When Strikes Meet Stocks: Organized Labor and the Politics of Employee Ownership

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Abstract

This paper examines how changes in organized labor's strength shape the adoption of employee stock ownership plans (ESOPs) in the United States. While ESOPs are often framed as cooperative institutions that improve worker welfare and firm performance, they may also function as strategic responses to union pressure on the side of management. Using Form 5500 filings from 2000 to 2024, this study exploits the staggered adoption of right-to-work laws as a shock to union strength. Difference-in-differences and event-study results show that right-to-work laws reduce union density and are followed by a decline in active ESOPs. The effect attenuates and becomes insignificant once union density is accounted for, suggesting that organized labor is a key channel linking right-to-work laws to employee ownership formation. These findings show that employee ownership is not only a firm-level compensation tool, but also part of a broader political economy of labor-management conflict.

Keywords: Employee stock ownership plans; union avoidance; right-to-work laws; difference-in-differences

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1 Introduction

How do firms strategically respond to workers' capacity to unionize and engage in disruptive action? In the post-war period, organized labor emerged as a powerful actor in American society, with union membership climbing to over one third of the workforce in the 1950s (Feiveson 2023). However, strikes and other forms of work stoppages are extremely costly for firms, creating incentives for managers to either coerce workers or accommodate their demands. While various scholars (Heckscher 1988; Logan 2006, 2020) have documented the rise of a third strategy 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 substitutes for or complements to unionization (Foley et al. 2025).

Employee stock ownership plans (ESOPs) represent a particularly interesting example of this ambiguity. ESOPs are programs that give workers company shares and tie a portion of compensation to firm performance. As such, they are typically framed as a tool for improving employee welfare and organizational productivity (Adrianto et al. 2026). However, some scholars (Chen et al. 2020; Pecheu 2023) have argued that these programs may also discourage workers from pursuing higher wages and improved working conditions in the future. ESOPs may therefore act as a form of union avoidance, enabling firms to channel worker demands through shared-capitalistic arrangements rather than collective bargaining. Yet, despite growing interest in the relationship between organized labor and employee ownership, the existing evidence remains highly context-dependent, making it difficult to generalize across firms and institutional settings. This study accordingly examines ESOP adoption as part of the broader set of strategies firms use to respond to workers' demands. If ESOPs are adopted in response to union pressure, then firms should be less likely to offer them when organized labor is weakened.

To evaluate this argument, I exploit the staggered adoption of right-to-work laws across U.S. states as a quasi-exogenous shock to union strength. Using data on ESOP formation from Form 5500 filings between 2000 and 2024, I estimate the effect of weakening unions on employee ownership. The results support the expected relationship: right-to-work laws reduce union density immediately, while the effect on ESOPs builds more slowly, reaching statistical significance at around year six with a decline of roughly 18.7 plans per state ($\pm$11.7). This decline is substantively meaningful, given that the average state in the dataset records approximately 140 ESOPs. Additionally, the effect attenuates and becomes insignificant once union density is accounted for, suggesting that right-to-work laws shape employee ownership primarily by weakening organized labor. Robustness and heterogeneity analyses—including cohort-specific event studies and a border county design—further support these findings. As such, this is the first study to use quasi-exogenous variation in union strength at the aggregate level to examine how changes in workers' bargaining power affect the adoption of employee ownership plans.

This paper contributes to two growing bodies of scholarship. First, while specific to employee ownership as one mechanism of union avoidance, it speaks to the broader industrial relations literature by examining how shifts in union strength lead firms to adopt alternative

labor relations arrangements. Second, it contributes to the management literature on employee ownership by situating ESOP adoption within broader political and historical struggles between labor and management. While agnostic regarding ESOPs' effects on workers, this paper suggests that institutions commonly portrayed as cooperative alternatives to collective bargaining may themselves be endogenously shaped by the strength of organized labor.

The remainder of the paper proceeds as follows. Section 2 reviews the literature on employee ownership and organized labor. Section 3 develops the analytical framework and derives testable expectations. Section 4 describes the data and empirical strategy. Section 5 presents the main results and robustness checks, and Section 6 discusses the findings and offers concluding remarks.

2 Employee Ownership and Organized Labor

Employee ownership has a long history in economic theory, but the roots of contemporary plans can be found most directly in Louis O. Kelso and Mortimer Adler's *The Capitalist Manifesto* (1958). As the movement's foremost advocate, Kelso proposed shared capitalism as the solution to wealth inequality, a "Third Way" synthesizing social democratic principles with the ethos of the free market. According to Kelso (1958), as both returns to capital and distributional conflict were expected to rise in the late twentieth century, society could become more equal only by making "everyone a capitalist," *i.e.* extending capital ownership to workers. The first employee stock ownership plan was consequently established in 1956 by Kelso himself, yet it was not until 1974 that ESOPs gained statutory footing under the Employee Retirement Income Security Act (ERISA), which created tax incentives for employers to place company stock in worker trusts rather than requiring employees to buy shares themselves (Freeman et al. 2010). ESOPs therefore function as a component of worker compensation, allowing employees to accumulate company stock in their retirement accounts over time and to redeem their shares upon leaving or retiring. Compared with other shared-capitalistic arrangements such as profit-sharing plans or stock bonuses, ESOPs are distinctly institutionally embedded: they operate through tax-advantaged trusts, are tied to retirement savings, and give workers a claim on firm equity without requiring them to purchase shares directly. Perhaps most importantly, the distinction is also rhetorical. Unlike profit-sharing or bonuses, ESOPs frame workers not as recipients of distributed surplus but as co-owners of the firm itself. Historically enjoying bipartisan support, today ESOPs cover nearly 15 million participants, roughly nine percent of the U.S. labor force, and span sectors from manufacturing and construction (37.1 percent) to professional services (19.2 percent) and finance and insurance companies (12.2 percent) (Blasi et al. 2010; NCEO 2025). While similar employee ownership and profit-sharing schemes exist in other capitalist economies—including employee share schemes in the United Kingdom and profit-sharing plans in France (Pendleton et al. 2023; Pecheu 2023)—this paper focuses on the American case, where such programs are most institutionally embedded and administrative records are most complete.

Firms adopt employee ownership for many reasons, and Kruse (1996) notably sorted these motivations into four broad categories in the 1990s: (1) enhancing productivity and workplace

cooperation, (2) increasing compensation flexibility, (3) securing tax, capital, or takeover advantages, and (4) discouraging unionization or extracting union concessions. Using a panel of U.S. public companies from 1975 to 1991, Kruse examines the effect of these four motivations on ESOP adoption, measuring unionization with a firm-level dummy for whether at least one employee is covered by a union contract. Since this measure does not seem to predict ESOP adoption, Kruse concludes that "ESOPs do not appear to have been used by many firms as an anti-union tool, or as a means of gaining union concessions" (1996, p. 530). However, this approach only tests whether a firm's existing union status predicts ESOP adoption, not whether changes in organized labor's broader bargaining power alter firms' incentives to adopt employee ownership. This distinction is important because a firm-level union indicator is endogenous, slow-moving, and insensitive to variation in organizing threat, strike capacity, or the wider political environment. A null relationship among already-unionized firms therefore does not imply that labor power is orthogonal to ESOP adoption; it only suggests that firms with union contracts were not systematically more likely to adopt ESOPs. Partly due to this finding, and partly because the more optimistic literature discussed below came to set the terms of debate, the union dimension of ESOP adoption has received little sustained attention since.

In this light, the decline of the union question in the ESOP literature over the past few decades can be best understood as a reflection of the field's broader orientation. Much of the research on shared capitalism is produced by business and management schools, and the topic's relatively niche status has kept the field concentrated among a small number of well-established scholars. A close reading of this literature suggests that these studies are typically guided by a normative commitment to promoting ESOP adoption, often in the name of greater workplace equality. Many papers finish with "best practice" suggestions for firms, and the most common conclusion is that shared-capitalistic policies work best in combination with employee involvement and a "better" corporate culture, that is, as complements rather than as standalone interventions (Whyte and Blasi 1984; Blasi and Kruse 1991; Freeman et al. 2010; Kruse et al. 2008; Blasi et al. 2010; Blasi and Kruse 2017; Foley et al. 2025; Blasi and Michie 2025). While these findings are illustrative, the scenario most often described in this literature is the prescriptive ideal, which obscures discussion of what happens when these conditions of employee involvement are absent—as well as how frequently that absence occurs. Similarly, concerns about free riding and heightened risk exposure appear in these studies (Kruse et al. 2019), but are not pursued in much depth. Guzek and Whillans (2024) also discuss the oddity of this literature, noting that much of the available data on ESOPs originates with these programs' own supporting organizations rather than independent researchers, making it difficult to separate narratives that encourage adoption from accounts of genuine causality.

The gap is therefore not necessarily surprising. A literature organized around employee ownership as an instrument of workplace equality may sit somewhat uneasily with an alternative interpretation that emphasizes its potential use as a strategy for weakening union strength over the long term. Of course, this is not to suggest that all approaches to ESOP adoption are

equally conducive to workplace democracy, or that these insights about more and less effective models should not be communicated to company leaders. However, the broader question of whether the adoption and maintenance of these plans is connected to the strength of organized labor in the first place remains largely absent in this literature.

ESOPs therefore tend to be framed as cooperative institutions: workers share the firms' success, while firms benefit from higher productivity, stronger loyalty, and improved morale. Nevertheless, recent work that is broadly sympathetic to this view has also revealed an underlying tension. Using data from the General Social Survey (GSS), Foley et al. (2025) find that unionized employee-owners report more negative views of management and workplace relations than either unionized non-owners or non-union employee-owners. This paradox sits uncomfortably with the longstanding emphasis on the complementarity of these two institutions, as well as the otherwise positive outcomes documented in the same study—including higher wages, greater training, and larger account values. This finding also suggests that the relationship between employee ownership and unionization may depend less on their formal compatibility than on the unique conditions under which the two institutions coexist in practice. For the most part, though, the evidence surrounding ESOP adoption and the union-ESOP nexus remains anecdotal, case-based, or institutionally specific. As a result, Townsend et al. (2024) emphasize the need for causal evidence on the drivers of ESOP adoption.

To the best of my knowledge, there are only two working papers (Chen et al. 2020; Pecheu 2023) that employ causal methods to explore the effect of organized labor on shared capitalism. Despite this progress, however, both offer highly context-dependent accounts that are difficult to generalize. Pecheu (2023) studies profit-sharing in France, where such plans are legally required for firms with more than 50 employees, complicating efforts to distinguish firms that simply comply with the law from those that use profit-sharing strategically. Similarly, Chen et al. (2020) provide evidence that shared-capitalistic instruments may be implemented strategically after unionization decisions. However, because the paper examines firms' responses to close union elections rather than the determinants of ESOP adoption more broadly, its findings cannot speak directly to whether firms adopt ESOPs to prevent unionization in the first place. We therefore still know little about whether changes in organized labor's power systematically shape firms' decisions to adopt employee ownership. This paper takes up that question directly.

3 Theory

Although the ESOP literature has largely set aside the question of workers' interests, there are multiple reasons to believe that organized labor's strength is directly connected to employee ownership in practice. In fact, the same features that make ESOPs attractive to their supporters also make them politically complicated. By giving workers a stake in firm performance, employee ownership tends to soften the traditional divide between labor and management, reducing the need for collective action. In this sense, I argue that employee ownership schemes are best understood as one component of firms' broader strategic response to worker demands.

3.1 The Logic of Union Avoidance

Under the basic assumption that strikes and other work stoppages are costly—both in terms of lost productivity and reputational damage—firms are incentivized to address employees' concerns when the threat of disruptive action is high. Faced with this pressure, firms have developed a range of responses that can be organized into three broad categories: coercion, accommodation, and avoidance. First, firms may coerce workers directly through anti-union campaigns, threats of branch closures, or lobbying for tighter legal constraints on union strength (Samnani et al. 2016; DiGrazia and Dixon 2020; Hertwig and Thünken 2024). This route depends on the strength of labor law enforcement, management's capacity to mobilize resources, and the legal permissibility of anti-union activity. When coercion is costly or illegal, firms may instead choose to accommodate worker demands by raising wages or improving conditions—a logic that closely parallels efficiency wage theory (Akerlof 1982), in which firms pay above market-clearing wages to raise the cost of job loss and thereby reduce workers' incentives to engage in disruptive collective action. Management's willingness to concede to workers is, however, ultimately constrained by the preferences of boards of directors and shareholders, who prioritize profit margins and pressure managers to find solutions that placate workers without permanently raising costs. As a hybrid of the two strategies, management may therefore turn to union "substitution" or "avoidance" practices—formal grievance procedures, internal complaint systems, or the hiring of union-avoidance consultants—with the objective of channeling workers' demands through nonunion avenues (Heckscher 1988; Dundon 2002; Logan 2002, 2006, 2020). Union avoidance has become a booming industry: the Economic Policy Institute estimates that U.S. firms spend more than $430 million a year on union-avoidance consultants alone (McNicholas et al. 2023).

3.2 Three Channels of ESOP Adoption

Since employee ownership is neither equivalent to explicit union busting nor is it reducible to a simple accommodation of wage demands, any link between ESOPs and organized labor is best situated within this third category of avoidance. In practice, however, which strategy firms adopt depends on the extent to which workers have organized. I therefore highlight the distinction between workers, as a type of actor, and unions, as a type of institution. In their most essential form, workers are heterogeneous actors, varying across industries, firm types, and labor markets. Unions, by contrast, are the organized institution that some workers choose to form, with the explicit mandate of representing their collective interests. Conflating the two concepts, I argue, obscures which mechanism is at work, since whether workers have formally organized determines who initiates the ESOP, under what conditions, and toward what end. This analytical distinction also allows me to identify the three theoretical channels through which organized labor may shape ESOP adoption. In the first two, unionization occurs first at the firm level, while in the third, unionization is a threat that exists outside the firm itself.

The first way in which workers' interests may shape ESOP adoption is through *collective*

bargaining. In this account, workers organize within the firm, gain bargaining power, and then use that leverage to negotiate employee ownership as part of a collective bargaining agreement. ESOPs are known for their heterogeneity (Blasi and Kruse 2017), and given the genuine benefits these plans can offer workers in some cases (Tucker et al. 1989; Jones and Kato 1995; Harbaugh 2005; Kim and Ouimet 2014; Xiao et al. 2019), it is plausible *a priori* that unions might demand them directly. As such, this approach implies that ESOPs are a worker-driven outcome, with agency resting within unions rather than management.

The second channel, *union containment*, captures a more adversarial possibility inside unionized firms, one in which management uses employee ownership not to satisfy workers' demands but to undermine the union that represents them. Facing an organized workforce, management may offer employee ownership as a way to stabilize labor-management relations or substitute for the wage demands that collective bargaining might otherwise produce (Cramton et al. 2015). This approach indicates that ESOPs function not as a bargaining win for workers but as a tool for neutralizing the union internally. As discussed above, the existing evidence on this channel is limited and mixed: Kruse (1996) finds no relationship between a firm's union status and ESOP adoption, while Chen et al. (2020) find some strategic adoption among firms already facing an organizing drive. Yet, while informative, this lens is inherently limited; by focusing on firms that are already unionized, adoption that happens before workers formally organize is excluded from the analysis.

The third channel, *preemptive avoidance*, is the one most directly connected to the substitution logic outlined above and operates before workers have organized at all. Here, the relevant source of pressure is not necessarily unionization inside the adopting firm, but the broader threat of unionization in the surrounding labor market or institutional environment. When unions are strong in a state or industry, non-union firms may view organized labor as a credible threat and adopt ESOPs preemptively in order to reduce the appeal of collective bargaining. Employee ownership may thus operate as a "pre-distributive" buffer, offering workers a limited ownership stake before labor demands become disruptive rather than redistributing surplus through collective bargaining after conflict has emerged. Higher unionization levels across a society should produce more ESOPs, as management responds strategically to the possibility of future organizing within their own firm.

Table 1 summarizes the three mechanisms. At the firm level, these channels are assumed to be mutually exclusive—the ESOP is either demanded by workers, offered by management to contain an existing union, or adopted preemptively before workers have organized—though they all may operate simultaneously across different firms in the same society. All three, of course, rest on the prior assumption that workers' interests are in fact related to the emergence of employee ownership in the first place. However, since ESOPs are costly and administratively complex to establish, their adoption requires a clear impetus on the side of either workers or management, one which is plausibly connected to the strength of organized labor.

Table 1: **Three Channels of ESOP Adoption**

	Collective bargaining	**Union containment**	**Preemptive avoidance**
Who initiates	Union	Management	Management
Firm's union status	Unionized	Unionized	Non-unionized
Management's goal	—	Weaken existing union	Prevent unionization
Workers' goal	Gain ownership stake	—	—

3.3 The Case for Avoidance

All three channels largely predict the same positive aggregate relationship between union strength and employee ownership, which raises the question of which mechanism is most likely to dominate in practice. Although it is difficult to observe managers' intentions directly, especially when the relevant motivation is socially undesirable, several considerations make *preemptive avoidance* the most plausible account.

The *collective bargaining* channel is the easiest to evaluate empirically, and the evidence against it is fairly direct. Only a small share of active ESOPs are collectively bargained, approximately two percent, which implies that employee ownership is rarely introduced as part of a union contract. If unions were the primary force demanding employee ownership, it would follow that a far higher share of plans would originate through formal bargaining. Additionally, as discussed above, recent work finds that unionized employee-owners report more negative views of management and workplace relations, suggesting that employee ownership may emerge in workplaces already marked by labor-management tension rather than only in cooperative settings (Foley et al. 2025).

The *union containment* channel is harder to dismiss outright, since it does not require ESOPs to be collectively bargained—only that managers inside unionized firms employ them strategically. Cramton et al. (2015) offer some evidence for this channel, showing that the announcement of an ESOP at unionized firms generates larger positive stock market reactions than at non-union ones, consistent with the idea that employee ownership creates value for firms by softening union bargaining power. However, if this was the sole mechanism, one would expect unionized firms to be systematically more likely to adopt ESOPs than non-union ones, which is not necessarily the case (Kruse 1996; Foley et al. 2025). Additionally, by the same logic, management would *a priori* find it more effective to offer employee ownership preemptively, before workers have organized, rather than after a union is already in place and the stakes of doing so are higher. Of the three channels, *preemptive avoidance* is therefore the most theoretically compelling and one that has not been directly tested.

3.4 Evidence from Union Newspapers

In addition to this theoretical reasoning, distinguishing between worker-driven and management-driven accounts also requires knowing something about how firms have historically thought about ESOPs, and whether they have employed them as a strategic response to labor pressure. Observing these intentions directly is difficult: firms do not announce that they are adopting employee ownership plans to reduce the appeal of collective action, and the quantitative design that follows cannot fully distinguish between ESOPs demanded by workers and those offered preemptively by management.

I therefore incorporate archival evidence to assess whether the avoidance logic is consistent with how unions have interpreted the plans being offered to them. If employee ownership is primarily a management instrument, one would expect union organizations to have treated ESOPs with suspicion rather than enthusiasm—viewing them as management tools rather than worker gains. To assess this expectation, I conduct a text analysis of all references to employee ownership in *AFL-CIO News* from 1955 to 1996.[1] The *AFL-CIO News* was the official weekly newspaper of the American Federation of Labor and Congress of Industrial Organizations—the largest labor federation in the United States during this period, representing the vast majority of organized workers across more than 50 affiliated unions. Its newsletter functioned as a reporting and discussion vehicle rather than a statement of official positions, and the articles reviewed here should be read accordingly. To examine the historical framing of ESOPs in union discourse, I searched the archive programmatically across the 18,906 pages digitized by the Internet Archive for five terms related to employee ownership. Each keyword match was then manually reviewed to identify dedicated articles in which employee ownership was the primary subject. While these sources reflect the perspective of organized labor rather than management, and cannot be taken as direct evidence of firms' strategic intentions, they provide a window into how unions interpreted the plans being offered to them—and whether that interpretation is consistent with an avoidance logic or a bargaining one.

Across the four decades covered by the newsletter, the dominant tone is one of skepticism, and in several cases outright hostility, toward employee ownership plans. Following the passage of ERISA in 1974, the AFL-CIO published a piece warning that ESOPs are "typically initiated by the employer and frequently offered as a means to get something for nothing" (1976, p. 391). In a similar vein, a 1987 piece outlining guidelines for how unions should interact with ESOPs documents a case in which "a firm attempted to use an ESOP to defeat the union, planning to sell 60 percent of its stock to a few managers and financiers at an average price of $3.30 a share and to sell the remainder to employees at over $44 a share" (1987a, p. 303). A separate article from the same year warns that ESOPs should be "handled with care," noting that the plans had been used "by anti-union management to thwart workers' desire for a union" (1987b, p. 438). The following year, the federation's position was stated plainly: "worker ownership is not a trade union goal" (1988, p. 195). Throughout the 1980s and 1990s, numerous stories

[1] A full description of the search and coding procedure is provided in Appendix B.

describe ESOPs being offered in exchange for wage concessions (1990, p. 47; 1992a, p. 28), and by 1992, AFL-CIO Secretary-Treasurer Thomas R. Donahue had come to compare ESOPs to fire—"a faithful servant but a cruel master" (1992b, p. 252).

Beyond general skepticism, the archive also contains a series of cases in which unions entered ESOP agreements only to find the arrangements working against them. A 1994 piece titled "Shipbuilders Contest ESOP Rip-Off" recounts how employees at the Avondale Shipyard in New Orleans watched the value of their ESOP holdings fall sharply after agreeing to the deal. Framing the case as a cautionary tale, the author encourages workers not to be "intimidated by slick corporate tactics" (1994, p. 136). The following year, a story about the North American Rayon Corporation plant in the small northeastern Tennessee town of Elizabethton describes workers who felt "double-crossed," alleging that "the spirit of the ESOP" had been abandoned: "Management is abusing the intention of the ESOPs. It's like workers giving up wages to buy a car and then being told they can't drive it" (1995, p. 4). Stripped of voting rights, workers continued to lose jobs with little recourse. The pattern continues in a 1996 story about Avis workers in Florida. One employee owner calls the ESOP "a pig in a poke to us," arguing that workers stand to receive only a fraction of what they were promised: "Management is watching out for itself; we have to watch out for ourselves" (1996, p. 161). Here too, the workers had "no say in who sits on the board" or whether management was "acting in their best interest."

This fear of workers being "duped" by an ESOP arrangement is not unique to the AFL-CIO newsletters. In an account of United Air Lines' 1994 ESOP adoption, *Labor Notes*, a grassroots labor media and organizing project, describes the negative consequences of the program on union strength (Miah and Sheppard 2000):

> "But the ESOP's worst effect was on union consciousness. After the ESOP, many workers no longer saw the value of unionism. We were now 'owners.' Union solidarity was out the window. The ESOP marked a turning point in labor-management relations. Most employees, union and non-union, were led to believe that a 'new type of company' was created by our becoming 'owners.' Management got labor peace and unprecedented cooperation on issues ranging from work rules to contracting out. They also got a massive tax write-off."

More recent evidence from labor activists supports this interpretation as well. In an article contrasting worker cooperatives with ESOPs, the United Electrical, Radio and Machine Workers of America (2018), a rank-and-file union founded in the 1930s, stated that it has always "vigorously opposed" ESOPs because they have "frequently been used by bosses as a means of getting workers to accept wage concessions."

Although these sources are largely editorial, and there are also cases in which ESOPs successfully preserved jobs during economic downturns, the overall pattern is difficult to reconcile with a story in which unions are the primary force demanding employee ownership. The archive instead suggests that unions have understood ESOPs as a management instrument rather than a bargaining win, consistent with both the *union containment* and *preemptive avoidance* chan-

nels. This sets the stage for the paper's central empirical expectation: if workers' interests are a determinant of ESOP adoption, then firms should be less likely to offer plans when organized labor is weakened (*Hypothesis 1*). This effect should also be concentrated in sectors where union strength has traditionally been highest and the threat of organizing is most credible (*Hypothesis 2*). The remainder of the paper tests these expectations directly, using quasi-exogenous variation in union strength to estimate the effect of organized labor on ESOP adoption.

4 Data and Methods

4.1 Data

Since the ESOP-union relationship is relatively underexplored (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 private-sector employers that offer any kind of employee benefit plans covered by ERISA (Employee Retirement Income Security Act) must fill out this form annually. Due to known data quality issues in 1999, I exclude this year from the analysis to avoid biasing the results. The final dataset thus spans all 50 U.S. states from 2000 to 2024, the last year for which complete data are available across all covariates.[2]

To measure union strength, I rely primarily on union density, defined as the share of employed workers who are union members within a given state. These data are drawn from Hirsch et al. (2026), which is derived from U.S. Bureau of Labor Statistics (BLS) Current Population Survey microdata. For robustness, I also use measures of union coverage, union density in the private sector, and union coverage in the private sector, all from the same source. However, the union density and union coverage measures are strongly correlated (Pearson's $r = 0.995$), so I mainly use union density as the key indicator of union strength.[3] While I also have data on the number of work stoppages per state-year, the BLS data only records stoppages involving more than 1,000 participants, which typically occur at large firms operating across multiple states. The volatility of this measure complicates identification, so I prefer the simple union density variable. Finally, data on other covariates come from a variety of sources. The state unemployment rate, real GDP per capita, and real median household income are drawn from the *Federal Reserve Bank of St. Louis* (FRED). The number of business establishments per state comes from the Census Bureau's Statistics of U.S. Businesses (SUSB), and state population from the Census Bureau's NST-EST annual estimates. Manufacturing's share of total employment is constructed from the Bureau of Labor Statistics Current Employment Statistics (BLS CES) state-level series. The binary left-wing indicator equals 1 if a state's governor is affiliated with the Democratic Party and 0 otherwise, coded manually using *National Governors Association* (NGA) records. I note that some of these variables might be bad controls—more on this later.

[2] More information on the construction of this dataset can be found in Appendix A.

[3] The primary analysis is conducted with multiple indicators of union strength in Appendix Figure A4.

Table 2: **Descriptive Statistics (2000–2024)**

Variable	Obs.	Mean	Std. Dev.	Min	Max
Employee Ownership					
Number of ESOPs	1250	140.3	152.6	6.0	1234.0
Union Measures					
Membership rate, total	1250	10.66	5.38	1.63	26.70
Coverage rate, total	1250	11.99	5.40	1.94	27.68
Membership rate, private	1250	6.47	3.43	0.81	16.91
Coverage rate, private	1250	7.24	3.50	1.38	17.54
Covariates					
Manufacturing employment (thousands)	1250	263.9	268.3	8.7	1854.0
State population (millions)	1250	6.23	6.91	0.49	39.56
Manufacturing employment (%)	1250	4.34	1.83	0.83	11.11
Unemployment rate (%)	1250	5.27	2.02	1.00	13.68
Number of firms (thousands)	1150	121.4	133.6	15.9	844.6
Real GDP per capita (thousands)	1250	55.0	11.1	32.7	92.0
Real median income (thousands)	1250	74.2	12.8	42.2	115.8
Left-wing governor (0/1)	1250	0.439	0.496	0.000	1.000

I present summary statistics at the state level in Table 2. While a more granular unit of analysis (*e.g.*, the firm) would have offered a different perspective, a broader aggregation strategy most closely aligns with the theoretical framework proposed above; if offering an ESOP is understood as union avoidance, then the relevant environment is not only the firm itself, but also the general labor-market context in which unionization is either more or less likely to emerge. Put differently, unionization at the firm level should not necessarily be observed if the primary mechanism is inherently *preventive*.

4.2 Limitations of Two-Way Fixed Effects

Using these data, the primary objective is to estimate the causal effect of union strength on ESOP formation and stability. One straightforward approach would be to conduct a conventional two-way fixed effects (TWFE) specification. While this analysis is reported in Appendix E, there are several reasons why a simple TWFE design may not yield reliable estimates in this setting. These concerns fall into three broad categories: (1) bias arising from heterogeneous treatment effects in staggered panel settings, (2) the strong assumptions required for causal interpretation in fixed-effects models, and (3) the difficulty of defining credible counterfactual trends when treatment is continuous rather than binary.

First, there is a growing consensus that TWFE results are biased when more than two periods τ are examined and treatment effects are heterogeneous (de Chaisemartin and d'Haultfoeuille 2020; Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Baker et al. 2026). Since a standard TWFE estimator is a weighted average of all possible 2×2 difference-in-difference comparisons, some weights could become negative if the effect of RTW on ESOP formation varies across states

or years. This heterogeneity is quite likely in this case, particularly given the idiosyncratic nature of U.S. states, pulling the aggregate estimate toward zero or even reversing its sign entirely.

Beyond these estimator-specific concerns, a causal interpretation of FE models also requires strong, often untested, assumptions that are difficult to justify in this context (Vaisey and Miles 2017; Leszczensky and Wolbring 2022). The first assumption is strict exogeneity: changes in union strength must not themselves respond to prior levels of ESOP activity. However, this assumption is tenuous because ESOP adoption may influence subsequent labor relations and unionization dynamics. One possible response would be to introduce lagged first differences or dynamic panel specifications, but these approaches introduce their own complications.[4] More fundamentally, however, is that causal interpretation also depends on beliefs about the counterfactual trajectory of ESOP formation. This parallel trends assumption becomes more difficult to establish when the "treatment" is always present and continuously varying, rather than a binary treatment cleanly switched "on" (Morgan and Winship 2007; Vaisey and Miles 2017). Since union density evolves gradually over time, it is unclear what should constitute the relevant untreated counterfactual in this case. Additionally, current levels of union strength may reflect cumulative effects built up over many years, complicating efforts to distinguish the effect of unionization today from its influence five, ten, or twenty years earlier.

Taken together, these issues suggest that a conventional TWFE strategy is better suited to identifying correlations than causal effects. A more credible design for this context therefore requires plausibly exogenous variation in union strength. As such, the most straightforward design choice is to exploit the staggered adoption of right-to-work (RTW) laws—legislation whose causal effect on union density is well-established—and to use RTW adoption as the source of variation through which the effect of union strength on ESOP formation can be identified. Before explaining this design in greater depth, however, the following section situates RTW laws historically and explains why examining their adoption may be useful.

4.3 Right-to-Work Laws

With the passage of the Taft–Hartley Act (Labor Management Relations Act) in 1947, Congress amended federal labor law to prohibit the closed shop and granted states the authority to enact so-called right-to-work (RTW) laws under Section 14(b). These laws further prohibited union-security agreements—including union shop and agency shop arrangements—that required workers to join a union or pay representation fees as a condition of continued employment. Although unions remained legally obligated to represent all workers in a bargaining unit, RTW laws weakened unions financially by allowing employees to receive the benefits of collective bargaining without contributing dues. 19 U.S. states initially adopted these laws in the two decades following the Second World War, with 26 out of 50 states ultimately maintaining them

[4] Monte Carlo simulations suggest that dynamic models perform poorly when the true timing of causal effects does not match the imposed lag structure (Vaisey and Miles 2017), a likely concern here given the long planning horizon involved in establishing an ESOP. Additionally, dynamic panel estimators inherently induce correlation between lagged dependent variables and the error term, generating the Nickell (1981) bias of order $1/\tau$.

(as of 2026). Displayed in Figure 1, adoption years are somewhat clustered yet still exhibit significant variation over time.

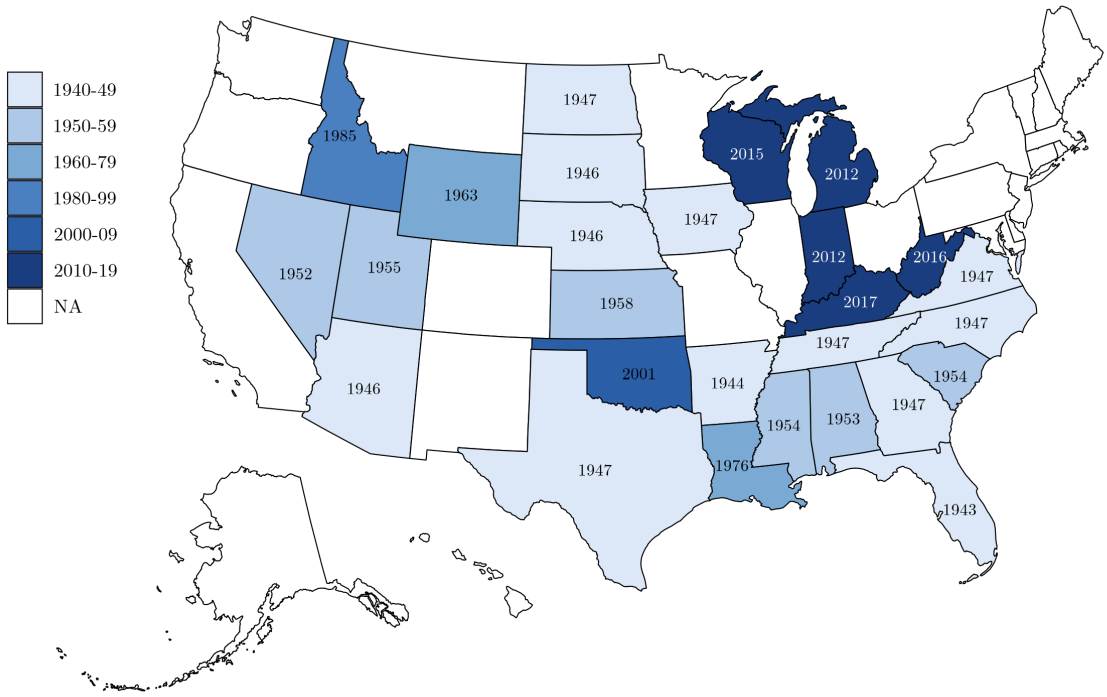

Figure 1: **RTW Adoption Across U.S. States**
Year of RTW adoption is shown for each adopting state. Color indicates the decade of adoption. White states had never adopted RTW laws as of 2024. Where the announcement or passage date of an RTW law differs from its effective date, the former is used to prioritize the point at which the law may have begun shaping economic expectations.

Despite its name, right-to-work legislation was primarily promoted by business interests seeking to weaken organized labor. By prohibiting union-security agreements while still requiring unions to represent all workers covered by a collective bargaining agreement, RTW laws reduce unions' financial resources and create incentives for workers to "free ride." Unions have historically rejected RTW laws for these reasons, and RTW states possess consistently lower union density rates than non-RTW states (Chun 2023). Recent studies using advanced identification methods have found that RTW laws do, indeed, cause a reduction in unionization. Focusing on the wave of Midwestern RTW adoptions during the 2010s, Fortin et al. (2022) estimate that RTW laws caused nearly a 2 percentage point decline in unionization rates within five years of adoption. Using the same set of Midwestern states, Butler (2024) applies a synthetic difference-in-differences approach and finds that RTW laws reduced union membership by roughly 1 percentage point overall and by about 2 percentage points in more highly unionized industries. Expanding the sample to include Oklahoma alongside these Midwestern adopters, Chun (2023) similarly finds that post-2000 RTW laws reduced private-sector union coverage by more than 10 percent. Collectively, these findings support the treatment of RTW laws as a quasi-exogenous shock affecting union strength.

Following these studies, I focus on the five Midwestern states that adopted RTW laws in the 2010s: Indiana (2012), Michigan (2012), Wisconsin (2015), West Virginia (2016), and Kentucky (2017). This choice is motivated by two considerations. First, ESOP data are only available through the Form 5500 filings as of 2000, so this approach reduces the measurement error in the dependent variable that would arise from relying on earlier or alternative data sources. More importantly, however, restricting the analysis to these five states helps hold important sources of historical variation relatively constant. For example, grouping Indiana in 2012 with Wisconsin in 2015 is more analytically coherent than comparing either case to Nevada's RTW adoption in 1952, which emerged from a very different context. Furthermore, across all five Midwestern states, the political circumstances surrounding RTW adoption were broadly similar: each law was enacted following Republican electoral victories in the aftermath of the 2008 financial crisis. Interestingly, RTW legislation was promoted aggressively by Republican policymakers during this era not only as an economic development strategy *per se*, but also as a means of weakening the political influence of organized labor and its longstanding financial support for the Democratic Party (Fortin et al. 2022; Chun 2023).

However, while these five Midwestern cases share broadly similar political and historical conditions, they are not identical. Indiana and Kentucky most closely resemble the prototypical Midwestern RTW transition emphasized in the recent literature. Both states possessed historically significant manufacturing sectors and moderate levels of unionization prior to adoption, yet shifted toward Republican control in the aftermath of the Great Recession. West Virginia differs somewhat from the other Midwestern adopters because of its historically exceptional dependence on extractive industries and its unusually strong legacy of labor militancy, particularly within coal mining. By the time RTW legislation was adopted in 2016, however, union density in the state had already been declining for decades alongside the contraction of the coal industry and the state's broader partisan realignment toward the Republican Party. RTW adoption in West Virginia therefore reflects both a continuation of long-run deunionization trends and a broader transformation in the state's political economy.

Wisconsin is somewhat more complicated. Governor Scott Walker's broader anti-union agenda substantially preceded the state's formal RTW adoption in 2015, most notably through the passage of Act 10 in 2011, which sharply restricted collective bargaining rights for public-sector unions. As a result, firms and workers may have adjusted their expectations and behavior prior to the formal implementation of RTW legislation itself. This raises potential concerns regarding the no-anticipation assumption and makes it more difficult to isolate the independent effect of the 2015 RTW law. To address this issue, I conduct the analysis both including and excluding Wisconsin from the treated sample. Finally, Michigan also presents a more nuanced case. Given the state's central historical role in American industrial unionism, the adoption of RTW legislation in 2012 represented a major shift. However, RTW was repealed in 2023 under Governor Gretchen Whitmer, with the repeal taking effect the following year. As such, Michigan no longer has active RTW legislation, and I pay special attention to this state in the

later years of the analysis. Robustness checks confirm that the main results are unchanged when Michigan's post-repeal years are excluded from the sample. Figure 2 presents descriptive trends in mean union density for the five treated Midwestern states relative to both always-treated and never-treated states.

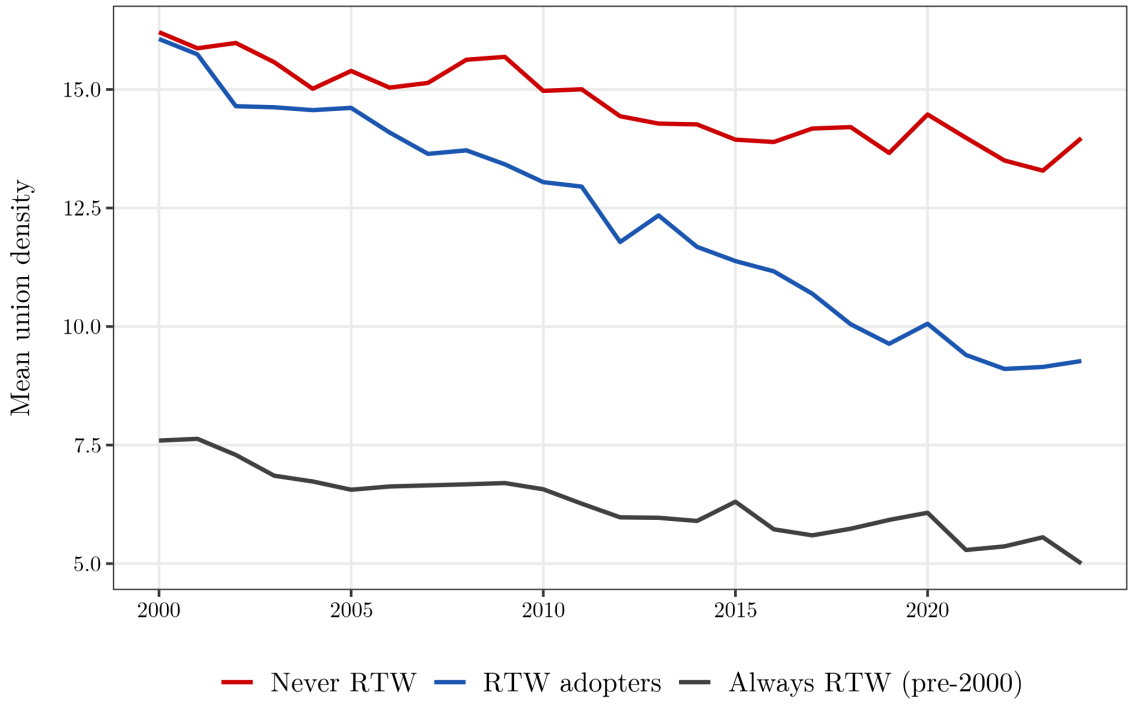

Figure 2: **Union Density by RTW Group, 2000–2024**
Mean union density (total) by group: never-RTW states, the five RTW-adopting Midwestern states (IN, MI, WI, WV, KY), and always-RTW states (pre-2000 adopters).

I also note that Oklahoma and Missouri are excluded entirely from the analysis, consistent with previous studies (Fortin et al. 2022; Butler 2024). While Oklahoma adopted RTW legislation in 2001, the early adoption date leaves too few pre-treatment observations in the Form 5500 data to convincingly establish parallel trends. I also exclude Missouri, which passed and then repealed RTW legislation between 2017 and 2018. This unusually short and contested policy episode makes it difficult to classify the state cleanly as either treated or untreated.

4.4 Staggered Adoption of RTW

With this context in mind, I estimate the effect of RTW laws on employee ownership using a difference-in-differences and event-study design. The identification strategy thus treats the staggered adoption of RTW legislation as a plausibly exogenous shock to organized labor, allowing changes in ESOP formation across treated and untreated states to be compared over time. Under the assumption that RTW laws affect ESOP formation only through a reduction in union strength, the results can be interpreted as reduced-form evidence of the causal effect of union strength on employee ownership (Fortin et al. 2022).

This interpretation, however, relies on the strong exclusion restriction. In reality, RTW laws may influence employee ownership through channels other than union strength, including broader shifts in economic conditions, credit access, industry composition, or firm relocation. To address this concern, I estimate the analysis both with and without controls for union strength. If RTW laws affect ESOP formation primarily through the weakening of organized labor, then controlling for union density should attenuate the reduced-form RTW estimate toward zero. Although this exercise can only provide suggestive evidence, it helps evaluate whether the effect operates through organized labor rather than through alternative channels. Potential violations of this exclusion restriction are discussed in greater depth in Section 5.

The empirical strategy employed here is also consistent with an established literature that uses RTW adoption as a source of quasi-exogenous variation in organized labor to study downstream social, political, and economic outcomes. Prior work has used RTW laws to examine effects on wages (Fortin et al. 2022; Butler 2024), electoral politics (Frymer et al. 2025), working hours (Gihleb et al. 2023), and subjective well-being (Makridis 2019), among other outcomes. Finally, even if the enactment of RTW laws in itself is not random, this is not particularly problematic in the difference-in-differences case, which relies primarily on similarities in pre-treatment trends rather than fully randomized treatment assignment.

4.5 Model Specification

To account for heterogeneous treatment effects across cohorts and over time, I employ the staggered difference-in-differences framework most prominently developed by Callaway and Sant'Anna (2021). This approach avoids the "forbidden" comparisons that arise in conventional two-way fixed effects models when already-treated units are inappropriately used as controls for later-treated units (de Chaisemartin and d'Haultfoeuille 2020; Borusyak et al. 2024; Sun and Abraham 2021; Goodman-Bacon 2021; Huntington-Klein 2021). I prefer the Callaway and Sant'Anna (2021) estimator over Sun and Abraham (2021) given the small number of treated cohorts in this setting. 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,

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

in which $Y_{it}(1)$ and $Y_{it}(0)$ denote potential outcomes with and without RTW adoption respectively, and $X_{s,t-1}$ represents a vector of lagged covariates. I measure the outcome as the stock of active ESOPs per state-year, which captures the net effect of both new plan formation and dissolution, while also maximizing statistical power. Appendix Figure A5 disaggregates the effect into the formation and exit margins separately. 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, and Delaware) also attempted but narrowly failed to adopt RTW laws during this period. I also re-estimate the models using only never-treated states as the control group, and the results remain substantively unchanged (see Table A5). Finally, I employ the doubly robust estimator, which remains consistent as long as either the outcome model or the propensity score model is correctly specified (Sant'Anna and Zhao 2020; Callaway and Sant'Anna 2021). The propensity score component additionally reweights control units to resemble the treated cohort on pre-treatment observables, partially mitigating the concern that not-yet-treated states differ systematically from treated ones.

5 Results

5.1 Identification Assumptions

As with all difference-in-differences designs, identification relies on the assumption that treated and control states would have followed similar trends in ESOP formation in the absence of RTW legislation. In the staggered-adoption setting, this assumption must hold not only between treated and never-treated states, but also between treated states and the not-yet-treated states that serve as comparison units prior to treatment. To evaluate whether this holds in practice, Figure 3 provides descriptive trends in ESOP counts for treated and control states over time.

Since states adopting RTW laws differ systematically from never-treated states along several pre-treatment dimensions that may vary over time and also influence ESOP formation (see Appendix Figure A8), I follow the approach outlined by Callaway and Sant'Anna (2021) and Caetano and Callaway (2026), which allows for the parallel trends assumption to hold after conditioning on observed covariates. I therefore include a set of lagged state-level covariates designed to capture labor market conditions, industrial composition, and firm-side economic capacity. The preferred specification includes manufacturing employment share, unemployment rate, number of firms, and real GDP per capita.[5] Manufacturing employment share is especially important in this case because RTW laws were disproportionately present in highly unionized manufacturing states, making industry composition the primary potential confounder. The remaining controls capture broader economic conditions, the pool of ESOP-eligible firms, and the financial environment relevant to employee ownership formation. Controls are therefore selected to improve the plausibility of conditional parallel trends while minimizing multicollinearity. Following this logic, I do not incorporate state population and median household income because their explanatory power is largely captured by firm counts and GDP per capita. I also exclude the partisan composition variable because nearly all post-2000 RTW adoptions occurred under

[5]Some variables are transformed using the inverse hyperbolic sine (IHS) transformation, defined as $\text{IHS}(x) = \ln\left(x + \sqrt{x^2+1}\right)$, based on skewness diagnostics and model fit tests. IHS is preferred to the natural log because it is defined at zero while retaining a near-logarithmic interpretation for large values.

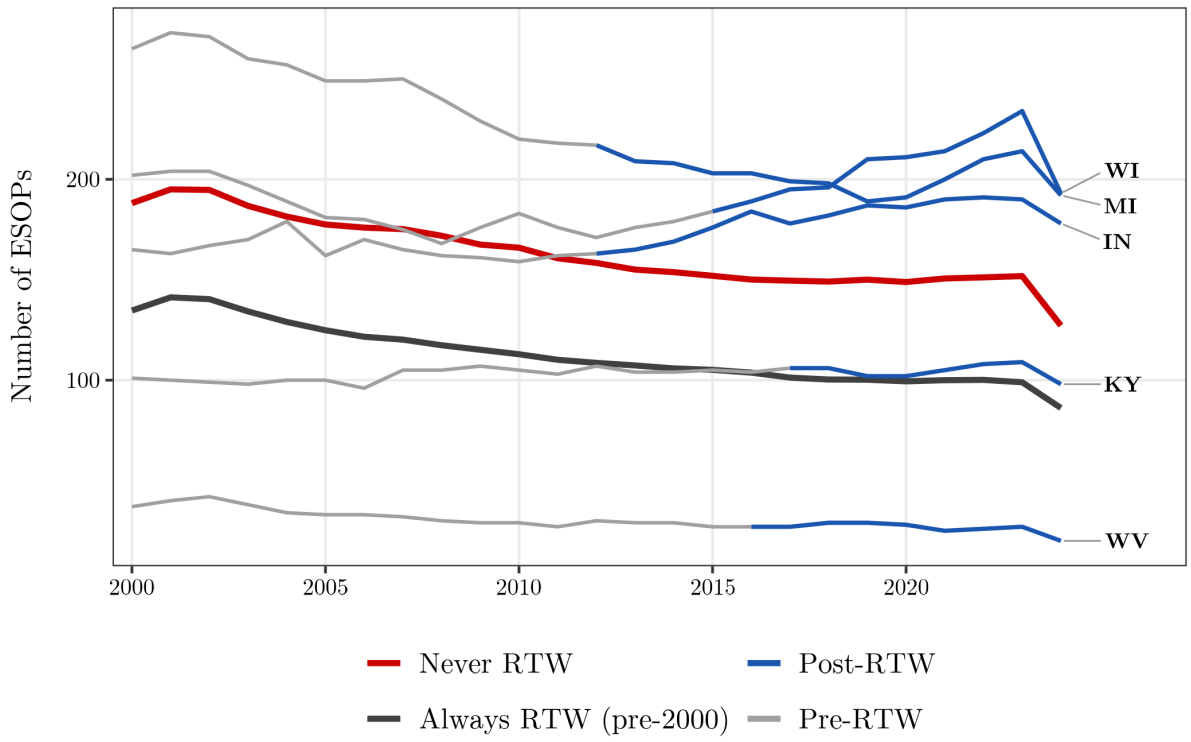

Figure 3: **ESOP Count by State, 2000–2024**

ESOP count per state. Group averages for never-RTW (red) and always-RTW (black) states shown as thick lines. Individual RTW-adopting states shown in grey (pre-adoption) and blue (post-adoption), labeled at the final year.

Republican control. As a result, partisanship almost perfectly predicts treatment assignment, creating poor overlap between treated and control states, which ultimately leads to unstable propensity score estimates and inflated standard errors. Among the specifications considered, the selected set of controls produces the flattest pre-treatment event-study coefficients across both estimation samples, providing the strongest empirical support for the identifying assumption. All covariates are lagged by one year to reduce the risk of post-treatment bias.

Turning to the formal event-study estimates after incorporating these covariates (Figure 4), the pre-treatment coefficients are visually stable and nearly all are statistically indistinguishable from zero for both union density and ESOP counts. However, one concern in settings with few treated units is that pre-trends tests may have limited power to detect violations of parallel trends even when they exist in reality (Roth 2022). To assess this, I conduct a formal power analysis using the `pretrends` package developed by Roth (2022), which computes the probability that the pre-trend test would return a significant result under a hypothesized linear violation. The analysis indicates that the test has greater than 99 percent power across all main specifications, suggesting that the flat pre-treatment coefficients reflect genuine stability rather than a failure to detect an existing trend.

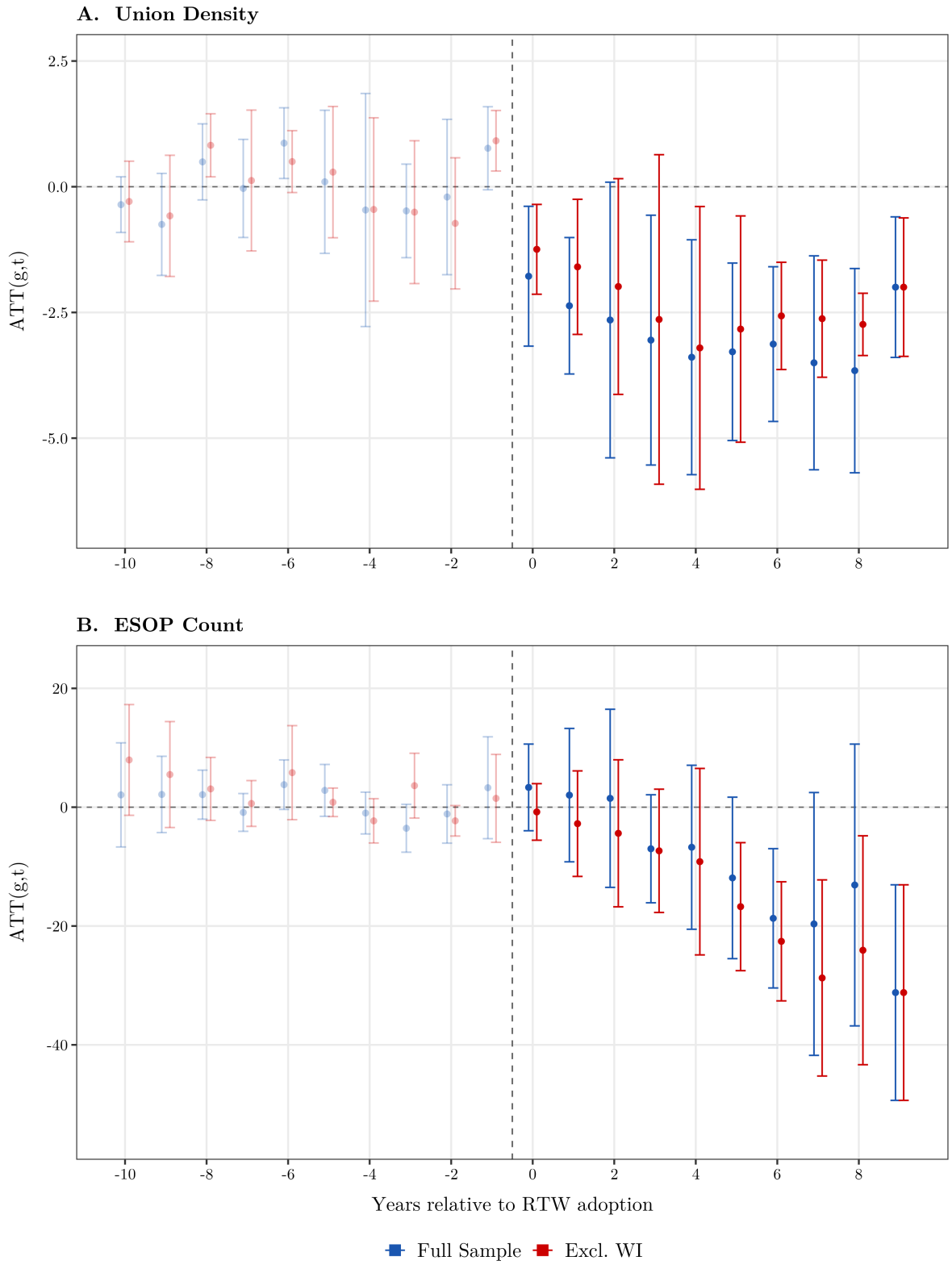

Figure 4: **Event Study Estimates of RTW Adoption on Union Density and ESOPs**
Doubly-robust ATT (g, t) estimates. Panel A shows the effect on union density; Panel B on ESOP count. Excl. WI series omits Wisconsin.

5.2 Primary Results

Having established that the identifying assumptions are plausible, 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 union density, (2) that the treatment has an effect on ESOPs, and (3) whether the treatment continues to affect ESOPs once union density is accounted for.

I present the results for each of the three stages in Table 3. 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 an immediate decrease in union density that builds over time, reaching approximately 3.1 percentage points (± 1.5) by six years after adoption. This matches the typically reported results of 2-4 percentage points in similar studies (Eren and Ozbeklik 2016; Fortin et al. 2022). In the second stage, I also find a negative effect of RTW on ESOPs. As depicted in the event study plots, the effect is not immediate; however, a temporal lag is to be expected since firms cannot form or cancel ESOPs overnight. Substantively, RTW adoption leads to an estimated decline of roughly 18.7 (±11.7) ESOPs per state by six years after adoption, with this estimate rising to 22.6 (±10.0) when Wisconsin is excluded. This estimate is substantively meaningful, considering that the average number of ESOPs per state is approximately 140 in this dataset. The results are also consistent when the analysis is restricted to counties bordering RTW-adopting states (Figure A3), as well as when ESOP formations are used in place of the total stock as the dependent variable (Figure A5). Finally, the effect of RTW on ESOPs attenuates and becomes statistically insignificant once union density is partialled out. This evidence supports the claim that the primary link between RTW and employee ownership occurs through shifts in organized labor's power, consistent with *Hypothesis 1*.

Table 3: **Effect of RTW on Union Density and ESOPs**

Outcome	**ATT**	**Std. Error**
First Stage: RTW → Union Density		
Union density	-2.848^{***}	(0.750)
Reduced Form: RTW → ESOPs		
ESOP count	-10.025^{*}	(4.604)
Conditional Effect: RTW → ESOPs		
ESOP count, controlling for union density	-7.316	(5.565)

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

Estimates are based on simple aggregation following Callaway and Sant'Anna (2021). Controls include one-year lags of manufacturing employment share, unemployment rate, log number of firms, and log real GDP per capita. Analytic standard errors are reported in parentheses.

5.3 Heterogeneity Analyses

Although aligned with the avoidance logic, attenuation alone cannot identify mediation. Rather, union density may, in part, proxy for the broader package of Republican policies that accompanied RTW adoption in the mid-2010s Midwest, which could have affected ESOP formation directly. I therefore conduct an industry heterogeneity analysis to shed additional light on this channel. Since firms in high-union industries face the strongest incentive to preemptively adopt employee ownership as a substitute for collective bargaining, the union avoidance mechanism predicts that the decline in ESOP formation should be concentrated precisely there (*Hypothesis 2*). To examine this, I disaggregate the main models by industry union density, classifying each ESOP-sponsoring firm into a high (>15%), medium (5–15%), or low (<5%) union density tier based on average 2-digit NAICS unionization rates and re-estimate the staggered difference-in-differences separately for each group. However, the small number of state-industry-year observations limits statistical power and makes point estimates sensitive to covariate specification, precluding strong conclusions about the concentration of effects across union density tiers. I therefore treat this as a descriptive exercise; full details are reported in Appendix D.

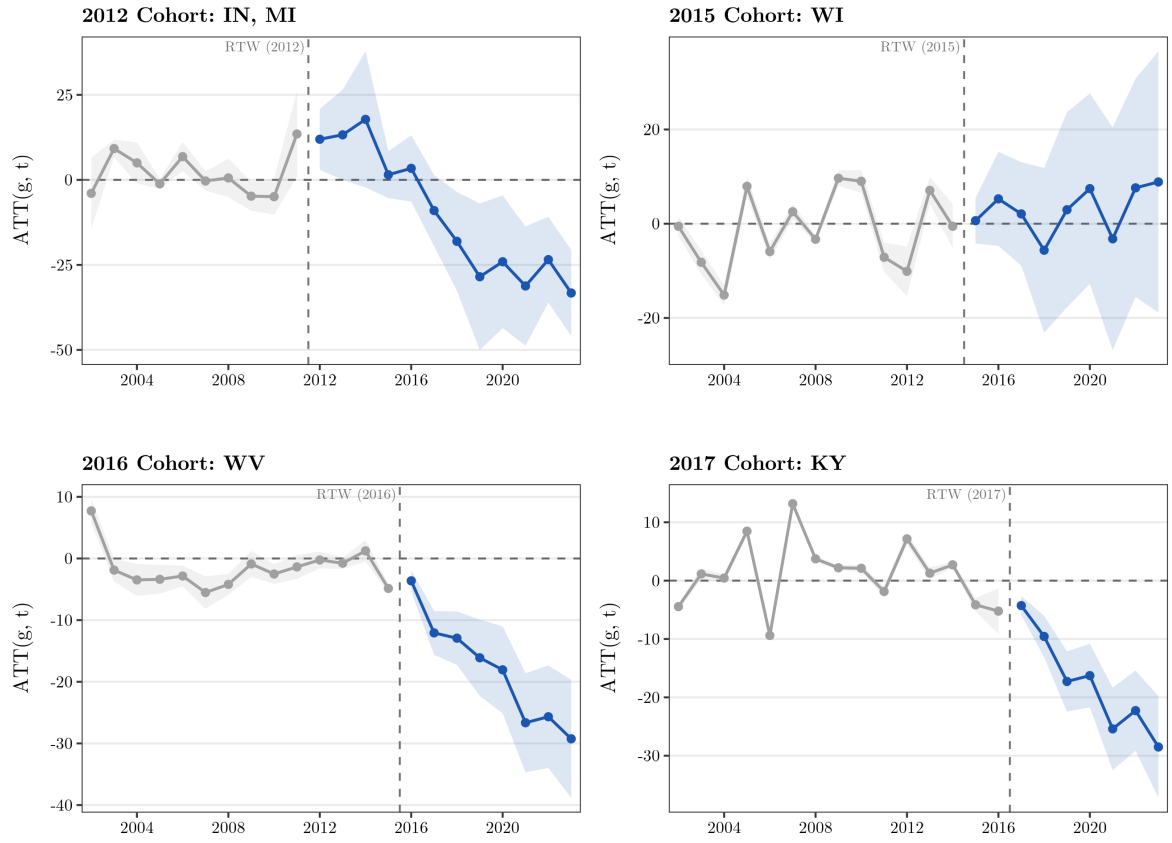

Figure 5: **Cohort-Specific Event Studies of RTW Adoption on ESOP Formation**
ATT (g, t) estimates by adoption cohort: Indiana and Michigan (2012), Wisconsin (2015), West Virginia (2016), and Kentucky (2017). Grey lines and shading denote pre-treatment periods; blue lines and shading denote post-treatment periods. Shaded bands show 95% pointwise confidence intervals.

In addition to the industry heterogeneity analysis, Figure 5 disaggregates the event-study estimates by treatment cohort (2012, 2015, 2016, and 2017) to assess whether the main finding holds across adopting states—particularly given Wisconsin's recurring sensitivity throughout the analysis. As expected, most cohorts display a negative relationship between RTW adoption and ESOP formation. Wisconsin, however, stands out: the estimated effects are statistically insignificant and the trends are considerably more volatile both before and after treatment. This instability complicates Wisconsin's role in the analysis and helps explain why the estimates become stronger and more stable when the state is excluded from the sample.

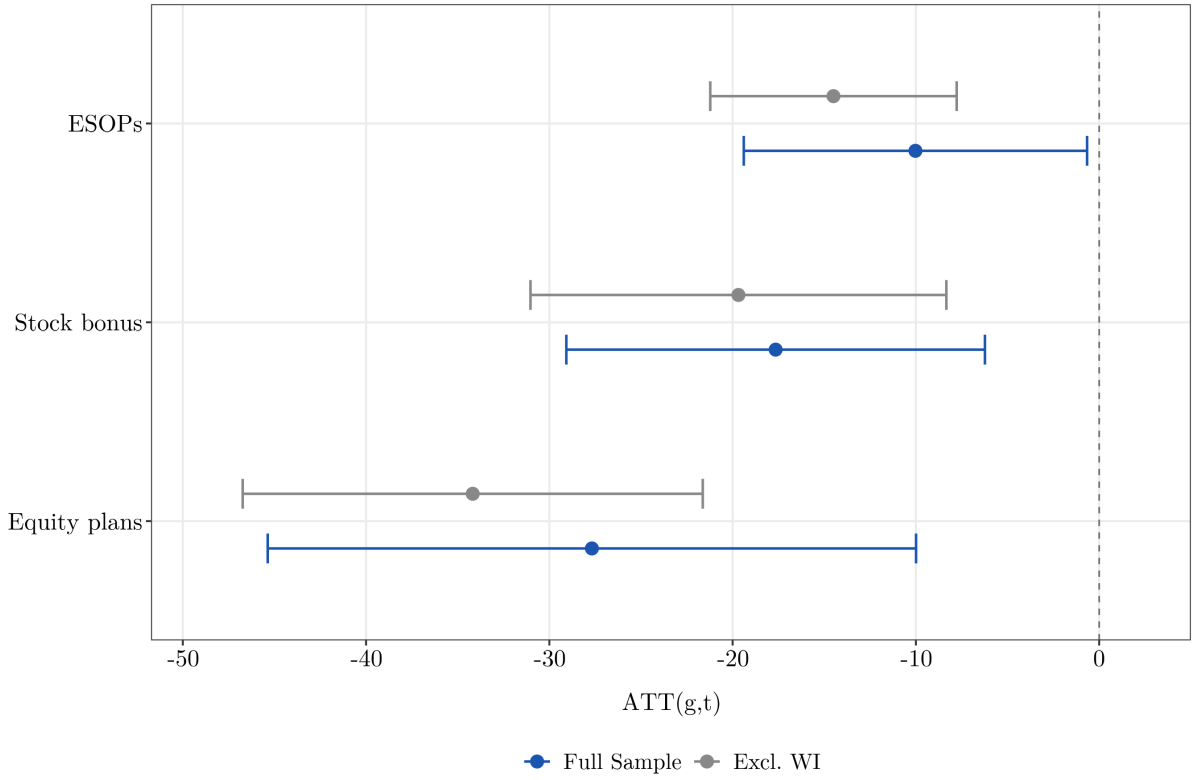

Figure 6: **ATT Estimates Across Equity Outcome Definitions**
Overall ATT estimates for three equity-related outcomes: ESOPs, stock bonus plans, and combined equity plans (ESOPs and stock bonus). Full sample (blue) and excluding Wisconsin (grey). Error bars show 95% confidence intervals.

5.4 Broader Equity Arrangements

Finally, if RTW affects employee ownership through the union avoidance channel, the effect should not necessarily be limited to the specific legal structure of ESOPs, but should instead extend to other equity-based arrangements that serve a similar function. To examine this, I re-estimate the models using all equity-based employee plans—including both ESOPs and stock bonus plans—as the outcome variable. Whereas ESOPs are retirement-oriented ownership trusts designed to provide broad-based employee ownership, stock bonus plans function more flexibly as compensation arrangements that distribute company stock to workers. Since ESOPs are the

most institutionally distinct and theoretically relevant form of shared capitalism, they remain the primary focus of the paper; however, if the findings were driven purely by the legal or accounting structure of ESOPs rather than by the underlying avoidance dynamic, they should not replicate when the outcome is broadened. Reassuringly, Figure 6 reveals that the estimated effect of RTW adoption remains negative and is, if anything, somewhat stronger when both plan types are included—suggesting that the relationship is not an artifact of ESOP-specific features but instead extends to equity-based arrangements more broadly. Further robustness checks are reported in Appendix C.

6 Discussion and Concluding Remarks

Through a staggered difference-in-differences and event-study design, this paper finds that right-to-work laws reduce union density by approximately 3.1 percentage points (± 1.5) and the number of active ESOPs by roughly 18.7 plans per state (± 11.7) six years after adoption. The effect on employee ownership is not immediate—firms cannot form an ESOP overnight—but it grows more substantively and statistically meaningful over time. The findings are also robust across alternative estimators, sample definitions, and ways of operationalizing the key concepts. Most directly relevant to the paper's central claim, the reduced-form effect of RTW on ESOPs becomes statistically insignificant once union density is accounted for, consistent with *Hypothesis 1*. Disaggregating the entry-exit margins further reveals that the decline operates primarily through the suppression of new plan formation rather than the accelerated exit of existing ones, consistent with the *preemptive avoidance* logic. Heterogeneity analyses also illuminate the proposed mechanism, although with important caveats. The cohort-specific event studies show that the negative relationship between RTW adoption and ESOPs holds across most adopting states, with Wisconsin as a notable exception. This empirical pattern is consistent with the idiosyncratic features of this case, where the state's pre-existing anti-union legislative environment (Act 10, 2011) meant that the formal RTW adoption constituted less of a "clean" policy shock relative to other states. Additionally, an exploratory industry-level analysis provides suggestive, though inconclusive, evidence of heterogeneous effects across sectors with different unionization rates (*Hypothesis 2*).

In short, this paper finds robust causal evidence that the adoption of right-to-work laws reduces both union density and the number of ESOPs within a state. The mechanisms underlying this relationship, however, are somewhat less certain. Although I find union avoidance to be the most plausible explanation, several alternatives also deserve consideration. I discuss each here in turn.

The first question is which of the three theoretical channels—*collective bargaining*, *union containment*, and *preemptive avoidance*—is most likely driving the aggregate result. All three predict the same negative relationship between union strength and ESOPs, so the reduced-form estimates alone cannot distinguish between them. Several considerations, however, point toward *preemptive avoidance* as the most plausible account. The *collective bargaining* channel

is the most straightforwardly testable and the least consistent with the evidence: only about two percent of active ESOPs are collectively bargained, and historical union attitudes toward ESOPs were consistently skeptical and at times openly hostile. The *union containment* channel is more difficult to rule out, but the available evidence does not decisively support it either. If containment were the primary driver, one would expect unionized firms to be systematically more likely to adopt ESOPs than non-union ones, which is not what the literature finds (Kruse 1996; Foley et al. 2025). *Preemptive avoidance*, by contrast, operates before workers have organized at all. The relevant pressure is the threat of unionization in the surrounding labor market, which weakens precisely when RTW reduces the incentive of organizing.

A separate concern is whether RTW legislation affects ESOP formation through channels other than union strength—that is, whether the exclusion restriction holds. Four such potential violations of this assumption are considered here. First, it is possible that the decline in ESOPs reflects concurrent changes in the legal and regulatory environment surrounding employee ownership plans, rather than any shift in union strength. Although a series of legal developments reshaped the administrative environment for employee ownership during the period of analysis,[6] these changes were federal in scope and applied uniformly across states, meaning they cannot account for the differential trends between RTW-adopting and non-adopting states that the design identifies. Second, RTW may have proxied for broader economic deterioration in manufacturing-heavy Midwestern states, which could have independently reduced ESOP formation. This account, however, implies the opposite of what is observed: ESOPs are commonly used as vehicles for employee buyouts of distressed or closing firms, so a worsening economic environment would predict more employee ownership in treated states, not less. Additionally, the preferred specification controls for the lagged unemployment rate, GDP per capita, and the manufacturing employment share to directly account for this concern. Third, spillover effects across state lines—firms relocating to RTW states or workers migrating away from them—could contaminate the control group and thus violate the Stable Unit Treatment Value Assumption (SUTVA). The border county analysis in Appendix C addresses this concern directly by comparing counties on either side of a state border, where spillovers should be most pronounced. Results remain consistently negative with flat pre-trends, and become statistically significant once Wisconsin is excluded. Regardless, contamination of this kind would attenuate the estimated effect *a priori*, suggesting the true impact of RTW on ESOP formation is larger than what is reported here.

Fourth, the observed decline in ESOPs may reflect the broader Republican policy agenda that accompanied RTW adoption rather than the weakening of union strength *per se*. This concern is difficult to dismiss entirely, since RTW legislation followed Republican electoral victories in each of the five treated states. However, it is not immediately clear how right-wing governance would reduce ESOP formation independently of union strength. Unlike minimum

[6]These include IRC §409(p) restrictions on S-corporation ESOP structures (2001), the Pension Protection Act's new vesting rules (2006), the Department of Labor's fiduciary and valuation standards (2014), and the SECURE 2.0 provisions (2022).

wage legislation or collective bargaining rights, ESOPs have historically enjoyed broad bipartisan support, championed by figures as ideologically distant as Ronald Reagan and Bernie Sanders (Blasi and Kruse 2017). Regardless, it should be noted that RTW adoption is not strictly exogenous, and no identification strategy of this kind can entirely rule out alternative explanations. This limitation is not unique to this study, however; any attempt to identify the causal effects of union strength must necessarily face similar challenges (Fortin et al. 2022). Future research could strengthen the argument developed here by exploiting alternative shocks to union strength, such as industry-level exposure to the China import shock (Autor et al. 2013; Ahlquist and Downey 2023) or other aggregate labor-market disruptions that are not accompanied by concurrent political changes.

Ultimately, this paper's central contribution lies in shifting the level of analysis. Earlier work asked whether a firm's own union status predicts ESOP adoption (Kruse 1996; Chen et al. 2020), but if *preemptive avoidance* is the operative mechanism, the relevant pressure is the organizing threat in the surrounding labor market rather than unionization inside the firm itself. Examining this threat requires aggregate variation in union strength, which is what the RTW design provides. The results offer robust evidence that organized labor does, in fact, matter for whether firms adopt shared-capitalistic practices—a finding that speaks directly to the literature on how firms manage worker demands. Whether ESOPs ultimately benefit or disadvantage workers is a question this paper leaves open; the literature documents genuine gains in wages, wealth accumulation, and job stability in some settings (Kim and Ouimet 2014; Xiao et al. 2019), alongside cases of wage concessions, reduced bargaining leverage, and diminished union consciousness in others (Miah and Sheppard 2000; Foley et al. 2025). What the evidence here establishes is that the prevalence of these arrangements is not independent of the political and institutional conditions that determine how much power workers hold in the first place. To the extent that ESOPs represent a vehicle through which workers accumulate equity, a decline in their formation may have consequences not only for income inequality but for the distribution of wealth more broadly as well.

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Supplementary Materials

Appendix A: Data Sources and ESOP Panel Construction

Table A1: **Data Sources**

Variable	**Source**
Plan level	
Number of ESOPs	U.S. Department of Labor (EBSA)
State level	
Union density (total & private sector)	Hirsch et al. (2026)†
Union coverage (total & private sector)	Hirsch et al. (2026)†
Population	U.S. Census Bureau
Number of firms	U.S. Census Bureau (SUSB)
Manufacturing employment share	Federal Reserve Bank of St. Louis (FRED)
Real median household income	Federal Reserve Bank of St. Louis (FRED)
Unemployment rate	Federal Reserve Bank of St. Louis (FRED)
Real GDP	Federal Reserve Bank of St. Louis (FRED)
Governor party affiliation	National Governors Association (NGA)
Industry level	
Union density (total)	Hirsch et al. (2026)†
State-industry level	
Number of establishments	U.S. Bureau of Labor Statistics (QCEW)
Employment level	U.S. Bureau of Labor Statistics (QCEW)
Average weekly wages	U.S. Bureau of Labor Statistics (QCEW)

†Derived from U.S. Bureau of Labor Statistics (BLS) Current Population Survey microdata.

Table A1 summarizes the data sources used in this paper, with each entry hyperlinked directly to the specific dataset. State-level covariates were downloaded via the Federal Reserve Bank of St. Louis (FRED) API, and state-industry-level controls were downloaded from the Quarterly Census of Employment and Wages (QCEW) API. Where variables are available at monthly frequency, I use the non-seasonally adjusted series and compute annual averages; annual series are used directly. Real GDP per capita is constructed by dividing state real GDP by state population, yielding a per-capita measure in real terms.

The ESOP measure is the most complex variable in the dataset, as it must be constructed directly from raw Form 5500 plan-level filings hosted by the U.S. Department of Labor (DOL), which all private-sector organizations offering employer-sponsored benefit plans are required to fill out annually. Plans coded as "Type of Pension Benefit Code" 2O (unleveraged ESOP) and 2P (leveraged ESOP) are kept. The resulting ESOP panel runs from 2000 to 2024. Although data collection officially starts in 1999, I exclude this year because it marked the introduction

of the EFAST electronic filing system. To account for technical difficulties, the DOL extended the deadline for filing 1999 plans from July 31 to October 16, 2000, waiving late penalties for transition-year difficulties. Since the 1999 data in this project shows 27 percent fewer ESOP plans than 2000, this year is a key outlier and is therefore excluded. Additionally, the panel drops observations from Washington D.C., Guam, Puerto Rico, and the U.S. Virgin Islands, as well as a negligible number of records (< 0.1 percent) with missing or invalid state codes.

Each plan is tracked using the combination of its Employer Identification Number (EIN) and Plan Number (PN), which the DOL and IRS formally define as the unique 12-digit identifier for a pension plan. The PN is permanent—once assigned, it is never reused, even after termination—making EIN+PN a stable longitudinal key. I create a composite variable `plan_id = EIN_PN` accordingly. From this aggregation method, I find that the panel covers 19,986 unique ESOP plans over 2000–2024, with a median of 6 years observed per plan and a mean of 8.6 years. 97.8 percent of year-to-year transitions are consecutive, confirming that EIN+PN is a stable identifier across the panel. One complication is that a small share of plan-years appear more than once due to amended re-filings. To prevent double counting, I retain only one record per `plan_id-year`, preferring the observation with the highest active participant count. The duplicate rate is much higher in early panel years than in later years, reflecting well-documented filing difficulties during the initial EFAST electronic submission transition. Inspection of duplicate pairs confirms that over 97 percent are identical across plan name, state, and plan type, consistent with re-filings rather than genuinely distinct plans.

Plans are then geolocated using their sponsor's reported mailing address, submitted in batch to the U.S. Census Bureau Geocoder API. P.O. boxes and unmatched addresses fall back to ZIP code centroids via `zipcodeR`. Of 171,911 plan-year observations, 133,012 (77.4 percent) are matched by the Census Geocoder, 26,652 (15.5 percent) fall back to a ZIP centroid, and 12,247 (7.1 percent) can only be located at the state level. I note that using the sponsor's mailing address introduces some potential for misattribution, as large ESOPs at multi-state companies may be assigned to the state of the corporate headquarters rather than where employees are located. In practice, however, ESOPs are heavily concentrated in small and medium-sized, closely held private companies, for which the sponsor address and employee location are essentially the same thing (Blasi et al. 2019). To the extent misattribution does occur, for instance, a company operating in a right-to-work state but headquartered elsewhere, the resulting measurement error would introduce a conservative bias, mixing outcomes across treatment and control states and pushing estimates toward zero rather than away from it.

Finally, since Form 5500 filings are subject to administrative delays and occasional reporting errors, a plan's absence from a given year does not necessarily indicate termination. ESOPs are a long-term company-wide decision, not something that can be turned "on" or "off." Therefore, a plan is counted as an active ESOP in any year where it did not submit an ESOP return, provided it filed as an ESOP in the previous year and resumed ESOP filing within two years. Gaps exceeding two years are treated as genuine breaks. A separate idiosyncratic issue af-

fects Wyoming in 2001–2003: approximately 970 "sham" ESOPs were filed in 2001 by a single promoter exploiting S-corporation ESOP tax shelters, all registered to a Wyoming shell company address (zip code 820013709) with sequential New Jersey EINs. This artificially inflated Wyoming's annual count from roughly 20 to 990, with the same address accounting for a further 98 filings in 2002 and 14 in 2003 before IRS and Treasury action under IRC section 409(p) and subsequent anti-abuse guidance (Rev. Rul. 2003-6) shut the scheme down. These plans are excluded from the final dataset, restoring Wyoming's counts to levels consistent with adjacent years (2001: 24, 2002: 21, 2003: 17). However, since Wyoming adopted RTW legislation in 1963, it is not included in the difference-in-differences analysis regardless, so this correction affects only the descriptive counts and OLS specifications.

The ESOP dataset and an interactive map are available at: github.com/lydiagcamp/esop_us_map.

Appendix B: Qualitative Analysis of AFL-CIO News

The *AFL-CIO News* was the official weekly newspaper of the American Federation of Labor and Congress of Industrial Organizations, the largest and most influential labor federation in the United States, which represented the vast majority of organized workers throughout the second half of the twentieth century. The newsletter was published continuously from 1955 until 1996. It reported on collective bargaining outcomes, legislative developments, union policy, and labor movement news across all affiliated unions. The 41 volumes digitized by the Internet Archive cover the full publication run and together span approximately 18,900 scanned pages. Full-text files were downloaded for each volume, produced via optical character recognition (OCR), and searched programmatically for keywords related to employee ownership. Five terms were retained for analysis: *ESOP*, *employee stock ownership*, *employee ownership*, *worker ownership*, and *stock ownership plan*. These terms were chosen to reflect the changing rhetoric surrounding such plans, since the acronym "ESOP" became popular only later in the twentieth century. For each match, 400 characters of surrounding context were extracted and stored for evaluation. Figure A1 shows that references to these plans were virtually nonexistent before the passage of ERISA in 1974, but rose rapidly in the late 1980s and 1990s.

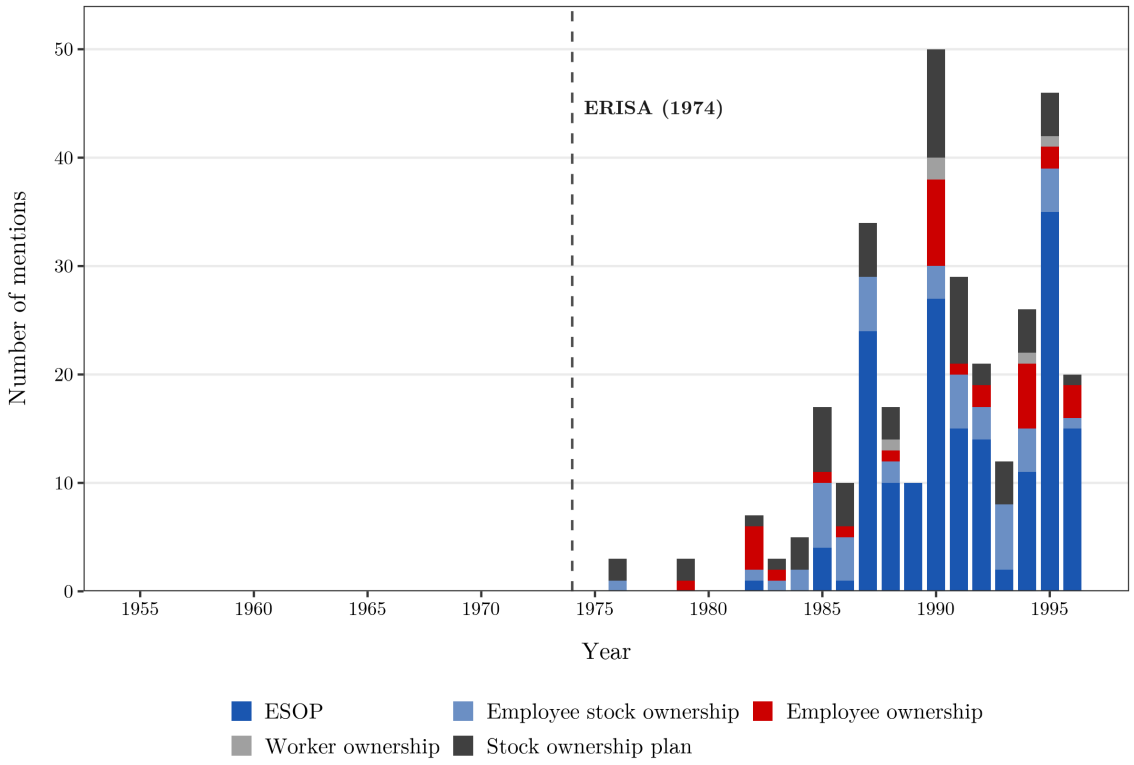

Figure A1: **Mentions of Employee Ownership Terminology in *AFL-CIO News***
Counts based on keyword searches of OCR-digitized full-text volumes (Internet Archive). The dataset spans from 1955-1996. The ERISA (1974) represents the first legal recognition of ESOPs as legitimate employee benefit plans.

I next identified and manually reviewed dedicated articles on ESOPs. I define dedicated articles as articles or self-contained passages in which employee stock ownership plans are the primary subject, rather than receiving an incidental mention. These articles were identified through a manual review of all keyword hits across the period, with the final list reported in Table A2. The articles provide a basis for summarizing how ESOPs were discussed in labor press coverage during this period. From this set, I selected several articles that appeared most representative for the main analysis. These articles were published before the primary ESOP database used in the quantitative analysis was constructed, so the examples are not necessarily "nested" within the sample. Nevertheless, they are useful for assessing the contemporary framing of employee ownership and for identifying recurring themes in union discussions of ESOPs over time.

Saturday, December 5, 1987 AFL-CIO NEWS 3

Stock ownership plans tagged 'handle with care'

New York—Properly structured employee stock ownership plans can create a true partnership between workers and owners. But the downside is that they're too often exploited by "fast-buck" operators to finance the "buyout-takeover-merger wave" that's engulfed corporate America.

That's the message AFL-CIO Occupational Safety, Health & Social Security Director Bert Seidman brought to the sixth annual conference on the historic New York State Compact that aims at creating a labor-management-government partnership to restore industrial vitality.

Seidman noted that 8,000 companies now have ESOPs covering 11 million workers, the majority of them established in the past decade. Most came about in crisis situations, "where workers are faced with the loss of their jobs" unless they dig down into their own pockets and provide financial assistance to a troubled firm.

Employee stock ownership schemes have also been used by anti-union management to thwart workers' desire for a union, Seidman said. He cited cases in which workers involved in organizing drives have been faced with a new or recently improved ESOP "designed to encourage employees to vote against unionization."

Proposals to raise funds from workers to fend off hostile takeovers "benefit the employer, not the employees," Seidman stressed. Instead of "increasing worker participation, productivity and income and contributing to the social good," he pointed out, the plans are too often used as a "financial guarantee to enrich fast-buck operators or a scheming management."

To be successful, Seidman emphasized, ESOPs should be designed to finance corporate growth and insure the firm a competitive advantage in the marketplace. And, he added, the plans should involve large enough employee stock ownership to give workers a significant voice in corporate management and provide them with regular dividends as a return on their investment.

Few on boards

Seidman cited a recent survey by the General Accounting Office—the congressional investigating arm—showing that workers lack representation on 96 percent of the boards of directors of firms with ESOPs, and 95 percent of the firms surveyed distributed no stock dividends to employees.

Labor is particularly concerned, he continued, when an ESOP is used as a substitute for a pension plan. "Providing a pension dependent solely on market fluctuations" in the value of a troubled company, Seidman said, "is playing Russian roulette with a worker's retirement security."

He praised the New York Compact for encouraging ESOPs built on the principle of genuine labor-management cooperation. The program also emphasizes such key elements as advance notice of plant closings and layoffs, extended health insurance for laid-off workers and their families, labor-management committees to deal with productivity and other issues that can forestall plant closings and worker displacement.

Steelworkers Sec. Edgar L. Ball told the conference that his union has been active in developing employee stock ownership in an industry battered by rising imports.

"We do not tell our members that this investment is as secure as a savings account in a federally insured bank," Ball emphasized. Instead, the USWA stresses that trading current wages for "a significant, but minority" share of the company is a trade-off that enhances the company's chance of survival and could help protect jobs.

The Steelworkers' position, Ball said, is that ESOPs should be part of the collective bargaining contract, should provide for payback beyond the term of the agreement, should guarantee employees equitable voting rights, and "must never be used as a substitute for an adequate, properly funded pension plan" safeguarded by the federal Pension Benefit Guarantee Corp.

Rick Dupinsky of the Airline Pilots concentrated on the effort launched last April by the union to buy United Airlines during a period when "cut-throat" competition threatens the viability of the industry. "If we invest in United, we not only invest in our own future but it will be a future over which we at least have some control," he emphasized.

"Bankruptcies, mergers and takeovers are concentrating the industry so that the real control over air commerce is in the hands of fewer and fewer individuals," Dupinsky pointed out. "Employee capitalism" can be a significant factor in saving the industry and protecting workers' interests.

'Merger mania'

New York Lieutenant Gov. Stan Lundine was sharply critical of "merger mania" which last year saw the consolidation of 3,000 firms worth $180 billion.

These mergers produce "a bonanza for stockholders," he said, "but the workers almost always lose," because the cost of financing "this kind of foolishness" is either layoffs or pay cuts.

Lundine told conferees that employee ownership that gives workers active participation in formulating company policy is "one of the best ways to increase job security, protect workers' rights, strengthen organized labor, and achieve a higher standard of living" for working people.

Figure A2: **Excerpt from an *AFL-CIO* Newspaper on Employee Stock Ownership Plans (Dec. 5, 1987)**

Table A2: **ESOP Dedicated Articles, *AFL-CIO* Newspapers (1985-1996)**

Year	Page	Title
1976	391	Unions urged to employ caution on stock plans
1982	396	Rail workers endorse plan to buy Conrail
1985	54	Unions approve Frontier Airlines stock buyout plan
1985	324	Frontier unions back buyout plan
1985	348	Unions challenge move to capture Frontier Airlines
1986	457	Negotiating Profit Sharing and Employee Stock Ownership
1987	5	Negotiating Profit Sharing & ESOPs
1987	303	AFL-CIO Executive Council endorses IUD guidelines on employee stock ownership plans
1987	438	Stock ownership plans tagged 'handle with care'
1988	147	Unique worker-owner plan provides control and profits
1988	195	Stock plans approached cautiously
1989	110	LBO damage to pensions spelled out to House
1989	143	ESOP negotiations begun by Steelworkers at LTV
1989	254	Labor tells Congress key concerns
1990	47	IUD unveils worker investment fund for corporate buyouts
1990	61	Workers buy profitable mill, take control of their destiny
1990	231	Committee to assist ESOP ventures
1991	13	Steelworkers split profits from bankrupt company's sale
1991	25	Kheel exploring possible Daily News ESOP buyout
1991	109	Unions plan to keep Acme buyout bid alive, just in case
1991	271	Negotiating Profit Sharing and Employee Stock Ownership Programs
1992	28	Factory gates close at USX South Works
1992	144	Nicaraguan unionists use ESOP to buy part of Pepsi plant
1992	252	Workers' ingenuity, pluck revive industry, jobs
1992	295	ACTWU gains two-year pact at N.C. plants
1994	136	Shipbuilders contest ESOP rip-off
1994	202	Unions cook up bakery buyout solution
1995	4	Textile Workers seek ESOP voting rights
1995	60	ESOP boosts firm and union
1995	70	Employee Ownership
1995	146	ESOP rescue pays dividends
1995	293	Ruling gives ESOP workers clout
1996	29	Unions seen as key to ESOP success
1996	161	Avis workers seek share of proceeds from stock sale

Appendix C: Robustness Checks

I next present several robustness checks to ensure that the main findings are not statistical artifacts of particular design choices. These tests fall into five broad categories: border county robustness, validity of concept operationalization, outlier sensitivity, statistical choices, and the probability of obtaining the result by chance. Code is available upon request.

C.1 Border County Robustness

First, I replicate the main analysis restricting the sample to border counties—counties that share a physical boundary with a county on the opposite side of a RTW/non-RTW state line. This approach, also used in Gihleb et al. (2023), holds geography and local industry mix roughly constant by comparing counties on either side of the same state border, providing a more direct test of whether the main result reflects a genuine policy effect rather than pre-existing cross-state differences. The unit of analysis is the county-year and the outcome is the count of active ESOPs. Figure A3 maps the 73 treated and 64 control border counties included in the analysis. The results, reported in Table A3, are negative in both samples and reach conventional significance when Wisconsin and its bordering counties are excluded (ATT = -0.56^{*}), consistent with the main state-level finding.

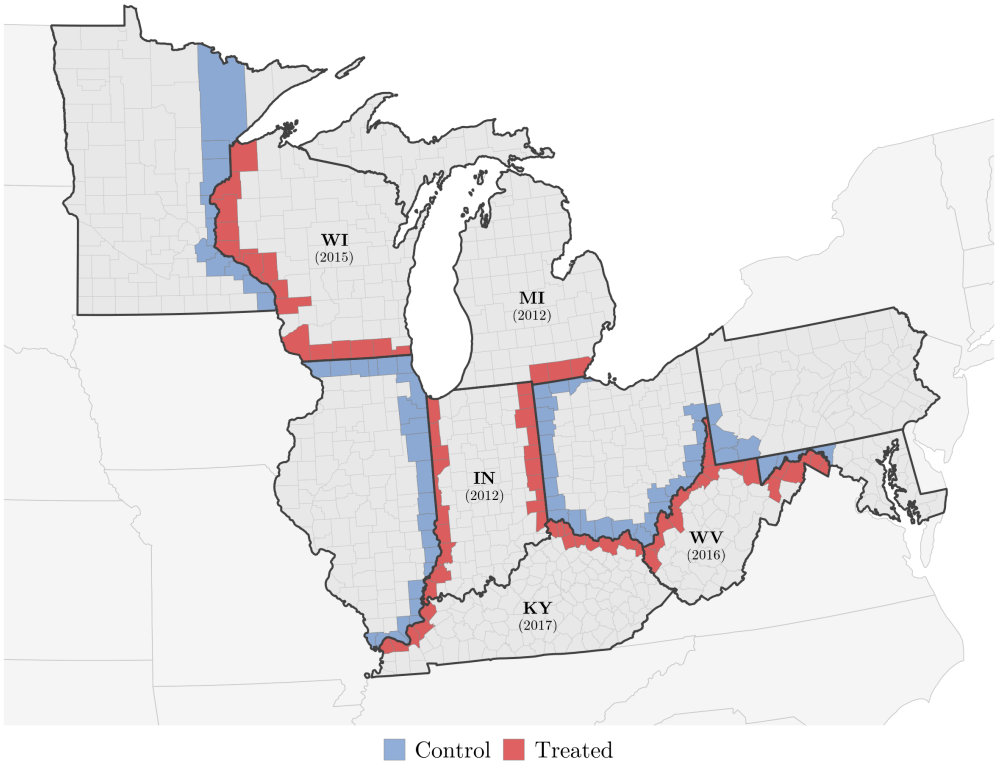

Figure A3: **Border Counties Included in Spillover Robustness Check**
Red counties are treated border counties in RTW-adopting states (IN, MI, WI, WV, KY); blue counties are control border counties in neighboring non-RTW states (IL, MN, OH, PA, MD).

Table A3: **Border County DiD Estimates**

	Full Sample	**Excl. Wisconsin**
ATT	−0.269	−0.564*
	(0.361)	(0.268)
Treated counties	73	57
Control counties	64	48
County-year observations	3,432	2,625

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

Standard errors in parentheses. Callaway and Sant'Anna (2021) doubly robust estimator, not-yet-treated control group, simple ATT aggregation. Controls: manufacturing share and unemployment rate (lagged one year). The dependent variable is the number of ESOPs per county. Period: 2000–2024.

C.2 Operationalization of Key Variables

Second, I turn to the operationalization of the key variables. Starting with union strength, Figure A4 replicates the main first-stage estimates across all four available measures: total and private-sector membership rates, and total and private-sector coverage rates. The results are robust to all measures.

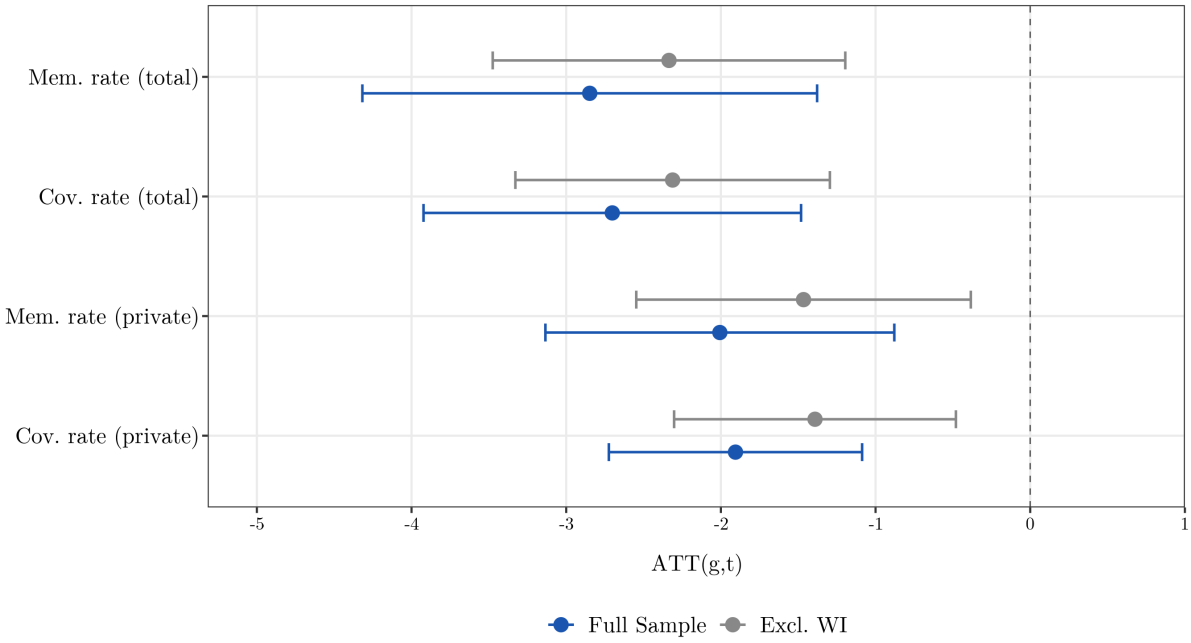

Figure A4: **ATT Estimates Across Union Strength Definitions**

Overall ATT estimates for four union density outcomes: total and private-sector membership and coverage rates, each shown for the full sample (blue) and excluding Wisconsin (grey). Membership rate (total) is the measure used in the main analysis. Error bars show 95% confidence intervals.

An additional operationalization check concerns employee ownership. While the main analysis uses the stock of active ESOPs per state-year, changes in this stock reflect the net balance of new plan formations and exits. Figure A5 disaggregates the main result into formations—plans appearing in the data for the first time—and exits—plans whose last filing occurs in year t, capped at 2023 to avoid right-censoring. Pre-trends are generally flat in both panels, though the power to detect a hypothetical violation is moderate (around 50 percent), meaning that the absence of visible pre-trends should be interpreted with some caution. Pre-trends are flatter when Wisconsin is excluded, providing stronger identification support for that sample. The formation estimates turn negative following RTW adoption and build gradually over time (ATT $= -5.14^{**}$ excluding Wisconsin), consistent with the main result and with the theoretical expectation that weakening unions reduces firms' incentive to preemptively adopt employee ownership. Exit estimates remain close to zero across both samples. This is theoretically intuitive: once an ESOP is established, dissolving it is costly—both financially and in terms of employee expectations—so firms are less likely to terminate existing plans in response to a change in the labor relations environment. The decline in the stock of ESOPs documented in the main analysis therefore reflects a slowdown in new plan formation rather than accelerated exit from existing ones.

Table A4: **ESOP Formations and Exits**

	Full Sample	**Excl. Wisconsin**
ESOP Formations		
ATT	−2.007	−5.138**
	(2.129)	(1.728)
ESOP Exits		
ATT	0.202	−0.244
	(1.451)	(1.510)

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

Standard errors in parentheses. Controls, estimator choice, and aggregation method mirror the main analysis.

C.3 Outlier Sensitivity

Third, I assess sensitivity to outliers through a leave-one-out analysis (Figure A6), sequentially dropping each of the 27 states in the difference-in-differences sample and re-estimating the main ATT to test whether any single state is driving the result. The analysis is generally reassuring, though Ohio stands out as an influential observation whose removal meaningfully weakens the estimate. This is partly explained by the Ohio Employee Ownership Center (OEOC) at Kent State, one of the most active state-level ESOP promotion organizations in the country, which drove a sharp increase in ESOP formation from around 2015 onward.

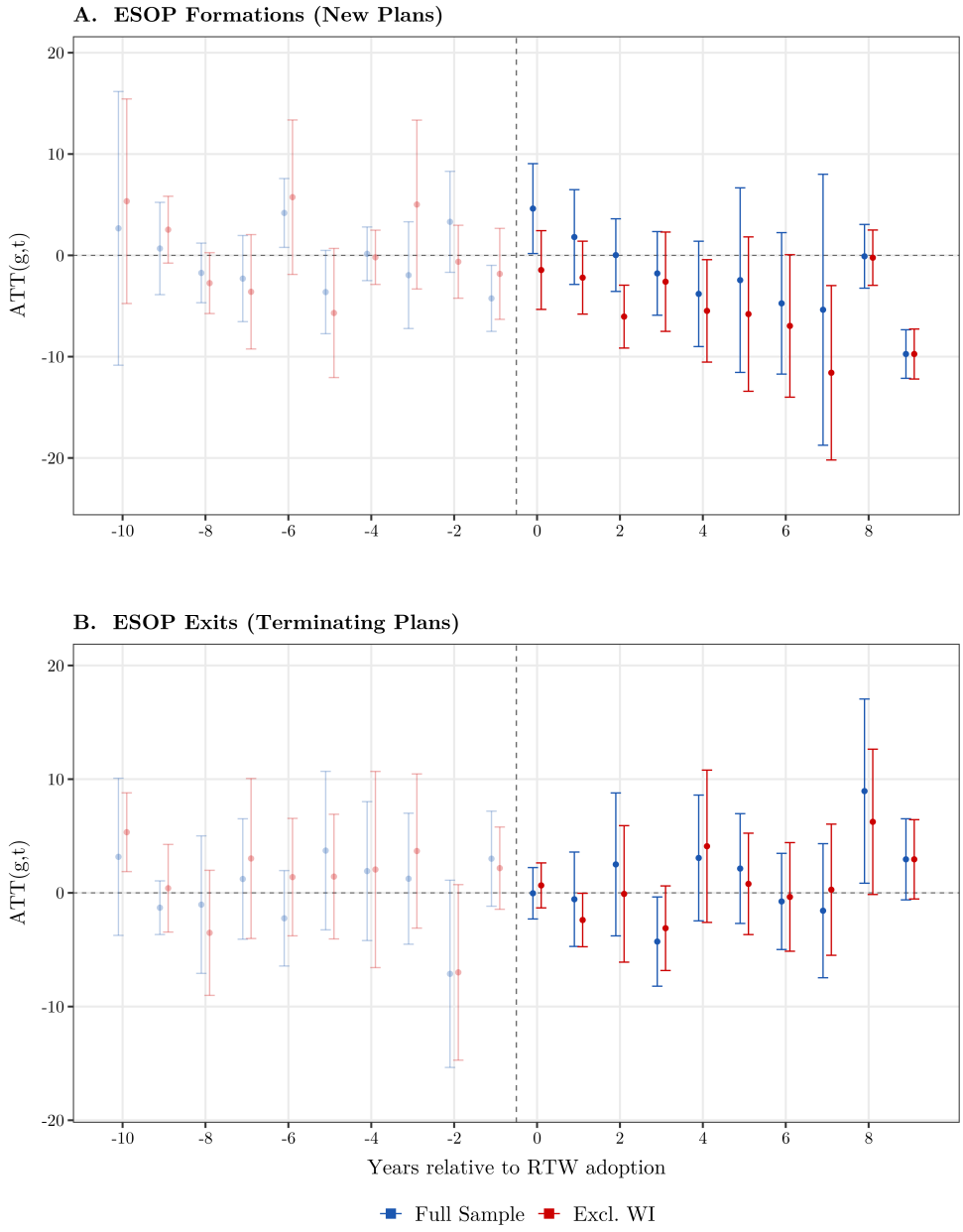

Figure A5: **Event Study Estimates Disaggregated by Formation and Exit**

Panel A shows event-study estimates for ESOP formations and Panel B shows estimates for ESOP exits. Error bars show 95% confidence intervals.

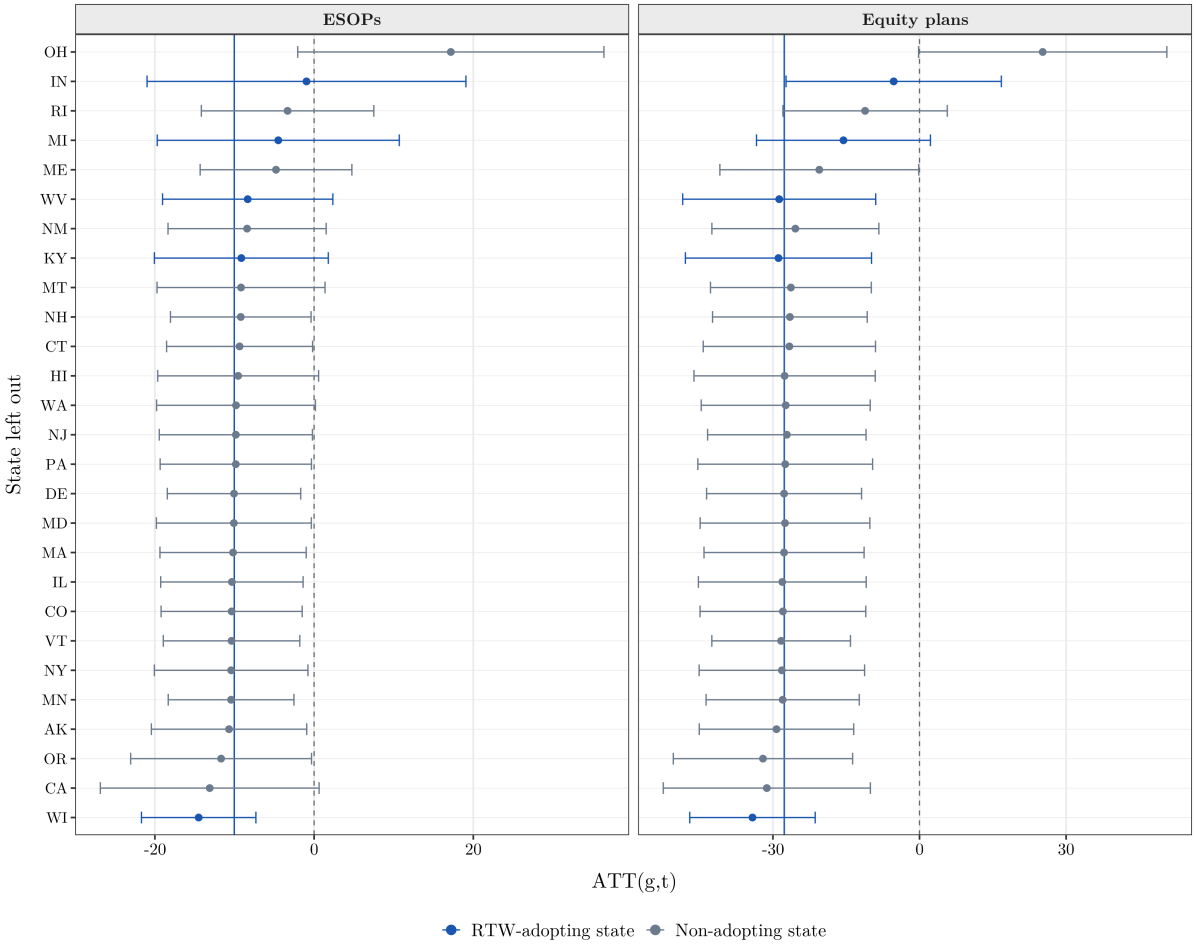

Figure A6: **Leave-One-Out Sensitivity Analysis**

Each row shows the overall ATT estimate when one state is excluded from the sample. The solid vertical line marks the full-sample ATT. Error bars show 95% confidence intervals.

C.4 Statistical Choices

Fourth, I examine sensitivity to statistical choices, varying the control group, standard errors, and aggregation scheme. Table A5 compares results under alternative control group and standard error choices. The first set of columns contrasts the not-yet-treated control group used throughout with the more restrictive never-treated group. Estimates remain negative and of similar magnitude across both definitions. The second set of columns evaluates the three main outcomes under analytical and multiplier bootstrap standard errors, which are more reliable when the number of clusters is small. Replacing analytical with bootstrap standard errors has negligible impact on the confidence intervals, leaving all significance conclusions unchanged.

Table A5: **Control Group and Standard Error Assumptions**

	Control Group		Standard Errors	
	Not-yet-treated	**Never-treated**	**Analytical**	**Bootstrap**
Union density	−2.848***	−2.777***	−2.848***	−2.848***
	(0.750)	(0.775)	(0.750)	(0.824)
ESOPs	−10.025*	−11.182*	−10.025*	−10.025*
	(4.604)	(4.738)	(4.604)	(4.920)
ESOPs | union density	−7.316	−8.546	−7.316	−7.316
	(5.565)	(5.403)	(5.565)	(5.840)

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

Estimates are average treatment effects from the doubly robust Callaway and Sant'Anna (2021) estimator using simple ATT aggregation and the preferred control specification. The first two columns compare the main not-yet-treated control group to a never-treated control group. The last two columns compare analytical influence-function standard errors to multiplier bootstrap standard errors.

I also verify that the main finding is not an artifact of the particular aggregation scheme used to summarize the group-time ATTs. Figure A7 replicates the estimate under all four aggregation methods proposed by Callaway and Sant'Anna (2021): overall group-time, group, dynamic, and calendar-time. The group-time estimate aggregates all cohort- and period-specific treatment effects into a single summary measure, weighting by the distribution of treated units across cohorts and time periods. The group aggregation first averages within cohorts and then across them. The dynamic aggregation averages by event time, capturing how effects evolve since treatment. The calendar-time aggregation averages across cohorts within each calendar period. Across all four approaches, the estimated effect remains negative and broadly consistent.

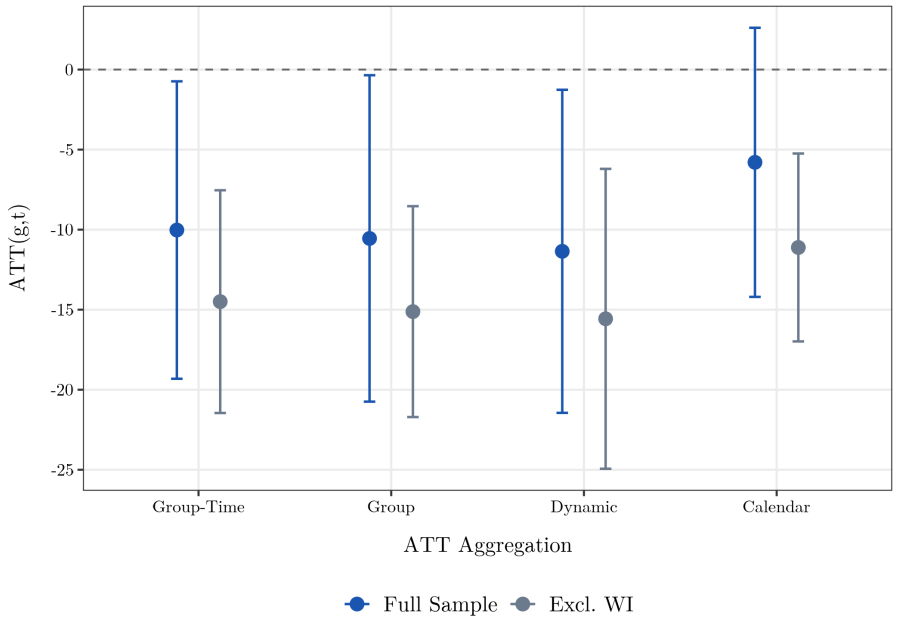

Figure A7: **ATT Estimates Across Aggregation Schemes**

Overall ATT estimates under all four Callaway and Sant'Anna (2021) aggregation methods: group-time, group, dynamic, and calendar. Error bars show 95% confidence intervals.

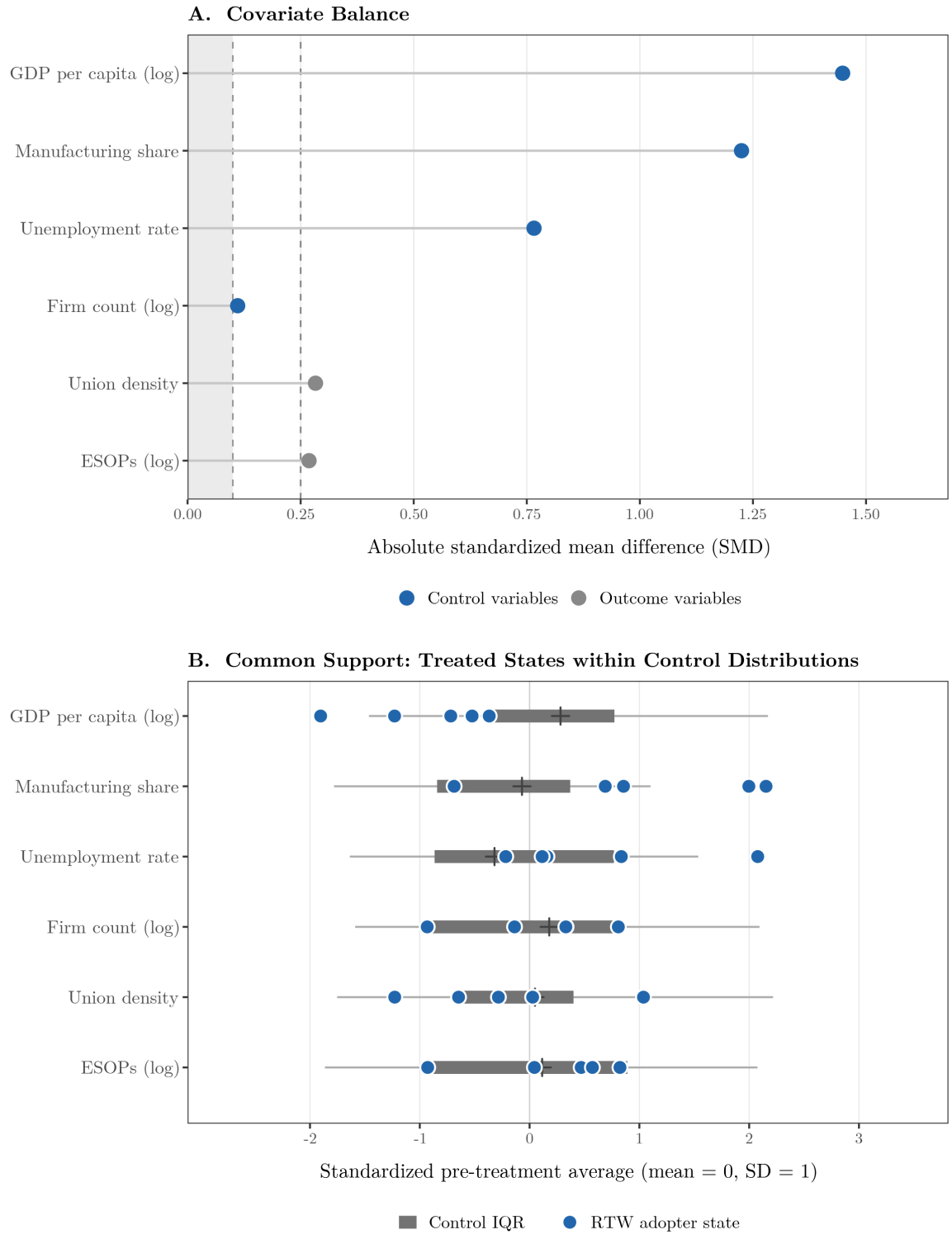

Figure A8: **Covariate Balance and Common Support**

Panel A shows absolute standardized mean differences (SMDs) between RTW adopters and never-adopters across pre-treatment covariates and outcome variables. *Panel B* plots each RTW adopter state's standardized pre-treatment average against the interquartile range of the control distribution for each covariate.

Figure A8 confirms that while RTW-adopting states differ from non-adopters on several pre-treatment dimensions, treated states generally fall within the observed distribution of control states, supporting the common support assumption.

C.5 Timing Permutation Test

Finally, Figure A9 presents a timing permutation test. Keeping the five RTW-adopting states fixed, I randomly reassign each state's adoption year from a uniform draw over 2004–2019 and re-estimate the ATT, repeating this procedure 1,000 times to construct a null distribution representing what would have been found by chance under random treatment timing. The upper bound of 2019 is chosen to ensure a sufficient number of post-treatment observations for each permuted assignment and to avoid conflating treatment effects with the disruption caused by the COVID-19 pandemic in 2020. The lower bound of 2004 is chosen symmetrically, to ensure that each permuted assignment also retains a sufficient number of pre-treatment periods for the parallel trends assumption to be evaluated. The actual ATT of -10.0 falls at the fifth percentile of this null distribution (one-sided $p = 0.043$; two-sided $p = 0.152$). While the two-sided p-value exceeds conventional significance thresholds, the observed estimate is considerably more negative than the typical result obtained by chance, suggesting the decline in ESOP formation is unlikely to reflect random variation in treatment timing.

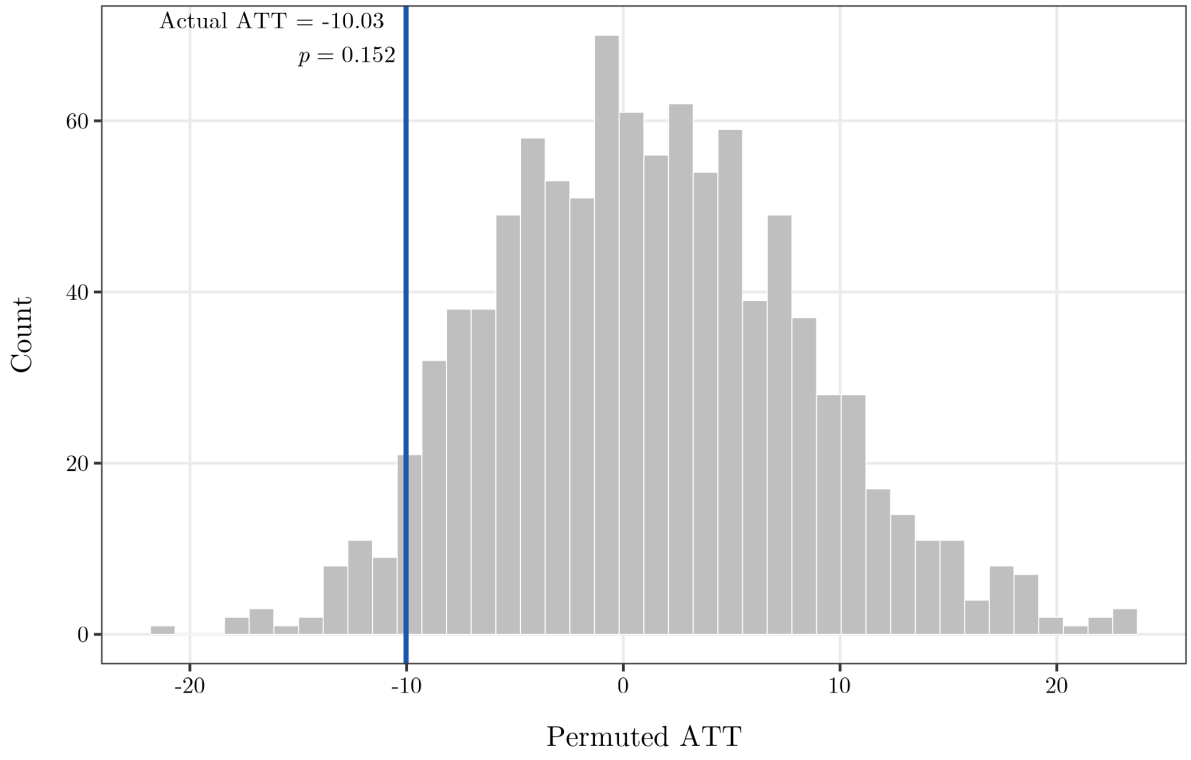

Figure A9: **Timing Permutation Test**
Distribution of ATT estimates from 1,000 permutations in which each treated state's adoption year is randomly reassigned within 2004–2019. The solid blue line marks the actual estimated ATT.

Appendix D: Industry Heterogeneity

This section presents the industry heterogeneity analysis described in Section 5. If RTW laws reduce ESOP formation through a union avoidance mechanism, the effect should be concentrated in high-union industries, where the threat of organizing is most credible and the incentive to adopt employee ownership preemptively is strongest. Industry heterogeneity thus provides a useful within-sample check on the proposed mechanism. Given the limited number of treated observations at the disaggregated industry level, industries are pooled into three union density groups to improve statistical reliability. These groups are defined based on average 2004–2011 unionization rates from Hirsch et al. (2026): high (>15%), medium (5–15%), and low (<5%). The thresholds bracket the sample mean (10.66%) and median (9.85%), with the medium group capturing industries close to the center and the high and low categories those furthest from it. The fixed thresholds are preferred over empirical tertiles, which produce highly unequal plan-year counts across groups, reducing comparability of statistical power. I also base the categorization on an eight-year average rather than a single pre-treatment year for stability. The window begins in 2004 because the Bureau of Labor Statistics revised its industry coding system to align with NAICS in that year; prior to 2004, the CPS industry classification follows SIC-era categories that cannot be mapped cleanly to 2-digit NAICS sectors. NAICS codes are successfully matched to 99.8% of ESOP plan-year observations; the remaining 0.2% are excluded. The full classification is reported in Table A6, including average union density and plan-year observations for each industry.

ESOP counts are aggregated to the state-industry-year level and the Callaway–Sant'Anna doubly-robust estimator is re-estimated separately for each union density group using the 2000–2024 panel. This shift in the unit of analysis introduces two important limitations. First, because the three groups differ substantially in size—the low-union group contains roughly three times as many ESOP plan-years as the high-union group—raw ATT counts are not directly comparable across groups and differences in statistical power may drive apparent patterns rather than true heterogeneity. Second, the thinness of observations within state-industry cells weakens the power of pre-trend tests and makes the parallel trends assumption harder to evaluate than in the state-level analysis (Roth 2022). I therefore treat this as an exploratory descriptive exercise rather than a standalone causal test.

Both industry-level and state-level covariates are included, all lagged by one year. Industry-level controls are drawn from the Quarterly Census of Employment and Wages (QCEW) and include establishment count, employment level, and average weekly wages, capturing variation in the size and labor market conditions of each industry group within each state. State-level controls include the unemployment rate, log real GDP per capita, and manufacturing employment share. The state-level number of firms is excluded since, once observations are aggregated to the state-industry level, it is collinear with the industry-level establishment count, which more precisely captures size variation at the unit of analysis. This yields a pool of six candidate controls.

Table A6: **Industry Classification by Union Density Group**

NAICS	Industry	Union Density (%)	Plan-Years
High union density *(>15%)*			
61	Educational Services	33.6%	307
92	Public Administration	32.1%	1
48–49	Transportation & Warehousing	30.0%	2,152
22	Utilities	17.1%	1,267
23	Construction	15.5%	11,618
Medium union density *(5–15%)*			
51	Information	12.2%	2,035
31–33	Manufacturing	11.7%	25,249
62	Health Care	9.4%	1,904
21	Mining	8.4%	364
71	Arts & Entertainment	7.4%	205
44–45	Retail Trade	5.2%	6,065
42	Wholesale Trade	5.1%	10,198
Low union density *(<5%)*			
55	Management	4.7%	2,654
53	Real Estate	4.3%	1,232
56	Administrative Services	4.0%	2,441
81	Other Services	3.2%	1,647
11	Agriculture	2.6%	976
72	Accommodation & Food	2.3%	625
52	Finance & Insurance	1.9%	16,144
54	Professional Services	1.7%	18,862

A key challenge in this setting is that the overall ATT for each group is highly sensitive to which covariates are included. Preliminary exploration reveals that point estimates vary considerably across specifications. This volatility is unsurprising given the small number of effective observations per state-industry cell and the limited variation in treatment timing, both of which make the doubly-robust propensity score reweighting unstable. To address this honestly, I follow the specification curve approach of Simonsohn et al. (2020) and estimate the model under all $\sum_{k=0}^{6} \binom{6}{k} = 64$ possible subsets of the six candidate controls, for each group and both with and without Wisconsin, for a total of 384 models. Rather than selecting a single point estimate, this approach exposes the full range of estimates that can be obtained under defensible covariate choices. Figure A10 presents the results. Each point shows the overall ATT and 95% confidence interval for one covariate subset, sorted from lowest to highest ATT.

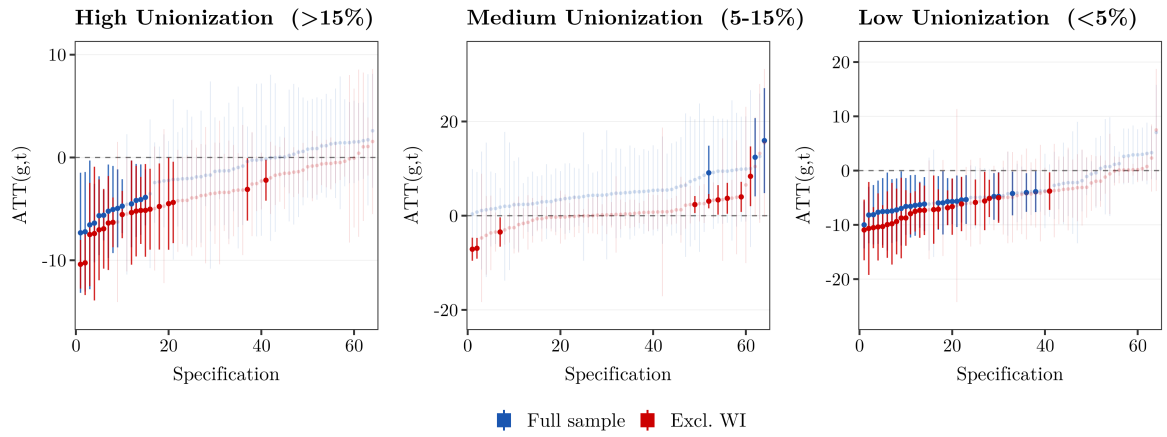

Figure A10: **ATT Estimates Across All 64 Covariate Combinations, by Industry Union Density Group**

Each point shows the overall ATT and 95% confidence interval from one covariate subset, sorted from lowest to highest ATT. Saturated points are statistically significant at the 5% level. Callaway–Sant'Anna doubly-robust DiD, not-yet-treated control group, 2000–2024.

The wide confidence intervals and the considerable vertical spread across the curve confirm that the identifying variation at the state-industry level is too thin to support reliable group-specific causal estimates. Against that backdrop, the results should be interpreted with considerable caution. However, three descriptive patterns are worth noting. For the high-union group, estimates are predominantly negative across covariate choices, though few reach conventional significance levels in either sample. For the medium-union group, results are highly sensitive to the inclusion of Wisconsin: a substantial share of full-sample specifications yield positive and significant ATTs, but this falls sharply once Wisconsin is excluded. The low-union group shows the most consistent pattern—roughly half of all specifications are significant in both samples—though this likely reflects the greater statistical power of this group, which contains roughly three times as many ESOP plan-years as the high-union group, rather than a stronger underlying effect.

What is perhaps most notable is that the medium-union group—which includes manufacturing and several other sectors that have seen renewed interest in employee ownership—shows positive and frequently significant ATTs in the full sample, pointing to an increase in ESOP formation in these industries following RTW adoption rather than a decline. Together with the high-union estimates, this suggests that the RTW effect is not uniform across industries, and may operate through different mechanisms depending on the degree of unionization. ESOPs are a heterogeneous instrument, adopted across very different industries for different reasons, and the aggregate state-level finding may mask considerable variation in the mechanisms at play. These questions are left to future research with richer industry-level data.

Appendix E: Two-Way Fixed Effects Results

Finally, despite the limitations of a standard two-way fixed effects (TWFE) approach in staggered adoption settings, I conduct this analysis as a complementary exercise here. The estimating equation is:

$$\text{ESOPs}_{s,t} = \beta_1 \text{union\_density}_{s,t-k} + \beta_2 \text{union\_density}^2_{s,t-k} + \gamma X_{s,t-k} + \alpha_s + \delta_t + \varepsilon_{s,t}$$

where $\text{ESOPs}_{s,t}$ is the count of active ESOPs in state s in year t, $\text{union\_density}_{s,t-k}$ is the union membership rate lagged k years, $\text{union\_density}^2_{s,t-k}$ is its square, $X_{s,t-k}$ is a vector of controls, and α_s and δ_t denote state and year fixed effects, respectively. While the appropriate lag length (k) is difficult to determine *ex ante*, I present results using multiple lag specifications. I model the union density–ESOP relationship as curvilinear because the direction of the association plausibly varies across the distribution of union strength, as discussed in Appendix D. At low levels of union density, ESOPs may substitute for collective bargaining, while at higher levels of union density, unions may gain sufficient bargaining power to negotiate these arrangements directly. In addition to the core controls used in the difference-in-differences specification—manufacturing employment share, unemployment rate, number of firms, and real GDP per capita—I also include an indicator for left-wing governor, which proxies for state-level political economy conditions that may affect both union strength and employer willingness to adopt employee ownership plans. RTW adoption status is excluded as a regressor; since RTW laws operate in part by suppressing union density, conditioning on RTW status would absorb the variation of interest.

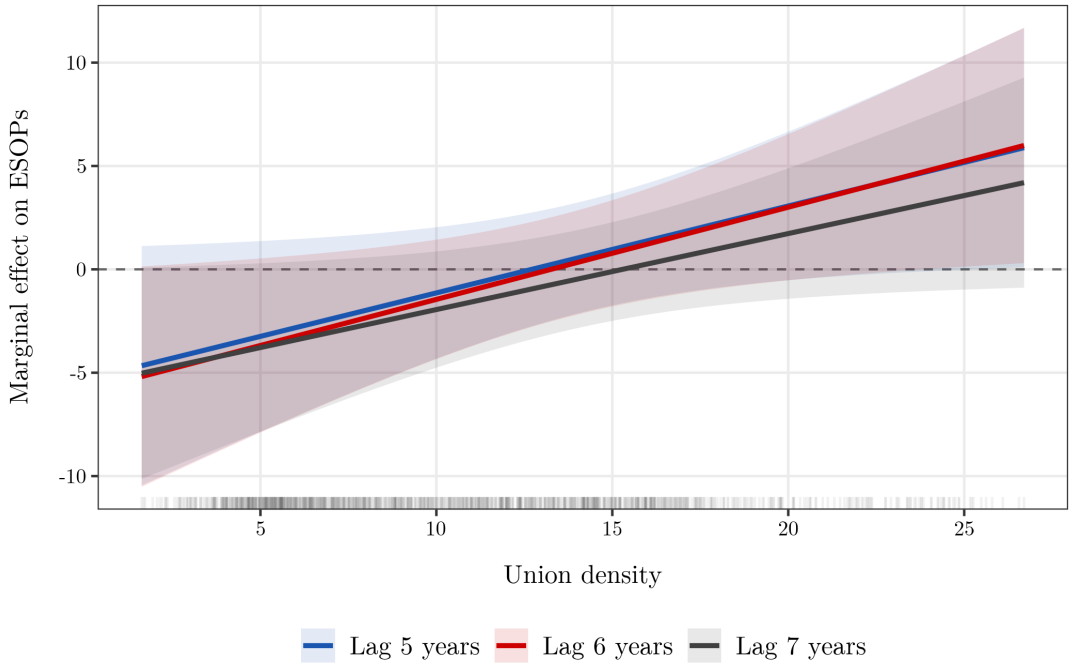

Figure A11: **Predicted Marginal Effect of Union Density on ESOPs**
The figure plots the implied marginal effect of union density on ESOP formation from the quadratic OLS specifications in Table A7 (even columns, with covariates). Lines correspond to models using five-, six-, and seven-year lags of union density. Shaded bands show 95% confidence intervals. The rug plot shows the distribution of observed union density values.

Table A7: **Union Density and ESOPs (OLS)**

	Lag 5 Years		**Lag 6 Years**		**Lag 7 Years**	
	(1)	(2)	(3)	(4)	(5)	(6)
Union density	−5.057	−5.346	-5.537^{+}	-5.912^{+}	-5.112^{+}	-5.622^{+}
	(3.302)	(3.257)	(2.992)	(3.008)	(2.674)	(2.874)
Union density^2	0.180^{+}	0.210^{*}	0.192^{*}	0.223^{*}	0.148^{+}	0.184^{+}
	(0.098)	(0.104)	(0.092)	(0.100)	(0.080)	(0.092)
Left-wing governor		−2.700		−2.135		−1.141
		(2.410)		(2.406)		(2.382)
Manufacturing emp. share (%)		5.005		3.831		3.329
		(3.839)		(3.541)		(3.688)
Unemployment rate (%)		−3.551		-3.540^{+}		-3.451^{+}
		(2.193)		(2.060)		(1.989)
Number of firms		−72.943		−67.565		−78.249
		(44.969)		(43.730)		(53.079)
Real GDP per capita		−34.407		−36.510		−35.751
		(55.840)		(52.793)		(50.600)
Num. Obs.	1,000	1,000	950	950	900	900
R^2	0.981	0.982	0.983	0.983	0.984	0.984

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

Standard errors clustered by state are reported in parentheses. The dependent variable is the number of ESOPs, and all regressors are lagged by k years. State and year fixed effects are included in all models.

As expected, Table A7 and Figure A11 reveal a curvilinear relationship between union strength and ESOP adoption. Across lag specifications, the linear term on union density is negative, while the squared term is positive, though results do not always reach conventional significance levels. This pattern is consistent with the prediction that at low levels of union density, increases in unionization are associated with fewer ESOPs, but that this relationship attenuates and eventually reverses as unions become more influential. Based on these specifications, the implied turning point occurs at a union density of approximately 13–15 percent, close to but somewhat above the sample mean of 10.66 percent.

This pattern sheds light on the substitute-or-complement debate in the union–employee ownership literature. When unions are weak, rising union density does not appear to trigger ESOP adoption—establishing an ESOP requires substantial time and resources, and the threat may not yet be credible. Once unions are sufficiently strong, however, managers may perceive organized labor as a viable threat and adopt an ESOP preemptively to forestall union demands. Strong unions may also be better positioned to negotiate employee ownership arrangements directly. Although the quadratic relationship is suggestive and consistent with the theoretical mechanism, I treat it as complementary evidence and primarily rely on the difference-in-differences estimates for causal interpretation.