Online Appendix for "The Impact of Benefit Generosity on Workers' Compensation Claims: Evidence and Implications" by Marika Cabral and Marcus Dillender

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.

[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.

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, 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:

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

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:

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

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:

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

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

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

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

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

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

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

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,

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

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,

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

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,

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

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

	Impairment Benefit Rate	Total Impairment Benefits	Benefit Duration	Medical Spending	Number of Bills	Impairment Rating	Impairment Benefits > 0
Panel A: Effect of Impairment Rate Increase							
	(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

	Impairment Benefit Rate	Total Impairment Benefits	Benefit Duration	Medical Spending	Number of Bills	Impairment Rating
Panel B: Effect of Impairment Benefit Increase, Scaled by Impairment Severity among Permanently Impaired Claimants						
	(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			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
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

	coef	ln(Weekly Benefit) std error	p-value	Pre-mean dep var	N
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

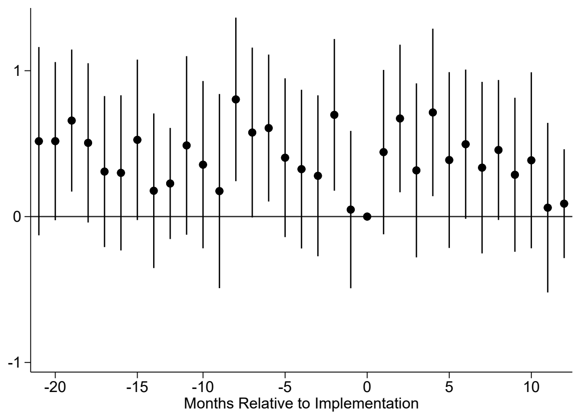
Notes: This table displays estimates of the mean change in food consumption after workplace injury from regressions of Equation (19). The baseline sample includes HRS respondents who report having had a workplace injury since their previous interview for the 1994 to 2016 waves of the HRS. Each regression 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

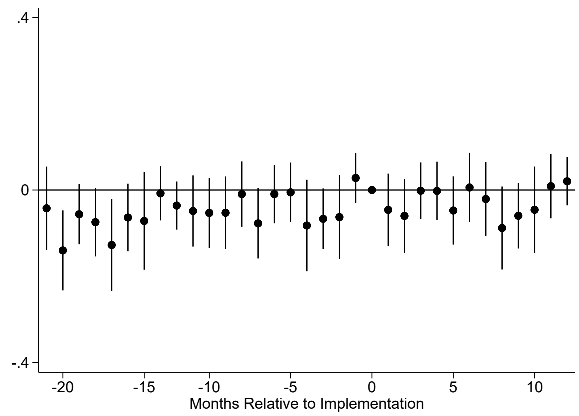
Consumption Drop upon Workplace Injury (%)	Marginal Welfare Gain (Baily-Chetty Framework)	MVPF
	(1)	(2)
2.5%	-\$0.088	0.40
5.0%	-\$0.085	0.42
7.5%	-\$0.082	0.44
10.0%	-\$0.079	0.46
12.5%	-\$0.076	0.48
15.0%	-\$0.072	0.50
17.5%	-\$0.069	0.52
20.0%	-\$0.066	0.53
22.5%	-\$0.063	0.55
25.0%	-\$0.059	0.57
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



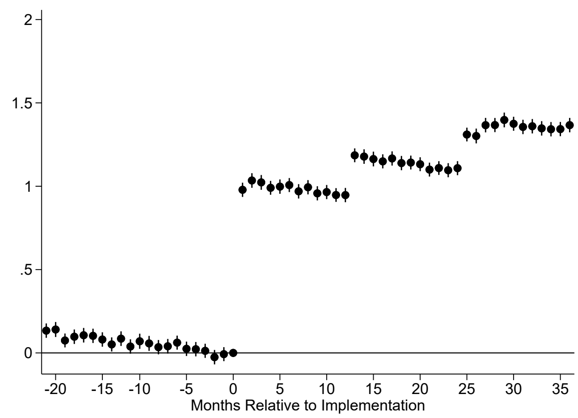
(a) ln(Covered Payroll)



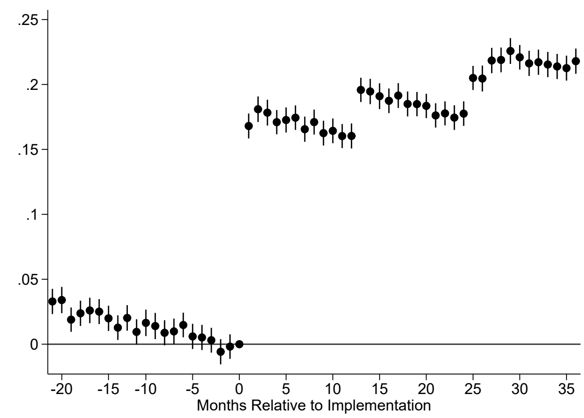
(b) ln(Premiums per Covered Payroll)

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]



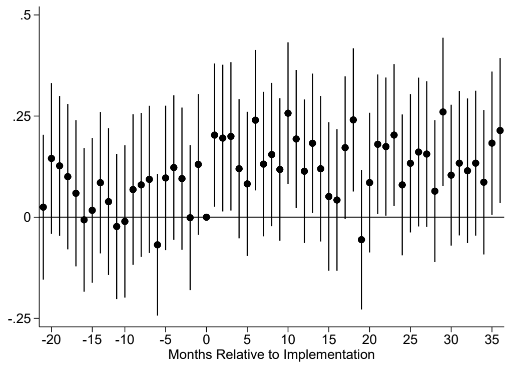
(a) Benefit Rate, Unscaled



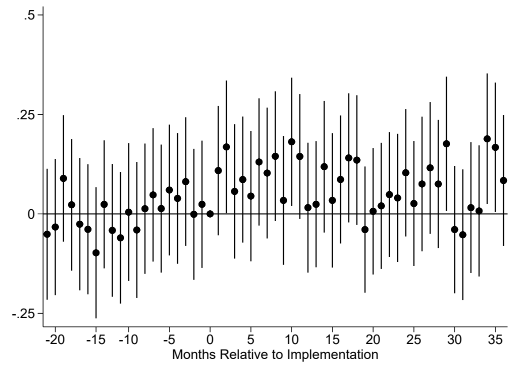
(b) ln(Benefit Rate), Scaled

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.

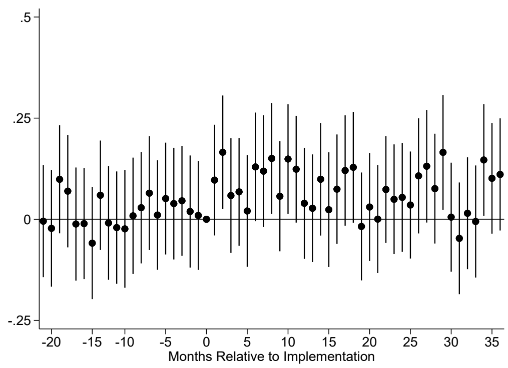
Figure A3: Impact of Benefit Change on Benefit Duration and Medical Utilization [Expanded Sample]



(a) ln(Benefit Duration)



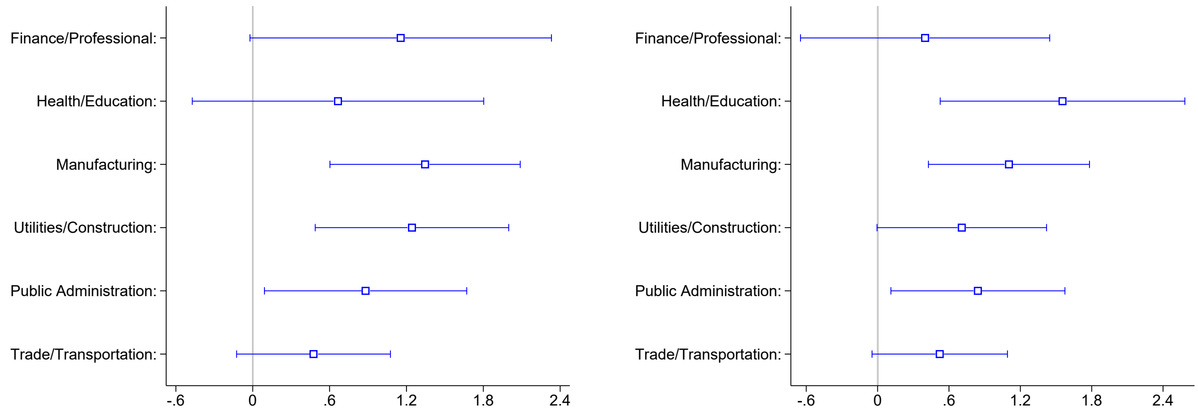
(b) ln(Medical Spending)



(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

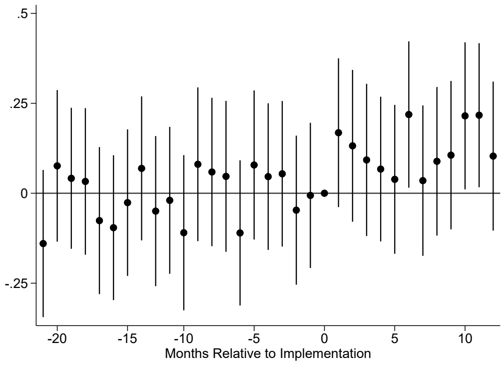


(a) ln(Benefit Duration)

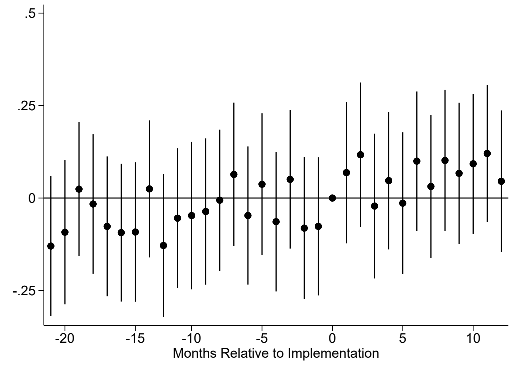
(b) ln(Medical Spending)

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.

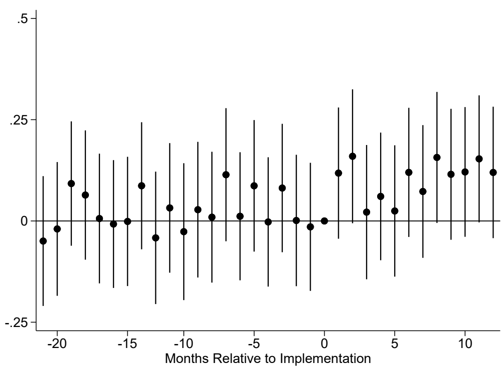
Figure A5: Impact of Benefit Change on Benefit Duration and Medical Utilization, Alternative Measure of Exposure to the Reform (Treatment Indicator, $\mathbb{1}(\Delta b_{it} > 0)$)



(a) ln(Benefit Duration)



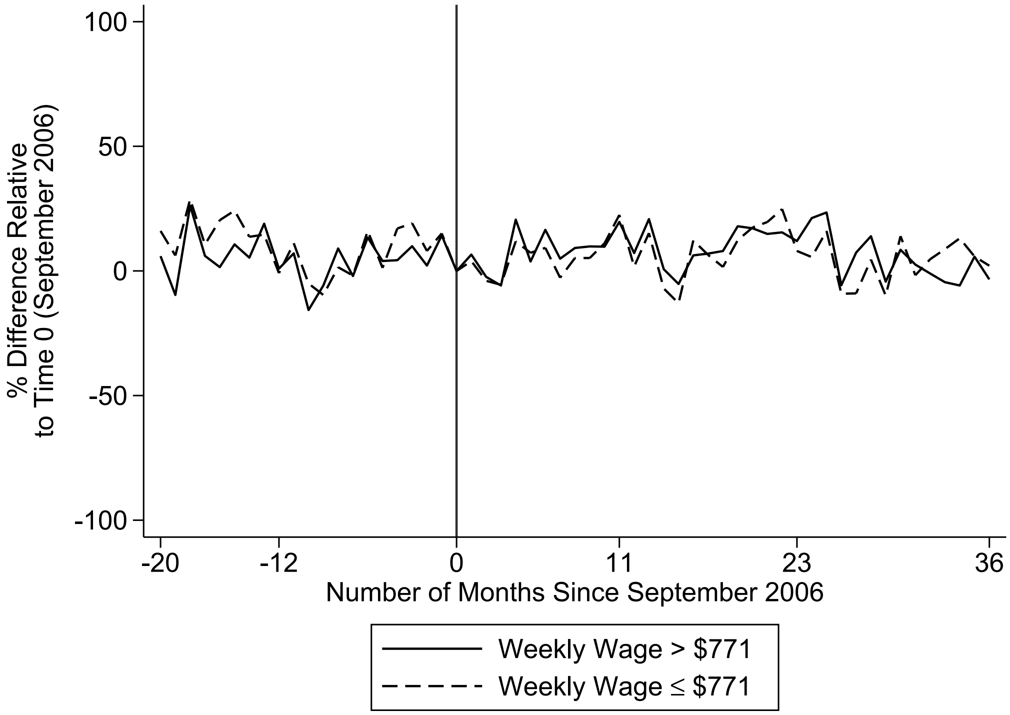
(b) ln(Medical Spending)



(c) ln(Number of Bills)

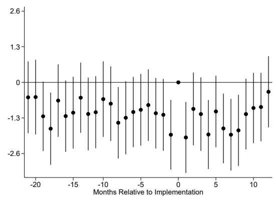
Notes: Each graph in the figure above displays coefficients on a treatment indicator variable interacted with indicators for the month the injury occurred relative to the implementation of the reform from separate regressions of Equation (2) along with 95% confidence intervals calculated using robust 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

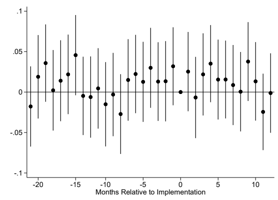


Notes: The figure above displays monthly claim rates from January 2005 to September 2009 for claimants with weekly earnings of $540 to $771 and for claimants with weekly earnings of $772 to $2,000 in September 2006 dollars. Each line shows the percent difference in claims for the income group relative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

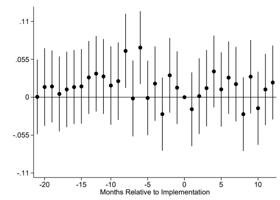
Figure A7: Balance on Observables



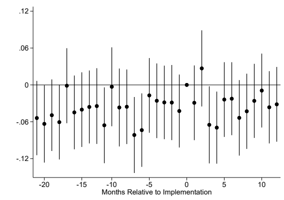
(a) Age



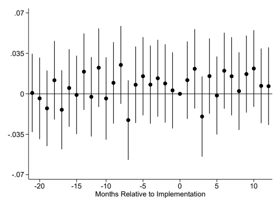
(b) Male



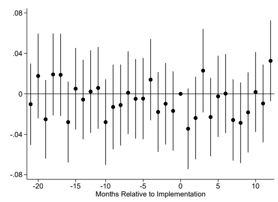
(c) ED Claim



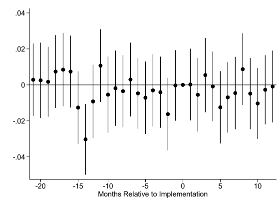
(d) Married



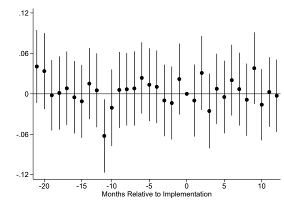
(e) Contusion



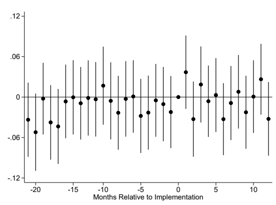
(f) Fracture



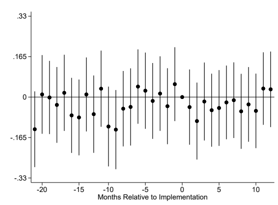
(g) Laceration



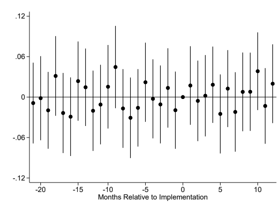
(h) Muscle Issue



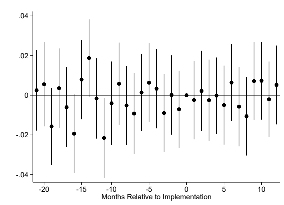
(i) Sprain



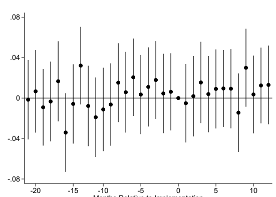
(j) ln(First Day Medical Spending)



(k) Industry: More Dangerous



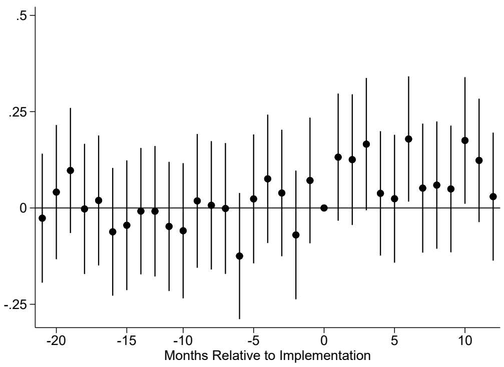
(l) Predicted ln(Benefit Duration)



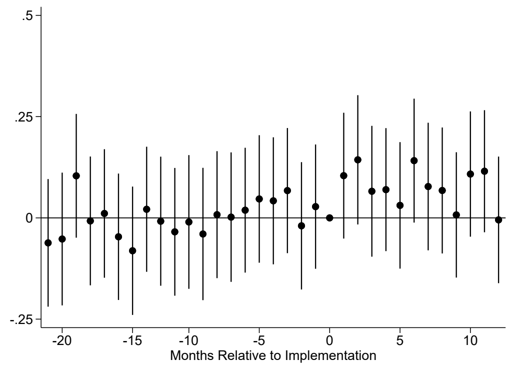
(m) Predicted ln(Five Year Medical Spending)

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.

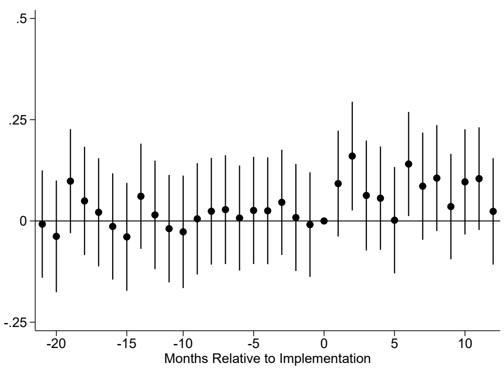
Figure A8: Impact of Benefit Change on Benefit Duration and Medical Utilization [No Claim-Level Controls]



(a) ln(Benefit Duration)



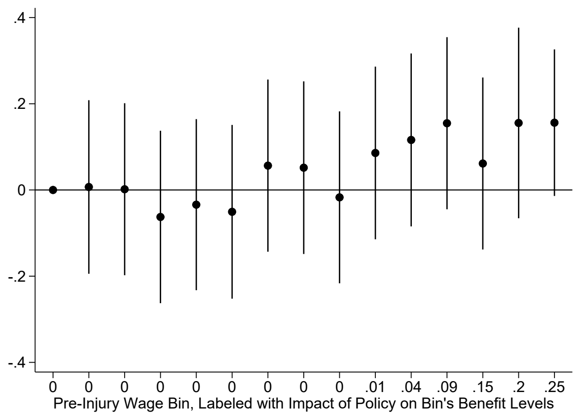
(b) ln(Medical Spending)



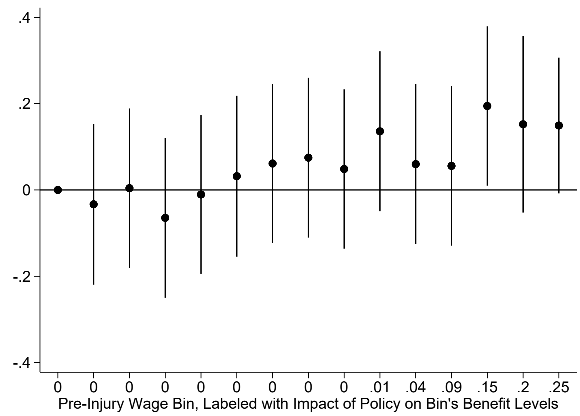
(c) ln(Number of Bills)

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



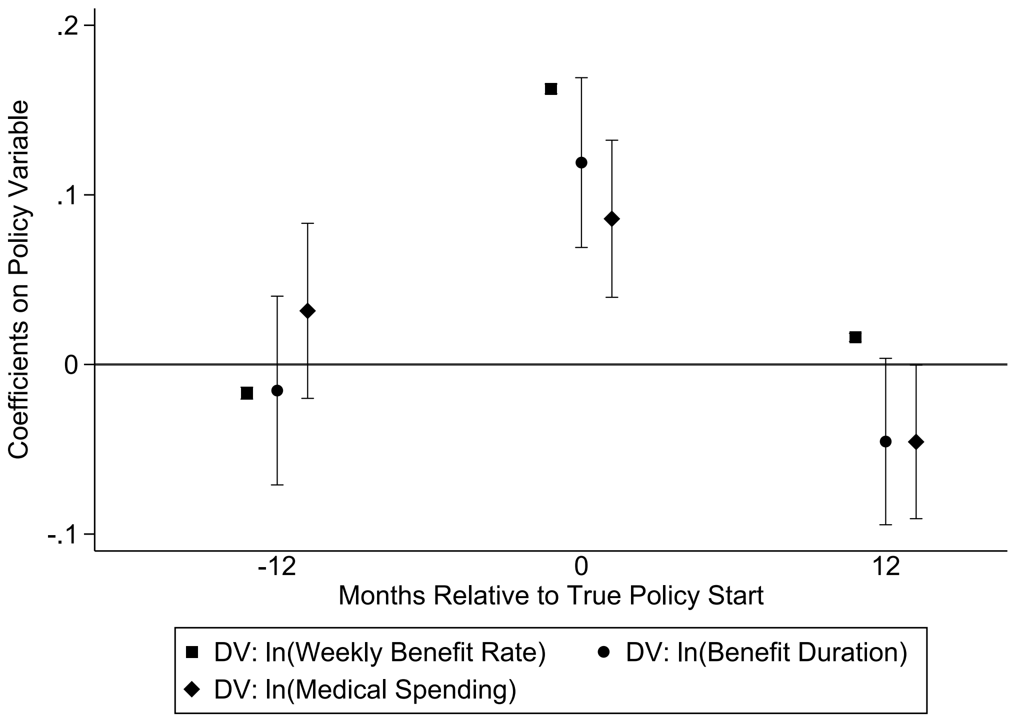
(a) ln(Benefit Duration)



(b) ln(Medical Spending)

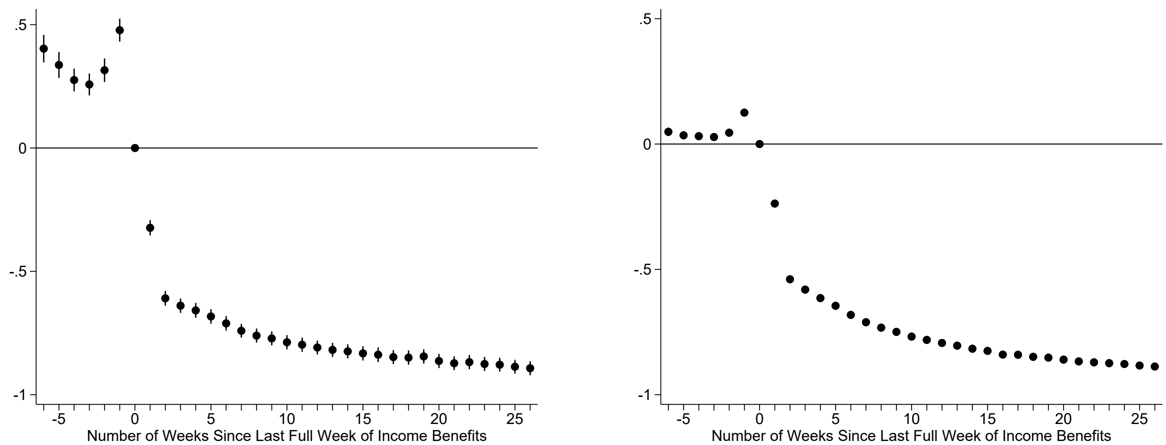
Notes: The above figure illustrates the impact of the policy across the pre-injury wage distribution from estimating Equation (13). Each marker represents pre-injury wage bin, where each bin represents a ventile aside from the top bin which pools all fully treated ventiles in a single bin. The effect 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.

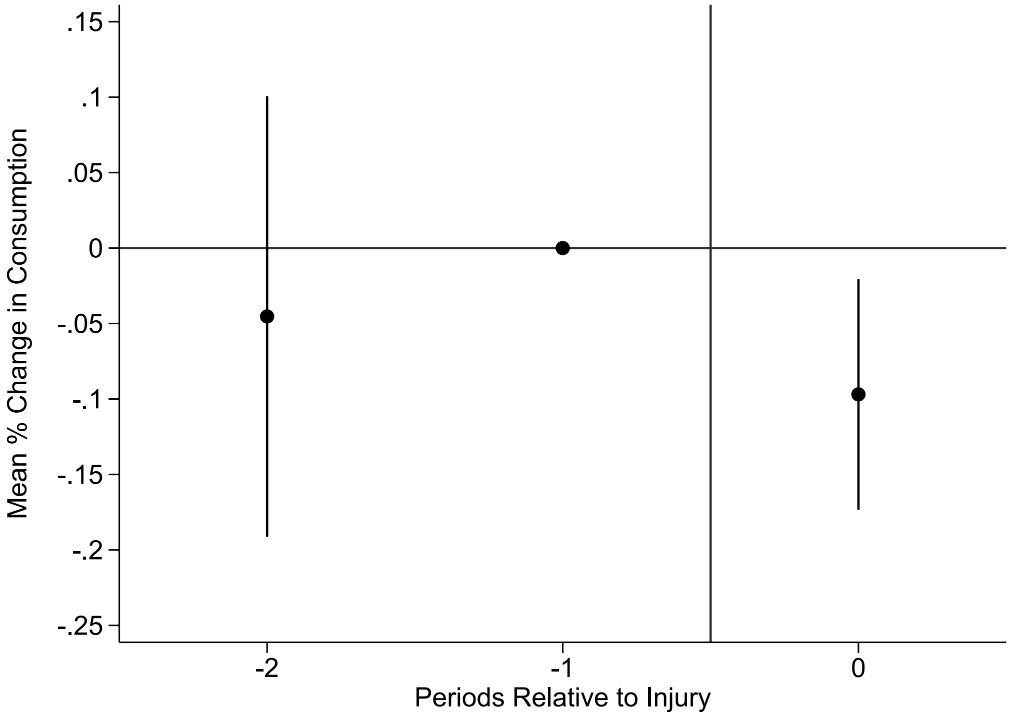
Figure A11: Medical Utilization and Income Benefit Termination



(a) Medical Spending (b) Number of Bills

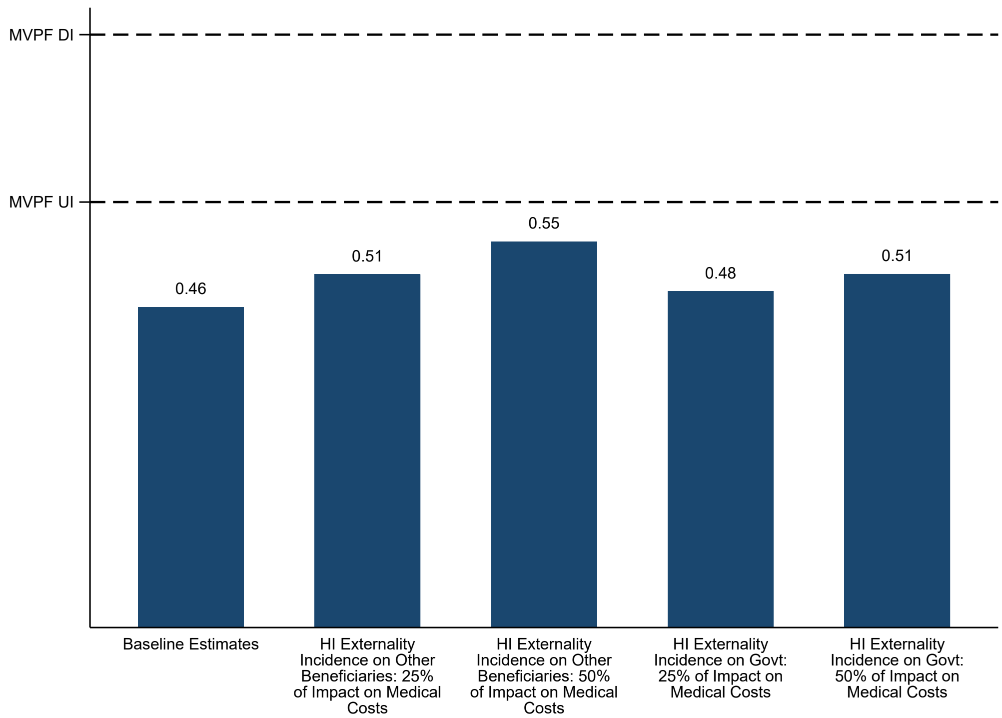
Notes: The above figure illustrates the relationship between the end of income benefits and the amount of medical care claimants receive. The data set consists of separate observations for each claimant for each week relative to the end of income benefits for 6 weeks before income benefits end until 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



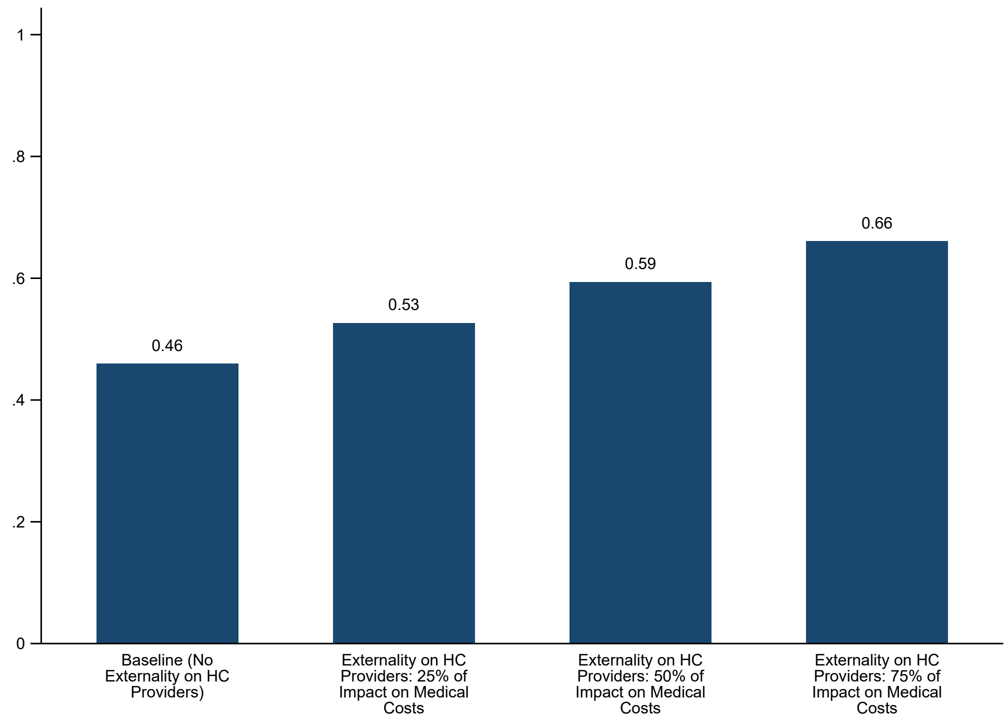
Notes: The above figure displays estimates of the mean change in food consumption after workplace injury relative to the mean change between the two waves prior to the injury from estimating Equation (20). The sample includes HRS respondents who report having had a workplace injury since their previous 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.