UNION CERTIFICATION SUCCESS UNDER VOTING VERSUS CARD-CHECK PROCEDURES: EVIDENCE FROM BRITISH COLUMBIA, 1978–1998

CHRIS RIDDELL*

The author estimates the impact of compulsory election laws on certification success using data on over 6,500 private sector certifications from British Columbia over the years 1978–98. A unique quasi-experimental design is used by exploiting two changes in the union recognition law: first, in 1984, the introduction of mandatory elections; and second, in 1993, the repeal of elections and their replacement by the original card-check procedure. The author also estimates the effectiveness of management opposition tactics across union recognition regimes. Success rates declined by an average of 19 percentage points during the voting regime, and then increased by about the same amount when card-checks were re-instituted. The results indicate that the mandatory election law can account for virtually the entire decline. In addition, the findings suggest that management opposition was twice as effective under elections as under card-checks.

One of the most important public policies affecting the union organizing process is the recognition procedure, that is, the mechanism by which the union attempts to demonstrate that it has sufficient support within the proposed bargaining unit to be certified. Up to the late 1980s, all Canadian provinces (except Nova Scotia after 1977) had "card-check" procedures, whereby the union could be certified without an election if it signed up a sufficient proportion of the proposed bargaining unit. The proportion required to avoid an election varied by province, but generally ranged between 50% and 55%. While it was possible for a certification bid to require a vote in a card-check province, few organizing drives actually went to an election. The Canadian system in those years contrasts starkly with the system in the United States and, until the 1999 Employment Relations Bill, with that in the United Kingdom—although for very different reasons. As a result, the union recognition procedure

*Chris Riddell is a Post-Doctoral Fellow with the Centre for Labour and Empirical Economic Research, Department of Economics, University of British Columbia. He thanks Michele Campolieti, Morley Gunderson, Felice Martinello, and Craig Riddell for useful suggestions.

The data used in this paper may be obtained from the British Columbia Labour Relations Board, Suite 600, Oceanic Plaza, 1066 West Hastings Street, Vancouver, BC, CANADA V6E 3X1; 604-660-1300. A data appendix with additional results, and copies of the computer programs used to generate the results, are available from the author at University of British Columbia, Department of Economics, #997-1873 East Mall, Vancouver, BC, CANADA V6T 1Z1; chrisrid@interchange.ubc.ca.

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became a key component in theories explaining the three countries' divergence in union density (Weiler 1983, 1990; Freeman 1985, 1989; Disney, Gosling, and Machin 1995; Johnson 2002).

Since the late 1980s, the certification process has changed in Canada. Now six provinces, covering about 75% of the labor force, have mandatory elections as the union recognition procedure. In addition, over the 1984–98 period, union density fell by about 7 percentage points in Canada—similar to the 8 percentage point decline in the United States during the same years (Riddell and Riddell 2004). Many observers believe it is no coincidence that unions declined concurrently with the abandonment of the card-check system.

While there is a literature in both Canada and the United States that emphasizes the importance of the certification process, only a handful of studies have empirically investigated the impact of the union recognition procedure. Those studies suggest that mandatory elections, or legislation instituting them, has a strong negative effect on union organizing (Martinello 2000; Johnson 2002). However, no study has investigated *why* voting deters union organizing, which is perhaps the most important question of all.

This paper estimates the impact of the recognition procedure, and addresses the question of why elections reduce certification success. The analysis is based on over 6,500 private sector certifications in British Columbia, covering the years 1978 to 1998. A mandatory election law was adopted by British Columbia in 1984, and was then repealed in 1993. We thus have data from six years of card-check laws, followed by nine years of a voting era, followed by six years of card-check laws—a regime succession that allows for a unique quasi-experimental study. British Columbia is the only province in Canada that has changed union recognition regimes more than once, with each regime lasting multiple years. Moreover, the timing of the various legislative changes makes it possible to isolate the effect of voting from the effects of other amendments.

Previous Studies of the Union Recognition Procedure

Many observers have noted that a possible explanation for the large gap in union density between Canada and the United States, which emerged in the mid-1960s, is the union recognition procedure. In a seminal paper, Paul Weiler argued that the election system in the United States allows employers greater opportunity to oppose the organizing drive (Weiler 1983). Both that study and Meltz (1985) provided some evidence to suggest that elections reduce union organizing success.

While the potential importance of the union recognition law has not been overlooked, the empirical literature investigating it is very limited and largely restricted to Canada and the United Kingdom. One Canadian study, Martinello and Meng (1992), estimated the impact of labor legislation (including elections, dues check-offs, and strike-replacement worker laws) on the likelihood that an individual will be covered by a collective agreement. No statistically significant effects were found with respect to voting, but, as the authors noted, there is simply not enough variation across provinces in their single cross-section to identify such an effect. In an analysis that exploited variation over time and across provinces, Johnson (2002) estimated the impact of voting laws over the 1978–96 period using aggregate data from nine Canadian provinces. She concluded that elections reduced union organizing success rates by 9 percentage points.

Following the Employment Relations Bill in 1999, the union recognition procedure in the United Kingdom became more similar to the Canadian card-check system (Wood and Godard 1999). In the two decades prior to the Bill, union density had fallen substantially, a change that was largely attributed to a series of legislative changes in the 1970s and 1980s (Freeman and Pelletier 1990). Overall, U.K. union density fell by 20 percentage points over the period from the late 1970s to 1992, a time when union recognition and decertification were voluntary and not covered by

legislation (Beaumont and Harris 1995). Disney, Gosling, and Machin (1995) showed that difficulty in achieving union recognition in *new* establishments was the key factor in the 1980s decline, and that an increase in management opposition can explain some of the decline.

In the United States, the near-hegemony of elections has discouraged systematic study of union organizing under different union recognition procedures. One study analyzing the recognition procedure is Eaton and Kriesky (2001). The authors concluded that success rates are higher in cases decided by card-check than in those decided by elections, although their finding is based on a sample size of only around 100 certifications. They also provided evidence that the incidence of management opposition is reduced in a card-check system. However, the latter conclusion appears to be primarily based on a comparison of U.S. evidence with evidence from two Canadian studies—Thomason (1994) and Thomason and Pozzebon (1998)—neither of which had variation in the union recognition law. Comparing results from the two countries may not yield appropriate conclusions.

While there has not been systematic study of the union recognition procedure in the United States, there is a growing U.S. literature on the importance of the union recognition procedure that draws on qualitative and case study evidence. For example, Budd and Heinz (1996) found that successful certification at a Minneapolis hotel occurred in only 32 days under a card-check arrangement. Benz (1998) found similar evidence in favor of card-checks, but concluded that there is insufficient data to determine whether the results can be generalized. Additional studies were reviewed by Eaton and Kriesky (2001).

In summary, there is a literature suggesting that the union recognition procedure is related to certification success. However, only one paper has estimated the impact of voting alone (Johnson 2002), and none of these studies examined *why* mandatory elections reduce union organizing success.

Legislative Background

Three legislative changes were made to the British Columbia Labour Code over the sample period of October 1978 to May 1998. The first change, which introduced mandatory elections, was in 1984. Second, in 1987, sweeping changes were made to the Code when the Industrial Relations Reform Act was introduced. Finally, in 1993, mandatory elections were repealed. However, many of the "voting era" amendments were maintained. Appendix 1 presents an analysis of the various legislative changes, which are discussed in more detail below, under "Framework for Analysis."[1]

The key amendment made in May 1984 required the British Columbia Labour Relations Board (BCLRB) to order a representation vote on all certification applications for which at least 45% of the proposed unit was "signed up" through the card-check process. Prior to the 1984 Act, a vote was required if the union was only able to sign up between 45% and 55% of the proposed unit, which rarely occurred (see Appendix 2). Automatic certification without a vote was granted if 55% or more of the proposed unit was signed up. The 1984 Act also introduced expedited certification procedures as a regulation (that is, not formal law), which dictated that the vote was to be held within "approximately" 10 days of the application filing date. The 10-day rule was formalized in the 1987 Act, but the data examined in this paper indicate that the 1984 Regulation was followed (see Appendix 2). It is useful to stress from the outset that employers had only a short time period to launch a campaign against the union. Two other substantive changes were made

[1]The following laws are not discussed because no amendment (or only re-labeling or very minor amendment) was made over the sample period: first contract arbitration, professional strikebreakers, unfair labor practices relating to union certification/decertification, duty to bargain in good faith, and fair representation; union successorship; grievance mediation and arbitration; and Board review of arbitration awards.

in 1984: the decertification rules were amended to parallel the new certification procedures, and unions were no longer *automatically* allowed to picket an ally of the employer or another location of the employer.

In 1987, several additional amendments were made.[2] The most substantive change was the creation of the new Dispute Resolution Division, which placed several restrictions on strikes, and introduced mediation and fact-finding. Two other changes made in 1987 were the introduction of final offer voting and the broadening of the essential services designation. In 1993, the voting law was repealed, and replaced with the pre–1984 card-check system whereby a union could avoid an election if 55% of the unit was signed up. However, the other key 1984 and 1987 amendments were retained.[3]

Figure 1 shows success rates for the private and public sectors, with raids and withdrawn certifications excluded.[4] The pattern in Figure 1 is striking. Success rates for private sector organizing drives fell by nearly 20 percentage points following the introduction of mandatory elections in May 1984. Moreover, in 1993, success rates appear to have recovered to their pre-voting levels, where they held fairly steady through the remainder of the sample period.

Interestingly, public sector success rates did not systematically decline. This finding is suggestive of an explanation based on management opposition, since employer resistance to certification is likely to be negligible in the public sector.[5] However, Figure 1 does not provide a sufficient basis for concluding that mandatory elections alone caused the decline in success rates, because other potentially important legislative changes were made over the sample period. In addition, Figure 1 does not reveal anything conclusive about the role of management opposition. The task for the remainder of the paper is therefore twofold: first, to use the legislative analysis to isolate the effect of voting from the effects of other amendments; and second, to use data on unfair labor practices (ULPs) to examine the role of management opposition.

Framework for Analysis

I begin by outlining a simple conceptual framework that hypothesizes why the union recognition procedure could affect certification success and why employer behavior might change with a change in the union recognition law. In general, mandatory election laws are believed to lead to lower certification success rates for two reasons: management has more opportunity to oppose the bid in a voting system; and peer

[2]A careful reading of the legislation indicates that many of the 1987 changes were procedural in nature. For instance, all applications had to be filed in a timely manner (that is, a strict number of days was explicitly given for virtually all applications) and in writing, including appropriate documentation. Many types of applications were made on a more informal basis prior to 1987, and supporting documentation was rarely required (unless requested by the Board at a later date). These various procedural changes were maintained in 1993.

[3]In addition, any pure effect of the government on union organizing is "controlled for" since there was only one change in government over the entire sample period: the election of the New Democratic Party in 1991.

[4]A large majority of the social services certifications are public sector, but a few are private sector, and cannot be isolated. Withdrawn certifications are excluded for three reasons: such cases are often withdrawn almost immediately after the filing date, and thus important variables used in the analysis are missing; classifying withdrawn applications as a failure may be inappropriate; and including withdrawn applications could result in double-counting. With respect to double-counting, I have found that in a majority of withdrawn cases, the union re-applied shortly after (in fact, the Board eventually instituted a six-month waiting policy for this reason). In results available upon request, it is shown that withdrawn certifications simply shift the series downward in a parallel fashion, and that the trend in Figure 1 cannot be explained by a Canada-wide shock, as certification success rates in most other provinces were largely constant over the 1984–92 period (also see Martinello 1996).

[5]Due to concerns over public image and the lack of profit maximization objectives, management opposition (particularly egregious management opposition) is likely to be negligible in the public sector.

Figure 1. Certification Success Rates, British Columbia, 1978-1998.

pressure from pro-union colleagues and union organizers may artificially inflate the "true" level of support in a card-check system. With respect to management opposition, a change in the union recognition law may change both the *effectiveness* and the *incidence* of management opposition.

A voting regime may increase the *effectiveness* of employer tactics for two reasons. The amount of time employers have to influence the organizing drive is unambiguously greater in the voting system than in the card-check system. In the card-check system in British Columbia prior to May 1984 and from 1993 to 1998, union organizers collected signatures from members of the proposed bargaining unit, and then made an application to the BCLRB. The employer was contacted when the BCLRB conducted its investigation to determine whether the petition requirements (which are discussed below) were satisfied. As shown in Appendix 2, it was rare for a union in one of the card-check regimes in British Columbia to fail to gather 55% support—sufficient for certification without a vote. This was also the case in Ontario over the 1980 to 1988 period (Thomason 1994). Thus, in the card-check years, many organizing drives were virtually completed before the employer was aware of the drive. While Riddell (2001) showed that some employers find out about the bid and engage in an anti-union campaign prior to the application, in general, management does not have sufficient time to launch a meaningful campaign. Under voting, unions still had to gather cards and make an application, but then a secret ballot vote was held 10 days later. This extra step in the certification process gave the employer greater opportunity to launch a campaign.

The secret ballot vote itself is a second reason management opposition is likely to

be more effective in a voting regime. Under card-checks, if the employer coerces employees into refusing to sign cards, union organizers and pro-union colleagues can counteract the coercion tactics. In a secret ballot vote, the opportunity to counteract employer threats is likely diminished.

Elections may also increase the *incidence* of management opposition. If the effectiveness of management opposition is greater in a voting regime—and assuming employers know this—then a cost-benefit calculation implies that employers will adopt such tactics more frequently, given that the chances of defeating the bid are greater while the costs remain the same. As well, there is simply more time available to the employer under elections. Thus, whereas employers in a card-check system may not realize organizing activity is occurring until after the cards had been collected, under a voting system they will have an opportunity to oppose the union.

The empirical analysis proceeds in two stages. First, I use data on certifications from 1978 to 1998 and changes in legislation over that period to estimate the impact of mandatory elections on certification success. Second, I merge data on unfair labor practice complaints, which are available only for the years 1987 to 1998, to examine whether the incidence and effectiveness of management opposition are greater in a voting regime. The second part of the analysis also estimates what part of the total change in success rates across the two union recognition regimes—from voting (1987–92) to cards (1993–98)—can be attributed to management opposition.

To analyze the impact of elections on certification success, I estimate the probit model

$$(1)\quad \text{SUCCESS}_i = f(\alpha_0 + \alpha_1 \text{LEG84}_i + \alpha_2 \text{LEG87}_i + \alpha_3 \text{LEG93}_i + \alpha_4 X_i + \alpha_5 \text{IND}_i + \alpha_6 \text{REG}_i + \alpha_7 \text{YEAR}_i + \varepsilon_i),$$

where LEG84 = 1 if the i^{th} certification was applied for under the 1984 Labour Code Amendment Act; LEG87 = 1 if the certification was applied for under the 1987 Industrial Relations Reform Act; LEG93 = 1 if the certification was applied for under the 1993 Labour Code Amendment Act; X is a vector of other explanatory variables (discussed below); IND is a vector of industry dummies to capture unobserved factors associated with the industry of the unit; REG is a vector of regional dummies to capture unobserved factors associated with the region within the province;[6] and YEAR is a vector of year dummies to capture unobserved changes over time, such as changes in economic conditions.

The coefficients on the three legislative variables are evaluated relative to the omitted reference group, which is the pre–1984 legislation (the 1973 Labour Code Amendment Act) that was in effect from October 1978 until May 1984. A hypothesis test of the equality of α_1 and α_2 will indicate whether there was any difference between the 1984 and 1987 reforms, both of which had mandatory elections. If α_1 and α_2 are statistically different, then $\alpha_2 - \alpha_1$ yields the impact of the additional reforms made in 1987. Note that it is not possible to isolate any of the individual law changes made in 1987.

The 1984 legislation made relatively few changes to the Labour Code, so the coefficient α_1 mainly reflects the impact of compulsory elections. However, α_1 does not fully isolate the effect of elections, since amendments to decertification and picketing provisions may have reduced the benefits to unionization, and thus may have affected certification success.[7] The decertification and picketing rules were both maintained in 1993 along with the key 1987

[6]Statistics Canada's UI Regions are used to create the region dummies, as well as to define the unemployment rate. These are the only regional definitions available over the sample period that reflect economic and social differences within a province.

[7]My reading of the legislation is that decertification became much easier following the 1984 law, and in fact the number of decertifications increased markedly (although this may not be a causal relationship). Regulations governing picketing unambiguously became much more restrictive following the 1984 law, as indicated in Appendix 1.

changes. Thus, if α_1 and α_2 are not statistically different (that is, there is no additional impact of the 1987 reforms), $\alpha_1 - \alpha_3$ yields the impact of mandatory elections alone. If α_1 and α_2 are statistically different, then the voting effect is $\alpha_1 - [(\alpha_2 - \alpha_1) + \alpha_3]$.

To examine whether management opposition is more effective under voting than under card-checks, I use data on ULPs—available only since 1987—and estimate the following probit model:

$$(2)\quad \text{SUCCESS}_i = f(\gamma_0 + \gamma_1 \text{LEG87}_i + \gamma_2 \text{ULP}_i + \gamma_3 \text{LEG87}_i * \text{ULP}_i + \gamma_4 X_i + \gamma_5 \text{IND}_i + \gamma_6 \text{REG}_i + \gamma_7 \text{YEAR}_i + \nu_i)$$

The inclusion of the interaction term between the voting regime and ULPs allows for testing the hypothesis that management opposition is greater under elections. That is, γ_1 indicates the impact on certification success rates of the 1987 voting legislative package relative to the omitted legislative regime (card-check, 1993–98), γ_2 indicates the average effect of ULPs, and γ_3 indicates the *additional* impact, if any, that ULPs had when voting legislation was in effect.

A further question is: how much of the fall in success rates associated with the voting regime can be attributed to a change in the *incidence* of management opposition, and how much can be attributed to a change in the *effectiveness* of management opposition? To address this question, I estimate regressions separately for each legislative regime in years for which ULPs are available—one regression for certifications disposed under the voting regime (1987–92) and another for those handled under the card-check regime (1993–98). Applying the standard Oaxaca-Blinder technique, modified for a probit (see Appendix 3), I can then use the estimated coefficients from the two regressions to decompose the change in success rates into two components: the part due to changes in the means of the *X*'s across legislative regimes (capturing changes in the *incidence* of management opposition), and the part due to changes in the coefficients across legislative regimes (capturing changes in the *effectiveness* of management opposition). It is not straightforward to sub-decompose the total coefficient component into each individual coefficient's contribution, which is necessary in order to estimate how much the change in the *effectiveness* of management opposition contributed to the change in success rates. I use the method developed by Nielson (1999) to provide an estimate of the contribution of the change in effectiveness of management opposition (see Appendix 3 for further details).

Data and Methodological Issues

The principal data were collected from the records of the British Columbia Labour Relations Board. A number of sample restrictions were imposed. For reasons discussed above, public sector certifications were dropped from the sample. Also dropped were observations involving raids, since such cases have always required a vote, and involve workers choosing between the incumbent union and the raiding union. Certain construction applications were dropped because a representation vote has never been required in temporary construction projects. Certifications were also omitted if they were dismissed as a result of petition violations, either because of jurisdictional issues (deemed to be under federal jurisdiction) or because the bargaining unit was deemed inappropriate for collective bargaining. These restrictions removed 992 certifications from the original private-sector sample, resulting in a final sample of 6,650 certifications.[8]

As noted, the measure of management opposition is whether a ULP was filed against the employer for illegally attempting to coerce employees from supporting the union.[9] An additional advantage of

[8] In some cases it was difficult to determine whether a given construction certification was exempted from the vote; however, the results are not sensitive to the exclusion of all construction certifications.

[9] Penalties for ULPs are largely compensatory, and there was no change in these penalties over the sample period. For intimidation, the BCLRB would post a cease and desist order. In cases of dismissal, the BCLRB could order reinstatement and payment of

examining British Columbia is that the BCLRB main database, unlike data provided by many of the provincial labor relations boards, separates ULPs by the section of the legislation under which they were filed. Thus, only ULPs that were filed where it was indicated that management had illegally attempted to influence one or more persons' decision regarding certification are included in the sample.[10]

It is appropriate to consider the advantages and disadvantages of using ULPs, which are the only available measure of management opposition with which to exploit the unique quasi-experimental design of the study. A key issue is that ULPs capture illegal employer tactics, and thus I am not able to examine the effectiveness of legal employer tactics across union recognition regimes.[11] In addition, I cannot distinguish between different types of employer tactics. It may be the case that certain types of illegal employer tactics (such as dismissals) have varying effects across union recognition regimes while others (such as broad threats) do not.

Some light can be shed on the tactics the ULP variable may be capturing by examining the results reported by Martinello and Yates (2003). The authors identified five categories of employer strategies (where one category is "do nothing"). Their results indicated that ULPs tend to be filed in cases where management goes full out and adopts legal (or largely legal) strategies—challenging the bargaining unit or holding captive-audience meetings—and more marginally legal/outright illegal strategies such as threatening plant closure, improving working conditions during the campaign, and dismissing employees for union involvement. If their results for Ontario can be generalized to British Columbia, our ULP variable is likely capturing anti-union campaigns in which the employer is using the full array of tactics.

Appendix 2 shows the number of dismissal and non-dismissal ULP cases. Two broad types of ULPs are included in the non-dismissals: general coercive activities, such as threats of certain consequences in the event of successful certification; and non-discharge actions taken against an employee (or group of employees), including demotions, denial of overtime pay, and changing working conditions. Historically, dismissals dominated the ULP filings in the province; unfortunately, the non-dismissal ULP numbers are not reliable after 1988.[12] Riddell (2001) showed that non-dismissal ULPs tend to be threats made against the workplace in general, and also found that, for British Columbia, it is unusual for a non-dismissal ULP to be filed in isolation

lost wages under Subsection 8(4)(c). Appendix 2 shows the proportion of dismissal ULPs in which some form of compensation was granted. The final power available, Subsection 8(4)(e), allowed automatic certification to be granted where the Board deemed that the true wishes of the workers were not expressed *and* could not be expressed through a second vote. As indicated in Appendix 2, the Subsection 8(4)(e) power was rarely used.

[10]This includes all ULPs falling under Section 3 of the Industrial Relations Act for 1987 to 1992 and Section 6 of the Labour Code for 1993 to 1998.

[11]One piece of evidence that increases my confidence in the reliability of ULPs as a measure is the rarity of applications that are rejected (that is, not found to be meritorious; this can be seen in the BCLRB annual reports over the 1978 to 1992 years only). A strong majority of applications either are found to be meritorious or are withdrawn. Moreover, Riddell (2001) found that in a majority of the withdrawn cases in British Columbia a settlement was reached.

[12]There has been a dramatic inflation of ULP charges in British Columbia, especially since 1993. However, much of this is artificial, due to the creation of a new ULP section in 1993 that was already covered under previous legislation. Also, unions filed more ULP charges (that is, under different subsections) per case. Riddell (2001) found no support for the notion that a second ULP has a cumulative effect on certification success. Given all of this, the results presented in the paper use a ULP variable defined as being at least one ULP filed. In this way, the mean of the ULP variable will not be affected by any artificial inflation. Finally, the BCLRB changed its system of counting ULPs in 1989. Readers should therefore be wary of the ULP numbers in the BCLRB annual reports.

(that is, without a dismissal ULP also being filed). The raw data on ULPs from British Columbia thus appear consistent with the Martinello and Yates (2003) finding that ULPs tend to be filed in cases where management "goes all out."[13]

There are advantages of using ULPs rather than survey-based measures of employer tactics. Administrative data are much less prone than survey data to measurement problems such as non-response bias and recall error (see, for instance, Pierret 2001). In addition, a comparison of two Canadian surveys—Martinello and Yates (2003), which surveyed *unions* about employer tactics, and Bentham (2002), which surveyed *employers* about employer tactics—illustrates a potential pitfall of self-reported measures. A cross-survey comparison of the reported incidence of most employer tactics shows dramatically lower reported incidences in the employer-based survey than in the union-based survey, with gaps on the order of 20 percentage points for particularly egregious practices such as threats of relocation.

Another issue is the potential endogeneity of ULPs.[14] Employers who believe success is contestable and can be influenced are more likely to pursue an anti-union campaign than employers who see success as either very likely or very unlikely. Standard regression techniques may thus provide estimates that underestimate or overestimate the true effect of employer resistance. Lawler (1984), Freeman and Kleiner (1990), and Riddell (2001) are the only studies I know of to have estimated equations that correct for endogeneity.[15]

To address the possible endogeneity of management opposition, I estimate a two-stage least squares model. For an instrument to be valid it must be correlated with the presence of an unfair labor practice, but must not directly influence organizing success. There are two main factors contributing to the presence of a ULP: factors that would explain why employers resist unions, and factors that affect the union's (or employees') decision to file a ULP. For instance, there may be an organizing drive in which the employer is engaging in "suppression" tactics but the union does not feel it is worthwhile to file a complaint.

I can think of no credible instrument that would explain why employers resist unions, since any obvious factor could be argued to explain certification success as well. Thus, I focus on factors that may affect the decision to file a ULP. The first instrument used is the processing time of ULPs lagged by one month.[16] The rationale is that if it takes a long time for the BCLRB to render a decision on ULP cases, the union may feel it is not worthwhile to file. This is particularly the case where ULP processing times are substantially longer than certification processing times. The second instrument is a dichotomous variable equaling one if ULP processing time in the month prior to the certification application was greater than the processing

[13]I asked the Board for their opinion of the nature of non-dismissal ULPs, and also examined the Board's *Decisions* for ULP cases (unfortunately, the decisions published in the Board's annual review are not randomly selected). Both pieces of evidence suggest that the non-dismissal ULPs tend to be broad, serious threats against the workplace, with plant closure/relocation and (negative) changes in working conditions being the two most common threats.

[14]There is also a sample selection bias present, since management opposition may have prevented some certification bids from being launched; however, such a bias is likely minimal in the case of British Columbia (and Canada, for that matter) relative to the United States due to the high probability of winning the bid.

[15]Canadian studies of employer tactics include Thomason (1994), Thomason and Pozzebon (1998), Riddell (2001), Bentham (2002), and Martinello and Yates (2003). U.S. studies include Dickens (1983), Lawler (1984), Lawler and West (1985), Cooke (1985), Freeman and Kleiner (1990), and Bronfenbrenner (1997), among others.

[16]ULP processing time is the number of days between the ULP filing date and the decision date. The results (particularly with respect to the difference in the ULP coefficient across union recognition regimes) are quite robust with respect to the choice of lag.

time of the current organizing drive. Use of this instrument, like the first, is based on the reasoning that ULP processing times in excess of certification processing times reduce the incentive to file a ULP. In this case, it is implicitly assumed that the union's expectation of the length of the organizing drive equals the *ex post* processing time. While this is obviously not entirely realistic, it is a reasonable assumption, since organizers, at the time the union is considering filing a ULP, will likely have a good feeling for how long the BCLRB will take to process the application.[17] The final instrument is the lagged success rate of compensation claims (for three months prior to bid) for dismissal-related ULPs under subsection 8(4)(e). The intuition in this case is that unions may be more likely to file a ULP if the likelihood of receiving compensation is higher.

This paper exploits two legislative changes, occurring seven years apart, to examine the impact of the mandatory election law and the effectiveness of management opposition across legal regimes. Thus, the appropriate data to use are from a large administrative dataset on certification attempts that covers the necessary years. Although reliance on this dataset constrains my ability to control for other factors that may influence certification success, I am able to construct some controls for union characteristics.

I derive two variables meant to capture the decision-making power of the proposed bargaining unit if certified. The first variable, called unit representation, is the size of the unit divided by the size of the union local to which the bargaining unit belongs. This variable may indicate how "important" the unit will be relative to the union local, and thus the unit's perceived decision-making power. The second variable, local representation, is the size of the local to which the unit belongs divided by the union's total membership in the province. A variable indicating the share of the unit's local that is female is also included. Bronfenbrenner (1997) found that successful certification was more likely in units that were 60% or more female than in other units.

An industrial specialization index, which equals the percentage of a given union's certifications that occur in the union's primary jurisdiction industry, is also computed for each individual union. This is identical to the "representational specialization" variable used in Fiorito, Jarley, and Delaney (1995), which the authors found to have an important positive influence on organizing effectiveness. However, given that there are around 150 unions in the data, we have much more variation in this variable than did Fiorito, Jarley, and Delaney.

I also create a set of controls for the affiliation of the union. In Canada, there is considerably more variation in affiliation than in the United States, and to some extent, affiliation can control for union characteristics.[18] Moreover, the role of union affiliation has not, to date, been

[17]The BCLRB has ordered a second vote in cases involving a ULP, but if the delay in the second vote is quite long (due to submissions being made) employees may lose interest in the organizing drive.

[18]The affiliations are the following: an international union affiliated with the AFL-CIO and the Canadian Labour Congress ("AFL-CIO/CLC"); an international union not affiliated with the CLC (AFL-CIO only), which includes all trades unions (the Electrical Workers, for example) that formed the Canadian Federation of Labour ("CFL") in 1982; a CLC national union ("CLC"); a Confederation of Canadian Unions ("CCU") national union, which consists of a group of left-wing, former national-independent unions that merged in 1983; a national independent union ("NIU"); a provincial independent union ("PIU"), which is a multi-employer independent union operating only in British Columbia and likely to be a specialist union, such as the Health Science Association; and an independent local union ("ILU"). The Teamsters are separated because they were a U.S. independent for most of sample period, then became an AFL-CIO only (but should not be grouped with the CFL trade unions), and then joined the CLC in 1993. All union-related variables were created using the annual British Columbia Directory of Labour Unions.

examined in the context of certification success.[19]

Results

Table 1 presents estimates of the impact of mandatory elections on certification success rates where I attempt to isolate the voting effect by exploiting the fact that different laws were changed at different times over the 1984 to 1993 period. To simplify the table, I show only marginal effects.[20] Summary statistics for all variables are listed in Appendix 4.

Column (1) shows that certification success rates were, on average, 24 percentage points lower under the 1984 voting rules, and 22 percentage points lower under the 1987 voting rules, than under the pre–1984 rules. A hypothesis test of the null that the 1984 coefficient equals the 1987 coefficient cannot be rejected ($\chi^2 = 0.02$). As regressors are added, the gap between the 1984 and 1987 estimates grows somewhat, but the 1984 effect is systematically *larger* than the 1987 estimate. In the preferred specification, which includes year effects, the 1984 impact is 23 percentage points, but the 1987 effect has fallen to 17 percentage points. Not surprisingly, given the standard error on the 1984 legislation coefficient, an equality test still cannot be rejected ($\chi^2 = 0.34$). Thus, I conclude that there is no discernible difference between the 1984–86 voting regime and the 1987–92 voting regime. Essentially, the set of additional reforms made in 1987 had no additional impact on certification success rates.

While the 1987 reforms had no additional impact on certification success, the 1984 estimate alone still does not isolate the effect of voting, given the decertification and picketing changes that were also made in 1984. As discussed, the effect of these other factors can be estimated using the 1993 legislation, since the other 1984 amendments were maintained in 1993. Column (1) shows that only three percentage points separate the certification success rates of the 1993 legislative regime and the pre-1984 regime, both of which had a card-check union recognition procedure. This effect holds up in the second specification, where a set of controls is added. At this point the 1993 estimate would suggest that we should deduct 3 percentage points from the 1984 estimate, to net out the effect of the decertification and picketing rules. However, after I add year effects, which absorb unobserved factors correlated with time, the 1993 estimate is not statistically different from zero. Column (4) adds another set of controls—industry effects interacted with a voting (1984–92) dummy—to test for any differences in the voting effect across industries, which the industry effects alone would miss. However, the legislation estimates remain virtually the same. Thus, I conclude that voting accounts for virtually the entire fall in success rates that occurred over the 1984 to 1992 period, and that the various other reforms had no impact on certification success. Of course, it must be emphasized that these other reforms may have had important effects along other dimensions.

One concern is that the unit characteristics, union affiliation, unemployment rate, industry, region, and year dummies are not adequate controls, and that unobserved factors are contaminating the results. That is, something else may be explaining the dramatic decline in certification success rates that we observe over the June to December 1984 period when voting was introduced (Figure 1), and the subsequent in-

[19]Two striking features of Canadian unionization are the decline of U.S. internationals and the rise of the independent. In 1962, two of every three union members belonged to an international union, but by 1995 this ratio had fallen to about 20% (CALURA, various years). Conversely, while there were very few independent unions in Canada in 1962, by 1995 over 20% of the some 4 million Canadian union members belonged to an independent.

[20]The reported estimates are the change in the probability of certification for an infinitesimal change in each continuous independent variable and, for each dummy independent variable, the discrete change in the probability of certification when the dummy variable takes on a value of 1 compared to 0. See the dprobit routine in Stata version 8.0, which was the statistical package used in the study.

Table 1. Estimates of the Change in Probability of Certification Success: Isolating the Impact of Mandatory Elections, 1978–1998. (Standard Errors in Parentheses)

	Specification				
Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
1984 LCA Act	–.241***	–.226***	–.229***	–.229***	–.234***
	(.023)	(.023)	(.076)	(.081)	(.051)
1987 IRR Act	–.218***	–.200***	–.167***	–.153***	–.177***
	(.015)	(.015)	(.047)	(.052)	(.044)
1993 LCA Act	–.031***	–.033***	–.001	–.023	–.011
	(.012)	(.012)	(.037)	(.040)	(.038)
Industrial Concentration	—	.071***	.071***	.067***	—
		(.026)	(.026)	(.026)	
Unit Representation	—	–.283***	–.281***	–.280***	—
		(.047)	(.047)	(.047)	
Local Representation	—	.002	.002	.006	—
		(.018)	(.018)	(.019)	
% Female	—	–.019	–.016	–.018	—
		(.030)	(.030)	(.030)	
AFL-CIO/CLC	—	–.035**	–.037***	–.037***	—
		(.016)	(.016)	(.016)	
CFL	—	–.013	–.015	–.016	—
		(.015)	(.015)	(.015)	
CCU	—	–.016	–.017	–.018	—
		(.030)	(.030)	(.030)	
NIU	—	.042**	.041**	.042*	—
		(.017)	(.017)	(.020)	
PIU	—	–.023	–.022	–.016	—
		(.046)	(.046)	(.045)	
ILU	—	.075**	.075**	.072**	—
		(.024)	(.024)	(.025)	
Teamsters	—	–.064***	–.066***	–.068***	—
		(.022)	(.022)	(.023)	
Unemployment Rate	—	–.000	–.013**	–.013**	–.008
		(.002)	(.006)	(.006)	(.005)
Industry Dummies	No	Yes	Yes	Yes	Yes
Regional Dummies	No	Yes	Yes	Yes	Yes
Year Dummies	No	No	Yes	Yes	Yes
Industry * 1984–92 Dummies	No	No	No	Yes	Yes
394 Union Local Dummies	No	No	No	No	Yes
Log Likelihood	–2,639.7	–2,553.1	–2,547.8	–2,537.9	—
R Squared	—	—	—	—	.38
Sample Size			6,650		

Notes: The omitted union category is national unions affiliated with the CLC. The calculation of the marginal effect is discussed in the text.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

crease in success rates in 1993 when voting was repealed. Two final strategies are adopted to address this concern. First, I examine all other legislative changes that were made in 1984 and in 1992/early 1993. No other single piece of legislation was changed at both times. In addition, I can find no other legislation in 1984 or 1992

that could have plausibly affected organizing success. Second, the final column in Table 1 displays the results from a specification that includes dummy variables for union locals—a total of 394 dummy variables. This specification absorbs all variation across union locals, controlling for unobserved factors such as union strategies, union staff, union finances, and union mergers. The coefficients on the legislation variables remain the same.[21]

The union affiliation estimates merit discussion. The three sets of international unions are all less likely to certify than were the CLC national unions (the omitted category), although the estimate is not statistically significant in the case of the CFL unions. Second, the NIUs and ILUs were much more likely to successfully certify than were the CLC unions. The above results are of interest because, as noted, two striking features in Canadian union incidence are the dramatic decline of international (U.S.-based) unions and increase of independent unions. The results suggest that union organizing effectiveness may be one reason for these changes. Finally, the industrial concentration variable is statistically significant and positively related to certification success, supporting the finding of Fiorito, Jarley, and Delaney (1995), while the unit representation variable is statistically significant and negatively related to certification success.[22]

I now turn to the issue of whether management opposition underlies the effect of elections. Column (1) in Table 2 presents the estimated marginal effects from the first regression in the second stage of the analysis, wherein I test the hypothesis that management opposition is greater under elections. The average negative impact of ULPs on certification success is estimated to be 5 percentage points. Note that this is a larger impact than that found by Thomason (1994) in the case of Ontario under card-check rules. However, the estimated marginal effect on the interaction term (1987 legislation * ULP) is very large, and implies that ULPs reduced certification success rates by an *additional* 8 percentage points in the voting regime—when workers voted 10 days following the application. That is, the full effect of ULPs over the 1987 to 1992 period is estimated to be a 13 percentage point reduction in success rates. In percentage terms, this represents an increase in the effectiveness of management opposition of about 160%.

The remaining results in Table 2 are from regressions estimated separately by legislative regime. Columns (2)–(5) indicate that the OLS and probit marginal effects for the impact of a ULP are virtually identical. The estimates imply that a ULP under card-checks reduces success rates by nearly 6 percentage points, while the impact for the voting years is 12 percentage points. This suggests that management opposition is twice as effective under voting—a smaller increase than is found using the interaction approach. The results for the remaining variables are very similar to those presented in Table 1 and, to simplify the exposition, are not presented in Table 2.

The two-stage least squares estimates suggest that OLS and probit estimates of the

[21]The union "fixed effects" specification is based on a linear probability model. However, OLS estimates of models (1)–(4) are very similar to the marginal effects predicted from the probit models (for example, OLS estimates for the column 3 specification are –.215 and –.164 for the 1984 and 1987 legislation variables, respectively, and –.229 and –.161 for the column 4 specification).

[22]The magnitude of these results is small. A one standard deviation increase in industrial concentration increases the likelihood of success by 1.5 percentage points, while an analogous increase in unit representation decreases the probability of success by 3 percentage points. The negative relationship between unit size and certification success may be driving the unit representation estimate. However, in other analysis (not reported here) I examined the distribution of the unit representation variable (with and without unit size included as a covariate), and the results indicate that those certifications with very high values (above the 90th percentile) of the unit representation index are driving the results, even after unit size is included.

Table 2. Estimates of the Change in Probability of Certification Success: The Impact of Management Opposition, 1987–1998. (Standard Errors in Parentheses)

	Specification						
Variable	*Interaction Model: Probit*	*Voting Regime Only: Probit*	*Card-Check Regime Only: Probit*	*Voting Regime Only: OLS*	*Card-Check Regime Only: OLS*	*Voting Regime Only: 2SLS*	*Card-Check Regime Only: 2SLS*
1987 IRR Act	–.173*** (.015)	—	—	—	—	—	—
Unfair Labor Practice	–.051*** (.019)	–.118*** (.031)	–.057*** (.016)	–.120*** (.028)	–.059*** (.013)	–.353*** (.098)	–.281*** (.069)
ULP * Voting Regime	–.084*** (.028)	—	—	—	—	—	—
Log Likelihood	–1040.9	–673.1	–356.2	—	—	—	—
R Squared	—	—	—	.17	.14	—	—
Sample Size	3,023	1,325	1,698	1,325	1,698	1,325	1,698

Notes: For probit models, only the estimated marginal effect is shown. The calculation of the marginal effect is discussed in the text. All specifications include controls for industrial concentration, unit and local representation, proportion female, union affiliation, and the unemployment rate, as well as region, industry, and year effects. The 2SLS estimates are computed using the regression results from a first stage, where ULP is instrumented with three instruments (in addition to the other above-noted variables): lagged ULP processing time, a dummy equal to one if lagged ULP processing time is greater than certification processing time, and the success rate for ULP-compensation claims under subsection 8(4)(e) from the previous three months.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

impact of ULPs *understate* the true impact, a finding that is consistent with previous research. The results from the first stage are as anticipated, with the two ULP processing time–related variables being negatively associated with the likelihood of a ULP and statistically significant, and the subsection 8(4)(c) success rate being positively related but not statistically significant. Industry and the unemployment rate are also key factors in the first stage. The F-statistics from the first stage are 30.99 and 42.84 for the card-check and voting regime regressions, respectively. However, caution is required in interpreting the 2SLS estimates, since the standard errors increase dramatically, and the magnitudes of the estimated effects appear too large to be credible. Nevertheless, the voting regime estimate is still larger than the card-check regime estimate.

Table 3 presents the decompositions of the differences in certification success rates that occurred between the card-check and voting regimes, with the decomposition computed so as to yield a positive gap (card-check success minus voting success). The decompositions are an attempt to address the question of how much of the change in success rates across union recognition regimes can be attributed to changes in the *incidence* and *effectiveness* of management opposition. The actual difference in success rates was 18.6 percentage points and the predicted difference, based on a probit model, is somewhat lower at 17.4 percentage points, while the predicted difference from OLS is 18.8 percentage points.[23] The main result from the second panel of Table 3 is that changes in the means of the *X*'s are unimportant. Essentially all of the gap in success rates can be attributed to differ-

[23]The predicted success rates are higher than those in Figure 1 because those certifications rejected due to petition violations have been excluded from the sample.

Table 3. Decomposition of the Difference in Certification Success across Legislative Regimes, 1987–1998.

	Actual	*Probit*	*OLS*
Predicted Certification Success Rates			
Card-Check Regime	0.931	.939	.931
Mandatory Elections Regime	0.745	.765	.743
Difference in Success Rate	0.186	.174	.188
Standard Decompositions			
Total Difference Due to Means	—	–.003	–.001
Sub-Decomposition of Means	—	—	—
ULP	—	–.002	–.001
Union Characteristics	—	–.006	–.001
Union Affiliation	—	.001	.000
Industry	—	.003	.001
Other Controls	—	.001	.000
Total Difference Due to Coefficients	—	.183	.189
Approximation Residuals	—	–.006	—
Total	—	.174	.188
Nielson Decomposition			
Difference Due to Change in Mean of ULP	—	—	–.002
Difference Due to Change in Coefficient of ULP	—	—	.045
Difference Due to Change in Coefficient of Constant Term	—	—	.139
Predicted Gap	—	—	.182

Notes: See Appendix 3 for details on the decompositions and Appendix 4 for means.

ences in the estimated coefficients across the two regimes. This result is identical for both the probit and the standard Oaxaca-Blinder decomposition (based on a linear probability model). In addition, decompositions that switch the weights (that is, holding coefficients constant at voting levels instead of card-check, and vice-versa for means) yield virtually the same results.

The finding that the incidence of management opposition did not change across regimes is contrary to the conventional wisdom that management opposition is higher in a voting regime. For instance, many authors have noted that there appears to be more employer opposition to unions in the United States—where elections are typically required—than in Canada—where, until recently, elections have typically not been required. The results from British Columbia, which exploit variation in the union recognition law, demonstrate that caution is required when comparing results from different countries.

However, there is reason to expect similar incidence of management opposition across the two union recognition procedures if the inherent level of support *in attempted organizing drives* is higher in a voting regime. This possibility is an implication of management opposition being endogenous. Specifically, given that successfully certifying a unit is more uncertain under elections, unions may respond in either of two ways when a voting regime is introduced: by increasing the number of attempted certifications, or by making fewer attempts but focusing on campaigns in which they believe success is very likely—that is, on campaigns in units with a higher inherent level of support. As seen in Appendix 2, the number of certification attempts fell by around 50% in British Columbia following the introduction of mandatory elections. This may indicate that unions focused their organizing efforts on units with a high inherent level of support. Of course, unions may simply have become

discouraged during the voting years, in which case it is unclear whether the inherent level of support would be higher or lower in the voting regime.

If unions are more selective in a voting regime, then we may expect *fewer* ULPs under a voting regime than under a card-check regime, because employers will recognize the greater inherent level of support and decide that an anti-union campaign is not worthwhile. Thus, while there are reasons to anticipate a higher incidence of ULPs in a voting regime—higher effectiveness of anti-certification tactics, and more time to employ them—there is also reason to expect fewer ULPs under voting.

Given the fact that most structural factors (such as shifts in the industrial and occupational distribution of employment) do not change abruptly, it seems unlikely that structural factors explain the striking observed changes in certification success shown in Figure 1. Moreover, if structural factors were an important determinant of the decline in success rates during the voting era, we should see some change in the industrial or regional distribution of certification attempts. On the contrary, as shown in Table 3, changes in the means of the explanatory variables contributed nothing to the overall change in success rates. As noted above, it is possible that unobserved structural change in the economy affected the incidence of management opposition.

As a check on the finding that structural factors are unimportant, I estimated separate regressions of the probability of unionization for each time period (1984 versus 1998) using data on non-agricultural, paid workers from the 1984 Survey of Union Membership—a supplement to the Labour Force Survey—and the 1998 Labour Force Survey.[24] Essentially, I replicated the analysis of Riddell and Riddell (2004), but restricted the sample to British Columbia workers. This approach allows us to take into account various demographic and labor market characteristics that are unavailable in the certifications data. Using the same procedures discussed in Appendix 3, I decomposed the change in probability of unionization over the 1984 to 1998 period into two components: the part due to changes in the coefficients (changes in the propensity to be unionized) and the part due to changes in the means (changes in the proportion of the labor force that have given characteristics, such as gender, education, part-time status, industry, occupation, and job tenure).

The results from these nationally representative surveys indicate that the decline in the probability of unionization in British Columbia over the 1984 to 1998 period was 10 percentage points, well above the 7 percentage point decline for Canada. The decomposition results for British Columbia are quite similar to the results for Canada, as estimated by Riddell and Riddell, with only 5% of the change in the probability of a non-agricultural, paid worker being unionized over the 1984 to 1998 period being attributable to changes in the means of personal characteristics.[25] It therefore appears unlikely that structural factors have played even a modest role in the decline of unionization in British Columbia.

The final panel in Table 3 uses the approach suggested by Nielson (1999) to subdecompose the total coefficient component. The estimated impact of a ULP is only about one percentage point higher in a regression that includes only a control for ULP (and a constant term). Given the huge volume of statistics produced by this approach and the fact that the omission of the other control variables does not change the ULP estimate, I only present the results from the parsimonious regression, as suggested by Nielson under such circumstances. The part of the difference in success rates that can be attributed to a change

[24]The 1984 Survey of Union Membership was the first Canadian survey comparable to the 1998 Labour Force Survey that included a question on coverage by collective agreement.

[25]These results are available upon request to the author.

in the mean is essentially zero with an estimate similar to the other decompositions (–.002), while the part of the gap that can be attributed to a change in the effectiveness of ULPs (that is, a change in coefficients) is estimated to be 4.5 percentage points, or about 25% of the total gap. I emphasize that while the Nielson method is intuitively appealing, it is only one possible approach to sub-decomposing the coefficient component, and so some caution is required in interpreting the 25% estimate.

Overall, the results in Tables 2 and 3 illustrate that even in an environment with very strict time limits on the election process—time limits that were complied with—management opposition is highly *effective* when workers must vote.

Conclusions

It has been argued that mandatory elections reduce certification success. A key factor believed to underlie the effect of voting is management opposition to the certification bid. In particular, it has been hypothesized that the *incidence* and *effectiveness* of management opposition tactics are greater when workers' preferences are expressed via a secret ballot vote. This paper exploits variation in the union recognition law to test the management opposition hypothesis using data on over 6,500 private sector certifications from British Columbia. The union recognition law changed from card-check to mandatory elections, then back to card-check, providing a unique opportunity to identify the impact of the legal regime on union organizing.

My analysis uses the fact that different laws were changed at different times in the province, allowing for the impact of elections to be isolated from other legislative changes such as decertification, picketing, first contract arbitration, essential services, strike and lockout, dues check-off, dispute resolution, and last offer laws. The results indicate that mandatory elections can account for virtually the entire 19 percentage point decline in private sector certification success rates that occurred over the 1984 to 1992 period.

Management opposition—as measured by unfair labor practices—was at least twice as effective in the voting regime as in the card-check regime. The *additional* impact of ULPs in the voting regime ranged from 6 to 8 percentage points. However, the incidence of management opposition was slightly lower in the voting regime. This apparent anomaly may have arisen because unions, perceiving a reduced likelihood of success, reduced their number of certification attempts and concentrated only on units with high levels of inherent support. Faced with high union support in those settings, employers may have been less likely to oppose the bid even though their opportunity to do so was greater. The decompositions indicate that management opposition can account for 25% of the overall decline in certification success rates. However, other evidence suggests that ULPs may only be capturing the more egregious cases of management opposition. Thus, there is reason to believe that the 25% contribution is understated.

To conclude, I wish to discuss some limitations of this study, some caveats, and some suggestions for future research on this important topic. Perhaps most important, caution should be exercised in extending the results in this paper to the decline of union organizing success in the United States, and to the differing fortunes of unionization in the United States and Canada. Recall that elections in British Columbia included a *10-day* expedited voting procedure—a law that was strictly enforced. At no time did British Columbia truly have a U.S.-style system in which there was voting and no time limit on when the vote occurred. That said, given that management opposition was twice as effective under elections and that organizing drives have become increasingly long in the United States, I feel that the results are consistent with the "management opposition" story of the decline in U.S. certification success rates as told by Weiler, Freeman, and others.

Although restraint is called for in using the results reported here as a lens for interpreting union certifications in the United

States, I believe the results have clear policy implications for U.S. labor law reform. In particular, many authors have recently argued that card-check procedures—through negotiated agreements—show considerable promise for reducing illegal employer tactics, and generally creating a fairer playing field in the union organizing process (Budd and Heinz 1996; Benz 1998; Eaton and Kriesky 2001). Other authors argue that card-check procedures artificially inflate support levels (Yager, Bartl, and LoBue 1998). An obvious compromise is so-called "instant" elections, as proposed by Weiler (1983, 1990), whereby the vote takes places a few days following the application. The case of British Columbia—with a strictly enforced 10-day time limit during the voting years—illustrates that employers can still influence the election outcome even with a short time limit.

Finally, the analysis in this paper demonstrates that elections appear to account for the entire decline in certification success rates. However, as noted, while success rates remained high—relative to those in other countries—the number of certification attempts fell by around 50% in 1984, and even after the 1993 legislation restoring card-check rules it never recovered to the pre–1984 levels. While a number of explanations could exist for the decline in organizing activity, it may be the case that other industrial relations legislation had an impact on the number of certification attempts. Future research should investigate the effect of both union recognition laws and other industrial relations laws on overall union organizing.

Appendix 1
Labour Code in British Columbia, 1978 to 1998

Law	*Labour Code in Effect as of 1978*	*1984 Labour Code Amendment Act*	*1987 Industrial Relations Reform Act*	*1993 Labour Code Amendment Act*
Union Recognition	Section 39: Automatic certification if 55%+ sign cards, vote held if between 45% and 55%.	Section 39 repealed and replaced: Vote held if 45%+ sign cards; expedited voting regulation of "approximately" 10 days.	Section 43 replaces: Vote held if 45%+ sign cards; expedited voting of 10 days.	Section 18 replaces: automatic certification if 55%+ sign cards; vote held if between 45% and 55%; expedited voting of 10 days (if vote required).
Decertification	Section 52: Upon application by some party, Board rules on whether union has ceased to be agent of employees or employer has ceased to employ workers.	Section 52 repealed and replaced: Vote held if 45%+ sign cards; expedited voting regulation of "approximately" 10 days.	Section 52 amended: Vote held if 45%+ sign cards; expedited voting of 10 days.	Section 33 replaces: Vote held if 45%+ sign cards; expedited voting of 10 days.
Strike and Lockout Regulations	Section 79: No strike or lockout can take place while a collective agreement is in force (subject to essential service designation), or before bargaining and strike vote.	Section 79: No trike or lockout scan take place while a collective agreement is in force (subject to essential service designation), or before bargaining and strike vote.	Part 8.1 introduced: Written notice first required plus minimum 72-hour wait, potential 48-hour wait following mediator review if mediator appointed, potential 48-hour wait following fact-finding report if fact-finder appointed; Section 79 in effect as well.	Part 7 replaces Part 8.1 (see below): Time limits modified; Section 70 introduced; Section 57 and 59 replaces Section 79: Temporary strike replacement workers restricted. No strike or lockout can take place while a collective agreement is in force (subject to essential service designation), or before bargaining and strike vote.
Picketing	Sections 85 and 86: Employees can picket at employer, any other location of the employer, a common site, or any location of an ally of the employer.	Sections 85 and 86 repealed and replaced: After application and review, Board may allow employees to picket at another location of the employer or a location of an ally of the employer; cannot picket a common site unless common site is an ally (and the application is approved).	Section 85 amended: After application and review or by its own motion, the Board may allow employees to picket at another location of the employer or a location of an ally of the employer; cannot picket a common site unless common site is an ally (and the application is approved).	Section 65 replaces: After application and review or by its own motion, the Board may allow employees to picket at another location of the employer or a location of an ally of the employer; cannot picket a common site unless common site is an ally (and the application is
Last Offer Received Voting	No law applicable.	No law applicable.	Section 137.7 introduced: Employer may request that, before a strike vote, employees vote on last offer received.	Section 78 replaces: Employer may request that, before a strike vote, employees vote on last offer received.

continued

Appendix 1 ***Continued.***

Law	*Labour Code in Effect as of 1978*	*1984 Labour Code Amendment Act*	*1987 Industrial Relations Reform Act*	*1993 Labour Code Amendment Act*
Essential Services	Section 73: Firefighters, police, and hospital unions cannot strike and cannot be locked out; no other essential service designation.	Section 73: Firefighters, police, and hospital unions cannot strike and cannot be locked out; no other essential service designation.	Section 137.8 replaces: Employer can apply for "cooling-off period" and/or essential service designation if dispute threatens "economy of province, health/safety/welfare of province, or education."	Section 72 replaces: Employer can apply for essential service designation if dispute threatens "health/safety/welfare of province."
Collective Bargaining Dispute Resolution Procedures	Section 69: Mediation services (Minister may upon application appoint a mediation officer to confer with both parties and assist in reaching an agreement). First contract arbitration available under section 70.	Section 69: Mediation services (Minister may upon application appointa mediation officer to confer with both parties and assist in reaching an agreement). First contract arbitration available under section 70.	Part 8.1 (sections 81 and 82) replaces: Mediation introduced by application,at the direction of the Commissioner or by IRC's own motion (non-binding; full review and report conducted, can be made public); fact-finding introduced as above (non-binding; full review and report conducted, can be made public); public interest inquiry board introduced (employees can be made to vote on decision); intervention power (by resolution of Legislative Assembly) to end a dispute. First contract arbitration available under section 1375.5.	Part 7 (sections 76, 77, and 79) replaces: Mediation by application, or at the direction of the Board (non-binding; full review and report conducted, can be made public); fact-finding as above (non-binding; full review and report conducted, can be made public); public interest inquiry board relabeled as Industrial Inquiry Commission (employees can be made to vote on decision). First contract arbitration available under section 55.
Additional Union Membership and Dues Regulations	No law applicable (mandatory dues check-off in effect, section 10).	Section 5 introduced: Job loss or job-related discrimination prohibited for not participating in unlawful industrial action (mandatory dues check-off in effect, section 10).	Section 5.1 introduced: Job loss or job-related discrimination prohibited for not being a union member unless individual failed to pay dues; mandatory dues check-off removed in cases of religious convictions, in effect in all other cases.	Section 5 replaces: Job loss or job-related discrimination prohibited for being member of different union; mandatory dues check-off removed in cases of religious convictions, in effect in all other cases.

Notes: The 1973 LCA (including 1974 and 1976 amendments) was the legislation in effect as of 1978. To simplify the exposition, the following terms are adopted: *introduced* means that the section is making its first appearance in the Code, *replaces* means that the section replaces the existing section (often the section number remains the same). Note that in many cases the language of the law was identical even if a new section number was introduced.

Appendix 2
Selected Statistics from British Columbia, 1978 to 1998

Year	*Certifications Filed, Annual Report*	*Certifications Filed, Micro-Data*	*Cases Filed Sub-Section 8(4)(E) [Number Successful]*	*Cases Filed Sub-Section 8(4)(C) [Number Successful]*	*Average Total Processing Time for Vote Certification*	*% of Cases in Micro-Data Going to a Vote*	*Dismissal ULPs*	*Non-Dismissal ULPs*
1978	672	54[a]	17 [1]	55 [37]	145.60	4.00	71	46
1979	662	500	25 [1]	59 [40]	167.27	3.24	69	34
1980	790	556	22 [0]	53 [38]	116.83	4.67	77	39
1981	848	659	34 [2]	69 [58]	135.24	3.79	99	49
1982	916	593	15 [2]	74 [65]	126.15	4.60	107	54
1983	678	410	18 [0]	56 [47]	141.95	5.31	81	67
1984	376	221	21 [3]	62 [52]	39.03	55.45	80	56
1985	333	197	16 [2]	42 [36]	21.94	99.00	62	69
1986	385	249	18 [2]	56 [39]	25.70	99.02	74	84
1987	343	164	17 [0]	41 [30]	23.15	100.00	77	73
1988	509[b]	238	10 [0]	37 [25]	14.75	100.00	53	55
1989	424	253	10 [0]	23 [13]	18.39	100.00	37	97
1990	437	287	18 [3]	34 [26]	17.84	99.01	64	250[c]
1991	414	268	20 [1]	35 [34]	15.83	99.72	73	232
1992	340	207	32 [6]	31 [28]	24.44	97.11	56	220
1993	683	424	31 [2]	NA	40.43	14.31	NA	NA
1994	646	336	31 [2]	NA	43.56	8.50	NA	NA
1995	606	298	35 [0]	NA	35.93	10.58	NA	NA
1996	611	348	41 [1]	NA	37.33	9.54	NA	NA
1997	589	279	52 [3]	NA	37.55	7.61	NA	NA
1998	528	109[a]	40 [0]	NA	39.25	9.49	NA	NA

Notes: Column (1) is the total number of certifications filed, as given in the BCLRB annual report. Column (2) is the total number of certifications based on the micro-data used in the analysis, and thus excludes raids, withdrawn and "public-sector" certifications, as well as petition violation cases (except <45% signed up). Column (3) refers to the power—subsection 8(4)(e), relabeled subsection 14(4)(f) in 1993—of the BCLRB to impose automatic certification without a vote in cases of a ULP. Subsection 8(4)(c) in column (4) refers to compensation claims for dismissal ULPs. With respect to the number of successful 8(4)(c) cases in parentheses, only those dismissal ULPs that resulted in a decision are included (that is, withdrawn cases excluded, which includes some settlements). I consider a successful case as being an individual who received *some* form of compensation (that is, re-instatement, back-pay, or both). Column (5) is the time from the application date to the decision date, since the date of the vote is not always recorded in the data; the 1984 cell consists of both voting and card-check laws. Column (6) is the percentage of column (2) cases (that is, excluding raids, withdrawn, and public sector certifications as well as petition violations) that went to a vote. Columns (7) and (8) show the number of ULP cases by the nature of the charge. The source for columns (2) and (6) is the micro-data on certification applications collected from the BCLRB, while all other statistics are from the Annual Report of the BCLRB.

[a]For the micro-data, the 1978 cell consists of October to December only and the 1998 cell consists of January to May only.

[b]Includes 75 certifications of teachers (over 31,000 employees—all successful), who were given the right to unionize in 1988.

[c]The non-dismissal ULP numbers for post–1989 are not comparable to the numbers for earlier years (see the text for further discussion).

Appendix 3
Decomposition Techniques

If we rewrite (1) from the text as

$$\text{(2a)} \qquad \text{Prob(SUCCESS =1)} = \mathrm{F}(X_{ij}'\beta_j),$$

then the predicted probability of certification success is

$$\text{(2b)} \qquad \hat{P}(X_{ij}\beta_j) = 1/\mathrm{N_j} * \sum_{i=1}^{n_j} \Phi\ (X_{ij}'\hat{\beta}_j),$$

where X_{ij} is a vector of the covariates of the i^{th} bargaining unit in legislative regime j; $\hat{\beta}_j$ is a vector of probit parameters estimated from legislative regime j; N_j is the number of bargaining units in regime j; and Φ is the standard normal cumulative distribution function.

If a jurisdiction changes union recognition procedures, then the change in predicted certification success rates can be written as

$$\hat{P}(X_{\text{CARD}}, \beta_{\text{CARD}}) - \hat{P}(X_{\text{VOTE}}, \beta_{\text{VOTE}}),$$

which yields a positive gap. This can be decomposed as

$$\text{(2c)} \qquad \hat{P}(X_{\text{CARD}}, \beta_{\text{CARD}}) - \hat{P}(X_{\text{VOTE}}, \beta_{\text{VOTE}}) = [\hat{P}(X_{\text{CARD}}, \beta_{\text{CARD}}) - \hat{P}(X_{\text{VOTE}}, \beta_{\text{CARD}})] + [\hat{P}(X_{\text{VOTE}}, \beta_{\text{CARD}}) - \hat{P}(X_{\text{VOTE}}, \beta_{\text{VOTE}})].$$

The first term on the right-hand side of (2c) indicates what part of the decline in success rates is due to a change in the means of the covariates holding the coefficients constant at their card-signing levels. The second term indicates what part of the decline in success rates is due to a change in the coefficients holding the means constant at their voting levels. An alternative to (2c) reverses the comparison group (that is, difference in means holding the coefficients constant at voting levels and difference in coefficients holding the means constant at card-check levels).

Due to Φ being non-linear, there is no unique way to decompose the decline in success rates. Following Doiron and Riddell (1994) and Yun (2000), I adopt a modification of (2c) that allows the decline in success rates to be decomposed despite the non-linear specification. The approach adopted is based on a linear approximation of the probability of certification and proceeds in two stages. In the first stage, the sample average of the standard normal CDF is approximated by the standard normal CDF at the sample mean of the explanatory variables. In the second stage, the differences between the two standard normal CDFs from stage one are approximated by a first-order Taylor expansion about $\bar{X}_j\hat{\beta}_{j'} = \bar{X}_j\hat{\beta}_j$, where $j \neq j'$. For more details, see Doiron and Riddell (1994) and Yun (2000). The final decomposition formula is

$$\text{(2d)} \qquad \hat{P}(X_{\text{CARD}}, \beta_{\text{CARD}}) - \hat{P}(X_{\text{VOTE}}, \beta_{\text{VOTE}}) = (\bar{X}_{\text{CARD}} - \bar{X}_{\text{VOTE}})\ \hat{\beta}_{\text{CARD}}\phi(\bar{X}_{\text{CARD}}\ \hat{\beta}_{\text{CARD}}) + (\hat{\beta}_{\text{CARD}} - \hat{\beta}_{\text{VOTE}})\ \bar{X}_{\text{VOTE}}\phi(\bar{X}_{\text{VOTE}}\ \hat{\beta}_{\text{VOTE}}) + R_1 + R_2,$$

where R_1 and R_2 are residual terms from each approximation stage (see Yun 2000 for the R_1 and R_2 formulas) and ϕ is the standard normal probability density function.

Essentially the only difference between (2d) and a standard Oaxaca-Blinder decomposition is the approximation error and the fact that the coefficients are multiplied by a standard normal PDF. Given that nonlinear decompositions remain an uncertain area, in the sense that there are a number of methods available with no consensus on which method is more reliable, I also estimate the certification success equations by union recognition regime using a linear probability model and decompose the change in success rates using a standard Oaxaca-Blinder decomposition.

As is well known (Jones 1983), it is not straightforward to further sub-decompose the total coefficient component—that is, $(\hat{\beta}_{\text{CARD}} - \hat{\beta}_{\text{VOTE}})\,\bar{X}_{\text{VOTE}}\phi(\bar{X}_{\text{VOTE}}\ \hat{\beta}_{\text{VOTE}})$—into each individual coefficient contribution, which is required to identify how much the change in the *effectiveness* of management opposition contributed to the change in success rates. Using the method in Nielson (1999), I provide one possible estimate of the contribution of the change in effectiveness of management opposition. The drawback of Nielson's approach is that even with a relatively small number of covariates, a huge number of computations are required. The Nielson method essentially uses the implicit constant term for each "group of people" (in the present case, each "legislative regime") as a basis for the decomposition, since these parameters are the same regardless of what omitted categories are used, and is computed as follows (in the case of only a single dichotomous regressor, ULP, and simplifying to a linear probability model):

$$\text{(2e)} \qquad (\hat{\beta}_{\text{CARD}} - \hat{\beta}_{\text{VOTE}})\,\bar{X}_{\text{VOTE}} = \zeta_{\text{VOTE1}}[(\hat{\beta}_{0,\text{CARD}} - \hat{\beta}_{0,\text{VOTE}}) + (\hat{\beta}_{\text{ULP,CARD}} - \hat{\beta}_{\text{ULP,VOTE}})] + \zeta_{\text{VOTE2}}[\hat{\beta}_{0,\text{CARD}} - \hat{\beta}_{0,\text{VOTE}}],$$

where ζ_{VOTE1} and ζ_{VOTE2} are the proportion of voting regime certifications with and without a ULP, respectively (Nielson 1999).

Appendix 4
Summary Statistics

Variable	*Full Sample, 1978–1998*	*Card-check/ ULP Sample, 1993–1998*	*Voting/ ULP Sample, 1987–1992*	*Variable*	*Full Sample, 1978–1998*	*Card-check/ ULP Sample, 1993–1998*	*Voting/ ULP Sample, 1987–1992*
Unfair Labor Practice	—	.231	.213	NIU	.041	.077	.070
		(.422)	(.409)		(.198)	(.266)	(.256)
1976 LCA Act	.432			PIU	.010	.020	.007
	(.495)	—	—		(.101)	(.140)	(.082)
1984 LCA Act	.084	—	—	ILU	.013	.009	.017
	(.279)				(.114)	(.097)	(.131)
1987 IRR Act	.213	—	—	Teamsters	.079	.065	.091
	(.410)				(.269)	(.246)	(.288)
1993 LCA Act	.270	—	—	Manufacturing	.185	.191	.227
	(.444)				(.388)	(.394)	(.419)
Industrial Concentration	.625	.565	.606	Construction	.381	.304	.297
	(.212)	(.210)	(.213)		(.486)	(.460)	(.457)
Unit Representation	.038	.048	.041	Transportation	.068	.079	.090
	(.121)	(.126)	(.126)	and Storage	(.252)	(.270)	(.286)
Local Representation	.261	.236	.310	Primary Industries	.043	.058	.046
	(.268)	(.271)	(.309)		(.202)	(.234)	(.210)
% Female	.165	.176	.191	Trade	.120	.114	.120
	(.204)	(.199)	(.211)		(.325)	(.317)	(.325)
CLC	.188	.346	.222	Hotel and	.064	.075	.077
	(.391)	(.476)	(.416)	Restaurant	(.245)	(.264)	(.267)
AFL-CIO/CLC	.192	.162	.251	Business Services	.027	.029	.026
	(.394)	(.368)	(.434)		(.163)	(.167)	(.160)
CFL	.456	.312	.314	Other Services	.112	.149	.116
	(.498)	(.463)	(.464)		(.315)	(.356)	(.321)
CCU	.021	.010	.028	Unemployment	9.705	8.907	9.924
	(.143)	(.099)	(.165)	Rate	(2.557)	(0.635)	(1.222)
				Sample Size	6,650	1,698	1,325

Notes: Means (and standard deviation in parentheses) are based on the certifications micro-data used in the regression analysis (that is, raids, petition violations, "public" sector certifications excluded). To simplify the table, summary statistics for region (UI region within British Columbia) and year are not shown.

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