

# Labor Organizations and Corporate Leverage: The Effect of Right-To-Work Laws

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## **Abstract**

I use the staggered rollout of Right-to-Work (RTW) policies, which prohibit agency shop agreements, to examine the relationship between labor unions' financial conditions and firms' capital structure. I use a novel data set that links large public corporations financial reports to unions balance sheet data. I find that in the years following RTW, unions that previously relied on agency fees experience a significant loss of revenue before they report a significant loss of membership. Unions exposed to RTW policies, through their reliance on agency fee income, respond to the loss of income by decreasing their spending on operations and increasing the income share of membership contributions, which increase the relative cost of union services on members. I show that large public firms have greater leverage when they are exposed to unions with strong financial balance sheets.

## **1 Introduction**

Do labor regulations affect firms' capital structure? While a large literature shows that labor regulations determine firms' financing decisions, these studies rely on the assumption that labor regulations affect workers or firms directly. While this assumption is made to address data limitations, it does not always reflect the intended target of some policies. Certain labor regulations target labor unions specifically. For example, the Taft-Hartley Act of 1947

permitted the inclusion of agency shop clauses in collective bargaining agreements. Agency shop clauses allow unions to collect mandatory fees from workers who are not members of a union (known as agency fee payers). Since a union’s ability to finance operations determines their workers’ bargaining power, labor unions’ financial conditions should mediate the effect of labor regulations on firms’ capital structure.

In this paper, I use the staggered rollout of Right-to-Work (RTW) policies, which prohibit the collection of agency fees, to examine the relationship between labor union’s financial conditions and firms’ capital structure. I use a novel data set that links large public corporations’ financial reports to unions’ balance sheet data. This research design uses a differential exposure design to measure effect of RTW on unions’ revenue and a panel regression to estimate the relationship between unions’ revenue and firms’ leverage. Together, these estimates allow a subsequent counterfactual analysis of the effect of RTW on firms’ capital structure.

First, I use a differential exposure design to estimate the direct effect of RTW on unions’ balance sheets. As the Taft-Hartley Act of 1947 only permitted the inclusion of agency shop clauses in collective bargaining agreements, there is intra-state heterogeneity in the pre-treatment take-up of agency shop clauses by unions and firms. The take-up of agency shop clauses is not systematically correlated with firms’ capital structure or unions’ membership or financial conditions. As RTW prohibits the mandatory collection of agency fees through agency shop clauses, unions’ exposure to RTW depends on whether they benefited from an agency shop clause prior to RTW. As such, I measure the differential effect of RTW on unions with agency shop agreements prior to RTW. The identifying assumption of this design is that, in the absence of RTW laws, trends in agency shop agreements would be the same across states.

Second, I use a panel regression to estimate the cross-sectional relationship between unions’ balance sheets and firms’ leverage. Public firms often distribute economic activity across different jurisdictions in response to regional variation in economic regulations, input prices, and output demand. This behavior makes it difficult to identify the causal effect of RTW on firms’ leverage. RTW affects local unions’ ability to organize workers at the establishment level, and a firm’s leverage reflects strategic decisions made at the firm level. As such, I use a panel regression to recover the semi-elasticity of a firm’s leverage to

unions' revenue. This panel regression and subsequent counterfactual analysis assumes that firms do not change their allocation of their operations in the short run. I then use estimates from the panel regression and event study estimates to construct the counterfactual effect of RTW on a unionized firms with business activity limited to one state. This counterfactual analysis applies to a counterfactual, average firm with operations limited to one state that unexpectedly enacts RTW. This analysis offers a useful benchmark of the effect of RTW on firms' capital structure.

The effect of RTW on unions' balance sheets is large and persistent. Unions with agency fee payers lose 15 percent of their total receipts over five years relative to unions without agency fee payers. This persistent decline in receipts accompanies a proportional fall in total disbursements. As liabilities and assets decrease, the unions' net worth remain constant. To make up for lost revenue, unions who lost agency fee payers relied on members and their affiliates including intermediate and national labor organizations for financial support. Interestingly, affiliate contributions to unions with unexposed balance sheets falls by 5% during the same period. This asymmetric response suggests that labor federations use intra-state transfers to help insure local unions against income shocks. While affiliates were able to partially insure against the income shock associated with RTW, unions with agency fee payers lose 16% of their membership over the five year sample period, while unions without agency fee payers maintain their pre-reform membership level.

For the average firm in the sample, the decrease in exposed union income following with RTW is associated with a 0.5 standard deviation decrease in firm leverage, assuming that the firms' activity is limited to a state affected by the reforms included in the sample. The counterfactual effect of RTW on firms' leverage is consistent with prior estimates found by [Matsa \(2010\)](#) and [Chava \*et al.\* \(2020\)](#). This relationship between unions' revenue and firms' leverage provides novel insights on the determinants of workers' bargaining power. Additionally, firms' leverage decisions are not responsive to union's net worth. As unions have limited ability to finance operations due to their limited collateralizable capital, a decrease in a union's income decreases their ability to invest in bargaining campaigns and membership drives, which further decreases worker bargaining power within firms. More broadly, this result shows how the financial constraints of firm stakeholders shape the distribution of

surplus within a firm.

These results also respond to persistent disagreement surrounding the effect of RTW on local unions. As RTW outlaws the collection dues from nonmembers, the reform increases the relative cost of union services for workers employed in positions covered by a collective bargaining agreement. When workers are no longer required to pay for a union's services, they may decide to relinquish their membership. As they benefit from a collective bargaining contract without paying dues (known as "free-riding"), relinquishing their membership is a more financially attractive option in the short-run. The economics literature argues that workers' relative preference for union services, the union-wage premium, non-pecuniary benefits of unions, and workers' income also determine the effect of RTW across states [Moore and Newman \(1985\)](#). Contrary to the economics literature, national labor representatives argue that the effect of RTW on unions depends primarily on local organizers' ability to increase the salience of the long-term costs of free-riding. Specifically, labor representatives highlight the need to push back against "drop your membership" campaigns (see [Will \(2018\)](#)). These campaigns seek to increase the salience of the short-term benefits of free-riding in states that have recently enacted RTW. National labor representatives have also dismissed concerns that losing mandatory agency fee payers could have a significant effect on their federations' income, which would increase the relative cost of representational services for members (see [Weingarten et al. \(2018\)](#)). This paper responds to this disagreement and finds an answer somewhere in the middle. In the years following RTW, unions with agency fee payers experience a significant loss of revenue before they report a significant loss of membership. Unions exposed to RTW respond by decreasing their spending on operations and increasing the income share of membership contributions, which increases the relative cost of union services for members.

This project contributes to three strands of the literature. First, it contributes to a broad literature on the relationship between labor regulations and corporate capital structure. This literature has persisted at the intersection of corporate finance and industrial relations. [Bronars and Deere \(1993\)](#) measure a correlation between unionization rates and leverage across industries. They emphasized that firms increase leverage in response to unions' bargaining power, thus diminishing workers' ability to negotiate for a larger share

of firm revenue. [Matsa \(2010\)](#), [Chava \*et al.\* \(2020\)](#), and [Simintzi \*et al.\* \(2015\)](#) consider the relationship between labor protection policies and firms financing decisions. [Matsa \(2010\)](#) considers the link between workers’ bargaining power and firms’ use of debt financing. [Chava \*et al.\* \(2020\)](#) shows that firms increase leverage following changes in the minimum wage, expanding on [Matsa \(2010\)](#). [Simintzi \*et al.\* \(2015\)](#) show how labor regulations increase firms’ leverage, constraining their investment decisions. I contribute to these results by showing the effect of labor organizations’ financial health on firms’ outcomes.

Second, this project contributes to the corporate finance literature linking input and product markets to firms’ capital structure. In her study of supermarkets, [Chevalier \(1995\)](#) shows that supermarkets are more likely to enter into markets wherein their potential competitors have higher leverage. In their study of LBOs, [Brown \*et al.\* \(2009\)](#) observed that increasing leverage does not lead to a significant decrease in the price of inputs charged by suppliers. As unions have limited ability to secure external finance, this project contributes to these results by measuring the effect of input market constraints on firms’ capital structure.

Third, my paper contributes to the literature on the economic effects of unionization and RTW policies. [Moore and Newman \(1985\)](#) summarizes the early literature on the economic effects of RTW, emphasizing the challenge of addressing the correlation between the passage of RTW policies and unionization rates. Specifically, RTW and unionization rates are correlated with anti-union sentiments. [Carroll \(1983\)](#) and [Ellwood and Fine \(1987\)](#) use measures of gender diversity, democratic control of the legislature, and public education bargaining policies to control for the effect of anti-union sentiments with mixed results. To control for state-level variation in anti-union sentiments, [Fortin \*et al.\* \(2022\)](#) uses a similar differential exposure design to the one employed in this paper. They measure the effect of RTW by measuring the response of wages in industries with high unionization rate relative to wages in industries with low unionization rates following the reform, as industry-level unionization rates do not vary systematically across their treatment and control groups. [Ellwood and Fine \(1987\)](#) find that RTW decreases the flow into membership through certified elections. [Zullo \(2020\)](#) shows that RTW decreases the number of decertifying elections, where members of the bargaining unit vote to remove a union from the workplace, as non-members are no longer required to support the union financially. This project contributes to this literature

by highlighting the importance of union revenue as the primary mechanism through which RTW affects the provision of union services and unionization rates.

## 2 Data Description

In this section, I discuss the data that I use to estimate the effect RTW policies on unions and large private firms. The analysis relies on a sample of five RTW reforms in Wisconsin, Michigan, Indiana, Kentucky, and West Virginia. The reforms in my sample occurred between 2012 and 2017 and resulted from legislative decisions. This project uses a novel data set that links large public corporations' financial reports from Compustat to administrative unions' balance sheet data from LM-2 filings. Administrative data on local unions' membership composition allow me to measure each union's differential exposure to RTW given their pre-treatment take-up of agency fees. These data also include financial information on unions' income sources and spending patterns, which allow for research designs that study the effect of RTW on unions' financial health. Data on large public corporations' financial reports, once linked with local union data, allow me to estimate each firms' exposure to local unions.

Local unions, intermediate bodies, and national labor organizations must file an annual report to the United States Department of Labor (DOL), in accordance with the Labor Management Reporting and Disclosure Act (LMRDA) of 1959. This act covers labor organizations in the private sector, as well as federal agencies and the US postal service. Labor organizations' reporting requirements vary by the level of their annual total receipts and governance structure.

A labor organization with annual gross receipts greater than \$250,000 must file an LM-2, unless they are in trusteeship (i.e. have relinquished governance to an intermediate or national organization). On the LM-2, unions must report detailed information on their income sources and their outlays, including the number of agency fee payers in their bargaining unit and their spending on representational services, personnel, and overhead. Organizations with less than \$250,000 in receipts must file an LM-3, and organizations with less than \$10,000 in receipts must file an LM-4. These annual report includes detailed data on spending on representational services, personnel, and overhead but does not include data on the amount

of agency fee payers. As such, I restrict my baseline sample to include only local unions with greater than \$250,000 in annual reports. This threshold is low enough to include local unions relevant to the study of firm dynamics.

To match labor organizations to relevant firms, I use separate administrative collective bargaining data collected by the DOL and the Federal Mediation and Conciliation Service (FMCS). Congress formed the FMCS in 1947 to expedite the resolution of labor disputes, as a neutral body to support unions and employers engaged in bargaining, arbitration, and mediation. When a union or employer wants to negotiate, modify, or terminate a collective bargaining agreement, they must file a notice of bargaining (an F-7) with the FMCS. These forms include data on the union membership, bargaining unit size, and location of operation. Unions and firms are also encouraged, but not required, to submit CBAs to the DOL. The DOL collects CBA's from public and private firms, in accordance with Section 211(a) of the Taft-Hartley Act "for the guidance and information of interested representatives of employers, employees, and the general public." As the National Mediation Board (NMB) is responsible for collecting the CBAs for firms in the railroad or airline industries, DOL does not collect CBAs from these firms. These CBA reports include data on the local unions affiliation and operation location, although data on the specific local unions is sometimes missing or incomplete.

The firm and union data included in F7 and CBA reports allow me to construct a crosswalk between union's annual reports and firms financial reports. I use a fuzzy matching algorithm to match employer names in CBA and F7 filings to financial reports in Compustat. I use a records-linking algorithm to match local unions' affiliations and addresses in the CBA and F7 filings to local unions' annual reports. I then verify the validity of the crosswalk by hand. As a result, the crosswalk also includes data on the distribution of firm operations across state lines. Since RTW policies only affect local unions within the state, the location of a firm's chief executives does not determine their exposure to RTW. As such, this crosswalk improves upon prior estimates of the effects of RTW using Compustat data. A firm's location reported in Compustat is the address of their principal executive offices. The location of a firm's principal executive offices does not necessarily signal a firm's exposure to collective bargaining reform and other labor regulations. Appendix 7 discusses the construction of the

matched panel in detail.

This project relies on two separate samples. First, I estimate the effect of RTW on union balance sheets using LM-2 data from 2007-2019. I restrict my sample to 2007-2019, as the DOL began collecting more detailed data on agency fee payers in 2007. I limit the end of the sample to 2019 to avoid variation due to the coronavirus pandemic. Second, I estimate the cross-sectional relationship between unions' balance sheets and firms' leverage using a matched LM-2 and Compustat panel. I use an extended sample from 2000-2019 to take advantage of the entire span of LM-2 data on unions' balance sheets.

Prior to the construction of the second panel, I clean the Compustat data using the following steps. I remove financial and utility firms from my sample (i.e. SIC 6000-6999, 4900-4999), in accordance with the data cleaning practices employed [Chava \*et al.\* \(2020\)](#), which is a combination of those used in [Vuolteenaho \(2002\)](#) and [Whited and Wu \(2006\)](#). I also winsorize each variable by 0.5% to diminish the effect of outliers on the estimates. I also drop firms with equity valuations below \$10 million dollars in 2009 dollars. I also drop firms with negative total assets, total liabilities, long-term debt, employees, cash, and dividends. In accordance with the sample selection of [Chava \*et al.\* \(2020\)](#), I filter out firm observations that are likely the result of mergers or acquisitions by dropping observations in which capital expenditures are greater than 50% of total property, plant, and equipment owned by the firm. Table I reports the summary statistics of all firms in the sample.

**Table I** – Firms in Matched Panel

	Mean	Std. Dev.	10 <sup>th</sup>	25 <sup>th</sup>	Median	75 <sup>th</sup>	90 <sup>th</sup>
Leverage	0.407	1.191	0.000	0.021	0.208	0.408	0.693
Tobin's Q	4.167	15.325	0.895	1.111	1.583	2.719	5.430
Tangibility	0.245	0.252	0.017	0.051	0.143	0.368	0.684
Log Sales	5.173	2.756	1.560	3.503	5.481	7.124	8.424
Profitability	-0.211	1.426	-0.529	-0.060	0.080	0.140	0.205
Observations	77786						

*Notes:* This table reports summary statistics for firms in the matched LM-2 and Compustat sample. Observations reported are firm-year observations. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FMCS. Table reports union financial and membership data at the union level.

*Sources:* DOL, FMCS, Compustat and author's calculations.



Table II reports the summary statistics of the baseline sample constructed using LM-2 Data. The two analysis samples provide insight into the role of agency shop agreements in unions' financial agreements and firms' leverage decisions. Most unions in the sample do not report agency fee payers. Around 26% of the unions in the sample ever report agency fee payers, and make up around 20% of the total union by year observations.

**Table II** – Unions in Baseline Sample

	Mean	Std. Dev.	10 <sup>th</sup>	25 <sup>th</sup>	Median	75 <sup>th</sup>	90 <sup>th</sup>
Receipts	329.06	843.05	29.20	44.27	90.90	236.67	623.22
Disbursements	321.07	831.33	27.87	42.86	88.20	228.38	603.58
Assets	356.07	902.07	13.85	34.94	89.72	257.87	751.73
Rec / Members	0.31	3.29	0.03	0.05	0.08	0.17	0.31
Liabilities	39.95	165.32	0.00	0.04	1.27	9.95	56.83
Net Worth	292.08	673.36	10.73	30.79	80.93	231.27	660.57
Members	5160.51	16477.75	255.00	532.00	1120.00	2696.00	8116.00
AFP Rate	0.50	2.26	0.00	0.00	0.00	0.00	0.42
Representation	71.66	179.07	1.55	7.66	19.80	53.53	147.94
Political	6.60	25.45	0.00	0.00	0.34	2.35	10.28
Overhead	40.01	109.97	0.94	4.21	11.31	28.66	75.21
Observations	74506						

*Notes:* This table reports summary statistics for local unions in the baseline LM-2 sample. Variables are winsorized at the 0.5% level. Observations reported are union-year observations. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2007-2019. Table reports union financial and membership data at the union level.

*Sources:* DOL, FMCS, and author's calculations.

Table III reports the summary statistics of unions with and without agency fee payers in this sample. Unions with agency fee payers are more financially stable relative to unions without agency fee payers on average, although these differences are not statistically significant. Unions with agency fee payers report annual receipts of 7.74 million annually, while unions without agency fee payers report 2.39 million in receipts. Unions with agency fee payers also tend to be larger. The average union with agency fee payers reports an average membership of 12472 workers, while unions without agency fee payers report 3686 members. This difference in receipts and membership does not necessarily correlated with greater receipts per members. On average, unions without agency fee payers report \$2600 more income per member than unions with agency fee payers. That said, these differences in income and

**Table III** – Unions with and without Agency Fee Payers in Baseline Sample

	Reported Agency Fee Payers			No Agency Fee Payers		
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median
Receipts	774.20	1466.71	215.00	239.31	610.19	80.34
Disbursements	757.73	1444.75	209.22	233.04	602.88	77.83
Assets	725.82	1511.07	145.72	281.53	695.95	82.56
Liabilities	118.33	305.62	5.79	24.15	111.91	0.95
Rec / Members	0.10	0.27	0.07	0.36	3.60	0.09
Net Worth	511.40	1030.61	118.20	247.86	564.85	75.67
Members	12472.40	27149.02	3046.00	3686.34	12834.05	966.00
Representation	180.81	310.85	57.73	49.66	127.15	16.59
Political	18.23	45.81	1.34	4.25	17.95	0.23
Overhead	96.10	197.13	22.10	28.70	77.03	10.06
Observations:	12501			62005		

*Notes:* This table reports summary statistics for local unions without agency fee payers in the baseline LM-2 sample. Observations reported are union-year observations. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2007-2019. Table reports union financial and membership data at the union level.

*Sources:* DOL, FMCS, and author’s calculations.

membership across these two groups translates to differences in financial health and organizing capacity. Unions with agency fee payers have greater assets and net worth, relative to unions without agency fee payers. Unions with agency fee payers also tend to be larger and spend more on general overhead, representational services, and political activities. While these differences suggest that unions with agency fee payers tend to be larger unions with greater organizing capacity, these differences are insignificant. I discuss the implications of these summary statistics for the research design in the next section.

In the matched Compustat and LM-2 panel, these differences persist. The sample selection decision to use large public corporations to measure leverage selects unions that are larger and more likely to have agency fee payers. I am able to match around 13 percent of the first sample to Compustat firms. This share is reasonable, given that larger firms are more likely to be unionized. The matching procedure does not affect the average union included in the sample. Appendix 7 presents the summary statistics for unions in the matched panel, as well additional discussion of the matching procedure and distribution of unions across industries.

The matched sample of Compustat and LM-2 data is consistent with prior work on

the relationship between unionized firms and non-unionized firms. Table IV reports the summary statistics of unionized and nonunionized firms in this sample, and Table V reports the summary statistics of unionized firms without and without agency fee payers.

**Table IV** – Unionized and Nonunionized Firms in Matched Panel

	Unionized			Nonunionized		
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median
Leverage	0.35	0.18	0.32	0.41	1.20	0.20
Tobin's Q	1.48	0.70	1.28	4.22	15.46	1.59
Tangibility	0.43	0.24	0.41	0.24	0.25	0.14
Log Sales	8.43	1.38	8.34	5.09	2.73	5.41
Profitability	0.12	0.09	0.11	-0.22	1.44	0.08
Observations:	1799			75987		

*Notes:* This table reports summary statistics for unionized and nonunionized firms in the matched LM-2 and Compustat sample. Observations reported are firm-year observations. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019.  
*Sources:* DOL, FMCS, Compustat and author's calculations.

**Table V** – Unionized Firms with and without Agency Fee Payers in Matched Panel

	Reported Agency Fee Payers			No Agency Fee Payers		
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median
Leverage	0.36	0.20	0.32	0.34	0.17	0.32
Tobin's Q	1.38	0.45	1.25	1.51	0.76	1.29
Tangibility	0.33	0.22	0.27	0.46	0.24	0.44
Log Sales	8.54	1.48	8.43	8.40	1.35	8.33
Profitability	0.12	0.06	0.11	0.12	0.10	0.11
Observations:	387			1412		

*Notes:* This table reports summary statistics for unionized firms with and without agency fee payers in the matched LM-2 and Compustat sample. Observations reported are firm-year observations. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FMCS. Table reports firm financial data from Compustat.  
*Sources:* DOL, FMCS, Compustat and author's calculations.

Unionized and nonunionized firms in the matched patterns exhibit differences in investment opportunities, profitability, leverage, size, and tangibility although these differences are not statistically significant. The median non-unionized firm in the matched panel reports book leverage of 0.35, while the median unionized firm reports a leverage of 0.41. Unionized

firms also tend to be larger. The average unionized firm reports annual log sales of 8.43. The average nonunionized firm reports annual log sales of 5.09. Unionized firms also tend to have a lower Tobin's  $q$ , defined here as a firm's market to book ratio. The average unionized firm reports a Tobin's  $q$  of 1.48, while the average nonunionized firm reports a Tobin's  $q$  of 4.22. This difference is consistent with patterns of unionization across firms. As newer firms are less likely to be organized than older larger firms, one should expect that unionized firms report a lower Tobin's  $q$ . Unionized firms also hold a greater share of tangible capital. The median non-unionized firm in the matched panel reports a ratio of tangible capital to total assets of 0.405, while the median unionized firm reports a ratio of 0.139. As unionization varies across industries, these differences reflect variation in investment opportunities, profitability, input market, and output demand, which vary across industries, in addition to industrial relations. As such, I use these variables, in addition to fixed effects, to identify the cross-sectional relationship between unions' balance sheets and firms' capital structure. Notably, I find that unionized firms with agency fee payers are remarkably similar to firms without agency fee payers. In the next section, I discuss how these findings provide support for the identifying assumptions of my research design.

### 3 Effect of RTW on Unions

In this section, I estimate the effect of RTW on local union's balance sheets. First, I explain how institutional details of union governance inform my research design. Second, I explain how these features of labor law and union governance shape unions' differential exposure to RTW policies. Third, I present the effect of RTW on union balance sheets and discuss how local unions and their affiliates responded to RTW.

#### 3.1 Institutional Arrangements

While state legislatures enacted the RTW policies studied here, a union's past collective bargaining outcomes determine how RTW affects that union's membership and balance sheet. RTW prohibits agency shop agreements between unions and employers. These contract clauses stipulate that all employees covered by the contract must contribute a portion of

their wages to support the union’s bargaining and representative services. Employees who contribute to the union that represents them under such agreement are known as agency fee payers. While the National Labor Relations Act (NLRA) established the legality of agency shop agreements in 1935, it did not mandate the inclusion of agency shop agreements in all collective bargaining agreements. Unions must bargain with their employer for the ability to enforce mandatory collection of agency fees, as well as the size of those fees. Employers do not need to grant unions the ability to collect agency fees. Assuming that denying agency fees weakens workers’ bargaining position, employers often seek to exclude these clauses from collective bargaining agreements to limit workers’ bargaining power. This practice is widespread in states without RTW policies. In 2017, 80% of construction projects in New York City were open-shop (see ?).

Subsequently, this variation in agency shop agreements across unions shapes unions’ exposure to RTW policies. Some unions do not receive income from agency fee payers, while others rely on agency fee payers to fund their operations. Following the passage of a RTW law, nonmembers are no longer required to pay agency fees to the union that represents them, once the contract containing the agency shop agreement expires. Under RTW, unions with agency shop agreements lose income from agency fee payers and face the organizing challenge of convincing members to pay a higher relative cost of membership. As free riding becomes more attractive, these unions may also lose income from members leaving the organization. While the passage of RTW laws sometimes occur alongside other rollbacks in labor regulations and redistributive policies that disadvantage workers, RTW only affects the income of unions with agency fee payers. Unions without agency fee payers are not immediately affected by the passage of RTW, as RTW does not initially affect these unions’ incomes or their relative cost of membership. As such, unions with agency shop agreements are more exposed to RTW policies than unions without such agreements.

The presence of agency fee payers is the binary exposure measure in this research design. The use of this measure relies on the assumption that unions that report agency fee payers are more likely to have an agency shop agreement than unions that do not report agency fee payers. Local unions do not report whether they have an agency shop agreement in their contract to the DOL. Rather they report agency fee payers in the LM-2 filings. Some

collective bargaining contracts in RTW and non-RTW states allow for voluntary agency fee payers. This allows unions to collect fees from workers who are not willing to become a member. For example, Wayne State University AAUP-AFT Local 6075 offers employees in their bargaining unit the option of paying voluntary agency fees. Organizers affiliated with AFT Michigan claim that organizing new employees to chose to become voluntary agency fee payers is easier than organizing them into full union members. As such, there are unions in RTW states that report agency fee payers. Interestingly, agency fee payers does not decrease to zero after RTW.

Voluntary agency fee payers are rare in RTW states. Table VI reports the share of local unions with agency fee payers in states that passed RTW prior to the start of the sample period and states that have never passed RTW legislation. In states without RTW, approximately 20% of unions report agency fee payers. In states where RTW laws passed prior to 2007, 5.5% of unions report agency fee payers. The latter statistic represents either unions with voluntary agency fee payers or ones with misreported data. The latter is unlikely as unions are not required to report agency fee payers and may simply leave the entry blank. In states without RTW, this ratio is above 12 agency fee payers per 100 members for most of the sample. In states with RTW, this ratio is less than 10 agency fee payers per 100 members for most periods. In states without RTW, the ratio of agency fee payers varied insignificantly throughout the sample period. In states with RTW before 2007, the ratio of agency fee payers to members increased slightly throughout this period. As such, these data suggest that voluntary agency fee payment is rare in RTW states.

Following RTW, the share of unions reporting agency fee payers and the ratio of agency fee payers to members decreases. As unions allow for voluntary agency fee payers and agency shop clauses persist until a collective bargaining agreement ends, RTW does not completely eliminate agency fee payers in the short run. Instead, RTW removes mandatory agency fee payers, causing a significant decrease in the share of unions reporting agency fee payers and the ratio of agency fee payers to members. Table VII reports the share of local unions with agency fee payers and the ratio of agency fee payers to members for unions with agency fee payers in the states that enacted RTW within the analysis sample. Across all treatment groups, the share of unions reporting agency fee payers fell from 14% in the year RTW passed

to 8% five years after the reform. Over the five years following the passage of RTW, this ratio decreases almost 50% from approximately 8 agency fee payers per 1000 members to 6 agency fee payers per 1000 members.

The dynamics of agency fee payment are heterogenous across states. A year after RTW passed in West Virginia, no unions reported any agency fee payers. Two years after RTW passed in Kentucky, unions reported 1 agency fee payer to 10,000 members. Michigan and Wisconsin are the two states in the sample with the largest share of unions with agency fee payers prior to RTW. As such, the share of unions reporting agency fee payers remains higher than that of the other states in the treatment group, but the ratio of agency payers to members fell significantly. Following the passage of RTW in Wisconsin, the share of unions reported agency fee payers decreased from 24% to 17%, while the average ratio of agency fee payers to members decreased from 0.017 to 0.001 after four years. In Michigan, the share of unions reported agency fee payers decreased from 24% to 17%, while the average ratio of agency fee payers to members decreased from 0.009 to 0.0007 after five years. While the effect of RTW led to gradual, heterogenous decreases in agency fee payers across states, the decreases were significant and persistent. Appendix 8 reports additional information on membership trends across these cross-sections.

**Table VI** – Trends in Agency Fee Payers Outside of Treatment Group

Year	Share of Unions with AFP			AFP/Members		
	Total	RTW Never	RTW Pre-2007	Total	RTW Never	RTW Pre-2007
2007	0.1617 (0.0063)	0.1959 (0.0079)	0.0566 (0.0080)	0.0113 (0.0011)	0.0131 (0.0013)	0.0058 (0.0019)
2008	0.1521 (0.0062)	0.1855 (0.0077)	0.0543 (0.0077)	0.0105 (0.0011)	0.0114 (0.0012)	0.0079 (0.0023)
2009	0.1548 (0.0063)	0.1903 (0.0079)	0.0501 (0.0075)	0.0106 (0.0011)	0.0122 (0.0013)	0.0058 (0.0019)
2010	0.1629 (0.0069)	0.1998 (0.0086)	0.0466 (0.0080)	0.0128 (0.0013)	0.0140 (0.0015)	0.0090 (0.0028)
2011	0.1760 (0.0067)	0.2172 (0.0084)	0.0532 (0.0079)	0.0143 (0.0014)	0.0152 (0.0015)	0.0116 (0.0030)
2012	0.1769 (0.0067)	0.2197 (0.0084)	0.0486 (0.0076)	0.0140 (0.0013)	0.0155 (0.0015)	0.0096 (0.0028)
2013	0.1829 (0.0068)	0.2270 (0.0086)	0.0531 (0.0079)	0.0147 (0.0014)	0.0162 (0.0016)	0.0101 (0.0028)
2014	0.1857 (0.0069)	0.2304 (0.0086)	0.0524 (0.0079)	0.0144 (0.0013)	0.0164 (0.0016)	0.0084 (0.0025)
2015	0.1856 (0.0069)	0.2315 (0.0086)	0.0513 (0.0077)	0.0141 (0.0013)	0.0167 (0.0016)	0.0064 (0.0019)
2016	0.1841 (0.0068)	0.2272 (0.0085)	0.0583 (0.0082)	0.0145 (0.0014)	0.0163 (0.0016)	0.0094 (0.0025)
2017	0.1857 (0.0069)	0.2303 (0.0086)	0.0569 (0.0081)	0.0139 (0.0013)	0.0152 (0.0015)	0.0101 (0.0027)
2018	0.1719 (0.0067)	0.2129 (0.0084)	0.0579 (0.0080)	0.0098 (0.0011)	0.0102 (0.0013)	0.0084 (0.0022)
2019	0.1627 (0.0065)	0.1971 (0.0081)	0.0646 (0.0085)	0.0105 (0.0013)	0.0096 (0.0013)	0.0132 (0.0031)

*Notes:* This table reports means and their standard errors in agency fee payers in local unions located in states which were unaffected by RTW reforms from 2007-2019.

*Sources:* DOL



**Table VII** – Trends in Agency Fee Payers Relative to the Enactment of RTW

$h$	Share of Unions with AFP						AFP/Members					
	Total	MI	IN	WI	WV	KY	Total	MI	IN	WI	WV	KY
-5	0.1130 (0.0146)	0.1230 (0.0241)	0.0714 (0.0230)	0.1791 (0.0472)	0.0833 (0.0467)	0.1132 (0.0439)	0.0047 (0.0019)	0.0015 (0.0008)	0.0079 (0.0063)	0.0017 (0.0009)	0.0064 (0.0062)	0.0108 (0.0066)
-4	0.1173 (0.0149)	0.1016 (0.0222)	0.0840 (0.0255)	0.2192 (0.0488)	0.1053 (0.0505)	0.1154 (0.0447)	0.0045 (0.0018)	0.0013 (0.0007)	0.0085 (0.0066)	0.0047 (0.0021)	0.0053 (0.0052)	0.0062 (0.0041)
-3	0.1093 (0.0149)	0.1091 (0.0243)	0.0877 (0.0266)	0.1892 (0.0458)	0.0606 (0.0422)	0.0755 (0.0366)	0.0058 (0.0025)	0.0018 (0.0008)	0.0084 (0.0068)	0.0026 (0.0011)	0.0031 (0.0030)	0.0184 (0.0147)
-2	0.1185 (0.0154)	0.1341 (0.0255)	0.0816 (0.0278)	0.2000 (0.0465)	0.0541 (0.0377)	0.0600 (0.0339)	0.0036 (0.0018)	0.0019 (0.0007)	0.0094 (0.0079)	0.0031 (0.0013)	0.0010 (0.0010)	0.0014 (0.0011)
-1	0.1326 (0.0161)	0.1412 (0.0263)	0.0901 (0.0273)	0.2464 (0.0523)	0.0526 (0.0367)	0.1000 (0.0429)	0.0046 (0.0019)	0.0029 (0.0014)	0.0104 (0.0073)	0.0021 (0.0008)	0.0007 (0.0006)	0.0040 (0.0031)
0	0.1490 (0.0169)	0.1686 (0.0286)	0.0973 (0.0280)	0.2394 (0.0510)	0.0513 (0.0358)	0.1458 (0.0515)	0.0084 (0.0032)	0.0079 (0.0048)	0.0102 (0.0072)	0.0146 (0.0110)	0.0007 (0.0006)	0.0027 (0.0018)
1	0.1403 (0.0165)	0.1744 (0.0290)	0.0714 (0.0244)	0.2329 (0.0498)	0.0000 (0.0000)	0.1458 (0.0515)	0.0055 (0.0026)	0.0016 (0.0007)	0.0101 (0.0073)	0.0139 (0.0107)	0.0000 (0.0000)	0.0004 (0.0002)
2	0.1024 (0.0143)	0.1124 (0.0237)	0.0811 (0.0260)	0.2059 (0.0494)	0.0000 (0.0000)	0.0556 (0.0315)	0.0045 (0.0024)	0.0011 (0.0005)	0.0150 (0.0098)	0.0024 (0.0012)	0.0000 (0.0000)	0.0001 (0.0001)
3	0.1145 (0.0161)	0.1322 (0.0258)	0.0541 (0.0216)	0.2192 (0.0488)	0.0000 (0.0000)		0.0051 (0.0028)	0.0057 (0.0045)	0.0078 (0.0070)	0.0020 (0.0009)	0.0000 (0.0000)	
4	0.1114 (0.0166)	0.1294 (0.0258)	0.0439 (0.0193)	0.1733 (0.0440)			0.0052 (0.0030)	0.0056 (0.0046)	0.0072 (0.0068)	0.0012 (0.0006)		
5	0.0836 (0.0164)	0.1034 (0.0232)	0.0531 (0.0212)				0.0061 (0.0038)	0.0007 (0.0004)	0.0144 (0.0096)			

*Notes:* This table reports means and their standard errors in agency fee payers in states affected by RTW reforms from 2007-2019. *Sources:* DOL

## 3.2 Identification

This research design exploits this differential exposure to measure the effect of RTW on unions. In this setting, treatment occurs when a state’s legislature passes RTW. I measure a union’s exposure to RTW as a binary indicator that takes the value of one if they report agency fee payers prior to the passage of RTW and zero otherwise. Differential exposure designs rely on two identifying assumptions. First, differential exposure designs rely on the assumption that the gap between exposed and unexposed unions in the control group would have evolved similarly to the gap between exposed and unexposed unions in the treatment group, if the unions in the treatment group did not experience RTW. While this assumption is ultimately untestable, as the treatment group’s counterfactual is unobserved by the econometrician, this assumption is ex-ante plausible in this setting. Unions rely on their employer’s economic performance, regardless of whether they operate under a union security agreement. Mandatory agency fee payments cannot offset variation in employment and wages resulting from state-level trends.

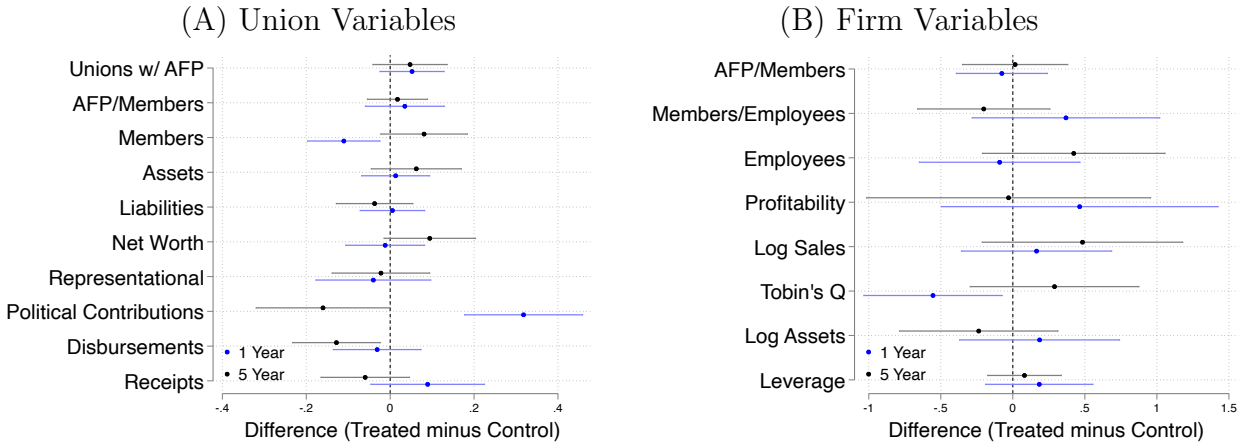
Second, differential exposure difference-in-differences research designs rely on the assumption that unions’ adoption of agency shop agreements would be the same across states in the absence of RTW. This assumption is reasonable in this setting. Unions do not change agency shop agreements in anticipation of RTW. Unions cannot adjust their agency shop clause at will, so terms of agency fee payment cannot be adjusted outside of collective bargaining. It is also unlikely that unions would adjust their reliance on agency fee payers in anticipation of RTW. Raising dues on members is generally unpopular and requires union organizers to build membership support for raising the cost of membership. As organizing resources are limited, it is unlikely that unions would allocate effort towards building membership support for a dues increase, rather than allocating effort towards mobilizing membership in opposition of RTW.

In the rare case that a union is actively bargaining for a new contract when a state’s legislature signs RTW into law, it is unlikely that a union without an agency shop agreement would use their bargaining power to establish a short-lived agency shop agreement prior to the RTW policy taking effect. That said, a union with a preexisting agency shop agreement may take steps to adjust the length of their contract in order to delay the loss of their agency

shop agreement. In 2012, the Graduate Employees Organization AFT-MI 3550, AFL-CIO (GEO), a union with a preexisting agency shop agreement, bargained to extend the length of their contract from three to four years. This preemptive decision was possible because the union was actively bargaining a new contract when the Michigan Legislature passed RTW during its 2012 lame duck session. This bargaining decision was costly for GEO's members. To extend the length of their contract, members of GEO accepted historically lower raises. This example shows that although unions with preexisting agency shop agreements can delay the effect of RTW policies, they cannot change their exposure to RTW even if they are in active contract negotiations.

Figure I reports differences in union and firm trends across RTW and non-RTW states in the estimation sample. Trends in the share of unions with reported agency fee payers do not vary systematically between RTW and non-RTW in the pre-treatment period. Union net worth, disbursements, and receipts exhibit a parallel trend across states in the sample period. There are limited departures from parallel trends for members, assets and liabilities, although those departures are less than 5% of the sample standard deviation of either variable.

**Figure I** – Difference in Pre-Treatment Trends Across RTW and Never-RTW Unions



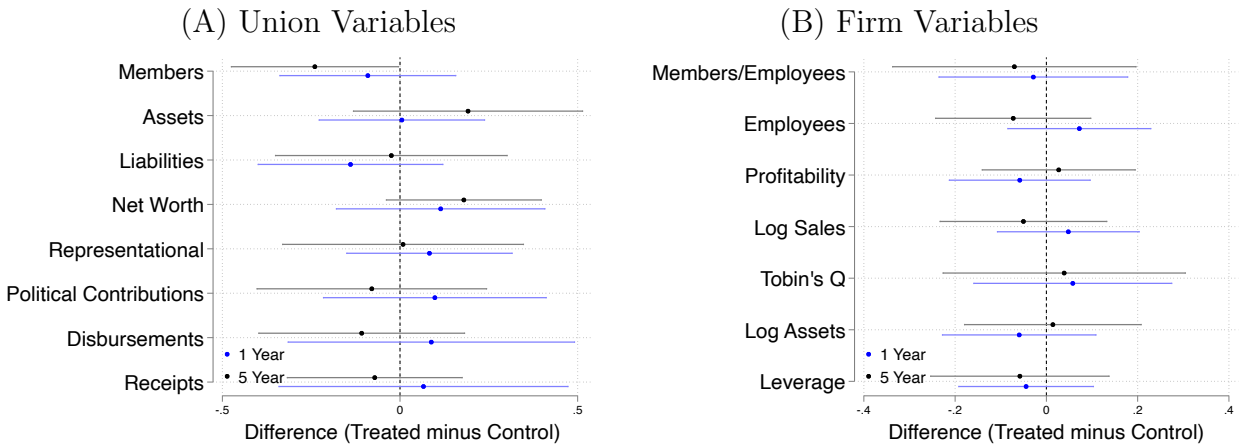
*Notes:* This plot contains two subplots, which report the difference in pre-treatment trends across treated and untreated unions. Differences are scaled by the standard deviation of the entire sample. These data are from the LM-2 annual reports filed with the DOL. Sample spans from 2007 to 2019.

*Sources:* DOL, Compustat, FMCS, and author's calculations.

The observable characteristics of unions and firms in this data set also support this assumption. Figure II shows that unions, regardless of exposure to RTW, have similar pre-RTW, within-state trends. Figure II shows that unions with agency fee payers exhibit

parallel trends across RTW and non-RTW states. Moreover, Figure III shows that the take-up of agency shop clauses is not systematically correlated with firms' capital structure or unions' membership or financial conditions. As such, exposed and unexposed unions appear to exhibit parallel trends, which supports the validity of this research design choice. Some unions with agency fee payers may not operate under an agency shop clause, so estimates using this research design may exhibit attenuation bias, as the exposure indicator variable may overstate the level of exposure of some local unions.

**Figure II** – Difference in Pre-Treatment Trends Across Agency-Shop and Open-Shop Unions

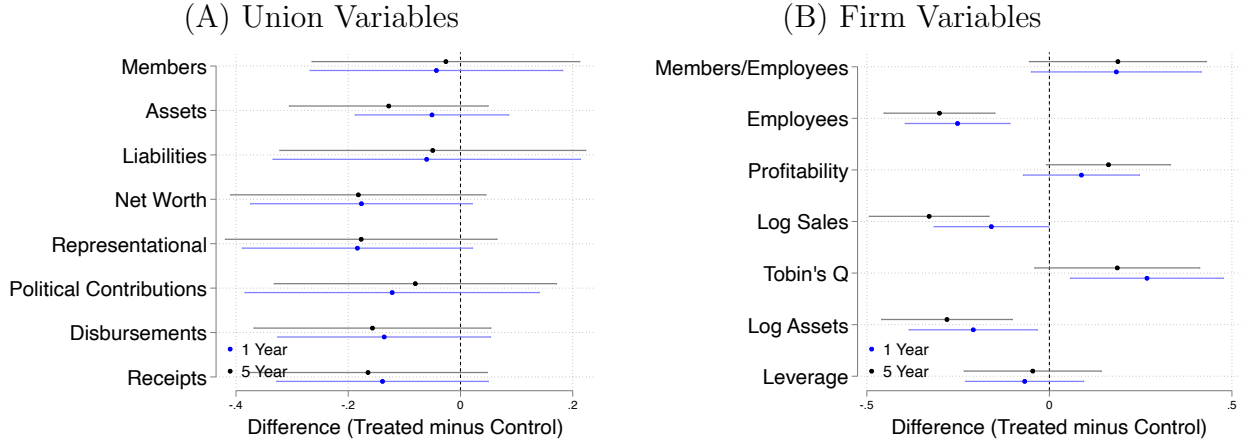


*Notes:* This plot contains two subplots, which report the difference in pre-treatment trends across exposed and unexposed unions. Differences are scaled by the standard deviation of the entire sample. These data are from the LM-2 annual reports filed with the DOL. Sample spans from 2007 to 2019.

*Sources:* DOL, Compustat, FMCS, and author's calculations.

In this differential exposure design, a firm's exposure to RTW policies depends on their employees' local union's balance sheet. Local unions typically represent workers employed at one firm, due to the cost of coordinating organizing across different companies or institutions. Some local unions operate across a number of similar, smaller firms, due to the fixed overhead cost of providing union services. While a few larger local unions operate across state lines due to the idiosyncratic governance structure of national organizations, inter-state coordination is typically coordinated through intermediate and national bodies. As such, I limit my sample to local unions who do not file F7 notices in two states.

**Figure III** – Difference in Pre-Treatment Levels Across Agency-Shop and Open-Shop Unions



*Notes:* This plot contains two subplots, which report the difference in pre-treatment levels across exposed and unexposed unions. Differences are scaled by the standard deviation of the entire sample. These data are from the LM-2 annual reports filed with the DOL. Sample spans from 2007 to 2019.

*Sources:* DOL, Compustat, FMCS, and author's calculations.

### 3.3 Effect of RTW on Unions

These institutional details inform my research design. I use a local projection difference-in-differences estimator with clean controls, following [Dube \*et al.\* \(2022\)](#) to estimate the RTW's effect on unions with agency fee payers, relative to unions without agency fee payers. This approach avoids improper comparisons between the just-treated group and previously treated groups, which can bias fixed-effects estimators in settings with heterogeneous treatment effects and staggered treatment. For each dependent variable  $y_{it}$  at horizon  $h$ , I run the following baseline specification:

$$(1) \Delta^h y_{i,t} = \alpha_t + \alpha_s + \beta_{RTW}^h RTW_{i,t} + \beta_{AFP}^h AFP_{i,t} + \beta_{RTW \times AFP}^h RTW_{i,t} \times AFP_{i,t} + \varepsilon_{i,t},$$

where  $RTW_{i,t}$  is a binary treatment indicator and equals 1 when RTW is enacted;  $AFP_{i,t}$  is the exposure indicator and equals 1 if union  $i$  claims agency fee payers in the period  $t$ . The specification includes year and state fixed effects and cluster coefficients' standard errors at the state-year level. Clean controls refers to the process of restricting the sample to observations that either receive treatment in period  $t$  or do not receive treatment within a specified window. For clarity, I restrict the sample to unions that experience RTW in period  $t$  or have never experienced RTW. The main coefficient of interest is  $\hat{\beta}_{RTW \times AFP}^h$ ,

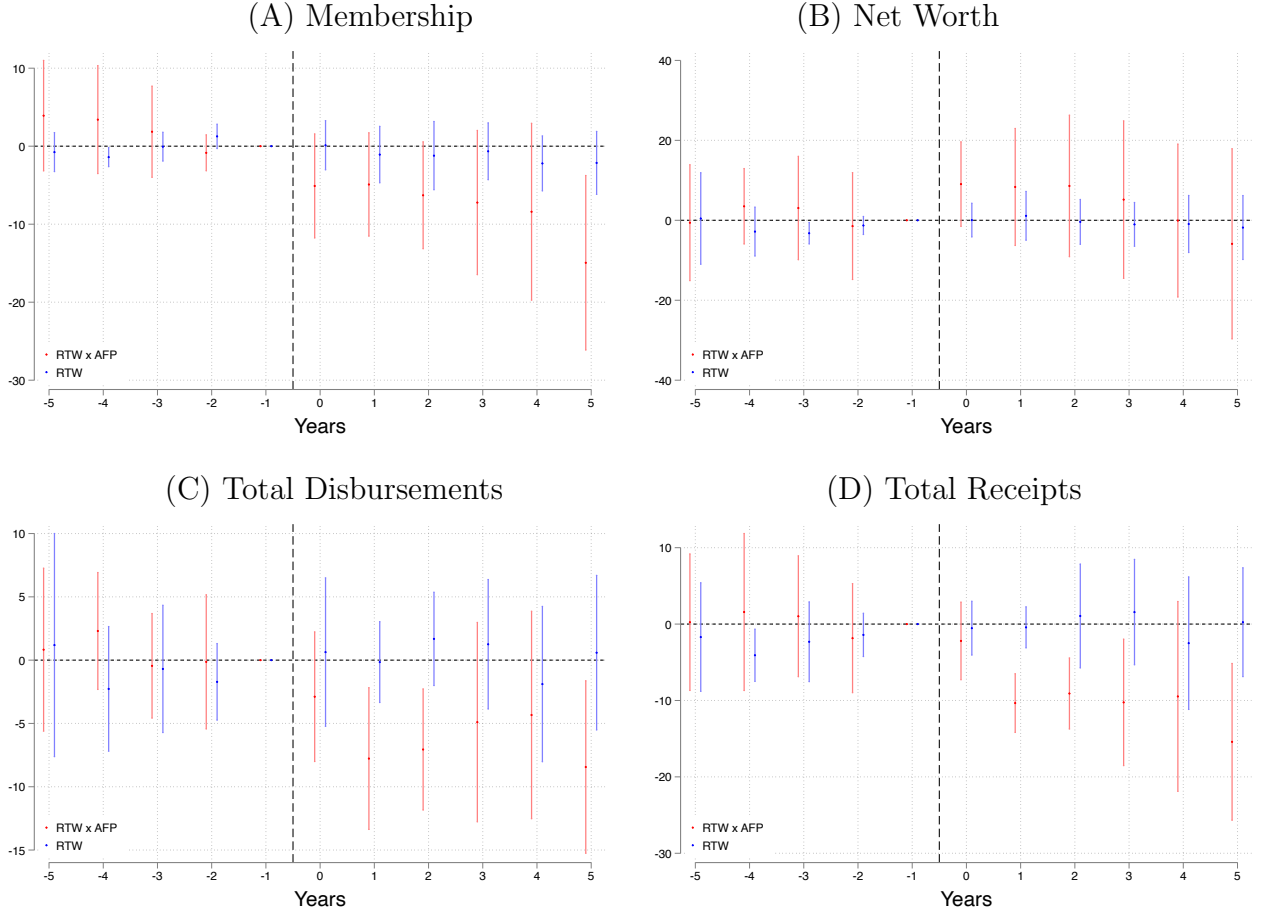
which measures the mean change in the outcome variable for unions that entered RTW with agency fee payers, relative to unions which entered RTW without any agency fee payers. The coefficient  $\beta_{RTW}^h$  measures the mean change in the outcome variables for unions that experienced RTW, relative to unions that never experienced RTW legislation. The coefficient  $\beta_{AFP}^h$  measure the mean change in the outcome variable for unions that with agency shop agreements, relative to unions that do not have these agreements.

Figures IV and Figure V plot  $\hat{\beta}_{RTW \times AFP}^h$  and  $\hat{\beta}_{RTW}^h$  for the baseline specification. Panel A plots the percentage point change in union membership for exposed unions following the introduction of RTW. Membership in exposed unions fell by 15% over five years, relative to membership in unexposed unions. This decline in membership is consistent with previous estimates of Wexler (2022), who found that union coverage fell by nearly 14% percentage points following the introduction of RTW in the same sample state reforms used here, using CPS MORG data. The research design employed here provides additional texture to previous estimates of the effect of RTW on union membership. Specifically, Panel A shows that unexposed unions did not experience a statistically significant decrease in membership following RTW, relative to unexposed unions in the control group.

The effect of RTW on union balance sheets also varies by unions' exposure. Panel C plots the log difference in total receipts or exposed unions relative to unexposed unions. Panel D plots the effect on total disbursements. In the year after RTW took effect, exposed unions' total disbursements fell by 7% and total receipts fell by 10% after 5 years. Unions without agency fee payers did not experience a reduction in total receipts and total distributions following RTW. This spending decrease is spread across various spending categories, including political contributions and representative services.

Unions rely on members and affiliates to accommodate income shortfalls associated with RTW. Unions' income share of membership contributions to total receipts increases by 5% following RTW. Affiliate contributions as a share of union income increased by 20% in exposed unions, and decreased by 5% in unexposed unions. This asymmetric, heterogenous response to RTW suggests that affiliates insure local unions from aggregate shocks through transfers financed by other local unions in the same federation. Finger and Hartney (2021) document similar phenomenon. They find that unions transfer funds from states with high union

**Figure IV – Union Responses to RTW Reforms**



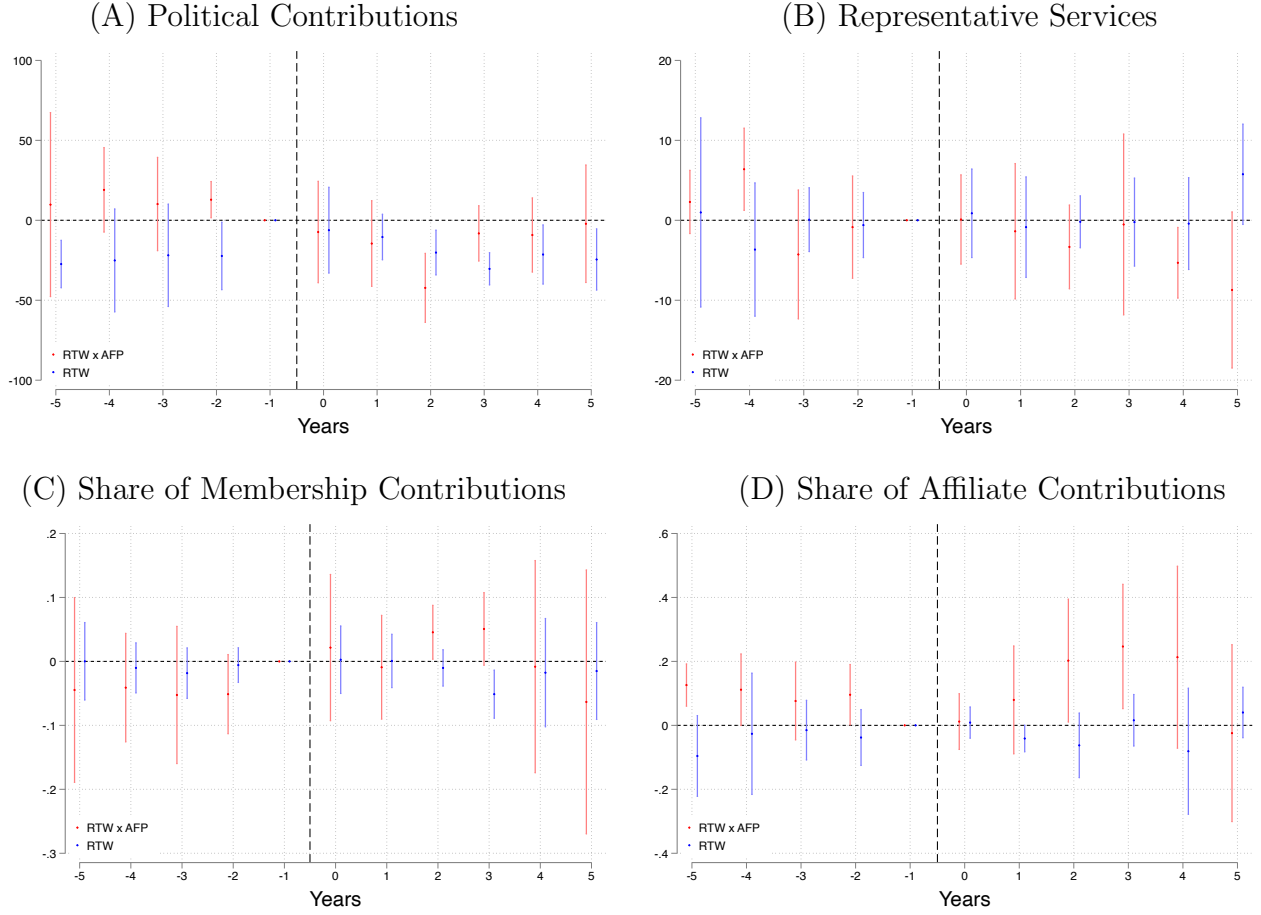
*Notes:* This plot contains four subplots, which report the estimation results of an exposure local projection difference in differences estimator from [Dube \*et al.\* \(2022\)](#). Standard errors are clustered at the state and year level. Nominal variables are reported in 2009 U.S. dollars. Estimates are the percentage point change in the variable. The red line plots  $\beta_{AFP \times RTW}^h$  for different horizons. This coefficient measures the excess effect of RTW on unions with agency fee payers, relative to unions without agency fee payers that also experienced RTW in the sample. The blue line plots  $\beta_D^h$  for different horizons. This coefficient measures the effect of RTW on unions relative to unions that have never experienced RTW in the sample. Panel A plots the response of unions' membership. Panel B plots the response of unions' net worth. Panel C plots the change in the change of unions' disbursements. Panel D plots the change in unions' revenue. 95% confidence intervals reported. Sample spans from 2007 to 2019.

*Sources:* DOL, FMCS, and author's calculations.

security to states that experience RTW and other reductions in labor protection, which they call financial solidarity. These results show that financial solidarity between unions occurs within states as well. That said, additional contributions of members and affiliates does not offset the income loss associated with RTW.

While unions without agency fee payers do not experience a statistically or economically significant decrease in revenues after RTW, they exhibit a strategic shift in their spending

**Figure V – Union Responses to RTW reforms (Additional Effects)**



*Notes:* This plot contains four subplots, which report the estimation results of an exposure local projection difference in differences estimator from [Dube et al. \(2022\)](#). Standard errors are clustered at the state and year level. Nominal variables are reported in 2009 U.S. dollars. Estimates are the percentage point change in the variable. The red line plots  $\beta_{AFP \times D}^h$  for different horizons. This coefficient measures the excess effect of RTW on unions with agency fee payers, relative to unions without agency fee payers that also experienced RTW in the sample. The blue line plots  $\beta_{RTW}^h$  for different horizons. This coefficient measures the effect of RTW on unions relative to unions that have never experienced RTW in the sample. Panel A plots the response of unions' spending on political causes. Panel B plots the response of unions' spending on representational services. Panel C plots the change in the change of the share of membership contributes of total union revenue. Panel D plots the change in the share of affiliate contributions of total union revenue. 95% confidence intervals reported. Sample spans from 2007 to 2019.

*Sources:* DOL, FMCS, and author's calculations.

allocation. Following RTW, the overall political donations of all unions experienced a persistent 10 percent decline over 5 years. This shift in unions' strategy, originally documented by [Zullo \(2020\)](#), suggests that unions' strategies depend on factors outside of their financial health. Rather, union strategy may depend more on the organizing incentives faced by unions subject to different labor regulations and political environments.



## 4 Analysis of Leverage and Unionization

In this section, I estimate the relationship between union income and firm leverage using repeated cross-sections of firms identified from 2000-2019. This section proceeds in three steps. First, I discuss the data limitations that motivate this approach and the assumptions present in this analysis. Second, I estimate the cross-sectional semi-elasticity of firm leverage with respect to unions' income. Third, I use these estimates and estimates of the effect of RTW on unions' income from the previous section to conduct a counterfactual analysis of the effect of RTW on firms' leverage.

### 4.1 Identification

I use a fixed-effects panel specification to estimate the cross-sectional semi-elasticity of firms' leverage with respect to unions' income and membership. Using my novel data set, I innovate on previous estimates of the effect of RTW on firms' leverage. Specifically, I use a panel regression to estimate the cross-sectional relationship between firms' leverage and unions' finances without assuming that firms' exposure to unionization is limited to the location of their executive offices. Later, I will combine these cross-sectional estimates with the LP-DD estimates from the previous section to construct a counterfactual estimate of the effect of RTW on firms' leverage. The counterfactual estimate could also be interpreted as the expected change in leverage for firms' with agency-shop agreements following RTW.

This panel specification assumes that firms allocation of business activity is fixed in the short run, across states and bargaining units. Firms' production, financing options, and exposure to labor regulations vary across states, years, and industries. As such, I include fixed effects at the state, year, and 2-digit NAICS level. Information on firms' location is limited to the location of firms' executive offices. As prior studies show that firms' size, investment opportunities, capital tangibility, and profitability determine capital structure, I include these variables as controls in the panel regression. Firms may adjust the location of production across states or within states based on collective bargaining outcomes. This behavior can take the form of simply relocating production to another establishment operated by the firm or sub-contracting production to another firm. If this behavior occurs, the fixed effects of the

regression would be misspecified, as the union variables would not longer represent a firm's relative exposure to local unions. These behaviors are untestable in this data set. If they were to occur, we should expect that the estimates of the semi-elasticity of firms' leverage to unions' receipts would be biased downward, as firms' are incentivized to shift operations away from unionized workers when possible. This attenuation bias would result from union receipts affecting a smaller share of overall production.

This approach deviates from prior corporate finance research on the affect of workers' bargaining power on firms' financing decision. Prior work measure the effect of workers' bargaining power on firms' leverage using firm-level policy variation in the state wherein a firms' executive offices are located (see [Chava \*et al.\* \(2020\)](#) [Matsa \(2010\)](#)). This prior approach does not capture the firms' exposure to fluctuations in unions' bargaining power across states. Firms' business activity is seldom limited to the confines of the state in which their executive offices are located. Moreover, firms' may reallocate activity across establishments over time in response to changes in state-level changes in labor regulations. As such, data limitations preclude sorting firms into treatment and control groups.

The underlying assumptions of the panel specification are consistent with the assumptions required for the subsequent counterfactual analysis. The effect of RTW on unions' income measured in the previous section is for local unions that reported agency fee payers prior to the enactment of RTW. As such, the counterfactual analysis is consistent under the following set of assumptions. First, the firm's operations are limited to one state that unexpectedly enacts RTW. Second, the firm does not change their allocation of their production activities in the short-run. Third, the firm allowed for the mandatory collection of agency fees prior to the passage of RTW. While these assumptions provide a stylized counterfactual estimate, they provide a useful estimate of the effect of RTW on firms' leverage.

## 4.2 Cross-sectional Estimates

For this exercise, I add measures of union income and membership into a typical leverage panel regression with financial controls. I estimate the following baseline specification:

$$(2) \quad Lev_{i,t} = \alpha_t + \alpha_n + \alpha_s + \beta_{REC} REC_{i,t} + \beta_{MEM} MEM_{i,t} + \beta_x \mathbf{X}_{i,t} + \varepsilon_{i,t},$$

where  $REC_{i,t}$  is the log annual total receipts of unions associated with firm  $i$ , and  $MEM_{i,t}$  is the membership rate of union members to employees of firm  $i$ . I include state  $\alpha_s$ , NAICS 2-digit industry  $\alpha_n$ , and year  $\alpha_t$  fixed effects. Standard errors are clustered at the state and year level. The vector of financial controls  $\mathbf{X}_{it}$  includes log sales, asset tangibility, Tobin's  $q$ , and profitability.

Table VIII reports the relationship between firm leverage and union balance sheets and membership rates using a standard fixed effects panel estimator. The left hand side variable is leverage measured as the ratio of a firm's total debt to assets. There is a strong relationship between union revenue and firm leverage. A one percent increase in union receipts increases firm leverage by 110 basis points. Union revenue and membership determine the use of leverage; however, union revenue has a stronger relationship with firm leverage. Controlling for the membership rate of a firm, a one percent increase in log union receipts increases firm leverage by 90 basis points, while the effect of membership is economically small and statistically insignificant. Column 3 shows a one percent increase in the membership rate increases leverage by 13.4 basis points. This result likely follows from the contemporaneous correlation between dues and membership.

In addition to contemporaneous correlation between dues and membership, membership rates shape incentives faced by union organizers in the administering the collection of dues. Unions with higher membership rates may be able to organize members to contribute a greater share of their income to the union, as they face a lower risk of members leaving the organization. Unions with higher membership may also be able to ensure that their employer deducts dues from employees' paychecks, thereby increasing the rate of dues deductions within the bargaining unit. If the employer refuses to deduct dues, unions must set up separate contributions systems at the union's expense, which require more staff and volunteer hours to manage and provides a less reliable stream of income. Union receipts may also correlate with members' wages. Wages are an outcome of union bargaining power, labor productivity, and firm financial health. Labor productivity affects union bargaining power and union income through different channels. First, more productive workers pay more dues to their unions on average, as most locals collect a percentage of workers' wages as dues. Workers with higher wages may also be able to provide more organizing hours to the union,

**Table VIII** – Leverage Regression Estimates: Baseline Specifications

	1	2	3	4	5
Log Receipts	0.01107** (0.00472)	0.00901* (0.00505)			
Membership Rate		0.00039 (0.00067)	0.00134** (0.00060)	0.00096* (0.00049)	0.00086 (0.00060)
Receipts/Members				0.22483 (0.21635)	
Receipts					0.00006* (0.00004)
Log Sales	0.00843* (0.00465)	0.00846* (0.00463)	0.00874* (0.00461)	0.00861* (0.00463)	0.00860* (0.00462)
Tobin's Q	0.02907*** (0.00132)	0.02907*** (0.00132)	0.02907*** (0.00132)	0.02907*** (0.00132)	0.02907*** (0.00132)
Tangibility	0.35428*** (0.05070)	0.35424*** (0.05077)	0.35465*** (0.05085)	0.35419*** (0.05072)	0.35491*** (0.05078)
Profitability	-0.35144*** (0.00613)	-0.35146*** (0.00622)	-0.35165*** (0.00615)	-0.35157*** (0.00623)	-0.35154*** (0.00612)
R-squared	0.422	0.422	0.422	0.422	0.422
Observations	65943	65943	65943	65943	65943

*Notes:* This table reports leverage panel regression results. Standard errors are clustered at the state and industry level. These regressions are estimated on the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FMCS. Table reports firm financial data from Compustat.

*Sources:* DOL, FMCS, Compustat, and author's calculations.

as they are less likely to hold additional employment. Second, more productive workers may have greater bargaining power with the firm. More productive workers may be more capable of disrupting production with a labor dispute.

To address these considerations, I consider an alternative specification that regresses firm leverage on the membership rate and the ratio of total receipts to membership. A ratio of total receipts to membership of \$10,000 is associated with a statistically insignificant 0.225 increase in leverage. The effect of membership is significant, as a 1 percent increase in membership increases leverage by 9.6 basis points. These results suggest that a union's total income is a stronger determinant of firm leverage than a union's income per member. This relationship has a number of possible explanations. First, great membership and income allows unions to cover overhead, legal expenses, or wages distributed to a union's staff and officer wages. Second, this relationship could suggest that unions' receipts and membership rate proxy a unions' size, and these effects capture a unions' size effect on a firm's leverage.

**Table IX** – Leverage Regression Estimates: Additional Specifications

	6	7	8	9	10
Log Union Assets	0.01121** (0.00441)			0.00896* (0.00442)	
Membership Rate		0.00096 (0.00059)		0.00043 (0.00067)	
Net Worth		0.00006** (0.00002)	0.00010*** (0.00003)		
Voting Membership					0.11964 (0.07230)
Log Sales	0.00842* (0.00465)	0.00858* (0.00462)	0.00863* (0.00460)	0.00846* (0.00463)	0.00883* (0.00458)
Tobin's Q	0.02907*** (0.00132)	0.02907*** (0.00132)	0.02907*** (0.00132)	0.02907*** (0.00132)	0.02907*** (0.00132)
Tangibility	0.35430*** (0.05075)	0.35489*** (0.05087)	0.35561*** (0.05092)	0.35424*** (0.05079)	0.35502*** (0.05084)
Profitability	-0.35143*** (0.00612)	-0.35153*** (0.00611)	-0.35154*** (0.00614)	-0.35146*** (0.00616)	-0.35171*** (0.00615)
R-squared	0.422	0.422	0.422	0.422	0.422
Observations	65943	65943	65943	65943	65943

*Notes:* This table reports leverage panel regression results. Standard errors are clustered at the state and industry level. These regressions are estimated on the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FMCS. Table reports firm financial data from Compustat.

*Sources:* DOL, FMCS, Compustat, and author's calculations.

While these specifications are not able to evaluate these different explanations, they highlight the importance of union income for shaping firms' use of leverage as a strategic variable.

I also consider additional specifications that measure the relationship between firms' use of leverage and other measures of unions' membership and financial conditions. Table IX reports the estimates of these specifications. I estimate the cross-sectional relationship between unions' log assets and firms' leverage. There is a strong relationship between unions' log assets and firm leverage. A one percent increase in union assets increases firm leverage by 112 basis points. Controlling for the membership rate of a firm, a one percent increase in log union receipts increases firm leverage by 89 basis points, while the effect of membership is economically small and statistically insignificant. This point estimates coincide with the point estimates on log receipts in the baseline specification. As unions' assets and receipts are positively correlated, these point estimates suggest that these point estimates indicate the relationship between local unions' size and firms' use of leverage. That said, a large union

may face financial constraint. Unions' with greater net worth may exhibit greater ability to respond to members' organizational needs. I estimate the cross-sectional relationship between unions' net worth and firms' leverage. A \$100,000 increase in unions' net worth increases firm leverage by 6 basis points. These results, taken together, suggests that union's size and net worth codetermine firms' leverage.

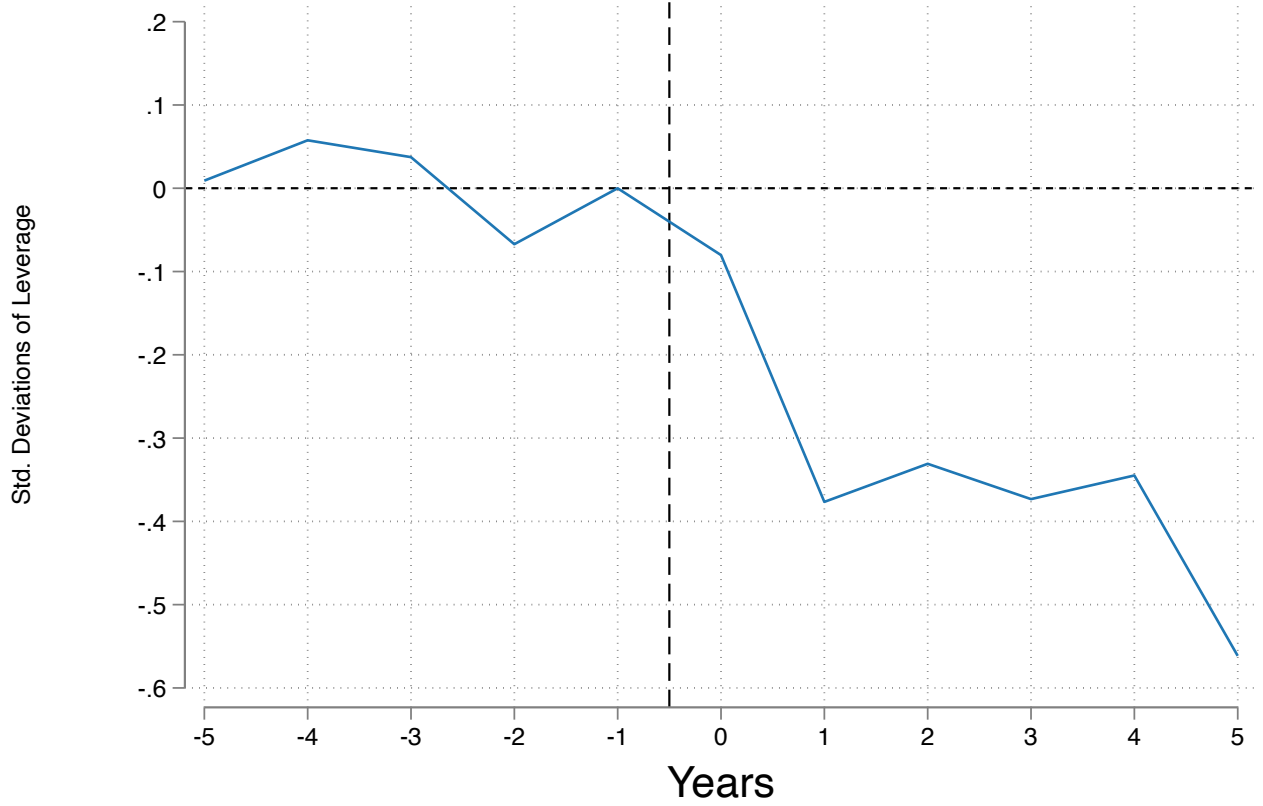
These estimates confirm and expand our understanding of the relationship of worker bargaining power and leverage. The relationship between membership rate and firm leverage is slightly larger than the coefficient estimated in [Matsa \(2010\)](#), which found that leverage is 10.2 basis points higher when an additional one percent of a firm is covered by a collective bargaining agreement. These two point estimates are not directly comparable. First, [Matsa \(2010\)](#) uses an earlier sample period of Compustat firms, during which the private sector unionization rate was higher. It follows that firms with unionized employees may now face a stronger incentive to raise leverage to maintain their competitiveness against other market participants without unionized employees. Second, [Matsa \(2010\)](#) regresses leverage on firms' coverage rate, defined as the share of a firm's workers covered by a collective bargaining agreement. not the membership rate. Membership rate, relative to the coverage rate, includes other features of union strength and capacity, not proxied by the coverage rate. That said, limitations of the available administrative data prevents further investigation of this relationship in this context.

### 4.3 Counterfactual Analysis

The relationship between union income and firm leverage highlights the importance of agency fees for union bargaining power within firms. A decrease in agency fee payers decreases the ratio of union receipts to members. Following RTW, union receipts decrease more than membership, decreasing the ratio of income to members. This decrease affects unions ability to perform outreach to members and to organize collective bargaining campaigns. Using the point estimates of the panel estimator and the causal estimates of the LP-DD estimator, we can construct a counterfactual effect of RTW on firm leverage. For the average firm in the sample, the decrease in exposed union income following RTW is associated with a 0.6 standard deviation decrease in firm leverage. This finding assumes that the average firms' activity

is limited to a state affected by the reforms included in the sample. Figure VI plots the counterfactual response. This result suggests that firms exposed to RTW face heterogeneous outcomes of the policy. Firms without agency shop agreements do not experience an increase in bargaining power related to diminished union income.

**Figure VI** – Counterfactual Effect of Union Receipts on Firm Leverage



*Notes:* This figure plots the counterfactual effect of RTW on firms' leverage if the firm's operations are limited to one state that experiences RTW and the firm's collective bargaining is limited to unions with agency fee payers. The point estimate plotted is the product of the revenue effect from the baseline LP-DD reported in Figure IV and the point estimate of union receipts in the first leverage regression in Table VIII.

*Sources:* DOL, FMCS, Compustat, and author's calculations.

This counterfactual estimate of the effect of RTW is consistent with findings by Chava *et al.* (2020) and Matsa (2010). Assuming firms' business activity is limited to the state in which their executive offices are located, Chava *et al.* (2020) finds a transitory effect of RTW on firm leverage, which bottoms out at -5 percentage points after four years. Using a two-way fixed effects estimator, Matsa (2010) finds that firms reduce leverage by 5 percentage points after the enactment of RTW.

## 5 Robustness

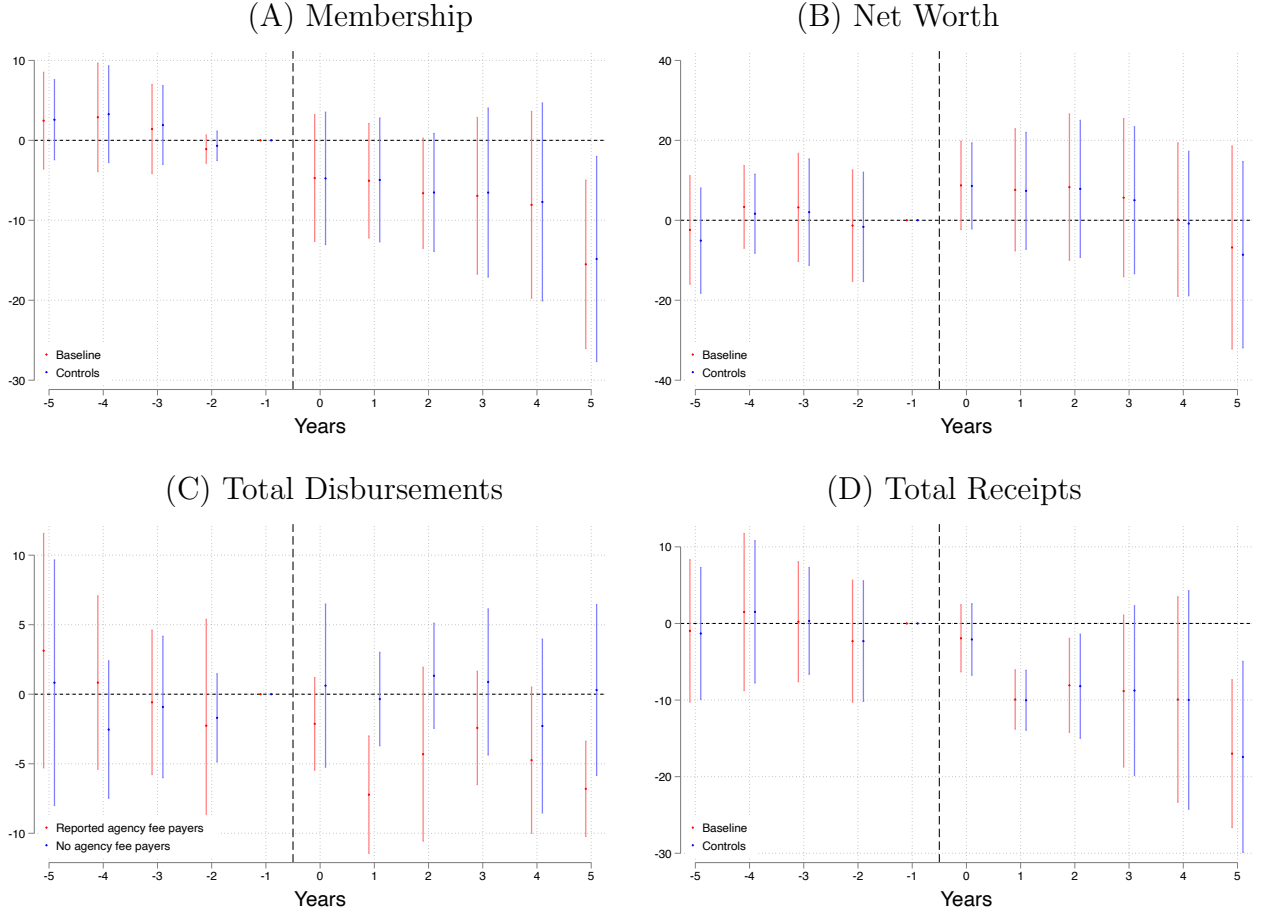
In this section, I evaluate the robustness of the LP-DD and panel estimates presented above. First, I evaluate whether the effect of RTW on unions' balance sheets is the result of differences in pre-reform membership, financial conditions, or labor federation. These characteristics shape unions' ability to organize and may affect their capacity to respond to changes in labor regulations. Second, I evaluate whether the effect of RTW on union membership is the result of unions misreporting agency fee payers as members by estimating the effect of RTW on two other measures on union membership reported in LM-2 filings. Third, I evaluate the effect of mismeasured firm characteristics in the leverage regressions using the [Erickson \*et al.\* \(2014\)](#) panel estimator, which provides a consistent point estimates when multiple regressors are mismeasured.

Figure [VII](#) and Figure [VIII](#) report the effect of RTW on unions' balance sheets and membership controlling for pre-reform log membership, log assets, and labor federation. The inclusion of these controls do not significantly affect the baseline estimates. As such, there is no evidence that the effect of RTW on unions' balance sheets varies across unions of different sizes or federation affiliations. This result is not at odds with [Zullo \(2020\)](#), which found that skilled trade unions were less affected by RTW, as they served a second role of providing occupational licensing. Rather, this result implies that all unions, regardless of their constituencies, face similar financial and organizational consequences of losing agency fee payers.

The results of the LP-DD estimates are also robust to alternative measures of union membership. Union membership is measured in two ways in LM-2 filings. First, unions report their total share of members. Second, unions report their share of voting members. This second field is not required and is not always filled out by a union's designated filer. Additionally, one may be concerned that unions are reporting agency fee payers twice, first as members and again as agency fee payers. To address these sources of mis-measurement, Figure [IX](#) presents estimates the baseline LP-DD specification of all three measures of active membership: total membership, voting membership, total membership less agency fee payers, alongside estimates of the effect of RTW on agency fee payers. The choice of membership



**Figure VII – Effect of RTW Laws: Additional Controls**



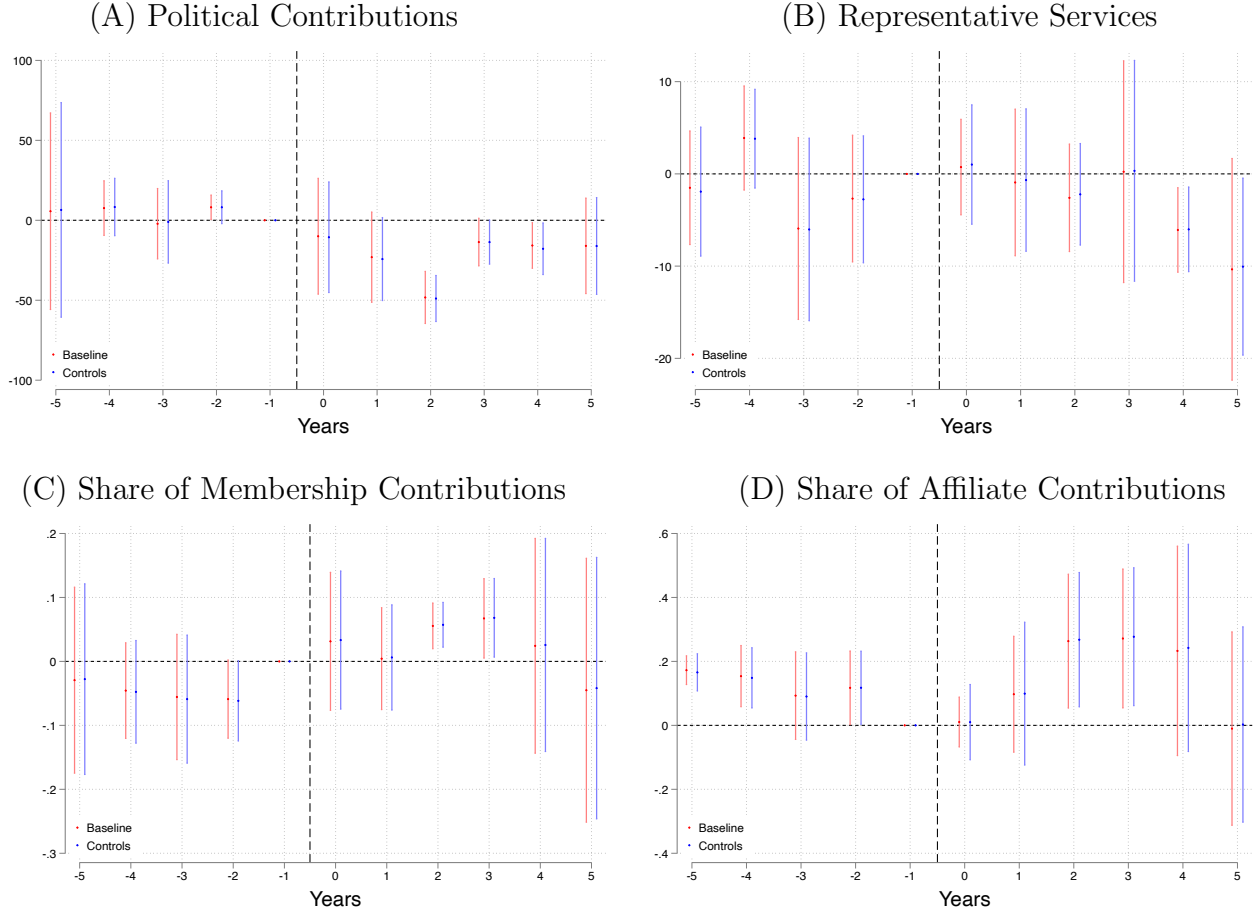
*Notes:* This plot contains four subplots, which report the estimation results of an exposure local projection difference in differences estimator from [Dube et al. \(2022\)](#). Standard errors are clustered at the state and year level. Nominal variables are reported in 2009 U.S. dollars. Estimates are the percentage point change in the variable. The red line plots  $\beta_{AFP \times D}^h$  for different horizons. This coefficient measures the excess effect of RTW on unions with agency fee payers, relative to unions without agency fee payers that also experienced RTW in the sample. Panel A plots the response of unions' membership. Panel B plots the response of unions' net worth. Panel C plots the change in the change of unions' disbursements. Panel D plots the change in unions' revenue. 95% confidence intervals reported. Sample spans from 2007 to 2019.

*Sources:* DOL, FMCS, and author's calculations.

measure has an insignificant effect on the statistical inference. In both alternative estimates, the response is consistent with the baseline estimate of total membership. The estimates of all measures of active membership do not conform to the effect of RTW on agency fee payers, which decreases upon impact.

As firms' asset tangibility and Tobin's q may be mismeasured, I also estimate the panel specifications using the [Erickson et al. \(2014\)](#) panel estimator. I present the estimates of

**Figure VIII** – Effect of RTW Laws: Additional Controls (Additional Effects)



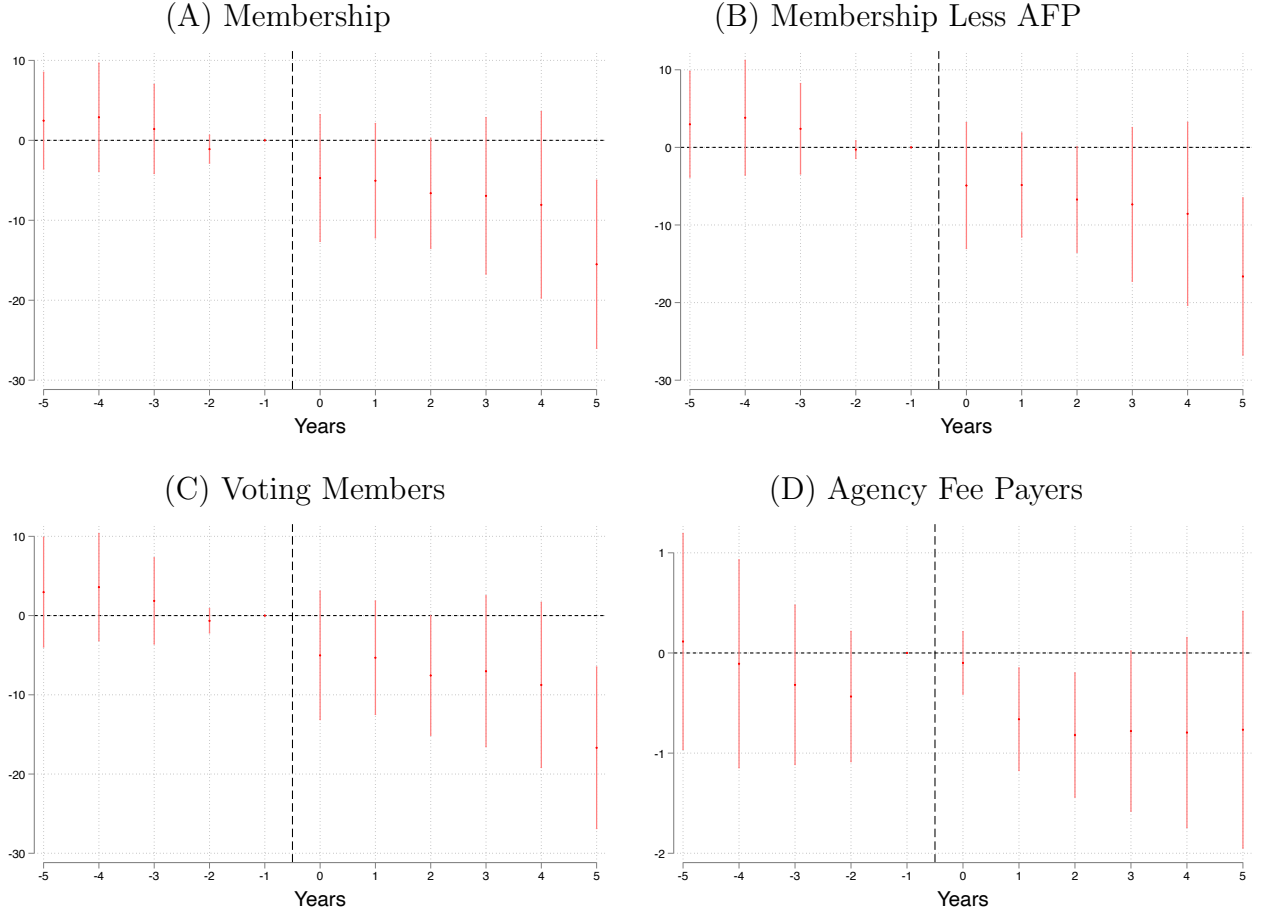
*Notes:* This plot contains four subplots, which report the estimation results of an exposure local projection difference in differences estimator from [Dube \*et al.\* \(2022\)](#). Standard errors are clustered at the state and year level. Nominal variables are reported in 2009 U.S. dollars. Estimates are the percentage point change in the variable. The red line plots  $\beta_{AFP \times D}^h$  for different horizons. This coefficient measures the excess effect of RTW on unions with agency fee payers, relative to unions without agency fee payers that also experienced RTW in the sample. Panel A plots the response of unions' spending on political causes. Panel B plots the response of unions' spending on representational services. Panel C plots the change in the change of the share of membership contributes of total union revenue. Panel D plots the change in the share of affiliate contributions of total union revenue. Sample spans from 2007 to 2019.

*Sources:* DOL, FMCS, and author's calculations.

different specifications of this estimator for the leverage regression of unions' membership and firms' financial controls for different measures of firms' financial health. Table [XI](#) reports estimates for the leverage regressions on log union receipts; Table [X](#) reports estimates for the leverage regressions on total union receipts; And, Table [XII](#) reports estimates for the leverage regressions on union net worth.

Correcting for measurement error decreases the effect of log receipts and membership on

**Figure IX – Union Responses to RTW Reforms: Alternative Membership Measures**



*Notes:* This plot contains four subplots, which report the estimation results of an exposure local projection difference in differences estimator from [Dube \*et al.\* \(2022\)](#). Standard errors are clustered at the state and year level. Nominal variables are reported in 2009 U.S. dollars. Estimates are the percentage point change in the variable. The red line plots  $\beta_{AFP \times D}^h$  for different horizons. This coefficient measures the excess effect of RTW on unions with agency fee payers, relative to unions without agency fee payers that also experienced RTW in the sample. Panel A plots the response of unions' membership. Panel B plots the response of unions' membership less agency fee payers. Panel C plots the change in the change of unions' voting membership. Panel D plots the change in unions' agency fee payers. 95% confidence intervals reported. Sample spans from 2007 to 2019.

*Sources:* DOL, FMCS, and author's calculations.

firm leverage. This is an expected outcome, as firm size and tangibility correlate with union's size and financial health. Historically, union density and membership has been higher in manufacturing and resource extraction industries. As such, the estimator should decrease the size of coefficients on union coefficients. While the point estimates on log receipts is insignificant for all specifications, the point estimates for total receipts are significant. That said, the effect of net worth, while smaller is significant for most specifications. In all specifications,

the effect of the membership rate on firm leverage remains insignificant.

**Table X** – Error-in-Variables Leverage Regressions with Union Receipts

	Measurement Error in q			Measurement Error in q & Tangibility		
	3rd Order	4th Order	5th Order	3rd Order	4th Order	5th Order
Receipts	0.0002* (0.0001)	0.0001* (0.0001)	0.0001* (0.0001)	0.0004 (0.0002)	0.0002** (0.0001)	0.0002*** (0.0001)
Member Rate	0.0011 (0.0010)	0.0010 (0.0007)	0.0010 (0.0007)	-0.0051 (0.0062)	-0.0033 (0.0031)	-0.0024 (0.0017)
Tobin's Q	0.2006*** (0.0536)	0.0887*** (0.0051)	0.0979*** (0.0039)	0.1634*** (0.0383)	0.0880*** (0.0045)	0.0968*** (0.0035)
Tangibility	0.3539*** (0.0743)	0.3364*** (0.0394)	0.3378*** (0.0416)	5.2832 (4.4917)	3.7723* (1.9747)	3.0229*** (0.7061)
Log Sales	0.0064 (0.0076)	0.0081* (0.0044)	0.0080* (0.0046)	-0.0407 (0.0449)	-0.0251 (0.0203)	-0.0180** (0.0089)
Profitability	0.8358** (0.3827)	0.0610 (0.0372)	0.1252*** (0.0391)	0.6156** (0.2783)	0.0830** (0.0415)	0.1382*** (0.0396)
Observations	59963	59963	59963	59963	59963	59963
$\rho^2$	0.7173	0.5224	0.5386	0.6949	0.5509	0.5598
J-Statistic	0.0000	20.5894	49.9437	3.7418	26.3393	71.7737
$\tau - q$	0.5625	0.6538	0.6383	0.5800	0.6562	0.6409
$SE_{\tau} - q$	0.0392	0.0383	0.0368	0.0400	0.0383	0.0368
$\tau - \text{Tangibility}$	.	.	.	0.0823	0.1051	0.1273
$SE_{\tau} - \text{Tangibility}$	.	.	.	0.0577	0.0502	0.0317

*Notes:* This table reports leverage panel regression results from the higher-order cumulant estimators of [Erickson et al. \(2014\)](#). The first three columns report regression results where firms' market to book ratio is assumed to be measured with error. The columns report the higher-order cumulant estimator used in the estimator. The fourth, fifth, and sixth columns report regression results where firms' market to book ratio and their tangibility is assumed to be measured with error.  $\rho^2$  reports an estimate of the  $R^2$  of the regression.  $\tau_i$  reports the measurement quality for proxy variable  $i$ . J-statistic reports the model over-identifying statistic from [Sargan \(1958\)](#). These regressions are estimated on the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FMCS. Table reports firm financial data from Compustat.

*Sources:* DOL, FMCS, Compustat, and author's calculations.

**Table XI** – Error-in-Variables Leverage Regressions with Union Log Receipts

	Measurement Error in q			Measurement Error in q & Tangibility		
	3rd Order	4th Order	5th Order	3rd Order	4th Order	5th Order
Log Receipts	0.0071 (0.0112)	0.0078 (0.0066)	0.0077 (0.0069)	-0.0166 (0.0310)	-0.0089 (0.0182)	-0.0053 (0.0136)
Member Rate	0.0016 (0.0012)	0.0010 (0.0008)	0.0010 (0.0008)	-0.0008 (0.0036)	-0.0005 (0.0023)	-0.0002 (0.0018)
Tobin's Q	0.2006*** (0.0536)	0.0887*** (0.0051)	0.0979*** (0.0039)	0.1633*** (0.0383)	0.0880*** (0.0045)	0.0968*** (0.0035)
Tangibility	0.3529*** (0.0743)	0.3357*** (0.0394)	0.3371*** (0.0416)	5.2685 (4.5128)	3.7667* (1.9756)	3.0106*** (0.7065)
Log Sales	0.0066 (0.0076)	0.0081* (0.0045)	0.0080* (0.0047)	-0.0394 (0.0441)	-0.0243 (0.0199)	-0.0173* (0.0088)
Profitability	0.8358** (0.3827)	0.0609 (0.0372)	0.1252*** (0.0391)	0.6144** (0.2781)	0.0822** (0.0414)	0.1376*** (0.0396)
Observations	59963	59963	59963	59963	59963	59963
$\rho^2$	0.7173	0.5224	0.5386	0.6947	0.5508	0.5596
J-Statistic	0.0000	20.5914	49.9538	3.7429	26.3412	71.9027
$\tau - q$	0.5625	0.6538	0.6383	0.5800	0.6562	0.6409
$SE_{\tau} - q$	0.0392	0.0383	0.0368	0.0399	0.0383	0.0368
$\tau - \text{Tangibility}$	.	.	.	0.0823	0.1051	0.1275
$SE_{\tau} - \text{Tangibility}$	.	.	.	0.0581	0.0503	0.0319

*Notes:* This table reports leverage panel regression results from the higher-order cumulant estimators of [Erickson et al. \(2014\)](#). The first three columns report regression results where firms' market to book ratio is assumed to be measured with error. The columns report the higher-order cumulant estimator used in the estimator. The fourth, fifth, and sixth columns report regression results where firms' market to book ratio and their tangibility is assumed to be measured with error.  $\rho^2$  reports an estimate of the  $R^2$  of the regression.  $\tau_i$  reports the measurement quality for proxy variable  $i$ . J-statistic reports the model over-identifying statistic from [Sargan \(1958\)](#). These regressions are estimated on the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FMCS. Table reports firm financial data from Compustat.

*Sources:* DOL, FMCS, Compustat, and author's calculations.

**Table XII** – Error-in-Variables Leverage Regressions with Union Net Worth

	Measurement Error in q			Measurement Error in q & Tangibility		
	3rd Order	4th Order	5th Order	3rd Order	4th Order	5th Order
Member Rate	0.0013 (0.0010)	0.0012* (0.0007)	0.0012* (0.0007)	-0.0035 (0.0050)	-0.0021 (0.0026)	-0.0014 (0.0016)
Net Worth	0.0002 (0.0001)	0.0001* (0.0001)	0.0001* (0.0001)	0.0002 (0.0001)	0.0001 (0.0001)	0.0001 (0.0001)
Tobin's Q	0.2006*** (0.0536)	0.0887*** (0.0051)	0.0979*** (0.0039)	0.1633*** (0.0383)	0.0880*** (0.0045)	0.0968*** (0.0035)
Tangibility	0.3532*** (0.0742)	0.3360*** (0.0394)	0.3374*** (0.0416)	5.2863 (4.4987)	3.7663* (1.9732)	3.0179*** (0.7061)
Log Sales	0.0064 (0.0076)	0.0081* (0.0044)	0.0080* (0.0046)	-0.0404 (0.0446)	-0.0248 (0.0201)	-0.0177** (0.0089)
Profitability	0.8358** (0.3827)	0.0610 (0.0372)	0.1252*** (0.0391)	0.6151** (0.2781)	0.0827** (0.0415)	0.1379*** (0.0396)
Observations	59963	59963	59963	59963	59963	59963
$\rho^2$	0.7173	0.5224	0.5386	0.6948	0.5508	0.5597
J-Statistic	0.0000	20.5874	49.9393	3.7452	26.3437	71.8084
$\tau - q$	0.5625	0.6538	0.6383	0.5800	0.6562	0.6409
$SE_{\tau} - q$	0.0392	0.0383	0.0368	0.0400	0.0383	0.0368
$\tau - \text{Tangibility}$	.	.	.	0.0820	0.1050	0.1272
$SE_{\tau} - \text{Tangibility}$	.	.	.	0.0576	0.0503	0.0318

*Notes:* This table reports leverage panel regression results from the higher-order cumulant estimators of [Erickson et al. \(2014\)](#). The first three columns report regression results where firms' market to book ratio is assumed to be measured with error. The columns report the higher-order cumulant estimator used in the estimator. The fourth, fifth, and sixth columns report regression results where firms' market to book ratio and their tangibility is assumed to be measured with error.  $\rho^2$  reports an estimate of the  $R^2$  of the regression.  $\tau_i$  reports the measurement quality for proxy variable  $i$ . J-statistic reports the model over-identifying statistic from [Sargan \(1958\)](#). These regressions are estimated on the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FMCS. Table reports firm financial data from Compustat.

*Sources:* DOL, FMCS, Compustat, and author's calculations.

## 6 Conclusion

The effect of RTW is large and persistent for unions and firms with agency shop agreements. Using a differential exposure design, I show that unions with agency fee payers lose 15 percent of their total receipts and 16 percent of their membership over five years, relative to unions without agency fee payers. A panel regression estimated on a novel data set that links local unions' balance sheet data to firms' balance sheet data shows that unions' financial conditions shape firms' financing decisions. A one percent increase in union receipts increases firm leverage by 110 basis points. These two research designs allow for a counterfactual analysis. This counterfactual analysis shows that, for the average firm in the sample, the decrease in exposed union income following with RTW is associated with a 0.5 standard deviation decrease in firm leverage, assuming that the firms' activity is limited to a state affected by the reforms included in the sample and the firms' allocation of activity remains fixed in the short run. The counterfactual effect of RTW on firms' leverage is consistent with prior estimates found by [Matsa \(2010\)](#) and [Chava \*et al.\* \(2020\)](#).

Labor unions' financial conditions determine the effect of labor regulations on firms' capital structure. Unions' financial health is not only central to their capacity to organize and retain membership, but also to their bargaining power in negotiations with firms. As unions' revenues shape the strategic use of debt by firms, so too may the financial health of other input suppliers affect a firm's capital structure. Future work should consider the effect of supplier's revenue and net worth on firms' financing decisions.

The heterogenous response of labor federations to RTW suggests the importance of labor federations in unions' membership dynamics. Local unions partially offset the loss of income from RTW through financial transfers within state and national labor federations. The presence of these transfers presents another question: Is the fiscal solidarity provided by national federations optimal? Labor federations often allocate funding across political lobbying, external consultancies, and state-level organizing differently. Future work should consider the determinants of spending patterns within federations, and whether these spending patterns are responsive to the organizational needs of their affiliates.



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## 7 Construction of Linked Panel

In this appendix, I discuss the construction of the linked union and firm panel. I begin with a discussion of the data sources that I use to construct the crosswalk between firms and local unions. I then discuss the construction of the crosswalk.

When a firm or union intends to engage in collective bargaining, whether they intended to bargain for a new contract or renegotiate a current contract, they must file a notice to FMCS of upcoming collective bargaining (F7). As such, these administrative data provide the universe of collective bargaining activities for a given period. The sample of F7 filings spans from 2015 to 2022, and the sample of DOL collective bargaining agreements data set (hereafter, CBA) spans agreements with expiration dates from 1969 to 2029. These data sets were accessed electronically on December 21, 2022.

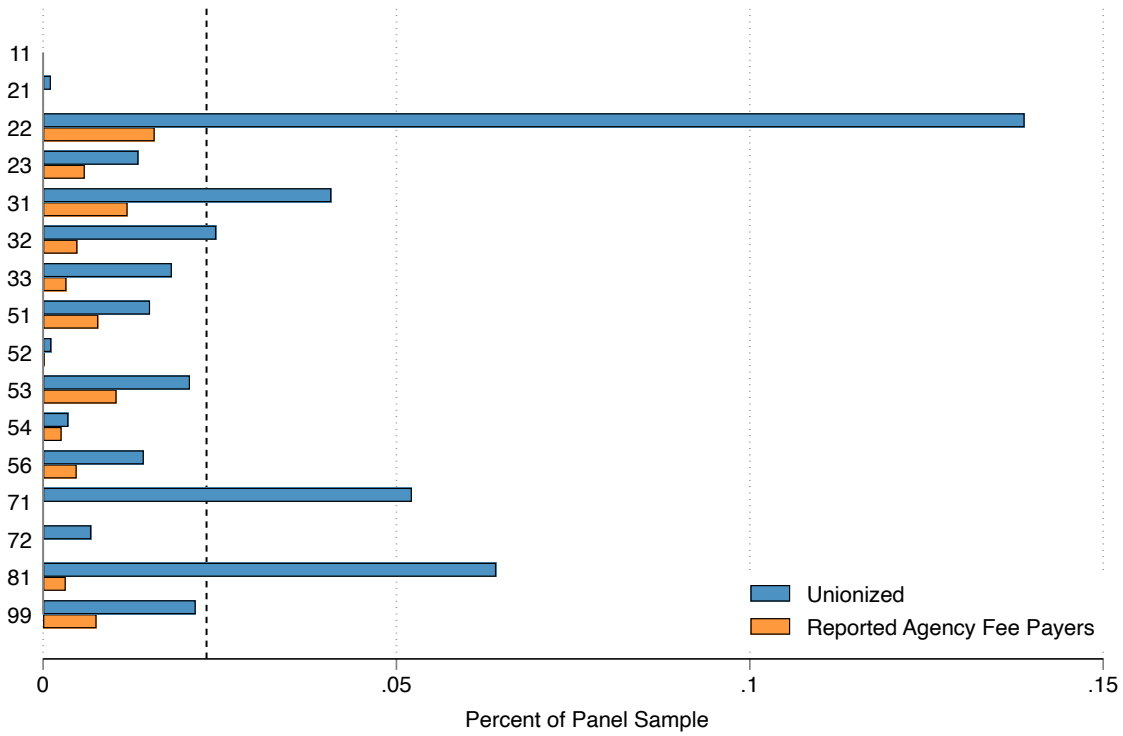
First, I use the fuzzy matching algorithm of [Raffo and Lhuillery \(2009\)](#) to construct a crosswalk between firms in Compustat and their collective bargaining records of FCMS and DOL. Before I apply the fuzzy matching algorithm, I construct stems for each firm name in each dataset. The process of stemming text strings is standard data cleaning process used in text analysis, which reduces strings to a string of stems. Stems are not necessarily the morphological root of each word, but often provide sufficient information on the meaning of each word. Fuzzy matching without stemming increases the false positive rate of the algorithm, as derivations of different words may share a large number of characters. Company names in Compustat often share entire words that do not uniquely identify the company (e.g. industries, ventures, international), so stemming is necessary to recover accurate matches. For this process, I remove all spaces, articles, conjunctions, and symbols from firm names. I also remove all firm name suffixes from Compustat names that represent the type of non-trading firm a record represents. I also remove the following words that do not uniquely identify firms: company, industries, ventures, technology, restaurant, technologies, corp, communications, energy, services, construction, enterprises, casino, holdings, pharmaceuticals. I record all firm name matches with above an 85% match rate. This process creates two crosswalks: the Compustat-F7 crosswalk and the Compustat-CBA crosswalk. Of the 2981 unique employer names listed in the DOL collective bargaining agreement sample, this approach identified

unique 118 Compustat firm matches. For the FCMS sample, this approach identified 335 Compustat firms of 25593 unique employer names. I then combine these two crosswalks into one Compustat-union crosswalk that uniquely identifies 436 Compustat firms with exposure to unions

Second, I match LM-2 filings to these two crosswalks using union affiliate abbreviation and local number. Local numbers are unique identifiers assigned to local unions within affiliate unions during the local’s recognition process. I verify each match by hand. This crosswalk identifies the link between 322 Compustat firms and 737 local unions. All local unions matched to the crosswalk uniquely identify firms. I exclude 114 Compustat firms that were listed on FCMS or DOL filings that did not match with LM-2 filings. This match rate could be the result of firms negotiating with local unions with less than \$250,000 dollars in total receipts, intermediate labor organizations, or national labor organizations. As these data do not provide information on the firms’ exposure to local unions revenue and membership, I exclude them from the analysis. Table XIII reports the summary statistics for Compustat firms matched with local unions in the sample. Most Compustat firms only match with one local union. The firms that match with multiple unions include the Big 3 automobile manufacturers, steel manufacturers, and aerospace engineering firms. Table I reports the summary statistics of all firms in the matched sample. Table XIV reports the summary statistics of all unions in the sample. Table XV reports the summary statistics of unions with and without agency fee payers in this sample. Figure X plots the share of firms in each industry that is unionized. The distribution of unionized firms across industries conforms to expectations, with the largest shares in manufacturing and mining. The relative financial health and spending patterns of unions with and without agency fee payers persists in this sample. Unions with agency fee payers are larger, reporting greater income, assets, and members. They also spend more on representational services, political action, and general overhead.

While this data set does not capture the universe of local union and firm linkages, it provides a useful sample for analyzing the relationship between local union balance sheets and firm income. As this data set likely does not include all local union-firm pairs, one should expect that the estimates using this data set may exhibit bias.

**Figure X** – Industry Share of Unionized Firms in Matched Sample



*Notes:* This figure plots industry share of unionized firms in matched LM-2 and Compustat sample. The dotted line plots the sample average. Sample ranges from 2000-2019.

*Sources:* DOL, FCMS, Compustat and author's calculations.

## 8 Membership Trends Across States

In this appendix, I present additional information on trends in agency fee payers across states in the sample period. The share of unions with reported agency fee payers and the ratio of agency fee payers to members summarize the uptake of agency fee payment in unions. That said, one may be interested in general trends in members and agency fee payers across certain cross-sections. The results of this section provide additional support for the findings in the main paper.

Table XVI reports the average membership level and standard error of the average outside of the treatment groups. This table shows that the average union in the sample experienced an increase in membership and decrease in agency fee payers in states which allow agency shop clauses over the sample period. In states with RTW policies in effect, the size of unions varied insignificantly, while the number of agency fee payers increased slightly. Table XVII

**Table XIII** – Local Unions Matched to Compustat Firms

	Mean	Std. Dev.	5 <sup>th</sup>	10 <sup>th</sup>	Median	90 <sup>th</sup>	95 <sup>th</sup>
Local Unions	2.289	3.575	1	1	1	5	6
Observations	322						

*Notes:* This table reports the count of local unions with annual receipts above \$250,000 that matched with each Compustat firm. The sample spans from 2000 to 2019.

**Table XIV** – Unions in Matched Panel

	Mean	Std. Dev.	10 <sup>th</sup>	25 <sup>th</sup>	Median	75 <sup>th</sup>	90 <sup>th</sup>
Receipts	361.00	744.84	32.94	58.39	126.28	322.60	854.31
Disbursements	354.23	744.71	32.66	57.19	124.06	320.02	833.41
Assets	363.35	827.79	18.18	41.81	113.75	315.61	784.49
Liabilities	37.58	169.02	0.00	0.05	2.42	14.56	67.47
Net Worth	320.28	699.56	13.63	36.43	100.86	290.15	717.08
Members	3609.48	6144.54	435.00	804.00	1564.00	3600.00	7743.00
AFP Rate	0.63	2.26	0.00	0.00	0.00	0.02	1.25
Rec/Members	0.15	0.69	0.04	0.05	0.08	0.12	0.22
Representation	84.70	159.35	5.07	13.62	31.56	84.76	212.39
Political	4.82	16.18	0.00	0.00	0.46	2.46	10.09
Overhead	43.51	103.83	1.31	5.17	14.28	35.41	94.85
Observations	6043						

*Notes:* This table reports summary statistics for unions in the matched LM-2 and Compustat sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2000-2019. Merged sample constructed by matching unions to firms using F7 and collective bargaining reports compiled by the DOL and FCMS. Table reports union financial and membership data at the union level.

*Sources:* DOL, FCMS, Compustat and author's calculations.

reports the membership trends in treatment groups in the years preceding and following RTW. The average union in the treatment groups did not experience a significant variation in membership after RTW, while agency fee payers fell significantly in all states, at least temporarily. Now, as this table reports the average across all unions in a specific cross-section, it does not consider the trends specific to unions that report agency fee payers.

Table XVIII reports the membership trends in unions with reported with agency fee payers outside of the treatment groups. This table shows that the average union with reported agency fee payers in the sample experienced an insignificant increase in membership and decrease in agency fee payers in states which allow agency shop clauses over the sample

**Table XV** – Unions with and without Agency Fee Payers in Matched Panel

	Reported Agency Fee Payers			No Agency Fee Payers		
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median
Receipts	516.60	888.41	235.65	304.68	676.86	101.71
Disbursements	509.41	893.19	232.46	298.06	674.43	99.68
Assets	468.85	1012.36	133.48	325.17	746.33	105.91
Liabilities	61.44	207.01	4.70	28.94	152.07	1.98
Net Worth	390.58	832.42	109.22	294.84	642.97	98.48
Members	5896.56	7758.10	3157.00	2781.66	5202.71	1310.00
Rec/Members	0.09	0.06	0.07	0.17	0.80	0.08
Representation	140.68	210.20	66.55	64.43	130.58	24.89
Political	8.47	22.56	1.12	3.50	12.88	0.31
Overhead	65.26	135.72	22.13	35.64	88.24	11.92
Observations:	1606			4437		

*Notes:* This table reports summary statistics for local unions without and without agency fee payers in the baseline LM-2 sample. Variables are winsorized at the 0.5% level. Nominal variables are reported in 2009 U.S. dollars and divided by 10,000. Sample ranges from 2007-2019.

*Sources:* DOL, FCMS, and author's calculations.

period. In states with RTW policies in effect, unions with agency fee payers experience an insignificant increase in membership while the number of agency fee payers significantly. The increase in agency fee payers is either the result of voluntary agency fee payers or misreporting. An increase in voluntary agency fee payers suggests a lack of organizing capacity at the union level or a shift in workers preferences for union representation. Table XIX reports the membership trends in unions with reported with agency fee payers in treatment groups in the years preceding and following RTW. The average union in the treatment groups experience a decrease in members and agency fee payers in all states, at least temporarily, although it is statistically insignificant. In general, changes in average membership across unions does not capture changes in average union's bargaining power, as the membership rate varies across unions and across firms.

**Table XVI** – Trends in Membership Outside of Treatment Groups

Year	Members			AFP		
	Total	RTW Never	RTW Pre-2007	Total	RTW Never	RTW Pre-2007
2007	2503.5 (122.2)	2806.9 (158.2)	1572.4 (102.4)	95.0 (24.8)	123.2 (32.9)	8.5 (2.8)
2008	2560.8 (130.1)	2884.3 (170.3)	1612.4 (105.3)	80.0 (19.5)	104.0 (26.1)	9.5 (3.4)
2009	2549.2 (129.6)	2877.2 (169.4)	1582.2 (105.4)	113.7 (36.7)	149.6 (49.2)	8.0 (2.2)
2010	2640.2 (145.3)	2951.8 (186.2)	1659.5 (134.0)	144.4 (42.4)	186.3 (55.9)	12.5 (3.4)
2011	2463.1 (126.5)	2781.6 (164.7)	1516.0 (106.3)	133.5 (37.3)	168.3 (49.5)	29.7 (16.8)
2012	2479.4 (129.4)	2795.6 (168.2)	1531.7 (108.4)	127.7 (36.2)	167.3 (48.2)	9.1 (2.3)
2013	2508.5 (131.1)	2832.3 (170.6)	1553.9 (115.3)	132.7 (33.9)	171.5 (45.3)	18.3 (7.4)
2014	2568.3 (136.9)	2898.6 (178.4)	1583.2 (112.6)	74.7 (13.9)	96.4 (18.6)	9.9 (2.6)
2015	2544.3 (129.2)	2874.5 (168.6)	1578.3 (111.1)	69.3 (11.9)	89.3 (15.9)	10.7 (2.9)
2016	2609.7 (136.9)	2971.5 (179.6)	1553.0 (104.5)	73.9 (12.1)	93.6 (16.1)	16.5 (4.8)
2017	2608.8 (134.5)	2955.6 (176.8)	1606.2 (104.1)	73.4 (12.1)	91.0 (16.0)	22.7 (8.1)
2018	2664.0 (138.4)	3056.4 (183.8)	1571.5 (102.8)	34.9 (6.1)	40.8 (7.9)	18.6 (7.0)
2019	2675.7 (138.0)	3042.2 (181.9)	1627.9 (106.6)	26.8 (4.1)	29.6 (5.0)	18.9 (6.6)

*Notes:* This table reports means and their standard errors in agency fee payers in local unions located in states which were unaffected by RTW reforms from 2007-2019.

*Sources:* DOL



**Table XVII** – Trends in Membership Relative to the Enactment of RTW

<i>h</i>	Members						AFP					
	Total	MI	IN	WI	WV	KY	Total	MI	IN	WI	WV	KY
-5	1843.5 (168.7)	2238.8 (359.2)	1430.7 (173.7)	2048.3 (315.2)	778.9 (124.5)	1894.1 (514.9)	7.1 (2.5)	9.3 (5.7)	2.7 (1.2)	6.3 (3.2)	9.4 (9.0)	9.9 (5.9)
-4	1824.7 (175.7)	2178.9 (380.8)	1507.0 (193.2)	1844.8 (273.3)	863.1 (130.7)	1952.8 (520.2)	7.4 (2.4)	7.8 (5.0)	4.5 (2.1)	10.7 (6.4)	8.1 (7.8)	7.3 (5.3)
-3	1803.9 (187.8)	2188.0 (425.6)	1471.3 (195.8)	1796.7 (264.3)	765.4 (130.1)	1980.0 (577.5)	9.1 (3.0)	10.5 (5.6)	4.1 (2.0)	7.7 (5.3)	4.3 (4.3)	20.3 (15.1)
-2	1829.4 (185.9)	2164.7 (386.1)	1487.8 (224.2)	1785.7 (248.5)	751.6 (116.8)	2161.5 (632.1)	7.0 (2.3)	9.4 (4.8)	4.0 (2.2)	9.6 (6.1)	1.3 (1.3)	4.9 (4.7)
-1	1809.9 (184.6)	2155.3 (391.8)	1485.9 (207.3)	1782.1 (265.9)	728.6 (111.8)	2166.7 (636.7)	9.6 (3.4)	14.7 (7.9)	9.7 (5.1)	3.9 (1.5)	0.8 (0.8)	5.9 (3.8)
0	1747.9 (146.1)	1995.1 (276.2)	1500.5 (202.7)	1707.7 (256.2)	726.2 (108.6)	2334.6 (665.0)	13.0 (4.8)	8.7 (3.2)	15.4 (8.9)	31.7 (25.5)	0.8 (0.8)	4.7 (3.0)
1	1747.9 (143.7)	2021.3 (276.4)	1508.4 (206.9)	1737.7 (247.3)	760.8 (112.5)	2103.8 (612.5)	6.0 (2.1)	2.4 (0.9)	11.1 (6.8)	13.0 (7.5)	0.0 (0.0)	1.0 (0.5)
2	1701.7 (137.1)	1932.9 (256.2)	1497.3 (207.1)	1743.5 (258.5)	819.5 (138.6)	1928.1 (533.4)	3.1 (0.9)	2.1 (0.9)	6.5 (3.0)	4.1 (1.6)	0.0 (0.0)	0.9 (0.6)
3	1716.6 (141.8)	1994.0 (267.4)	1534.5 (213.5)	1777.5 (245.3)	788.0 (147.7)		6.6 (3.4)	9.9 (7.2)	5.7 (3.8)	2.9 (1.0)	0.0 (0.0)	
4	1821.9 (149.7)	2054.2 (261.4)	1500.8 (205.6)	1783.7 (251.1)			4.2 (1.6)	5.1 (2.6)	4.3 (3.3)	2.2 (0.8)		
5	1819.3 (175.2)	2037.0 (253.8)	1484.1 (210.5)				5.3 (2.6)	1.8 (0.9)	10.8 (6.4)			

*Notes:* This table reports means and their standard errors in agency fee payers in states affected by RTW reforms from 2007-2019. *Sources:* DOL

**Table XVIII** – Trends in Membership Outside of Treatment Groups (Reported AFP)

Year	Members			AFP		
	Total	RTW Never	RTW Pre-2007	Total	RTW Never	RTW Pre-2007
2007	6370.2 (600.5)	6714.0 (653.4)	2720.0 (489.1)	587.6 (152.0)	628.8 (166.1)	150.5 (45.9)
2008	6699.5 (670.0)	7092.0 (732.9)	2765.6 (482.0)	525.8 (126.6)	560.9 (139.0)	174.2 (58.8)
2009	6753.9 (660.4)	7042.2 (716.4)	3527.3 (548.7)	734.6 (235.7)	785.9 (256.6)	160.2 (36.2)
2010	7138.0 (754.5)	7325.0 (804.7)	4613.2 (1222.4)	886.6 (258.1)	932.4 (277.1)	268.0 (57.3)
2011	6145.5 (621.6)	6442.2 (670.5)	2544.7 (377.6)	758.4 (210.3)	774.9 (226.3)	558.1 (308.7)
2012	6428.5 (642.2)	6685.2 (687.4)	2954.4 (516.9)	721.8 (202.8)	761.3 (217.7)	187.0 (36.9)
2013	6267.6 (623.0)	6511.9 (670.2)	3188.2 (487.3)	725.5 (183.3)	755.8 (197.5)	344.0 (130.9)
2014	6130.2 (609.9)	6317.5 (653.8)	3672.9 (664.5)	402.3 (73.7)	418.6 (79.2)	188.5 (41.1)
2015	6253.9 (592.5)	6433.8 (634.7)	3881.2 (690.8)	373.4 (62.7)	385.9 (67.3)	208.5 (47.0)
2016	6535.6 (603.0)	6829.1 (653.2)	3197.0 (471.5)	401.5 (64.0)	411.8 (69.3)	283.7 (72.2)
2017	6663.9 (591.1)	6894.5 (638.1)	3964.9 (680.9)	395.4 (63.4)	395.2 (67.9)	398.4 (132.5)
2018	6885.3 (619.3)	7169.7 (675.1)	3971.9 (699.7)	203.3 (34.8)	191.8 (36.5)	320.9 (114.0)
2019	6981.0 (652.4)	7363.3 (721.4)	3645.8 (655.7)	164.9 (24.0)	150.2 (24.4)	293.0 (94.7)

*Notes:* This table reports means and their standard errors in agency fee payers in local unions located in states which were unaffected by RTW reforms from 2007-2019.

*Sources:* DOL

**Table XIX** – Trends in Membership Relative to the Enactment of RTW (Reported AFP)

$h$	Members						AFP					
	Total	MI	IN	WI	WV	KY	Total	MI	IN	WI	WV	KY
-5	4719.8 (1024.3)	5543.3 (2090.1)	4571.2 (1544.7)	4409.4 (992.1)	987.0 (460.0)	3443.6 (2150.4)	46.8 (18.8)	58.3 (38.7)	16.9 (9.8)	29.3 (13.9)	163.0 (162.0)	47.0 (38.8)
-4	4794.6 (1087.7)	5815.0 (2245.3)	4788.4 (1612.7)	4023.4 (853.7)	1004.0 (499.0)	3492.6 (2208.5)	50.6 (17.9)	52.1 (33.8)	27.7 (13.4)	50.1 (32.2)	148.0 (147.0)	50.6 (37.8)
-3	4948.9 (1147.3)	6172.4 (2432.0)	4571.0 (1590.2)	3955.1 (839.1)	940.5 (471.5)	4135.3 (2629.8)	49.9 (17.3)	66.3 (35.0)	22.4 (11.4)	38.1 (27.2)	71.5 (70.5)	47.7 (39.4)
-2	4753.0 (1091.5)	5649.3 (2136.3)	5109.8 (1963.3)	3619.4 (734.9)	893.5 (422.5)	4215.2 (2722.7)	45.8 (16.3)	50.6 (29.3)	44.4 (24.3)	42.9 (29.6)	24.5 (23.5)	40.0 (38.6)
-1	4613.1 (1042.2)	5840.9 (2208.4)	4384.2 (1537.8)	3554.9 (730.8)	874.0 (404.0)	3572.6 (2277.2)	66.2 (23.5)	92.0 (49.9)	95.8 (45.5)	16.1 (5.7)	15.5 (14.5)	41.9 (24.1)
0	3710.3 (624.2)	4052.2 (1144.8)	4331.8 (1507.3)	3141.8 (658.4)	844.0 (402.0)	3517.0 (2232.0)	87.1 (31.2)	51.8 (17.2)	158.0 (83.2)	132.5 (105.1)	15.5 (14.5)	32.0 (18.7)
1	3863.5 (640.2)	4231.6 (1200.1)	4314.9 (1510.2)	3329.6 (649.6)	851.0 (406.0)	3807.2 (2295.0)	32.5 (12.3)	13.8 (5.2)	113.0 (63.6)	22.4 (9.3)	0.0 (0.0)	6.8 (3.8)
2	3835.8 (649.8)	4324.4 (1200.6)	4222.7 (1477.2)	3155.8 (688.2)	816.0 (368.0)	3715.5 (2483.8)	20.7 (5.5)	13.7 (5.6)	51.6 (23.9)	18.3 (6.2)	0.0 (0.0)	7.7 (4.9)
3	3874.8 (660.0)	4306.1 (1187.3)	4714.6 (1612.2)	3079.9 (613.0)	826.5 (319.5)		22.5 (8.0)	16.8 (6.3)	63.8 (39.3)	9.6 (3.2)	0.0 (0.0)	
4	4148.6 (673.2)	4355.1 (1109.1)	4589.2 (1563.5)	3483.1 (718.8)			18.3 (7.6)	12.5 (5.4)	49.0 (36.1)	8.2 (3.0)		
5	4329.7 (907.8)	4255.5 (1104.8)	4530.0 (1634.2)				24.2 (12.0)	9.7 (5.2)	63.4 (41.2)			

*Notes:* This table reports means and their standard errors in agency fee payers in states affected by RTW reforms from 2007-2019. *Sources:* DOL