Effects of Job Search on Union Certification Elections

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Abstract

Using data from the U.S. National Labor Relations Board, we find labor market tightness and unemployment insurance are determinants in explaining the likelihood of voting in favor of union representation. Specifically, a one standard deviation increase in labor market tightness increases vote share and the likelihood of certification of representation by roughly 1.5% and 3%, respectively. Furthermore, we find the length of unemployment insurance (UI) benefits affects vote share although it is not statistically significant in the specification using certification as the dependent variable. Using vote share, our model predicts the likelihood of certification would rise from 57% to 81% when setting the maximum length of UI to 24.5 weeks.

Keywords: job openings; unemployment; labor market tightness; search; unemployment insurance; union elections; unionization strategies

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1 Introduction

Strong opposition by firms is known to reduce the likelihood of representation as documented by Freeman and Kleiner [1990] among many others. In particular, Bronfenbrenner [2009] documents employers threaten a plant shutdown in 57% of U.S. elections and plants actually close after the election 15% of the time. Bronfenbrenner [1997] describes the effects of firm shutdown in more detail. The bottom line is the likelihood of representation is significantly less when such a threat is used. Using a different approach, Ferguson [2008] tracks unfair labor practice charges against the firm, in particular an 8(a)(3), which is often associated with firings for union activity. The data shows roughly 7.5% of elections contain such a charge and the U.S. National Labor Relations Board (NLRB) has ruled in favor of the unfair labor practice in roughly half of the filings. Therefore, the threat of layoff is real. Going further, Bronfenbrenner [2009] provides evidence that the number of charges filed, including 8(a)(3), could be half the number of actual occurrences as union organizers often don't file charges as it will delay the vote and therefore put the success of the drive in jeopardy.

As the threat of job loss is a prominent tactic used to oppose the union certification process and there is evidence it has been successful, we expand on the hypothesis of the effect of job loss on certification by testing whether a more favorable labor market, in particular the number of job openings relative to the number unemployed, or labor market tightness, increases vote share and representation. Furthermore, we test whether government provided unemployment insurance (UI), payments that reduce the cost of job loss, also increases vote share and certification. We find both have a positive and statistically significant effect on vote share. In particular, a one standard deviation in the natural log of labor market tightness increases the number of votes in favor of representation, as well as certification, by roughly 1.5% and 3%, respectively. In terms of UI, we find the maximum length of UI has a staticially significant impact on vote share although its impact declines as the length of UI payments increases. We predict using our model and sample that setting UI to its optimal length from the various levels states set now would increase the success of certification from 57% to 81%. We test whether the level of weekly UI payments affects vote share and certification and find no statistically significant effects in either specification.

The literature on the effect of union strategies on certification has been surveyed by Heneman III and Sandver [1982]. More recent analyses include Bronfenbrenner [1996], Ferguson

[2008], and Flanagan [2005] among others. These analyses take one of two different approaches. One approach is to use data from the NLRB on the outcomes of elections, potentially combined with aggregate level measures like unemployment, and tests the effects of different strategies such as delay or targeting firms with smaller or bigger size bargaining units. The second approach uses survey data of firms and union activists. Both approaches find similar results in terms of the effect of bargaining unit size, delay in election, etc. We take the first as the data is publicly available.

Besides election strategies, a strong link between UI and unions has already been investigated empirically and theoretically in countries with the "Ghent" system. Specifically, unions are a predominant administer of UI in Belgium, Denmark, Finland, and Sweden. Holmlund and Lundborg [1999] states "the G(h)ent system is more conducive to unionization than a compulsory UI system if the Ghent system is heavily subsidized by the government or if workers are strongly risk averse." We provide empirical support for Holmlund and Lundborg [1999] although the mechanism we analyze is narrower. Specifically, we analyze how UI affects elections without any institutional connection between UI and unions. Furthermore, Schnabel and Wagner [2003] states "Empirical tests of this hypothesis (link between UI and unions) consistently find that the presence of a union-administered unemployment insurance is an important determinant of cross-national differences in union density levels" citing Western [1997] and Ebbinghaus and Visser [1999] among others. Going further, Dimick [2011] makes the case for the Ghent system to be applied to the U.S. However, all of these study the Ghent system as it pertains to unions as an institutional change, while we analyze how UI affects costs related to the certification process. Therefore, our results are related and support this part of the literature. However, we use exclusively U.S. data and therefore cannot analyze how such an institutional link would affect certification.

2 Data and model

The NLRB provides information on elections including the date the petition for election was filed, the date the election took place, the number of eligible voters, the number who voted in favor of representation, the number voting against, and the city and state of the bargaining unit. From October 1999 through September 2010, the NLRB also provides the North American Industry Classification System (NAICS) classification code for the bargaining unit. In order to include the additional information from the NAICS, we will focus on the 1999-2010 window of observations.

To analyze the effect of the job search on elections, we merge a variety of job search related variables to the NLRB dataset. Specifically, we use the Job Openings and Labor Turnover Survey (JOLTS) and merge job openings and separation rates at the four regional levels on a monthly basis using the election date as the merging date. More localized regions, such as at the state level, are not publicly available.[1] JOLTS data are available starting December 2000. Furthermore, we acquire UI data from the Benefit Accuracy Measurement (BAM) database collected by states and provided to the U.S. Department of Labor Employment & Training Administration. UI benefits are primarily determined and administered by the states. Rather than tracking changes in regulations in the six major determinants of UI across all 50 states over time,[2] regulations that are not always easily comparable, we use actual payment data provided by BAM. The variables we use in our analysis are the median weekly UI payment and the median length of time UI pays out - commonly 26 weeks.

In addition to election, industry, and job search related information, we include an income variable (in 2013 dollars) for each election that tracks the bargaining unit's average individual's industry level income in the state and year of the election. Similarly, we incorporate unemployment data at the state and month level to construct the labor market tightness variable. To reiterate, labor market tightness is the number of job openings divided by the number of unemployed. The income data are available from the Quarterly Census of Employment and Wages, collected by the Department of Labor Statistics, and publicly available from 2001 to present, and the unemployment data are available from the Current Popultion Survey, also made available by the Bureau of Labor Statistics. We incorporate the right to work laws for each state in the summary statistics, but it did not change during the period of analysis. Therefore, the state-level controls eliminate the variable from analysis due to perfect collinearity. The data is summarized in Table 1.

To test the link between certification and job search, we use the standard linear approach

[1]The region definitions are the Northeast: Connecticut, Maine, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, and Vermont; South: Alabama, Arkansas, Delaware, District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia; Midwest: Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, and Wisconsin; West: Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington, and Wyoming.

[2]The primary determinants tracked by the U.S. Department of Labor's Office of Workforce Security as published in "Significant Provisions of State Unemployment Insurance Laws" include earnings/employment needed in base period to qualify, computation of weekly benefit amount, minimum and maximum benefit, weekly earnings disregard, calculation of number of benefit weeks, and coverage.

$$
\begin{aligned}
y_i &= \beta_0 + \beta_1 ln\_eligible_i + \beta_2 ln\_delay_i + \beta_3 inc_i + \beta_4 ldr_i + \beta_5 ln\_market\_tightness_i \\
&\quad + \beta_6 weekly\_ui_i + \beta_7 weekly\_ui_i^2 + \beta_8 length\_ui_i + \beta_9 length\_ui_i^2 + \gamma_i + \delta_i + \alpha_i + \varepsilon_i
\end{aligned} \tag{1}
$$

where we run three specifications for y_i: (1) percent who vote in favor of union representation, referred to as vote share hereafter, (2) the natural log of the vote share, and (3) a dummy variable where $y_i = 1$ if certification of representation occurred and zero otherwise. In the first two cases, we use least squares and in the last a probit regression. The remaining variables are $ln\_eligible_i$ for the natural log of eligible voters, ln_delay_i for the natural log of days between the date the petition was submitted and the date the election occurred, inc_i is the average industry income level for the bargaining unit in the state and year of the election, ldr_i is the layoff and discharge rate for the region and month, $ln\_market\_tightness_i$ is the natural log of labor market tightness, or the number of job openings divided by the number unemployed, in the month and region of the election, $weekly\_ui_i$ is the median person's weekly unemployment insurance payment in the state and year of the election, $length\_ui_i$ is the median length of eligibility for unemployment insurance in the state and year of the election, and finally, controls for the bargaining unit's industry (γ), state (δ), and year of election (α) are included to capture any industry, state, or year-specific effects on certification.

We incorporate several important no-linear terms. First, we incorporate curvature in the labor market tightness variable to account for the search literature that assumes a matching function that is increasing at a decreasing rate with respect to labor market tightness. Pissarides [2000] is the canonical source. Second, we include a squared term for both forms of the UI variables. Curvature in the $weekly\_ui_i$ allows us to account for a declining marginal utility of income. Curvature in $length\_ui_i$ also allows us to capture the fact that the probability of receiving UI each week is declining as time passes. Nearly everyone who files collects UI in the first week, but only 35-40% are collecting in week 26 as many have located a job and are no longer eligible to collect UI.

3 Results

The results are in Table 2. As is standard in previous work, such as those discussed in Heneman III and Sandver [1982], we find the size of the bargaining unit reduces both the likelihood of voting in favor of representation as well as electing the union, voter turnout reduces both the likelihood

of voting in favor of representation as well as electing the union, and the delay in the election also reduces both vote share and the likelihood of certification.

The key result is the effect of the job search process on the likelihood of certification. In particular, labor market tightness has an important and statistically significant effect on the share of votes in favor of unionization and the outcome of the election. In particular, a one standard deviation change in the natural log of labor market tightness increases the percent vote for representation by roughly 1.5 percentage points in the linear specification, roughly 3% in the log-linear specification, and 2.3% in the probit specification.

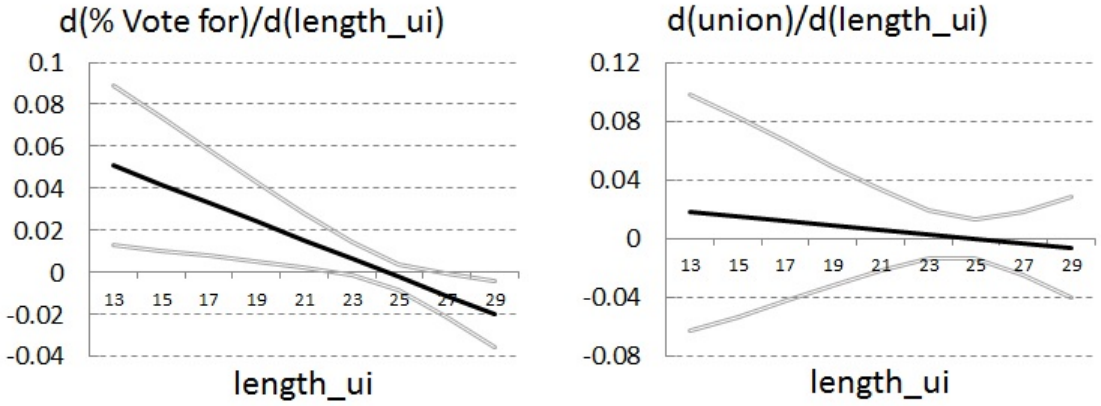
In addition to the likelihood of unionization when job openings are high, we investigate the effect of UI on votes and unionization. If the fear of layoff is high as a result of unionization and decreases the likelihood of voting for representation, then a robust UI should decrease the costs of such a layoff and thereby increase the likelihood of voting in favor of a union. Our results suggest such a link although it is not robust across all specifications. First, although the point estimates are positive, we do not find $weekly\_ui_i$ has a statistically significant effect on vote share or certification. The formula for UI is relatively complicated and usually incorporates an individuals previous two quarters of earnings, has a min and max level, an earnings disregard, and other complications. Therefore, it isn't surprising the variation in weekly UI payments does not affect unionization given uncertainty about benefits as many may be unsure what benefit they will be entitled to in the event of a layoff or discharge. Alternatively, the length of UI payments is statistically significant and shows a positive benefit, but at a decreasing rate, for the first two specifications. In particular, the marginal effect is

$$dy/(length\_ui) = \beta_8 + 2\beta_9 length\_ui. \tag{2}$$

The results for the linear specification are plotted in figure 1. To interpret the results, if a state such as North Carolina increased benefits from 14 to 26 weeks, then the likelihood of a individual voting in favor of representation would rise by 23.7%. North Carolina is an extreme example because it has the smallest maximum number of weeks although their are many comparable state UI systems. However, Massachusetts has a maximum length of 26 weeks in our sample. Therefore, the marginal impact of raising UI benefits is effectively zero, or at least zero cannot be rejected using a 95% confidence level.

As an alternative calculation, one could estimate the optimal level of UI benefits to deter-

Figure 1: Marginal Effects of Length of UI Payments



Note: The center black line is the point estimates. The surrounding gray lines use the delta method to plot +/- two standard errors from the point estimate.

mine vote share. The optimal is found by setting 2 equal to zero, which determines $length\_ui^* = 24.46$. The in-sample prediction using the vote share results predicts a union win rate rising from roughly 57% at the current varying length of UI across states to 81% at $length\_ui^* = 24.46$ for all states. Therefore, the optimal level of UI can have significant effects on unionization certifications. Roughly 900,000 individuals gained representation through the election process during our sample period. Our results suggest setting the optimal level of UI would have increased the number by roughly 400,000.

Alternatively, the probit results of the certification model do not find a statistically significant result although the point estimates have the same signs. The marginal effects of the *length_ui* is smaller as seen in Figure 1. However, as a comparison, the optimal length of UI is 24.88 weeks in the probit specification. Furthermore, the optimal length of UI predicts in our sample an increase of union election wins from 57% to 68%. Therefore, the optimal level of UI would increase unionization by 200,000 in the certification model. To reiterate, this discussion is highly speculative as the standard errors are too large to suggest the effect is not zero.

To end the results section, we want to reiterate the fact that we did not include a right-to-work variable in the results as it is perfectly collinear with the state dummies due to the fact it is unchanged in the U.S. during our sample period. When comparing the statistically significant results in the "% Vote for Representation" and those of actual certification, we see the point estimates of the *marginal effects* are similar. However, the standard errors become larger probably because

the dependent variable for certification has limited variation, i.e., 0 or 1. Therefore, although the statical significance across the specifications are not perfectly comparable, they deviate in an unsurprising way.

4 Conclusion

Building on the union certification literature, we focus on the effect layoffs, discharges, and plant closings have on union certification elections. As employers often threaten layoffs if certification occurs, we hypothesize the job prospects and safety net in such a case effects voting behavior. Our results confirm this hypothesis. We find job openings increase votes, and as a further check depending upon the specification, we find increasing the length of unemployment insurance also increases the likelihood of voting in favor of union representation.

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Table 1: Summary Statistics

Variable	Full Sample	No Certification	Union Certification
% Vote For Representation	0.594	0.349	0.78
	(0.272)	(0.156)	(0.179)
Union Certification	0.568	0	1
	(0.495)	(-)	(-)
Mining, utilities, and construction	0.149	0.129	0.165
	(0.356)	(0.335)	(0.371)
Manufacturing	0.203	0.266	0.155
	(0.402)	(0.442)	(0.362)
Trade	0.242	0.246	0.239
	(0.428)	(0.431)	(0.426)
Business	0.178	0.168	0.186
	(0.382)	(0.373)	(0.389)
Education and Health	0.18	0.144	0.207
	(0.384)	(0.351)	(0.405)
Entertainment and Leisure	0.05	0.05	0.05
	(0.21)	(0.21)	(0.22)
Right to Work	0.209	0.208	0.21
	(0.407)	(0.406)	(0.407)
Natural Log of Eligible Voters	3.392	3.611	3.225
	(1.28)	(1.204)	(1.31)
Voter Turnout	0.85	0.88	0.82
	(0.19)	(0.13)	(0.22)
Natural Log of Time	3.87	3.93	3.83
	(0.65)	(0.68)	(0.61)
Annual Income	50,232	51,061	49,603
	(16,194)	(16,282)	(16,098)
Layoffs and Discharges Rate	1.723	1.721	1.725
	(0.218)	(0.216)	(0.22)
Natural Log of Labor	3.769	3.787	3.755
	(0.432)	(0.414)	(0.445)
Weekly UI Payments	337.84	337.31	338.24
	(46.23)	(47)	(45.63)
Length of UI Payments	25.536	25.531	25.54
	(1.371)	(1.347)	(1.389)
Observations	17489	7550	9939

Note: Standard deviations are in parentheses.

Table 2: Results

	% Vote For Representation	Ln % Vote For Representation	Representation
Natural Log of	-0.07***	-0.117***	-0.205***
Eligible Voters	(0.002)	(0.004)	(0.009)
Voter Turnout	-0.388***	-0.562***	-1.553***
	(0.011)	(0.025)	(0.064)
Natural Log of Time btw	-0.031***	-0.06***	-0.157***
Petition and Election	(0.003)	(0.007)	(0.016)
Income	-3.45E-07**	-3.93E-07	-2.67E-06***
	(1.45E-07)	(3.40E-07)	(7.61E-07)
Layoffs and Discharges Rate	-0.001	0.001	0.035
	(0.01)	(0.024)	(0.055)
Ln Labor Market Tightness	0.029***	0.056**	0.119**
	(0.011)	(0.026)	(0.059)
Weekly UI Payments	1.82E-05	1.99E-04	8.68E-04
	(6.95E-04)	(1.63E-03)	(3.66E-03)
Weekly UI Payments Squared	5.59E-08	-8.32E-09	-7.23E-07
	(1.01E-06)	(2.36E-06)	(5.30E-06)
Length of UI Payments	0.108***	0.211**	0.094
	(0.04)	(0.095)	(0.217)
Length of UI Payments Squared	-0.002***	-0.004**	-0.002
	(0.001)	(0.002)	(0.004)
Constant	-0.251	-2.588**	1.326
	(0.506)	(1.188)	(2.716)
R^2	0.189	0.106	
LogL			-11,043.17

Note: Standard errors are in parentheses. *** represents significance at 1%, ** at 5%, and * at 10%. The estimates are the parameters of equation 1. State, year and industry controls are included in all regressions but excluded from the table.