Workplace Litigiousness and Labor Market Outcomes: Evidence from a Workers' Compensation Reform*

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Abstract

We study a reform to the workers' compensation system in Argentina that, motivated by a large increase in workplace litigiousness, mandated workers to go through a mediating government medical commission after a workplace accident to determine the degree of disability, whether the injury happened in the workplace, and the corresponding compensation, before additional legal actions could be taken. Leveraging the staggered implementation of the reform across provinces, we find that the reform substantially reduced workplace lawsuits with no effects on reported accidents. Employment increased by more than 5% one year after the reform in highly exposed industries, with no effects on average earnings or the number of active firms.

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1 Introduction

Addressing the consequences of work-related accidents and illnesses is an important policy challenge. According to the Bureau of Labor Statistics, around 2.8 million workplace injuries and illnesses – including more than 5,000 fatal injuries – were reported in the United States in 2019. Since most of these accidents may result in job absenteeism or other work-related restrictions, they can affect earnings for both workers and employers. Hence, in the absence of insurance or regulation, accidents may lead to workplace conflicts to determine who should pay for their costs, which can result in costly lawsuits between both parties.

Workers' compensation (WC) schemes – mandated insurance programs that pay for health expenses and a wage replacement for injured workers – can help to solve these conflicts by establishing guidelines on how to proceed after workplace accidents. Importantly, reducing work-related litigation costs is an explicit objective of WC schemes (Fishback and Kantor, 1998, 2007), in part due to the potential efficiency gains derived from reduced litigiousness. If WC schemes reduce the litigation costs of workplace accidents, the rents of labor market matches increase, especially in industries where workplace accidents are commonplace. Larger rents may encourage employers to post more vacancies and attract more applicants, eventually affecting employment. Likewise, reduced litigiousness can affect wages, depending on how the additional rents are split between workers and employers. To the best of our knowledge, however, there is no evidence of how effective WC schemes are for reducing litigation costs in the workplace. Empirical evidence on the effects of reducing workplace lawsuits on labor market outcomes is also missing. The answers to both questions are important inputs for thinking about the optimal design of WC schemes and, more generally, the effects that litigiousness can have on the performance of the labor market.

To contribute to this discussion, this paper studies a WC reform in Argentina that sought to reduce workplace lawsuits between workers and employers. Argentina established a WC system similar to the United States system in 1996, where employers were mandated to provide no-fault insurance for workers. In exchange, workers waived their right to sue employers and insurance companies. In the mid-2000s, several Supreme Court rulings opened up room for suing employers and insurance companies (Galiani, 2017), eroding the efficacy of the system's litigation-containment mechanism. As a result, litigiousness escalated, generating large costs for both employers and workers. To address this problem, the system was reformed in February 2017. The new law required injured workers to go through a local government medical commission as a mandatory step before any further action can be taken. This commission determines the degree of disability, whether the injury was related to the worker's occupation, and the corresponding compensation according to the Law. The decision could be appealed to a higher-order commission, and eventually to labor courts, although this possibility was deemed unlikely: the reform tried to appeal to employees by providing quicker compensation and to employers by reducing the large and unpredictable costs from litigiousness. Importantly, the reform did not change the formulas used to calculate WC benefits, so the expected effect was to reinstate the litigiousness-mitigating mechanism of the WC system, thus generating efficiency increases through reduced litigation without affecting the effective insurance of workers.

We leverage the staggered introduction of the law across provinces to estimate the effect of the reform using an event study design. The new system was sanctioned at the federal level in February 2017, but each provincial legislature had to sanction its own law to adhere to the federal law.[1] Upon approval of the law, each provincial government had to set up the medical commissions, which then had to be approved by the federal agency in charge of the WC system. Only after the approval of the medical commissions, the law entered into effect at the province level. Provinces were heterogeneous in how they carried out these steps, leading to a staggered adoption of the policy. We cover the period January 2015 to July 2019, when the law entered into effect in 5 out of 24 provinces.[2] We study the effects of the reform on workplace litigiousness and labor market outcomes using quarterly province- and sector-by-province-level aggregates built from administrative records. For each unit of analysis, we observe the number of accidents, lawsuits, and amounts claimed by workers in lawsuits, in addition to equilibrium outcomes of the formal labor market such as employment counts, average wages, and the number of firms.

We find that the reform was very effective at reducing workplace litigiousness and its associated costs, with no effect on reported accidents. The number of lawsuits fell by about 0.7 log-points after the reform. The costs of litigiousness –measured as the amount of money claimed in lawsuits as a share of the wage bill– dropped by about 0.4 percentage points after the reform. The effect is twice as large in sectors most affected by litigiousness (measured as the sectors with larger shares of employers that had lawsuits before the reform), namely construction, mining, and manufacturing. We find no significant effect on the number of accidents reported, suggesting that the drop in litigiousness was not due to lower accident reporting or higher safety standards in the workplace. These results suggest that the reform increased the efficiency of the labor market by reducing the costs of managing workplace accidents.

We then explore the effects of the reform on the labor market. Province-level employment increased by about 1.8% after the reform, although the effect is not precisely estimated. The number of active firms was not affected by the reform, suggesting that the employment effect was driven by existing firms increasing their employment levels. Average wages were also unaffected by the reform, suggesting that employed workers did not capture the gains of the smaller litigation costs. The employment effects become larger and more precise when zooming at the sector-by-province level: sector-level employment experienced a significant increase of 2.8% one year after the reform. The total effect is almost exclusively driven by the sectors most affected by litigiousness, whose employment level one year after the reform was

[1] The exception was the City of Buenos Aires where the law automatically entered into effect in February 2017.

[2] We omit the months after July 2019 because of an unanticipated result in the primary election of August 2019 that led to a stock market crash and a substantial overnight depreciation of the currency. These events, in turn, led to significant changes in economic institutions, such as reinstating capital controls and taxes on agricultural exports. This negative economic shock had a differential effect across provinces and sectors, potentially affecting our identification strategy.

more than 5% larger. Wage effects continue to be noisy and negligible when using province-by-sector-level data. We find no effect on the insurance fees that employers pay, suggesting that the effects do not seem to be driven by the reform somehow reducing the costs of the WC system by itself to employers or the associated benefits to workers.

We end the paper by proposing a simple model of the labor market to rationalize the results. We extend the basic matching model of Pissarides (1985, 2000) to allow for workplace accidents. The model can rationalize positive employment effects when litigation costs decrease through an increase in posted vacancies. The wage effects are ambiguous since the reduction in the expected costs of litigation increases the rents of labor market matches, eventually pushing wages up, but also induces a compensating differential force that pushes wages down. The relative bargaining power between workers and employers mediates how these two forces balance in equilibrium.

This paper contributes to the literature on WC by providing, to the best of our knowledge, the first analysis of the effects of the policy on workplace litigiousness and aggregate labor market outcomes. The literature has mostly focused on moral hazard questions by estimating worker-level behavioral responses on accidents, claims, or private health expenditures (Krueger, 1990; Dionne and St-Michel, 1991; Meyer et al., 1995; Kantor and Fishback, 1996; Dillender, 2015; Hansen et al., 2017; Powell and Seabury, 2018; Huet-Vaughn and Benzarti, 2020; Cabral and Dillender, 2021). Cabral et al. (2021) discuss the role of WC schemes for dealing with other market failures such as adverse selection and market power in private insurance markets and externalities on workers' health. With the exception of the early evidence on wage incidence provided by Fishback and Kantor (1995), there is no evidence of the labor market effects of WC schemes. We show that the design of WC schemes can significantly reduce labor market litigation, which can in turn positively affect aggregate employment. The lack of effects on earnings also makes explicit the distributional impact of the policy. The fact that external government commissions can effectively reduce workplace lawsuits could eventually inform policy-making in other contexts where workplace conflicts could lead to costly litigation as, for example, workplace discrimination (Darity and Mason, 1998; Bohren et al., 2022; Kline et al., 2022) or sexual harassment (Folke and Rickne, 2022).

More generally, the labor market effects of different labor market institutions have been extensively studied. A large literature studies the labor market effects of unemployment insurance policies, both at the individual (Schmieder et al., 2016; Nekoei and Weber, 2017; Lindner and Reizer, 2020) and aggregate (Hagedorn et al., 2017; Marinescu, 2017; Johnston and Mas, 2018; Chodorow-Reich et al., 2019; Boone et al., 2021) levels. Similar analyses exist regarding health insurance (Gruber, 1994; Baicker and Chandra, 2006; Baicker et al., 2014; Kucko et al., 2018; Duggan et al., 2019; Fang et al., 2020; Heim et al., 2021), family policies (Rossin-Slater et al., 2013; Schönberg and Ludsteck, 2014; Givord and Marbot, 2015; Dahl et al., 2016; Olivetti and Petrongolo, 2017; Tamm, 2019), the EITC (Eissa and Hoynes, 2006; Kleven, 2020), the minimum wage (Manning, 2021), and universal basic income policies (Hoynes and Rothstein, 2019). We add to this literature by providing evidence on the labor market effects of WC policies.

This paper also contributes to the literature on compensating differentials that emphasizes the importance of non-wage job amenities for workers' choices and outcomes (Bonhomme and Jolivet, 2009; Mas and Pallais, 2017; Lavetti and Schmutte, 2018a,b; Maestas et al., 2018; Sorkin, 2018; Lavetti, 2020; Taber and Vejlin, 2020; Anelli and Koenig, 2021; Jäger et al., 2021; Le Barbanchon et al., 2021; Lindenlaub and Postel-Vinay, 2021; Marinescu et al., 2021; Sockin, 2021; Lamadon et al., 2022; Roussille and Scuderi, 2022). One particular (dis)amenity that enters the bundle of job characteristics is the likelihood of workplace accidents. The evidence provided in this paper can be thought of as measuring the effect of reducing the cost of this disamenity on labor market outcomes. While the proposed model suggests that employers may use this rationale to push wages down, the increase in labor market rents pushes the wage in the opposite direction, to the extent that workers are able to capture some of these rents. Then, our analysis contributes to the understanding of compensating differential wage effects in contexts where bargaining matters and changes in amenities also affect the value of the job for the employer.

Finally, this paper relates to the literature on regulatory versus litigation-based approaches to resolving conflicts (Djankov et al., 2003; Glaeser and Shleifer, 2003; Medema, 2020) by providing evidence of efficiency-enhancing regulation in the presence of large litigation costs, applied to the case of WC policies.

The rest of the paper is structured as follows. Section 2 provides an overview of WC schemes and the institutional setting and reform studied in this paper. Section 3 describes the data. Section 4 describes the empirical strategy and presents the main empirical results. Section 5 presents a simple theoretical framework of labor markets with litigiousness and WC. Finally, Section 6 concludes.

2 Workers' compensation schemes and institutional setting

Defining WC WC schemes provide some type of insurance for workers who experience accidents or illnesses related to their job. The insurance usually covers the health expenses related to the treatment and provides wage replacement for the duration of the injury, and in some cases they also provide compensation to the families of workers who have fatal injuries. Also, these systems typically incorporate mechanisms to limit the need to resort to lawsuits (or forbid them altogether) with the intention of avoiding large and unpredictable costs for both workers and employers (Fishback and Kantor, 2007). Some countries, such as many in Western Europe, implement a "social insurance" system, where the benefits are delivered through a government program and funded through payroll taxes. Other countries, like the United States and Argentina, use an "employer liability" system, where employers are mandated to provide no-fault insurance for their employees and workers cannot sue their employers for negligence.

WC in Argentina before the reform Argentina established its first WC system in 1915. This system was changed multiple times and frequently experienced issues with litigiousness (Galiani, 2017). In 1995, a new law was passed, which established a WC scheme similar to the United States system, whose main features are valid until today. Under this new law, employers are mandated to provide no-fault insurance for injured workers.[3] This is typically purchased from insurance companies, called Work Hazards Insurers (*Aseguradoras de Riesgos del Trabajo*), while a few employers choose to self-insure.[4] When insured, workers waive their right to sue employers and insurance companies.

The system achieved the goal of limiting litigiousness for about a decade. However, between 2004 and 2007, several Supreme Court rulings gradually allowed workers to sue both employers and insurance companies (Galiani, 2017).[5] This resulted in a massive escalation of the number of lawsuits, imposing a large burden on the WC system by increasing bureaucracy and waiting times, and leading to concerns about excessive and unpredictable costs due to litigiousness. Panel (a) of Figure 1 shows the number of newly reported lawsuits for each quarter since the system started reporting in January 2010 until the second quarter of 2017. The number of new quarterly lawsuits more than tripled between 2010 and 2017. Panel (b) of Figure 1 shows the share of firms in each sector that had lawsuits during 2016. The incidence of litigiousness was substantial: in the most affected sectors –construction, mining, and manufacturing– almost one in five firms faced at least one lawsuit in 2016.

The reform In February 2017, Law 27,348 was introduced. The new law established a mandatory first step after work-related accidents: injured workers' claims have to be processed by a Jurisdictional Medical Commission that determines the degree of disability, whether the injury is related to the worker's occupation, and the corresponding compensation as determined by the law passed in 1995, before any further legal action can be taken. This decision could be appealed by any party involved to a higher-level commission and, eventually, to labor courts, although few cases end up doing so. The intention behind the reform was to appeal to workers by streamlining the process and ensuring a quick compensation, and to employers and insurance companies by reducing the large and unpredictable costs due to litigiousness. Importantly, the law did not change the formulas used for calculating workers' benefits due to workplace injuries, nor the injuries covered by the system, both of which are set at the Federal level. Thus, adhering to the law only implied a change in whether workers are required to go through the government medical commission or not, and not in how workers' benefits are calculated.

[3] The formula for benefit calculation consists of a replacement rate over past earnings with a floor and a ceiling that varies with age and the degree of disability caused by the injury.

[4] Insurance companies charge employers an insurance fee in the form of a fraction of the wage bill, which is negotiated between the insurance company and the firm, and varies depending on the type of industry.

[5] In September 2004, the *Astudillo* and *Aquino* rulings established that provincial labor courts (instead of federal courts) were responsible for handling workplace accidents and established that employers could be liable for workplace accidents. The *Llosco* ruling of June of 2007 confirmed employees' possibility of civil action against employers and insurance companies, while still receiving the wage replacement payments from insurance companies.

The law was passed at the national level, but provinces were free to adhere to it by sanctioning their own adherence laws at the provincial level.[6] Multiple provinces adhered in the years that followed. Upon adherence to the law, the provincial government has to set up its medical commissions, which then have to be approved by the Superintendence of Work Hazards. Once this approval takes place, the law enters into effect in that province, which happened in 5 out of 24 provinces during the sample period we cover (January 2015 to July 2019).[7] These 5 provinces are larger than the average province in the country, but they span the whole pre-reform lawsuit distribution.

3 Data

To estimate the effects of the reform, we combine administrative data from two sources. The first source informs about labor market outcomes, while the second source contains information about the WC system. For the labor market data, we collect administrative records from the Ministry of Employment and Social Security (*Ministerio de Trabajo, Empleo, y Seguridad Social*). These records are constructed from the payroll tax forms that firms have to file monthly to submit their payroll taxes to the Social Security Agency. We have access to quarterly province-level and 1-digit sector-by-province-level aggregates of the number of workers, number of active firms, and average monthly wages.[8]

We combine the labor market data with information from the government agency in charge of the WC system, the Superintendence of Work Hazards (*Superintendencia de Riesgos del Trabajo*). These records are constructed from insurance companies' reports that are submitted each month to the Superintendence of Work Hazards. The Superintendence then constructs comprehensive information on the number of accidents reported, the number of lawsuits started, and the amounts claimed in lawsuits in each sector-by-province cell. We have access to quarterly province-level and 1-digit sector-by-province level aggregates of the number of lawsuits, the number of accidents, and the average amount claimed in lawsuits as a share of total labor costs.[9]

Our final dataset consists of a quarterly panel of employment counts, firm counts, average monthly wages, number of lawsuits, number of accidents, and amounts claimed in lawsuits as a share of labor costs at the province and sector-by-province-level. The sample period is January 2015 (two years before the first province adopts the law) through July 2019. Summary statistics are shown in Table 1. Panel

[6] The one exception to this was the City of Buenos Aires, where the law went into effect immediately after it was passed at the National level.

[7] The first instance of law adoption is from the City of Buenos Aires in February of 2017. This was followed by Córdoba in September 2017, Mendoza in February 2018, Buenos Aires in October 2018, and Río Negro in December 2018.

[8] Since we do not observe hours, we indistinctly refer to earnings and monthly wages.

[9] The universe of workers in the Superintendence of Work Hazards data is not exactly the same as the one in the Ministry of Employment and Social Security data, since the former also includes public sector workers and autonomous workers who choose to self-insure. Since we are interested in the effects on private-sector employment, we conduct the labor market analysis using the Ministry of Employment and Social Security data.

A presents variables aggregated at the province-level and Panel B presents variables aggregated at the sector-by-province-level. There are, on average, 5,767 accidents and 1,051 new lawsuits reported each quarter in each province, with substantial heterogeneity across provinces. On average, the amount claimed in lawsuits represents 0.4% of total labor costs. The degree of heterogeneity increases when zooming at the sector-by-province level, which is consistent with the heterogeneity documented in Figure 1.

4 Results

Empirical strategy We leverage the staggered introduction of the law across provinces to estimate the effect of the reform using an event study design. We present results using both province-level and sector-by-province-level data. For the province-level event studies, we estimate the following equation by Ordinary Least Squares (OLS):

$$Y_{pt} = \alpha_p + \mu_{r(p)t} + \sum_{k \neq -1} \beta_k \cdot \mathbb{1}\{t = e_p + k\} \cdot \text{Treated}_p + \varepsilon_{pt}, \tag{1}$$

where Y_{pt} is an outcome of interest in province p at quarter t, α_p is a province fixed effect, $\mu_{r(p)t}$ is a region-by-quarter fixed effect with $r(p)$ the region of province p, Treated_p is a dummy variable equal to 1 if province p is ever treated, $1\{t = e_p + k\}$ is a dummy variable equal to 1 if province p was treated k quarters ago at quarter t with e_p the calendar quarter in which the province is treated, and ε_{pt} is the error term. The coefficients of interest are $\{\beta_k\}$, which measure the differences in trends between treated and untreated provinces within a window of quarters around the adoption of the law. We normalize $\beta_{-1} = 0$ and cluster the standard errors at the province level. We fully saturate the regression including all time and treatment interactions and report the coefficients for a balanced window of 8 quarters prior and 5 quarters after the reform. For the sector-by-province-level analysis, we estimate the same event-study equation, but include sector-by-province fixed effects (instead of province-level fixed effects). Note that province-level and sector-by-province-level estimates need not be exactly the same, since province level results would arise from a sector-level regression weighing observations by their share of total province-level employment.[10]

We also estimate difference-in-differences regressions that summarize the post-reform effect:

$$Y_{pt} = \alpha_p + \mu_{r(p)t} + \beta \cdot \text{Treated}_p \cdot \text{Post}_{pt} + \varepsilon_{pt}, \tag{2}$$

where $\text{Post}_{pt} = \mathbb{1}\{t \geq e_p\}$ is a dummy variable that takes value 1 if province p was already treated at quarter t, and all other variables are defined as in equation (1). In this regression, β summarizes the

[10] In addition, two sectors are dropped for the sector-by-province-level analysis because multiple provinces do not have any private-sector employees in those sectors (fishing and utilities), in addition to a minimal number of employees who are not classified at the sector level but are counted for province-level aggregates.

aggregate post-reform treatment effect. While we continue to cluster the standard errors at the province level, given the small number of provinces, we also report Wild Bootstrap p-values (Cameron et al., 2008) for the main coefficient of interest. Noting that the length of the post-period differs by treated province, tables report the average effect on the 5 quarters after the reform.

To preserve the sample size across specifications, we use the inverse hyperbolic sine (IHS) transformation to study effects on variables with occasional zeros, such as lawsuits, accidents, employment counts, or firms. This concern is mostly absent but sometimes manifests in the sector-by-province analysis of lawsuits because some sectors are small enough to occasionally have zero lawsuits in a given quarter in some provinces.[11] Recent research has indicated that the use of the inverse hyperbolic sine is problematic since it is scale-dependent, possibly compromising the percentage interpretation of the estimate. Chen and Roth (2023) show that this problem is rooted in the existence of extensive margin responses whose percentage change is not well-defined. In Appendix B, we show that the policy did not induce extensive margin responses. Consequently, we show that results are essentially identical in regressions that use the natural logarithm transformation and treat zeros as missing values.

Province-level results Province-level estimates inform about the aggregate effects of the policy. Figure 2 plots the $\{\beta_k\}$ coefficients from equation (1) with their corresponding confidence intervals. Panel A of Table 2 presents the difference-in-differences estimates of the β coefficient from equation (2), with the corresponding clustered standard errors and the Wild Bootstrap p-value.

Panel (a) uses the IHS transformation of the number of lawsuits as a dependent variable. Lawsuits show no differential trends between treated and control provinces before the reform, however, after the adoption of the law, a sharp decrease in lawsuits is documented. The effect is quantitatively important: assuming a log interpretation of the IHS transformation (see robustness checks below), Table 2 shows a significant decrease of 77% in the number of lawsuits as a consequence of the reform.

One concern with the previous result is that, because of the reform, workers may have fewer incentives to report accidents, thus being the decrease in lawsuits a mechanical consequence of a worsening of the system's performance. Panel (b) shows that, while there is a mild negative trend, there is no significant drop in reported accidents after the implementation of the law. We interpret this result as the reform effectively decreasing the scope for litigiousness without affecting the functioning of the WC system.

Panel (c) shows that the total amount of money claimed in lawsuits as a percentage of the total labor costs also falls significantly after the adoption of the law, suggesting that the decrease in lawsuits generates monetary gains for firms. This result suggests that the marginal lawsuits the reform prevented are of quantitative importance in terms of costs. This result should be interpreted as a lower bound on the reduction of the costs associated with lawsuits, because our cost measure does not account for the costs

[11] For labor market variables at the sector-by-province level, zeroes are present in less than 1% of observations.

related to, for example, lawyers and human resources offices. Table 2 shows a decrease of 0.4 percentage points in amounts claimed as a share of labor costs, which is of similar magnitude to the unconditional mean in the complete estimation sample.

Regarding the labor market effects, Panel (d) reports coefficients on the IHS transformation of the total number of workers. We find a positive, albeit noisy, increase in the total number of workers. Table 2 reports an imprecisely estimated 1.8% increase in aggregate employment, with a corresponding p-value of 0.14. Panels (e) and (f), together with Table 2, show that there is no effect on the total number of active firms and on the average monthly wage at the province level.

Sector-by-province-level results The province-level results inform about the aggregate effects of the policy. We complement these results with sector-by-province level regressions to both increase the statistical power and estimate the sector-level impact of the reform.

Figure B.I of Appendix B plots the $\{\beta_k\}$ coefficients from equation (1) with their corresponding confidence intervals for the lawsuits, accidents, and amount claimed outcomes. Results essentially mirror the province-level results. This is confirmed in Panel B of Table 2: when zooming at the sector-by-province level, results also indicate a drop in lawsuits and their corresponding costs after the implementation of the reform with no corresponding change in reported accidents.

Figure 3 plots the $\{\beta_k\}$ coefficients from equation (1) with their corresponding confidence intervals for the labor market outcomes. The employment effect is larger and more precisely estimated when using the sector-by-province data. Panel B of Table 2 shows that the estimated employment effect is 2.8% and is significant at the 5% level. At the sector-by-province level, the effects on average monthly wages and the number of firms are also negligible.

One advantage of using sector-by-province level data is that we can estimate heterogeneous effects by exposure to the reform, specifically by classifying sectors based on the degree of litigiousness they experienced in 2016. We take the share of employers within the sector that had lawsuits during 2016 (see Panel (b) of Figure 1) and classify construction, mining, and manufacturing as sectors with "high litigiousness" and estimate separate event studies for this group and the residual sectors. Panel (b) of Figure 2 shows the results for the employment count, indicating that the increase in employment is driven by the high-litigiousness sectors. Panel C of Table 1 shows that the reform increased employment by about 5% in these sectors. Panel (d) shows that high-litigiousness sectors seem to show a modest wage increase, although the estimated effect is small, imprecisely estimated, and arises visually due to the normalization at event time -1 (if the normalization is made at time -2, post-event coefficients are not statistically distinguishable from 0). Finally, these results confirm the null effect on the number of firms.[12]

[12]Panels (b), (d), and (f) of Figure B.I of Appendix B, and Panel C of Table 2 show the heterogeneities for the lawsuits

Ruling out other changes in the WC system Our interpretation of the results is that the reform induced a reduction in costly lawsuits, thus generating efficiency gains that materialized in an increase in employment. An alternative explanation could be that the reform introduced other changes that reduced costs for employers and therefore the decrease in lawsuits and the increase in employment are just a consequence of a smaller scale of the WC system. For instance, it could be the case that having to go through a medical commission ends up reducing WC payouts to workers and that these lower payouts to workers translate into a lower cost of hiring for employers, for example, via lower insurance fees. A skeptical reader could even conjecture that the reform de facto eradicated the use of WC, so employment effects are rationalized by the lack of effective regulation.

We first note that, as discussed in Section 2, the law maintained the coverage and benefits calculation intact. Then, this alternative explanation contradicts the institutional design. Second, the null effect on reported accidents is also consistent with workers not changing their reporting behavior, which suggests that the WC system kept working as intended. Finally, we assess this alternative explanation by analyzing the effects of the reform on the insurance fees that employers pay. If the de facto functioning of the WC system was affected by the reform, we should expect a decrease in the insurance fees firms pay. Figure B.II of Appendix B and Table 2 show a precise zero effect on insurance fees, suggesting that the labor market effects are not driven by some alternative reduction in the insurance costs for employers.

Robustness checks A first concern is that the IHS transformation is not providing interpretable estimates. As documented by Chen and Roth (2023), this can be the case when the treatment induces extensive margin responses. We show in Appendix B that the reform had null effects at the extensive margin and, therefore, that the results are equivalent when using a natural logarithm transformation that drops the zero outcomes, thus ruling out concerns of the interpretability of our main results.

A second concern is that staggered event studies estimated using two-way fixed effects models may be biased when treatment effects are heterogeneous (de Chaisemartin and D'Haultfœuille, 2022; Roth et al., 2022). This potential bias comes from "forbidden comparisons" between treated units, that is, when already treated units form part of the control group of units treated in later periods. We perform two exercises that suggest that this source of bias is negligible in our setting. First, we implement the decomposition suggested by Goodman-Bacon (2021) that shows the relative importance that different pairwise comparisons play when computing the aggregate estimate. As shown in Appendix A, all regressions are almost exclusively estimated using comparisons between treated and never-treated units. This is not surprising given the small number of treated provinces relative to the never-treated ones. These results suggest that the scope for negative weighting is negligible. To further address this concern, we estimate stacked event study specifications (Cengiz et al., 2019, 2022; Gardner, 2021; Baker et al., 2022)

and accidents outcomes. The main difference between sectors relates to the amount claimed as a share of labor costs, which is twice as large for the more exposed sectors.

where we force the event-specific control groups to be exclusively composed of never-treated provinces. As we show in Appendix D, results remain virtually unchanged under this alternative specification.

Another concern is the small number of treated provinces. This could be a concern if the estimated difference-in-differences effects capture some differential trend for some treated provinces and not the inherent effect of the reform. Alternatively, the main results could be driven by specific provinces which could compromise the external validity of the result. To assess whether this concern bears some relevance to our results, we replicate our main results with several "leave-one-out" estimations in which we sequentially drop one of the treated provinces and compare these results to our baseline estimates using all of the provinces. The results from these exercises can be found in Appendix C. All of our results remain very similar to our baseline estimates in all of the leave-one-out estimations, suggesting that results are not driven by some differential trend of a particular treated province.

Finally, in some of our results, the standard errors are large enough to cast doubt about the lack of differential pre-treatment trends between treated and control provinces (see, for example, Panel (b) of Figure 2). To assess whether this concern bears some relevance to our results, we replicate our main results after adjusting all of our event study coefficients by a linear pre-treatment trend between event times -8 and -1. The results from these exercises can be found in Appendix E. In general, all of our results remain very similar to (and, in some cases, more significant than) our baseline estimates throughout our adjusted estimates, suggesting that our results are robust to the adjustment of linear trends prior to the adoption of the law.

5 Model

To rationalize the estimated employment and wage effects after a decrease in litigation costs, this appendix extends the standard Pissarides (1985, 2000) matching model to incorporate workplace accidents. In the model, reduced litigation costs generate employment increases. Wage effects are ambiguous, with the relative bargaining power determining the balance of two competing forces: compensating differentials and larger labor market rents.

Preliminaries Labor supply L is exogenous. Let u be the unemployment rate and v the vacancies per worker rate, both endogenous. The number of matches is given by the matching function $M = M(uL, vL)$, which is assumed to be increasing and concave and to have constant returns to scale (CRS). Define labor market tightness as $\theta = v/u$. CRS in M implies that the job filling rate, $M(uL, vL)/vL$, is given by $q(\theta)$, with $q_\theta := \partial q(\theta)/\partial\theta < 0$. Likewise, the job finding rate, $M(uL, vL)/uL$, is given by $p(\theta) = \theta q(\theta)$, with $p_\theta := \partial p(\theta)/\partial\theta > 0$. The exogenous job destruction rate is given by δ. The unemployment law of motion is given by $\dot{u} = \delta(1-u) - \theta q(\theta)u$. In steady state, $\dot{u} = 0$, which implies that:

$$u = \frac{\delta}{\delta + \theta q(\theta)}. \tag{3}$$

Equation (3) is called the Beveridge curve and establishes an equilibrium relationship between u and θ.

Value functions Firms are atomistic and decide whether to post a vacancy at cost c. If the vacancy is filled, it produces ϕ and pays wage w. Filled vacancies have a probability a of having a workplace accident. When accidents occur they induce a cost for the firm, k_F. Let V and J be the values for the firm of a vacant job and a filled vacancy, respectively. Then, if r is the discount rate, the value functions can be written as:

$$rV = -c + q(\theta)(J - V), \tag{4}$$

$$rJ = \phi - w - ak_F + \delta(V - J). \tag{5}$$

Free entry implies $V = 0$, so equation (4) is reduced to $J = c/q(\theta)$. Replacing in equation (5) yields:

$$\phi - w - ak_F = \frac{(r+\delta)c}{q(\theta)}, \tag{6}$$

which is called the job-creation curve.

Define by b the workers' reservation value and by k_W the cost of a workplace accident for the worker. Let U and W be the values for the worker of being unemployed and employed, respectively. Then:

$$rU = b + \theta q(\theta)(W - U), \tag{7}$$

$$rW = w - ak_W + \delta(U - W), \tag{8}$$

We assume that $k_F + k_W > 0$, that is, the process of a workplace injury is not a zero-sum game where employers just compensate workers. The potential presence of, for example, costly lawsuits implies that there is a deadweight loss associated with accidents.

Wage setting There is Nash bargaining over the total match surplus, with β the workers' bargaining power, so $w = \arg\max_w (W-U)^\beta (J-V)^{1-\beta}$. Solving the problem yields:

$$w = (1-\beta)(b + ak_W) + \beta(\phi + c\theta - ak_F). \tag{9}$$

Equation (9) coincides with the standard solution of the basic DMP model when $k_F = k_W = 0$. The fact that, in partial equilibrium, w depends positively on k_W, suggests that compensating differentials play a role in wage determination.

Equilibrium We interpret a reform that reduces workplace litigation costs as a reduction in k_F and, possibly, k_W. To explore the equilibrium effects of such a reform, we replace equation (9) in equation (6) and differentiate, which yields:

$$\frac{d\theta}{dk_F} = \frac{q(\theta)(1-\beta)a\left(\frac{dk_W}{dk_F}+1\right)}{q_\theta(\phi - w - ak_F) - q(\theta)\beta c}, \tag{10}$$

which is unambiguously negative provided that $J \geq 0$ and $dk_W/dk_F \geq 0$. The former is a standard assumption that implies that there is value for employers to create vacancies, and the latter implies that the hypothetical reform that lowers the costs of accidents for employers do so for workers as well. That assumption holds in the reform we study since the reduction in lawsuits implies lower costs for both workers and employers. Equation (10) implies that higher (lower) costs for firms of workplace injuries decrease (increase) the vacancies to applicants ratio. Together with equation (3), this implies that higher (lower) costs of workplace injuries induce higher (lower) equilibrium unemployment rates. Then, this simple model rationalizes how a reform that reduces k_W and k_F can induce positive employment effects.

A number of things are worth discussing about equation (10). First, the magnitude of $d\theta/dk_F$ depends positively on a: the employment effect is larger when workplace accidents are more prevalent. This is consistent with the heterogeneous results presented in Section 4. Second, the magnitude of $d\theta/dk_F$ depends negatively on β: the employment effect is larger when workers' bargaining power is low. This is due to the fact that when β is large, employers anticipate that workers capture a large share of the increase in rents. Therefore, the incentives for creating more vacancies are attenuated. Third, the magnitude of $d\theta/dk_F$ depends positively on dk_W/dk_F, that is, the employment effect is larger when workers' costs are also reduced with the reform. This comes from the fact that the value workers put on the reform induces a compensating differential force that employers can use to push wages down and, therefore, capture more rents from the labor market matches, thus increasing the incentives of posting more vacancies.

Using the fact that $p(\theta) = \theta q(\theta)$, we can replace equation (6) in equation (9) and then differentiate to explore the equilibrium change in w. This yields the following expression:

$$\frac{dw}{dk_F} = \frac{\left[(r+\delta)(1-\beta)a\frac{dk_W}{dk_F} + \beta p_\theta \frac{d\theta}{dk_F}(p - w - ak_F) - \beta(r+\delta+p(\theta))a\right]}{r+\delta+\beta p(\theta)}. \tag{11}$$

The sign of the expression is ambiguous. The first term in the numerator is positive and reflects the compensating differential force that pushes wages downward when k_F decreases. The second and third terms are negative, implying that they push the wage upwards when k_F decreases. The second term measures the increase in rents in the labor market given by the change in θ because of the larger amount of vacancies, and the third term measures the direct benefits on employers given by the reduction of k_F. The parameter that mediates the sign of the wage effect is β. When β is small, workers are unlikely to capture the additional rents, thus the compensating differential force dominates pushing wages downwards. As β increases, workers gradually capture additional rents, making the wage effect eventually positive. As in the employment analysis, the magnitude of the effect is proportional to a.

While simple, this model helps to rationalize why a reduction in litigiousness may have a positive employment effect with no change in average wages.

6 Conclusion

WC schemes may be beneficial to workers and employers if they streamline the process of compensation for workplace accidents and limit the need to resort to costly and inefficient litigation. This paper shows that a reform in Argentina that imposed a government medical intermediary to mediate between parties was successful at reducing lawsuits, implying a substantial reduction in litigation costs. We find that this efficiency gain had effects on the labor market equilibrium: the reform increased aggregate employment with no aggregate effect on the number of active firms or average monthly wages. In the most affected sectors –construction, mining, and manufacturing– the employment effect is especially pronounced.

Our results suggest that the efficiency-enhancing potential of WC schemes depends on their ability to limit litigation. WC policies, however, should not be uniquely analyzed from this angle since they also affect job quality (ILO, 2017) and may have distributional effects. Our analysis shows that the positive employment effects are not tied to significant changes in wages, suggesting that employers are capturing the incremental job surplus derived from the decrease in litigation. The heterogeneous effects by economic sector also suggest that the benefits of the policy are not evenly distributed in the labor market.

More research is needed to have a more comprehensive picture of the winners and losers of the policy. Knowing if the profits of firms and insurers were affected by the reform would shed light on the conjectured redistributive consequences of WC policies. Other policy tools, such as income and corporate taxes or sector-specific minimum wages, could help to balance asymmetric rent-sharing when efficiency gains are not translated to higher wages.

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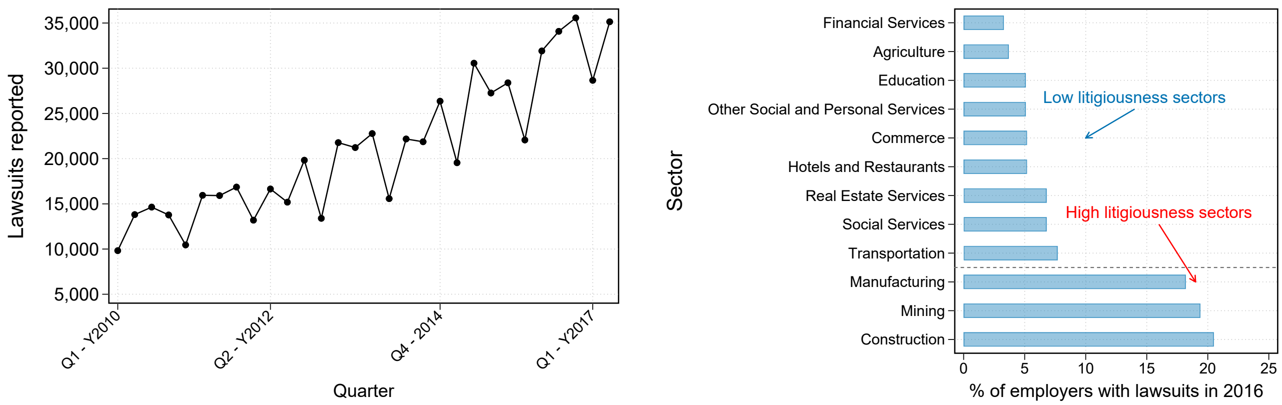
A Figures and Tables

Figure 1: Workplace litigiousness before the reform

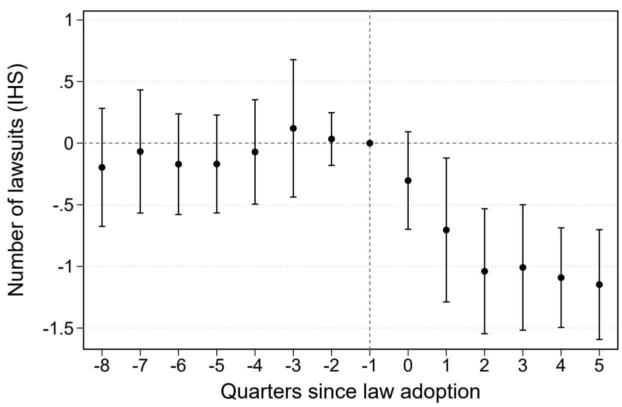


(a) New lawsuits reported by quarter

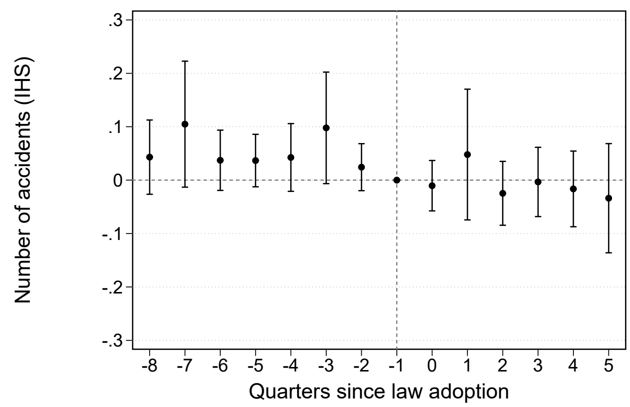
(b) Litigiousness by sector

Notes: Panel (a) shows the total number of new lawsuits reported in the country in each quarter, from the first period in which the system for reporting lawsuits entered into effect (January of 2010) to the quarter in which the reform we study was sanctioned at the Federal level (February of 2017). Panel (b) shows the share of employers in each sector that had lawsuits during the 2016. Manufacturing, Mining, and Construction are indicated as sectors highly affected by litigiousness.

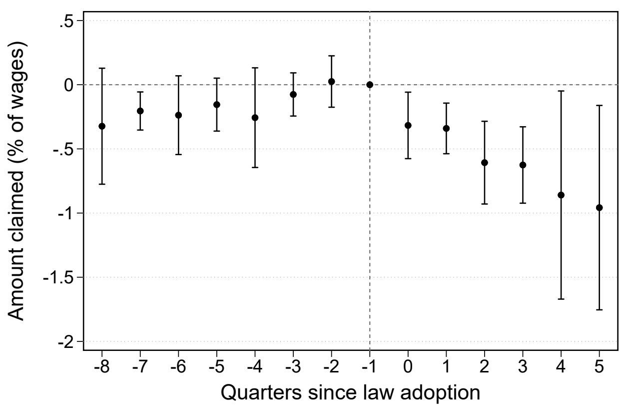
Figure 2: Province-level results



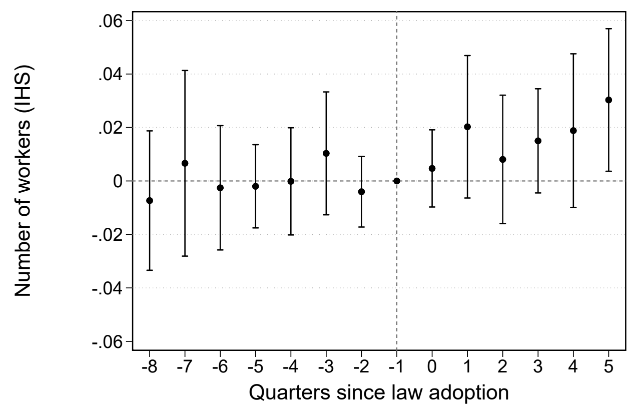
(a) Lawsuits



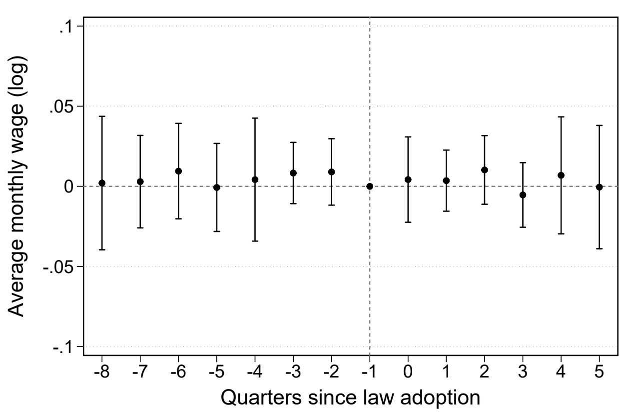
(b) Accidents



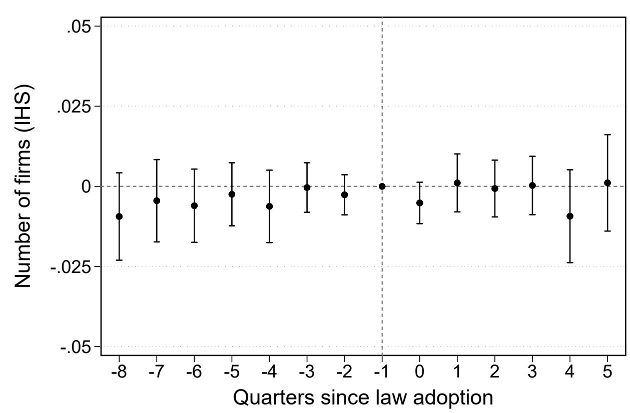
(c) Amount claimed



(d) Employment



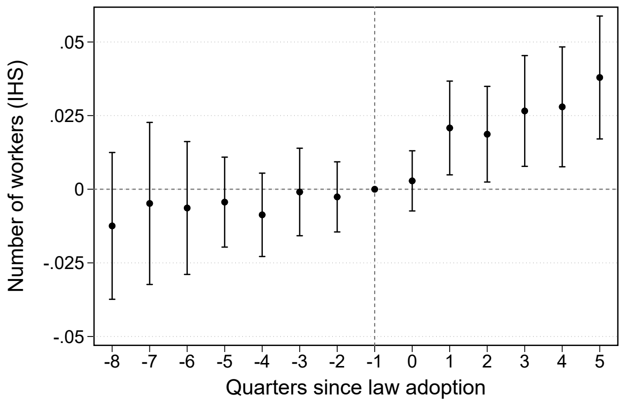
(e) Average monthly wage



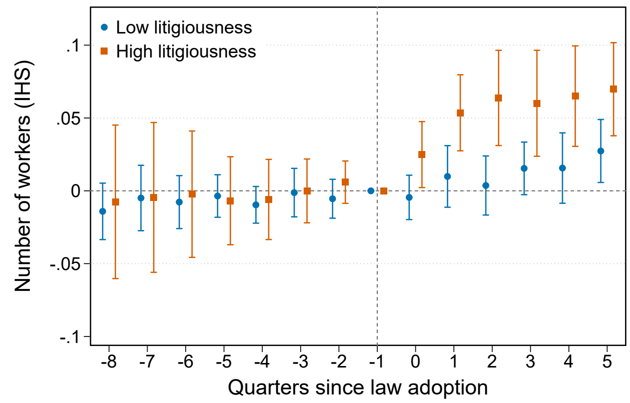
(f) Number of firms

Notes: This figure plots the β_k coefficients from equation (1) using different dependent variables. The unit of observation is a province-by-quarter. Standard errors are clustered at the province level. Vertical bars represent 95% confidence intervals. The dependent variable in Panel (a) is the inverse hyperbolic sine of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

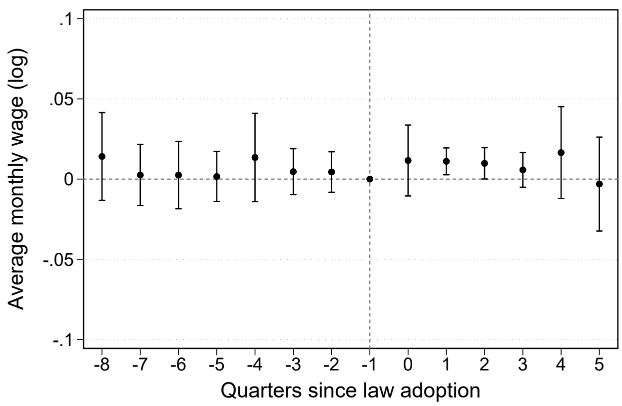
Figure 3: Sector-by-province level results: Labor market outcomes



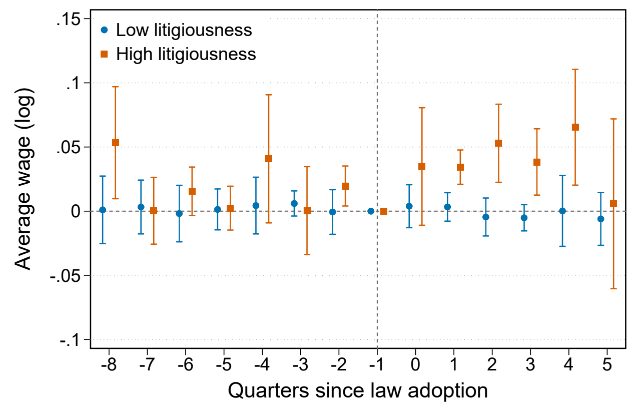
(a) Employment



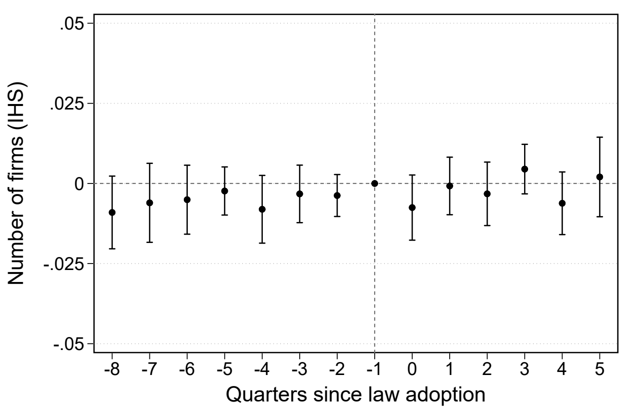
(b) Employment - Heterogeneity



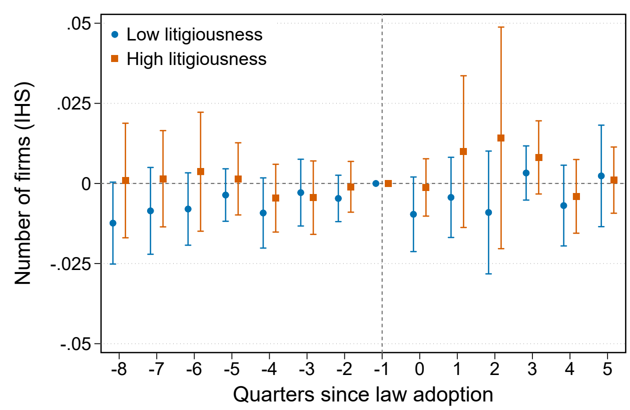
(c) Average monthly wage



(d) Average monthly wage - Heterogeneity



(e) Number of firms



(f) Number of firms - Heterogeneity

Notes: This figure plots the β_k coefficients from equation (1) using different dependent variables. The unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Coefficients in orange correspond to the event study for sectors indicated as "high litigiousness" in Figure 1: construction, mining, and manufacturing. Coefficients in blue correspond to the event study for the rest of the sectors. Vertical bars represent 95% confidence intervals. The dependent variable in Panels (a) and (b) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panels (c) and (d) is the natural logarithm of the average monthly wage. The dependent variable in Panels (e) and (f) is the inverse hyperbolic sine of the total number of firms.

Table 1: Summary statistics

	Observations	Mean	Standard Deviation	Median
Panel A. Province level				
Number of lawsuits	432	1051	2531	115
Amount claimed	432	.386	.496	.229
Insurance fee	432	3.33	.483	3.35
Number of accidents	432	5767	11493	1867
Number of workers	432	271499	500124	90448
Average wage	432	24109	12714	21263
Number of firms	432	23430	42262	7789
Panel B. Sector-by-province level				
Number of lawsuits	5,184	77.8	288	4
Amount claimed	5,182	.47	.985	.112
Insurance fee	5,182	4.01	2.32	3.71
Number of accidents	5,184	130	365	29.3
Number of workers	5,184	22362	55879	6085
Average wage	5,158	25066	18404	20076
Number of firms	5,184	1946	5271	421

Notes: Panel A shows summary statistics of variables aggregated at the province-by-quarter level and Panel B shows summary statistics of variables aggregated at the sector-by-province-by-quarter. Number of lawsuits is the total number of lawsuits reported during the quarter. Amount claimed is the total amount claimed in lawsuits as a percentage of the total wage bill, equal to total employment times the average wage, in a given quarter. Insurance fee is the average insurance fee charged by occupational hazards insurance companies, measured as a percentage of the total wage bill in a given quarter. Number of accidents is the total number of accidents reported during the quarter. Number of workers is the average number of workers employed during a quarter. Average wage is the average monthly wage during the quarter. Number of firms is the average number of active firms during a quarter.

Table 2: Main results

	(1) Lawsuits	(2) Amount claimed	(3) Accidents	(4) Employment	(5) Average wage	(6) Active firms	(7) Insurance fee
Panel A. Province-level results							
Treated	-0.771***	-0.406***	-0.0445	0.0179	0.000267	0.00150	-0.0261
	(0.193)	(0.132)	(0.0305)	(0.0129)	(0.00997)	(0.00592)	(0.0653)
Wild bootstrap p	0.0000	0.0110	0.2462	0.1381	0.9820	0.7778	0.7037
Observations	432	432	432	432	432	432	432
Number of provinces	24	24	24	24	24	24	24
Panel B. Sector-by-province analysis: overall effect							
Treated	-0.710***	-0.455***	-0.0412	0.0275**	0.00341	0.00176	-0.0213
	(0.177)	(0.0985)	(0.0453)	(0.0113)	(0.00838)	(0.00538)	(0.0775)
Wild bootstrap p	0.0000	0.0000	0.5776	0.0380	0.7357	0.7287	0.8418
Observations	5184	5182	5184	5184	5158	5184	5182
Number of provinces	24	24	24	24	24	24	24
Panel C. Sector-by-province analysis: heterogeneity by litigiousness							
Treated × Low Litigiousness	-0.704***	-0.341***	-0.0276	0.0187	-0.000828	0.00113	0.00103
	(0.176)	(0.0743)	(0.0313)	(0.0118)	(0.00791)	(0.00596)	(0.0476)
Treated × High Litigiousness	-0.727***	-0.800***	-0.0819	0.0537**	0.0161	0.00366	-0.0884
	(0.188)	(0.254)	(0.0956)	(0.0196)	(0.0131)	(0.00841)	(0.203)
Wild bootstrap p (low)	0.0000	0.0000	0.5415	0.1762	0.9530	0.8468	0.4414
Wild bootstrap p (high)	0.0230	0.0490	0.6647	0.0450	0.3213	0.6957	0.3403
Observations	5184	5182	5184	5184	5158	5184	5182
Number of provinces	24	24	24	24	24	24	24

Notes: This table reports OLS estimates of difference-in-differences coefficients from equation (2). Panel A reports results for regressions at the province level, with all specifications including province fixed effects and region-by-time fixed effects. Panels B and C report results for regressions at the sector-by-province level, with all specifications including sector-by-province fixed effects and region-by-time fixed effects. In column (1) the dependent variable is the inverse hyperbolic sine of the number of lawsuits reported during the quarter. In column (2) the dependent variable is the total amount of money claimed in lawsuits as a percentage of the total wage bill. In column (3) the dependent variable is the inverse hyperbolic sine of the total number of accidents reported during the quarter. In column (4) the dependent variable is the inverse hyperbolic sine of the number of workers reported during a quarter. In column (5) the dependent variable is the natural logarithm of the average monthly wage. In column (6) the dependent variable is the inverse hyperbolic sine of the total number of firms. In column (7) the dependent variable is the average insurance fee charged, measured as a percentage of the total wage bill. Treated is a dummy variable equal to 1 for treated provinces in the 6 quarters after the law entered into effect. High Litigiousness is a dummy variable equal to 1 for high litigiousness sectors as defined in Panel (b) of Figure 1. Wild bootstrap p is the p-value for the statistical significance of the difference-in-differences coefficient using the Wild Bootstrap that imposes the null from Cameron et al. (2008) with 1000 replications. Wild bootstrap p (interaction) is the p-value for the statistical significance of the interaction term between Treated and High Litigiousness coefficient using the Wild Bootstrap that imposes the null from Cameron et al. (2008) with 1000 replications. In all specifications, standard errors are clustered at the province level. * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

Workplace Litigiousness and Labor Market Outcomes: Evidence from a Workers' Compensation Reform

Appendix - For online publication only

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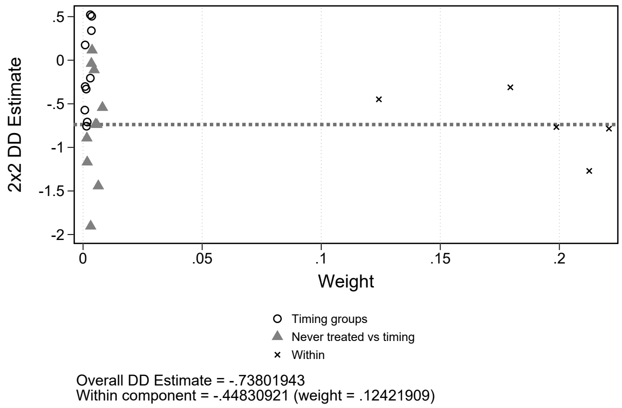
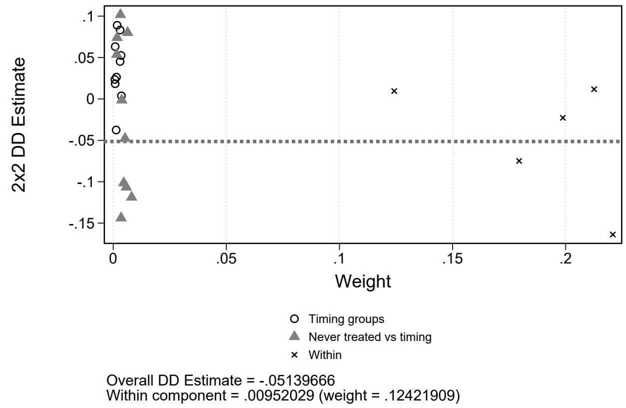
A Goodman-Bacon (2021) decompositions

This section presents decompositions based on Goodman-Bacon (2021). Intuitively, with staggered implementation, the difference-in-differences coefficient constitutes a weighted average of post-pre comparisons between early treated units and never treated units and not-yet-treated units, but also "forbidden comparisons" using early treated units as control for late treated units. The decomposition from Goodman-Bacon (2021) assesses the degree to which each type of comparison drives the results. Reassuringly, in our case, the estimation for the difference-in-differences coefficient relies almost exclusively on comparisons between treated units and never-treated units.

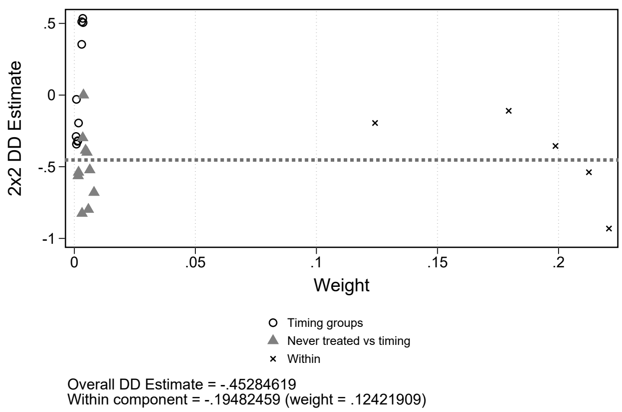
Figure A.I: Goodman-Bacon (2021) decomposition of province-level results



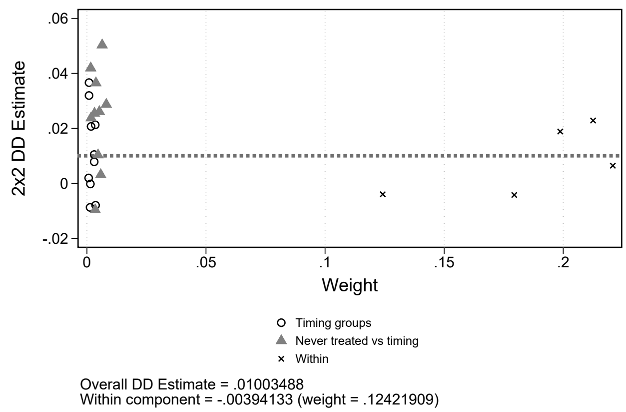
(a) Lawsuits



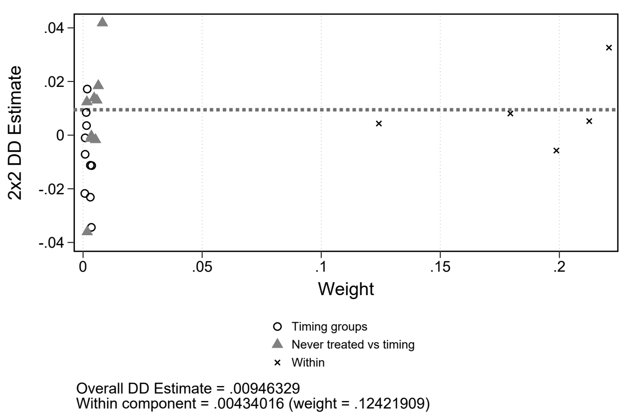
(b) Accidents



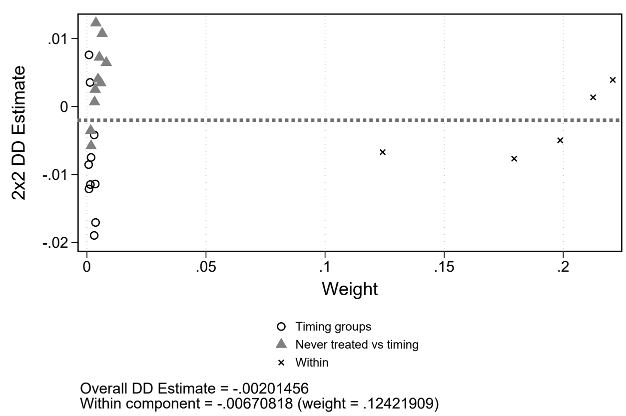
(c) Amount claimed



(d) Employment



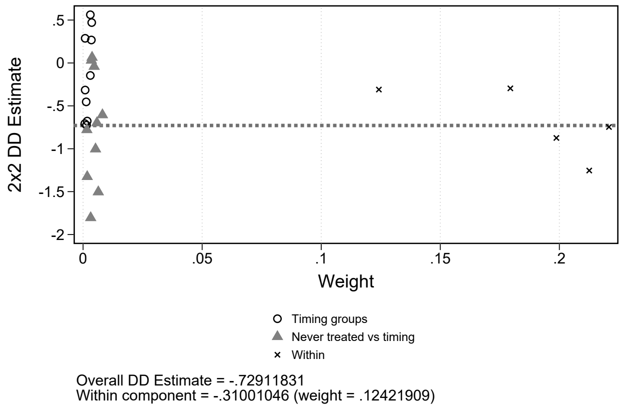
(e) Average monthly wage



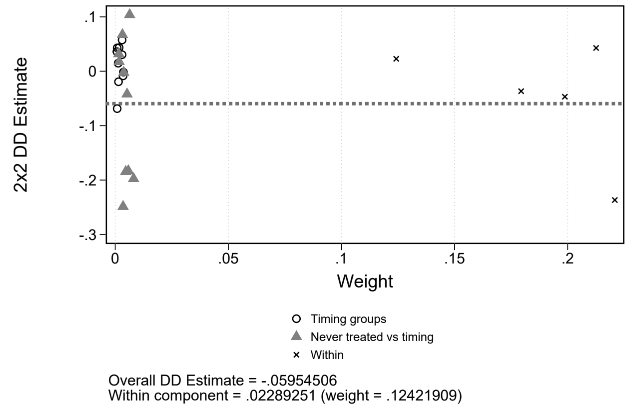
(f) Number of firms

Notes: This figure shows the 2x2 difference-in-difference coefficients and weights assigned by the Goodman-Bacon (2021) decomposition for the estimation of equation (2) including time and province fixed effects using different dependent variables. The unit of observation is a province-by-quarter. The dependent variable in Panel (a) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

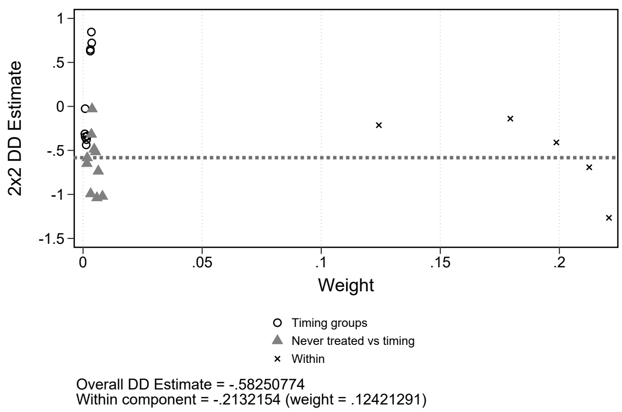
Figure A.II: Goodman-Bacon (2021) decomposition of sector-by-province level results



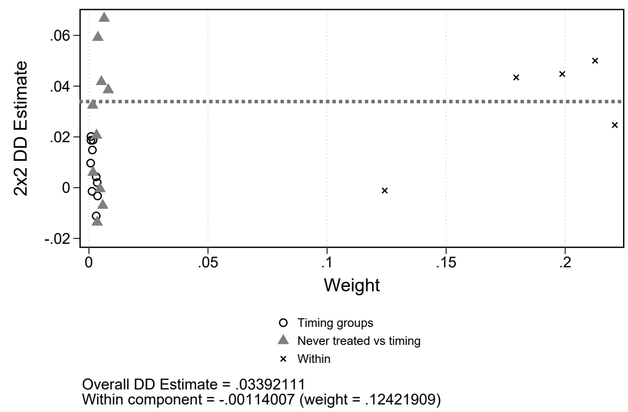
(a) Lawsuits



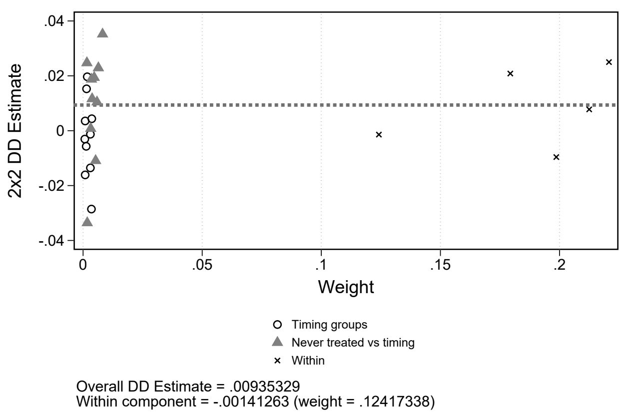
(b) Accidents



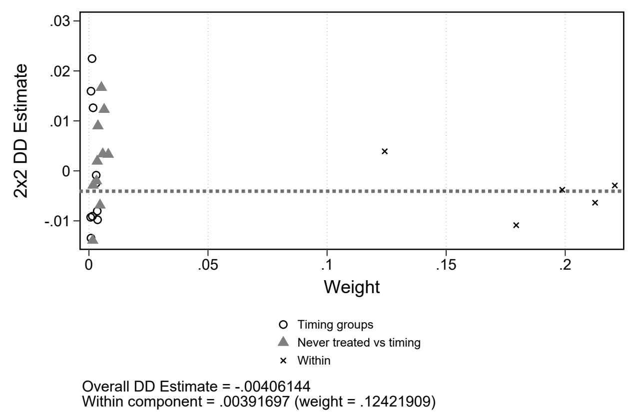
(c) Amount claimed



(d) Employment



(e) Average monthly wage



(f) Number of firms

Notes: This figure shows the 2x2 difference-in-difference coefficients and weights assigned by the Goodman-Bacon (2021) decomposition for the estimation of equation (2) including time and province fixed effects using different dependent variables. The unit of observation is a sector-by-province-by-quarter. The dependent variable in Panel (a) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

B Additional results

Table B.I: Effects on extensive margin

	(1) 1{Lawsuits>0}	(2) 1{Accidents>0}	(3) 1{Employment>0}	(4) 1{Active firms>0}
Panel A. Sector-by-province analysis				
Treated	-0.0103	0.00185	-6.99e-19	7.84e-18
Wild bootstrap p	0.5225	0.5275	0.5716	0.6146
Observations	5184	5184	5184	5184
Number of provinces	24	24	24	24
Panel B. Sector-by-province analysis: heterogeneity by litigiousness				
Treated $\times$ Low Litigiousness	-0.00976	0.00185	-4.89e-19	5.54e-18
Treated $\times$ High Litigiousness	-0.0119	0.00185	-3.42e-17	2.00e-17
Wild bootstrap p (low)	0.5275	0.5526	0.7177	0.7177
Wild bootstrap p (high)	0.4585	0.5475	0.6587	0.6587
Observations	5184	5184	5184	5184
Number of provinces	24	24	24	24

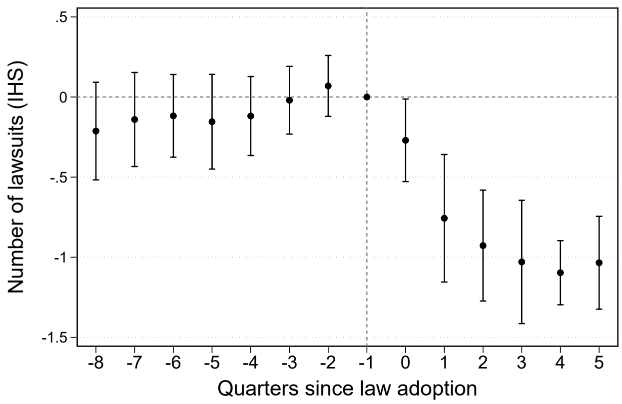
Notes: This table reports OLS estimates of difference-in-differences coefficients from equation (2). All estimates are for regressions at the sector-by-province level, with all specifications including sector-by-province fixed effects and region-by-time fixed effects. In column (1) the dependent variable is an indicator of having a positive number of lawsuits reported during the quarter. In column (2) the dependent variable is an indicator of having a positive number of accidents reported during the quarter. In column (3) the dependent variable is an indicator of having a positive number of employment reported in the quarter. In column (4) the dependent variable is an indicator of having a positive number of firms reported in the quarter. Treated is a dummy variable equal to 1 for treated provinces in the 6 quarters after the law entered into effect. High Litigiousness is a dummy variable equal to 1 for high litigiousness sectors as defined in Panel (b) of Figure 1. Wild bootstrap p is the p-value for the statistical significance of the difference-in-differences coefficient using the Wild Bootstrap that imposes the null from Cameron et al. (2008) with 1000 replications. Wild bootstrap p (interaction) is the p-value for the statistical significance of the interaction term between Treated and High Litigiousness coefficient using the Wild Bootstrap that imposes the null from Cameron et al. (2008) with 1000 replications. Clustered standard errors are not reported since the cannot be estimated given the lack of variation in the dependent variable.

Table B.II: Main results using log specifications

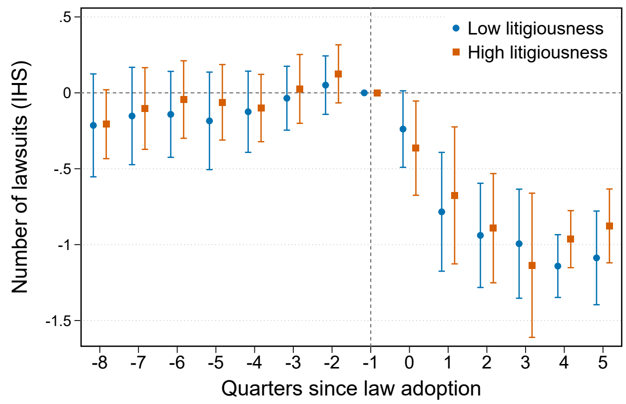
	(1) Lawsuits	(2) Amount claimed	(3) Accidents	(4) Employment	(5) Average wage	(6) Active firms	(7) Insurance fee
Panel A. Province-level results							
Treated	-0.766***	-0.406***	-0.0445	0.0179	0.000267	0.00150	-0.0261
	(0.193)	(0.132)	(0.0305)	(0.0129)	(0.00997)	(0.00592)	(0.0653)
Wild bootstrap p	0.0000	0.0110	0.2462	0.1381	0.9820	0.7778	0.7037
Observations	431	432	432	432	432	432	432
Number of provinces	24	24	24	24	24	24	24
Panel B. Sector-by-province analysis: overall effect							
Treated	-0.747***	-0.455***	-0.0389	0.0275**	0.00341	0.00171	0.00176
	(0.184)	(0.0985)	(0.0461)	(0.0113)	(0.00838)	(0.00541)	(0.00538)
Wild bootstrap p	0.0000	0.0000	0.6026	0.0380	0.7357	0.7317	0.8418
Observations	3835	5182	5155	5158	5158	5180	5184
Number of provinces	24	24	24	24	24	24	24
Panel C. Sector-by-province analysis: heterogeneity by litigiousness							
Treated × Low Litigiousness	-0.743***	-0.341***	-0.0252	0.0187	-0.000828	0.00107	0.0589
	(0.182)	(0.0743)	(0.0327)	(0.0118)	(0.00791)	(0.00600)	(0.0659)
Treated × High Litigiousness	-0.758***	-0.800***	-0.0800	0.0537**	0.0161	0.00361	-0.262
	(0.198)	(0.254)	(0.0960)	(0.0196)	(0.0131)	(0.00839)	(0.193)
Wild bootstrap p (low)	0.0000	0.0000	0.5646	0.1762	0.9530	0.8569	0.4414
Wild bootstrap p (high)	0.0220	0.0651	0.6907	0.0450	0.3213	0.6987	0.3403
Observations	3835	5182	5155	5158	5158	5180	5182
Number of provinces	24	24	24	24	24	24	24

Notes: Notes: This table reports OLS estimates of difference-in-differences coefficients from equation (2). Panel A reports results for regressions at the province level, with all specifications including province fixed effects and region-by-time fixed effects. Panels B and C report results for regressions at the sector-by-province level, with all specifications including sector-by-province fixed effects and region-by-time fixed effects. In column (1) the dependent variable is the natural logarithm of the number of lawsuits reported during the quarter. In column (2) the dependent variable is the total amount of money claimed in lawsuits as a percentage of the total wage bill. In column (3) the dependent variable is the natural logarithm of the total number of accidents reported during the quarter. In column (4) the dependent variable is the natural logarithm of the number of workers reported during a quarter. In column (5) the dependent variable is the natural logarithm of the average monthly wage. In column (6) the dependent variable is the natural logarithm of the total number of firms. In column (7) the dependent variable is the average insurance fee charged, measured as a percentage of the total wage bill. Treated is a dummy variable equal to 1 for treated provinces in the 6 quarters after the law entered into effect. High Litigiousness is a dummy variable equal to 1 for high litigiousness sectors as defined in Panel (b) of Figure 1. Wild bootstrap p is the p-value for the statistical significance of the difference-in-differences coefficient using the Wild Bootstrap that imposes the null from Cameron et al. (2008) with 1000 replications. Wild bootstrap p (interaction) is the p-value for the statistical significance of the interaction term between Treated and High Litigiousness coefficient using the Wild Bootstrap that imposes the null from Cameron et al. (2008) with 1000 replications. In all specifications, standard errors are clustered at the province level. * Significant at the 10% level ** Significant at the 5% level *** Significant at the 1% level.

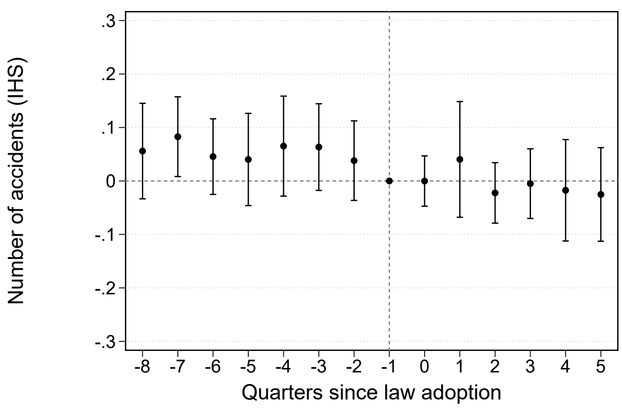
Figure B.I: Sector-by-province level results: Lawsuits, accidents, and amounts claimed



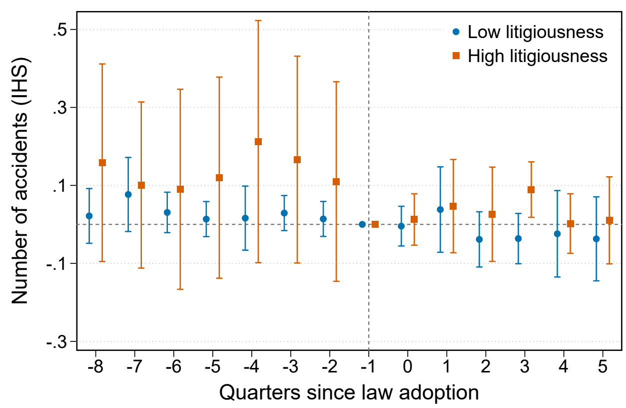
(a) Lawsuits



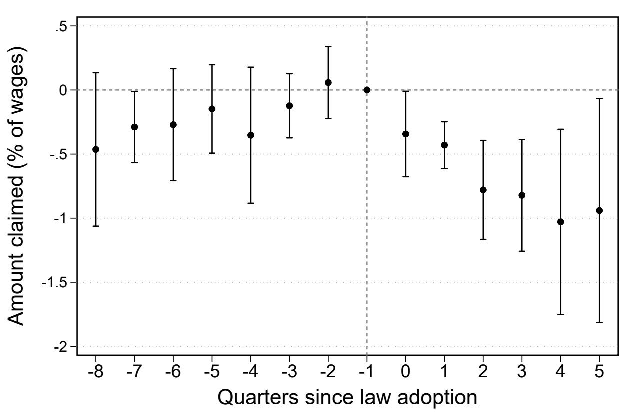
(b) Lawsuits - Heterogeneity



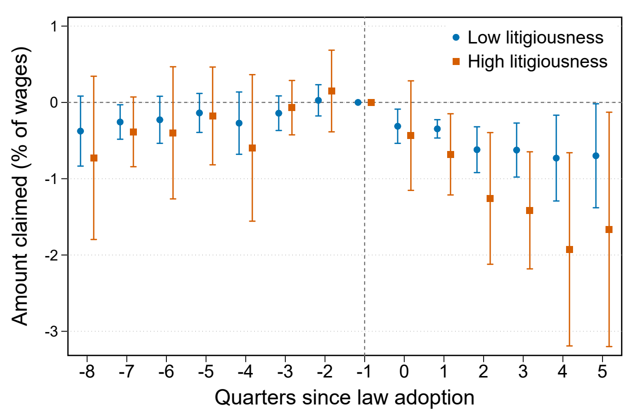
(c) Accidents



(d) Accidents - Heterogeneity



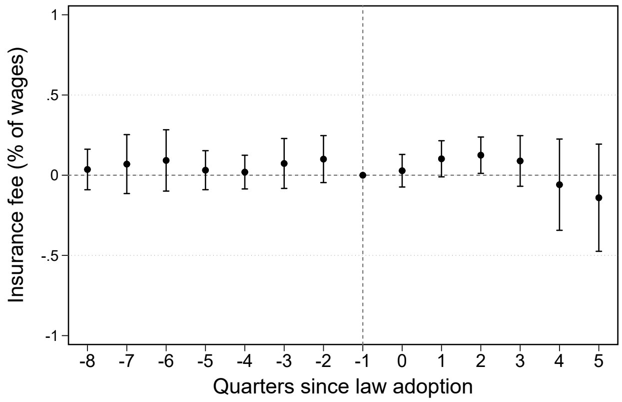
(e) Amount claimed



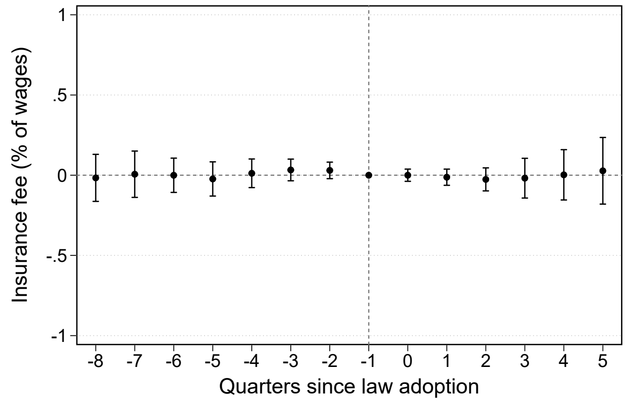
(f) Amount claimed - Heterogeneity

Notes: This figure plots the β_k coefficients from equation (1) at the sector-by-province-by-quarter level using different dependent variables. The unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Coefficients in orange correspond to the event study for sectors indicated as "high litigiousness" in figure 1: construction, mining, and manufacturing. Coefficients in blue correspond to the event study for the rest of the sectors. Vertical bars represent 95% confidence intervals. The dependent variable in Panels (a) and (b) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panels (c) and (d) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panels (e) and (f) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage).

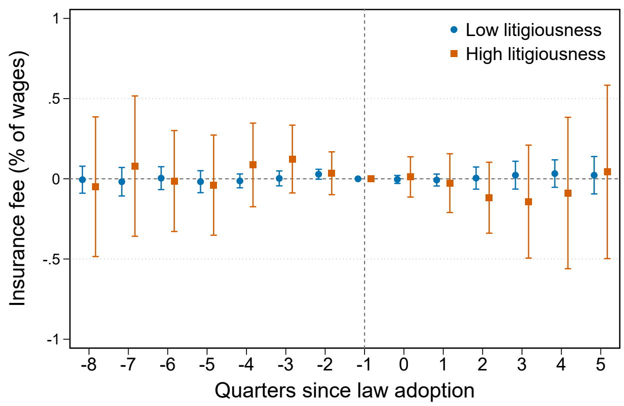
Figure B.II: Insurance fee results



(a) Province level



(b) Sector-by-province level



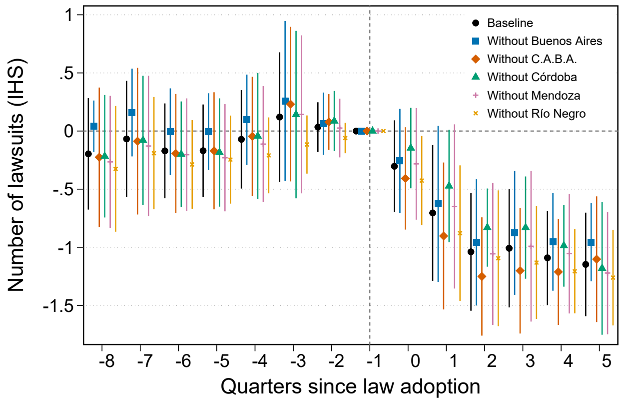
(c) Heterogeneity by litigiousness

Notes: This figure plots the β_k coefficients from equation (1). The dependent variable is the average insurance fee charged measured as a percentage of the total wage bill. In panel (a) the unit of observation is a province-by-quarter and in panels (b) and (c) the unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Vertical bars represent 95% confidence intervals.

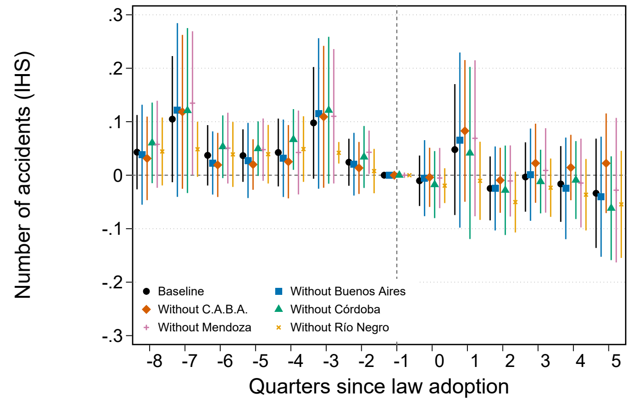
C Leave-one-out regressions

This appendix compares the baseline estimates to leave-one-out alternative specifications, where we sequentially drop one of the 5 treated provinces from the sample and run the event study using the remaining 23 provinces. We first present leave-one-out comparisons for province-level results and then for sector-by-province-level results.

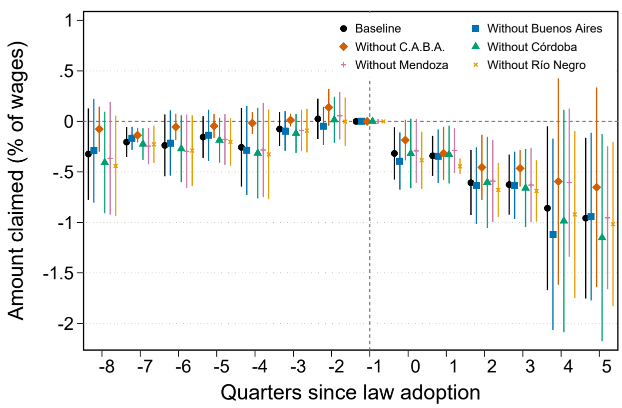
Figure C.I: Leave-one-out regressions: province-level results



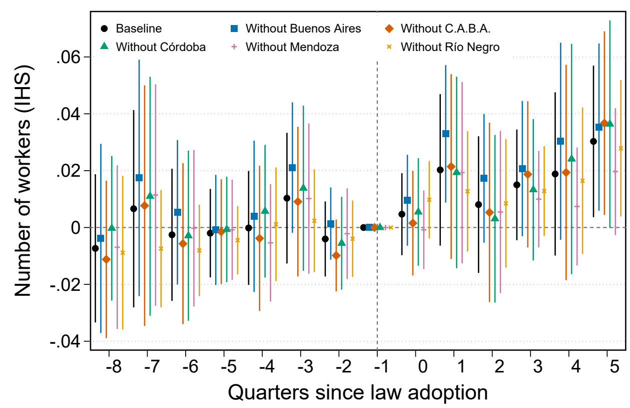
(a) Lawsuits



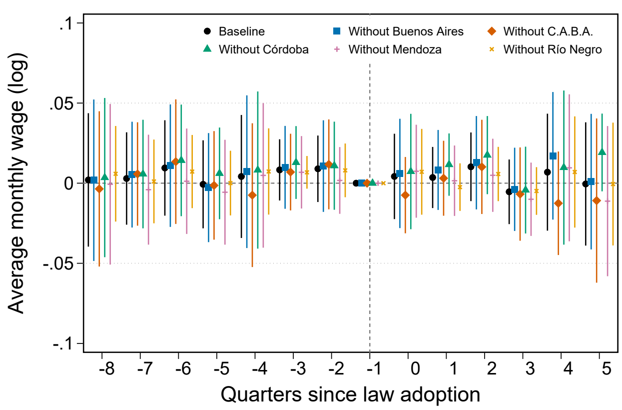
(b) Accidents



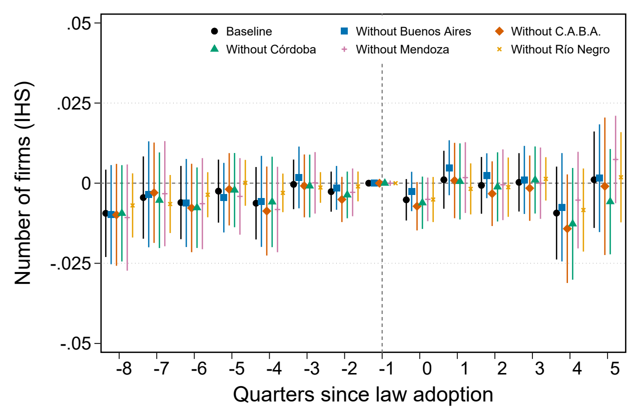
(c) Amount claimed



(d) Employment



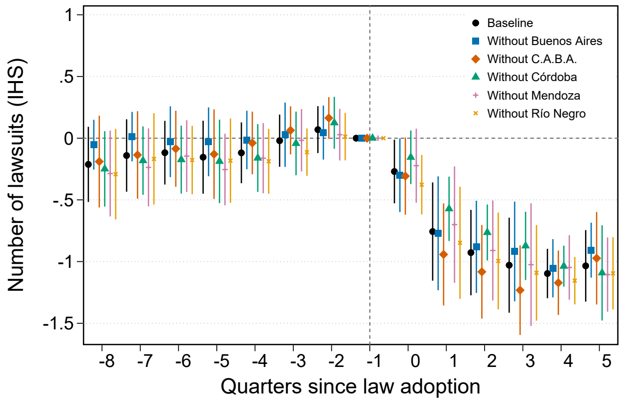
(e) Average monthly wage



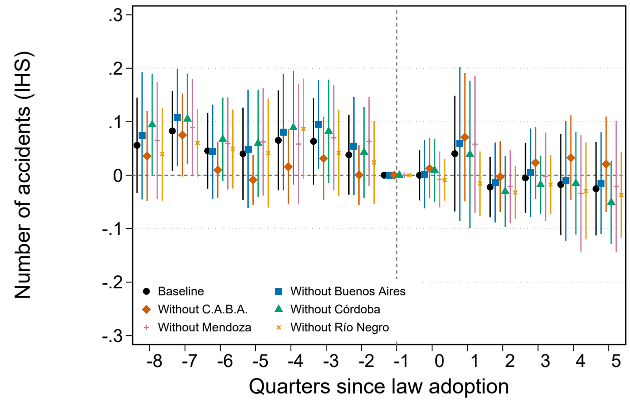
(f) Number of firms

Notes: This figure plots the β_k coefficients from equation (1) using different dependent variables. The unit of observation is a province-by-quarter. Standard errors are clustered at the province level. Vertical bars represent 95% confidence intervals. Panel (a) is the inverse hyperbolic sine of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms. Black coefficients correspond to our baseline estimates using the full sample. Blue coefficients correspond to estimates dropping the Province of Buenos Aires. Orange coefficients correspond to estimates dropping the Autonomous City of Buenos Aires (C.A.B.A. stands for *Ciudad Autónoma de Buenos Aires*). Green coefficients correspond to estimates dropping the province of Córdoba. Pink coefficients correspond to estimates dropping the province of Mendoza. Yellow coefficients correspond to estimates dropping the province of Río Negro.

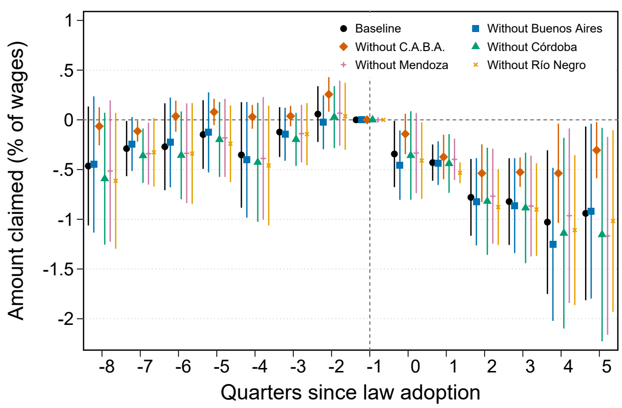
Figure C.II: Leave-one-out regressions: sector-by-province-level results



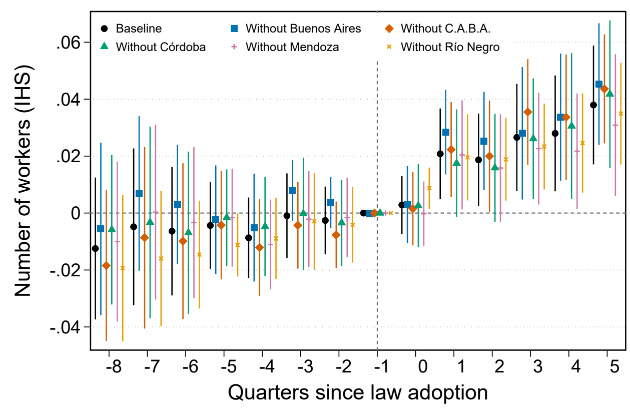
(a) Lawsuits



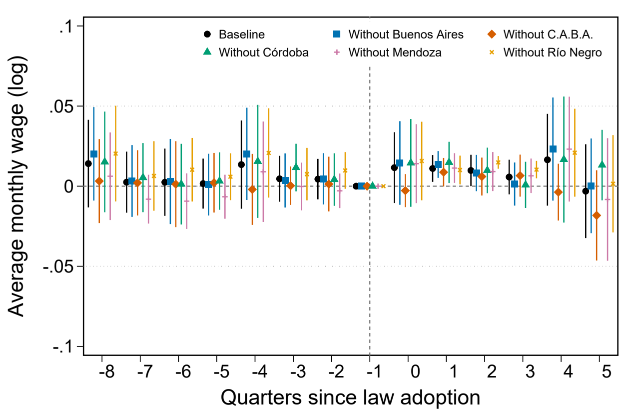
(b) Accidents



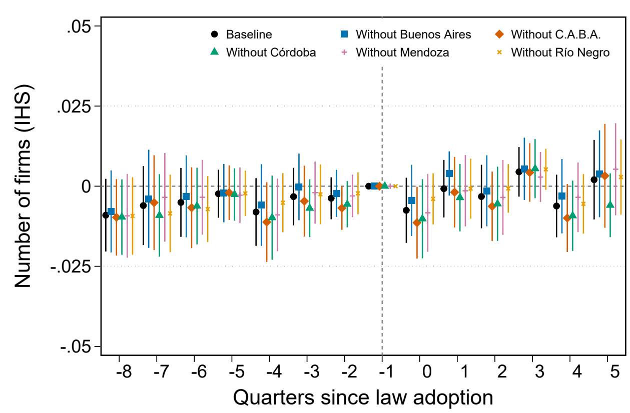
(c) Amount claimed



(d) Employment



(e) Average monthly wage



(f) Number of firms

Notes: This figure plots the β_k coefficients from equation (1) using different dependent variables. The unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Vertical bars represent 95% confidence intervals. Panel (a) is the inverse hyperbolic sine of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms. Black coefficients correspond to our baseline estimates using the full sample. Blue coefficients correspond to estimates dropping the Province of Buenos Aires. Orange coefficients correspond to estimates dropping the Autonomous City of Buenos Aires (C.A.B.A. stands for *Ciudad Autónoma de Buenos Aires*). Green coefficients correspond to estimates dropping the province of Córdoba. Pink coefficients correspond to estimates dropping the province of Mendoza. Yellow coefficients correspond to estimates dropping the province of Río Negro.

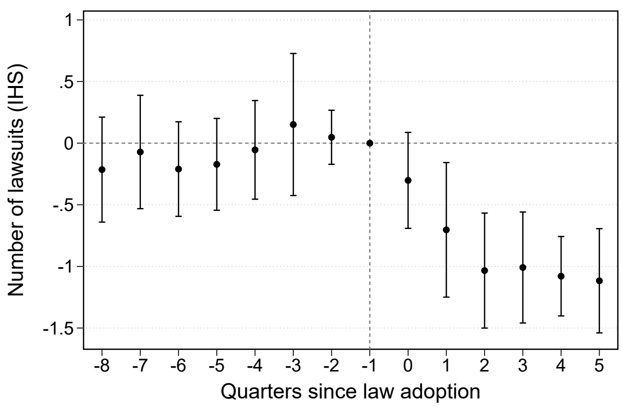
D Stacked event studies

In this subsection we estimate the main event studies of interest using a stacked event study approach. For each treated province, we define a window of 14 quarters, 8 before the reform and 6 after. We then define an event-specific control group for that province consisting of never treated provinces. This creates a data-set for each specific event. We then stack all the event-specific data-set and estimate event-study regressions quarter-by-region-by-event fixed effects. We include province-by-event fixed effects for the province-level analysis and sector-by-province-by-event fixed effects for the sector-by-province-level analysis. The equation we estimate is given by:

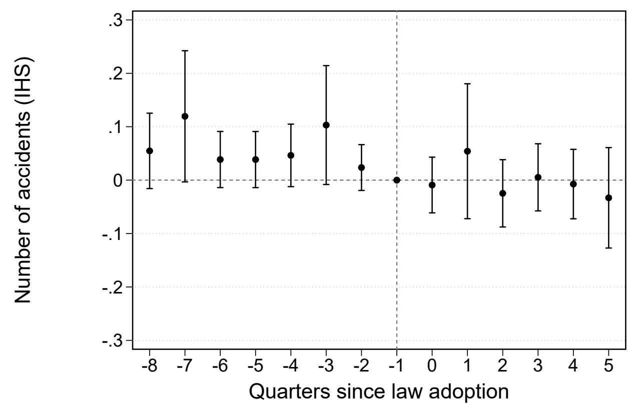
$$Y_{pt} = \alpha_{pe} + \mu_{r(p)te} + \sum_{k=-8}^{5} \beta_k \mathbb{1}\{t = e_p + k\} \times \text{Treated}_p + \varepsilon_{ept}, \tag{D.I}$$

where e is an index for events.

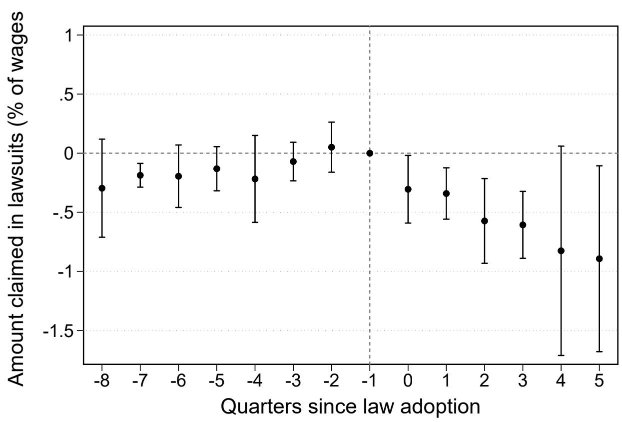
Figure D.I: Stacked event studies: province-level results



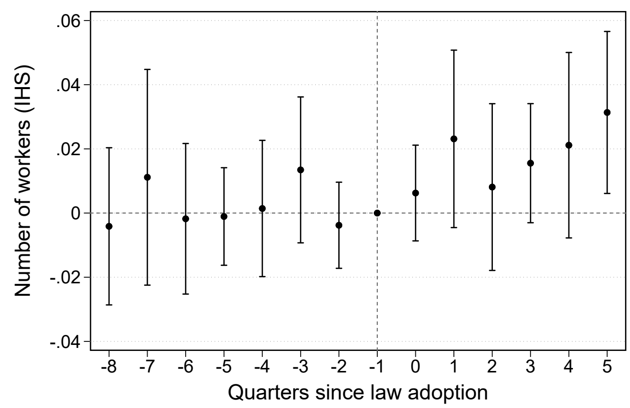
(a) Lawsuits



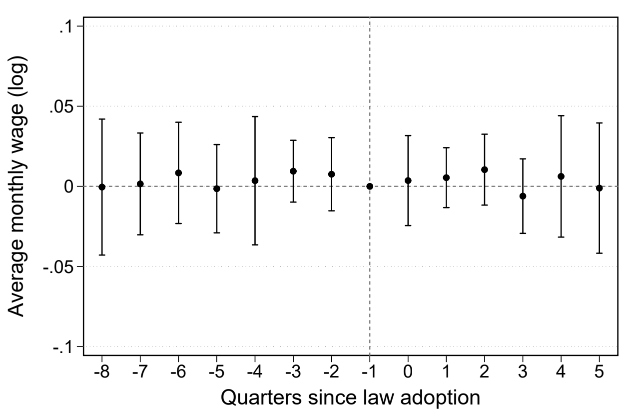
(b) Accidents



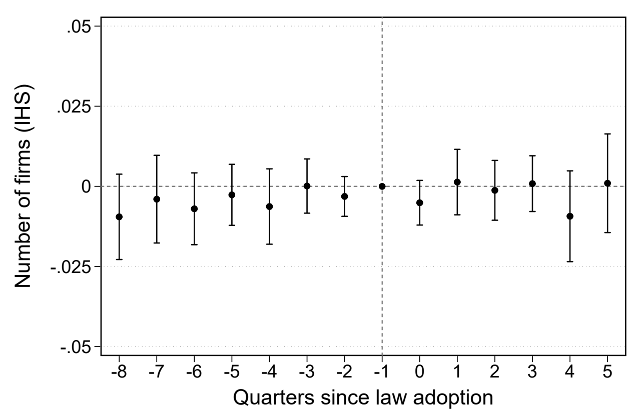
(c) Amount claimed



(d) Employment



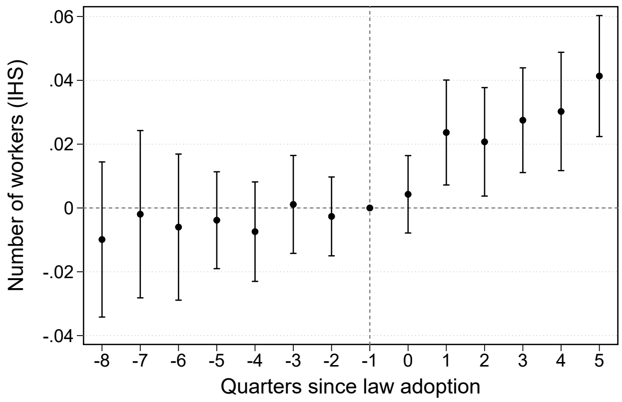
(e) Average monthly wage



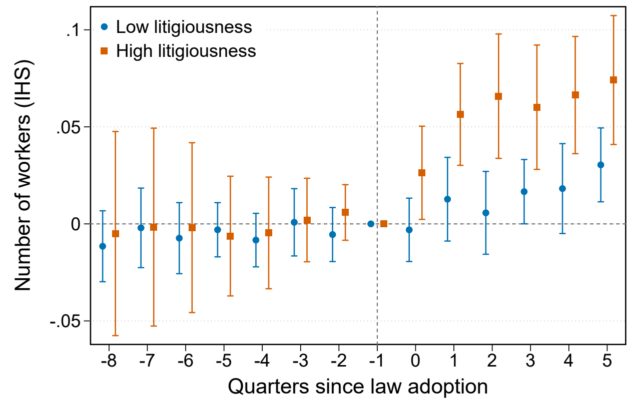
(f) Number of firms

Notes: This figure plots the β_k coefficients from equation (D.I) using different dependent variables. The unit of observation is a province-by-quarter. Standard errors are clustered at the province level. Vertical bars represent 95% confidence intervals. The dependent variable in Panel (a) is the inverse hyperbolic sine of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

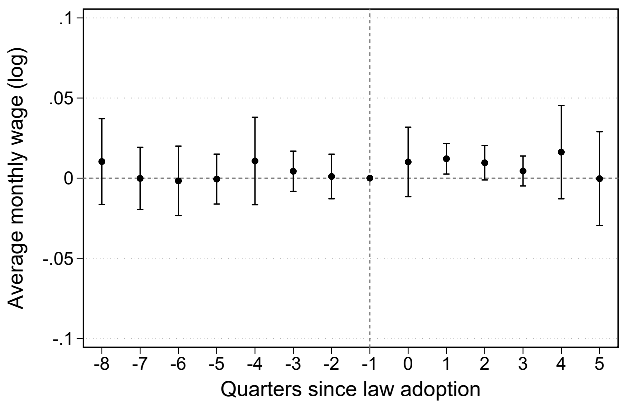
Figure D.II: Stacked event studies: sector-by-province level results - labor market outcomes



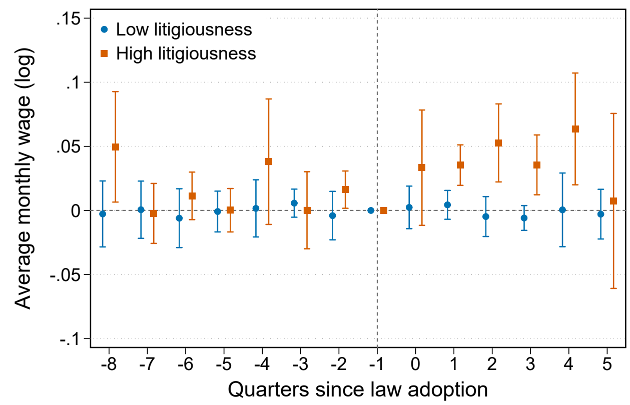
(a) Employment



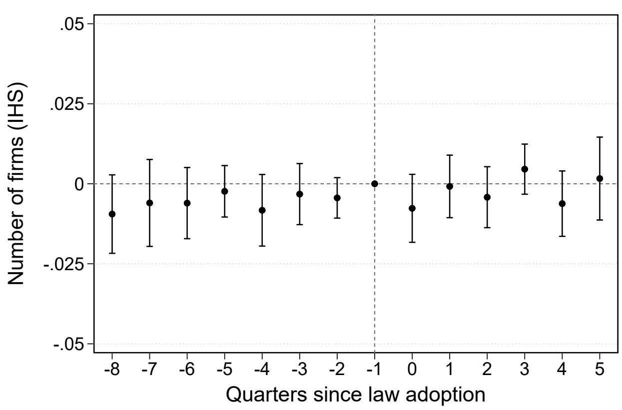
(b) Employment - Heterogeneity



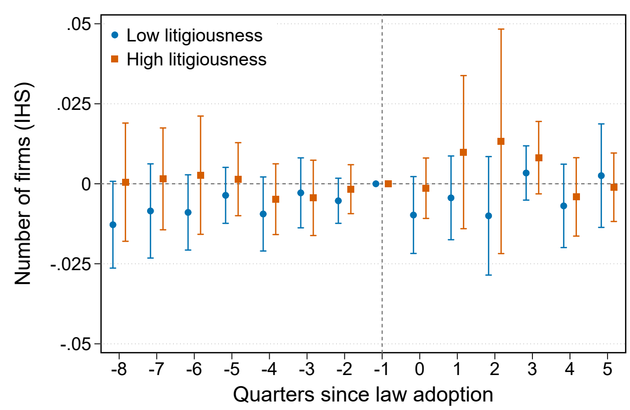
(c) Average monthly wage



(d) Average monthly wage - Heterogeneity



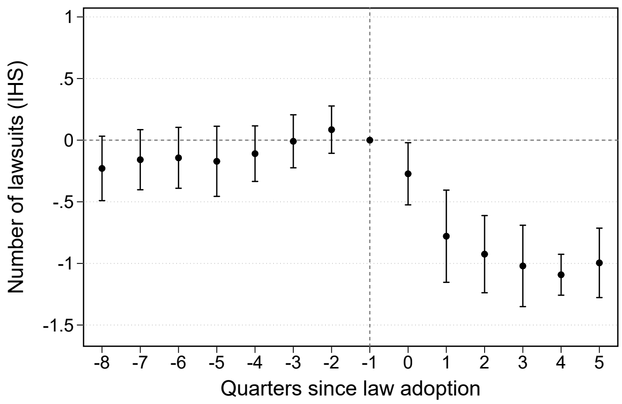
(e) Number of firms



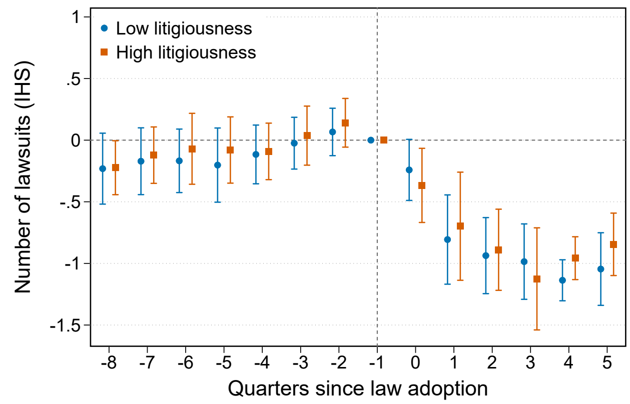
(f) Number of firms - Heterogeneity

Notes: This figure plots the β_k coefficients from equation (D.I) at the sector-by-province-by-quarter level using different dependent variables. The unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Coefficients in orange correspond to the event study for sectors indicated as "high litigiousness" in figure 1: construction, mining, and manufacturing. Coefficients in blue correspond to the event study for the rest of the sectors. Vertical bars represent 95% confidence intervals. The dependent variable in Panels (a) and (b) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panels (c) and (d) is the natural logarithm of the average monthly wage. The dependent variable in Panels (e) and (f) is the inverse hyperbolic sine of the total number of firms.

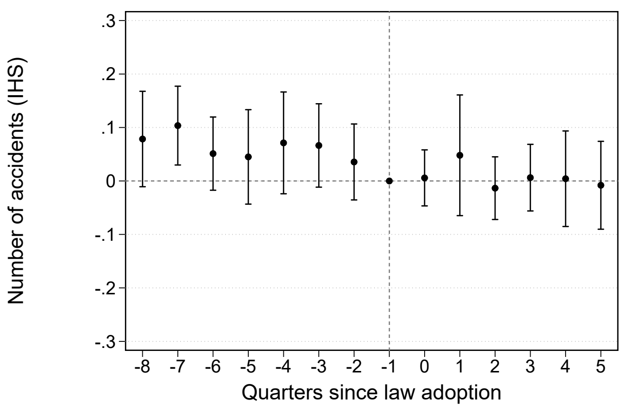
Figure D.III: Stacked event studies: sector-by-province level results - lawsuits and accidents



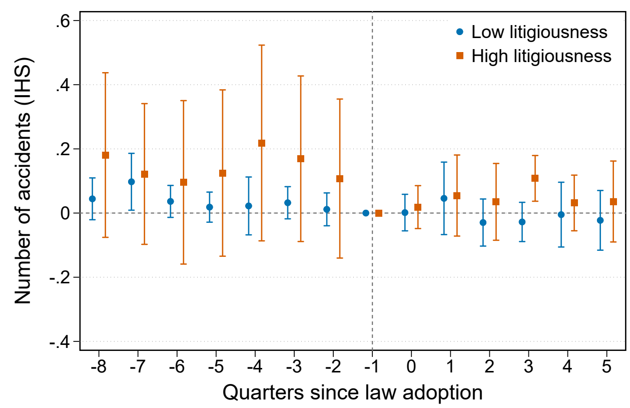
(a) Lawsuits



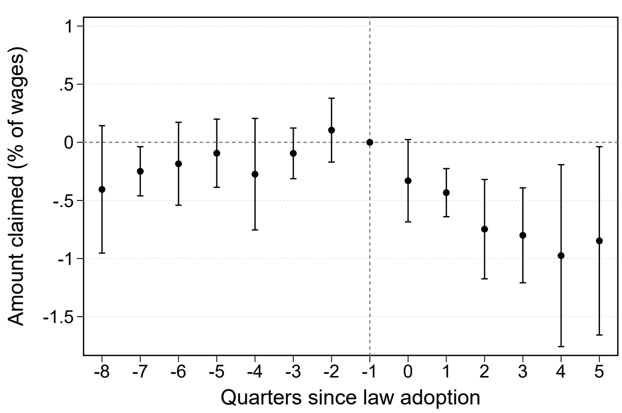
(b) Lawsuits - Heterogeneity



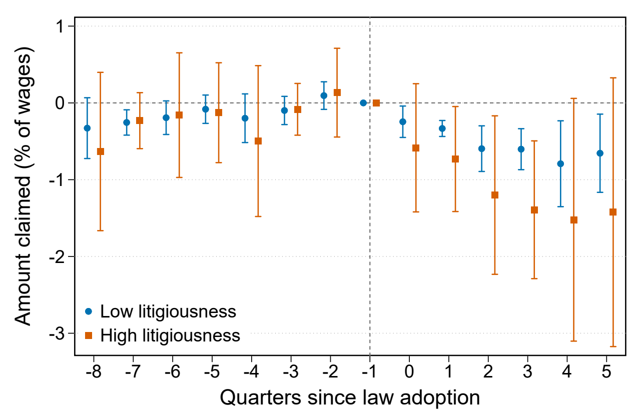
(c) Accidents



(d) Accidents - Heterogeneity



(e) Amount claimed



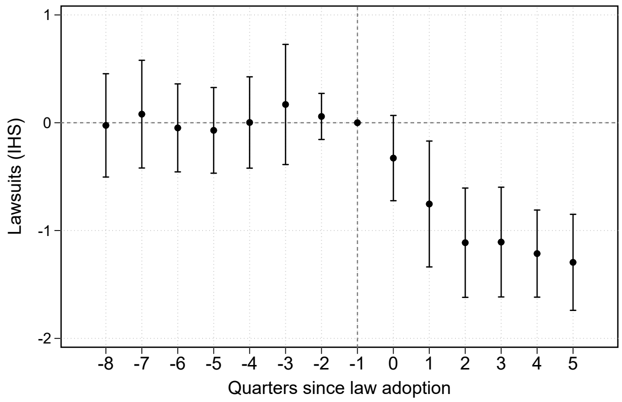
(f) Amount claimed - Heterogeneity

Notes: This figure plots the β_k coefficients from equation (D.I) at the sector-by-province-by-quarter level using different dependent variables. The unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Coefficients in orange correspond to the event study for sectors indicated as "high litigiousness" in figure 1: construction, mining, and manufacturing. Coefficients in blue correspond to the event study for the rest of the sectors. Vertical bars represent 95% confidence intervals. The dependent variable in Panels (a) and (b) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panels (c) and (d) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panels (e) and (f) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage).

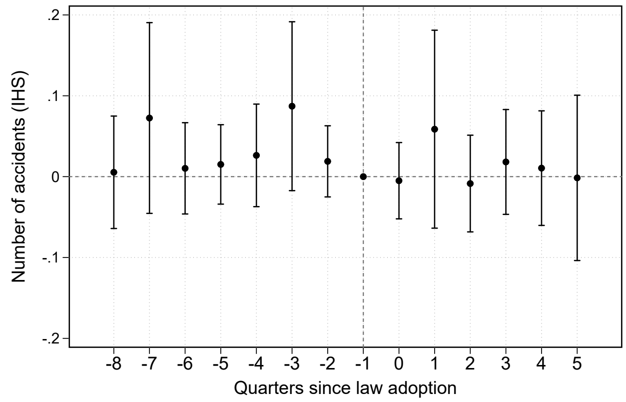
E Event studies adjusted for linear pre-treatment trend

In this subsection, we present our event-study results while adjusting for a linear pre-treatment trend. We take the coefficients from our main event-study specifications and calculate a straight line between event time -8 and event time -1 (which is normalized to zero). Then, we take each coefficient from the main event-study specification and substract the value predicted by the straight line for each event time. Results are virtually identical to other methods of linear extrapolation, such as by estimating a straight line on the pre-treatment periods using OLS.

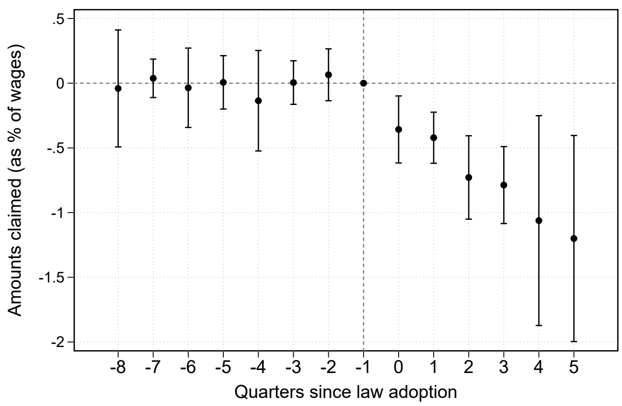
Figure E.I: Linear pre-treatment trend adjustment: province-level results



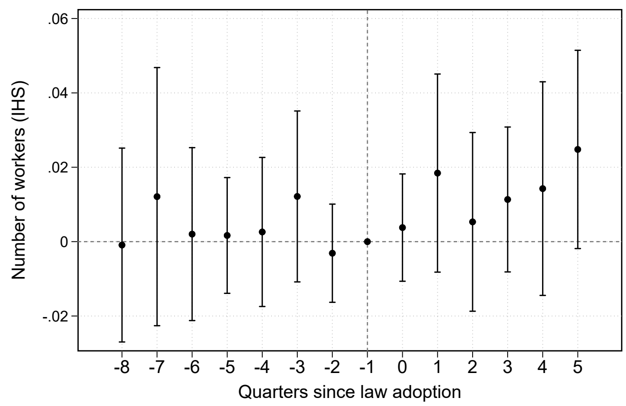
(a) Lawsuits



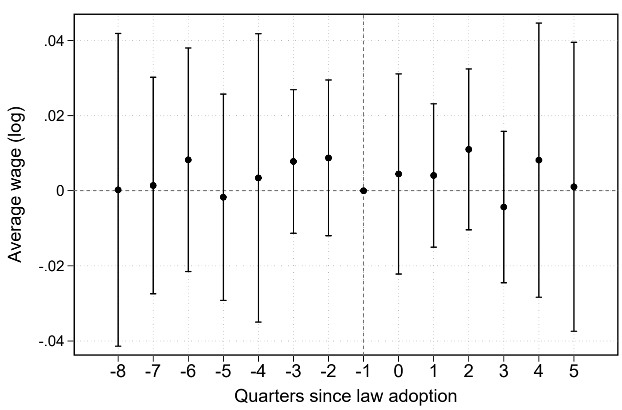
(b) Accidents



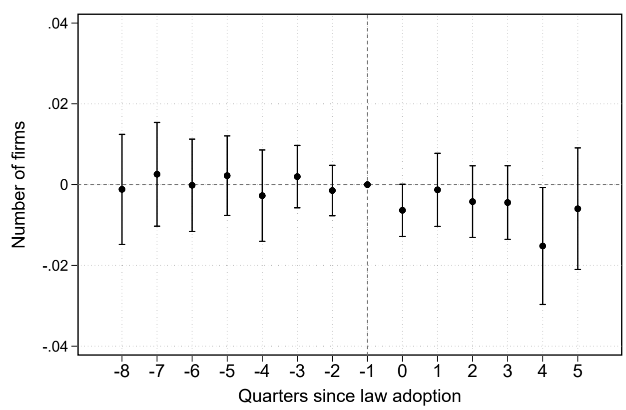
(c) Amount claimed



(d) Employment



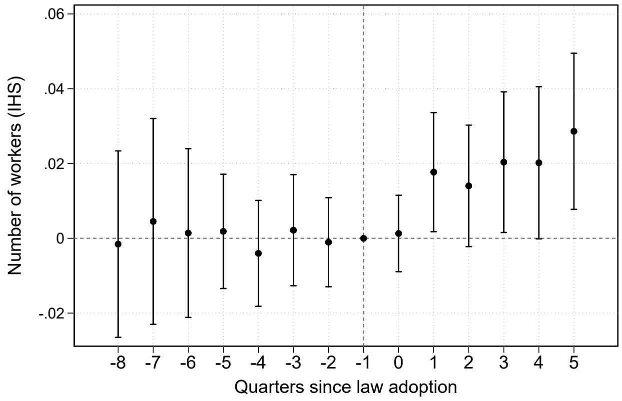
(e) Average monthly wage



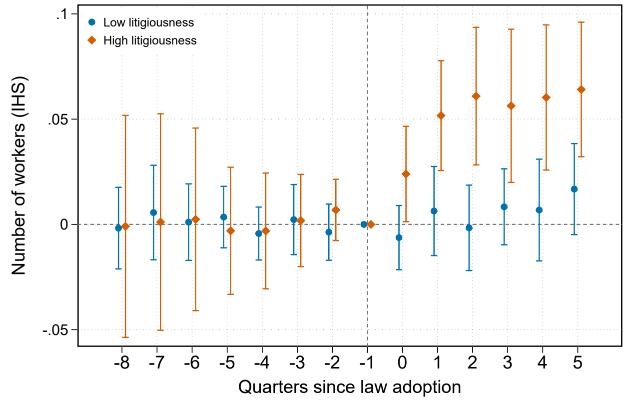
(f) Number of firms

Notes: This figure plots the β_k coefficients from equation (1) using different dependent variables, adjusted for a linear pre-treatment trend. The unit of observation is a province-by-quarter. Standard errors are clustered at the province level. Vertical bars represent 95% confidence intervals. The dependent variable in Panel (a) is the inverse hyperbolic sine of the total number of lawsuits reported. The dependent variable in Panel (b) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panel (c) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage). The dependent variable in Panel (d) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panel (e) is the natural logarithm of the average monthly wage. The dependent variable in Panel (f) is the inverse hyperbolic sine of the total number of firms.

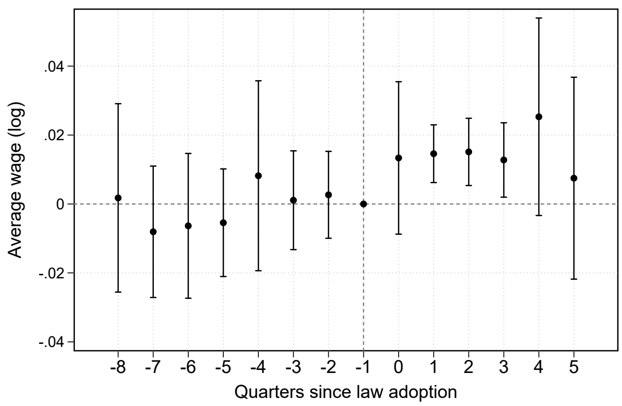
Figure E.II: Linear pre-treatment trend adjustment: sector-by-province level results - labor market outcomes



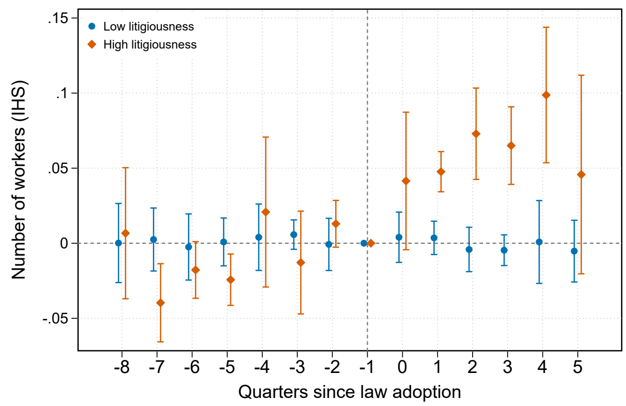
(a) Employment



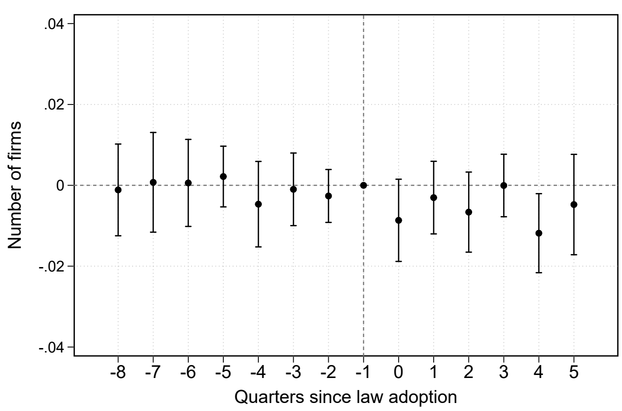
(b) Employment - Heterogeneity



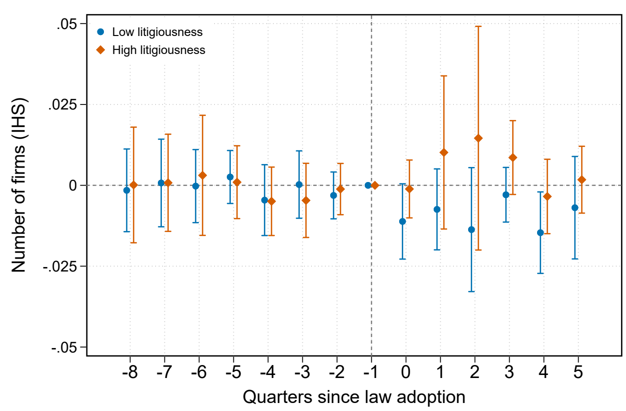
(c) Average monthly wage



(d) Average monthly wage - Heterogeneity



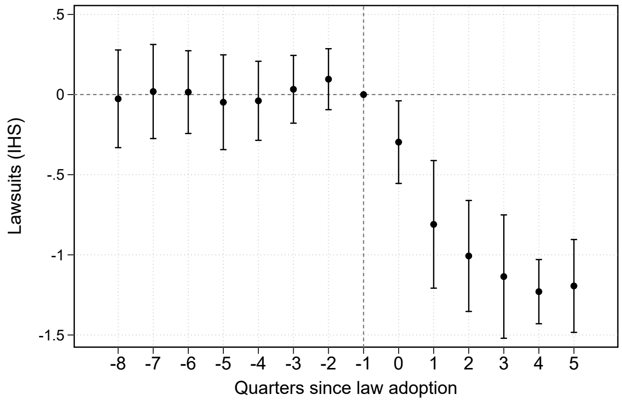
(e) Number of firms



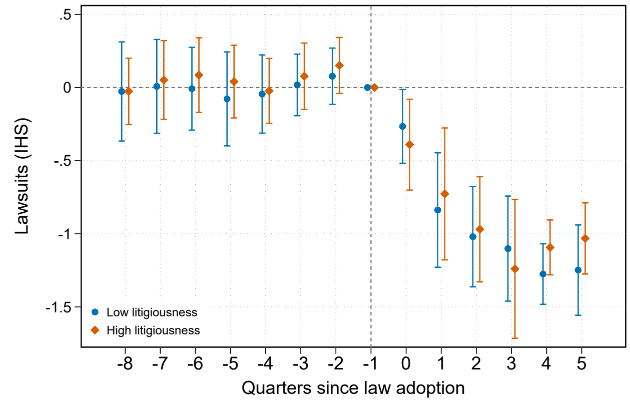
(f) Number of firms - Heterogeneity

Notes: This figure plots the β_k coefficients from equation (2) at the sector-by-province-by-quarter using different dependent variables, adjusted for a linear pre-treatment trend. The unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Coefficients in orange correspond to the event study for sectors indicated as "high litigiousness" in figure 1: construction, mining, and manufacturing. Coefficients in blue correspond to the event study for the rest of the sectors. Vertical bars represent 95% confidence intervals. The dependent variable in Panels (a) and (b) is the inverse hyperbolic sine of the total number of workers. The dependent variable in Panels (c) and (d) is the natural logarithm of the average monthly wage. The dependent variable in Panels (e) and (f) is the inverse hyperbolic sine of the total number of firms.

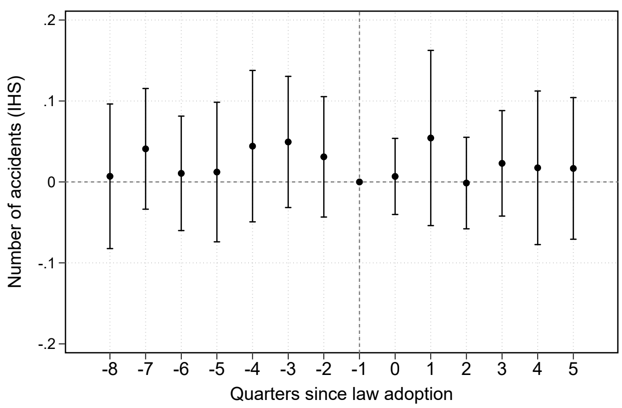
Figure E.III: Linear pre-treatment trend adjustment: sector-by-province level results - lawsuits and accidents



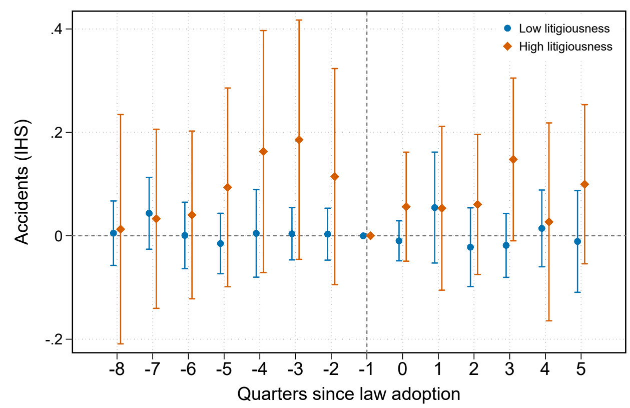
(a) Lawsuits



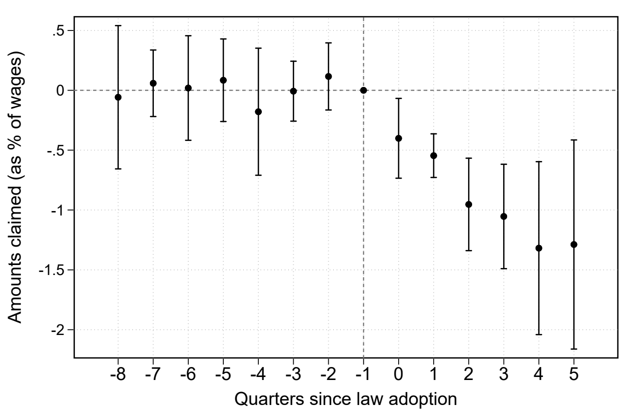
(b) Lawsuits - Heterogeneity



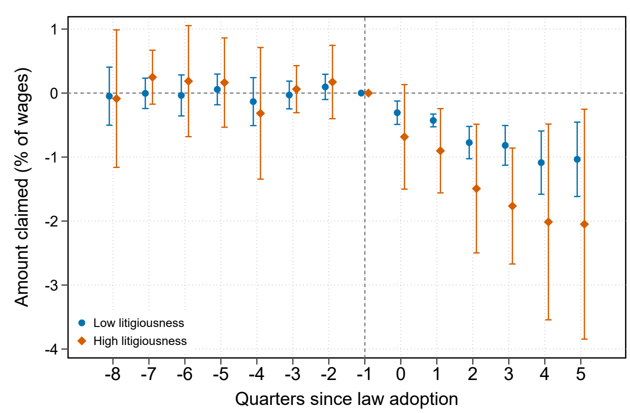
(c) Accidents



(d) Accidents - Heterogeneity



(e) Amount claimed



(f) Amount claimed - Heterogeneity

Notes: This figure plots the β_k coefficients from equation (2) at the sector-by-province-by-quarter level using different dependent variables, adjusted for a linear pre-treatment trend. The unit of observation is a sector-by-province-by-quarter. Standard errors are clustered at the province level. Coefficients in orange correspond to the event study for sectors indicated as "high litigiousness" in figure 1: construction, mining, and manufacturing. Coefficients in blue correspond to the event study for the rest of the sectors. Vertical bars represent 95% confidence intervals. The dependent variable in Panels (a) and (b) is the inverse hyperbolic sine transformation of the total number of lawsuits reported. The dependent variable in Panels (c) and (d) is the inverse hyperbolic sine of the total number of accidents reported. The dependent variable in Panels (e) and (f) is the amount claimed in lawsuits as a share of labor costs (total employment times average monthly wage).