POSTGRADO
ECONOMÍA Y NEGOCIOS
UNIVERSIDAD DE CHILE

"The effects of chilean Labor Reform of 2017 on strike activity"

TESIS PARA OPTAR AL GRADO DE

MAGISTER EN ECONOMÍA

Alumno: Martín Dibarrart Muñoz

Profesor Guía: Esteban Puentes Encina

Santiago, Noviembre 2020

Contents

Acknowledgments

First of all, I want to thank my parents, to whom I owe everything I have achieved and to whom I will always be in debt. No doubt whatever my achievements are from now on, will be their achievements too. I also want to thank my sister for putting up with me all these years.

Also, I want to thank Professor Esteban Puentes for his patience and knowledge, and for help me introduce myself in what is the academic world in which I want to go from here on ahead. His advice and instructions were essential to be able to carry out this thesis.

It is also necessary to thank my fellow economists, especially my colleagues from the masters program. To Nicolás Rojas, José Valenzuela, Brian Castro, Ignacio Rojas, Vivente Corral and Manuel Taboada whose support was important to make the process easier to carry. Also thank Juan Cristóbal Ayala, Diego Gallardo, Felipe Leiter, Florencia Aguilera and Joaquín Cifuentes for their unconditional friendship.

Finally, I would like to thank Macarena Vergara, who has accompanied me in these last years with her support and affection, and whose arrival in my life has made me very lucky. A thousand thanks to her too.

1 Introduction

Labor unions is a widely studied topic in economics. One less studied but related topic is labor strikes, the most notorious manifestation of labor conflict and unrest. The definition may not be unique depending on the country legislation but in general terms a labor strike consists of a deliberate production stoppage by a organized group of workers and its most important economic issue are the effects (i.e. costs) that they produce not only for the participants of a collective negotiation but to third parties that have nothing to do with it (Armstrong and Águila, 2005).

Because of this, strikes are nonoptimal from a Pareto perspective (Kennan, 1980), altough they are widely accepted in labor legislations as a right and underpinned by international institutions as the International Labour Organization (OIT) (whose conventions have been ratified by Chile). The latter recognizes the right to strike as one of the essential means that workers and its organizations have to promote and defend its economic and social interests (International Labour Organization, 2006). So strikes are usually not forbidden by governments. Nevertheless, collective bargaining legislation is enacted to improve the efficiency of negotiations and to reduce possible costs originated from strikes (Cramton et al., 1999).

Then, is important to evaluate whenever specific labor laws have their intended effects on collective negotiation outcomes, including strike activity. A portion of strike literature have focused on this (i.e. Budd, 1996; Campolieti et al., 2014; Cramton et al., 1999; Gunderson et al., 1989). But this kind of literature (and strikes literature in general) suffers an external validity problem. In practice, United States and Canada are countries that concentrate almost all the attention of the studies of this topic. Conclusions about labor legislation effects on strike outcomes barely could be extrapolated to other countries, especially developing countries like Chile.

In Chile a Labor Reform was enacted during 2016 and implemented in April 2017. It modernized the chilean collective bargaining system which was considered outdated at that moment. Despite the magnitude of the changes it introduced can be questioned, it represented (or intended to) one of the biggest changes in this matter, especially considering the introduction of strikers replacement bans during a strike (also called no-scab law) and also the possibility of pacting the extension of negotiation benefits to non-unionized workers given the union approval, with the objetive of strengthening unions during collective bargaining. This law could be a significant source of shift in strike activity that can be studied.

The effects of the Labor Reform of 2017 on chilean legal strike activity are studied in this paper. By strike activity, it is meant two dimensions of it: monthly number of strikes and duration of strikes in days. The data recollected by the Labor Strike Observatory (OHL) can be used to assess the shifts of these two dimensions along the quarters of study, taking advantage of the fact that industries and regions are subjected to different levels of exposition to the law according to their union rates. This paper is important because it can help to develop more evidence about the collective bargaining legislation and strike outcomes for developing countries.

The results indicate that the law could have some negative effect in strike duration, but had no effect in strike incidence. The effect in duration became visible one year after the law implementation, during in 2018, possibly explained by collective bargaining cycles. In a joint costs model framework, the negative effects on strike duration can be explained by the increase of strike costs from the law, especially by the replacement bans that impide that the firm continue its production during the conflict. But the low magnitude of the effects could indicate that the law did not introduce the fundamental collective bargaining legislation changes it had promised.

The paper is divided in the following sections. Section 2 is an institutional framework, reviewing a glimpse of chilean strike history and to deep in the Labor Reform of 2017. Section 3 is a literature review concerning the theoretical models that can help to understand and predict some possible results and to review the most relevant international evidence about labor legislation and strikes. Section 4 describes the methodology, this is, the identification strategy and the respective count and duration models fitted. Section 5 describes the data used and presents some descriptive analysis. Section 6 shows the results obtained for the count and duration models and finally Section 7 concludes.

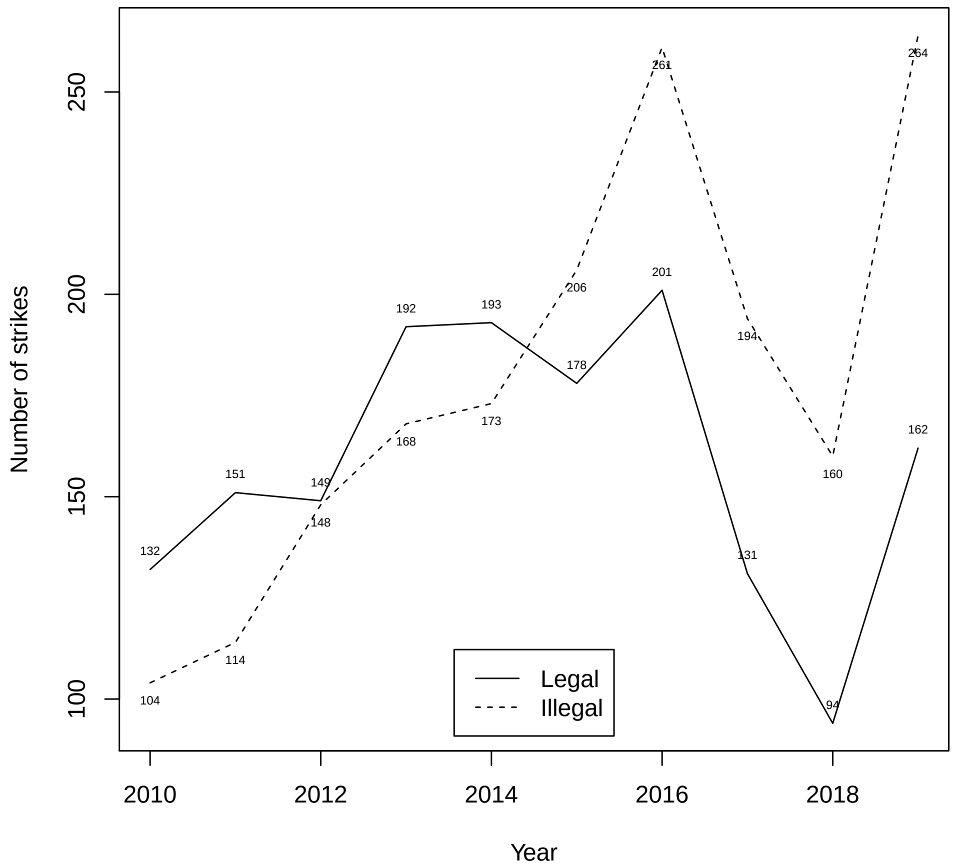
2 Institutional Framework

2.1 Strikes in Chile

There exists a line of research about chilean strikes started by Armstrong and Águila (2005), who in their book detail the main descriptive conclusions of the universe of strikes that happened in Chile between 1961 and 2002. They divided their sample in two periods: 1961-1973 and 1979-2002[1]. The first period is characterized by high labor conflict with a 17% of labor force involved in strikes while in the second period this value reduces to 3%. The authors say that political shifts between the two periods can explain this difference. In the first period there was a closed economy, an obsolete labor legislation, a powerful union central, among other things. In the second period the Chilean Military Dictactorship was imposed at the end of 1973 and the labor legislation changed. Extralegal strikes (strikes originated from informal negotiation procedures or that are forbidden by legislation, also called illegal strikes) were prohibited until 1987 and the labor conflict was low in the period even by international standards. Another big conclusion is related with the legal classification of strikes between legal and extralegal conflicts. Extralegal conflicts (without counting public sector for which strikes are forbidden) had bigger protagonism in the second period. The authors point out that this is because the cumbersome formal process of legal conflict and the lower costs that workers deal in informal processes of conflict. The last important conclusion is

[1]The authors point out that some laws suspended collective negotiations between 1973 and 1979.

Figure 1: Number of chilean strikes by legality



Source: Chilean Labor Strikes Database from Labor Strike Observatory (OHL)

the significant difference in duration between legal and extralegal conflicts. Extralegal strikes are shorter than their legal counterparts but this gap had tighten in time.

The OHL, created in 2014 by the Study Center of Conflict and Social Cohesion and the Bussiness School of Alberto Hurtado University, has the goal of studying the chilean labor conflict continuing Armstrong and Águila (2005) work by registering strikes in the country. The OHL also presents annual technical reports using their constructed data base. Their 2017 report (Gutiérrez et al. (2018)) focus on the (recent in that time) application of the Labor Reform of 2017 and its possible effects in strike activity. From a descriptive point of view in the last decade the number of strikes raised until 2016 to then sharply fell out in 2017 (as can be seen in Figure 1). A similar trend is observed for the number of committed workers in strikes but the peak is in 2012. Also, almost the total percentage of them came from extralegal conflicts. Then, the report shows the results from a linear model with the objective of finding possible law effects in the monthly number of strikes. Their results show that after the implementation of the law the monthly number of strikes significally decreased. This decrease was not significant for the extralegal strikes. The unemployment rate is included in this regressions but it did not have significant effects for both kind of strikes. The authors explain that this decrease of legal strike incidence can be attributed to the uncertainity generated by the recent new legislation. The parts involved decided to evade new conflict until they adapt to the new law. While the results seem promising, the measurement of a simple difference through the inclusion of a post treatment dummy hardly can measure the law effects without bias, because the coefficient captures not only the labor law effect but temporal effects that affect strike activity through the time. So, these results account for correlation and not for causality.

Following this insight, the main aspect of this study that differences the results shown here and those in OHL study is that attemps are made to obtain causal estimates of the law effects using a diff-in-diff approach. This paper also differs from previous related literature in some other issues that are important to point out. First, Armstrong and Águila (2005) did not work with econometric tools to obtain conclusions and their analysis is a mere descriptive analysis. Second, Gutiérrez et al. (2018) study presents econometric results but only focusing on the monthly number of strikes, leaving strike duration aside. Third, the OHL report fits a linear model for the monthly count data. As it will be seen later, more appropiate count and logit models will be fitted, according to the nature of the dependent variable. Lastly, this study will include a larger period since the OHL report encompasses a post-law period of less than one year, while here three years are encompassed.

2.2 Labor Reform of 2017

The 20.940 law, also known as the Labor Reform of 2017, was discussed through 2015 and 2016, enacted in August 29th 2016 and went into effect in April 1st 2017. It was considered the biggest modification to the Labor Code in terms of collective and union rights since the democracy recovery in 1990 (Consejo Superior Laboral, 2018). The law consisted of a series of individual changes to the Labor Code and a complete renovation of the collective negotiation section. Among the most notable indications are the extension of collective bargaining coverage to temporary workers and workers subject to apprenticeship contracts, expansion of the negotation issues, regulations to the information rights, set of the negotiation floor as the previous collective contract terms or the employer first answer, among other things. All this with the objective of, in the president at that period Michelle Bachelet own words: "*aim at the development of modern, fair and balanced labor relations between the parties, in which dialogue and agreement predominate, combining objectives of equity, efficiency and productivity*".

Between the most important changes is, on one hand, the possibility, if both employer and union agreed, to expand the accorded negotiation benefits to the not unionized workers. Of course, the latter must accept this too. Before the law, the employer could unilaterally extend the benefits without the permission of unions. So now, the union has power in the decision, incresing the attractiveness for non-unionized wokers of joining the union. On the other hand, and definitely the most controversial change imposed by the law, was the strengthening of the workers strike right manifested by the strikers replacement bans. Before the reform, replacements were permitted provided certain simple conditions were accomplished by the employer[2]. Now, thanks to the law, strikers replacement is illegal in all forms.

Despite the latter, the law included a figure that limits the strike right: the minimum services. This counterpart to the replacements ban compulse strikers to provide necessary personal to attend needed minimum services to: "*... protect physical goods and instalations of the firm and to prevent accidents...*", among other things. This figure is controversial because can attenuate the strike impact. Another problematic issue is the establishment of which are the minimum services: they must be agreed by the involved parties before negotiations. This can generate pre-negotiations for the minimum services (Consejo Superior Laboral (2018)).

This is important to point out because is an example of how despite being the law presented as a mayor change in labor legislation it is possible to not appreciate from it mayor changes in strike and negotiation outcomes. In fact, the law was very critized for not introducing more fundamental changes to the worker-firm relationship legislation. It was certainly enacted with the intention of strengthening the union bargaining power in collective negotiations. Thus, from a general perspective the law can be understood as a shift of negotiation power in favor of unions. The issue of how the higher bargaining power of workers is manifested and which are the effects expected from it in strike activity is approached is the next theoretical review.

3 Literature Review

3.1 Theoretical review

Strike theoretical literature has been built around three main lines of thought: the first is the model from Ashenfelter and Johnson (1969). The second one is the joint costs idea from Kennan (1980) and Reder and Neumann (1980). The last one corresponds to the asymmetric information models developed by a variety of authors, for example by Hayes (1984). Each one has its own advantages and disadvantages in explaining the possible effects of the reform in strike activity.

Ashenfelter and Johnson model was the first theoretical model that presented microeconomic hypothesis to be tested. This model focus on how union leaders in the attempt to maintain their political position

[2]That the last offer by the employer includes the same previous contract conditions (adjusted by inflation) at least, a minimum adjustment defined by the inflation excluding the 12 last months contempled and a replacement 4 UF grant by worker replaced.

must use strikes to deal with the high wage settlements expectations of workers, because these expectations decrease with the duration of a strike. This last relationship is the famous resistance curve, known and used by the firm to choose an optimal wage increase and strike duration. The biggest problem of this model is that assumes that workers are irrational (Kennan, 1980).

The joint costs model is the second and probably the most simple theoretical view of labor strikes. While was hard to conciliate a rational bargaining process with the existence of strikes, because they imply costs to both parts, Kennan (1980) proposed to look the problem from another perspective from those of that moment: in a labor conflict there exists a trend between parts to achieve the Pareto optimum (end of the strike) and this is more probable to be done when the higher are the joint costs (the sum of union and employer costs) associated to the strike. Under this perspective strikes arise from the wrong calculations of each part when measuring the other part intentions and not necessarily from the result of a negotiation between irrational parts. The idea that strike activity depends negatively on strike joint costs for the parts is at least economically intuitive. One disadvantage of this model is that it does not specify the underlying mechanism that produces the strike.

Lastly, asymetric information models are modern and more complex than their counterparts. The setup can vary, but in general these models are based in the same premise. They assume that a fraction of the benefits of the firm is unobservable to workers, so they must use strikes as a discrimination mechanism between firms. Firms can be in a good or bad situation but having incentives to declare themselves in a bad situation. The strike-wage scheme choosen by the union is such that good firms, unwilling to take a strike, will truthfully declare themselves in good situation, while bad firms will take the strike to demonstrate their true type. Despite these models can be more sophisticated and attractive because of their features, the asymetric information assumption can be questioned in the chilean case if we consider that chilean legislation guarantees workers the right of information. Periodically, employers must give to unions information about the balance sheet, the income statement and financially audited states of the firm, though this is true for big firms. For micro and medium firms, employers must inform unions only about the incomes and expenses of the firm. Anyway, the extent to which this background refutes the assumptions of these models is certainly unknown. It worths mentioning that both asymmertic information and joint costs models are non-mutually exclusive: the first preserves the latter hypothesis (Schnell and Gramm, 1994).

Respect to the Labor Reform of 2017, as previously mentioned, the law wanted to strenghen unions bargaining power. One of the ways this is accomplished is through enhancing the threat of the strike, i.e. increasing its associated costs for the employer. For instance, the replacements ban can be considered usually as an increase in joint costs of a strike. When the firm can hire replacements, some output can be produced during the conflict. Banning this possibility increases the output loss during labor unrest and, in consequence, the joint costs associated to a strike. Finally, this increase in joint costs has the effect of reducing strike activity. An opposing effect is mentioned by Campolieti et al. (2014) where replacement bans could reduce strikers costs associated to strikes because workers would have a lower probability of losing their jobs to replacements, so the effect of the bans could still be uncertain.

Despite this, Kennan and Wilson (1989) notes that asymmetric information models are capable to explain the paradoxical feature that strikes for US manufacturing last longer when bans of replacements are applied. The "no-scab" law tend to raise the uncertainity of the union about the firm reservation terms so the strikes duration will increase. The Cramton asymmetric model (Cramton et al., 1999; Cramton and Tracy, 1992) predicts the same results: strikers replacements prohibition enhances the use of strike and increases the uncertainity of unions about firms willingness to pay: the strike activity should increase. This means that can be argued, from a theoretical point of view, that the bans of replacement workers can have both positive and negative effects in strike activity.

It is also plausible to assume that firms with larger portion of unionized workers will be more vulnerable to the law since a bigger fraction of its workforce is supposedly empowered or, at least, a bigger portion of its labor-firm relationships or contracts are subjected to these new rules. This could mean, taking as an example the joint costs model framework, that the strike costs of the more unionized firms are higher (i.e. a larger fraction of output is lost in the case of a strike considering that strikers can not be replaced, something noted by Card and Olson (1995)) or the strike costs of their respective unions are lower (i.e. the number of jobs that can not be lost by replacements are higher).

Lastly, the joint costs model presents ambiguous hypothesis for the cyclical effects in strike activity. On the one hand, one should expect that strike activity is countercyclical because more "pie" is lost in the conflict. On the other hand, better labor market conditions facilitate workers to obtain alternative labor income sources during a strike, resulting in cyclical strike activity. Because of this, recent studies look up for controlling both labor market conditions and product market demand conditions (Card, 1990)).

3.2 Empirical review

Labor legislation effects in strike activity has been studied by Cramton et al. (1999) and Campolieti et al. (2014). Both studied a wide variety of laws related to collective bargaining as compulsory conciliation broads, cooling-off periods, mandatory strike vote, employer-initiated strike vote, compulsory dues check-off, reinstatement rights, negotiated contract reopener, technical change reopener and of course, bans on replacement workers. Both studies also use Canadian contract data to their purposes, but differ on the periods encompassed (Cramton et al. (1999) from 1967 to 1993 and Campolieti et al. (2014) from 1978 to 2008). As mentioned by the first, Canadian data is appropiate for studying labor legislation effects over strike activity because its labor law is determined at the province level, so this provincial level differences can be exploted. This is different for the chilean case, where these kind of laws are applied at the national level so determining its effects is a difficult endeavor, as Micco and Pagés (2006) state.

Cramton et al. (1999) found that unconditional strike duration increases in two weeks when there are replacement bans. They found no effect in strike incidence (probability that a negotiation ends in a strike). They also found a positive average effect of 4% in settlement wages. They explain the positive effects of bans in strike duration by their asymmetric information model: banning replacements increases uncertainity and maintains strikes costs high on the long run. One downside of the paper is that a subset of negotiations of 500 workers or more is used. Skeels et al. (1988) criticizes this kind of practices because found that smaller strikes can significally differ in behavior from bigger ones.

Campolieti et al. (2014) found that in general, labor legislation does not affect much strike incidence. In fact, only reinstatement rights and bans of replacements seem to have positive effects in strike duration and negative effects in wage settlements. They also found that this effects can vary in magnitude and sign when comparing before and after 1992. As the previous authors, they explain these positive effects on strike duration with the asymmetric information assumptions.

Lastly, while Schnell and Gramm (1994) did not study effects of legislation in strike activity, they actually studied effects of replacement strategies employed by employers during collective negotiations in strike duration of negotiations in United States between 1985 and 1989. In particullary, they looked up for the effects of both announcing permanent replacements of strikers and actually replacing them. Their results indicate that both actions were associated with an increase of strike duration. When employers actually permanently replaced strikers the probability of settlement fell about 30%. Their results are rationalized by both joint costs and asymmetric information models.

4 Methodology

4.1 Incidence model

Two dimensions are studied in this paper: incidence and duration. The data available does not make possible to study the probability that a negotiation ends in strike. Instead, the monthly-industry-regional number of strikes is used for incidence. The strike duration is the difference in days between the end and the beginning of a strike. These two kind of dependent variables have their own nature to be exploited with alternative estimation tools besides common linear models.

In the case of incidence, the monthly strikes for each industry-region pair represents count data (non-negative integers), so count modelsare going to be fitted (since the linear model is not adapted to explain how discretes variables depend on other explanatory variables as stated in Gourieroux et al. (1984)). In the first place, a Poisson model is going to be estimated. The basic Poisson model states that:

$$\Pr(Y = y_{i,r,k}) = \frac{e^{-\lambda_{i,r,k}} \lambda_{i,r,k}^{y_{i,r,k}}}{y_{i,r,k}!} \quad y_{i,r,k} = 0, 1, 2, 3, ... \tag{1}$$

Where $y_{i,r,t}$ is the number of strikes of industry i and region r in month k. To include explanatory variables is assumed that

$$E[y_{i,r,k}|x_{i,r,k}] = \lambda_{i,r,k} = e^{x'_{i,r,k}\beta} \tag{2}$$

So the logarithm of the conditional mean of the dependent variable is linear in the parameters. This means that coefficients are interpreted as the marginal constant proportional effect of the variable in the conditional mean of monthly-industry-regional strikes and the exponentiated coefficients are the conditional *mean-ratios*.

$$e^{\beta_j} = \frac{e^{(x_j+1)\beta_j + x'_{-j}\beta_{-j}}}{e^{x_j\beta_j + x'_{-j}\beta_{-j}}} = \frac{E[y_{i,r,k}|x_j + 1, x_{-j}]}{E[y_{i,r,k}|x_j, x_{-j}]} \tag{3}$$

The Poisson model is simple and relies on very strong assumptions, being equidispersion the principal one. Equidispersion means that $E(y_{i,r,k}|x_{i,r,k}) = V(y_{i,r,k}|x_{i,r,k}) = \lambda_{i,r,k}$. Count data, including the data described in the next section, usually is overdispersed so the variance exceeds the mean. This suggests that the specified density for the count data is erroneous. The maximum likelihood estimator of the parameters using a misspecified density is called pseudo-maximum likelihood estimator (PML). Despite this, the PML estimator will mantain its consistency if the density used is a member of a linear exponential functions family (Poisson distribution for instance) and provided that the conditional mean is correctly specified. It will be important in this case to correct standard errors for correct inference about the parameters (Cameron and Trivedi, 2013).

Another issue presented in the data is the overwhelming quantity of observations where no strikes ocurred on a specific industry-region pair in a particular month. In other words, there is an excess of zeros for the dependent variable. This raises a second problem because Poisson distribution alone can not explain this kind of observed distribution. Despite PML estimators for Poisson model will still being consistent and robust to missespecifications, it is true that their precision can be compromised (Staub and Winkelmann, 2012). With this in mind, a variation of the simple count model will be estimated, named zero-inflated model. It states that:

$$\Pr[Y = y_{i,r,k}] = \begin{cases} \pi + (1-\pi)f(0) & \text{if } y_{i,r,k} = 0 \\ (1-\pi)f(y_{i,r,k}) & \text{if } y_{i,r,k} > 0 \end{cases} \tag{4}$$

where $f(.)$ is a base density, in this case the Poisson density with parameters specified as in the simple case (henceforth the entire estimated model is going to be called ZI Poisson model). The only difference between the simple Poisson model and its ZI variant is that there is an *inflation* parameter π to discreetly increase the density mass of zero values. This new parameter is estimated jointly with the variable parameters[3].

For the last, despite not being a count model, a logit model can be estimated to measure the effects of variables in the probability of at least one strike in a particular month and industry-region pair:

$$\Pr(1\{y_{i,r,k} > 0\} = 1|x_{i,r,k}) = \text{logit}^{-1}(x'_{i,r,k}\beta) \tag{5}$$

where $logit(.)$ is the logit transformation. Note that while the interpretation of β in this last model changes respect to the count models, because here the coefficients are the constant proportional effect of the variables in the odd-ratios for the presence of at least a monthly strike, both approaches take into account alternative measures of strike incidence. The three models will be weighted by the monthly number of occupied workers in the region-industry to account for size differences of observations.

4.2 Duration model

For the duration data a linear model is estimated using the log duration as dependent variable (as with the count data duration distribution is skewed). The other usually used approach for this kind of data is estimating the proportional hazards model. Before get into it, an overview of survival anylisis will be described. Be T the duration of a strike which presents the distribution

$$F(t) = \Pr(T < t) \tag{6}$$

In survival analysis the survival function is defined as the probability that a strike lasts at least t days. So $S(t) = 1 - F(t) = \Pr(T \geq t)$. The hazard function is defined as

$$\lambda(t) = \frac{f(t)}{S(t)} \tag{7}$$

where $f(t)$ is the density function associated to $F(t)$. Then, the hazard function is the rate at which strikes end at time t, given that they lasted at least t. An increase (decrease) of this function for all t means that strikes will be shorter (longer) because at each moment the probability of settlement of a strike got higher (lower). The concept of positive duration dependence means that through time the probability of a strike end increases: $\frac{d\lambda(t)}{dt} > 0$. Likewise, a negative duration dependence means that as time passes the probability of settlement decreases: $\frac{d\lambda(t)}{dt} < 0$.

Since now, covariates have not been included in the procedure of estimating the distribution of duration data. In fact, there are two problems when introducing explanatory variables in the model. First, as opposed to classic linear regression models, there is no direct way to include explanatory variables in a

[3]With a logit link the inflation parameter can depend on explanatory variables. For simplicity and to secure the convergence of the likelihood function, it will be considered as a constant.

duration model (Kiefer, 1988). The proportional hazards model proposses that the covariates multiply the hazard function by a scalar factor. Then

$$\lambda(t_{n,i,k,r}, x_{n,i,k,r}, \beta, \lambda_0) = e^{x'_{n,i,k,r}\beta}\lambda_0(t_{n,i,k,r}) \tag{8}$$

for strike n, ocurred in industry i, in region r and at month k[4]. $\lambda_0(.)$ is a baseline hazard. The use of the exponential function to rescale the covariates helps to avoid non-negativity problems. Assuming a specific baseline hazard the model can be estimated using maximum likelihood estimation. But this raises the second problem: estimates will depend on the baseline hazard assumed. To avoid any result depending on parametric assumptions an alternative way of estimation can be used. In particular, estimates from the conditional maximum likelihood, or also called Cox regression (Cox, 1972), do not depend on λ_0. Then, this procedure is adopted for the proportional hazards model estimation.

The coefficients in this model are interpreted as the constant proportional effect of the variables on the conditional probability of ending a strike (Kiefer, 1988). Negative coefficients mean that the respective variables increase the duration of strike, while positive coeficients mean that the duration of strike decrease with the respective variables. Another usually used measures are the *hazard-ratios*:

$$e^{\beta_j} = \frac{e^{(x_j+1)\beta_j + x'_{-j}\beta_{-j}}}{e^{x_j\beta_j + x'_{-j}\beta_{-j}}} = \frac{\lambda(t, x_j+1, x_{-j}, \beta, \lambda_0)}{\lambda(t, x_j, x_{-j}, \beta, \lambda_0)} \tag{9}$$

so hazard-ratios are just the ratio between the hazard functions of two observations that only differ in the marginal increase of the covariable of interest. A hazard-ratio greater than 1 means that the variable has a negative effect in strike duration (positive effect in the hazard) and a hazard ratio lower than 1 means that the variable has a positive effect in strike duration (negative effect in the hazard).

4.3 Explanatory variables and Identification strategy

The approach to estimate the effects of the law in strike activity is a difference-in-differences one, like ones used by Card (1992), Micco and Pagés (2006) and Pedemonte (2019) where the treatment evaluated is not binary. Instead, the treatment is measured by the exposition of the groups to the treatment. In this case, the industry-region pairs which have a larger fraction of unionized workers at the moment of the law implementation are said to be more exposed to the negotiation changes introduced. In other words, they have a larger fraction of workers-employer relationships that are subjected to the new collective negotiation rules.

The count data consists in a balanced panel of 52 industry-region pairs through 120 months, starting from January 2010 and ending at December 2019. This makes for 40 available quarters. Following this insight, the variables included both in ZI Poisson and logit models are:

[4]The data does not have to be aggregated at the region-industry-month level in the duration model

$$x'_{k,r,t}\beta = \alpha_0 + \sum_{j=1}^{40} \beta_j UR_{k,r} * \mathbb{1}\{t \in Q_j\} + \phi_1 UOR_{r,t} + \phi_2 \ln F_{k,r,t} + \sum_{j=1}^{40} \gamma_j \mathbb{1}\{t \in Q_j\} + \delta_{k,r} \quad (10)$$

for industry k, region r and month t. UR is the industry-regional union rate (i.e. unionized workers divided by the number of workers for a industry-region pair) at the year of the law implementation (results using 2015, 2016 and 2017 rates are presented for robustness), introduced as percent. UOR is the monthly regional unoccupation rate (ratio between unoccupied workers and the sum of occupied and unoccupied workers) to control for cyclical effects, also introduced as percent. F is the anual number of firms for the industry-regional pair to control for the fact that the number of strikes can increase (decrease) by the increase (decrease) of the number of firms and not necessarily because of a more (less) conflictive labor environment. $\mathbb{1}\{t \in Q_j\}$ is an indicator of whenever the observation is in the quarter j. Also industry-regional control effects $\delta_{k,r}$ are included to complete the difference-in-difference approach.

The duration data consist of a data base where each observation correspond to a strike indexed by n. The variables included are similar to those in the count equation:

$$x'_{n,k,r,t}\beta = \sum_{j=1}^{40} \beta_j UR_{k,r} * \mathbb{1}\{t \in Q_j\} + \phi_1 UOR_{r,t} + \phi_2 \ln CW_n + \sum_{j=1}^{40} \gamma_j \mathbb{1}\{t \in Q_j\} + \delta_{k,r} \quad (11)$$

The new variable included CW is the number of committed workers in each strike to control for strikes size effects in the strike duration.

As said before, the main fact that justifies the identification strategy above is that industries with a larger portion of their workforce unionized are more exposed to the law. This helps to obtain estimates that exclude temporal effects that correlates with the implementation of the law (taking into account that labor legislation is implemented at a national level) and also excludes intrinsic differences between industry-region pairs. In the equation above, quarter 29 (January to March 2017) inmediately preceeds the law implementation (April 2017) and is took as the base trimester. β_j is interpreted as the difference between trimester j and 29, per each union rate percent. As mentioned previously, these differences are translated, in the incidence models, as proportional shifts in the conditional mean of the number of strikes and in the odd ratios for a probability of a strike, in the ZI Poisson model and logit model, respectively. For the duration models, they are interpreted as porcentual effects in the duration of strikes in the case of the linear model, and as proportional shifts in the hazard function in the case of the proportional hazards model.

Three other facts are important for the validity of this strategy. First, the Labor Reform must be the only significant change on legislation that affects industry-region pairs differently by their union rates and that at the same time affects strike activity, at least near the moment of application of the law. Otherwise, estimations may capture effects of these other changes besides the law. Secondly, and related with the latter, as in any differences-in-differences application the parallel trend assumption must be fullfilled. In terms of the equation, we must expect $\beta_j = 0$ for $j < 29$ (at least true for quarters inmediate preceding the implementation of the law).

Third, this strategy is ideal assuming that the treatment exposure (union rates in this case) keeps constant level both before and after the implementation of the law. This is reflected in the inclusion of UR corresponding one particular year in the equations above. For instance, if the law accomplished the strengthening of unions through the increasing of union rates (one of the reform objectives) and at the same time more unionized industry-region pairs are more exposed to the changes that the law introduced, this means that some observations were actually more exposed to the treatment than it is registered in the data. For these observations, part of the change in strike activity induced by the law is explained by the increased exposure, and presumibly the law effects estimations will be downward biased in terms of magnitude. While this issue is parcially approached using UR from three diferent years close to the reform implementation, in the next section it will be shown that union rates did not change significally through time.

Standard errors in both models are estimated by the clustered sandwich estimator at region-industry pair level. This allows for industry-region pair error correlation. It is probable that labor conflict correlates within industries and regions, due social and political contingency, local context, enviromental and technological changes, and other things that could be correlated inside these clusters and that are not included in the equations. While this error structure does not take into account cluster correlation in the count data (because the observational unit is the same industry-region pair level used for clustering) it also permits serial correlation for both duration and count models, because it is also plausible that strike activity in one period could be correlated with previous and subsequent periods (i.e. if in a collective bargaining there was a strike and it lasted long maybe in the next it wont be any strikes or it will last shorter to compensate the joint costs of the previous negotiation). Finally, is necessary robust standard errors for the correct inference in the Poisson model, as there exists some overdispersion in the count data.

5 Data

The main data used comes from the Statistic Data Base of Chilean Labor Strikes (2010-2019), constructed by the OHL. It contains a systematically registered list of strikes effected between 2010 and 2019, plus some other information obtained from a variety of sources, mainly national and regional newspapers and complemented with some administrative data from Internal Taxes Services (SII) and the Labour Direction (DT).

The variables used are strike duration, the year and month (of the beginning of the strike), the number of committed workers in the strike, the region and the industry of the firm. The base also presents a lot of variables not used here but their use could be interesting for another studies like the type of union involved, the firm size, the kind of institution where strike occurs, if there was mediation or compulsory conciliation, the labor days lost, the strike demands, the tactics implemented by strikers, if there was police pressence, among others.

There is another feature of the base that makes it unique: the fact that it registers not only legal conflicts but also illegal conflicts. The illegal conflicts, as can be seen in Figure 1, conform a big fraction of the full sample of strikes. An analysis of both legal and illegal strike activity can retrieve information about the different behavior of labor conflict by its legal status. However, this paper will only focus only on legal strikes. It may be implausible that the level of exposition of illegal conflicts can be measured by the union rates because these do not necessarily involve legal negotiations and so unions do not necessarily take part in the illegal negotiations.

Some complementary data is used from DT open data. Particularly, the number of unionized workers and number of occupied workers to generate the variable of union rates by industry-region. Also, the unemployed workers is used to generate unemployment rates by region when dividing it by the labor force (occupied plus unemployed workers).

In Table 1 some descriptive data is presented for the monthly number of strikes[5]. First, from the count data panel we can characterize some aspects of chilean strike incidence. When adding up the strikes to the month level, the monthly strikes before April 2017 takes the average of 13.57 strikes, while after the implementation of the law decreases to 9.79 strikes, implying that one could expect a negative effect of the Labor Reform in strike incidence. The median is lower than the mean, both before and after April 2017, which means that the distribution of monthly strikes is right-skewed. Also, the variance of the monthly strikes is higher that the mean, meaning that the count data is overdispersed. The overdispersion for the actual panel data used is lower due the high amount of zero observations that produces the disaggregation at the industry-regional level of monthly strikes. This supports the idea that the Poisson distribution probably is not the real distribution of the number of strikes, but as argumented before this does not put in danger the hypothesis testing made in the next section with the use of robust estimators for standard errors. In Figure 2 the distribution of the monthly strikes is presented. More than 80% of the industry-region pairs months had no strikes. It confirms the overwhelming number of zero observations of the sample, justifying the use of the ZI variant of the Poisson model.

Looking into the number of strikes through years, it can be confirmed the pattern of an incresingly

[5]The actually used count panel data is aggregated in different levels to generate the statistics presented in Table 1 due to the zero observations problem. The descriptive results for the actual panel used in the estimations are presented in Table 5 in the Appendix.

Table 1: Descriptive data for number of strikes

	Monthly strikes			
	Mean	Median	SD	Max
Law				
0	13.57	13	6.257	32
1	9.788	9	5.925	23
Year				
2010	10.92	10	4.209	17
2011	12.08	11.50	3.679	19
2012	12.17	10	5.906	22
2013	15.67	16.50	5.069	22
2014	15.67	16.50	8.038	30
2015	14.08	16.50	6.052	21
2016	16.25	14	8.114	32
2017	8.583	8.500	4.252	18
2018	7	6	5.576	17
2019	12.92	12.50	6.201	23
Industry				
Actividades inmobiliarias, empresariales y de alquiler	0.892	0	1.158	5
Agricultura, ganadería, silvicultura y pesca	0.342	0	0.510	2
Comercio al por mayor y al por menor	1.667	1	1.595	9
Construcción	0.675	0	1.030	6
Enseñanza	1.942	2	1.858	8
Explotación de minas y canteras	0.567	0	0.827	3
Hoteles y restaurantes	0.400	0	0.760	4
Industrias manufactureras	2.975	3	2.273	11
Intermediación financiera	0.200	0	0.512	3
Otras actividades de servicio comunitario, sociales y personales	0.558	0	0.807	3
Servicios sociales y de salud	0.792	0	1.020	4
Suministro de electricidad, gas y agua	0.100	0	0.328	2
Transporte, almacenamiento y comunicaciones	1.425	1	1.382	7
Region				
Norte	1.608	1	1.552	7
Centro	2.717	2	2.320	14
Sur	1.067	1	1.067	5
RM	7.142	7	4.435	22

Figure 2: Distribution of monthly strikes

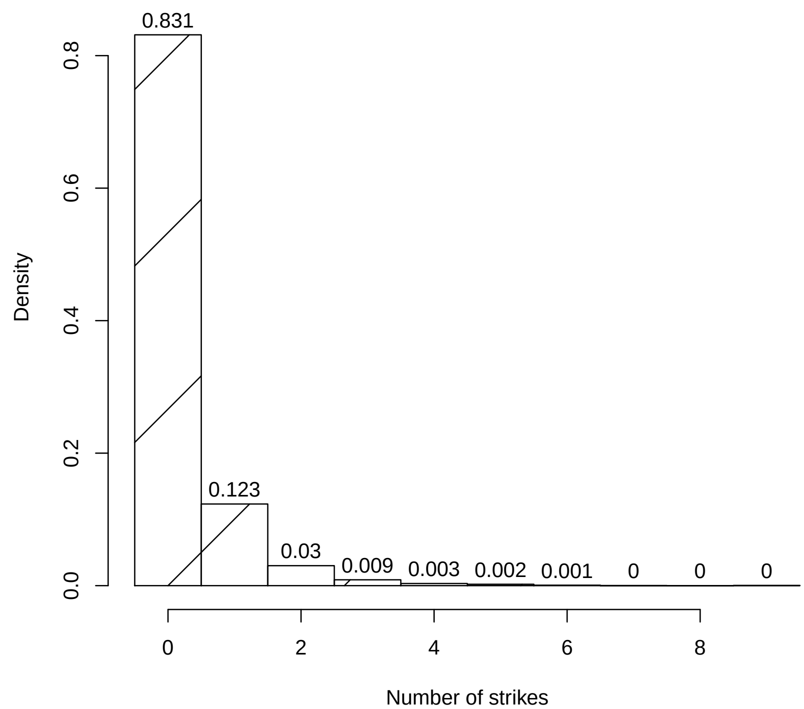
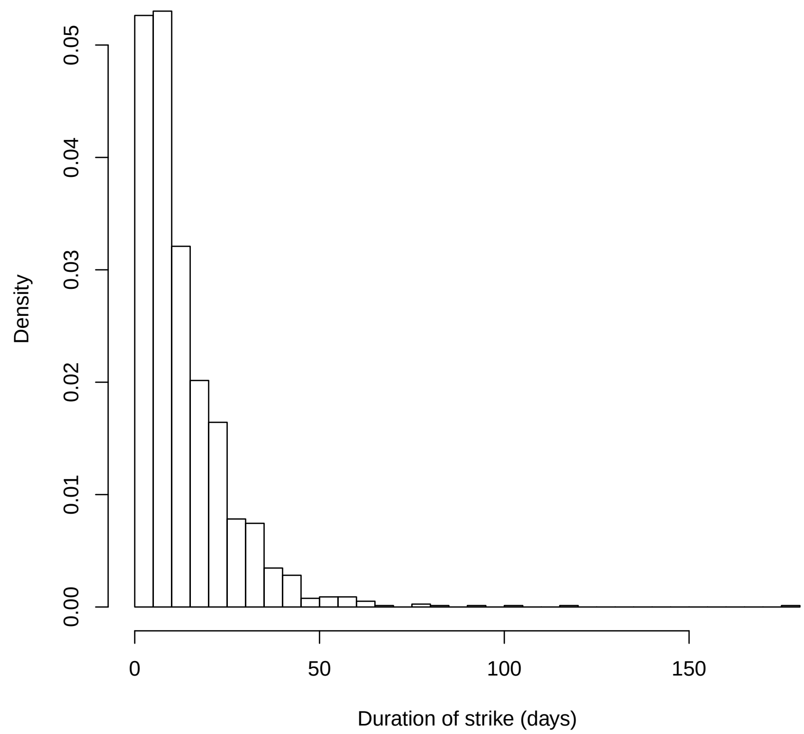


Figure 3: Distribution of strike duration



labor unrest through 2010 and 2016, reaching in the latter its peak of 16.25 average monthly strikes, and suddenly decreasing to 7 average strikes each month at 2018. In 2019 the strikes number average increases to 12.92, meaning some recovery of the previous pattern. This could mean that the effects of the law were transitory.

Adding up to the monthly industrial level, the manufacturing sector presents the higher average number of monthly strikes, reaching 3 strikes per month, followed by the education industry with 2 strikes per month. The industries that present the lowest average number of monthly strikes are the water, gas and electricity supply industry; and the finance industry with 0.1 and 0.2 monthly strikes, respectively. Adding up to the monthly regional level, the region that presents the highest average number of monthly strikes by far is the Región Metropolitana, with 7 strikes per month (it is followed by the Centro with 2.7 strikes per month), probably due its size and density in comparison with the other regions. Conversely, Sur presents the lowest average monthly of 1 strike.

Second, the duration data presented in Table 2 indicates that strikes used to be shorter before the im-

Table 2: Descriptive data for duration of strikes

	Duration of strikes (days)				
	N	Mean	Median	SD	Max
Law					
0	1168	13.19	10	12.81	179
1	315	15.04	11	12.75	105
Year					
2010	129	13.23	10	11.69	60
2011	144	12.50	9	11.20	68
2012	145	11.65	8	13.19	93
2013	187	13.70	9	12.34	82
2014	188	11.80	9	9.154	42
2015	164	15.09	12	13.64	117
2016	192	13.34	9	16.30	179
2017	101	16.77	12	12.60	78
2018	82	15.76	12.50	13.06	63
2019	151	14.23	10	12.91	105
Industry					
Actividades inmobiliarias, empresariales y de alquiler	104	13.56	9.500	14.06	93
Agricultura, ganadería, silvicultura y pesca	40	13.80	12	10.72	46
Comercio al por mayor y al por menor	199	13.84	9	12.83	82
Construcción	80	12.23	9	14.62	117
Enseñanza	230	13.36	11	10.63	78
Explotación de minas y canteras	67	15.40	10	15.80	68
Hoteles y restaurantes	48	14.52	10	12.01	54
Industrias manufactureras	354	14.56	10	14.44	179
Intermediación financiera	24	13	8.500	11.96	54
Otras actividades de servicio comunitario, sociales y personales	66	12.05	10	9.181	36
Servicios sociales y de salud	91	14.73	10	11.98	63
Suministro de electricidad, gas y agua	12	12.58	11.50	9.728	31
Transporte, almacenamiento y comunicaciones	168	11.31	9	11.39	105
Region					
Norte	188	15.05	11	13.49	78
Centro	323	13.82	10	13.24	93
Sur	122	12.33	9	8.942	43
RM	850	13.36	10	12.96	179

plementation of the law, with an average of 13 days. After April 2017 this mean increases to 15, more than two weeks. Contrary to the monthly number of strikes, average duration of strikes does not seem to have a yearly pattern. It reaches its peak in 2017 and 2015, with almost 17 and 15 days, respectively. In 2012 and 2014 the average duration reaches its lowest values, almost reaching 12 days in both years. As in count data, duration data is overdispersed, with a higher variance than its average. Also, median is lower than mean, meaning that the duration distribution is right-skewed. This is also confirmed in Figure 3. This justifies the using of logarithm tranformation when introducing it as dependent variable in the linear duration model.

By industry, it can be seen slight differences in the average duration of strikes. Mining industry presents the highest duration mean of 15 days, followed by social and health services with 14.7 days. The lowest duration mean is in transportation, storage and communications industry, with 11 days followed by other activities industry. By region, Norte presents the highest average duration of 15 days followed by Centro with 13.82 days average. In the other hand, Sur and RM present the lowest average strike duration of 12.33 and 13.36 days, respectively.

Continuing the duration analysis, other possible exercise is plotting the survival function. The Kaplan-Meier estimator (Kaplan and Meier, 1958) can be very helpful for this purpose. Figure 4 plots it for both subsets of strikes before and after April 2017. It can be seen that both curves are almost identical, with the post-implementation curve slightly above from its counterpart. If this difference is important, it could mean that the law had a positive effect for the duration of strikes because strikes after implementation had a higher probability of lasting more. The log-rank test (Kalbfleisch and Prentice, 2002; Savage, 1956) evaluates the nule hypothesis of equivalent supervival functions for two subsamples. The χ^2_1 statistic obtained for the test that the supervival functions of pre and post law strikes are the same is 15.73 with a p-value 0.0001. So it can be rejected the hypotesis that both aggregated hazard functions plotted in Figure 4 are identical. While this initially supports the idea that the law had an effect in strikes duration, it could also be true that the changes betwen both curves are induced by temporal effects that nothing had to do with the law.

Lastly, some added descriptive data is presented in Table 3 for union rates at industrial and regional level. As said in the previous section, the union rates from 2015, 2016 and 2017 are going to be used, as they represent the most inmediate periods from the law implementation. Despites this, as can be seen in the table, the rates do not vary significantly through years. Finance and mining industries are the most unionized ones with 33% and 29% rates respectively in 2017, while real estate, business and rental activities, and agriculture, forestry and fishing are those less unionized, with rates of 10.27% and 10.75%, respectively, in 2017. The RM is the region with higher union rate in 2017 at 17.5%, followed by Norte, with 16.46%. Sur and Centro have the lower union rates of 9.7% and 11.8%, respectively.

Figure 4: Kaplan-Meier estimator for acumulated hazard function of strike duration

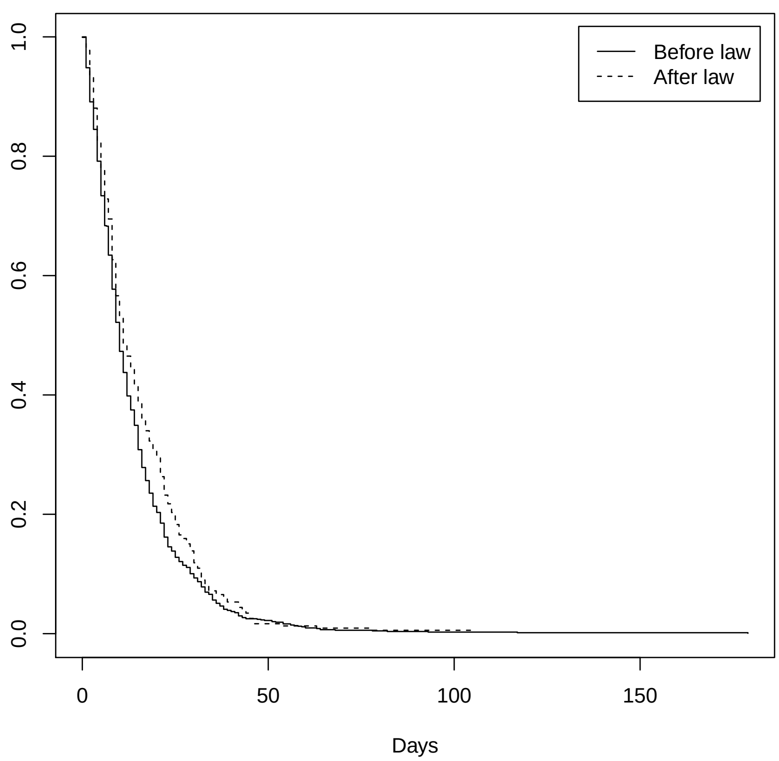
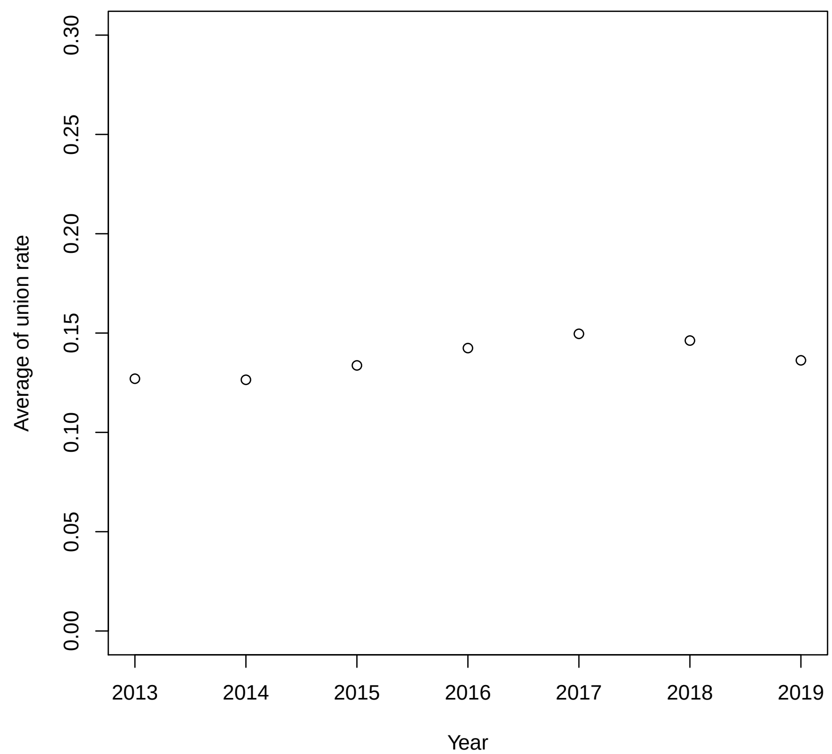


Table 3: Descriptive data

	Union rates (%)		
	2015	2016	2017
Industry			
Actividades inmobiliarias, empresariales y de alquiler	11.71	10.78	10.27
Agricultura, ganadería, silvicultura y pesca	9.650	10.51	10.75
Comercio al por mayor y al por menor	14.26	13.81	13.37
Construcción	6.767	11.56	11.35
Enseñanza	9.633	10.51	10.80
Explotación de minas y canteras	22.29	26.16	29.38
Hoteles y restaurantes	10.61	10.10	11.29
Industrias manufactureras	13.95	14.52	14.55
Intermediación financiera	21.34	30.34	33.20
Otras actividades de servicio comunitario, sociales y personales	24.37	24.90	23.92
Servicios sociales y de salud	9.490	10.96	11.91
Suministro de electricidad, gas y agua	14.66	14.59	15.01
Transporte, almacenamiento y comunicaciones	21.88	22.26	21.77
Region			
Norte	15.97	16.22	16.46
Centro	10.97	11.46	11.83
Sur	9.557	9.280	9.706
RM	16.13	17.88	17.53

Figure 5: Yearly evolution of average union rate



The potential identification issue of moving union rates through the implementation of the law is not really a problem for the strike data. In Figure 5 the yearly average union rate for industry-region pairs is displayed. Two things are worth mention. First, despite the flat shape of the plot, is true that in 2017 is the peak of unionization, what could indicate a positive effect of the reform in union rates. But it is also true that the increasing trend observed for the average union rate starts before the labor reform, since 2014. So it is hard to attribute this increase to the reform. Second, this increase is hardly significant, with a change of less than 2.5% in the union rates between 2015 and 2017. It is unlikely that this changes could affect the identification of the differences in differences setup. Anyway, this is one of the reason because three years for the union rates are used. The results are almost identical.

Reinforcing this argument, results for a simple linear regression with the yearly union rate as dependent variables over year effects are presented in Table 4, using 2016 as base year (with clustered standard errors at the region-industry level). The results reflect the increasing trend from previous years. There is a difference between 2013 and 2014 of 1.5%, while from 2015 this difference is almost 1%. Forward, in

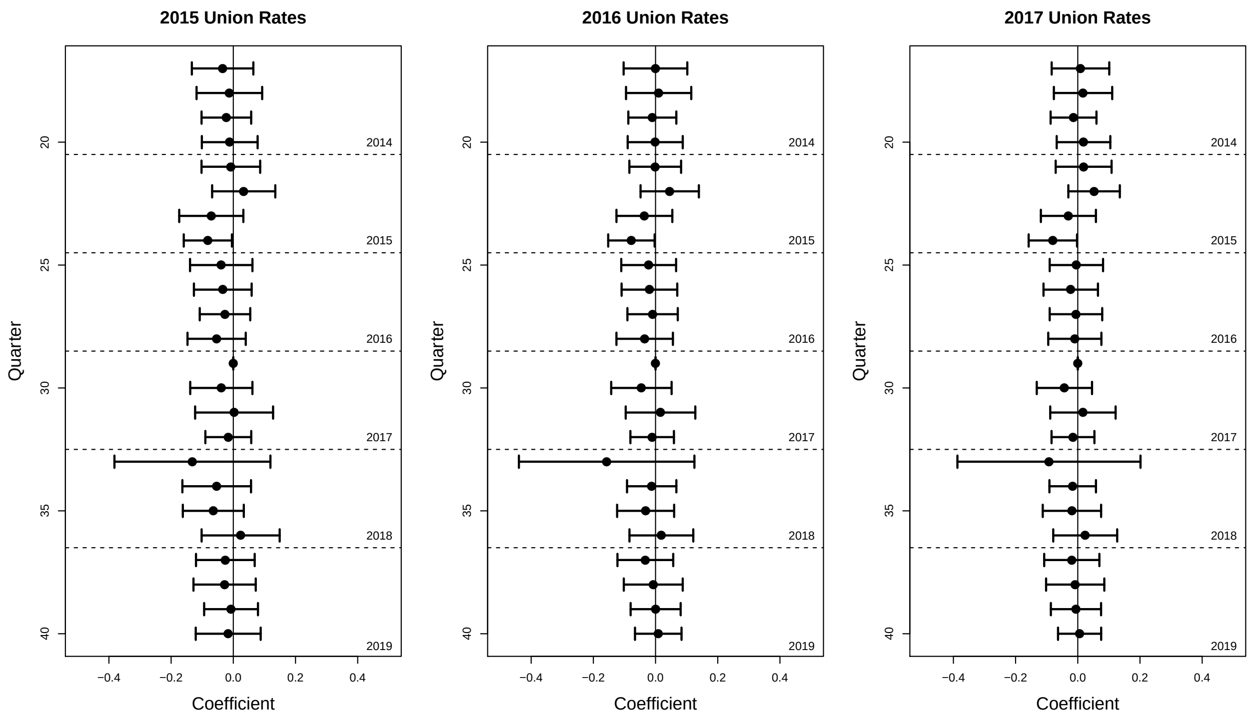
Table 4: Regression results for reform effects in region-industry yearly union rates

	Union rate		
	(1)	(2)	(3)
2013	-0.015***	-0.015***	-0.015***
	(0.005)	(0.005)	(0.005)
2014	-0.016***	-0.016***	-0.016***
	(0.004)	(0.004)	(0.005)
2015	-0.009**	-0.009**	-0.009**
	(0.004)	(0.004)	(0.004)
2017	0.007*	0.007*	0.007*
	(0.004)	(0.004)	(0.004)
2018	0.004	0.004	0.004
	(0.006)	(0.006)	(0.006)
2019	-0.006	-0.006	-0.006
	(0.006)	(0.006)	(0.007)
Fixed effects	No	Industry and region	Industry-region pairs
BIC	-807.230	-963.700	-1631.984
R^2	0.012	0.496	0.896
F	4.863	9.075	.
p value	0.001	0.000	0.002
N	364	364	364

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 6: Reform effect coefficients in Poisson model for strike incidence



2017 the increase in union rates is less than 1%, and barely significant. 2018 and 2019 do not present significant differences from 2016. The results support the idea that union rates have not changed significally in years near the reform implementarion.

6 Results

6.1 Incidence results

The complete results for the incidence models are presented in Tables 10 and 11 in the Appendix[6]. In those tables main control variables coefficients are shown too. In Figures 6, 7 and 8 the reform effect coefficients are plotted with their respective 95% confidence intervals for Poisson, Poisson ZI and Logit models, respectively. For simplicity, the coefficients plotted are those between years 2014 and 2019 (between quarters 16 and 40). Recall that quarter 29 is the base quarter, just before the law implementation. For each figure, three plots are displayed that differ by the year of the union rates used (2015, 2016 and 2017).

The three models indicate that the labor reform did not have a significant effect in the monthly number of strikes. This can be seen as the coefficients after quarter 29 are not significant. Coefficients are

[6]The results for unweighted models are presented in the Appendix too.

Figure 7: Reform effect coefficients in Poisson ZI model for strike incidence

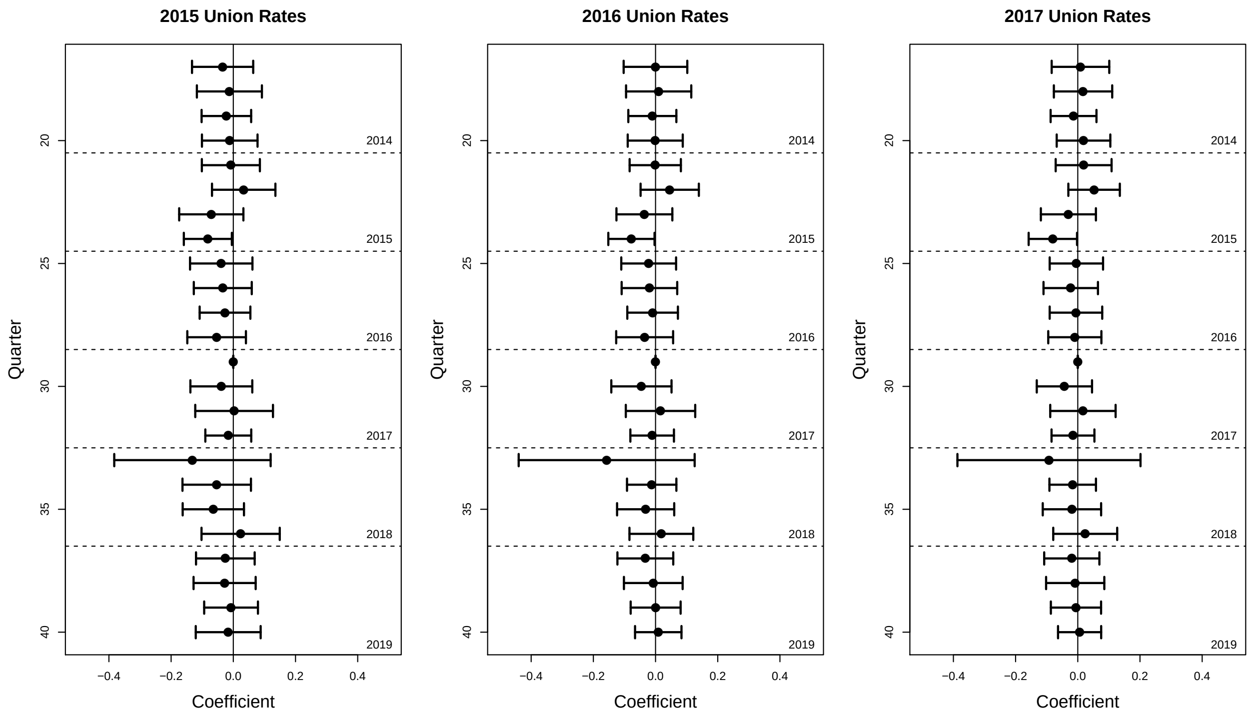
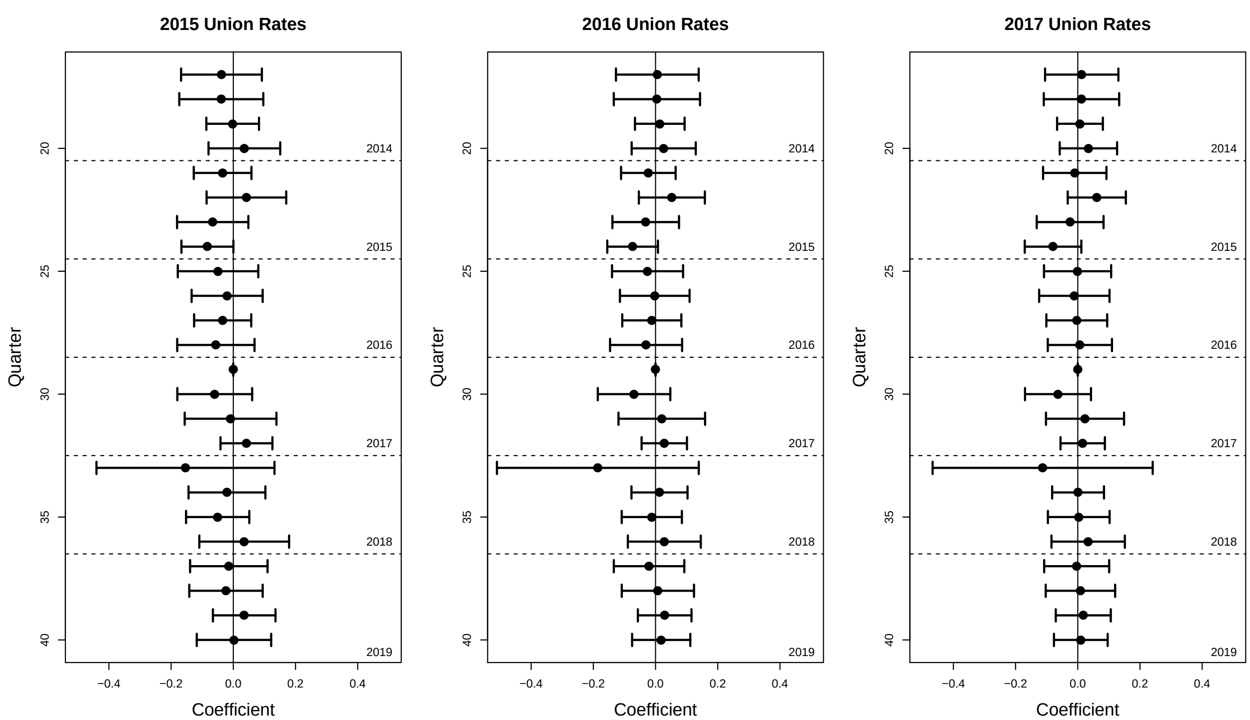


Figure 8: Reform effect coefficients in Logit model for strike incidence



negative for periods after the implementation of the law, especially in quarter 33, where its coefficient values reach levels over 15% in magnitude, but also its standard errors are higher than usual, neglecting their significance.

One interesting thing is that coefficients from both standard Poisson and ZI Poisson are almost identical, indicating the robustness for the estimations. ZI Poisson coefficients are slightly more precise and lower in general respect to the normal Poisson counterparts. Actually, the inflation parameter π from ZI Poisson model is estimated to be a little higher than 4%, but no statistically different from 0. This provides some evidence to the fact that it is not important to choose between the standard Poisson and the ZI Poisson models.

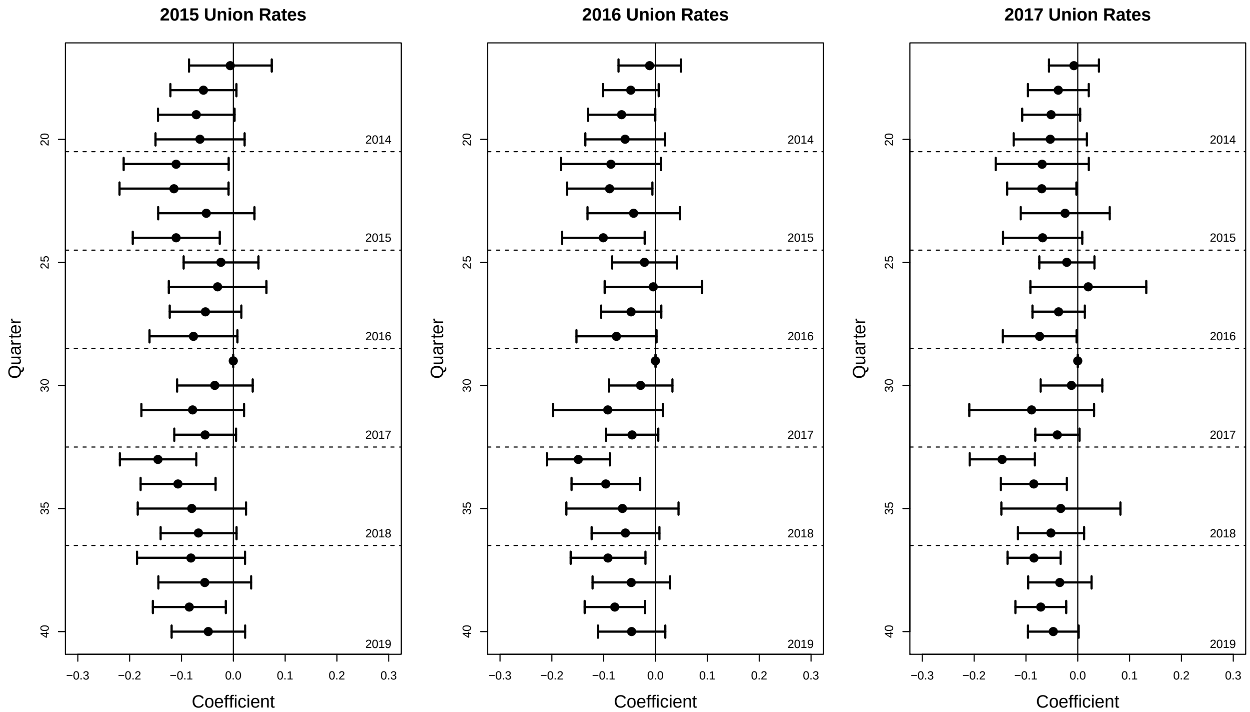
There are two ways to explain the results found for strike incidence. The first way is to assume that the reform did effectively raise union strenghethening and strike costs and uncertainity by the replacements bans, but its predicted effects are not observed in chilean strike activity because the theoretical framework of strikes is partially wrong, or at least can not be applied totally to the chilean case. As theoretical models take into account only economic variables and determinants, maybe there are other social, political and institutional factors that enter into play.

The other way to explain the fact that the law did not have effects in strike incidence is assuming that the reform did not accomplish its intended objective. The strengthening of strikes was not accomplished through the replacement bans because it was also limited, for example, by the minimum services which reduce the strike costs for the firm and neglects the higher costs of the absense of production. This goes in line with the criticism that the reform received because it did not present bigger changes in labor institutional framework, especially referred to collective bargainings.

Looking into the covariates, the number of yearly industry-regional firms does not have an effect in the monthly number of strikes. In the other hand, unemployment rate does have a significant negative effect of reducing in 18.5% the average mean by each increase of 1% unemployment rate, in the ZI Poisson model. The effect in the logit model is about 20% decrease in the odd ratios by an increase of 1% in the unemployment rate, reducing this way the probability of a monthly strike. Higher unenmployment reducing strike incidence could be explained by the joint costs model: the possibility of job loss increase and so the workers strike costs. This effect predomines over the countercyclical effect mentioned in the theoretical review section, i.e. the strike activity could increase with unemployment because the "pie" to share is lower.

One last concern is about in what extent the parallel trends assumption is satisfied. From base quarter 29, in the previous period of 3 years only in quarter 24 a significant shift is observed. This can not be

Figure 9: Reform effect coefficients in Linear model in strike duration



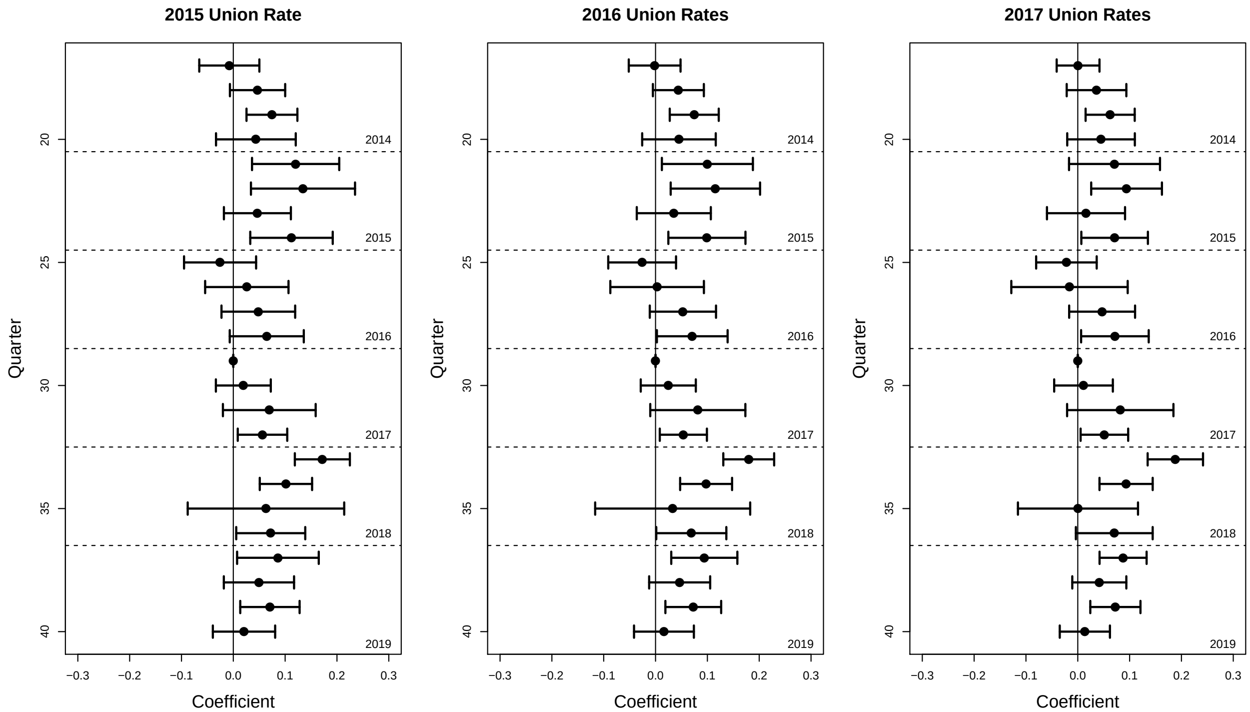
considered as consistent evidence that refutes the parallel trends assumption. In the first years of the study some more significant quarters can be observed, particularly quarters 5, 14, 15 and 16. The latter three are interesting because coincide with the presidential transition between Sebastian Piñera and Michelle Bachelet first and second governments, respectively, when the 2013 ended (the oficial change was in March 2014). Is hard to believe that this shift compromises the parallel trends assumption since the year distance is large.

6.2 Duration results

The complete results for the duration models are presented in Tables 8 and 9 in the Appendix. In Figures 9 and 10 the coefficients are plotted in a similar way as in count models results. Recall the fact that the sign of coefficients between the two models will oppose each other because in proportional hazards model a positive coefficient implies a higher hazard function, a higher conditional probability of strike settlement and finally a negative effect in duration.

The results indicate that the law had a negative effect in strikes duration, but this effect seem to be limited in magnitude. The decrease in strike duration ocurred one year later from the implementation, as it starts at the first two quarters of 2018. Also, there is some decrease in strike duration also during 2019, in first and third quarters. This second effect is lower than the first one.

Figure 10: Reform effect coefficients in Proportional Hazards Model in strike duration



In the linear model the law seems to have a negative effect around 15% in strike duration per 1% union rate exactly at the first quarter in 2018. This effect decrease to 10% in the second quarter of the same year. The decrease returns in quarter 37 and 39, with differences around 8% respect to the base quarter. Similar numbers are found in the proportional hazards model, where in the first quarter of 2018 there is a positive effect of 18% in the hazards function per union rate percent, which transaltes in lower strikes duration. This differences respect to the base quarter decrease but keep significant in quarters 36 and 37, with values around 7% and 9%, respectively, and also in quarter 39 with a decrease of 7%. The timing of the effects are similar in both models. Some perduration in strike duration is observed, which can be an argument for the idea that the law effects were not transitory, as OHL 2018 report stated.

Just before the base quarter is observed some negative and union rate-dependant shift of strike duration in the form of a negative 7.5% in linear model and positive 7% in proportional hazards model per union rate. This can endanger the indentification strategy or also can be the result of an anticipation of the agents to the changes of the law. Contrary to the incidence models, more previous shifts are observed before the base quarter. Particularly, in quarters 24, 22, 21 and 19 are the most close to the implementation date. Anyways, the frequency of shifts before the base quarter seems to have increased after the labor reform implementation, so is still plausible that the law, in some degree, had a significant negative effect in strike duration.

These results can partially explained by the joint costs model, which states that the higher costs for the firm provoqued by the no replacements law and the strengthening of unions incentive the bargaining parts to make a faster settlement. It also seems that the asymmetric information models hypothesis of the effects of no-scab laws, which predict positive effects in strike duration, have not a big role, so maybe the information rights for the union neglect information asymmetries. If the negative effects in strike duration are significant, this means that the minimum services legal figure was not enough to neglect the strengthening of strikes. This way, the most plausible option is that the joint costs model hypothesis enter into play between strike activity and replacement bans laws, but in an incomplete way, as the law had no effect in incidence and a low effect in strike duration.

From the control variables is observed that neither the unemployment rate nor the comitted workers have an effect in strike duration. Taking into account the results for the incidence models, this means that while higher joint costs of strikes from the law means that negotiation will less often end in a labor unrest, they do not affect the duration of strikes. Some studies like Schnell and Gramm (1994) and Campolieti et al. (2005) also found no cyclical effects in strike duration.

The fact that the number of comitted workers in the strike has no influence in the duration of it can be explained by the fact that it could not be a precise measure of the bargaining unit size in the negotiation. Usually, the bargaining unit size is included in estimations to control for size effects. The hypothesis that bigger unit sizes have worse communication with their employers than smaller ones, so end up with more conflicts in negotiations (Schnell and Gramm, 1994; Siebert and Addison, 1981) is usually confirmed by some studies, like Card and Olson (1995); Schnell and Gramm (1994), where bigger bargaining units usually have longer conflicts. The restults obtained for the chilean case do contradict this theory.

7 Conclusion

In this paper the effects of the chilean Labor Reform of 2017 in strike activity are examined. This law changed some collective negotiation rules, being the most important change the incorporation of replacement bans. This means that employers can not replace strikers during labor unrest. The reform had as objective the collective negotiations modernization through the union bargaining power strengthening.

Joint costs model is one of the main theoritecal point of views of strike activity. It says that strike activity will depend inversely to the joint costs associated to the strike. Both workers and employers costs jointly explain the fluctuations in strike activity. While the objectives of the law could be achieved by decreasing workers costs or increasing firms costs of a strike, the latter option is more plausible to be done through replacement bans, because firms can not maintain production with replacement workers during a strike and then its costs increase. So, one first hypothesis is that the Labor Reform of 2017 should have reduced strike activity. The second hypothesis, from assymetric information models, says that strike activity should increase with the replacement bans because of the increase in the uncertainity of unions about the reservation terms of the firm.

The main source of data is the strikes data constructed by the OHL. This data base registers information for each strike made in Chile through 2010 and 2019. Relevant information of strikes is obtained from there, like duration, beginning date, industry, region, etc. Also information about illegal conflicts is registered, but not used for these purposes. Some labor statistics also are extracted for open data sources of DT, as occupation and unemployment.

Strike activity is measured in two main dimensions: incidence and duration. First, for the incidence dimension, the monthly number of strikes is inputed for each industry-region pair, resulting this way in a panel for each pair through 2010 to 2019. A standard Poisson model, a ZI Poisson count model and a logit model for the probability of a monthly strike are estimated using this data. Secondly, for the duration data, both standard linear model with log-transformed duration and proportional hazards model are estimated. The identification strategy used here is a difference in difference specification. The fact that more unionized industry-region pairs are more exposed to the law changes is taken advantage of, interpreting the law in a non-binary treatment fashion. Also some relevant controls are included in equations, like the unemployment rate.

The results indicate that the Labor Reform could had some negative effect in strike duration and no effect at all in strike incidence. This partially suggests the theory that the law increased the strike costs of the firms, then reducing the duration of strikes. The effects in strike duration seems to have some delay, because they materialized during 2018. This can be explained by collective negotiation cycles that could last around 2 and 3 years.

While some progress is made here in the attempt of estimating country-level labor legislation effects in strike activity, some pending issues are mentioned here. First, the dimension of incidence usually is studied as the probability of a negotiation ending in a strike. This takes advantage of micro level information and means that information at the negotiation level is needed. OHL data unfortunately have information of strikes, but no information about collective negotiations that do not end in strike is provided. Anyway, the administrative data exists so the incidence analysis can be improved.

Second, two dimensions of strike activity have been studied: incidence and duration. Actually there is a third one that was not included in this study: wages. Many studies include wages analysis because it can be considered the strike outcome. Theoretical models usually include wage predictions, so providing evidence on this topic can be very useful. However, the available data does not include wage variables.

Third, various studies estimations take into account previous negotiation outcomes effects in actual negotiation effects. Some variables as previous contract duration, strike in previous negotiation and real wage changes during previous contract are usually the most used. For example, one interesting issue is the state dependence of strikes: does a previous strike affect the strike probability in the actual negotiation? As said before, collective negotiation data is needed to be able of controlling for this issues and could be helpful to improve the estimations for the law effects.

Fourth, in some cases can be interesting focusing in the issue of the "winner" in a negotiation. As said through the paper, the objective of the Labor Reform of 2017 was to give workers more bargaining power during the collective negotiations. But did the law actually induced, for example, more negotiations where workers "won"? Assessing this is hard to do because it requires very specific information and is not always clear how to define the "winner". For example, Card and Olson (1995) built an attrition model taking advantage of the fact that american strikes in 1880s had an "winner-take-all" nature. This means that strikes where workers won discrete wages increases were observed. Is unlikely that this feature replicates in chilean data.

Lastly, it is worth recognizing that the plausibility of evaluating this Labor Reform could be compromised if one thinks about taking more post-treatment periods. In October 19th in 2019 occurred the so-called *social outbreak*, that ended in unrest and a big number of riots throughout Chile, especially in the capital Santiago, and that it is said that also caused some lasting economic effects. Likewise, the COVID pandemic arrived to Chile in March of the next year that paralyzed economic activity worldwide and, to the day that this thesis is written, its presence is still affecting the day to day in the country. These two events make implausible to make inference about effects of the Labor Reform of 2017 in strike activity from 2020 onwards with the same methodology used here without incurring on some kind of bias. Despite the effects of the social outbreak and the pandemic in labor conflict could be interesting to evaluate in the future, they are not among the purporses of this work. Focusing in the effects of the law, it will be needed the course of some more years without bigger changes in collective bargaining legislation to being able of retake this research. Nevertheless, this seems to be an adequate way to start.

8 Appendix

8.1 Descriptive data for data panel

As mentioned in the data section, the descriptive data provided there for the count data was aggregated due the zero observations problem. In Table 5 is the same table, including a column for the number of observations and the rest descriptive variables without aggregating the number of strikes in any dimension. There is also a column indicating the fraction of observations with a number of strikes value different from zero.

Table 5: Descriptive data for number of strikes in the panel data

	Monthly number of strikes				
	N	Mean	Sd	Max	Perc $\neq 0$
Law					
Pre	4524	0.261	0.692	9	17.772%
Post	1716	0.188	0.529	7	14.452%
Total	6240	0.241	0.652	9	16.859%
Year					
2010	624	0.210	0.667	9	14.423%
2011	624	0.232	0.574	4	18.269%
2012	624	0.234	0.575	5	17.308%
2013	624	0.301	0.727	6	20.192%
2014	624	0.301	0.849	9	17.628%
2015	624	0.271	0.695	6	18.109%
2016	624	0.313	0.769	6	20.192%
2017	624	0.165	0.487	4	12.821%
2018	624	0.135	0.398	3	11.699%
2019	624	0.248	0.634	7	17.949%
Total	6240	0.241	0.652	9	16.859%
Region					
Norte	1560	0.124	0.375	2	10.769%
Centro	1560	0.209	0.521	6	16.923%
Sur	1560	0.082	0.299	2	7.500%
RM	1560	0.549	1.032	9	32.244%
Total	6240	0.241	0.652	9	16.859%
Industry					
Actividades inmobiliarias, empresariales y de alquiler	480	0.223	0.558	4	16.250%
Agricultura, ganadería, silvicultura y pesca	480	0.085	0.280	1	8.542%
Comercio al por mayor y al por menor	480	0.417	0.863	9	27.500%
Construcción	480	0.169	0.446	3	14.375%
Enseñanza	480	0.485	0.850	5	32.500%
Explotación de minas y canteras	480	0.142	0.434	3	11.042%
Hoteles y restaurantes	480	0.100	0.333	2	8.958%
Industrias manufactureras	480	0.744	1.292	9	38.542%
Intermediación financiera	480	0.050	0.262	3	4.167%
Otras actividades de servicio comunitario, sociales y personales	480	0.140	0.387	3	12.708%
Servicios sociales y de salud	480	0.198	0.533	4	15.208%
Suministro de electricidad, gas y agua	480	0.025	0.156	1	2.500%
Transporte, almacenamiento y comunicaciones	480	0.356	0.690	6	26.875%
Total	6240	0.241	0.652	9	16.859%

8.2 Tables of regressions

The main results for the monthly number of strikes models are presented in Tables 10 and 11. Estimations of the main β coefficients for the law effects are presented. Main control variables coefficients are shown too. The first three columns (1)-(3) present the results for the Poisson model, columns (4)-(6) presents the results for the ZI Poisson model and the last three columns (7)-(9) present the results for the Logit model. Columns of each model differs from each other on the union rate year used, using values from 2015, 2016 and 2017 for robustness.

In Tables 8 and 9 the results for the duration models are presented in a similar way as in count models results. Columns (1)-(3) presents linear model results while columns (4)-(6) presents proportional hazards model results. Recall the fact that the sign of coefficients between the two models will oppose each other because in proportional hazards model a positive coefficient implies a higher hazard function, a higher conditional probability of strike settlement and finally a negative effect in duration.

Table 6: Monthly number of strikes results

	Monthly number of strikes								
	Poisson			*ZI Poisson*			*Logit model*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\ln NFIRMS$	-0.095	-0.097	-0.095	-0.095	-0.098	-0.095	-0.103	-0.109	-0.107
	(0.091)	(0.087)	(0.091)	(0.092)	(0.088)	(0.091)	(0.082)	(0.078)	(0.084)
$UNEMP$	-0.237***	-0.246***	-0.240***	-0.237***	-0.245***	-0.240***	-0.284***	-0.299***	-0.293***
	(0.051)	(0.051)	(0.053)	(0.056)	(0.056)	(0.053)	(0.099)	(0.102)	(0.105)
Quarter 1	0.011	0.029	0.033	0.011	0.029	0.033	0.044	0.058	0.059
	(0.064)	(0.060)	(0.054)	(0.064)	(0.060)	(0.054)	(0.086)	(0.073)	(0.063)
Quarter 2	-0.043	-0.017	-0.004	-0.043	-0.017	-0.004	-0.087	-0.044	-0.024
	(0.068)	(0.053)	(0.044)	(0.068)	(0.052)	(0.044)	(0.072)	(0.058)	(0.047)
Quarter 3	0.000	0.009	0.013	0.000	0.009	0.013	-0.014	0.000	-0.004
	(0.046)	(0.044)	(0.039)	(0.046)	(0.044)	(0.039)	(0.051)	(0.047)	(0.041)
Quarter 4	0.009	0.010	0.018	0.009	0.010	0.018	0.047	0.034	0.042
	(0.050)	(0.048)	(0.042)	(0.050)	(0.048)	(0.042)	(0.075)	(0.063)	(0.051)
Quarter 5	-0.110***	-0.093***	-0.097***	-0.110**	-0.093***	-0.097***	-0.088**	-0.096**	-0.107***
	(0.042)	(0.035)	(0.036)	(0.043)	(0.035)	(0.036)	(0.037)	(0.038)	(0.040)
Quarter 6	-0.022	-0.003	0.020	-0.022	-0.003	0.020	-0.000	0.013	0.032
	(0.057)	(0.056)	(0.051)	(0.057)	(0.056)	(0.051)	(0.075)	(0.068)	(0.056)
Quarter 7	-0.022	0.006	0.005	-0.022	0.006	0.005	0.002	0.034	0.027
	(0.053)	(0.058)	(0.059)	(0.053)	(0.058)	(0.059)	(0.069)	(0.061)	(0.056)
Quarter 8	-0.069	-0.060	-0.049	-0.069	-0.060	-0.049	-0.090	-0.083	-0.072
	(0.048)	(0.048)	(0.044)	(0.047)	(0.048)	(0.044)	(0.062)	(0.060)	(0.056)
Quarter 9	-0.067	-0.059	-0.050	-0.067	-0.059	-0.050	-0.055	-0.044	-0.033
	(0.043)	(0.047)	(0.056)	(0.044)	(0.048)	(0.056)	(0.051)	(0.053)	(0.063)
Quarter 10	-0.051	-0.044	-0.044	-0.051	-0.044	-0.044	-0.053	-0.045	-0.049
	(0.048)	(0.046)	(0.041)	(0.048)	(0.046)	(0.041)	(0.059)	(0.057)	(0.049)
Quarter 11	-0.030	-0.011	0.004	-0.030	-0.011	0.004	-0.015	0.005	0.022
	(0.045)	(0.044)	(0.045)	(0.045)	(0.044)	(0.045)	(0.053)	(0.051)	(0.055)
Quarter 12	-0.048	-0.058	-0.059	-0.048	-0.058	-0.059	-0.063	-0.073	-0.070
	(0.047)	(0.046)	(0.042)	(0.047)	(0.046)	(0.042)	(0.059)	(0.055)	(0.050)
Quarter 13	-0.023	0.005	0.011	-0.023	0.005	0.011	0.004	0.030	0.031
	(0.056)	(0.053)	(0.047)	(0.056)	(0.053)	(0.047)	(0.076)	(0.065)	(0.055)
Quarter 14	-0.060	-0.065*	-0.067**	-0.060	-0.065*	-0.067**	-0.049	-0.065*	-0.074**
	(0.038)	(0.034)	(0.032)	(0.038)	(0.034)	(0.032)	(0.043)	(0.036)	(0.033)
Quarter 15	-0.034	-0.029	-0.027	-0.034	-0.029	-0.027	-0.062	-0.067	-0.069
	(0.046)	(0.044)	(0.039)	(0.047)	(0.044)	(0.039)	(0.063)	(0.059)	(0.051)
Quarter 16	-0.103**	-0.104**	-0.095**	-0.103**	-0.104**	-0.095**	-0.114*	-0.106*	-0.097*
	(0.047)	(0.047)	(0.044)	(0.047)	(0.047)	(0.044)	(0.059)	(0.057)	(0.054)
Quarter 17	-0.034	-0.000	0.009	-0.034	-0.000	0.009	-0.038	0.006	0.013
	(0.050)	(0.052)	(0.047)	(0.050)	(0.052)	(0.047)	(0.066)	(0.068)	(0.060)
Quarter 18	-0.013	0.010	0.017	-0.013	0.010	0.017	-0.039	0.004	0.012
	(0.054)	(0.054)	(0.048)	(0.053)	(0.054)	(0.048)	(0.069)	(0.071)	(0.062)
Quarter 19	-0.022	-0.010	-0.014	-0.022	-0.010	-0.014	-0.002	0.014	0.007
	(0.041)	(0.039)	(0.038)	(0.041)	(0.039)	(0.038)	(0.043)	(0.041)	(0.038)
Quarter 20	-0.012	-0.001	0.018	-0.011	-0.001	0.018	0.036	0.026	0.034
	(0.046)	(0.045)	(0.044)	(0.046)	(0.045)	(0.044)	(0.059)	(0.053)	(0.047)
Quarter 21	-0.008	-0.001	0.019	-0.008	-0.001	0.019	-0.034	-0.023	-0.010
	(0.048)	(0.042)	(0.046)	(0.048)	(0.042)	(0.046)	(0.047)	(0.045)	(0.052)
Quarter 22	0.033	0.046	0.052	0.033	0.046	0.052	0.042	0.052	0.061
	(0.052)	(0.048)	(0.042)	(0.052)	(0.048)	(0.042)	(0.065)	(0.054)	(0.048)

Table 7: Monthly number of strikes results (cont.)

Quarter 22	0.033	0.046	0.052	0.033	0.046	0.052	0.042	0.052	0.061
	(0.052)	(0.048)	(0.042)	(0.052)	(0.048)	(0.042)	(0.065)	(0.054)	(0.048)
Quarter 23	-0.071	-0.036	-0.030	-0.071	-0.036	-0.030	-0.066	-0.032	-0.025
	(0.053)	(0.046)	(0.045)	(0.053)	(0.046)	(0.045)	(0.058)	(0.055)	(0.055)
Quarter 24	-0.082**	-0.078**	-0.080**	-0.082**	-0.078**	-0.080**	-0.083*	-0.074*	-0.080*
	(0.040)	(0.038)	(0.040)	(0.039)	(0.038)	(0.040)	(0.043)	(0.042)	(0.046)
Quarter 25	-0.039	-0.022	-0.005	-0.039	-0.022	-0.005	-0.049	-0.026	-0.001
	(0.051)	(0.045)	(0.044)	(0.051)	(0.045)	(0.044)	(0.066)	(0.058)	(0.055)
Quarter 26	-0.034	-0.020	-0.023	-0.034	-0.020	-0.023	-0.020	-0.003	-0.011
	(0.047)	(0.046)	(0.045)	(0.048)	(0.046)	(0.045)	(0.058)	(0.057)	(0.058)
Quarter 27	-0.027	-0.010	-0.006	-0.027	-0.009	-0.006	-0.034	-0.012	-0.003
	(0.041)	(0.041)	(0.043)	(0.042)	(0.042)	(0.043)	(0.047)	(0.048)	(0.050)
Quarter 28	-0.054	-0.035	-0.010	-0.054	-0.035	-0.010	-0.056	-0.030	0.007
	(0.048)	(0.046)	(0.044)	(0.048)	(0.047)	(0.044)	(0.063)	(0.059)	(0.053)
Quarter 29	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Quarter 30	-0.039	-0.045	-0.043	-0.039	-0.045	-0.043	-0.060	-0.069	-0.064
	(0.051)	(0.050)	(0.045)	(0.051)	(0.049)	(0.045)	(0.061)	(0.059)	(0.054)
Quarter 31	0.003	0.016	0.017	0.003	0.016	0.017	-0.009	0.020	0.023
	(0.064)	(0.057)	(0.054)	(0.064)	(0.057)	(0.054)	(0.075)	(0.071)	(0.064)
Quarter 32	-0.016	-0.011	-0.015	-0.016	-0.011	-0.015	0.042	0.028	0.016
	(0.038)	(0.036)	(0.035)	(0.038)	(0.036)	(0.035)	(0.043)	(0.037)	(0.036)
Quarter 33	-0.131	-0.157	-0.093	-0.131	-0.157	-0.093	-0.154	-0.186	-0.113
	(0.128)	(0.144)	(0.150)	(0.128)	(0.144)	(0.150)	(0.146)	(0.166)	(0.181)
Quarter 34	-0.053	-0.012	-0.016	-0.053	-0.012	-0.016	-0.020	0.013	0.001
	(0.056)	(0.040)	(0.038)	(0.056)	(0.041)	(0.038)	(0.063)	(0.046)	(0.043)
Quarter 35	-0.064	-0.032	-0.019	-0.064	-0.032	-0.019	-0.050	-0.012	0.003
	(0.050)	(0.047)	(0.048)	(0.050)	(0.047)	(0.048)	(0.052)	(0.049)	(0.051)
Quarter 36	0.024	0.019	0.024	0.024	0.019	0.024	0.035	0.028	0.033
	(0.064)	(0.052)	(0.052)	(0.064)	(0.052)	(0.052)	(0.074)	(0.060)	(0.060)
Quarter 37	-0.026	-0.033	-0.019	-0.026	-0.033	-0.019	-0.014	-0.021	-0.004
	(0.048)	(0.046)	(0.045)	(0.048)	(0.046)	(0.045)	(0.064)	(0.058)	(0.053)
Quarter 38	-0.028	-0.007	-0.008	-0.028	-0.007	-0.008	-0.024	0.007	0.009
	(0.051)	(0.048)	(0.048)	(0.051)	(0.048)	(0.048)	(0.060)	(0.059)	(0.057)
Quarter 39	-0.007	0.000	-0.006	-0.007	0.000	-0.006	0.035	0.029	0.018
	(0.044)	(0.041)	(0.041)	(0.044)	(0.041)	(0.041)	(0.051)	(0.044)	(0.045)
Quarter 40	-0.016	0.009	0.006	-0.016	0.009	0.006	0.002	0.018	0.010
	(0.053)	(0.038)	(0.035)	(0.053)	(0.038)	(0.035)	(0.061)	(0.048)	(0.044)
Union rate	2015	2016	2017	2015	2016	2017			
BIC	1.45e+07	1.45e+07	1.45e+07	1.45e+07	1.45e+07	1.45e+07	9.94e+06	9.92e+06	9.92e+06
log-likelihood	-7.24e+06	-7.23e+06	-7.23e+06	-7.24e+06	-7.23e+06	-7.23e+06	-4.97e+06	-4.96e+06	-4.96e+06
χ^2	.	.	.	.	.	.	.	.	.
p value	.	.	.	.	.	.	.	.	.
N	6188	6188	6188	6188	6188	6188	6188	6188	6188

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Duration of strikes results

	Strikes Duration					
	OLS			*Proportional Hazards*		
	(1)	(2)	(3)	(4)	(5)	(6)
$UNEMP$	-0.031	-0.033	-0.036	0.058	0.058	0.051
	(0.047)	(0.047)	(0.049)	(0.065)	(0.064)	(0.064)
$\ln TC$	-0.012	-0.014	-0.015	-0.007	-0.009	-0.012
	(0.025)	(0.025)	(0.025)	(0.021)	(0.021)	(0.021)
Quarter 1	-0.085	-0.076	-0.064	0.058	0.063	0.054
	(0.067)	(0.065)	(0.074)	(0.048)	(0.048)	(0.054)
Quarter 2	-0.029	-0.034	-0.029	0.043	0.044	0.039
	(0.035)	(0.039)	(0.044)	(0.040)	(0.043)	(0.051)
Quarter 3	0.011	0.011	0.030	-0.030	-0.023	-0.040
	(0.033)	(0.031)	(0.031)	(0.026)	(0.025)	(0.028)
Quarter 4	-0.066	-0.063	-0.054*	0.051	0.054*	0.043
	(0.048)	(0.040)	(0.031)	(0.032)	(0.027)	(0.026)
Quarter 5	-0.031	-0.032	-0.012	-0.003	0.001	-0.025
	(0.051)	(0.047)	(0.051)	(0.053)	(0.057)	(0.068)
Quarter 6	-0.019	-0.001	0.032	-0.026	-0.043	-0.062
	(0.070)	(0.060)	(0.054)	(0.074)	(0.065)	(0.052)
Quarter 7	-0.041	-0.044	-0.030	0.049**	0.055***	0.050**
	(0.034)	(0.029)	(0.030)	(0.020)	(0.017)	(0.020)
Quarter 8	-0.038	-0.024	-0.015	0.048*	0.048*	0.047
	(0.038)	(0.033)	(0.034)	(0.026)	(0.025)	(0.028)
Quarter 9	-0.076	-0.057	-0.034	0.034	0.017	0.000
	(0.051)	(0.043)	(0.036)	(0.061)	(0.052)	(0.041)
Quarter 10	-0.082	-0.081	-0.075*	0.077	0.095*	0.093**
	(0.059)	(0.052)	(0.041)	(0.063)	(0.058)	(0.042)
Quarter 11	-0.038	-0.034	-0.028	0.033	0.037	0.035
	(0.045)	(0.041)	(0.037)	(0.042)	(0.040)	(0.036)
Quarter 12	-0.101**	-0.098***	-0.089***	0.124**	0.130***	0.130***
	(0.043)	(0.036)	(0.032)	(0.049)	(0.046)	(0.042)
Quarter 13	-0.081	-0.078*	-0.063	0.052	0.070	0.059
	(0.051)	(0.045)	(0.042)	(0.049)	(0.048)	(0.053)
Quarter 14	-0.035	-0.015	-0.001	0.039	0.030	0.025
	(0.054)	(0.049)	(0.043)	(0.052)	(0.047)	(0.042)
Quarter 15	-0.091**	-0.081**	-0.062*	0.084**	0.087**	0.072**
	(0.038)	(0.036)	(0.034)	(0.036)	(0.035)	(0.032)
Quarter 16	-0.034	-0.042	-0.020	0.053	0.062	0.058
	(0.031)	(0.038)	(0.043)	(0.037)	(0.039)	(0.046)
Quarter 17	-0.006	-0.011	-0.007	-0.008	-0.002	0.001
	(0.040)	(0.030)	(0.024)	(0.030)	(0.025)	(0.021)
Quarter 18	-0.058*	-0.048*	-0.038	0.047*	0.044*	0.036
	(0.032)	(0.027)	(0.029)	(0.027)	(0.025)	(0.029)
Quarter 19	-0.071*	-0.065**	-0.051*	0.075***	0.075***	0.062***
	(0.037)	(0.032)	(0.028)	(0.025)	(0.024)	(0.024)
Quarter 20	-0.064	-0.059	-0.053	0.044	0.045	0.045
	(0.043)	(0.038)	(0.035)	(0.039)	(0.036)	(0.033)
Quarter 21	-0.110**	-0.086*	-0.069	0.120***	0.100**	0.071
	(0.050)	(0.048)	(0.045)	(0.043)	(0.045)	(0.045)

Table 9: Duration of strikes results (cont.)

Quarter 22	-0.114**	-0.088**	-0.069**	0.134***	0.115***	0.094***
	(0.052)	(0.041)	(0.033)	(0.051)	(0.044)	(0.035)
Quarter 23	-0.052	-0.042	-0.024	0.046	0.035	0.016
	(0.046)	(0.044)	(0.043)	(0.033)	(0.036)	(0.038)
Quarter 24	-0.110**	-0.101**	-0.068*	0.112***	0.099***	0.071**
	(0.042)	(0.040)	(0.038)	(0.041)	(0.038)	(0.033)
Quarter 25	-0.024	-0.021	-0.021	-0.026	-0.026	-0.022
	(0.036)	(0.031)	(0.026)	(0.035)	(0.033)	(0.030)
Quarter 26	-0.030	-0.004	0.020	0.026	0.003	-0.016
	(0.047)	(0.047)	(0.056)	(0.041)	(0.046)	(0.057)
Quarter 27	-0.054	-0.047	-0.037	0.048	0.053	0.047
	(0.034)	(0.029)	(0.025)	(0.036)	(0.033)	(0.032)
Quarter 28	-0.077*	-0.075*	-0.074**	0.065*	0.071**	0.072**
	(0.042)	(0.038)	(0.035)	(0.036)	(0.035)	(0.033)
Quarter 29	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)
Quarter 30	-0.036	-0.029	-0.012	0.019	0.025	0.011
	(0.036)	(0.030)	(0.030)	(0.027)	(0.027)	(0.029)
Quarter 31	-0.078	-0.092*	-0.089	0.069	0.082*	0.082
	(0.049)	(0.053)	(0.060)	(0.046)	(0.047)	(0.052)
Quarter 32	-0.054*	-0.045*	-0.039*	0.056**	0.054**	0.051**
	(0.030)	(0.025)	(0.021)	(0.024)	(0.023)	(0.023)
Quarter 33	-0.145***	-0.149***	-0.146***	0.172***	0.180***	0.188***
	(0.037)	(0.030)	(0.031)	(0.027)	(0.025)	(0.027)
Quarter 34	-0.107***	-0.096***	-0.085**	0.102***	0.098***	0.093***
	(0.036)	(0.033)	(0.032)	(0.026)	(0.026)	(0.026)
Quarter 35	-0.080	-0.064	-0.033	0.063	0.033	0.000
	(0.052)	(0.054)	(0.057)	(0.077)	(0.076)	(0.059)
Quarter 36	-0.067*	-0.058*	-0.052	0.072**	0.069**	0.070*
	(0.036)	(0.033)	(0.032)	(0.034)	(0.034)	(0.038)
Quarter 37	-0.082	-0.092**	-0.084***	0.086**	0.094***	0.087***
	(0.052)	(0.036)	(0.026)	(0.040)	(0.033)	(0.023)
Quarter 38	-0.055	-0.047	-0.035	0.049	0.047	0.042
	(0.045)	(0.037)	(0.030)	(0.035)	(0.030)	(0.027)
Quarter 39	-0.085**	-0.079***	-0.071***	0.071**	0.073***	0.073***
	(0.035)	(0.029)	(0.024)	(0.029)	(0.027)	(0.025)
Quarter 40	-0.048	-0.046	-0.047*	0.021	0.016	0.014
	(0.035)	(0.032)	(0.024)	(0.031)	(0.029)	(0.025)
Union rate	2015	2016	2017	2015	2016	2017
BIC	4128.116	4127.957	4129.215	18775.611	18772.852	18772.224
R^2	0.125	0.126	0.125			
log-likelihood	-1889.058	-1888.979	-1889.608	-9201.868	-9200.489	-9200.175
N	1468	1468	1468	1468	1468	1468

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

8.3 Unweighted incidence results

In the Tables 10-11 are presented the results for the incidence models without weighting the data. The coefficients are presented in the same way of the other coefficient tables. Also, in Figures 11-13 the unweighted regression coefficients are plotted in the same fashion as in the results section.

Table 10: Monthly number of strikes results (unweighted)

	Monthly number of strikes								
	Poisson			*ZI Poisson*			*Logit model*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ln $NFIRMS$	-0.077	-0.079	-0.077	-0.078	-0.081	-0.078	-0.084	-0.088	-0.086
	(0.088)	(0.087)	(0.090)	(0.086)	(0.085)	(0.088)	(0.082)	(0.083)	(0.086)
$UNEMP$	-0.189***	-0.195***	-0.189***	-0.185***	-0.190***	-0.184***	-0.194***	-0.203***	-0.200***
	(0.043)	(0.044)	(0.045)	(0.046)	(0.047)	(0.048)	(0.074)	(0.075)	(0.076)
Quarter 1	-0.021	-0.003	-0.000	-0.021	-0.003	-0.001	-0.006	0.014	0.015
	(0.070)	(0.065)	(0.062)	(0.070)	(0.065)	(0.061)	(0.084)	(0.075)	(0.069)
Quarter 2	-0.056	-0.037	-0.026	-0.056	-0.037	-0.025	-0.091	-0.059	-0.041
	(0.072)	(0.058)	(0.051)	(0.071)	(0.057)	(0.051)	(0.071)	(0.057)	(0.050)
Quarter 3	-0.024	-0.015	-0.011	-0.024	-0.014	-0.010	-0.044	-0.026	-0.026
	(0.057)	(0.050)	(0.046)	(0.057)	(0.049)	(0.045)	(0.060)	(0.051)	(0.045)
Quarter 4	-0.008	-0.010	-0.002	-0.009	-0.010	-0.002	0.012	0.001	0.011
	(0.049)	(0.044)	(0.041)	(0.048)	(0.043)	(0.041)	(0.067)	(0.056)	(0.050)
Quarter 5	-0.118**	-0.107**	-0.107**	-0.118**	-0.106**	-0.107**	-0.103**	-0.109**	-0.113***
	(0.052)	(0.046)	(0.043)	(0.051)	(0.045)	(0.043)	(0.049)	(0.046)	(0.044)
Quarter 6	-0.041	-0.024	-0.002	-0.041	-0.024	-0.002	-0.025	-0.010	0.006
	(0.062)	(0.057)	(0.054)	(0.060)	(0.056)	(0.053)	(0.072)	(0.062)	(0.055)
Quarter 7	-0.046	-0.020	-0.026	-0.046	-0.020	-0.026	-0.039	-0.005	-0.015
	(0.061)	(0.063)	(0.064)	(0.060)	(0.062)	(0.064)	(0.073)	(0.066)	(0.063)
Quarter 8	-0.066	-0.063	-0.055	-0.066	-0.062	-0.055	-0.077	-0.076	-0.070
	(0.057)	(0.051)	(0.048)	(0.056)	(0.050)	(0.048)	(0.070)	(0.058)	(0.054)
Quarter 9	-0.049	-0.038	-0.029	-0.048	-0.036	-0.028	-0.034	-0.021	-0.013
	(0.035)	(0.032)	(0.037)	(0.035)	(0.032)	(0.037)	(0.040)	(0.034)	(0.039)
Quarter 10	-0.053	-0.046	-0.048	-0.053	-0.046	-0.048	-0.061	-0.052	-0.057
	(0.046)	(0.039)	(0.036)	(0.045)	(0.039)	(0.036)	(0.053)	(0.045)	(0.041)
Quarter 11	-0.046	-0.029	-0.016	-0.046	-0.030	-0.016	-0.034	-0.016	-0.001
	(0.049)	(0.045)	(0.046)	(0.049)	(0.044)	(0.045)	(0.056)	(0.048)	(0.051)
Quarter 12	-0.069	-0.076	-0.079	-0.069	-0.077	-0.079	-0.090	-0.092*	-0.093*
	(0.057)	(0.052)	(0.049)	(0.056)	(0.051)	(0.048)	(0.061)	(0.054)	(0.051)
Quarter 13	-0.045	-0.019	-0.015	-0.045	-0.019	-0.015	-0.030	-0.002	-0.001
	(0.063)	(0.056)	(0.053)	(0.062)	(0.056)	(0.052)	(0.075)	(0.063)	(0.057)
Quarter 14	-0.068	-0.072**	-0.075**	-0.069*	-0.072**	-0.075**	-0.061	-0.068**	-0.077**
	(0.042)	(0.037)	(0.034)	(0.042)	(0.036)	(0.033)	(0.042)	(0.034)	(0.031)
Quarter 15	-0.060	-0.059	-0.058	-0.061	-0.059	-0.058	-0.094*	-0.099**	-0.101**
	(0.050)	(0.044)	(0.041)	(0.050)	(0.043)	(0.040)	(0.056)	(0.048)	(0.044)
Quarter 16	-0.101**	-0.101**	-0.093**	-0.102**	-0.101**	-0.093**	-0.102*	-0.095*	-0.086*
	(0.052)	(0.048)	(0.046)	(0.051)	(0.048)	(0.045)	(0.059)	(0.053)	(0.049)
Quarter 17	-0.021	0.003	0.006	-0.020	0.004	0.007	-0.026	0.007	0.008
	(0.043)	(0.041)	(0.038)	(0.043)	(0.040)	(0.038)	(0.057)	(0.052)	(0.048)
Quarter 18	-0.028	-0.008	-0.003	-0.027	-0.006	-0.002	-0.065	-0.024	-0.018
	(0.056)	(0.053)	(0.049)	(0.056)	(0.052)	(0.049)	(0.065)	(0.064)	(0.059)
Quarter 19	-0.030	-0.019	-0.024	-0.029	-0.019	-0.023	-0.012	0.004	-0.004
	(0.048)	(0.043)	(0.041)	(0.047)	(0.043)	(0.040)	(0.051)	(0.043)	(0.039)
Quarter 20	-0.032	-0.023	-0.004	-0.032	-0.023	-0.004	-0.002	-0.004	0.003
	(0.049)	(0.045)	(0.046)	(0.048)	(0.044)	(0.046)	(0.059)	(0.049)	(0.047)
Quarter 21	-0.031	-0.023	-0.004	-0.030	-0.022	-0.003	-0.056	-0.043	-0.031
	(0.059)	(0.051)	(0.052)	(0.058)	(0.050)	(0.052)	(0.057)	(0.051)	(0.054)

Table 11: Monthly number of strikes results (unweighted) (cont.)

Quarter 22	0.018	0.028	0.035	0.018	0.029	0.036	0.018	0.031	0.042
	(0.059)	(0.052)	(0.047)	(0.058)	(0.051)	(0.047)	(0.073)	(0.057)	(0.052)
Quarter 23	-0.086	-0.058	-0.053	-0.086	-0.059	-0.053	-0.085	-0.056	-0.049
	(0.058)	(0.050)	(0.049)	(0.057)	(0.049)	(0.048)	(0.066)	(0.057)	(0.055)
Quarter 24	-0.084**	-0.076**	-0.083***	-0.084**	-0.075**	-0.082***	-0.081**	-0.066**	-0.077**
	(0.037)	(0.032)	(0.032)	(0.036)	(0.031)	(0.031)	(0.036)	(0.031)	(0.034)
Quarter 25	-0.032	-0.025	-0.014	-0.032	-0.025	-0.013	-0.044	-0.033	-0.015
	(0.040)	(0.032)	(0.034)	(0.039)	(0.032)	(0.034)	(0.054)	(0.042)	(0.044)
Quarter 26	-0.058	-0.046	-0.049	-0.058	-0.046	-0.049	-0.066	-0.049	-0.056
	(0.055)	(0.051)	(0.050)	(0.055)	(0.051)	(0.049)	(0.066)	(0.063)	(0.062)
Quarter 27	-0.052	-0.035	-0.031	-0.052	-0.035	-0.030	-0.059	-0.037	-0.027
	(0.048)	(0.046)	(0.046)	(0.047)	(0.046)	(0.046)	(0.047)	(0.046)	(0.048)
Quarter 28	-0.077	-0.059	-0.035	-0.077	-0.060	-0.036	-0.081	-0.054	-0.020
	(0.055)	(0.049)	(0.048)	(0.055)	(0.049)	(0.047)	(0.063)	(0.054)	(0.052)
Quarter 29	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Quarter 30	-0.039	-0.047	-0.044	-0.039	-0.046	-0.043	-0.050	-0.058	-0.052
	(0.053)	(0.047)	(0.044)	(0.052)	(0.047)	(0.043)	(0.062)	(0.053)	(0.048)
Quarter 31	-0.030	-0.016	-0.016	-0.030	-0.016	-0.016	-0.040	-0.012	-0.010
	(0.068)	(0.060)	(0.057)	(0.067)	(0.059)	(0.056)	(0.071)	(0.065)	(0.060)
Quarter 32	-0.039	-0.035	-0.039	-0.039	-0.035	-0.040	0.005	-0.001	-0.012
	(0.048)	(0.044)	(0.040)	(0.047)	(0.044)	(0.040)	(0.052)	(0.044)	(0.039)
Quarter 33	-0.171	-0.186*	-0.138	-0.170	-0.185*	-0.137	-0.193	-0.210*	-0.157
	(0.104)	(0.107)	(0.114)	(0.104)	(0.107)	(0.114)	(0.119)	(0.122)	(0.133)
Quarter 34	-0.081	-0.048	-0.050	-0.079	-0.047	-0.049	-0.057	-0.025	-0.033
	(0.067)	(0.056)	(0.051)	(0.065)	(0.054)	(0.050)	(0.073)	(0.058)	(0.052)
Quarter 35	-0.080	-0.051	-0.038	-0.079	-0.050	-0.037	-0.073	-0.037	-0.021
	(0.061)	(0.054)	(0.054)	(0.060)	(0.053)	(0.053)	(0.061)	(0.054)	(0.055)
Quarter 36	-0.024	-0.028	-0.020	-0.023	-0.028	-0.020	-0.019	-0.024	-0.015
	(0.074)	(0.065)	(0.062)	(0.073)	(0.064)	(0.061)	(0.080)	(0.067)	(0.065)
Quarter 37	-0.005	-0.009	-0.002	-0.004	-0.009	-0.001	0.013	0.007	0.015
	(0.037)	(0.031)	(0.032)	(0.037)	(0.031)	(0.031)	(0.046)	(0.035)	(0.033)
Quarter 38	-0.045	-0.026	-0.028	-0.045	-0.026	-0.027	-0.041	-0.014	-0.015
	(0.055)	(0.049)	(0.047)	(0.054)	(0.049)	(0.047)	(0.054)	(0.051)	(0.049)
Quarter 39	-0.026	-0.019	-0.025	-0.025	-0.018	-0.024	0.001	0.001	-0.008
	(0.048)	(0.044)	(0.041)	(0.048)	(0.043)	(0.041)	(0.056)	(0.048)	(0.045)
Quarter 40	-0.006	0.004	0.001	-0.005	0.004	0.001	0.003	0.006	-0.003
	(0.042)	(0.031)	(0.029)	(0.041)	(0.031)	(0.028)	(0.052)	(0.038)	(0.035)
Union rate	2015	2016	2017	2015	2016	2017	2015	2016	2017
BIC	6319.338	6312.514	6308.781	6318.294	6311.367	6307.759	4787.438	4780.940	4779.555
log-likelihood	-2937.045	-2933.632	-2931.766	-2936.523	-2933.059	-2931.255	-2171.094	-2167.845	-2167.153
N	6188	6188	6188	6188	6188	6188	6188	6188	6188

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 11: Reform effect coefficients in Poisson model for strike incidence (unweighted)

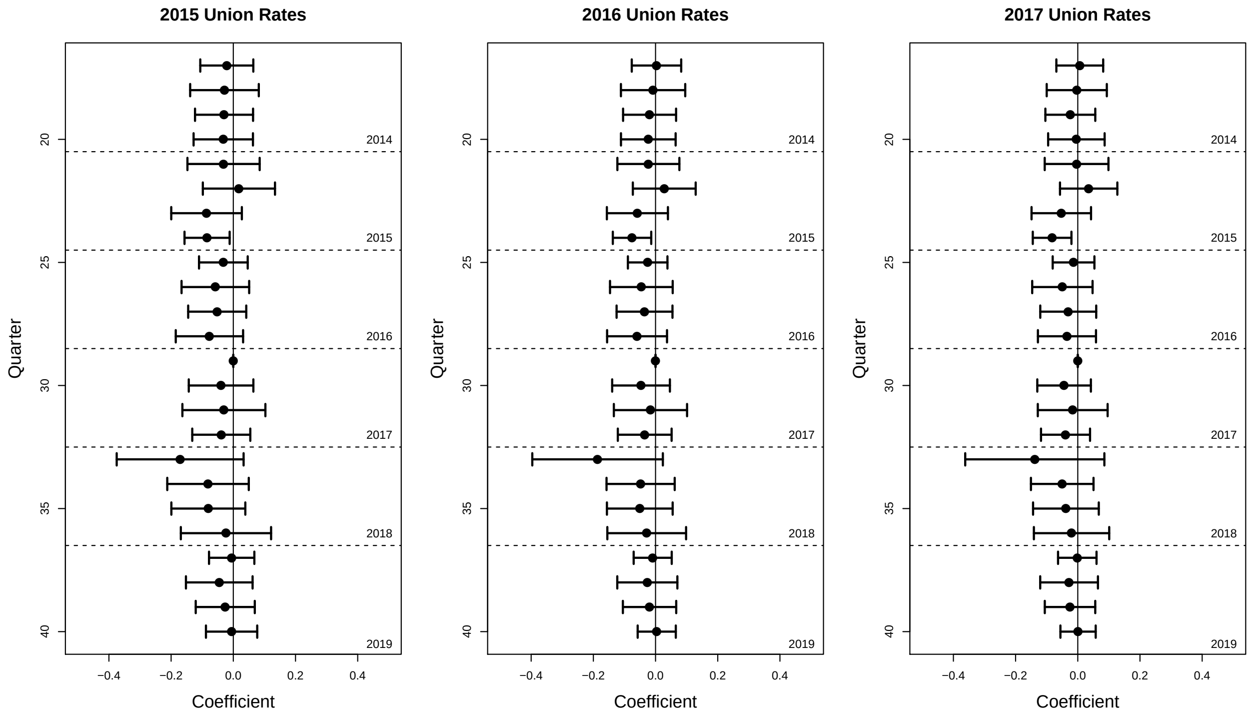


Figure 12: Reform effect coefficients in Poisson ZI model for strike incidence (unweighted)

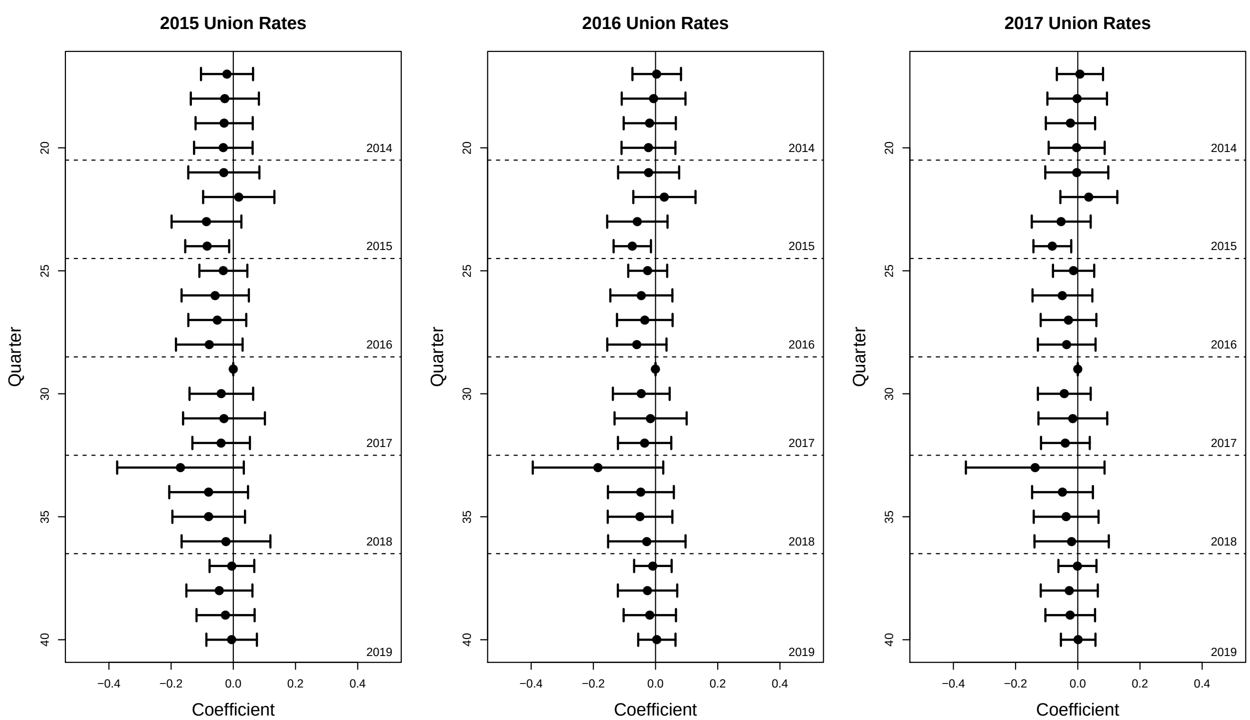
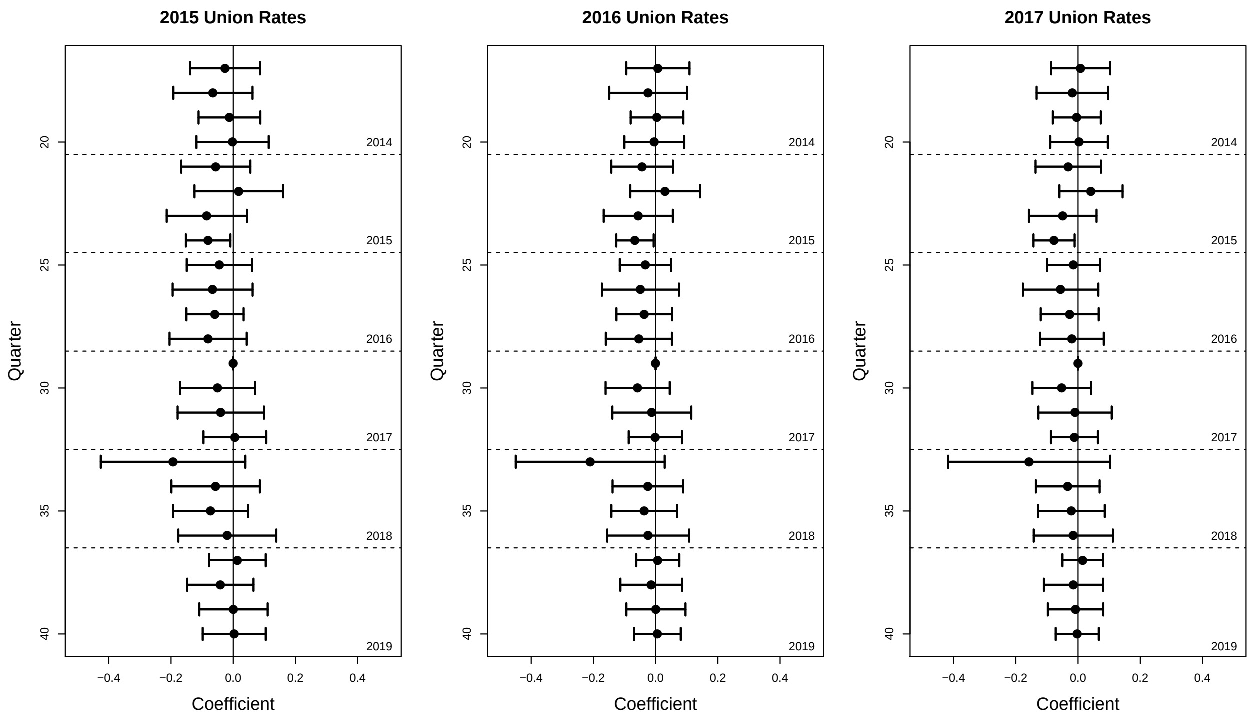


Figure 13: Reform effect coefficients in Logit model for strike incidence (unweighted)



References

Armstrong, A. and Águila, R. (2005). *Evolución del conflicto laboral en Chile 1961-2002*. Ediciones Universidad Católica de Chile.

Ashenfelter, O. and Johnson, G. E. (1969). Bargaining Theory, Trade Unions, and Industrial Strike Activity. *The American Economic Review*, 59(1):35–49.

Budd, J. W. (1996). Canadian Strike Replacement Legislation and Collective Bargaining: Lessons for the United States. *Industrial Relations*, 35(2):245–260.

Cameron, A. C. and Trivedi, P. K. (2013). *Regression Analysis of Count Data*. Cambridge University Press, second edition.

Campolieti, M., Hebdon, R., and Dachis, B. (2014). The impact of collective bargaining legislation on strike activity and wage settlements. *Industrial Relations*, 53(3):394–429.

Campolieti, M., Hebdon, R., and Hyatt, D. (2005). Strike incidence and strike duration: Some new evidence from Ontario. *Industrial and Labor Relations Review*, 58(4):610–630.

Card, D. (1990). Strikes and Bargaining: A Survey of the Recent Empirical Literature. *The American Economic Review*, 80(2):410–415.

Card, D. (1992). Using Regional Variation in Wages to Measure the Effects of the Federal Minimum Wage. *NBER Working Paper*.

Card, D. and Olson, C. A. (1995). Bargaining Power, Strike Durations, and Wage Outcomes : An Analysis of Strikes in the 1880s. *Journal of Labor Economics*, 13(1):32–61.

Consejo Superior Laboral (2018). Informe de seguimiento y evaluación sobre la implementación y aplicación de la Ley N° 20.940 que Moderniza el Sistema de Relaciones Laborales. Technical report.

Cox, D. R. (1972). Regression Models and Life-Tables. *Journal of the Royal Statistical Society. Series B (Methodological)*, 34(2):187–220.

Cramton, P., Gunderson, M., and Tracy, J. (1999). The Effect of Collective Bargaining Legislation on Strikes and Wages. *The Review of Economics and Statistics*, 81(3):475–487.

Cramton, P. C. and Tracy, J. S. (1992). Strikes and Holdouts in Wage Bargaining: Theory and Data. *American Economic Review*, 82(1):100–121.

Gourieroux, C., Monfort, A., and Trognon, A. (1984). Pseudo Maximum Likelihood Methods: Applications to Poisson Models. *Econometrica*, 52(3):701–720.

Gunderson, M., Kervin, J., and Reid, F. (1989). The Effect of Labour Relations Legislation on Strike Incidence. *The Canadian Journal of Economics*, 22(4):779–794.

Hayes, B. (1984). Unions and Strikes with Asymmetric Information. *Journal of Labor Economics*, 2(1):57–83.

International Labour Organization (2006). *Freedom of Association: Digest of decisions and principles of the Freedom of Association Committee of the Governing Body of the ILO*. International Labour Organization Office, fifth edit edition.

Kalbfleisch, J. D. and Prentice, R. L. (2002). *The Statistical Analysis of Failure Time Data*, volume 45. John Wiley & Sons, Inc., Hoboken, New Jersey, second edi edition.

Kaplan, E. L. and Meier, P. (1958). Nonparametric Estimation from Incomplete Observations. *Journal of the American Statistical Association*, 53(282):457–481.

Kennan, J. (1980). Pareto Optimality and the Economics of Strike Duration. *Journal of Labor Research*, 1(1):77–94.

Kennan, J. and Wilson, R. (1989). Strategic Bargaining Models and Interpretation of Strike Data. *Journal of Applied Econometrics*, 4(1):87–130.

Kiefer, N. (1988). Economic Duration Data and Hazard Functions. *Journal of Economic Literature*, 26(2):646–79.

Micco, A. and Pagés, C. (2006). The Economic Effects of Employment Protection: Evidence from International Industry-Level Data. *IZA Discussion Paper Series*, (2433).

Pedemonte, M. (2019). Fireside Chats: Communication and Consumer Expectations in the Great Depression.

Reder, M. W. and Neumann, G. R. (1980). Conflict and Contract : The Case of Strikes. *Journal of Political Economy*, 88(5):867–886.

Savage, I. R. (1956). Contributions to the Theory of Rank Order Statistics - The Two Sample Case. *The Annals of Mathematical Statistics*, 27(3):590–615.

Schnell, J. F. and Gramm, C. L. (1994). The Empirical Relations between Employers' Striker Replacement Strategies and Strike Duration. *Industrial and Labor Relations Review*, 47(2):189–206.

Siebert, W. S. and Addison, J. T. (1981). Are Strikes Accidental? *The Economic Journal*, 91(362):389–404.

Skeels, J. W., McGrath, P., and Arshanapalli, G. (1988). The Importance of Strike Size in Strike Research. *Industrial and Labor Relations Review*, 41(4):582.

Staub, K. E. and Winkelmann, R. (2012). Consistent Estimation of Zero-Inflated Count Models. *Health Economics*.