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The Impact of Anti-Temporary Replacement Legislation on Work Stoppages: Empirical Evidence from Canada

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Les lois qui interdisent l'embauche de travailleurs suppléants (ou de remplacement) pendant des arrêts de travail sont controversées. Des « lois déclarant non temporaires les travailleurs suppléants », ou lois anti-briseurs de grève (« lois anti-scabs »), sont en vigueur au Québec et en Colombie-Britannique, et l'Ontario a appliqué ce type de loi pendant une courte période. Dans cet article, nous analysons de façon empirique l'impact de ces lois sur la durée des arrêts de travail, sur la base des variations que nous observons dans le temps, de 1978 à 2003, et selon les provinces. Nos résultats montrent que ces lois ont pour effets d'augmenter la fréquence des grèves et de réduire leur durée – ces deux effets étant statistiquement significatifs et importants en termes de grandeur. De plus, ils ont des effets opposés sur le nombre de jours de travail perdus à cause d'arrêts de travail. Les preuves empiriques démontrant que les lois anti-briseurs de grève auraient pour effet d'augmenter le nombre de jours de travail perdus à cause d'arrêts de travail durant la première année de leur application ne sont pas concluantes ; et aucune preuve n'indique que ces lois ont un effet significatif sur le nombre de jours de travail perdus à cause d'arrêts de travail après deux années d'application.

Mots clés : arrêt de travail, grève, lock-out, négociation collective, politiques publiques, loi déclarant non temporaires les travailleurs suppléants, loi anti-briseurs de grève, loi sur les travailleurs de remplacement

Legislation that prevents the hiring of temporary replacement workers during a work stoppage is controversial. “Anti-temporary replacement worker” legislation (ATR) or “anti-scab” legislation is currently in effect in Quebec and British Columbia and existed for a short period of time in Ontario. This paper uses variation over time (1978–2003) and across provinces to provide empirical evidence concerning the impact of ATR on work stoppages. The results show that ATR increases strike incidence and decreases strike length—both effects are statistically significant and substantial in magnitude. Incidence and length have opposing effects on days lost to work stoppages. Empirical results provide weak evidence that anti-scab legislation may increase days lost to work stoppages in the first two years after the legislation takes effect but no evidence that such legislation has a statistically significant effect on days lost when it has been in effect for more than two years.

Keywords: work stoppages, strike, lockout, collective bargaining, public policy, anti-temporary replacement legislation, anti-scab legislation, replacement worker legislation

OVERVIEW

Legislation that prohibits the use of temporary replacement workers during a work stoppage continues to be an important and controversial public policy issue. In May 2006, anti-temporary replacement legislation, also known as anti-scab legislation, was introduced in the federal jurisdiction. This is the 13th time that this type of labour legislation has come before the House of Commons since 1993.¹ Anti-temporary replacement worker legislation (ATR) is currently in force in British Columbia and Quebec, and existed in Ontario from 1993 to 1995. Labour organizations press for the adoption of anti-scab legislation in all Canadian jurisdictions.² Proponents of ATR argue that it reduces picket line violence, benefits the long-run bargaining relationship, protects the strike threat, ensures a balance of power between union and management, prevents the erosion of worker bargaining rights,³ and reduces the number and duration of work stoppages.⁴ Opponents, often from the business community,⁵ argue that such legislation limits the right of the employer to continue to try to operate during a work stoppage, gives the union an unfair advantage at the bargaining table, reduces employment⁶ and investment,⁷ and is detrimental to long-run growth. Some of these arguments revolve around issues that are largely ideological or distributional in nature. Others concern efficiency—the impact of ATR on the level of output in the short-run or on the growth of output in the long-run.

The primary purpose of this paper is to provide empirical evidence concerning the impact of ATR on work stoppages in Canada. First, we examine the effect of ATR on the *incidence* of work stoppages as measured by the number of work stoppages begun in a calendar year per thousand firms. Second, we examine the impact of ATR on the *length* of work stoppages as measured by the number of days lost in a calendar year per employee on strike. Finally, we examine the impact of ATR on *days lost* as measured by total person days of work lost due to

work stoppages in a calendar year per thousand firms.

In Canada, labour law is under provincial jurisdiction. This means there is considerable variation over time and across provinces in the use of various types of labour legislation. This variation can be used to identify the effect of ATR on work stoppages. We use annual province-level data for all firms in the private, non-construction sector from 1978 to 2003 for nine provinces and perform cross-sectional time-series analysis to estimate the impact of anti-scab legislation on work stoppages. In order to isolate the impact of anti-scab legislation, it is necessary to control for other factors that may influence work stoppages. Therefore we include a number of legislative variables in the analysis: reinstatement rights, ban on professional strikebreakers, compulsory conciliation, cool-off periods, first agreement arbitration, compulsory dues check-off, mandatory strike votes, employer-initiated strike votes, and technological contract reopener clauses. As a consequence, the empirical results also provide evidence of how these other types of labour legislation affect work stoppages.

The results indicate that anti-scab legislation increases the number of work stoppages and decreases their length. Both these results are statistically significant at the 99 percent level and substantial in magnitude. Since incidence and length have opposing influences on person days lost to work stoppages, it is interesting to examine the evidence concerning the overall impact of anti-scab legislation on person days lost. The results show that ATR may increase days lost in the two years immediately after the legislation comes into effect, but this result is not always statistically significant. There appears to be no statistically significant effect of anti-scab legislation on days lost when the legislation has been in effect for more than two years.

The paper is organized as follows. First, the results of earlier studies are reviewed and the

contribution of this paper to the existing literature is outlined. Second, the empirical approach is discussed. Third, the data and variables are described. Next, the results are presented and analyzed. Finally, the results are summarized and policy implications are discussed.

LITERATURE REVIEW

Previous research has provided empirical evidence of the impact of ATR on the probability of work stoppages and on the duration of work stoppages.

Evidence on the probability of a strike occurring is presented by Gunderson, Kervin, and Reid (1989), Budd (1996), and Cramton, Gunderson, and Tracy (1999).⁸ All these studies use monthly data at the level of the individual bargaining unit. Gunderson et al. and Cramton et al. focus on private sector, non-construction bargaining units in firms with 500 or more employees. Budd looks at all bargaining units in the manufacturing sector. The period of time differs from study to study: Gunderson et al. use data from 1971 to 1985, Budd uses data from 1966 to 1985, and Cramton et al. use data from 1967 to 1993. The first two studies use probit analysis, while Cramton et al. use logit. The studies control for other factors that may influence strikes. Gunderson et al. and Cramton et al. include many other public policy variables and also discuss the impact of these policies on strike incidence. All the studies show that the presence of ATR increases the probability of a work stoppage occurring. Though the effect is of substantial magnitude, it reaches statistical significance only in Gunderson et al.

Evidence of the effect of ATR on strike duration is presented by Gunderson and Melino (1990), Budd (1996), and Cramton et al. (1999). All these studies use data at the bargaining unit level. Gunderson and Melino use data on private sector strikes from January 1967 to December 1985; data used in the other two studies are described above. Budd, and

Gunderson and Melino, use hazard models. Cramton et al. use regression analysis with $\ln(\text{duration})$ as the dependent variable. The studies control for other factors that may influence duration. Cramton et al., and Gunderson and Melino, include many policy variables and also discuss their influence on duration. All three studies find that ATR increases the duration of a strike. The results in Budd are not statistically significant. The results in the other two studies are statistically significant and are also substantial in magnitude. Cramton et al. find that the presence of anti-scab legislation increases the expected duration of a strike by at least 50 percent. Gunderson and Melino find that ATR is associated with an increase in strike duration of 7 to 10 days (the average duration of a strike in their sample is approximately 35 days). Gunderson and Melino, and Cramton et al., also consider the impact of a wide array of other types of labour legislation on strike duration.

The authors of all these earlier studies recognize that the time periods covered by their data do not contain much variation in the use of ATR. Quebec introduced ATR in February 1978, British Columbia introduced it in January 1993, and Ontario passed the legislation in January 1993 and repealed it in October 1995. The data used by Gunderson et al. (1989), Gunderson and Melino (1990), and Budd (1996) end in 1985. Data used by Cramton et al. (1999) end in March 1993. The lack of variation in the use of anti-scab legislation in the time periods covered by these studies often results in imprecise estimates and means that most or, in some cases, all of the identification of the impact of anti-scab legislation on strike incidence is based on the experience of Quebec.

Moreover, two of the three studies that look at the probability of a work stoppage occurring examine the experience of very large bargaining units with 500 or more employees (Cramton et al. 1999; Gunderson et al. 1989). In 2003, these large firms represented only 0.2 percent of all firms, employed

approximately 22 percent of unionized workers and accounted for approximately 43 percent of employment.⁹ Empirical results based on very large firms may not fairly reflect the impact of ATR. Lemay and Kozhaya (2005) suggest large firms may be less vulnerable in the presence of ATR and therefore may behave differently than small firms. For example, large firms with a number of production facilities could maintain production during a work stoppage by temporarily transferring production from a jurisdiction with ATR to another location. Small, single-establishment firms do not have this option. Cramton et al. (1999) and Gunderson and Melino (1990) include firm size in their duration analyses with mixed results: Cramton et al. find that firm size has a negative statistically significant effect on strike duration, while Gunderson and Melino find no statistically significant effect of firm size on strike duration. The third study, Budd (1996), focuses only on firms in the manufacturing sector.

The empirical analysis presented in our paper differs from that of earlier studies in a number of dimensions. First, we use data that cover the period from 1978 to 2003. This longer period of time encompasses substantially more variation in the use of anti-scab legislation than the time frame used in the earlier studies. Therefore we expect our results will provide more precise estimates of the impact of ATR on work stoppage incidence and length, and will not depend as heavily on the experience of Quebec. Second, we use province-level data. Earlier studies relied on data at the bargaining unit level. The higher level of aggregation in this study provides a different, not necessarily better, perspective than that of the earlier studies. A drawback of using province-level data is that bargaining theory concerning work stoppages is framed at the level of union–employer negotiations; with province-level data, it is not possible to control directly for specific employer or union characteristics. Nevertheless, it can be argued that since policy is made at the provincial level, empirical evidence concerning the province-level effects of ATR on work stoppages is

useful and appropriate for policy-making. Third, due to the nature of our data, we use cross-sectional time-series analysis rather than the probit, logit, or hazard analysis used in most of the earlier studies. Fourth, our dataset is not restricted to firms with 500 or more workers or to the manufacturing sector—it covers firms with one or more workers in the private, non-construction sector. These data allow us to address concerns about whether the results from some of the earlier studies, based on data for large firms or for the manufacturing sector, are representative of all firms. Finally, following Budd (1996, 2000), we use a more comprehensive set of replacement legislation dummies (described later in the paper) than some of the earlier Canadian studies.

EMPIRICAL APPROACH

We use cross-sectional time-series analysis to examine the impact of anti-scab legislation on work stoppages. This methodology is appropriate for the province-year data used in this project. Budd (2000) and Budd and Wang (2004) have used this type of analysis and province-month data to examine the impact of ATR on employment-to-population ratios and investment rates, respectively.¹⁰ In our study, reduced-form equations of the following general form are estimated:

$$\begin{aligned} (1) \text{workstoppage}_{it} = & \beta_0 + \beta_1(\text{ATR}_{it}) + \beta_2 \text{L2}_{it} \dots \\ & + \beta_k \text{LK}_{it} + \beta_{k+1} \text{UE}_{it} + \gamma_2 \text{PROV2}_{it} \\ & + \dots + \gamma_9 \text{PROV9}_{it} + \delta_2 \text{YEAR79}_t \\ & + \dots + \delta_2 \text{YEAR03}_t \\ & + \eta_1 \text{PROVTIME1}_{it} + \dots \\ & + \eta_9 \text{PROVTIME9}_{it} + \varepsilon_{it}, \end{aligned}$$

where i indicates province and t year. The dependent variable, *workstoppage*, measures incidence, length, or days lost. *ATR* is an indicator variable for the presence of anti-temporary replacement worker legislation. *L2* . . . *LK* are other labour law indicator variables. *UE* is the unemployment rate. The work stoppage, legislative, and unemployment

rate variables are described in more detail in the next section of the paper. PROV2 . . . PROV9 are province dummies. YEAR79 . . . YEAR03 are year dummies. PROVTIME1 . . . PROVTIME9 are province-specific time trends. The rationale for the inclusion of province dummies, year dummies, and province-specific time trends is described below. The error term is ε_{it} .

In order to isolate the effect of ATR on work stoppages, it is important to take into account other variables that influence work stoppages. This is the reason that the other legislative variables and the unemployment rate are included in the estimations. If variables are left out of the analysis that should have been included and these variables are related to ATR, the estimated effect of ATR on work stoppages will be biased. If this happens the influence of ATR on work stoppages will be contaminated by the effect of other omitted factors, and the empirical results will not correctly isolate (identify) the effect of ATR on work stoppages. Needless to say, this can be a serious empirical problem particularly since it is not possible to include all the variables in the analysis that may be important. Fortunately, cross-sectional time-series analysis permits the inclusion of variables that take into account unobserved factors that differ across provinces and over time. Province fixed effects (PROV2 . . . PROV9) capture the influence of factors not included in the analysis that differ across provinces but do not change over time (for example, different attitudes toward unions or different industry composition across provinces). Year effects (YEAR79 . . . YEAR03) capture the influence of factors not included in the analysis that differ over time but do not change across provinces (for example, countrywide macroeconomic shocks). Province-specific time trends (PROVTIME1 . . . PROVTIME9) capture the influence of factors not included in the analysis that change smoothly over time within a province but that can exhibit differing trends across provinces (for example, the declining private-sector unionization rate and changes in the provincial industry composition over time). We es-

timate specifications that include province fixed effects, year effects, and province-specific time trends, and therefore we have greater confidence that the empirical results correctly isolate the influence of ATR on work stoppages.

In cross-sectional time-series analysis, the nature of the error term (ε_{it}) can be considerably more complicated than in a cross-sectional analysis or in a time-series analysis. A number of possible error-relationships can exist: heteroskedasticity across provinces, correlation across provinces, common autocorrelation across provinces, and/or province-specific autocorrelation. If any of these error relationships are present, they must be taken into account in order to get appropriate standard errors. Diagnostic tests are performed for each estimated equation to determine whether any of these error-relationships exist. The results presented in the paper have been corrected, when appropriate, for the presence of these error-relationships.

This paper focuses on developing empirical evidence concerning the impact of anti-scab legislation on work stoppages. We do not attempt to place these results in the context of a particular theoretical model. There are a number of reasons for this. First, no general consensus has emerged concerning the most appropriate model. Second, the theories available often do not include the influence of specific public policy variables.¹¹ Third, when attempts are made to incorporate public policy variables into these models, the predictions concerning the impact of these variables on work stoppage incidence and duration are often ambiguous (Cramton et al. 1999; Gunderson et al. 1989). Therefore the models do not yield testable predictions for the variables of interest. Finally, testing the predictions of a particular bargaining model in order to determine its veracity is not the focus of this paper. Rather, its purpose is to provide policy-makers with evidence concerning the impact of anti-scab legislation (as well as other industrial relations public policies) on work stoppage incidence, length, and days lost.

DATA AND VARIABLES OF INTEREST

The data are for nine provinces from 1978 to 2003. Prince Edward Island and the territories are not included because of the small number of collective agreements in these jurisdictions. The federal sector is not included because data on the number of firms (needed to define the incidence and days lost variables) are not available for this jurisdiction. Data on firm counts by province, from the Longitudinal Employment Analysis Program (LEAP), are available from 1978 to 2003 and this determines the length of time covered and the periodicity of the analysis. Summary statistics are presented in Table 1.

Work Stoppage Variables

Work stoppage data are from the Workplace Information Directorate produced by Human Resources Development Canada. These data include all work stoppages, involving either first agreements or re-negotiated agreements, and do not distinguish between strikes and lockouts. We limit our analysis to all firms in the private, non-construction sector because the labour legislation we examine applies in this context.

The measure of *work stoppage incidence* is defined as

$$(2) \text{ incidence}_{it} = \frac{\text{work stoppages begun}_{it}}{\text{number of firms}_{it} \text{ (,000s)}}$$

It is necessary to scale the number of work stoppages that begin in a province in a year because the provinces differ in size. By doing so, we create the incidence variable—an index of work stoppage activity in a province in a calendar year. It would be ideal to create this variable by dividing by the number of collective agreements under negotiation or even by the number of bargaining units; unfortunately, such data are not available. We therefore divide the number of work stoppages by the total number of firms (measured in thousands) for each province-year.

Work stoppage length is measured as

$$(3) \text{ length}_{it} = \frac{\text{persondays lost in work stoppages}_{it}}{\text{number of workers involved}_{it}}$$

This measure is the number of person days lost per worker involved in a work stoppage in a calendar year.¹²

It is tempting to interpret this variable as the simple average days lost per work stoppage in a province in a year, but this is not possible. *If we assume, for the moment, that all work stoppages begin and end in a calendar year*, “length” is equivalent to the *weighted* average of days lost per work stoppage. The weights capture the size of the bargaining unit—larger bargaining units have higher weights in the calculation.¹³ If we were interested in evaluating anti-scab legislation from society’s perspective, it could be argued that a weighted average of days lost per work stoppage would be a better measure of the cost of labour disputes than a simple average of days lost per work stoppage that does not reflect the number of workers involved. However, “length” accurately measures the weighted average of days lost per work stoppage only if we can assume that all work stoppages begin and end in a calendar year. Unfortunately, this is not the case; work stoppages can last longer than one year and sometimes straddle two years. In these situations the total person days lost associated with a given work stoppage would be divided between two (or more) calendar years. Therefore “length” does not provide us with an accurate weighted average of days lost to work stoppages. If we can assume that most work stoppages last less than one year and are randomly distributed over calendar years across all provinces and time periods, regardless of the presence of ATR, then for our purposes “length” may not be a bad approximation of the weighted average of days lost per work stoppage. Evidence from studies that examine duration at the bargaining unit level suggests that most work stoppages last considerably less than one year.¹⁴ However, there is evidence that work stoppages are associated with the business cycle and

may have seasonal components (see, for example, Vroman 1989). The unemployment rate is included in the analysis to control for cyclical factors that affect work stoppages.

The *days lost to work stoppages* variable is measured as

$$(4) \text{ dayslost}_{it} = \frac{\text{person days lost to work stoppages}_{it}}{\text{number of firms}_{it}(.000s)}$$

This variable captures the number of days lost to work stoppages in a year per thousand firms in a jurisdiction. Both incidence and length affect days lost to work stoppages:

$$(5) \text{ dayslost}_{it} = \text{incidence}_{it} * \text{length}_{it} * \frac{\text{\#workers involved}_{it}}{\text{\#work stoppages begun}_{it}}.$$

An increase in either incidence or length, holding all else constant, would increase the number of days lost to work stoppages.¹⁵

Legislation Variables

Many explanatory variables are included in the analysis to capture the effect of different labour laws on work stoppages. Table 2 provides information on the labour laws included for each jurisdiction in Canada from 1978 to 2003. In most provinces, once a policy is introduced it remains in effect (where this is not the case, a second year:month number indicates when the legislation was repealed). Dummy variables are created for each type of legislation.¹⁶ Table 2 provides evidence that there is considerable variation in labour laws over time and across provinces in Canada. This variation is important for identifying the impact of ATR on work stoppages. Each type of legislation is described briefly below.¹⁷

TABLE 1
Descriptive Statistics

<i>Variable</i>	<i>Mean</i>	<i>Standard Deviation</i>
Incidence	.356	.376
Length	34.15	28.85
Days lost	2297.46	5006.62
Anti-temporary replacement	.17	.37
Reinstatement rights	.28	.45
Ban professional strikebreakers	.06	.24
First agreement arbitration	.51	.50
Compulsory dues check-off	.62	.48
Cool-off period (days)	7.36	5.83
Mandatory strike vote	.78	.41
Employer-initiated vote	.38	.48
Technological reopener	.40	.49
Unemployment rate	10.01	3.73
No. observations	234	

Source: Authors' calculations.

TABLE 2
Canadian Labour Legislation, 1978 to 2003

	<i>Anti-temporary Replacement</i>	<i>Ban on Permanent Replacement</i>	<i>Reinstatement Rights</i>	<i>Ban on Strikebearers</i>	<i>Compulsory Dues Check-off</i>	<i>First Agreement Arbitration</i>	<i>Mandatory Strike Vote</i>	<i>Employer-Initiated Strike Vote</i>	<i>Compulsory Conciliation^a</i>	<i>Cool-off Period^b</i>	<i>Technological Reopener</i>
BC	93:1			78	78	78	78	87:8		78(3)	78
AB			88:11				78	88:12	78–81:2 88:12	78–81:2(14) 81:3–88:11(3) 88:12(14)	
SK			94:10		78	94:10	78	83:7		83:7(2)	78
MB		85:1	85:1	85:1	78	82:2	85:1	97:2–00:10			78
ON	93:1–95:11		78–92:12 95:11	83:6	80:7	86:5	95:11	80:7	78–86:12	78(14)	
QC	78:2		78:2		78:4	77:12	78:4	02:11	76–78:1	77:12(2)	
NB							78		78	78(7)	89:4
NS							78	94:5	78	78(14)	
NF					85:7	85:6			78	78(14)	
PEI		87:5	87:5			95:5	78		78	78(14)	
Fed.			99:1	99:1	84:7	78:4	99:1	93:6	78	78(7)	78

Notes: The numbers in the table refer to the year:month the legislation takes effect. If the legislation was already in effect in 1978, only the year, 78, is given. In many instances, once introduced the legislation remains in effect. If the legislation is repealed, the year:month is provided.

^a Compulsory conciliation refers to either a conciliation officer or a board.

^b The number of cooling-off days is shown in parentheses.

Source: This information is compiled from a number of sources. Data on replacement worker legislation to 1993 (anti-temporary replacements, ban on permanent replacements, reinstatement rights and ban on professional strikebreakers) are from Budd (2000, 227–29). Data on compulsory conciliation, cooling-off period, mandatory strike vote, and employer-initiated vote to 1993 are from Cramton, Gunderson, and Tracy (1999, 478; Table 3). Data on first agreement arbitration and dues check-off to 1997 are from Johnson (2002, 349; Table 1). The information has been updated to 2003 using *Highlights of Major Developments in Labour Legislation* (Human Resources and Skills Development Canada 1978–2003).

Replacement worker legislation takes three forms. The strongest form is anti-temporary replacement worker legislation. This type of legislation prohibits the hiring of temporary replacement workers during a work stoppage and places limitations on the use of existing employees.¹⁸ ATR is introduced with the intention of reducing picket line violence, improving the quality of the relationship between union and management, and reducing other societal costs associated with work stoppages. Ideological and distributional considerations may also play an important role in the decision to adopt ATR. Other replacement worker legislation prohibits firms from hiring permanent replacement workers or guarantees striking workers their jobs back within a specified length of time. This guarantee, in essence, prohibits the hiring of permanent replacement workers. Although reinstatement rights legislation is unlikely to reduce picket line violence, other reasons given for the introduction of ATR also apply to the decision to adopt this type of legislation. Finally, there is legislation that prohibits the hiring of professional strikebreakers—workers hired with the purpose of undermining the union. This type of legislation is designed to prevent the industrial relations experience of the United States, where professional strikebreakers have disrupted labour relations, from being imported to Canada. We follow Budd (1996, 2000) and define three indicator variables that reflect the strongest form of replacement worker legislation in effect in a jurisdiction in each year. Anti-scab legislation is the strongest form of replacement worker legislation and, when in effect in a jurisdiction, this indicator variable is equal to one. The reinstatement¹⁹ indicator variable equals one if this is the only type of replacement worker legislation in effect or if there is also a ban on professional strikebreakers. The professional strikebreaker variable equals one if this is the only form of replacement worker legislation in effect.

First agreement arbitration (FAA) allows the union or firm to request an arbitrator to decide the terms and conditions of a first collective agreement. The first collective agreement is prone to work stop-

pages for a number of reasons. First, the parties are engaged in the difficult task of negotiating an agreement that covers all facets of the employment relationship, rather than making marginal changes to an existing agreement. Second, the management and union attempt to negotiate this agreement in the context of a new, immature bargaining relationship. Third, management may adopt bargaining tactics (stonewalling, bargaining in bad faith) that are really attempts to defeat the union. FAA is designed to protect the bargaining rights of workers and ensure that a first collective agreement is achieved. The presence of FAA is expected to reduce the number of work stoppages by providing the parties with an alternative dispute-resolution mechanism.

Compulsory dues check-off requires that at the request of the union the employer must deduct the equivalent of union dues from all members of the bargaining unit whether or not they are members of the union and remit the revenue to the union. This legislation is introduced to limit the “free rider” problem in a bargaining unit and to provide the union with the financial wherewithal to effectively represent the workers. In the past, the negotiation of dues check-off clauses has been a contentious issue. By removing this issue from the bargaining table, dues check-off legislation is expected to reduce the number and possibly the length of work stoppages.

Compulsory conciliation involves third parties in the dispute resolution process. Under compulsory conciliation, if union and management reach an impasse, a conciliation process must be followed before a legal work stoppage can begin. Some jurisdictions have provision for a conciliation officer to intervene. Other jurisdictions have an additional level of intervention involving boards that can make public recommendations. We include both types of legislation in the compulsory conciliation indicator variable. This legislation is designed to ensure that there is no miscommunication between the parties and, if possible, to encourage a settlement. By ensuring clear communication on critical issues and

mediating issues in dispute, the conciliation process may reduce the number and length of work stoppages.

Cool-off period is the requirement for a period of time after other legal requirements for a legal work stoppage have been fulfilled before the work stoppage can begin. The cool-off period varies from 2 to 14 days.²⁰ Providing the parties with an opportunity to “cool down” before proceeding with a work stoppage may reduce the incidence and length of work stoppages.

Mandatory strike vote, as the name suggests, requires that the union conduct a secret ballot to determine whether the bargaining unit supports the strike. The intent of this policy is to ensure that the majority of workers in the bargaining unit actually support the strike. This policy could reduce the number of strikes in situations where the union leadership is out of touch with its members. To the extent that union leadership fairly represents the bargaining unit, this policy may have no impact on reducing work stoppages.

An *employer-initiated strike vote* allows the employer to request that the bargaining unit vote, in a secret ballot, on their final offer. The intent of this policy is to ensure that the union leadership is fairly reflecting the interests of the majority of its members in its negotiations. Employers would use this policy when they believe that union negotiators are out of touch with the majority of members. By taking the final offer to the membership, the employer may prevent a work stoppage. As in the case of the mandatory strike vote, the practical importance of this policy in reducing work stoppages is questionable if the union fairly represents the interests of a majority of its members.

A *technological reopener* is legislation that allows, at the union’s request, for a collective agreement to be reopened before its expiry in order to address issues concerning technological change. This legislation removes complicated, difficult negotiations concerning job security from the

bargaining table and may reduce the number and length of work stoppages.

The other measured variable included in the analysis is the provincial unemployment rate for people over the age of 15 years. These data are from the Labour Force Survey, Annual Averages, downloaded from CANSIM (Matrix no. 282002). Additional variables include the province fixed effects, year fixed effects, and province-specific time trends. These variables were discussed earlier.²¹

RESULTS

Results are presented in Tables 3, 4, and 5. Specifications were first estimated using ordinary least squares without error-correction. Diagnostic tests on these regressions detect the presence of groupwise heteroskedasticity and autocorrelation in all specifications. Therefore the equations were re-estimated using feasible generalized least squares, taking into account the presence of groupwise heteroskedasticity, panel correlation, and panel-specific autocorrelation. In addition, Wald tests show that province-specific time trends are jointly significant and not equal. Therefore, specifications with a common time trend were dropped and province-specific time trends were included in all specifications. Results presented in Tables 3, 4, and 5 include province fixed effects and province-specific time trends either with or without year effects.

Results for Work Stoppage Incidence

Table 3 presents the results for work stoppage incidence, the number of work stoppages begun per thousand firms in a jurisdiction per year. The coefficient on anti-temporary replacement worker legislation is positive and has a statistically significant effect (at the 99 percent level) in both specifications. The coefficient is also of substantial magnitude. The mean of incidence is .356; thus the coefficients imply that anti-scab legislation increases strike incidence by about 50 percent. Reinstatement rights legislation also has a positive, statistically

TABLE 3
Work Stoppage Incidence

<i>Variable</i>	<i>Specification 1</i>	<i>Specification 2</i>
Anti-temporary replacements	.184** (.053)	.180** (.062)
Reinstatement rights	.140** (.041)	.184** (.039)
Ban strikebreakers	.189 (.106)	.161* (.077)
First agreement arbitration	-.155** (.042)	-.173** (.042)
Dues check-off	-.177** (.051)	-.093 (.064)
Cool-off period (days)	-.021** (.005)	-.012* (.006)
Compulsory conciliation	.092 (.048)	-.053 (.060)
Mandatory strike vote	.060 (.048)	.022 (.053)
Employer-initiated vote	-.006 (.029)	.032 (.024)
Technological reopener	-.030 (.110)	-.004 (.152)
Unemployment rate	-.038** (.004)	-.006 (.005)
Province effects	YES	YES
Year effects	NO	YES
Province-specific time trends	YES	YES
Mean of dependent variable	.356 (.376)	

Notes: Numbers in parentheses are standard errors of the coefficient. Diagnostic tests were performed to determine the appropriate error-correction. All the equations were estimated using feasible generalized least squares correcting for groupwise heteroskedasticity, panel-correlation, and panel-specific autocorrelation. Regression results are robust to the exclusion of outliers.

**Indicates significant at the 99 percent level. *Indicates significant at the 95 percent level.

Source: Authors' calculations.

significant effect (at the 99 percent level) on incidence. The estimated coefficients imply that the presence of reinstatement rights increases the work stoppage rate by at least 39 percent. A ban on professional strikebreakers also appears to have a positive effect on incidence that is substantial in magnitude, but the estimated coefficient is statistically significant only in the second specification.²² The unemployment rate can be used as an additional benchmark for judging the magnitude of the effects of ATR and reinstatement rights legislation on work stoppage incidence: the effect of ATR legislation is roughly equivalent to a 5 percentage point change in the unemployment rate, while the effect of reinstatement rights legislation is roughly equivalent to a 4 percentage point change in the unemployment rate.

Now let's turn to an examination of the other variables included in the strike incidence estimations. First agreement arbitration (FAA) appears to reduce work stoppage incidence. The estimated coefficients are negative and significant (at the 99 percent level) in both specifications. The coefficients suggest that the presence of FAA reduces incidence by at least 43 percent. Dues check-off may also reduce work stoppages, but this effect is statistically significant only in the first specification. The presence of a cool-off period also reduces work stoppage incidence; the coefficients on this legislative variable are negative and statistically significant in both specifications. The magnitude of this effect depends on the number of days provided in the cool-off period and can be quite large. The other legislative variables (compulsory conciliation, mandatory strike vote, employer-initiated strike vote, and technological reopener clauses) do not appear to have a statistically significant effect on strike incidence. The coefficient on the unemployment rate is negative. It makes sense that this coefficient is statistically significant only in the specification that does not include year effects.

Gunderson et al. (1989), Cramton et al. (1999), and Budd (1996), using data at the bargaining unit level, discovered that the presence of anti-scab leg-

islation increased the likelihood of a work stoppage. The effect was substantial in size; however, in two of the three studies the estimated coefficients were not statistically significant. Our results, using province-level data, also show that ATR increases work stoppage incidence. The results in this study are measured with more precision because there is greater variation in the use of replacement worker legislation over the longer time period covered by our analysis. In addition, our results show that the positive effect of ATR on work stoppages applies generally to all firms in the private non-construction sector—it is not limited to very large firms or to the manufacturing sector.²³

Results for Work Stoppage Length

The strike length (days lost per year for each worker involved in a work stoppage) results are presented in Table 4. The presence of anti-scab legislation appears to reduce the number of days that a typical worker can expect to lose to work stoppages per year by a substantial amount. The estimated coefficients in each specification are negative and statistically significant (at the 99 percent level). The mean of length is 34.15; the sizes of the coefficients (–34.87 and –48.90) therefore suggest that the presence of anti-temporary replacement worker legislation reduces length by more than 100 percent. Reinstatement rights legislation also has a statistically significant, large negative effect on length. The ban on professional strikebreakers appears to have an even larger statistically significant negative impact on strike length.²⁴

Other legislative variables do not appear to have as large an impact on work stoppage length as the replacement worker legislation variables. The results are mixed for cool-off period, mandatory strike vote, and employer-initiated vote—these public policies appear to increase work stoppage length but are statistically significant in only one of the two specifications. First agreement arbitration, dues check-off, compulsory conciliation, and technological reopener do not have a statistically significant impact on work stoppage length in either specification.

TABLE 4
Work Stoppage Length

<i>Variable</i>	<i>Specification 1</i>	<i>Specification 2</i>
Anti-temporary replacements	-34.87** (11.44)	-48.90** (12.85)
Reinstatement rights	-44.96** (11.78)	-42.05** (12.77)
Ban strikebreakers	-54.78** (17.23)	-68.55** (20.08)
First agreement arbitration	1.41 (5.58)	3.14 (6.52)
Dues check-off	1.00 (7.74)	-9.18 (8.52)
Cool-off period (days)	3.97* (1.80)	3.41 (1.90)
Compulsory conciliation	-8.48 (8.88)	-1.48 (9.70)
Mandatory strike vote	28.20** (9.82)	16.42 (10.04)
Employer-initiated vote	8.91 (5.28)	13.08* (6.22)
Technological reopener	54.34 (33.33)	52.99 (34.51)
Unemployment rate	1.75** (.594)	3.19* (1.35)
Province effects	YES	YES
Year effects	NO	YES
Province-specific time trends	YES	YES
Mean of dependent variable	34.15 (28.85)	

Notes: Numbers in parentheses are standard errors of the coefficient. Diagnostic tests were performed to determine the appropriate error-correction. All the equations are estimated using feasible generalized least squares correcting for groupwise heteroskedasticity, panel-correlation, and panel-specific autocorrelation. Regression results are robust to the exclusion of outliers.

**Indicates significant at the 99 percent level. *Indicates significant at the 95 percent level.

Source: Authors' calculations.

Earlier studies have found positive effects of anti-temporary replacement worker legislation on strike duration. It is difficult to compare our results with the findings of earlier literature because in those studies the dependent variable is measured differently, a different empirical methodology is used, and the data are at a different level of aggregation. As well, the time period covered by the data in the earlier hazard-model studies means the identification of the impact of ATR on duration depends very heavily on the Quebec experience. Nevertheless, our results do appear to be somewhat at odds with the results of earlier duration studies.

Results for Days Lost

The results described above show that ATR has a *positive*, statistically significant and substantial effect on strike incidence, and a *negative*, statistically significant and substantial effect on strike length. Based on the relationship described in equation (5), these results imply that the presence of anti-scab legislation has an ambiguous effect on days lost: anti-scab legislation increases the incidence of strikes, which holding all else constant increases days lost, yet anti-scab legislation decreases the length of strikes, which holding all else constant decreases days lost. Therefore in order to determine the overall impact of ATR on days lost to work stoppages per thousand firms, a separate empirical analysis is performed. The results are presented in Table 5.

The evidence suggests that ATR may be associated with an increase in days lost to work stoppages. The estimated coefficient on ATR is positive and substantial in magnitude in both specifications; however, the coefficient is statistically significant (at the 95 percent level) only in the first specification. Reinstatement rights and banning professional strikebreakers appear to have no statistically significant effect on days lost.

Results for the other public policy variables show that first agreement arbitration and dues check-off are associated with substantial and statistically significant (at the 99 percent level) decreases in days

lost to work stoppages. Technological reopener legislation increases days lost by a substantial and significant amount in both specifications. Compulsory conciliation and employer-initiated votes appear to decrease days lost, but the coefficients on these variables are not statistically significant. Cool-off period and mandatory strike vote are not statistically significant in either specification.

Results for Time-Varying Specifications

Specifications are also estimated that allow the relationship of ATR legislation to vary over time with each different work stoppage measure. Following Budd (2000), specifications are estimated that examine the time-varying effects on work stoppages for the period one to two years before the legislation takes effect, the first two years the legislation is in effect, and two or more years after the legislation takes effect.²⁵ There are two reasons for doing this. First, it is important for the analysis that policy changes are exogenous. Examining the behaviour of the work stoppage variables in the years prior and two years after ATR comes into effect provides an informal check on whether exogeneity is a plausible assumption. Second, it is possible to determine the longer-term impact of ATR by examining its relationship to work stoppages when it has been in effect longer than two years. A discussion of the time-varying effects of the other forms of replacement legislation is presented in the Appendix.

Table 6 presents the results of time-varying specifications for anti-scab legislation. These results provide support for the claim that ATR is exogenous. The coefficient on the period up to two years before the legislation is introduced is not statistically significant in five of the six specifications. In contrast, the coefficient covering the two year period immediately after ATR comes into effect is statistically significant (and substantial in size) in five of the six specifications. This evidence suggests there is no systematic change in incidence, length, or days lost in the two years prior to ATR taking effect. However, in the two years immediately after it takes effect, ATR is correlated with a statistically

TABLE 5
Days Lost

<i>Variable</i>	<i>Specification 1</i>	<i>Specification 2</i>
Anti-temporary replacements	1547.91* (654.50)	879.39 (627.11)
Reinstatement rights	229.94 (525.68)	720.09 (459.19)
Ban strikebreakers	1715.29 (2670.12)	165.72 (2211.74)
First agreement arbitration	-2054.62** (414.44)	-2234.35** (482.82)
Dues check-off	-3245.42** (743.91)	-3545.05** (667.57)
Cool-off period (days)	72.43 (77.71)	41.63 (67.84)
Compulsory conciliation	-984.63 (698.36)	-1951.98** (671.44)
Mandatory strike vote	824.98 (567.11)	208.02 (519.96)
Employer-initiated vote	-975.61** (345.88)	-5.79 (338.07)
Technological reopener	3439.75* (1543.23)	5239.91** (1690.35)
Unemployment rate	-447.68** (67.01)	7.39 (81.6)
Province effects	YES	YES
Year effects	NO	YES
Province-specific time trends	YES	YES
Mean of dependent variable	2297.46 (5006.62)	

Notes: Numbers in parentheses are standard errors of the coefficient. Diagnostic tests were performed to determine the appropriate error-correction. All the equations are estimated using feasible generalized least squares correcting for groupwise heteroskedasticity, panel-correlation, and panel-specific autocorrelation. Regression results are robust to the exclusion of outliers.

**Indicates significant at the 99 percent level. *Indicates significant at the 95 percent level.

Source: Authors' calculations.

significant, substantial impact on the dependent variable. While these results cannot prove ATR legislation is exogenous, this circumstantial evidence lends fairly persuasive support to the argument that ATR legislation is exogenous.

It is interesting to examine the behaviour of the coefficient on ATR when this legislation has been in effect for more than two years. In the incidence and length regressions, the coefficient continues to be statistically significant and substantial (though somewhat smaller in absolute size compared with the coefficient for the period immediately after the policy takes effect). This evidence shows that the effect of anti-scab legislation on incidence and length persists over time. Note, however, that in the

days lost regressions neither coefficient on the period when ATR has been in effect for more than two years is statistically significant. Thus there is weak evidence that ATR may increase days lost to work stoppages in the two years immediately after it takes effect but no evidence that anti-scab legislation increases days lost to work stoppages when it has been in effect for more than two years.

POLICY IMPLICATIONS AND CONCLUSION

Legislation that prohibits the use of temporary replacement workers during work stoppages is controversial. The decision whether or not to adopt ATR is multidimensional: many benefits and costs

TABLE 6
Time-Varying Effects of Anti-temporary Replacement Worker (ATR) Legislation

	<i>Incidence</i>		<i>Length</i>		<i>Days Lost</i>	
1 to 2 years before ATR in effect	-.014 (.038)	-.081* (.041)	-1.08 (6.42)	-8.07 (7.51)	-142.25 (640.93)	-635.7 (517.5)
ATR in effect for up to 2 years	.181** (.054)	.157** (.060)	-33.6** (10.7)	-48.9** (12.56)	1567* (697.2)	1177 (631.2)
ATR in effect for more than 2 years	.143* (.060)	.138** (.064)	-31.5** (11.29)	-41.98** (13.32)	493.5 (746.4)	-878.0 (737.5)
Controls ^a	YES	YES	YES	YES	YES	YES
Province fixed effects	YES	YES	YES	YES	YES	YES
Province-specific time trends	YES	YES	YES	YES	YES	YES
Year effects	NO	YES	NO	YES	NO	YES

Notes: The numbers in parentheses are standard errors of the coefficient. All equations were estimated with feasible generalized least squares correcting for groupwise heteroskedasticity, panel-correlation, and panel-specific autocorrelation. The ATR variables are defined by dummies equal to one for each year the statement describing the variable is true.

^a All specifications include a common set of control variables: reinstatement rights, ban on strikebreakers, first agreement arbitration, dues check-off, cooling-off period, compulsory conciliation, mandatory strike vote, employer-initiated vote, technological reopener, and the unemployment rate.

**Indicates significant at the 99 percent level. *Indicates significant at the 95 percent level.

Source: Authors' calculations.

need to be considered. The empirical results provided by this study contribute constructively to this policy debate by providing objective evidence concerning the impact that anti-scab legislation is likely to have on work stoppage incidence, length, and days lost. These results are particularly useful for policy-makers for a number of reasons. First, the evidence applies to the context in which labour policy is made. The results apply to *all* private-sector, non-construction work stoppages. Second, the data cover a long period of time, 1978 to 2003; therefore the results do not rely as heavily (as some earlier research) on the experience of Quebec, and the impact of ATR can be estimated more precisely. Third, the empirical results, based on province-level data, provide evidence of the impact of labour policy at the provincial level of aggregation. Since labour policy is made at the provincial level, it can be argued that evidence provided at this level of aggregation provides a useful perspective for policy-makers.

The results show that ATR increases the expected number of work stoppages per thousand firms (incidence) and decreases the number of days lost per worker on strike in a calendar year (length). Both of these effects are robust across specifications, substantial in magnitude, and statistically significant at the 99 percent level. The results concerning the influence of anti-scab legislation on total person days lost in a calendar year per thousand firms (days lost) are not as strong as those of the other work stoppage variables. This is not surprising, given that incidence and length have opposite effects on days lost. There is mixed evidence concerning whether or not ATR increases days lost to work stoppages in the two years immediately after the legislation comes into effect; however, ATR appears to have no statistically significant effect on days lost when the policy has been in effect for longer than two years. The evidence concerning the impact of ATR on days lost suggests that any short-run output losses that might be associated with the introduction of anti-scab legislation are limited to the two-year period immediately after the legislation comes into effect. The results also show that it may be possible to off-

set the potentially detrimental effect of ATR of increasing work stoppage incidence by introducing a reform package that also includes first agreement arbitration or a cooling-off period of seven days.

NOTES

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¹ This proposed federal legislation, Bill C-257, passed its second reading in the House of Commons in October 2006 by a margin of 167 to 101. It was defeated on 21 March 2007 by a vote of 177 to 122.

² See, for example, the Canadian Labour Congress's statement of support of Bill C-257 at http://canadianlabour.ca/index.php/antiscab_legislation. This bill is also supported by the Canadian Auto Workers, Canadian Union of Public Employees, and other union groups.

³ The hiring of permanent replacement workers during a work stoppage may result in de facto decertification of the union. (Note that this type of union resistance can also be prevented by weaker forms of replacement worker legislation that either prohibits the hiring of permanent replacements or provides striking workers with reinstatement rights.)

⁴ However, existing empirical evidence suggests that ATR increases the probability and also the duration of work stoppages. Specific studies will be discussed in more detail later in the paper.

⁵ See, for example, the position of the Canadian Federation of Independent Business on Bill-257 at http://www.cfib.ca/legis/national/nat_billc257.asp

⁶ Budd's (2000) results show that ATR is associated with statistically significant decreases in provincial employment-to-population ratios of 0.5 to 1.3 percentage points on average. When he examines the employment effects of ATR using data at the bargaining unit level, he finds statistically significant reductions in bargaining unit employment rates of 8 to 12 percentage points.

⁷ Budd and Wang (2004) provide evidence that anti-scab legislation is associated with statistically significant decreases in investment rates (investment net of depreciation/capital stock) of 25 percent.

⁸ Gunderson and Melino (1990) also present results on strike incidence, but the results discussed here are from Gunderson, Kervin, and Reid (1989). Gunderson and Melino use their results to calculate changes in unconditional strike duration.

⁹ Calculated from data provided by Akyeampong (2004) and Kanagarajah (2006). These data do not distinguish between the public and private sectors.

¹⁰ Budd (2000) also performs a separate analysis that uses disaggregated bargaining unit-level data.

¹¹ When theory provides predictions concerning the impact of anti-scab legislation on work stoppages, the predictions are sensitive to the assumptions of the model. For example, Cramton and Tracy (2003) provide a model of work stoppages based on uncertainty and asymmetric information. The model predicts that anti-scab legislation increases the number of work stoppages and also increases their duration. The predictions of this model are sensitive to assumptions made concerning the nature of the asymmetry of the information and the behaviour of fixed costs over time.

¹² When working with province-level data, it is not possible to define the duration of a work stoppage in the same way as when using bargaining unit-level data. The length variable is an attempt to define a measure at the province-level that is analogous to duration at the bargaining unit level. However, as the following discussion makes clear, length is not the same as duration.

¹³ If work stoppage length is independent of bargaining unit size, this average will reflect the simple unweighted average strike length. If larger units have longer strikes, then the weighted average will be higher than the unweighted average. If larger units have shorter strikes, the weighted average will be lower than the unweighted average. Earlier studies present mixed results on the impact of bargaining unit size on strike duration: Gunderson and Melino (1990) conclude bargaining unit size has no impact on duration, while Cramton et al. (1999) find that larger bargaining units have significantly shorter strike durations.

¹⁴ Gunderson and Melino (1990), using data for all sizes of private sector firms from 1967 to 1985, find the average strike duration is 36 days and the median duration is 19 days. Cramton et al. (1999), using data for all sizes of private sector firms from 1967 to 1993, find the average strike duration is 49 days. Budd (1996), using data for all firms in the manufacturing sector from 1965 to 1985, finds the average strike duration is 55.5 days. These earlier studies, which correctly measure strike duration, suggest that strikes typically last considerably less than one year. It is interesting to note that the length variable in our study has remarkably similar descriptive statistics to that of Gunderson and Melino (1990) for a similar sample but different time period. In our sample, the length variable has a mean of 34 days and a median of 27 days.

¹⁵ The final term in equation (5) is an approximate measure of the typical size of bargaining units involved in a work stoppage.

¹⁶ The dummy takes the value one when the legislation is in effect and the value zero when it is not in effect. In years where legislation is introduced or repealed, the dummy indicates the proportion of the year that the legislation was in effect. The exception to this is the cool-off legislation variable that is coded as the number of days of the cool-off period.

¹⁷ These legislation variables are almost identical to those included in the studies of Gunderson et al. (1989), Gunderson and Melino (1990), and Cramton et al. (1999). Gunderson et al. provide very helpful descriptions of these policies and their intended purpose. The following discussion relies heavily on information provided in their paper. One type of labour legislation included in these earlier studies but not in our study is negotiated reopeners. Over the time period that we study, every jurisdiction either permitted negotiated reopeners in all years (Saskatchewan, Manitoba, Quebec, New Brunswick, Nova Scotia, and Newfoundland) or did not permit this type of legislation in any year (British Columbia, Alberta, and Ontario). Therefore the impact of negotiated reopeners is completely captured by the province dummies. Variables included in our study but not in these earlier papers include first agreement arbitration, reinstatement rights, and a ban on professional strikebreakers.

¹⁸ ATR legislation in Quebec will not allow employers to replace striking workers with anyone hired after the

start of negotiations, workers from the striking bargaining unit who do not support the strike, or workers from other areas of the firm who are not in the striking bargaining unit. The legislation also limits employers' ability to have the work of the striking workers performed on site by employees of another employer.

¹⁹ Reinstatement legislation includes reinstatement rights and a ban on permanent replacement workers.

²⁰ The cool-off variable reflects the number of days in the cool-off period. It is not a binary variable.

²¹ We did not include such variables as political climate, union strength, and industrial structure in the analysis because we are not specifically interested in the impact of these variables on work stoppages. It is expected that the influence of these factors is adequately captured by the province fixed effects, province-specific time trends, and year effects.

²² A number of outliers occur in the early years of the strike incidence data. To ensure that the results were not being driven by outliers, the regressions were run for the period 1982–2003. The results are robust to the exclusion of these outliers.

²³ Cramton and Tracy (2003) provide some economic intuition for these results in their non-competitive wage bargaining model of strikes and holdouts. This is a model with asymmetric information; the firm knows its willingness to pay and the cost of hiring replacement workers, but the union does not. The firm has no incentive to reveal this information to the union. Labour disputes occur in order for the union to obtain credible information from the firm. This model predicts that by prohibiting the hiring of replacement workers, ATR legislation makes the strike threat more attractive, and so the union is more likely to choose a strike rather than a holdout in a labour dispute.

²⁴ It is hard to understand why a ban on professional strikebreakers could have such a large impact on strike length. This is not particularly "strong" legislation—professional strikebreakers have not been prevalent in Canada and the legislative dummy is only turned on in BC to 1993. We ran all the regressions again without this variable. The results for strike incidence are robust to its exclusion—the coefficients on ATR and reinstatement legislation are still positive, statistically significant, and large in mag-

nitude (though smaller than in the original specification). The results for the strike length regressions are somewhat sensitive to the exclusion of the ban on professional strikebreakers. The coefficients on ATR are still negative but are smaller in size and no longer statistically significant. The coefficients on reinstatement legislation are still negative, statistically significant, and large in size (–36 and –30).

²⁵ Budd (2000) also examines the time-varying effects of legislation concerning reinstatement rights and professional strikebreakers.

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APPENDIX

TIME-VARYING RESULTS FOR OTHER FORMS OF REPLACEMENT WORKER LEGISLATION

Reinstatement Rights

Table A1 presents the results for the time-varying effects of reinstatement rights. The coefficient for the period up to two years before reinstatement rights legislation comes into effect is statistically significant in five of the six specifications. When significant the coefficients indicate that incidence, length, and days lost are higher in the two years prior to the introduction of reinstatement legislation. While these results do not prove that reinstatement rights are not exogenous, they provide no support for the claim that reinstatement rights legislation is exogenous. As in the non-time-varying results, reinstatement rights are associated with a statistically significant increase in incidence for most specifications in the years after the legislation is in effect. However, reinstatement rights no longer have a statistically significant negative effect on length—though the coefficient does become smaller in the periods after the legislation comes into effect. Reinstatement rights are associated with a statistically significant increase in days lost for all time periods—before and after reinstatement rights legislation is in effect. It is interesting to note that Budd (2000, see 236-40) cannot show that reinstatement rights are exogenous in his time-varying specifications and has difficulty interpreting the coefficients in these specifications.

Ban on Professional Strikebreakers

We cannot examine the time-varying effects of a ban on professional strikebreakers using our data. British Columbia is the only jurisdiction where a ban on professional strikebreakers was the strongest form of replacement worker legislation in effect—from 1974 to 1993. Since the legislation came into effect in 1974, it is not possible to define a period in our data, which begins in 1978, for the two years before and after the legislation came into effect. (Budd [2000] is able to construct the time-varying dummy on the ban on professional strikebreakers because his data cover 1966 to 1993.)

TABLE A1
Time-Varying Effects of Reinstatement Rights Legislation (RRL)

	<i>Incidence</i>		<i>Length</i>		<i>Days Lost</i>	
1 to 2 years before RRL in effect	.009 (.025)	.062** (.023)	29.07** (8.26)	30.57** (8.30)	1678.5** (263.77)	1333.70** (310.49)
RRL in effect for up to 2 years	.062 (.038)	.179** (.034)	3.89 (14.80)	1.87 (15.26)	1180.16** (400.08)	2043.43** (458.48)
RRL in effect for more than 2 years	.093* (.041)	.219** (.036)	-.205 (12.64)	-2.69 (13.00)	1732.34** (465.78)	2402.82** (464.61)
Controls ^a	YES	YES	YES	YES	YES	YES
Province fixed effects	YES	YES	YES	YES	YES	YES
Province-specific time trends	YES	YES	YES	YES	YES	YES
Year effects	NO	YES	NO	YES	NO	YES

Notes: The numbers in parentheses are standard errors of the coefficient. All equations are estimated with feasible generalized least squares correcting for groupwise heteroskedasticity, panel-correlation, and panel-specific autocorrelation. The reinstatement legislation dummies are equal to one for the years in which the statement describing the variable is true.

^a All specifications include a common set of control variables: ban on replacement workers, ban on strikebreakers, first agreement arbitration, dues check-off, cooling-off period, compulsory conciliation, mandatory strike vote, employer-initiated vote, technological reopener, and the unemployment rate.

**Indicates significant at the 99 percent level. *Indicates significant at the 95 percent level.

Source: Authors' calculations.