive margin effect, reported earlier) which we estimate as increasing by 1.4 percentage points, equivalent to a 14% increase from baseline. We also estimate a statistically significant positive effect on the probability of switching firms completely (positive earnings elsewhere but no earnings at original firm), equivalent to a 10% increase over baseline. This firm switching is consistent with the prior finding of large intensive margin effects.

We find evidence that workers delay their return to the labor force but also that they transition to different firms as a result of the reduced medical care generosity. While these measures do not capture within-firm job changes which result in earnings reductions, the results are consistent with workers experiencing reductions in labor productivity and transitioning to lower-paying jobs.

5.6 Understanding the Change in Medical Care

5.6.1 Prices vs. Utilization—Our focus has been on total medical expenditures but, given the details of the reforms, the differential effect of the policies on medical costs for low back injuries primarily represents a utilization effect. The ACOEM guidelines and the utilization caps targeted care disproportionately used by workers with low back injuries. Research

⁵⁵Also, when calculating the mean of pre-reform earnings by quarter, we only use individuals who worked in a previous quarter.

using the Industry Claims Information System (ICIS) reports utilization rates for several categories.⁵⁶ Using these reported data, we estimate a differential change in the log of spending equal to -0.051 . This estimate is smaller in magnitude than the Table 2 estimate, but it is reassuring that the two data sources are generating similar results. When we estimate the differential change in the log of utilization with the ICIS data, we arrive at an estimate of -0.067 . The differential spending and utilization effects are relatively similar and, in fact, we find that the utilization effect is *larger* in magnitude than the spending effect, suggesting that any price changes were small and worked in the opposing direction.⁵⁷ We conclude that our medical spending estimates are driven by utilization changes, consistent with the nature of the reforms.

5.6.2 Substitution to Private Insurance—The relationship of the workers' compensation system with private health insurance was discussed in Section 2.3. In Section A.5, we test whether the reforms led to increased private health insurance claims, using administrative claims data from the Truven MarketScan Commercial Claims & Encounter Database for 2002–2006. We find no evidence of substitution (see Figure A16). It may be surprising that private claims do not respond to restrictions implemented in the workers' compensation system, especially if the care promotes workers' abilities to increase their own earnings. However, workplace injuries represent large income shocks and liquidity constraints may prevent individuals from seeking their own care. Other factors, such as workers trusting the treatment guidelines enacted by the reforms and not seeking additional care, are also possible.

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 7.6% more than spending on other injuries. We also find that earnings associated with low back injuries decreased by 8.1% relative to other injuries, using our triple-differences estimates. 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. We find complementary evidence that workers with low back injuries were more likely to switch firms after the reforms.

To understand the cost-savings associated with the reforms, we created a metric of the total costs associated with each injury including medical spending, legal costs, indemnity benefits, and vocational rehabilitation costs. This variable permits us to study the differential cost-savings of the reforms for a given injury. While medical care spending decreased according to our estimates, we also estimate increases in injury duration which, for temporary injuries, usually implies a longer duration of cash benefits. Estimating equation (1), we find that the total cost of a low back injury was reduced by 4.5% (standard

⁵⁶Evaluation & 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 low back injuries in Swedlow (2005b). Using the data for all injuries and the relative frequency of the injuries, we are able to calculate the utilization for non-back injuries.

⁵⁷The difference of 0.016 is equivalent to the standard error for our medical expenditure estimate.

error=1.6%) after the reforms relative to other injuries, but at substantial cost to the injured workers.

Medical care coverage in workers' compensation protects workers from the full health shock associated with a workplace injury while also, based on our findings, improving post-injury labor outcomes and providing consumption smoothing benefits beyond the cash benefits provided by the program. Reforms such as those implemented in California have the potential to improve welfare by committing workers to consume less care upon injury. The concern, however, is that policy may not implement such restrictions effectively. We find that the reductions in medical care in California substantially reduced post-injury earnings. Each dollar saved resulted in well over a dollar lost in post-injury earnings. Unless there are strong consumption-health complementarities such that workers would be willing to give up large amounts of money in injured states for small income increases in healthy states, the medical care restrictions were too restrictive.

These results are consistent with findings in other social insurance settings that more generous insurance enables personal investments which enhance future productivity. There is evidence of social insurance investments improving long-term labor productivity in the case of Medicaid coverage for children (Goodman-Bacon (2016)) as well as in the unemployment insurance context by improving job match quality (e.g., Nekoei and Weber (2017)).⁵⁸ The returns to such investments are important components of any optimal social insurance calculation. These returns potentially represent long-term and large streams of additional labor productivity, and are distinct from short-term considerations of distorting behavior (often reducing labor productivity) versus consumption smoothing. While the returns to such investments should vary across different settings, we present evidence of substantial scope for post-injury labor productivity improvements in the context of workers' compensation medical care generosity.

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⁵⁸More generous unemployment insurance benefits may also increase unemployment duration, leading to decreased wages, and there is empirical evidence of this relationship as well (e.g., Schmieder et al. (2016)).

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A Appendix: For Online Publication Only

A.1 Linking WCIRB and Earnings Data

The WCIRB and earnings data are linked by Social Security Number (SSN). Earnings information was collected from the first quarter of 1998 to the second quarter of 2009. Data are not available for all workers in each quarter, and we cannot discern whether a worker did not work in that quarter, left the state, or was self-employed.

We observed multiple records for some individuals in the quarterly wage files. Multiple records may occur because of name changes, variations in how employers submit name (e.g., submitting “Jon” instead of “Jonathan”), typographical errors, and illegal uses of SSNs. To account for these concerns and capture all wages for an individual within a quarter, we applied the Soundex phonetic filing system to the first and last name and kept records that had only one last name across all employers. To keep individuals with legitimate name changes, we also kept records with multiple last names but only one first name. Due to these selection criteria, approximately 19% of injured workers were not matched to wages.

A.2 Matching Injured to Uninjured Workers

For each injured worker, we selected up to five workers employed at the same firm at the time of injury who had earnings close to the injured worker's over the year prior to injury. 6.6% of our sample only has one match, 5.1% has two matches, 4.0% has three matches, 3.4% has four matches, and 81.0% has five matches. Our results are robust to using only workers with five matches. We define “close” as being within a band equal to the wage of the injured worker plus or minus 15 percent of the log standard deviation of the earnings of all injured workers. The comparison workers were also required to have similar tenure, where tenure is measured using three levels: less than or equal to one year on the job, one to two years, or more than two years. If more than five comparison workers met the matching criteria, we selected the five workers with the lowest absolute value of the difference in wages from the injured workers (ties were decided randomly). We sampled with replacement, meaning that the same comparison workers could be matched to more than one injured worker. If a worker were injured in a later year, however, that worker was not allowed to be a comparison worker. More information about the matching can be found in Seabury et al. (2011).

The selection of the uninjured “control” workers was completed in a secure data facility. We were only permitted to leave the data facility with information about the average earnings of the control workers (by quarter) and the labor force participation rates of the control workers (by quarter). More complicated matching procedures would have been difficult to implement given the available resources and would not have added much value to our analysis. We

show in the paper that, on average, these control workers have similar earnings to the injured workers before the injury (see Figures A4 and A5), which was our primary goal for this part of the data collection.

A.3 Sample Selection Adjustment Details

We model sample selection more explicitly by including a sample selection adjustment in our main specification. If selection is important, we should observe: (1) a differential change in the share of injuries which are back injuries; (2) outcome changes should be associated with these composition changes (i.e., selection is only potentially problematic if it is systematic). Our selection instrument is motivated by the testable hypothesis that industries with a higher fraction of severe injuries should experience a relative increase in the number of reported injuries post-reforms (because fewer injuries are reported in other industries). We find that our instrument significantly predicts the number of injuries in each period, ruling out alternative hypotheses about reporting behavior which would suggest that we should not observe this relationship. The selection instrument (h_{jkt}) is a measure of initial severity at the industry level interacted with the post indicator:⁵⁹

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

The industry-injury type interactions in our main specification account non-parametrically for the independent effects of the pre-reform injury severity measure. We should observe a differential change in the number of injuries reported post-reform based on the initial severity distribution. We model the log of the number of reported injuries ($\ln R_{jkqt}$) as a function of our exogenous variables and h_{jkt} , which is excluded from equation (1).⁶⁰

Intuitively, we are estimating the changes in the number of reported injuries for each industry-injury type cell. 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 $\text{Post}_t \times \mathbf{1}(\text{Back Injury}_k)$. More generally, our approach is to find groups/cells that should experience (predictably) smaller compositional changes and use this variation to separately account for sample selection. Given the possibility that any social insurance policy change may impact sample composition, this

⁵⁹“Selection” may not occur simultaneously with the effect of the reforms. For example, workers may not change reporting behavior at first but learn through co-workers about changes to the system. Then, there could be a delay in the changes to reporting injuries. This is not problematic for our strategy since we are only concerned with the extent to which selection is correlated with our variable of interest. If selection occurs with a lag, this impacts the relationship between our selection adjustment and the outcomes, but we are interested exactly in how the selection adjustment term is correlated *on average* with outcomes after the reforms, regardless of whether the relationship changes over the post-reform period.

⁶⁰We 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 reported injuries for a given industry and injury type regardless of the source.

general approach is a straightforward way to address selection concerns in many public finance contexts.

In the first step of our approach, we estimate the relationship between our covariates, $\text{Post}_t \times \mathbf{1}$ (Back Injury_k), and h_{jkt} on the log of the number of reported injuries. Using notation similar to Newey (2009), we model selection as

$$\ln R_{jkqt} = W'_{jkqt} \delta + \varepsilon_{jkqt}$$

where W includes all exogenous variables in equation (1) and the selection instrument, h_{jkt} . We test that our selection instrument predicts the number of reported injuries and find that it does. 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. This term is allowed to have an independent effect on the outcomes, estimating the relationship between changes in the number of reported injuries and the outcomes.

In the second step, we estimate our main specification (equation (1)) while including the predicted log of the number of reported injuries ($W'_{jkqt} \hat{\delta}$) as a control. This variable is allowed to have its own effect on the outcomes, separately accounting for changes (since we control for industry-injury type interactions) in the number of reported injuries over time. We report clustered bootstrap estimates of the 95% confidence intervals to account for the fact that the sample selection adjustment is estimated.

This approach is similar to a selection model which relates the probability of being in the sample to the outcome variable. Note that this approach assumes a constant treatment effect across all industries since the selection adjustment enters additively.

A.4 Non-Medical 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 other changes to the workers' compensation program, motivating our approach to compare outcome changes for lower back injuries to other injury types to account for the other aspects of the reforms. We test our assumption here and re-estimate equation (1) using other outcomes that may have been affected by the reforms. We start with replacement rates which were affected by changes in the formula for indemnity benefits. We calculate replacement rates for each worker in our data and then estimate equation (1) with the replacement rate as the outcome variable. Table A4 presents these results in Column (1). There is no evidence that lower back injuries were differentially affected. The point estimates 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.⁶¹

We are also concerned that the reforms altered the incentives to contest judgments or make settlements. Many workers do not return to work until their dispute is formally resolved (Hyatt (2010)) so differential incentives to litigate claims could impact labor supply behavior. If disputes became more common among workers with low back injuries after the reforms, then we should observe a relative rise in defense costs for these injuries. We study the differential effect of the reforms on total defense costs associated with each claim. Again, we find no evidence that these factors can explain our results, shown in Column (2).

The reforms affected generosity of vocational rehabilitation vouchers.⁶² In Column (3), we find little evidence of a relationship with voucher use. Overall, we conclude that workers with lower back injuries did not benefit more or less than workers with other types of injuries due to the reforms on dimensions other than medical care.

A.5 Substitution to Private Insurance

To test for substitution between workers' compensation medical care coverage and private insurance claims in our context, we use administrative claims data from the Truven MarketScan Commercial Claims & Encounter Database for 2002–2006, testing for differential effects beginning in 2004. We select on occupational therapy, physical therapy, and spinal manipulation and adjustment.⁶³ We use the diagnoses code to categorize each claim by injury type: low back, shoulder, knee, and hand/wrist. MarketScan includes ICD-9 diagnosis codes which permits us to categorize claims into one of four categories: low back, knee, shoulder, and hand/wrist. We searched the literature for ICD-9 codes used by workers' compensation system to appropriately code injuries and medical care. We found several sources and categorized the injuries using the lists included in these papers (Wang et al. (2008); Yang and Fomenko (2012); Silverstein and Adams (2007)).

We select on individuals ages 18–60. In Figure A16, we graph the trends in number of claims for low back injuries as well as for all other injury types. We do this for the full sample (N=5,927,475) and for a balanced sample of people in the data for the entire time period (N=1,646,500). For the full sample, we observe a small relative decrease in the number of back claims in 2004 and 2005, though the trends for the two groups converge in 2006. When we study the balanced sample, the trends are relatively similar to each other. There are more back claims relative to other claims in 2003, which is unlikely to result from the reforms. While we consider injuries incurred in 2003 as partially-treated in our main analysis, this is because those injuries are later (potentially) affected by the reforms. Here, we are studying claims at time of care (not time of injury) so 2003 is an untreated year. Overall, we find little evidence that private claims are increasing in response to the reforms. Regression analysis supports this conclusion as all estimates are negative, small, and

⁶¹Alternatively, we could separately identify the effect of changes in the replacement rate from the differential impact of the medical reforms using a simulated instrument strategy (predicting the replacement rate by industry, injury type, and time by applying each period's benefit schedule to a fixed sample) in the spirit of Currie and Gruber (1996). Our results are similar when we use this approach. Our point estimates are -0.072 and -0.098 for medical expenditures and post-injury earnings, respectively.

⁶²There is evidence that these vouchers had little impact on returning to work (Seabury et al. (2011)).

⁶³The results of this analysis are not meaningfully changed by examining only one of these categories at a time or by using a broader utilization measure not limited by service types.

statistically insignificant. Results are similar if we use expenditures instead of number of claims.

Note that we only observe privately-insured claims so we miss utilization from the uninsured. However, the uninsured should be even less likely to pay for their own treatment since they must pay full price for medical services.

B Appendix Figures and Tables: For Online Publication Only

Figure A1.

Relative Trends in Medical Expenditures and Labor Earnings

Calculations made using 2000–2006 WCIRB data (N=189,253). The ratios are defined as the mean for low back injuries relative to the mean for other injuries. Other injuries are shoulder, knee, hand/wrist injuries.

Figure A2.**Relative Trends in Medical Expenditures and Working Status**

Calculations made using 2000–2006 WCIRB data (N=189,253). The ratios are defined as the mean for low back injuries relative to the mean for other injuries. Other injuries are shoulder, knee, hand/wrist injuries.

Figure A3.**Empirical CDF by Injury Type: 2000–2002**

Calculations made using 2000–2002 WCIRB data. We construct the empirical cumulative distribution functions for low back injuries and other injury types up to \$200,000 in post-injury earnings. Earnings are defined for the six quarters after injury.

Figure A4.**Earnings Trajectory of Injured Workers**

Calculations made using 2000–2002 (Pre-Reform) and 2004–2006 (Post-Reform) WCIRB data (158,065 injured workers and 158,065 sets of matched control workers). Earnings expressed as earnings in quarter divided by earnings in quarter of matched control workers. A ratio of 1 means that the injured worker and control workers have equal post-injury earnings.

Figure A5.**Employment Trajectory of Injured Workers**

Calculations made using 2000–2002 (Pre-Reform) and 2004–2006 (Post-Reform) WCIRB data (158,065 injured workers and 158,065 sets of matched control workers). Fraction of individuals with positive earnings in quarter divided by fraction of individuals with positive earnings of matched control workers.

Figure A6.**Event Studies: OLS Estimates**

Each figure represents estimates generated from an event study specification using 2000–2006 WCIRB data ($N=189,253$). Medical expenditures and post-injury outcomes defined for six quarters post-injury. $1(\text{Work})$ is equal to 1 for workers with positive post-injury earnings. All specifications include quarter fixed effects and industry-year and industry-injury type interactions, estimated using OLS. The estimates for quarter 3 of 2002 are normalized to zero. Each estimate represents the conditional relationship between low back injuries and the outcome in that quarter. The shaded region represents the “implementation region” in which injuries become increasingly exposed to the medical care reforms. 95% confidence intervals presented, adjusted for clustering at industry-level.

Figure A7.
Industry Composition Changes

We present the estimate on $\text{Post} \times \text{Back}$ using 2000–2002 and 2004–2006 WCIRB data ($N=158,065$). Quarter and injury type fixed effects are included. The outcome is equal to one for the industry listed on the x-axis. None of the estimates are statistically significant from zero (at the 10% level). 95% confidence intervals presented are adjusted for clustering at the industry-level.

Figure A8.**Event Studies: Balance Tests**

Each figure represents estimates generated from an event study specification. The first two rows use 2000–2006 WCIRB data (N=189,253). Disability Rating is the administrative impairment level between 0 and 100 as determined by a qualified medical evaluator. Pre-injury earnings are the total earnings in the six quarters prior to the injury. Control Wages are the average earnings of matched control workers in the six quarters after the injured worker's injury. Tenure is the total number of years worked at the firm in which injury occurred. The age event study uses DEU data for 2000–2006 (N=115,337); the tenure event study uses DEU data for 2001–2006 (N=93,243) since tenure is not observed before 2001 in the data. All specifications include quarter fixed effects and industry-year and industry-injury type interactions. Figures (a), (d), and (e) are estimated using OLS; Figures (b) and (c) are estimated using Poisson. 95% confidence intervals presented, adjusted for clustering at industry-level. The estimates for quarter 3 of 2002 are normalized to zero. Each estimate represents the conditional relationship between low back injuries and the outcome in that quarter. The shaded region represents the “implementation region” in which injuries become increasingly exposed to the medical care reforms.

Figure A9.**Comparing Event Study Earnings Results**

Both figures include the event study estimates for post-injury earnings already shown in Figure 3b. Post-injury earnings are the total earnings earned by the injured worker in the six quarters after the injury. Pre-injury earnings are the total earnings earned by the injured worker in the six quarters prior to the injury. Control Wages are the average earnings of matched control workers in the six quarters after the injured worker's injury. The left figure repeats Figure A8(b); the right figure repeats Figure A8(c). Each figure represents estimates generated from event study specifications using 2000–2006 WCIRB data (N=189,253). All specifications include quarter fixed effects and industry-year and industry-injury type interactions and are estimated using Poisson regression.

Figure A10.

Triple Difference Event Studies (Injured and Control Workers)

Each figure represents estimates generated from a triple difference event study specification using 2000–2006 WCIRB data (N=378,506). Post-injury earnings are the total earnings earned by the injured worker (or the control workers) in the six quarters after the injury. Pre-injury earnings are the total earnings earned by the injured worker in the six quarters prior to the injury. All specifications include industry-quarter-group, injury type-group, and quarter-industry-injury type interactions where “group” is either injured or control workers. Estimates are normalized to 0 in 2002, quarter 3. Each estimate represents the conditional relationship between low back injuries for the injured workers and the outcome in that quarter. 95% confidence intervals presented, adjusted for clustering at industry-level.

Figure A11.

Comparing Triple Difference Event Study Earnings Results

Each figure represents estimates generated from a triple difference event study specification using 2000–2006 WCIRB data (N=378,506). Post-injury earnings are the total earnings earned by the injured worker (or the matched control workers) in the six quarters after the injury. Pre-injury earnings are the total earnings in the six quarters prior to the injury. All specifications include industry-quarter-group, injury type-group, and quarter-industry-injury type interactions where “group” is either injured or control workers. The figure on the left includes estimates already shown in Figures A10(a) and (c). The figure on the right includes estimates shown in Figures A10(b) and (d). Confidence intervals suppressed.

Figure A12.

Estimates of Effect on Cumulative Distribution Function

We present the estimate on $\text{Post} \times \text{Back}$ in a linear probability model using 2000–2002 and 2004–2006 WCIRB data ($N=158,065$). Industry-injury type interaction, quarter of injury fixed effects, and industry-year fixed effects are included the model. The outcome is $\mathbf{1}(\text{Earnings} < s)$ where s is the value on the x-axis. Positive estimates imply an increased likelihood of earning less than s . 95% confidence intervals are adjusted for clustering at the industry-level.

Figure A13.

Differential Effect on Reaching Fixed Percentage of Prior Earnings or Control Workers' Earnings

Each point represents the coefficient on $\text{Post} \times \text{Back}$ for outcome equal to $\mathbf{1}(\text{Post-Injury Labor Earnings} > s \times \text{Pre-Injury Labor Earnings})$, where s is defined by the number on the x-axis, from a linear probability model using 2000–2002 and 2004–2006 WCIRB data ($N=158,065$). The right hand figure uses each individual's control workers' post-injury

earnings. A negative coefficient implies that workers with low back injuries had a decreased chance of reaching that fraction of their prior earnings (or the earnings of their control workers). All regressions condition on industry-injury type indicators, quarter indicators, and industry-year interactions. Confidence intervals are adjusted for within-industry clustering.

Figure A14.

Event Studies: Number of Injuries

The outcome is the number of claims for a cell defined by quarter of injury, industry, and injury type (Number of cells=2800). Each event study specification includes interactions for each quarter-industry and industry-injury type. We graph the estimates associated with 1(Back) interacted with each quarter of injury indicator. 95% confidence intervals are adjusted for clustering at the industry level. The left figure uses 2000–2006 WCIRB data; the right figure uses 2000–2006 administrative workers' compensation claims from the Oregon Department of Consumer and Business Services, Workers' Compensation Division. Estimates are normalized to 0 in 2002, quarter 3. Each estimate represents the conditional relationship between low back injuries and the number of injuries in that quarter.

Figure A15.**Differential Effects on Injury Duration**

Using 2000–2002 and 2004–2004 WCIRB data, a linear probability model is estimated in which the outcome is equal to 1 if the worker has positive earnings up to that point in time since the injury. The model includes quarter of injury fixed effects, industry-injury type interactions, and industry-year interactions. We only have data for the full sample for 6 quarters post-injury ($N=158,065$). For the subsequent quarters, we have longer post-injury periods in the earlier years. Thus, the sample composition is changing after 6 quarters. Confidence intervals are adjusted for clustering at the industry level.

Figure A16.**Private Medical Claims**

Using Truven MarketScan Commercial Claims & Encounter Database for 2002–2006, we categorize claims using diagnosis codes by body part and by type of service, limiting claims to occupational therapy, physical therapy, and chiropractic care. The sample composition is the same for the two categories. Each time series is normalized to 100 in 2002. On the left, we use the full sample (N=5,927,475). On the right, we perform the same analysis but only use individuals in each year of the sample (N=1,646,500).

Table A1

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 with Post-Injury Earnings	Pre-Injury Earnings	0.24	0.43	0.30	0.46
Fraction with Post-Injury Earnings	0.75 × Pre-Injury Earnings	0.39	0.49	0.50	0.50
Fraction with Post-Injury Earnings	0.5 × Pre-Injury Earnings	0.50	0.50	0.63	0.48
Fraction with Post-Injury Earnings	0.25 × Pre-Injury Earnings	0.61	0.49	0.74	0.44
Fraction with Post-Injury Earnings	Control Earnings	0.23	0.42	0.30	0.46
Fraction with Post-Injury Earnings	0.75 × Control Earnings	0.37	0.48	0.49	0.50
Fraction with Post-Injury Earnings	0.5 × Control Earnings	0.48	0.50	0.49	0.49
Fraction with Post-Injury Earnings	0.25 × Control Earnings	0.60	0.49	0.44	0.44
Fraction of Injuries that are Permanent		0.62	0.48	0.65	0.48
Severity Rating		19.9	14.5	16.3	13.0
N		50,342		107,723	

Notes: WCIRB data for 2000–2002, 2004–2006 injuries. “Other Injuries” are shoulder, knee, and hand/wrist injuries. All dollar values reported in 2006 dollars. “Control Earnings” refer to the earnings of the control workers. Severity ratings are conditional on being a permanent injury.

Table A2

Relationship Between Medical Expenditures and Post-Injury Earnings

	(1)	(2)	(3)	(4)
Outcome:	Post-Injury Earnings (Mean=\$33,793)			
ln(Medical Expenditures)	−0.030*** (0.002)	−0.029*** (0.002)	−0.029*** (0.002)	−0.012*** (0.002)
Fixed Effects:	Year	Quarter	Quarter	Quarter

(1)	(2)	(3)	(4)
Injury Type	Injury Type	Industry x Injury Type	Industry x Injury Type
		Industry x Year	Industry x Year
			Severity Rating x Year

Notes: WCIRB data for 2000–2002, 2004–2006 injuries: N=158,065.

Significance 1%,
**
Significance 5%,
*
Significance 10%.

Standard errors in parentheses adjusted for clustering at industry level. Post-injury earnings are defined as total earnings in the six quarters post-injury; medical expenditures are total medical care costs in six quarters post-injury. The “Fixed Effects” row lists all the fixed effects included in the column’s model. “Severity Rating” refers to the official measure of impairment assigned to injured workers.

Table A3

Balance Tests

	(1)	(2)	(3)
Outcome:	Disability Rating	Disability Rating (2000–2004)	Pre-Injury Wages
Post x Back Injury	–0.085 (0.266)	0.065 (0.355)	–0.029 (0.026)
Mean	11.037	11.949	47,256.44
	(4)	(5)	(6)
Outcome:	Control Wages	Age	Tenure
Post x Back Injury	–0.029 (0.025)	–0.249 (0.170)	0.055 (0.096)
Mean	47,886.75	43.62	8.36

Notes: WCIRB data for 2000–2002, 2004–2006 injuries: N=158,065.

Significance 1%,
**
Significance 5%,
*
Significance 10%.

Standard errors in parentheses adjusted for clustering at industry level. Disability Rating is the administrative impairment level between 0 and 100 as determined by a qualified medical evaluator. We assign temporary injuries a disability rating of 0 (see footnote 36). Pre-injury earnings are the total earnings in the six quarters prior to the injury. Control Wages are the average earnings of matched control workers in the six quarters after the injured worker’s injury. Tenure is the total number of years worked at the firm in which injury occurred. Other variables included in each model: industry-injury type interaction, industry-year interactions, quarter fixed effects. Columns (1), (2), (5), and (6) are estimated using OLS; Columns (3) and (4) estimated using Poisson regression. Column (2) limits analysis sample to years 2000, 2001, 2002, and 2004. Columns (5) and (6) use 2000–2002, 2004–2006 DEU data (N=97,611). The tenure variable is only available beginning in 2001 (N=76,196).

Table A4

Other Workers’ Compensation Benefits

	(1)	(2)	(3)
Dependent Variable:	Replacement Rate	Defense Costs	1(Vocational Rehabilitation)
Post x Back Injury	–0.036	–0.005	–0.001

	(1)	(2)	(3)
Dependent Variable:	Replacement Rate	Defense Costs	1(Vocational Rehabilitation)
	(0.075)	(0.042)	(0.007)
Mean	1.198	969.145	0.262

Estimates generated using 2000–2002 and 2004–2006 WCIRB data (N=158,065).

Significance 1%,
**
Significance 5%,
*
Significance 10%.

Standard errors in parentheses adjusted for clustering at industry level. The replacement rate is the ratio of weekly indemnity benefits to average pre-injury weekly earnings. Defense costs are the total legal costs associated with the claim. 1(Vocational Rehabilitation) is equal to 1 for injured workers who received a job training voucher. Other variables included but not shown: quarter fixed effects, industry-year interactions, industry-injury type interactions. Poisson used for replacement rate and defense costs. OLS used for vocational rehabilitation. Note that the replacement rate is highly-skewed because of workers with low earnings subject to the minimum level of benefits. The median replacement rate is 0.461.

Table A5

Triple Difference Estimates

	(1)	(2)	(3)
A. Post-Injury Wages and Control Wages (Mean=\$40,904)			
Post-Reform x Back x Injured	−0.088 *** (0.021)	−0.088 *** (0.022)	−0.081 *** (0.019)
Effect in Dollars:	−2377.32	−2391.41	−2198.86
Fixed Effects:	Quarter x Group Injury Type x Group Quarter x Injury Type	Quarter x Group Injury Type x Group Year x Industry x Injury Type	Quarter x Industry x Group Injury Type x Group Quarter x Industry x Injury Type
B. Post-Injury Wages and Pre-Injury Wages (Mean=\$40,570)			
Post-Reform x Back x Post-Injury	−0.083 *** (0.023)	−0.084 *** (0.023)	−0.072 *** (0.020)
Effect in Dollars:	−2259.49	−2272.66	−1944.92
Fixed Effects:	Quarter x Group Injury Type x Group Quarter x Injury Type	Quarter x Group Injury Type x Group Year x Industry x Injury Type	Quarter x Industry x Group Injury Type x Group Quarter x Industry x Injury Type
C. Placebo Test: Pre-Injury Wages and Pre-Injury Control Wages (Mean=\$46,775)			
Post-Reform x Back x Injured	0.006 (0.008)	0.006 (0.008)	−0.001 (0.003)
Effect in Dollars:	239.62	244.89	−32.37
Fixed Effects:	Quarter x Group Injury Type x Group	Quarter x Group Injury Type x Group	Quarter x Industry x Group Injury Type x Group

(1)	(2)	(3)
A. Post-Injury Wages and Control Wages (Mean=\$40,904)		
Quarter x Injury Type	Year x Industry x Injury Type	Quarter x Industry x Injury Type

Notes: 2000–2002 and 2004–2006 WCIRB data (N=316,130).

*** Significance 1%,

** Significance 5%,

* Significance 10%.

Standard errors in parentheses adjusted for clustering at industry level. Post-injury Wages and Control Wages are total earnings in the first six quarters post-injury. Pre-Injury Wages are total earnings in the six quarters immediately prior to injury. We include results using Pre-Injury Wages for both the injured and control workers as a placebo test. “Group” refers to injured or control in Panels A and C. “Group” refers to pre-injury or post-injury in Panel B. Poisson used for all regressions.

Table A6

OLS Triple Difference Estimates

	(1)	(2)	(3)
	Post-Injury Wages and Control Wages (Mean=\$40,904)		
Post x Back x Injured	–1550.69** (663.77)	–1550.69** (664.21)	–1556.19** (706.86)
Fixed Effects:	Quarter x Group Injury Type x Group Quarter x Injury Type	Quarter x Group Injury Type x Group Year x Industry x Injury Type	Quarter x Industry x Group Injury Type x Group Quarter x Industry x Injury Type
	Placebo Test: Pre-Injury Wages and Pre-Injury Control Wages (Mean=\$46,775)		
Post x Back x Injured	246.94 (405.22)	246.94 (405.48)	73.82 (285.94)
Fixed Effects:	Quarter x Group Injury Type x Group Quarter x Injury Type	Quarter x Group Injury Type x Group Year x Industry x Injury Type	Quarter x Industry x Group Injury Type x Group Quarter x Industry x Injury Type

Notes: 2000–2002 and 2004–2006 WCIRB data (N=316,130).

*** Significance 1%,

** Significance 5%,

* Significance 10%.

Standard errors in parentheses adjusted for clustering at industry level. Outcomes are defined as earnings within 6 quarters of injury. We include results using Pre-Injury Wages (defined as earnings in the six quarters prior to injury) as a placebo test. “Group” refers to injured or control. OLS used for all regressions.

Table A7

Placebo Tests using Oregon

	Oregon	California
Dependent Variable:	Medical Expenditures	
Post x Back	0.018	–0.075***

	Oregon	California
	(0.016)	(0.013)
Mean	7,118	9,377
Dependent Variable:	Post-Injury Earnings	
Post-Reform x Back	-0.026	-0.104 ***
	(0.021)	(0.018)
Mean	35,773	33,793
Dependent Variable:	1(Work)	
Post x Back	-0.002	-0.014 ***
	(0.004)	(0.004)
Mean	0.96	0.90
	Triple Difference Estimates	
Dependent Variable:	Post-Injury & Pre-Injury Earnings	
Post-Reform x Back x Post-Injury	-0.002	-0.072 ***
	(0.013)	(0.020)

Notes: Oregon column uses 2000–2002 and 2004–2006 administrative Oregon WC data (N=67,464; 134,928 for triple differences). California column uses 2000–2002 and 2004–2006 WCIRB data (N=158,065; 316,130 for triple differences). The California results have been reported previously above.

*** Significance 1%,

** Significance 5%,

* Significance 10%.

Standard errors in parentheses adjusted for clustering at industry level. Medical expenditures are total medical costs in six quarters post-injury for California claims and total medical costs overall for Oregon claims. Earnings defined as total earnings in six quarters post-injury (for both California and Oregon). 1(Work) is equal to 1 if post-injury earnings are positive. Pre-injury earnings are used in the triple difference specification and equal total earnings in the six quarters prior to injury. Other variables included in each model: industry-injury type interaction, industry-year interactions, quarter-of-injury fixed effects. Triple Differences Estimates include quarter-industry-group, industry-group, and quarter-industry-injury type interactions and use post-injury and pre-injury earnings of injured workers. “Groups” are defined by pre-injury and post-injury.

Figure 1.

National Health Care Expenditures and Workers' Compensation Medical Costs
Per Capita Medical Expenditures are derived from the National Health Expenditure Accounts, published by Centers for Medicare and Medicaid Services. 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 1996.

Figure 2.

Relative Trends in Medical Expenditures and Labor Earnings

All calculations made using 2000–2006 WCIRB data (N=189,253). The ratios are defined as the mean for low back injuries relative to the mean for other injuries. Both medical expenditures and labor earnings are defined as the totals in the 6 quarters post-injury. Other injuries are shoulder, knee, hand/wrist injuries. The shaded region represents the period in which the outcomes would have been affected by the implementation of the reforms. We shade the final quarter of 2002, though only a fraction of these injuries would have been affected within 18 months by the adoption of the guidelines. To the right of the shaded region, we observe injured workers who are subject to the reforms for the entire post-injury period.

Figure 3.**Event Study Estimates**

Each figure represents estimates generated from an event study specification using 2000–2006 WCIRB data ($N=189,253$). All outcomes refer to the 6 quarters post-injury. “Work” is equal to 1 if the injured worker has positive earnings during that time period. All specifications include quarter fixed effects and industry-year and industry-injury type interactions. The estimates for quarter 3 of 2002 are normalized to zero. Each estimate represents the conditional relationship between low back injuries and the outcome in that quarter. The shaded region represents the “implementation region” in which injuries become increasingly exposed to the medical care reforms. Figure (a) and (b) are estimated using Poisson; Figure (c) estimated using probit (coefficients, not marginal effects, presented here). 95% confidence intervals presented, adjusted for clustering at industry-level.

Figure 4.**Differential Effect by Quarter Relative to Injury**

The Section 5.1.3 triple-difference specification is estimated for each quarter relative to the time of the injury using 2000–2002 and 2004–2006 WCIRB data. Earnings refer to total earnings earned in the quarter relative to injury. 1(Work) is equal to 1 if and only if earnings are positive in that quarter. This specification includes quarter-industry-group interactions, injury type-group interactions, and quarter-industry-injury type interactions, where “group” is defined by “injured worker” or “control workers.” For earnings, estimates are generated using Poisson; for 1(Work), OLS is used. The injury occurs in quarter 0 so there should be a partial effect in that time period. We only have data for the full sample for quarters –8 to 6. For these quarters, there are 158,065 injured workers and 158,065 sets of control workers. For the other quarters, we have longer post-injury periods for the earlier years and more pre-injury data for later years. Thus, the sample composition is smaller and changing outside of the –8 to 6 range. Confidence intervals are adjusted for clustering at the industry level.

Table 1

OLS Estimates

	(1)	(2)	(3)	(4)
Outcome:	A. Medical Expenditures (Mean=\$9,377)			
Post x Back Injury	-731.11 *** (126.75)	-718.16 *** (122.69)	-710.52 *** (113.75)	-702.83 *** (113.96)
Outcome:	B. ln(Medical Expenditures)			
Post x Back Injury	-0.115 *** (0.016)	-0.114 *** (0.016)	-0.110 *** (0.015)	-0.111 *** (0.015)
Outcome:	C. Post-Injury Earnings (Mean=\$33,793)			
Post x Back Injury	-3071.65 *** (935.58)	-3024.16 *** (925.85)	-2665.08 *** (564.79)	-2769.39 *** (530.96)
Outcome:	D. 1(Work) (Mean=0.90)			
Post x Back Injury	-0.022 *** (0.007)	-0.021 *** (0.007)	-0.014 *** (0.004)	-0.014 *** (0.004)
Fixed Effects:	Year	Quarter	Quarter	Industry x Quarter
	Injury Type	Injury Type	Industry x Injury Type	Industry x Injury Type
			Industry x Year	

Notes: N=158,065 using 2000–2002 and 2004–2006 WCIRB data.

*** Significance 1%,

** Significance 5%,

* Significance 10%.

Standard errors in parentheses adjusted for clustering at industry level. All dollar estimates in 2006 dollars. Outcomes are defined as totals in the first six quarters post-injury. 1(Work) is equal to 1 if the worker has positive post-injury earnings. The “Fixed Effects” row lists all the fixed effects included in the column’s model. OLS used to generate all estimates.

Table 2**Differential Impact of Reforms on Medical Expenditures and Labor Outcomes**

	(1)	(2)	(3)	(4)
Outcome:	A. Medical Expenditures (Mean=\$9,377)			
Post x Back Injury	-0.079 *** (0.015)	-0.077 *** (0.015)	-0.076 *** (0.013)	-0.075 *** (0.013)
Effect in Dollars:	-612.59	-598.51	-590.13	-581.17
Fixed Effects:	Year	Quarter	Quarter	Industry x Quarter
	Injury Type	Injury Type	Industry x Injury Type	Industry x Injury Type
			Industry x Year	
Outcome:	B. Post-Injury Earnings (Mean=\$33,793)			
Post x Back Injury	-0.122 *** (0.030)	-0.120 *** (0.030)	-0.102 *** (0.019)	-0.104 *** (0.018)
Effect in Dollars:	-3358.55	-3317.42	-2800.96	-2837.01
Fixed Effects:	Year	Quarter	Quarter	Industry x Quarter
	Injury Type	Injury Type	Industry x Injury Type	Industry x Injury Type
			Industry x Year	
Outcome:	C. 1(Work) (Mean=0.90)			
Post x Back	-0.020 *** (0.006)	-0.020 *** (0.006)	-0.014 *** (0.003)	-0.014 *** (0.003)
Fixed Effects:	Year	Quarter	Quarter	Industry x Quarter
	Injury Type	Injury Type	Industry x Injury Type	Industry x Injury Type
			Industry x Year	

Notes: N=158,065 using 2000–2002 and 2004–2006 WCIRB data.

*** Significance 1%,

** Significance 5%,

* Significance 10%.

Standard errors in parentheses adjusted for clustering at industry level. Outcomes are defined as totals in the first six quarters post-injury. 1(Work) is equal to 1 if the worker has positive post-injury earnings. The “Fixed Effects” row lists all the fixed effects included in the column’s model. When outcome is medical expenditures or post-injury earnings, the model is estimated using Poisson. For the probability of working, probit regression is used and marginal effects (Puhani (2012)) reported.

Table 3

Robustness Tests

	(1) Include 2003	(2) Include More Injury Types	(3) Trauma Only	(4) Severity * Time Controls	(5) Employer * Time Controls	(6) Selection Adjustment
A. Medical Expenditures						
Outcome:						
Post x Back	-0.090 *** (0.012)	-0.102 *** (0.013)	-0.078 *** (0.014)	-0.062 *** (0.018)	-0.064 *** (0.025)	-0.075 *** (0.015)
Mean of Outcome:	9,554.91	9,477.23	9,542.81	9,376.69	9,596.96	9,376.69
Effect in Dollars:	-704.78	-809.66	-611.92	-475.54	-591.94	-586.57
B. Post-Injury Earnings						
Outcome:						
Post x Back	-0.096 *** (0.016)	-0.087 *** (0.016)	-0.108 *** (0.023)	-0.100 *** (0.019)	-0.088 *** (0.033)	-0.114 *** (0.020)
Mean of Outcome:	33,416.12	34,622.32	33,353.38	33,793.21	35,599.31	33,793.21
Effect in Dollars:	-2,613.02	-2,357.31	-2,983.20	-2,729.81	-2,084.00	-3,124.64
C. 1(Work)						
Outcome:						
Post x Back	-0.014 *** (0.004)	-0.009 ** (0.003)	-0.014 *** (0.004)	-0.014 *** (0.003)	-0.015 * (0.008)	-0.015 *** (0.004)
Mean of Outcome:	0.90	0.90	0.90	0.90	0.92	0.90
N	189,253	244,996	146,153	158,065	98,876	158,065

Notes: WCIRB data used in all regressions.

Significance 1%,**
Significance 5%,*
Significance 10%.

Standard errors in parentheses adjusted for clustering at industry level. Outcomes are defined as totals in the first six quarters post-injury. 1(Work) is equal to 1 if the worker has positive post-injury earnings. All models include quarter fixed effects, industry-injury type interactions, and industry-year interactions. Poisson used to generate estimates for Panels A and B. OLS used for Panel C. Column (1) includes 2003 as a pre-reform year. Column (2) includes upper back, neck, other upper extremity, and other lower extremity injuries. Column (3) selects on traumatic injuries. Column (4) includes interaction terms by year and each value of the disability rating (631 indicators). Column (5) includes interactions based on employer and year. The number of observations reported reflects that any injury incurred at a firm with no other injuries in the same year is effectively dropped. Column (6) includes a selection adjustment as discussed in Section A.3. The standard errors for these estimates are generated using a clustered bootstrap to account for the inclusion of an estimated variable in the model.

Table 4

Extensive Margin Effects Only

	Total Effect	Extensive Only	Duration	Duration (excluding 1st qtr of work)
Post x Back	-0.102 *** (0.019)	-0.016 *** (0.005)	-0.042 *** (0.007)	-0.049 *** (0.009)
Mean	33,793.21	34,441.66	34,663.47	28,120.25

Notes: N=158,065 using 2000–2002 and 2004–2006 WCIRB data.

Significance 1%,

**
Significance 5%,

*
Significance 10%.

Standard errors in parentheses adjusted for clustering at industry level. “Total Effect” column is effect shown in Table 2, Column 3. Poisson is used to generate all estimates. Outcome variable for “Extensive Only” is equal to 0 if injured worker did not work in 6 quarters post-injury and equal to industry-injury type earnings average in pre-reform period for individuals with positive earnings. Outcome variable for “Duration” is the sum of 6 variables, each pertaining to a quarter after injury. Each variable is equal to 0 if injured worker did not work in that quarter post-injury and equal to industry-injury type earnings average in pre-reform period for individuals with positive earnings in the quarter relative to injury. Outcome variable for “Duration (excluding 1st qtr of work)” is the same as the outcome for “Duration” except earnings are set to 0 in the first period worked for all workers. The average earnings (conditional on working) are calculated using only workers that had previously worked in a quarter post-injury for this final metric. All models include quarter-of-injury fixed effects, industry-injury type interactions, and industry-year interactions.

Table 5

Post-Injury Earnings by Firm

Panel A		Same Firm		Different Firm	
	Post-Injury Earnings	Any Post-Injury Earnings	Post-Injury Earnings	Any Post-Injury Earnings	
Post x Back	-0.115 *** (0.017)	-0.022 *** (0.003)	-0.063 (0.062)	0.010 (0.006)	
Mean	27,188.94	0.82	6,604.28	0.35	
Effect in Dollars	-2429.84		-398.51		
Panel B		Working at Same Firm, Different Firm			
Outcome:	Work at Same Firm, No Work at Different Firm	No Work at Same Firm, Work at Different Firm	Work at Same Firm, Work at Different Firm	No Work at Same Firm, No Work at Different Firm	
Post x Back	-0.024 *** (0.005)	0.008 *** (0.003)	0.002 (0.004)	0.014 ** (0.004)	
Mean	0.553	0.083	0.261	0.102	

Notes: N=158,065 using 2000–2002 and 2004–2006 WCIRB data.

*** Significance 1%,

** Significance 5%,

* Significance 10%.

Standard errors in parentheses adjusted for clustering at industry level. Post-Injury Earnings in same firm refers to total earnings in six quarters post-injury earned in the same firm of injury. Post-Injury Earnings in different firm refers to total earnings in six quarters post-injury earned in all other firms. For Panel B, outcomes are defined by whether worker had positive earnings at firm in which injury occurred and whether worker had positive earnings at any other firm. Outcome variable is equal to 1 if both conditions listed in header are true. Poisson regression is used when the outcome is earnings; OLS is used when the outcome is binary. The four categories in Panel B are mutually exclusive. All models include quarter-of-injury fixed effects, industry-injury type interactions, and industry-year interactions.