Workers, Wages, and Economic Mobility: The Long-Run Effects of Right-to-Work Laws

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Introduction

Right-to-work (RTW) laws are one of the most prominent and politically contentious economic policy issues in the U.S. today. RTW laws are enacted by state governments to prohibit what are called “union security clauses” in collective bargaining agreements. These union-supported clauses can require workers at unionized firms to join the union or pay agency fees to the union as a condition of employment. In practice, when a state has RTW protections, workers at unionized firms cannot be required to pay fees to the union in order to retain their job.

Both sides of the political divide make emphatic claims about the impacts of RTW laws, and about unions more broadly. Despite many historical studies, there is little consensus on the impact of RTW laws on local economies. RTW proponents, such as business lobbies, argue that RTW laws make states more attractive for investment, and they typically point to faster growth in income and total employment in RTW states over recent decades as supporting evidence. For example, states that were RTW in 1977 experienced aggregate employment and population growth during 1978–2017 of 105% and 90%, respectively, compared with 49% and 35% among non-RTW states.1 Conversely, RTW opponents, including unions and union-supported think tanks, argue that stronger unions benefit both unionized and nonunion workers and that RTW laws weaken unions and thus undermine their ability to produce these benefits. They highlight that wages and nonwage compensation levels in RTW states are lower than in non-RTW states.2

Who is right? Alas, interpreting the statistics behind these competing claims is a fraught task.
different in numerous ways, some observable and measurable, others not. These differences include variations in climate; population demographics, culture, and cost of living; historical differences in economic development and wage levels; and different exposure to technological change and changes in transportation costs. Because of these fundamental differences, there is no good reason to expect that, except for right-to-work laws, RTW and non-RTW states would have equal economic outcomes or experience economic growth at the same rate.

My academic research with Benjamin Austin addresses this issue and serves as the basis of this Manhattan Institute brief. We use modern econometric techniques to study a narrow geographic sample where RTW and non-RTW areas are plausibly otherwise similar. In this case, we compare counties that are in different states (one state is RTW, the other is not) but border each other. Then, we estimate the long-term impact of RTW laws, and whether those laws cause the observable economic differences between RTW and non-RTW areas.

Our method allows us to measure the impacts of RTW laws on a wide range of economic outcomes. In addition to revisiting debates about the impact of RTW laws on manufacturing employment, overall employment, and wage outcomes for workers, we study how RTW affects migration and where people choose to live. We leverage more recently available data to study the impact of RTW laws on downstream socioeconomic outcomes, including poverty rates and intergenerational mobility. We present evidence about which workers are affected.

In this issue brief, I provide a nontechnical overview of this academic work and discuss its policy implications. Our results suggest that RTW laws produce substantial economic benefits, which accrue to workers through stronger local labor markets, and more generally through improved socioeconomic outcomes, especially for children and families that are more economically at risk. Specifically (and consistent with previous research), we find that RTW laws sharply raise a state’s manufacturing share of employment by approximately 28%, or 3.23 percentage points. This increase in manufacturing is not from crowding out other industries; rather, it results in stronger local labor markets in general. Residents of the RTW border counties have a 1.58-percentage-point higher employment rate and a 0.39-percentage-point lower unemployment rate than non-RTW border counties, and more people commute into (than out of) RTW counties for work. Contrary to claims by unions and their advocates, we fail to find any evidence that RTW laws reduce wages—in fact, our results suggest that RTW laws slightly increase per-hour wages, especially for low-income workers.

Consistent with this pattern of stronger labor-market outcomes for individual workers, we observe that the RTW sample experienced 19.1 percentage points extra population growth between 1940 and 2010, indicative of stronger labor markets driving greater net in-migration. We find that RTW laws lead to improved social outcomes: childhood poverty rates are lower by 2.29 percentage points in RTW border counties, and there is substantially higher upward income mobility for people who grew up in the bottom half of the socioeconomic distribution.

### Origin of Right-to-Work Laws

The National Labor Relations Act of 1935 (NLRA; also called the Wagner Act) is the basis of U.S. labor law. NLRA defines the legal powers of unions in the U.S., including guaranteeing the right of private-sector workers to organize into unions, engage in collective bargaining with firms, and take collective actions, such as strikes. Crucially, under NLRA, a union elected by a majority of workers can opt to become the exclusive representative of workers in collective bargaining with
firms, including of nonmembers or those opposed to the union. Furthermore, employers are compelled to bargain with such a union in good faith, with resulting labor agreements being legally binding on all related labor hired by the firm.

In 1947, the Labor Management Relations Act (the Taft-Hartley Act) amended NLRA. Among its changes was giving states the power to pass laws prohibiting union security agreements in employment contracts. These laws, commonly referred to as “right-to-work” (RTW) laws, are state-specific laws that generally prohibit agreements requiring employees to either join the union or pay agency fees—which, unions argue, recoup the costs of worker representation—to the union as a condition of employment. RTW laws quickly spread, especially throughout the American South and west of the Mississippi, where unions appear to have had relatively little political influence. By the end of 1963, RTW coverage had expanded to 19 states, a tally that has gradually increased to 27 today (Figure 1).

Figure 1

Right-to-Work States, Contiguous U.S.

Unsurprisingly, unions strongly oppose RTW laws, since a ban on unions requiring nonmembers to pay agency fees reduces revenues. However, the key question is whether RTW laws reduce unionization rates and union coverage rates; there is a fairly strong consensus that they do. David Ellwood and Glenn Fine find a sharp decline in new union formation after the passage of RTW laws. Further, Joe Davis and John Huston use individual-level data on a subset of private-sector workers and find evidence that RTW laws significantly reduce union membership rates. In a survey of the relevant academic literature, William Moore concludes that the accumulated evidence suggests that RTW laws have a significant negative effect on union organizing in the
short term and reduce long-run unionization rates by 5–8 percentage points. Recent work by Kevin J. Murphy focuses on midwestern states that enacted RTW laws in the aftermath of the 2008 financial crisis, and similarly finds subsequent reductions in union coverage and membership rates.

**Challenging the “Free-Rider” Claim**

Since RTW laws do not alter the legal ability of workers to form unions or restrict the legal rights of unions in collective bargaining with firms, this pattern requires careful explanation. The leading hypothesis for this pattern is that since workers at unionized firms can opt not to join (and thus not pay union dues), unions expect that unionized workplaces in RTW states will have fewer dues-paying members. This makes unionizing workplaces in RTW states less profitable. Anticipating this, unions will expend less effort into organizing workers, yielding lower unionization rates.

Unions decry RTW laws as enabling “free-riding” behavior by workers who, in the union’s view, obtain the benefits of unionization while opting not to pay dues. Under this view, requiring nonmembers to pay agency fees is nothing more than charging them for their “fair share” of the costs of representing workers and engaging in collective bargaining. However, this argument begs the question. Unions claim that by acting as the exclusive representative of all workers in bargaining, they benefit all the firm’s workers. However, in a world where most private-sector workers in the U.S. do not choose to unionize, such a presumption is highly questionable. It is instead entirely plausible that unionization is truly opposed by some of the firm’s employees, for financial or nonfinancial reasons. For example, many union contracts contain features like last-in, first-out layoff rules, which disadvantage recent employees, or highly compressed wage schemes, which disadvantage high-productivity employees relative to others. For some workers, those union policies may seem unfair or blind to the value of individual employees. In addition, workers may oppose unions on principle, or disagree with their political activities and stances. Describing such an individual in an RTW state who elects not to pay union dues as a free rider is highly dubious.

Further, unions often claim that RTW laws force them to provide representation to nonmembers for free. This is false. While unions can compel any employees to accept the wage that they negotiate, the reverse is not true: U.S. labor law does not require unions to offer the terms of a union contract to nonmembers. NLRA allows for “members-only” unions, in which unions bargain only for workers who elect to be covered by the agreement. However, in the U.S., unions almost invariably opt to act as “exclusive bargaining representatives,” in which the employer recognizes one union as the exclusive representative of workers. Usually, unions seek this status in order to bargain for higher wages, but U.S. labor law requires the union to treat nonmember workers the same (by forcing nonmember workers to accept the negotiated wage) if one union has exclusive bargaining status. If nonmembers “free ride” on the union, the only reason this can occur is that the union forces their participation in the first place by choosing to be the exclusive representative. In essence, unions are analogous to cartels that attempt to raise the price of a good (in this case, labor). Under RTW laws, unions retain the ability to compel workers to accept the wage that they negotiate, akin to being able to prevent undercutting of the cartel’s preferred price, but cannot charge nonmembers dues for the privilege of doing so.

**Theoretical Background: Unions as Monopolists**

Supposing that RTW laws reduce union coverage, what implications does this have for overall economic activity?
Wage Negotiation ... and Renegotiation

In many ways, there is little disagreement about the direct effects of unions. The fundamental justification for unions is to raise the wages of or otherwise benefit their members. Unions tout this justification. Firms and business lobbies, in complaining about increased labor costs, reduced economic competitiveness, and higher prices for customers, merely describe the flip side of the same coin. This is easiest to conceptualize by thinking of unions as bargaining for above-free-market wages; in practice, it can also include higher nonwage compensation (e.g., more expensive health insurance and pension schemes), reduced expectations of worker effort or hours of work for salaried workers, and greater protection from being laid off, each of which will tend to raise the labor costs faced by firms.

While it is difficult to cleanly estimate how much unions raise member wages, both empirical and theoretical evidence suggests that unions can be successful at doing this. Since unions usually act as the exclusive representative of workers in collective bargaining with firms, and firms are compelled by NLRA to bargain in good faith, unions have substantial leverage in affecting the level of wages and other compensation paid by the firm. The framework that economists use to model firm union bargaining is called the “monopoly union model.” This model makes the simplifying assumption that unions are able to choose the wage at which the firm can hire. In doing so, unions are constrained only by the firm’s labor demand curve, which shows how many workers the firm will employ at any given wage and contract agreement.

To produce a product or output, firms use a combination of worker labor and capital (physical assets such as machinery, equipment, and land). Since labor and capital are somewhat substitutable, as the union increases the wage it demands, economic theory posits that the firm will respond by shifting to more capital-intensive production and employing fewer, but not zero, workers. I.e.—and this is where the name “monopoly union model” comes from—unions face the same trade-off as any monopolist: they can choose at what price to sell their wares (here, worker labor), and the optimal price involves a trade-off between profit extracted per unit (the degree to which the unionized wage exceeds the free-market wage, the union wage premia) and the number of units “sold” (or, in this case, workers employed). Generally speaking, in industries where machinery and other capital is more important in producing a product than labor (and thus wages are a smaller component of a firm’s production costs), firms’ hiring decisions are less sensitive to the union bidding up wages. This raises the union wage premia in capital-intensive industries such as manufacturing and makes those firms more exposed to unionization.

Another potential direct effect of unions stems from their being able to raise wages. Consider a firm choosing whether to invest in technology that increases worker productivity, and thus output. In a standard situation, the firm will make the investment if the extra revenue it yields exceeds the cost of the new technology. However, when unions are able to extract profits from firms, the firm knows that the union can subsequently renegotiate for higher wages after the firm has already paid for the irreversible capital investment. The threat of such future behavior can create a holdup problem, in which firms will fail to make an otherwise profitable investment because the threat of wages being renegotiated renders the investment potentially unprofitable. In addition to reducing capital investment in locations with greater exposure to unionization, the threat of capital holdup may lead rational firms to relocate to areas with a lower risk of unionization.

Impact on the Labor Market

More seriously disputed is how unions—and thus RTW laws—affect the broader labor market and economy. Union advocates typically claim that RTW lowers wages in nonunionized firms because unions effectively pull all wages up. This is nice rhetoric; but upon careful inspection, it is difficult to rationalize.
Let’s say we take as given that unionized workers earn higher wages. This is intractably linked to the fact that not all workers can attain the limited number of unionized jobs (indeed, this is why union contracts tend to have detailed rules about who gets priority for the limited number of union jobs). In the same way that competition drives prices down when several firms are jockeying for customers, competition drives wages up when several firms are pushed to match or exceed other employment officers. If they fail to compete, the firm will lose customers, or workers, or both. Since the higher union wages are not freely available to outside workers, it is hard to understand why their existence would force nonunionized firms to offer higher wages. After all, nonunionized firms are typically competing for only a subset of workers—the nonunionized ones.

Indeed, economic theory suggests that the opposite is likely to be the case. Consider two otherwise identical locations that vary only in their RTW status. Since unionized firms are forced to pay higher wages, fewer workers will be hired in union-exposed industries in the non-RTW location where more of these firms will be unionized. Given a fixed population of individuals seeking employment, this leaves more workers to be absorbed by other industries in the non-RTW location. In a free market, this movement from one industry to another doesn’t happen by fiat. Since lower wages incentivize firms to hire more workers, economic logic suggests that equilibrium in the labor market requires lower nonunion wages in the non-RTW location.

Put differently, by raising wages, unions effectively displace some workers from firms that would otherwise employ them. Holding all else equal, wages have to fall in the rest of the local labor market to induce nonunion firms to employ these additional, displaced workers. This predicted pattern of results—one, higher wages for unionized workers; and two, nonunion workers having higher wages in RTW locations coupled with lower union density in RTW locations—makes whether RTW or non-RTW locations have higher average wages, all else constant, theoretically ambiguous. It also has potential implications for where individuals choose to live and work. The natural hypothesis is that nonunion wages being higher in RTW locations is likely to induce inward migration or commuting. As elaborated on in our results section below, we present evidence supporting this hypothesis.

Research Method

With these predictions of the model in mind, we turn to the data. The key challenge in identifying the effects of RTW laws is that they are not randomly assigned. On average, RTW and non-RTW states vary substantially when it comes to characteristics such as population demographics, education, economic and social history, climate, and natural resources. These differences could lead to substantial differences in economic outcomes.

Controlling for Differences: An Insufficient Method

One approach to studying the effects of policy across disparate geographies is to attempt to statistically control for each of the underlying differences between the locations, with any remaining difference in the outcome of interest then ostensibly attributable to the policy. However, unless the effect of every difference can be accounted for, properly isolating the effects of the RTW policy is extremely difficult.

For example, compare the annual salary of a 42-year-old college-educated white male working in finance in New York City with that of a 27-year-old Hispanic female in Arizona working in retail who has a high school diploma. Each of these differences (holding all else constant)—age, education, race, gender, and industry of employment—is associated with differences in wages. (For example, those with higher levels of education tend to earn more.) Furthermore, New York
City and Arizona have different economic policies, which may also affect wages differently. If we wished to isolate the effect of these policy differences on wages, we could attempt to adjust the wages of the two workers, controlling for all the differences in their demographic profile, and then estimate the residual difference. However, unless all relevant differences between the two workers are adjusted for, this will not generally correctly isolate the effects of other factors, such as policy differences.

In the scenario where we wish to isolate the wage effects of policy differences between Arizona and New York City, the residual difference in wages ascribed to economic policy will be tainted by a problem called “omitted variable bias.” That is, our estimate will be incorrect if there remain relevant factors—beyond age, race, gender, education, and industry—that have not been controlled for that are correlated with wages. Obvious examples of such an omitted variable include skills and intelligence, and how career-motivated an individual is. This is a difficult problem to avoid in studies where treatment is determined by geography, and people’s choices of where to live can depend on personal attributes that cannot be measured (e.g., skill, intelligence, and motivation levels), in complicated ways.

Consider a different comparison: a 42-year-old college-educated white male working in finance in New York City with a 42-year-old college-educated white male working in finance in Arizona. Is it reasonable to think of the two individuals as essentially identical with equal earning potential? Likely no. An individual who works in finance in New York City is likely different in a variety of ways from one who works in Arizona by virtue of the fact that he works in New York City, and it is nigh on impossible for a researcher to control these differences away. For example, the highest-prestige firms in finance are disproportionately located in New York City. Since these firms are highly selective in whom they hire, and people can move across the country in pursuit of jobs with high pay and status, a worker in finance in New York City, on average, will likely have higher intelligence and skills or be more career-driven than an otherwise identical worker in Arizona. The New York City worker will also earn more. But this difference in wages is at least partly due to the individual workers being different in hard-to-measure ways. It would be a mistake, then, to ascribe differences in economic outcomes purely to the difference in policy.

**An Intuitive Method: Border Counties Comparison**

To estimate the effects of RTW laws in this brief, we take a fundamentally different approach. We focus on a narrow geographical band where the RTW status suddenly changes. We consider adjacent pairs of counties in different states where one county has RTW laws and the other does not. Except for differences in policy, which change at state borders, it is reasonable to expect these RTW and non-RTW locations to be similar by virtue of their proximity and because the other factors affecting economic outcomes that we have mentioned (demographics, climate, cost of living, etc.) tend to have little variation across small distances. Accordingly, for each border pair, the non-RTW county acts as a natural control group for the RTW county. We found that these border-adjacent counties are highly similar across a wide range of observable characteristics, suggesting that they have similar unobservable characteristics as well. Additionally, the two sets of locations had very similar economic outcomes prior to the passage of RTW laws. The RTW border pairs (Figure 2) provide a set of hundreds of policy experiments for identifying the impact of RTW laws on a range of economic outcomes. The key assumption underlying this analysis is that, except for the policy discontinuity at the state border, these units are expected to have equal economic outcomes. As a result, any difference in observed economic outcomes can be attributed to the difference in policy.
Specifically, we construct a sample of all pairs of adjacent counties in the continental U.S. that had different RTW status as of 2010. This sample involves 380 pairs, comprising 373 counties overall. The 2010 RTW borders, which exclude the midwestern states that changed their RTW status after the 2008 financial crisis, yield a set of policy borders that had been very stable over time. For example, as of 2010, the average county pair in our sample had contained an RTW policy border for 57 years. Since the long-term impact of RTW is likely to take at least several years to manifest, and the vast majority of our data (which primarily cover 1990–2017) predate recent policy changes, this analysis of 2010 borders is more conducive to estimating the long-run effects of RTW laws. With the 380 border pairs selected, we then use regression analysis to determine the effect of RTW laws on various economic and social outcomes. See the Appendix for additional detail on the data and analysis.

Results and Discussion

To conduct this research, we first must verify that the demographics of RTW and non-RTW border counties are highly similar. Using county-level aggregates from the 1970–2010 Decennial Censuses and the American Community Survey (ACS), we found that differences in demographics, education, and marital status in our sample are nonexistent or very small in magnitude. This means that our selected border counties are ideal for this analysis. Our major findings are elaborated below; for more information and results of the regression analysis, see the Appendix to this brief. For even further discussion, see our original paper.
Manufacturing Employment Outcomes

In the context of RTW laws, manufacturing employment is especially ripe for study because manufacturing is particularly exposed to the threat of unionization and has historically had higher union coverage rates than most other industries. With this industry, we can test the theoretical prediction that RTW leads to higher employment in union-exposed industries.

We consider two outcomes. First, consistent with theory and the seminal finding of Thomas Holmes,\textsuperscript{25} there is a large increase in manufacturing activity when crossing from an “antibusiness” state to a “probusiness” state: we find a 3.23-percentage-point increase in the manufacturing share of employment on the RTW side of the border.\textsuperscript{26} This difference is substantial, equivalent to a 28% increase in manufacturing employment in RTW counties relative to their non-RTW neighbors.\textsuperscript{27}

Labor-Market Outcomes

Second, we want to know whether locations with RTW experience stronger labor-market outcomes than their non-RTW neighbors, or whether higher manufacturing employment simply crowds out other industries. For example, it could be the case that the difference in manufacturing employment is merely the result of local employers sorting themselves across the RTW border, based on how at risk of unionization they perceive themselves to be. Firms that perceive a higher risk of unionization would move to the RTW side of the border, increasing demand for workers in those industries. This demand could then simply crowd out firms with a low perceived risk of unionization, which would locate on the non-RTW side. Alternatively, the increased presence of manufacturing could have positive spillovers onto the broader local economy, yielding increased total employment.

We measure the impact of RTW on aggregate employment in two ways: first, by location of workplace; and second, by location of residence. Data from the Quarterly Census of Employment and Wages (QCEW) allow us to measure whether RTW regions are greater sources of employment; and data from the Local Area Unemployment Statistics (LAUS) provide insight on who benefits from those additional jobs.

We find that more jobs are located in RTW areas, with the employment-to-population ratio 3.51 percentage points higher than non-RTW counties, when measured by workplace location.\textsuperscript{28} When measured by residence location, the employment-to-population ratio is 1.58 percentage points higher in the RTW border counties. This suggests a substantial portion of the increase in job availability in RTW counties accrues to residents of the RTW border county. Our analysis confirms that the gap between the two estimates is explained by net commuting behavior—some of the additional jobs in RTW areas are obtained by residents of non-RTW areas who commute to obtain a better job.\textsuperscript{29}

Since some of the additional jobs in RTW counties flow to residents of the county, we undertake a closer examination of the effects of RTW on resident labor-market outcomes in order to gain insight regarding which individuals benefit. Intuitively, if RTW increases make it easier to find employment, we would expect this to have particularly strong effects for people on the margins of the formal job sector who have below-average employment prospects.

To investigate this, we supplement our county analysis with 2010 Census Public Use Microdata Areas (PUMAs; geographic areas that in rural areas are typically larger than counties), which are available in ACS. We analogously construct all the pairs of adjacent PUMAs that had different RTW status as of 2010 (Figure 3).\textsuperscript{30} In this test, we find that residents of RTW areas have higher employment and labor participation rates, and lower probability of unemployment, in both the county and PUMA samples.\textsuperscript{31}
We consider two specific at-risk population subgroups to test whether the effects of RTW on labor-market outcomes are magnified for people at high risk of nonemployment. First, people with disabilities often face barriers to holding formal employment. A substantial literature\textsuperscript{32} establishes that the national unemployment rate and Social Security Disability Insurance (SSDI) payouts are positively correlated. I.e., disability insurance is being treated as a substitute for unemployment benefits. If RTW counties yield stronger labor markets for their residents, we might also expect them to have notably fewer SSDI recipients. Our analysis confirmed this: we find that there is a 0.34-percentage-point lower rate of SSDI receipt in RTW counties.

Second, there has been a marked decline in employment for prime working-age males in recent decades.\textsuperscript{33} We find that long-term joblessness (nonemployment for 12 months or longer) for prime-age males is markedly lower in RTW locations. In fact, the estimated effects of RTW amount to a 19% reduction in long-term male joblessness.

**Labor Compensation Outcomes**

How does RTW affect wages and other measures of labor compensation? Contrary to claims by unions, we find that average weekly wages for workers in RTW counties are $27.97 higher, on average, using data from QCEW. When we adjust the estimates so that we are only comparing wages across locations while holding industry fixed (because different subindustries have different average wages and locations differ in their industry composition), this differential is reduced by approximately one-quarter.

Focusing on the disproportionately union-exposed manufacturing industry, we are unable to detect any meaningful difference in wages between RTW and non-RTW locations. The ACS microdata allow us to test whether these mean effects mask important differences across the wage distribution, using the sample of border PUMAs. Our analysis found that wages are higher at the bottom (10th
percentile) and middle (median) of the distribution in RTW areas, but we observed no clear difference between RTW and non-RTW at the top (90th percentile). This is broadly consistent with the prediction discussed above of lower wages being required in the nonunion sector in non-RTW locations in order to induce firms to absorb surplus labor displaced from unionized firms.

An important consideration is that differences in wages may be offset by nonwage compensation, like health insurance and retirement savings programs. While the generosity of such benefits is beyond our ability to measure, our analysis was unable to detect any meaningful effect of RTW laws on whether individuals possess health insurance. A second possibility is that differences in wages may be compensation for greater work effort. Indeed, some of the raw difference in wages in the PUMA sample can be explained by the estimated additional 0.49 hours worked per week by individuals employed in RTW locations.

Because nonwage compensation is difficult to measure, these patterns need to be interpreted with caution. But it is also important to note what we do not find. RTW opponents often employ cross-sectional analysis to claim that RTW substantially reduces wages and benefits like health insurance for nonunionized workers. In our border-pairs sample, where location characteristics (such as cost of living) and both unobservable and observable demographic characteristics are plausibly closely balanced, we detect no such pattern.

**Population Growth and Migration Outcomes**

The results discussed so far point to RTW counties having superior and more desirable labor-market outcomes. Better labor markets would presumably bring increased net migration to RTW states. One way to measure this is to analyze population growth. Using the Census Bureau's extensive time series of population data, we obtain difference-in-differences estimates of the effect of RTW on population, setting 1940 as the base year.

Prior to the passage of the Taft-Hartley Act in 1947, counties that subsequently became RTW had similar population growth as their non-RTW neighbors. But beginning almost immediately afterward, it appears that the population of RTW counties began to grow more rapidly than their non-RTW neighbors (Figure 4). Between 1940 and 2010, we estimate a 19.1% increase in population of RTW counties, relative to their non-RTW neighbors.
Turning to the question of whether these differences in economic outcomes bring about differences in social outcomes, we focus on two socioeconomic measures: poverty rates and intergenerational mobility. Of particular interest is whether the combination of these stronger labor markets and weaker union presence in RTW locations improves or hinders social well-being and opportunities for future generations.

We first consider the relative poverty rates between RTW and non-RTW border counties. It turns out that RTW counties have clear advantages. Perhaps unsurprisingly—given the improved probability of employment and slightly higher wage levels found in RTW counties—the overall poverty rate is 1.41 percentage points lower in RTW counties. Even larger differences are found for households with children. Childhood poverty rates are 2.29 percentage points lower in RTW counties, while the difference is slightly larger, at 2.43 percentage points, in families with children aged 5–17. On both measures, this amounts to approximately a 10% proportional decline in poverty—substantial improvements in outcomes for those at the bottom of the socioeconomic distribution.
The last outcome that we examine is economic mobility across generations. The bottom line is that RTW status improves the economic status of future generations. Using data from Opportunity Insights, a data project out of Harvard University, we look at how the rate of mobility of children into the top quintile of family income, as measured during adulthood in 2014–15, varies by the RTW status of the county in which they were born.\textsuperscript{39} Considering the raw share of children from each county who end up in the top quintile as adults, we find that it is 1.68 percentage points higher in RTW than in non-RTW border counties.\textsuperscript{40}

Given the previous results showing that RTW counties have higher employment rates and higher average wages, this difference may be driven at least in part by differences in the relative income of parents. To avoid this problem, we analyze the rate of mobility into the top income quintile for children who grew up in families at given positions in the national household income distribution. Importantly, we find that the positive effect of RTW is concentrated among children from lower parental-income levels. Children whose parents were in the 25th percentile experienced a 1.66-percentage-point increase in the probability of reaching the top income quintile. By contrast, the estimated effect on children born to parents at the 75th percentile is small and insignificant. In other words, the poorest children get the biggest benefit from RTW status when it comes to economic mobility.

Consistent with our previous results, it appears that RTW has beneficial effects for individuals at the bottom of the socioeconomic distribution but has little effect at the top. Whatever benefits unions may bring to their members, this pattern of results suggests that RTW particularly benefits individuals who may otherwise be priced out of the labor market when unions set above-market-clearing wages.

### Conclusion

Unions and their advocates typically decry right-to-work laws, arguing that they lead to worse wages and other outcomes for workers, while downplaying the potential beneficial effects of attracting businesses and encouraging them to make investments. Our research suggests that these criticisms are misplaced. Few people dispute that RTW laws are indeed bad for union coffers, because the laws allow workers to avoid paying for union representation that they do not want. But our findings suggest that, contrary to union claims, RTW laws benefit workers by creating stronger labor markets and yielding higher employment without reducing wages. Through these stronger labor markets, RTW laws play an important social role by reducing poverty rates and increasing upward socioeconomic mobility. Notably, our results consistently suggest that the individuals at the bottom of the socioeconomic distribution, who risk being priced out of the labor market when unions set above-market wages, benefit the most from RTW laws. Unions may benefit their members, but our results suggest that this comes at a heavy cost to many of the low-skilled workers they claim to champion.

In recent years, lawmakers in several non-RTW states, including Missouri, New Hampshire, and Ohio, have considered introducing RTW laws. Amid competing claims from union and business lobbyists, our findings shed light on the likely benefits that these states could, in time, bring to their economies—especially their manufacturing sectors—and their workers, by doing so. For the last 75 years, workers have been taking notice—commuting and moving with their feet. Politicians might consider taking notice, too.
Appendix

1. Regression Analysis

In this brief, the thought experiment underlying the border-pairs analysis is to isolate differences in the outcomes of interest between treated and control units, averaged across county pairs. To do so, we include a dummy variable for the unit’s RTW status, and add both unit-pair (e.g., county-pair) fixed effects $\gamma_{i,i'}$ and year fixed effects $\delta_t$. The year fixed effects remove the impact of national shocks affecting all locations, while the unit-pair fixed effects absorb differences between pairs. Our equation of interest is thus:

\[
y_{it} = \beta_{RTW} + \gamma_{i,i'} + \delta_t + \epsilon_{it}
\]

In this specification, the RTW coefficient $\beta_{RTW}$ reflects only the difference in outcomes within each pair, i.e., between each RTW county and its adjacent non-RTW-pair county, consistent with the ideal thought experiment. Notably, this regression is numerically identical to constructing the mean treatment effect $\bar{y}_t - \bar{y}_c$ for each pair $\{i, i'\}$, and taking a weighted average across pairs.\(^{41}\)

Some of the measures we study (specifically, population growth) have readily available data series that extend back to before the adoption of RTW laws. This allows for difference-in-differences regression analysis relying on a weaker assumption that outcomes in RTW and non-RTW areas should move in tandem (i.e., exhibit parallel trends) in the absence of policy effects.\(^{42}\) Put differently, if outcomes in RTW locations followed the same trend prior to the passage of RTW, and then diverged after its introduction, the magnitude and direction of this divergence are attributed as being caused by the RTW policy change. To implement this, we utilize the classic difference-in-differences estimator (i.e., with dummies for geographic units and time), and then pool it across county pairs. The resulting difference-in-differences equation of interest is thus:

\[
y_{it} = \beta_{RTW} + \gamma_{i,i'} + \delta_{i,i'} + \epsilon_{it}
\]

where $\gamma_{i,i'}$ are county pair by county fixed effects that capture baseline differences specific to each geographic unit, and $\delta_{i,i'}$ are county pair by year fixed effects that extract pair-specific time shocks. The key parameter is $\beta_{RTW}$, which traces out the average treatment effect (and for $t < t_0$, the pre-trends) of RTW over time. This regression is identical to constructing the treatment effect $(y_{tr} - y_{cr}) - (y_{tr} - y_{ct})$ for each pair year $\{i, i', t\}$ (the canonical difference-in-differences) and averaging across pairs for each year $t$.

2. Data

We use a wide variety of publicly available data sources in order to analyze the effects of RTW on many dimensions.\(^{43}\)
We supplement our county analysis with 2010 Census Public Use Microdata Areas (PUMAs), which are available in the American Community Survey (ACS), analogously constructing the set of all pairs of adjacent PUMAs that had different RTW status as of 2010. Since PUMAs are substantially larger geographic areas than counties, particularly in sparsely populated regions of the country, the “all else equal” assumption crucial to our analysis is somewhat less compelling. Accordingly, we focus primarily on the county-level data and use the PUMA data primarily for supporting analysis.

Labor-force data are obtained from the Local Area Unemployment Statistics (LAUS) for 1976–2017, provided by the Bureau of Labor Statistics (BLS). This is used to calculate county-level unemployment, employment-to-population (EPOP) ratios, and labor-force participation rates using the average annual data at the county level. Population-based ratios are obtained by combining data from LAUS with intercensal estimates of the age 15+ population from the Census Bureau.

To measure industry composition, particularly the manufacturing share of employment, we use the Quarterly Census of Employment and Wages (QCEW) for 1990–2017, provided by BLS. These data also include measures of average weekly wages, overall and by industry.

Historical decennial census data at the county level for 1880–2010 are obtained for a range of variables including county population and demographic composition, using data released by IPUMS National Historical Geographic Information System (NHGIS).

Data on disability enrollment for 2009–2017 are calculated using the number of Social Security Disability Insurance (SSDI) recipients reported by the Social Security Administration. Data on poverty rates for 1997–2017 are obtained from the Small Area Income and Poverty Estimates, and data on health-insurance coverage for 2008–17 are obtained from the Small Area Health Insurance Estimates, both of which are released by the Census Bureau.

Data on upward mobility, originally published by Raj Chetty et al. and Chetty and Nathaniel Hendren, are provided by Opportunity Insights, a data research organization based at Harvard University.

3. Results

Table 1

<table>
<thead>
<tr>
<th>(1) Manufacturing Share</th>
<th>(2) Manufacturing (p.c.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Right-to-Work</td>
<td>0.0323**</td>
</tr>
<tr>
<td></td>
<td>(0.0052)</td>
</tr>
<tr>
<td>County-Pair Fixed Effects</td>
<td>Yes</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>Yes</td>
</tr>
<tr>
<td>Control Mean</td>
<td>0.1139</td>
</tr>
<tr>
<td>County Observations</td>
<td>16,876</td>
</tr>
</tbody>
</table>

* p<0.05, ** p<0.01

Note: Standard errors clustered by county. Manufacturing employment is defined using NAICS codes 31–33.

### Table 2

**Employment Differentials at RTW Borders, Workplace vs. Residence**

<table>
<thead>
<tr>
<th></th>
<th>(1) Employment (Workplace)</th>
<th>(2) Employment (Residence)</th>
<th>(3) Commuting</th>
<th>(4) Commuting (Cross-state)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Right-to-Work</strong></td>
<td>0.0351**</td>
<td>0.0158**</td>
<td>0.0247</td>
<td>0.0242*</td>
</tr>
<tr>
<td></td>
<td>(0.0078)</td>
<td>(0.0042)</td>
<td>(0.0177)</td>
<td>(0.0101)</td>
</tr>
<tr>
<td><strong>County-Pair Fixed Effects</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Year Fixed Effects</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td><strong>Control Mean</strong></td>
<td>0.4276</td>
<td>0.5760</td>
<td>-0.0321</td>
<td>-0.0149</td>
</tr>
<tr>
<td><strong>County Observations</strong></td>
<td>21,280</td>
<td>21,280</td>
<td>760</td>
<td>760</td>
</tr>
</tbody>
</table>

*p<0.05, **p<0.01

Note: All columns measure outcomes normalized by population aged 15+ in the county. Standard errors clustered by county.


### Table 3

**Employment Differentials at RTW Borders, County vs. PUMA**

#### Panel A: County

<table>
<thead>
<tr>
<th></th>
<th>(1) Unemployment</th>
<th>(2) Employment to Population</th>
<th>(3) Participation</th>
<th>(4) Disability</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Right-to-Work</strong></td>
<td>-0.0038**</td>
<td>0.0158**</td>
<td>0.0141**</td>
<td>-0.0034**</td>
</tr>
<tr>
<td></td>
<td>(0.0011)</td>
<td>(0.0042)</td>
<td>(0.0042)</td>
<td>(0.0007)</td>
</tr>
<tr>
<td><strong>County-Pair Fixed Effects</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Year Fixed Effects</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Control Mean</strong></td>
<td>0.0661</td>
<td>0.5760</td>
<td>0.6123</td>
<td>0.0426</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>31,868</td>
<td>21,280</td>
<td>21,280</td>
<td>6,822</td>
</tr>
</tbody>
</table>

#### Panel B: PUMA

<table>
<thead>
<tr>
<th></th>
<th>(1) Unemployment</th>
<th>(2) Employment to Population</th>
<th>(3) Participation</th>
<th>(4) Long Term Jobless (Prime-Aged Male)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Right-to-Work</strong></td>
<td>-0.0111**</td>
<td>0.0254**</td>
<td>0.0200**</td>
<td>-0.0260**</td>
</tr>
<tr>
<td></td>
<td>(0.0023)</td>
<td>(0.0062)</td>
<td>(0.0058)</td>
<td>(0.0062)</td>
</tr>
<tr>
<td><strong>PUMA Pair Fixed Effects</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Year Fixed Effects</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Control Mean</strong></td>
<td>0.0718</td>
<td>0.5626</td>
<td>0.6045</td>
<td>0.1389</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>1,944</td>
<td>1,944</td>
<td>1,944</td>
<td>1,944</td>
</tr>
</tbody>
</table>

*p<0.05, **p<0.01

Note: Standard errors clustered by county.

Source: 1976–2017 LAUS, BLS; U.S. Census Bureau intercensal population estimates; 2009–17 Disability Data, Social Security Administration; 2012–17 American Community Survey (ACS), U.S. Census Bureau
Table 4

Compensation Differentials at RTW Borders, County vs. PUMA

Panel A: County

<table>
<thead>
<tr>
<th></th>
<th>Mean Weekly Wage</th>
<th>Health Insurance</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) All</td>
<td>(2) Composition Adjusted</td>
</tr>
<tr>
<td>Right-to-Work</td>
<td>27.92***</td>
<td>20.30**</td>
</tr>
<tr>
<td></td>
<td>(5.41)</td>
<td>(5.14)</td>
</tr>
<tr>
<td>County-Pair Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Industry x Year</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Fixed Effects</td>
<td>Control Mean</td>
<td>521.36</td>
</tr>
<tr>
<td></td>
<td>Observations</td>
<td>20.832</td>
</tr>
</tbody>
</table>

Panel B: PUMA

<table>
<thead>
<tr>
<th></th>
<th>Full-Time Annual Wage</th>
<th>Weekly Hours</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Median</td>
<td>(2) P10</td>
</tr>
<tr>
<td>Right-to-Work</td>
<td>1929.87***</td>
<td>761.32**</td>
</tr>
<tr>
<td></td>
<td>(752.86)</td>
<td>(229.34)</td>
</tr>
<tr>
<td>PUMA Pair Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Control Mean</td>
<td>40.695</td>
<td>18.264</td>
</tr>
<tr>
<td>Observations</td>
<td>1,944</td>
<td>1,944</td>
</tr>
</tbody>
</table>

*p<0.05, **p<0.01

Note: Standard errors clustered by county.

Panel A: Columns 4–5 report the share of the under-65 population without health-insurance coverage. Column 5 is restricted to those above 138% of the Federal Poverty Line, who are not eligible for Medicaid.

### Table 5

**Poverty Differentials at RTW Borders**

<table>
<thead>
<tr>
<th></th>
<th>(1) Poverty Rate</th>
<th>(2) Childhood Poverty</th>
<th>(3) Poverty, Ages 5–17</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Right-to-Work</strong></td>
<td>−0.0141**</td>
<td>−0.0229**</td>
<td>−0.0243**</td>
</tr>
<tr>
<td></td>
<td>(0.0027)</td>
<td>(0.0036)</td>
<td>(0.0034)</td>
</tr>
<tr>
<td><strong>County-Pair Fixed Effects</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Year Fixed Effects</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Control Mean</strong></td>
<td>0.1577</td>
<td>0.2210</td>
<td>0.2037</td>
</tr>
<tr>
<td><strong>County Observations</strong></td>
<td>15,960</td>
<td>15,960</td>
<td>15,960</td>
</tr>
</tbody>
</table>

*p<0.05, **p<0.01

Note: Standard errors clustered by county.

Source: 1997–2017 Small Area Income and Poverty Estimates (SAIPE), U.S. Census Bureau

### Table 6

**Average Mobility into Top Income Quartile at RTW Borders**

<table>
<thead>
<tr>
<th></th>
<th>(1) Unweighted Mean</th>
<th>(2) 25th Percentile (By Parental Income)</th>
<th>(3) 50th Percentile (By Parental Income)</th>
<th>(4) 75th Percentile (By Parental Income)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Right-to-Work</strong></td>
<td>0.0168**</td>
<td>0.0166**</td>
<td>0.0114**</td>
<td>0.0037</td>
</tr>
<tr>
<td></td>
<td>(0.0047)</td>
<td>(0.0036)</td>
<td>(0.0038)</td>
<td>(0.0057)</td>
</tr>
<tr>
<td><strong>County-Pair Fixed Effects</strong></td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Control Mean</strong></td>
<td>0.1961</td>
<td>0.1190</td>
<td>0.1878</td>
<td>0.2880</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>756</td>
<td>756</td>
<td>756</td>
<td>756</td>
</tr>
</tbody>
</table>

*p<0.05, **p<0.01

Note: Standard errors clustered by county.

Source: Opportunity Insights; Chetty et al. (2018); Chetty and Hendren (2018)
Endnotes


5 “1947 Taft-Hartley Substantive Provisions,” NLRB.


7 Unionization rates refer to the share of employed workers who are members of a union. Union coverage rates refer to the share of workers who are employed at unionized workplaces and are thus covered by collective bargaining agreements.


11 Kevin J. Murphy, “What Are the Consequences of Right-to-Work for Union Membership?” ILR Review 76, no. 2 (2023): 412–33.

12 Indeed, one of the most common criticisms of unions is that they make it difficult for firms to fire lazy or incompetent workers.


If a union contract does not cover all workers at a firm, the firm can attempt to undercut the union by hiring new workers without being bound by the terms of the union contract.

If this seems unrealistic because firms typically propose contracts that workers may then accept, note that a unionized workforce can reject any wage offer that differs from what they would unilaterally choose, and the firm is unable to hire workers until it attains agreement from the workers. If a critical mass of workers is united, the union can act as the monopoly supplier of labor to the firm.

Consider, e.g., a car company choosing whether to automate a component of a production line rather than use manual labor, or a fast-food company that can take orders either using human cashiers or provide automated kiosks where customers enter their own orders.

This is called a “downward-sloping labor-demand curve”—meaning, the lower the free-market wage, the more workers firms will choose to hire.

Further, the more willing people are to move for higher wages, the more that the burden of excess workers displaced from unionized firms is in equilibrium spread across both RTW and non-RTW locations, rather than only suppressing wages in non-RTW locations.

In some of the supporting analysis, we use data from the American Community Survey (ACS); Public Use Microdata Areas (PUMAs) are used as the unit of geography. “American Community Survey,” U.S. Census Bureau; “Public Use Microdata Areas,” Census Bureau.

An important caveat: several policies, not just RTW, can change at state borders. Thus, our method captures the effect of RTW plus other policies that change at RTW borders. See Austin and Lilley, “The Long-Run Effects of Right to Work Laws,” sec. 7, p. 56, for a discussion of the extent to which the differences in outcomes we observe appear to be attributable to RTW laws.

I.e., counties $i$ and $i'$ are adjacent counties in different states with different RTW policies and, in combination, constitute the county pair $i, i'$.

We include Indiana, Michigan, Wisconsin, West Virginia, and Kentucky as non-RTW states, based on their status in 2010. Only the last three states appear in our border-pairs sample, with Wisconsin the earliest to adopt RTW, in 2015.

See Austin and Lilley, “The Long-Run Effects of Right to Work Laws,” table 2, for details.


See Appendix, Table 1.

To ensure that this result is not an anomalous result caused by reduced total employment, a second specification finds a similarly sharp increase in manufacturing employment as a share of the 15-and-older population at the RTW border.

While this effect is large, it is much smaller than the proportional increase in manufacturing employment. Indeed, the additional manufacturing employment in RTW locations accounts for close to half the total employment effect, consistent with manufacturing being particularly exposed to unionization.

See Appendix, Table 2.
Ibid., Table 3.

The larger value in PUMAs may reflect the ACS sample period of 2012–17. Examining the time series, the largest unemployment differential between RTW and non-RTW counties tends to occur early in expansionary periods following recessions.


See, e.g., Gould and Kimball, “‘Right-to-Work’ States Still Have Lower Wages.”

This assumes that differences in population growth of adjacent counties are far more likely to be driven by migration differences than birthrates.

This difference-in-differences methodology calculates the difference between the cumulative growth of population in RTW and non-RTW locations, respectively, since 1940 (i.e., the decennial census prior to the passage of the Taft-Hartley Act). See Appendix, Equation 2, for more detail.

To aid precision, we exclude a small number of counties that have very low populations in any year, since they can have extremely high-variance population growth.

See Appendix, Table 5.

Income quantiles are measured using children born in the same year, rather than the entire adult population, in order to avoid mechanical age effects.

See Appendix, Table 6.

The pair weights are the number of years of data for the respective unit-pair.

Note that this is a weaker assumption than assuming outcomes in adjacent RTW and non-RTW regions would be equal on average except for policy effects.

For full details, see Benjamin Austin and Matthew Lilley, “The Long-Run Effects of Right to Work Laws,” working paper (Nov. 16, 2021), sec. 5 and appendix D.

“Public Use Microdata Areas,” U.S. Census Bureau.


To avoid capturing effects from the Medicaid expansion, we focus on coverage for people above 138% of the federal poverty level.