

The Right to Work, Power Resources, and Economic Inequality

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Abstract

How do Right to Work laws affect the distribution of economic resources? While sociological theories would predict inequality to increase following the passage of Right to Work laws, previous research has found these laws to be largely inconsequential for economic inequality. Drawing on power resources theory, I reassess the consequences of Right to Work laws and allow their impact to depend upon local union membership. To do so, I construct unique datasets at the state and commuting zone levels of income and wage inequality, merging data from the Internal Revenue Service, the US census, the American Community Survey, the US Union Sourcebook, the Current Population Survey, and the National Labor Relations Board for years 1939 to 2016. After using two-way fixed effects and instrumental variable regression models to replicate inconsistent results of previous studies, I show that these mask substantial and robust heterogeneity across local areas. Simply put, Right to Work laws are highly consequential when passed in times and places where labor has something to lose. Right to Work laws remove the negative association between labor union membership and inequality, while the consequences of Right to Work passage are greatest in highly unionized areas. In total, results suggest that Right to Work laws work as intended, increasing economic inequality indirectly by lowering labor power resources. Theoretical and policy implications are discussed.

How do Right to Work laws affect the distribution of economic resources? Stratification scholars working from a power resources theoretical perspective would predict that these laws have inequality inducing consequences (Korpi 1985, Brady and Leicht 2008, Devnatz 2011, Jacobs and Dirlam 2016). Yet surprisingly, the vast majority of empirical research on this topic finds that Right to Work laws have at most a marginal influence on distributional outcomes (Moore 1998, Farber 2005, Hanley 2010, Eren and Ozbeklik 2016, Kogan 2017). Why, then, have many employers and Republican lawmakers engaged in decades-long policy and legal battles with organized labor to change labor bargaining arrangements through Right to Work legislation (Dixon 2007, 2010, Lee 2012, Hogler 2015)? Local and national union and business organizations spend significant human and financial resources lobbying for their preferred policy outcome, while politicians take meaningful political risks to enact or block these resonant laws (Hogler 2015), as demonstrated through the contentious social movements surrounding the passage of Right to Work legislation in Michigan and Wisconsin and the referendum blocking Right to Work legislation in Missouri. An understanding of the economic and social consequences of these laws are of increasing relevance, as some Right to Work legislation, such as the legal framework used in the recent *Janus vs. the American Federation of State, County, and Municipal Employees (AFSCME)* Supreme Court decision, are being settled at the federal level and thus applicable to a broader segment of the US workforce, including public sector workers in highly unionized states. Right to Work laws sit at the center of local debates over labor rights and struggles over the distribution of economic and power resources, and they have been described as some of the most significant antilabor provisions enabled in the mid 20th century’s backlash against labor victories (Farhang and Katznelson 2005). It is difficult to square the null findings of previous academic research with the spirited debates and sizable investments occurring on the ground.

Theoretically, the dispersal of Right to Work laws across US states over three quarters of a century presents a unique opportunity to extend scholarly understanding of how the long-run dynamics of economic inequality in the subnational US case are responsive to policy choices. Long relegated as marginal to more fundamental changes in technological progress and skill development (Jacobs and Myers 2014), stratification scholars from multiple disciplinary traditions have identified politics and policy change as important mechanisms that both recalibrate market inequality levels—as typified by Autor’s metaphorical association between government policy and corrective

eye glasses (2014, see also Bradley et al. 2003, Kenworthy and Pontusson 2005, Garfinkel et al. 2010)—and influence market inequality itself—as typified by Volscho and Kelly’s theory of market conditioning (2012, see also Moller et al. 2009, Western and Rosenfeld 2011, Jacobs and Dirlam 2016, VanHeuvelen 2018b). A mature comparative literature has documented how political legacies structure the relationship between labor market institutions and labor market outcomes (Bradley et al. 2003, Kenworthy and Pontusson 2005, Brady and Leicht 2008, Thelen 2014, Kerrissey 2015). In this paper, I extend these insights to the subnational US context, demonstrating how policy choices can alter the fundamental operations of labor market institutions, as well as the mobilization of key actors in the struggle over valuable power resources, which all culminate in variation in the relationship between labor’s power resources and economic outcomes.

In this research I combine data from a variety of sources, including the US census, American Community Survey, Internal Revenue Service income data, Current Population Survey, the US Union Sourcebook, and the National Labor Relations Board to examine wage and income inequality change before and after the passage of every state-level Right to Work law between 1939 and 2016, using state and commuting zone information. Using two-way fixed effects regression and instrumental variable modeling techniques, I find that Right to Work laws operate as intended and draw four main conclusions. (1) *Behavior of key organizational actors differ across Right to Work contexts.* Both union membership and antiunion firm behavior vary depending on the combination of Right to Work and local power resource concentration, providing a behavioral foundation to expect Right to Work to influence inequality differently in different social contexts. These findings underscore the inherently relational nature of power resources theory, and in doing so, bridge the theory to contemporary theories of relational inequality by emphasizing power resource distribution as the outcome of multiple parties strategically responding to opportunities enabled by policy change (Tomaskovic-Devey and Avent-Holt 2019). (2) *Right to Work laws have inconsistent direct associations with inequality.* Similar to previous research, I demonstrate the inconsistent associations that can be drawn from simple analyses of pre- and post-Right to Work policy enactment. (3) *Right to Work laws remove the power of unions to affect inequality.* Extending power resources theory to show the dependence of its theorized mechanisms on local policy context, (e.g. Tope and Jacobs 2009, Volscho and Kelly 2012, Brady et al. 2013, Jacobs and Dirlam 2016), the significant and negative association between union membership and economic inequality is removed in states

and commuting zones following the passage of Right to Work laws. Passage of Right to Work laws in the most densely unionized areas associates with a change to inequality that is approximately 25% the magnitude of within-state inequality growth over the entire post World War 2 era. (4) *Right to Work laws affect top- and bottom-ends of the wage distribution differently.* Low-end wage inequality is more directly and uniformly affected by Right to Work laws across contexts, while these laws have positive associations with top-end inequality in places where unions are strong. Thus, Right to Work affects the distribution of resources in ways theoretically anticipated by previous stratification research on the heterogeneity of unions and wage setting (Firpo et al. 2009, Western and Rosenfeld 2011, Shin 2014).

This research has several implications. First, practically, findings show that Right to Work laws *do* in fact affect subsequent distributions of economic resources, when they are passed in times and places where labor has something to lose. Using the rhetoric of freedom of choice, Right to Work laws have been championed by employers and conservative policymakers to reduce union power, minimize labor costs to business, and create business friendly markets. A predictable, but hitherto undetected, change in inequality subsequently follows. Second, results move forward a power resources explanation for the long-run historical changes in subnational US inequality (Brady and Leicht 2008, Brady and Sosnaud 2010, Jacobs and Myers 2014, Jacobs and Dirlam 2016). While previous subnational research has relied on broadly specified connections between labor power and inequality, I show how changes in specific policies translate into inequality via redistribution of power resources (cf. Brady et al. 2013). In doing so, this manuscript demonstrates how the insights from comparative research on politics, labor market institutions, and inequality can be applied to the subnational context of a single liberal market economy (Bradley et al. 2003, Kenworthy and Pontusson 2005, Brady and Leicht 2008, Thelen 2014). Political decisions can fundamentally alter the way that labor market institutions affect market functioning and thus, inequality. Finally, this paper underscores the point that seemingly similar policies passed in different social contexts can result in substantially different distributional outcomes. Right to Work laws are designed to alter power resources between key institutional actors. It is logical that such laws should depend on the degree to which the local distribution of power resources are amenable to change. Treatment of Right to Work laws as one-size-fits-all conceptualizes the local social context too bluntly to effectively detect where, and how, such laws should result in higher inequality.

Background

Power Resources, Politics, and Union Change

Perhaps the leading sociological explanation of the relationship between labor unions, politics, and economic outcomes is power resources theory. This theory argues that economic resources are distributed in relation to class-based collective action of political actors (Korpi 1985, Brady and Leicht 2008, Brady et al. 2009, 2013, Jacobs and Dirlam 2016). Workers have significant power and economic disadvantages compared to employers and businesses over control of the distribution of economic resources, and so they must organize in politics and workplaces to have a reasonable chance at higher pay, more generous benefits, and better working conditions (Brady and Leicht 2008). Mobilization into unions allows for greater relative bargaining power against management and reduces retaliatory risks against individuals, while mobilization into party politics can result in elected policymakers favorable to labor (Brady et al. 2009, Tope and Jacobs 2009). Successful mobilization allows for the chance of enactment and enforcement of favorable economic regulation, redistributes economic risks away from workers, and helps to establish normative expectations of egalitarianism (Brady et al. 2013). From this theoretical perspective, to understand the long-run nature of economic inequality, one must understand the ebb and flow of class-based power and mobilization (Jacobs and Dirlam 2016).

Union membership in the 20th and 21st century began with rapid growth, followed with a long decline. After decades of low union density at the start of the 20th century (Kimeldorf 2013), unions gained a variety of rights and legislative victories during Franklin Roosevelt’s administration, leading union membership to grow rapidly at the end of the 1930s through the early 1950s (Eidlin 2015). Union membership began to fall from peak rates in the 1950s, and this decline accelerated during the 1970s in the face of strong antiunion action by corporate leaders (Mizruchi 2013) and then by the Reagan administration’s antiunion federal policy priorities (Voss and Sherman 2000, Tope and Jacobs 2009). Many employers were emboldened to oppose union demands, contest and delay National Labor Relations Board elections, and fire union activists (Voss and Sherman 2000), which combined to weaken labor bargaining position from the 1980s onward. In total, a key transition in the American labor market across the 20th and 21st century has been the declining institutional and political power of labor unions over wage setting and employment relations in the private sector. Nonagricultural union membership peaked at around 35% in the mid 1940s through

the early 1950s, and has declined steadily since then. Today, approximately 6.5% of private sector workers are members of labor unions.¹

The rise and decline of labor unions has been highly consequential for long-run changes in US economic conditions, as one of the core pieces of stratification knowledge is that strong labor unions reduce economic inequality. Membership in a labor union has been found to raise wages for all but the highest wage earners (Firpo et al. 2009), with effects particularly pronounced for blue-collar workers (Western and Rosenfeld 2011), less skilled workers (Freeman and Medoff 1984, Farber et al. 2018), and less educated workers (Rosenfeld et al. 2016). Previous research has shown that the wage premium paid to union members ranges between 5% and 30%, depending on how unobserved heterogeneity is accounted for (Card et al. 2004, Maxwell 2008, Rosenfeld 2014, Wilmers 2017, VanHeuvelen 2018a). This union premium has been remarkably consistent over time. For example, Farber et al. (2018) show that the union family logged income premium fluctuated between 0.12 and 0.20 between 1935 and 2015, despite substantial changes in the composition of union workers.

Labor unions not only affect the pay of union members and unionized workplaces, but also affect the distribution of nonunion wages. Originally conceptualized through a rationalistic threat mechanism whereby employers would preemptively raise nonunion wages in their firms to prevent their workforce from unionizing (Freeman and Medoff 1984, Leicht et al. 1993), scholars found wages of nonunion workers to be higher in densely unionized states (Farber 2005) and cities (Leicht 1989). Western and Rosenfeld (2011) extended the mechanisms linking union power and nonunion wages to allow for social, cultural, and political pathways influencing norms and values of fairness, which both raises wages and reduces inequality among union and nonunion workers alike (see too Card et al. 2004). These linkages have been found to occur within industry-regions (Western and Rosenfeld 2011, Rosenfeld et al. 2016), occupation-regions (Denice and Rosenfeld 2018), and along career trajectories (VanHeuvelen 2018a). Strong unions also shift a greater share of total national income to labor (Kristal 2013) and keep in check the takeoff of top managerial pay (DiNardo et al. 1997, Shin 2014, Jaumotte and Buitron 2015). Through their influence on wages of members and similar non-members, unions have a direct effect on inequality in the middle and bottom of the

¹While not entirely reducible to political choices, scholars generally agree that political and policy decisions have been central for changes in union membership (Wallerstein and Western 2000, Farber and Western 2001, Brady 2007, Tope and Jacobs 2009, Eidlin 2015, 2016). Other explanations include globalization, technological change, sectoral shifts in employment, changes in public values, and increased employer pressure.

wage distribution, while broader mechanisms linked to union strength—normative and political pressure, for example—results in an indirect effect on inequality among high wages. In total, scholars conclude that the decline of unionization has been responsible for upwards of $\frac{1}{4}$ of total wage inequality growth since the 1980s (Card 2001, Lemieux 2008, Western and Rosenfeld 2011).

Right to Work Laws

Right to Work (RTW) policies have long been central to political contests over union power at the state level and, less frequently, the federal level (Hogler 2015). The National Labor Relations Act (NLRA), commonly known as the *Wagner Act*, was enacted in 1935 and upheld as constitutional in the 1937 Supreme Court case, *NLRB v. Jones and Laughlin Steel Corp.* The NLRA provided basic rights to private sector workers to join trade unions, collectively bargain, and strike if necessary, while also establishing federal institutions, such as the National Labor Relations Board (NLRB), to provide enforcement of the act. This act catalyzed the early rise of US labor unions by providing federal institutional protections to labor and banning many anti-union tactics used by employers. Critical to the current study, the NLRA allowed for union security agreements, or contract provisions requiring all workers covered by a collective bargaining contract to pay dues to the negotiating organization (Collins 2014).

Employers quickly mobilized in the early 1940s to counter the growth of labor unions and labor’s policy victories during the second World War (Jacobs and Dixon 2006, Dixon 2007, 2008, Lee 2012). In response, some states passed early RTW laws, yet their legitimacy was challenged until the 1947 passage of the Labor Management Relations Act (Feigenbaum et al. 2018), commonly referred to as the Taft-Hartley Act. Section 14(b) of this bill marked the origins of state-level policy debates over union dues, providing federal support for state-level policies to prohibit compulsory collection of dues by employers or labor unions as a condition of employment.² In states with RTW laws, unions are unable to enact contracts requiring all covered workers of an establishment to join the union and/or pay union fees.³ The law became solidified after the Supreme Court’s 1949 ruling upholding RTW legality in *Lincoln Federal Labor Union v. Northwestern Iron and Metal Company* (Jacobs and Dixon 2006). Farhang and Katznelson (2005) described RTW as one of the most consequential

²It states specifically, “nothing in this subchapter shall be construed as authorizing the execution or application of agreements requiring membership in a labor organization as a condition of employment in any State or Territory in which such execution or application is prohibited by State or Territorial law” (Legal Information Institute 2017)

³More generous union rules, such as secondary strikes and closed shops, were forbidden through federal policies from 1947 onward (Baird 1998, Eidlin 2018).

provision in Taft-Hartley. Opponents to RTW laws argue that they enable free riding, decrease union organizational capacity, and ultimately undermine union legitimacy and power, as workers can receive benefits from union activity without sharing costs. Unsurprisingly, many national and regional employers and business organizations mobilized to financially support the passage of RTW laws (Dixon 2007, Lee 2012).

Research on RTW suggests that such action successfully assisted in reducing labor power. Farber (1985) finds RTW laws to account for 13% of total union decline through the 1980s, particularly through the flow of manufacturing jobs from highly unionized north eastern states to the US South. Ellwood and Fine (1987) show that unions become less successful in gaining new members through elections and in organizing larger workplaces following RTW passage. Moore (1998) synthesizes many early studies of economics and argues that the range of decline in union membership attributable to RTW laws is between 3-8%, with effects due to both loss of union membership and reduced likelihood of joining unions (see too Devinez 2011, Eidlin 2018). In total, RTW laws tend to successfully increase the costs of remaining in and joining labor unions.

Although RTW laws had substantial support from large employers and business organizations (Lee 2012, Hogler 2015), successful passage of RTW occurred unevenly due to local circumstances of politics and social movements. Historical studies of social movements designed around RTW laws illustrate the uncertainty and contentiousness surrounding attempts to pass or block these laws. Dixon (2008), for example, shows the contentious nature of movements and countermovements between unions and RTW advocates in Ohio and Indiana. Organizational features of local social movements—connection to national organizations, successful local coalition building of groups with disparate interests, and learning from previous social movement failures—helped explain why RTW briefly passed in Indiana but failed in the similar Ohio context. Canak and Miller (1990) document the historical accident of Louisiana’s laggard status among southern states, largely due to the eccentricities in the decision making of its country music star governor, Jimmy Davis. Lee (2012) outlines the historical system of social movement coordination between national and regional employer organizations needed to pass RTW laws. Dixon (2007) focuses on Texas, motivated by the fact that in the 1930s and early 1940s, Texas seemed to be a prime candidate for the reinvigoration of southern unions and was not an obvious candidate for RTW passage. Yet the coordination of local, regional, and national employer groups helped push the law through. And in Texas, like in

many Southern states, RTW was enabled partially through racial anxieties, as the labor movement faced unified political opposition when it became apparent that unions provided the potential to undermine the firmly established racial hierarchy in southern labor markets (Farhang and Katznelson 2005, Dixon 2007). Altogether, these case studies reveal that RTW spread not as a mechanical response to mass opinion, as has sometimes been suggested (Moore 1998, Collins 2014), but rather as a contentious political process.

Figures 1 and 2 show the historical rise of RTW laws across states and the contiguous US population. Figure 1 shows states with passed RTW laws and years of adoption. The initial push of RTW laws came in the 1940s, largely in Southern (Florida, Georgia, Tennessee, Arkansas, North Carolina, Texas, Virginia) and West North Central (Nebraska, North and South Dakota) states. Several East North Central states (Wisconsin, Michigan, Indiana) have recently passed RTW laws, while Kentucky and West Virginia have passed RTW laws after the current study's year sample.

[Figure 1 About Here]

Figure 2 shows the expansion of RTW laws across states, and across the contiguous US population.^{4,5} Passage of RTW laws accelerated between 1945 and 1960, with the number of states having passed RTW laws increasing from three to 18. The number of states with RTW laws remained relatively constant until the late 1970s, and since then RTW laws have steadily spread across states, with the contemporary number at 25. By 1960, approximately 30% of the US population resided in states with RTW laws. The percentage of the US population residing in RTW states increased gradually from 30% to 40% between 1960 and 2010. Since then, the proportion of the population in RTW states increased sharply, so that nearly 1/2 of the current US population lives in RTW states. Note the convergence of the percentage of RTW states and total population residing in RTW states. Through the 1980s, RTW was largely the domain of less populated states (e.g. Iowa, the Dakotas, Wyoming, Utah). RTW laws have been recently passed in more populous states (e.g. Michigan, Indiana), and population has grown rapidly in some early adopting states (e.g. Florida, Texas, Nevada).

[Figure 2 About Here]

⁴I use Frank's definition of *Tax Unit* to approximate US population.

⁵The axis shows the percentage of contiguous US states (and Washington D.C.) with RTW laws.

RTW provides a unique opportunity to assess how policy decisions in the subnational US context affect the ability of labor market institutions to shape market processes, and thus, inequality. Comparative scholars have long been interested in these processes and, drawing from the rich diversity of national contexts and histories, have thoroughly documented how partisan histories, policy bundles, class interests, and political mobilizations translated into cross-national variation of path dependent labor market institutional regimes that subsequently generate variation in how market processes occur (Rueda and Pontusson 2000, Huber and Stephens 2010, Brady and Leicht 2008, Pontusson 2013, Huber and Stephens 2014, Thelen 2014). Broadly speaking, policy decisions can affect the extent and form in which labor can mobilize workers, negotiate contracts, and join political debates over the distribution of economic resources. Fewer studies have applied these same insights to the subnational US case (but see Jenkins et al. 2006). Yet local political actors have grown in importance for social and economic policies (Brady et al. 2013), social policies have been shown to be consequential for economic wellbeing (Moller 2008, Moller et al. 2009), and some research shows how RTW laws affect union-based political mobilization (Feigenbaum et al. 2018). All these demonstrate substantial heterogeneity across local areas wholly described as a liberal market economy (see too Lobao and Hooks 2003, Lobao et al. 2008). Motivated by cross-national research showing that politics can alter the functioning of labor market institutions, and that labor market institutions are fundamental for understanding market inequality outcomes (Huber and Stephens 2014, Thelen 2014), the current research examines how RTW laws affect inequality across different local areas and historical eras.

The Right to Work and Economic Outcomes

How might RTW laws affect economic inequality? I argue that, should RTW laws have any economic consequences at all, they would do so *indirectly*, by reorganizing bargaining positions and power between employees and employers. Passage of RTW should create both real and symbolic changes that reduce labor power in a state, allowing for employers to more aggressively pursue their interests. For example, RTW laws should increase the freerider problem experienced by unions and reduce local union financial resources (Moore 1998), undermine future efforts for unions to recruit members and fragment efforts to successfully bargain (Ellwood and Fine 1987, Lee 2012), and signal a policy and business environment in which employers are placed in a privileged position over labor

(Rao et al. 2011). It is intuitive that such changes should vary in their consequences for inequality depending on the preexisting system of power resource distribution in the time and place where RTW is passed.

An indirect association would help reconcile seemingly discrepant empirical patterns in the literature. On the one hand, most previous research on the link between RTW and inequality is unified in finding no direct association. Moore (1998) summarizes early economic studies, finding little support that RTW laws do much to wage attainment or union premiums. Farber (2005) examines union and nonunion wage changes in Idaho and Oklahoma using Current Population Survey (CPS) data, finding insignificant changes following RTW passage. Hanley (2010) finds inconsistent effects of RTW laws across 80 metropolitan areas from years 1970 to 2000, with inequality perhaps growing faster in non-RTW metropolitan areas than in RTW metropolitan areas, and RTW states having lower levels of inequality than non-RTW states. Devinatz (2011) similarly synthesizes studies, finding RTW laws do little for wage attainment. Eren and Ozbeklik (2016) use synthetic control methods to compare the inequality trajectory of Oklahoma before and after the 2001 passage of a RTW law, finding little post-RTW change to wage attainment or inequality. Similarly, Kogan (2017) uses a difference-in-difference methodology between 1940 and 2012 and finds null associations for a variety of inequality measurements. Together, these findings suggest that RTW laws have minimal influence on, or theoretically unanticipated associations with, inequality.⁶ On the other hand, the absence of an effect of RTW on inequality runs counter to the substantial body of stratification research showing unions to be central to inequality outcomes, both in the United States (Freeman and Medoff 1984, Lemieux 2008, Western and Rosenfeld 2011, Kristal 2013, Autor 2014, Rosenfeld 2014, Jacobs and Dirlam 2016, Wilmers 2017, VanHeuvelen 2018a) and other high income countries (Alderson and Nielsen 2002, Moller et al. 2003, Brady et al. 2009, Pontusson 2013, Kerrissey 2015, Vanheuvelen 2018). How could laws that so straightforwardly reduce union power and align with power resources expectations be so inconsequential?

An indirect pathway between RTW and inequality is more sensible and helps reconcile these seemingly paradoxical sets of findings. I argue that previous studies have conceptualized the re-

⁶Several theories exist to explain the null effects of previous studies. Some have attempted to reconcile these findings through a *taste* hypothesis, arguing that laws are passed among publics who have little desire to unionize, and so laws are endogenous to social conditions where unions have little power or validity. Others suggest that RTW laws have the counterintuitive effect of *increasing* the motivation, efficiency, and effectiveness of unions in RTW states, effectively cancelling out any of the negative consequences of reduced membership and finances (Moore 1998, Devinatz 2011, Collins 2014). Still others argue that local policy changes affecting organizational capacity is dwarfed by more substantial economic factors that are of greater consequence than local policy change (Kogan 2017).

lationship between RTW and economic outcomes too bluntly, which has incorrectly led to null findings. Most importantly, previous studies treat RTW laws as uniformly consequential across eras and across states, with modeling decisions relying on the implicit assumption that the 2001 law in Oklahoma, the 2012 law in Michigan, and the 1947 law in Georgia are similarly consequential on economic outcomes. Such a decision makes little sense when considering the wide variation of union membership, political support, and intermingling of labor and racial rights across regions and eras (Katznelson et al. 1993, Farhang and Katznelson 2005, Jacobs and Dirlam 2016, Eidlin 2018). When passed in places with relatively strong unions, RTW may signal a real opportunity for employers to claim a greater share of power resources from labor, whereas when passed in places with relatively weak unions, RTW may simply be a signal of reaffirmation of a preexisting social context. In short, should RTW laws be consequential for distributional outcomes, they should be only when passed in times and places where labor has something to lose.

My argument highlights the inherently *relational* nature of RTW and, more generally, power resources theory. RTW was largely designed as a reactionary policy to reduce the growing power of labor over local workplace conditions. It should therefore matter in places where interested parties can use this potential change in labor’s power to successfully claim greater shares of status and economic rewards for themselves. A union’s successful organization of a workplace, and thus their capacity to both increase the force behind workers’ claims over the local distribution of economic resources and potentially labor’s place in local political decisions (Feigenbaum et al. 2018), occurs in relation to employers and other organizations similarly interested in maintaining control over economic resources and local political conditions (Tomaskovic-Devey and Avent-Holt 2019). Ferguson (2008), for example, demonstrates that successful union organizing can be thwarted through antiunion firm actions. More generally, firm-level research on changing structural conditions and labor market institutions shows how managers and corporate elites strategically address challenges to their own power and capacity to increase claims over status and rewards (Jung 2016, Benton et al. 2019, Benton and Cobb 2019). These points reinforce the basic fact that inequality is a result of interested groups with different power levels vying for valued resources in local spaces. I argue that, should RTW be consequential for inequality, its impact should occur in these local, relational spaces, adjusting local negotiating conditions by undermining labor’s ability to maintain a strong coalition that can successfully claim rewards for workers.

To assess this indirect association, I focus analyses on three key dimensions of the union-inequality link: behavioral responses of key actors, changes in the overall distribution of economic resources, and inequality change in specific locations of the wage distribution. First, if RTW rearranges power relationships in places where unions are strong, there must necessarily be a corresponding change in wage setting practices. Following previous research, I assess union membership change (e.g. Moore 1998). Furthermore, given that power resources is an inherently relational theory and that employers are unlikely to be passive agents responding to RTW status (Ferguson 2008, Rao et al. 2011), I also examine how firm behaviors towards labor unions varies based on union membership and RTW status (e.g. Rao et al. 2011). Second, I assess changes in overall inequality across RTW status, asking whether the association between RTW and inequality depends on the local concentration of union membership. Relatedly, I assess whether the well-established negative association between union membership and inequality is altered by the passage of RTW (Moller et al. 2009, Jacobs and Dirlam 2016, VanHeuvelen 2018b). If RTW affects the capacity for unions to affect the wage distribution, presumably the overall association between union membership and inequality should be altered following RTW passage. Third, I extend the assessment of the RTW-inequality link by focusing on different locations in the wage distribution. Union membership's positive effects on wages are largest for workers at the middle and lower end of the wage distribution (Firpo et al. 2009). Strong unions also tend to exert pressure to mitigate the takeoff of top managerial pay (Shin 2014), and more generally, have a normative influence over nonunion wages, keeping them relatively compressed (Western and Rosenfeld 2011, Denice and Rosenfeld 2018). These facts demonstrate that different mechanisms link unions to inequality at different locations in the wage distribution: a more immediate connection to union member wages in the middle and lower end of the wage distribution and a broader, more contextual link to top end wages. I assess how the association of each of these mechanisms respond to RTW.

Data

Main inequality data for this project are collected from three sources. First, I use census microdata from years 1940, 1950, 1960, 1970, 1980, 1990, and 2000 (Ruggles et al. 2017). Second, I use microdata from the American Community Survey (ACS) waves 2005-2016. I use three-year combinations

of ACS data to improve the precision of local area estimations,⁷ and so have data for combined waves 2005-2007, 2008-2010, 2011-2013, and 2014-2016.⁸ Third, I use Frank’s state-level inequality measurements collected from individual tax filing data from the Internal Revenue Service (IRS) covering 1939 to 2015 (Frank 2009, 2014).

I assess these data at two levels of analysis. First, I construct two datasets of contiguous states using the Frank data and a combination of the census and ACS data, resulting in datasets with 2,499 (Frank data) and 539 (Census/ACS data) state-year observations in main analyses.^{9,10} Second, given that some inequality change is due to labor markets pulling apart from one another within states (Moretti 2013), I use census and ACS data to construct a set of local labor markets covering the entire contiguous United States, commuting zones (CZ) (see VanHeuvelen (2018b) and Dorn (2009) for extended discussions of this unit of analysis).¹¹ CZs are census definitions of local labor markets, which are clusters of counties based on home and work locations (Tolbert and Sizer 1996). Simply put, there is more commuting between home and work across counties within a single CZ than there is across two CZs, meaning that labor market definitions are based on residence and work location rather than political jurisdictions. I use the publicly available codes from Dorn’s personal website to create CZs for years 1950, 1970, 1980, 1990, 2000, and 2005-2011. I then extend his sorting logic to geographical identifiers in years 1940, 1960, and 2012-2016. The contiguous United States is made up of 722 CZs, resulting in 7,942 CZ-year observations.¹² In total, main results are tested using three datasets covering two units of analysis, representing the most exhaustive and high quality set of data used to date to test the association between RTW laws, unionization, and inequality.

⁷This decision is akin to Autor and Dorn (2013), who combine five ACS years to measure ACS data at the commuting zone level.

⁸Results are unchanged if only the 2011-2013 ACS waves are used in an effort to create symmetrical gaps across census / ACS data.

⁹Main analyses using Frank data include years with observed state-level union data, which drop years 1940-1952, 1954-1959, and 1961-1963.

¹⁰Because the historical range of years used in analyses precede state admittance, I exclude Alaska and Hawaii.

¹¹While Right to Work policies apply at the state level, median household income and economic inequality vary dramatically across labor markets within states, such as differences between Silicon Valley and inland farming communities in California (Moretti 2013). In these commuting zone analyses, state policy and union membership are conceptualized as contextual variables.

¹²In Appendix Figure 10, I replicate all results using only CZs that do not overlap state boundaries.

Dependent Variable

Main results predict either *income inequality* or *wage inequality*. When using the Frank data, I assess the *Gini coefficient of individual income*. Inequality measurements are computed from binned categories of adjusted gross income, or all income for an individual from all sources, minus a various set of deductions. Frank computes the *compromise Gini coefficient* from these binned categories, suggested by Cowell (1995), that is the weighted combination of the Gini coefficient computed under the assumption that all individuals receive the mean bin income and under the assumption that all individuals receive income equal to either the lower or upper bound of the group interval.

This measure of income has several benefits, including the probable accuracy of income reported to the IRS, the coverage rate of the state-level income producing populations, and the long historical span of available data. However, it introduces a number of undesirable features that motivate the comparison across multiple datasets. First, the measurement is based on annual income and so inequality measurements are the partial combination of wage setting, hours worked, and income from non-employment sources. Union membership might decrease wage inequality but increase inequality in hours worked, which could result in spurious null associations between unionization and income inequality. Because unions influence both wage setting and work hours (Finnigan and Hale 2018), many stratification scholars focus on the association between wage inequality and unionization (Western and Rosenfeld 2011). Second, it is unclear how zero and negative incomes are treated in the data. Data management decisions for these cases can potentially result in widely variable inequality measurements. Third, it is unclear how unions might affect the distribution of certain sources of income, such as self-employment, interest, dividends, pensions, capital gains, and alimony, which are included in gross adjusted incomes. For example, union membership might increase pension inequality among older individuals, and may reduce alimony-based income inequality due to stabilization of marriage patterns (Schneider and Reich 2014). Fourth, inequality measurements might be skewed by individuals partially attached to the labor market, such as older and part-time or less workers. Fifth, female employment, wage setting, educational attainment, and occupational attainment dynamics have undergone substantial changes over the period studied, while unions differ in their influence over male and female wage setting (McCall 2001, Milkman 2007, Crocker and Clawson 2012, Rosenfeld and Kleykamp 2012). Thus,

stratification scholars routinely examine historical inequality processes separately by sex (Western and Rosenfeld 2011, Rosenfeld and Kleykamp 2012, Vanheuveren 2018).

To account for these shortcomings, I construct measurements of *wage inequality* from census and ACS microdata. I restrict samples to non-self employed, non-institutionalized workers aged 16-64 with a non-zero number of work hours. Observations are weighted by the product of IPUMS survey weights and the respondent’s annual number of hours worked over 1,400 (Acemoglu and Autor 2011). This decision allows for the incorporation of part time workers but guards against possibly biased estimates of inequality from individuals only marginally attached to the labor market. Wages are constructed by dividing a worker’s total pre-tax wage and salary income (inclusive of wages, salaries, commissions, cash bonuses, tips, and other money income received from an employer) by annual hours worked.¹³ Top coded wage incomes are multiplied by 1.5 (Acemoglu and Autor 2011),¹⁴ wages are bottom-coded to half the year-specific federal minimum wage (Goldin and Margo 1992), and wages are adjusted to 2009 dollars using the personal consumption expenditure index (VanHeuvelen 2018). I compute Gini coefficients for wages separately by sex, for all workers, and for prime-aged private sector workers.¹⁵ Finally, because unions have been shown to affect inequality at different locations in the wage distribution differently (e.g. Firpo et al. 2009), I compute two additional measures of inequality in the census/ACS samples: the 90/50 logged wage ratio and the 50/10 logged wage ratio (Lemieux 2006, Autor et al. 2008).

Labor Market Institutional Variables

This study focuses on two main independent variables. The first is a binary indicator of the passage of a *Right to Work* law (RTW). Observations receive a value of 1 if they occur in a state-year with a passed RTW law, and 0 otherwise. Year-specific passage of RTW is collected from Moore (1998), Kogan (2017), and Feigenbaum et al. (2018). Some laws are passed late in the year. For example, Nebraska’s RTW law passed on December 11, 1946. In cases where RTW laws are passed in the last three months of the year, I begin coding years as 1 in the subsequent year, although sensitivity

¹³Wage inequality measurements are similar, and typically slightly lower, when this weighting strategy is used. Results are substantively unchanged if the standard weights provided by IPUMS are used instead.

¹⁴In years 2000 onward, states differ in their topcoded incomes. I construct topcoded wages applying a uniform topcode of 250,000, following the logic of (Acemoglu and Autor 2011). While this discards top income information, it creates more consistency in top coded income across census and ACS waves.

¹⁵Results by gender yielded substantively similar results. They are presented in Appendix Table 1.

analyses show that results are the same if the simple year of passage is used instead.¹⁶

The second variable is *union membership*, measured as the percentage of a state’s non-agricultural workforce that are union members.¹⁷ I collect data for early years from the U.S. Union Sourcebook (Troy 1985), with data coming from Troy’s collection of union membership in organizations reporting annual financial reports under the Labor Management Reporting and Disclosure Act, organizations in the Bureau of Labor Statistics’ Directory of National Unions and Employee Associations, as well as other miscellaneous unlisted historical sources. Data in later years come from Hirsch and MacPherson’s Unionstat database, with original data coming from the Current Population Survey (CPS).¹⁸ One complication for the 1950 census dataset is that union information is only available in 1953. This may bias results, as union and inequality measurement are mismatched by four years. This issue might be particularly problematic for Nevada (RTW law passed in 1952) and Alabama (1953). In Appendix Table 5, I replicate main results excluding Nevada and Alabama, and the entire 1950 census wave.¹⁹ For CZ data, I construct union membership as a population-weighted average of state values, and RTW status is set by the dominant population center.

I adjust these data in two ways to reduce potential bias in the reporting of union membership. First, Farber et al. highlight the caution needed when using union data prior to the CPS’ collection of union coverage and membership (2018). They show that data might overstate union membership

¹⁶In their analysis of Oklahoma RTW, Eren and Ozbeklik (2016) note the long legal buildup and lingering legal disputes that surround the passage of RTW laws, suggesting that RTW laws might not affect economic outcomes cleanly at the year of passage. Unions and employers might change behavior during political debate prior to RTW passage (cf. Dixon 2008), and the certainty of the law’s trajectory may be ambiguous immediately following its passage. I therefore construct a four-category measure that indicates (1) early years (2) two years preceding RTW passage (3) the year of RTW passage and two subsequent years (4) all following years. Replication using these categories are included in Appendix Tables 2 and 3. Similarly, RTW laws may have different implications depending on when they passed, with first movers and late adopters facing different constraints and expectations. I replicated main results using a categorical variable distinguishing eras of RTW passage (Appendix Table 4). Analyses are further discussed in the Appendix.

¹⁷I tested an alternative commuting zone-level measure of union membership by combining industry-level union membership and state-level relative union membership data, using early industry-unionization information from Troy (1954) and applying his industry categorization to the CPS and census industry employment information. Unfortunately, Troy’s data only includes union membership information for the total service industry. This results in substantial within-state heterogeneity in 1940 and 1950, where service industry was less dominant in US employment, and almost exact similarity of state-level union membership in later years, where service industry employment is dominant. Overall, this strategy allows for within state union heterogeneity in early years, but not later ones, introducing an importance source of time variant bias. Data and results are available upon request.

¹⁸I compute union information one year prior to the year wage information is collected.

¹⁹Hirsch and Macpherson provide state-level union membership data for years 1964-1976 using the Bureau of Labor Statistics’ Directory of National Unions and Employee associations. The years 1964 to 1967 possibly include a discontinuity in counting and representation of public sector union membership in the BLS data, as noted by Troy. I therefore drop these years. Replication using years 1964-1967 from Hirsch and Macpherson in the IRS data yield similar conclusions.

by 5 percentage points prior to 1959, and by approximately 2.5 percentage points between 1959 and 1977. At the macrolevel, this means that union membership may be 81.5% the magnitude as documented by Troy prior to 1959 and 91% the magnitude from 1959 onward. I assume a uniform overstatement of union membership across states and in these year bands. Similarly, Card (1996) compares union membership from the CPS and employer data in 1977 and found a consistent 2.5 percent misclassification across union and non-union members, and across industries. I transform union membership across all years assuming a year- and state-uniform 2.5 percent union and nonunion member classification. Main variables are listed in Table 1.

To assess the theorized mechanism of the link between inequality, unionization, and RTW laws, I use two additional outcome variables originally collected by Andrew Martin from the National Labor Relations Board (NLRB). The first is the logged number unfair labor practices (ULPs) filed by unions against firms per 1,000 enterprises. The second is number of *decertification elections* per 1,000 enterprises, or NLRB-sponsored elections allowing a firm’s employees to vote to remove an existing unit as their collective bargaining representative. These two measures represent *firm threat*, or resistance by firms against unionization (Martin and Dixon 2010, Dixon and Martin 2012).²⁰ Data are measured at the state-year and available for years 1984 to 2002. No states clearly transitioned into RTW status during this era,²¹ and so I interpret results as partial and associational evidence for the theorized mechanism.

[Table 1 About Here]

Control Variables

In main results, I adjust for several state- and commuting zone-characteristics which might confound the association between RTW passage, union membership, and inequality. I follow the general logic of Moller et al. (2009) who detail how local area inequality is predicted by local conditions of economic development, demographic composition, politics, and social policy (see too Jacobs and Dirlam 2016, VanHeuvelen 2018b). Some local inequality is due to the mechanisms associated with the nonlinear association between development and inequality, some is a mechanical response to the aging of a population, some is due to discriminatory wage setting against minorities, and

²⁰Data were generously provided by Andrew Martin, per an emailed request.

²¹Idaho passed RTW laws in 1985 and Oklahoma in 2001. In each case there is insufficient pre- or post-RTW law implementation, and the shortcomings of focusing on aberrant cases discussed above apply.

some is due to related but distinct partisan and policy contexts. I include a set of controls to address these additional local sources of inequality: (1) the percent of individuals aged 25 and older with a high school degree or more, (2) population density (3) logged population (4) female labor force participation (5) the percent of black individuals (6) the percent of individuals aged 65 and over (7) the percent of foreign born residents (8) the unemployment rate (9) the minimum wage value, adjusted to 2009 dollars,²² (10) the percent of Republican control over both chambers of the the state legislature,²³ (11) whether Republicans hold unified control over the governorship and both chambers of the state legislature, (12) public sector employment I do not adjust for sector employment in main results. Doing so may bias results by conditioning on a collider (Elwert and Winship 2014). Rao et al. (2011) show that employment along state boundaries tends to be concentrated in RTW states, and Wright and Dwyer (2019) argue convincingly that inequality can create feedback loops into employment polarization. Similarly, Mazzolari and Ragusa (2013) find that the concentration of affluent households necessarily increases the use of home substitution industries, implying growth in the service industry following inequality growth. In sensitivity analyses, I find that both RTW and inequality significantly predict sector employment, a situation not found for other controls. Because this is an unsettled argument, I replicate all main findings controlling for manufacturing and agricultural employment and include these results in appendix materials (Appendix Figures 4, 5, Appendix Tables 11, 12). Measurement decisions and data sources of control variables are included in Appendix Table 6.

Methods

In all results that follow, I use linear regression models to examine the association between income or wage inequality, RTW legislation, and union membership. The basic logic of the state-level regression equations for main results are as follows:

$$Y_{st} = \beta_0 + \beta_1 RTW_{st} + \beta_2 Union_{st} + \beta_3 RTW * Union_{st} + \gamma_s + \theta_t + \epsilon_{st} \quad (1)$$

Y_{st} is the Gini coefficient for either income or wage inequality in state s and year t , while RTW

²²State-years without a state-level minimum wage are coded as zero, and a binary indicator is included for state-years with zero values

²³Nebraska does not have partisan information due to its unicameral legislative body. I impute Nebraska values using the year-specific mean of West North Central states (North Dakota, South Dakota, Kansas, Minnesota, Iowa, and Missouri). I similarly impute Minnesota's value in 1939.

is a binary indicator of whether a state-year has passed Right to Work legislation (1) or not (0). $Union$ is the adjusted state-year union membership, while $RTW*Union$ is the interaction term that tests for differences in the association between union membership and income / wage inequality before and after RTW legislation. γ is a set of year fixed-effects, and θ is a set of state fixed-effects. The basic specification of the association between RTW passage and inequality resembles a difference-in-difference model with time-varying treatments across units and more than two time points (Angrist and Pischke 2008, Kogan 2017).²⁴ Models include robust standard errors clustered at the state level, which Bertrand et al. (2004) demonstrate to be an important and rigorous correction for serial correlation in these models. Additional regression models include a vector of coefficients, $\beta_4 Z$, for control variables.

CZ data include three nested units: CZ-years nested in CZs nested in states. For these data, I follow the methodological approach of Moller et al. (2009) and VanHeuvelen (2018b) and estimate three-level mixed-effects regression models. The basic logic of these models are as follows:

$$y_{cst} = \pi_1 RTW_{st} + \pi_2 Union_{within} + \pi_3 RTW_{st} * Union_{within} + \pi X_{between} + \pi X_{within} + \gamma_{st} + \epsilon_{cst} + \mu_c \quad (2)$$

RTW status is interacted with the within-CZ deviation of state-level union membership.²⁵ There are two types of control variables: $X_{between}$ indicates the CZ-specific mean value of an independent variable. X_{within} indicates the CZ-year deviation from the CZ-mean value (see Allison (2009) for more information on these models). Regression models are estimated with unstructured residual errors in the lowest-level group, CZ-years nested in CZs, that allows for any pattern of correlation in the residuals over time (see Moller et al. (2009) for additional details).

These methods provide broadly appropriate estimation techniques to detect the association between RTW status, union membership, and inequality, as they adjust for all time-invariant state- or commuting zone-level characteristics as well country-level changes across time periods. Previous

²⁴Appendix Figure 1 shows state trends in Gini coefficients prior to RTW adoption, across states that did and did not adopt RTW laws. These allow for an assessment of parallel trends across states that do and don't adopt RTW laws, a critical requirement for difference-in-difference models. Although I do not attempt to rely on these models to make strong causal claims, it is nonetheless useful to observe similar pre-trends in inequality across RTW and non-RTW states.

²⁵I tested results using a simple binary indicator of RTW status, a within-CZ deviations of RTW status, and restricting the sample to CZs that experienced RTW change. Main results present the former, but results using alternative measurement decisions did not change main results.

research has relied on these methodological decisions to effectively argue that estimated coefficients represent causal effects (Schneider and Hastings 2017). However, RTW laws are not randomly assigned across state and historical contexts, a critical condition if using these methods for strong causal inference (Bertrand et al. 2004), and more generally, recent methodological research shows that two-way fixed effects are no panacea for causal inference (Imai and Kim 2019). Thus, additional care is needed to ensure that results are not overly driven by confounding time-varying unobserved factors.

I further assess two-way fixed effects results using an instrumental modeling strategy for RTW status.²⁶ I construct two sets of instrumental variables. First, I compute the percent of neighboring states that have passed RTW laws, as a local state may experience pressure to pass RTW laws if neighboring states have done so. Second, I use CZs that overlap state boundaries to create two variables indicating the number of overlapping labor markets in a local state with neighboring RTW states. For example, the Vicksburg CZ in western Mississippi includes Louisiana’s Madison Parish. In years following Mississippi’s RTW passage in 1954, but before Louisiana’s passage in 1976, Madison Parish may have existed under a de facto RTW status. More of these overlapping markets may increase pressure for a state to pass RTW. I divide this concept into two variables: core-into-periphery and periphery-into-core. The former variable measures the RTW status of the main population hub of a CZ spilling over into more sparsely populated counties of a state. Consider the Spokane, WA CZ, which includes a few less populated Idaho counties: the core-into-periphery variable would be coded as 0 (non-RTW) for Idaho in the year 2000, as the larger population center of the Spokane, WA does not have RTW status. Conversely, the periphery-into-core variable would be coded as 1 for Washington State in 2000, as the smaller population Idaho counties have RTW status. Values for these overlapping statuses are summed together, so higher values indicate a greater number of potential spillover pressures.

Valid instrumental variables must be related to the independent variables of interest, and must only be related to the outcome of interest through the independent variable of interest. The former can be empirically tested, the latter cannot. Specification tests suggest that the instruments in this study are appropriately related to local RTW. F-tests for each instrumental variable for each dataset are well above the standard threshold of 10. Regarding the relationship between instruments

²⁶For interactions between union membership and RTW, I also use interactions between instrumental variables and union membership as instruments.

and local inequality, it is reasonable to assume that connections between a neighboring state’s RTW status and inequality should occur primarily through the local state’s RTW status. For example, an employer may move a firm across state lines based on RTW laws (Rao et al. 2011), thus altering employment levels, the distribution of wages, and subsequently inequality in the local state. However, such processes should be channeled solely through local RTW status and thus not open a back door path between neighboring RTW and inequality.²⁷ Of course, neighboring RTW might proxy as a general anti-labor political sentiment, which may pressure local policymakers to similarly enact anti-labor legislation and local employers to take stronger stances against unions.²⁸ I therefore include two additional controls in models: (1) the mean Republican control of neighboring state legislatures and (2) the ratio of the local control of Republican control over the state legislature to the mean of the neighbors. Caution is merited: conditioning on neighboring Republican control creates at best a conditional instrumental variable, which is less desirable than an instrumental variable that is as good as randomly assigned (e.g. Harding et al. 2018). Appropriate skepticism must be therefore applied when interpreting results. Nevertheless, these additional analyses help increase confidence that results are not overly driven by spurious, unobserved factors.

Results

Behavioral responses to RTW

The main argument of this paper is that the consequences of RTW should affect inequality differently depending on the local conditions of labor power. To justify such a focus, I begin by demonstrating that behavioral responses to these laws of employees and firms differ across contexts based on labor strength. Having established a behavioral foundation for heterogeneous responses to RTW, I then assess in the following sections of the Results where, and how, RTW affects inequality.

Do RTW laws affect union membership? In places where unions are strong, yes. Table 2 shows results from regression models predicting union membership before and after RTW passage using two-way fixed-effects models, with and without controls, and interacted with a state’s average level

²⁷A firm would be unlikely to relocate across state boundaries based on a neighboring state’s RTW status unless the local state were not a RTW state. Thus, the influence of neighboring RTW on local inequality should occur through the local state’s RTW status

²⁸Appendix Table 8 shows associations between controls and instruments. Four of the 36 associations are statistically significant. Areas with more neighboring RTW pressure tend to be less educated, have smaller black populations, have lower minimum wages, and a greater share of Republicans in the local state legislature.

of union membership.

[Table 2 About Here]

Models 1 and 2 include results from all contiguous US states, without and with controls. While the coefficients for RTW are negative in both models, they do not reach conventional statistical significance levels. However, this null association is due to a concentration of the association in states where unions have traditionally been stronger. Models 2 and 3 include an interaction term between RTW and state-average union membership rates. In these cases, a statistically significant and negative association between union membership and RTW is detectable among more highly unionized states. The rightmost columns show the RTW coefficient across unionization levels. For low through average unionization levels, the passage of RTW has an insignificant association with union membership change. From states with an average membership rates at about 16% (corresponding with Alabama and Kentucky's overall average), RTW has a larger substantive effect on subsequent union membership at higher average union levels, with the magnitude growing from about a 2 percentage point decline to a 5.5 percentage point decline in the historically most unionized RTW states (Michigan, Wisconsin, Indiana). Notably, the relative magnitude at these high levels is large. The largest inter-year contrast from Model 1 is approximately 13 (a coefficient of 7.88 in 1953 minus the coefficient of -5.33 in year 2014, both coefficients relative to the base year of 1939). Thus the passage of RTW legislation in the most unionized states associates with a decline of union membership that is approximately 40% the magnitude of the shared decline of union membership between the peak of the 1950s and the contemporary low-water mark ($5.5 / (7.88 - -5.33)$), while RTW in a typical union context corresponds with a decline that is 15% the magnitude of the shared maximum year contrasts. Ellwood and Fine (1987) showed that most of the effect of RTW on union membership occurs through reducing the number of elections held, followed by a reduction in the typical size of organized workplaces. Presumably, densely unionized places face a greater shock in their organizing efforts, relative to state contexts where unions have little presence. Sensitivity analyses show a high correlation between year of RTW passage and average union membership ($\rho=0.70$), suggesting that RTW laws passed in early eras—largely in the US South—may have been designed to thwart union organizing efforts, while later RTW laws may have been passed to reduce or reverse union power.

RTW might not only affect inequality through reducing union numbers, but also by allowing

for employers to more aggressively pursue their interests. I now examine how two antiunion firm behaviors, the logged number of unfair labor practice filings and decertification elections per 1,000 firms, vary across unionization rates and state RTW status. Firm threat behaviors are measured between years 1984 and 2002.²⁹ Figure 3 visualizes results of the predicted level of firm threat behaviors across union membership, by RTW passage. All three coefficients for interaction terms, included in Appendix Table 7, are statistically significant for both outcomes.³⁰

[Figure 3 About Here]

Results are largely similar across outcomes, and two patterns are noteworthy. First, among RTW state-years with relatively high unionization, firms tended to pursue anti-union practices more frequently.³¹ Rates of decertification elections and filed unfair labor practices tended to be higher in places like Nevada and Iowa (RTW states) than in Illinois, Minnesota, and Oregon (non-RTW states). Second, among states with lower unionization levels, RTW states had relatively *lower* firm threat rates compared to similarly low unionized non-RTW states. Rates of decertification elections and filed unfair labor practices tended to be higher in Colorado and pre-2001 Oklahoma (non-RTW states) than in Louisiana, Texas, Virginia, and post-1985 Idaho (RTW states), for example. Unsurprisingly, firm threat behaviors were more frequent in state-years with higher unionization rates (ULP $\beta=0.06$, $p < 0.001$, decertification $\beta=0.10$, $p < 0.001$, two-tailed tests), and these firm threats similarly tended to be *lower* both in less unionized RTW and non-RTW compared to their corresponding more unionized RTW and non-RTW states (e.g., for decertification, RTW states: $\beta=-1.61$, $p < 0.001$, non-RTW states: $\beta=-0.60$, $p < 0.05$, both two-tailed tests). Notably, Ferguson (2008) showed that unfair labor practice filings were highly correlated with both the successful halting of union elections and undermining the successful enactment of a union contract, noting that firm threat behaviors likely were implemented when a union was perceived to be in a weak bargaining position. Altogether, firms tend to engage in these anti-union behaviors when RTW laws

²⁹All independent variables are lagged by one year. All independent variables used in previous models are used in the models of this section.

³⁰I do not include state fixed effects because the large majority of variation of both outcomes occurs between states: 96% for unfair labor practices and 85% for decertification elections. As one would expect, the addition of state fixed effects removes all significance for unfair labor practice results, although the coefficients point to the same pattern as those in the main text. Results for decertification elections with state fixed effects replicate main results, with a few notable caveats. The main union coefficient is statistically insignificant ($\beta = -.006$, $p=0.70$), while the interaction between RTW and union membership is positive and significant ($\beta=0.05$, $p< 0.01$), which shows that decertification elections take place more frequently in highly unionized RTW states compared to highly unionized non-RTW states.

³¹The RTW contrasts at high and low unionization rates are statistically significant at $\alpha = 0.05$ for both outcomes.

signal that they may be *effective*, and stop when RTW laws signal that they may be *unnecessary*.

Considered together, results from Table 2 and Figure 3 are suggestive that key stakeholders—workers and firms—tend to behave differently in different contexts based on the combination of RTW status and local power resources. RTW tends to decrease union membership and co-occurs with more anti-union firm behaviors in times and places where unions are strong. Together, these results highlight that RTW laws may be of greater consequence for real and symbolic power held by unions in more densely unionized local areas, suggesting that RTW laws may be more consequential for changes to power resources held in these places. I next turn to test main expectations, that RTW laws affect inequality in places where labor unions hold greater power.

Inconsistent Main Effects of RTW on Inequality

The following sections include the main results of the paper, where I investigate the association between RTW and inequality. I first replicate inconsistent main effects of RTW found in previous research. I then allow associations to vary across social contexts based on union membership. Finally, I allow these findings to vary across low-end inequality, where unions should have both direct and indirect effects on wage setting, and top-end inequality, where union power should be more contextual.

Table 3 shows results from two-way fixed-effects models estimating the association between RTW law passage and income / wage inequality across the three datasets. Models in the lower panel include instrumental variables.

[Table 3 About Here]

Although results across datasets point towards a positive association between RTW passage and inequality, no consistent results are found, as statistically insignificant, or negative, associations are detected in seven of the twelve estimated models. For IRS and CZ datasets with instrumental variables, models result in significant and positive associations, suggesting that a positive association may be masked by confounding factors not fully accounted for by observable characteristics of local areas. Yet no pattern of result is detectable across modeling decisions or datasets. Taken as a whole, results in Table 3 are inconclusive and could be easily understood as aligning with previous research skeptical that RTW has any consistent association with inequality. However, I argue that these inconsistent results mask substantial variation across places where changing bargaining power

relationships should be more and less consequential. I therefore turn to results that interact union membership and RTW law passage to predict income and wage inequality.

RTW, Union Decline, and Inequality

I next examine the expected mechanism linking RTW passage and local inequality change, via the reduction of union-based power resources. If RTW laws reduce the real or perceived power that unions have over work conditions and wage setting and embolden employers to take additional anti-union actions, then presumably the association between union membership and inequality should be smaller following passage of RTW laws. To measure this anticipated effect, I interact union membership with RTW passage and replicate models from Table 2. I assess these union-by-RTW interactions in all three datasets, in models without and with controls, and models that do and do not use instrumental variables on RTW status. To ease interpretation across datasets with different levels of analysis and measures of inequality, I standardize both the inequality outcome and union membership. These fully-standardized coefficients of union membership pre- and post-RTW passage are visualized in Figure 4.

[Figure 4 About Here]

Figure 4 clearly illustrates the theoretically anticipated mechanism linking RTW law passage and union decline. Across datasets and specifications, union membership has the predictable negative and significant association with income/wage inequality, *prior to the passage of RTW laws*.³² In RTW states prior to the law’s passage, the rise and decline of labor unions associates with the decline and rise of inequality.³³ A standard deviation increase in unionization is associated with between a 0.1 and 0.28 standard deviation reduction in inequality. However, the negative association between union membership and inequality is removed following the passage of RTW laws, as 10 of the 12 confidence intervals in the right panel include zero. In total, unions are consequential for reducing inequality outside RTW contexts, but not inside them.³⁴

³²Of course, one must also mind the variation of effects. Interactions vary substantially in their magnitudes. I draw claims based on consideration of the whole set of results.

³³All results replicate if states that never pass RTW laws are removed from the sample.

³⁴The consistent results across datasets, levels of analysis, and modeling decisions increase confidence in the robustness of these findings. Furthermore, all results presented are net of unobserved time invariant heterogeneity and mutually shared heterogeneity in change over time, and are estimated from models that include robust clustered standard errors, an effective method to account for serial correlation stemming from state/CZ-specific inequality trajectories (Bertrand et al. 2004).

Predicted Effects for Inequality

To illustrate the meaning of the above results, Figure 5 presents predicted Gini coefficients from the three datasets following models with controls and instrumental variables. For example, focusing on census/ACS state data, the predicted levels of state wage Gini coefficients in RTW status are stable at just under 35, while non-RTW state-years range from 31.5 to 35.5. The largest substantive differences across RTW status occurs among state-years where union membership is highest. At these levels, the RTW gap in the Gini coefficient is approximately 2 Gini points,³⁵ or $\frac{2}{3}$ of a standard deviation in the observed Gini coefficient across the entire sample. To put this magnitude in comparison, the maximum between-year contrast from a simple regression model include year contrasts occurs between 1949 and 2014-16 of 7.44. Thus, the maximum relative magnitude of RTW on inequality is approximately 25% the magnitude of the shared growth of wage inequality between its low value at the end of the Great Compression decade and the contemporary heights of inequality growth.

[Figure 5 About Here]

In order to provide a more intuitive understanding of the variation of RTW consequences, I compute counterfactual levels of inequality in four states: Idaho, New Hampshire, Ohio, and Oklahoma. Idaho and Oklahoma are frequently used to argue against the importance of RTW on economic outcomes (Farber 2005, Eren and Ozbeklik 2016, Kogan 2017). New Hampshire briefly passed RTW legislation in 1947 before a subsequent repeal in 1949, while Dixon (2008) highlights that the failed 1958 RTW referendum in Ohio was a far from certain outcome in a key battleground state in the industrial Midwest. I first predict wage inequality by year in each state using instrumental variable regression models with controls used to construct Census/ACS results in Figures 4 and 5. I then compute a counterfactual level of inequality in each state, either removing RTW status (Idaho 1990 onward and Oklahoma 2005 onward) or adding RTW status (New Hampshire 1950 onward and Ohio 1960 onward). I then adjust union membership levels based on the state's mean union level, in reference to results from Table 2.³⁶ Results are visualized in Figure 6.

³⁵While the maximum contrast for the whole sample is approximately 3 Gini points at the highest level of union membership, I make my comparison at the value of 35% union membership, which is the maximum value for RTW state-years in the data.

³⁶I replicated these counterfactual changes using the Frank IRS and CZ data, drawing similar conclusions. Figures are available upon request.

[Figure 6 About Here]

As with previous studies (Farber 2005, Eren and Ozbeklik 2016, Kogan 2017), I find that RTW laws are inconsequential for inequality in Idaho and Oklahoma, two relatively late-adopters of RTW with historically low levels of union membership. However, the null findings in these states cannot be generalized to other contexts. For example, using the same model predicting these null results, I observe significantly higher counterfactual wage inequality in Ohio in each time period in 1960 and beyond. The initial increase is substantively large, about 30% of the magnitude of wage inequality change in Ohio during the great compression decade of the 1940s. The magnitude of RTW in Ohio declines over time, from about 1.5 in 1960 to 0.64 in 2014-2016, illustrating the declining potential importance of RTW for inequality as Ohio deunionized. In New Hampshire, inequality was predicted to be significantly higher through 1980 following counterfactual RTW passage, albeit at much more modest magnitudes, with the maximum contrast at about 0.95 in 1950. Smaller magnitudes in this context make sense, given that New Hampshire's maximum union membership was relatively modest, at 22% in 1950.³⁷ These counterfactuals underscore the basic conclusion of this section. RTW laws are consequential for economic inequality when they are passed in times and places where weakening labor unions would result in a meaningful readjustment of local power resources.

Sensitivity Tests

I conducted several additional analyses to assess the robustness of these results. To determine whether results are unduly driven by a particular state, I replicated results first using jackknife estimation and then bootstrap estimation at the state- and CZ-levels, and reached the same results. To determine whether RTW affects union membership specifically, or whether one simply observes broadly shared changes in associations between inequality and local characteristics before and after RTW passage, I interacted all control variables with RTW status, but did not find changes in statistical significance similar and similarly consistent to union membership results. Might these interaction effects detect some spurious unobserved form of regional change in unionization's association with inequality over time not accounted for by year contrasts? To address this possibility,

³⁷Earlier, 16% was described as an average level of union membership. That referred to the average level across 1940-2016. New Hampshire's overall mean is 14%, close to the typical value. New Hampshire's maximum union membership in this time period, in contrast, is the 15th lowest of the contiguous states.

I created a placebo treatment variable among non-RTW and late adopting RTW states with randomly assigned region-specific RTW passage dates, following the general logic of Bertrand et al. (2004). I replicated Figure 4 1,000 times with a new placebo variable (more detail is included in the Appendix). Results show that in the large majority of cases, the association between union membership and inequality remains significant and negative before and after these placebo treatment across datasets. This finding partially assuage concerns that results in Figure 4 are spuriously detecting some alternative and broadly shared change in the association between unionization and inequality that happens to have occurred around the times of RTW passage. To ensure that results are not overly driven by the earliest or latest RTW laws, I replicate main results ending analyses at 2005 (Appendix Figures 6, 7). Building on this, I also verify that union membership levels are the appropriate measurement for union strength, as compared to relative union strength. No result replicates if deviations from year-specific means replace union membership levels. Finally, to guard against the possibility that results are driven by spurious local area-specific inequality trajectories, I refit models using fixed-effects and individual slopes models (Appendix Table 9) and reached the same conclusions (Ludwig and Brüderl 2018).

One sensitivity analysis did consistently alter main results. I replicated results in Southern and non-Southern regions, based on previous research showing the unique and unified anti-labor politics existing in the US South (Katznelson et al. 1993, Rosenfeld 2014, Jacobs and Dirlam 2016, Eidlin 2018). Results hold in non-Southern states and CZs, while results are mixed across models in Southern states. Notably, sensitivity results in Southern states reveal generally nulls associations between union membership and inequality before RTW passage, which helps explain why RTW passage would not remove this association. This variation raises the possibility that the passage of RTW laws stemmed from substantively different goals in different regions and eras: in the US South in the 1940s, RTW laws may have been passed to preemptively thwart labor organizing and growing in strength, while in non-South regions, RTW laws may have been passed to reverse or reduce union strength (Farhang and Katznelson 2005). Yet these results further reinforce the logic of the main conclusion of Figure 4: *RTW laws remove the inequality-reducing influence that labor unions have on inequality.*

RTW, Union Decline, and Distributional Properties of Wage Inequality

I conclude results by assessing *where* in the distribution of wages RTW translates into inequality, motivated by the fact that union membership affects wage attainment differently across the wage distribution (Firpo et al. 2009). I interrogate how overall results from Figure 4 occur in high- and low-end wage inequality using the 90/50 and 50/10 logged wage ratios (Lemieux 2006, Autor et al. 2008). Results are presented in Table 4.³⁸

[Table 4 About Here]

Of the many patterns notable in Table 4, I highlight two. First, contrary to expectations and the above results, RTW passage has a simple positive association among low-end wage inequality. This is shown in the bottom panel of Table 4. Results are substantively large. For example, the magnitude of 50/10 inequality change associated with RTW status among the state sample is equivalent to one standard deviation of the 50/10 logged wage gap across the whole sample. Note that interactions between union membership and RTW are only marginally detectable in the CZ dataset. Thus, across state contexts with higher and lower union membership, RTW laws are possibly associated with greater separation between middle and lower wages. Given that the direct effect of unionization of wages is heavily concentrated in the 10th through 50th percentiles of the wage distribution (Firpo et al. 2009), these findings represent compelling evidence that RTW has broadly felt consequences on unionization’s link to inequality among those who have the most to gain from strong unions and union membership.

Second, the significant interaction between RTW status and union membership is found in inequality among higher-end wages, the 90-50 log wage ratio. The interactions and sensitivity tests visualizing predicted values show that the gap between high and middle wages to be positive and statistically significant among RTW states with high union membership. The difference between RTW and non-RTW states in higher-end wage inequality converges, and reverses, at low levels of union membership. Sensitivity analyses yield insights into this initially counterintuitive finding among low-unionized state-years. Results are primarily driven by changing median wages. Median wages are approximately 4% (2.25 v. 2.29) higher in low-unionized RTW states compared to low-unionized non-RTW states. However, median wages are 15% higher in high-unionized RTW

³⁸Main analyses replicated separately by sex yield the same conclusions.

states compared to low unionized RTW states (2.28 vs. 2.43), and 27% higher in high-unionized non-RTW states compared to low-unionized non-RTW states (2.25 vs. 2.49). Simply put, median wages decline substantially in lower-unionization contexts, overwhelming the smaller RTW effect in these low-unionization contexts.³⁹ Overall, these results are suggestive that RTW laws influence inequality differently in different locations of the wage distribution. Among low-end wage inequality, RTW laws simply associate with higher inequality. Among top-end wage inequality, RTW laws further reduce the power of unions to keep middle and top wages close together.⁴⁰

To recapitulate, RTW laws do affect inequality, but in no uniform way. I draw conclusions in this paper based on four sets of results. First, behaviors of key organizational actors differ across RTW contexts. Union membership declines at a greater rate in more unionized places following RTW, while employers take greater antiunion actions in highly unionized RTW states and low unionized non-RTW states. I argue that these behaviors of key stakeholders provide empirical justification to move beyond overall effects of RTW. Second, mirroring previous research, RTW has no consistent main association with inequality. But this null finding masks substantial and robust heterogeneity. Third, RTW removes the ability of unions to affect inequality. In times and places where labor is weak, RTW laws have little effect on overall inequality. In contrast, these laws can be substantively meaningful when passed in times and places with strong union membership. Finally, RTW affects different distributional types of inequality differently. Variation across local context is primarily driven by top-end inequality, while RTW laws have uniform and large consequence for workers in the middle and lower end of the wage distribution.

Conclusion

Right to Work (RTW) laws have been central to local policy battles over labor rights for over three quarters of a century. Although these laws infrequently receive attention in sociological stratification research, they address core theoretical tenets of the subdiscipline and have proven to have long lasting political resonance (Farhang and Katznelson 2005, Hogler 2015). Given their central place in the local determination of labor rights and labor's control over power resources, it is critical for stratification scholars to fully assess their consequences on local distributional economic

³⁹The 90th wage percentile levels similarly decline among lower-unionization contexts.

⁴⁰As with overall results from Figure 3, these interactions are primarily detectable outside the US South.

outcomes.

How do RTW laws affect the distribution of economic resources? Based on the results of this paper, I argue that RTW laws have an *indirect* association with inequality. I construct income and wage inequality information at both the state and commuting zone levels from IRS, census, and American Community Survey data, and merge these to a unique dataset of union membership constructed from the US Union Sourcebook, Bureau of Labor Statistics, and Current Population Survey. I then assess inequality trends before and after the passage of every RTW law between 1939 and 2016. In doing so, I clarify mixed results from previous research by demonstrating that RTW laws are consequential for inequality when they are passed in social contexts where labor has substantial power resources to lose.

I replicate null findings in previous research, demonstrating that tests based on the assumption that RTW laws are uniformly consequential across times and places produce, at most, inconclusive results. Yet these belie consistent and robust heterogeneity in the consequences of RTW across local contexts. RTW removes the negative association between union membership and inequality, which translates into higher inequality following RTW passage in times and places with high union membership. Substantively, this heterogeneity means that a similar law passed in different social contexts can have different consequences. RTW can have large effects in the more industrialized and unionized Ohio, meager effects in Idaho and Oklahoma, and modest, albeit detectable, effects in mid-20th century New Hampshire.

This finding is the central contribution of the current paper, as it challenges the null conclusion of most previous RTW studies (Moore 1998, Farber 2005, Hanley 2010, Eren and Ozbeklik 2016, Kogan 2017). Yet a more substantive consideration of these studies help explain why they found null results. Many restrict focus to the contemporary era with low union membership and pay special attention to states with available pre- and post-RTW data in the Current Population Survey, Idaho and Oklahoma (Farber 2005, Eren and Ozbeklik 2016, Kogan 2017). While these studies provide valuable contributions to the inequality and causal literatures, they face theoretical challenges when assuming that findings from these cases can be generalized to other states and historical eras. Oklahoma and Idaho were late adopters of RTW, both had low levels of union membership—less than 10%—around the time of RTW passage, and both states have unique characteristics that may bias the association between RTW and inequality, including low population density, low racial and

ethnic diversity, low female labor force participation, low manufacturing employment prior to and during deindustrialization, and Oklahoma’s heavy reliance upon oil extraction for employment and state funding. Simply put, these studies tend to select on times and places where RTW laws should be less consequential.

I then extend analyses by demonstrating how RTW laws affect the wage distribution. Scholars have routinely shown that the union wage premium is greatest in the middle and lower end of the of the wage distribution (Firpo et al. 2009). If RTW laws reduce union power and shift individuals out of union jobs, this change should intuitively have the greatest relative impact among middle- and lower-paid and skilled jobs, where union premiums are greatest. In contrast, inequality among middle and high paid workers reflects labor’s broader, contextual ability to keep top pay in check. Shin (2014) showed that dense unionization in an industry kept top pay within firms in check, while Western and Rosenfeld (2011) and Denice and Rosenfeld (2018) show that unions are broadly consequential for nonunion wage setting across industries, regions, and occupations. I find that low-end wage inequality, or the 50-10 logged wage ratio, is consistently and positively affected by RTW laws, regardless of union membership levels, while top-end wage inequality, or the 90-50 logged wag ratio, is positively affected by RTW among more highly unionized contexts.⁴¹ These results provide a critical additional wrinkle to main findings. For workers most directly affected by union decline, RTW has a more uniform inequality inducing effect. For workers more connected to unions by broader, contextual mechanisms, RTW is consequential where it most directly challenges union power.

To establish a theoretical foundation for the results in this paper, I show that the behaviors of key organizational stakeholders vary in relation to local labor power and RTW status. Union membership typically declines by approximately 15% the magnitude of the total within-state union decline observed between the peak and nadir of union membership in the post World War 2 era following RTW passage, with the magnitude of decline growing in more unionized state contexts. Similarly, union avoidance tactics taken by employers vary based on the combination of policy context and labor power resources. In states with RTW laws and relatively high union membership, employers participate more frequently in anti-union behaviors compared to highly unionized non-RTW states, while employers engage less frequently in anti-union behavior in RTW states with

⁴¹At low unionization levels, RTW has a negative and significant association with 90-50 wage inequality. This is primarily a function of the substantial decline of median wages in both RTW and non-RTW contexts in low-unionized state-years.

low union membership compared to low union non-RTW states. These suggest employers base anti-union behavior on its perceived effectiveness and necessity to pursue their firm's interests. Considered as a whole, I argue these findings signal shifts in the effectiveness of the claims made by key groups over control of economic rewards following RTW passage (Tomaskovic-Devey and Avent-Holt 2019). Passage of RTW laws increases the difficulty of labor mobilization, reduces the financial standing of unions, and signals a pro-business environment where firms are less likely to face an opposition that can successfully organize (Rao et al. 2011). These combine into real changes in labor power, such as the reduction of members and financial resources, greater effort and less predictability in organizing efforts, as well as symbolic changes in labor's standing and status, which should increase the difficulty of workers to successfully make claims in local places (Tomaskovic-Devey and Avent-Holt 2019). RTW thus potentially reorganizes the bargaining position of employers and employees, opening up a new relational space between actors as they compete for control over economic resources. The primary consequence of this reorganization is to allow firms and employers to more aggressively pursue their interests. In locations where such a change represents a significant departure from preexisting relationships, key actors should face different probabilities of successfully bargaining for a greater share of economic rewards. Conversely, in contexts where such a change does not fundamentally alter bargaining relationships, little change should occur, as RTW laws may simply be a reaffirmation of an established set of policies and power relations. Or, in Southern states in the 1940s, they may have been passed to preemptively thwart gains in labor power. Thus, economic inequality should change following RTW passage in contexts where such passage establishes a new space for relative bargaining positions to adjust.

This article helps move forward the subnational study of power resources theory in three ways. First, recent studies of unions, inequality, and poverty have made noteworthy contributions to the stratification literature by applying the logic of power resources theory to the variable policy contexts of US states (Brady et al. 2013, Jacobs and Dirlam 2016). However, these studies primarily rely on broadly defined and frequently unobserved mechanisms linking local labor power resources to inequality change. I move forward this field by demonstrating how theoretically anticipated heterogeneity in the link between labor power and inequality can be detected surrounding the passage of specific policies. Given that RTW laws are by no means the only potential policies to incorporate into power resources studies, future studies would benefit with a similar focus on the

specific observed mechanisms of inequality change associated with union decline.

Second, this study moves forward the study of politics and inequality in the subnational US context. Comparative stratification research has extensively documented how policies can fundamentally structure the operations of labor market institutions, which then subsequently affect market processes (e.g. Rueda and Pontusson 2000, Kenworthy and Pontusson 2005, Brady and Leicht 2008, Pontusson 2013, Thelen 2014). With some notable exceptions (e.g. Jacobs and Myers 2014), few studies of the US context apply these insights. I demonstrate that, across the set of local labor markets that make up an overall liberal market economy, the ability for labor unions to affect local inequality critically depends on the local system of politics and policies. In states where RTW places unions at a significant power disadvantage, predicted inequality looks similar to that found in low unionized contexts. These findings dovetail with cross-national research that focuses on institutional variation, and institutionally-rooted changes in response to policy change (e.g. Thelen 2014).

Third, I highlight the need for further application of power resources to firm behavior. Often lost in empirical components of power resources studies is the employer side of attempts to leverage and control power resources. Many previous studies understandably focus on macrolevel outcomes associated with labor strength and left politics. However, the focus of Power Resources Theory is inherently *relational*, conceptualizing multiple actors working to leverage greater control over valued resources (Korpi 1985). RTW laws affect the balance against competing parties, and there is no theoretical reason to expect firm behavior to remain constant across policy change (e.g. Rao et al. 2011). Although such movements and countermovements between competing factions have been demonstrated in historical social movement research in the events leading to decisions on RTW passage (Dixon 2008) and more broadly in the social movements literature focusing on antibusiness protests (Bartley and Child 2014, Dixon et al. 2016, Martin and Dixon 2019), these relational insights have not been fully incorporated into studies of the consequences of RTW laws.⁴² While firm-level results in the current project must be understood as tentative, they nevertheless illustrate a fruitful path for future power resources research.

This research leaves several questions unanswered, which can hopefully be addressed in future research. I focus on four. First, and perhaps most critically, this research only superficially addresses

⁴²Of course, some studies, most notably by Brady and Leicht (2008) and Jacobs and Dirlam (2016), focus on the consequences of Right party power. However, this too can be considered an outcome of negotiation between employees and employers.

the topic of race. Yet as shown by Farhang and Katznelson (2005), Dixon (2007), and Rosenfeld and Kleykamp (2012) racial stratification has been central to the development and decline of labor rights and union membership. Indeed, while power resources theory mostly focuses on class distinctions in the formation of social policy and inequality, it is unlikely that one can successfully apply this theory to the subnational US context without more fully addressing the historical and conceptual intersections between labor and race. Future research on power resources in the US context would do well to address the intersection of racial stratification, labor rights and union decline.

Second, I focus primarily on geographical units, states and local labor markets. Yet sociologists and economists have highlighted the critical importance of within- and between-firm processes for inequality trends (Tomaskovic-Devey and Avent-Holt 2019). For example, several scholars examine firm-level data to understand how labor unions affect work conditions and pay distribution (Shin 2014, Jung 2016, 2017), while Mun and Jung (2018) assess the influence of policy change at the firm level regarding maternity leave in Japan. Similarly, Wilmers (2018) has demonstrated the need to examine between-firm power relationships to fully understand wage inequality, and he also develops a promising method to examine the contemporary firm-level influence of unions on wage attainment and dispersion (Wilmers 2017). Unfortunately, most firm-level data is available only in recent years and for a small number of states (but see Wilmers 2019). Should the theoretical mechanisms presented in this article prove to be empirically sound, future firm level research would help illuminate more precisely the processes at play. A fuller shift to firm-based studies of power resources theory is a critical avenue for future research, to which the current study can provide a historical context of wage attainment, union membership, and policy change.

Third, workers and firms might adjust mobility decisions in response to local policy changes. If a sufficient number of workers move across states or across local labor markets in response to RTW, these decisions could aggregate up to detectable inequality changes. Furthermore, mobility decisions could occur unevenly across workers. Perhaps more financially secure middle-income workers have greater ability to relocate following policy changes, potentially leaving behind the most and least vulnerable workers. Similarly, firms might relocate to new state locations based on how friendly local politics are for employers. RTW laws might draw firms from heavily unionized industries, and RTW might ease the ability for firms to shed employment without fear of an organized pushback. Alternatively, unequal mobility may create important individual-level trajectories missed in the

aggregate measurements of inequality used in the current study. Future longitudinal research following individuals or firms would help clarify the full consequences of RTW passage.

Fourth, future work on the composition of RTW laws could better reveal their consequences. Although RTW laws passed in different states are broadly similar in goals and content, they do vary in meaningful ways, such as the degree of penalty for violations and the segments of the workforce affected. Similarly, the significance of RTW passage might vary meaningfully across states, depending on whether it was passed in relative isolation from, or bundled with many other, antiunion policies. In the current research, I implicitly assumed zero variation of content in RTW laws and their passage across states conditional on local political partisanship. Yet the appropriateness of this assumption should be interrogated in future work.

Will RTW laws be consequential in the future? My results suggest how they might be. Perhaps of greatest consequence is the recent Supreme Court decision in *Janus Vs. AFSCME*, which some describe as a federal-level application of RTW to the public sector. If this description is accurate, then my results suggest a potentially large positive effect on wage inequality, especially among highly unionized states, given the high union density among the public sector. Of course, such an outcome is far from certain. Bargaining and employment relationships in the public sector differ markedly from the private sector, and different values and relationships held by employers might make them more reluctant to aggressively undertake union avoidance tactics. In contrast, Wilson et al. (2015) demonstrate the negative consequences for racial stratification following the broad, if uneven, push in “new governance” public sector employment that resembles the more discretionary form of wage setting and employment in the private sector. The economic consequences of the *Janus* case may similarly play out unevenly across public sectors depending on their preexisting set of labor rights.

To further address this idea, Appendix Figure 9 shows changes to total, public, and private sector unions from 2008 to 2018 in Indiana, Wisconsin, and Michigan, three recent RTW states with traditionally strong public sectors. Whereas public sectors trends followed national trends before RTW in Indiana and Michigan, one can observe wild fluctuations in Indiana and steady decline in Michigan after their laws were passed. Similarly, in the private sector, one can observe these states departing from national trends to more volatile downward fluctuations. Wisconsin merits special consideration. Wisconsin passed a bill limiting public sector unions in 2011, and one

can easily see the consequence of that pre-RTW bill among the state’s public sector trends. RTW laws were passed in the middle of a general downward trajectory. Where one can observe RTW’s effect in Wisconsin is in the private sector, where union membership dropped from 85% of 2008’s level to 55%. These examples show how public unions might respond to Janus. There may be more temporal fluctuation in membership as stability and predictability are removed from organizing efforts. But these descriptive trends also underscore the point that RTW laws are by no means the only policy options available to alter public sector labor power. Any analysis of public sector unions following the *Janus* decision would do well to focus on such between-state heterogeneity.

In total, results of this project show that Right to Work laws work as intended. They weaken unions, and that in some places, such a weakening of labor power results in high growth of inequality. Market earnings distributions are the product not only of market forces, but also of power distribution. Local policy battles and political mobilization are highly consequential for the distribution of economic resources, but the detection of this relationship relies on an understanding of the preexisting political and social context.

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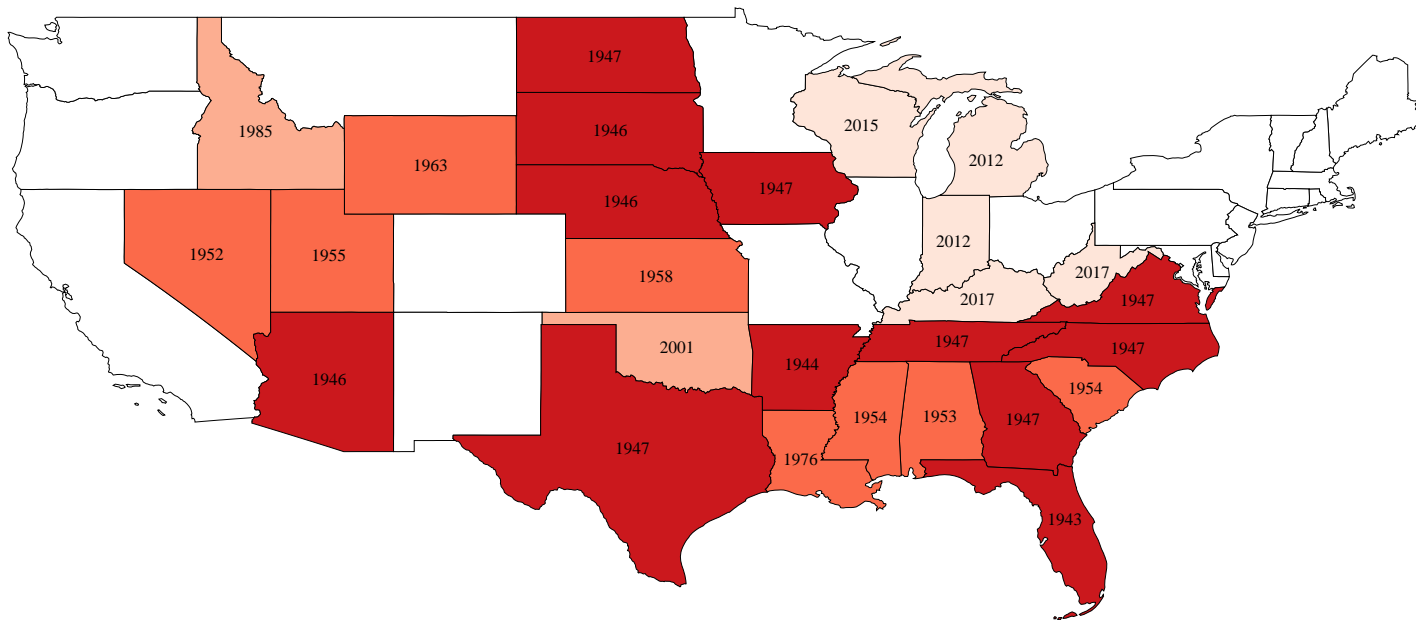


Figure 1: Right to Work Passage, by State

Notes: Darker shades indicate earlier passage of Right to Work laws.

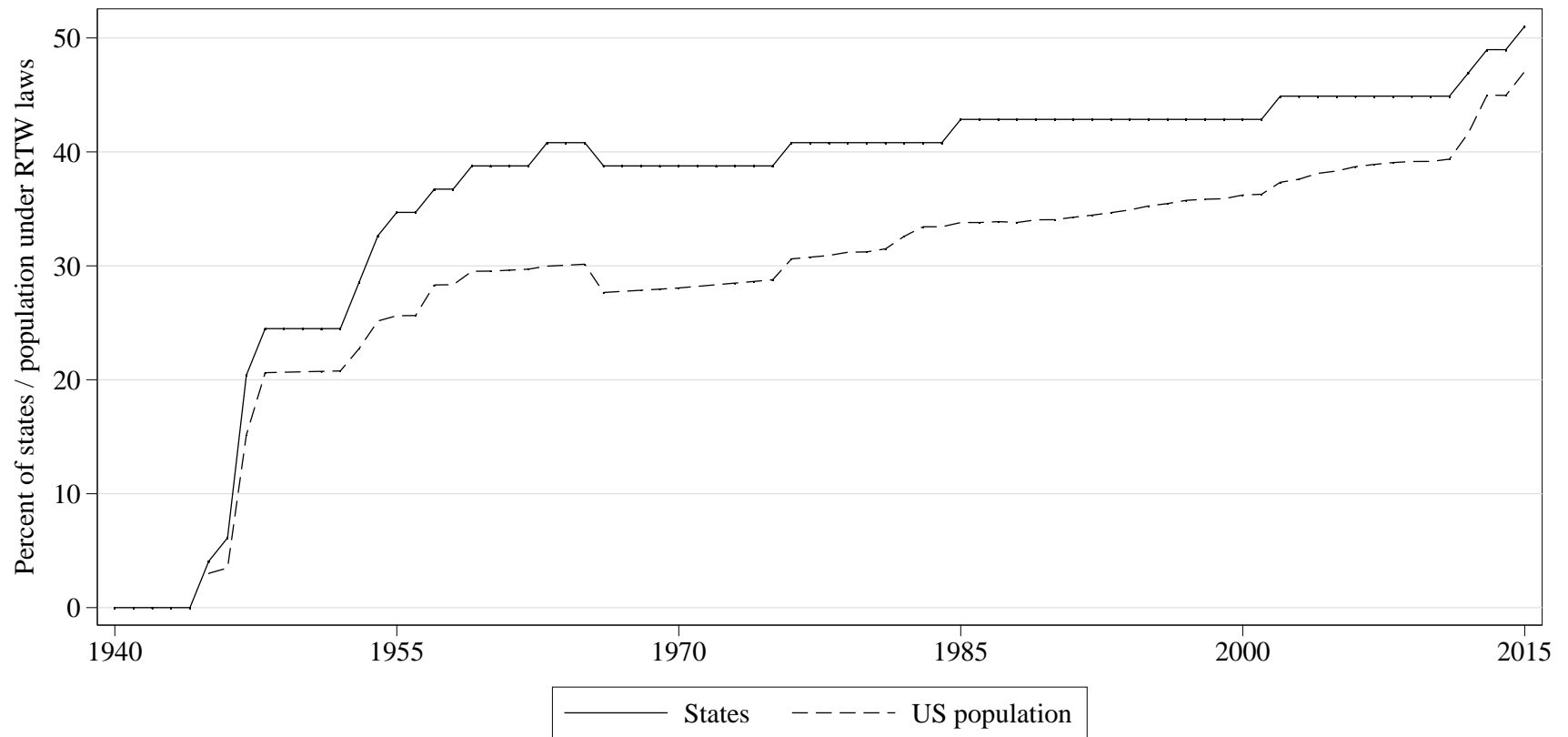


Figure 2: Expansion of Right to Work Laws across Contiguous States and Population

Notes: US population is proxied using tax units from Franks (2009) state level income inequality IRS data. Alaska and Hawaii are excluded from analyses.

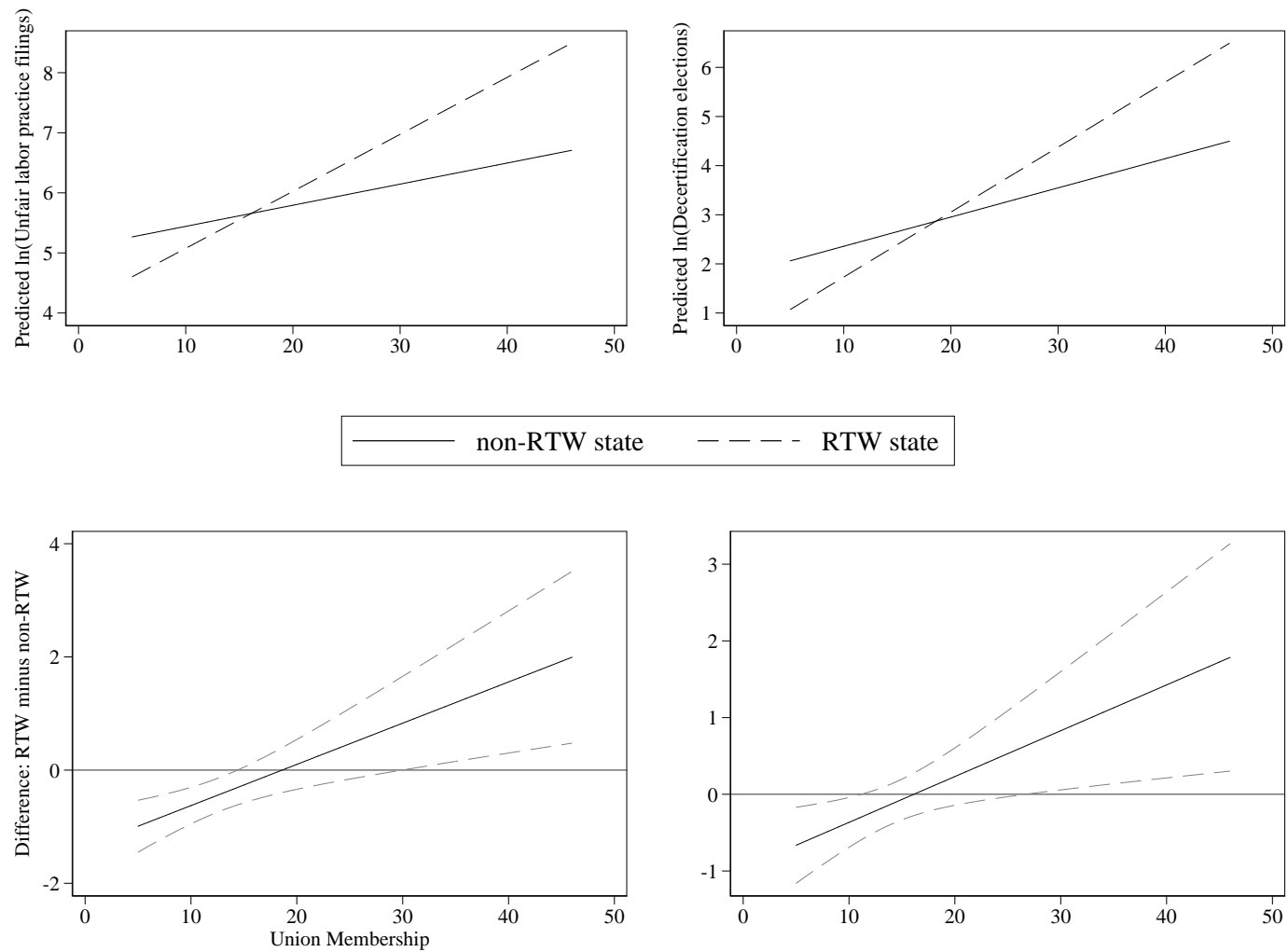


Figure 3: Predicted Anti-Union Firm Behavior 1984-2002, by Union Membership and Right to Work Status

Notes: Data are from years 1984-2002. Anti-union firm behavior per establishment from National Labor Relations Board (Martin and Dixon 2010). Top row shows predicted values of logged firm threat behaviors. Bottom row shows contrast of RTW and non-RTW values across levels of state union membership. Dashed lines in bottom row are 95% confidence intervals. Results predicted from models that include controls listed in Table 3.

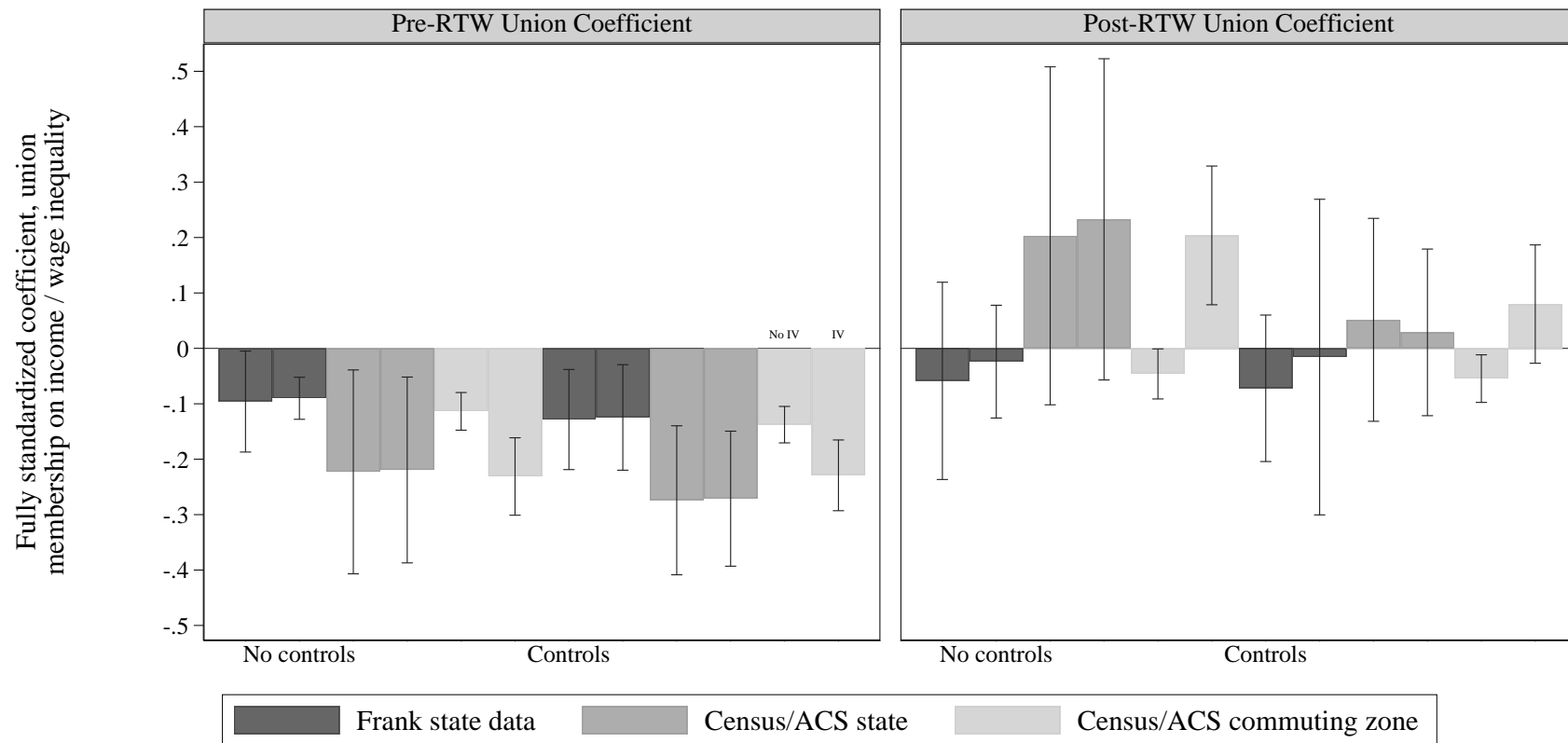


Figure 4: Union Coefficient for Income / Wage Inequality, Before and After Right to Work (RTW) Passage

Notes: All models include state or CZ fixed-effects, year-fixed effects, and use robust clustered standard errors. Control models include the following covariates: percent female labor force participation, percent black, percent immigrant, logged population density, logged population, percent aged 65 and over, minimum wage value, unemployment rate, Republican control of state legislature, unified Republican control, public employment concentration, mean value of Republican control of neighboring states, and the ratio of local and neighboring Republican control. Capped lines indicate 95 percent confidence intervals. In matched shaded bars, left bars do not include instrumental variables, right bars do. Instrumental variables include the percent of neighboring states with RTW laws and the number of labor markets in a state that overlap into a Right to Work state. See Table 3 for data sources.

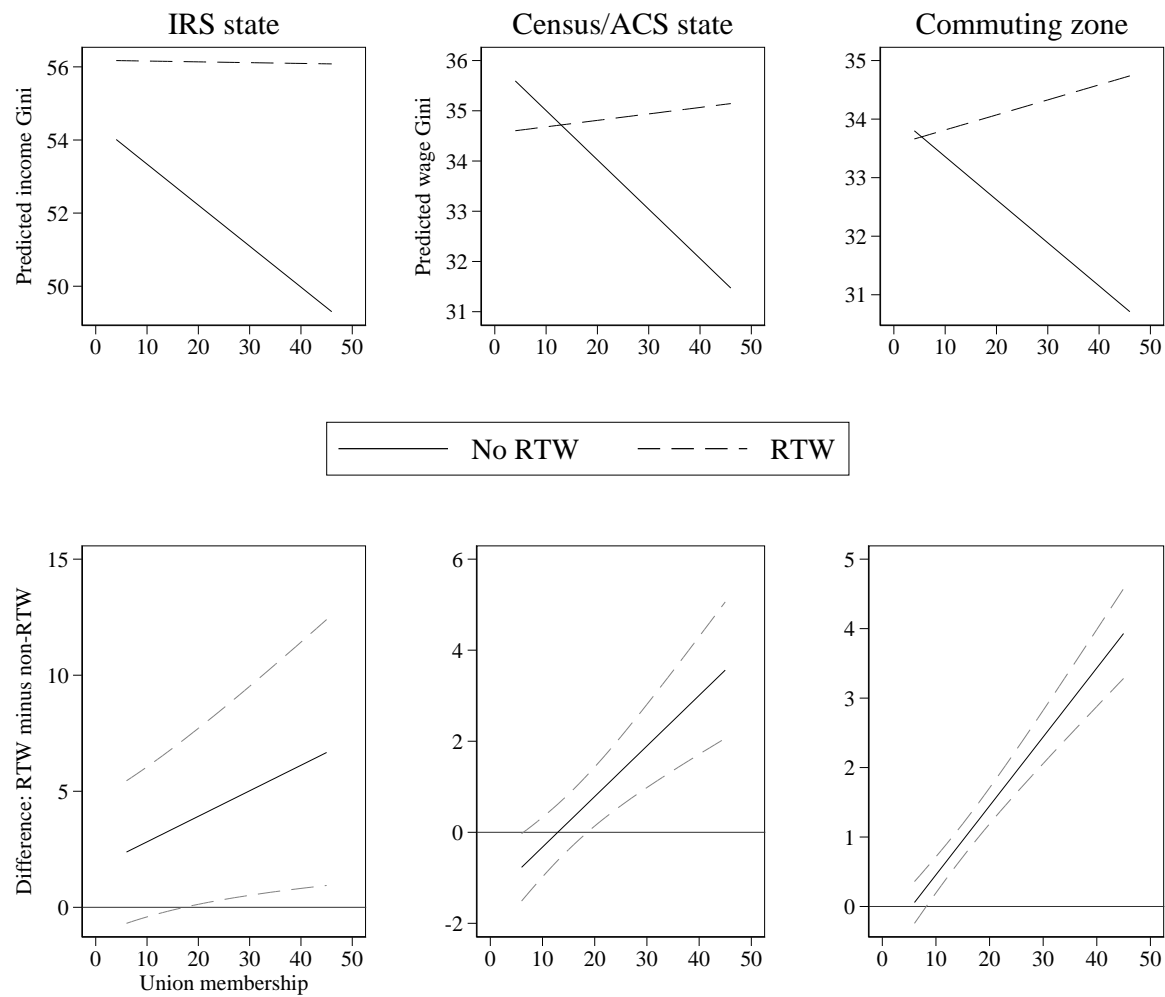


Figure 5: Predicted Wage and Income Inequality, by Union Membership and Right to Work Status

Notes: Values are predicted from regression models used to compute Figure 4 with controls and instrumental variables, but with inequality and union membership not standardized. Dashed lines in bottom row indicate 95 percent confidence intervals.

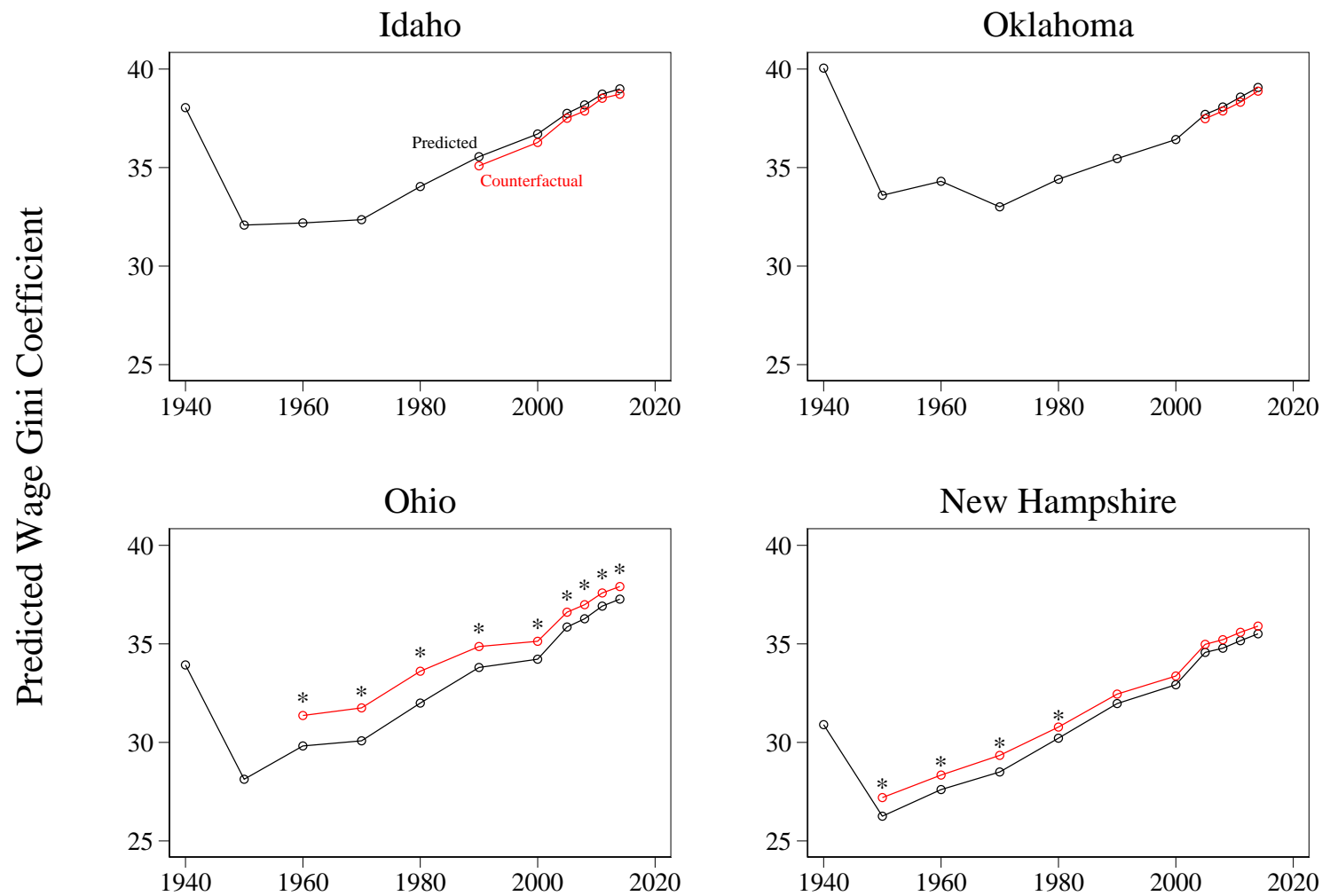


Figure 6: Counterfactual Changes to Inequality in Four States

Notes: Predicted values follow from state census/ACS wage inequality data with controls and instrumental variables. Predicted values (black lines) estimated for state-specific values of independent variables following regression models. Counterfactual models (red lines) switch RTW status and adjust union membership based on results from Table 2. Stars in figures indicate significant gaps between predicted and counterfactual inequality levels at 0.05 level. ⁶

Table 1: Descriptive Statistics for Main Variables

	Mean	SD	Min.	Max.	non-RTW mean	RTW mean	Dif
<i>Frank IRS state data, n=2,499</i>							
Income Gini coefficient	53.69	6.44	33.65	71.72	52.78	54.97	2.18
Right to Work	0.42	0.49	0	1			
Union membership	17.01	7.44	3.92	46.23	20.39	12.25	-8.14
<i>Census/ACS state data, n=539</i>							
Wage Gini coefficient	34.27	3.04	24.87	41.75	33.91	34.88	0.97
90/50 log wage ratio	0.80	0.10	0.51	1.10	0.79	0.82	0.03
50/10 log wage ratio	0.90	0.12	0.33	1.30	0.90	0.92	0.02
Right to Work	0.36	0.48	0	1			
Union membership	17.48	8.48	3.63	46.23	20.55	11.99	-8.55
<i>Census/ACS commuting zone data, n=7,942</i>							
Wage Gini coefficient	33.20	2.71	19.13	46.02	32.83	33.59	0.76
90/50 log wage ratio	0.73	0.12	0.30	1.48	0.73	0.74	0.01
50/10 log wage ratio	0.80	0.15	0.00	1.38	0.79	0.81	0.02
Right to Work	0.50	0.50	0	1			
Union membership	16.28	8.55	3.63	46.23	20.80	11.78	-9.02

Notes: Frank data include 51 year observations (1939 to 2015) from 48 contiguous states and Washington D.C. Census/ACS data include 11 year observations (1940 to 2014-2016) from 48 contiguous United States plus Washington D.C. Commuting zone data include 11 year observations (1940 to 2014-2016) from 722 commuting zones making up entire contiguous United States. All mean differences are significant at $p < 0.001$ (two-tailed tests).

Table 2: Two-Way Fixed-Effects Linear Regression Models on Union Membership

	Main effect		Interaction: average union membership		RTW effect at levels of mean union membership from (4)		
	(1)	(2)	(3)	(4)	Mean union level	RTW effect	SE
RTW passed	-1.200 (0.887)	-1.265 (0.901)	4.371** (1.564)	3.798* (1.854)	7	1.40	(1.27)
					9.5	0.54	(1.08)
					12	-0.32	(0.90)
RTW X			-0.390*** (0.078)	-0.343*** (0.092)	14.5	-1.17	(0.76)
Average unionization					17	-2.03**	(0.66)
					19.5	-2.89***	(0.64)
					22	-3.75***	(0.69)
					24.5	-4.60***	(0.81)
Controls?	No	Yes	No	Yes	27	-5.46***	(0.97)
N=2,499							

Robust clustered standard errors in parentheses

+ p<0.10 * p<0.05 ** p<0.01 *** p<0.001, two-tailed tests

Notes: Controls include female labor force participation, percent aged 25+ with high school degree, unemployment rate, percentage black population, percent aged 65+, percent foreign born, minimum wage value, logged population, logged population density, percent of state chamber seats held by Republicans, uniform Republican control of state government, percent public employment, mean neighboring Republican control, and ratio of neighboring and local Republican controls. All models include state- and year- fixed-effects. See Data section for description of union membership data and sources.

Table 3: Two-Way Fixed-Effects Linear Regression Models of Income / Wage Inequality on Right-to-Work Laws

	State				Commuting Zone	
	Income Gini		Wage Gini		Wage Gini	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Model a: Two-way fixed-effects</i>						
Right to Work	0.6255 (0.551)	0.5399 (0.392)	-0.7255 (0.456)	0.2964 (0.337)	0.2227 (0.174)	0.7917*** (0.173)
<i>Model b: +Instrumental variable</i>						
Right to Work	2.3723*** (0.482)	6.9101** (2.143)	-0.8933* (0.403)	0.1844 (0.351)	0.4735*** (0.104)	0.5672*** (0.106)
Controls?	No	Yes	No	Yes	No	Yes
N	3,773	3,773	539	539	7,942	7,942

Robust clustered standard errors in parentheses

+ p<0.10 * p<0.05 ** p<0.01 *** p<0.001, two-tailed tests

Notes: Controls include female labor force participation, percent aged 25+ with high school degree, unemployment rate, percentage black population, percent aged 65+, percent foreign born, minimum wage value, logged population, logged population density, Republican control of state legislatures, presence of unified Republican control of state government, public employment rate, mean Republican control of neighboring states, and ratio of neighboring and local Republican control. All models include state- and year- fixed-effects. Instrumental variables include the percent of neighboring states with RTW laws and the number of labor markets in a state that overlap into a Right to Work state. Frank dataset includes larger number of observations because sample not restricted by union membership data availability. State income gini from IRS state data 1940-2015 (Frank 2009). State and commuting zone wage gini data from census and ACS 1940-2016 (Ruggles et al. 2019).

Table 4: Two-Way Fixed-Effects Linear Regression Models of the 90/50 and 50/10 Logged Wage Ratios on Right to Work, Union Membership, and Controls

	<i>90/50 Logged Wage Ratio</i>							
	State				Commuting Zone			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Right to Work	-0.0385** (0.013)	-0.1177*** (0.023)	-0.0320* (0.013)	-0.1102*** (0.021)	-0.0055 (0.004)	-0.0049 (0.004)	-0.0160* (0.007)	-0.0673*** (0.012)
Union Membership	-0.0040*** (0.001)	-0.0040*** (0.001)	-0.0040*** (0.001)	-0.0044*** (0.001)	-0.0018*** (0.000)	-0.0020*** (0.000)	-0.0031*** (0.000)	-0.0029*** (0.000)
Right to Work X Union Membership		0.0053*** (0.001)		0.0049*** (0.001)		0.0011*** (0.000)		0.0031*** (0.001)
Instrumental variables?	No	No	Yes	Yes	No	No	Yes	Yes
	<i>50/10 Logged Wage Ratio</i>							
	State				Commuting Zone			
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Right to Work	0.1317*** (0.032)	0.1368** (0.040)	0.1238** (0.039)	0.1335* (0.054)	0.0138** (0.005)	0.0177*** (0.005)	0.1146*** (0.014)	0.0882*** (0.020)
Union Membership	-0.0008 (0.002)	-0.0008 (0.002)	-0.0008 (0.002)	-0.0008 (0.002)	-0.0000 (0.001)	-0.0002 (0.001)	-0.0001 (0.001)	-0.0000 (0.001)
Right to Work X Union Membership		-0.0003 (0.001)		-0.0001 (0.002)		0.0011** (0.001)		0.0017* (0.001)
Instrumental variables?	No	No	Yes	Yes	No	No	Yes	Yes
N	539	539	539	539	7,942	7,942	7,942	7,942

Robust clustered standard errors in parentheses

+ p<0.10 * p<0.05 ** p<0.01 *** p<0.001, two-tailed tests

Notes: All models include control variables. Controls include the female labor force participation rate, logged population, logged population density, the percent of the area that is black, percent that is foreign born, the percent that is over the age of 65, the percent of individuals aged 25 and over with a high school degree or more, the unemployment rate, the minimum wage value, the percent of employed individuals 16 and over working in the public sector, the mean control of Republican control over the state legislature, whether Republicans control both houses and the governorship, the mean Republican control of bordering states, and the ratio of local and neighboring Republican control. All models include state- and year- fixed-effects. Instrumental variables include the percent of neighboring states with RTW laws and the number of labor markets in a state that overlap into a Right to Work state. State income gini from IRS state data 1940-2015 (Frank 2009). State and commuting zone wage gini data from census and ACS 1940-2015 (Ruggles et al. 2019).