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THE IMPACT OF BENEFIT GENEROSITY ON WORKERS' COMPENSATION CLAIMS: EVIDENCE AND IMPLICATIONS

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ABSTRACT

Optimal insurance benefit design requires understanding how coverage generosity impacts individual behavior and insured costs. Using unique comprehensive administrative data from Texas, we leverage a sharp increase in the maximum weekly wage replacement benefit in a difference-in-differences research design to identify the impact of workers' compensation wage replacement benefit generosity on individual behavior and program costs. We find that increasing the generosity of wage replacement benefits does not impact the number of claims but has a large impact on claimant behavior, leading to longer income benefit durations and increased medical spending. Our estimates indicate that behavioral responses to increased benefit generosity raised insured costs nearly 1.5 times as much as the mechanical effect on insured costs from the benefit increase. Drawing on these estimates along with an estimate of the consumption drop experienced by injured workers, we calibrate a model to estimate the marginal welfare impact of increasing the generosity of workers' compensation wage replacement benefits. This calibration suggests that increasing benefit generosity would not improve welfare, with much of the projected welfare loss attributable to the impact of income benefit generosity on medical spending.

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1 Introduction

Social insurance programs are ubiquitous and cover some of the largest risks individuals face. Policymakers are responsible for designing the generosity of benefits within these programs. While these programs provide individuals valuable protection from risk exposure, the welfare benefits generated by this risk protection may be partially offset when individuals change their behavior in response to program incentives. The optimal design of social insurance involves balancing the value gained from risk protection against the costs associated with behavioral responses to this coverage. Thus, it is important to characterize how the generosity of coverage impacts individual behavior in these programs. In this paper, we analyze how coverage generosity impacts claims within the setting of workers' compensation insurance, and we explore the potential implications for benefit design.

Workers' compensation insurance is among the first examples of large-scale social insurance in the United States, with the establishment of state workers' compensation programs dating back to the 1910s. Workers' compensation is a large state-regulated insurance program that provides employees with state-standardized cash and medical benefits in the event of work-related injuries or illnesses.[1] While there is some variation in the details across state workers' compensation programs, the basic structure of benefits is common across states: workers' compensation insurance provides complete coverage of medical expenses associated with injury, partial wage replacement for the duration of time out-of-work due to an injury up to a specified maximum duration, and additional cash indemnity benefits in specified circumstances such as permanent impairments or workplace fatalities.

The generosity of wage replacement benefits is a source of perpetual policy debate within the setting of workers' compensation insurance. Proponents of increasing benefit generosity argue that injured workers do not have adequate resources to buffer themselves against lost wages due to workplace injuries, while opponents cite concerns about blunting workers' incentives to recover from their injuries and return to work. Workers' compensation wage replacement benefit schedules are set by the state, where the weekly benefit amount paid is a linear function of pre-injury average weekly earnings, up to a maximum weekly benefit. Much of the recent policy debate centers around the appropriate level of the maximum weekly benefit, which implicitly defines the generosity of wage replacement benefits for high-income workers. There is tremendous variation across states in their legislated maximum benefit, with maximum weekly benefit levels ranging from $494 in Mississippi to $1,819 in Iowa in 2019. Recently, several states have moved to increase or decrease their maximum weekly benefit level. For instance, at least three states—Maine, Kentucky, and Georgia— have enacted reforms increasing their maximum benefit levels by up to 25% in the last two years. While wage replacement benefit levels are the subject of active policy debate, there is very limited evidence on the impacts of the generosity of workers' compensation wage replacement benefits.

In this paper, we utilize unique administrative claims data and sharp legislative variation to study the impact of workers' compensation wage replacement benefit generosity on claimant behavior and to explore the implications of these behavioral responses for benefit design. Specifically, we estimate the impact of a recent, large-scale reform in the state of Texas which sharply increased the generosity of wage replacement benefits for high-income workers through increasing the maximum weekly benefit. The Texas Legislature passed House Bill 7 in 2005 which increased the weekly benefit cap from $540 to $674 for workers injured on or after October 1, 2006. This policy had the effect of increasing the wage replacement weekly benefit

[1] Workers' compensation insurance covers both on-the-job injuries and illnesses related to occupational exposure. Throughout, we often simply use "injuries" to refer to both qualifying injuries and illnesses.

amount by approximately 16% on average among injured workers marginal to the initial cap, while leaving benefits unaffected for workers with prior earnings below the initial cap. To identify the effect of the benefit rate on claims, we leverage this sharp increase in the maximum benefit cap in a difference-in-differences research design by comparing outcomes for workers differentially exposed to the initial maximum benefit cap who were injured either just before or after the reform was implemented.

We first illustrate that the increase in benefit generosity did not impact the number of claims with income benefits. We also demonstrate that the composition of claimants based on observable characteristics (e.g., demographic, industry, and injury characteristics) was not affected by the reform. Given this evidence, we focus throughout on the effects of the increase in benefit generosity on the behavior of claimants conditional on filing a workers' compensation claim for income and medical benefits. Specifically, we focus on two primary outcomes: the income benefit duration and insurers' medical spending for claimants. While the change in benefit generosity did not directly affect the price of medical care (which is provided to claimants at no out-of-pocket cost), there are several channels through which medical spending may be affected by income benefit generosity. For example, longer induced out-of-work durations may reduce the opportunity cost of engaging in medical care and thereby increase medical utilization. Further, claimants motivated to stay on income benefits longer may report more severe symptoms to their doctor, leading to increased medical utilization. In addition, longer induced income benefit durations may increase the number of doctor visits necessary to monitor a claimant's work capacity, which could increase medical utilization. We discuss these and other possible mechanisms at length in Section 2. While our research design does not allow us to decompose the extent to which any particular mechanism drives our overall estimated effects, we find a large impact on medical spending overall and provide suggestive evidence on the relevance of some of these mechanisms through exploring heterogeneity across types of medical care that are more or less impacted by the increase in income benefit generosity.

Our estimates indicate that workers' compensation claimant behavior is responsive to the replacement rate paid for income benefits. The reform caused a roughly 11% increase in the income benefit duration of workers' compensation claims among affected claimants, or about 2 weeks relative to the pre-policy mean of 17.8 weeks. Given the 16% average increase in the weekly benefit rate induced by the reform, this implies a benefit duration elasticity of 0.70 with a 95% confidence interval spanning 0.43 to 0.97. We find that medical utilization increased substantially when the generosity of income benefits increased. The reform caused a roughly 10% increase in the medical spending (within the first five years post injury) associated with workers' compensation claims among affected claimants, or $1,226 increase relative to the pre-policy mean of $12,461. These estimates imply that the elasticity of medical spending with respect to the income benefit rate is 0.62 with a 95% confidence interval spanning 0.37 to 0.87. Heterogeneity analysis suggests that some types of medical services were particularly responsive—including office visits, physical therapy visits, and case management services— while there is no evidence of a response for surgeries or emergency visits.

To interpret the magnitude of our main estimates, we calculate the effects of an increase in the weekly benefit rate on program costs incorporating both the direct effect (holding behavior constant) and indirect effects due to behavioral responses (in both the income benefit duration and medical spending). This calculation reveals three key facts. First, based on our estimates, the impact of behavioral responses along these two margins for responses—income benefit duration and medical spending—are equally important drivers of increased program costs. Second, collectively these behavioral responses predict increases in insurer costs that are nearly 1.5 times the magnitude of the direct, mechanical effect of an increase in benefit generosity. Third, the impact of behavioral responses on program costs is roughly four times the effect that would have been predicted based on most of the prior work on workers' compensation insurance, where roughly two-thirds of this increase relative to prior work is due to the previously unexplored connection between benefit generosity and medical spending.[2,3]

Beyond our primary estimates, we also present several pieces of supplemental evidence suggesting a connection between the estimated responses on income benefit durations and medical spending. First, we present difference-in-differences estimates for medical spending and income benefit receipt by two-week increment since injury; this analysis reveals that the timing of the effects on both outcomes aligns. Second, our analysis of heterogeneity by claimant characteristics reveals that magnitudes of the effects on income benefit duration and medical spending tend to move together when comparing estimates across subgroups. Lastly, we present correlational evidence indicating that medical spending drops sharply upon the termination of income benefit receipt.

While our estimates indicate that there are large behavioral responses to benefit generosity, individuals likely value the consumption-smoothing benefits afforded by more generous coverage and thus these estimates alone are not sufficient to conclude whether increasing the generosity of benefits would improve or harm welfare. To explore the potential welfare implications of our estimates, we extend the classic Baily-Chetty framework of optimal benefit design for the application to workers' compensation insurance.[4] In extending this framework, we model individuals as having utility in each period over both non-medical consumption and medical consumption, where these components are additively separable. In each period, an individual maximizes his/her expected utility going forward by selecting: the assets to consume this period (implicitly defining non-medical consumption), medical care to consume this period (subject to constraints), and effort to expend to recover from the injury (if the worker has not yet returned to work). The social planner's problem is to maximize the individual's ex ante utility subject to a budget constraint and worker optimization. We derive a simple formula for the marginal welfare impact of increasing the generosity of benefits based on sufficient statistics. This formula illustrates that the marginal welfare impact crucially depends on how the benefit level impacts both the income benefit duration and the medical spending of injured workers.

We then calibrate the marginal welfare impact of increasing the generosity of benefits using our estimates of the impact of benefits on the income benefit duration and medical spending along with a prior estimate of the drop in consumption experienced by injured workers upon workplace injury (Bronchetti (2012)). These calibrations suggest that a marginal increase in the generosity of benefits reduces welfare, with much of this welfare loss attributable to the previously unexplored connection between income benefit generosity and medical spending. Further, under a range of typical risk aversion values, we illustrate that

[2] A few prior papers have investigated the impact of income benefit generosity on the duration of workers' compensation income claims, largely using data and variation from the 1970s and 1980s. See Krueger and Meyer (2002) for a review of this literature. We note that there is some variation in prior estimates of the duration elasticity. While most of the commonly cited estimates imply duration elasticities in the range of 0.3 to 0.4 (e.g., Meyer, Viscusi and Durbin (1995), Neuhauser and Raphael (2004)), Krueger (1990*b*) estimates duration elasticities that range from 1.7 to 3.7. It is worth noting that much of the prior literature analyzed smaller samples or smaller scale changes in benefits, giving these studies limited statistical power to rule out relatively large ranges of duration elasticities.

[3] While our study is the first study to investigate medical spending as a margin for adjustment (to the best of our knowledge), we note one prior study aimed at estimating duration elasticities showed related evidence in a covariate balance test. Specifically, Meyer, Viscusi and Durbin (1995) interpret medical spending as a proxy for injury severity and investigate patterns in medical spending in a covariate balance test looking at mean differences in medical spending over time and across more and less treated workers. Given limited statistical power, their differences in means estimates do not allow one to rule out large increases or decreases in medical spending coincident with increases in income benefit generosity, with 95% confidence intervals on implied elasticities spanning -2.9 to 1.0 across specifications. The lack of statistical power to investigate patterns in medical spending in the Meyer, Viscusi and Durbin (1995) study may have been due to the study's limited sample size (e.g., the number of treated claimants are 219 and 1,161 for the two state samples the study investigates) and the large variance in medical spending.

[4] For more background on this framework, see Baily (1978), Chetty (2006), and Chetty and Finkelstein (2013).

the analogous calculation using commonly cited duration elasticity estimates from the prior literature and ignoring any effect on medical spending would reach the opposite conclusion: increasing the generosity of benefits would increase welfare. In other words, these welfare calibrations suggest that incorporating the new evidence presented in this paper on the impact of income benefit generosity may change the predicted desirability of increasing the generosity of workers' compensation income replacement benefits.

This paper contributes to the broader literature quantifying behavioral responses to coverage generosity in various insurance settings and evaluating the implications for benefit design. Most of the recent studies in this literature have focused on investigating these topics within the settings of health insurance (e.g., Cabral and Mahoney (2019), Brot-Goldberg et al. (2017), Einav et al. (2013), Chandra, Gruber and McKnight (2010), Powell and Goldman (2016)), unemployment insurance (e.g., Chetty (2008), Kroft and Notowidigdo (2016), Landais (2015), Landais and Spinnewijn (2019), Card et al. (2015), Schmieder, von Wachter and Bender (2012), Johnston and Mas (2018)), and disability insurance (e.g., Autor et al. (2019), Autor, Duggan and Gruber (2014)). Within the context of workers' compensation insurance, there are a few prior studies investigating the impacts of wage replacement benefit generosity on the number of income claims (e.g., Krueger (1990*a*)) and income benefit duration (e.g., Meyer, Viscusi and Durbin (1995), Krueger (1990*b*), Neuhauser and Raphael (2004)), largely using data and variation from the 1970s and 1980s.[5] This paper makes several contributions to this literature. First, we leverage sharp, recent policy variation and rich administrative data to provide transparent estimates of the comprehensive impact of workers' compensation income benefit generosity. Our estimates imply an income benefit duration elasticity that is larger than most of the commonly cited duration elasticity estimates in the prior literature. Further, we find that behavioral responses in claimant medical spending—a previously unexplored margin of adjustment—are equally important as the income benefit duration responses in terms of their impact on program costs. Collectively, across these two margins for adjustment, our estimates predict behavioral responses increase program costs considerably more than would have been predicted based on prior work. Second, we provide recent evidence on the impacts of workers' compensation benefit generosity that is pertinent to ongoing workers' compensation policy debates. Workers' compensation programs and the nature of workers' compensation claims have changed significantly over the last several decades, with the composition of benefits shifting from 71% cash benefits (29% medical benefits) in 1980 to 50% cash benefits (50% medical benefits) in 2008 and onward (McLaren, Baldwin and Boden, 2018). Still, relatively little is known about the determinants of workers' compensation medical spending; our estimates indicate that the wage replacement benefit rate—a key policy parameter in this setting—is an important determinant of medical spending and program costs.[6] Third, beyond estimating the impacts of benefit generosity on income benefit durations and medical spending, we also use the estimated elasticities to explore the implications of these behavioral responses for benefit design. We extend the classic Baily-Chetty framework often used to explore the impacts of unemployment insurance benefit design to the setting of workers' compensation insurance, where we incorporate multiple dimensions on which individuals may alter their behavior and thereby affect program costs. Our estimates suggest that a marginal increase in the generosity of benefits reduces welfare, with much of this welfare loss attributable to the previously unexplored additional margin for behavioral responses in this setting.

The remainder of the paper proceeds as follows. Section 2 provides details on the institutional setting

[5]See Krueger and Meyer (2002) for a review of this literature.

[6]In other related work, Powell and Seabury (2018) study a reform within the California workers' compensation system and find that utilization management techniques aimed at curbing medical spending of injured workers resulted in delayed return to work and lower earnings. In contrast, the increase in income benefit generosity that we analyze leads to an increase in both income benefit receipt and medical spending, suggesting a possible complementarity between medical spending and remaining out-of-work due to injury along the margin of income benefit generosity.

and the data. Section 3 outlines the empirical strategy, and Section 4 presents the estimates. Section 5 considers the implications for benefit design, outlining a welfare framework and presenting welfare calibrations. Lastly, Section 6 concludes.

2 Background and Data

This section begins by providing background information on workers' compensation systems more broadly, the structure of workers' compensation benefits, and the Texas workers' compensation system. We then describe the policy change we leverage, describe the data sources utilized in this study, and present descriptive statistics.

2.1 Background

2.1.1 Workers' Compensation Insurance

Workers' compensation is a state-regulated insurance system that provides covered employees with cash and medical benefits for work-related injuries or illnesses. Workers' compensation insurance provides coverage regardless of whether the employer or employee is at fault for the workplace injury, and it serves as the exclusive legal recourse for covered workers for workplace injuries, meaning that injured workers cannot sue their employers for negligence. The adoption of workers' compensation systems is frequently characterized as a "grand bargain" between workers and employers: relative to the status quo prior to the enactment of workers' compensation statutes in the early 20th century, workers gained a reliable source of no-fault compensation for workplace injuries while employers gained protection from tort liability resulting from workplace injuries.

Each state has its own workers' compensation program. In contrast to unemployment insurance or disability insurance, workers' compensation is entirely designed and regulated by states, with no significant federal involvement.[7] Employers purchase workers' compensation insurance from private insurers or directly from a public insurer. Typically, states allow very large employers the option to become a certified self-insured entity to directly provide this insurance to employees. States standardize the structure of benefits and regulate the pricing of policies, and there is extensive risk adjustment in this market through regulated industry-occupation rating and experience rating. According to the National Academy of Social Insurance, workers' compensation insurance costs accounted for approximately 1.3% of covered payroll in 2016 down from 1.7% in 2005 (McLaren, Baldwin and Boden, 2018). The costs of workers' compensation insurance vary substantially across industry-occupational groups. For instance, data from Texas reveals that workers' compensation costs comprise only 0.9% of covered payroll for college professional employees and 14.5% of covered payroll for oil and gas well employees (Cabral, Cui and Dworsky, 2019).

There have been substantial changes in workers' compensation insurance over the past several decades. First, the release of the National Commission on State Workmen's Compensation Laws report in 1972 spurred a wave of state legislative action which lead to significant increases in coverage generosity and standardization of workers' compensation systems across states in the late 1970s and early 1980s (Howard, 2002).[8] Second, more recent state legislation in the 1990s tightened the criteria for eligible injuries (Boden

[7] There are separate U.S. federal government programs which cover federal civilian employees and specific high-risk workers such as energy employees exposed to radiation.

[8] The Occupational Safety and Health Act of 1970 created the National Commission on State Workmen's Compensation Laws. The Commission was charged with reviewing state workers' compensation laws and recommending a set of national standards for state workers' compensation programs. The Commission's final 1972 report (National Commission on State Workmen's Compensation Laws, 1972) outlined "nineteen essential elements" of a good workers' compensation system. Further, the Commission recommended that states be given three years to comply with the nineteen essential elements, with Congressional action guaranteeing compliance if

and Ruser (2003)).[9] Third, medical costs have dramatically risen as a share of total workers' compensation costs over the past several decades. While medical benefits made up less than 30 percent of benefits paid by workers' compensation insurance in 1980, they made up around half of benefits paid by workers' compensation insurance by the mid-2000s (McLaren, Baldwin and Boden, 2018). This trend may reflect several factors including the more general increase in health care costs nationally, changes in medical technology available to address workplace injuries, and changes in the composition of workplace injuries over time. These changes in workers' compensation insurance and the nature of workplace injuries mean that current estimates of the impact of income benefits are important for assessing policy.

2.1.2 Structure of Workers' Compensation Benefits

While there is some variation across states in the details of the workers' compensation insurance systems, there are many commonalities across states in the basic structure of workers' compensation insurance. All covered employees are guaranteed standardized, state-defined benefits in the case of workplace injury. In all states, these benefits include full coverage of medical expenditures associated with the work-related injury, temporary income benefits that provide partial wage replacement for lost time out of work, and additional unconditional cash benefits for permanent impairments and workplace fatalities. Below, we provide more detail on the structure of workers' compensation insurance in Texas—the setting of our analysis—and discuss how this compares to the basic structure of workers' compensation systems more broadly.

Workers' compensation insurance provides complete coverage of injury-related medical expenditures at no out-of-pocket cost to the claimant, and workers' compensation is the first payer for any injury-related medical expenses. Workers' compensation insurance covers all injury-related medical spending indefinitely, regardless of a claimant's work status or receipt of cash benefits. In Texas, as in many states, the delivery of medical care in workers' compensation insurance follows a "gatekeeper" model. Workers' compensation claimants choose a "treating doctor", and this treating doctor is responsible for overseeing the claimant's medical care, evaluating the claimant's medical improvement, and assessing the claimant's work capacity. In addition to receiving reimbursement for typical procedures billed by physicians, physicians treating workers' compensation claimants receive payments for additional "case management services" that pertain to their particular role in overseeing the medical care and income benefit eligibility of injured workers. Prior studies have documented that physician payments for services provided to workers' compensation claimants exceed those for the same services provided to other patients (Baker and Krueger (1995), Johnson, Baldwin and Burton (1996)).

Workers' compensation insurance also provides temporary income benefits which follow a very similar structure across states. After a waiting period of three to seven days, an injured worker is eligible to receive income benefits which provide partial wage replacement during a temporary absence from work. Temporary income benefits are terminated when the earliest of the following three conditions are met: (i) the employee decides to return to work, (ii) the treating doctor has certified that the employee has reached his "maximum medical improvement", (iii) the income benefit maximum duration is met. In Texas, the temporary income maximum benefit duration is two years (104 weeks) and the waiting period is seven days. An

necessary. While Congress never passed legislation requiring that states meet the recommended standards, the release of the report and the threat of such legislation in the subsequent years may have contributed to significant increase in state activity expanding coverage and increasing benefit generosity the the wake of the report's release. In 1972, the average share of recommended essential elements met by state workers' compensation programs was 6.9 out of 19. Average state compliance increased to 9.4 of out 19 by 1975 and to 12.1 out of 19 by 1980. See Howard (2002) for more background on the report and subsequent state legislation.

[9] For example, several states restricted the criteria of a eligible impairment to exclude workplace disability that resulted from aggravating pre-existing conditions or exacerbating the aging process. Further, some states narrowed eligible impairments to be only those provable with objective medical evidence, narrowing the scope of allowable musculoskeletal injuries.

injured employee receives partial wage replacement during his temporary income benefit duration, where the weekly benefit amount is a linear function of a claimant's prior average weekly wage, subject to a maximum and minimum weekly benefit level. The maximum and minimum benefit levels vary across states, and we use a large update to the maximum benefit level in Texas in this paper to identify the impact of benefit levels on outcomes.

After the completion of temporary income benefits, injured workers with permanent impairments are eligible for additional cash indemnity benefits. While the details of compensation for these permanent impairment benefits depend on the state, the most common model is used in Texas. In this model, a worker's permanent impairment is rated upon completion of temporary income benefits, and the worker is provided unconditional cash benefits that are a function of the severity rating of his permanent impairment and his prior average weekly wage. Permanent impairment benefits are not contingent on the injured worker's subsequent work status or earnings, and most compensated permanent impairments represent relatively minor impairments.[10] Workers' compensation insurance also provides death and burial benefits to surviving family members in the case of workplace fatalities.

2.1.3 Description of Policy Variation and Setting

The most discussed policy parameter in the setting of workers' compensation insurance is the replacement rate for temporary income benefits. There are many reasons this parameter has been the primary focus of both policy discussions and academic work.[11] First, the temporary income benefit replacement rate is the only parameter governing the generosity of benefits that has direct incentive effects, and thus is the most likely parameter to affect claimant behavior. Receipt of temporary income benefits is contingent on being out-of-work, while medical care is always provided at no out-of-pocket cost and other workers' compensation income benefits are not contingent on behavior going forward (e.g., permanent impairment benefits, death benefits, burial benefits). Thus, the temporary income benefit replacement rate is the policy-relevant parameter that is ex ante most likely to affect claimant behavior. Second, temporary income benefits are by far the most common type of workers' compensation cash benefit, with 90% of workers' compensation claimants with cash benefits receiving temporary income benefits.

In this paper, we focus on estimating the impacts of changing the generosity of temporary income benefits, which we will refer to hereafter as simply income benefits. To do this, we take advantage of a sharp change in the generosity of income benefits within the Texas workers' compensation insurance system. Workers' compensation income benefit schedules are set by the state, where the weekly income benefit amount is a linear function of an injured worker's prior average weekly wage, up to a maximum weekly benefit cap. In 2005, the Texas Legislature passed House Bill 7 which increased the maximum weekly income benefit from $540 for workers injured prior to October 1, 2006 to $674 for workers injured on or after October 1, 2006. Figure 1 displays the maximum benefit by injury date over time. Prior to the implementation of House Bill 7, the maximum weekly income benefit was set statutorily and had been approximately $540 for several years. The passage of House Bill 7 changed how the maximum weekly

[10]While the receipt of permanent impairment benefits subsequent to income benefit termination is relatively common, these permanent impairments are typically minor, with the mean claimant rated as 2.75% impaired within our sample among those with some permanent impairment benefits. More generally, nearly all workers' compensation claimants receiving income benefits return to work within a few years, regardless of whether they have some degree of permanent impairment. TDI (2015) analyzes linked Texas workers' compensation insurance data and unemployment insurance earnings records, documenting that 76% of workers' compensation income benefit recipients returned to work within six months of injury and 95% returned to work within three years of injury among those injured in 2011.

[11]For examples of prior papers that study the impact of temporary income benefit rates on income benefit duration, see Meyer, Viscusi and Durbin (1995), Krueger (1990*b*), and Neuhauser and Raphael (2004). See Krueger and Meyer (2002) for a review of this literature.

income benefit is set, requiring that the maximum weekly benefit going forward: (i) would be a specified function of the state average weekly wage and (ii) would be updated annually by the Texas Workforce Commission for injuries on or after October 1 of each calendar year. In effect, this reform induced a sharp, large increase in the generosity of benefits for higher earner claimants injured on or after October 1, 2006, with smaller increases on October 1 of subsequent years as benefits are annually re-calibrated for inflation in the state average weekly wage.

We use the large, sharp increase in benefit generosity for high earner claimants by injury date around the implementation of the reform (October 1, 2006) to analyze the effect of benefit generosity on outcomes of interest. Our baseline analysis will focus on claimants with injury dates spanning January 2005 (the start of our data) to September 2007, as this is the period where the variation is the cleanest. For robustness, we report alternative specifications using an expanded sample that includes claimants injured up to three years after the reform is implemented.

Figure 1 plots the weekly benefit amount as a function of the average weekly wage, where the solid line depicts the "old schedule" applicable to individuals injured before October 1, 2006 and the dashed line depicts the "new schedule" applicable to claimants injured on or after October 1, 2006 (and before October 1, 2007). Further, this figure displays a histogram of the average weekly wage for workers' compensation claimants in Texas. Among the highest earners (those with prior earnings above the new schedule maximum), the reform causes an almost 25% increase in the weekly benefit rate. On average, the reform increased the weekly benefit rate by approximately 16% among affected claimants (those with prior earnings above the old schedule maximum).

In Section 3, we discuss the identifying variation in more detail and present evidence illustrating there is no change in the number of claims or the composition of claimants based on observable characteristics in response to the reform. Based on this evidence, our primary analysis focuses on changes in claimant behavior conditional on making a claim. Specifically, we investigate the impact of wage replacement generosity on two primary outcomes: income benefit duration and medical utilization. While no prior research to our knowledge has estimated the impact of wage replacement benefits on medical spending, higher replacement rates have the potential to affect workers' compensation medical spending through multiple mechanisms. If time away from work and medical care are complements, higher wage replacement rates could increase medical spending. One reason that medical care could be a complement to time away from work is that having additional time outside of work lowers claimants' opportunity cost of time, which could lead to claimants receiving additional medical care. Higher replacement rates could also lead to injured workers obtaining additional medical care if continued doctor visits are required to maintain income benefits or if workers report that their injuries are more severe to justify additional time away from work. Alternatively, higher wage replacement rates could lower medical spending if additional recovery time can substitute for medical care or if the additional money has a direct and positive effect on health.

This setting provides a uniquely good opportunity to study the impact of benefit generosity on workers' compensation insurance claims for several reasons. First, the reform in Texas provides sharp and substantial variation in the generosity of benefits. While many states have previously adopted policies to index their maximum weekly income benefit to inflation, this recent reform which induced a sharp, large change in the weekly maximum benefit in Texas represents very useful variation to study the impacts of income benefit generosity. Second, Texas collects uniquely detailed data on workers' compensation claims, and we have been able to obtain this data through a series of open records requests under the Texas Public Information Act. While prior research on workers' compensation insurance generosity has been limited to using claimant-level data on aggregate outcomes (e.g., total received benefits), the uniquely detailed administrative data from Texas allows us to explore broader questions by leveraging information on the timing of benefit receipt and types of medical care individuals utilize. Third, Texas is a large state and the structure of workers' compensation insurance benefits in Texas is fairly representative of workers' compensation systems more broadly. Because the workers' compensation insurance data and policy details vary state-to-state, studying the impact of workers' compensation insurance generosity requires focusing on a particular state. Among states, Texas has the advantage of being the second most populous state, with an estimated population of more than 28 million.[12] Further, the structure of income and medical benefits in Texas resembles other workers' compensation programs nationwide.

It is important to note that while many of the regulations governing the state workers' compensation market (e.g., benefit structure, insurer participation, pricing regulations) are very similar in Texas and other states, there is one notable exception: workers' compensation insurance coverage is voluntary in Texas while it is effectively mandatory in other states. While coverage mandates in 15 other states have exemptions for very small businesses and many states have additional exemptions for specific classes of workers such as agricultural or domestic workers, Texas is the only state where any employer can decide to opt out of the workers' compensation insurance system in favor of tort liability for workplace injuries. Though workers' compensation insurance is voluntary in Texas, coverage rates are high: roughly 87% of Texas workers statewide are covered compared to 97.5% of workers nationwide in 2016.[13] Though the Texas workers' compensation system has the peculiar voluntary coverage feature, institutional details and supplementary evidence suggest that this feature is not likely to affect the internal validity of our results. We find no change in the number of claimants or the composition of claimants based on observables with respect to our identifying variation and no change in firm coverage decisions among firms employing workers differentially exposed to the reform. This latter finding, which is discussed in Appendix Section A, is in line with our expectations, as we would not expect coverage decisions to adjust in the short-run because policy renewal dates are staggered throughout the calendar year and there are lags in the premium rating windows, preventing regulated premiums from adjusting to higher claim costs in the short-run.[14,15]

More generally, differences in the composition of workers' compensation claimants in Texas relative to broader populations—whether driven by institutional features or otherwise—may limit the external applicability of our findings beyond Texas. Table 1 provides some context by comparing individuals receiving workers' compensation benefits in Texas and nationwide using data from the Current Population Survey

[12] According to the United States Census in April 2010, the population of Texas was 25,145,561. As of July 2018, the Census estimates the population in Texas to be 28,701,845.

[13] According to a study conducted by the Texas Department of Insurance (TDI, 2019), 82% of private sector workers were covered by workers' compensation insurance in 2016. Further, all public sector workers are mandated to have workers' compensation insurance. The authors' calculate the fraction of workers covered by workers' compensation insurance in Texas is roughly 87% based on combining these statistics with the fraction of Texas workers in private sector employment relative to the Texas aggregate average annual workforce in 2016 using data from the Bureau of Labor Statistics. The nationwide average coverage rate is obtained from (McLaren, Baldwin and Boden, 2018).

[14] The state regulates all the relative premiums in this market through industry-occupational rating and experience rating. Any differential increase in the costliness of claims for employers with high earning employees would only be reflected in a differential change in premiums with a lag due to the lags built into the rating update algorithms. In setting industry-occupational rates, the state regulator uses historical claims from a five-year window lagged by three years. In determining employer experience rating multipliers, the regulator mandates the use of a three-year window with a 21 month lag.

[15] Though we find no evidence of a change in coverage, it is not ex ante obvious that a change in coverage rates would be problematic from the standpoint of internal validity. While there are not many studies analyzing employer participation decisions and potential selection in the Texas workers' compensation system, there are two notable exceptions: Cabral, Cui and Dworsky (2019) and Butler (1996). Leveraging plausibly exogenous premium variation, Cabral, Cui and Dworsky (2019) analyzes selection within the Texas workers' compensation insurance market and finds no evidence of adverse or advantageous selection. In an older study, Butler (1996) finds there is no correlation between workplace fatality rates and workers' compensation insurance provision, leading him to conclude that safety levels are likely similar among firms within and outside the Texas workers' compensation system.

(CPS) Annual Social and Economic Supplement 2002-2011 (representing years 2001-2010). Columns 1 and 2 describe all workers' compensation claimants in Texas and all states, respectively. Columns 3 and 4 focus on the subset of claimants who had inflation-adjusted earnings in the prior year that exceeded $771 per week (=$540/0.7) and thus would have been marginal to the initial maximum benefit cap if they had been in our sample. Claimants in Texas and the broader U.S. look similar to one another on demographic characteristics and earnings, both in the overall claimant population and the subset of high earner claimants. Differences in industry composition between Texas and the broader U.S. are reflected in industry composition among workers' compensation claimants, with fewer Texans working in education and health care services and more Texans working in mining, utilities, and construction. Overall, it is important to emphasize that neither the population of workers' compensation claimants in Texas nor the high earner subset of these claimants is representative of claimants in the U.S. as a whole, so one should exercise appropriate caution in extrapolating from our estimates. That being said, along the lines of observable attributes, high earner claimants in Texas look broadly similar to high earner claimants nationwide. Industry composition is one observable dimension on which these claimants look somewhat dissimilar. As discussed in Section 4, we find no meaningful heterogeneity in our estimated elasticities across industries, and our results are very similar when re-weighting our sample of Texas workers' compensation claimants on demographic and industry characteristics to resemble claimants nationwide.

Another relevant change in the Texas workers' compensation system that occurred concurrently with the increase to the maximum temporary income benefit rate was an increase in the maximum permanent impairment benefit rate paid for each percentage point of permanent impairment after the completion of temporary income benefits. In principle, unconditional cash transfers received after the completion of the temporary income benefit spell could potentially affect the duration claiming income benefits and medical spending, if individuals are forward-looking and informed of their later eligibility for these unconditional cash benefits. Further, if individuals are sufficiently forward-looking and informed, knowing the effect of an increase in unconditional cash benefits could potentially aid in understanding whether the increase in the income benefit rates affects claimants' behavior by providing claimants increased access to liquidity rather than through distortions in the marginal incentives to return to work. Since permanent impairment benefit rates are capped at lower levels of pre-injury earnings than income benefits in the Texas workers' compensation system, our setting allows for separate identification of the effects of both policy parameters because the maximums bind for different parts of the pre-injury income distribution. In Appendix Sections A and B, we present difference-in-differences estimates which indicate that increased permanent impairment benefit generosity does not appear to affect either the duration of income benefit receipt or medical spending, and we verify that the increase in permanent impairment benefit generosity does not confound the identification of the effect of income benefits.

2.2 Data

We have compiled a unique administrative dataset for this project through a series of open records requests submitted to the Texas Department of Insurance (TDI). The data consist of detailed information on workers' compensation claimants, including all medical and cash benefit claims with injury dates from 2005 to 2009. The medical benefit data include information on each workers' compensation insurance medical bill, including information on: procedure type (CPT codes), amount paid, amount charged, diagnoses (ICD-9 codes), date, place of service, and provider information. The medical data cover all medical utilization including physician care, outpatient care, inpatient care, and prescription drugs. The cash benefit data include information on: type of cash benefits received, prior average weekly wage, total benefits received, replacement rate for income benefits, benefit start and end dates, and injury date (month-year). The data also include rich demographic information about the claimant including: sex, birth date (month-year), zip code, and industry.

We define the injury date to be the month-year of the injury as specified by the insurer. Our main sample consists of claimants with injury dates from the start of our data (January 2005) until one year after the maximum weekly income benefit increase was implemented (September 2007), though we also consider an expanded sample that includes claimants injured up to three years post implementation of the new benefit schedule. We adjust claimants' average pre-injury weekly wages for inflation and restrict the sample to claimants with real average pre-injury weekly wages of $540 to $4,000 as of the first month of the benefit increase. From January 2005 to September 2007, 67,895 claims occurred that meet these criteria. To arrive at the analysis sample, we then drop observations with missing gender or age or with age calculated to be greater than 80 (2.0 percent), observations with non-positive medical spending (0.5 percent), observations with implausibly high income benefit amounts relative to the duration of benefits (3.4 percent), and observations with contradictory injury dates (less than 0.1 percent). The final analysis sample consists of 63,883 claims from January 2005 to September 2007.

Table 2 provides descriptive statistics of the baseline sample. The mean age in the baseline sample is 42.6 years, and 78% of claimants are men. Thirty-one percent of claimants' initial medical bill is for an emergency department (ED) visit or for an emergency admission into a facility. We refer to these claims as "ED claims" throughout. For some analyses, we concentrate on ED claims under the assumption that these claims are less discretionary than the average workers' compensation claim and the exact injury date is known with greater accuracy for these claims.

One key outcome we investigate is the income benefit duration, which we throughout simply refer to as "benefit duration" for brevity. The mean benefit duration in our sample is 18.0 weeks.[16] The mean weekly benefit amount is $525, and the mean replacement rate relative to prior earnings is 63%. Another key outcome we investigate is the medical spending associated with the claim. To minimize the influence of outliers in medical spending, we winsorize bill-level medical paid amounts at the 99th percentile for each year of the data before computing the aggregate measures of medical spending for each claimant and then also winsorize the claimant-level medical spending at the 99th percentile of the sample. The mean medical spending over the five years subsequent to injury is $12,504.[17]

[16]We compute this variable as the number of weeks from the day income benefits begin until the day that they end. The Texas legal code caps income benefit duration at 104 weeks with the only exception being for claimants who have spinal surgery after having received benefits for 101 weeks. The number of weeks between benefits starting and ending could also be greater than 104 weeks if claimants start a second spell of benefits after their first spell ends, though repeat spells of income benefits are rare in Texas workers' compensation (occurring in less than 0.1 percent of claims), or because of errors in the spell dates in insurers' records. For the less than 1 percent of claimants with more than 104 weeks between income benefits starting and ending, we set benefits to be 104 weeks, though the estimates are very similar if we do not adjust the variable in this way.

[17]By law, workers' compensation insurance is the first payer for medical spending related to workplace injuries for covered workers, regardless of income benefit receipt. Thus, in principle, our measure of medical spending should capture all medical spending that results from the workplace injury. In practice, however, it may be possible that some of the medical costs of treating a workplace injury could be shifted onto other payers. If higher income benefits reduce the amount of workers' compensation medical costs being shifted onto other payers, the reform could lead to workers' compensation medical spending rising even if the reform had no effect on total medical spending. In Appendix Section C, we look for evidence of any such spillovers. Specifically, we test whether the reform reduced the likelihood that workers' compensation insurers successfully deny payments to providers and whether the reform had smaller effects on spending for procedures that are typically subject to heightened monitoring by health insurers, both of which would be consistent with the reform increasing workers' compensation medical spending by reducing the prevalence of cost shifting onto other payers rather than by leading to increases in total medical spending. As discussed in more detail in Appendix Section C, we find no evidence that the reform affected cost shifting to other payers.

3 Empirical Strategy

Next, we outline the empirical strategy. Below, we describe the econometric model underlying our empirical analysis and the identifying variation.

3.1 Econometric Model

We examine the effect of the change in the weekly benefit amount using a difference-in-differences approach that compares outcomes for claimants differentially exposed to the update in the benefit schedule. Let i denote claimant and t month of injury. We measure exposure to the schedule change with a distance-to-max variable, Δb_{it}, which isolates the increase in the weekly benefit level due to the change in the maximum benefit:

$$\Delta b_{it} \equiv b^{new}(w_{it}) - b^{old}(w_{it}), \tag{1}$$

where $b^{new}(w)$ is the weekly benefit for an individual with prior wage w under the new benefit schedule, $b^{old}(w)$ is the weekly benefit for an individual with prior wage w under the old benefit schedule, and w_{it} is the pre-injury average weekly wage of individual i injured in month t. To contextualize the scale of the reform we examine, we often focus on reporting the overall effect of the schedule change on the affected claimants by scaling this exposure measure by the mean value among affected claimants:

$$\Delta b_{it}\text{\_scaled} = \frac{\Delta b_{it}}{\frac{1}{|\mathcal{J}|}\sum_{i \in \mathcal{J}} \Delta b_{it}}, \tag{2}$$

where $\mathcal{J}$ represents the set of claimants with non-zero distance-to-max ($\mathcal{J} \equiv \{i : \Delta b_{it} > 0\}$). This time-invariant scaling is applied to all claimants, and the coefficient on the interaction of this scaled measure and a post-reform indicator variable can be interpreted as the effect of the mean increase in benefits experienced by claimants whose benefits were affected by the schedule change.

We estimate a difference-in-differences specification that allows the coefficient on the scaled distance-to-max variable, Δb_{it}_scaled, to vary flexibly by bins on injury month. Let y_{it} be the outcome variable for claimant i with injury month t. Our baseline regression can be represented as follows:

$$y_{it} = \alpha_t + \theta \Delta b_{it}\text{\_scaled} + \left[\sum_{t \notin k_0} \beta_k \times \mathbb{1}(t = k) \times \Delta b_{it}\text{\_scaled}\right] + f(X_{it}) + \epsilon_{it}, \tag{3}$$

where α_t is an injury month fixed effect, Δb_{it}_scaled is scaled distance-to-max, and $f(X_{it})$ represent additional flexible controls included in some specifications. Our baseline specification includes the following controls: age, gender, county by injury-month fixed effects, ED claim indicator, and injury day-of-the-week fixed effects. We also report specifications with only age and gender controls. The coefficients of interest are the β_k's, where we use summation notation to make explicit that we allow these estimates to vary with bins on the injury date. We normalize the coefficient on the bin just prior to the reform implementation to zero ($\beta_{k_0} = 0$), so that the estimates can be interpreted as the change in the outcomes relative to the months directly preceding the implementation. In addition to estimating this flexible specification, we will also report the mean effect among all claimants subject to the benefit change, π, by estimating the following specification grouping injury months into either pre- or post-reform:

$$y_{it} = \rho_t + \delta \Delta b_{it}\text{\_scaled} + [\pi \times \mathbb{1}(t \geq t_0) \times \Delta b_{it}\text{\_scaled}] + f(X_{it}) + \varepsilon_{it}, \tag{4}$$

where t_0 represents the threshold injury date (October 1, 2006) such that claimants injured on or after this date were subject to the new benefit schedule.

The identification assumption for this difference-in-differences specification is the parallel trends assumption: in the absence of the maximum benefit change, the outcomes of interest would have evolved in parallel for claimants differentially exposed to the reform. We utilize several approaches to assess the validity of this assumption. Our first approach is to plot the β_k coefficients by injury date, allowing us to visually assess whether there are spurious pre-exisiting trends correlated with exposure to the policy. Our second approach is to demonstrate that there are no correlated changes in claimant characteristics based on observable attributes. Our final approach is to illustrate that our results are robust to alternative specifications which vary the set of included controls or the sample of included claimants.

3.2 Identifying Variation

Variation in Weekly Benefit Amount Table 3 investigates the impact of the reform on the weekly benefit rate claimants are paid using the difference-in-differences specification outlined in Equation (4). Panel A relates the level of the potential weekly benefit to the unscaled distance-to-max measure, while Panel B relates the natural logarithm of the potential weekly benefit to the scaled distance-to-max measure. Column 1 reports the baseline specification, while the remaining columns investigate alternative specifications: a specification with fewer controls (column 2), an analogous specification estimated in levels/logs as indicated (column 3), a specification focusing on claims originating in the ED (column 4), and a specification using an expanded sample including claimants injured up to three years after the reform (column 5).

Figure 2 plots the coefficients on the distance-to-max by injury month bin interactions from the difference-in-differences specification outlined in Equation (3). Panel A displays estimates corresponding to the flexible version of the specification in Table 3 Panel A column 1. Panel B displays estimates corresponding to the flexible version of the specification in Table 3 Panel B column 1.

Our focus is on the large increase in the benefit rate due to reform implemented on October 1, 2006. As noted in Section 2, there are subsequent small annual inflation adjustments to the maximum benefit effective for injuries on or after October 1 of each year. Our baseline analysis considers claimants injured up to one year after the reform, as this is where the variation is the clearest. For robustness, we also display results for an expanded sample that includes claimants injured up to three years after the reform in column 5, some of whom are subject to the subsequent maximum benefit inflation adjustments. Appendix Figure A2 plots the coefficients on the distance-to-max by injury month bin interactions from the difference-in-differences specification for the expanded sample and illustrates how the benefits of claimants injured after September 1, 2007 are affected by the subsequent inflation adjustments. Appendix Figure A3 plots the equivalent estimates for the expanded sample for the main outcomes of interest.

Figure 2 shows there is a sharp change in the weekly benefit amount when the new benefit schedule is implemented. Figure 2 Panel A illustrates that the distance-to-max measure of exposure to the reform causes a one-for-one change in the potential weekly benefit by comparing claimants injured just before and after the implementation. Over the entire baseline sample, Table 3 Panel A column 1 indicates that a $1 increase in the distance-to-max variable translates to an average increase of $0.93 in the weekly benefit rate paid. The coefficients are similar across alternative specifications and are precisely estimated with coefficients ranging from $0.93 to $1.10 and standard errors no larger than $0.01. We note that the coefficient is less than one averaging over the baseline sample period because the maximum weekly benefit is set in nominal terms. This means that in a given period with a fixed maximum weekly benefit, the weekly benefit of the high earners (at the maximum benefit) depreciates in real terms relative to the weekly benefit of the control group (below the maximum benefit) which increases over time with wage inflation.

To estimate the mean increase in the potential weekly benefit among exposed claimants, Table 3 Panel B and Figure 2 Panel B display the regression results relating the natural logarithm of the weekly benefit to the scaled distance-to-max measure. Table 3 Panel B column 1 indicates the reform increased the mean weekly benefit rate by 16.0% for the exposed claimants, representing an average increase of $95.42 in the weekly benefit level. The estimated coefficients are very similar across specifications, ranging from 16.0% to 19.0%, with an associated standard error never exceeding 0.2%. In the remainder of the paper, we focus on the scaled distance-to-max to measure exposure to the reform, which provides estimates of the mean effect of the change in the benefit schedule. While we often report reduced form estimates for our main outcomes of interest, we obtain elasticities with respect to the potential weekly benefit by estimating the analogous instrumental variables specifications which effectively scales these reduced form estimates by the first stage estimates in Table 3. See Appendix Table A4 for these instrumental variables estimates.

Claim Rates and Claimant Characteristics Figure 3 displays the number of income benefit claims by injury month relative to the number of income benefit claims in the month just prior to implementation. This series is displayed separately for "High Earners," those marginal to the initial maximum benefit (for whom $\Delta b_{it} > 0$), and for "Middle Earners," those not marginal to the initial maximum benefit (for whom $\Delta b_{it} = 0$). If the increase in benefit generosity caused an increase in claims, we would expect to see the High Earner and Middle Earner lines diverge following the implementation of the reform with the High Earner line lying consistently above the Middle Earner line. Instead, we see no such pattern, as the lines appear to track each other equally well before and after the new benefit schedule was implemented. This suggests that the increase in benefits did not affect the likelihood of claiming income benefits.

Table 4 explores whether the identifying variation is related to observable claimant characteristics. Each row of this table reports estimates from our baseline difference-in-differences specification excluding controls, replacing the dependent variable with a range of demographic characteristics (e.g., age, male, married) and claim characteristics (e.g., ED claim, impairment type, industry).[18] In addition, we investigate two composite measures, "Predicted Log(Benefit Duration)" and "Predicted Log(Five Year Medical Spending)". To calculate these composite measures, we first fit lasso models of the natural logarithm of benefit duration and five year medical spending on demographic and claim characteristics for the set of claimants eligible for the original benefit schedule and then use the coefficient estimates from the lasso models to predict benefit duration and medical spending for all claimants in the baseline sample.[19] In Table 4, we see the estimated coefficients relating these observable characteristics to the identifying variation are economically small and statistically indistinguishable from zero. Overall, these estimates indicate there is no relationship between the identifying variation and the composition of claimants on observable attributes. Further, we illustrate that our main results are robust to including or omitting controls for a rich set of claim characteristics.

Collectively, this evidence indicates that the increase in benefit generosity did not impact the number of claims or the composition of claimants based on observable characteristics. Given this evidence, we focus throughout on the effects of the increase in benefit generosity on the behavior of claimants conditional on filing a workers' compensation claim for income benefits and medical care.

[18]For this analysis and for subsequent analyses, we create a *Dangerous Industry* indicator variable equal to one for claimants working in agriculture, mining, construction, manufacturing, transportation, or warehousing.

[19]For the lasso models, we include age as indicator variables for ten-year age bins. We also include indicator variables for each day of the week of first medical treatment and indicator variables for wage deciles.

4 Results

4.1 Main Estimates

Benefit Duration We turn to our estimates of the impacts of income benefit generosity on income benefit duration. Table 5 displays the results from estimating Equation (4) with benefit duration as the dependent variable. Column 1 reports the baseline specification, where the dependent variable is the natural logarithm of benefit duration. The remaining columns investigate alternative specifications: a specification with fewer controls (column 2), an analogous specification in levels (column 3), a specification using the subset of claims initiated in the ED (column 4), and a specification using an expanded sample including claimants injured up to three years after the reform (column 5).

Figure 4 displays event study figures with injury-period-specific coefficients on the key exposure measure as outlined in Equation (3). Panel A displays a figure corresponding to a flexible version of the specification in Table 5 column 1. The plot shows no evidence of a trend for injuries initiated in the period prior to the reform, providing support for our parallel trends identifying assumption. For claimants injured following implementation, the income benefit durations sharply increase relative to prior claimants.

Based on the baseline specification reported in Table 5 column 1, the reform caused an 11.2% increase in the income benefit duration of workers' compensation claims among affected claimants, or 2.0 weeks relative to the pre-policy mean of 17.8 weeks. Given the 16% average increase in the replacement rate induced by the reform, this implies a benefit duration elasticity of 0.70 with a 95% confidence interval spanning 0.43 to 0.97.[20] Comparing across the specifications in Table 5, we see the estimates are similar when we vary the set of controls, focus on the subset of claims initiated through the ED, or focus on the expanded sample including claimants injured up to three years after the reform.

Medical Spending Next, we investigate the effect of the income benefit generosity on claimant medical spending. Table 6 displays the results from estimating Equation (4) where the dependent variable is either medical spending during the first five years after the injury (Panel A) or number of positive medical bills during the first five years after injury (Panel B). Like Table 5, column 1 reports the baseline specification, and the remaining columns investigate alternative specifications, as indicated within the table.

Figure 4 displays the event study figures corresponding to Equation (3). Panels B and C contain the results from more flexible versions of the specifications in Table 6 column 1. The figures show no evidence of a trend in medical spending or number of medical bills for injuries initiated in the period prior to the implementation, in line with the identification assumption. In contrast, medical spending and number of bills sharply increase for exposed claimants injured after the reform was implemented relative to the analogous prior claimants.

The baseline estimates in Table 6 column 1 indicate that the reform caused a 9.9% increase in the medical spending (within the first five years post injury) among affected claimants, or a $1,226 increase relative to the pre-policy mean of $12,461. These estimates imply that the elasticity of medical spending with respect to the income benefit rate is 0.62 with a 95% confidence interval spanning 0.37 to 0.87. The estimates are similar in alternative specifications with fewer controls, with only claims initiated through the ED, or with an expanded sample of claimants injured within a longer post-reform period.

Table 7 presents results from difference-in-differences specifications investigating subcategories of medical utilization. Columns 1 and 2 display the baseline aggregate utilization results for reference. The remain-

[20] The reported elasticity and associated confidence interval is based on an analogous instrumental variables specification. Appendix Table A4 reports the estimates for the instrumental variables specifications for each of the primary outcomes.

ing columns investigate various subcategories of medical care: office visits, case management services, physical therapy, prescription drugs, surgeries, emergency visits, and diagnostic radiology. Some categories of care appear more responsive than others, and the estimated heterogeneity largely aligns with ex ante predictions. The reform had no detectable effects on less discretionary types of care, such as emergency visits and surgeries. In contrast, the reform is associated with particularly large effects on physical therapy services (22.7% increase in spending) and case management services (18.8% increase in spending). Ex ante, we would have expected to see larger effects on categories of care that are time-intensive during business hours (such as physical therapy, office visits, case management services) to the extent that the opportunity cost mechanism is driving the effects. Further, we would have predicted that case management services would be particularly responsive, as doctors may require an increased number of case management check-ins to monitor a claimant's work capacity if he/she is on income benefits longer.

Summary The estimates above suggest that claimants substantially change their behavior—with respect to duration claiming income benefits and medical spending—when the generosity of income benefits increases. Next we discuss the effect of each margin for adjustment on insurer costs. The cost to the insurer for covering a workers' compensation claimant can be represented by:

$$Cost = D_B b + M, \tag{5}$$

where D_B is the benefit duration, b is the weekly benefit rate, and M is total claimant medical spending. The impact of a change in the benefit level on insurer costs is then:

$$\frac{dCost}{db} = D_B\big(1 + \epsilon_{D_B,b} + \frac{dM}{db}\frac{1}{D_B}\big). \tag{6}$$

The expression above depicting the total impact on insurer costs is the sum of three components. The first component is the mechanical effect: a \$1 increase in the weekly benefit will increase costs by the duration claiming income benefits (D_B). The second component is the behavioral effect due to induced changes in the duration of claiming income benefits. The third component is the behavioral effect due to induced changes in claimant medical spending.

Based on our estimates, the second component within the parenthetical expression ($\epsilon_{D_B,b}$) is 0.70, and the third component within the parenthetical expression ($\frac{dM}{db}\frac{1}{D_B}$) is 0.72.[21] There are several points worth highlighting. First, our estimates suggest that behavioral responses along the two margins of income benefit duration and medical spending are roughly equally important drivers of increased insurer costs. The point estimates for these behavioral response terms are very similar (0.70 and 0.72) and are statistically indistinguishable from one another.[22] This suggests that behavioral responses in medical spending are as important of an explanation for increased insurer costs as behavioral responses in the duration of income benefit receipt. Second, collectively across these two margins for adjustment, the magnitude of the effect of behavioral responses to benefit generosity on insurer costs is nearly 1.5 times the magnitude of the mechanical effect of benefit generosity on insurer costs. Finally, our estimates indicate that the impact of behavioral responses on insurer costs is four times the effect that would have been predicted based on most of the

[21]We obtain estimates for $\epsilon_{D_B,b}$ and $\frac{dM}{db}$ through analogous instrumental variables (IV) specifications reported in Appendix Table A4. These IV specifications effectively scale the reduced form results on benefit duration (0.112 from Table 5 column 1) and medical spending (1,225.54 from Table 6 Panel A column 3) by the first stage impact on the benefit rate in logs (0.160 from Table 3 Panel B column 1) and levels (95.42 from Table 3 Panel B column 3), respectively. We then obtain an estimate for $\frac{dM}{db}\frac{1}{D_B}$ by scaling our IV estimate of $\frac{dM}{db}$ by the mean duration of benefit receipt.

[22]We draw 1,000 bootstrap samples with replacement and estimate the IV specifications for each of these behavioral response terms. A t-test based on these boostrap estimates does not allow us to reject that these terms are equal (t-stat=0.004).

prior work on workers' compensation insurance, which has tended to find duration elasticity estimates in the range of 0.3 to 0.4 and has ignored any effects on medical spending.[23] Section 5 explores the potential implications of these estimates of behavioral responses for benefit design.

4.2 Supplemental Evidence

Below, we present supplemental evidence. First, we present evidence investigating the timing of the effects on income benefit receipt and medical spending relative to injury date. Second, we investigate heterogeneity in the estimated effects on benefit duration and medical spending across claimants. Third, we present correlational evidence illustrating how medical spending evolves around the termination of income benefits. Collectively, this supplemental evidence suggests there is a link between the observed effects on income benefit duration and medical spending, in line with many of the potential mechanisms behind behavioral responses in this setting.

Timing of Effects Let w index two-week bin relative to the injury date. Because the exact date of injury is not observed in the data (only injury month and year is included), we use date of first medical treatment as a proxy for injury date. We estimate regressions of the following form for each two-week bin, w:

$$y_{iw} = \beta_w \text{Post}_i \times \Delta_{\%} b_i\text{\_scaled} + \delta_w \Delta_{\%} b_i\text{\_scaled} + \theta_w \text{Post}_i + \alpha_w + \lambda_w^H Z_{iw} + \epsilon_{iw}, \tag{7}$$

where the vector of β_w's from these regressions represents the coefficients of interest. We investigate three dependent variables: (i) indicator for income benefit receipt in w, (ii) indicator for positive medical spending in w, and (ii) inverse hyperbolic sine of medical spending in w. Figure 5 plots these coefficients by two-week bin since injury (date of first treatment), where a vertical reference line at 104 weeks depicts the maximum potential duration of income benefits. Appendix Table A5 reports regression estimates which aggregate and summarize the effects on these outcomes over specified time horizons since injury.

Figure 5 illustrates that the timing of the effects aligns with incentives in this environment. There is little effect on income benefit receipt during the first two weeks after the date of first treatment, as for most individuals this will correspond to the waiting period for income benefits. Putting aside the first two weeks after the date of first treatment, we see that the effects on income benefit duration are relatively front loaded, with the largest effects roughly 10 to 36 weeks after the date of first treatment, with the effects declining thereafter and sharply dropping around the 104th week after the date of first treatment.

Further, Figure 5 illustrates that the timing of the effects on income benefit duration and medical spending generally align with one another. The periods with the largest effects on medical spending are also periods with the largest effects on income benefit receipt. Interestingly, the point estimates for the effects on medical spending in the long-run (more than two years post injury) are small but remain positive and statistically distinguishable from zero until four years after the injury. See Appendix Table A5. This suggests that the extra induced medical spending and time out-of-work in the short-run after an injury do not, on average, lead to less medical spending in the long-run.

Heterogeneity in Effects We investigate heterogeneity in the main effects by claimant characteristics. Table 8 reports the specifications where we split the sample on various claimant characteristics: age, impairment type, industry riskiness, and sex. In this subgroup analysis, we continue to scale the measure of

[23]To the best of our knowledge, no prior study has analyzed the effect of income benefit generosity on medical spending. A few prior papers have investigated the impact of income benefit generosity on the duration of workers' compensation income claims, largely using data and variation from the 1970s and 1980s. See Krueger and Meyer (2002) for a review of this literature. While there is some variation in prior estimates of the duration elasticity, the most commonly cited estimates imply duration elasticities in the range of 0.3 to 0.4 (e.g., Meyer, Viscusi and Durbin (1995), Neuhauser and Raphael (2004)).

exposure to the benefit change by the mean in the overall population, so it is possible to compare estimates across subgroups. There are a few patterns worth noting. First, while the subgroup estimates are often not statistically distinguishable from one another, the pattern of the point estimates suggests that the effects are more concentrated among older workers (over age 40), claimants with harder-to-diagnose injuries like sprains, and workers in less dangerous industries. Second, when comparing across subgroups, the impacts on income benefit duration and the impacts on medical spending tend to move together. That is, subgroups with larger estimated impacts on the benefit duration also tend to have larger estimated medical spending effects. The one exception is the comparison by sex, where women and men have similar income benefit duration effects but women have larger medical spending effects.

To complement this analysis, we explore heterogeneity in treatment effects using machine learning techniques and investigate the relationship between predicted treatment effects across claimants along these two margins for behavioral adjustment. For each outcome, we fit an honest causal forest to estimate the Conditional Average Treatment Effect (CATE) for each claimant based on a rich set of baseline observables (Wager and Athey (2018), Athey, Tibshirani and Wager (2019)). To analyze heterogeneity in treatment effects among claimants who receive the same treatment intensity, this analysis focuses on claimants who are "very high earners," which we define as claimants with pre-injury earnings exceeding the maximum benefit in the new benefit schedule (i.e., claimants eligible for a weekly benefit increase equal to the full 25% increase in the maximum benefit). This estimation proceeds in two stages. First, we remove injury time effects from high earner outcome data by subtracting injury month fixed effects estimated from an OLS regression on the sample of control claimants (i.e., claimants with prior earnings below the initial maximum benefit). Second, we use the de-trended outcome data for the very high earners to fit an honest causal forest for each outcome, drawing on a rich set of observable claimant characteristics to predict treatment effects including: age, day-of-the-week of injury, pre-injury average weekly earnings, sex, ED claim indicator, industry group, and impairment type.[24]

Figure 6 displays a histogram of the resulting out-of-bag CATE estimates for benefit duration (Panel A) and medical spending (Panel B). These histograms are roughly centered on the Average Treatment Effect (ATE) among these very high earners: 14.2% increase for benefit duration and 9.3% increase for medical spending. We would caution against over-interpreting these histograms, as these do not convey statistical significance of this treatment effect heterogeneity. In particular, it is possible to have a very dispersed histogram which is under-powered to detect heterogeneity or a tightly clustered histogram representing statistically meaningful heterogeneity.[25] We employ the "best linear predictor" test suggested in Chernozhukov et al. (2018) to test for the presence of heterogeneity, and we can reject the hypothesis that our estimates capture no treatment effect heterogeneity, with an associated p-value of 0.005 for the benefit duration estimates and 0.002 for the medical spending estimates.

Given there is some meaningful heterogeneity in this setting, we next turn to investigating whether treatment effect heterogeneity is correlated across outcomes. To do this, we investigate how the ATE estimates for one outcome vary across subgroups defined by the quartiles of CATE estimates for the other outcome. Figure 7 displays ATE estimates (and associated 95% confidence intervals) for each outcome for subgroups defined by quartiles of the out-of-bag CATE estimates for benefit duration (Panel A) and medical spending (Panel B). One expected pattern is apparent in this figure: the ATE estimate for an outcome

[24] We fit the honest causal forest with 15,000 trees using the R "grf" package. For more information on this estimation method, see Athey, Tibshirani and Wager (2019), Wager and Athey (2018). Also, see: `https://github.com/grf-labs/grf/blob/master/REFERENCE.md`.

[25] For example, in Figure 6 Panel A we see that a small share of claimants have a CATE estimate below zero (counter to expectations), but the average treatment effect among these claimants is statistically indistinguishable from zero.

is increasing across subgroups defined by the quartiles of the CATE estimates for that same outcome. The more interesting finding is that we see a similar pattern of increasing ATE estimates for one outcome across subgroups defined by the quartiles of the CATE estimates for the other outcome. In other words, the ATE estimate for medical spending (benefit duration) is increasing the quartile of the predicted treatment effect for benefit duration (medical spending). A Wald test for each of the four depicted series allows us to reject equality in these ATE estimates across quartiles, with a p-value for each test that is less than 0.001.

Both the standard heterogeneity analysis examining specified cuts on claimant characteristics and the treatment effect heterogeneity analysis leveraging CATE estimates suggest that claimant responses along these two margins—income benefit duration and medical spending—are positively correlated. Many of the potential mechanisms behind behavioral responses in this setting predict that claimants who are responsive to benefit generosity would change behavior along both margins. Thus, the findings from the heterogeneity analysis align with intuition and generally support the main findings.

Income Benefit Termination and Medical Spending Next, we provide supplemental evidence documenting patterns in medical spending around the termination of income benefits. Let s index time relative to the last week of income benefit receipt, where $s = 0$ during the week before the income benefit spell is complete. Let y_{is} represent the normalized utilization measure in week s for claimant i, where this measure is the claimant's utilization in week s scaled by the mean utilization across claimants during the week just prior to income benefit completion. We estimate the following regression:

$$y_{is} = \sum_{s} \beta_s \mathbb{1}(s) + \gamma_i + \epsilon_{is}, \tag{8}$$

where γ_i is a claimant fixed effect. We normalize $\beta_0 = 0$. The coefficients of interest is the vector β_s, which depicts the relationship between medical utilization and the week that income benefits are terminated. Figure 8 plots these estimates along with the associated 95% confidence intervals, where Panel A focuses on medical spending and Panel B focuses on the number of bills. Medical spending sharply drops at the termination of income benefits, where medical spending falls by roughly 60% (relative to the baseline week) by two weeks after income benefit completion. A similar pattern is observed with the number of medical bills. It is important to emphasize that these estimates represent a correlation and do not have a causal interpretation. Nevertheless, these patterns suggest a possible link between income benefit receipt and medical spending, providing further motivation for our primary analysis that investigates the casual impact of income benefit generosity on medical spending.

5 Welfare: Model and Calibration

Though our estimates indicate that there are large behavioral responses to benefit generosity, individuals likely value the consumption-smoothing benefits provided by more generous coverage and thus the estimates of behavioral responses alone are not sufficient to conclude whether increasing the generosity of benefits would improve or harm welfare. To explore the potential welfare implications of our estimates, we build on the classic Baily-Chetty framework to characterize the marginal welfare impact of increasing benefit generosity, where we adapt typical models applied in the setting of unemployment insurance (e.g., Chetty (2006), Kroft and Notowidigdo (2016)) to account for the multiple margins for adjustment within the setting of workers' compensation insurance. We begin by outlining the model we use to characterize welfare and describe the welfare formulas that can be implemented using sufficient statistics. We then present a calibration using the elasticities presented in the prior section along with an additional moment on the consumption drop experienced by workers upon workplace injury.

5.1 Model

Motivated by the near ubiquity of workers' compensation insurance coverage, this model considers the impact of the generosity of income benefits within a compulsory workers' compensation system. Below, we describe the model setup and the associated expressions representing the marginal welfare impact of increasing benefit generosity.

5.1.1 Model Setup

Agent's Problem Consider a single worker who lives for T periods, {0,...., T-1}. The worker becomes injured at time 0 with exogenous assets A_0. When the worker is out of work, the worker receives workers' compensation benefits b in each period for a maximum of B periods. If the worker is working in period t, the worker earns wage w, pays a lump-sum tax (or equivalently a premium) τ, and will continue working for $T-t$ periods. Let c_t^N denote consumption in period t if the worker is not working, and let c_t^W denote the consumption of the worker in period t if working. Let the interest rate and the agent's discount rate be zero, and we take as exogenous liquidity constraints by assuming the agent cannot deplete assets below $L < 0$ in any period.

In each period t, the individual chooses effort e_t he/she will expend to recover from the injury and return to work. The cost of expending effort is represented by the convex function $\psi(e_t)$. The individual also chooses the amount of injury-related medical spending m_t in each period, subject to constraints that depend on whether the individual is working or not working. These constraints may represent a variety of potential constraints a claimant faces including constraints imposed by the claimant's treating doctor and/or employer.

Let $V(A_t)$ denote the value function for the individual when working in period t:

$$V_t(A_t) = \max_{A_{t+1} \geq L;\ \underline{m}_t^W \leq m_t \leq \overline{m}_t^W} \quad u(A_t - A_{t+1} + w - \tau) + h_t^W(m_t) + V_{t+1}(A_{t+1}). \tag{9}$$

Let $U(A_t)$ denote the value function for the worker who has not returned to work in period t:

$$U_t(A_t) = \max_{A_{t+1} \geq L;\ \underline{m}_t^N \leq m_t \leq \overline{m}_t^N} \quad u(A_t - A_{t+1} + b) + h_t^N(m_t) + J_{t+1}(A_{t+1}), \tag{10}$$

where

$$J_t(A_t) = \max_{e} \quad e_t V_t(A_t) + (1-e_t)U_t(A_t) - \psi(e_t) \tag{11}$$

is the value of entering period t having not yet returned to work with assets A_t.[26] It is straightforward to show that the optimal effort decision solves the following first order condition:

$$\psi'(e_t) = V_t(A_t) - U_t(A_t) \tag{12}$$

[26] Note that in the dynamic problem outlined above the worker's valuation of medical spending is represented by the function $h_t^W(m)$ if working and $h_t^N(m)$ if not working, which is additively separable from the utility over non-medical consumption. If there is complementarity between utility over medical and non-medical consumption, the welfare formulas in Equations (14) and (15) would need to be modified to account for the degree of complementarity. Similarly, while the worker's valuation of leisure is not included in the dynamic problem above, the resulting welfare formulas would be the same if we had a richer model of utility that included the worker's utility of leisure as additively separable from the worker's utility over consumption.

which equates the marginal cost of effort to the marginal benefit of effort.

We define several objects to ease notation below. Let $S_t \equiv \prod_{i=0}^{t}(1-e_i)$ represent the survival function for being out-of-work on injury at least $t+1$ periods. Let $f_t \equiv \prod_{i=0}^{t-1}(1-e_i)e_t = S_{t-1}e_t$ represent the probability that the non-working spell lasts for exactly $t > 0$ periods, where $f_0 = e_0$. Let $D \equiv \sum_{t=0}^{T-1} S_t$ be the individual's expected non-working duration, and let $D_B \equiv \sum_{t=0}^{B-1} S_t$ be the individual's expected duration of collecting workers' compensation income benefits. Define the elasticity of the non-working duration with respect to the benefit level as $\epsilon_{D,b} \equiv \frac{dlogD}{dlogb}$ and the elasticity of benefit duration with respect to the benefit level as $\epsilon_{D_B,b} \equiv \frac{dlogD_B}{dlogb}$. Let $\theta \equiv \frac{D}{T}$ be the rate of non-working due to injury. Let $M = \sum_{t=0}^{T} m_t$.

Social Planner's Problem Below, we derive the marginal welfare gain from a change in the benefit level b, taking the maximum duration of workers' compensation benefits as given. The social planner's problem is to maximize the worker's expected utility at time 0 subject to agent optimization and balanced budget constraints. Agent optimization requires that the values of e_t, m_t, and A_t correspond to the agent's optimal choices based on the dynamic optimization problem outlined above. Let J_0 represent the individual's indirect utility at time 0 as a function of b and τ. Then, the planner solves:

$$\max J_0(b,\tau) \quad s.t.\ D_B b + M = (T-D)\tau \tag{13}$$

5.1.2 Marginal Welfare Impact of Increase in Generosity

Let us define a money-metric measure of welfare as $\frac{dW}{db} \equiv \frac{dJ_0}{db} / \frac{dJ_0}{dw}$. Define $\mu_t^N \equiv \frac{S_t}{D_B}$ and $\mu_t^W \equiv \frac{f_t(T-t)}{T-D}$. Under some additional assumptions, we can derive the exact marginal welfare gain and a feasible approximation:

Exact Formula *Suppose the borrowing constraint is not binding at time B. The money-metric welfare gain from raising the benefit level, b, is given by the following expression:*

$$\frac{dW}{db} = \frac{D_B}{D}\frac{\theta}{1-\theta}\left(\frac{\sum_{t=0}^{B-1}\mu_t^N u'(c_t^N) - \sum_{t=0}^{T-1}\mu_t^W u'(c_t^W)}{\sum_{t=0}^{T-1}\mu_t^W u'(c_t^W)} - \left(\epsilon_{D_B,b} + \epsilon_{D,b}\frac{\theta}{1-\theta}(1+\frac{M}{D_B b}) + \frac{dM}{db}\frac{1}{D_B}\right)\right). \tag{14}$$

Approximation *Suppose that: (i) the coefficient of relative prudence is zero ($\frac{-u'''(c)}{u''(c)}c = 0$) and (ii) the duration elasticities are equal ($\epsilon_{D_B,b} = \epsilon_{D,b}$). Then, the expression above is approximated by:*

$$\frac{dW}{db} \approx \frac{D_B}{D}\frac{\theta}{1-\theta}\left(\gamma\frac{\Delta c}{c} - \epsilon_{D_B,b} - \epsilon_{D_B,b}\frac{\theta}{1-\theta}(1+\frac{M}{D_B b}) - \frac{dM}{db}\frac{1}{D_B}\right), \tag{15}$$

where $\gamma = -\frac{u''(c)}{u'(c)}c$ is the coefficient of relative risk aversion, $\frac{\Delta c}{c} = \frac{\overline{c_W}-\overline{c_N}}{\overline{c_W}}$ is the consumption drop upon workplace injury, and $\overline{c_W} \equiv \sum_{t=0}^{T-1}\mu_t^W c_t^W$ and $\overline{c_N} \equiv \sum_{t=0}^{B-1}\mu_t^N c_t^N$ are the weighted-average consumption of the working and not working, respectively.

See Appendix Section D for a detailed derivation of these expressions.

5.2 Calibration

Next, we use the approximate formula described above in combination with our key estimated elasticities characterizing behavioral responses to benefit generosity and a few additional data moments to calibrate the marginal welfare impact of increasing the generosity of coverage for workers' compensation wage replacement benefits. Based on our estimates, $\epsilon_{D_B,b}$ is 0.70, where we scale the estimated percent change duration (in Table 5 column 1) by the percent change in the weekly benefit amount (in Table 3 Panel B column 1). Our estimates indicate that $\frac{dM}{db}$ is 12.84, where we scale the estimated change in medical spending (in Table 6 Panel A column 3) by the change in the weekly benefit amount (in Table 3 Panel B column 3). For the calibrations presented below, we approximate the out-of-work duration by the income benefit duration, $D \approx D_B$.[27] We calculate that the fraction of the covered workforce that is out-of-work due to workplace injury (θ) is approximately 0.24%, where this estimate is the product of the annual fraction of covered workers filing income-benefit eligible workers' compensation claims (0.7%, Cabral, Cui and Dworsky (2019)) and the mean duration of income benefit receipt (0.34 years).

There are two additional inputs needed in the approximation described above: the coefficient of relative risk aversion and a measure of the drop in consumption experienced by workers upon workplace injury. Our approach to the former is to estimate the marginal welfare gain under a range of plausible relative risk aversion values. For the latter, we use the estimated drop in consumption experienced by injured workers from Bronchetti (2012). Bronchetti (2012) uses data from the Health and Retirement Survey (the only dataset with information on both consumption and location of injury) to document that individuals injured at work experience approximately a 25% drop in consumption upon workplace injury.[28,29]

Table 9 reports the calibrated marginal welfare gain from a 5% increase in the weekly benefit rate on a base of $540 weekly benefit rate (the initial benefit cap prior to the reform). Each cell in this table represents a separate calibration, where the row indicates the coefficient of relative risk aversion used in the calibration ranging from one to five. Column 1 presents our baseline calibrations using our estimated elasticities. For comparison, columns 2 and 3 present some additional calibrations. Column 2 reports the analogous welfare calibrations using our estimated duration elasticity but ignoring medical spending effects—contrary to the evidence. To compare the baseline welfare calibration to analogous welfare impact implied by elasticities estimated in the prior literature, column 3 reports the associated welfare calibrations if we ignore medical spending effects and utilize the duration elasticity of 0.3 from Meyer, Viscusi and Durbin (1995), one of the

[27] Some approximation for the out-of-work duration is necessary, as our administrative data on the workers' compensation system is not linked to subsequent labor market outcomes. We think this is a reasonable first-order approximation in this setting. TDI (2015) analyzes linked Texas workers' compensation insurance data and unemployment insurance earnings records, documenting that 76% of workers' compensation income benefit recipients returned to work within six months of injury and 95% returned to work within three years of injury among those injured in 2011.

[28] To identify injured workers, Bronchetti (2012) uses a survey question "Do you have any impairment or health problem that limits the kind or amount of work that you can do?", focusing on workers who report a work-limiting injury in period t but not in period $t-1$. She concentrates on impairments that are reported to have been "caused by the nature of [the respondent's] work" and limits the sample to individuals employed in period $t-1$. She then quantifies changes in food, housing, and total consumption between survey period t and $t-1$ for respondents who experience the onset of work-related injuries and illnesses between survey period t and $t-1$. The Health and Retirement Survey (HRS) is conducted once every two years, and thus the consumption drop will represent the mean consumption drop among workers injured sometime in the last two years who are still impaired. Conceptually, this is very close to the consumption drop term in the marginal welfare impact in Equation (15) which indicates that the survival function should be used to created the weighted-average consumption drop upon workplace injury. Given that the HRS surveys respondents once every two years, it does not allow one to differentiate between workers with relatively short or long out-of-work durations to create a re-weighted mean of the consumption drop experienced by injured workers.

[29] To the best of our knowledge, Bronchetti (2012) is the only study to document the consumption drop experienced by injured workers. Bronchetti (2012) quantifies the drop in consumption experienced by injured workers upon workplace injury and investigates how this drop in consumption varies with the state workers' compensation wage replacement benefit generosity. Due to data limitations and the low frequency of workplace injuries, the size of the sample used in Bronchetti (2012) is limited to 372 injured workers. Thus, the study provides a relatively precise estimate of the level of the drop in consumption upon workplace injury and a somewhat less precise estimate of the slope—the relationship between the magnitude of this consumption drop and benefit generosity. Our analysis of the marginal welfare impact of increasing the generosity of benefits only requires an estimate of the level of the drop in consumption upon workplace injury, for which we draw on the relatively precise estimates within Bronchetti (2012). In contrast, Bronchetti (2012) uses both the level and slope consumption drop estimates to extrapolate further from the identifying variation to calculate the optimal replacement rate for workers' compensation benefits, following an approach analogous to that used by Gruber (1997) in the setting of unemployment insurance.

most commonly cited duration elasticities in the prior literature.

Consider the case when the coefficient of relative risk aversion equals to two. The baseline calibration using our estimated elasticities reported in column 1 indicates that a 5% balanced-budget increase in the weekly benefit rate would reduced each individual's ex ante utility by the equivalent of a \$0.060 weekly wage reduction. The cost associated with providing this incremental increase in benefits is approximately \$0.160 per capita, per week. Using this as a benchmark, the welfare loss associated with a 5% increase in the weekly benefit rate (in terms of an equivalent wage reduction) is 38% of the per capita cost of the extension. Comparing columns 1 and 2, we can see that much of the predicted welfare loss from increasing the generosity of benefits is attributable to the previously unexplored impact of benefit generosity on medical spending. To benchmark magnitudes, the change in the predicted welfare estimates from including the impact on medical spending is roughly equivalent to the change in the welfare estimates that would result from a three unit decrease in the coefficient of relative risk aversion, moving from $\gamma = 5$ to $\gamma = 2$. In contrast to the baseline welfare calibrations, column 3 indicates that an analogous welfare calibration relying on prior estimates of the duration elasticity and ignoring medical spending effects indicates a small welfare gain associated with increasing the generosity of coverage. In other words, relative to prior estimates, the new evidence in this paper on the behavioral responses to benefit generosity may change the prediction of whether increasing the generosity of benefits would improve welfare.

6 Conclusion

This paper investigates the impact of the generosity of wage replacement benefits on workers' compensation claims and explores the implications of these effects for benefit design. We leverage a policy change which caused a large, sharp increase in the effective wage replacement rate for time out-of-work for a subset of claimants within the Texas workers' compensation system. Our difference-in-differences estimates indicate that claimant behavior adjusts along two margins that increase insurer costs: increased duration claiming wage replacement benefits and increased medical spending. Despite the fact that medical spending has continued to grow as a share of total workers' compensation costs and now represents half of all workers' compensation benefits paid, no previous study has explored the impact of wage replacement benefits on medical benefits, and little is known about which factors influence the medical costs of claims more generally. We find the response of medical spending to increasing the generosity of wage replacement benefits is an equally important driver of increased insurer costs as the behavioral response of income benefit durations. Further, we find the benefit duration elasticity is 0.70, which is roughly twice as large as most prior estimates would have suggested. Aggregating across these two margins for adjustment, our estimates indicate that the magnitude of the effect of behavioral responses to benefit generosity on insurer costs is nearly 1.5 times the magnitude of the mechanical effect of benefit generosity on insurer costs.

To explore the potential welfare consequences of these behavioral responses, we specify a model in which claimants maximize their utility over medical and non-medical consumption by choosing the amount of medical care to engage in (subject to constraints), the effort put into returning to work, and the assets to consume each period. Using our estimated elasticities along with an estimate of the drop in consumption experienced by injured workers, this model suggests that increasing the generosity of workers' compensation wage replacement benefits would reduce welfare and that much of this welfare reduction is due to the previously unexplored impact of wage replacement benefits on medical spending. Further, under a range of common risk aversion values, the new evidence in this paper quantifying substantial behavioral responses to benefit generosity changes the prediction of whether increasing the generosity of benefits would improve welfare relative to commonly cited prior estimates of behavioral responses in workers' compensation insurance.

Overall, we find that behavioral responses are substantial in this setting, and the responses on the directly incentivized dimension (benefit duration) and indirectly affected dimension (medical spending) are equally important drivers of increased insurer costs. Further, including impacts on the indirectly affected dimension of coverage has important implications for the marginal welfare impact of increasing the generosity of wage replacement benefits in this setting. Our results suggest an important link between income benefit receipt and medical spending in the context of workers' compensation insurance. More generally, our results highlight the importance of considering the impacts on broader measures of insurer and social costs that go beyond the directly incentivized dimension when evaluating the impact of benefit generosity and optimal benefit design within social insurance programs.

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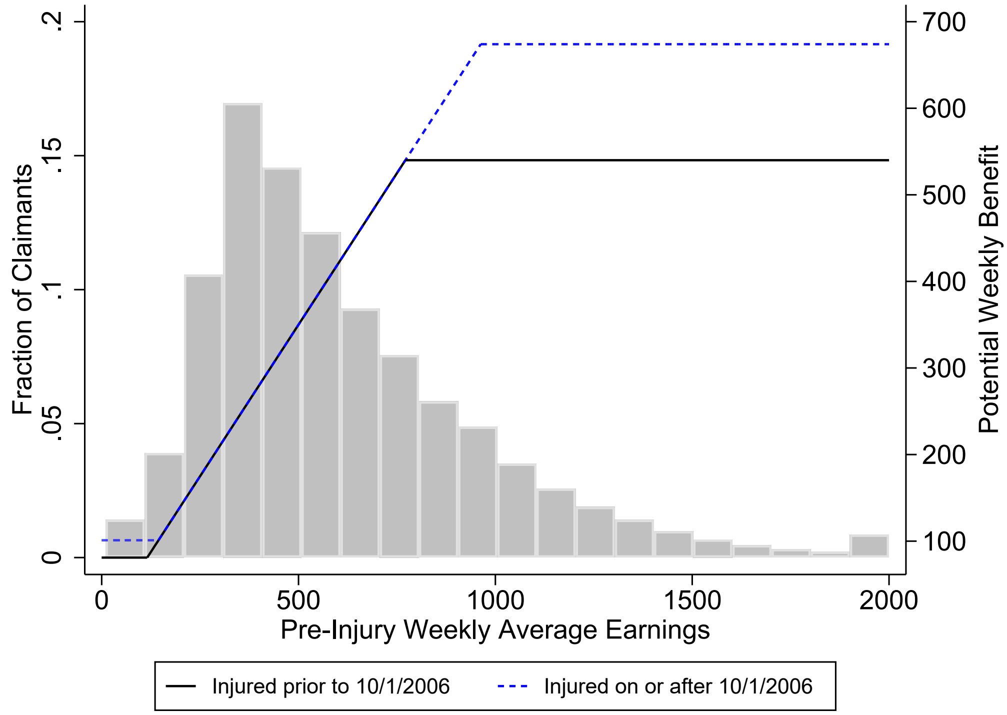
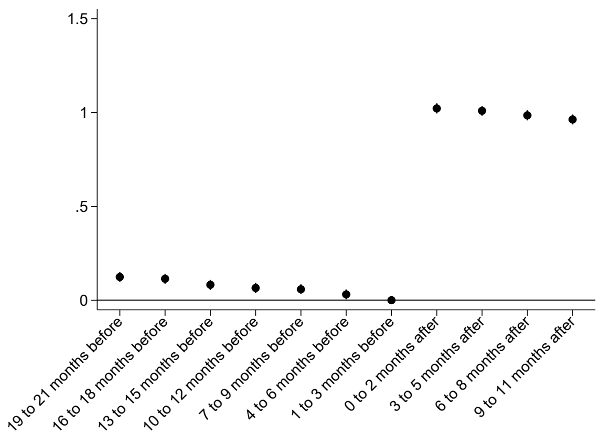
Figure 1: Weekly Benefit Rate Schedule Before and After Reform

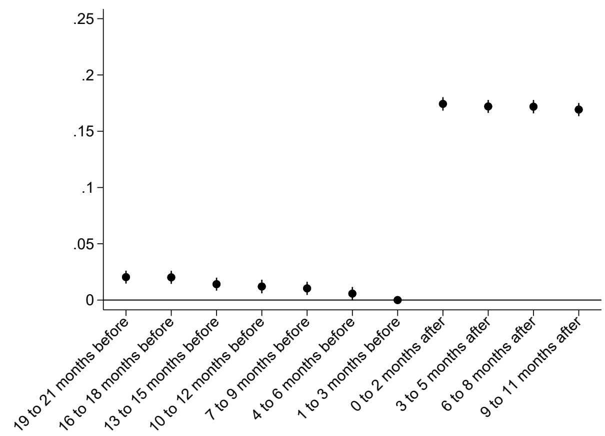


Notes: The above figure displays the benefit schedule—the mapping from pre-injury weekly earnings to potential weekly benefit— before and after the reform, along with a histogram that shows the distribution of pre-injury weekly average earnings for claimants injured from January 2005 to September 2007. The solid black line displays the benefit schedule applicable to claimants injured prior to October 2006. The dashed blue line displays the benefit schedule for claimants injured on or after October 2006.

Figure 2: Impact of Benefit Change on Benefit Rate



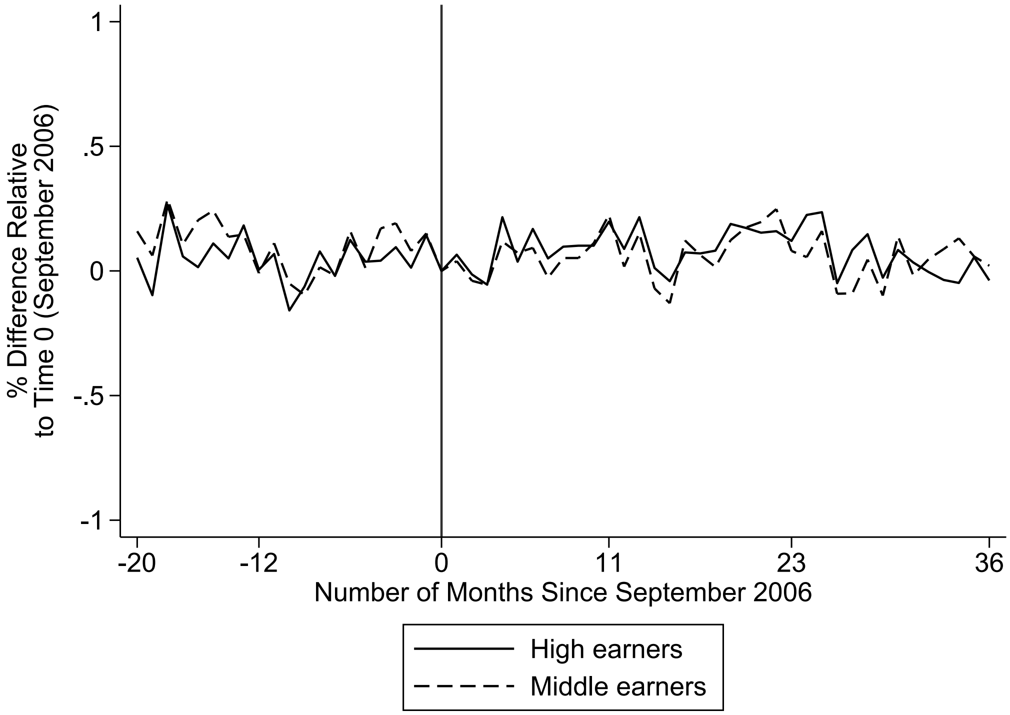
(a) Benefit Rate, Unscaled



(b) ln(Benefit Rate), Scaled

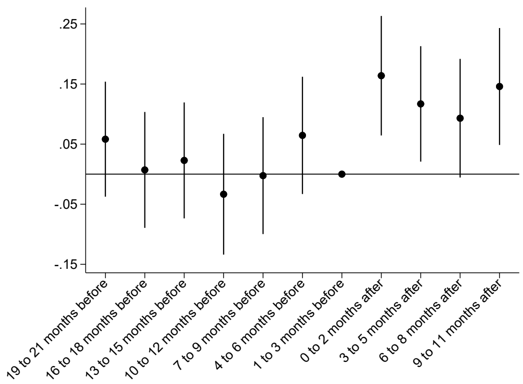
Notes: Each graph in the figure above displays coefficients on the distance-to-max or the scaled distance-to-max measure (as indicated above) interacted with time bins that indicate the number of months that the injury occurred relative to the implementation of the reform from separate regressions of Equation (3) along with 95-percent confidence intervals calculated using robust standard errors. The interaction for the time period immediately prior to the reform is omitted. The sample contains 63,883 claims that occurred from January 2005 to September 2007 that meet the sample restrictions described in the text. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's (scaled) distance-to-max, a male indicator variable, and a full vector of age indicator variables.

Figure 3: Impact of Benefit Change on Claim Rates

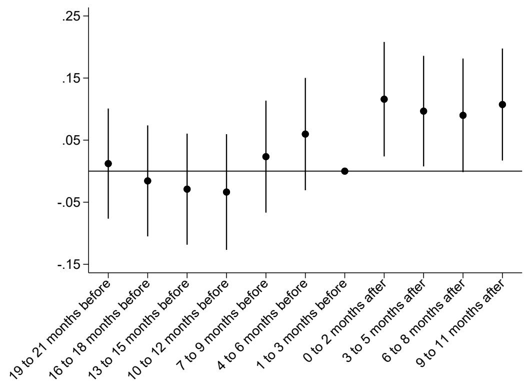


Notes: The figure above displays monthly claim rates from January 2005 to September 2009 for claimants with weekly earnings of \$540 to \$771 (those not exposed to the reform) and for claimants with weekly earnings of \$772 to \$4,000 (those exposed to the reform, for whom $\Delta b_{it} > 0$) in September 2006 dollars. Each line shows the percent difference in claims for the income group relative to the number of claims for that income group that occurred in September 2006, the month before the reform was implemented.

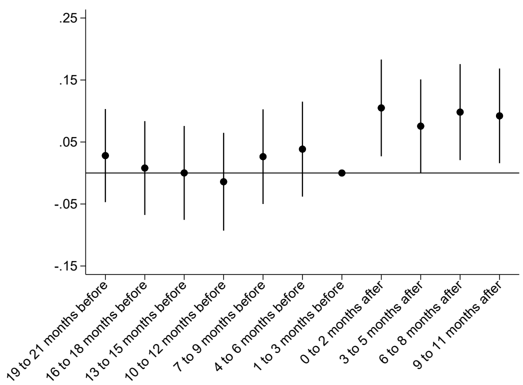
Figure 4: Impact of Benefit Change on Benefit Duration and Medical Utilization



(a) ln(Benefit Duration)



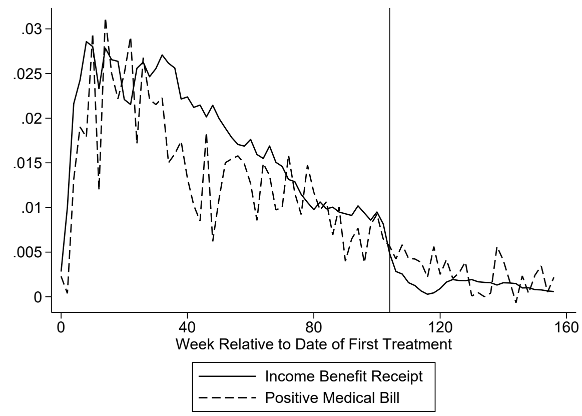
(b) ln(Medical Spending)



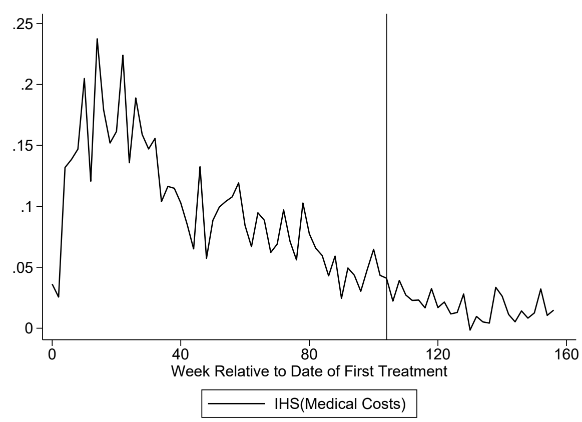
(c) ln(Number of Bills)

Notes: Each graph in the figure above displays coefficients on the scaled distance-to-max measure interacted with time bins that indicate the number of months that the injury occurred relative to the implementation of the reform from separate regressions of Equation (3) along with 95-percent confidence intervals calculated using robust standard errors. The interaction for the time period immediately prior to the reform is omitted. The sample contains 63,883 claims that occurred from January 2005 to September 2007 that meet the sample restrictions described in the text. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's (scaled) distance-to-max, a male indicator variable, and a full vector of age indicator variables.

Figure 5: Timing of Effects on Benefit Duration and Medical Utilization



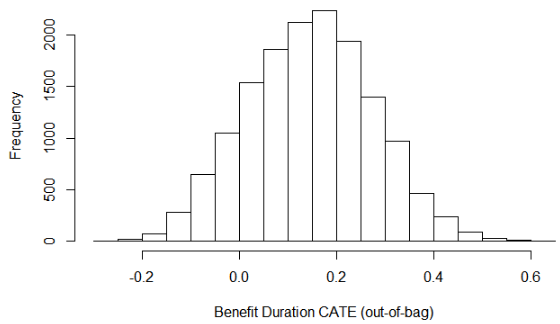
(a) Income Benefit Receipt and Positive Medical Spending



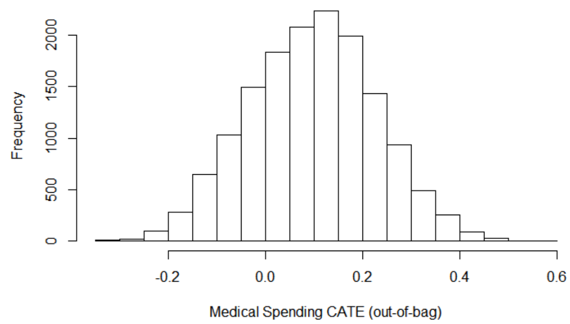
(b) Medical Spending, Inverse Hyperbolic Sine

Notes: The above figure displays the effect of the reform on claimants' receipt of income benefits and medical care for each two-week period since the injury occurred. We estimate separate regressions of the effect of the reform on income benefits and medical care for each two-week period relative to the start of the injury. To calculate time since injury, we use the first day of medical treatment as a measure of the injury date because only injury month and year are reported in the income benefit data. The graphs above plot each estimate of the coefficient on the claimant's scaled distance-to-max measure interacted with a post-reform indicator variable. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's (scaled) distance-to-max, a male indicator variable, a full vector of age indicator variables, and fixed effects for the calendar date of the two-week bin. Each regression has 63,883 observations, one for each claim that occurred from January 2005 to September 2007.

Figure 6: Heterogeneity: Distribution of Conditional Average Treatment Effect Estimates



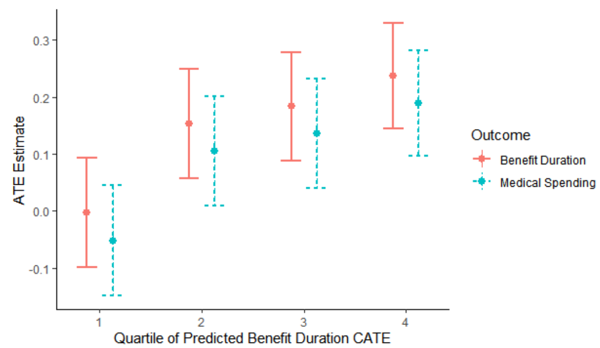
(a) Benefit Duration



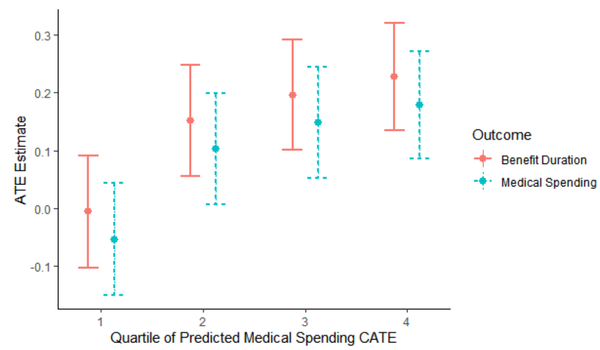
(b) Medical Spending (within 5 years of injury)

Notes: The above figure displays histograms of the out-of-bag Conditional Average Treatment Effect (CATE) estimates for benefit duration (Panel A) and medical spending (Panel B). As discussed in Section 4, we fit an honest causal forest to estimate the CATE for each claimant who is fully exposed to the reform (for whom Δb is equal to the change in the maximum benefit). The covariates used in the prediction include: age, day of the week of first medical treatment, pre-injury earnings, sex, indicator for ED claim, industry group, and impairment type.

Figure 7: Heterogeneity: Relationship Between Effects Estimated for Benefit Duration and Medical Spending



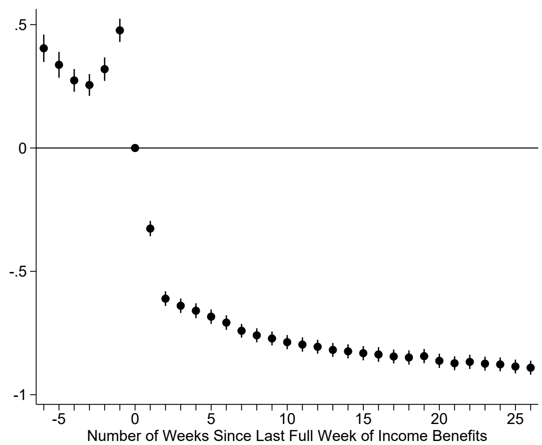
(a) Benefit Duration



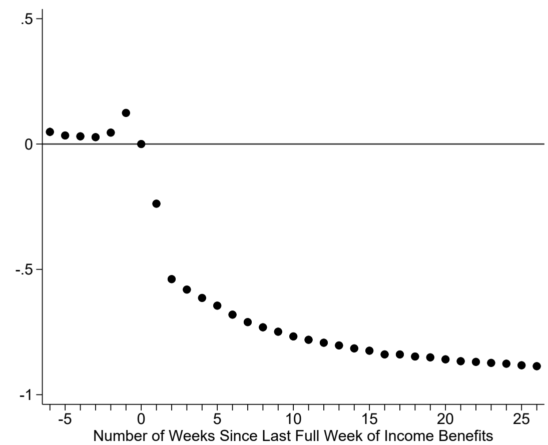
(b) Medical Spending (within 5 years of injury)

Notes: The above figure displays Average Treatment Effect estimates for each subgroup defined by the quartile of the out-of-bag Conditional Average Treatment Effect (CATE) estimates for benefit duration (Panel A) and medical spending (Panel B). As discussed in Section 4, we fit an honest causal forest to estimate the CATE for each claimant who is fully exposed to the reform (for whom Δb is equal to the change in the maximum benefit). The covariates used in the prediction include: age, day of the week of first medical treatment, pre-injury earnings, sex, indicator for ED claim, industry group, and impairment type. Within each subgroup, the figure displays the Augmented Inverse-Propensity Weighted Average Treatment Effect and associated 95% confidence interval for each outcome.

Figure 8: Medical Utilization and Income Benefit Termination



(a) Medical Spending



(b) Number of Bills

Notes: The above figure illustrates the relationship between the end of income benefits and the amount of medical care claimants receive. The data set consists of separate observations for each claimant for each week relative to the end of income benefits for 6 weeks before income benefits end until 26 weeks after income benefits end. The sample contains 2,067,851 observations from the 63,883 claims that occurred from January 2005 to September 2007. The dependent variables are normalized utilization measures for a claimant in a given week, where this measure is the claimant's utilization in the indicated week scaled by the mean utilization across claimants during the week just prior to income benefit completion (week 0). Each regression includes claim fixed effects. Each graph displays coefficients on indicator variables for the number of weeks relative to when the claimant stopped receiving income benefits along with 95-percent confidence intervals calculated using standard errors clustered at the individual level.

Table 1: Comparison of Injured Workers in Texas and All States

	Texas	All States	Texas High Earners	All States High Earners
Age	44.2	45.4	45.2	44.6
% Male	64.5%	61.3%	73.9%	71.1%
% White	81.3%	81.5%	82.5%	84.1%
% Married	58.2%	58.4%	62.8%	67.3%
% Worked last year	73.2%	68.3%	100.0%	100.0%
% Worked full time last year	65.7%	59.0%	97.9%	95.4%
Family income	$53,957	$60,919	$85,475	$91,827
Individual earnings	$20,933	$20,280	$55,438	$51,124
Weekly earnings (for weeks worked last year)	$747	$755	$1,512	$1,338
Industry Last Year (%)				
Agriculture/Forestry/Fishing/Hunting	1.3%	2.0%	2.3%	1.2%
Arts/Entertainment/Accomodation/Food Services	3.7%	6.4%	0.7%	3.1%
Finance/Real Estate/Professional Services	14.0%	11.4%	10.2%	9.6%
Health Care/Educational Services	14.8%	17.2%	4.8%	15.6%
Manufacturing	12.9%	17.6%	18.6%	18.1%
Mining/Utilities/Construction	18.5%	14.3%	28.6%	19.3%
Public Adminstration/Other Services	6.8%	6.2%	9.0%	9.9%
Wholesale Trade/Retail Trade/Transportation	28.1%	25.0%	25.7%	23.1%

Notes: This table compares the population of workers' compensation claimants in Texas and the entire United States using data from the Current Population Survey Annual Social and Economic Supplement 2002-2011 (representing years 2001-2010). The table displays summary statistics for all workers' compensation claimants in Texas (column 1) and in all states (column 2). Columns 3 and 4 display summary statistics focusing on relatively high earners based on prior earnings in Texas and all states, respectively. In this table, high earners are defined as workers who had earnings last year that exceeded $771 per week (=$540/0.7) and thus would have been marginal to the initial benefit cap in Texas had they been in our sample. All dollar values are CPI-U adjusted to 2006 dollars.

Table 2: Descriptive Statistics

	Mean	Std. Dev.	25th pctile	Median	75th pctile
Benefit duration	17.96	24.19	2.71	8.00	22.00
Medical spending (5 years)	12,503.95	18,500.01	2,157.12	6,119.97	14,236.17
Weekly benefit amount	525.05	82.86	455.00	539.09	563.83
Pre-injury weekly average earnings	880.21	330.01	640.00	794.00	1018.22
Replacement rate	0.63	0.11	0.57	0.70	0.70
ΔWeeklyBenefit	55.19	61.26	0.00	16.13	132.60
Age	42.64	11.13	34.00	43.00	51.00
1{Male}	0.78	0.41	1.00	1.00	1.00
1{Married}	0.61	0.49	0.00	1.00	1.00
Impairment Type:					
1{Contusion}	0.05	0.23	0.00	0.00	0.00
1{Fracture}	0.11	0.31	0.00	0.00	0.00
1{Laceration}	0.04	0.21	0.00	0.00	0.00
1{Muscle Issue}	0.32	0.47	0.00	0.00	1.00
1{Sprain}	0.25	0.44	0.00	0.00	1.00
1{ED Claim}	0.31	0.46	0.00	0.00	1.00

Notes: This table displays descriptive statistics for the 63,883 claims that occurred from January 2005 to September 2007 that meet the sample restrictions described in Section 2.

Table 3: Impact of Benefit Change on Weekly Benefit Rate

Panel A: Weekly Benefit Rate	(1)	(2)	(3)	(4)	(5)
ΔwkBenefit x Post	0.925	0.926	0.002	0.938	1.096
	(0.006)	(0.005)	(0.000)	(0.011)	(0.004)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Sample Restriction				ED Claims	Expanded Sample (Injured up to 3 years post reform)
Controls					
Time and ΔwkBenefit Controls	x	x	x	x	x
Basic Controls	x	x	x	x	x
Expanded Controls	x		x	x	x
Dep Var	Level	Level	Nat. Log	Level	Level
Pre-Mean Dep Var, Levels	554	554	554	553	554
N	63,883	63,883	63,883	20,018	110,269

Panel B: Weekly Benefit Rate	(1)	(2)	(3)	(4)	(5)
ΔwkBenefit_scaled x Post	0.160	0.160	95.416	0.162	0.190
	(0.001)	(0.001)	(0.614)	(0.002)	(0.001)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Sample Restriction				ED Claims	Expanded Sample (Injured up to 3 years post reform)
Controls					
Time and ΔwkBenefit Controls	x	x	x	x	x
Basic Controls	x	x	x	x	x
Expanded Controls	x		x	x	x
Dep Var	Nat. Log	Nat. Log	Level	Nat. Log	Nat. Log
Pre-Mean Dep Var, Levels	554	554	554	553	554
N	63,883	63,883	63,883	20,018	110,269

Notes: This table displays estimates of the coefficient on the distance-to-max or the scaled distance-to-max measure (as indicated above) interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (3) with the weekly benefit rate as the dependent variable. Specifications reported in columns 1 through 4 focus on claims with injury dates from January 2005 to September 2007. Specifications reported in column 5 focus on claims with injury dates from January 2005 to September 2009. Regressions in column 2 include basic controls: injury year-month fixed effects, the claimant's (scaled) distance-to-max, a male indicator variable, and a full vector of age indicator variables. In addition to these basic controls, regressions in the remaining columns also include the following controls: county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, and fixed effects for the day of the week that the claimant first received medical care. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table 4: Claimant Composition: Balance on Observable Characteristics

	ΔwkBenefit_scaled x Post			
	Coef	Std Err	P-value	Mean Dep Var
	(1)	(2)	(3)	(4)
Age	-0.213	(0.162)	[0.188]	43.63
Male	0.003	(0.006)	[0.592]	0.80
ED Claim	-0.009	(0.007)	[0.189]	0.30
Married	0.008	(0.008)	[0.273]	0.63
Impairment Type:				
Contusion	0.002	(0.003)	[0.525]	0.049
Fracture	-0.003	(0.005)	[0.472]	0.105
Laceration	-0.002	(0.003)	[0.498]	0.038
Muscle Issue	-0.001	(0.007)	[0.937]	0.342
Sprain	0.010	(0.006)	[0.126]	0.248
Log(First Day Medical Spending)	-0.002	(0.019)	[0.902]	5.933
Industry: More Dangerous	0.009	(0.007)	[0.207]	0.584
Predicted Log(Benefit Duration)	0.003	(0.002)	[0.233]	1.997
Predicted Log(Five Year Medical Spending)	0.006	(0.005)	[0.216]	8.592

Notes: This table displays estimates of the coefficient on the scaled distance-to-max measure interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (4) that control for county by injury year-month fixed effects and the claimant's scaled distance-to-max. Each row represents a separate regression with the dependent variable as indicated in the table. Column 1 displays the coefficient estimates, column 2 displays robust standard errors, column 3 displays p-values, and column 4 displays the mean of the dependent variable. In each specification, the sample includes claims that occurred from January 2005 to September 2007 that have non-missing values for the given dependent variable.

Table 5: Impact of Benefit Change on Benefit Duration

	Benefit Duration				
	(1)	(2)	(3)	(4)	(5)
ΔwkBenefit_scaled x Post	0.112	0.105	2.016	0.127	0.093
	(0.022)	(0.021)	(0.355)	(0.041)	(0.017)
	[<0.001]	[<0.001]	[<0.001]	[0.002]	[<0.001]
Sample Restriction				ED Claims	Expanded Sample (Injured up to 3 years post reform)
Controls					
Time and ΔwkBenefit Controls	x	x	x	x	x
Basic Controls	x	x	x	x	x
Expanded Controls	x		x	x	x
Dep Var	Nat. Log	Nat. Log	Level	Nat. Log	Nat. Log
Pre-Mean Dep Var, Levels	17.79	17.79	17.79	17.89	17.79
N	63,883	63,883	63,883	20,018	110,269

Notes: This table displays estimates of the coefficient on the scaled distance-to-max variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (4) with the income benefit duration as the dependent variable. Specifications reported in columns 1 through 4 focus on claims with injury dates from January 2005 to September 2007. The specification reported in column 5 focuses on claims with injury dates from January 2005 to September 2009. Regressions in column 2 include basic controls: injury year-month fixed effects, the claimant's (scaled) distance-to-max, a male indicator variable, and a full vector of age indicator variables. In addition to these basic controls, regressions in the remaining columns also include the following controls: county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, and fixed effects for the day of the week that the claimant first received medical care. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table 6: Impact of Benefit Change on Medical Spending

	Panel A: Medical Spending (cumulative in five years since injury)				
	(1)	(2)	(3)	(4)	(5)
ΔwkBenefit_scaled x Post	0.099	0.082	1225.537	0.095	0.080
	(0.020)	(0.019)	(270.891)	(0.036)	(0.016)
	[<0.001]	[<0.001]	[<0.001]	[0.009]	[<0.001]
Sample Restriction				ED Claims	Expanded Sample (Injured up to 3 years post reform)
Controls					
Time and ΔwkBenefit Controls	x	x	x	x	x
Basic Controls	x	x	x	x	x
Expanded Controls	x		x	x	x
Dep Var	Nat. Log	Nat. Log	Level	Nat. Log	Nat. Log
Pre-Mean Dep Var, Levels	12,461	12,461	12,461	14,500	12,461
N	63,883	63,883	63,883	20,018	110,269
	Panel B: Number of Bills (cumulative in five years since injury)				
	(1)	(2)	(3)	(4)	(5)
ΔwkBenefit_scaled x Post	0.079	0.070	3.348	0.077	0.058
	(0.017)	(0.016)	(0.897)	(0.031)	(0.013)
	[<0.001]	[<0.001]	[<0.001]	[0.012]	[<0.001]
Sample Restriction				ED Claims	Expanded Sample (Injured up to 3 years post reform)
Controls					
Time and ΔwkBenefit Controls	x	x	x	x	x
Basic Controls	x	x	x	x	x
Expanded Controls	x		x	x	x
Dep Var	Nat. Log	Nat. Log	Level	Nat. Log	Nat. Log
Pre-Mean Dep Var, Levels	44.12	44.12	44.12	45.70	44.12
N	63,883	63,883	63,883	20,018	110,269

Notes: This table displays estimates of the coefficient on the scaled distance-to-max variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (4) with the five-year medical spending or five-year paid medical bills as the dependent variable. Specifications reported in columns 1 through 4 focus on claims with injury dates from January 2005 to September 2007. Specifications reported in column 5 focus on claims with injury dates from January 2005 to September 2009. Regressions in column 2 include basic controls: injury year-month fixed effects, the claimant's (scaled) distance-to-max, a male indicator variable, and a full vector of age indicator variables. In addition to these basic controls, regressions in the remaining columns also include the following controls: county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, and fixed effects for the day of the week that the claimant first received medical care. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table 7: Impact of Benefit Change on Categories of Medical Spending

Panel A: Dependent Variable: Inv Hyp Sine (Measure)	Total		Office Visits		Case Management		Physical Therapy	
	Spending ($)	Bills (#)	Spending ($)	Bills (#)	Spending ($)	Bills (#)	Spending ($)	Bills (#)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔwkBenefit_scaled x Post	0.099	0.079	0.118	0.080	0.188	0.096	0.227	0.146
	(0.020)	(0.017)	(0.029)	(0.017)	(0.039)	(0.018)	(0.054)	(0.029)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Pre-Mean Dep Var, Levels	12,461	44.12	787	10.64	1,131	9.57	1,390	28.03
N	63,883	63,883	63,883	63,883	63,883	63,883	63,883	63,883

Panel B: Dependent Variable: Inv Hyp Sine (Measure)	Prescription Drugs		Surgeries		Emergency Visits		Diagnostic Radiology	
	Spending ($)	Bills (#)	Spending ($)	Bills (#)	Spending ($)	Bills (#)	Spending ($)	Bills (#)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔwkBenefit_scaled x Post	0.165	0.074	0.064	0.018	0.033	0.011	0.104	0.046
	(0.048)	(0.024)	(0.052)	(0.010)	(0.036)	(0.008)	(0.040)	(0.016)
	[0.001]	[0.002]	[0.213]	[0.075]	[0.359]	[0.182]	[0.009]	[0.005]
Pre-Mean Dep Var, Levels	1,036	12.88	355	0.84	1,146.00	1.06	765.70	6.30
N	63,883	63,883	63,883	63,883	63,883	63,883	63,883	63,883

Notes: This table displays estimates of the coefficient on the scaled distance-to-max variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (4) for different categories of medical care. In odd columns, dependent variables are the inverse hyperbolic sine of five-year medical spending for the indicated category. In even columns, dependent variables are the inverse hyperbolic sine of five-year number of bills for the indicated category. The sample includes claims that occurred from January 2005 to September 2007. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's scaled distance-to-max, a male indicator variable, and a full vector of age indicator variables. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table 8: Heterogeneity in Impacts by Claimant Characteristics

Panel A: Dependent Variable: Ln (Measure)								
	Age				Impairment Type			
	Age>=40		Age<40		Sprains and Muscle Issues		Other Injuries	
	Ben Duration	Med Spending	Ben Duration	Med Spending	Ben Duration	Med Spending	Ben Duration	Med Spending
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔwkBenefit_scaled x Post	0.124	0.110	0.093	0.088	0.110	0.117	0.079	0.031
	(0.029)	(0.026)	(0.038)	(0.035)	(0.028)	(0.026)	(0.039)	(0.037)
	[<0.001]	[<0.001]	[0.014]	[0.012]	[<0.001]	[<0.001]	[0.043]	[0.393]
Pre-Mean Dep Var, Levels	18.30	13,180	16.89	11,199	18.83	12,652	15.83	12,154
N	38,221	38,221	25,662	25,662	41,208	41,208	21,686	21,686

Panel B: Dependent Variable: Ln (Measure)								
	Industry				Sex			
	More Dangerous		Less Dangerous		Male		Female	
	Ben Duration	Med Spending	Ben Duration	Med Spending	Ben Duration	Med Spending	Ben Duration	Med Spending
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔwkBenefit_scaled x Post	0.113	0.100	0.137	0.121	0.114	0.084	0.106	0.157
	(0.031)	(0.029)	(0.034)	(0.031)	(0.025)	(0.023)	(0.051)	(0.046)
	[<0.001]	[0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[0.038]	[0.001]
Pre-Mean Dep Var, Levels	19.15	12,561	15.89	12,298	17.88	12,563	17.40	12,054
N	33,961	33,961	28,807	28,807	49,773	49,773	14,110	14,110

Notes: This table displays estimates of the coefficient on the scaled distance-to-max variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (4) for different categories of claims. In odd columns, dependent variables are the natural log of benefit duration. In even columns, dependent variables are the natural log of five-year medical spending. Each sample includes claims in the indicated category that occurred from January 2005 to September 2007. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's scaled distance-to-max, a male indicator variable, and a full vector of age indicator variables. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table 9: Marginal Welfare Impact of Increase in Benefit Rate

	Marginal Welfare Impact of Increase in Benefits, dW/db X 0.05b		
Coefficient of Relative Risk Aversion (γ)	Baseline Estimates	Baseline Duration Elasticity	Duration Elasticity from Prior Literature
	(1)	(2)	(3)
1	-$0.076	-$0.029	-$0.003
2	-$0.060	-$0.013	$0.013
3	-$0.044	$0.003	$0.029
4	-$0.028	$0.019	$0.045
5	-$0.011	$0.035	$0.061
Duration Elasticity, $\epsilon_{D,b}$	0.70	0.70	0.30
Medical Spending Derivative, dM/db	12.84	0.00	0.00

Notes: This table displays the calibrated marginal welfare impact of a balanced budget increase in the weekly benefit level by 5% of the pre-reform level of $540 per week, representing a $27 increase in the weekly benefit. The table displays quantities in terms of weekly dollars per capita. As discussed in Section 5, this calibration is based on the approximation in Equation (15) and relies on the relevant behavioral elasticity estimates, additional moments from our data, and an estimate from Bronchetti (2012) on the consumption drop experienced by injured workers upon workplace injury. Each cell represents the calibrated marginal welfare impact in a separate counterfactual. The row indicates the assumed value for the coefficient of relative risk aversion, and each column indicates the relevant duration elasticity and medical spending derivative included in the calibration. Column 1 reports calibrations based on our baseline duration and medical spending elasticities. Column 2 reports calibrations based on our duration elasticity estimate but assuming no effect on medical spending. For comparison, column 3 reports calibrations using a commonly cited duration elasticity from the prior literature (Meyer, Viscusi and Durbin (1995)) and assuming no effect on medical spending.

APPENDIX

A Additional Robustness

A.1 Coverage Rates

As discussed in Section 2, workers' compensation coverage is optional for Texas employers, while it is mandatory for most employers in other states. Nevertheless, coverage rates in Texas are high: roughly 87% of Texas workers statewide are covered compared to 97.5% of workers nationwide in 2016. Though the Texas workers' compensation system has the peculiar voluntary coverage feature, institutional details and supplementary evidence suggest that this feature is not likely to affect the internal validity of our results. We find no change in the number of claimants or the composition of claimants based on observables with respect to our identifying variation, as discussed in Section 3. Further, we investigate whether there is evidence of a differential change in firm coverage rates for firms employing workers differentially exposed to the reform. For each workers' compensation industry-occupation classification, we calculate the fraction of claimants who are "high earners", those whose pre-injury weekly earnings exceeded the initial maximum benefit, among all workers' compensation claimants. To assess whether more exposed classifications saw a differential change in coverage, we estimate a flexible difference-in-differences specification regressing the inverse hyperbolic sine of the number of workers' compensation insurance policies initiated in a given month within a classification on interactions of month relative to implementation and an indicator for the top quartile of the fraction high earner distribution of classifications. Figure A1 displays the resulting coefficients with the associated 95% confidence intervals. The figure suggests there is no evidence of a differential change in coverage rates for more exposed classifications. This lack of evidence of a correlated change in coverage rates is in line with our expectations, as we would not expect coverage decisions to adjust in the short-run because policy renewal dates are staggered throughout the calendar year and there are lags in the premium rating windows preventing regulated premiums from adjusting to higher claim costs in the short-run.[1]

A.2 Additional Specifications

Table A1 presents additional robustness analysis. The first two rows display our baseline estimates for reference. The remaining rows contain alternative specifications that help to assess the stability of our baseline estimates. First, we repeat our primary regressions re-weighting observations to be representative of workers' compensation claimants nationally along observable characteristics such as age, gender, and industry. We do this re-weighting based on propensity scores estimated using CPS data on workers' compensation claimants in Texas and nationally (described in Table 1 in the paper). The results from these re-weighted regressions are very similar to the baseline estimates. In addition, we investigate robustness when expanding the sample of claimants to include claimants with lower pre-injury earnings. The baseline sample restricts attention to claimants with pre-injury weekly earnings exceeding \$540 to focus attention on untreated claimants most similar to the treated high earner claimants. Table A1 reports specifications relaxing this restriction by including claimants with pre-injury weekly earnings exceeding \$400. The estimates using this expanded sample are very similar to the estimates using the baseline sample.

The next specifications consider different ways to account for the increase in permanent partial impairment benefit rates that permanently impaired claimants in the lower parts of the pre-injury wage distribution receive at the end of their spell of income benefits. First, we restrict the sample to claimants with a prior weekly wage above \$675, which focuses on a sample who received the same increase in the permanent impairment benefit rate. Next, we supplement Equation (4) with a control for the amount of the impairment benefit rate increase that claimants would be eligible for if they have permanent impairments, as well as with a control for this amount interacted with an indicator for the claim occurring on or after October 1, 2006. The next specification excludes anyone from the sample with a permanent impairment.

[1]The state regulates all the relative premiums in this market through industry-occupational rating and experience rating. Any differential increase in the costliness of claims for employers with high earning employees would only be reflected in a differential change in premiums with a lag due to the lags built into the rating update algorithms. In setting industry-occupational rates, the state regulator uses historical claims from a five-year window lagged by three years. In determining employer experience rating multipliers, the regulator mandates the use of a three-year window with a 21 month lag.

The final specification in Table A1 supplements Equation (4) with a control for the amount of additional benefits that claimants with permanent impairments would receive because of the increase in impairment benefits, as well as a control for this amount interacted with an indicator for the claim occurring on or after October 1, 2006. Regardless of how we treat permanently impaired claimants, our estimates of the effect of income benefits are similar to the baseline estimates.

B Effect of Permanent Impairment Benefits

As discussed in Section 2, another relevant change in the Texas workers' compensation system that occurred concurrently with the increase to the maximum temporary income benefit rate was an increase in the maximum permanent impairment benefit rate paid for each percentage point of permanent impairment after the completion of temporary income benefits. In principle, unconditional cash transfers received after the completion of the temporary income benefit spell could potentially affect the duration claiming income benefits and medical spending, if individuals are forward-looking and informed of their later eligibility for these unconditional cash benefits. Further, if individuals are sufficiently forward-looking and informed, knowing the effect of an increase in unconditional cash benefits could potentially aid in understanding whether the increase in the income benefit rates affects claimants' behavior by providing claimants increased access to cash (and hence a liquidity effect) rather than through distortions in the marginal incentives to return to work. Since permanent impairment benefit rates are capped at lower levels of pre-injury earnings than income benefits in Texas workers' compensation, our setting allows for separate identification of the effects of both policy parameters because the maximums bind for different parts of the pre-injury income distribution. Below, we provide more background on the change in permanent impairment benefit generosity and present estimates illustrating this change did not appear to impact income benefit duration and medical spending. In Appendix Section A, we present additional evidence verifying that the increase in permanent impairment benefit generosity does not confound the identification of the effect of income benefits.

Permanent impairment benefits are linear in the severity of the claimant's permanent impairment. The total unconditional cash benefits paid are a function of the claimant's pre-injury earnings (w_i) and the percentage point permanently impaired (s_i), such that:

$$\text{permanent impairment benefit} = \text{Rate}(w_i) \times s_i. \tag{16}$$

The rate at which each percentage point of permanent impairment severity is compensated, $Rate(w_i)$, is 210% of the claimant's pre-injury weekly average earnings up to a maximum benefit rate. Recall, the main focus of the paper is an increase in the maximum wage replacement benefit rate from \$540 to \$674, a reform impacting workers with pre-injury earnings exceeding \$771 (for whom the initial maximum benefit would have been binding). Coincident with this change in the maximum income benefit rate, there was a change in the maximum permanent impairment benefit rate at a lower level of the pre-injury earnings distribution: the rate increased from \$1,134 to \$1,416, meaning that permanently impaired claimants with pre-injury earnings above \$540 experienced some increase in unconditional cash impairment benefits while claimants with pre-injury earnings above \$675 experienced the full increase in unconditional cash impairment benefits.

Because permanent impairment benefit rates are capped at lower levels of pre-injury earnings than income benefits, our setting allows for separate identification of the effects of both policy parameters. We estimate difference-in-differences specifications investigating the impact of the impairment benefit change focusing on workers with some income benefits and pre-injury earnings between \$375 and \$750, meaning that none of these workers were affected by the increase in the maximum income benefit. We define exposure to the impairment benefit change in a parallel manner as we defined exposure to the income benefit change studied in the main text. In particular, we define:

$$\Delta\text{ImpairmentBenefit}_{it}\text{\_scaled} = \frac{\text{Rate}^{new}(w_{it}) - \text{Rate}^{old}(w_{it}),}{\frac{1}{|\mathcal{J}|}\sum_{i\in\mathcal{J}} \text{Rate}^{new}(w_{it}) - \text{Rate}^{old}(w_{it})}, \tag{17}$$

where $\text{Rate}^{new}(w)$ is the impairment rate for an individual with prior wage w under the new benefit schedule, $\text{Rate}^{old}(w)$ is the impairment rate for an individual with prior wage w under the old benefit schedule, w_{it} is the pre-injury average weekly wage of individual i injured in month t, and $\mathcal{J}$ represents the set of claimants exposed to the impairment rate reform ($\mathcal{J} \equiv \{i : \text{Rate}^{new}(w_{it}) - \text{Rate}^{old}(w_{it}) > 0\}$). Using this exposure measure, we estimate diffference-in-difference specifications of the following form:

$$y_{it} = \rho_t + \delta \Delta \text{ImpairmentBenefit}_{it}\text{\_scaled} + \left[\pi \times I_{t \geq t_0} \times \Delta \text{ImpairmentBenefit}_{it}\text{\_scaled}\right] + f(X_{it}) + \varepsilon_{it}. \quad (18)$$

Table A2 displays these estimates. Panel A focuses on all claimants with income benefits and pre-injury earnings between \$350 and \$750. For comparison, Panel B focuses on the subset of these claimants who *ex post* had positive impairment benefits and in these specifications we scale the exposure measure by the *ex post* permanent impairment severity rating. Specifications reported in columns 1 and 2 investigate the first stage of this reform, describing the mean effect of the reform on permanent impairment benefits paid in both percent and level terms. Columns 3 through 5 report estimates for specifications investigating whether the impairment benefit reform impacted our outcomes of interest in the main text: income benefit duration, medical spending, and number of medical bills. These estimates suggest there is no detectable impact of the reform on the outcomes of interest in our main analysis. Finally, columns 6 and 7 investigate the impact of the reform on impairment benefit claims, and there is no evidence that the reform affected the incidence or rated severity of permanent impairments.

We note that under some strong (and perhaps unrealistic) assumptions, the results in columns 3 through 5 may be viewed as a test of the importance of liquidity in this setting. To interpret this as a test of liquidity, we would need to assume that claimants anticipate upon injury whether they will be evaluated to have a permanent impairment, claimants can foresee the severity rating that will be assigned to them, and are aware of the payment rate for permanent impairments upon injury (though these benefits will not be paid for quite some time). In practice, permanent impairment severity is not assessed until the income benefit spell is complete, upon a final doctor's evaluation of the claimant's degree of permanent impairment, and there is a reasonable amount of ex ante uncertainty in these assessments. To interpret these results as a test of the importance of liquidity, one would also need to assume borrowing constraints are not binding until the completion of income benefit receipt.[2] Nevertheless, under these fairly strong assumptions, the unconditional cash benefit natural experiment could be informative about liquidity effects.

The results in Table A2 indicate that increasing the unconditional cash payment has no detectable effect on the duration claiming income benefits or medical spending. We note there a couple of possible ways to interpret these findings. First, it could be that this is a reasonable test of liquidity effects, with these findings suggesting that liquidity effects are not quantitatively important in this setting. In principle, we could use estimates of the liquidity effect as an alternative way to characterize the welfare impact of increasing income benefit generosity following the Chetty (2008) approach. In this context with no detectable liquidity effects, the Chetty (2008) approach would predict there is no consumption-smoothing benefit of additional coverage on the margin. While the implementation of the Chetty (2008) approach with no liquidity effects is somewhat degenerative, intuitively evidence of little or no liquidity effects suggests that the welfare analysis we employ using the "consumption drop" approach in Section 5 may be conservative with respect to the main finding: expanding the generosity of workers' compensation income benefits would reduce welfare. Second, it could be that the impairment benefit reform is not a reasonable test of liquidity effects because one or more of the required assumptions is not satisfied. We don't have a strong prior on which of these is a more reasonable interpretation.[3] Our preferred approach to analyze welfare is to use the consumption drop approach, which does not rely on additional strong assumptions needed to interpret the impact of permanent impairment benefits as the impact of liquidity but may be conservative with respect to the main finding in the case that these assumptions are satisfied.

[2] We note that this final assumption is employed within the derivation of the marginal welfare formulas, so this is not an extra assumption from the perspective of the welfare analysis.

[3] We note that the former interpretation—that liquidity effects are are not quantitatively important in our setting—is consistent with results from Rennane (2016), who finds no detectable liquidity effects among workers with weekly earnings exceeding \$615 (in 2006 dollars) in the context of small lump-sum payments among Oregon workers' compensation claimants with short spells lasting two to three weeks. We note that the sample and setting of the Rennane (2016) study has some important differences with our analysis of impairment benefit generosity, as that study focuses on very short duration claims, excludes claimants with any degree of permanent impairment, and interprets estimates under the assumption that borrowing is infeasible.

C Role of Alternative Sources of Medical Coverage

The primary estimates in the text indicate that the benefit change had a large impact on the medical spending covered under workers' compensation insurance. In this section, we explore whether these estimated effects represent changes in total medical spending or whether there may be complementary changes in medical expenditures paid through other sources (e.g., standard health insurance, self-pay, charity care). Workers' compensation insurance is the first payer for medical spending related to workplace injuries, regardless of income benefit receipt. Thus, all work-related medical spending should be reflected in the workers' compensation claims regardless of other sources of health insurance coverage. Still, some prior studies have documented a relationship between health insurance and workers' compensation coverage, illustrating some cost-shifting of health insurance expenditures towards workers' compensation insurance depending on the generosity in health insurance coverage (e.g., Dillender (2015), Bronchetti and McInerney (2017), Fomenko and Gruber (2019)).[4] We are not aware of any evidence pertaining to the opposite direction of causation—investigating whether workers' compensation coverage generosity impacts standard health insurer expenditures.

It is ex ante possible that the increased costs we observe from the reform could be partially offset or exacerbated by costs covered by standard health insurance, if the excess spending within workers' compensation insurance is a complement or substitute for medical spending covered by health insurance. We cannot quantify any such spillovers directly, as there is no comprehensive source of health insurer expenditure data for workers' compensation claimants. However, we explore the plausibility of spillovers with a number of empirical tests described below. Overall, we do not find any evidence for such spillovers, suggesting that the estimated change in workers' compensation medical spending likely reflects changes in aggregate medical utilization among injured workers.

C.1 Evidence from Unpaid Medical Bills

One potential mechanism for costs to be shifted from workers' compensation to other payers would be for workers' compensation insurers to deny a submitted medical bill, leaving a standard health insurer, patient, or other third party left paying the bill. A common reason for a denial would be if the bill was deemed to be unrelated to the workplace injury, but there are several other possible reasons for a denial (e.g., required documentation was missing, charge exceeded negotiated rate). Our data contain all bills, including both paid and unpaid medical bills. Some unpaid medical bills may represent medical utilization that took place but for which coverage was denied.

If the estimated effects represent a shifting of medical spending to workers' compensation insurance through a change in the bill denial rate, which could occur if workers' compensation insurers are more likely to deny payments for treatment once injured workers have returned to work, we would expect the reform to decrease the share of bills and the share of charges for which workers' compensation insurers deny payment. Table A3 repeats the baseline specification replacing the dependent variable with the inverse hyperbolic sine of the share of bills not paid and the share of charges not paid. The point estimates are small and statistically indistinguishable from zero, indicating that the reform did not lead to a change in the bill denial rate.

C.2 Evidence from Medical Procedures with Differential Monitoring

Health insurers have several tools to combat cost-shifting among procedures that are likely to involve liability from third parties, including workers' compensation insurance. One type of medical procedure subject to strict utilization review for outside sources of liability is diagnostic radiology, including costly advanced imaging such as MRIs, CT scans, and PET scans. Health insurers often require prior authorization for non-emergency diagnostic imaging. Further, upon receiving a claim for diagnostic imaging, it is common for health insurers to request further information from the patient about whether the imaging was due to an injury/accident, the location of the injury, and other potential liable parties/insurers. Collectively, these strategies to combat cost-shifting for diagnostic radiology suggest that cost-shifting among diagnostic radiology procedures may be more limited than among other types of procedures.

[4] Many have speculated that the increase in workers' compensation claims on Mondays reflects a shifting of uninsured medical expenses for off-the-job injuries to workers' compensation insurance. However, Card and McCall (1996) analyze the "first reports" of injuries filed with the Minnesota Department of Labor and find that employees with a low probability of medical coverage are no more likely to report Monday injuries than others.

If the reform increased medical spending for workers' compensation insurers merely because workers' compensation insurers are less aggressive about cost shifting when injured workers delay returning to work, we would not expect to see effects of the reform on types of procedures that health insurers strictly monitor to combat cost-shifting, since workers' compensation insurers would have been unlikely to have been able to shift the costs of these procedures onto health insurers prior to the reform. Table A3 displays the results for the baseline specification replacing the dependent variable with the number of diagnostic radiology claims or spending on diagnostic radiology, as well as the baseline results for the overall number of claims and overall spending. The estimated impact of the reform is similar for procedures differentially subject to monitoring by health insurers to combat cost-shifting.

D Welfare Formulas

D.1 Derivation of Exact Formula

Below, we describe the derivation of the exact welfare formula. The general strategy and notation draw upon previous work by Chetty (2006) and Kroft and Notowidigdo (2016). First, consider the effect of an incremental increase in the weekly benefit level on the value at time 0 upon workplace injury:

$$\begin{aligned} \frac{dJ_0}{db} &= (1-e_0)\frac{\partial U_0}{\partial b} + e_0\frac{\partial V_0}{\partial b} - \frac{\partial \tau}{\partial b}\big((1-e_0)\frac{\partial U_0}{\partial w} + e_0\frac{\partial V_0}{\partial w}\big) \\ &= (1-e_0)\frac{\partial U_0}{\partial b} - \frac{\partial \tau}{\partial b}\frac{dJ_0}{dw}. \end{aligned} \tag{19}$$

As defined in text, let $S_t \equiv \prod_{i=0}^{t}(1-e_t)$ represent the probability of being out-of-work on injury at least $t+1$ periods, and let $f_t \equiv \prod_{i=0}^{t-1}(1-e_i)e_t = S_{t-1}e_t$ represent the probability of being out-of-work on injury for exactly $t > 0$ periods, where $f_0 = e_0$.

Next, consider the effect of an incremental increase in the weekly wage upon return to work on the value at time 0 upon workplace injury:

$$\begin{aligned} \frac{dJ_0}{dw} &= (1-e_0)\frac{\partial U_0}{\partial w} + e_0\frac{\partial V_0}{\partial w} \\ &= \sum_{t=0}^{T-1} f_t(T-t)u'(c_t^W). \end{aligned} \tag{20}$$

The effect of an incremental increase in the weekly benefit level on the value of not returning to work at the beginning of period 0 can be characterized as:

$$\begin{aligned} (1-e_0)\frac{dU_0}{db} &= \sum_{t=0}^{B-1}\prod_{i=0}^{t}(1-e_t)u'(c_t^N) \\ &= \sum_{t=0}^{B-1} S_t u'(c_t^N). \end{aligned} \tag{21}$$

Lastly, the effect of a marginal increase in the weekly benefit level on the tax rate can be represented as:

$$\frac{d\tau}{db} = \frac{D_B}{T-D}\big[1 + \epsilon_{D_B,b} + \frac{dM}{db}\frac{1}{D_B} + \epsilon_{D,b}\frac{D}{T-D}(1+\frac{M}{D_B})\big]. \tag{22}$$

Using expressions (1) through (4) above, we can derive the money-metric welfare gain of increasing the generosity of benefits as follows:

$$
\begin{aligned}
\frac{dW}{db} &= \frac{\frac{dJ_0}{db}}{\frac{dJ_0}{dw}} \\
&= \frac{(1-e_0)\frac{\partial U_0}{\partial b}}{\frac{dJ_0}{dw}} - \frac{\partial \tau}{\partial b} \\
&= \frac{(1-e_0)\frac{\partial U_0}{\partial b}}{\frac{dJ_0}{dw}} - \frac{D_B}{T-D}\big[1+\epsilon_{D_B,b} + \frac{dM}{db}\frac{1}{D_B} - \epsilon_{D,b}\frac{D}{T-D}(1+\frac{M}{D_B})\big] \\
&= \frac{D_B}{T-D}\left\{ \frac{\frac{(1-e_0)}{D_B}\frac{\partial U_0}{\partial b} - \frac{1}{T-D}\frac{dJ_0}{dw}}{\frac{1}{T-D}\frac{dJ_0}{dw}} - \big[\epsilon_{D_B,b} + \frac{dM}{db}\frac{1}{D_B} + \epsilon_{D,b}\frac{D}{T-D}(1+\frac{M}{D_B})\big] \right\} \\
&= \frac{D_B}{T-D}\left\{ \frac{\sum_{t=0}^{B-1}\frac{S_t}{D_B}u'(c_t^N) - \sum_{t=0}^{T-1}\frac{f_t(T-t)}{T-D}u'(c_t^W)}{\sum_{t=0}^{T-1}\frac{f_t(T-t)}{T-D}u'(c_t^W)} - \big[\epsilon_{D_B,b} + \frac{dM}{db}\frac{1}{D_B} + \epsilon_{D,b}\frac{D}{T-D}(1+\frac{M}{D_B})\big] \right\} \\
&= \frac{D_B}{T-D}\left\{ \frac{\sum_{t=0}^{B-1}\mu_t^N u'(c_t^N) - \sum_{t=0}^{T-1}\mu_t^W u'(c_t^W)}{\sum_{t=0}^{T-1}\mu_t^W u'(c_t^W)} - \big[\epsilon_{D_B,b} + \frac{dM}{db}\frac{1}{D_B} + \epsilon_{D,b}\frac{D}{T-D}(1+\frac{M}{D_B})\big] \right\}.
\end{aligned}
$$

D.2 Derivation of Approximate Formula

We approximate the exact formula using approximations outlined in Chetty (2006) and Kroft and Notowidigdo (2016). For convenience, we describe these approximation strategies below in more detail.

To simplify the exact formula, we begin with the term $\sum_{t=0}^{B-1}\mu_t^N u'(c_t^N)$ and take a second-order Taylor approximation of u' around $\overline{c_N} \equiv \sum_{t=0}^{B-1}\mu_t^N c_t^N$:

$$u'(c_t^N) \approx u'(\overline{c_N}) + u''(\overline{c_N})(c_t^N - \overline{c_N}) + \frac{1}{2}(c_t^N - \overline{c_N})^2.$$

Plugging this into the expression above, we get:

$$
\begin{aligned}
\sum_{t=0}^{B-1}\mu_t^N u'(c_t^N) &\approx u'(\overline{c_N})\Big(1 + \frac{1}{2}\frac{u'''(\overline{c_N})}{u'(\overline{c_N})}\sum_{t=0}^{B-1}\mu_t^N(c_t^N - \overline{c_N})^2\Big) \\
&= u'(\overline{c_N})\Bigg(1 + \frac{1}{2}\Big(\overline{c_N}\frac{u''(\overline{c_N})}{u'(\overline{c_N})}\Big)\Big(\overline{c_N}\frac{u'''(\overline{c_N})}{u''(\overline{c_N})}\Big)\sum_{t=0}^{B-1}\frac{\mu_t^N(c_t^N - \overline{c_N})^2}{\overline{c_N}^2}\Bigg) \\
&= u'(\overline{c_N})\Big(1 + \frac{1}{2}\gamma\rho\phi_N^2\Big),
\end{aligned}
$$

where γ is the coefficient of relative risk aversion, ρ is the coefficient of relative prudence, and $\phi_N^2 = \sum_{t=0}^{B-1}\frac{\mu_t^N(c_t^N - \overline{c_N})^2}{\overline{c_N}^2}$ is a measure of the variation in consumption. We can perform analogous Taylor approximation for $\sum_{t=0}^{T-1}\mu_t^W u'(c_t^W)$ around $\overline{c_W} \equiv \sum_{t=0}^{T-1}\mu_t^W c_t^W$.

If $\rho = 0$, the exact formula for the marginal welfare impact of a benefit increase is approximated by

$$\frac{dW}{db} \approx \frac{D_B}{T-D}\Bigg[\frac{u'(\overline{c_N}) - u'(\overline{c_W})}{u'(\overline{c_W})} - \big[\epsilon_{D_B,b} + \frac{dM}{db}\frac{1}{D_B} + \epsilon_{D,b}\frac{D}{T-D}(1+\frac{M}{D_B})\big]\Bigg].$$

Further, assuming that $\epsilon_{D_B,b} = \epsilon_{D,b}$ and applying the first-order approximation in Chetty (2006), we obtain the approximate formula in the paper:

$$\frac{dW}{db} \approx \frac{D_B}{D}\frac{\theta}{1-\theta}\left(\gamma\frac{\Delta c}{c} - \epsilon_{D_B,b} - \epsilon_{D_B,b}\frac{\theta}{1-\theta}(1+\frac{M}{D_B b}) - \frac{dM}{db}\frac{1}{D_B}\right),$$

where $\theta \equiv \frac{D}{T}$ and $\frac{\Delta c}{c} \equiv \frac{\overline{c_N} - \overline{c_W}}{\overline{c_W}}$.

Table A1: Additional Robustness

	ΔwkBenefit_scaled x Post coef	std error	p-value	Pre-mean dep var	N
Baseline					
Benefit Duration	0.112	(0.022)	[<0.001]	17.79	63,883
Medical Spending	0.099	(0.020)	[<0.001]	12461	63,883
Re-Weighting based on Demographics					
Benefit Duration	0.114	(0.022)	[<0.001]	17.65	63,883
Medical Spending	0.104	(0.021)	[<0.001]	12342	63,883
Restrict Sample to Prior Wage > 400					
Benefit Duration	0.095	(0.020)	[<0.001]	17.79	90,342
Medical Spending	0.104	(0.018)	[<0.001]	12431	90,342
Restrict Sample to Prior Wage > 675					
Benefit Duration	0.084	(0.028)	[0.002]	17.79	44,884
Medical Spending	0.061	(0.026)	[0.018]	12461	44,884
Additional Control for PIB Reform					
Benefit Duration	0.102	(0.025)	[<0.001]	17.79	63,883
Medical Spending	0.077	(0.024)	[0.001]	12461	63,883
Restrict sample to those without permanent impairment					
Benefit Duration	0.079	(0.028)	[0.004]	9.983	35,931
Medical Spending	0.066	(0.027)	[0.013]	7355	35,931
Additional Control for PIB Reform (ΔImpairmentBenefit_scaled x PIB rating)					
Benefit Duration	0.116	(0.021)	[<0.001]	17.79	63,883
Medical Spending	0.100	(0.019)	[<0.001]	12461	63,883

Notes: This table displays estimates of the coefficient on the scaled distance-to-max variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (4) with the natural logarithm of benefit duration or five-year medical spending as the dependent variable. Column 1 displays the coefficient estimates, column 2 displays robust standard errors, column 3 displays p-values, and column 4 displays the mean of the dependent variable. The baseline sample includes claims that occurred from January 2005 to September 2007. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's scaled distance-to-max, a male indicator variable, and a full vector of age indicator variables.

Table A2: Effect of Permanent Impairment Cash Benefits

	Panel A: Effect of Impairment Rate Increase						
	Impairment Benefit Rate	Total Impairment Benefits	Benefit Duration	Medical Spending	Number of Bills	Impairment Rating	Impairment Benefits > 0
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔImpairmentBenefit_scaled x Post	0.126	411.369	-0.032	0.019	0.015	0.001	0.003
	(0.001)	(82.400)	(0.022)	(0.020)	(0.017)	(0.017)	(0.007)
	[<0.001]	[<0.001]	[0.154]	[0.338]	[0.375]	[0.953]	[0.709]
Dep Var	nat. log	level	nat. log	nat. log	nat. log	inv. hyp. sine	indicator
Pre-Mean Dep Var, Levels	377	3141	18.05	12642	47.06	2.778	0.439
N	61,167	61,167	61,167	61,167	61,167	61,167	61,167
	Panel B: Effect of Impairment Benefit Increase, Scaled by Impairment Severity among Permnanently Impaired Claimants						
	Impairment Benefit Rate	Total Impairment Benefits	Benefit Duration	Medical Spending	Number of Bills	Impairment Rating	
	(1)	(2)	(3)	(4)	(5)	(6)	
ΔImpairmentBenefit_scaled x PIB rating x Post	0.132	1,124.567	-0.032	0.050	0.042	0.020	
	(0.003)	(213.856)	(0.041)	(0.031)	(0.029)	(0.031)	
	[<0.001]	[<0.001]	[0.445]	[0.106]	[0.154]	[0.514]	
Dep Var	nat. log	level	nat. log	nat. log	nat. log	inv. hyp. sine	
Pre-Mean Dep Var, Levels	376.9	7162	28.47	20068	72.98	6.333	
N	25,489	25,489	25,489	25,489	25,489	25,489	

Notes: This table displays estimates of the coefficient on the scaled distance-to-impairment-max variable (as defined in the appendix text) interacted with an indicator that the injury occurred after the implementation of the new impairment benefit schedule from regressions of Equation (18) for the indicated dependent variables. The sample includes claims that occurred from January 2005 to September 2007 for claimants with pre-injury weekly wages of \$375 to \$750. Panel A displays the estimates for the full sample and constructs the distance-to-impairment-max variable based on claimants' pre-injury weekly wages. Panel B displays the estimates for the sample with permanent impairments and constructs the distance-to-impairment-max variable based on claimants' impairment ratings and pre-injury weekly wages. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's scaled distance-to-impairment-max variable, a male indicator variable, and a full vector of age indicator variables. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table A3: Alternative Sources of Medical Coverage

	ΔwkBenefit_scaled x Post			Pre-mean dep	N
	coef	std error	p-value	var	
Unpaid Bills					
Share of Bills Not Paid	0.001	(0.002)	[0.797]	0.117	63883
Share of Charges Not Paid	-0.003	(0.004)	[0.395]	0.480	63882
Differential Monitoring of Procedures					
All Medical Care					
Number of Bills	0.079	(0.017)	[<0.001]	44.12	63883
Spending	0.099	(0.020)	[<0.001]	12461	63883
Diagnostic Radiology					
Number of Bills	0.046	(0.016)	[0.005]	6.301	63883
Spending	0.104	(0.040)	[0.009]	765.7	63883

Notes: This table displays estimates of the coefficient on the scaled distance-to-max variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (4) with the dependent variables being the natural logarithm or inverse hyperbolic sine of the indicated variables. The sample includes claims that occurred from January 2005 to September 2007. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's scaled distance-to-max, a male indicator variable, and a full vector of age indicator variables. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table A4: Instrumental Variables Specifications for Primary Outcomes

	ln(Ben Duration) (1)	ln(Med Spending) (2)	ln(Num Med Bills) (3)	Ben Duration (4)	Med Spending (5)	Num of Med Bills (6)
ln(Weekly Benefit)	0.699	0.623	0.496			
	(0.138)	(0.128)	(0.108)			
	[<0.001]	[<0.001]	[<0.001]			
Weekly Benefit				0.021	12.844	0.035
				(0.004)	(2.840)	(0.009)
				[<0.001]	[<0.001]	[<0.001]
Controls						
Time and ΔwkBenefit Controls	x	x	x	x	x	x
Basic Controls	x	x	x	x	x	x
Expanded Controls	x	x	x	x	x	x
Pre-Mean Dep Var, Levels	17.79	12461	44.12	17.79	12461	44.12
N	63,883	63,883	63,883	63,883	63,883	63,883

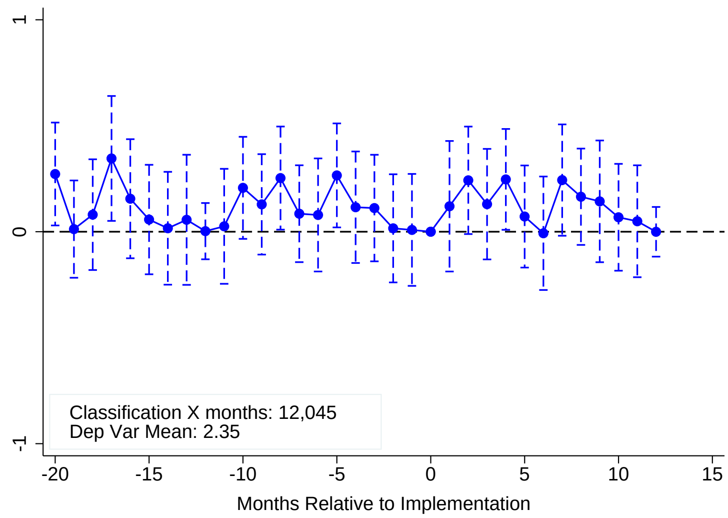
Notes: This table displays estimates from instrumental variables (IV) specifications for the primary outcomes, using the baseline sample and baseline set of controls. The instrument for the weekly benefit rate (in logs and levels) is the scaled distance-to-max variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule. The IV estimates in columns (1) through (3) display elasticites for the primary outcomes with respect to the weekly benefit rate; IV estimates in columns (4) through (6) display the derivative for the primary outcomes with respect to the weekly benefit rate. The sample includes claims that occurred from January 2005 to September 2007. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's scaled distance-to-max, a male indicator variable, and a full vector of age indicator variables. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table A5: Effect of Benefit Change over Different Horizons

Panel A: Dependent Variable: Inv Hyp Sine (Weeks Receiving Income Benefits)	0-3 months (1)	4-6 months (2)	6-12 months (3)	13-18 months (4)	19-24 months (5)	25-36 months (6)	37-48 months (7)	49-60 months (8)
ΔwkBenefit_scaled x Post	0.053	0.078	0.104	0.066	0.043	0.010	0.003	0.000
	(0.017)	(0.021)	(0.022)	(0.016)	(0.012)	(0.005)	(0.002)	(0.001)
	[0.002]	[<0.001]	[<0.001]	[<0.001]	[0.001]	[0.030]	[0.065]	[0.637]
Pre-Mean Dep Var, Levels	6.045	3.750	4.214	2.020	1.081	0.132	0.013	0.008
N	63,883	63,883	63,883	63,883	63,883	63,883	63,883	63,883
Panel B: Dependent Variable: Inv Hyp Sine (Number of Bills)	0-3 months (1)	4-6 months (2)	6-12 months (3)	13-18 months (4)	19-24 months (5)	25-36 months (6)	37-48 months (7)	49-60 months (8)
ΔwkBenefit_scaled x Post	0.034	0.099	0.100	0.078	0.064	0.036	0.020	0.007
	(0.013)	(0.022)	(0.024)	(0.020)	(0.018)	(0.017)	(0.014)	(0.012)
	[0.007]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[0.037]	[0.150]	[0.556]
Pre-Mean Dep Var, Levels	16.690	6.950	8.066	4.300	2.862	3.200	1.949	1.466
N	63,883	63,883	63,883	63,883	63,883	63,883	63,883	63,883
Panel C: Dependent Variable: Inv Hyp Sine (Medical Spending)	0-3 months (1)	4-6 months (2)	6-12 months (3)	13-18 months (4)	19-24 months (5)	25-36 months (6)	37-48 months (7)	49-60 months (8)
ΔwkBenefit_scaled x Post	0.050	0.240	0.210	0.179	0.198	0.121	0.061	0.021
	(0.019)	(0.057)	(0.061)	(0.054)	(0.047)	(0.043)	(0.034)	(0.029)
	[0.009]	[<0.001]	[0.001]	[0.001]	[<0.001]	[0.005]	[0.071]	[0.461]
Pre-Mean Dep Var, Levels	5298.000	1955.000	2256.000	1148.000	769.700	842.300	497.800	390.500
N	63,883	63,883	63,883	63,883	63,883	63,883	63,883	63,883

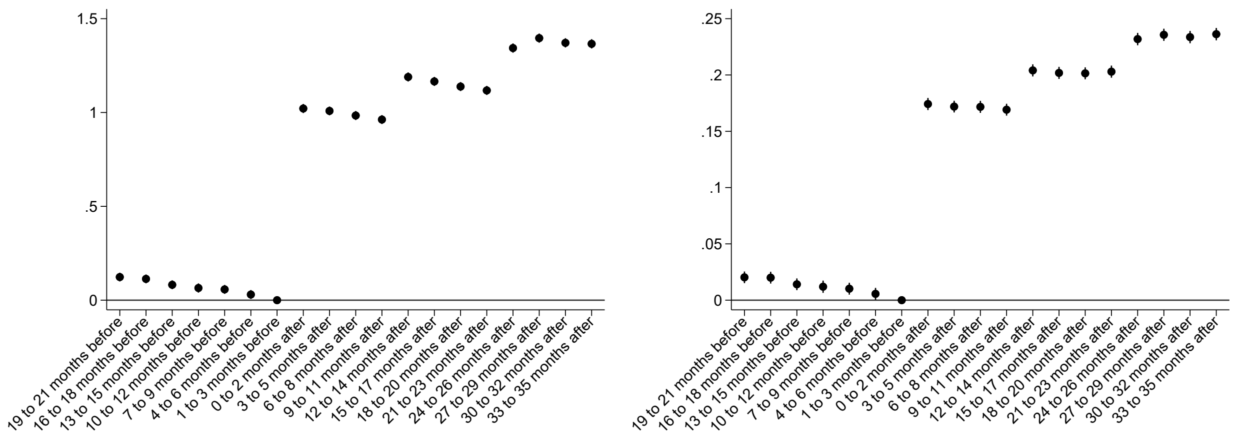
Notes: This table displays estimates of the coefficient on the scaled distance-to-max variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (4) for the indicated dependent variables. The sample includes claims that occurred from January 2005 to September 2007. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's scaled distance-to-max, a male indicator variable, and a full vector of age indicator variables. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Figure A1: Exposure to Reform and Coverage Rates



Notes: This figure reports the resulting coefficients and associated 95% confidence intervals from a difference-in-differences specification regressing the number of insurance policies initiated within a industry-occupation classification in a given month on month indicators interacted with an indicator for the top quartile of the distribution of fraction high earners among classifications. In this regression, we normalize the coefficient to zero for the month of September 2006, the month prior to the implementation of the new benefit schedule. Observations are at the classification-month level, and the dependent variable is the inverse hyperbolic sine of the number of new policies originated in that month. Robust standard errors are clustered at the industry-occupation classification level.

Figure A2: Impact of Benefit Change on Benefit Rate [Expanded Sample]

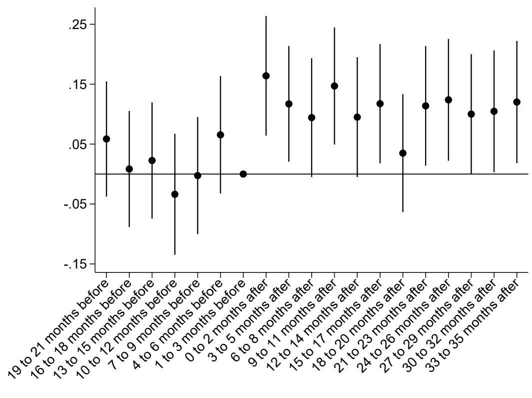


(a) Benefit Rate, Unscaled

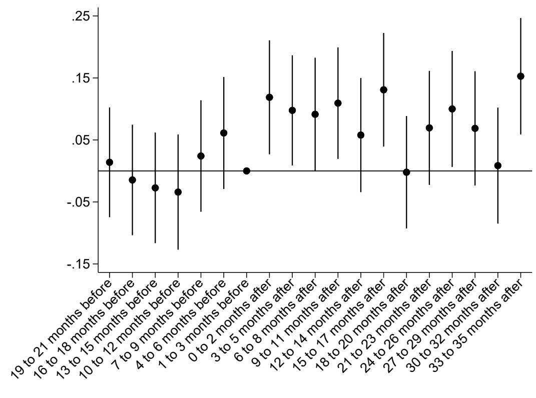
(b) ln(Benefit Rate), Scaled

Notes: Each graph in the figure above displays coefficients on the distance-to-max or the scaled distance-to-max measure (as indicated above) interacted with time bins that indicate the number of months that the injury occurred relative to the implementation of the reform from separate regressions of Equation (3) along with 95-percent confidence intervals calculated using robust standard errors. The interaction for the time period immediately prior to the reform is omitted. The sample contains 110,269 claims that occurred from January 2005 to September 2009 that meet the sample restrictions described in the text. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's (scaled) distance-to-max, a male indicator variable, and a full vector of age indicator variables.

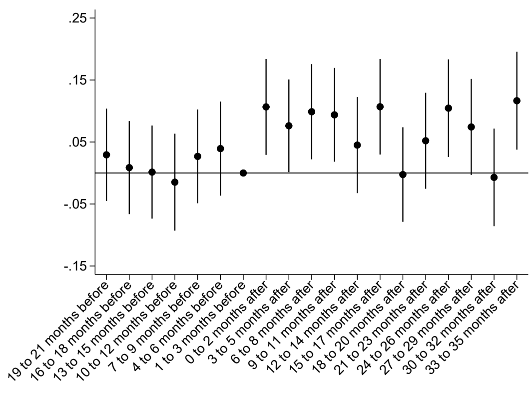
Figure A3: Impact of Benefit Change on Benefit Duration and Medical Utilization [Expanded Sample]



(a) ln(Benefit Duration)



(b) ln(Medical Spending)



(c) ln(Number of Bills)

Notes: Each graph in the figure above displays coefficients on the scaled distance-to-max measure interacted with time bins that indicate the number of months that the injury occurred relative to the implementation of the reform from separate regressions of Equation (3) along with 95-percent confidence intervals calculated using robust standard errors. The interaction for the time period immediately prior to the reform is omitted. The sample contains 110,269 claims that occurred from January 2005 to September 2009 that meet the sample restrictions described in the text. Each regression includes county by injury year-month fixed effects, an indicator variable equal to one if the claim began in the ED, fixed effects for the day of the week that the claimant first received medical care, the claimant's (scaled) distance-to-max, a male indicator variable, and a full vector of age indicator variables.