The Impact of Benefit Generosity on Workers' Compensation Claims: Evidence and Implications*

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Abstract

Leveraging unique administrative data and a sharp increase in benefit generosity in a difference-in-differences research design, we estimate the impact of workers' compensation wage replacement benefits on individual behavior and program costs. We find that increased benefit generosity leads to longer income benefit durations and increased medical spending. Responses along these two margins are equally important drivers of increased program costs, collectively increasing program costs 1.4 times the mechanical increase in costs. Using these estimates and an estimate of the consumption drop among injured workers, our welfare calibrations suggest that a marginal increase in benefit generosity would not improve welfare.

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Social insurance programs are ubiquitous and cover some of the largest risks individuals face. 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 welfare implications of the estimated behavioral responses for workers' compensation benefit design.

Workers' compensation insurance is a large, state-regulated program that is the primary source of insurance for lost wages and medical expenses associated with workplace injuries in the U.S.[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 an injury, partial wage replacement for the duration of time out-of-work due to an injury, and additional cash indemnity benefits in specified circumstances such as permanent impairments or workplace fatalities. In 2016, workers' compensation insurance paid $62 billion in benefits in the U.S., which was nearly twice the $32 billion paid in benefits for unemployment insurance that year and was equivalent in size to the Earned Income Tax Credit program and the Supplemental Nutrition Assistance Program.[2] Relative to many other large public programs, there has been less research on the impact of benefit design in workers' compensation insurance.

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 $551 in Mississippi to $2,081 in Iowa in 2023. Recently, several states have moved to increase their maximum weekly benefit level. For instance, thirteen states have enacted reforms increasing their maximum benefit levels by at least 10% (and up to 27%) in the last three years.[3] Despite the importance of workers' compensation insurance and the centrality of wage replacement benefits in current policy debates, there is limited recent evidence on the impact of workers' compensation wage replacement benefits on claimant behavior and program costs.

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

[1]While workers' compensation insurance covers both on-the-job injuries and illnesses related to occupational exposure, throughout we refer to both as injuries for simplicity.

[2]The federal Earned Income Tax Credit paid out $66.7 billion in benefits in 2016 (United States Internal Revenue Service, 2020). The Supplemental Nutrition Assistance Program paid out $66.5 billion in benefits in 2016 (United States Department of Agriculture, 2020). Workers' compensation insurance paid out $61.9 billion in benefits in 2016 (McLaren, Baldwin and Boden, 2018). Unemployment Insurance paid out $31.7 billion in benefits in 2016 (United States Department of Labor, 2020).

[3]See Social Security Administration (2023) for a summary of the maximum benefit level in each state over time.

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 which increased the weekly benefit cap by 25% 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 amount by approximately 16% on average among injured workers with prior earnings above 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 by injury date 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 or the composition of claimants based on observable characteristics (e.g., demographic, industry, and injury 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 and medical benefits. Specifically, we focus on two primary outcomes: the income benefit duration and insurers' medical spending for claimants. An important feature of workers' compensation insurance is that claimants have a "treating doctor" who acts as the gatekeeper for both medical care and income benefits received by the injured worker. Given details of this setting, there are several channels through which income benefit generosity may impact medical spending. For instance, claimants motivated to stay on income benefits longer may report more severe symptoms to their treating doctor, which may lead the doctor to approve of the claimant staying out of work longer but also to recommend more medical care to help the claimant heal from their injury. Further, claimants spending more time off of work may lead doctors to recommend more medical care or claimants to better comply with doctor-recommended care. Roughly half of all workers' compensation insurance program costs are due to medical spending, and to the best of our knowledge, our study provides the first evidence on the impact of income benefit generosity on medical spending.

Our estimates indicate that the reform caused an 11.1% increase in the income benefit duration of workers' compensation claims among affected claimants, or about 2 weeks beyond the pre-policy mean of 17.7 weeks. Given the 16% average increase in the weekly benefit rate induced by the reform, the analogous instrumental variables estimate implies a benefit duration elasticity of 0.72 with a 95% confidence interval spanning 0.43 to 1.00. We find that medical utilization increased substantially when the generosity of income benefits increased. The reform caused a 9.8% increase in the medical spending (within the first five years post injury) associated with workers' compensation claims among affected claimants, or roughly a $1,200 increase beyond the pre-policy mean of $12,451. The analogous instrumental variables estimates imply that the elasticity of medical spending with respect to the income benefit rate is 0.63 with a 95% confidence interval spanning 0.37 to 0.90. Heterogeneity analysis suggests that some types of medical services were particularly responsive—including physical therapy visits, case management services, and prescription drugs—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, behavioral responses along these two margins—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 1.4

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 some commonly cited estimates from older studies on workers' compensation insurance, where nearly two-thirds of this difference is due to the previously unexplored impact of income benefit generosity on medical spending.

Beyond our primary estimates, we also present several pieces of supplemental evidence. 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. In addition, we provide correlational evidence indicating that medical spending drops sharply upon the termination of income benefit receipt. Finally, we analyze heterogeneity by claimant characteristics, and this analysis reveals that effects appear across all subgroups, suggesting near universal impacts.

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] Leveraging this framework, we calibrate the marginal welfare impact of increasing the generosity of benefits using our estimates of the impact of benefits on income benefit durations and medical spending along with an estimate of the drop in consumption experienced by injured workers upon workplace injury. The results of this calibration suggest that a marginal increase in the generosity of benefits reduces welfare. To place our estimates within a broader context, we calculate the implied Marginal Value of Public Funds (MVPF). This calibration implies an MVPF is 0.46, which is lower than the average MVPF estimates calculated in some other social insurance settings such as unemployment insurance and disability insurance (Hendren and Sprung-Keyser, 2020). We conclude by exploring the robustness of the normative analysis and the interpretation of our findings.

This paper contributes to the broader literature quantifying behavioral responses to coverage generosity in various insurance settings and evaluating the welfare 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), Powell and Goldman (2016)), unemployment insurance (e.g., Chetty (2008), Kroft and Notowidigdo (2016), Landais (2015), Landais and Spinnewijn (2021), Card et al. (2015)), and disability insurance (e.g., Maestas, Mullen and Strand (2013), Autor, Duggan and Gruber (2014), Autor et al. (2019), Deshpande and Lockwood (2022)).[5] Within workers' compensation, prior work has explored the impacts of wage replacement benefit generosity on the number of income claims (e.g., Krueger (1990*a*), Bronchetti and McInerney (2012), Neuhauser and Raphael (2004)) and income benefit durations (e.g., Meyer, Viscusi and Durbin (1995), Krueger (1990*b*), Neuhauser and Raphael (2004)).[6]

Our paper makes several contributions to this literature. First, we leverage recent quasi-experimental

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

[5] In addition to research on common insurance in the United States, other studies have investigated the impact of the generosity of mandated sick pay for illnesses *unrelated* to work in European countries. For example, see Ziebarth and Karlsson (2014).

[6] See Krueger and Meyer (2002) for a review of the literature. For more work investigating workers' compensation income benefit durations see Butler and Worrall (1985), Butler and Worrall (1991), Curington (1994), Ruser (1991). Within the broader workers' compensation literature, there is also work on other determinants of workers' compensation claims (e.g., Biddle and Roberts (2003), Hirsch, MacPherson and Dumond (1997), Guo and Burton (2010)), the composition of injuries represented in claims (e.g., Ruser (1998)), and explanations for the "Monday effect" (e.g., Card and McCall (1996), Campolieti and Hyatt (2006), Hansen (2016)). Prior work has also explored the impact of benefits on later employment outcomes (Hyatt (2011), Powell and Seabury (2018)).

variation and unique administrative data to provide the first estimates of the comprehensive impact of workers' compensation income benefit generosity on individual behavior and program costs.[7] We find that increasing the generosity of wage replacement benefits has a large impact on claimant behavior, leading to substantial increases in income benefit durations and medical spending. We find that behavioral responses in claimant medical spending—a previously unexplored margin of adjustment—are as 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 1.4 times as much as the mechanical impact of benefit generosity on program costs and roughly four times the effect that would have been predicted based on the most commonly cited evidence from an older literature.

Second, this paper provides recent evidence on the impact of workers' compensation benefit generosity. While many classic papers on workers' compensation insurance rely on data from the 1970s and 1980s, there have been large changes since that period in the labor market, workers' compensation insurance, and workplace safety. The labor market has seen dramatic shifts since the 1970s, with a decline in unionization, a rise in female labor force participation, and shifts in industry composition from the manufacturing sector towards the service sector. Moreover, workers' compensation programs and the nature of workplace injuries have changed significantly over the last several decades, with an improvement in workplace safety and the dramatic increase in medical spending as a share of workers' compensation program costs.[8] Our study provides important recent evidence on the impacts of workers' compensation benefit design to inform current policy debates.

Finally, beyond estimating the impacts of workers' compensation income benefit generosity, we discuss the welfare implications of these behavioral responses for workers' compensation insurance benefit design. To do this, we calibrate the marginal welfare impact of increasing benefits and the MVPF of a benefit extension. These calibrations suggest that a marginal increase in the generosity of income benefits reduces welfare, and the implied MVPF is lower than often found in other related social insurance programs.[9]

1 Background and Data

This section provides background information on workers' compensation insurance and the Texas workers' compensation system. We then describe the data and variation leveraged in this study.

Background: 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. Employers purchase workers' compensation insurance from private insurers or directly from a public insurer, and many states allow very large employers to become a certified self-insured entity to

[7] There are other important strengths of our study relative to prior work on workers' compensation insurance. Relative to the two most widely cited studies in this literature (Meyer, Viscusi and Durbin (1995) and Krueger (1990*b*)), the reform we analyze is larger in scale—both in terms of the magnitude of the change and the population affected—which allows us to provide precise estimates of the impacts of workers' compensation wage replacement benefits and to provide evidence on heterogeneity and mechanisms. Further, our study is the first in this literature to employ an event study difference-in-differences research design, allowing us to provide transparent evidence supporting the parallel trends identification assumption.

[8] The composition of injuries covered by workers' compensation has changed dramatically over time, given shifts in industry composition and increasing workplace safety (Conway and Svenson, 1998). A few waves of state legislative activity spanning the 1970s, 1980s, and 1990s have transformed workers' compensation insurance, moving state programs toward standardization and tightening the criteria for eligible injuries. Over the last four decades, medical spending has become a much larger part of the workers' compensation program, with the composition of benefits shifting from 29% medical benefits (71% cash benefits) in 1980 to 50% medical benefits (50% cash benefits) in 2008 and onward (McLaren, Baldwin and Boden, 2018).

[9] Our findings in this analysis are broadly consistent with results from an optimal benefit calculation reported in Bronchetti (2012), which finds the optimal workers' compensation replacement rate is below typical levels observed in practice. We note there are important differences between our marginal welfare analysis and the optimal benefit calculation reported in Bronchetti (2012), both in terms of methods and underlying estimates. We discuss these in greater detail in Section 4.

directly provide this insurance. 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.

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 led to significant increases in coverage generosity and standardization of workers' compensation systems across states in the late 1970s and early 1980s (Howard, 2002). Second, more recent state legislation in the 1990s tightened the criteria for eligible injuries (Boden and Ruser, 2003).[10] 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% 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).[11] 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 informing policy.

Background: 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 work-related injuries, 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 serves as the gatekeeper for all the medical care and cash benefits received by the injured worker. The treating doctor is responsible for overseeing the claimant's medical care, evaluating the claimant's medical improvement, assessing the claimant's work capacity, and evaluating the claimant's eligibility for continued receipt of cash benefits.[12]

Workers' compensation insurance temporary income benefits also 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 that provide partial wage replacement during a temporary absence from work. A claimant's treating doctor is charged with assessing the claimant's work capacity and determining eligibility for temporary income

[10]For example, several states restricted the criteria of an 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.

[11]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.

[12]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)).

benefits throughout the worker's temporary income benefit spell. Temporary income benefits are terminated when the earliest of the following three conditions are met: (i) the worker decides to return to work and the treating doctor certifies the worker is ready to return to work, (ii) the treating doctor has certified that the worker has reached his "maximum medical improvement", or (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.[13] While the statutory maximum duration in Texas is two years, temporary income benefits are typically terminated well in advance of two years of receiving these benefits through either condition (i) or (ii) above, highlighting the importance of the treating doctor as a gatekeeper of cash benefits (in addition to medical care) in this setting.[14] An injured employee receives partial wage replacement during his temporary income benefit spell, 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 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.[15]

In Texas, the average workers' compensation temporary income benefit spell is approximately 18 weeks, and nearly all of these beneficiaries return to work within a few years, regardless of whether they have some degree of permanent impairment. Based on analyzing linked workers' compensation insurance data and unemployment insurance earnings records, the Texas Department of Insurance (2015) reports 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.

Background: Description of Policy Variation and Setting. The generosity of temporary income benefits is the focus of much of the policy and academic discussion of program parameters in workers' compensation insurance. There may be multiple reasons for the focus on this parameter. First, this is the primary parameter governing the generosity of benefits that has direct incentive effects for claimants and thus is the most likely parameter to affect claimant behavior.[16] 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 estimate the impact of 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 income ben-

[13]According to McLaren, Baldwin and Boden (2018), as of 2018, 22 states had a waiting period of seven days, while another 22 states had a waiting period of three days. The remaining states had a waiting period of four days (one state) or five days (five states).

[14]Among claims with positive temporary income benefits in our sample, 97.5% of temporary income benefit spells were terminated before the maximum duration of 104 weeks was met.

[15]While the receipt of permanent impairment benefits after income benefit termination is relatively common, these permanent impairments are typically minor, with the mean claimant rated as 6.3% impaired within our sample among those with some permanent impairment benefits.

[16]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 cash 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 may be ex ante most likely to affect claimant behavior.

efit generosity 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. Since the benefit schedule is determined by the injury date and applies for the entire duration of the spell, this reform induced sharp differences in benefit eligibility for those injured before versus after October 1, 2006. Before 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 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 new benefit cap (October 1, 2006) to analyze the effect of benefit generosity on outcomes of interest. Our baseline analysis focuses 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.[17] Appendix Figures A2 and A3 illustrate the results are very similar when 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 receiving income benefits 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. As depicted in Figure 1, claimants with earnings between the old and new schedule maximums received smaller increases in their weekly benefit rate. On average, the reform increased the weekly benefit rate by approximately 16% among affected claimants in our sample (those with prior earnings above the old schedule maximum).

Our main analysis investigates 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.[18] Increased generosity of income benefits could lead claimants to report more severe symptoms to their treating doctors in an effort to stay on income benefits longer, causing treating doctors to approve additional time on income

[17] Another advantage of focusing on claimants injured up to one year after the policy change is that it avoids overlap with the Great Recession. Though we know of no prior work on the impacts of recessions on workers' compensation claims, Boone and van Ours (2006) conduct cross-country analysis with data from the European Union and find that the rate of reported workplace injuries declines in recessions. Further, extensive prior and ongoing research points to the important impacts of recessions on the number and composition of disability insurance claims (e.g., Autor and Duggan (2003, 2006), Carey, Miller and Molitor (2022)).

[18] While our study is the first study to investigate medical spending as a margin for adjustment (to the best of our knowledge), we note Meyer, Viscusi and Durbin (1995)—which aims to estimate the duration elasticity—showed related evidence in a difference in means balance test. However, given limited statistical power, their difference in means test does not allow one to draw meaningful conclusions.

benefits and also to recommend more medical care to treat the worker's symptoms and help the worker heal to the point of returning to work. More generally, 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 during normal work hours, which could lead to better claimant compliance with recommended follow-up care (e.g., physical or occupational therapy). Claimants may also have increased incentive to comply with doctor-recommended follow-up care if they want to convince their doctor that they are still healing from their injury and thus remain eligible for income benefits. Additionally, increases in wage replacement benefits may lead to larger reimbursements at normally scheduled visits due to insurer-requested continued physician monitoring of claimant work capacity during longer income benefit durations.[19] Finally, in principle, more generous wage replacement benefits 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. Our research design and data do not allow us to disentangle which mechanisms contribute to the estimated effect on medical spending. While our analysis focuses on estimating the overall effect on medical spending, we return to discussing potential mechanisms and the interpretation of our estimates in Section 4.

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. 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 detailed linked income and medical benefit administrative data from Texas allows us to estimate the comprehensive effects of income benefit generosity and explore mechanisms. 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 using administrative data generally 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.[20] 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. 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.[21] Though the Texas workers' compensation system has the peculiar voluntary

[19]Treating doctors are required to submit (and are reimbursed to complete) a work status report form upon the initial evaluation of the claimant and whenever there is a substantial change in the work activity limitations of the claimant. Beyond these program-wide requirements, regularly scheduled time-frames can be specified by insurance carriers for treating doctors to continue to submit reports. However, the regulator places restrictions on insurer reporting requests, specifying that: (1) insurers cannot request more than one report every two weeks, and (2) insurers cannot request reports more often than the normally scheduled medical appointments with the employee.

[20]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.

[21]According to a study conducted by the Texas Department of Insurance (Texas Department of Insurance, 2019*a*), 82% of private

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 annual policy renewal dates are staggered throughout the calendar year and there are lags in the premium rating windows, preventing regulated premiums from adjusting to reflect higher claim costs in the short-run.[22,23]

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. Appendix Table A1 provides some context by comparing individuals receiving workers' compensation benefits in Texas and nationwide using data from the Current Population Survey (CPS) Annual Social and Economic Supplement 2002-2011 (Flood et al., 2022). Columns 1 and 2 compare workers' compensation claimants in Texas and all states, while columns 3 and 4 compare the subset of claimants whose inflation-adjusted weekly earnings exceed $771 (the earnings threshold corresponding to the old schedule maximum benefit). 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 among claimants with higher earnings. 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 subset of claimants with higher earnings is representative of claimants in the U.S. as a whole, so one should exercise appropriate caution in extrapolating from our estimates. However, along the lines of observable attributes, claimants in Texas look broadly similar to claimants nationwide. Industry composition is one observable dimension on which these claimants look somewhat dissimilar. As we demonstrate in Appendix Figure A4, 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

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).

[22] 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 four years. In determining employer experience rating multipliers, the regulator mandates the use of a three-year window with a 21-month lag.

[23] 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. Leveraging plausibly exogenous premium variation, Cabral, Cui and Dworsky (2022) analyze selection within the Texas workers' compensation insurance market and find 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.

spending, if individuals are forward-looking and informed about 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.[24] 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 Section 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.

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 information for claims with injury dates from 2005 to 2009 (Texas Department of Insurance, 2018*a*,*b*,*c*,*d*). The medical benefit data cover 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. For claimants who receive cash benefits, we have information on: type of cash benefits received, prior average weekly wage, total benefits received, and benefit start and end dates. The data also include rich demographic and injury information about the claimant including: sex, birth date (month-year), zip code, injury date (month-year), and industry.[25]

We define the injury date to be the month-year of the injury as recorded in the administrative data reported by insurers. 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). From January 2005 to September 2007, Texas had nearly 700,000 claimants, roughly 20% of whom received income benefits for missed work.[26] We restrict the sample to claimants receiving income benefits with wage-inflation-adjusted average pre-injury weekly earnings of \$540 to \$2,000 as of the month directly prior to the benefit increase. We drop observations with missing gender or age or with age calculated to be greater than 80 (2.4%), observations with non-positive medical spending (0.5%), observations with implausibly high income benefit amounts relative to the duration of benefits (3.4%), and observations with contradictory injury dates (0.1%). The final analysis sample consists of 63,155 claims from January 2005 to September 2007.

Table 1 provides descriptive statistics for the baseline sample. In addition to showing descriptive statis-

[24]Interpreting this supplemental evidence as a test of liquidity requires strong assumptions—individuals are sufficiently informed and forward-looking about benefits they will be eligible to receive weeks or months later. Given this, we view the results of this supplemental analysis as only suggestive evidence on the potential role of liquidity.

[25]In addition to these primary datasets, we also draw on supplemental data on workers' compensation coverage and premiums (Texas Department of Insurance, 2019*b*), workers' compensation program parameters (Cabral and Dillender, 2023), consumer price index data (United States Bureau of Labor Statistics, 2019, 2020*a*,*b*), and other crosswalks (United States Census Bureau, 2010; Centers for Medicare & Medicaid Services, 2019).

[26]In Texas, as in all state workers' compensation programs, most workers' compensation claims do not involve lost work time that exceeds the waiting period for temporary income benefits. While these "medical-only" claims are the most common types of claims, they represent a small share of spending. For instance, medical-only claims made up roughly 75% of all workers' compensation claims in 2015, though they accounted for roughly 7% of benefits paid (Weiss, Murphy and Boden, 2019).

tics for the full sample, this table displays statistics separately for "high earner" claimants (those who would have received higher benefits under the new schedule compared to the old schedule) and "middle earner" claimants (those who would have received the same benefits under either schedule).[27] 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 injury date is known with greater accuracy for these claims. We note that high earner and middle earner claimants look broadly similar to one another in terms of the observed demographic and injury characteristics.

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.[28] The mean weekly benefit amount is $517, 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. Our key measure of medical spending is five-year medical spending which captures medical spending over the first five years after the injury took place, where we proxy for the injury date using the first date of medical treatment for the injury.[29] The mean five-year medical spending is $12,484.[30].

2 Empirical Strategy

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

2.1 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 benefit update. Let i denote claimant. We measure exposure to the schedule change with a change-in-benefit variable, Δb_i, which isolates the increase in the weekly benefit level due to the change in the maximum benefit:

$$\Delta b_i \equiv b^{new}(w_i) - b^{old}(w_i), \tag{1}$$

[27] Following the definitions outlined in Section 2, high earners are those with exposure to the reform ($\Delta b_{it} > 0$) and middle earners are those with no exposure to the reform ($\Delta b_{it} = 0$).

[28] 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. For the less than 1% 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.

[29] Since medical spending is heavily front-loaded, our results are very similar if we use the administratively recorded month-year of injury to construct this measure rather than this proxy for the exact date of injury. For example, the IV estimate of the elasticity of medical spending with respect to income benefit generosity is 0.641 [95% CI: 0.372 to 0.910] using this alternative measure constructed based administratively recorded injury month-year, which is very similar to the baseline estimate of 0.634 [95% CI: 0.367 to 0.901].

[30] 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. As discussed in more detail in Section 4 and Appendix Section C, we find no evidence that the reform affected cost shifting to other payers, and the key findings of welfare analysis would still hold even if there are substantial spillovers on external healthcare payers.

where $b^{old}(\cdot)$ and $b^{new}(\cdot)$ represent the old and new benefit schedule, respectively, and w_i is the pre-injury average weekly wage of individual i. The change-in-benefit variable captures variation in exposure to the reform by pre-injury wages, where this exposure is depicted in Figure 1 as the vertical distance between the old and new benefit schedules at the indicated wage. To obtain reduced form estimates that reflect the magnitude of the reform, we scale this exposure measure by the mean change in benefits among affected claimants: $\Delta b_i\_\text{scaled} = \frac{\Delta b_i}{\frac{1}{|\mathcal{J}|}\sum_{i\in\mathcal{J}}\Delta b_i}$, where $\mathcal{J}$ represents the set of claimants with a non-zero change-in-benefit in the baseline sample ($\mathcal{J} \equiv \{i : \Delta b_i > 0\}$).[31]

We estimate a difference-in-differences specification that allows the coefficient on the scaled change-in-benefit variable, Δb_i_scaled, to vary flexibly by injury month. Let y_i be the outcome variable for claimant i with injury month $t(i)$. Our baseline regression can be represented as follows:

$$y_i = \alpha_{t(i)} + \theta \Delta b_i\_\text{scaled} + \left[\sum_{k \neq t_0} \beta_k \times \mathbb{1}(t(i) = k) \times \Delta b_i\_\text{scaled}\right] + \Omega \mathbf{X}_i + \epsilon_i, \tag{2}$$

where $\alpha_{t(i)}$ is an injury month-year fixed effect, Δb_i_scaled is scaled change-in-benefit, and $\mathbf{X}_i$ represents additional controls. Our baseline specification includes the following controls: age, gender, county by injury month-year fixed effects, ED claim indicator, and fixed effects for the day of the week the claimant first received medical treatment for the injury.[32] We also report specifications with only age, sex, and county by injury month-year fixed effects. 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 the injury date. We normalize the coefficient on the injury month just prior to the reform to zero ($\beta_{t_0} = 0$), so that the estimates can be interpreted as the difference in outcomes relative to those injured in the month directly preceding the reform, t_0 (September 2006).

In addition to estimating this flexible specification, we 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:

$$y_i = \rho_{t(i)} + \delta \Delta b_i\_\text{scaled} + [\pi \times \mathbb{1}(t(i) > t_0) \times \Delta b_i\_\text{scaled}] + \Theta \mathbf{X}_i + \varepsilon_i. \tag{3}$$

In the specifications outlined above, the reduced form impact of the reform is captured by the coefficient(s) on the interaction of the scaled change-in-benefit measure and indicator(s) for a post-reform injury date—β_k for $k > t_0$ (in Equation 2) and π (in Equation 3). These coefficient estimates can be interpreted as the effect of a 16% increase in benefits—the mean increase in benefits experienced by claimants exposed to the change in benefits.[33] In addition to presenting reduced form estimates, we also report elasticity estimates from an analogous instrumental variables (IV) specification which effectively scales the reduced form impact of the reform by the first-stage change in benefits.

[31] We demonstrate that we obtain similar estimates in specifications that replace this scaled change-in-benefit measure with a simple indicator for treated ($\mathbb{1}\{\Delta b_i > 0\}$). See Appendix Table A5 and Appendix Figure A5. While we obtain similar estimates with either measure, we prefer the scaled change-in-benefit measure of treatment because it accounts for variation in the degree to which claimants are treated while still illustrating the magnitude of the reform in the reduced form estimates.

[32] The day of the week the claimant first received medical treatment for the injury can be thought of as a proxy for the day of the week of the injury. As we discuss, we obtain very similar results when focusing on specifications with only age, sex, and geographic controls or specifications with no additional claim-level controls.

[33] See Figure 2 Panel B and Appendix Table A2 Panel B discussed below.

The identification assumption for these difference-in-differences specifications 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. While we cannot directly test this assumption, we use several approaches to assess the validity of this assumption. First, we plot the β_k coefficients by injury date; these plots reveal no evidence of spurious pre-existing trends correlated with exposure to the reform. Second, we demonstrate that there are no correlated changes in claimant observable characteristics. Third, we illustrate that our results are robust to alternative specifications which vary the set of included controls or the sample of included claimants. Finally, we conduct two placebo exercises which illustrate that there are no similar effects among non-treated claimants or during non-treated time periods.

2.2 Identifying Variation

Variation in Weekly Benefit Amount Figure 2 plots the first stage estimates—the coefficients on the change-in-benefit by injury month from the difference-in-differences specification outlined in Equation (2). Panel A relates the level of the potential weekly benefit to the unscaled change-in-benefit measure, while Panel B relates the natural logarithm of the potential weekly benefit to the scaled change-in-benefit measure. Appendix Table A2 reports the coefficient estimates from the analogous pooled difference-in-difference specification (described in Equation 3), as well as estimates from alternative specifications varying the included controls or sample.

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 change-in-benefit 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, Appendix Table A2 Panel A column 1 indicates that a $1 increase in the change-in-benefit variable translates to an average increase of $0.927 in the weekly benefit rate paid. The coefficients are similar across alternative specifications that vary the included controls or sample and are precisely estimated with coefficients ranging from $0.925 to $0.940 and standard errors no larger than $0.011.

To obtain estimates that summarize the mean increase in the potential weekly benefit among exposed claimants, Figure 2 Panel B and Appendix Table A2 Panel B display the regression results relating the natural logarithm of the weekly benefit to the scaled change-in-benefit measure. Appendix Table A2 Panel B indicates the reform increased the mean weekly benefit rate by 15.5% for the exposed claimants (based on column 1)—or a $96.79 average increase in the weekly benefit level for these claimants (based on column 5). The estimates are similar across specifications and samples, ranging from a 15.5% to 15.8% increase, with an associated standard error never exceeding 0.3%. In the remainder of the paper, we often focus on the scaled change-in-benefit to measure exposure to the reform, which provides estimates of the mean effect of the change in the benefit schedule among affected claimants.

Claim Rates and Claimant Characteristics Appendix Figure A6 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 those with wage-inflation-adjusted pre-injury weekly earnings exceeding $771—the level of earnings corresponding to the initial benefit cap—and for those with earnings below this level. If the increase in benefit generosity caused an increase in claims, we would expect to see these lines diverge following the implementation of the reform with the line representing those with higher earnings lying consistently above the other 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.[34]

Table 2 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 in Equation (3) excluding additional 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).[35] 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.[36] In Table 2, we see the estimated coefficients relating these observable characteristics to the identifying variation are economically small and statistically indistinguishable from zero. Appendix Figure A7 displays the analogous event study estimates for these claimant characteristics, and this figure similarly indicates there is no relationship between the identifying variation and claimant characteristics.

Collectively, this evidence suggests 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. In Section 3, we also illustrate that our main results are robust to including or omitting controls for baseline claimant characteristics.

3 Results

3.1 Main Estimates

We begin by presenting raw trends in Figure 3 for each of our three outcomes: benefit duration (in Panel A), medical spending within the first five years after the injury (in Panel B), and the number of positive medical bills within the first five years after the injury (in Panel C). These raw trends are plotted separately for high earners (who are exposed to the reform, $\Delta b_{it} > 0$) and middle earners (who are not exposed to the reform, $\Delta b_{it} = 0$). For each outcome, the trends appear similar in these groups prior to the benefit change. However, upon the implementation of the new benefit schedule, we see a divergence in these trends, with the high earner group, on average, having increased benefit durations, medical spending, and medical bills. The observed divergence persists in the months following the reform. These raw trend figures provide suggestive evidence that: (i) there is no noticeable difference in trends for outcomes prior to the reform across treatment and control groups, and (ii) the reform induced increases in benefit durations, medical spending, and medical bills for treated claimants. Our event study analysis confirms these conclusions are robust to controlling for claimant and injury characteristics.

Next, we turn to our difference-in-differences regression estimates. Table 3 displays the results from estimating Equation (3). Column 1 reports the baseline specification. The remaining columns investigate al-

[34]While there is some prior work suggesting that the number of workers' compensation claims may increase with the generosity of benefits (Krueger, 1990*a*; Neuhauser and Raphael, 2004), our finding that there is no evidence that claims respond to benefit levels is consistent with findings in recent work investigating the relationship between workers' compensation benefit generosity and claims (Bronchetti and McInerney, 2012; Guo and Burton, 2010).

[35]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.

[36]For the lasso models, we include the inverse hyperbolic sine of first-day medical spending and indicator variables for ten-year age bins, day of the week of first medical treatment, wage deciles, dangerous industry, injury type, ED start, and the claimant being married.

ternative specifications: a specification with fewer controls (column 2), a specification with additional controls for injury type and insurer fixed effects (column 3), a specification using the subset of claims initiated in the ED (column 4), and an analogous specification in levels (column 5). Table 4 displays the analogous instrumental variables elasticity estimates.

Figure 4 displays event study figures with injury-month-specific coefficients on the key exposure measure as outlined in Equation (2), where these event study figures correspond to the flexible version of the baseline specification (in Table 3 column 1).[37] For each outcome, the plots show no evidence of a trend for injuries initiated in the period prior to the reform, providing further support for our parallel trends identifying assumption. For claimants injured following implementation, all three outcomes (income benefit durations, medical spending, and the number of medical bills) sharply increase relative to prior claimants and remain stable for claimants injured after the implementation date.

Table 3 indicates that the reform caused an 11.1% mean increase in the income benefit duration of workers' compensation claims among affected claimants (based on column 1), or a 2.0 week average increase (based on column 5). Given the reform induced a 15.5% mean increase in the weekly benefit among affected claimants, the analogous instrumental variables estimate reported in Table 4 indicates a benefit duration elasticity of 0.72 with a 95% confidence interval spanning 0.43 to 1.00.

Additionally, estimates in Table 3 Panel B indicate the reform caused a 9.8% increase in medical spending (within the first five years post injury) among affected claimants (based on column 1), or a $1,203 average increase in medical spending (based on column 5). The analogous instrumental variables estimate reported in Table 4 indicates the elasticity of medical spending with respect to the income benefit rate is 0.63 with a 95% confidence interval spanning 0.37 to 0.90. We observe similar increases in the number of medical bills, with estimates in Table 3 Panel C indicating that the reform led to an 8.0% increase in the number of medical bills among affected claimants (based on column 1), or an average increase of 3.4 medical bills (based on column 5). The analogous instrumental variables estimate from Table 4 implies the elasticity of medical bills with respect to income benefits is 0.52 with a 95% confidence interval spanning 0.29 to 0.74. For each outcome, the estimates are similar in alternative specifications with more or fewer controls and with only claims initiated with an ED visit.

Table 5 presents estimates for 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 significant increases in all other types of care, though the point estimates suggest physical therapy services, case management services, and prescription drugs may be particularly responsive.

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: $Cost = D_B b + M$, where D_B is the benefit duration, b is the weekly benefit rate, and M is the total claimant medical spending. The impact of a

[37]While the regressions for Figure 4 control for the basic claim characteristics described in Section 2, the coefficient estimates are similar if only the scaled change-in-benefit measure and injury month-year fixed effects are included as controls. Refer to Appendix Figure A8 for the corresponding event study figures that exclude controls for basic claim characteristics.

change in the benefit level on insurer costs is then:

$$\frac{dCost}{db} = D_B\big(1 + \epsilon_{D_B,b} + \frac{dM}{db}\frac{1}{D_B}\big). \tag{4}$$

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 the instrumental variables estimates in Table 4, the second component within the parenthetical expression ($\epsilon_{D_B,b}$) is 0.72, and the third component within the parenthetical expression ($\frac{dM}{db}\frac{1}{D_B}$) is 0.70.[38] 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 (0.72 and 0.70) and are statistically indistinguishable from one another.[39,40] Second, collectively across these two margins for adjustment, the magnitude of the effect of behavioral responses to benefit generosity on insurer costs is 1.4 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 roughly four times the effect that would have been predicted based on most of the older work on workers' compensation insurance, which found duration elasticity estimates in the range of 0.3 to 0.4 and has ignored any effects on medical spending. Section 4 explores the potential implications of these estimates for benefit design.

Additional Robustness Beyond the robustness analysis discussed above, Appendix Table A5 reports the results of additional analysis exploring the sensitivity of our IV estimates. This analysis reveals that our estimates are similar when varying the sample by narrowing or widening the range of pre-injury earnings used for our sample definition. Further, we obtain similar estimates in specifications where we measure exposure to the reform through an indicator variable rather than our baseline continuous change-in-benefit measure. We also obtain similar estimates when including additional controls—such as controls for pre-injury wage, industry, industry X injury month-year, or insurer X injury month-year. The estimates are also similar to the baseline estimates when scaling the weekly benefit rate by pre-injury wages to reflect the claimant's replacement rate. Finally, we obtain similar estimates when we re-weight observations to be representative of workers' compensation claimants nationally along observable characteristics such as age, gender, and industry.

Additional analysis described in Appendix Section D further probes the robustness of our findings by conducting two placebo exercises—investigating either non-treated segments of the pre-injury wage distribution or non-treated time periods. The results from this analysis provide further support for the identification assumption behind our main estimates and the robustness of our findings.

[38] We obtain estimates for $\epsilon_{D_B,b}$ and $\frac{dM}{db}$ through the corresponding instrumental variables specifications reported in Table 4. We then obtain an estimate for $\frac{dM}{db}\frac{1}{D_B}$ by scaling our IV estimate of $\frac{dM}{db}$ ($= 12.43$) by the mean duration of benefit receipt ($= 17.71$).

[39] 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 bootstrap estimates does not allow us to reject that these terms are equal (t-stat=0.154).

[40] To see this another way, we can re-write Equation (4) as: $\frac{dCost}{db} = \frac{Cost}{b}\big(s + s\epsilon_{D_B,b} + (1-s)\epsilon_{M_B,b}\big)$, where s is the share of the program costs due to income benefits ($s \equiv \frac{bD_B}{Cost}$). In our sample, 43% of workers' compensation program costs are due to income benefits. If the elasticites are similar for both margins ($\epsilon_{D_B,b} \approx \epsilon_{M_B,b}$), this alternative expression allows one to directly see that the impact on workers' compensation program costs would be understated by about a factor of 2.3 ($= 1/0.43$) if one mistakenly ignored the impact of income benefit generosity on medical spending.

3.2 Supplemental Evidence

Below, we present supplemental evidence. First, we present evidence investigating the timing of the effects on income benefit receipt and medical care relative to the injury date. Second, we investigate heterogeneity in the estimated effects on benefit duration and medical spending across claimants.

Timing of Effects Let w index a 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 are included), we use the date of first medical treatment as a proxy for injury date in this analysis.[41] We estimate regressions of the following form for each two-week bin, w:

$$y_{iw} = \beta_w \text{Post}_i \times \Delta b_i\_\text{scaled} + \delta_w \Delta b_i\_\text{scaled} + \theta_w \text{Post}_i + \alpha_w + \lambda_w^H Z_{iw} + \epsilon_{iw}, \tag{5}$$

where Z_{iw} is the vector of additional controls included in the main analysis and the vector of β_w's from these regressions represents the coefficients of interest. We investigate two dependent variables: (i) indicator for income benefit receipt in w and (ii) indicator for positive 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.

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 aligns with one another. The periods with the largest effects on medical spending are also periods with the largest effects on income benefit receipt. To further explore the link between income and medical benefits, Appendix Section E presents supplemental correlational evidence illustrating how medical spending evolves upon the termination of income benefits. This evidence shows there is a sharp 60% drop in medical spending after income benefit termination. 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.

Heterogeneity in Effects We investigate heterogeneity in the main effects by claimant characteristics. Figure 6 reports the key coefficient estimates from estimating Equation (3) on the indicated subgroup defined based on baseline claimant characteristics: age, impairment type, industry riskiness, and sex. There are a few patterns worth noting. First, the effects for both outcomes are positive in each subgroup, suggesting the impact of income generosity on these outcomes is nearly universal. Second, the pattern in the point estimates suggests there may be more heterogeneity in medical spending responses than in income benefit duration responses, with point estimates suggesting stronger medical spending responses for women (compared to men) and for claimants with sprains and muscle issues (compared to those with other injuries). However, we note that none of the differences in estimates across subgroups are statistically distinguishable from zero at the 95% level.[42]

[41] We can compare the month implied by this injury date proxy to the administratively recorded injury month—the measure of injury timing we rely on in the main analysis. These are an exact match for 81% of claims and are within one month for 94% of claims.

[42] When testing whether differences across subgroups within Figure 6 are distinct from zero, the smallest p-value is 0.079 (for comparing medical spending effects for those with sprains and muscle issues versus other injuries) and the next smallest p-value is 0.093

4 Welfare Implications and Discussion

We explore the potential normative implications of our estimates using the elasticities presented in the prior section along with additional evidence on the consumption drop experienced by workers upon workplace injury. Motivated by the near ubiquity of workers' compensation insurance coverage, the analysis and discussion below consider the impact of increasing the generosity of workers' compensation income benefits within a compulsory workers' compensation system—which is analogous to a social insurance program financed by taxes on employers.[43]

We extend and apply the classic Baily-Chetty framework to characterize the marginal welfare impact of increasing benefit generosity, where we adapt models typically applied in the setting of unemployment insurance (e.g., Chetty (2006), Kroft and Notowidigdo (2016)) to the setting of workers' compensation insurance in which there are multiple dimensions for behavioral adjustments. Additionally, we calculate the implied Marginal Value of Public Funds (MVPF) (as in Hendren and Sprung-Keyser (2020)) associated with an increase in the generosity of workers' compensation insurance income benefits. We then provide more context through a discussion of these estimates and the implications of our findings.

4.1 Welfare Framework

4.1.1 Marginal Welfare Impact of Increase in Benefit Generosity

Model Setup 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 assume the agent cannot deplete assets below some constraint $L<0$ in any period.

In each period t, the individual chooses the effort e_t he/she will expend to recover from the injury and return to work. While the treating doctor of an injured worker must clear the claimant to return to work, the probability that the treating doctor will assess the individual as ready to return to work depends on the effort an employee dedicates to appearing ready to return to work to his/her treating doctor, to doing prescribed gym and home exercises, and to working with his/her employer to accommodate any work limitations. The cost of 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.[44]

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

$$V_t(A_t) = \max_{A_{t+1}\geq L;\ \underline{m}_t^W \leq m_t \leq \overline{m}_t^W} \quad u(A_t - A_{t+1} + w - \tau) + h_t^W(m_t) + V_{t+1}(A_{t+1}). \tag{6}$$

(for comparing medical spending effects for women versus men).

[43] Similar to social insurance programs funded through employer taxes (e.g., unemployment insurance), compulsory workers' compensation insurance is funded by employers, as employers purchase coverage at government-regulated prices. The discussion below treats workers' compensation insurance as analogous to other social insurance programs funded through government revenues raised through taxation.

[44] 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 $U_t(A_t)$ denote the value function for the worker who has not returned to work in period t:

$$U_t(A_t) = \max_{A_{t+1} \geq L;\ \underline{m}_t^N \leq m_t \leq \overline{m}_t^N} \quad u(A_t - A_{t+1} + b) + h_t^N(m_t) + J_{t+1}(A_{t+1}), \tag{7}$$

where $J_t(A_t) = \max_{e_t} e_t V_t(A_t) + (1-e_t)U_t(A_t) - \psi(e_t)$, is the value of entering period t having not yet returned to work with assets A_t. The inclusion of $h_t^W(m)$ and $h_t^N(m)$ above makes explicit that the dynamic optimization problem allows for the possibility that increased medical spending could increase the worker's health, which could have a positive impact on the worker's utility.

Given that workers are assumed to make privately optimal decisions and that utility over medical and non-medical consumption is separable, medical spending only affects the marginal welfare calculations through the fiscal externalities it generates for the workers' compensation program. The intuition for this comes from the envelope theorem. Because the worker is assumed to make privately optimal decisions at baseline and the worker always had it in his/her choice set to act sicker, engage in more medical treatment, or take longer to recover, any increased medical spending that happens in response to an increase in benefit generosity cannot have a first-order effect on the agent's utility. Similarly, while the worker's valuation of leisure is not included in the specification of utility above, the resulting sufficient statistics 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 non-medical consumption.[45]

Let D be the individual's expected non-working duration, and let D_B 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, and let $M = \sum_{t=0}^{T-1} m_t$. Let J_0 represent the individual's indirect utility at time 0 as a function of b and τ.

Below, we consider 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 a balanced budget constraint, solving: $\max J_0(b,\tau), s.t.\ D_B b + M = (T-D)\tau$.

Approximation of Marginal Welfare Impact of Increase in Generosity *Let us define a money-metric measure of welfare as* $\frac{dW}{db} \equiv \frac{dJ_0}{db} / \frac{dJ_0}{dw}$. *Suppose that: (i) the borrowing constraint is not binding at time B, (ii) the coefficient of relative prudence is zero (*$\frac{-u'''(c)}{u''(c)}c = 0$*), and (iii) the duration elasticities are equal (*$\epsilon_{D_B,b} = \epsilon_{D,b}$*). Then, the money-metric welfare gain from raising the benefit level, b, above can be approximated by:*

$$\frac{dW}{db} \approx \frac{D_B}{D}\frac{\theta}{1-\theta}\left(\gamma\frac{\Delta c}{c} - \epsilon_{D_B,b} - \epsilon_{D_B,b}\frac{\theta}{1-\theta}(1+\frac{M}{D_B b}) - \frac{dM}{db}\frac{1}{D_B}\right), \tag{8}$$

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* $\overline{c_W}$ *and* $\overline{c_N}$ *are the weighted-average consumption of the working and not working, respectively.*
See Appendix Section F for a detailed derivation of this expression.[46]

[45] If there is complementarity between utility over non-medical consumption and medical consumption (or utility over non-medical consumption and leisure), the welfare formulas in Equation (8) would need to be modified to account for the degree of complementarity.

[46] In Appendix Section F, we also derive an approximate welfare formula allowing for non-zero relative prudence. Appendix Table A7 illustrates the robustness of the marginal welfare analysis to allowing for a non-zero coefficient of relative prudence. In particular, this table shows that we obtain very similar marginal welfare estimates if we set the coefficient of relative prudence to $\gamma + 1$, as would be implied by Constant Relative Risk Aversion utility.

4.1.2 Marginal Value of Public Funds

In the model based on Baily-Chetty outlined above, the sign of the marginal welfare impact of increasing benefit generosity is determined by whether workers themselves are willing to pay the net cost of an extension of income benefits. In other words, the welfare formula above indicates a welfare gain associated with a marginal increase in income benefits if and only if the willingness to pay for a $1 incremental extension (approximated by 1+ $\gamma\frac{\Delta c}{c}$) exceeds the cost of this extension (1 + fiscal externalities on the workers' compensation program). In practice, workers' compensation insurance is not necessarily funded by the workers that benefit from the program, and there may be broader fiscal externalities on the government's budget beyond workers' compensation insurance (e.g., labor supply distortions may lead to losses in income tax revenues). We can place our findings in a broader context relative to other programs with distributional impacts and account for broader externalities on government finances by calculating the implied Marginal Value of Public Funds (MVPF) based on our estimates following Hendren and Sprung-Keyser (2020). To parallel estimates in other social insurance settings, our MVPF calculation below conceptualizes the government cost of the benefit extension as the sum of the impact on workers' compensation claim costs and any additional fiscal externalities on the government's budget.[47]

We calculate the MVPF implied by our estimates assuming that behavior is privately optimal. In line with the model above, we approximate the willingness to pay for a $1 incremental extension as 1+$\gamma\frac{\Delta c}{c}$. We calculate the fiscal externality associated with such an extension as the sum of the impact on workers' compensation claim costs ($\epsilon_{D_B,b} + \frac{1}{D_B}\frac{dM}{db}$) and the impact of labor supply distortions on government revenue ($\epsilon_{D,b}\frac{D}{D_B}\frac{\tau}{b}$). Under the assumption that there are no other external effects of the benefit extension, the MVPF can be expressed as,

$$MVPF = \frac{1+\gamma\frac{\Delta c}{c}}{1+\epsilon_{D_B,b}+\frac{1}{D_B}\frac{dM}{db}+\epsilon_{D,b}\frac{D}{D_B}\frac{\tau}{b}} \approx \frac{1+\gamma\frac{\Delta c}{c}}{1+\epsilon_{D_B,b}(1+\frac{D}{D_B}\frac{\tau}{b})+\frac{1}{D_B}\frac{dM}{db}}, \tag{9}$$

where τ is the total tax rate paid (net of transfers) when working, and b is the income benefits replacement rate for workers' compensation insurance.[48] Suppose the duration elasticities are equal ($\epsilon_{D,b} \approx \epsilon_{D_B,b}$). Then, the MVPF can be approximated by the final expression in Equation (9) above.

4.2 Calibration

Approach Next, we use the approximate formulas described above in combination with our key estimated elasticities and a few additional data moments to calibrate the marginal welfare impact of increasing the generosity of coverage for workers' compensation wage replacement benefits and to calculate the implied MVPF. The instrumental variables estimates in Table 4 indicate that $\epsilon_{D_B,b}$ is 0.72 and $\frac{dM}{db}$ is 12.43. For the calibrations presented below, we approximate the out-of-work duration by the income benefit duration, $D \approx D_B$.[49] We calculate that the fraction of the covered workforce that is out-of-work due to workplace in-

[47]To obtain estimates that are comparable to those from other social insurance settings, we imagine that the government finances workers' compensation insurance claim costs, though often this coverage is provided through private insurers rather than directly by the government.

[48]As in Equation (4), the first three terms in the denominator of the MVPF ($1+\epsilon_{D_B,b}+\frac{1}{D_B}\frac{dM}{db}$) capture the impact of a change in benefit generosity on workers' compensation claim costs. The final term in the denominator of the MVPF ($\epsilon_{D,b}\frac{D}{D_B}\frac{\tau}{b}$) captures other fiscal externalities on the government's budget beyond workers' compensation costs due to distortions in labor supply.

[49]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 approximation in this setting. Texas Department of Insurance (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.

jury (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 (2022)) and the mean duration of income benefit receipt (0.34 years). For the MVPF calibration, we set the replacement rate equal to the mean replacement rate in our sample (63%) and the tax rate equal to the mean tax-and-transfer rate among workers' compensation recipients nationally (17.6%).[50]

There are two additional inputs needed in the approximations 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 consider a range of plausible relative risk aversion values. For the latter, we draw on data from the Health and Retirement Survey (HRS) to estimate the mean drop in consumption after a work-limiting workplace injury. We provide a brief summary of this estimation below and the full details in Appendix Section G.

Consumption Drop: Estimates and Robustness Appendix Table A8 reports estimates of the consumption drop after workplace injury using data from the HRS, 1992 to 2016.[51] We use these data because the HRS is the only data that we are aware of that has information on both consumption and location of injury.[52] Leveraging the panel structure of the HRS, we identify workers who were injured between two survey waves and measure their change in consumption between these waves. We focus on food consumption for this analysis.[53] Focusing on a single category of consumption (e.g., food) is without loss of generality provided that we use the appropriate risk aversion parameter (e.g., curvature of utility over food) in the welfare analysis (Chetty, 2006).

We note some important limitations of the HRS data for estimating the consumption drop. First, while the HRS is the only dataset that includes the information needed to construct these estimates, the HRS only covers individuals over 50 years of age and thus is not representative of all workers at risk of workplace injury. Second, consumption is only measured every two years in the HRS and the sample of workers suffering a workplace injury is relatively small. Given these limitations, we also evaluate the robustness of the welfare analysis to considering alternative consumption drop values, as discussed further below.

We estimate that workers experience a mean drop in food consumption of 10.1% after a work-limiting workplace injury using the full sample of HRS respondents with workplace injuries (Appendix Table A8

[50] To facilitate comparisons to MVPF calculations from other settings, we follow the same approach as in Hendren and Sprung-Keyser (2020) to calculate the mean tax-and-transfer rate. Specifically, the mean tax-and-transfer rate is calculated by associating the rates reported in Hendren and Sprung-Keyser (2020) by income relative to the federal poverty line with each workers' compensation insurance recipient in the CPS data summarized in Appendix Table A1. For this calculation, we apply the federal poverty line for households with two children (United States Census Bureau, 2021) to each recipient's household earnings. The rates reported in Hendren and Sprung-Keyser (2020) exclude payroll taxes, as payroll taxes collected for an individual's earnings may translate to future benefits for that individual.

[51] Health and Retirement Study (2021*a*,*b*, 2022*a*,*b*,*c*); RAND (2021, 2022*a*,*b*)

[52] To the best of our knowledge, Bronchetti (2012) is the only prior study to analyze the consumption drop experienced by injured workers. Like our study, Bronchetti (2012) uses the HRS data to analyze the consumption patterns among individuals experiencing a workplace injury, focusing on a subset of years used in our analysis. While Bronchetti (2012) does not provide a direct estimate of the consumption drop experienced by injured workers upon workplace injury, Bronchetti investigates how this consumption drop varies with a state's workers' compensation benefit generosity. While the HRS data allows one to obtain a precise estimate of the consumption drop experienced by those with workplace injuries, the limited sample size of the HRS leads to more imprecise estimates of the "slope"—the relationship between the consumption drop and benefit generosity. An advantage of our marginal welfare analysis is that it only requires an estimate of the level of the consumption drop upon workplace injury rather than the slope. In contrast, Bronchetti (2012) uses an estimate of the slope of the consumption drop with respect to benefits 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. Despite differences in the approaches and underlying estimates, the findings of our marginal welfare analysis are broadly consistent with the results of the optimal benefit calculation in Bronchetti (2012).

[53] Food consumption is often used in a related literature analyzing household consumption behavior (see, e.g., Gruber (1997); Kroft and Notowidigdo (2016); Stephens (2003); Haider and Stephens (2007)).

column 1).[54,55] We obtain similar estimates—ranging from 7.2% to 11.2%—when restricting the sample to workers who are more similar to the workers' compensation claimants marginal to the reform we analyze in our primary analysis, in terms of pre-injury earnings and weekly benefit levels. While we use the 10.1% consumption drop estimate from the full sample for the baseline welfare analysis, the key findings from our welfare analysis are unchanged if we instead use the estimated consumption drop within the more restricted samples or a wide range of other plausible consumption drop values.[56] See Appendix Table A9 for this robustness analysis.

Marginal Welfare Impact of Increase in Generosity Table 6 Panel A reports the calibrated marginal welfare gain from a 5% increase in the weekly benefit on a base of a $540 weekly benefit (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. Column 3 reports the analogous welfare calibrations using our estimated medical spending elasticity but ignoring impacts on the duration claiming income benefits—again, contrary to the evidence.

Across the range of risk aversion values considered, calibrations based on the estimated elasticities indicate that extending the generosity of income benefits generates welfare losses. Consider the case when the coefficient of relative risk aversion equals 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 reduce per capita ex ante utility by the equivalent of a $0.079 weekly wage reduction. The cost associated with providing this incremental increase in benefits is approximately $0.156 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 roughly half of the per capita cost of the extension. Comparing columns 1 and 2, we can see that ignoring the impact on medical spending leads one to underestimate the predicted welfare loss by 58%. To further benchmark magnitudes, the change in the predicted welfare estimates from ignoring the impacts on medical spending is more than twice 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 $\gamma = 2$.

Comparing columns 2 and 3, we see that the two margins for adjustment—income benefit duration and medical spending—are roughly equally important contributors to the predicted welfare loss from a marginal expansion of income benefits. For instance, if the coefficient of relative risk aversion is two, considering the impact on benefit duration and ignoring the impact on medical spending (as in column 2) would underestimate the predicted welfare loss by 58%, while considering the impact on medical spending and ignoring the impact on benefit duration (as in column 3) would underestimate the predicted welfare loss by 59%. Further, the estimates in columns 2 and 3 indicate that the key qualitative finding— that a marginal increase in benefits does not improve welfare— is robust to considering responses along only one

[54] Our baseline estimated 10.1% drop in consumption is in the same range, though slightly larger, than the roughly 7% drop in consumption upon unemployment estimated by Gruber (1997) and Kroft and Notowidigdo (2016). Our consumption drop estimate is also larger than the mean consumption drop among injured workers implied by estimates reported in Bronchetti (2012), though we note that caution is warranted in this comparison because Bronchetti (2012) does not provide a direct estimate of the consumption drop.

[55] Appendix Section G displays an event-study figure displaying consumption patterns leading up to an injury; this additional analysis reveals no evidence of pre-existing trends in consumption leading up to an injury—supporting our empirical strategy for estimating the consumption drop among injured workers.

[56] Appendix Table A9 demonstrates that our key qualitative findings are similar when using consumption drop values ranging from 2.5% to 40%.

of the two margins of adjustment observed in practice.

MVPF Table 6 Panel B reports the calibrated values of the MVPF based on our estimates. While we consider relative risk aversion values spanning one to five, in this discussion we focus on the case when the value of relative risk aversion equals two. The MVPF based on our estimates is 0.46, indicating that $1 of additional public spending generates 46 cents for beneficiaries. If we use our estimated duration elasticity but ignore the effect on medical spending, we would mistakenly conclude the MVPF is 0.63. If we instead ignore the effect on income benefit duration and only considered the effect on medical spending, the implied MVPF would be 0.71. Thus, ignoring either the effect on income benefit duration or medical spending would significantly overstate the MVPF.

To provide some context, we can compare the implied MVPF in this setting to that of other social insurance programs. Using estimates from the prior literature, Hendren and Sprung-Keyser (2020) report the category average of MVPF estimates for unemployment insurance policies is 0.61 and for disability insurance expansions is 0.85. Our estimated MVPF of 0.46 in the context of workers' compensation income benefits is lower than these reported average MVPF estimates for unemployment insurance or disability insurance—the other key social insurance programs that insure lost earnings. It is important to note that the MVPF calculations for unemployment insurance and disability insurance focus on the fiscal externalities associated with labor supply effects of these programs. If we ignored the effect of workers' compensation income benefits on medical spending and only considered the effect on labor supply (through income benefit durations), we would obtain an implied MVPF for workers' compensation income benefits of 0.63 (from column 2), which is close to the average MVPF estimate for unemployment insurance policies (0.61) though still substantially below the average MVPF for disability insurance expansions (0.85). In other words, the additional margin of behavioral adjustment in the setting of workers' compensation insurance is one explanation for why the MVPF is lower in this setting than often estimated in the setting of unemployment insurance.

4.3 Discussion

The welfare calibration above suggests that a marginal increase in income benefits would not lead to welfare improvements in this setting. Further, the implied Marginal Value of Public Funds (MVPF) calculated above suggests that each dollar of public spending results in just 46 cents for beneficiaries.

An important caveat is that the assumptions behind the welfare analysis may not hold. If the assumptions do not hold, there may be other channels through which increasing benefit generosity leads to welfare improvements. The welfare analysis outlined above relies on several assumptions. For instance, the model based on the Baily-Chetty framework assumes utility is not state-dependent, there is no complementarity between non-medical consumption and either leisure or medical consumption, agents make privately optimal decisions, and there are no externalities on other parties. While making assumptions along these lines is common in applications of the Baily-Chetty framework, these assumptions are not innocuous and may not hold. Below, we discuss two of the key assumptions behind the welfare analysis and evidence related to the plausibility of these assumptions.

First, consider the assumption that agents make privately optimal decisions. We maintain this assumption throughout the normative analysis. While this is a standard assumption in revealed preference welfare analysis, it is not without loss of generality as it rules out internalities due to behavioral biases or information frictions. We know of no research investigating internalities in the setting of workers' compensation claimant decisions, and we note this is an important area for future work. Suppose, for instance, there is a violation of this assumption and agents under-consumed leisure or medical services prior to benefits being increased. Then, induced increases in leisure and medical spending due to increased benefit generosity may work to alleviate these distortions and could lead to unmodeled increases in utility. It is not possible to test whether agents make privately optimal decisions. However, we can look for indirect evidence on the plausibility of this assumption. To do this, we investigate whether increased benefit generosity leads to improvements in health (and thus plausibly utility). If increased benefit generosity leads to large impacts on health, one might question the reasonableness of the assumption that agents are optimizing. Moreover, if the model is not applicable because agents do not make privately optimal decisions, quantifying the effect of benefit generosity on health may help us value the impact of increasing benefit generosity.[57]

We look for evidence related to the effect of benefit generosity on health in two ways. First, we estimate the effect of the reform on claimants' permanent impairment ratings. If induced increases in leisure or medical utilization led to health improvements, we might expect that fewer individuals would be assessed as permanently impaired after the completion of their temporary income benefits or that permanent impairment severity ratings would decline. Table 7 presents results examining the effects of the reform on permanent impairment ratings. The reform-induced increase in benefit generosity had no detectable impact on the share of claimants assessed as permanently impaired or on the assigned permanent impairment severity ratings. Second, we examine the impact of the reform on claimants' long-run utilization of medical services. If induced increases in leisure or medical utilization concurrent with temporary income benefits improved health, we might expect claimants to have fewer complications and less medical utilization in the long run, after income benefit eligibility has lapsed. We present related evidence in Table 8, where we estimate the effect of the reform on medical spending at various time horizons since injury. While the increases in medical spending are largest during periods coincident with income benefit eligibility, the point estimates for the effect 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. These findings suggest that the reform-induced additional 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. Taken together, these analyses reveal no evidence that increased income benefit generosity impacted health. An important caveat is that the measures we analyze do not capture all aspects of health and thus this evidence does not allow us to rule out that the increased benefit generosity impacted health. However, we note that it may be plausible that additional medical spending and leisure on the margin may not have a first-order impact on health (whether workers make privately optimal decisions or not), given incentives within workers' compensation insurance could lead to excessive consumption of medical care and time off of work.[58]

Second, consider the assumption that there are no other external effects of workers' compensation in-

[57] The increase in income benefit generosity induces increases in leisure and/or medical spending that could potentially improve worker health by helping claimants heal from their injuries. Further, the observed parallel increases in both leisure and medical spending could potentially help claimants heal from their injuries more so than they would have if they had experienced the same size increases in leisure in isolation.

[58] Incentives in workers' compensation insurance may lead workers' compensation claimants to receive more medical care than individuals with the same injuries outside of workers' compensation insurance (with standard health insurance). Workers' compensation patients face no out-of-pocket cost for medical care, whereas typical cost-sharing faced by patients with standard health insurance is in the range of 10% to 30% after meeting an annual deductible. Health care providers are generally paid at a higher rate for services provided to workers' compensation patients compared to the same services provided to other patients (Baker and Krueger (1995), Johnson, Baldwin and Burton (1996)). Given that provider payment rates are generally higher and patient cost-sharing is lower than in standard health insurance, typical concerns about "flat of the curve" medicine in the U.S. health care system may be even greater in workers' compensation insurance and suggest that there could be little health benefit on the margin for additional medical care for workers' compensation claimants. Further, because workers may enjoy time off of work for reasons other than healing from their injury, incentives in workers' compensation could lead workers to take more time off work than needed to recover from their injuries. Thus, it may be plausible that additional time off work on the margin has little impact on worker health.

surance income benefit generosity. There is almost no prior work analyzing external effects of workers' compensation insurance income benefits. A notable exception is McInerney and Simon (2012) who find no relationship between workers' compensation insurance income benefit generosity and public disability insurance claims. It is perhaps not surprising that they find no evidence of externalities on disability insurance. Because workers' compensation wage replacement benefits provide temporary coverage while disability insurance covers longer spells after a waiting period, there is very limited overlap in the types of spells covered by these insurance programs and the direction of any such externality is ex ante theoretically ambiguous.

Another external group that may potentially be affected by workers' compensation income benefit generosity is formal or informal health insurers. Though workers' compensation insurance is legally the first payer for medical spending related to workplace injuries, it may be that medical spending within workers' compensation may substitute for (or complement) medical spending outside of workers' compensation insurance. To our knowledge, there is no work analyzing the impact of workers' compensation insurance benefit generosity on health care spending outside of workers' compensation insurance.[59] While we do not have data on medical spending outside of workers' compensation insurance to directly quantify any impacts of benefit generosity, we conduct additional analysis reported in Appendix Section C that leverages our administrative data on workers' compensation medical spending to investigate the possibility of spillovers on other health care spending in two ways. First, we investigate whether there is a change in the bill denial rate when the reform is implemented. 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. Our data contains both bills that are paid and unpaid, so we can investigate whether the rate of bill denial changes with the reform. Estimates reported in Appendix Table A6 illustrate the reform had no impact on the bill denial rate. Second, we investigate whether our results are similar for diagnostic radiology as for other care. Diagnostic radiology procedures—including costly advanced imaging such as MRIs, CT scans, and PET scans—are subject to strict utilization review by health insurers for outside sources of liability. 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 potentially liable parties/insurers. Collectively, these strategies may successfully limit cost-shifting for diagnostic radiology procedures relative to other types of procedures. 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 the 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 costs for these procedures onto health insurers prior to the reform. In contrast, estimates reported in Appendix Table A6 illustrate that diagnostic radiology spending and bills react to the same degree as spending and bills overall. Taken together, these analyses suggest no evidence of spillovers on other health care spending. While we cannot definitively rule out (either positive or negative) spillovers on other health spending, we view these analyses as broadly reassuring that the estimated change in workers' compensation medical spending based on

[59] There is some prior work investigating externalities in the opposite direction: the impact of health insurance generosity on workers' compensation medical spending. Specifically, a few prior studies have shown that having any health insurance or having more generous health insurance is associated with a decline in workers' compensation medical spending (e.g., Dillender (2015), Bronchetti and McInerney (2021), Fomenko and Gruber (2019)).

the identifying variation likely reflects changes in aggregate medical utilization among injured workers.

The baseline welfare calculations presented in this section abstract from impacts on external parties because we are unaware of prior evidence showing that workers' compensation benefit generosity affects other payers and our own analysis does not point to any evidence of external effects of increased benefit generosity. However, for completeness, we discuss how incorporating potential externalities on other health care payers would impact the welfare analysis. The main qualitative finding from the model based on Baily-Chetty—that increasing the generosity of coverage would not improve welfare—is not sensitive to the assumption that there are no external impacts on other health care payers.[60] In Appendix Section I.1 we analyze the sensitivity of the implied MVPF to incorporating potential externalities on other health care payers, where the incidence of these outside health care costs falls either on the government or on other individuals. When holding relative risk aversion fixed at two and varying the share of induced increases in medical spending due to cost-shifting from 0% to 50%, the implied MVPF ranges from 0.46 to 0.55. Thus, the key qualitative takeaways from the normative analysis would still hold even if there were moderate to large externalities on other health care payers.

Finally, we consider potential externalities on health care providers. If health care is competitively provided—such that the price paid by workers' compensation reflects the opportunity cost of the next best use of the inputs used to provide that health care—then there are no externalities on health care providers, as we assume in our baseline welfare calibrations. If instead health care providers make rents on care provided to workers' compensation patients—such that the price paid by workers' compensation insurance exceeds the opportunity cost of the next best use of the inputs used to provide that health care—then a broader welfare evaluation should account for these rents as positive externalities on health care providers from increased income benefit generosity. The key qualitative finding from the model based on Baily-Chetty—that increasing the generosity of coverage would not improve welfare—is not sensitive to the assumption that there are no external impacts on health care providers.[61] In Appendix Section I.2, we analyze the sensitivity of the calibrated MVPF when considering potential plausible transfers to health care providers. Holding risk aversion fixed at two and varying the rents that health care providers collect on the marginal care provided to workers' compensation patients from 0% to 75%, we find the implied MVPF ranges from 0.46 to 0.66. This analysis highlights that the main qualitative findings of the normative analysis are robust to accounting for potential rents to health care providers.

[60] To see this, consider the most extreme case where all the increase in workers' compensation medical spending is driven by cost-shifting from standard health insurance. Note that ignoring the effect of benefit generosity on medical spending (as in Table 6 column 2) is equivalent to thinking about this extreme case where the increase in workers' compensation medical spending is entirely explained by cost-shifting from health insurance with health insurance costs financed similarly to workers' compensation insurance—through lump sum taxation on workers. Across the range of plausible risk aversion values, we consistently obtain a negative value for the marginal welfare impact from increasing benefit generosity even in this extreme case of full cost-shifting (see Table 6 Panel A column 2).

[61] To see this, consider the most extreme case where the opportunity cost of inputs is zero, so the full increase in medical spending should be thought of as a transfer to providers. Further, suppose the social planner has the same social welfare weights on health care providers as workers more generally and the marginal utility of consumption is no larger for health care providers than it is for workers (when working) for the relevant ranges of consumption, which may be reasonable given health care providers are at the high end of the income distribution. Then, the calibrated welfare benefit of an extension in this extreme case is no larger than it would be if we ignored the effect of benefit generosity on medical spending (as in Table 6 column 2), and thus we can see that even in this extreme case the qualitative findings from the Baily-Chetty calibration are unchanged. We view this extreme scenario as unrealistic because the opportunity cost of the inputs used to provide health care is generally positive—for example, these inputs could be used to provide care to other patients or could be associated with costs to the provider (e.g., wages paid to office staff, forgone leisure, etc.).

5 Conclusion

Workers' compensation is one of the largest social insurance programs in the United States, but relative to other insurance programs, little research has explored the effects and implications of benefit design in the program. We leverage a legislated increase in the maximum weekly benefit level in the Texas workers' compensation insurance system to provide recent and transparent evidence on the impact of workers' compensation income benefit generosity on program costs. Our estimates indicate an income benefit duration elasticity of 0.72, which is roughly twice as large as the most commonly cited prior estimates would have suggested. Further, we find the effect of the generosity of wage replacement benefits on medical spending is as important of a driver of increased insurer cost as is the effect of benefit generosity on income benefit duration. We explore the potential welfare consequences of these behavioral responses. This analysis suggests increasing the generosity of workers' compensation wage replacement benefits would reduce welfare, and the implied Marginal Value of Public Funds (MVPF) based on our estimates is 0.46, lower than what is often estimated for other social insurance programs insuring income shocks.

The evidence presented in this paper is relevant for ongoing workers' compensation policy debates and for understanding the determinants of workers' compensation costs more broadly. While medical spending now represents half of all workers' compensation benefits paid, little is known about what factors influence workers' compensation medical spending. Our findings indicate that the replacement rate for income benefits is an important determinant of both workers' compensation income benefits and medical spending. These findings have direct relevance for ongoing policy debates, as the maximum weekly benefit level—the source of the variation in this study—is arguably the most frequently debated (and revised) policy parameter in workers' compensation insurance programs. More broadly, the findings from this paper highlight the importance of considering the impact of social insurance benefit generosity on measures of insurer and social costs that are broad enough to incorporate behavioral responses on all dimensions that may be affected.

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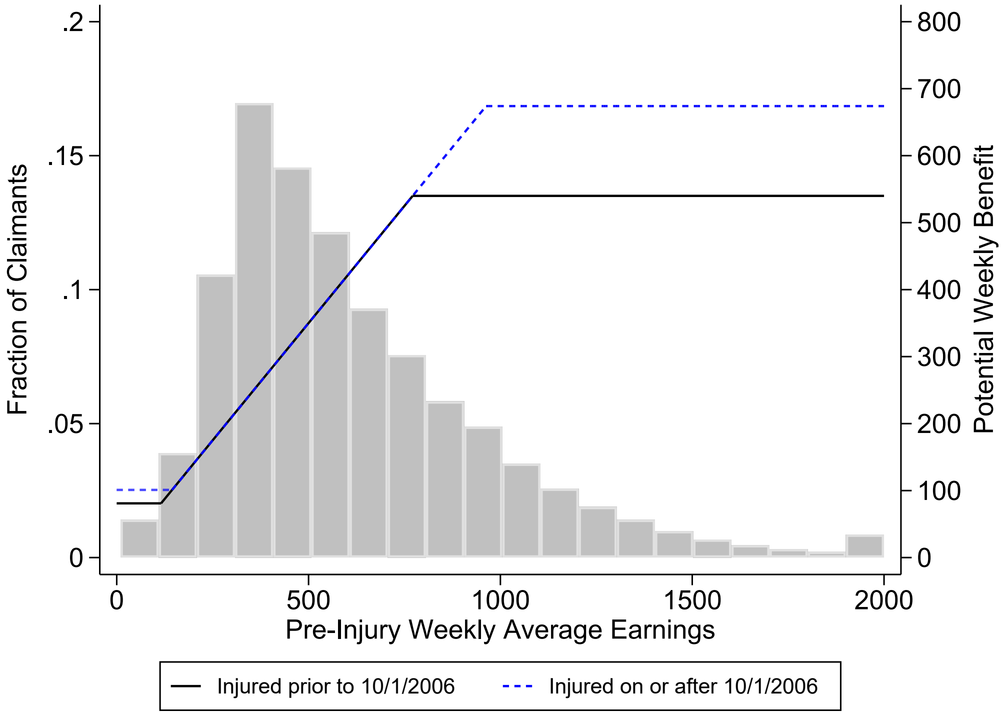
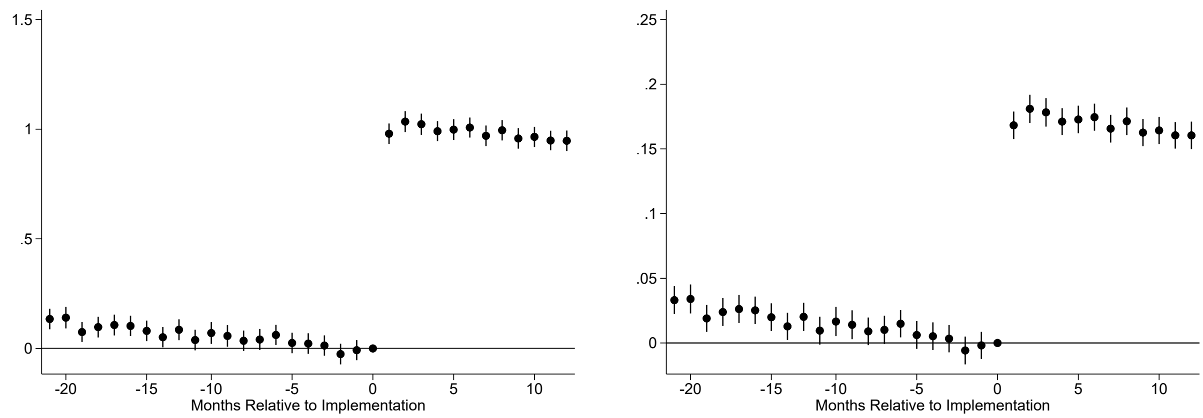
Figure 1: Weekly Benefit Rate Schedule Before and After Reform



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 with income benefits 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

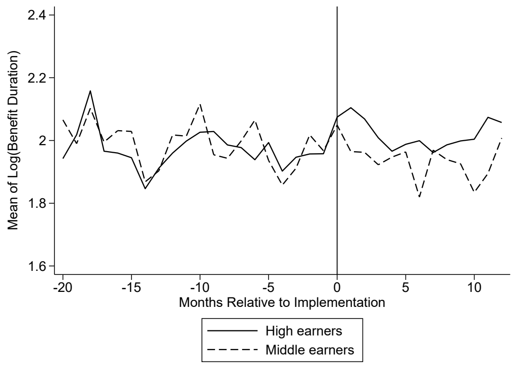


(a) Benefit Rate, Unscaled

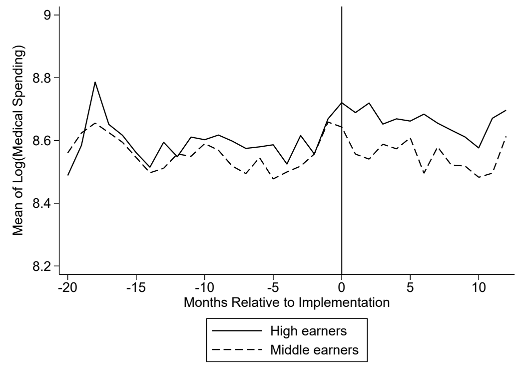
(b) ln(Benefit Rate), Scaled

Notes: Each graph in the figure above displays coefficients on the change-in-benefit or the scaled change-in-benefit measure (as indicated above) interacted with indicators for the month the injury occurred relative to the implementation of the reform from separate regressions of Equation (2) along with 95% confidence intervals calculated using robust standard errors. The interaction for the injury month immediately prior to the reform is omitted. The sample contains 63,155 claims that occurred from January 2005 to September 2007 that meet the sample restrictions described in the text. Each regression includes county by injury month-year 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) change-in-benefit, a male indicator variable, and a full vector of age indicator variables.

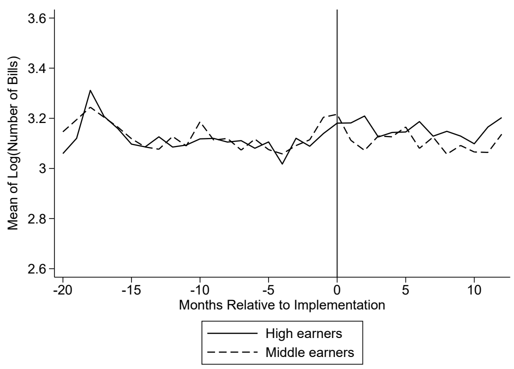
Figure 3: Raw Trends over Time: Benefit Duration and Medical Utilization



(a) ln(Benefit Duration)



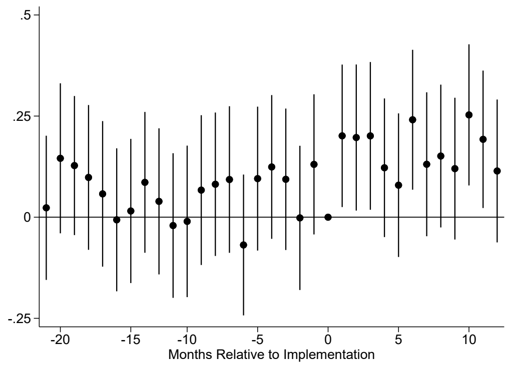
(b) ln(Medical Spending)



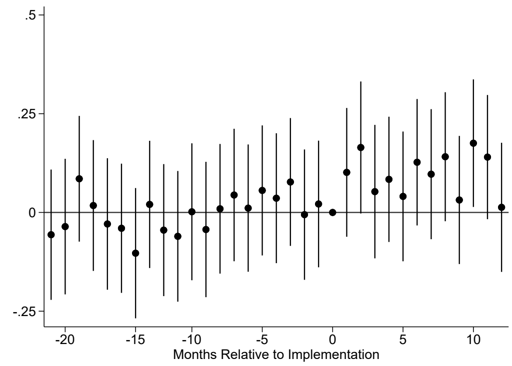
(c) ln(Number of Bills)

Notes: Each graph in the figure above displays monthly means of the indicated variable separately for "Middle Earners" (those not exposed to the reform, for whom $\Delta b_{it} = 0$) and for "High Earners" (those exposed to the reform, for whom $\Delta b_{it} > 0$). The sample contains 63,155 claims that occurred from January 2005 to September 2007 that meet the sample restrictions described in the text.

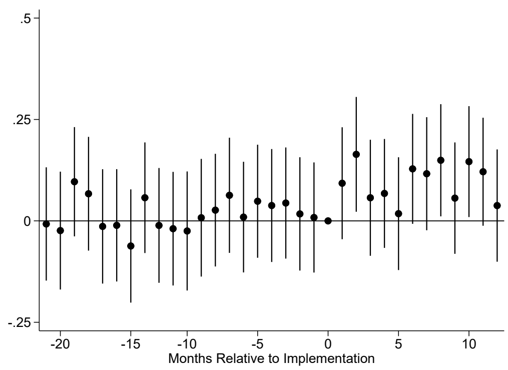
Figure 4: Impact of Benefit Change on Benefit Duration and Medical Utilization



(a) ln(Benefit Duration)



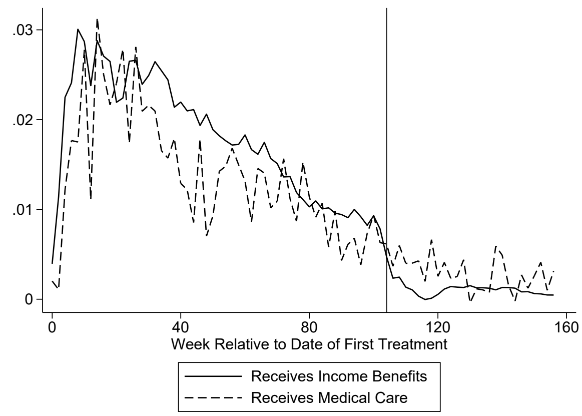
(b) ln(Medical Spending)



(c) ln(Number of Bills)

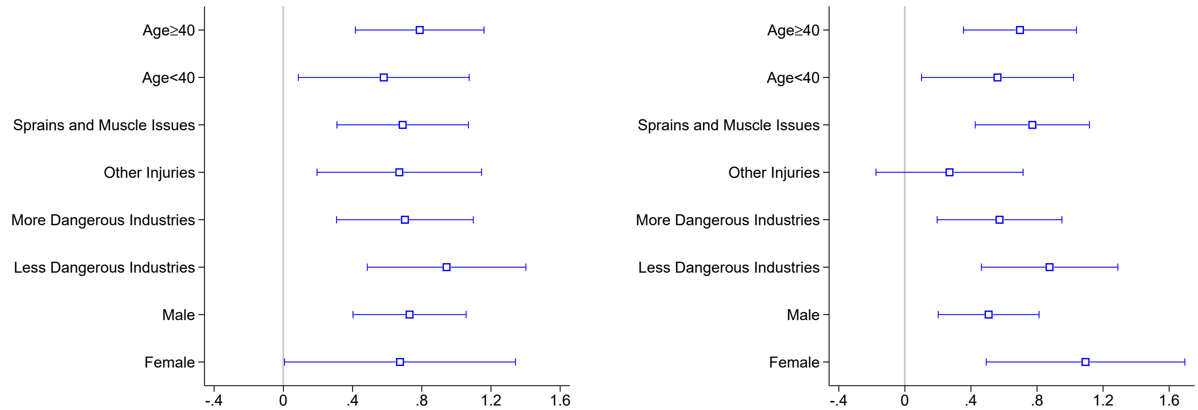
Notes: Each graph in the figure above displays coefficients on the scaled change-in-benefit measure interacted with indicators for the month the injury occurred relative to the implementation of the reform from separate regressions of Equation (2) along with 95% confidence intervals calculated using robust standard errors. The interaction for the injury month immediately prior to the reform is omitted. The sample contains 63,155 claims that occurred from January 2005 to September 2007 that meet the sample restrictions described in the text. Each regression includes county by injury month-year 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) change-in-benefit, a male indicator variable, and a full vector of age indicator variables.

Figure 5: Timing of Effects on Receipt of Benefits and Medical Care



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 the receipt of income benefits and medical care for each two-week period relative to the start of the injury. To calculate the 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 change-in-benefit measure interacted with a post-reform indicator variable. Each regression includes county by injury month-year 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) change-in-benefit, 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 63,155 observations, one for each claim that occurred from January 2005 to September 2007.

Figure 6: Heterogeneity in Impacts by Claimant Characteristics



(a) ln(Benefit Duration)

(b) ln(Medical Spending)

Notes: This figure displays IV estimates (and the associated 95% confidence intervals) from separate regressions including the indicated subgroup of claimants and the baseline controls. Claims with missing values for industry and impairment type are not included in either sample when assessing heterogeneity along the respective dimension. Reported confidence intervals are based on robust standard errors.

Table 1: Descriptive Statistics

	All		High Earners		Middle Earners	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Benefit duration	17.96	24.19	17.85	23.99	18.07	24.42
Medical spending (5 years)	12,484	18,464	12,730	18,788	12,213	18,096
Weekly benefit amount	517	85	581	58	448	49
Pre-injury weekly average earnings	863	284	1,065	251	640	69
Replacement rate	0.63	0.10	0.57	0.11	0.70	0.00
ΔWeeklyBenefit	53.62	60.26	102.36	44.09	0.00	0.00
Age	42.60	11.13	43.62	10.71	41.46	11.48
1{Male}	0.78	0.42	0.80	0.40	0.75	0.43
1{Married}	0.61	0.49	0.63	0.48	0.57	0.49
Impairment Type:						
1{Contusion}	0.09	0.29	0.08	0.28	0.10	0.29
1{Fracture}	0.14	0.34	0.14	0.35	0.13	0.34
1{Laceration}	0.03	0.17	0.03	0.16	0.03	0.18
1{Muscle Issue}	0.30	0.46	0.30	0.46	0.29	0.45
1{Sprain}	0.31	0.46	0.31	0.46	0.31	0.46
1{Other}	0.14	0.34	0.14	0.34	0.14	0.34
1{ED Claim}	0.31	0.46	0.32	0.47	0.30	0.46
1{Permanent Impairment}	0.44	0.50	0.45	0.50	0.42	0.49
Permanent impairment rating (if > 0)	6.29	7.15	6.32	7.43	6.25	6.81

Notes: This table displays descriptive statistics for the 63,155 claims that occurred from January 2005 to September 2007 that meet the sample restrictions described in Section 1. Descriptive stats are shown for the baseline sample ("All"), and these statistics are also shown separately for "Middle Earners" (those not exposed to the reform, for whom $\Delta b_{it} = 0$) and for "High Earners" (those exposed to the reform, for whom $\Delta b_{it} > 0$).

Table 2: Claimant Composition: Balance on Observable Characteristics

	ΔwkBenefit_scaled x Post			
	Coef	Std Err	P-value	Mean Dep Var
	(1)	(2)	(3)	(4)
Age	-0.301	(0.167)	[0.072]	43.68
Male	0.001	(0.006)	[0.855]	0.801
ED Claim	-0.009	(0.007)	[0.209]	0.303
Married	0.007	(0.008)	[0.351]	0.632
Impairment Type:				
Contusion	0.006	(0.004)	[0.187]	0.083
Fracture	-0.005	(0.005)	[0.365]	0.130
Laceration	0.000	(0.003)	[0.927]	0.024
Muscle Issue	0.000	(0.007)	[0.996]	0.306
Sprain	0.010	(0.007)	[0.135]	0.307
ln(First Day Medical Spending)	-0.005	(0.020)	[0.812]	5.933
Industry: More Dangerous	0.008	(0.008)	[0.292]	0.583
Predicted ln(Benefit Duration)	0.002	(0.003)	[0.373]	1.997
Predicted ln(Five Year Medical Spending)	0.005	(0.005)	[0.310]	8.592

Notes: This table displays estimates of the coefficient on the scaled change-in-benefit measure interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (3) that control for county by injury month-year fixed effects and the claimant's scaled change-in-benefit. 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 3: Impact of Benefit Change: Reduced Form Estimates

	(1)	(2)	(3)	(4)	(5)
Panel A: Benefit Duration					
ΔwkBenefit_scaled x Post	0.111	0.098	0.101	0.118	2.037
	(0.023)	(0.021)	(0.022)	(0.043)	(0.366)
	[<0.001]	[<0.001]	[<0.001]	[0.006]	[<0.001]
Panel B: Medical Spending (cumulative in five years since injury)					
ΔwkBenefit_scaled x Post	0.098	0.076	0.097	0.084	1,203.055
	(0.021)	(0.020)	(0.021)	(0.037)	(279.328)
	[<0.001]	[<0.001]	[<0.001]	[0.024]	[<0.001]
Panel C: Number of Medical Bills (cumulative in five years since injury)					
ΔwkBenefit_scaled x Post	0.080	0.067	0.079	0.066	3.352
	(0.018)	(0.017)	(0.017)	(0.032)	(0.927)
	[<0.001]	[<0.001]	[<0.001]	[0.036]	[<0.001]
Sample Restriction				ED Claims	
Controls					
Time and ΔwkBenefit Controls	x	x	x	x	x
Basic Controls	x		x	x	x
Expanded Controls			x		
Dep Var	Nat. Log	Nat. Log	Nat. Log	Nat. Log	Level
Pre-Mean Dep Var, Levels					
Benefit Duration	17.71	17.71	17.71	17.90	17.71
Medical Spending	12,451	12,451	12,451	14,406	12,451
Number of Medical Bills	44.06	44.06	44.06	45.47	44.06
N	63,155	63,155	63,155	19,765	63,155
First Stage					
ΔwkBenefit_scaled x Post	0.155	0.155	0.155	0.158	96.794
	(0.001)	(0.001)	(0.001)	(0.003)	(0.627)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]

Notes: This table displays estimates of the coefficient on the scaled change-in-benefit variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (3) with the indicated dependent variables. The sample includes claims that occurred from January 2005 to September 2007. All regressions include injury month-year fixed effects and the claimant's (scaled) change-in-benefit. In addition to these controls, regressions in columns 1 and 3-5 also include the following controls: county by injury month-year fixed effects, a male indicator variable, a full vector of age indicator variables, 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. The regression in column 3 also includes insurer fixed effects and controls for injury type. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table 4: Impact of Benefit Generosity: Instrumental Variables Estimates

	Elasticity (Ln-Ln specification)				Level-Level Specification
	(1)	(2)	(3)	(4)	(5)
Panel A: Benefit Duration					
Weekly Benefit	0.716	0.632	0.652	0.747	0.021
	(0.147)	(0.138)	(0.143)	(0.271)	(0.004)
	[0.000]	[0.000]	[0.000]	[0.006]	[0.000]
Panel B: Medical Spending (cumulative in five years since injury)					
Weekly Benefit	0.634	0.491	0.625	0.533	12.429
	(0.136)	(0.130)	(0.133)	(0.236)	(2.887)
	[0.000]	[0.000]	[0.000]	[0.024]	[0.000]
Panel C: Number of Medical Bills (cumulative in five years since injury)					
Weekly Benefit	0.518	0.430	0.510	0.420	0.035
	(0.115)	(0.109)	(0.112)	(0.200)	(0.010)
	[0.000]	[0.000]	[0.000]	[0.036]	[0.000]
Sample Restriction				ED Claims	
Controls					
Time and ΔwkBenefit Controls	x	x	x	x	x
Basic Controls	x		x	x	x
Expanded Controls			x		
Pre-Mean Dep Var, Levels					
Benefit Duration	17.71	17.71	17.71	17.90	17.71
Medical Spending	12,451	12,451	12,451	14,406	12,451
Number of Medical Bills	44.06	44.06	44.06	45.47	44.06
N	63,155	63,155	63,155	19,765	63,155
First Stage					
ΔwkBenefit_scaled x Post	0.155	0.155	0.155	0.158	96.794
	(0.001)	(0.001)	(0.001)	(0.003)	(0.627)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]

Notes: This table displays estimates from instrumental variables (IV) specifications for the primary outcomes, using the baseline sample and the indicated controls. See Table 3 table notes for more information on the sample and included controls. In specifications reported in columns 1 through 4, the dependent variable and the weekly benefit rate are transformed by the natural logarithm. In the specifications reported in column 5, the dependent variable and weekly benefit rate enter linearly. The instrument for the weekly benefit rate (in logs and levels) is the scaled change-in-benefit variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table 5: Impact of Benefit Generosity on Categories of Medical Spending

Panel A

	Total		Office Visits		Case Management		Physical Therapy	
	Spending ($)	Bills (#)	Spending ($)	Bills (#)	Spending ($)	Bills (#)	Spending ($)	Bills (#)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln(Weekly Benefit)	0.634	0.518	0.744	0.528	1.137	0.603	1.440	0.942
	(0.136)	(0.115)	(0.190)	(0.113)	(0.261)	(0.123)	(0.360)	(0.195)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Pre-Mean Dep Var, Levels	12,451	44.1	781	10.5	1,134	9.6	1,376	27.6
N	63,155	63,155	63,155	63,155	63,155	63,155	63,155	63,155

Panel B

	Prescription Drugs		Surgeries		Emergency Visits		Diagnostic Radiology	
	Spending ($)	Bills (#)	Spending ($)	Bills (#)	Spending ($)	Bills (#)	Spending ($)	Bills (#)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln(Weekly Benefit)	1.150	0.524	0.377	0.121	0.165	0.067	0.730	0.321
	(0.319)	(0.157)	(0.344)	(0.068)	(0.237)	(0.054)	(0.264)	(0.107)
	[<0.001]	[0.001]	[0.272]	[0.078]	[0.487]	[0.209]	[0.006]	[0.003]
Pre-Mean Dep Var, Levels	1,036	12.9	355	0.8	1,161	1.1	762	6.3
N	63,155	63,155	63,155	63,155	63,155	63,155	63,155	63,155

Notes: This table displays IV estimates from separate regressions with the indicated dependent variables for different categories of medical care, using the baseline sample and controls. See Table 3 table notes for more information on the baseline sample and controls. The dependent variables are the natural logarithm of the total spending measures (in Panel A columns 1 and 2) and the inverse hyperbolic sine of the indicated measures in the remaining columns. The instrument for the natural logarithm of the weekly benefit rate is the scaled change-in-benefit variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table 6: Marginal Welfare Impact of Increase in Benefits and MVPF

Coefficient of Relative Risk Aversion (γ)	Baseline Estimates	Baseline Duration Elasticity (ignoring impact on medical spending)	Baseline Medical Spending Elasticity (ignoring impact on income benefit duration)
	(1)	(2)	(3)
Panel A. Marginal Welfare Impact of Increase in Benefits, dW/db X 0.05b			
1	-\$0.085	-\$0.040	-\$0.039
2	-\$0.079	-\$0.033	-\$0.032
3	-\$0.072	-\$0.027	-\$0.026
4	-\$0.066	-\$0.020	-\$0.019
5	-\$0.059	-\$0.014	-\$0.013
Panel B. MVPF			
1	0.42	0.57	0.65
2	0.46	0.63	0.71
3	0.50	0.68	0.77
4	0.54	0.73	0.82
5	0.58	0.79	0.88
Duration Elasticity, $\epsilon_{D\_B,b}$	0.67	0.67	0.00
Medical Spending Derivative, dM/db	12.39	0.00	12.39

Notes: Each cell in Panel A displays the calibrated marginal welfare impact (in terms of weekly dollars per capita) 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. As discussed in Section 4, this calibration is based on the approximation in Equation (8) and relies on the relevant behavioral elasticity estimates, additional moments from our data, and an estimate of the mean consumption drop experienced by workers nationally after a work-limiting workplace injury. Each cell in Panel B represents the calibrated MVPF in a separate scenario. This calibration is based on the approximation in Equation (9) and relies on the relevant behavioral elasticity estimates, additional moments from our data, an estimate of the mean tax-and-transfer rate, and an estimate of the mean consumption drop experienced by workers nationally after a work-limiting workplace injury. In both panels, 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 estimates. Column 2 reports calibrations based on our duration elasticity estimate but assumes no effect on medical spending. Column 3 reports calibrations based on our medical spending estimate but assumes no effect on the income benefit duration.

Table 7: Effect of Income Benefit Change on Permanent Impairments

	I(Impairment benefits>0) (1)	Impairment Severity (2)	ln(Impairment Severity) (3)
ΔwkBenefit_scaled x Post	0.002	0.038	-0.022
	(0.009)	(0.110)	(0.032)
	[0.868]	[0.731]	[0.497]
Pre-Mean Dep Var, Levels	0.448	2.854	6.373
N	44,259	44,259	19,682

Notes: This table displays estimates of the coefficient on the scaled change-in-benefit variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (3) with the indicated dependent variables. The sample includes claims that occurred from January 2005 to September 2007 with pre-injury weekly wages of $675 to $2,000. See Appendix Section H for further discussion on the definition of the sample and dependent variables in this analysis. Each regression includes county by injury month-year 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 change-in-benefit, a male indicator variable, and a full vector of age indicator variables. Robust standard errors are reported in parentheses and p-values in brackets.

Table 8: Effect of Income Benefit Change: Outcomes Measured Over Different Horizons

Panel A: Dependent Variable: Inv Hyp Sine (Weeks Receiving Income Benefits)								
	0-3 months	4-6 months	7-12 months	13-18 months	19-24 months	25-36 months	37-48 months	49-60 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔwkBenefit_scaled x Post	0.056	0.079	0.104	0.068	0.044	0.009	0.003	0.000
	(0.018)	(0.021)	(0.022)	(0.017)	(0.013)	(0.005)	(0.002)	(0.001)
	[0.002]	[<0.001]	[<0.001]	[<0.001]	[0.001]	[0.064]	[0.079]	[0.637]
Pre-Mean Dep Var, Levels	6.04	3.74	4.19	2.00	1.07	0.13	0.01	0.01
N	63,155	63,155	63,155	63,155	63,155	63,155	63,155	63,155

Panel B: Dependent Variable: Inv Hyp Sine (Number of Bills)								
	0-3 months	4-6 months	7-12 months	13-18 months	19-24 months	25-36 months	37-48 months	49-60 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔwkBenefit_scaled x Post	0.034	0.097	0.101	0.078	0.064	0.038	0.023	0.009
	(0.013)	(0.022)	(0.025)	(0.021)	(0.018)	(0.018)	(0.014)	(0.012)
	[0.009]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[0.032]	[0.110]	[0.452]
Pre-Mean Dep Var, Levels	16.70	6.94	8.06	4.28	2.85	3.18	1.95	1.47
N	63,155	63,155	63,155	63,155	63,155	63,155	63,155	63,155

Panel C: Dependent Variable: Inv Hyp Sine (Medical Spending)								
	0-3 months	4-6 months	7-12 months	13-18 months	19-24 months	25-36 months	37-48 months	49-60 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔwkBenefit_scaled x Post	0.051	0.230	0.210	0.173	0.198	0.125	0.065	0.026
	(0.020)	(0.059)	(0.063)	(0.056)	(0.048)	(0.044)	(0.035)	(0.030)
	[0.010]	[<0.001]	[0.001]	[0.002]	[<0.001]	[0.005]	[0.062]	[0.377]
Pre-Mean Dep Var, Levels	5,310	1,959	2,256	1,146	768	848	504	397
N	63,155	63,155	63,155	63,155	63,155	63,155	63,155	63,155

Notes: This table displays estimates of the coefficient on the scaled change-in-benefit variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule from regressions of Equation (3) with the indicated dependent variables. See Table 3 table notes for more information on the sample and included controls. Robust standard errors are reported in parentheses and p-values are reported in brackets.

APPENDIX

A Coverage Rates

As discussed in Section 1, 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. While coverage is voluntary in Texas, institutional details and supplementary evidence suggest that this feature is not likely to affect the internal validity of our results. There is no evidence of a change in the number of claimants or the composition of claimants based on observables with respect to our identifying variation, as discussed in Section 2. Further, below 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 with wage-inflation-adjusted pre-injury weekly earnings exceeding the initial maximum benefit, among all workers' compensation claimants with cash benefits. To assess whether more exposed classifications saw a differential change in coverage, we estimate a flexible difference-in-differences specification regressing the natural logarithm of covered payroll initiated in a given month within a classification on interactions of month relative to implementation and an indicator for the top quartile of classifications based on the fraction of claimants with earnings above the initial cap. We also estimate a parallel specification replacing the dependent variable with mean premiums within each classification-year. Appendix 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 or mean premiums 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.

B Permanent Impairment Benefits

As discussed in Section 1, 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. We note that 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. And, if individuals are sufficiently forward-looking and informed, quantifying 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, the data and variation allow 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 addition, 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:

$$\text{permanent impairment benefit} = \text{Rate}(w_i) \times s_i. \tag{10}$$

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 that 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 the scaled change-in-impairment-benefit variable as:

$$\Delta\text{ImpairmentBenefit}_i\text{\_scaled} = \frac{\text{Rate}^{new}(w_i) - \text{Rate}^{old}(w_i),}{\frac{1}{|\mathcal{J}|}\sum_{i\in\mathcal{J}}[\text{Rate}^{new}(w_i) - \text{Rate}^{old}(w_i)]}, \tag{11}$$

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_i is the pre-injury average weekly wage of individual i who was injured in month-year $t(i)$, and $\mathcal{J}$ represents the set of claimants exposed to the impairment rate reform ($\mathcal{J} \equiv \{i : \text{Rate}^{new}(w_i) - \text{Rate}^{old}(w_i) > 0\}$). Using this exposure measure, we estimate difference-in-differences specifications of the following form:

$$y_i = \rho_{t(i)} + \delta\Delta\text{ImpairmentBenefit}_i\text{\_scaled} + \left[\pi \times I_{t(i)\geq t_0} \times \Delta\text{ImpairmentBenefit}_i\text{\_scaled}\right] + \Theta\mathbf{X_i} + \varepsilon_i. \tag{12}$$

Appendix Table A3 displays these estimates. Panel A focuses on all claimants with income benefits and pre-injury earnings between \$375 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 the 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.[1] Nevertheless, under these fairly strong assumptions, the unconditional cash benefit natural experiment could be informative about liquidity effects.

The results in Appendix Table A3 indicate that increasing the unconditional cash payment has no detectable effect on the duration claiming income benefits or medical spending. We note there are a couple of possible ways to interpret these findings. First, it could be that this is a reasonable test of liquidity effects,

[1]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.

with these findings suggesting that liquidity effects are not quantitatively important in this setting. 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.[2]

Appendix Table A4 presents additional robustness analysis verifying that the increase in permanent impairment benefit generosity does not confound the identification of the main estimates of interest: the effect of income benefits on income benefit duration and medical spending. The first two rows display our baseline estimates for reference. The remaining rows contain alternative specifications which 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 supplement Equation (3) 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. The final specification in Appendix Table A4 supplements Equation (3) 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.

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 (2021), Fomenko and Gruber (2019)).[3] 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 the 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.

[2] We note that the former interpretation—that liquidity effects 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 have 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.

[3] 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.

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. Appendix Table A6 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, and, upon receiving a claim for diagnostic imaging, health insurers often request further information from the patient about whether the imaging was due to an injury/accident, the location of the injury, and other potentially liable parties/insurers. Overall, these strategies to combat cost-shifting for diagnostic radiology may limit cost-shifting for these procedures relative to other types of procedures.

If the reform increased workers' compensation insurers' medical spending simply 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, as 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. Appendix Table A6 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 Additional Evidence on Robustness

In this section, we describe additional evidence on robustness. We conduct two types of placebo exercises, and we describe each of these below.

Placebo test 1. Holding fixed the reform timing, varying the "treated" group In this exercise, we estimate changes in outcomes at the time of the reform across the pre-injury wage distribution. This analysis helps us assess whether there appear to be potential confounding factors related to pre-injury wages that changed when the reform was implemented and whether the effects scale with the size of the benefit change among treated claimants. For this analysis, we first classify injured workers into ventiles based on pre-injury wages. We then estimate separate impacts of the policy by pre-injury wage bin, where each bin represents a ventile aside from the top bin which pools all fully treated ventiles in a single bin. Specifically, we estimate the following expanded version of our difference-in-difference equation:

$$y_{it} = \rho_t + \delta_v + \left[\sum_v \gamma_v \times \mathbb{1}(t(i) \geq t_0) \times \mathbb{1}(w_i \in v)\right] + f(X_{it}) + \varepsilon_{it}, \tag{13}$$

where v indicates bin based on pre-injury wages w_i. We report the bin-specific coefficients γ_v in Appendix Figure A9, where the horizontal axes are labeled with the mean size of the increase in benefits (in percent terms) from the reform within that bin. As would be expected, the policy does not appear to be associated with differential changes in outcomes among those in non-treated bins. While the analysis is not precise enough to statistically distinguish among estimates for partially treated bins, the estimated policy coefficients generally rise with treatment intensity for the partially treated bins.

Placebo test 2. Holding fixed the treatment and control groups, varying the timing of "treatment" Next, we conduct a placebo exercise where we hold the definition of the treatment and control groups fixed but consider whether there were changes in outcomes one year before or after the actual reform was implemented. To conduct this analysis, we estimate our baseline pooled specification in Equation (3), where we vary the cutoff in the injury date that is used to define the "after" period. Specifically, we separately estimate this specification using three different cutoff dates—October 1, 2005 (one year before the reform was implemented), October 1, 2006 (the reform implementation date), and October 1, 2007 (one year after the reform was implemented). In this estimation, we constrain the sample to be workers injured within 12 months of the relevant cutoff date. The results are displayed in Appendix Figure A10, where we show difference-in-differences estimates depicting how benefits changed at each of these dates as well as mean changes in our main outcomes—income benefit duration and medical spending. As can be seen in Appendix Figure A2, inflation adjustments lead to minor changes in the weekly benefits at the placebo implementation dates, though these changes are small relative to the large increase in benefits for the treated claimants at the true implementation date. At the true implementation date, we see large changes in our main outcomes, consistent with our baseline analysis. In contrast, we observe no statistically significant changes in our main outcomes—income benefit duration or medical spending—at the placebo implementation dates.

E Additional Supplemental Evidence

In this section, 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:

$$y_{is} = \sum_{s} \beta_s \mathbb{1}(s) + \gamma_i + \epsilon_{is}, \tag{14}$$

where γ_i is a claimant fixed effect. We normalize $\beta_0 = 0$. The coefficients of interest are the vector β_s, which depicts the relationship between medical utilization and the week that income benefits are terminated. Appendix Figure A11 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 the primary analysis investigating the causal impact of income benefit generosity on medical spending.

F Welfare Formulas

We define some notation used in the derivations 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. Let $M = \sum_{t=0}^{T-1} m_t$. Define $\mu_t^N \equiv \frac{S_t}{D_B}$ and $\mu_t^W \equiv \frac{f_t(T-t)}{T-D}$. Then $\overline{c_W} \equiv \sum_{t=0}^{T-1} \mu_t^W c_t^W$ 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.

F.1 Derivation of Exact Formula

We begin by describing the derivation of the exact welfare formula stated below. We then turn to the derivation of the approximation described in Equation (8) in the paper.

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:*

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

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:

$$\begin{aligned}\frac{dJ_0}{db} &= (1-e_0)\frac{\partial U_0}{\partial b} + e_0\frac{\partial V_0}{\partial b} - \frac{\partial \tau}{\partial b}((1-e_0)\frac{\partial U_0}{\partial w} + e_0\frac{\partial V_0}{\partial w}) \\ &= (1-e_0)\frac{\partial U_0}{\partial b} - \frac{\partial \tau}{\partial b}\frac{dJ_0}{dw}. \end{aligned} \tag{15}$$

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:

$$\begin{aligned}\frac{dJ_0}{dw} &= (1-e_0)\frac{\partial U_0}{\partial w} + e_0\frac{\partial V_0}{\partial w} \\ &= \sum_{t=0}^{T-1} f_t(T-t)u'(c_t^W). \end{aligned} \tag{16}$$

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:

$$\begin{aligned}(1-e_0)\frac{dU_0}{db} &= \sum_{t=0}^{B-1}\prod_{i=0}^{t}(1-e_i)u'(c_t^N) \\ &= \sum_{t=0}^{B-1} S_t u'(c_t^N). \end{aligned} \tag{17}$$

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

$$\frac{d\tau}{db} = \frac{D_B}{T-D}\left[1 + \epsilon_{D_B,b} + \frac{dM}{db}\frac{1}{D_B} + \epsilon_{D,b}\frac{D}{T-D}(1+\frac{M}{D_B})\right]. \tag{18}$$

Using expressions 15 through 18 above, we can derive the money-metric welfare gain of increasing the generosity of benefits as follows:

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

F.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:

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

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

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

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 $\frac{\Delta c}{c} \equiv \frac{\overline{c_W}-\overline{c_N}}{\overline{c_W}}$.

It is straightforward to generalize this formula to the case when $\rho \neq 0$. Following the approximation in Kroft and Notowidigdo (2016), we get the following approximate formula if $\rho \neq 0$:

$$\frac{dW}{db} \approx \frac{D_B}{D}\frac{\theta}{1-\theta}\left(\left[\gamma\frac{\Delta c}{c}(1+\frac{1}{2}\rho\frac{\Delta c}{c})+1\right]F-1-\epsilon_{D_B,b}-\epsilon_{D_B,b}\frac{\theta}{1-\theta}(1+\frac{M}{D_B b})-\frac{dM}{db}\frac{1}{D_B}\right),$$

where $F \equiv 1+\frac{1}{2}\gamma\rho\phi_N^2$. Under the assumption that the coefficient of variation in consumption when not working is zero ($\phi_N = 0$), then we get the following approximation:

$$\frac{dW}{db} \approx \frac{D_B}{D}\frac{\theta}{1-\theta}\left(\left[\gamma\frac{\Delta c}{c}(1+\frac{1}{2}\rho\frac{\Delta c}{c})\right]-\epsilon_{D_B,b}-\epsilon_{D_B,b}\frac{\theta}{1-\theta}(1+\frac{M}{D_B b})-\frac{dM}{db}\frac{1}{D_B}\right).$$

G Estimation of the Consumption Drop Among Injured Workers

We estimate the consumption drop among workers who experience a workplace injury using data from the Health and Retirement Study (HRS), pooling data from 1992 to 2016. The HRS is the only dataset with information on both consumption and location of injury.[4]

We follow Bronchetti (2012) in our approach to identifying injured workers and measuring food consumption in the HRS data, though we pool data from a longer time span to maximize our sample size. To identify injured workers, we use 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 $t-1$. We concentrate on impairments that are reported to have been "caused by the nature of [the respondent's] work" and limit the sample to individuals employed in period $t-1$. Food consumption is measured as the sum of three components: (i) food consumption at home (excluding food stamps), (ii) food consumption away from home (including "take out" food), (iii) the value of food stamps used by the household.

Our strategy uses the change in total food consumption upon workplace injury as a proxy for the change in total consumption. We measure changes in total food 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 $t-1$.[5,6]

An advantage of our approach to analyzing welfare is that it only requires estimating the mean consumption drop, which is possible to estimate precisely using HRS data. We estimate the following regression:

$$\Delta \log C_{ist} = \theta_0 + \theta_t + \theta_s + \mathbf{X_{ist}}\beta + e_{ist}, \tag{19}$$

where we include state fixed effects (θ_s), year fixed effects (θ_t), and a vector of control variables ($\mathbf{X_{ist}}$) that includes age, household size, change in household size from the previous interview, the log of the weekly wage in the previous interview, the log of the weekly workers' compensation benefits the injured worker would be entitled to (based on injury date, state, and prior weekly wage), indicators for the respondent being white, black, Hispanic, male, and married, and indicators for respondent education (having graduated from high school, having some college, and having graduated from college). We de-mean all the right-hand-side variables, so the estimate of θ_0 can be interpreted as the mean consumption drop among injured workers.

Appendix Table A8 reports the estimates. Each column reports the mean consumption drop from sep-

[4]In this analysis, all dollar values are adjusted to 2006 values using the CPI-U.

[5]The 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 (8) which indicates that the survival function should be used to create 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.

[6]We exclude the few observations for which respondents report an increase in food consumption of more than 300%.

arate regressions, where sample restrictions are as indicated in the columns. Column 1 includes the full sample. Columns 2 and 4 include only respondents with a benefit level within 10% of Texas's pre-reform maximum benefit level. Columns 3 and 4 include only respondents whose pre-injury inflation-adjusted weekly wages exceeded $771 (the earnings threshold corresponding to the old schedule maximum benefit in Texas).

Based on the estimate for the full sample, injured workers experience a 10.1% drop in consumption upon workplace injury. We obtain similar estimates when restricting the sample to workers that are similar to the population marginal to the reform we analyze in terms of weekly benefit level and/or earnings, with estimates ranging from a 7.2% drop to a 11.2% drop.

To verify that the drop in consumption does not reflect a pre-existing trend of decreased consumption prior to an injury, Appendix Figure A12 displays the estimated consumption change after injuries for the individuals in the sample from column 1 of Appendix Table A8 relative to their change in consumption over the two periods prior to the injury. Specifically, this figure displays estimates ψ_k from the following specification:

$$\Delta \log C_{ist} = \sum_{k=-1}^{1} \psi_k \times \mathbb{1}(r(i,t) = k) + \gamma_t + \gamma_s + \mathbf{X_{ist}}\omega + \mu_{ist}, \tag{20}$$

where $r(i,t)$ indicates the period relative to injury (with 0 indicating the period just prior to the injury), and the remaining controls are as in Equation (19). We normalize the consumption change in the period immediately prior to the injury to zero ($\psi_0 = 0$). Appendix Figure A12 shows no evidence of trend in consumption changes prior to the injury, and the estimated drop in consumption after the injury is similar to that in Appendix Table A8 column 1.

H Impact of Income Benefits on Permanent Impairment Benefits

Table 7 displays estimates of the effect of the temporary income benefit increase on claimants' permanent impairment severity ratings. For this analysis, we limit the sample to claimants with pre-injury weekly average earnings of at least $675 so that all claimants in the sample experience the same changes in permanent impairment benefit rates over time from the change in the maximum permanent impairment benefit rate discussed in Section 1 and in Appendix Section B. In column 1 of Table 7, the dependent variable is an indicator variable equal to one for claimants assessed as having a permanent impairment. In column 2, the dependent variable is claimants' permanent impairment ratings, which range from 0 for claimants with no permanent impairment to 100 for claimants assessed as being completely unable to work again because of the injury. The estimated impact of the increase in temporary income benefits is statistically indistinguishable from zero across all specifications. Thus, this analysis provides no evidence that the reform-induced increase in income benefit generosity affected the share of claimants assessed as being permanently impaired or claimants' permanent impairment severity ratings.

I Robustness of MVPF Analysis

I.1 Accounting for Potential Externalities on Other Health Care Payers

Building on a related discussion in the text, here we present additional analysis of the sensitivity of the implied MVPF to incorporating potential externalities on other health care payers. Specifically, we consider scenarios where we suppose X% (where we vary X) of the induced increases in workers' compensation medical spending represents medical utilization that would have occurred in the absence of the reform and that would have been eligible for coverage from other sources of health insurance, where the incidence of other sources of health insurance either falls on the government (e.g., government-reimbursed charity care, public health insurance programs) or falls on other individuals (e.g., other employees, other health care consumers, business owners or shareholders). In these calculations, we assume that health insurance provides coverage of 70% actuarial value (whereas workers' compensation insurance provides full coverage of medical expenses), and we assume other individuals have the same social welfare weights as individuals covered by workers' compensation insurance. When considering scenarios where the incidence of other health care costs falls on the government, we alter the MVPF approximation from Equation (9) as follows,

$$MVPF = \frac{1 + \gamma \frac{\Delta c}{c}}{1 + \epsilon_{D_B,b}(1 + \frac{D}{D_B}\frac{\tau}{b}) + (1 - 0.7X)\frac{1}{D_B}\frac{dM}{db}}. \tag{21}$$

When we instead consider scenarios where the incidence of other health care costs falls on other individuals, we alter the MVPF approximation from Equation (9) as follows,

$$MVPF = \frac{1 + \gamma \frac{\Delta c}{c} + 0.7X\frac{1}{D_B}\frac{dM}{db}}{1 + \epsilon_{D_B,b}(1 + \frac{D}{D_B}\frac{\tau}{b}) + \frac{1}{D_B}\frac{dM}{db}}. \tag{22}$$

Appendix Figure A13 presents this robustness analysis. Recall the MVPF under the baseline assumption of no other external impacts is 0.46. In the scenario where 25% of the induced increase in workers' compensation medical spending represents cost-shifting from health insurance, the implied MVPF is 0.51 if the incidence falls on other individuals and 0.48 if the incidence falls on the government. In the more extreme scenario where 50% of the induced increase in workers' compensation medical spending represents cost-shifting from health insurance, the implied MVPF is 0.55 if the incidence falls on other individuals and 0.51 if the incidence falls on the government. Across the range of potential external effects considered, the MVPF ranges from 0.46 to 0.55.

I.2 Accounting for Potential Externalities on Health Care Providers

As discussed in the text, if health care is competitively provided then there are no externalities on health care providers. However, if instead health care providers make rents on care provided to workers' compensation patients (relative to the outside option), then a broader welfare evaluation should account for these rents as positive externalities on health care providers from increased income benefit generosity. We consider the impact on the calibrated MVPF when accounting for potential externalities on health care providers, assuming health care providers have the same social welfare weights as individuals covered by workers' compensation insurance. In these calculations, we hold risk aversion fixed at two. We vary the rents that health care providers collect on the marginal care provided to workers' compensation patients (relative to the outside option) and denote these rents as X below, where X represents the share of medical spending that is attributed to rents to health care providers relative to the outside option.[7] We can then write the adjusted MVPF as follows,

$$MVPF = \frac{1 + \gamma \frac{\Delta c}{c} + X\frac{1}{D_B}\frac{dM}{db}}{1 + \epsilon_{D_B,b}(1 + \frac{D}{D_B}\frac{\tau}{b}) + \frac{1}{D_B}\frac{dM}{db}}. \tag{23}$$

Appendix Figure A14 presents the results of this analysis. The baseline MVPF is 0.46 under the assumption of no external impacts on health care providers. In the scenario where 25% of the induced increase in workers' compensation medical spending represents rents collected by health care providers relative to the outside option, the implied MVPF is 0.53. In the more extreme (and, in our view, less realistic) scenarios where 50% (75%) of the induced increase in workers' compensation medical spending represents rents collected by health care providers relative to the outside option, the implied MVPF is 0.59 (0.66). Across the range of potential external effects considered, the MVPF ranges from 0.46 to 0.66.

[7] If $X = 0$, there is no positive externality on health care providers (i.e., rents are zero because the price paid by workers' compensation equals the opportunity cost of the inputs used to provide these services). If $X = 1$, all of the additional medical spending reflects rents collected by providers (i.e., rents equal the full cost of the medical spending paid by workers' compensation insurance because the opportunity cost of inputs to provide these services is zero). Because the opportunity cost of inputs to provide medical care is typically positive, we view this latter scenario as unrealistic.

Table A1: Comparison of Injured Workers in Texas and All States

	Texas	All States	Texas Weekly Earnings > $771	All States Weekly Earnings > $771
Age	44.2	45.3	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	$60,931	$85,475	$91,833
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/Accommodation/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 Administration/Other Services	6.7%	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 those with inflation-adjusted prior earnings exceeding $771 (=$540/0.70) in Texas and all states, respectively. All dollar values are CPI-U adjusted to 2006 dollars.

Table A2: Impact of Benefit Change on Weekly Benefit Rate

Panel A: Weekly Benefit Rate	(1)	(2)	(3)	(4)	(5)
ΔwkBenefit x Post	0.927	0.928	0.925	0.940	0.001
	(0.006)	(0.006)	(0.006)	(0.011)	(0.000)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Sample Restriction				ED Claims	
Controls					
Time and ΔwkBenefit Controls	x	x	x	x	x
Basic Controls	x		x	x	x
Expanded Controls			x		
Dep Var	Level	Level	Level	Level	Nat. Log
Pre-Mean Dep Var, Levels	540	540	540	540	540
N	63,155	63,155	63,155	19,765	63,155

Panel B: Weekly Benefit Rate	(1)	(2)	(3)	(4)	(5)
ΔwkBenefit_scaled x Post	0.155	0.155	0.155	0.158	96.794
	(0.001)	(0.001)	(0.001)	(0.003)	(0.627)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Sample Restriction				ED Claims	
Controls					
Time and ΔwkBenefit Controls	x	x	x	x	x
Basic Controls	x		x	x	x
Expanded Controls			x		
Dep Var	Nat. Log	Nat. Log	Nat. Log	Nat. Log	Level
Pre-Mean Dep Var, Levels	540	540	540	540	540
N	63,155	63,155	63,155	19,765	63,155

Notes: This table displays estimates of the coefficient on the change-in-benefit or the scaled change-in-benefit 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. The sample includes claims that occurred from January 2005 to September 2007. All regressions include injury month-year fixed effects and the claimant's (scaled) change-in-benefit. In addition to these controls, regressions in columns 1 and 3-5 also include the following controls: county by injury month-year fixed effects, a male indicator variable, a full vector of age indicator variables, 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. The regressions in column 3 also include insurer fixed effects and controls for injury type. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table A3: 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	418.226	-0.035	0.016	0.012	0.000	0.001
	(0.002)	(86.467)	(0.023)	(0.021)	(0.018)	(0.018)	(0.007)
	[<0.001]	[<0.001]	[0.138]	[0.452]	[0.512]	[0.996]	[0.847]
Dep Var	nat. log	level	nat. log	nat. log	nat. log	inv. hyp. sine	indicator
Pre-Mean Dep Var, Levels	377.6	3151	18.17	12652	46.80	2.782	0.439
N	61,170	61,170	61,170	61,170	61,170	61,170	61,170

Panel B: Effect of Impairment Benefit Increase, Scaled by Impairment Severity among Permanently 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	0.041	1,048.222	-0.024	0.024	0.011	0.000
	(0.002)	(82.148)	(0.020)	(0.015)	(0.014)	(0.014)
	[<0.001]	[<0.001]	[0.218]	[0.100]	[0.437]	[0.991]
Dep Var	nat. log	level	nat. log	nat. log	nat. log	inv. hyp. sine
Pre-Mean Dep Var, Levels	377.6	7183	28.59	20148	72.74	6.340
N	25,491	25,491	25,491	25,491	25,491	25,491

Notes: This table displays estimates of the coefficient on the scaled change-in-impairment-benefit variable (as defined in the Appendix Section B) interacted with an indicator that the injury occurred after the implementation of the new impairment benefit schedule from regressions of Equation (12) 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 change-in-impairment-benefit variable based on claimants' pre-injury weekly wages. Panel B displays the estimates for the sample with permanent impairments and constructs the change-in-impairment-benefit variable based on claimants' impairment ratings and pre-injury weekly wages. Each regression includes county by injury month-year 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 change-in-impairment-benefit 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 A4: Further Robustness

	ΔwkBenefit_scaled x Post coef	std error	p-value	Pre-mean dep var	N
Baseline					
Benefit Duration	0.716	(0.147)	[<0.001]	17.71	63,155
Medical Spending	0.634	(0.136)	[<0.001]	12,451	63,155
Additional Control for PIB Reform					
Benefit Duration	0.587	(0.152)	[<0.001]	17.71	63,155
Medical Spending	0.436	(0.141)	[0.002]	12,451	63,155
Restrict Sample to Those without Permanent Impairment					
Benefit Duration	0.494	(0.184)	[0.007]	9.91	35,555
Medical Spending	0.437	(0.176)	[0.013]	7,329	35,555
Additional Control for PIB Reform (ΔImpairmentBenefit_scaled x PIB rating)					
Benefit Duration	0.757	(0.138)	[<0.001]	17.71	63,155
Medical Spending	0.652	(0.126)	[<0.001]	12,451	63,155

Notes: This table displays IV estimates from alternative specifications. 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. Unless otherwise indicated, these regressions use the baseline sample and controls. See Table 3 table notes for more information on the baseline sample and controls.

Table A5: Additional Robustness

	ln(Weekly Benefit)			Pre-mean	N
	coef	std error	p-value	dep var	
Baseline					
Benefit Duration	0.716	(0.147)	[<0.001]	17.71	63,155
Medical Spending	0.634	(0.136)	[<0.001]	12,451	63,155
Restrict Sample to Prior Wage in [540, 1500]					
Benefit Duration	0.638	(0.153)	[<0.001]	17.69	60,545
Medical Spending	0.576	(0.141)	[<0.001]	12,416	60,545
Restrict Sample to Prior Wage in [540, 1000]					
Benefit Duration	0.770	(0.294)	[0.009]	18.22	45,995
Medical Spending	0.737	(0.272)	[0.007]	12,627	45,995
Restrict Sample to Prior Wage in [675, 2000]					
Benefit Duration	0.521	(0.181)	[0.004]	17.71	44,156
Medical Spending	0.360	(0.168)	[0.032]	12,451	44,156
Restrict Sample to Prior Wage in [400, 2000]					
Benefit Duration	0.621	(0.134)	[<0.001]	17.71	89,617
Medical Spending	0.668	(0.125)	[<0.001]	12,421	89,617
Indicator Variable for Treatment					
Benefit Duration	0.834	(0.170)	[<0.001]	17.71	63,155
Medical Spending	0.665	(0.157)	[<0.001]	12,451	63,155
Re-Weighting Based on Demographics					
Benefit Duration	0.729	(0.149)	[<0.001]	17.71	63,155
Medical Spending	0.671	(0.137)	[<0.001]	12,451	63,155
Additional Controls: Insurer X Injury Month Fixed Effect					
Benefit Duration	0.614	(0.157)	[<0.001]	17.71	63,155
Medical Spending	0.573	(0.146)	[<0.001]	12,451	63,155
Additional Controls: Industry Fixed Effect					
Benefit Duration	0.623	(0.144)	[<0.001]	17.71	63,155
Medical Spending	0.625	(0.135)	[<0.001]	12,451	63,155
Additional Controls: Industry X Injury Month Fixed Effect					
Benefit Duration	0.677	(0.147)	[<0.001]	17.71	63,155
Medical Spending	0.605	(0.137)	[<0.001]	12,451	63,155
Additional Control: ln(Prior Wage)					
Benefit Duration	0.723	(0.148)	[<0.001]	17.71	63,155
Medical Spending	0.640	(0.137)	[<0.001]	12,451	63,155
Coefficient on ln(Replacement Rate)					
Benefit Duration	0.650	(0.147)	[<0.001]	17.71	63,155
Medical Spending	0.579	(0.136)	[<0.001]	12,451	63,155

Notes: This table displays IV estimates from alternative specifications. Column 1 displays the dependent variable, column 2 displays the coefficient estimates, column 3 displays robust standard errors, column 4 displays p-values, and column 5 displays the mean of the dependent variable. Unless otherwise indicated, these regressions use the baseline sample and controls. Sample restrictions referenced in the table above refer to restrictions on claimants' wage-inflation-adjusted pre-injury weekly earnings. See Table 3 table notes for more information on the baseline sample and controls.

Table A6: Alternative Sources of Medical Coverage

	ln(Weekly Benefit)			Pre-mean dep	N
	coef	std error	p-value	var	
Unpaid Bills					
Share of Bills Not Paid	0.002	(0.014)	[0.868]	0.117	63,155
Share of Charges Not Paid	-0.020	(0.021)	[0.342]	0.511	63,154
Differential Monitoring of Procedures					
All Medical Care					
Number of Bills	0.518	(0.115)	[<0.001]	44.06	63,155
Spending	0.634	(0.136)	[<0.001]	12451	63,155
Diagnostic Radiology					
Number of Bills	0.321	(0.107)	[0.003]	6.293	63,155
Spending	0.730	(0.264)	[0.006]	761.7	63,155

Notes: This table displays IV estimates from separate regressions with shares (rows 1 and 2), the natural logarithm (rows 3 and 4), or inverse hyperbolic sine (rows 5 and 6) of the indicated variables. These regressions use the baseline sample and controls. See Table 3 table notes for more information on the baseline sample and controls. Robust standard errors are reported in parentheses and p-values are reported in brackets.

Table A7: Marginal Welfare Impact of Increase in Benefit Rate - Alternative Approximation with Coefficient of Relative Prudence Equal to $\gamma + 1$

Marginal Welfare Impact of Increase in Benefits, dW/db X 0.05b			
Coefficient of Relative Risk Aversion (γ)	Baseline Estimates	Baseline Duration Elasticity (ignoring impact on medical spending)	Baseline Medical Spending Elasticity (ignoring impact on income benefit duration)
	(1)	(2)	(3)
1	-$0.085	-$0.039	-$0.038
2	-$0.077	-$0.031	-$0.030
3	-$0.068	-$0.023	-$0.022
4	-$0.059	-$0.014	-$0.013
5	-$0.049	-$0.004	-$0.003
Duration Elasticity, $\epsilon_{D\_B,b}$	0.67	0.67	0.00
Medical Spending Derivative, dM/db	12.39	0.00	12.39

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. This calibration is based on the approximation derived in Appendix Section F where the coefficient of relative prudence is one plus the indicated coefficient of relative risk aversion. This calibration relies on the relevant behavioral elasticity estimates, additional moments from our data, and an estimate of the mean consumption drop experienced by workers nationally after a work-limiting 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 assumes no effect on medical spending. Column 3 reports calibrations based on our medical spending estimate but assumes no effect on the income benefit duration.

Table A8: Estimated Change in Consumption After Workplace Injury

	(1)	(2)	(3)	(4)
Mean annual consumption drop (in logs) upon workplace injury	-0.101	-0.072	-0.112	-0.080
	(0.001)	(0.005)	(0.002)	(0.006)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Individual Controls	x	x	x	x
Year FE	x	x	x	x
State FE	x	x	x	x
Sample Restrictions				
Wages	None	None	Weekly Earnings >$771	Weekly Earnings >$771
Weekly Benefit Level	None	Within 10 Percent of TX baseline level	None	Within 10 Percent of TX baseline level
N	763	88	230	77

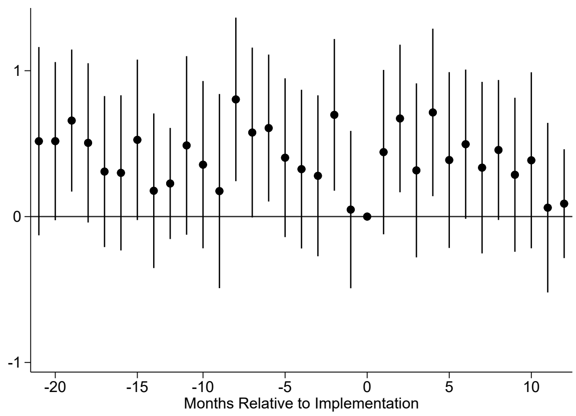
Notes: This table displays estimates of the mean change in food consumption after workplace injury from regressions of Equation (19). The baseline sample includes HRS respondents who report having had a workplace injury since their previous interview for the 1994 to 2016 waves of the HRS. Each regression includes the following demeaned controls: state fixed effects, year fixed effects, age, household size, change in household size from the previous interview, the log of the weekly wage in the previous period, the log of the weekly workers' compensation benefits the injured worker would be entitled to based on injury date, state, and prior weekly wage, indicators for the respondent being white, black, Hispanic, male, and married, and indicators for the respondent having graduated from high school, having some college, and having graduated from college. All dollar values are inflation-adjusted using the CPI-U. Standard errors are clustered by state and reported in parentheses and p-values are reported in brackets.

Table A9: Robustness: Calibrated Marginal Welfare Gain and MVPF for Range of Consumption Drop Values

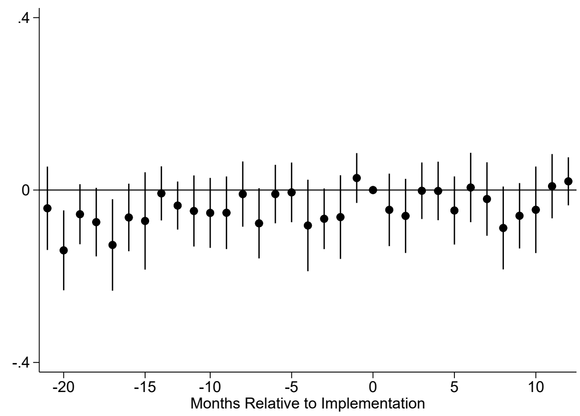
Consumption Drop upon Workplace Injury (%)	Marginal Welfare Gain (Baily-Chetty Framework)	MVPF
	(1)	(2)
2.5%	-\$0.088	0.40
5.0%	-\$0.085	0.42
7.5%	-\$0.082	0.44
10.0%	-\$0.079	0.46
12.5%	-\$0.076	0.48
15.0%	-\$0.072	0.50
17.5%	-\$0.069	0.52
20.0%	-\$0.066	0.53
22.5%	-\$0.063	0.55
25.0%	-\$0.059	0.57
27.5%	-\$0.056	0.59
30.0%	-\$0.053	0.61
32.5%	-\$0.050	0.63
35.0%	-\$0.047	0.65
37.5%	-\$0.043	0.67
40.0%	-\$0.040	0.69

Notes: This table displays robustness analysis for the calibrated marginal welfare impact of an increase in benefits (in column 1) and the calibrated MVPF (in column 2). See Table 6 for more details on these calibrations. Each cell in this table represents a separate calibration, where the value of relative risk aversion is held fixed at two and the value of the consumption drop is as indicated in the relevant row.

Figure A1: Exposure to Reform: Coverage and Premiums



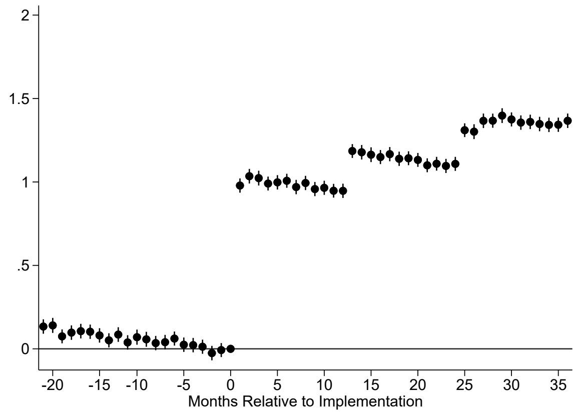
(a) ln(Covered Payroll)



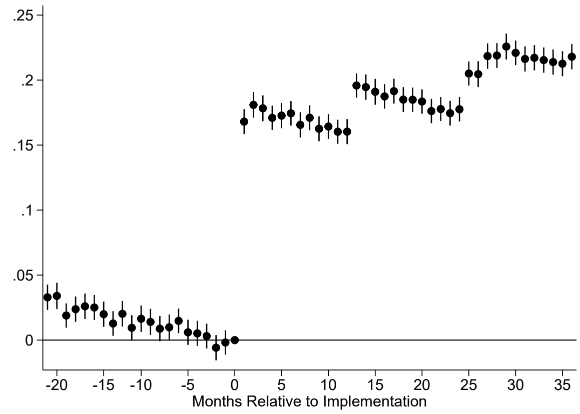
(b) ln(Premiums per Covered Payroll)

Notes: This figure reports the resulting coefficients and associated 95% confidence intervals from a difference-in-differences specification regressing covered payroll or premiums paid per covered payroll within an industry-occupation classification in a given month on month indicators interacted with an indicator for the top quartile of the distribution of the fraction of claimants with earnings above the initial cap among classifications in the sample. 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. The sample excludes the 25% of classifications with the lowest amount of payroll covered during the sample period and includes 4,818 observations from January 2005 to September 2007. The dependent variable is the natural log of covered payroll or premiums paid per covered payroll. Robust standard errors are clustered at the industry-occupation classification level.

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



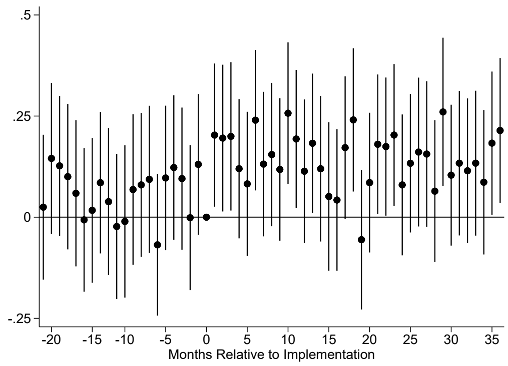
(a) Benefit Rate, Unscaled



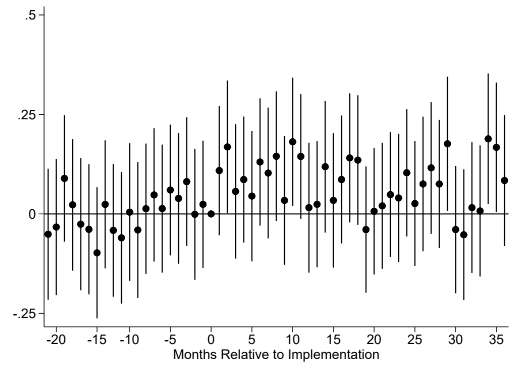
(b) ln(Benefit Rate), Scaled

Notes: Each graph in the figure above displays coefficients on the change-in-benefit or the scaled change-in-benefit measure (as indicated above) interacted with indicators for the month the injury occurred relative to the implementation of the reform from separate regressions of Equation (2) along with 95% confidence intervals calculated using robust standard errors. The interaction for the injury month immediately prior to the reform is omitted. The sample contains 108,860 claims that occurred from January 2005 to September 2009 that meet the sample restrictions described in the text. Each regression includes county by injury month-year 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) change-in-benefit, a male indicator variable, and a full vector of age indicator variables.

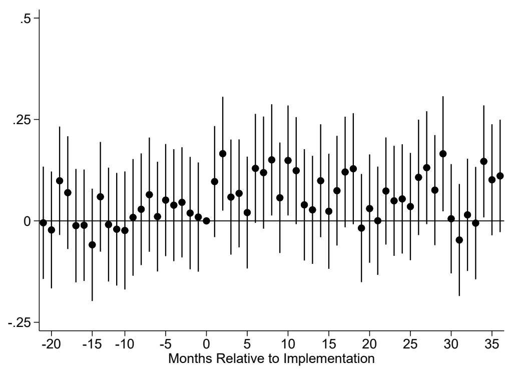
Figure A3: Impact of Benefit Change on Benefit Duration and Medical Utilization [Expanded Sample]



(a) ln(Benefit Duration)



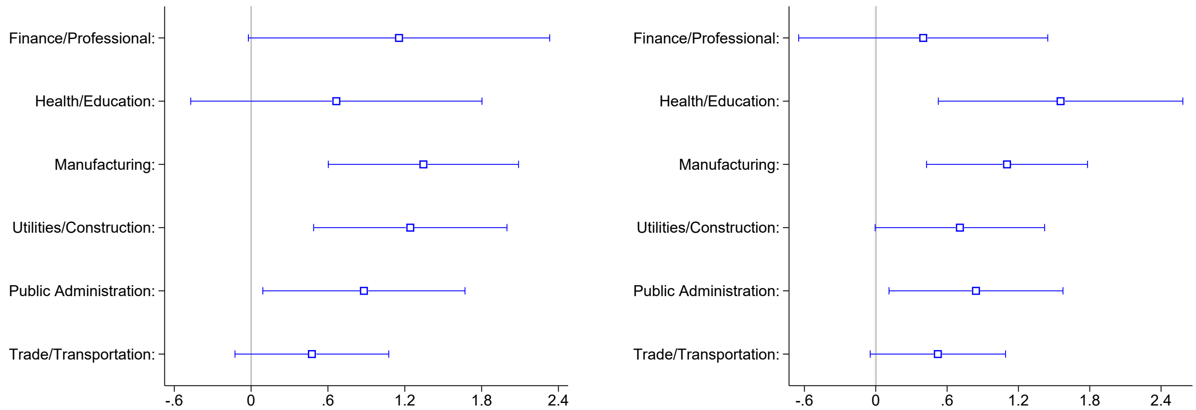
(b) ln(Medical Spending)



(c) ln(Number of Bills)

Notes: Each graph in the figure above displays coefficients on the scaled change-in-benefit measure interacted with indicators for the month the injury occurred relative to the implementation of the reform from separate regressions of Equation (2) along with 95% confidence intervals calculated using robust standard errors. The interaction for the injury month immediately prior to the reform is omitted. The sample contains 108,860 claims that occurred from January 2005 to September 2009 that meet the sample restrictions described in the text. Each regression includes county by injury month-year 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) change-in-benefit, a male indicator variable, and a full vector of age indicator variables.

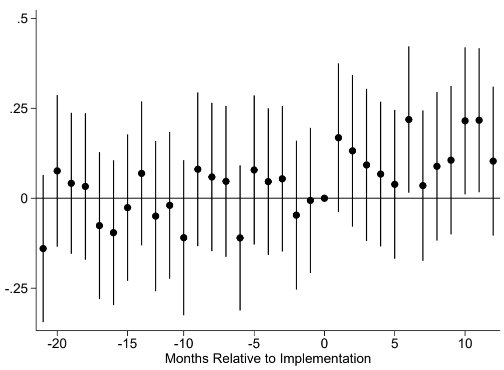
Figure A4: Heterogeneity in Impacts by Industry



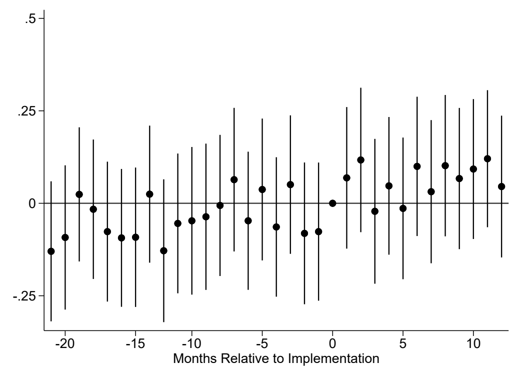
(a) ln(Benefit Duration) (b) ln(Medical Spending)

Notes: This figure displays IV estimates (and the associated 95% confidence intervals) from separate regressions including the indicated subgroup of claimants and the baseline controls. Reported confidence intervals are based on robust standard errors.

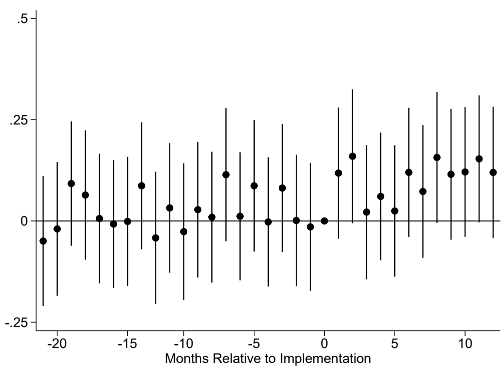
Figure A5: Impact of Benefit Change on Benefit Duration and Medical Utilization, Alternative Measure of Exposure to the Reform (Treatment Indicator, $\mathbb{1}(\Delta b_{it} > 0)$)



(a) ln(Benefit Duration)



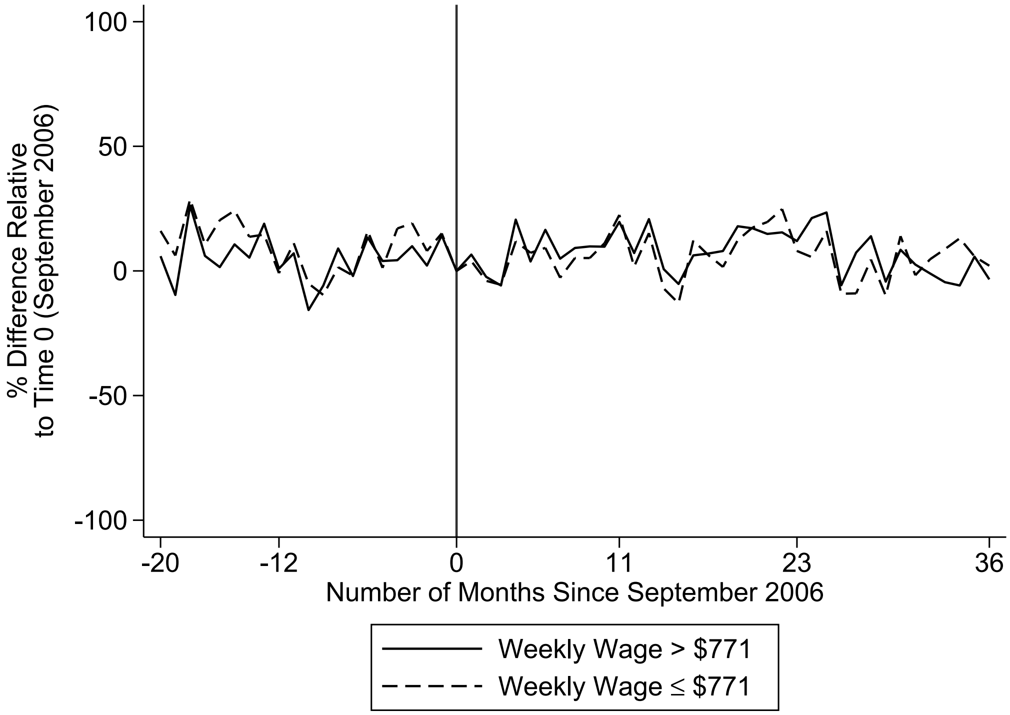
(b) ln(Medical Spending)



(c) ln(Number of Bills)

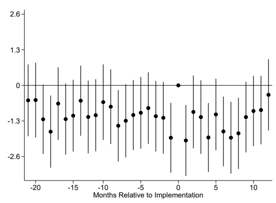
Notes: Each graph in the figure above displays coefficients on a treatment indicator variable interacted with indicators for the month the injury occurred relative to the implementation of the reform from separate regressions of Equation (2) along with 95% confidence intervals calculated using robust standard errors. The interaction for the injury month immediately prior to the reform is omitted. The sample contains 63,155 claims that occurred from January 2005 to September 2007 that meet the sample restrictions described in the text. Each regression includes county by injury month-year 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, an indicator variable for the claimant being a high earner ($\mathbb{1}(\Delta b_{it} > 0)$), a male indicator variable, and a full vector of age indicator variables.

Figure A6: Impact of Benefit Change on Claim Rates

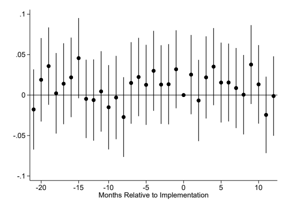


Notes: The figure above displays monthly claim rates from January 2005 to September 2009 for claimants with weekly earnings of $540 to $771 and for claimants with weekly earnings of $772 to $2,000 in September 2006 dollars. Each line shows the percent difference in claims for the income group relative to the number of claims for that income group that occurred in September 2006, the month before the reform was implemented.

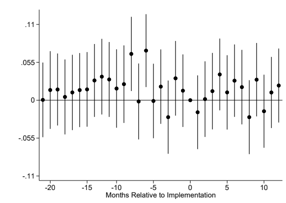
Figure A7: Balance on Observables



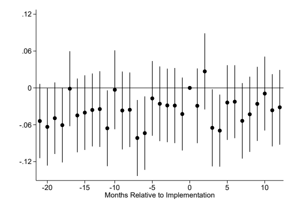
(a) Age



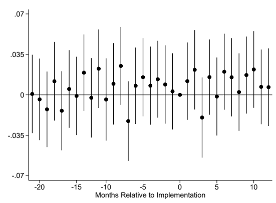
(b) Male



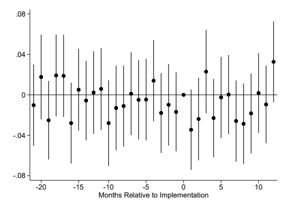
(c) ED Claim



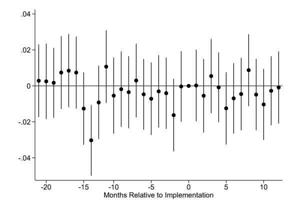
(d) Married



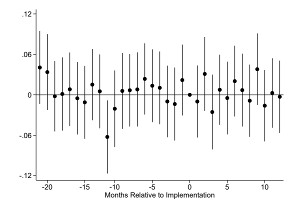
(e) Contusion



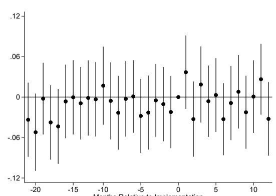
(f) Fracture



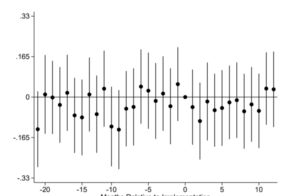
(g) Laceration



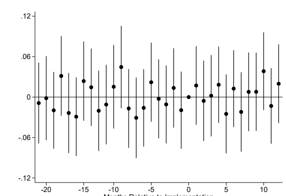
(h) Muscle Issue



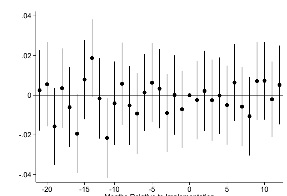
(i) Sprain



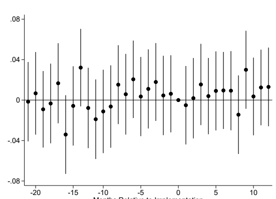
(j) ln(First Day Medical Spending)



(k) Industry: More Dangerous



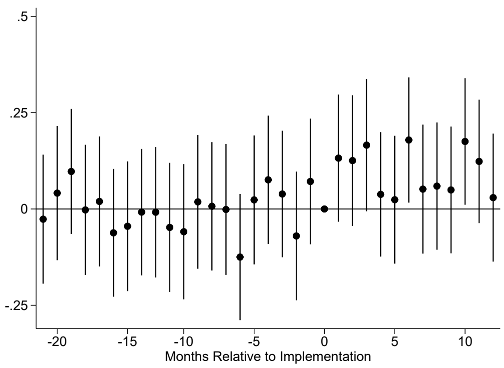
(l) Predicted ln(Benefit Duration)



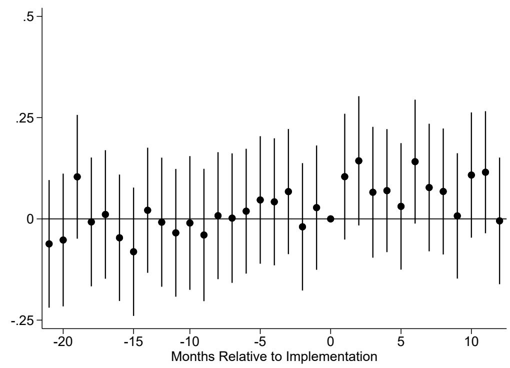
(m) Predicted ln(Five Year Medical Spending)

Notes: Each graph in the figure above displays coefficients on the scaled change-in-benefit measure interacted with indicators for the month the injury occurred relative to the implementation of the reform from separate regressions of Equation (2) along with 95% confidence intervals calculated using robust standard errors. The interaction for the injury month immediately prior to the reform is omitted. The sample includes claims that occurred from January 2005 to September 2007 that have non-missing values for the given dependent variable.

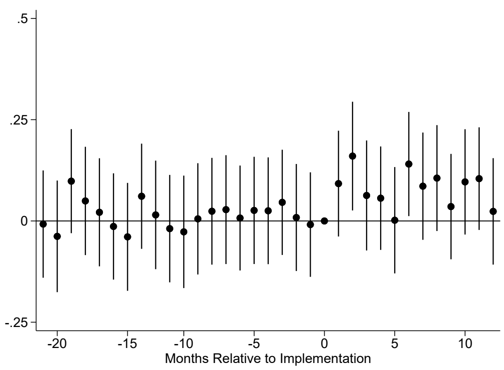
Figure A8: Impact of Benefit Change on Benefit Duration and Medical Utilization [No Claim-Level Controls]



(a) ln(Benefit Duration)



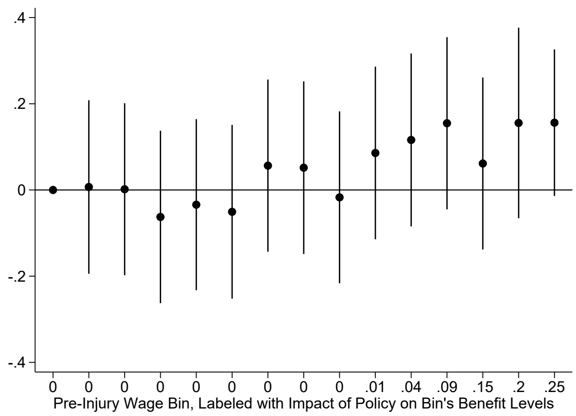
(b) ln(Medical Spending)



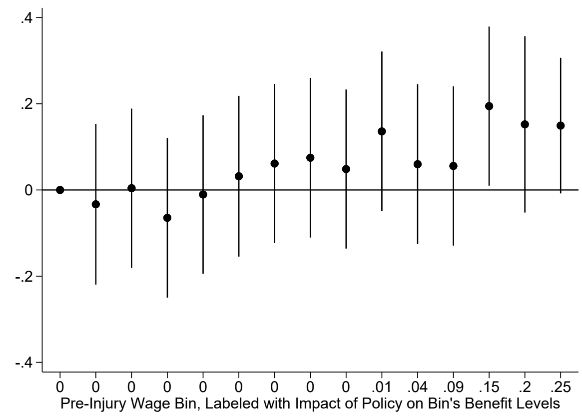
(c) ln(Number of Bills)

Notes: Each graph in the figure above displays coefficients on the scaled change-in-benefit measure interacted with indicators for the month the injury occurred relative to the implementation of the reform from separate regressions of Equation (2) along with 95% confidence intervals calculated using robust standard errors. The interaction for the injury month immediately prior to the reform is omitted. The sample contains 63,155 claims that occurred from January 2005 to September 2007 that meet the sample restrictions described in the text. Each regression includes injury month-year fixed effects and the claimant's (scaled) change-in-benefit.

Figure A9: Impacts Across Pre-Injury Wage Distribution



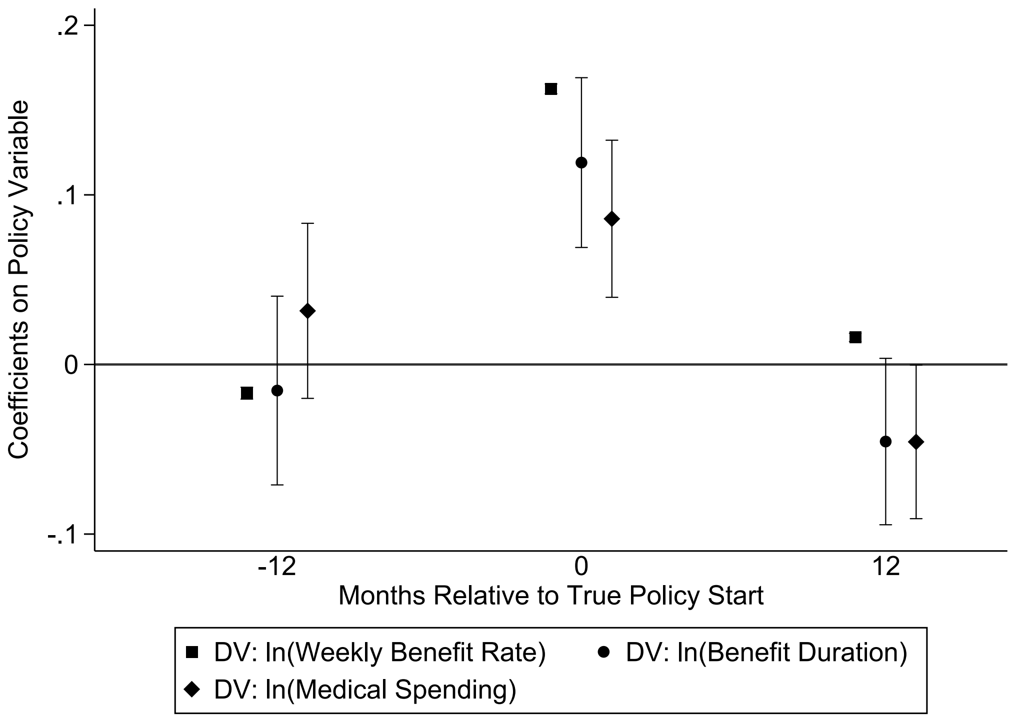
(a) ln(Benefit Duration)



(b) ln(Medical Spending)

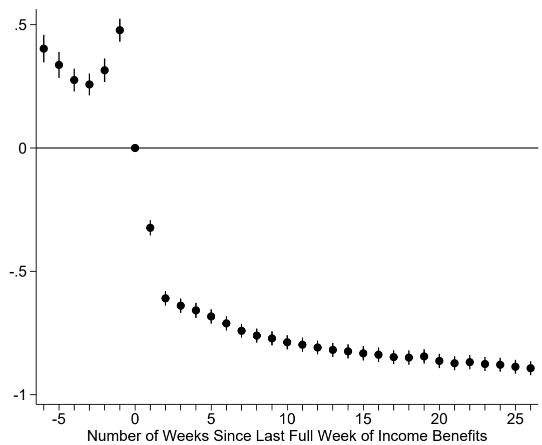
Notes: The above figure illustrates the impact of the policy across the pre-injury wage distribution from estimating Equation (13). Each marker represents pre-injury wage bin, where each bin represents a ventile aside from the top bin which pools all fully treated ventiles in a single bin. The effect of the reform on the bottom bin (ventile) is omitted. The horizontal axis indicates the mean impact of the policy on the benefit rate for the group. See Appendix Section D for more details on this analysis. The sample contains 63,155 claims that occurred from January 2005 to September 2007 that meet the sample restrictions described in the text. Each regression includes county by injury month-year 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, indicator variables for the claimants' pre-injury wage bin, a male indicator variable, and a full vector of age indicator variables.

Figure A10: Effect on Benefits and Outcomes: Comparing Actual Implementation Date and Placebo Dates

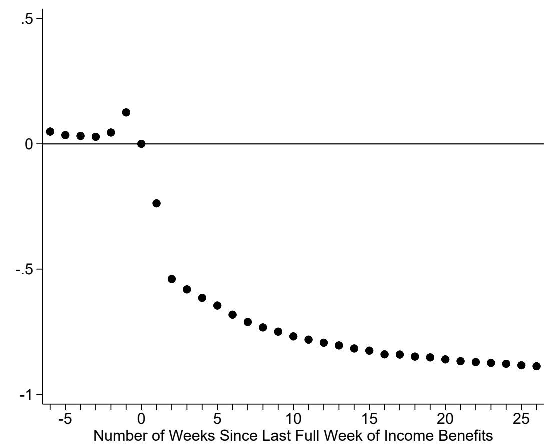


Notes: The above figure displays estimates of the coefficient on the scaled change-in-benefit variable interacted with an indicator that the injury occurred after the implementation of the new benefit schedule (the middle set of markers) or after the placebo implementation dates (12 months before and after the true implementation date) from separate regressions of Equation (3) for the indicated dependent variable. Specifically, the figure depicts coefficients from estimating Equation (3) using three different cutoff dates to define the "after" period—October 1, 2005 (one year before the reform was implemented; corresponding to -12 on the horizontal axis), October 1, 2006 (the reform implementation date; corresponding to 0 on the horizontal axis), and October 1, 2007 (one year after the reform was implemented; corresponding to 12 on the horizontal axis). In this estimation, the sample is restricted to workers injured within 12 months of the relevant cutoff date. All regressions include county by injury month-year fixed effects, a male indicator variable, a full vector of age indicator variables, an indicator variable equal to one if the claim began in the ED, the claimant's scaled change-in-benefit, and fixed effects for the day of the week that the claimant first received medical care.

Figure A11: Medical Utilization and Income Benefit Termination



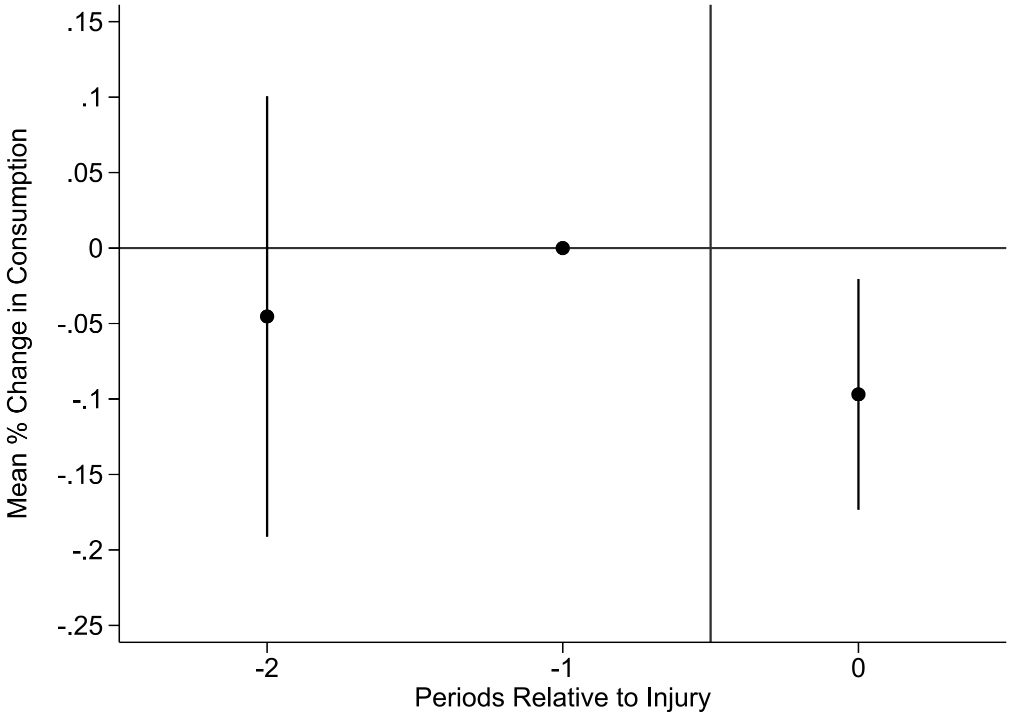
(a) Medical Spending



(b) Number of Bills

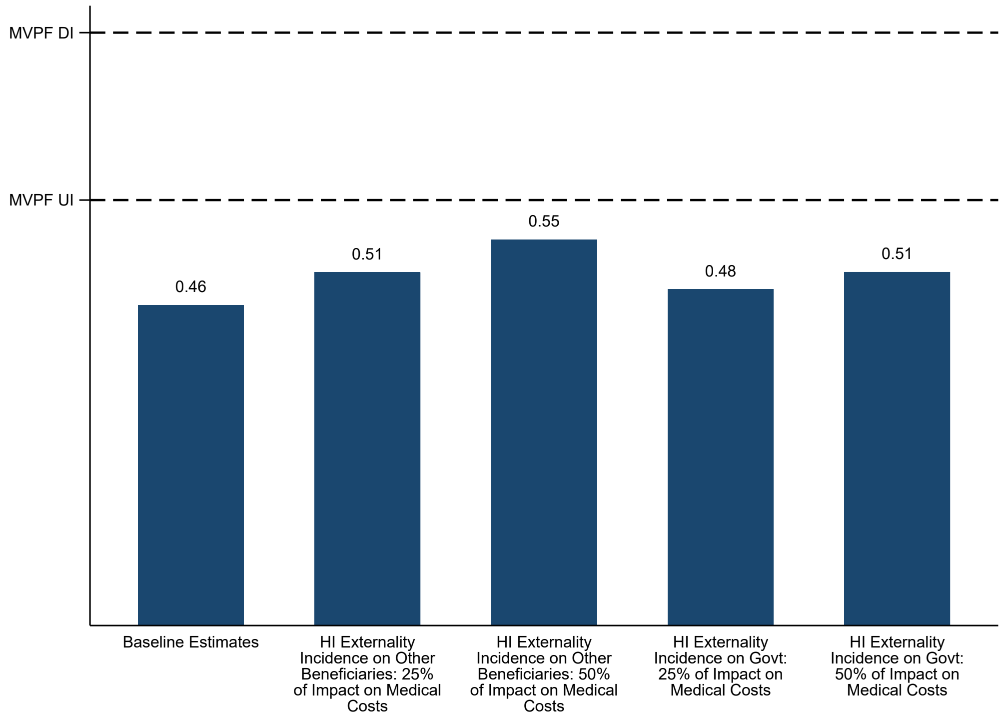
Notes: The above figure illustrates the relationship between the end of income benefits and the amount of medical care claimants receive. The data set consists of separate observations for each claimant for each week relative to the end of income benefits for 6 weeks before income benefits end until 26 weeks after income benefits end. The sample contains 63,155 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% confidence intervals calculated using robust standard errors clustered at the claim level.

Figure A12: Estimated Changes in Consumption After Workplace Injury



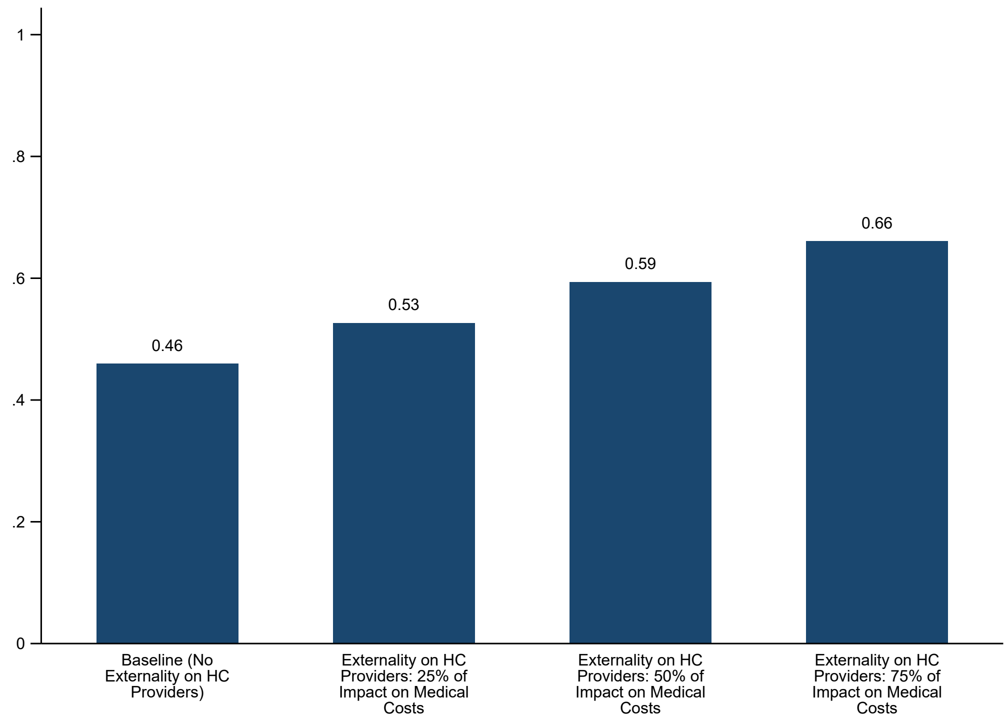
Notes: The above figure displays estimates of the mean change in food consumption after workplace injury relative to the mean change between the two waves prior to the injury from estimating Equation (20). The sample includes HRS respondents who report having had a workplace injury since their previous interview for the 1994 to 2016 waves of the HRS and have observations for the interview after the injury, as well as observations for the two periods prior to the injury. The vertical line indicates the injury timing. The regression includes the following demeaned controls: state fixed effects, year fixed effects, age, household size, change in household size from the previous interview, the log of the weekly wage in the previous period, the log of the weekly workers' compensation benefits the injured worker would be entitled to based on injury date, state, and prior weekly wage, indicators for the respondent being white, black, Hispanic, male, and married, and indicators for the respondent having graduated from high school, having some college, and having graduated from college. 95% confidence intervals calculated from robust standard errors clustered by state are shown along with the estimates.

Figure A13: MVPF: Robustness to Accounting for Potential Impacts on External Payers for Health Care



Notes: This figure displays the implied MVPF based on our estimates under the indicated assumptions about the magnitude and incidence of impacts on external payers for health care. See Appendix Section I.1 for details on these calculations. The first bar displays the baseline estimates for reference where we assume there are no external impacts. For reference, the figure indicates the average implied MVPFs for unemployment insurance and disability insurance, as calculated in Hendren and Sprung-Keyser (2020).

Figure A14: MVPF: Robustness to Accounting for Potential Externalities on Health Care Providers



Notes: This figure displays the implied MVPF based on our estimates under the indicated assumptions about the magnitude of externalities on health care providers. See Appendix Section I.2 for details on these calculations. The first bar displays the baseline estimates for reference where we assume there are no external impacts.