Minimum Wages and Workplace Injuries

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Abstract

Do minimum wage changes affect workplace health and safety? Using the universe of workers' compensation claims in California over 2000-2019, we estimate whether minimum wage shocks affect the rate of workplace injuries. Our identification exploits both geographic variation in state- and city-level minimum wages and local occupation-level variation in exposure to minimum wage changes. We find that a 10% increase in the minimum wage increases the injury rate by 11% in an occupation-metro area labor market which is fully exposed to the minimum wage increase. Our results imply an elasticity of the workplace injury rate to minimum-wage-induced wage changes of 1.4. We find particularly large effects on injuries relating to cumulative physical strain, suggesting that employers respond to minimum wage increases by intensifying the pace of work, which in turn increases injury risk.

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1 Introduction

Minimum wage research has predominantly focused on estimating the wage and employment effects of higher minimum wages.[1] But firms can also adjust to minimum wages by altering non-wage aspects of jobs, potentially with first-order welfare implications for workers (Clemens, 2021).

In this paper, we ask: *Do minimum wages affect workplace health and safety?* Health and safety is a central dimension of job quality: in the US, nearly 3 million individuals are hurt on the job each year (BLS, 2024). Workplace injuries are especially common for the low-wage workers affected by minimum wage policies. For instance, the average injury rate in leisure and hospitality is 6 times that in management, scientific and technical services, and 9 times that in finance and insurance (BLS , 2020). Workplace injuries can often have serious and lasting consequences, including permanent disability and sometimes death.

Whether minimum wages help or harm workplace safety is theoretically unclear. Higher minimum wages might induce employers to cut costs, reducing spending on safety measures, or to push for productivity gains by intensifying the pace of work. Either of these would lead to more injuries. Alternatively, higher minimum wages could lead to safer workplaces by reducing turnover in low-wage labor markets, through efficiency wage-type effects, or by incentivizing capital intensification (which often improves workplace safety).

To study this question, we use restricted access administrative data on workplace injuries from over 13 million worker's compensation insurance claims from California (representing the universe of claims over 2000-2019). We leverage two

[1] See e.g. Neumark and Wascher (1992), Card and Krueger (1994), Cengiz et al. (2019), and Dube and Lindner (2021).

sources of variation in exposure to minimum wage changes: geographic and occupational. First, many cities in California have introduced local minimum wages that exceed the state minimum, which allows us to utilize geographic variation in the size and timing of minimum wage hikes across metro areas.[2] Second, we exploit wage variation across occupations within metro areas, as local occupational labor markets with a larger share of workers earning close to the local minimum should be more affected by minimum wage shocks.

More precisely, in our baseline specification we regress the log annual injury rate of a metro-occupation labor market on the interaction between the minimum wage change (real year-on-year growth in the metro minimum wage) and the metro-occupation's exposure to that minimum wage change (the estimated share of the wage distribution that falls below 1.3x the local minimum).[3] We use metro-year fixed effects to control flexibly for time-varying confounders that may be systematically correlated with localities' decisions to raise minimum wages (a virtue of our strategy relative to those which rely on geographic policy variation alone). We also use occupation-year and metro-occupation fixed effects to avoid identifying off occupation-level trends and time-invariant differences between local labor markets.

We find that minimum wage increases lead to significantly higher injury rates. Our headline estimate is that a 10% increase in the local minimum wage increases the injury rate by 11% in an occupation which is fully exposed to the minimum

[2] Though metros contain municipalities with different minimum wage policies, we need occupational employment figures to construct our dependent variable, and the BLS OEWS metro-occupation-year level estimates are the most granular available. We therefore calculate annual metro average minimum wages and use these as the basis for our analysis. For more information see Section 3.

[3] The choice of 1.3x is based on estimates of the spillover effects of minimum wages (Cengiz et al., 2019), and calculated using BLS OEWS data. Our results are robust to alternative exposure thresholds.

wage change (i.e. where all workers earn less than 1.3x the minimum). These effects are concentrated in low-wage occupations: specifically those for whom at least 50 percent of workers in a metro area earn less than 130% of the local minimum (for example fast food workers, cleaners, cashiers, agricultural workers, and personal care aides). Consistent with potentially large welfare consequences, we find effects across a wide range of injury and illness types, including relatively severe incidents that lead to disability, as well as those that require external medical treatment.

Estimating the same model with log mean wages in a given local occupational labor market as the dependent variable, we find that the same 10% minimum wage increase leads to an 8% average wage increase in a fully-exposed occupation (cross-validating our identification strategy). To the extent that minimum wage increases affect injury rates only when they actually force wage increases, this estimate allows us to infer an elasticity of injury rates to policy-induced wage changes of 1.4: that is, an exogenous 10% increase in the cost of labor leads to a 14% increase in the workplace injury rate.

What does this increase in the injury rate mean for different workers? The average injury rate for low-wage workers in our data is 4.4% per year; applying our coefficient estimate to these workers, we estimate that a 10% increase in the minimum wage leads to a 0.3 percentage point increase in the injury rate, or on average an additional 3 injuries per 1,000 low-wage workers per year. For certain highly-exposed high-injury occupations, this is even starker: for example we estimate a 10% minimum wage increase leads to an additional 7 injuries per 1,000 agricultural workers per year, and an additional 5 injuries per 1,000 building cleaning workers per year.

Our results are robust to a wide range of alternative specifications, including altering our definition of the minimum wage shock or occupation-level exposure, and restricting our dataset in various ways to account for potential mismeasurement or reporting biases. Together, our robustness checks suggest that the effects we find are unlikely to be driven by spurious correlations between economic and worker safety-related trends in jurisdictions that adopted higher minimum wages, nor by endogenous changes in the rate of injury reporting. They are also robust to falsification tests that replace our independent variable with future minimum wage increases.

The effects of a minimum wage shock on injuries are persistent. Across models with up to four lags, we estimate that the effect of a minimum wage shock in year t increases wages substantially in year t and then fades over the following four years; we estimate a very similar dynamic for injuries, with the increase in injury rates occurring in year t and fading at a similar rate as the wage effect over the following four years. These results indicate that, when minimum wage hikes are binding (aka when they increase wages), they also induce a persistent increase in injury rates.

We next explore three potential mechanisms: work intensification, safety spending, and capital-labor or labor-labor substitution. We find evidence strongly consistent with work intensification as an important channel. Specifically, we make use of a unique aspect of our data – that it includes, for each claim, detailed information on both the nature and cause of the injury. This allows us to construct a category of cumulative physical injuries by isolating claims where the reported cause and nature of injury, together, make clear that the injury is both related to physical exertion *and* the result of repeated stress, rather than a one-off accident. Example injuries of this type include carpal tunnel syndrome and repetitive strain injury. These kinds of injuries are most likely to be caused by an intense pace of work, rather than disinvestments in safety equipment. We find that the effect of minimum wages is nearly twice as large for these cumulative physical injuries: a 10% minimum wage increase leads to a 21% increase in cumulative physical injuries per worker. We cannot directly construct a counterpart test for the safety spending mechanism: we find a large effect of minimum wage increases on accidents, which could be caused either by work intensification (if stress increases error likelihood) or by safety disinvestment (if protective equipment is not used or maintained). We find, however, little evidence consistent with capital-labor or labor-labor substitution explaining our results.[4]

Our paper proceeds as follows. In section 2 we outline our conceptual framework and discuss our contribution to the literature. In section 3 we detail our data sources and variable construction, and section 4 outlines our empirical strategy. In Section 5 we present our results and show that our findings are robust to a wide variety of alternative specifications, before an exploratory discussion of mechanisms. We then conclude in Section 6.

2 Theory and Related Literature

Whether minimum wages should affect workplace safety, and if so in which direction, is *ex ante* unclear.

There are two core mechanisms by which minimum wage increases might *increase* injury rates. First, the provision of health and safety measures are costly for firms. Thus, they may respond to labor cost shocks by cutting spending on safety gear, equipment maintenance, cleaning, training, or other areas that reduce

[4] Note that because our analyses take place at the level of highly granular local occupational labor markets, and incorporate similarly granular changes in employment, the effects we document can be interpreted as being net of/inclusive of any endogenous employment effects.

workplace safety. We term this the *safety spending* mechanism.[5] Second, an increase in the minimum wage may lead to *work intensification* - pushing employees to work harder and faster - leading to increased physical strain and increased risk of accidents. Firms might intentionally intensify work demands to improve labor productivity to keep apace with the increase in labor costs, for example by raising targets or increasing production line speeds.[6] Work intensification may also arise as a result of disemployment effects of the minimum wage, if short-staffing increases task demands on remaining employees.[7] In addition, workers may increase their own work intensity in response to an increased perceived threat of being fired: Coviello et al. (2022) and Ku (2022) find evidence that workers increase effort after minimum wage increases.

On the other hand, there are two core channels through which minimum wage increases might *reduce* the likelihood of injury at work. Higher minimum wages tend to increase retention and therefore increase average worker tenure (Dube and Lindner, 2021). Since low-tenure workers are more prone to workplace accidents (Bena et al., 2013), this may reduce injuries. Higher wages may also reduce worker stress by relieving financial pressures, allowing them to work fewer hours, or otherwise facilitating pro-health behaviors (Lenhart, 2017), which could improve focus and so reduce likelihood of accidents.

[5] There is evidence that firms' safety spending is responsive to financial pressures: Charles et al. (2022) find that positive cash flow shocks reduce workplace injury rates in the U.S. mining sector by alleviating the financial pressures that constrain investment in safety measures.

[6] There is evidence that firms intensify the pace of work in response to financial pressures: Caskey and Ozel (2017) study how firms respond to the pressure generated by earnings expectations, finding that in years where firms barely meet expectations, they are found to have higher injury rates, alongside higher revenue and production per employee (evidence of work intensification).

[7] Chakrabarti et al. (2017) argue that, following a minimum wage hike, Seattle restaurants' extensive and/or intensive (hours) disemployment responses increased task demands on servers, and that this explains a subsequent increase in hygiene violations — a similar mechanism could lead to increased likelihood of injury.

More ambiguously, minimum wages may induce employers to invest in technologies that increase worker productivity (Aaronson and Phelan, 2017; Aaronson, French, et al., 2018) or to substitute towards higher-skilled labor (Clemens, Kahn, et al., 2020). The resulting task reallocation and/or compositional changes may also affect injury rates.

Our paper contributes to a large literature on the effects of minimum wages on economic outcomes. Most of this literature, however, focuses on employment and wages (Neumark and Wascher, 1992; Card and Krueger, 1994; Dube, Lester, et al., 2010; Cengiz et al., 2019; Dube and Lindner, 2021). There are relatively few studies which examine other margins of adjustment to job quality (Simon and Kaestner, 2004; Clemens, Kahn, et al., 2018; Clemens, 2021).[8]

In particular, we are one of the first papers to study the relationship between minimum wages and workplace health and safety - a crucially important margin for worker welfare. In doing so, we build on an unpublished manuscript by Hradil (2018) and contemporaneous work by Liu et al. (2024). These papers use state-level minimum wage changes in the US to estimate effects on workplace injuries in a difference-in-difference specification, with Hradil (2018)'s estimates at the state-industry level using BLS Survey of Occupational Injuries and Illness data and Liu et al. (2024)'s estimates at the establishment level using OSHA data. Both papers find an increase in injury rates in states where minimum wages are raised.[9]

[8] Non-employment margins of adjustment may have first order effects on worker welfare if firms respond to minimum wage increases by reducing other workplace amenities. Moreover, understanding non-employment margins of adjustment is relevant to interpreting the broader minimum wage literature: specifically, the finding that most minimum wage increases have relatively small disemployment effects. The more firms are able to respond by either cutting costs or raising productivity, the more likely it is that disemployment effects of the minimum wage may be small even with limited employer monopsony power (Clemens, 2021).

[9] A third paper, Merrill-Francis et al. (2022), examines the correlation between minimum wages and fatal workplace injuries.

Our paper builds on these two papers in important ways. First, we use a different source of identification, leveraging not only geographic variation in minimum wage policies but also occupation-level variation in exposure to these minimum wages, enabling us to use metro-year fixed effects to control flexibly for time-varying geographical confounders which may be correlated with both minimum wages and workplace injury rates.[10] Moreover, our primary source of geographic variation is city-level minimum wage changes rather than state-level changes. Second, our use of workers' compensation data to measure workplace injuries, rather than data based on employer reporting, enables us to capture a wider range of occupational injuries, including those which we might expect to be particularly affected by the minimum wage (such as those related to muscular strain). Third, we make use of very detailed claim-level data on the nature and cause of injuries to establish work intensification as a likely mechanism.[11] While we focus here on the safety implications of work intensification, the intensification of work pressure also has broader welfare ramifications for workers.

Overall, the fact that three recent papers all find that increased minimum wages in the US increase workplace injury rates – using different (i) sources of minimum wage variation (geographic & time period), (ii) definitions of exposure to minimum wage increases, and (iii) data sources to measure injuries – suggests that raising minimum wages has important consequences for workplace health and safety - and perhaps other non-wage amenities - across a wide range of settings.

In addition, we contribute to the literature on the economic determinants of

[10] Utilizing only state-level variation in minimum wages, in contrast, raises concerns about endogeneity of state minimum wage changes.

[11] No prior paper has been able to examine this to our knowledge; Liu et al. (2024) rule out work intensification due, we believe, to an erroneous interpretation of the effect of minimum wages on illness cases.

workplace safety and on the relationship between wages and amenities (e.g. Marinescu et al., 2021; Sockin, 2021). Our findings imply that when an exogenous increase in the wage floor forces firms to raise wages, firms respond in a way which reduces the value of at least one important non-wage amenity (workplace safety). This is an interesting contrast to Dube, Naidu, et al. (2022), who find that after a *voluntary* corporate minimum wage increase, there is no deterioration in non-wage amenities.

3 Data

Workplace injury rates:

To measure workplace health and safety, we use data on workplace injuries from the California Division of Workers' Compensation (DWC), consisting of the universe of over 13 million individual worker's compensation claims administered by the DWC over the period 2000-2019. We assign each injury to a California metro area based on the ZIP code of injury (the best proxy we have for workplace ZIP code) and to a SOC code using the variables and parsing tool described in detail below. Using metro-occupation level employment data from the BLS Occupational Employment and Wage Statistics (OEWS), we can then construct annual workplace injury rates (injuries/employment) for each occupation-metro labor market.

To assign each claim to a SOC occupation code, we use the NIOSH Industry and Occupation Computer Coding System (NIOCCS) tool. The DWC data contains an unstructured text field for occupation description, alongside structured industry code and description fields. Using occupation and industry fields, the NIOCCS tool probabilistically assigns a SOC code to each claim. Using an 80% confidence threshold, we assign SOC codes to 74% of raw claims. In our baseline analyses, we use 5-digit SOC codes, and show robustness checks with 3-digit and 6-digit SOC codes. While this parsing process may introduce measurement error, we think this concern is minimal in our empirical setup: the likelihood of a match using the parser may differ across occupations, but our analyses leverage within-occupation variation in injuries since we use occupation-year and metro-occupation fixed effects.[12]

How does an injury appear in our data? Note that no dataset of workplace injuries captures all injuries: for an injury to appear in a dataset, it must be reported (typically either by a worker or employer). In our case, using workers' compensation data means we rely on worker reports of work-related injuries or illnesses. The state of California has the largest workers' compensation system in the U.S. – and one of its strictest. All workers are eligible to make a worker's compensation claim after a work-related injury or illness, defined as an injury or illness "resulting from a single event or from repeated exposure to injury-inducing conditions". The insurance covers the cost of the worker's medical care and, when applicable, temporary or permanent disability benefits, job displacement benefits, and death benefits for dependents. The scope of coverage is wide: it includes seasonal and migrant workers, and undocumented workers. The state of California legally requires employers to maintain worker's compensation insurance, regardless of the number of employees or size of establishment. Failure to provide coverage is a criminal offense punishable by a fine of not less than $10,000 or imprisonment for up to one year or both, and the state can issue penalties of up to $100,000 against illegally uninsured employers.[13]

Given (i) the wide scope of injuries that can be reported, (ii) the strong finan-

[12] We discuss the process of assigning occupation codes in more detail in the Appendix.
[13] Per Section 3700.5 of the California Labor Code.

cial incentives for employees to make claims, and (iii) full coverage of employers, we believe that this workers' compensation data constitutes a more comprehensive record of workplace injuries than is typically captured in other data sources on workplace injuries. The primary other data sources for workplace injuries are OSHA or BLS SOII data. Employer coverage is likely wider in our workers' compensation data than in OSHA data (since some firms are exempt from reporting), and substantially wider than in SOII data (which is a representative sample of firms rather than the universe). Moreover, both OSHA and SOII data are employer-reported. The criteria that govern reporting requirements for employers vs. eligibility for workers' compensation differ; and incentives for reporting also differ. The balance of evidence suggests under-reporting is more of an issue for employer-reported data: Boden (2008) for example found that for California in 2008, coverage of injuries or illnesses requiring at least 4 days away from work was about 40% greater in workers' compensation data than in SOII data from the equivalent establishments. And a simple comparison of the raw number of injuries in our workers' compensation data as compared to the OSHA injury reports for California in 2018 shows that the workers' compensation data captures around 40% more injuries.[14] Finally, our workers' compensation data contains more detailed information on the nature and cause of injury than other data sources, as well as information on the severity of the case and associated economic costs.

The average injury rate across our data in 2010, the middle of our sample, is 3.1%, meaning that there are 3.1 injuries per 100 workers per year. There is substantial variation in injury rates: across occupation-metro labor markets in 2010,

[14]This coverage gap between workers' compensation data and employer-reported OSHA or SOII data appears to be most acute for certain types of injuries — e.g. for those relating to muscular strain (Ruser, 2008; Phipps and Moore, 2010; Boden et al., 2010; Wuellner et al., 2017).

the 25th percentile had an annual injury rate of 0.5%, the median 1.9%, and the 75th percentile 4.7%. On average, higher-wage occupations have lower injury rates (Appendix Figure A1).

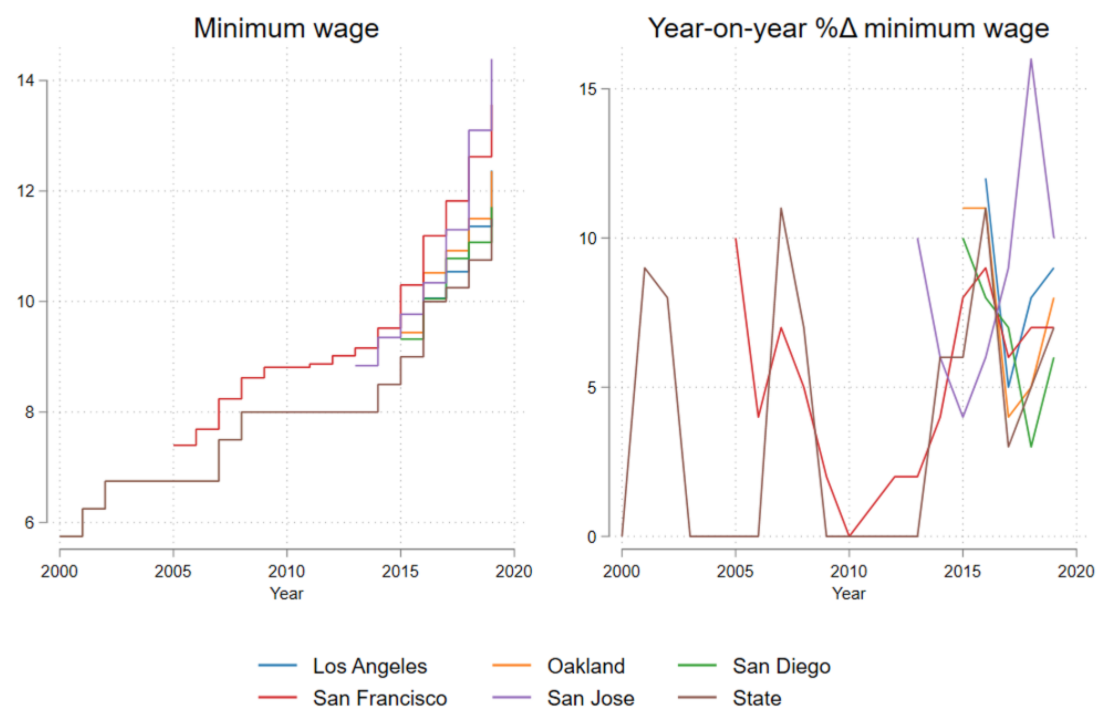
We can use our data on injury type to understand what kinds of injuries are most prevalent. 30% of injuries in our data are a strain or tear, 11% a contusion, 11% a laceration, and 10% a sprain or tear, 3% a puncture, and 3% a fracture. Among high-exposure occupations (which includes many workers in food service), burns are also a common injury type. We provide additional summary statistics on our injury data in Appendix Tables A1-A9.

Minimum wages:

Our minimum wage variation comes both from changes in the state-level minimum wage (for those areas without a higher city-level minimum), and from changes in city-level minimum wages across different parts of California. Over the period in question, several cities/municipalities set higher minimum wages than the California state minimum. We collected data on these city-level minimum wages from UC Berkeley Labor Center (2024) and Economic Policy Institute (2024), as well as data from local government websites.

Ideally, our unit of analysis would correspond directly to the level at which the minimum wage changes occurred: that is, at the level of low-wage occupations within specific cities/municipalities. However, we need employment data to construct injury rates, and the most granular geographic level for which we can obtain employment data by narrow occupation is the metropolitan statistical area (MSA), or the metropolitan division in the case of Los Angeles and San Francisco MSAs. We therefore calculate the population-weighted average minimum wage that applied in each MSA or metropolitan division (henceforth "metro") in each year across its component cities/municipalities and non-incorporated areas, and use this as our core measure of the minimum wage. Across the 28 metropolitan areas in California, we have 23 which are only subject to the state minimum wage, and 5 (large) metros which have higher minimum wages than the state minimum for at least some of the sample period (San Francisco, Los Angeles, San Jose, Oakland, and San Diego). Figure 1 shows the evolution of state and metro minimum wages over time, alongside the year-on-year growth rates for each minimum wage.

Figure 1: State and metro minimum wage levels and year-on-year growth rates over time.



Occupation-level exposure to local minimum wage changes:

When minimum wages are changed in a given city or state, not all jobs are affected equally. A relatively small percentage of workers have wages that are at or below the minimum, and evidence indicates that wage spillovers occur only for some limited subset of workers earning close to the minimum (Cengiz et al., 2019). Our identification strategy relies in part on comparing occupations which are more "exposed" to a minimum wage shock — i.e. those who earn close to the local minimum — with higher-wage occupations who should not be affected. For each metro-occupation labor market, we construct a continuous "exposure" measure as the share of workers in that metro-occupation in year t estimated to earn no more than 1.3x the local minimum wage. We estimate this share using BLS OEWS data, which gives the 10th, 25th, 50th, 75th, and 90th percentile of hourly pay for each occupation-metro-year, fitting a lognormal distribution to these percentiles, and using this lognormal distribution to impute the share of workers earning below the 1.3x minimum threshold.[15] To ensure our results do not overly rely on these modeling assumptions, we also conduct robustness checks below using (a) exposure variables based exclusively on reported percentiles without any further imputation of the wage distribution, and (b) exposure variables based on different multiples of the minimum wage as thresholds (e.g. 1.1x, 1.5x).

4 Empirical strategy

Our headline specifications take the following form:

$$y_{o,m,t} = \alpha_{o,m} + \alpha_{m,t} + \alpha_{o,t} + \beta \left(min_{m,t} * exposure_{o,m,t} \right) + \gamma min_{m,t} + \delta exposure_{o,m,t} + \epsilon_{o,m,t} \tag{1}$$

[15] Our choice of 1.3x as our "exposure threshold" is informed by the findings in Cengiz et al. (2019) that minimum wages affect the wages of those earning up to $3 above the minimum wage via spillover effects, and that these indirect effects represent a significant share (c. 40%) of the overall wage increase driven by minimum wage changes. This $3 range for spillover effects constitutes less than 1.3x the minimum for almost all minima in their sample, save for Washington state's 2016 minimum of $11.

The unit of observation is a local labor market: an occupation o in metro m in year t. We estimate regressions for two outcome variables $y_{o,m,t}$: (1) log of the injury rate, and (2) log mean hourly wage. The injury rate outcome is the core outcome of interest for this paper; the wage outcome variable specifications serve to cross-validate our identification strategy, since we expect minimum wages to affect injury rates only when they are actually binding on employers, i.e. when they meaningfully affect wages. We use logged dependent variables to enable a proportional interpretation of our results across occupations with different baseline injury rates and wages.

We use two different minimum wage shock variables $min_{m,t}$ in our baseline specifications: (i) the real increase in the minimum wage from the prior year, and (ii) an indicator variable taking value 1 if the nominal increase in the minimum was greater than 5%. In doing so, we show that our results are robust to different modeling assumptions. For instance, using real year-on-year growth as our minimum wage independent variable implies that the treatment effect is proportionate to the magnitude of the increase throughout the range of minimum wage changes and exposure we see, and that falls in the real minimum (when inflation erodes the value of a steady nominal minimum) will have the reverse effect. The indicator variable allows a non-linear responsiveness of injuries to minimum wage changes, estimating only effects for large minimum wage shocks.[16]

The $exposure_{o,m,t}$ variable refers to an occupation-metro-year's exposure to a

[16] For example, the cost of adjustment through work intensification may be increasing in the size of the minimum wage increase (e.g. in terms of employee turnover, stress, costs of stricter management), in which case we might expect the treatment effect on injuries to be concave in dose. We might alternatively expect work intensification to only "kick in" for major minimum wage shocks, because of e.g. fixed costs of adjustment or the need for the minimum wage increase to outstrip both inflation and underlying productivity growth in order to perturb the wage distributions. Finally, we might not expect the effect of falls in the real minimum to be the exact opposite of increases.

minimum wage change, defined as the share of the occupation-metro estimated to earn less than 1.3x the minimum wage in our headline specifications.

The β coefficient on the interaction term between exposure and the minimum wage variable is our coefficient of interest, measuring how injuries and wages in local labor markets respond to minimum wage shocks. Our specification assumes that an occupation-metro labor market only faces a minimum wage shock if both (a) it is in a metro area with a rising minimum wage and (b) pay is close enough to the minimum that it is plausibly affected by this minimum wage change.

Injury rates and exposure to minimum wage changes may be correlated for reasons other than through the causal mechanisms we propose above. We therefore incorporate three sets of fixed effects in our baseline specifications to better isolate our effect of interest. First, low-wage occupations in major metro areas, which see more minimum wage growth than the rest of California, may also experience consistently higher injury rates for unrelated reasons. We incorporate metro-occupation fixed effects $\alpha_{o,m}$ to avoid falsely inferring a treatment effect of the minimum wage from comparisons across such different labor markets. Second, minimum wage policies are not randomly assigned, and may be correlated with other policies or macroeconomic conditions that could independently affect injury rates. We use metro-year fixed effects $\alpha_{m,t}$ to control for such variation in time-varying unobservables at the metro levl. Third, low-wage "'treatment" occupations and high-wage "control" occupations may have different trends in injury rates over time that correlate with the observed trend in increased minimum wage activity over our sample period. For instance, low-wage frontline occupations may be trending more dangerous over time across the U.S., while high-wage occupations may exhibit stable injury rates, for reasons independent of the minimum wage. To control flexibly for such time trends and similar confounders across occupations, we use occupation-year fixed effects $\alpha_{o,t}$.

These fixed effects also help address concerns relating to both variation in (a) the rate of reporting among the injured, and (b) the rate at which we are able to confidently assign a SOC occupation to a claim using the NIOCCS parsing tool, as described above. The reporting margin, for instance, would threaten our identification strategy if changes in the rate of reporting were systematically correlated with exposure to minimum wage changes. Our fixed effects mean we need not be concerned by systematic correlations that can be explained by (a) time-invariant differences in reporting across local labor markets (absorbed by $\alpha_{o,m}$) or (b) occupation- or metro-level trends in reporting rates (absorbed by $\alpha_{o,t}$ and $\alpha_{m,t}$). Our fixed effects also mitigate reasonable concerns about systematic correlation between our "parsing rate" and minimum wage exposure.[17]

We cluster standard errors at the metro-occupation level in order to account for possible serial correlation within panel units, following Bertrand et al. (2004) and Cameron and Miller (2015).[18] We weight all regressions by the average employment of the metro-occupation labor market over our sample period.[19]

Intuitively, therefore, our identification strategy takes the injury rates of workers in low-wage (highly exposed) occupations in places where the minimum wage has increased, and compares it to the injury rate we would *predict* in that year for

[17] For instance, to the extent that the parser more accurately assigns occupations to later observations because of more standardized industry code inputs, our time-variant fixed effects should absorb this trend. Similarly, occupation-year and metro-occupation fixed effects should absorb differences between occupations in "parsibility". For example, occupations tightly associated with a single industry and with standardized occupation names will have a higher parsing rate than others, but our fixed effects mean we do not identify off this variation.

[18] In Appendix Figure A2 we show our results are robust to clustering instead at the metro level. However, in our approach a local labor market's treatment status is in part assigned at the metro level and in part by the occupation-year's wage distribution, so our setting corresponds to the "partially clustered" situation outlined in Abadie et al. (2023), implying that clustering at the metro level is overly conservative.

[19] We also present unweighted results in a robustness check.

these workers based on their occupation (i.e. the injury rates of the same occupation in other metro areas) and their metro area (i.e. the injury rates of other higher-wage occupations in the same metro area). The high-exposure occupations our identification strategy captures are predominantly workers in food service (including cooks and servers), agricultural workers, retail sales workers and cashiers, cleaning workers, laborers and material moving workers, and personal care aides. We list all large high-exposure occupations in Appendix Tables A7 and A8.

5 Results

Table 1 below shows our headline results. As shown in column 1, a 10% real year-on-year increase in the local minimum leads to an 11% increase in the injury rate for a fully exposed occupation (an occupation-metro whose entire wage distribution fell below 1.3x the new minimum wage in that year), conditional on our fixed effects. As expected, we also find that a minimum wage increase increases wages in low-wage occupations, with a 10% real year-on-year increase in the local minimum leading to an 8% increase in the mean hourly wage for a fully exposed occupation. The ratio of the two provides us with the elasticity of workplace injuries to minimum wage-induced wage increases. This suggests that an exogenous 10% increase in the cost of labor as a result of a minimum wage change leads to a 14% increase in the workplace injury rate: an elasticity of 1.4.[20]

To ensure our results are not sensitive to the modelling assumptions behind the real minimum wage growth variable, we also show results in columns 3 and

[20] This 2SLS-type inference requires an exclusion assumption that may not hold if, for instance, minimum wages affect injury rates in labor markets that do not see wage increases if cost-pressured work intensification affects a broader swathe of workers than just those directly affected by the increase.

4 for an indicator variable taking value 1 if nominal year-on-year minimum wage growth exceeds 5%. The coefficient in column 3 suggests that a "fully exposed" labor market experiencing a large minimum wage shock on average sees an 8% increase in its injury rate (and a 4% increase in average hourly wages). Once again taking the ratio of the two coefficents, we can estimate the elasticity of injury rates to wage shocks, finding here a larger elasticity of 1.97 (a 19.7% increase in injury rates for each 10% increase in wages induced by the minimum wage increases).

Table 1: Effect of exposure-minimum wage shock interaction on injury rates and wages, 2000-2019

Minimum wage variable:	Real year-on-year growth		Shock indicator variable	
Dependent variable:	Injury	Wage	Injury	Wage
Minimum-exposure interaction	1.145***	0.812***	0.077***	0.039***
	(0.382)	(0.062)	(0.023)	(0.004)
Fixed effects	Metro-Occ Metro-Year Occ-Year	Metro-Occ Metro-Year Occ-Year	Metro-Occ Metro-Year Occ-Year	Metro-Occ Metro-Year Occ-Year
N	116,318	116,318	116,318	116,318

This table reports estimates of the coefficient β on the minimum wage-exposure interaction term from equation 1 for different minimum wage and dependent variables. The minimum wage variables used are (a) real minimum wage growth year-on-year (e.g. 0.05 corresponding to 5% growth) in the first two columns (b) an indicator variable taking value 1 if nominal minimum wage growth is higher than 5% in the last two columns. The dependent variables used are log injury rate and log hourly wage. The exposure variable is the share (between 0 and 1) of employment in a given metro-occ-year cell below 1.3x the metro minimum wage, estimated by imputing a lognormal distribution of cell wages from observed percentile data. All regressions are weighted by total employment of in-sample cells in the metro-occ panel unit over the sample period. Occupations are defined at the 5-digit SOC level. Standard errors are clustered at the metro-occ level. The estimated exposure coefficient is not shown, and the minimum wage variable for all specifications is fully absorbed by the metro-year fixed effect. * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$)

These are economically significant effects. To see this, we can estimate the implied increase in the injury risk for a 10% minimum wage increase for the average low-wage worker, applying our coefficient from Table 1 column 1 to the average low-wage worker's injury rate and exposure to minimum wage changes. According to this calculation, a 10% increase in the minimum wages leads to an additional 3 injuries per 1,000 low-wage workers per year.[21] We can also carry out this calculation for specific occupations: after a 10% increase in the minimum wage, the additional injuries per 1,000 workers per year would be: 7 for agricultural workers, 5 for building cleaning workers, 4 for material moving workers, 3 for cooks and food preparation workers, 2.5 for health aides, and 1.5 for food and beverage serving workers.[22]

5.1 Robustness checks and falsification tests

5.1.1 Falsification tests

Following Cengiz et al. (2019) and Autor et al. (2016), we can subject our identification strategy to falsification tests by examining whether we find an effect of minimum wage shocks on groups that should not be affected. We do this by: (i) examining the effect of minimum wage shocks on marginally exposed cells; (ii) checking that our identification strategy does not find an effect of *future* minimum wage shocks, and; (iii) showing that our results do not hold for measures of exposure that would imply unreasonably wide spillover effects of the minimum wage.

First, we check that our results are not driven by strong estimated effects for labor markets where very few workers are exposed to minimum wages — as would be the case if, for instance, our coefficients in Table 1 were the best linear estimates

[21] This is calculated as follows: $injuryrate_t = injuryrate_{t-1}\,(1 + \beta \cdot \Delta minwage \cdot exposure)$. In this case, the calculation is $4.36\% \times (1 + 1.1 \times 0.1 \times 0.593) = 4.64\%$.

[22] The order of magnitude of our estimate is similar to the effect of financial pressures on injury rates documented in other work. Liu et al. (2024) find that a large state-level minimum wage increase of $ 1.29 per hour leads to a 4.6% increase in injuries per worker in the average establishment; this effect is smaller than ours, but also covers all workers (not only highly exposed workers). Our estimate is also broadly consistent with Charles et al. (2022), who find that a 1.1% increase in international metal commodities prices leads to a 0.15% increase in injuries per FTE worker in mining firms who produce those commodities.

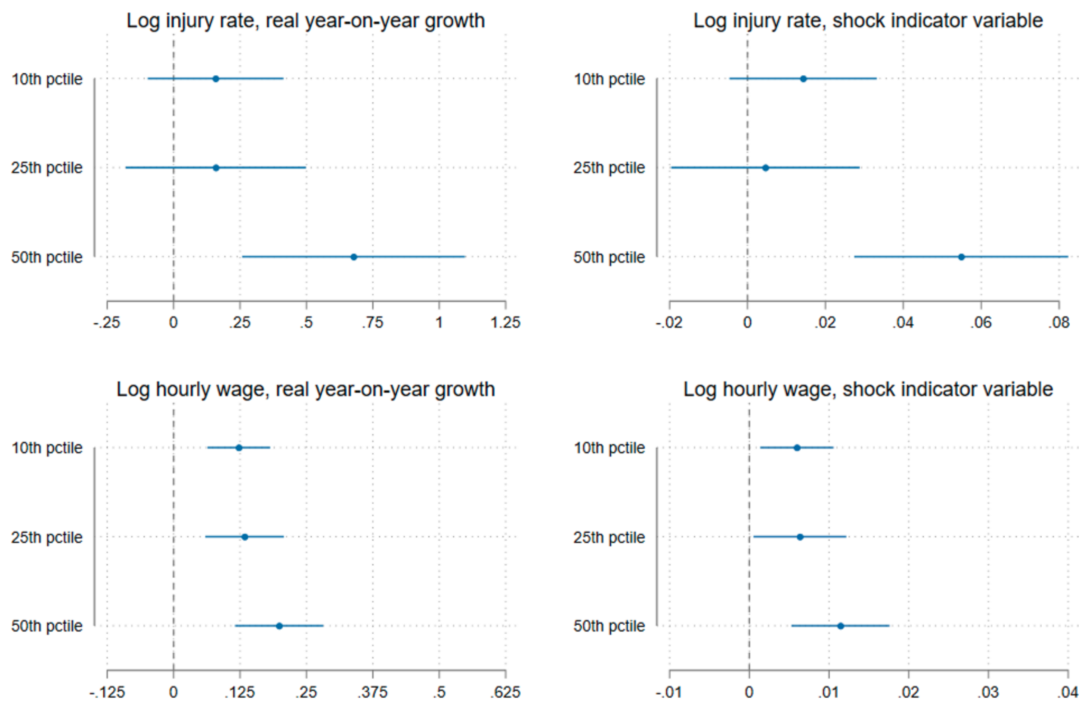
of a negative quadratic relationship — as this would significantly undermine our inference. In Figure 2 we report estimates of interaction coefficients β^i from:

$$y_{o,m,t} = \alpha_{o,m} + \alpha_{m,t} + \alpha_{o,t} + \sum_{i \in I} \left\{ \beta^i \left(min_{m,t} * D^i_{o,m,t} \right) + \delta^i D^i_{o,m,t} \right\} + \gamma min_{m,t} + \epsilon_{o,m,t} \tag{2}$$

where $I = \{10, 25, 50\}$. $D^i_{o,m,t}$ are a set of mutually exclusive exposure indicator variables that take a value of 1 if the ith percentile of a local labor market's wage distribution at time t is the highest out of the reported percentile values in I that fall below 1.3x the local minimum.[23] The excluded category, local labor markets with wage distributions almost entirely above the 1.3x threshold, contains 76,424 occ-metro-year observations, while the exposure indicator variables split the remaining observations into three approximately equal buckets (c. 13,000 observations in each). As shown in Figure 2, the effects of a minimum wage shock are primarily concentrated in local labor markets with high exposure shares, as would be expected.

[23] For example, $D^{25}_{o,m,t} = 1$ if occupation o in metro m in year t has the 25th percentile of its wage distribution below 1.3x the minimum wage, but the 50th percentile of its wage distribution above 1.3x the minimum wage.

Figure 2: Heterogeneous effects of minimum wage shocks for low-, medium- and high-exposure cells



These plots report estimates of interaction coefficients β^i from equation 2, where e.g. "25th pctile" shows estimates of β^{25}, the coefficient on the interaction term between the minimum wage variable and an indicator variable taking value 1 if the 25th percentile is the highest out of the reported 10th, 25th, and 50th percentiles of a cell's wage distribution that falls below 1.3x the local minimum wage. Each plot corresponds to a column in 1, i.e. a combination of dependent variable (log injury rate and log hourly wage) and minimum wage variable. The minimum wage variables are (a) real minimum wage growth year-on-year (e.g. 0.05 corresponds to 5% growth) in the first row, and (b) an indicator taking value 1 if nominal minimum wage growth is higher than 5% (constituting a "shock") in the second row. All regressions are weighted by total employment of in-sample cells in the metro-occupation panel unit over the sample period. Occupations are defined at the 5-digit SOC level. Standard errors are clustered at the metro-occupation level. 95% confidence intervals shown.

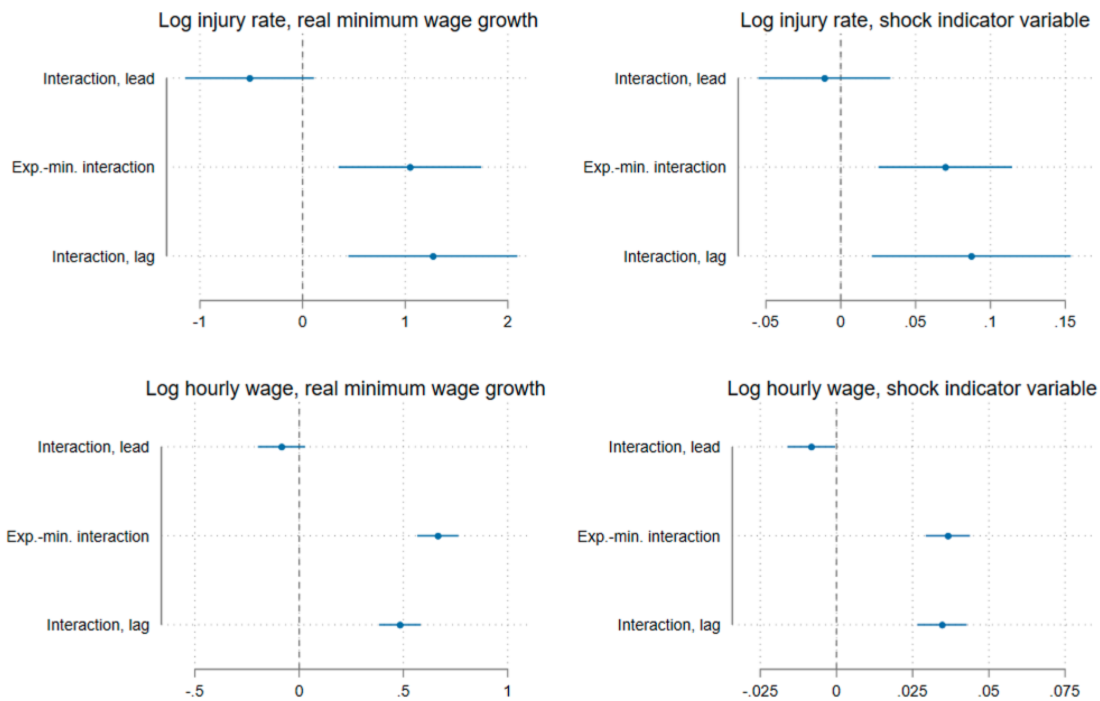
Second, we test whether our identification strategy implies a causal effect of a *future* minimum wage shock on a local labor market's wage or injury rates. The coefficient plots in Figure 3 show estimates of interaction term coefficients β_τ from:

$$y_{o,m,t} = \alpha_{o,m} + \alpha_{m,t} + \alpha_{o,t} + \\ \sum_{\tau=-1}^{1} \{\beta_\tau (min_{m,t+\tau} * exp_{o,m,t+\tau}) + \delta_\tau exp_{o,m,t+\tau} + \gamma_\tau min_{m,t+\tau}\} + \epsilon_{o,m,t} \quad (3)$$

This specification is identical to the specification in equation 1, but with lag and lead terms for exposure, minimum wage, and exposure-minimum interaction variables.

As shown in Figure 3, this approach identifies an effect of same year and previous year minimum wage shocks on wages and injury rates, but no significant effect of shocks in the next year. Significant coefficients on the lagged interaction term are consistent with our proposed mechanism: a minimum wage shock in one year is likely to continue to affect the wage distribution in the next year, and therefore to continue to affect firms' incentives to cut costs or intensify the pace of work. Moreover, the injury effect of minimum wages may take time to fully play out if, for instance, a high pace of work sustained over one year gives rise to increased risk of a repetitive strain injury the following year. We explore dynamics further in section 5.2.

Figure 3: Effect of same-period, previous-period, and future minimum wage shocks



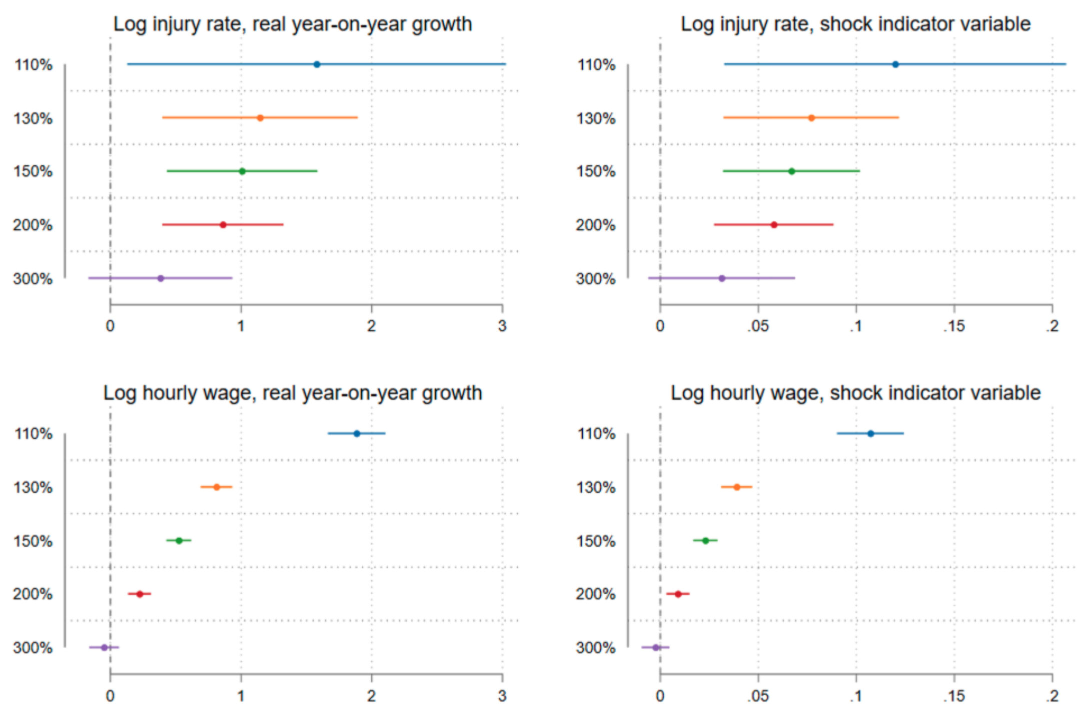
These plots show estimates of coefficients β_τ from equation 3. "Exp.-min. interaction" is the coefficient on the contemporaneous interaction term β_0, while the other coefficients shown are the 1 year lag and lead interaction terms. Each plot corresponds to a column in 1, i.e. a combination of dependent variable (log injury rate and log hourly wage) and minimum wage variable. The minimum wage variables are (a) real minimum wage growth year-on-year (e.g. 0.05 corresponds to 5% growth) in the first row, and (b) an indicator taking value 1 if nominal minimum wage growth is higher than 5% (constituting a "shock") in the second row. All regressions are weighted by total employment of in-sample cells in the metro-occupation panel unit over the sample period. Occupations are defined at the 5-digit SOC level. Standard errors are clustered at the metro-occupation level. 95% confidence intervals shown.

Third, in Figure 4 we assess whether designating a "treatment" group that consists of relatively high wage occupations leads to similar results, by setting a a threshold for exposure — in this case, 3x the minimum wage — that would imply "unrealistically large" spillover effects of the minimum wage (Cengiz et al., 2019, p.1416; Autor et al., 2016). The plots in this figure show how the coefficient on the interaction term changes as we vary the exposure threshold for each specification in Table 1. We find no significant effect at the 3x threshold, suggesting that higher minimum wages do not meaningfully affect injury rates in higher wage occupations, as would be expected.[24] Further, the consistent decline in magnitude of these coefficients as we increase the exposure threshold across both wage and injury regressions is consistent with the idea that, within metro areas that experience a minimum wage change, local occupational labor markets are affected differentially depending on the number of workers earning near the new minimum, as would be expected. While our 1.3x threshold is informed by the literature, these plots show our results are robust to both more stringent (1.1x) and more lax (1.5x) thresholds for exposure that might also be considered reasonable.[25]

[24]This is despite (a) correlation between exposure measures (the share of a wage distribution below 1.3x the minimum correlates with the share below 3x the minimum) and (b) there being greater variance in exposure when measured at the 3x than the 1.3x threshold.

[25]They also suggest that the relatively small effect sizes found in Liu et al., 2024 may be a consequence of combining different occupational labor markets in their unit of analysis. Their data is at the establishment level, with treatment status designated by state. A given establishment may employ a range of occupations, including, for instance, service workers who are likely to earn close to the local minimum, and managerial and technical occupations who are not, leading to relatively low estimates of minimum wage-injury elasticities.

Figure 4: Effect of minimum wage shocks for varying exposure thresholds

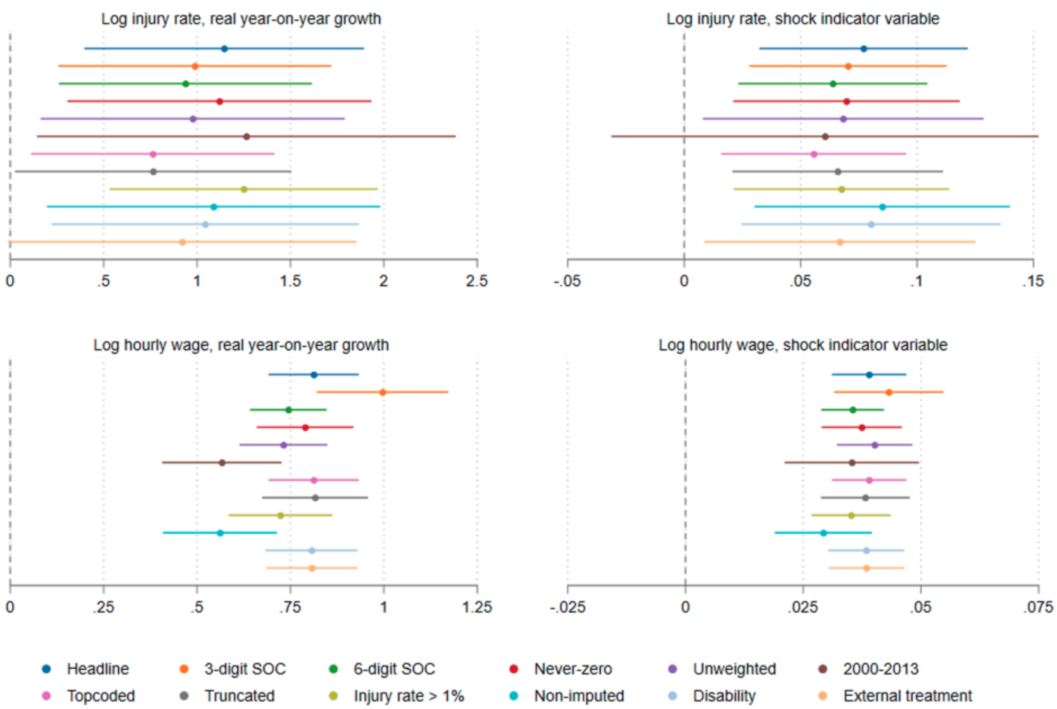


These plots show the interaction coefficients from Table 1 regressions, but with varied measures of exposure to minimum wage shocks, as measured by multiples of the prevailing local minimum. Each subplot corresponds to a column of Table 1 (i.e. a dependent variable-minimum wage variable combination). The minimum wage variables are (a) real minimum wage growth year-on-year (e.g. 0.05 corresponding to 5% growth) in the first row, and (b) an indicator taking value 1 if nominal minimum wage growth is higher than 5% (constituting a "shock") in the second row. The exposure variable is the share (between 0 and 1) of employment in a given metro-occ-year cell below x% of the metro minimum wage, estimated by imputing a lognormal distribution of cell wages from observed percentile data, as detailed above. All regressions are weighted by total employment of in-sample cells in the metro-occupation panel unit over the sample period. Occupations are defined at the 5-digit SOC level. Standard errors are clustered at the metro-occupation level. 95% confidence intervals shown.

5.1.2 Robustness checks

In Figure 5, we show the results of a range of robustness checks based on our main specification in equation 1. We briefly cover the rationale for each of these below.

Figure 5: Robustness checks



These plots show the interaction coefficients from each specification in Table 1 alongside a range of robustness checks, which deviate from our headline specification in the following ways. **3- and 6-digit SOC:** vary the level of granularity at which we classify occupations. **Never-zero:** restricts the sample to occupation-metro panel units with injuries recorded in each year of the sample. **Unweighted:** removes the employment-weighting from our headline specification. **2000-2013:** restricts the sample to the period before every metro in California sees some minimum wage growth in each year. **Topcoded:** topcodes injury rates at 5%. **Truncated:** restricts the sample to cells with injury rates below 5%. **Injury rate $>$ 1%:** removes the lowest tercile of cells by injury rate from the sample. **Non-imputed**: calculates exposure variable based purely on reported percentiles, without any further imputation of wage distributions. **Disability and external treatment:** use the log (reported) disability rate and log (reported) external treatment rate as injury dependent variables, respectively. For each robustness check, the wage and injury regressions are calculated using the same sample. Standard errors are clustered at the metro-occupation level, and bars represent 95% confidence intervals.

Choosing the level of granularity at which we classify occupations involves trading off precision with noise, as well as with increased likelihood of error in how the occupation descriptions in the WCIS data are "parsed" into standardized SOC codes. In our **3- and 6-digit SOC** robustness checks, we show our results are robust to different decisions regarding this trade-off by using both broader and finer occupational taxonomies than our headline specification.

Our **never zero** specification responds to the concern that our results might be affected by excluding cells with 0 reported injuries from our analysis (since we use logged injury rate as our dependent variable). By restricting the sample to occupation-metro panel units with injuries, wage, and employment data in every sample year, we focus exclusively on units where only the intensive margin is relevant and where, for instance, calculation of occupation-metro fixed effects is not biased by selective missingness. This sample covers 43% of observations and 82% of underlying injury claims from our main analysis sample, and shows similar results. We also conduct analyses using Poisson pseudo-maximum likelihood (PPML) specifications that allow us to capture both intensive and extensive margin effects in Appendix A2.1.

The **unweighted** coefficients show estimates for Table 1 coefficients but without weighting by employment.

The **2000-2013** coefficients restrict our sample only to years 2000-2013. As shown in Figure 1 above, from 2014 onwards each metro area in California sees a minimum wage increase in each year (albeit of very different magnitudes). Over 2000-2013, in contrast, identifying variation is driven only by (infrequent) changes in the state minimum wage or by changes in the San Francisco metro area. Our estimates of the effects on both injuries and wages are very similar over 2000-2013 as for the full period, demonstrating our results are not being driven only by this period of correlated, successive shocks from 2014 onward.

Our next three robustness checks aim to address concerns that misreported or trivial changes in injury rates may be driving our results. First, some cells in our sample have extremely high injury rates, potentially arising from systematic misreporting or parser error. Our fixed effects should mostly address these issues (e.g. if the parsing tool systematically over-assigns claims to a particular occupation), but to further ensure our results are not driven by these high-injury rate cells, we also conduct analyses where we **topcode** and **truncate** the analysis sample using a 5% injury rate as the upper limit. These changes affect approximately one quarter of our analysis sample, and represent a very conservative cut-off point, since the average annual injury rate in our data is 2-3%. Nevertheless, we find our results are not sensitive to either change. Conversely, since we use a logged dependent variable, another concern might be that cells with very *low* injury rates are overly driving our results: very small changes in the number of injuries in low-rate cells will represent extremely large *proportional* changes in the log injury rate. We address this concern by showing our results are not sensitive to dropping cells with injury rates below 1% (comprising two thirds of our main sample).

In our headline specifications, we estimate exposure by imputing a lognormal wage distribution for each local labor market based on reported wage percentiles in the OEWS data. We show our results are not overly sensitive to this modelling assumption by conducting a robustness check using **non-imputed** exposure variables based exclusively on reported wage percentiles without any further imputation of the wage distribution (see Appendix A3.1.2 for details on the construction of our exposure variables).

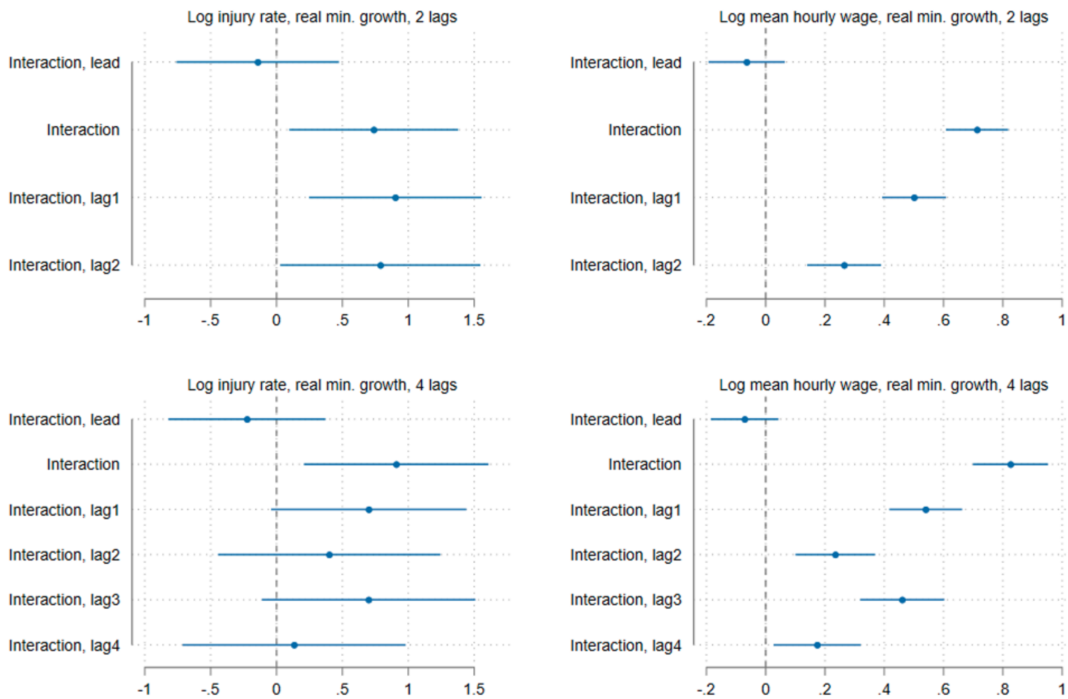
While we believe our fixed effects mitigate much of the threat that reporting issues pose to our identification strategy, we further address this concern by restricting our attention to injuries where workers face particular incentives to report by virtue of the severity of injury (Biddle and Roberts, 2003). First, we find an effect of minimum wage shocks on the rate of **disability** (claims where a date of disability is recorded in the FROI, c. 26% of sample claims). Approximately one fifth of these claims are permanent impairment cases, which are serious cases wherein an employee experiences lasting disability resulting in reduced earning capacity after maximum medical improvement is reached. Second, we find an effect of minimum wage shocks on injury rates when we restrict the sample to claims that record that initial **external medical treatment** was sought (38% of sample claims). Further, if there were to be some systematic relationship between minimum wage increases and reporting rates (beyond the correlations absorbed by our fixed effects), this would likely be that in a labor market where minimum wage increases may induce job loss, workers *reduce* their likelihood of reporting an injury by virtue of an increased fear of retaliation.

5.2 Timing and dynamics

How long does the effect of minimum wage increases on workplace injuries last? We explore the dynamics by estimating our baseline regression with lags and leads (equation 3) over a multi-year timeframe, incorporating one year lead and up to four years' lags of the minimum wage shock. Since our sample period is only twenty years, this substantially reduces our identifying variation. We show plots with 2 years' and 4 years' lags in Figure 6, using our minimum wage growth shock, and show the equivalent plots with the large minimum wage indicator variable in Appendix Figure A3.

Our estimates show that a minimum wage increase has a large same-period effect on both average wages and injury rates, which slowly fades over the following four years. Notably, the rate at which the effect fades is very similar for both the effect on wages and the effect on injury rates. This implies a persistent effect of higher minimum wage on injuries: for the period over which a minimum

Figure 6: Effect of minimum wage shocks with 2 and 4 lags



The above coefficient plots report estimates of lead, same-period, and lagged interaction terms from 3 (modified to have 2 or 4 lags), for dependent variables log injury rate and hourly mean wage; the real year-on-year growth minimum wage variable; and 2/4 lags. Standard errors are clustered at the metro-occupation level, bars represent 95% confidence intervals.

wage increase raises wages above their counterfactual level, it will also lead to a sustained increase in injuries.[26] In Online Appendix A2.2 we present an alternative specification that uses year-on-year within-unit changes in injury rates/wages as dependent variables, which corroborates our findings on the dynamics of our effect.[27]

[26]Specifically, the implied elasticity of the injury rate to minimum wage induced wage changes is between 1.1 and 2 – similar to our headline estimate of 1.4 – for the contemporaneous period and each year up to 3 years after the initial minimum wage change. For the fourth lag, the implied elasticity is 0.6.

[27]This specification also shows our results are robust to a different identification approach.

5.3 Mechanisms

In our conceptual framework (section 2) we outlined three mechanisms by which minimum wages might increase injury rates: reduced safety spending, work intensification, or capital-labor/labor-labor substitution.

Our data provide us with detail on the nature and cause of each injury/illness (NOI and COI). This enables us to shed light on the work intensification mechanism in particular, since certain types of injuries are most consistent with work intensification (as opposed to safety spending or capital-labor/labor-labor substitution). Specifically, we isolate what we term "cumulative physical" (CP) injuries. These injuries are characterized by two key features: (i) they are clearly related to physical activity, and (ii) they arise from repeated strain rather than a single, isolated incident. For example, claims with an NOI of carpal tunnel syndrome, or an NOI of "Strain or Tear" combined with COI of "Cumulative, No Other Cause", would be classified as CP injuries. These injuries represent 7.5% of our sample, though this likely underestimates their true prevalence.[28]

Table 2 reports interaction coefficients from equation 1 for the log CP injury rate, log injury rate (all claims), and log hourly wage dependent variables, limiting the sample to cells with at least one CP injury to ensure consistency across specifications. We see that the coefficients for the overall injury rate and wage specifications are essentially the same as in Table 1, but that the coefficient for the CP injury

[28] This is due to limitations in both workers' compensation and SOII data, which tend to underreport conditions like carpal tunnel compared to sudden strains (Boden et al., 2010; Ruser, 2008). Further, in isolating injuries we are confident are cumulative in nature, we might for instance miss a number of claims with a NOI relating to physical motion, such as strain, but where the COI is unclear as to whether the injury was the result of an accident or cumulative process (e.g. COI "using tool or machinery"). One illustrative example: there are approximately 20,000 claims that contain "RSI" or "repetitive strain" in the injury description field, but with COI and NOI combinations that do not fit our measure of a CP injury.

Table 2: Effect of exposure-minimum wage shock interaction on cumulative physical injury rates and wages, 2000-2019

Minimum wage variable:	Real year-on-year growth			Shock indicator variable		
Dependent variable:	CP	All claims	Wage	CP	All claims	Wage
Min.-exp. interaction	2.174***	1.169***	0.815***	0.117***	0.079***	0.039***
	(0.594)	(0.411)	(0.066)	(0.037)	(0.025)	(0.004)
Fixed effects	Metro-Occ	Metro-Occ	Metro-Occ	Metro-Occ	Metro-Occ	Metro-Occ
	Metro-Year	Metro-Year	Metro-Year	Metro-Year	Metro-Year	Metro-Year
	Occ-Year	Occ-Year	Occ-Year	Occ-Year	Occ-Year	Occ-Year
N	67,768	67,768	67,768	67,768	67,768	67,768

This table reports estimates of the coefficient β on the minimum wage-exposure interaction term from equation 1 for log cumulative physical (CP) injury rate, log injury rate (all claims), and log hourly mean dependent variables. The sample is restricted to cells with one CP injury to ensure the same sample across specifications. The minimum wage variables used are (a) real minimum wage growth year-on-year (e.g. 0.05 corresponding to 5% growth) in the first two columns (b) an indicator variable taking value 1 if nominal minimum wage growth is higher than 5% in the last two columns. The dependent variables used are log injury rate and log hourly wage. The exposure variable is the share (between 0 and 1) of employment in a given metro-occ-year cell below 1.3x the metro minimum wage, estimated by imputing a lognormal distribution of cell wages from observed percentile data. All regressions are weighted by total employment of in-sample cells in the metro-occupation panel unit over the sample period. Occupations are defined at the 5-digit SOC level. Standard errors are clustered at the metro-occupation level. The estimated exposure coefficient is not shown, and the minimum wage variable for all specifications is fully absorbed by the metro-year fixed effect. * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$)

dependent variable is 1.5x to 2x bigger than the overall injury rate coefficient, suggesting a much more elastic relationship between minimum wage-induced wage changes and this type of injury (though this difference is not statistically significant).

The increase in cumulative physical injuries is most consistent with a work intensification channel (rather than a reduced safety spending channel). Pace and intensity of work are widely recognized as risk factors for musculoskeletal disorders (Punnett and Wegman, 2004). In one recent evocative example, a Washington Department of Labor and Industries investigation into Amazon workhouses alleged that the pace of work and associated monitoring systems had a "direct connection" with musculoskeletal disorders (Lewis, 2021).[29] The two occupations in our sample with the highest number of cumulative physical injuries are Laborers/Material Movers (which would include warehouse workers) and Building Cleaning Workers (Appendix Table A9). Both are highly exposed to minimum wage shocks and are occupations for which the work intensification mechanism is particularly plausible.

Unfortunately, we cannot conduct a similar test to isolate the safety spending mechanism. The majority of claims in our sample appear to be related to one-off accidents — for example, the NOI might be "laceration" and the COI might be "struck or injured by moving parts of machine". Accidents, however, might be the result either of reduced safety spending (if injury is caused by poor equipment maintenance or lack of protective gear), or of work intensification (if a heightened pace of work increases the likelihood of human error). We nonetheless construct an "Accidents" category of injuries by restricting the sample to claims where the COI and NOI together strongly suggest the injury was caused by an accident.[30] These represent 61% of all injuries in our sample, and 65% of injuries for highly-exposed labor markets (50% earning less than 1.3x the local minimum).[31] We estimate very

[29] Given the immediacy of our effect, we believe the sort of safety spending that could respond quickly to cost shocks (e.g. reducing spending on maintenance and cleaning) is unlikely to lead to an increase in this type of injury. One plausible way that the safety spending mechanism could affect the CP injury rate is if firms do not adequately train workers. We would then expect this channel to operate through increasing the injury rate of low-tenure workers — in our data, however, minimum wage shocks are associated with a small (but statistically significant) increase in the tenure of the injured (Appendix Table A11), consistent with the literature on minimum wages reducing worker turnover.

[30] This is not an exact complement of the CP injury type, as some types of injury/illness are excluded from both categories (e.g. asbestosis, cancer), while other physical injury claims may not provide enough information for us to confidently assign it to either accident to CP injury-type.

[31] We develop a less stringent accident category by excluding NOIs and COIs inconsistent with an accident-type injury and then requiring that *either* the NOI or COI suggest an accident: 84% of injuries and 86% of injuries in high-exposed labor markets are identified as accidents by this method. See Appendix Section A3.4.2 for detail on construction of these variables.

similar estimates of the effects of minimum wages when we restrict our analysis to accident-type claims as in our full sample analysis (Table A10).

Thus, our findings strongly suggest that work intensification is one mechanism by which minimum wage increases affect workplace injury rates; it is also possible that reduced safety spending plays a role. We see our findings as an important addition to Liu et al. (2024), who find direct evidence for the safety spending mechanism, but do not find evidence for the work intensification mechanism.[32]

We now turn our attention to the substitution and selective disemployment channels. Minimum wages may lead to changes in workplace injury rates if they induce firms to substitute low-wage labor for capital or for higher-skilled labor. Such an effect could be driven either by increasing the likelihood of injury for affected workers through changes in the allocation of tasks, or by compositional effects that affect measured injury rates at the aggregate level. The latter would affect the normative force of our result. We believe that compositional effects are unlikely to drive our result for the following reasons.

First, we detect a very short-term impact of the minimum wage on injuries. Work on the capital-labor substitution effect suggests a longer lead-in time; Aaronson, French, et al. (2018) argue for a "putty-clay" analysis of capital-labor substitution in the restaurant industry following minimum wages, as in the short-term individual restaurants are unable to adjust their capital-labor ratio, but over the longer-term industry-level changes in the capital-labor ratio are driven by entry

[32] Liu et al. (2024) claim their results are unlikely to be driven by work intensification/effort requirements. They argue that if work intensification is the primary mechanism, minimum wages should increase the *illness* rate as well as the injury rate, but they detect null effects for illness dependent variables. However, the illnesses these authors cite as motivating examples — "stress, depression, heart diseases, and strokes" (9) — are conditions for which employers hold considerable discretion in deciding whether to report them to OSHA ((CFR 29 C.F.R. § 1904.5(b)(2)(ix)); and many illnesses for which OSHA mandates reporting are not those which one would expect to be responsive to work intensification (e.g. respiratory disease, hearing loss).

and exit of relatively more and less capital-intensive establishments.[33]

Further, we do not think our results are driven by the compositional effects of labor-labor substitution or selective disemployment. While labor-labor substitution responses to the minimum wage might be rapid (Clemens, Kahn, et al., 2020), to explain our results with labor-labor substitution it would need to be the case either that (i) higher-skilled workers within a specific occupation have higher injury rates or that (ii) firms selectively retain injury-prone workers during this process. Since the occupational health literature consistently finds that higher-tenure, higher-experience workers tend to have lower injury rates, and since these workers also tend to be more productive, neither of these explanations seem plausible.

6 Conclusion

Workplace injuries are disproportionately concentrated among low-wage earners, with significant and lasting consequences, including permanent disability and even death. In the US alone, nearly 3 million workers experienced an injury on the job in 2022, over 5,000 of which were fatal (BLS, 2023). Understanding the impact of minimum wage policies on workplace safety is particularly important as minimum wage increases are often advocated as a way to improve the well-being of low-wage workers.

Using detailed administrative data on workplace injuries for the US' largest worker's compensation system, we find that minimum wage increases lead to higher workplace injury rates in the short term. Our empirical strategy exploits

[33]Other work by Aaronson and Phelan (2017) suggests there may be shorter-term capital adjustment for low-wage jobs with a larger share of routine cognitive tasks. The prevalence of physical injuries (strains, sprains) in our data, the range of occupations and our finding that cumulative physical injuries appear to be particularly sensitive to minimum wage increases suggests that automation of routine cognitive work cannot fully explain our detected effect.

variation in city-level minimum wages and the share of low-paid work in metro-occupation labor markets to identify this effect. A 10% increase in the local minimum wage is estimated to increase the injury rate by 11% in an occupation fully exposed to the minimum wage change.

Our analysis provides suggestive evidence that work intensification may be an important mechanism, with broader welfare implications extending beyond workplace safety. The effect of minimum wage increases is particularly pronounced for injuries related to cumulative physical strain, such as carpal tunnel syndrome and repetitive strain injuries, where a 10% increase in minimum wage leads to a 21% increase in the rate of such injuries per worker. This suggests that employers may respond to higher labor costs by intensifying the pace of work, leading to an elevated risk of injuries.

Future empirical research could further disentangle the mechanisms behind the effect we detect, while theoretical work could model the complex interactions between these non-employment channels of worker and firm adjustment to minimum wage shocks. Our findings are also relevant for policymakers, who may want to consider the potential countervailing effects of minimum wage hikes on worker welfare, and develop policy accordingly.

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Online Appendix

A1 Additional tables and figures

Table A1: Summary statistics for selected variables, 2010

Name	Mean	P10	P25	P50	P75	P90
Employment	1,760	50	110	340	1,250	3,830
Injury Rate	4.1	0	0.5	1.9	4.7	9.2
Hourly Wage	26.03	11.71	15.55	22.15	33.20	45.28

This table reports the mean and selected percentiles of employment, injury rate, and hourly wages (2010 USD) across metro-occupation labor market cells in 2010 (the midpoint of our sample).

Table A2: Employment-weighted summary statistics for selected variables, 2010

Name	Mean	P10	P25	P50	P75	P90
Employment	18,341	900	2,740	8,374	23,900	51,320
Injury Rate	3.1	0.5	0.9	2.1	4.1	6.9
Hourly Wage	24.07	10.65	12.5	18.48	31.09	45.17

This table reports the mean and selected percentiles of employment, injury rate, and hourly wages (2010 USD) across metro-occupation labor markets in 2010 (the midpoint of our sample), weighted by cell employment.

Table A3: Most common causes of injury

Cause of injury	Number of injuries	Share of total (%)
Lifting	933,516	10.3
Strain or Injury by, NOC	711,512	7.9
Other Miscellaneous, NOC	691,014	7.6
Repetitive Motion	440,950	4.9
Fall, Slip, Trip, NOC	427,620	4.7
Cumulative, NOC	381,247	4.2
On Same Level	353,415	3.9
Pushing or Pulling	330,783	3.7
Cut, Puncture, Scrape	306,222	3.4
Falling or Flying Object	295,543	3.3

Table A4: Most common causes of injury (high-exposure cells)

Cause of injury	Number of injuries	Share of total (%)
Lifting	170,146	11.0
Strain or Injury by, NOC	114,601	7.4
Other Miscellaneous, NOC	95,349	6.2
Fall, Slip, Trip, NOC	83,527	5.4
On Same Level	69,407	4.5
Cumulative, NOC	62,903	4.1
Repetitive Motion	60,991	3.9
Falling or Flying Object	59,520	3.9
Cut, Puncture, Scrape	57,030	3.7
Hand Tool, Utensil (Not Powered)	52,539	3.4

In this table, we restrict attention to cells where over 50% of the workforce earns less than 1.3x the local minimum wage — the share column corresponds to the share of each COI in the total number of injuries in high-exposure cells.

Table A5: Most common natures of injury

Nature of injury	Number of injuries	Share of total (%)
Strain or Tear	2,727,539	30.1
Contusion	997,491	11.0
Laceration	984,242	10.9
Sprain or Tear	859,693	9.5
All Other Specific Injuries, NOC	774,178	8.6
All Other Cumulative Injuries	440,977	4.9
Puncture	310,555	3.4
Multiple Physical Injuries Only	250,993	2.8
Inflammation	244,963	2.7
Fracture	233,941	2.6

Table A6: Most common natures of injury (high-exposure cells)

Nature of injury	Number of injuries	Share of total (%)
Strain or Tear	451,273	29.2
Laceration	209,162	13.5
Contusion	199,731	12.9
All Other Specific Injuries, NOC	134,362	8.7
Sprain or Tear	126,404	8.2
All Other Cumulative Injuries	73,217	4.7
Burn	47,064	3.0
Multiple Physical Injuries Only	44,472	2.9
Inflammation	40,407	2.6
Puncture	39,282	2.5

In this table, we restrict attention to cells where over 50% of the workforce earns less than 1.3x the local minimum wage — the share column corresponds to the share of each NOI in the total number of injuries in high-exposure cells.

Table A7: Summary statistics for large, high-exposure occupations (3-digit SOC)

Occupation title	Inj. rate	Emp. share	Exp.
Other Food Prep./Serving Related Workers	2.9	1.3	89.4
Food and Beverage Serving Workers	1.7	4.6	80.6
Agricultural Workers	8.2	1.2	76.1
Cooks and Food Preparation Workers	4.6	2.5	60.6
Retail Sales Workers	2.2	6.5	56.6
Other Personal Care/Service Workers	2.6	1.7	51.9
Building Cleaning/Pest Control Workers	9.8	2.2	47.6
Material Moving Workers	8.1	3.7	46.1
Other Protective Service Workers	3.0	1.3	38.6
Nursing/Psychiatric/Home Health Aides	6.3	1.1	34.1

This table shows, in % terms, the injury rate, share of sample employment, and average exposure across cells (share of cell estimated to earn less than 1.3x local minimum) for the top 10 most exposed occupation groups (3-digit SOC) representing over 1% of employment in our sample.

Table A8: Summary statistics for large, high-exposure occupations (5-digit SOC)

Occupation title	Inj. rate	Emp. share	Exp.
Dishwashers	4.0	0.5	90.6
Dining Room/Cafeteria Attendants, Bartender Helpers	2.4	0.5	90.2
Fast Food and Counter Workers	1.1	2.5	83.3
Waiters and Waitresses	1.8	1.8	79.3
Miscellaneous Agricultural Workers	8.3	1.2	76.3
Food Preparation Workers	3.5	0.7	67.0
Cashiers	1.9	2.7	63.7
Personal Care Aides	3.3	0.9	62.1
Cooks	5.0	1.8	57.7
Retail Salespersons	2.8	3.3	53.8
Laborers and Material Movers, Hand	8.7	3.2	53.1
Building Cleaning Workers	9.8	2.2	48.8
Stock Clerks and Order Fillers	4.0	1.5	46.2
Miscellaneous Production Workers	8.3	0.6	44.5
Security Guards and Gaming Surveillance Officers	3.0	1.1	42.5
Counter and Rental Clerks and Parts Salespersons	0.7	0.7	39.5

This table shows, in % terms, the injury rate, share of sample employment, and average exposure across cells (share of cell estimated to earn less than 1.3x local minimum) for the top 15 most exposed occupations (5-digit SOC) representing over 0.5% of employment in our sample.

Table A9: Occupations with highest number of cumulative physical (CP) injuries

Occupation Title	CP Injuries	CP Injury Rate	Exp.
Laborers/Material Movers, Hand	46,276	3.1	53.1
Building Cleaning Workers	35,881	3.0	48.8
Customer Service Reps.	28,071	4.9	14.2
Office Clerks, General	26,212	2.9	22.1
Secretaries/Admin. Assistants	16,626	1.1	7.8
Driver/Sales Workers/Truck Drivers	15,646	1.6	16.9
Misc. Assemblers/Fabricators	14,988	3.8	32.5
Police Officers	14,748	6.6	0.1
Cooks	12,655	1.3	57.7
Cashiers	11,900	0.8	63.7

In this table, we show total cumulative physical injury count, average occupational CP injury rate over the sample, and average occupational exposure for the 5-digit SOC occupations with the highest number of CP injuries.

Table A10: Effect of exposure-minimum wage shock interaction on injury and accident rates, 2000-2019

Minimum wage variable:	Real year-on-year growth			Shock indicator variable		
Dependent variable:	Acc.	Acc. (strict)	All claims	Acc.	Acc. (strict)	All claims
Minimum-exposure interaction	1.129***	1.033***	1.140***	0.081***	0.072***	0.076***
	(0.382)	(0.394)	(0.382)	(0.024)	(0.025)	(0.023)
Fixed effects	Metro-Occ Metro-Year Occ-Year	Metro-Occ Metro-Year Occ-Year	Metro-Occ Metro-Year Occ-Year	Metro-Occ Metro-Year Occ-Year	Metro-Occ Metro-Year Occ-Year	Metro-Occ Metro-Year Occ-Year
N	113,296	107,613	113,296	113,296	107,613	113,296

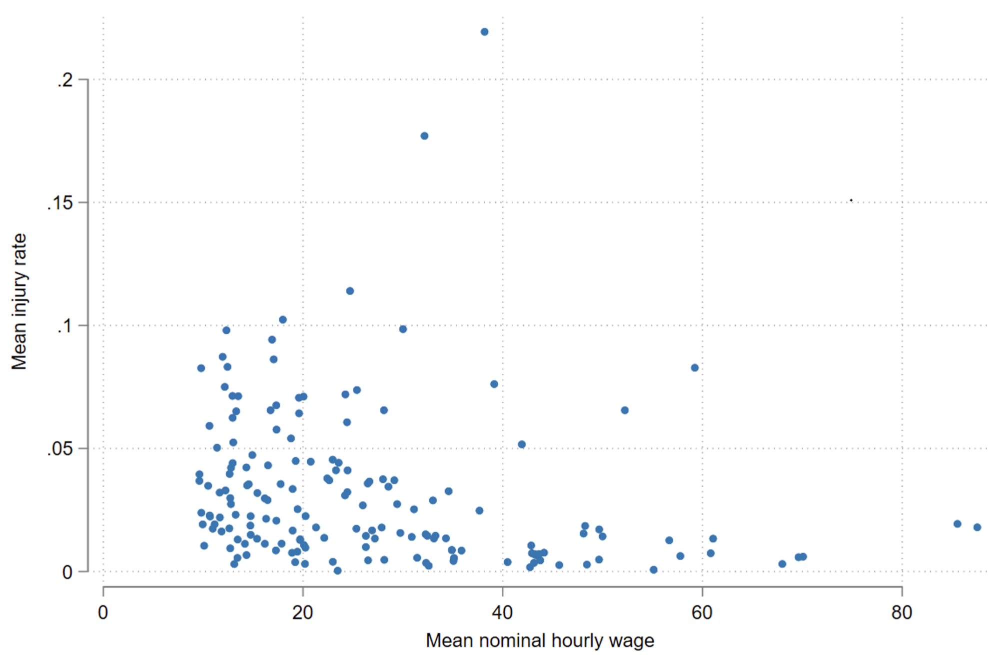
This table reports estimates of the coefficient β on the minimum wage-exposure interaction term from equation 1 for dependent variables (i) log injury rate; (ii) log accident rate; and (iii) log accident rate (strict definition), with the sample restricted to cells where data on all three dependent variables is available for consistency. See Section A3.4.2 for dependent variable definitions. The minimum wage variables used are (a) real minimum wage growth year-on-year (e.g. 0.05 corresponding to 5% growth) in the first two columns (b) an indicator variable taking value 1 if nominal minimum wage growth is higher than 5% in the last two columns. The dependent variables used are log injury rate and log hourly wage. The exposure variable is the share (between 0 and 1) of employment in a given metro-occ-year cell below 1.3x the metro minimum wage, estimated by imputing a lognormal distribution of cell wages from observed percentile data. All regressions are weighted by total employment of in-sample cells in the metro-occupation panel unit over the sample period. Occupations are defined at the 5-digit SOC level. Standard errors are clustered at the metro-occupation level. The estimated exposure coefficient is not shown, and the minimum wage variable for all specifications is fully absorbed by the metro-year fixed effect. * (p<0.10), ** (p<0.05), *** (p<0.01)

Table A11: Effect of exposure-minimum wage shock interaction on log average tenure rate of injured, 2000-2019

	Real year-on-year growth	Shock indicator variable
Minimum-exposure interaction	0.942***	0.032*
	(0.264)	(0.020)
Fixed effects	Metro-Occ	Metro-Occ
	Metro-Year	Metro-Year
	Occ-Year	Occ-Year
N	103,678	103,678

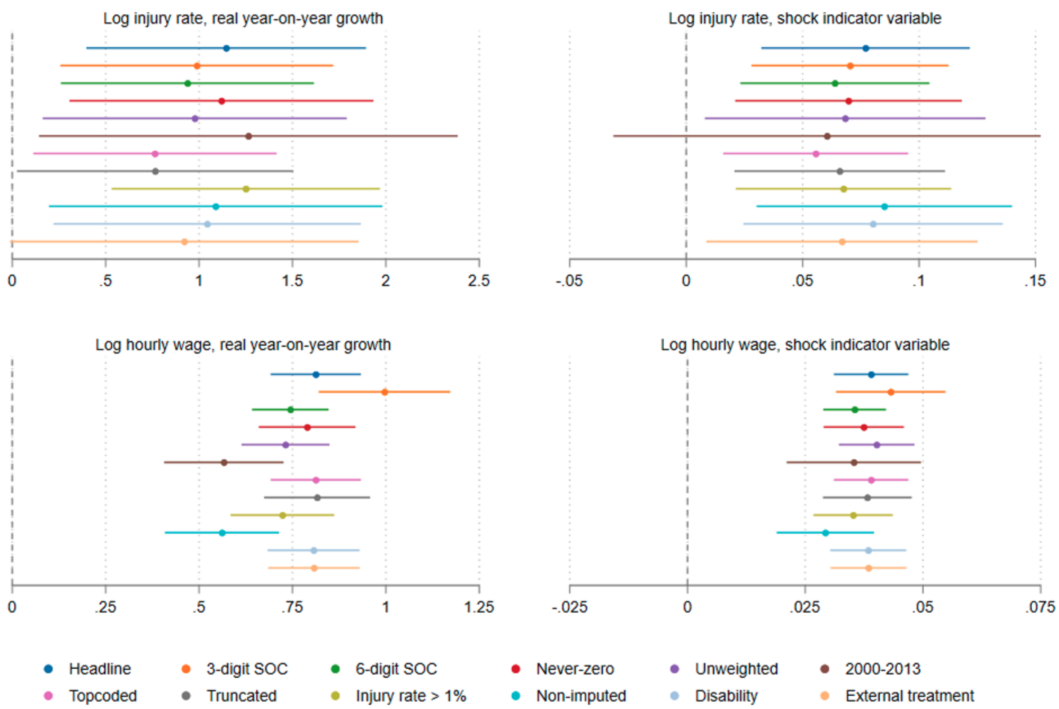
The minimum wage variables used are (a) real minimum wage growth year-on-year (e.g. 0.05 corresponding to 5% growth) in the first two columns (b) an indicator variable taking value 1 if nominal minimum wage growth is higher than 5% in the last two columns. The exposure variable is the share (between 0 and 1) of employment in a given metro-occ-year cell below 1.3x the metro minimum wage, estimated by imputing a lognormal distribution of cell wages from observed percentile data. All regressions are weighted by total employment of in-sample cells in the metro-occupation panel unit over the sample period. Occupations are defined at the 5-digit SOC level. Standard errors are clustered at the metro-occupation level. The estimated exposure coefficient is not shown, and the minimum wage variable for all specifications is fully absorbed by the metro-year fixed effect. * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$)

Figure A1: Average wage and average injury rate by occupation



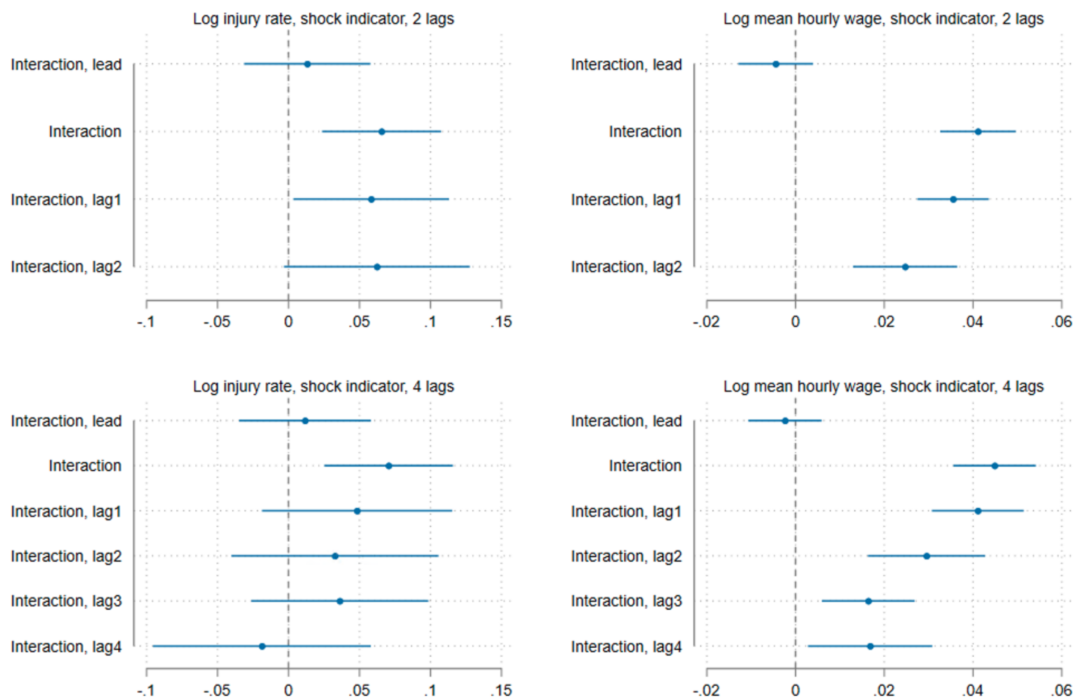
This figure shows the average hourly wage and average annual injury rate across the SOC 5-digit occupations in our data.

Figure A2: Robustness checks with CBSA-level clustered standard errors



This plot mirrors Fig. 5, but clusters standard errors at the metro level (versus metro-occupation clustering in our main analyses). Each plot reports interaction coefficients for a different combination of dependent variable (log injury rate, log mean hourly wage) and minimum wage variable (real year-on-year growth, shock indicator variable), corresponding to a column in Table 1, alongside a range of robustness checks, which deviate from our headline specification in the following ways. **3- and 6-digit SOC:** vary the level of granularity at which we classify occupations. **Never-zero:** restricts the sample to occupation-metro panel units with injuries recorded in each year of the sample. **Unweighted:** removes the employment-weighting from our headline specification. **2000-2013:** restricts the sample to the period before every metro in California sees some minimum wage growth in each year. **Topcoded:** topcodes injury rates at 5%. **Truncated:** restricts the sample to cells with injury rates below 5%. **Injury rate** $>$ **1%:** removes the lowest tercile of cells by injury rate from the sample. **Non-imputed**: calculates exposure variable based purely on reported percentiles, without any further imputation of wage distributions. **Disability and external treatment:** use the log (reported) disability rate and log (reported) external treatment rate as injury dependent variables, respectively. **Lagged shock** uses lagged exposure, minimum wage, and interaction independent variables. For each robustness check, the wage and injury regressions are calculated using the same sample. Bars represent 95% confidence intervals. See main text for more details on each robustness check and for discussion of clustering.

Figure A3: Effect of minimum wage shocks with 2 and 4 lags (using the large minimum wage shock variable)



The above coefficient plots report estimates of lead, same-period, and lagged interaction terms from 3 (modified to have 2 or 4 lags), for dependent variables log injury rate and hourly mean wage. The minimum wage shock is defined here as the shock indicator minimum wage variable (taking value 1 if the nominal year-on-year minimum wage increase is greater than or equal to 5%). This Figure is the direct analog of Figure 6, but using the large minimum wage shock indicator rather than real year-on-year minimum wage growth. Standard errors are clustered at the metro-occupation level, bars represent 95% confidence intervals.

A2 Alternative specifications

A2.1 Pseudo Poisson maximum likelihood (PPML) specification

We use a log dependent variable in our main specification to estimate the effect of minimum wage hikes on injury rates. One drawback of this approach is that it forces us to exclude cells with zero reported injuries from our analysis.[34] We deal with this concern in our "never-0" robustness check (Section 5.1.2), in which we restrict the analysis sample to metro-occupation panel units with reported injuries (as well as wage and employment data) in each year. This sample, in which only the intensive margin is relevant, covers 82% of injury claims, and we report similar estimates of the effect of minimum wages on injuries when using this restricted sample.

Here, we report estimates of the effect of the minimum wage on injury rates from unweighted Poisson pseudo maximum likelihood (PPML) specifications. This approach allows us to incorporate zero-injury cells into the analysis, avoids the potential pitfalls of OLS estimation described above, and more generally shows our results are robust to alternative methods of estimation. In particular, this allows us to show that our results are robust to full consideration of potential extensive margin effects. Note that the PPML approach does not require us to specify the distribution of the dependent variable — we need only assume we have correctly specified the conditional mean of the dependent variable, as for OLS estimation

[34] A further potential drawback is that, under heteroskedasticity, OLS estimates of the parameters of log-linearized models are inconsistent (Silva and Tenreyro, 2006). However, this concern only applies if we seek to use the log-linearized model to identify the parameters of $E[y_i|x_i]$ — that is, if we want to interpret our coefficient estimates with respect to the non-logged injury rate. If the conditional expectation function $E[log(y_i)|x_i]$ is a linear function of the regressors, OLS estimates of its parameters are consistent. If it is not linear, OLS provides consistent estimates of the best linear appromixation of this CEF (Silva and Tenreyro, 2006, p. 644).

(Correia et al., 2020).

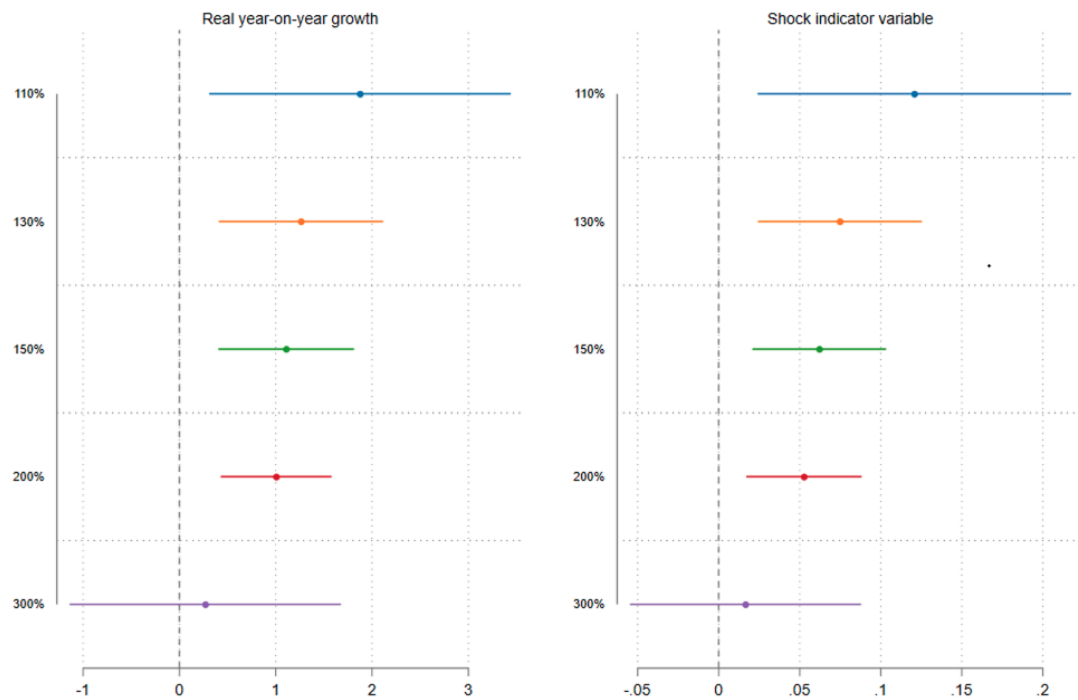
If we apply the exponential function to both sides of equation 1 for our injury rate dependent variable $y_{o,m,t} = log(inj_{o,m,t}/emp_{o,m,t})$, where $inj_{o,m,t}$ is cell injury count and $emp_{o,m,t}$ is cell employment), then rearrange and take expectations, we have:

$$
\begin{aligned}
& E\left[inj_{o,m,t}\right] \\
&= exp\left[\alpha_{o,m} + \alpha_{m,t} + \alpha_{o,t} + \beta\left(min_{m,t} * exposure_{o,m,t}\right) + \gamma min_{m,t} + \delta exposure_{o,m,t} + log(emp_{o,m,t})\right] \\
&= exp\left[\alpha_{o,m} + \alpha_{m,t} + \alpha_{o,t} + \beta\left(min_{m,t} * exposure_{o,m,t}\right) + \gamma min_{m,t} + \delta exposure_{o,m,t}\right] * emp_{o,m,t}
\end{aligned} \tag{4}
$$

We estimate the parameters of this model using the `ppmlhdfe` Stata package from Correia et al. (2020). We constrain the coefficient on $log(emp_{o,m,t})$ to 1; the remaining parameters can therefore be interpreted as parameters of expected injury count faced by an individual in a given local labor market.

Figure A4 below reports estimates of β from equation 4 for different exposure thresholds, mirroring Fig. 4. Across the board, the estimated PPML interaction term coefficients are close in magnitude to their OLS estimate counterparts, including the coefficient for the 130% exposure threshold (our headline specification).

Figure A4: Effect of minimum wage shocks for varying exposure thresholds, PPML specfication



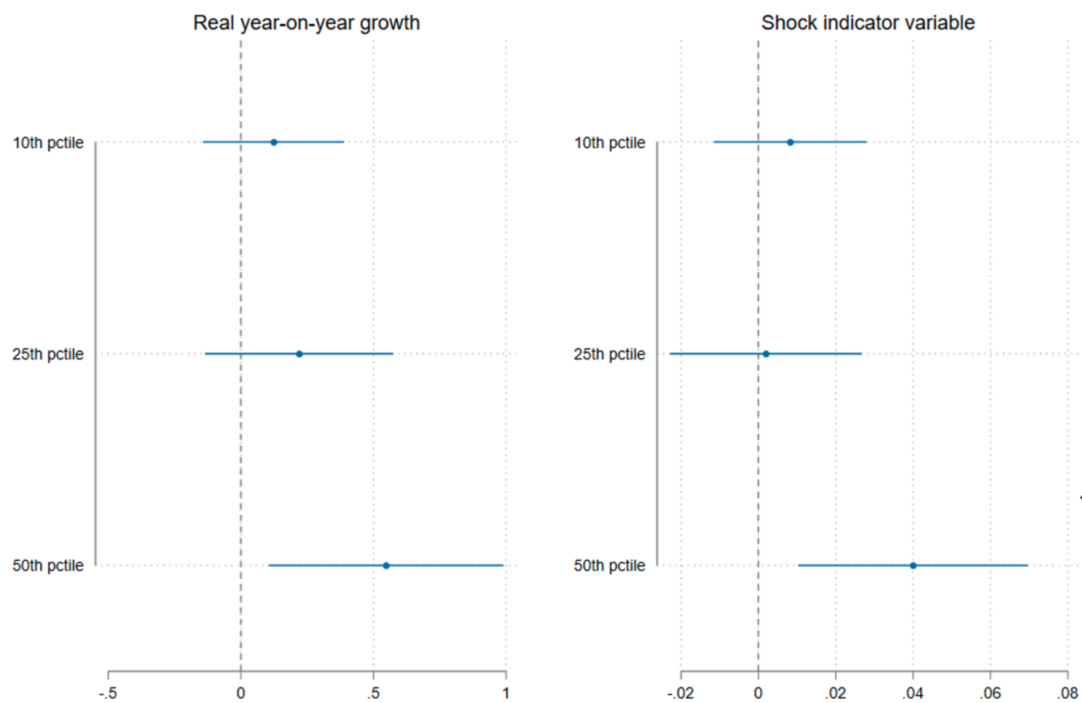
These plots report the estimated interaction coefficients β from equation 4 for different minimum wage variables and exposure thresholds. The dependent variable is the injury count in a given metro-occupation-year cell. The minimum wage variables are (a) real minimum wage growth year-on-year (e.g. 0.05 corresponding to 5% growth) and (b) an indicator taking value 1 if nominal minimum wage growth is higher than 5% (constituting a "shock"). The exposure variable is the share (between 0 and 1) of employment in a given metro-occ-year cell below XXX% of the metro minimum wage, estimated by imputing a lognormal distribution of cell wages from observed percentile data, as detailed above. Occupations are defined at the 5-digit SOC level. Standard errors are clustered at the metro-occupation level. 95% confidence intervals shown.

Similarly, we can modify equation 2 to conduct an analogous falsification test for our PPML specification. We would like our effect to be driven by cells with high exposure to minimum wage shocks (see main text for more details), and so in Figure A5 we report estimates for β^i from the following equation:

$$E\left[inj_{o,m,t}\right] =$$
$$exp\left[\alpha_{o,m} + \alpha_{m,t} + \alpha_{o,t} + \sum_{i\in I}\left\{\beta^i\left(min_{m,t} * D^i_{o,m,t}\right) + \delta^i D^i_{o,m,t}\right\} + \gamma min_{m,t} + log(emp_{o,m,t})\right] \tag{5}$$

where $I = \{10, 25, 50\}$. $D^i_{o,m,t}$ are a set of mutually exclusive exposure indicator variables that take a value of 1 if the ith percentile of a local labor market's wage distribution at time t is the highest out of the reported percentile values in I that fall below 1.3x the local minimum. The excluded category constitutes the majority of observations and consists of local labor markets with wage distributions almost entirely above the 1.3x threshold. As desired, the effect appears to be concentrated in highly-exposed labor markets, and the coefficients for our PPML specification appear to be similar in magnitude to our OLS estimates.

Figure A5: Heterogeneous effects of minimum wage shocks for low-, medium-, and high-exposure cells (PPML specification)



These plots report estimates of interaction coefficients β^i from equation 5, where e.g. "25th pctile" shows estimates of β^{25}, the coefficient on the interaction term between the minimum wage variable and an indicator variable taking value 1 if the 25th percentile is the highest out of the reported 10th, 25th, and 50th percentiles of a cell's wage distribution that falls below 1.3x the local minimum wage. The dependent variable is the injury count in a given metro-occupation-year cell. The minimum wage variables are (a) real minimum wage growth year-on-year (e.g. 0.05 corresponding to 5% growth) and (b) an indicator taking value 1 if nominal minimum wage growth is higher than 5% (constituting a "shock"). Occupations are defined at the 5-digit SOC level. Standard errors are clustered at the metro-occupation level. 95% confidence intervals shown.

A2.2 Growth rate dependent variable specifications

In this section, we (a) show our results are robust to a different identification approach and (b) expand on our analysis of the dynamics of minimum wage shocks (Section 5.2) by estimating the following equation:

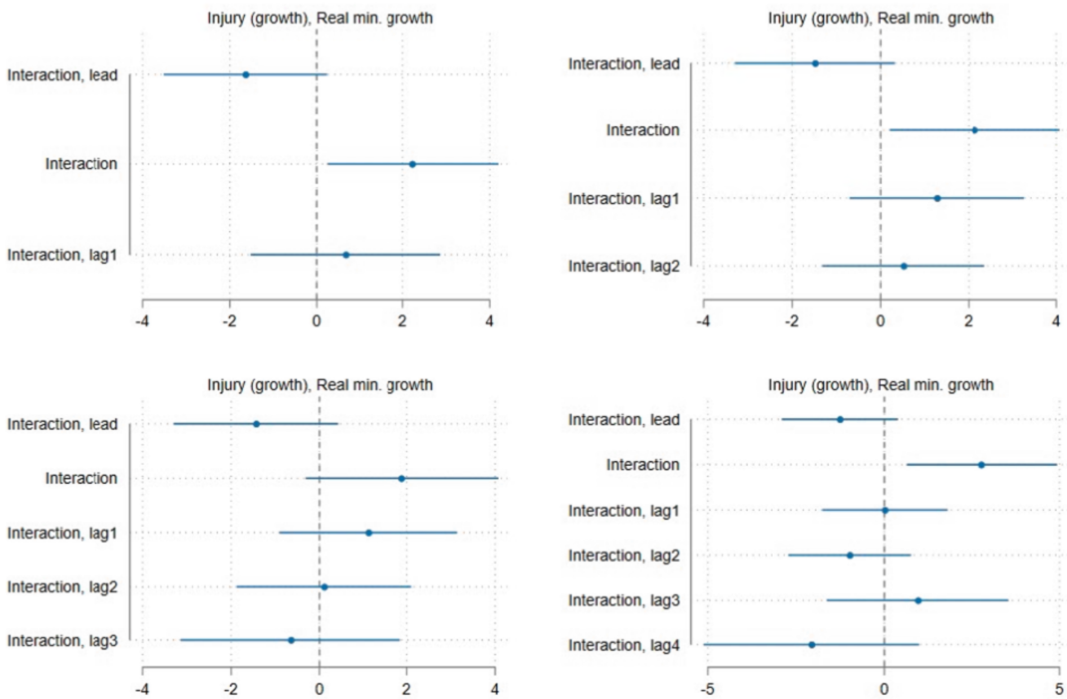
$$\Delta y_{o,m,t} = \alpha_{m,t} + \alpha_{o,t} + \sum_{\tau=-T}^{1} \{\beta_\tau (min_{m,t+\tau} * exp_{o,m,t+\tau}) + \delta_\tau exp_{o,m,t+\tau} + \gamma_\tau min_{m,t+\tau}\} + \epsilon_{o,m,t} \quad (6)$$

This specification is similar to equation 3, but differs in three ways. First, the dependent variable is the year-on-year rate of change in either injury rates or hourly wages within a given local labor market between the current and previous period. For example, $\Delta y_{o,m,t} = 0.02$ corresponds to a 2% year-on-year increase in either the injury rate or the hourly mean wage. Second, we remove the metro-occupation fixed effect; within-unit changes in injury rates/wages as the dependent variable provides an alternative way to control for, and avoid erroneously identifying off, time-invariant differences in injury rates and wages across local labor markets.[35] We continue to use metro-year and occupation-year fixed effects $\alpha_{m,t}$ and $\alpha_{o,t}$ to control flexibly for city- and occupation-level time-varying confounders relating both to "real world" injury rates *and* the rate at which injuries are reported & our success rate in assigning a claim to a given local labor market (our parsing rate), as discussed in Section 4 above. Third, parameter T represents the number of lags in the model ($T = 1$ in equation 3).

The coefficient plots in Figures A6 and A7 show estimated interaction coefficients β_τ for injury rate and wage dependent variables, the real growth minimum wage variable, and values of T from 1 to 4. Across these specifications, we see that the same-period interaction term is consistently associated with larger and more

[35] Using metro-occupation fixed effects in this context amounts to controlling for differences between labor markets in the average year-on-year rate of change in injury rates/wages over the sample period. We believe incorporating this fixed effect is overly restrictive, but we observe largely the same pattern of results when we do so (albeit noisier).

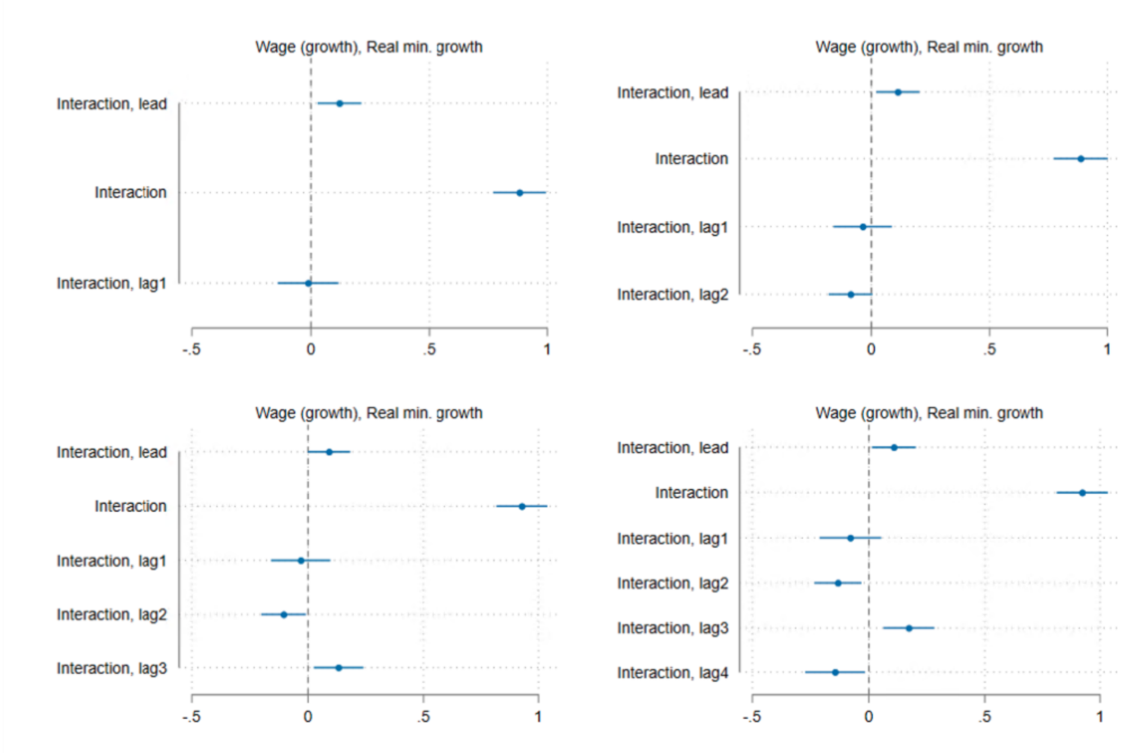
Figure A6: Effect of minimum wage shocks on year-on-year change in injury rates.



The above coefficient plots report estimates of β_τ from equation 6 for the Δ injury rate dependent variable, real year-on-year growth minimum wage variable, and $T = 1, 2, 3, 4$. Standard errors are clustered at the metro-occupation level, bars represent 95% confidence intervals.

significant effects on changes in the injury rate and in wages than the lag and lead interaction terms. Results for the shock indicator minimum wage variable are very similar. We can contrast these results with the levels plots in Section 5.2 of the main paper — for the growth dependent variable specification, there is no significant effect of a lagged shock even with only 1 lag and lead term. Together, these results suggest that minimum wage shocks lead to a same-period increase in the injury rate/wage that then persists insofar as wages remain above their counterfactual level.

Figure A7: Effect of minimum wage shocks on year-on-year change in wages.



The above coefficient plots report estimates of β_τ from equation 6 for the Δ hourly mean wage dependent variable, real year-on-year growth minimum wage variable, and $T = 1, 2, 3, 4$. Standard errors are clustered at the metro-occupation level, bars represent 95% confidence intervals.

A3 Data construction

A3.1 Occupational wage and employment data

We use the BLS Occupational Employment and Wage Statistics (OEWS, formerly Occupational Employment Statistics) in three ways. First, we use OEWS employment figures as denominators when constructing our annual metro-by-occupation injury rate dependent variable. Second, we use (log) metro-occupation hourly mean wage statistics as our wage dependent variable to cross-validate our injury results. Third, we use data on metro-occupation labor market wage percentiles to estimate the share of workers "exposed" to the local minimum in each labor market — that is, the share earning e.g. less than 130% of the local minimum.

A3.1.1 Occupational and geographic crosswalks

To ensure consistency over our sample period, we crosswalk across changing occupational taxonomies and geographical boundaries to create stable metro-occupation panel units for analysis.

Over the course of our sample period, the BLS changes the taxonomy it uses to classify occupations twice, transitioning between 2000, 2010, and 2018 Standard Occupational Classification codes (SOC codes). We use SOC 2010 codes for our analysis, and follow the crosswalk methodology used by Schubert et al. (2024) to construct occupational employment and mean hourly wage figures for pre-2010/post-2018 years using the SOC 2010 taxonomy so that we have consistent metro-occupation panel units across our sample period.

There are also a number of changes in the geographical boundaries of metro areas used by the BLS. Most of these changes are small, occur in 2003, and show up in our data in 2005. Two small metro areas (Harford-Corcoran and El Centro) enter our sample in 2005, having previously been considered non-metropolitan areas. Two metro areas split into two new metros each (Vallejo-Fairfield-Napa into Vallejo-Fairfield and Napa; Fresno into Fresno and Madera). For consistency, we use the new metro areas as our geographic unit of analysis. For 2000-2004 years, we estimate employment figures by allocating employment from the old metro area based on the aggregate population share of the new metros, and use old metro area wage statistics for both new metro areas. Our results are robust to dropping these observations. Sacramento and Yolo merge to become Sacramento-Rosenville-Arden Arcade — again, we use the newer metro as our unit of analysis, and for 2000-2004 we aggregate occupational employment statistics and estimate the hourly mean wage by averaging the hourly mean wage for each older metro, weighted by occupation-specific employment.

Some large metropolitan statistical areas (MSAs) are divided into metropolitan divisions. In our data, the San Francisco MSA is split into San Francisco (San Francisco and San Mateo Counties), Oakland (Alameda and Contra Costa Counties), and San Rafael (Marin County) divisions, while the Los Angeles MSA is split into Los Angeles County and Orange County divisions. We generally use these divisions as our unit of analysis, with one exception. The San Rafael division was established in 2016, and prior to this, Marin County was part of the San Francisco division. For consistency, we treat the pre-2016 San Francisco division as our unit of analysis. We construct 2016–2019 occupation-level statistics for this area by aggregating occupational employment across the new San Francisco and San Rafael divisions and taking the average hourly mean wage for each occupation across both new divisions, using occupation-specific employment as weights.

The BLS stopped publishing division-level occupational wage and employment statistics after 2017, but the California Employment Development Department (EDD) continues to publish these (alongside MSA-level statistics). For 2018 and 2019 we therefore use EDD estimates. For metros where both BLS and EDD data are available (including pre-2018 division-level estimates), the EDD occupational employment estimates for a given year are the same as the BLS estimates published in the previous year, while the EDD wage estimates are constructed by taking the previous year's BLS estimates and rescaling them using the occupation-specific U.S. Department of Labor Employment Cost Index (ECI). To make the EDD division-level estimates consistent with the rest of our data, we impute the scalar used to transform BLS wage figures from MSA-level statistics where both BLS and EDD estimates are available, and then "un-transform" the EDD division-level figures by dividing by this scalar.

A3.1.2 Imputing exposure

Our identification strategy relies on variation in how "exposed" metro-occupation labor markets are to the local minimum wage, as measured by the share of workers in that labor market who earn close to the minimum (less than 1.3x the minimum in our headline specifications).

We estimate this share using the OEWS wage percentile figures (10th, 25th, median, 75th, and 90th) in two ways. First, we construct our **imputed exposure** estimate by estimating a log-normal wage distribution for each metro-occupation labor market. We use the reported log median wage as our mean parameter μ, and then estimate the standard deviation parameter σ by finding the value that minimizes the sum of squared errors between the logged OEWS wage percentile figures and the log wage percentile values implied by a log-normal wage distribution with mean μ and standard deviation σ. That is, our estimate $\hat{\sigma}$ solves:

$$\hat{\sigma} = \min_{\sigma} \sum_{i \in I} (log(pctile\_i) - (\mu + \Phi^{-1}(\frac{i}{100}) * \sigma))^2$$

where $I = \{10, 25, 75, 90\}$, $pctile\_i$ is the OEWS ith wage percentile, μ is the mean parameter/log OEWS median, and $\Phi^{-1}(\cdot)$ is the inverse standard normal CDF. Our imputed share measure for a given threshold (e.g. 1.3x the local minimum wage) is then the share of our imputed log-normal distribution that falls below this threshold.

To ensure our results are not sensitive to our method of imputation, we also construct more conservative **non-imputed exposure** estimates that use only reported wage percentiles — the non-imputed exposure value for a given threshold corresponds to the highest reported percentile that falls below this threshold, measured as a share between 0 and 1.

As part of the crosswalking process, some metro-occupation-year observations are constructed by combining multiple component observations. For example, if two SOC 2000 occupations merge into one SOC 2010 occupation, pre-2010 observations are aggregated to ensure consistent occupational categories across the sample period. We calculate exposure for these constructed observations by calculating the imputed and non-imputed exposure shares for each component occupation, then taking the average across all component observations, weighted by each component's share of employment in the new (merged) occupation. For our non-imputed exposure measures, we then round this value down to .1, .25, .5, .75 or .9 — since our non-imputed exposure measure is conservative and can only take these values, we do not want composite observations to have systematically higher/less conservative exposure estimates.

A3.1.3 Granularity of occupation codes

We use 5-digit SOC codes in our main analyses (and 3-digit SOC codes in robustness checks).[36] We follow similar processes to the above for constructing employment, mean hourly wage, and exposure statistics at the 3- and 5-digit SOC code level from 6-digit SOC code level data — we use total employment and employment-weighted average hourly wage and exposure across component 6-digit SOC codes.

[36] See section A3.2.1 for our rationale.

A3.2 Workers' compensation data

A3.2.1 Assigning occupations to claims

Our claims data contains the following variables that we use to assign occupation to claims:

- **Occupation description:** Unstructured field containing raw text input on occupation.
- **Industry code and description:** Structured fields with industry code (SIC for earlier observations, then NAICS) and description.
- **Class code and description:** Structured fields containing class code and description — these are codes used by workers' compensation and insurance companies to assess the risks associated with different jobs.

We use the NIOSH Industry and Occupation Computer Coding System (NIOCCS) parsing tool to probabilistically assign a 6-digit SOC code to each claim. We clean occupation description strings and crosswalk SIC/older NAICS codes to NAICS 2017 codes. For observations where we are not able to successfully crosswalk the industry code, we provide the industry description; for observations with no industry data, we use class code description as a substitute. Using the cleaned occupation description and industry/class code data, the parser tool probabilistically assigns an occupation to each claim. Using an 80% confidence threshold (as estimated by the parser) we are able to match 74% of claims to a 6-digit SOC.

However, we know the likelihood and accuracy of a match is unlikely to be random across occupations. Occupations with greater variation in titles used in practice, or where industry is an unreliable predictor of occupation, are less likely to be matched by the tool. Our occupation-year and metro-occupation fixed effects should address much of the concern that this would affect our results.

We are also aware that the tool makes certain systematic errors. For example, within the 5-digit "Cooks" SOC code, the 6-digit occupation "Cooks, all other" makes up a very small share of employment compared to e.g. "Cooks, Fast Food" or "Cooks, Restaurant" codes, but the parser often confidently assigns the former when it cannot "decide" on the worker's precise establishment type. Our choice to use 5-digit SOC codes as the main occupational unit of analysis should mitigate much of this parser error while maintaining a significant degree of granularity.

A3.3 Minimum wage data

As detailed in the main text, our minimum wage variation comes from changes in both city-level minimum wages and the state minimum wage. Ideally, we would also be able to exploit city-level variation in injury rates to identify our effect, but we need local occupational employment data to construct our injury rate dependent variable, and the smallest geography for which these statistics are available is the metropolitan statistical area (MSA), or the metropolitan divisions of San Francisco and Los Angeles. We therefore construct average annual minimum wages for each MSA/metropolitan division (henceforth "metro") as the basis for our analysis as follows.

First, we collected data on city-level minimum wages from UC Berkeley Labor Center (2024) and Economic Policy Institute (2024), supplemented by information from local government websites. For some periods in our sample, the state minimum wage and some city-level minimum wages differed for large and small employers (generally with a cut-off of 25 employees). We use a simple average of the large and small employer minimum wages in our analyses, as we cannot cleanly differentiate between these employers in our injury data. Further, we do not incorporate minimum wages for particular groups of workers — e.g. hotel workers — into our analysis.

For each municipality with a local minimum, and for the state minimum wage, we then construct an average minimum wage for each calendar year. We use calendar year as our time unit of analysis as using shorter periods of time significantly increases the noise of our local, fine-grained occupational injury rate estimates and introduces concerns over seasonal injury and reporting patterns.

Finally, we calculate the population-weighted average annual minimum wage for each metro by averaging over each metro's component municipalities and non-incorporated areas, using the average population share of each municipality in its metro over our sample period as weights.

A3.4 Injury types

A3.4.1 Cumulative physical injuries

We use both the nature and cause of injury variables (NOI and COI) to identify injuries relating to cumulative physical strain (as opposed to one-time accidents).[37] We classify a claim as relating to a cumulative motion injury if:

- The NOI is Carpal Tunnel Syndrome.
- The COI is Code 97 ("Cumulative injury or condition caused by continual, repeated motions; strain by excessive use. Includes Carpal Tunnel Syndrome") or Code 94 ("Caused by repeated rubbing or abrading; applies to non-impact cases in which the injury was produced by pressure, vibration or friction between the person and the source of injury. Includes callous, blister").

[37] Narrative descriptions for COI and NOI values can be found at https://www.dir.ca.gov/dwc/WCIS/Cause_Of_Injury.pdf and https://www.dir.ca.gov/dwc/WCIS/Nature_Of_Injury.pdf on the CA Department for Industrial Relations website.

- A combination of NOI "All Other Cumulative Injuries" with one of the following COIs: lifting; pushing or pulling; holding or carrying; twisting; reaching; strain or injury by, NOC [No Other Cause]; rubbed or abraded, NOC; or hand tool/machine in use.
- A combination of COI "Cumulative, NOC" with one of the following NOIs: strain or tear; inflammation; sprain or tear; multiple physical injuries only; fracture; dislocation; multiple injuries including both physical and psychological; or hernia.

Based on both the narrative descriptions of the COI and NOI codes used in making claims, and samples of the narrative descriptions of injury within the data itself, we believe that any claim with this combination of codes is likely to represent an injury arising from cumulative physical strain/stress, and not one-off accidents.

A3.4.2 Accidents

It is harder to identify injuries arising from accidents as many combinations of COI and NOI are consistent with an accident type injury. We identify accidents by:

- Excluding claims where the NOI or COI is strongly suggestive of cumulative injury/illness or that the claim does not involve a clearly work-related accident. Excluded NOIs: angina pectoris; myocardial infarction; vascular; dust diseases e.g. asbestosis, black lung; mental disorder; cancer; AIDS; mental stress; carpal tunnel; all other cumulative injury, NOC; and COVID-19. Excluded COIs: dust, gases, fumes, or vapors; continual noise; repetitive motion (strain or abrasion); mold; cumulative, NOC; terrorism; pandemic; natural disasters; and collapsing materials (slides of earth).
- Including claims where the cause or nature of injury strongly suggest a one-off work-caused accident, given the exclusion of the above claims. Included

NOIs: amputation; burn; concussion; contusion; crushing; dislocation; electric shock; enucleation; foreign body (specific injury); fracture; freezing (specific injury); hearing loss/impairment; hernia; inflammation; laceration; puncture; rupture; severance; sprain; strain; syncope; and asphyxiation. Included COIs: burns or scalds by chemicals, hot objects or substances, fire or flame, steam or hot fluids, welding operation, or electrical current; caught in or between machinery, object handled, or NOC; cuts, punctures, and scrapes; falls (except on ice or snow); strain or injury except by continual noise or repetitive motion; striking against or stepping on any object; struck or injured by any cause except NOC (examples given are all intentional attacks, e.g. kicked, stabbed, bitten), animal or insect, or motor vehicle.

Using the above defition, 86% of claims in our analysis sample are accidents. If we use a more restrictive version that requires both the COI and NOI to be in the inclusion lists detailed in the second bullet above, 61% of claims are accidents. For both definitions, the estimated effect of minimum wages on accident rates are very similar to the effect estimated for the overall injury rate: see Table A10.