Governors' Party Affiliation and Unions*

Louis-Philippe Beland Bulent Unel

May 2016

Abstract

Employing a Regression Discontinuity (RD) approach on gubernatorial elections in the U.S. over the last three decades, this paper investigates the causal effects of governors' party affiliation (Democratic vs Republican) on unionization of workers, and unionized workers' working hours and earnings. Surprisingly, we find no significant impact from the party affiliation of governors on union membership and union workers' labor-market outcomes.

JEL Classification: J58, J31, D72
Keywords: Political Parties, Unions, Voting behavior, Regression Discontinuity

*L. Beland and B. Unel: Department of Economics, Louisiana State University, Baton Rouge, LA 70803. E-mail: lbeland@lsu.edu and bunel@lsu.edu. This paper previously was circulated as "Democrats and Unions." We are indebted to Ozkan Eren, Barry Hirsch, Chris Riddell, and an anonymous referee for their helpful comments and suggestions.

1 Introduction

Since the success of Franklin D. Roosevelt's New Deal in the 1930s, which greatly benefited labor organizations by giving workers the right to join a union, unions have shown a strong allegiance to the Democratic Party. Unions have played an important role in the Democrats' success by encouraging their members to support the party and raising money for Democratic candidates. For instance, according to National Institute for Labor Relations Research (NILRR) estimates, unions spent about $1.4 and $1.7 billion in the 2010 and 2012 election cycles, respectively (NILLR, 2013), and the overwhelming majority of this spending went to Democrats. In response to this strong support from unions, Democrats claim that "[F]or decades, Democrats have stood alongside labor unions in defense of fair pay and economic security."[1]

In this paper, we investigate whether governors' party affiliation (Democratic vs Republican) has had any impact on unionization (and deunionization) of workers as well as their working hours and earnings. Using data on union membership in the Current Population Survey (CPS) Outgoing Rotation Group (ORG) files over 1983–2013 together with gubernatorial election results in 50 states, we address the question by exploiting random variation associated with *close* elections in a regression discontinuity (RD) design. We utilize an RD design for our analysis, because the simple OLS approach suffers from the endogeneity problem arising from factors such as voter characteristics, party incumbency, labor-market conditions, etc. Surprisingly, we find that governors' party affiliation has had no significant effect on unionization of workers. We also find that party affiliation of governors has no impact on labor-market outcomes of unionized workers (relative to non-unionized ones).

These findings are surprising because U.S. governors have a high degree of autonomy in exercising their power in their policy choices. Governors head the executive branch, which is responsible for proposing the budget, recommending legislation, and appointing key personnel. In addition, state governments have powers to levy taxes, establish license fees, spend tax revenues, regulate businesses, manage the health-care system, and provide emergency services. By having the right to veto state bills, governors have considerable control over state policies. Several studies have documented that the party allegiance of governors has a significant impact on their actions (Besley and Case (1995), Knight (2000), Alt and Lowry (2000), Beland and Oloomi (2015), among many others). It has been shown that Democrats affect the labor markets of groups voting for them (target based policies): blacks (Beland, 2015) and immigrants (Beland and Unel, 2015).

Party affiliation may have different effects on unionization of workers in different earning groups and we also investigate this. In the union-wage literature, several authors have found that unions compress the structure of wages in the sense that it increases wages in the lower end of earning distribution and decreases wages in the upper end (see Card (1996), Frandsen (2014), and Rios-Avila and Hirsch (2014) among many others). We divide our sample into five earning groups based on predicted earning distribution, and investigate the impact of party affiliation on (de)unionization of workers and their earnings for each sub-group. We find no significant impact of the party affiliation on any earning groups.

We also investigate whether a governors' party affiliation has different effects on the unionization of skilled and unskilled workers and their corresponding labor-market outcomes. This issue is important because many economists have argued that skill-biased technical change (SBTC) is the driving factor behind the steady decline in union power in the U.S.[2] For example, Acemoglu et al. (2001) develop a model where SBTC undermines unionization by providing better outside options for skilled workers (see also Dinlersoz and Greenwood (2012)). If SBTC affects skilled and unskilled workers asymmetrically, the party affiliation might then have a positive impact on unionization of unskilled workers and their labor-market outcomes. However, our analysis reveals that this is not the case.

Almost half of the states have the right-to-work (RTW) law, which prohibits agreement between employers and unions that prevent them from excluding non-union workers. It essentially gives workers the right to benefit from unions without paying for it, and thus the law weakens union power. Consequently, the party affiliation may matter for the unionization of workers and their labor-market outcomes in non-RTW states. We therefore restrict our sample to non-RTW states, and find that governors' party affiliation has no significant impact on unionization and related labor-market outcomes in such states, either.

One can argue that governors are more likely to make a difference if they are matched with legislatures that are from the same party. The recent passage of RTW laws in states following the election of Republican legislatures and governors lends credence to this argument. Therefore, we investigate the impact of party affiliation on unionization when both governors and legislatures are from the same party. However, our RD analysis based on this restricted sample yields qualitatively the same results.

Finally, on the methodological side, following Lee and Lemieux's (2014) checklist, we conduct an extensive set of robustness tests to evaluate the validity of our RD designs. For example, for our RD designs to be valid, the states where Democrats barely won should be similar to the states where they barely lost elections. In addition, party candidates should have no control over the election results. We provide evidence that supports the validity of the RD approach in the present context (see Section 4.2). In sum, our results are robust to a number of different specifications, controls, and samples.

2 Related Studies

This paper is related to a strand of the political economy literature that explores whether partisan allegiance of policy makers matters for policy outcomes. Several studies in this literature have analyzed the impact of party affiliation of governors on taxes, minimum wages, total spending, distribution of spending, family assistance, and worker compensation in the U.S. (Besley and Case (1995) and (2003), Reed (2006), Beland and Oloomi (2015), and Leigh (2008), among many others). A growing number of studies in this literature have used RD designs to evaluate party effects in various contexts. In an influential paper, Lee et al. (2004), using an RD design, find that party affiliation has a large impact on a legislator's voting behavior.[3] Beland (2015) studies whether party allegiance of governors has any differential impact on the labor-market outcomes of blacks relative to whites, and finds that Democratic governors cause an increase in the annual hours worked by blacks relative to whites. Beland and Unel (2015) investigate the importance of the party affiliation of U.S. governors on immigrant workers' outcomes. They find that immigrants are more likely to be employed, work longer hours and more weeks, and have higher earnings under Democratic governors. Our paper is the first to address the impact of party affiliation on unions, using an RD design.

Our study is also related to a large empirical literature that examine effects of unions on economic outcomes. Card (1996) analyzes the effects of unions on the structure of wages, and finds that unions raise wages more for workers with lower skills. Using a semiparametric approach, DiNardo et al. (1996) find that *deunionization* along with supply and demand shocks were important factors behind the rising wage inequality in the U.S. from 1970 to 1980. Using establishment-level data sets in the U.S. during 1984–99, DiNardo and Lee (2004) use an RD design and close union elections to estimate the impact of unionization on wages along with employment, output, and business survival, and find small-to-zero effects on the outcomes. In a recent study, Frandsen (2014) estimates the effects of unionization on establishment and worker outcomes in an RD design based on close union elections, and finds that unionization significantly decreases establishment-level payroll and average worker earnings.[4] Relatedly, Sojourner et al. (2015) examine nursing home unionization, and their RD analysis suggests that unionization increases labor productivity and quality of care per nursing hour.

3 Empirical Framework and Main Results

3.1 Econometric Specification

We employ a regression discontinuity (RD) design to determine the effect of party affiliation of U.S. governors on unionization of workers. Since several factors such as labor-market conditions, voter characteristics, party incumbency, etc. can also affect election results, the results based on simple OLS will be biased. Following Lee (2008), we address the endogeneity problem by exploiting random variations associated with *close* elections.[5]

We begin our analysis by estimating the causal impact of party affiliation on union membership status. Let U denote a dummy variable such that $U_{ist} = 1$ if individual i in state s at time t is a union member, and is 0 otherwise. We estimate the following specification:

$$U_{ist} = \beta_D D_{st} + F_D(MV_{st}) + \beta_X X_{ist} + \beta_s + \beta_t + \varepsilon_{ist}, \tag{1}$$

where D is the treatment variable that equals one if a Democratic governor is in power, zero otherwise; $F(MV)$ is a polynomial function of the margin of victory MV; X denotes a vector of control variables; β_s and β_t denote state and time fixed effects, respectively; and ε the error term.[6] The coefficient of interest is β_D.

The set of control variables, X, includes each individual's gender, race, age, marital status, and education.[7] We define MV as the percentage of votes cast for the winner minus the percentage of votes cast for the second-place candidate, and MV_{st} denotes the margin of victory in the most recent gubernatorial election in state s.[8] We exclude all elections where a third party candidate won, and set the election where the Democratic candidate won to be positive and negative otherwise. The cutoff point for the MV is 0 percent, and thus a positive (negative) MV indicates that a Democratic (Republican) governor won.

Following Gelman and Imbens (2014), we assume that $F_j(MV)$ is a second-order polynomial function and use parametric regression discontinuity approach to estimate equation (1). In addition, we limit our estimation sample to within 50 percent of the margin of victory. As a sensitivity check, we consider different order polynomials and local-linear regression discontinuity design. We also present results based on 5, 15, and 25 percent of the margin of victory. The results obtained from these alternative specifications are similar to our benchmark results (see Section 4). Finally, to account for possible serial correlation, standard errors are clustered at the state level.

Estimates based on specification (1) essentially measure the *net* impact of party affiliation on union membership. As a complementary analysis, we are also interested in the impact of party affiliation on individuals' entry to and exit from unions, i.e. unionization and de-unionization at the individual level. We take advantage of the CPS data which allows us to match individuals in two adjacent years, and thus we can record entries to and exits from unions.[9] We then define

$$U^+_{ist} = \begin{cases} 0 & \text{if } U_{ist} = 0,\ U_{ist+1} = 0 \\ 1 & \text{if } U_{ist} = 0,\ U_{ist+1} = 1 \end{cases}, \qquad U^-_{ist} = \begin{cases} 0 & \text{if } U_{ist} = 1,\ U_{ist+1} = 1 \\ 1 & \text{if } U_{ist} = 1,\ U_{ist+1} = 0 \end{cases}. \tag{2}$$

That is, U^+ is a dummy variable that identifies individuals who are not a union member in one year but become a union member in the next year. Similarly, U^- is a dummy that identifies individuals who are a union member in one year but are not a member in the next year. In sum, U^+ is dummy for entry to the union, and U^- is a dummy for exit from the union in each year. We then estimate the following specification:

$$U^j_{ist} = \beta_D D_{st} + F_D(MV_{st}) + \beta_X X_{ist} + \beta_s + \beta_t + \varepsilon_{ist}, \tag{3}$$

where $j = +, -$.

Finally, we investigate whether party affiliation has any differential effects on the labor-market outcomes of unionized workers relative to those who are not. We use hours worked per week, weekly income, and hourly income as labor-market outcomes. Let Y be a labor-market outcome, we then estimate the following specification:

$$Y_{ist} = \beta_D D_{st} + \beta_U U_{ist} + \beta_{DU} D_{st} \times U_{ist} + F_D(MV_{st}) + \beta_X X_{ist} + \beta_s + \beta_t + \varepsilon_{ist}, \quad (4)$$

where U_{ist} equals one if individual i in state s at time t is a union member, zero otherwise. The coefficient β_D measures the impact of Democratic governors on labor-market outcomes of non-unionized workers, whereas the coefficient β_{DU} measures the impact of Democratic governors on labor-market outcomes of union members (i.e., $U_{ist} = 1$) relative to that of non-unionized workers (i.e., $U_{ist} = 0$).[10]

In our main analysis, we estimate the above equations using all data. However, we later present results using different samples based on income and skill distributions. Before presenting the results, we now turn to discuss the data that we use in our analysis.

3.2 Data

The source of our labor data is the monthly Current Population Survey (CPS) Merged Outgoing Rotation Group (MORG) files from Unicon Corporation (2015) covering 1983 to 2013. Our time period is dictated by the availability of the data on unions. Our sample consists of all wage and salary workers, ages between 16 and 64 years old. We exclude self-employed workers as well as those covered by a collective bargaining agreement who are not union members. We also exclude all workers with allocated union status, weekly hours, and weekly earnings.[11] Earnings are converted into real values (in 2009-chained prices) using the personal consumption expenditure (PCE) index from the Bureau of Economic Analysis (2014). Top-coded earnings are multiplied by 1.5 and workers with (real) income below \$3.65 per hour and above \$150 per hour are dropped (following Autor et al. (2008), Hirsch and Schumacher (1998)). In all our calculations and estimates, we use CPS weights.

The data are sorted into three races (black, white, and other), marital status, and five education categories (less than high school, high school graduate, some college, college graduate, and advanced degree). We also record each individual's union-membership status (denoted by U_{ist}) as well as the industry in which she works, and worker class.[12] The fraction of all wage and salary workers who are union members has been steadily decreasing in the U.S. from about 20 percent in 1983 to 11 percent in 2013. Despite this dramatic decline in union membership, unionization has still remained strong in certain occupations. For instance, the unionization rates among teachers and construction & extraction workers in 2013 were about 38 and 19 percent, respectively.[13]

To estimate equation (3), in each year we need to identify new union members and those who exit from unions. As briefly mentioned in the previous section, the CPS ORG data allow us to match individuals in two adjacent years. The CPS does not have individual identifiers, but it contains a household identification number and record line numbers. Uniquely matched pairs were identified with identical household ID, record lines, survey month, sex, and race (Card (1996) and Schumacher (1999)). We only consider individuals with a schooling difference in two successive years less than one year and an age difference less than two.[14] Once we match individuals in two successive years, we can easily identify individuals entering to or exiting from unions in each year. Using this information, we construct the dummy variables U_{ist}^{+} and U_{ist}^{-} defined in equation (2).

The data on gubernatorial elections are from the *Atlas of U.S. Presidential Elections* (Leip (2015)) and the ICPSR 7757 (1995) files. In each state and year, we record the governor's party affiliation and the year she was most recently elected. From 1983–2013, there are 1527 state×year observations, of which Democrats governed 772 times, which is about 51% of the sample. As discussed in the previous section, we define the margin of victory (MV) (in each election in each state) as the percentage of votes cast for the winner minus the percentage of votes cast for the second-place candidate; and we keep only election where either a Democrat or Republican won. For Louisiana, a Democratic governor was elected in 2003 with a 3.9 percent of MV and held the office between 2004 and 2007; as a result, MV of 3.9 percent is used in regressions for the years 2004–2007.

3.3 Main Results

We begin our analysis by providing some graphical evidence on (insignificant) effects of governors' party affiliation on unionization and related labor-market outcomes for union workers. Figures

1.a–1.f show the implications of the discontinuity at the cutoff point where a party barely wins the election. Figures 1.a–1.c suggest that party affiliation has no significant impact on workers' (de)unionization. Figures 1.d–1.f show the impact of governors' party affiliation on labor-market outcomes of unionized workers. These graphs do not show any discernible changes around the cutoff point, implying that gubernatorial party affiliation has not had any impact on earnings or weekly hours worked of unionized workers.

We now turn to estimate the effect of party allegiance on these key variables using the RD designs outlined in the previous section. Table 1.A reports the impact of party affiliation on union membership, unionization, and deunionization of workers using RD designs.[15] According to the RD estimates, the Democratic Party has no significant impact on union membership, unionization, and deunionization of workers.[16]

Table 1.B presents the impact of party affiliation on labor-market outcomes of unionized workers. The estimated coefficients for U[nion] in Table 1.B imply that union members on average work longer and earn higher. Findings that union members earn a higher income may reflect the fact that unions bargain for wages that are above the market level. The coefficient of interest is β_{DU}. Note that the estimated coefficient on D$\times$U is insignificant in all specifications, suggesting that governors' party affiliation has not had any significant effect on the labor-market outcomes of unionized workers relative to those who did not unionize.

4 Sensitivity Analysis

This section investigates how robust our results are to a number of different specifications. We consider two types of sensitivity analyses: robustness of our results to different samples and conditioning variables, and robustness of our RD designs to different specifications.

4.1 Different Samples and Additional Conditional Variables

We begin our analysis by investigating how party affiliation affects unionization and labor market outcomes of workers in different income groups. Such an extension is important because several studies have provided evidence that unions compress the structure of wages. For instance, Frandsen (2014) compares workers' earnings before and after close union elections, and finds evidence that unionization has positive effects in the lower end and negative effects in the higher end of the earning distribution (see also Card (1996), DiNardo et al. (1996)).

Following Card (1996), income groups are determined using the predicted earning distribution, which is obtained by regressing in each year log weekly earnings on gender, marital status, the dummy variables for four education categories, three race dummies, a quartic in age, industry dummies, occupation dummies, and state and time fixed effects.[17] We next sort individuals based on the predicted earning distribution into the following five income groups (measured in percent): 0–20, 20–40, 40–60, 60–80, and 80–100. We then estimate equations (1), (3), and (4) for each of these income groups, and Table 2 reports the results.

Several interesting points in this table are worth noting. First, party affiliation has no impact on (de)unionization of workers. Second, except for the last column, the estimated coefficients on union in earning regressions are significant and positive for all income groups (see Panels E and F). They are negative and statistically significant for the highest income group [80-100], and these findings are in line with Dinardo et al. (1996) and Frandsen (2014). Finally, the estimated coefficient for $D \times U$ is insignificant in all regressions, suggesting that governors' party affiliation has had no significant impact on labor-market outcomes of unionized workers (relative to non-unionized ones).

Several authors have argued that skilled-biased technical change (SBTC) has been the driving factor behind rapid deunionization in the U.S. over the past three decades (Acemoglu et al. (2001); Dinlersoz and Greenwood (2012)). According to these studies, SBTC undermines the coalition among skilled and unskilled workers by providing better outside options to skilled workers. With this structural transformation stemming from directed technical change, how do our results change if we consider these two groups separately? Table 3.A shows that Democratic governors have a negative and barely significant impact on only the unionization of skilled workers (at 10%), and their impact on other outcomes of either group is insignificant. Table 3.B presents the impact of the party affiliation on the labor-market outcomes of skilled and unskilled workers. Note that none of the coefficients for the interaction term D$\times$U are significant.

Almost half of U.S. states have a right-to-work (RTW) law which essentially gives employees the right to benefit from unions without paying for it.[18] Since the RTW law allows employees to benefit from unions without having to join, unions are weaker in RTW states. In the present context, this further suggests that party affiliation might have a stronger impact on union membership and labor-market outcomes in non-RTW states.[19] Tables 4.A and 4.B report the regression results (based on equations (1), (3), and (4)) for non-RTW states. According to Table 4.A, the estimated coefficients for D[emocrat] are small and statistically insignificant, i.e. Democratic governors have no impact on union membership and (de)unionization of workers in non-RTW states either. Table 4.B presents the results for the impact of Democratic governors on labor-market outcomes of unionized workers in non-RTW states. They have a very small, positive, and (barely) significant effect on weekly hours worked, but no effects on other outcome variables.

The recent passage of the RTW law in states where governors and legislatures are of the same party suggest that the impact of Democratic governors could be more significant if they are matched with a Democratic legislatures. To see whether this is the case, we restrict our sample to the state-time observations where governors and legislatures are from the same party. The results as shown in Tables 5.A and 5.B indicate that even when governors and legislatures are from the same party, the impact of governors' party affiliation on unionization and labor-market outcomes are insignificant.

We also estimate specifications (1), (3), and (4) considering only individuals working in public sector. Tables 6.A and 6.B report results, and their comparisons with those in Tables 1.A and 1.B indicate that this restriction has no significant impact on the results.[20] Furthermore, as a complementary analysis, we restrict our sample by considering occupations with high unionization rates (e.g., teachers, construction workers, government employees, police, and firefighters). The results (available upon request) are qualitatively similar to our benchmark results.

Finally, we investigate the impact of party affiliation excluding states that consistently elect a governor from a single party. More specifically, we include only state where both Democrats and Republicans were in office at least 30% of the time over the period 1983–2013. Tables A.1.A and A.1.B in the appendix report the results, which are similar to those presented in Tables 1.A and 1.B.[21]

4.2 Evaluation of the RD Design

The validity of our regression results presented in the previous sections depends on whether our RD approach is a valid way to evaluate the impact of party affiliation on unions and their members' labor-market conditions. This section addresses this question, and to this end we follow a checklist proposed by Lee and Lemieux (2010 & 2014). First, a crucial assumption in our RD designs is that states where Democrats marginally won elections must be similar to states where they marginally lost elections. To test the validity of this assumption, we regress certain characteristics of states (such as fraction of minority in population, fraction of unskilled workers, fraction of skilled workers, fraction of female) on the indicator variable D[emocrat] in a pooled RD designs to determine whether the estimated coefficient for D[emocrat] is statistically significant. However, our regressions yield statistically insignificant coefficients for D[emocrat], suggesting that the above identification assumption is not violated (see Table A.2 in the appendix).

Another important assumption about the validity of our RD approach is that candidates should not have any control over the election results. One quick way to determine the validity of this assumption in our framework is to look at the histogram of the Margin of Victory (MV). If a candidate had control over the election results, we should observe unusual jumps around the cutoff point (i.e., zero) and/or distribution of the MV skewed towards one party. According to Figure 2.a, none of the aforementioned anomalies is present. A more precise way to assess the validity of this assumption is to use the McCrary (2008) test. Figure 2.b plots the density function of the MV based on the procedure in McCrary (2008), and there are no unusual jumps around the cutoff.[22]

Third, we need to show that our results are robust to different orders of the polynomials, local-linear regressions, and different bandwidths. Tables A.3.A and A.3.B in the appendix present the results based on the first-order polynomial, and the results presented in these tables are qualitatively the same as those in Tables 1.A and 1.B.[23] We also investigated the robustness of our results using local-linear RD and optimal bandwith procedures of Imbens and Kalyanaraman (2012).[24] Table A.4 in the appendix presents the results for the local-linear specifications using grouped data by state and year, and note that estimated coefficients are qualitatively similar to those in our benchmark results. Another important sensitivity check is to see how robust our results are to different bandwidths. In our main specification, we limited our estimation sample to within 50 percent of the margin of victory. Our analysis using 5, 15, and 25 percent of the margin of victory indicates that results are not sensitive to different bandwidths (see Tables A.5.A and A.5.B in the appendix).[25]

Finally, we need to show that our results are not driven by the persistence of the outcome variables. For example, the RD designs yield biased estimates if Democratic governors are more likely to be elected in state-years when union members have better labor-market outcomes. To address this problem, we use a placebo test where we replace the outcome variables in each specification with the corresponding variables measured one term ago, and check for the balance between the control and treatment group. The results are presented in Tables A.6.A and A.6.B in the appendix, and suggest no discontinuity in the last term outcomes.

5 Conclusion

For decades, unions have been strong supporters of the Democratic Party. They rallied their members to vote for Democrats and funneled money to Democratic candidates so that they could win elections. Intuition suggests that the steady and strong support from unions stems from the Democratic Party's positive effects on unions. But how significant have been the effects of Democrats on unions?

In this paper, we investigate the causal impact of U.S. governors' party affiliation on organized-labor markets (i.e., unions). To deal with the endogeneity of party affiliation of governors, we implement a regression discontinuity (RD) design using data on gubernatorial elections in U.S. states between 1983 and 2013. Exploiting the variation in close elections, we find no significant impact of party affiliation on union status of workers. Furthermore, we find no impact of gubernatorial party affiliation on unionized workers' labor-market outcomes. Our sensitivity analysis confirms our basic conclusion: contrary to the common perception, Democratic governors have not had any significant positive impact on unions.

References

Acemoglu, Daron, Philippe Aghion, and Giovanni L. Violante, "Deunionization, Technical Change, and Inequality," *Carnegie-Rochester Conference Series on Public Policy*, 2001, *55*, 229–64.

Alt, James E. and Robert C. Lowry, "A dynamic model of state budget outcomes under divided partisan government," *The Journal of Politics*, 2000, *62*, 1035–1069.

Autor, David H., Lawrence F. Katz, and Melissa S. Kearney, "Trends in U.S. Wage Inequality: Revising the Revisionists," *Review of Economics and Statistics*, 2008, *90*, 300–23.

Beland, Louis-Philippe, "Political Parties and Labor Market Outcomes: Evidence from U.S. States," *American Economic Journal: Applied*, 2015, *7* (4), 198–220.

—— and Bulent Unel., "The Impact of Party Affiliation of U.S. Governors on Immigrants' Labor-market Outcomes," *LSU Working Paper*, 2015.

—— and Sara Oloomi, "Party Affiliation and Public Spending," *LSU Working paper*, 2015.

Besley, Timothy and Anne Case, "Does Electoral Accountability Affect Economic Policy Choices? Evidence from Gubernatorial Term Limits," *The Quarterly Journal of Economics*, 1995, *110*, 769–798.

—— and ——, "Political Institutions and Policy Choices: Evidence from the United States," *Journal of Economic Literature*, 2003, *41*, 7–73.

Bollinger, Christopher R. and Barry T Hirsch, "Match Bias From Earnings Imputation in the Current Population Survey: The Case of Imperfect Matching," *Journal of Labor Economics*, 2006, *24*, 483–519.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik, "Robust Nonparametric Confidence Intervals for Regression-discontinuity Designs," *Econometrica*, 2014, *82* (6), 2295–2326.

Card, David, "The Effect of Unions on the Structure of Wages: A Longitudinal Analysis," *Econometrica*, 1996, *64*, 957–79.

——, "The Effect of Unions on Wage Inequality in the U.S. Labor Market," *Industrial and Labor Relations Review*, 2001, *54*, 296–315.

——, Thomas Lemieux, and W.Craig Riddell, "Unions and Wage Inequality," *Journal of Labor Research*, 2004, *25*, 519–59.

Dell, Melissa, "Trafficking Networks and the Mexican Drug War.," *American Economic Review*, 2015, *fortcoming.*

DiNardo, John and David S. Lee, "Economic Impacts of New Unionization on Private Sector Employers: 19842001," *The Quarterly Journal of Economics*, 2004, *119*, 1383–1441.

——, Nicole M. Fortin, and Thomas Lemieux, "Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach," *Econometrica*, 1996, *64*, 1001–44.

Dinlersoz, Emin M. and Jeremy Greenwood, "The Rise and Fall of Unions in the U.S," NBER Working Papers, National Bureau of Economic Research, Inc 2012.

Eren, Ozkan, Masayuki Onda, and Bulent Unel., "Effects of FDI on Entrepreneurial Activity: Evidence from Right-to-Work and Non-Right-to-Work States," *LSU Working Paper*, 2016.

Ferreira, Fernando and Joseph Gyourko, "Do Political Parties Matter? Evidence from U.S. Cities," *The Quarterly Journal of Economics*, 2009, *124* (1), 399–422.

Frandsen, Brigham R., "The Surprising Impacts of Unionization: Evidence from Matched Employer-employee Data," 2014.

Gelman, Andrew and Guido Imbens, "Why High-order Polynomials Should not be Used in Regression Discontinuity Designs," Technical Report, NBER Working paper 2014.

Gosling, Amanda and Thomas Lemieux, "Labour Market Reforms and Changes in Wage Inequality in the United Kingdom and the United States," Working Paper 8413, National Bureau of Economic Research 2001.

Hirsch, Barry T. and Edward J. Schumacher, "Unions, Wages, and Skills," *The Journal of Human Resources*, 1998, *33*, 201–219.

Imbens, Guido and Karthik Kalyanaraman, "Optimal Bandwidth Choice for the Regression Discontinuity Estimator," *The Review of Economic Studies*, 2012, *79*, 933–59.

Jensen, Jennifer M and Thad Beyle, "Of Footnotes, Missing Data, and Lessons for 50-State Data Dollection: The Gubernatorial Campaign Finance Data Project, 1977–2001," *State Politics & Policy Quarterly*, 2003, *3* (2), 203–214.

Knight, Brian, "Supermajority Voting Requirements for Tax Increases: Evidence from the States," *Journal of Public Economics*, 2000, *76*, 41–67.

Lee, David S., "Randomized Experiments from Non-random Selection in U.S. House Elections," *Journal of Econometrics*, 2008, *142* (2), 675–697.

—— and Thomas Lemieux, "Regression Discontinuity Designs in Economics," *Journal of Economic Literature*, 2010, *48* (2), 281–355.

—— and ——, "Regression Discontinuity Designs in Social Sciences," in Henning Best and Christof Wolf, eds., *The SAGE Handbook of Regression Analysis and Causal Inference*, SAGE Publications, 2014, pp. 301–27.

——, Enrico Moretti, and Matthew J. Butler, "Do Voters Affect or Elect Policies? Evidence from the U. S. House," *The Quarterly Journal of Economics*, 2004, *119*, 807–859.

Leigh, Andrew, "Estimating The Impact of Gubernatorial Partisanship on Policy Settings and Economic Outcomes: A Regression Discontinuity Approach," *European Journal of Political Economy*, 2008, *24* (1), 256–268.

Leip, David, *Dave Leip's Atlas of U.S. Presidential Elections*, http://uselectionatlas.org, 2015.

McCrary, Justin, "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test," *Journal of Econometrics*, 2008, *142*, 698–714.

Pettersson-Lidbom, Per, "Do Parties Matter for Economic Outcomes? A Regression-Discontinuity Approach," *Journal of the European Economic Association*, 2008, *6*, 1037–56.

Reed, W. Robert, "Democrats, Republicans, and Taxes: Evidence that Political Parties Matter," *Journal of Public Economics*, 2006, *90* (4-5), 725–750.

Rios-Avila, Fernando and Barry T. Hirsch, "Unions, Wage Gaps, and Wage Dispersion: New Evidence from the Americas," *Industrial Relations: A Journal of Economy and Society*, 2014, *53*, 1–27.

Schumacher, Edward J., "What Explains Wage Differences Between Union Members and Covered Nonmembers?," *Southern Journal of Economics*, 1999, *65*, 493–512.

Sojourner, A., Brigham Frandsen and Robert J. Town and David C. Grabowski and Min M. Chen, "Impacts of Unionization on Quality and Productivity: Regression Discontinuity Evidence from Nursing Homes," *Industrial and Labor Relations Review*, 2015, *68*, 771-806.

Unicon Research Corporation, "Outgoing Rotations Monthly Earner Study Data Files (1979-2014)," *Santa Monica, CA*, 2015.

Western, Bruce and Jake Rosenfeld, "Unions, Norms, and the Rise in US Wage Inequality," *American Sociological Review*, 2011, *76*, 513–37.

Table 1.A. Impact of Party Affiliation on Unionization

Variable	Union Membership	Entry to Union	Exit from Union
D[emocrat]	0.0025	0.0010	−0.0059
	(0.0031)	(0.0013)	(0.0090)
Obs.	1,803,391	1,050,769	206,657

Table 1.B. Impact of Party Affiliation on Labor-Market Outcomes

Variable	Weekly Earning	Hourly Earning	Hours per Week
D[emocrat]	−0.0023	−0.0043	0.0017
	(0.0063)	(0.0059)	(0.0027)
U[nion]	0.2000***	0.1980***	0.0436***
	(0.0108)	(0.0113)	(0.0051)
D×U	0.0024	0.0010	0.0047
	(0.0157)	(0.0160)	(0.0036)
Obs.	1,501,291	1,501,291	1,501,291

Notes: The data draw on the CPS-ORG samples from Unicon Corporation for 1983–2013. All regressions include state fixed effects, time effects, and all other control variables specified in equations (1)–(3). Numbers in parentheses are standard errors based on clustering data at state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 2. Impact of Party Affiliation on Unionization, Different Income Groups

Variable	[0-20] I	[20-40] II	[40-60] III	[60-80] IV	[80-100] V
Panel A. Union Membership					
D[emocrat]	0.0005	0.0036	0.0065	0.0039	0.0027
	(0.0024)	(0.0046)	(0.0043)	(0.0068)	(0.0031)
Obs.	360,152	335,717	285,992	420,802	400,728
Panel B. Entry to Unions (Unionization)					
D[emocrat]	0.0009	0.0015	0.0040*	0.0004	−0.0015
	(0.0025)	(0.0018)	(0.0021)	(0.0027)	(0.0016)
Obs.	226,173	199,905	167,521	208,727	248,432
Panel C. Exit from Unions (Deunionization)					
D[emocrat]	−0.0016	−0.0099	−0.0115	−0.0050	0.0102
	(0.0149)	(0.0123)	(0.0128)	(0.0073)	(0.0142)
Obs.	20,738	28,982	27,303	86,969	42,666
Panel D. Hours per Week					
D[emocrat]	−0.0004	0.0044	−0.0026	0.0004	0.0035
	(0.0069)	(0.0046)	(0.0035)	(0.0031)	(0.0026)
U[nion]	0.1221***	0.0927***	0.0375***	0.0236***	−0.0109**
	(0.0087)	(0.0108)	(0.0057)	(0.0023)	(0.0050)
D×U	0.0091	0.0029	0.0089	0.0009	0.0004
	(0.0093)	(0.0079)	(0.0066)	(0.0031)	(0.0056)
Panel E. Weekly Earning					
D[emocrat]	−0.0056	−0.0068	−0.0031**	−0.0067	0.0033
	(0.0093)	(0.0089)	(0.0061)	(0.0059)	(0.0063)
U[nion]	0.2346***	0.2248***	0.2161***	0.2853***	−0.0482***
	(0.0104)	(0.0128)	(0.0125)	(0.0138)	(0.0098)
D×U	0.0007	−0.0071	0.0033	−0.0085	−0.0073
	(0.0097)	(0.0101)	(0.0197)	(0.0102)	(0.0141)
Panel F. Hourly Earning					
D[emocrat]	−0.0057	−0.0079	0.0013	−0.0082	−0.0017
	(0.0077)	(0.0070)	(0.0075)	(0.0062)	(0.0059)
U[nion]	0.2124***	0.1942***	0.2112***	0.2740***	−0.0267**
	(0.0091)	(0.0166)	(0.0112)	(0.0132)	(0.0106)
D×U	−0.0036	−0.0112	−0.0036	−0.0075	−0.0049
	(0.0129)	(0.0132)	(0.0158)	(0.0090)	(0.0118)
Obs	502,104	497,541	476,187	501,634	512,892

Notes: The data draw on the CPS-ORG samples from Unicon Corporation for 1983–2013. All regressions include state fixed effects, time effects, and all other control variables specified in equations (1)–(3). Numbers in parentheses are standard errors based on clustering data at state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.A. Impact of Party Affiliation on Unionization, Different Skill Groups

Variable	Union Membership	Entry to Union	Exit from Union
Panel A. Skilled Workers			
D[emocrat]	0.0080	-0.0077^{*}	0.0118
	(0.0050)	(0.0040)	(0.0225)
Obs.	476,787	299,918	62,898
Panel B. Unskilled Workers			
D[emocrat]	0.0011	-0.0069	-0.0040
	(0.0033)	(0.0044)	(0.0155)
Obs.	1,326, 602	750,851	143,759

Table 3.B. Impact of Party Affiliation on Labor Markets, Different Skill Groups

Variable	Weekly Earning	Hourly Earning	Hours per Week
Panel A. Skilled Workers			
D[emocrat]	0.0033	-0.0023	0.0018
	(0.0074)	(0.0070)	(0.0029)
U[nion]	0.0037	0.0052	0.0258^{***}
	(0.0092)	(0.0107)	(0.0079)
D×U	-0.0073	-0.0065	0.0018
	(0.0131)	(0.0128)	(0.0048)
Obs.	369,768	369,768	369,768
Panel B. Unskilled Workers			
D[emocrat]	-0.0054	-0.0055	0.0011
	(0.0072)	(0.0070)	(0.0031)
U[nion]	0.2757^{***}	0.2694^{***}	0.0491^{***}
	(0.0107)	(0.0114)	(0.0043)
D×U	0.0009	-0.0001	0.0052
	(0.0128)	(0.0134)	(0.0039)
Obs.	1,114,017	1,114,017	1,114,017

Notes: The data draw on the CPS-ORG samples from Unicon Corporation for 1983–2013. All regressions include state fixed effects, time effects, and all other control variables specified in equations (1)–(3). Numbers in parentheses are standard errors based on clustering data at state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 4.A. Impact of Party Affiliation on Unionization, Non-RTW States

Variable	Union Membership	Entry to Union	Exit from Union
D[emocrat]	0.0053	0.0010	−0.0059
	(0.0036)	(0.0013)	(0.0090)
Obs.	1,110,867	1,050,769	206,657

Table 4.B. Impact of Party Affiliation on Labor Markets, Non-RTW States

Variable	Weekly Earning	Hourly Earning	Hours per Week
D[emocrat]	−0.0007	−0.0006	−0.0032
	(0.0117)	(0.0107)	(0.0037)
U[nion]	0.1914***	0.1952***	0.0388***
	(0.0149)	(0.0151)	(0.0056)
D×U	−0.0051	−0.0089	0.0071*
	(0.0201)	(0.0196)	(0.0040)
Obs.	919,964	919,964	919,964

Notes: The data draw on the CPS-ORG samples from Unicon Corporation for 1983–2013. All regressions include state fixed effects, time effects, and all other control variables specified in equations (1)–(3). Numbers in parentheses are standard errors based on clustering data at state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 5.A. Unionization: Legislatures and Governors from the Same Party

Variable	Union Membership	Entry to Union	Exit from Union
D[emocrat]	0.0033	−0.0001	−0.0127
	(0.0039)	(0.0011)	(0.0097)
Obs	1,324,725	938,408	118,649

Table 5.B. Outcomes: Legislatures and Governors from the Same Party

Variable	Weekly Earning	Hourly Earning	Hours per Week
D[emocrat]	0.0073	0.0047	0.0040
	(0.0051)	(0.0051)	(0.0029)
U[nion]	0.2085***	0.2034***	0.0465***
	(0.0118)	(0.0133)	(0.0054)
D×U	−0.0052	−0.0047	−0.0002
	(0.0170)	(0.0180)	(0.0033)
Obs.	1,103,227	1,103,227	1,103,227

Notes: The data draw on the CPS-ORG samples from Unicon Corporation for 1983–2013. All regressions include state fixed effects, time effects, and all other control variables specified in equations (1)–(3). Numbers in parentheses are standard errors based on clustering data at state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 6.A. Unionization: Public Sector

Variable	Union Membership	Entry to Union	Exit from Union
D[emocrat]	0.0120	0.0025	−0.0097
	(0.0076)	(0.0048)	(0.0087)
Obs.	304,698	135,138	95,084

Table 6.B. Outcomes: Public Sector

Variable	Weekly Earning	Hourly Earning	Hours per Week
D[emocrat]	−0.0009	−0.0002	0.0022
	(0.0100)	(0.0109)	(0.0036)
U[nion]	0.1931***	0.1372***	0.1231***
	(0.0088)	(0.0082)	(0.0065)
D×U	−0.0216	0.0203	−0.0032
	(0.0141)	(0.0126)	(0.0065)
Obs.	476,213	476,213	476,213

Notes: The data draw on the CPS ORG samples from Unicon Corporation for 1983–2013. The sample contains only public sector workers. All regressions include state fixed effects, time effects, as well as all other control variables specified in equation (1). Numbers in parentheses are standard errors based on clustering data at state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

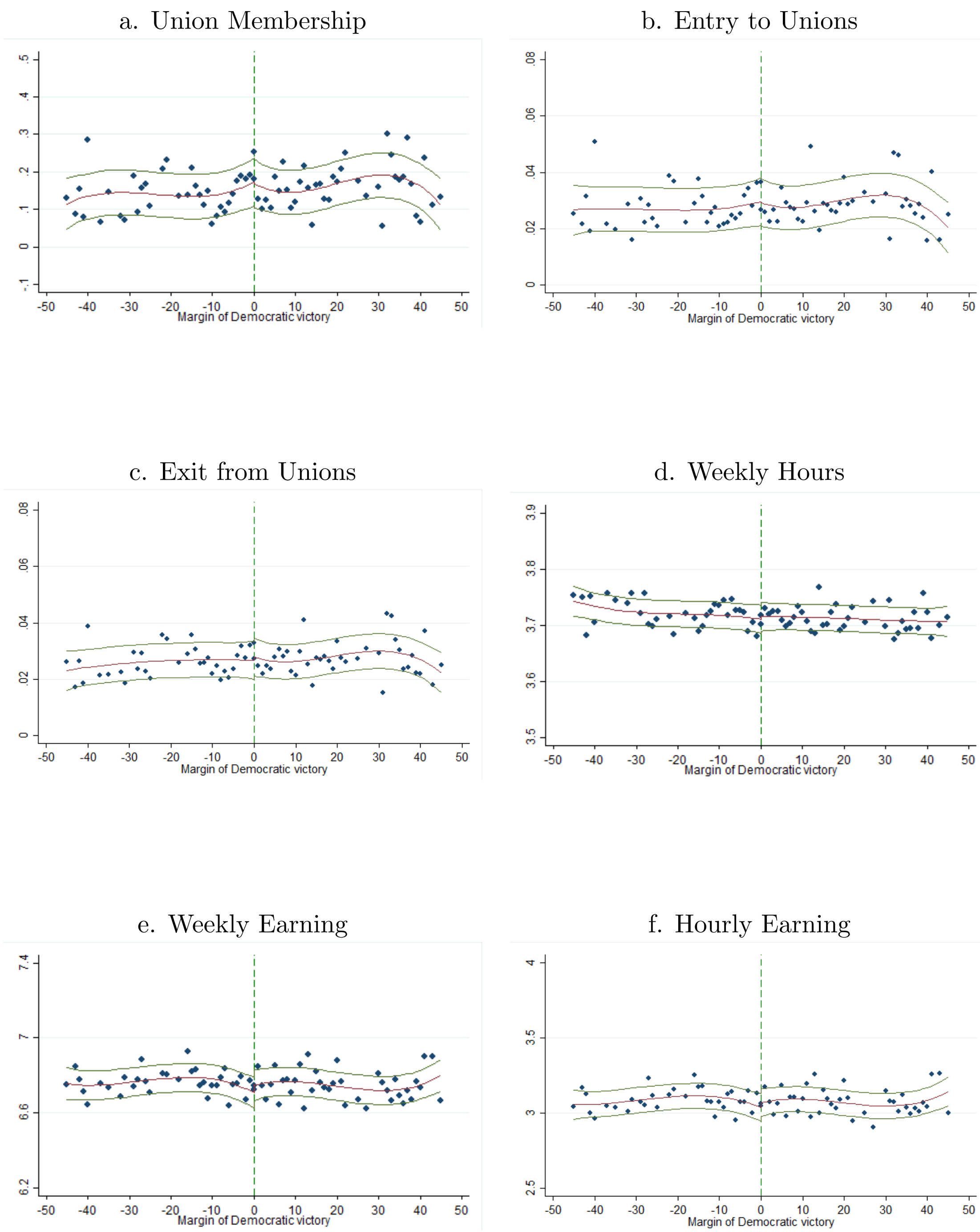
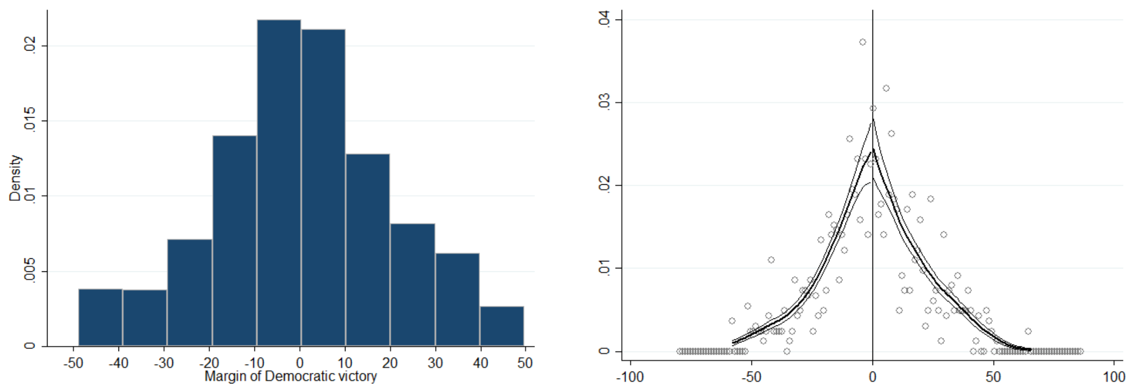


Figure 1: The Impact of Democratic Governors on Labor Markets



a. Density Based on Histograms

b. Density Based on McCrary

Figure 2: Distribution of the Margin of Victory

APPENDIX

Table A.1.A. Unionization: Dropping States with Longer Incumbents

Variable	Union Membership	Entry to Union	Exit from Union
D[emocrat]	0.0046	0.0023	−0.0038
	(0.0042)	(0.0017)	(0.0105)

Table A.1.B. Labor-Market Outcomes: Dropping States with Longer Incumbents

Variable	Weekly Earning	Hourly Earning	Hours per Week
D[emocrat]	−0.0097	−0.0122***	0.0026
	(0.0064)	(0.0062)	(0.0034)
U[nion]	0.1971***	0.1969***	0.0415***
	(0.0128)	(0.0131)	(0.0058)
D×U	0.0150	0.0108	0.0078**
	(0.0172)	(0.0177)	(0.0035)

Notes: Sample is restricted to state which elect each party at least 30% of the time. Numbers in parentheses are standard errors based on clustering data at state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table A.2. Covariates Balance - Pooled Regressions

	Minority	HS or less	College graduate	Female
D[emocrat]	0.0167	0.0011	0.0025	0.0021
	(0.0116)	(0.0019)	(0.0027)	(0.0015)

Notes: Numbers in parentheses are standard errors based on clustering data at state level. Numbers in parentheses are standard errors based on clustering data at state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table A.3.A. Impact of Party Affiliation on Unionization, First-Order Polynomial

Variable	Union Membership	Entry to Union	Exit from Union
D[emocrat]	0.0046	0.0017	−0.0047
	(0.0042)	(0.0011)	(0.0082)

Table A.3.B. Impact of Party Affiliation on Outcomes, First-Order Polynomial

Variable	Weekly Earning	Hourly Earning	Hours per Week
D[emocrat]	−0.0007	−0.0014	0.0016
	(0.0043)	(0.0040)	(0.0022)
U[nion]	0.1999***	0.1979***	0.0435***
	(0.0109)	(0.0114)	(0.0051)
D×U	0.0026	0.0012	0.0048
	(0.0158)	(0.0161)	(0.0036)

Notes: Numbers in parentheses are standard errors based on clustering data at state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table A.4. Local Linear Analysis Based on Imbens and Kalyanaraman (2012)

	Union Member	Entry to Union	Exit from Union	Weekly Earn	Hourly Earn	Hrs per Week
D[emocrat]	0.0002	0.0035	−0.0171	0.0255	0.0376	−0.0073
	(0.0027)	(0.0029)	(0.0150)	(0.0372)	(0.0344)	(0.0087)

Notes: Numbers in parentheses are standard errors. ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table A.5.A. Unionization with Various Bandwidths

Variable	Union Membership	Entry to Union	Exit from Union
MV=5%			
D[emocrat]	0.0032	−0.0115	0.0050
	(0.0103)	(0.0071)	(0.0292)
MV=15%			
D[emocrat]	0.0082	0.0043	−0.0171
	(0.0056)	(0.0026)	(0.0150)
MV=25%			
D[emocrat]	0.0038	0.0032	−0.0161
	(0.0031)	(0.0023)	(0.0126)

Table A.5.B. Labor-Market Outcomes with Various Bandwidths

Variable	Weekly Earning	Hourly Earning	Hours per Week
MV=5%			
D[emocrat]	0.0106	−0.0052	0.0024
	(0.0217)	(0.0190)	(0.0119)
U[nion]	0.2089***	0.2108***	0.0372***
	(0.0195)	(0.0210)	(0.0053)
D×U	−0.0250	−0.0335	−0.0000
	(0.0251)	(0.0251)	(0.0077)
MV=15%			
D[emocrat]	0.0061	0.0012	0.0041
	(0.0093)	(0.0084)	(0.0040)
U[nion]	0.2130***	0.2116***	$0.0451^{t}s$
	(0.0121)	(0.0137)	(0.0052)
D×U	−0.0163	−0.0218	0.0072
	(0.0170)	(0.0180)	(0.0056)
MV=25%			
D[emocrat]	−0.0043	−0.0105	−0.0046
	(0.0093)	(0.0084)	(0.0040)
U[nion]	0.2036***	0.2024***	0.0428***
	(0.0113)	(0.0113)	(0.0059)
D×U	−0.0053	−0.0084	0.0070
	(0.0181)	(0.0171)	(0.0053)

Notes: Numbers in parentheses are standard errors based on clustering data at state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table A.6.A. Impact of Party Affiliation on Unionization, One Term Ago

Variable	Union Membership	Entry to Union	Exit from Union
D[emocrat]	−0.0014	0.0018	0.0034
	(0.0028)	(0.0014)	(0.0112)

Table A.6.B. Impact of Party Affiliation on Labor Markets, One Term Ago

Variable	Weekly Earning	Hourly Earning	Hours per Week
D[emocrat]	−0.0018	−0.0002	−0.0040
	(0.0074)	(0.0067)	(0.0033)
U[nion]	0.2010***	0.1978***	0.0447***
	(0.0094)	(0.0087)	(0.0058)
D×U	−0.0015	−0.0006	0.0012
	(0.0108)	(0.0111)	(0.0039)

Notes: Numbers in parentheses are standard errors based on clustering data at state level; ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Notes

[1]This is the statement on the Democratic Party's official web site.

[2]Union power has declined considerably over the last three decades: while 25 percent of workers were union members in 1985, this fraction dropped to less than 12 percent in 2013.

[3]Ferreira and Gyourko (2009) exploit the random variation associated with close U.S. municipal elections between 1950 and 2000. They find that the party affiliation of mayors has no significant impact on the size of local government, the composition of local public expenditure, or crime rate. Employing an RD design on panel data from Swedish local governments, Pettersson-Lidbom (2008) finds that left-wing governments spend and tax more than right-wing governments. Finally, Dell (2015) shows that drug-related violence increases substantially after *close* elections of Mexico's conservative PAN party's mayors.

[4]Other important contributions to this literature are Card (2001), Hirsch and Schumacher (1998), Gosling and Lemieux (2001), Rios-Avila and Hirsch (2014) among many others. See Card et al. (2004) for an early review of this literature.

[5]As discussed in the previous section, exploiting variations in *close* elections is used in several other election contexts (see, e.g., Lee et al. (2004), Ferreira and Guyourko (2009), Dell (2015), and Beland (2015) among many others).

[6]We also considered the above specification at state-year and state-term levels. In these cases, our dependent variable U_{st} denotes the fraction of workers who are union members in state s and year (term) t. Results based on these alternative specifications are qualitatively the same as those obtain from (1). We prefer to report results based on (1), because having more controls and a substantially higher number of observations will make point estimates more precise.

[7]Our regressions include dummies for sex, marital status, three race dummies, four education dummies, and a quartic in age.

[8]For Texas, for example, 2006 election results (the political party of the winner and the margin of victory) are used in regressions for 2007–2010 years.

[9]However, the data are not in a panel structure: an individual first interviewed in year t will be interviewed in year $t + 1$, but after that she will be dropped from the sample.

[10]Alternatively, we can run regressions separately for these two groups, but analysis based on this approach yields qualitatively the same results.

[11]Dropping imputed earning figures is not a straightforward exercise. In doing so, we closely follow Hirsch and Schumacher (2004), Bollinger and Hirsch (2006), and in particular, Western and Rosenfeld (2011). However, our results remain largely similar even if we do not drop workers with imputed figures.

[12]We classify workers as private or public employees. In estimating equations (1) and (2), we don't include industry dummies due to the fact that this variable is endogenously determined. However, as discussed in detail in Section 3.1, including industry dummies and industry-specific time effects into the models do not have any significant impact on the results.

[13]In the CPS sample, there are individuals who are members of some collective bargaining units that are not unions. In our analysis, we exclude these individuals. However, extending the analysis by including these collective members has no significant impact on the main results. See Tables A.5.A and A.5.B in the appendix.

[14]There are problems with assigning household IDs in 1985 and 1995; consequently matching rates between 1984 and 1985, 1985 and 1986, 1994 and 1995, and 1995 and 1996 were less than 30 percent. As a robustness check, we run regressions excluding these years; and results are qualitatively the same.

[15]Using data on union membership at the state-year and state-term levels yields qualitatively similar results to that reported in "Union Membership" column in Table 1.A. State-year level analysis uses 1,479 observations and yields $\hat{\beta}_D = 0.0016\,(0.0025)$, whereas state-term level analysis uses 423 observations and yields $\hat{\beta}_D = -0.0019\,(0.0044)$.

[16]The results based on OLS estimates are qualitatively similar to those in Tables 1.A and 1.B.

[17]More specifically, for each year we run the following regression

$$\ln \mathrm{w}_{ist} = X_{ist} + Ind_{ist} + Occp_{ist} + \beta_s + \beta_t + \varepsilon_{ist},$$

where X_{ist} is the set of variables such as gender, race, marital status, education level, etc.; and Ind_{ist} and $Occp_{ist}$ are categorical variables that indicate where the individual works and what occupation she holds. We use predicted values $\widehat{\mathrm{w}}_{ist}$ in constructing income distribution. Our results qualitatively remain the same if we use observed values w_{ist} in constructing income distribution.

[18]In 2014, the states that have this law are: Alabama, Arizona, Arkansas, Florida, Georgia, Idaho, Indiana, Iowa, Kansas, Louisiana, Michigan, Mississippi, Nebraska, Nevada, North Carolina, North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, Wyoming. Indiana and Michigan passed the law in 2012, and Wisconsin in 2015. Since our time period covers 1983-2013, we do not include them into the RTW states. Idaho passed this law in 1985, so we exclude it from our sample that covers only non-RTW states.

[19]Wages in RTW states are about 10% lower than those in non-RTW states, whereas the unemployment rate is lower in RTW states (Eren et al. 2016).

[20]Running the same regressions considering only private sector workers yielded qualitatively the same results.

[21]We extend our analysis by considering all individuals who are part of any collective bargaining units. Results (available upon request) remain mostly the same.

[22]We also verified that states where Democrats barely won and states where Democrats barely lost are not statistically different from each other in their pre-treatment covariates. To address the issues raised in Caughey and Sekhon (2011), using data from Jensen and Beyle (2003), we found that campaign spending is not different when Democrats barely win compared to when they barely lose. In addition, for close elections to be regarded as random, such elections won by Democratic governors should not be more likely to come with a Democratic House or Senate. We checked and confirmed that those variables are not statistically different when Democrats barely won.

[23]The results based on third-order polynomial (available upon request) are qualitatively the same

as those in our benchmark results.

[24] Using the procedure developed by Colanico et al. (2014) yields similar results.

[25] We repeated this exercise with 10, 20, and 35 percent of the margin of victory, but results remained unchanged.