When Strikes Meet Stocks: The Complex Interplay Between Organized Labor and Employee Stock Ownership

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ABSTRACT

This paper examines how changes in union strength affect employee stock ownership plan (ESOP) formation. Exploiting the staggered adoption of right-to-work (RTW) laws across U.S. states, the analysis employs a difference-in-differences design with instrumental-variable logic. The results show that RTW laws significantly reduce union density and, in turn, lead to fewer ESOPs. Five years after adoption, RTW laws result in an estimated decline of roughly 2.9 percentage points in union density and approximately 21 fewer ESOPs per state. These findings suggest that employee ownership is more likely to emerge where unions pose a credible threat to firms' profitability, speaking to the broader literature on union avoidance strategies.

Keywords: Right-to-work laws; employee ownership; staggered difference-in-differences; union avoidance; instrumental variable

Word Count: 2905

1 Introduction

How do firms strategically deter workers from unionizing and engaging in disruptive action? In the post-war period, organized labor emerged as a powerful actor in American society, with union membership climbing to nearly 35% of the workforce in the 1950s (Feiveson 2023). However, strikes and other forms of work stoppages are extremely costly for firms, incentivizing managers to either coerce workers or accommodate their demands. While various scholars (Heckscher 1988; Logan 2006, 2020) have documented the rise of "union avoidance" during the late twentieth century, some of these preventive tools have a complicated relationship with unions, making it unclear whether they function as a substitute for or a complement to organized labor (Blasi and Kruse 1991).

In this paper, I consider employee ownership to be a paragon of these ambiguous strategies. First designed in 1956 by American economist Louis O. Kelso (1958), employee stock ownership plans (hereafter ESOPs) are programs that give workers company shares and tie compensation to firm performance. Although consistently framed as a tool for improving employee welfare (Kim and

[†]Data and replicable code can be found here: https://github.com/lydiagcamp/when_strikes_meet_stocks.

Ouimet 2014; Xiao et al. 2019; Corfe and Kirkup 2020; Foley et al. 2025), two working papers (Chen et al. 2020; Pecheu 2023) have recently described employee ownership as a strategic bargaining tool employed by managers to placate workers. However, these papers rely on identification strategies that are limited in scope, and their conclusions are drawn from highly specific contexts.[1] I therefore aim to provide new empirical evidence that locates ESOP adoption within firms' broader social and historical contexts. As such, this is the first study to use quasi-exogenous shocks at the state-level to examine the effect of union strength on employee ownership—with implications for both management and union scholars alike.

Since ESOPs reduce the utility workers receive from engaging in actions that harm firm performance (Cramton et al. 2015), I expect firms to be more likely to offer ESOPs when managers view labor as a credible threat to profitability, and vice versa when unions are weakened.

Hypothesis 1: Increases in union strength increase the probability that firms offer an ESOP.

To test this expectation, I first outline the relevant data sources and provide descriptive statistics in Section 2. I then outline my identification strategy in Section 3, which exploits the staggered adoption of right-to-work laws across U.S. states. Drawing from recent developments in the econometric literature (Callaway and Sant'Anna 2021; Ye et al. 2023; Miyaji 2024), I conduct an staggered difference-in-differences design that incorporates instrumental-variable logic. The results support the expected relationship: The adoption of right-to-work laws weaken unions, which, in turn, reduces ESOP adoption. After completing the primary analysis, I provide suggestive evidence for this proposed channel, along with additional robustness checks in Section 4. Section 5 concludes.

2 Data and Descriptive Statistics

Since the ESOP-union relationship is a relatively novel line of research (Foley et al. 2025), there is no universal database containing information on both ESOPs and union activity. I therefore integrate data from a variety of sources. For ESOP-offering firms, I draw from the raw Form 5500 filings, collected by the *U.S. Department of Labor* (EBSA). As of 1999, all organizations that offer any kind of employee benefit plans covered by ERISA (Employee Retirement Income Security Act) must fill out this form annually.[2] To measure union strength, I use the *U.S. Bureau of Labor Statistics* (BLS) measure of union density, which reflects the share of employed workers who are union members within a given state. I prefer union density to alternative measures, such as the number of work stoppages, since BLS data only capture stoppages involving more than 1,000 participants, which typically occur at large firms operating across multiple states. The distribution of work stoppages is also relatively volatile and strongly right-skewed.[3]

Finally, data on control variables is taken from the *United States Census Bureau*, the *Federal*

[1] For example, Pecheu (2023) focuses on profit-sharing plans in France, which are legally required by the French government in firms with more than 50 employees. This approach therefore complicates efforts to distinguish firms that are simply complying with the law from those using profit-sharing strategically as a bargaining tool.

[2] I remove duplicate plans that share the same combination of plan name, sponsor name, and start year. Due to data quality issues in 1999, including substantial missing plan information, I exclude this year from the analysis to avoid biasing the results.

[3] The pairwise correlation between union density and work stoppages at the state-year level is also not spectacular (Pearson's $r = 0.299$).

Reserve Bank of St. Louis (FRED), and the *National Governors Association* (NGA). I control for the total number of firms, real median income (measured in constant 2024 dollars), the size of the population, and the manufacturing employment share. As a proxy for the size of the traditional working class, the latter variable is critical, as it allows me to examine the effect of union strength independent of the size of the working class. I also include a binary left-wing indicator that equals 1 if a state's governor is affiliated with the Democratic Party and 0 otherwise. Since data on some of these key control variables are only available through 2021, the primary analysis covers the period from 2000 to 2021. Table 1 summarizes the data sources.

Table 1: **Data Sources**

Variable	**Source**
Number of ESOPs	U.S. Department of Labor (EBSA)
Union density	U.S. Bureau of Labor Statistics (BLS)
Work stoppages	
State population	U.S. Census Bureau
Number of firms	
Manufacturing employment	Federal Reserve Bank of St. Louis (FRED)
Real median household income	
Unemployment rate	
Left-wing (governor party affiliation)	National Governors Association (NGA)

Using these data, I present descriptive statistics at the state level ($N = 50$) in Table 2. While a more granular level of analysis (*e.g.* the firm) might be preferable, most of the relevant variables are measured at the state level. Nevertheless, this limitation is inadvertently useful because it allows me to analyze firms' overall legal and social environments rather than exclusively considering individual company experiences. For instance, since union avoidance is an inherently *preemptive* strategy, workers' threats need not be firm-specific; rather, managers likely make decisions based on their perceptions of the broader contexts in which they are embedded.

Table 2: **Descriptive Statistics (2000–2021)**

Variable	**Obs.**	**Mean**	**Std. Dev.**	**Min**	**Max**
Number of ESOPs	1100	139.07	150.50	6	1201
Union density (%)	1100	10.93	5.44	1.60	26.90
Number of work stoppages	1100	0.49	1.11	0	14
Left-wing government (binary)	1100	0.44	0.50	0	1
ln Population	1100	15.15	1.01	13.11	17.49
ln Manufacturing employment	1100	11.92	1.19	9.07	14.43
Manufacturing employment share (%)	1100	4.42	2.00	0.83	29.68
ln Real median income	1100	11.19	0.17	10.65	11.66
ln Number of firms	1100	11.27	0.93	9.68	13.62

Notes: The table reports statistics at the state–year level ($T = 22$; $N = 50$).

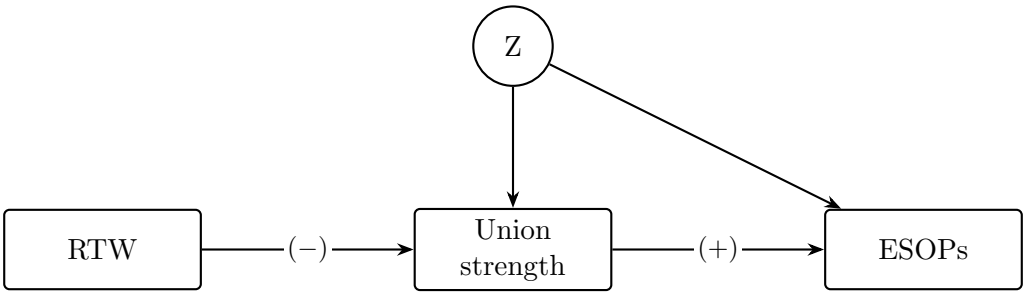
3 Identification Strategy

3.1 Staggered Adoption of Right-to-Work Laws

To isolate the causal effect of union strength, I conduct a staggered difference-in-differences design that exploits the staggered adoption of right-to-work (RTW) laws across states. RTW laws stem from the 1947 Taft-Hartley amendments to the Wagner Act, which granted states the authority to prohibit "union shop" provisions (*i.e.* contracts that require new employees to join the union and pay dues). A substantial body of research has shown that these laws weaken organized labor by reducing union membership and bargaining power (Ellwood and Fine 1987; Hogler 2015; Makridis 2019; Chava et al. 2020; Chun 2023; Gihleb et al. 2023). While many states first adopted these policies in the 1940s and 1950s, implementation has also been rolled out gradually, particularly after the 2008 financial crisis: Indiana (2012), Michigan (2012), Wisconsin (2015), West Virginia (2016), and Kentucky (2017).

Following a fascinating paper by Fortin et al. (2022),[4] I agree that the recent implementation of RTW can be used as a valid instrumental variable (IV) for unionization under the assumption that RTW only affects the dependent variable of interest by reducing the unionization rate. If this assumption holds, an IV analysis should reduce the confounding bias introduced by unobserved variables Z, as depicted in Figure 1.

Figure 1: **Proposed Theoretical Relationship**



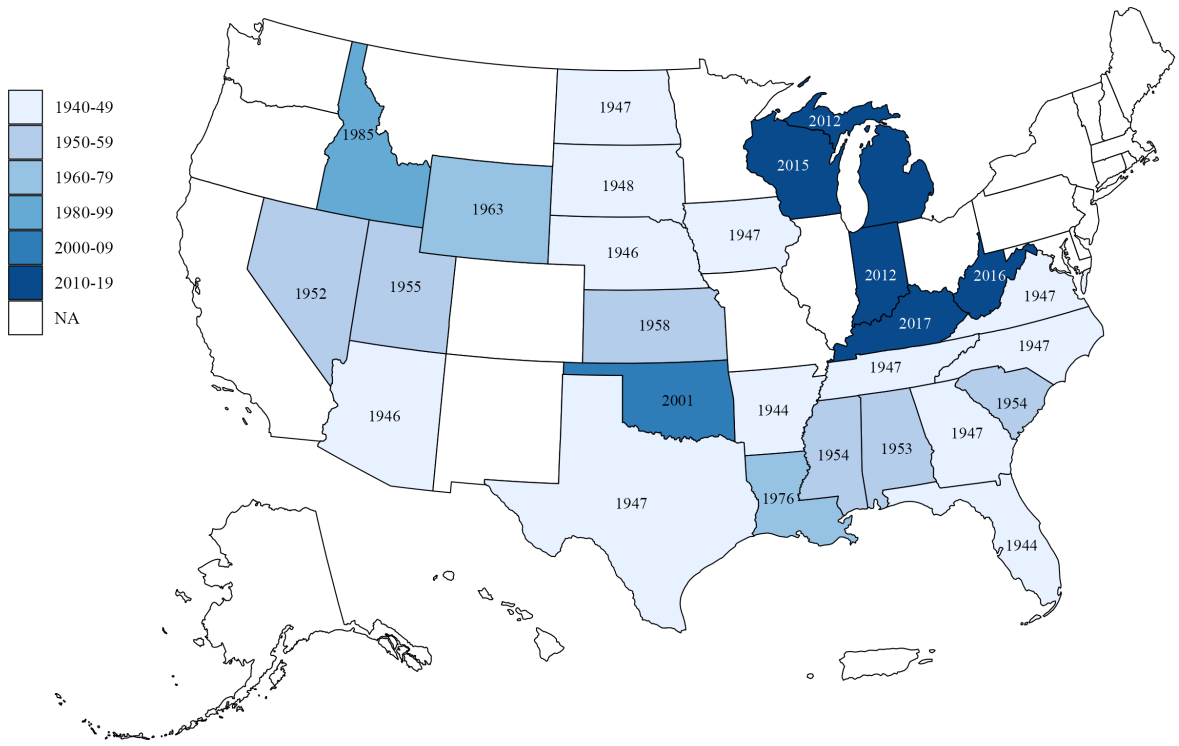
However, while the relevance of RTW as an instrument is well supported in the literature, the exclusion restriction may be violated since states' selection into RTW adoption is non-random. I therefore retain the general IV logic but instead implement a staggered difference-in-differences (DiD) design, an approach that is sometimes referred to as an instrumented difference-in-differences (DiD-IV) (Ye et al. 2023; Miyaji 2024). This identification strategy is preferable because it relies on the weaker parallel trends assumption, allowing the instrument to be correlated with unobservables if it is still independent of counterfactual trends. In short, this approach helps "relax some of the most disputable assumptions of the standard IV" (Ye et al. 2023).

While researchers have been conducting similar analyses for decades (Duflo 2001), this hybrid design has only recently received formal attention (Ye et al. 2023; Miyaji 2024). DiD-IV is most commonly applied in settings when treatment is strongly predictive of the key explanatory variable, but likely endogenous—such as in this case. As such, my methodological choice is not particularly novel in the industrial relations literature, with numerous studies incorporating RTW laws into

[4]For more information regarding the historical context that prompted RTW adoption, see Table A1 in the Appendix and Fortin et al. (2022).

DiD designs (Makridis 2019; Chava et al. 2020; Fortin et al. 2022; Gihleb et al. 2023). However, previous work has primarily considered wages as the dependent variable, with this paper being the first to theoretically connect and empirically test the laws' effect on employee ownership. I therefore compare ESOP formation between the five Midwestern "adopters"[5] (the treated group) to the "never adopters" (the control group) before and after the RTW laws are enacted. "Always adopters" (*e.g.* states that adopted RTW before 2000) are excluded from the econometric analyses to preclude inappropriate comparisons. I also omit Oklahoma, which adopted RTW in 2001, both because parallel trends cannot be evaluated in my dataset and to maintain sample homogeneity. This decision is consistent with Fortin et al. (2022).

Figure 2: **RTW Adoption Across U.S. States**



3.2 Assumptions

I first formally evaluate the endogeneity of the treatment. Table 3 presents differences in the key observable variables among the treated and control groups. Interestingly, RTW does not have a statistically significant "right-wing bias," and its adoption is unrelated to the number of firms located in each state. However, treated states do tend to be larger and poorer, possessing a higher share of manufacturing employment yet lower unionization rates prior to treatment. As such, while I cannot claim that the treatment is "as good as random," this test does help me get a sense of which factors may confound the analysis (*i.e.* what to control for).

[5]This includes: Indiana (2012), Michigan (2012), Wisconsin (2015), West Virginia (2016), and Kentucky (2017).

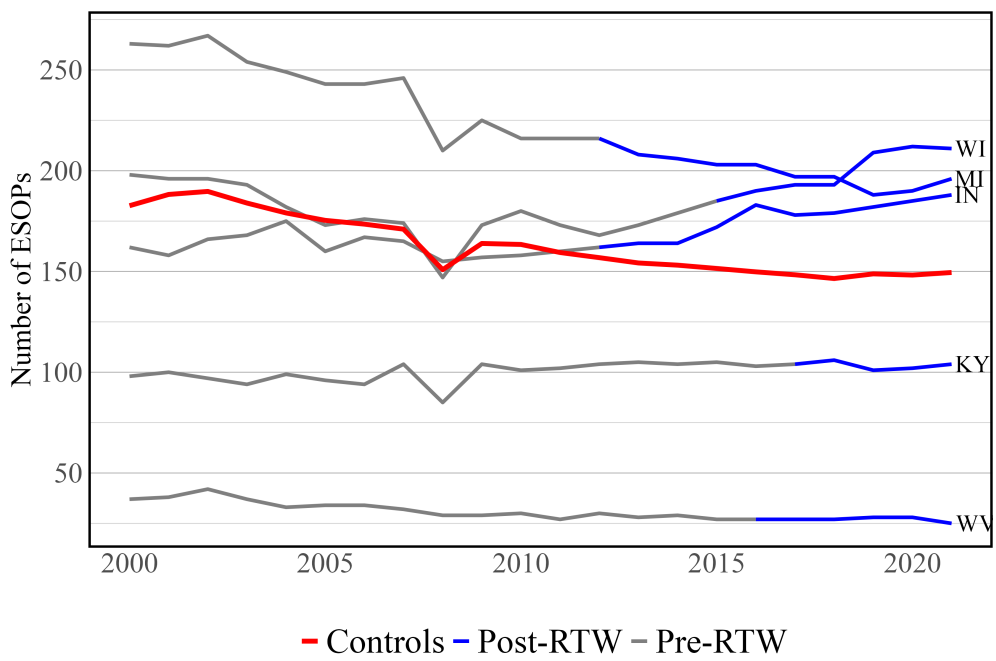
Table 3: **Difference-in-Means Test**

Variable	**Treated Mean**	**Control Mean**	**Difference**
Left-wing government	0.639	0.603	0.036
ln Number of firms	11.323	11.296	0.028
Union density	13.897	14.978	−1.081**
ln Real median income	11.054	11.279	−0.225***
ln State population	15.340	15.106	0.234***
Manufacturing employment share	6.475	3.900	2.576***

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Nonetheless, balance tests capture differences in levels rather than trends, with the latter being the relevant identifying condition in DiD analyses, as mentioned above. Depicted in Figure 3, the number of ESOPs in treated and control states follows relatively stable trends during the pre-treatment period, with no observable anticipatory effect. The dip that all states experienced around 2008, for instance, is plausibly the result of the financial crisis's effect on the stock market and firm profitability, which in turn discouraged ESOP formation. The key exception to the parallel trends assumption, however, is Wisconsin, which displays a markedly different trajectory in the years leading up to the state's introduction of RTW in 2015. While there may be several explanations for this divergence, Fortin et al. (2022) also highlights Wisconsin as an unusual case, noting that RTW was phased in much more gradually in this state. Similarly, Biasi and Sarsons (2022) present qualitative evidence that Wisconsin firms, in particular, were more likely to adhere to RTW conditions only at the expiration of previous contracts. For robustness, I conduct the analysis both including and excluding Wisconsin. Nevertheless, the joint Wald test of pre-treatment coefficients provides no evidence against the parallel trends assumption at conventional significance levels ($p = 0.61$ with Wisconsin included; $p = 0.88$ without Wisconsin).

Figure 3: **Trends in ESOP Formation**



3.3 Model Specification

While there are various ways to conduct an instrumented DiD design, I structure my analysis around three phases of evidence: The "first stage" tests the direct effect of RTW laws on union density; the "reduced form" examines the effect of RTW on ESOPs; and the third model also regresses ESOPs on RTW, but controls for union density. The purpose of this disaggregated approach can therefore be understood as separately establishing (1) that the treatment meaningfully affects the independent variable, (2) that the treatment has an effect on the dependent variable, and (3) whether the treatment continues to affect the dependent variable after accounting for the independent (or "mediating") variable.

I also conduct each stage individually because the treatment is staggered across units over time, complicating traditional DiD-IV analyses. In particular, since a growing body of work has identified the challenges associated with "roll-out" designs, (de Chaisemartin and d'Haultfoeuille 2020; Borusyak et al. 2021; Sun and Abraham 2021; Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Huntington-Klein 2021), I utilize the Callaway and Sant'Anna (2021) estimator for the subsequent analysis. In contrast to traditional two-way fixed effect models, this staggered DiD estimator prevents the comparison of just-treated observations to already-treated observations—a consideration that is particularly important when treatment is adopted at different times across units. To conduct the analysis, I group the five treated states (Indiana, Michigan, Wisconsin, Kentucky, and West Virginia) into four cohorts based on the year of RTW adoption (2012, 2015, 2016, 2017). I then estimate the "group-time" treatment effect $ATT(g,t)$, which represents the Average Treatment Effect on the Treated for the group first treated in year g, and measured in year t. Formally,

$$ATT(g,t) = \mathbb{E}[Y_{it}(1) - Y_{it}(0) \mid G_i = g,\ X_{i,t-1}], \quad t \geq g, \tag{1}$$

in which $Y_{it}(1)$ and $Y_{it}(0)$ denote potential outcomes with and without RTW adoption respectively. Informed by the balance test results, I also control for the relevant time-varying factors in vector $Xi, t-1\gamma$, including the number of firms, manufacturing employment share, and real median income. To avoid post-treatment bias, I lag all controls by one year, as RTW adoption may also affect these covariates. I also note that the control group consists of both never-treated states and treated states prior to RTW adoption. Using both groups is standard in the literature (Callaway and Sant'Anna 2021) and improves precision, while also avoiding reliance on a potentially unrepresentative control group. Nonetheless, qualitative evidence suggests that the treated and control states may not differ so sharply; several states (New Hampshire, Illinois, Delaware, and Missouri) also attempted but narrowly failed to adopt RTW laws during this period.

3.4 Results

I present the results for each of the three stages in Table 4. Consistent with the causal literature on RTW (Makridis 2019; Chava et al. 2020; Chun 2023; Gihleb et al. 2023), I find that states that adopt the laws experience a marked decrease in union density. This decline is approximately equivalent to a reduction of 2.9 percentage points ($\pm$ 2.0) after five years, which matches the typically reported results of 2-4 percentage points (Eren and Ozbeklik 2016; Fortin et al. 2022).[6]

[6]Table A2 in the Appendix provides estimates of the effect each year after RTW adoption.

Table 4: **Staggered Difference-in-Differences Estimates of the Effect of RTW on Union Density and ESOPs**

First Stage: RTW → Union Density

	All States	**Excluding Wisconsin**
ATT	−2.427***	−1.909***
	(0.717)	(0.564)
95% CI	[−3.832, −1.021]	[−3.015, −0.803]

Reduced Form: RTW → ESOPs (*Not Controlling for Union Density*)

	All States	**Excluding Wisconsin**
ATT	−13.098*	−18.428***
	(6.480)	(4.671)
95% CI	[−25.800, −0.397]	[−27.582, −9.273]

Conditional Effect: RTW → ESOPs (*Controlling for Union Density*)

	All States	**Excluding Wisconsin**
ATT	−7.131	−10.285
	(7.616)	(7.551)
95% CI	[−22.059, 7.797]	[−25.084, 4.515]

OLS Models: Union Density → ESOPs

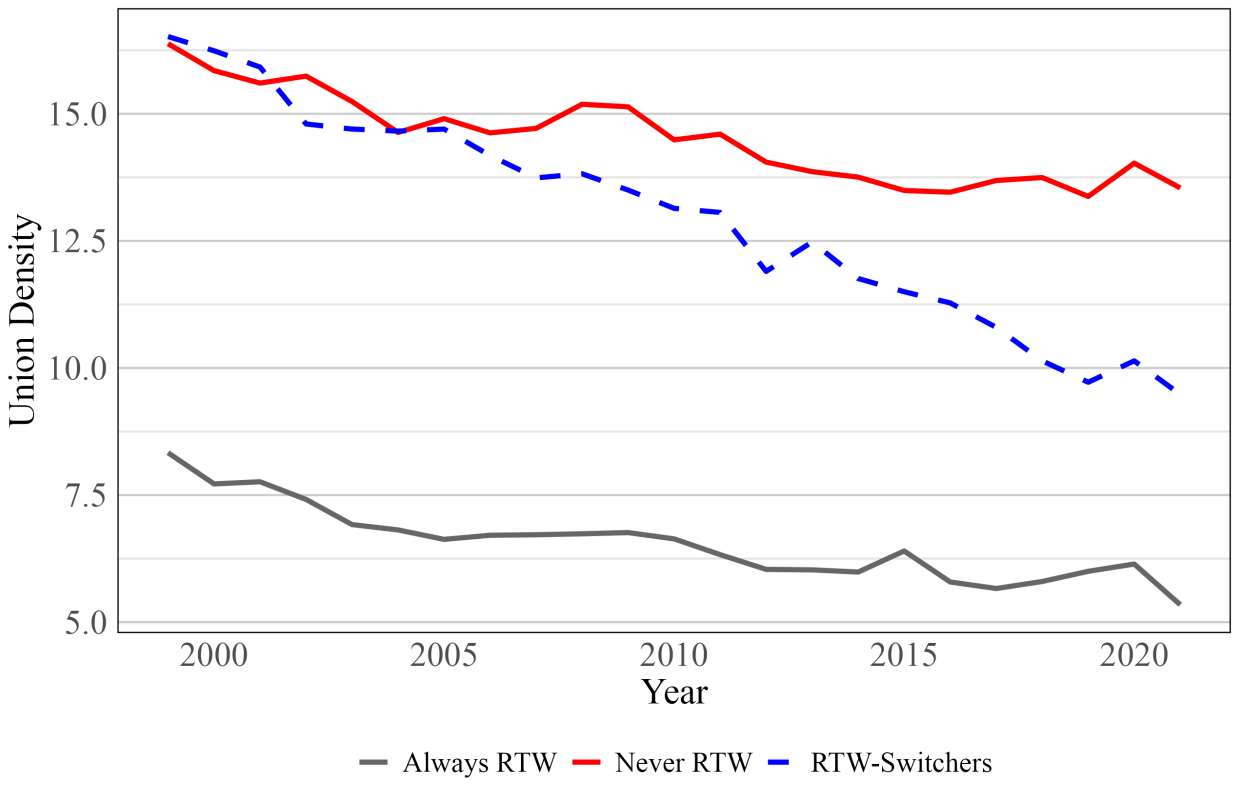
	Lag 4 Years	**Lag 5 Years**	**Lag 6 Years**
Union Density	−2.517*	−3.438**	−3.498**
	(1.017)	(1.109)	(1.191)
Union Density^2	0.059$^+$	0.088*	0.089*
	(0.035)	(0.038)	(0.042)
Num. Obs.	900	850	800
R^2	0.530	0.442	0.301

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Notes: The first three panels reports average treatment effects on the treated (ATT) from a staggered difference-in-differences design that uses the Callaway and Sant'Anna (2021) estimator. Standard errors are in parentheses and are clustered at the state level. Control states are those that never adopt RTW laws. The five treated states where RTW was adopted are Indiana (2012), Michigan (2012), Wisconsin (2015), West Virginia (2016), and Kentucky (2017). Models control for the number of firms, the proportion of employment in manufacturing, and real median income. All controls are lagged by one year to prevent post-treatment bias. The OLS regressions incorporate the same set of controls and include state and year fixed effects. All variables are lagged according to the specification shown.

Figure 4 presents the raw trends, while Figure 5 displays the dynamic ATT estimates for the first stage, revealing a clear decline in union density among RTW states. In the second stage, I also find a negative effect of RTW on ESOPs, with estimates becoming both statistically and substantively larger when Wisconsin is excluded. As depicted in Figure 6, the effect is not immediate; however, a temporal lag is to be expected since firms cannot form or cancel ESOPs overnight. Also reported in Table A2 in the Appendix, RTW adoption leads to an estimated decline of roughly 21.2 ($\pm$13.49) ESOPs per state five years after adoption. This estimate is substantively meaningful, considering that the average number of ESOPs per state is approximately 139 in this dataset. Additionally, Figure 6 further supports the use of RTW in a causal design, as none of the pre-treatment estimates are statistically different from zero.

Figure 4: **Trends in Union Density**



Next, I provide suggestive evidence for union strength being the key mediator in the effect of RTW on employee ownership. This "conditional effect" cannot comprehensively rule out other channels; however, it can strengthen my application of quasi-"instrumental" logic to this design. As expected, the effect of RTW on the number of ESOPs becomes statistically insignificant once the effect of union density is partialled out. This suggests that the primary link between RTW and employee ownership occurs through changes in organized labor's power. Finally, I estimate ordinary least squares (OLS) models, which regress the number of ESOPs on union density and its square. While not directly causal, these models provide more comprehensive evidence across all U.S. states. In addition to controlling for the relevant factors that vary over time, I also specify a dynamic model with a lagged dependent variable (LDV) and two-way fixed effects.[7]

[7] Nickell bias is a potential concern in models that specify both an LDV and two-way fixed effects; however, since this bias is of order $\frac{1}{T}$, it is generally nominal when T is greater than 20.

Figure 5: **First Stage: RTW on Union Density**

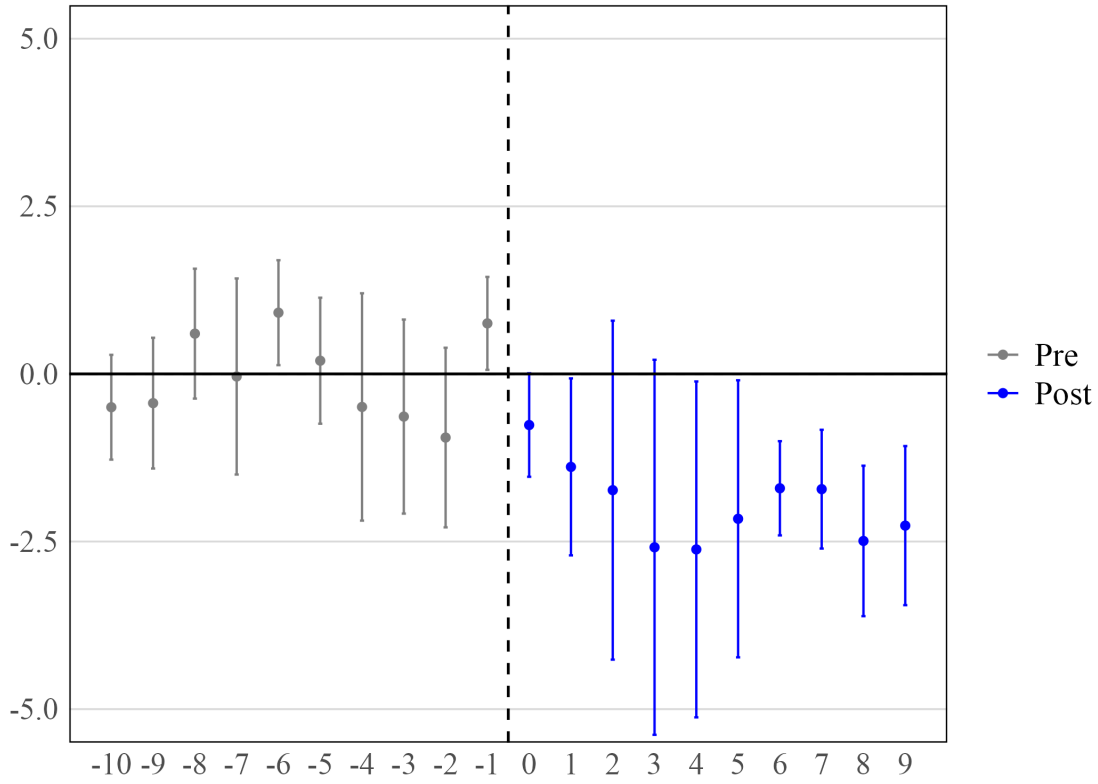
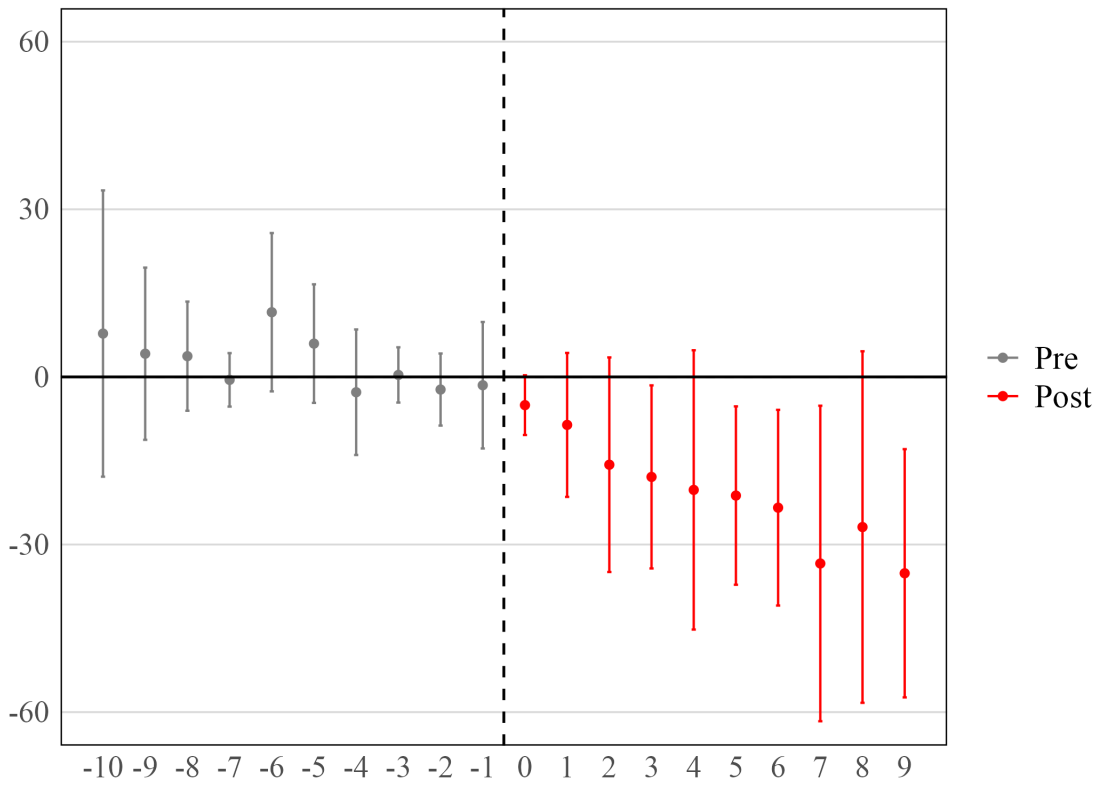


Figure 6: **Reduced Form: RTW on ESOP Formation**



While the appropriate lag length (n) is difficult to determine *ex ante*, I present results using multiple lag specifications, with effects that are relatively consistent over time. The models reveal a curvilinear relationship between measures of union strength and the number of ESOPs in each state. These results indicate that when unions are relatively weak actors, increases in unionization or work stoppages are associated with fewer ESOPs; however, this relationship reverses once unions become more influential (at approximately 20 percent union density). However, while this curvilinear relationship is intriguing, I primarily rely on the DiD estimates for interpretation; ultimately, models with two-way fixed effects cannot fully satisfy the conditional independence assumption because unobserved confounders may also change over time (Imai and Kim 2021).

4 Robustness Checks

Following the standard practice in the literature (Fortin et al. 2022), I re-estimate the primary analysis in Table 5, but this time disaggregate the effects across treated states. This approach helps assess whether the results are driven by any single observation. As observed in the first column, RTW consistently reduces union density. This finding therefore establishes the monotonocity assumption, demonstrating that RTW laws only move union density in one direction, namely, downward. However, I find that neither Indiana nor Wisconsin display a reduction in the number of ESOPs after RTW adoption, which indicates the theorized effect is only observed in Michigan, West Virginia, and Kentucky. This finding does not necessarily invalidate the overall theoretical framework, but it does imply heterogenous treatment effects. Put simply, a quasi-exogenous reduction in unionization can explain part of the story, but likely not all of it.

Table 5: **Difference-in-Differences Estimates: State-By-State Estimates**

	Union Density (1)	**ESOPs** (2)
1. Benchmark estimator	—2.427***	—13.098*
	(0.717)	(6.480)
2. State-by-state estimates		
a) Michigan (2012)	—1.496***	—18.823***
	(0.078)	(5.639)
b) Indiana (2012)	—0.945**	1.408
	(0.289)	(5.170)
c) Wisconsin (2015)	-4.687***	10.054
	(0.484)	(5.961)
d) West Virginia (2016)	—2.899***	—21.542***
	(0.186)	(2.667)
e) Kentucky (2017)	—3.684***	—31.421***
	(0.406)	(3.458)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Notes: Benchmark estimators are taken from the initial analysis which includes all states.

5 Discussion

Through a staggered difference-in-differences approach incorporating instrumental-variable logic, this paper finds that right-to-work laws reduce union density by approximately 2.9 (± 2.0) percentage points and the number of ESOPs by roughly 21.2 (±13.49) plans five years after adoption. This effect also becomes both more substantively and statistically significant over time. While the possibility of alternative channels through which RTW affects the number of ESOPs complicates an unqualified causal claim, the suggestive evidence presented in the third panel highlights how this relationship most likely operates through union density.

This paper therefore makes significant progress in understanding the relationship between unions and employee ownership, speaking in particular to the literature on the paternalistic "benefits" that firms offer their workers. However, the insignificant effect of the law change in both Indiana and Wisconsin invites qualitative research on the reasons why these states display disparate trajectories. In light of the curvilinear effect found in the OLS regressions, there may be heterogeneous treatment effects depending on the unique history of organized labor's power in each state. Future research should therefore explore the conditions under which RTW laws—and changes in union strength more broadly—reduce the probability that firms adopt employee stock ownership plans.

Appendix

1. **Table A1: The Role of Political Events in the Adoption of RTW Laws**
2. **Table A2: Dynamic Effects of Right-to-Work Laws**
3. **Table A3: Union Strength and ESOP Adoption (OLS)**

Table A1: **The Role of Political Events in the Adoption of RTW Laws**

State	Date Effective	Previous gubernatorial election year	Incumbent	Winner
Indiana	1 February 2012	2008	R	R
Michigan	11 December 2012	2010	D	R
Wisconsin	9 March 2015	2010	D	R
West Virginia	12 February 2016	2012	D	D
Kentucky	7 January 2017	2015	D	R

Notes: Incumbent and Winner refer to the party affiliation (Democrat or Republican) of the sitting governor and the election winner, respectively. Information from this table comes from Fortin et al. (2022).

Table A2: **Dynamic Effects of Right-to-Work Laws**

Years After RTW	Union Density (pp)			ESOPs (count)		
	ATT	95% CI (Lower)	95% CI (Upper)	ATT	95% CI (Lower)	95% CI (Upper)
1	-1.977*	-3.495	-0.460	-8.590	-19.026	1.846
2	-2.332*	-4.531	-0.132	-15.716	-31.528	0.096
3	-2.972*	-5.066	-0.879	-17.898*	-31.764	-4.033
4	-2.846*	-4.792	-0.899	-20.224	-41.655	1.207
5	-2.913*	-4.861	-0.965	-21.236*	-34.724	-7.749
6	-3.142*	-5.917	-0.367	-23.406*	-37.847	-8.966
7	-1.712*	-2.516	-0.908	-33.393*	-58.250	-8.536
8	-2.503*	-3.565	-1.441	-26.871*	-51.982	-1.759
9	-2.296*	-3.346	-1.245	-35.145*	-53.381	-16.909

Notes: Entries report event-time average treatment effects on the treated (ATT) from a staggered difference-in-differences design. Union density effects are measured in *percentage points*. ESOP effects are measured in *counts* (number of ESOPs). Confidence intervals are at the 95% level.

Table A3: **Union Strength and ESOP Adoption (OLS)**

	Lag 4 Years	**Lag 5 Years**	**Lag 6 Years**
	(1)	**(2)**	**(3)**
LDV	0.534^{***}	0.431^{***}	0.340^{***}
	(0.019)	(0.020)	(0.021)
Union density	-2.517^{*}	-3.438^{**}	-3.498^{**}
	(1.017)	(1.109)	(1.191)
Union density^2	0.059^{+}	0.088^{*}	0.089^{*}
	(0.035)	(0.038)	(0.042)
Left-wing government	0.416	0.277	-0.598
	(1.051)	(1.120)	(1.168)
Manufacturing employment share	1.696	1.531	0.809
	(1.373)	(1.463)	(1.519)
Log real median income	7.754	5.438	5.664
	(9.504)	(10.188)	(10.731)
Log population	70.398^{**}	80.621^{***}	86.497^{***}
	(22.137)	(23.932)	(25.821)
Log number of firms	-68.939^{***}	-80.750^{***}	-89.311^{***}
	(19.712)	(21.337)	(22.704)
Num. Obs.	900	850	800
R^2	0.530	0.409	0.301
State FE	✓	✓	✓
Year FE	✓	✓	✓

$^{+}p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Standard errors in parentheses. The dependent variable is the number of ESOPs, and the number of observations varies with the temporal lag.

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