The Long-Run Effects of Right to Work Laws

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Abstract

In recent decades, states with Right-To-Work (RTW) laws have experienced higher employment and population growth than states without such laws. We investigate the extent to which these patterns, and other related labor market phenomena, are causally explained by these laws and closely related policies. Using border-pair differences, we find RTW laws are associated with a 3.2 percentage point increase in the manufacturing share of employment. This increase in manufacturing does not merely crowd out other economic activity; we find that people who live in RTW regions have 1.6 percentage points higher employment, 1.4 percentage points higher labor force participation, and 0.34 percentage points lower disability receipt than residents of similar non-RTW areas. However, wages and labor compensation do not appear to be lower on average. In turn, these differences appear to influence both individual residence and workplace location choice. Since their passage, locations with RTW laws have seen higher population growth, and on net attract commuters from non-RTW locations. These labor market effects also spill over into socioeconomic outcomes; RTW laws are also associated with lower childhood poverty rates and greater upward mobility. In particular, children at the 25th percentile of the parental income distribution during childhood have a 1.7 percentage point higher probability of reaching the top income quintile during adulthood if they grew up in a RTW location. These differences in outcomes were not present prior to the passage of RTW laws, persist after controlling for other major policy differences between states, and do not appear primarily attributable to local substitution. Together, this provides evidence that these patterns are substantially caused by RTW laws.

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1 Introduction

Under US labor law, unions can engage in collective bargaining with employers over the contractual terms that apply to the firm's workers. Unsurprisingly, unions almost universally desire that the resulting employment agreements include *union security clauses*, such as the requirement that workers join the union or pay fees to the union as a condition of employment. A Right-to-Work (RTW) law is a policy enacted by state governments prohibiting union security clauses. In practice, this means that workers at unionized firms cannot be required to pay fees to the union in order to retain their job.

While most existing RTW laws have been in place for several decades, there has been renewed political interest in Right-to-Work following the 2008 recession. Numerous claims are made about the impacts of RTW laws and unions by policy advocates on both sides of the political divide. Indeed, despite many historical studies, the impact of RTW laws on local economies remains poorly understood. RTW proponents such as business lobbies argue that RTW laws make locations more attractive for business investment and typically point to faster growth in income and total employment in RTW states over recent decades. For example, between 1978-2017, states that were RTW in 1977 had employment grow by 105 percent compared to 49 percent amongst non-RTW states.[1] Likewise, these RTW states saw their collective population grow by 90% over this period, compared to 35% growth in non-RTW states. Conversely, RTW opponents including unions and union-funded think tanks argue that RTW laws undermine the benefits unions produce for both union and non-union workers, and highlight that wages and non-wage compensation levels in RTW states are lower than in non-RTW states. Clearly, there are a wide range of factors potentially contributing to these differences. These include differences in climate, population demographics, and cost of living; historical differences in economic development and wage levels; and different exposure to technological change and changes in transportation costs. Accordingly, interpreting such statistics is fraught.

The aim of our paper is to address this issue and obtain careful causal estimates of the long-run impact of RTW laws, using modern econometric methods to focus on a narrow geographic sample where RTW and non-RTW areas are otherwise similar. In addition to revisiting the impact of RTW laws on the manufacturing share of employment, wages and other aggregate local labor market outcomes, we study effects on migration and residence location choice, and also leverage more

[1] Employment growth based on change in total currently employed individuals from the Current Population Survey Annual Social and Economic Supplement, 1976-2016. We consider the 48 states in the contiguous US and exclude states that changed their RTW status during this period from the sample.

recently available data to study the impact of RTW laws on downstream socioeconomic outcomes including poverty rates and intergenerational mobility.

The fundamental difficulty in identifying the effect of RTW laws is that such laws are not randomly assigned and the states with and without RTW laws differ in numerous ways. When studying how outcomes differ by RTW status across large geographies (such as states or the entire US), unless the effect of every such difference can be accounted for, properly isolating the residual difference in outcomes attributable to differences in policy is extremely difficult. To resolve this, we focus our attention on a narrow geographical band around RTW policy borders where the RTW status discontinuously changes. That is, we focus on the set of adjacent pairs of sub-state geographic units (primarily counties) where one unit has RTW laws and the other does not. Except for differences in policy, which change discontinuously at state borders, *a priori*, we can expect these RTW and non-RTW locations to be similar by virtue of their proximity and because other factors affecting economic outcomes such as demographics, climate, cost of living and exposure to technological change tend to vary smoothly across space. Accordingly, for each border pair, the non-RTW unit acts as a natural control group for the RTW unit. Indeed, as we will show, these border-adjacent non-RTW units are highly similar to their RTW neighbours across a wide range of observable demographic dimensions, which in turn suggests that they are likely to have highly similar unobservable characteristics as well. Additionally, the two sets of locations had very similar economic outcomes prior to the passage of RTW laws. The set of RTW-border pairs thus provides a set of policy experiments for identifying the causal effect of RTW laws. Our identifying assumption is that except for the policy discontinuity at the state border, we would expect these units to have equal economic outcomes. Any difference in observed economic outcomes can thus be attributed to the difference in policy.

An important complication of administrative-border based regression discontinuity strategies is that multiple policies, not just RTW laws, can change discontinuously at state borders. Thus, our method captures the combined effects of RTW and other policies that change at RTW borders. We return to this issue, to verify the extent to which the differences in outcomes we observe appear to be attributable to RTW laws, in depth later. Specifically, we utilise double machine learning techniques to assess the effect of RTW while allowing for outcomes to be affected by a flexible nonlinear function of a battery of other policy controls. We consistently find that the RTW-border differences we observe appear to be driven by RTW and not our other policy controls.

Formally, our analysis allows us to test the null hypothesis that RTW laws have no effect on union activity and resulting economic outcomes. The nature of RTW laws means that rather than merely identifying the effects of this particular policy, we can obtain insight into a broader question: the long run economic effect of laws that exogenously reduce (in equilibrium) union prevalence and power. The reason for this is simple. There are strong theoretical reasons (supported by previous empirical research) to expect RTW laws to reduce unionization rates. However, as we will explain in the following section, conditional on a union existing and a firm being unionized, RTW laws tend not to change the legal power of unions when bargaining with firms. Thus, as per Moore et al. (1986), any effects of RTW laws should operate primarily by exogenously reducing union coverage (and the threat of unionization that firms face), and secondarily by altering how unions bargain with firms conditional upon existing.[2]

Supposing RTW laws reduce unionization, the relevant question becomes how this differential union density produces broader impacts on labor market outcomes. To this end, we propose a simple spatial model where free-riding leads to reduced union coverage in RTW relative to non-RTW areas. Under US law, a union elected by a majority workplace vote has the right to become the exclusive representative of all eligible workers (including workers not in the union) in collective bargaining with the firm, which is compelled to bargain in good faith over wages and other conditions. This gives unions the ability to raise wages above the competitive equilibrium market-clearing wage. To capture this, we embed the canonical *monopoly union model* of wage setting, under which unions choose the wage level that maximises the extraction of rents from the firm, into a spatial framework, . In our model's equilibrium, wages in unionized sectors are set with a markup over the free-market wage (i.e. pertaining to non-unionized industries) in the same location. The wage markup from unionization is increasing in the capital share of income, such that industries where capital is more important in production are more likely to unionize.[3] Facing higher wages, unionized firms respond by hiring less labor. Market clearing then leads to lower wages in non-unionized industries to absorb the surplus job applicants. Individuals, who have idiosyncratic preferences over where to live and work observe wages (and the feasibility of obtaining a job) in each industry-location pair and thus choose their preferred workplace-residence pair. The model predicts that in equilibrium, by exogenously reducing union coverage, RTW areas will have greater employment than, and experience

[2]This latter channel should only operate to the extent that the union's bargaining strategy can influence the share of covered workers who opt to become members and pay dues.

[3]We do not explicitly test this, but it is well established that union density is relatively higher in capital-intensive industries such as manufacturing.

net inward migration from, non-RTW areas.

Since most RTW laws, and thus extant RTW borders, have been in place for multiple decades, our identification strategy allows us to use modern econometric methods to study the long-run economic impact of RTW laws. Why focus on long-run border differences? Theoretically, the most plausible effects of RTW laws operate through capital investment and firm location choice, and it may take many years for convergence to long-run equilibrium capital stock differences to occur. Thus, analysing long-run outcomes is contextually advisable, and is a task for which our border-pairs differences approach is well suited. By comparison, since most states that adopted RTW laws did so in the 1940s and 1950s, for most of the outcomes of interest we do not have granular data at sufficient frequency to undertake alternative empirical approaches such as difference-in-differences analysis. To be clear, the relative dearth of historical data is a limitation upon our analysis. If consistent historical data series were available, a difference-in-border-differences strategy would likely be preferable. In this spirit, where possible, we use historical data to show that there were no meaningful differences in outcomes between RTW and non-RTW border areas prior to the adoption of RTW laws. While there are a small number of states that have adopted RTW policies recently enough that a difference-in-differences or synthetic control approach is feasible, it would be less compelling than our long-run border discontinuity approach for three reasons. First, consistent with the argument above, studying effects from short-duration policy experiments may substantially underestimate the long-run causal impact of RTW passage. Second, many of these states (Michigan, Wisconsin, Indiana) adopted RTW laws in the aftermath of the 2008 Financial Crisis, and during a period in which their large automobile industries received a substantial bailout making a *ceteris paribus* comparison difficult. Finally, given that these RTW laws were an endogenous policy response from legislatures, we would be very concerned about violations of the parallel trends assumption prior to enactment of the laws.

Our headline border-pairs discontinuity estimates suggest that RTW laws have economically important impacts on labor market and socioeconomic outcomes. First, we estimate that the manufacturing share of employment is 3.23 percentage points (or approximately 28%) higher on the RTW side of the policy border. This is consistent with the theoretical prediction that RTW laws reduce production costs for firms in union-exposed industries, and confirms the result of Holmes (1998). Additionally, we find little evidence that this border effect has changed meaningfully over recent decades. We then turn to the question of whether this increase in manufacturing employment merely crowds out employment in other industries. We find that it does not. Specifically, in our sample, RTW counties have employment-to-population ratios (measured by location of workplace) 3.51 percentage points higher than their non-RTW neighbours on average. Some of this greater availability of nearby jobs accrues to residents of the RTW counties, who we estimate enjoy 1.58 percentage point higher employment-to-population ratios than their non-RTW neighbours. The remainder appears to be well explained by induced net interstate commuting into the RTW location, which we estimate amounts to 2.42 percent of the adult population.

Next, we turn to a detailed analysis of the labor market outcomes of residents of RTW locations. In addition to the aforementioned higher employment rates, we also estimate that RTW increases labor force participation by 1.41 percentage points while reducing unemployment by 0.39 percentage points. Particularly notably, we also estimate that RTW reduces Social Security Disability Insurance (SSDI) receipt by 0.34 percentage points, suggesting that RTW in particular improves employment prospects of people counterfactually on the margins of the labor force. We find supporting evidence for this thesis in microdata from the American Community Survey, where (in a more limited duration sample) we estimate that RTW reduces long-term joblessness for males by a particularly sharp 2.60 percentage points, a 19% relative decline. By contrast, we are unable to find any evidence that RTW laws reduce labor compensation, either on average or at the bottom of the distribution. In fact, our estimates suggest that, if anything, wages are slightly higher in RTW locations, especially at the lower end of the distribution. While this may seem surprising, our estimates largely align with the predictions of our model, where higher wages for unionised workers (in the non-RTW location) require more labor to be absorbed by non-unionised industries in the same location, which in turn drives down wages in the latter. Thus theory is ambiguous about the effect of RTW on aggregate wages. However, we suggest caution in interpreting these labor compensation results, because unions may plausibly have particularly large effects on non-wage compensation. We estimate that employed residents in RTW areas work 0.49 additional (self-reported) hours per week, but do not detect meaningful differences in health insurance coverage rates. However, we cannot rule out differences in health insurance or employer-provided retirement income on the extensive margin, which is harder to measure and potentially more empirically important.

We then turn to how RTW affects where people choose to live. People are unlikely to explicitly consider a location's RTW status when choosing between locations, and thus any effects on migration presumably occur as a downstream response to the effects of RTW on labor market outcomes. To best measure how RTW affects location choice, we study both data on migration and population, since differences in birth-rates are unlikely to be first-order. From the migration flows data, we estimate that RTW locations have both higher inward and outward migration rates. Data from decennial censuses allows us to estimate difference-in-difference effects on population growth over a long time-horizon. Between 1940 and 2010, we estimate that RTW caused a 19.1 percent increase in population of RTW counties relative to their non-RTW neighbours.

Finally, we analyze whether RTW laws have downstream economic effects, particularly pertaining to exposure during childhood. Studying poverty rates, we estimate that RTW reduces the share of individuals below the federal poverty line by 1.41 percentage points, with an even larger 2.29 percent reduction in childhood poverty. These estimates are consistent with our earlier results that RTW laws increase employment and also seemingly wages at the bottom of the distribution. We then turn to data on upward mobility, recently made available from Chetty et al. (2014) and Chetty and Hendren (2018). We estimate that children at the 25th percentile of the parental income distribution during childhood have a 1.66 percentage point higher probability of reaching the top income quintile during adulthood if they grew up in a RTW location, but do not observe a significant difference for those raised at the 75th percentile. This again is consistent with the thesis that RTW laws are particularly beneficial for those at the margins of the labor force who are most exposed to disemployment effects that may occur when unions set wages above the market clearing rate.

Having established that economic outcomes change in important ways when crossing RTW borders, and that such differences do not appear to have existed prior to RTW being in place, we then demonstrate that our estimates are little changed when flexibly controlling for differences in other state economic policies. Together, this evidence suggests that RTW laws have important causal effects on labor market and socioeconomic outcomes. Since we leverage spatial discontinuities in policy, these estimates are of the local causal impact of RTW laws near the border. The advantage of our identification strategy is that we can be more confident in our ability to identify the true causal impact of RTW laws at a local level. However, it also has several limitations. Strictly speaking, our border discontinuity strategy is potentially uninformative about the effects of RTW away from state borders. To bolster our evidence, we compare nearby non-policy county-pairs to show that the effects we observe are neither simply due to nor appear to be driven by local substitution, and instead persist at least initially as we move away from state borders. Nonetheless, our identification strategy gives us at best modest evidence on the impact of RTW laws far away from the policy borders. Additionally, we are not able to identify the general equilibrium effects of a hypothetical national policy change to RTW laws.

The paper proceeds as follows. In Section 2 we provide some background context on US labor law and the origins of RTW laws, and also discuss prior literature on both unions in general and RTW laws specifically. In Section 3, we introduce a simple model that embeds the structure of the monopoly union model into a spatial framework with two locations that differ in their RTW status. This yields a range of testable hypotheses regarding the effects of RTW laws that map directly to our empirical setting. In Section 4 we discuss our identification strategy and empirical methods in detail. Section 5 describes our data sources. We then present the results from the border discontinuity analysis in Section 6. Section 7 then examines whether these differences are due to differences in RTW laws or other state policies, and finally Section 8 examines whether the differences are due to local substitution or persist away from state borders.

2 Unions - Institutional Context and Prior Literature

The National Labor Relations Act of 1935 (Wagner Act), passed during the Great Depression, is the basis of US labor law. The NLRA defines the legal powers of unions in the United States, including guaranteeing the right of private sector workers to organise into unions, engage in collective bargaining with firms, and take collective actions such as strikes. Notably, Section 9 of the NLRA allows a union elected by a majority of workers to opt to become the *exclusive representative* of workers in collective bargaining with firms, including of non-members and/or those opposed to the union. Furthermore, Section 7 gives this legal teeth by compelling employers to bargain with a chosen exclusive representative of workers in good faith. This collective bargaining process yields labor agreements that are legally binding on all (related) labor hired by the firm. These early agreements were permitted to contain a variety of union security clauses including closed shop agreements, whereby the employer would agree that only union members would be hired, and union shop agreements, where the employer was free to hire anyone but workers were subsequently required to join the union.[4] In 1947, the Labor Management Relations Act (Taft-Hartley Act) amended the National Labor Relations Act. This amendment made a number of national changes including banning closed shops and various forms of unfair labor practices, such as secondary boycotts

[4] A related form of agreement is an agency shop agreement, where all employees are required to either join the union or pay agency fees to the union.

and wildcat strikes. In addition, it gave states the power to pass laws prohibiting union security agreements in employment contracts. While some states had then recently passed such laws, this legislation guaranteed their legality. These laws, commonly referred to as 'Right-to-Work' (RTW) laws, are state-specific laws that generally prohibit agreements that require employees to either join the union or pay dues or *agency fees* to the union as a condition of employment. Since 1985, when the US Supreme Court outlawed union shops in its *Pattern Makers v. NLRB* decision, the practical effect of RTW laws has been to prevent unions charging non-members agency fees, which unions argue recoup the costs of worker representation, as a condition of employment.

By the end of 1949, 12 states had passed RTW laws, and by the end of 1963 this had increased to 19 states, with little further change as of 2010 by which point 22 states were RTW. The initial uptake of RTW laws was notably non-random, with adopting states primarily located in the South and West of the country. While a role for cultural and political differences cannot be ruled out, economic differences prior to the Taft-Hartley Act appear to have been the primary determinant of whether states subsequently adopted RTW laws. A natural hypothesis is that presence of unions prior to Taft-Hartley determined whether states were able to adopt RTW laws, or were dissuaded from doing so due to union pressure. Although data on unionization rates in the 1940s is limited, union presence can be proxied by measures of union exposure, such as the share of employment in industries that are now heavily unionized. Ranking states by their 1940 share of employment in manufacturing and coal mining, each percentage point higher employment share in these union-exposed industries yields an 18% reduction in the odds-ratio of adopting a RTW law by 2010. Particularly notably, none of the 15 most union-exposed states under this measure had a RTW law by 2010, while all of the 8 least union-exposed states had adopted RTW. This strongly suggests that states with limited union presence prior to Taft-Hartley were able to adopt RTW laws, while those which had substantial contemporaneous union presence did not.[5] An important qualifier is that while RTW policy adoption was clearly endogenous to pre-existing state industry composition (and presumably union density), we do not observe any meaningful pre-existing differences in economic measures of interest once restricting attention to our chosen border-pairs sample. In other words, within this sample, it is as if counties (or other geographic units) were randomly assigned a RTW status.

More recently, the fallout from the Great Recession led to renewed interest in RTW laws as an

[5] Note that this pattern is hard to explain if not due to union resistance to RTW laws. Firms and business groups in states with a higher share of union-exposed industries, for example, would have been expected to lobby more, not less, aggressively for the adoption of RTW laws.

ostensible means to bolster economic competitiveness, culminating in rapid adoption of RTW laws throughout the Midwest. An additional 5 states have enacted RTW laws since 2010, namely Indiana (2012), Michigan (2013), Wisconsin (2015), West Virginia (2016), and Kentucky (2017). Separately, in 2018 the US Supreme Court ruled in *Janus v. AFSCME* that mandated agency fees in the public sector violate the First Amendment, effectively making the entire public sector RTW.

Previous literature has explored three hypotheses about the impact of RTW laws, as summarised by Moore et al. (1986). The first is referred to by Moore as the "taste-based hypothesis" which states that RTW laws themselves merely reflect the preexisting preferences of voters. Legislatures pass RTW laws in response to these preferences, yet the passage of the law itself does not have a direct causal impact. The second hypothesis commonly discussed is referred to as the "free-rider hypothesis". This states that RTW laws reduce unionization levels because workers can obtain the benefits of unionization while opting not to pay union dues, potentially leaving unionized workplaces with few dues-paying members. Endogenously, this leads to unions rationally putting less effort into organising workers, yielding lower unionization in turn. Finally the "bargaining-power hypothesis" postulates that RTW laws directly decrease a union's bargaining power with firms. Unionization rates then decrease in response to the decreased ability of unions to secure additional benefits for members.

For our purposes, the key distinction is whether RTW laws have a causal impact on unionization rates or not. If RTW laws merely reflect preexisting preferences of voters, observed differences between RTW and non-RTW locations would reflect the impact of differences in other policies, geography, and population characteristics. Alternatively if RTW laws do have a causal impact on unionization rates, this impact could then result in differences in economic outcomes between regions.

There is a substantial previous literature examining the impact of RTW laws on unionization rates. The outcomes of these studies are mixed depending on the methodology employed including whether they consider the stock of union members versus the flow into unions, the private or public sector unionization rate, state specific or national trends, and whether they try to model unionization and RTW laws as jointly endogenous outcomes. While Farber (1984) finds evidence consistent with RTW laws simply reflecting preexisting tastes rather than having a causal impact, there are a substantial number of studies that find an impact from RTW on unionization. Ellwood and Fine (1987) study the flow into unions after the passage of RTW laws, and find the passage has a sizeable initial impact on union organizing. Further, Davis and Huston (1995) uses microdata on a subset of private sector workers and finds evidence that RTW laws significantly reduce union density. Surveying the literature, Moore (1998) 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 to 8 percent. More recently using a synthetic control methodology Eren and Ozbeklik (2016) finds evidence that the passage of a RTW law in Oklahoma was associated with a decrease in private sector unionization. Summarizing the literature, it is clear there is substantial evidence that RTW laws do reduce unionization.

In theory, the effects of RTW occur by reducing union density, and thus the influence of unions on economic activity, in RTW areas. This makes many of the active literatures regarding the effects of unions pertinent to understanding the effects of RTW. Most importantly, a large body of literature has generally found that unions directly impact wages and firm profitability. Card (1996) finds unions raise wages, especially for workers with lower levels of observable skills. Lemieux (1998) finds evidence that unions increase the average wage of workers while compressing returns to skill. Lee and Mas (2012) find evidence that union election wins reduce equity value of firms by $\$40,500$ per worker, suggesting unions either reduce firm efficiency or are able to extract rents.

If unions are able to extract rents from the firm, then it is possible that such behavior will create a holdup problem where firms will reduce their capital investment due to the knowledge that workers can renegotiate wages after irreversible capital investments are made. Grout (1984) provides the canonical theoretical model using generalized Nash bargaining, and shows that if unions have any bargaining power then capital investment is decreased relative to the efficient binding contracts case. In addition to reducing capital investment in locations with greater risk of unionization on the intensive margin, the threat of capital hold-up may lead rational firms to make extensive margin adjustments such locating in areas with a lower risk of unionization. This would be particularly true for producers of easily tradable goods, such as durable manufacturing. There is a broad empirical literature examining whether holdup occurs in practice with varying findings. Connolly et al. (1986) examines a firm's investment in R&D, and finds unionization reduces returns to R&D and correspondingly the level of investment by the firm. Similarly Cardullo et al. (2015) finds evidence that union power is associated with lower investment and labor productivity, and that these effects are more pronounced in industries with large irreversible capital investments. However, using matched Social Security earnings and detailed firm data from Italy, Card et al. (2014) finds evidence of rent sharing between firms and workers after the full cost of capital is deducted, consistent with no capital investment holdup. Reviewing the evidence it is clear that unionization leads to rent sharing between firm and workers, implying higher wages and lower firm values, although the evidence for investment holdup is less conclusive. By letting unions set wages in our model, we essentially embed the result, consistent with this literature, that unions raise wages.[6]

Second, spillovers from unions onto other non-union firms and their workers is central to determining the aggregate effects of RTW laws. For example, in our model, a location's RTW status has an ambiguous effect on its aggregate wages when both unionized and non-unionized firms are present because of negative spillovers of union wage-setting on wages in other industries.[7] Possible employment effects depend on whether there is increased firm investment due to RTW laws, and whether attracting union-exposed industries causes crowd-out or crowd-in of other industries. In turn, differences in job opportunities and wages (and non-wage amenities such as job security) could influence migration decisions and population growth.

A number of studies have attempted to measure many of these effects, but there is no consensus in the literature. Much of this literature was written in the 1970s and 1980s, and predates "credible" identification strategies prevalent in modern applied microeconomic research. The most compelling study is Holmes (1998) who studies variation of manufacturing employment across state policy borders. He finds an approximately one-third increase in the share of manufacturing employment in a RTW county relative to non-RTW counties. Using the 2000 Census, Kalenkoski and Lacombe (2006) finds a smaller but still significant impact on manufacturing employment when controlling for geographically correlated errors. Kunce (2006) finds some evidence for higher employment in RTW states in the paper industry using panel data. The effects of RTW on aggregate wages in the literature are particularly mixed. Reed (2003) finds evidence that RTW states have higher wages after controlling for state economic conditions at the time of passage using a cross-sectional regression. In contrast, Farber (2005) finds mixed evidence on the impact of the passage of RTW on non-union wages, with some evidence of decreasing wages using a state level difference-in-difference in Idaho

[6]Additionally, our choice of production function turns out to preclude a role for capital hold-up. This is not essential to our analysis; introducing a specific role for capital hold-up in our model would largely reinforce its predictions. Of course, to the extent that capital hold-up exists, the effects it produces should be captured in our empirical analysis.

[7]This is very similar to the result found by Lazear (1983), which presents a canonical model where higher wages reduce employment in the unionized industry. Market-clearing then requires lower wages in the non-union industry to absorb the surplus job applicants. At the same time, a higher union wage also means more firms avoid unionization implying there are more firms that employ individuals in the non-union industry. The model has the non-union wage always lower than the competitive equilibrium, and asymptotes to the competitive wage when the union wage is so high that all workers and firms are in the non-union industry.

in 1985. Eren and Ozbeklik (2016) does not find an impact on employment-to-population ratios or wages in Oklahoma following the passage of a RTW law.

More recent work has extended the study of the impact of RTW laws in new directions beyond labor market outcomes. Freeman et al. (2015) examines the impact of union density on intergenerational mobility. They find evidence in a cross-sectional regression that regions with higher union density have higher levels of upward mobility, defined as average income of children relative to their parents. In a similar manner, Hu and Hanink (2018) find that unionization is associated with reduced income inequality in cross-sectional regressions. However, Hanley (2010) finds the opposite effect with RTW laws being associated with reduced income inequality. Makridis (2018) studies the introduction of RTW laws after 2008 and finds they are associated with improved worker life satisfaction using Gallop survey data. Finally Feigenbaum et al. (2017) argues that RTW laws are associated with decreased Democratic party vote share, using a border discontinuity design.

We add directly to these literatures. In addition to confirming the findings of Holmes (1998) regarding the effect of RTW laws on manufacturing employment, we also find clear evidence of beneficial effects on aggregate labor market outcomes including higher total employment, and lower unemployment and long-term joblessness. Notably, our border-pairs discontinuity method is able to shed light on the effect of RTW on aggregate wages, failing to find any evidence of a negative impact in aggregate. We also extend on the existing literature by showing how these differences in labor market outcomes affect individual decision-making regarding where to live and work, as measured in commuting, migration and population data. Finally, while previous analysis of the relationship between union density and intergenerational mobility such as in Freeman et al. (2015) relies on cross-sectional correlations, our border-pairs discontinuity method enables us to provide causal evidence that RTW raises upward mobility.

3 A Model of Unionization and RTW

Before commencing on our empirical analysis, the natural starting point is to seek to understand *why* Right-to-Work laws may affect economic activity. Recall from above that under US law, it is not apparent why RTW laws should have meaningful economic impacts once conditioning on union density, since they typically do not restrict the legal rights of unions in collective baragaining with firms. Accordingly, we proceed in three steps.

First, we take the unionization decision as given, and construct a simple model of union wage setting. Here, we closely follow the canonical *monopoly union model* (see Farber (1986) for an overview, the idea extends back at least to Dunlop (1944)), in particular utilizing the same workhorse assumption that unions are able to choose the wage the firm can hire at and do so cognisant of the firm's labor demand curve. This yields equilibrium wages as a function of unionization status. Second, allowing for unionization to be costly, we analyze which firms will unionize in equilibrium, and how this is affected by RTW status. Two important implications arise. First, firms where capital is more important in production are more exposed to unionization. Additionally, the ability for individuals to choose to not pay union dues, provided by RTW laws, makes organizing firms less attractive to unions, thus endogenously leading to lower rates of union coverage.[8]

Finally, we then embed this wage-setting equilibrium into a spatial model where regions exogenously differ in their RTW status. Since RTW reduces union density, counterfactually union-exposed industries face lower wages and thus produce higher output and employ more labor in the RTW location. This has positive spillover effects on the wages in non-union exposed industries, by (holding all else equal) reducing the amount of labor they must absorb. Individuals, who have preferences not only over consumption but also where to live and work, are induced to live and work disproportionately in the RTW location.[9] This in turn implies RTW induces net inward migration and net inward cross-border commuting.

3.1 A Model of Union Wage Setting

Consider a firm j that is unionized. The union, U_j, by virtue of its legal role as *exclusive representative* of the firm's workers, controls the wage for all labor that the firm is able to hire. The firm chooses the level of labor hired according to $L_j = d(w_j)$. In particular, a unionized firm has no ability to hire non-union labor at the competitive market price w_n (courtesy of legal fiat).

Let firms be atomless and price takers in product and factor markets. The firm produces output of $g_j = F(K_j, L_j)$ with output of good g having equilibrium price p_g. For analytical tractability, we assume the firm has Cobb-Douglas production, such that $F(K, L) = K^{\alpha} L^{1-\alpha}$. Product markets are competitive with free entry and exit and firms accordingly earn zero profits in equilibrium.

[8] In the simple model we present, where all non-financial motives from unionization are abstracted away from, this ability to free ride will in fact lead to zero union density in RTW locations.

[9] Relative to either the equilibrium where both locations have RTW laws, or the equilibrium where neither do.

Timing

We utilize the structure of the canonical *monopoly union model*. The timing of decisions is as follows.

- At $t = 0$, firm j chooses non-reversible capital investment, K_j
- At $t = 1$, the union covering j, U_j, chooses the wage $w_{uj}(K_j)$.
- At $t = 2$, the firm chooses $L_j(w_{uj}, K_j)$ to maximise profit, given its initial investment K_j and the wage w_{uj} chosen by the union. Output is produced, and the product market clears.

This timing structure and objective functions of all parties are public knowledge. Accordingly, unions choose the unionized wage w_u knowing how much labor the firm will then hire, and firms choose their initial capital investment taking into account how both w_u and L_j will in turn be affected.

Union Objective

The firm objective is simple, to maximise profits. In principle, the union objective may be any function that is increasing in wages and the quantity of labor hired by the firm. For simplicity, we assume the union objective is to maximise the extraction of rent from the firm, defined as the sum of wages above the free-market wage paid to labor. Since it engages only in *enterprise bargaining*, the union pertains to a single firm j, and thus cares about only the workers at firm j.[10] The union understands that higher w_j corresponds to fewer unionized workers employed by the firm, and thus the set of workers it cares about is defined ex post.[11] Specifically, the union observes the non-union wage w_n and sets w_{uj} to maximise *rent* R_j

$$\max_{w_{uj}} R_j = (w_{uj} - w_n).L_j(w_{uj}, K_j) \tag{1}$$

This is *enterprise bargaining* or *localised bargaining*. Locals do not take into account the effect of their wage setting on other unionized firms in the same industry. If locals were able to co-ordinate, they could instead solve

$$\max_{\{w_{uj}\}_{j=1,\ldots,m}} R = \sum_{i=1}^{m} (w_{ui} - w_n).L_i(w_{ui}, K_j)$$

[10] Since firms are atomless, spillover effects on workers in other firms in the same industry are second order. In the aggregate these can still have effects, but under enterprise bargaining, the union only cares about its own workers and thus ignores any such aggregate effects. Under *sectoral bargaining*, where a single union represents workers across firms in an industry, these aggregate effects are internalised. In Appendix B.2, we outline how this can yield different wages and industry employment.

[11] This ex ante versus ex post distinction is unimportant if the firm treats any workers who in equilibrium are not hired by the firm as if they earn the free market wage, even though in equilibrium they may either work at another unionized firm, or in a richer model, in another location.

Proposition 1. *Suppose the union covering j, U_j, sets the wage $w_{uj}(K_j)$ firm j must pay, after observing the firm's capital investment decision. Then, to maximise extraction of rent, wages at unionized firms will be set as a markup over the non-union wage w_n, where the markup is increasing in the capital share of income;* $w_{uj} = \dfrac{w_n}{1-\alpha}$.

Proof. See Appendix A.1 □

The union wage is increasing in α. This fits with intuition. Unions set wages once capital is sunk, and because capital is sunk, firms will still employ labor even at very elevated wages. The more important capital is in production, ceteris paribus the higher the marginal product of labor for any given L (and K). The union can accordingly extract more rents. Since the unionized wage is invariant in K_j and the firm has constant-returns-to-scale production, the size of each firm is not tied down. Equilibrium merely requires industry size is such that p_g causes firms to earn zero profits.

3.2 Union Formation and Right to Work

To this point, we have discussed the equilibrium that arises when a firm is unionized. It remains to discuss the conditions under which the workers of a firm will unionize. Suppose unionization comes at a cost c, which captures both union membership dues (and agency fees) but also non-monetary costs (or benefits) from unionization such as worker-union-firm personal frictions and strike costs. Workers in a unionized firm earn w_u, while those in non-unionized firms earn w_n.

Proposition 2. *Suppose unionization is costly, with cost $c > 0$. Then there exists some $\alpha^* \in (0,1)$ such that firms will be unionized if and only if $\alpha \geq \alpha^*$ and the firm operates in a non-RTW location.*

Proof. See Appendix A.2. □

Intuitively, unions will only form where the union wage premium is sufficiently large to compensate for the costs of unionization. Since the wage premia is increasing in α, so is union exposure.

However, in RTW states, non-members of unions do not have to pay unions any dues or agency fees. Since workers can obtain any benefits of unionization without paying dues, paying dues is not rational, even for workers that prefer the union to operate.[12] Thus in equilibrium, unions do not

[12] This abstracts away from any non-private non-pecuniary motives for being a union member. Of course in practice financial incentives are not the sole reason to join unions, and thus unions still exist in RTW states, but at lower frequencies. Workers may also be motivated to maintain (or avoid) union membership for ideological reasons, or because quitting membership is socially costly. Indeed, unions rely on a variety of forms of social pressure to dissuade non-membership. While we abstract away from such motives for simplicity, the result that RTW reduces union coverage and the economic effects this produces require only that some workers would elect not to pay dues if given the opportunity. Foreseeing this, unions will rationally expend less effort to organize workplaces in RTW locations, since doing so is less profitable.

operate in RTW locations.

It is common to refer to this as a *free-riding problem*. While this term, commonly used by union proponents, is somewhat loaded, we follow it by convention.[13]

3.3 A Spatial Model of Right to Work

We now embed this union wage-setting model into a spatial model where regions are exogenously designated to either be RTW or non-RTW, and individuals choose where to live and work. This allows us to map out the theoretical implications of RTW laws for economic activity. The spatial environment that we consider closely maps to our border-pairs empirical setting. In particular, the equilibrium that we focus on captures (border) differences between RTW and non-RTW locations in a world where policy differs across locations. This is not the same as the difference in outcomes between a world where all location are RTW, and a second world where all locations are non-RTW. However, we briefly discuss the model's implications for these counterfactual worlds in Appendix C.2.

We begin by considering a spatial environment where individuals have preferences over both their location of residence and employment. This follows closely from existing models in the trade, economic geography and tranportation literature including Allen and Arkolakis (2014, 2019); Heblich et al. (2020); Ahlfeldt et al. (2015); and Eaton and Kortum (2002).

Specifically, consider an environment with a unit mass of individuals split across N locations. For simplicity, we consider $N = 2$ and label the locations $\mathcal{A}$ and $\mathcal{B}$, and the set of locations $\mathbb{L}$. Denote by L_{ij} the number (and share) of people who live in i and work in j. From this, we can additionally define R_i as the resident population of i and E_j to be the number of people who work in j, with

$$R_i = \sum_{j \in \mathbb{L}} L_{ij} \tag{2}$$

$$E_j = \sum_{i \in \mathbb{L}} L_{ij} \tag{3}$$

[13] While unions can compel any employee to accept the wage they negotiate, the reverse is not true: US labor law does not require unions to offer the unionized wage w_u to non-members. Members-only unions, where unions only bargain for workers who elect to be covered, are legal in the United States. However, unions almost invariably opt for *exclusive representation*, in order to obtain higher wages. Thus, RTW proponents commonly argue that if non-members free-ride on the union, they only do so because the union forces their participation in the first place. Within the confines of the most basic model, where workers have identical outside options, this distinction is not very meaningful. There, in a unionized workplace, all workers prefer unionization, so non-payment of dues is tantamount to free riding. However, in a richer environment, with non-pecuniary motives for unionization and/or heterogeneous effects of unionization (e.g. by flattening the wage distribution, unions may make some workers worse off while benefiting others), it is entirely plausible that unionization is opposed by some of the firm's employees. In such circumstances, use of the term *free riding* is questionable.

Locations are assumed to have specific exogenous capital and land endowments ϕ_j that allows them to produce (multiple) location-unique varieties, v. In particular, some firms in each location can make a capital-intensive variety kj while others produce a labor-intensive variety lj. L_{vij} is the number of people who are employed making variety vj in location j and live in location i, and $E_{vj} = \sum_{i \in \mathbb{L}} L_{vij}$ is the number of people employed in industry v in location j.

Without loss of generality (since locations are otherwise symmetric), location $\mathcal{A}$ is RTW and location $\mathcal{B}$ is non-RTW.

Firms

Atomistic firms in each location-industry pair act to maximise profit. Firms take factor and product market prices as given, and there is free entry and exit such that firms earn zero economic profit in equilibrium. The production functions (of the representative firms[14]) for the respective varieties are given by $f_j(\cdot)$ for the capital-intensive variety and $g_j(\cdot)$ for the labor-intensive variety, with

$$f_j(K, L) = K^{\alpha} L^{1-\alpha}$$
$$g_j(L) = L$$

Each variety is sold on a world market. Since varieties are differentiated, each faces downward sloping demand. We assume that the product demand for each variety has price elasticity $\sigma > 1$, with

$$p_{vj}(Q_{vj}) = Q_{vj}^{-\frac{1}{\sigma}} \lambda^{\frac{1}{\sigma}} \tag{4}$$

Location $\mathcal{A}$ has competitive labor markets, by virtue of its RTW law.[15] However, in $\mathcal{B}$, unions raise the wage for workers in the capital intensive industry.[16] Accordingly, wages in the industry-location pairs are

$$w_{k\mathcal{A}} = w_{l\mathcal{A}} \tag{5}$$

[14]Firm subscripts omitted.

[15]Recall that the ability to engage in free-riding endogenously results in unions not forming in RTW locations.

[16]That is, we assume α is sufficiently high that industry $k\mathcal{B}$ is unionized in equilibrium. Technically, the unionization decision is more complicated than that considered in Section 3.2, because the firm's workers can have varying outside options (i.e. in different locations). Theoretically, what matters is the preferences and outside option of the firm's median worker. In our model, by the Weak Axiom of Revealed Preference it must be that all of the firm's workers prefer unionization, because decertifying the firm's union merely shrinks the set of (wage, location) bundles workers can optimise over. Thus it eventuates that union formation is still actually governed according to Equation 28, and we consider the interesting case where α satisfies this condition.

$$w_{k\mathcal{B}} = \frac{w_{l\mathcal{A}}}{1-\alpha} \tag{6}$$

Individuals

Individuals have preferences over consumption and the locations in which they live and work. Each individual inelastically supplies a unit of labor in location j, earning w_j. They use these wages to consume $\frac{w_j}{P}$ units of a composite consumption good (which has price P), in which their utility is assumed to be linear.[17] Similar to Redding (London Metropolis), the utility of an agent a who is a resident of i and works in j can be represented by the indirect utility function

$$U_{ij}(a) = \frac{\xi_{ij}(a)w_j}{P.T_{ij}} \tag{7}$$

where T_{ij} is an iceberg transportation (commuting) cost between i and j,[18] and $\xi_{ij}(a)$ is an idiosyncratic taste draw for agent a that captures all of their idiosyncratic preferences over factors (e.g. amenities, social considerations, moving costs from an endowed birthplace) that contribute to their desire to live in location i and work in location j.[19] We assume that the idiosyncratic amenity shocks are drawn from a Fréchet distribution with shape parameter ϵ, $F(\xi) = \exp(-\xi^{-\epsilon})$.[20] Given their own taste preferences and the wages that are available in each location, individuals choose which ij pair to live and work in to maximise their utility.

For individuals to make this choice, the wages they can attain must be specified. Any individual can earn $w_{\mathcal{A}} = w_{k\mathcal{A}} = w_{l\mathcal{A}}$ in $\mathcal{A}$. unionization of the capital-intensive industry in $\mathcal{B}$ complicates matters. Since $w_{k\mathcal{B}} > w_{l\mathcal{B}}$, there is excess demand from workers for union jobs. Specifically, anyone who works in $l\mathcal{B}$ would prefer to work in $k\mathcal{B}$, and some individuals who (in equilibrium) work in $\mathcal{A}$ would prefer this too. Jobs in the unionized industry $k\mathcal{B}$ are thus subject to rationing. We assume this is resolved by a lottery which allocates offers for union jobs ex ante, before individuals choose

[17]We set P as the numeraire, so all other prices are in real terms.

[18]Commuting costs are assumed symmetric, $T_{ij} = T_{ji}$ $\forall i, j$. We also assume $T_{ii} = 0$ $\forall i$, so commuting is costless if residence and workplace are in the same location. Of course, within a geographic unit, there are generally still non-zero transportation costs. Our approach is isomorphic to allowing $T_{ii} = c$ $\forall i$, but then normalising all travel costs relative to c, (i.e. commensurately reducing T_{ij}) since all that matters for choices (albeit not welfare) is relative transport costs.

[19]This setup can be enriched by allowing for common amenities (in both residence and workplace locations, and also location pairs) which produce location-specific utility components that are common for all individuals. We abstract from this for two reasons: it complicates the model without allowing for greater insight, and to match our border-pairs setting we wish to consider regions that are similar except for their differing RTW status.

[20]Higher ϵ reduces the heterogeneity in idiosyncratic amenities. This means that worker location decisions are more sensitive to economic fundamentals, and small changes in these have larger effects on the number of individuals who choose a given location pair.

where to live and work.[21] An individual who wins the lottery and receives an offer can elect, if they wish, to work in $k\mathcal{B}$ and earn the unionized wage. This means, in choosing ij, that individuals know whether they are eligible for a union job in $\mathcal{B}$, and thus whether they can attain $w_{k\mathcal{B}}$ or will be forced to accept $w_{l\mathcal{B}}$ if they choose $j = \mathcal{B}$. Individual idiosyncratic preferences mean that some individuals will prefer to work in $\mathcal{A}$ than to earn the union wage in $\mathcal{B}$.[22] Observing the non-union wage levels and understanding the distribution of idiosyncratic taste shocks, the firms issue $O_{k\mathcal{B}}$ offers to fulfill their labor demand of $L_{k\mathcal{B}}$.

Timing

The timing of decisions proceeds as follows

- At $t = 0$, firms make non-reversible capital investments
- At $t = 1$, the unions set the wage in $k\mathcal{B}$, and unionized firms issue $O_{k\mathcal{B}}$ offers
- At $t = 2$, each individual a learns their individual taste draws $\xi_i j(a)\ \forall i, j$.
- At $t = 3$, observing the wage they can earn in each location, individuals choose the utility maximising residence-workplace pair ij, yielding L_{vij}.

Equilibrium

Given a vector of wages $(w_{\mathcal{A}}, w_{k\mathcal{B}}, w_{l\mathcal{B}})$, the probability that an individual chooses location-pair ij is given by[23]

$$p_{ij} = \frac{(w_j)^{\epsilon}(T_{ij})^{-\epsilon}}{\sum_{m\in\mathbb{L}}\sum_{n\in\mathbb{L}}(w_n)^{\epsilon}(T_{mn})^{-\epsilon}},\ i, j \in \mathbb{L} \tag{8}$$

Given the outcomes of the lottery, the probabilities of working in the respective industries are characterised by

$$\tilde{p}_{k\mathcal{B}} = \frac{\sum_{i\in\mathbb{L}}(w_{k\mathcal{B}})^{\varepsilon}(T_{i\mathcal{B}})^{-\varepsilon}}{\sum_{m\in\mathbb{L}}\sum_{n\in\mathbb{L}}(w_{kn})^{\varepsilon}(T_{mn})^{-\varepsilon}} \tag{9}$$

[21] Abowd and Farber (1982) point out that in a model of queueing for union jobs, workers in non-union jobs become progressively less attracted to gaining a union job as they accrue experience. Accordingly, the long-term union status of workers tends to be strongly dependent on being selected from the queue (i.e. winning a lottery) early in their career. Accordingly, from a lifetime perspective, it makes sense in this context to allow individuals to make location choices *after* the outcome of this early life lottery is known. This contrasts with Harris and Todaro (1970), where uncertainty of the lottery outcome when making location decisions (there, moving from rural to urban areas in a developing country) is paramount.

[22] Note than no individual who wins the lottery will choose to work in $l\mathcal{B}$ over $k\mathcal{B}$. Thus, lottery winners choose between working in location $\mathcal{A}$ and $k\mathcal{B}$, while lottery losers choose between $\mathcal{A}$ and $l\mathcal{B}$.

[23] This is a standard result for the class of spatial models with location choice and Fréchet shocks. We provide a derivation in Appendix .

for offer recipients (winners of the lottery), and

$$\tilde{p}_{l\mathcal{B}} = \frac{\sum_{i\in\mathbb{L}}(w_{l\mathcal{B}})^{\varepsilon}(T_{i\mathcal{B}})^{-\varepsilon}}{\sum_{m\in\mathbb{L}}\sum_{n\in\mathbb{L}}(w_{lm})^{\varepsilon}(T_{mn})^{-\varepsilon}} \tag{10}$$

for non-recipients (losers of the lottery).[24]

Then, equilibrium can be characterised by output prices p_{vj} $\forall v, j$, wages $(w_{\mathcal{A}}, w_{k\mathcal{B}}, w_{l\mathcal{B}})$, and the lottery outcomes. p_{vj} induces product demand Q_{vj}, which then requires $L_{vj}(w_{vj}, Q_{vj})$ in order to clear the product market. The level of employment is tied down by the price p_{vj} because at any output quantity there is a unique capital-labor ratio (and level of labor) that minimises the representative firm's expenditure function. The vector of wages and the distribution of taste shocks imply the proportion of union job offers that will be accepted, $\tilde{p}_{k\mathcal{B}}$, and more generally the number of individuals L_{vij} who choose each industry-residence-workplace combination. To induce $E_{k\mathcal{B}}$, the firms collectively issue $O_{k\mathcal{B}} = \dfrac{E_{k\mathcal{B}}}{\tilde{p}_{k\mathcal{B}}}$ offers, with each issuing an amount proportional to the scale of its capital investment.[25]

It is possible to characterise a number of useful results that any such equilibrium must satisfy. To prove several of these results, the symmetric nature of the locations (differing only in their RTW status) and the i.i.d. nature of the idiosyncratic taste shocks can be exploited. Except for differences in wages, exactly half the population would prefer to work in either location (for their preferred residence location given their workplace preference), and similarly, exactly half the population would prefer to live in either location (for their preferred workplace location given their residence preference). Accordingly we can refer to the choices of a median individual who, when wages in the two locations are common, is indifferent between workplaces (or residences).

Proposition 3. *In equilibrium, compared to the wage level in the RTW location ($\mathcal{A}$), wages in the non-RTW location ($\mathcal{B}$) are higher in the union-exposed (capital intensive) industry but lower in the non-exposed (labor intensive) industry, $w_{k\mathcal{B}} > w_{\mathcal{A}} > w_{l\mathcal{B}}$*

Proof. First, from the union wage-markup rule in Equation 23 and the labor market equilibrium conditions in Equations 5 and 6, we have $w_{k\mathcal{B}} = \dfrac{w_{l\mathcal{B}}}{1-\alpha} > w_{l\mathcal{B}}$. It remains to show that w_A lies between these two wages. Proceed by contradiction. Except for differences in wages, exactly half

[24]Note that the price of consumption does not affect location choice because it does not vary across space (i.e. differential transport costs for goods by location are assumed de minimis).

[25]Formally this math assumes that an individual never receives offers of unionized jobs from multiple firms. Sufficient for this is that firms either make offers to non-overlapping tranches of individuals, or that firms make offers sequentially from the set of individuals yet to receive an offer. This is fundamentally unimportant - if individuals can receive multiple offers, $O_{k\mathcal{B}}$ can be adjusted to achieve the desired industry employment level.

the population would prefer to work in either location. Suppose that $w_{\mathcal{A}} > w_{k\mathcal{B}}$. Then a majority of individuals would choose to work in $\mathcal{A}$, $E_{\mathcal{A}} > E_{\mathcal{B}}$. However, for any variety $v \in \{k, l\}$, $w_{v\mathcal{A}} > w_{v\mathcal{B}} \Rightarrow p_{v\mathcal{A}} > p_{v\mathcal{B}}$ for the zero profit condition to hold. Then product market demand $Q_{v\mathcal{A}} < Q_{v\mathcal{B}}$. To clear the product market, firm labor demand would then need to satisfy $E_{v\mathcal{A}} < E_{v\mathcal{B}}$, both due to lower output supply and substitution away from labor in $\mathcal{A}$. But then in aggregate $E_{\mathcal{A}} < E_{\mathcal{B}}$, a contradiction. The reverse argument holds if $w_{\mathcal{A}} < w_{l\mathcal{B}}$. □

Proposition 3 reveals that while the unions (via the absence of RTW) can raise wages in unionized industries, this does not extend to industries that are not union exposed. Rather, raising the wage in the unionized industry reduces labor demanded by firms. The excess residual labor must then be absorbed by other industries, which in turn requires them to have lower wages than under the counterfactual. Since commuting and migration are costly, these spillovers most strongly affect depress wages in labor intensive industries (which are not union-exposed) in the non-RTW location. Accordingly, the model predicts that wages in industries like manufacturing, which are capital intensive and tend to be highly union-exposed, would be lower in RTW locations than otherwise similar RTW locations. However no such prediction can be made for average wages aggregating across industries, and indeed it is possible that RTW either increases or decreases worker wages on average.

Proposition 4. *In equilibrium, employment in the union-exposed (capital intensive) industry will be higher in the RTW location ($\mathcal{A}$) than in the non-RTW location ($\mathcal{B}$), both overall and in terms of a share of employment.*

$$E_{k\mathcal{A}} > E_{k\mathcal{B}} \tag{11}$$

$$\frac{E_{k\mathcal{A}}}{E_{\mathcal{A}}} > \frac{E_{k\mathcal{B}}}{E_{\mathcal{B}}} \tag{12}$$

Proof. From Proposition 3, $w_{k\mathcal{B}} > w_{k\mathcal{A}} = w_{l\mathcal{A}} > w_{l\mathcal{B}}$. Since the production technology is common (except for the identifies of the varieties produced) between locations, this in turn implies that $p_{k\mathcal{B}} > p_{k\mathcal{A}}$ and $p_{l\mathcal{B}} < p_{l\mathcal{A}}$. The elasticity of product demand is fixed at σ for all varieties, so $Q_{k\mathcal{B}} < Q_{k\mathcal{A}}$ and $Q_{l\mathcal{B}} > Q_{l\mathcal{A}}$. Higher production of the capital intensive good in $\mathcal{A}$, combined with relative substitution away from labor to produce variety $k\mathcal{B}$ (relative to the capital-labor ratio in $k\mathcal{A}$) implies that $E_{k\mathcal{B}} < E_{k\mathcal{A}}$ and thus $\frac{E_{k\mathcal{B}}}{E_{l\mathcal{B}}} < \frac{E_{k\mathcal{A}}}{E_{l\mathcal{A}}}$. Rearranging algebra, $\frac{E_{k\mathcal{A}}}{E_{\mathcal{A}}} > \frac{E_{k\mathcal{B}}}{E_{\mathcal{B}}}$, a higher employment share in the union-exposed industry in $\mathcal{B}$. □

Higher wages for the capital intensive industry in $\mathcal{B}$ has two effects on total employment by location. First, it reduces $E_{k\mathcal{B}}$ relative to $E_{k\mathcal{A}}$, due to firm's facing higher wages. In doing so, it makes $\mathcal{B}$ more attractive to lottery winners. Second, it causes workers to substitute into $l\mathcal{B}$, with $w_{l\mathcal{B}}$ falling accordingly, and $E_{l\mathcal{B}} > E_{l\mathcal{A}}$, such that the effect on total employment by location is attenuated relative to the employment differential in the capital-intensive industry. The lower $w_{l\mathcal{B}}$ means that some lottery-losers who would have counterfactually worked in $\mathcal{B}$ in the absence of unions, prefer to work in $\mathcal{A}$ instead. While $E_{l\mathcal{B}} > E_{l\mathcal{A}}$, we would typically expect this attenuation to be partial, such that $E_{\mathcal{A}} > E_{\mathcal{B}}$. For illustration, consider the following two extreme cases. As $\epsilon \to \infty$, individuals approach only caring about wage differences when making workplace location choices. Thus $w_{l\mathcal{A}} \to w_{l\mathcal{B}}$ and $E_{l\mathcal{B}} \to E_{l\mathcal{A}}$, so differences in employment in the capital-intensive industry fully pass through to differenes in total employment by location. Second, consider as $\epsilon \to 0$, so individuals only care about their idiosyncratic location preference, and will not alter their workplace choice at any wage differential. Then individuals who would have (in the absence of unions) been employed in $k\mathcal{B}$ substitute only into $l\mathcal{B}$, and $E_{\mathcal{A}} = E_{\mathcal{B}}$.

Proposition 5. *Suppose $E_A > E_B$. Then, in net, there is both migration and commuting to the RTW location, such that $R_{\mathcal{A}} > R_{\mathcal{B}}$ and $L_{\mathcal{AB}} < L_{\mathcal{BA}}$.*

Proof. Consider the probability of residing in i conditional on working in j. From Equation 8, this is

$$\begin{aligned} p_{i|j} &= \frac{(w_{vj})^{\epsilon}(T_{ij})^{-\epsilon}}{\sum_{m\in\mathbb{L}}(w_{vj})^{\epsilon}(T_{mj})^{-\epsilon}} \\ &= \frac{(T_{ij})^{-\epsilon}}{\sum_{m\in\mathbb{L}}(T_{mj})^{-\epsilon}} \end{aligned}$$

That is, conditional on choosing to work in j, the wage in j is irrelevant to location choice because it is definitionally unaffected by residence choice.[26] Note that by being invariant in wages, the conditional residence probability is also common across v. Further, since the matrix of transportation costs is symmetric, $p_{i|j} = p_{j|i}$. Denote $p_{j|j} = p_{i|i} = \rho > \frac{1}{2}$.[27] Then $R_A = \rho E_A + (1-\rho)E_B$ and $R_B = \rho E_B + (1-\rho)E_A$. Since $\frac{1}{2} < \rho < 1$ and $E_A > E_B$ by assumption, it follows that $E_A > R_A > R_B > E_B$. Net migration $m_{\mathcal{AB}} = R_A - R_B > 0$.[28] Net commuting flows $c_{\mathcal{AB}} = L_{\mathcal{AB}} - L_{\mathcal{BA}} = E_A - R_A > 0$. $\square$

The intuition here is fairly straightforward. If RTW locations induce extra employment (so that

[26]Instead, conditional on workplace choice, residence choice probabilities are dictated by transportation costs.

[27]This latter result follows from $T_{ii} < T_{ij}\ \forall i, j \neq i$.

[28]This is net migration relative to the counterfactual of no policy differences, in which case all regions would have equal population

extra employment in union-exposed industries is less than fully offset by differential employment in non-exposed industries), this must either occur through people (in net) moving to RTW locations, or (in net) commuting to them from non-RTW locations. The exact mixture of these two components will depend on how costly it is to migrate (in terms of accepting a worse taste draw $\xi_{\mathcal{AA}} < \xi_{\mathcal{BA}}$, relative to the cost of commuting $T_{\mathcal{AB}}$. The lower transportation costs are, the greater role for net commuting relative to migration.

In the basic model that we have outlined, all individuals are employed in the formal economy. To allow for non-employment, it is trivial to allow for an outside option for non-employment or home production. Indeed, suppose that individuals have an additional option of non-employment (such as home production) that yields utility equivalent to earning $\tilde{w}$, where $\tilde{w}$ is restricted to being below the median wage in equilibrium.[29] For clarity of notation, define E_j to be the number of individuals who are *employed* in workplace j (i.e. not including those who choose non-employment in j), and $\tilde{E}_j$ to be the number of individuals who have their "workplace" in j (i.e. including those who choose non-employment in j). Then a fraction $\frac{E_j}{\tilde{E}_j}$ of people who choose workplace j are gainfully employed.

Proposition 6. *When an outside option which yields utility equivalent to a wage $\tilde{w}$ (of less than the median wage) is available for non-employment, the employment-to-population ratio will be higher for residents of the RTW location ($\mathcal{B}$) than for residents of the non-RTW location $\mathcal{B}$.*

Proof. If $\tilde{w}$ is sufficiently low, the non-employment option may be non-binding and have no effect. However, if it binds, it will bind upon industry $l\mathcal{B}$, by virtue of $w_{l\mathcal{B}} < w_{\mathcal{A}}, w_{k\mathcal{B}}$. Indeed, since by assumption $\tilde{w}$ is below the median wage in equilibrium, this is the only variety that it can directly bind upon. Then, individuals choosing non-employment will force $w_{l\mathcal{B}} = \tilde{w}$.[30] Each non-lottery winner in $l\mathcal{B}$ is accordingly indifferent between employment in $l\mathcal{B}$ and non-employment; equilibrium merely requires enough to choose non-employment to bid the market-clearing wage up to $\tilde{w}$. This produces $\frac{E_{\mathcal{B}}}{\tilde{E}_{\mathcal{B}}} < \frac{E_{\mathcal{A}}}{\tilde{E}_{\mathcal{A}}} = 1$. Employment rates by residence are merely the residence-probability weighted average of gainful employment across workplaces j. Since living in a location is predictive of working in the same location (due to transportation costs), it follows that residents of $\mathcal{A}$ have higher employment rates. □

Finally, define poverty as earning labor income below w_p, for some w_p below the median wage in

[29]For tractability, this option is available in each workplace location j, so individuals still acquire the associated idiosyncratic taste shock.

[30]In turn, this will indirectly affect wages in other variety-workplace pairs.

equilibrium. An individual may be in poverty either through being employed at a low wage, or not being employed.

Corollary 7. *The poverty rate is lower for residents of the RTW location* $(\mathcal{A})$ *than for residents of the non-RTW location* $(\mathcal{B})$*.*

Proof. Denote the poverty status of an individual by ρ, and their employment status as e. The poverty rate for a group of individuals can be calculated by

$$\begin{aligned}\Pr(\rho) &= (1 - \Pr(e = 1)) + \Pr(e = 1). \Pr(w < w_p | e = 1) \\ &= 1 - \Pr(e = 1). \Pr(w > w_p | e = 1)\end{aligned}$$

It immediately follows that the poverty rate is lower in location $\mathcal{A}$ than $\mathcal{B}$ because both the employment rate and the probability of earning above w_p conditional on employment are higher in the RTW location, since $w_{l\mathcal{B}} < w_{\mathcal{A}}$. □

4 Empirical Strategy

With these predictions of the model in mind, we turn to the data. The key challenge in identifying the effects of RTW laws are that such laws are not randomly assigned. On average RTW and non-RTW states vary substantially in demographics, education, economic and social history, climate, and natural resources amongst other factors. All these differences could lead to substantial differences in outcomes, both when measured in levels or in growth rates. Even with exhaustive controls, omitted variable bias is a significant concern in any cross-sectional analysis of the impact of RTW legislation at the national level. Therefore we focus on local differences generated by policy borders where these differences are likely to be significantly reduced.

4.1 Border discontinuity design

Our approach is to focus on neighboring regions at state borders where RTW policies discontinuously change. By focusing on a narrow band around the policy borders, we are able to obtain a sample where the treatment (RTW) and control (non-RTW) areas resemble each other with a common climate, similar natural resources, transportation infrastructure, and similar demographics and history. This motivates our formal identifying assumption that the RTW and non-RTW regions would have equal outcomes in expectation except for the policy discontinuity at the state border. By averaging over many state-border pairs, we also minimize the effect of any other state policy differences that are not highly correlated with RTW legislation. In Section 7 we also explicitly test for the explanatory power of other observable state policy differences.

There is a significant literature that makes use of border-pair identification to isolate the impact of discontinuous state policies. Holmes (1998) pioneered this approach examining the impact of RTW laws on manufacturing share. A more recent literature has used state border discontinuities to study policies including the impact of minimum wages (Dube et al., 2010) and the impact of unemployment insurance extensions (Hagedorn et al., 2019).

The key assumption behind all border discontinuity research designs is that economic geography is smooth, and therefore we would not expect to find significant differences in the absence of policy discontinuities. In other words, border regions should exhibit either common levels or (in the presence of other time-invariant differences) parallel trends in the absence of policy shocks. This assumption has been challenged by Coglianese (2015) in respect to Unemployment Insurance duration, and Neumark et al. (2014) in respect to minimum wages. In both these papers, the key question is whether there exist other state-level employment shocks that stop at the border, violating the assumption of smooth trends.

While these may be valid criticisms of the use of border discontinuity methods to study recent policy changes, they are less applicable to our case. In our study, we examine the average differences over many years between regions decades after a policy change. As a result, we do not have to worry about the impact of reverse-causality contaminating our results due to policymakers implementing a policy change in response to a state-level economic shock. We also do not have to worry that transitory state-level economic shocks would significantly bias our estimates, as these will tend to average out over the our sample period. As described below, we conduct placebo tests at the state-border level to specifically address additional concerns of contamination from state-level shocks.

To ensure our identification strategy is plausible, we focus on narrow geographic units adjacent to state borders where data is consistently available. Our primary unit of analysis is the county. We construct a sample of all pairs of adjacent counties i and i' in the continental US that had different RTW status as of 2010, as shown in Figure 1.[31] This border-pairs sample involves 380 adjacent border-pairs comprised from 373 counties overall.

[31] That is, counties i and i' are adjacent counties in different states with different RTW policies, and in combination constitute the county-pair i, i'.

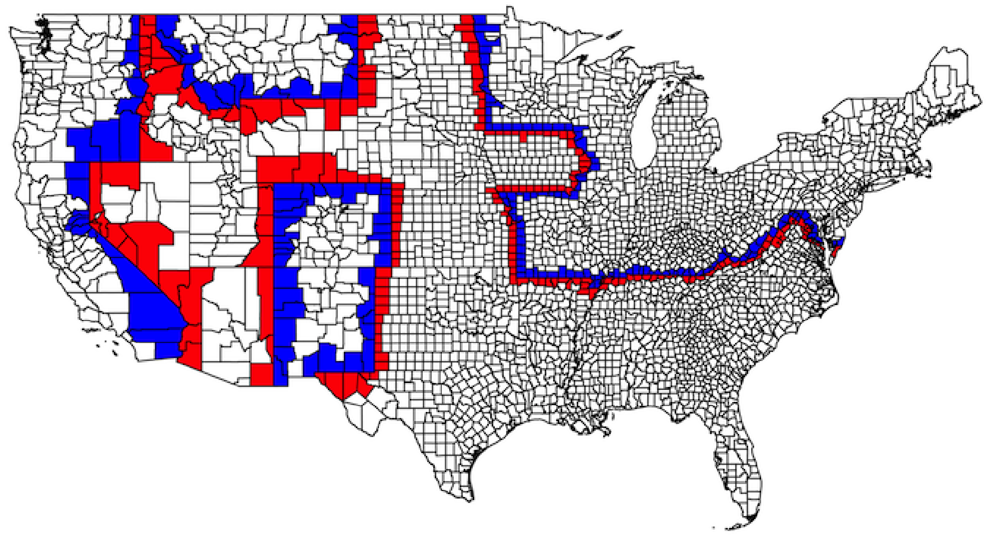

Figure 1: *RTW Border Counties*

Our choice of 2010 borders excludes states that changed their RTW status in the past decade, but includes all other policy borders including Oklahoma which adopted RTW in 2001. These policy borders had been very stable over time. For example, as of 2010, the average county-pair in our sample had contained a RTW policy border for 57 years. We include Indiana, Michigan, Wisconsin, West Virginia and Kentucky as non-RTW states based on their status in 2010.[32] These states subsequently enacted RTW legislation after 2010, however given the long-term impact of RTW is likely to take at least several years to manifest, we do not exclude them from analysis. In any case, the vast majority of our data (which primarily covers 1990-2017) predates these recent policy changes. Given our border-pairs comparison, a short-term impact of RTW in these states would slightly attenuate our estimated effects.

We supplement our county analysis with 2010 Census Public Use Microdata Areas (PUMAs) that are available in the American Community Survey. PUMAs are defined by the US Census Bureau for the purpose of disseminating Public Use Microdata Samples, and are geographic regions that consist of a population of at least $100,000$ people. They are built on census tracts and counties, and importantly for our purposes, nest within states. We analogously construct the set of all pairs of adjacent PUMAs that had different RTW status as of 2010. As seen in Figure 2, PUMAs are

[32]Only the latter three states appear in our border pairs sample, with Wisconsin the earliest to adopt RTW in 2015.

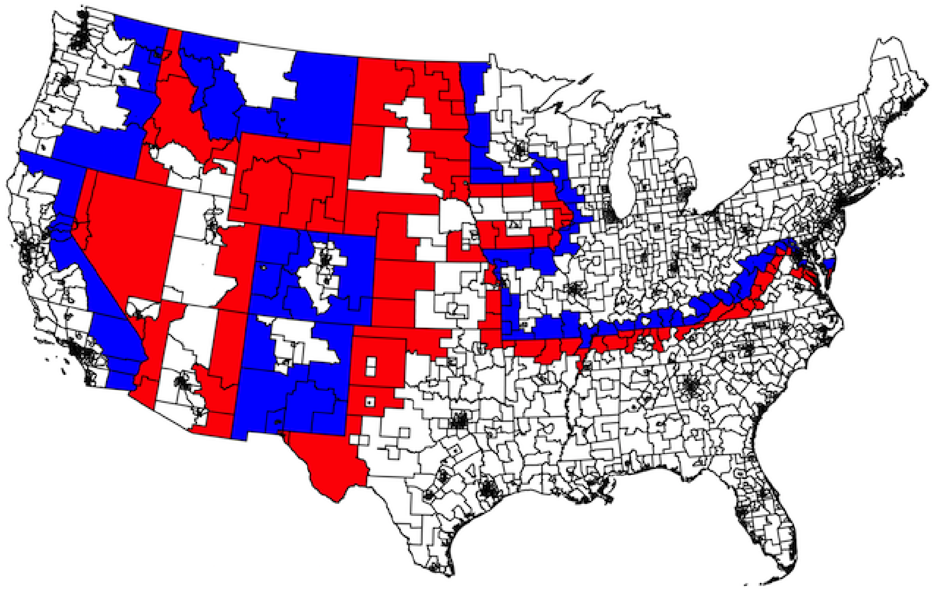

Figure 2: *RTW Border PUMAs*

substantially larger geographic areas than counties, particularly in sparsely populated regions of the country. Therefore while we are able to leverage additional data at the PUMA level, it is at the expense of less compelling geographic identification of differences. Therefore we estimate the effects of RTW primarily from county level data, and use PUMA data to support our analysis.

The thought experiment underlying our border differences analysis is to isolate differences in outcome of interest between treated and control units, pooling across pairs.[33] To do so, we include both unit-pair (e.g. county-pair) fixed effects $\gamma_{i,i'}$ and year fixed effects δ_t. The unit-pair fixed effects absorbs differences between pairs, and therefore we are only using variation within the local geographic area surrounding the policy discontinuity. The year fixed effects remove the impact of national shocks impacting all locations. Our equation of interest is thus:

$$y_{it} = \beta_R RTW_i + \gamma_{i,i'} + \delta_t + \varepsilon_{it} \tag{13}$$

Our parameter of interest is β_R, the average treatment effect of RTW. This regression is 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.[34] Since the level of observation is the unit-year, this gives a weighted-average treatment

[33] In constructing the sample, observation (i, t) is dropped if (i', t) is missing, to preserve panel balance.

[34] Specifically, note that the year FE explain both treatment and control observations equally, leaving the residual border

effect where each pair-year receives equal weight, and thus the pair weights are the number of years of data for the respective unit-pair.

While the vast majority of our analysis utilises this border-differences strategy, some of the measures we study have readily available data series that extend back to before adoption of RTW laws. Unlike the border-difference analysis where the identifying assumption is that outcomes in RTW and non-RTW regions would be equal in expectation except for policy effects, here we are able to invoke a weaker assumption that outcomes in RTW and non-RTW areas should exhibit parallel trends in the absence of policy effects. Under this assumption, the effect of the RTW policy discontinuity can be analyzed using difference-in-differences regression.[35]

Here, the natural thought experiment is to isolate the change in the difference between treated and control units at time t relative to the pre-existing difference in a base period t_0, pooling across pairs. For each county-pair experimental unit, this requires county fixed effects to absorb the base period difference, and time fixed effects to absorb common shocks that affect both treatment and control locations in the post-period (or more generally, in every other time period). This yields the classic difference-in-differences estimator for a given experimental pair. Since each county-pair is a separate experimental unit, pooling requires interacting these controls with county-pair fixed effects. This yields a specification with county-pair by county fixed effects $\gamma_{ii',i}$ to extract baseline differences within each county-pair, and county-pair by year fixed effects $\delta_{ii',t}$ to extract pair-specific time shocks. As above, this ensures the identifying variation used to estimate the RTW effect only captures differential changes *within* county-pairs across time, averaged across experimental pairs. The resulting difference-in-differences equation of interest is thus:

$$y_{it} = \beta_{R,t} RTW_i + \gamma_{ii',i} + \delta_{ii',t} + \varepsilon_{it} \tag{14}$$

The parameter of interest is $\beta_{R,t}$, 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 $(y_{T,t} - y_{C,t}) - (y_{T,t_0} - y_{C,t_0})$ for each pair-year $\{i, i', t\}$ and averaging across pairs for each year t.[36]

difference unchanged. Similarly, the astute reader will note that neither the inclusion of unit-pair fixed effects $\gamma_{i,i'}$ nor year fixed effects δ_t will alter the point estimate $\hat{\beta}_R$. One way of seeing this is to note that having constructed a perfectly balanced panel, these controls are by definition orthogonal to RTW status. An alternate explanation is that these controls amount to taking within transformations (by unit-pair and year), which thus leave differences in outcomes *within* the unit-pair-year unchanged.

[35]In addition to allowing for fixed differences between locations, the difference-in-differences analysis has the advantage of being able to analyze whether the effects materialised over a plausible time schedule following the onset of treatment.

[36]Analogously to Equation 13, with a balanced panel the inclusion of county-pair by year fixed effects only impacts precision, not the point estimate of $\beta_{R,t}$.

Rather than normalising against a single pre-period t_0, this equation can also be modified to give an event-study interpretation by indexing time relative to the onset of treatment in each pair.[37]

An important caveat to our analysis is that our border-discontinuity strategy allows us to estimate the causal impact of RTW laws at the local level near a policy border. It does not allow us to identify the broader effect of RTW laws far away from a policy border, nor does it specify the general equilibrium effect of national policy changes to RTW laws.

4.2 Standard Errors

To properly calculate the standard errors of our estimate, we need to incorporate potential correlation in errors due to common economic shocks. A variety of approaches are adopted in the literature to address this issue, including the use of spatial error models, the use of block-bootstrap in Hagedorn et al. (2019), and two-way clustered standard errors by state and state-border pair in Dube et al. (2010).

We adopt two methods to calculate our standard errors. First, note that a given unit can appear in multiple unit-pairs in our sample, since for example a county i can border both counties j and j' in another state. Thus, at a minimum, we need to account for correlated errors for each county. Accordingly, in our regressions we cluster at the county level. However, this yields standard errors that are likely insufficiently conservative because both the RTW treatment assignment and other policy and economic shocks arise at the state level. At first glance, this would argue for clustering by state and border pair jointly. Empirically we observe that this produces more conservative, but broadly similar, standard errors. However, in our case, we are only using a subset of 29 states and 35 state-border pairs where RTW status differs, and therefore clustering standard errors by state is inappropriate given the small number of clusters.

Thus, our baseline method (clustering only at the county level) does not account for state-level shocks that end at the border. To account for arbitrary state-level shocks as well as local spatial correlation, we conduct two permutation test based on a block-bootstrap. For the first permutation test, we consider the set of RTW state border pairs. Accordingly, we refer to this as the *policy borders* permutation test. For each state pair, we randomly assign (pseudo) treatment to either side of the state-border before calculating the average estimated treatment effect over all state-pairs in our bootstrapped sample (weighting by the number of unit-pair years). An issue here is that since a state

[37] That is, RTW effects by calendar year $\beta_{R,t} RTW_i$ can be replaced by effects by years since RTW adoption, $\beta_{R,y} RTW_i$.

can occur in multiple state-border pairs, independent randomisation could assign state *j* as treated in some pairs and untreated in others. Thus, when randomising, for each draw we ensure that each RTW state is consistently assigned to either treatment or control.[38]

Our second permutation test instead uses the set of state border-pairs where RTW status is common ({RTW, RTW} and {Non-RTW, Non-RTW}). Accordingly, we refer to this as the *non-policy borders* permutation test. Assuming that border differences for this sample and the true RTW border differences are drawn from the same distribution, this enables us to calculate how likely effects at least as large as our observed RTW border effects were to arise by chance under the null of no true RTW border differences. We randomly assign each state (and D.C.) in the contiguous United States to a pseudo-policy status, effectively generating random 'maps' of pseudo-treatment status. From this, we extract the state-border pairs where pseudo-treatment status differs (but true RTW status is common). Across draws, we thus calculate the distribution of the pseudo-treatment effect.[39] For each version, we perform $100,000$ bootstraps, and report the distribution of treatment effects and p-value of the true RTW treatment effect estimate. While both methods are informative, the latter appears a more natural exercise.

5 Data

We draw on data from a variety of publicly available sources at the county, PUMA, and state level. We describe the data utilized at each level of aggregation below, and provide additional details of dataset construction in the Data Appendix, Section D.

5.1 County data

Our primary source for labor force data are the Local Area Unemployment Statistics (LAU) provided by the Bureau of Labor Statistics (BLS). From this data, we calculate county level unemployment, employment-to-population (EPOP) ratios, and labor force participation rates using the average

[38] This is nonetheless imperfect. Non-RTW states can, and will, still be assigned varying pseudo-treatment status within a given bootstrap draw. The only way to avoid this completely is to jointly assign contiguous border segments, but this would be impractical as very few unique permutations are possible.

[39] A complicating factor is that the number of pseudo-treatment state-border pairs given will vary by draw, and may be substantially different than the 35 state-pairs in our analysis sample. By the LLN, we would expect that draws with fewer (more) pseudo-treatment state border pairs will lead to higher (lower) variance of the pseudo-effects. To prevent this, we match the number of pseudo state-border state-pairs to the true number in our sample. To achieve this, we exclude random map draws that yield too few state-border pairs and extract a random subset of the state-border pairs in maps with a greater number. Fortunately, this turns out to have little effect because by happenstance, the number of pseudo border-pairs from random map draws is fairly tightly distributed around approximately the true number in our analysis sample.

annual data at the county level. To calculate population based ratios, we combine data from the LAU with intercensal estimates of the age 15+ population from the Census Bureau.

We collect LAU data from 1976 to 2017. The LAU data is available publicly on the BLS website from 1990. Data prior to 1990 is available by request from the BLS, but is not publicly released due to concerns about comparability with the latter series. In our case, all our specifications include year fixed-effects which should remove the impact of variation in methodology across years provided that values are calculated consistently between RTW and non-RTW border counties in each year.[40]

For county level analysis of industry composition, we use the Quarterly Census of Employment and Wages (QCEW) provided by BLS. We make use of the annual averages of quarterly data produced by BLS. The data includes total employment by industry, the average number of establishments, total annual wages, and average weekly wages.

We collect data from 1990 to 2017, when NAICS based industry breakdowns are available. We aggregate industries based on the QCEW top level industry codes using NAICS industry classifications. The manufacturing employment share is calculated by aggregating employment in NAICS codes 31-33, divided by total employment in the county from all sources (private sector and government). This definition minimizes the impact of data supressions in the QCEW, which become more problematic for sub-divisions of manufacturing. Nonetheless, some counties still have manufacturing employment data suppressed, especially prior to 2001. To avoid data suppression leading to endogenous sample selection, when analysing manufacturing employment we restrict the sample to counties that either have no suppressed data, or include only data from 2001 onwards for counties with no suppression since 2001. We convert wages into real 2016 dollars using the Consumer Price Index research series for all Urban Consumers (CPP-U-RS).

The measurement of employment varies between the LAU and QCEW in an important way for our analysis. The LAU statistics are based on the location of residence of individuals, while QCEW employment statistics are based on the location of the employer. This means that if an individual commutes across a county (or state) border for their job, their employment will be recorded in different locations in the two data sources. We calculate employment-to-population ratios and labor force participation using the LAU data, as that can be mapped directly to intercensal population estimates at the county level. However, if there were a significant number of people commuting from non-RTW to RTW states for employment, we would arguably understate the impact of RTW on the

[40]As a robustness check, we confirm that our estimated effects are similar if we restrict our sample to the publicly released series beginning in 1990.

unemployment rate and employment rate due to the measurement at location of residence rather than employment. We are not concerned about this for two reasons. First, any bias introduced from measurement at the location of residence would likely attenuate our measured effect of the impact of RTW laws. Second, cross-state commuting is a theoretically important channel of interest, and using data from the American Community Survey we are able to directly measure commuting flows that cross RTW borders.

Data on upward mobility and social outcomes is provided by Opportunity Insights. We make use of publicly released data at the county level that includes a small amount of noise to ensure differential privacy.[41] Data on income mobility was originally published in Chetty et al. (2018), data on the causal impacts of counties was published in Chetty and Hendren (2018), and additional social outcomes were published in Chetty et al. (2014).

We collect additional outcome data at the county level where available. Data on migration at the county level is from the SOI Tax Statistics provided by the Internal Revenue Service. Migration is calculated based on year-to-year address changes of individual income tax return filers. We collect data from 1991 to 2016 and calculate the migration rate based on the filing year of the tax return. Data on disability enrollment is calculated using the number of Social Security Disability Insurance (SSDI) recipients reported by the Social Security Administration. We collect data from 2009 to 2017. Data on poverty rates from 1997 to 2017 is obtained from the Small Area Income and Poverty Estimates, and data on health insurance coverage between 2008 and 2017 is obtained from the Small Area Health Insurance Estimates, both of which are released by the Census Bureau. To avoid capturing effects from the Medicaid Expansion, we focus on coverage for people above 138% of the federal poverty level. Finally we collect historical decennial Census data at the county level. We use both the 1930 and 1940 complete count census datasets released by IPUMS as detailed in Ruggles et al. (2019). We also obtain a range of variables including county population and demographic characteristics in the county level aggregates from the 1880 through 2010 censuses released by IPUMS NHGIS, as detailed in Manson et al. (2019).

5.2 PUMA data

We augment our primary analysis at the county level with additional analysis of border PUMAs in the American Community Survey (ACS). The use of ACS microdata provides a richer set of outcomes

[41] Given this noise is randomly added to each county, it should not impact our estimates of RTW effects other than slightly increasing our standard errors.

than county analysis.

There are three disadvantages to the use of ACS data. First, the PUMAs in the ACS are typically larger geographic areas than counties. To minimize the impact of this, we restrict our analysis to the 2012-2017 ACS which use the 2010 Census PUMAs, which are the smallest geographic area identifiable in the publicly released microdata. Extending the analysis prior to 2012 would require us to use the 2000-2010 Consistent PUMAs (as PUMA definitions changed in 2010), which are significantly larger than the 2010 PUMAs. Second, the ACS is only available for a small number of years, and so we have less power than in our county sample. Finally, the ACS is only a sample of the population rather than based on administrative records, and therefore sampling error is a greater concern than in our county analysis. For these reasons, we use the ACS to supplement our county-based analysis rather than as the primary source of data.

We collect a range of variables including total employment, unemployment, and labor force participation by PUMA. We also subdivide employed persons by industry of employment. As with county data, all wage data is converted to real 2016 dollars using CPI-U-RS. In the ACS we can also observe average reported hours worked per week.

In addition to labor market outcomes we can also observe demographics, racial and ethnic composition, and education levels for the population. The ACS also provides two measures of migration. First, it asks respondents where they resided 12 months previously at the migration PUMA level, which provides a measure of 12-month migration rates. Second, it asks state of birth, which allows us to additionally measure the share of the population born out of state. We provide further details on the construction of these variables in the Data Appendix, Section D.

5.3 State data

Our key variable of interest, namely which states have adopted Right to Work laws, is measured at the state level. We obtain a historical series of adoption dates of Right to Work laws maintained by the National Right to Work Committee. Some states implemented RTW laws by statute before later embedding RTW in their state constitution. We use the earlier date of adoption in each such case since this records the time since when RTW has prevailed.

In assessing the impact of RTW laws on neighboring geographic areas, an immediate concern is that changes in RTW laws are coincident with other policy discontinuities at state borders. To address these concerns, we collect data on a variety of state policies to enable us to control for many potentially relevant confounding policies. Reassuringly, we consistently find that including a battery of state policy controls has relatively little effect on our point estimates.

We collect data on state fiscal policies from the Government Finance Database, as described in Pierson et al. (2015). The database includes state fiscal policies from 1972 to 2016, although consistent annual reporting for all states is only available from 1978 to 2016. Our variables of interest include total revenue, total taxes, individual income taxes, corporate net income taxes, total state expenditure, state pension expenditure, and state debt. All variables are measured in real 2016 dollars per capita, indexed using CPI-U-RS. In addition we calculate state unemployment expenditure in average dollars per unemployed individual. This captures the total impact of unemployment policies at the state level, including eligibility, the generosity in benefits per week, and the duration of benefits.

We control for historical annual average state minimum wages using data from Vaghul and Zipperer (2016) from 1974 to 2016. We include controls for the share of employment requiring a state licence or certification. We use data from Kleiner and Vorotnikov (2017) for these variables, which is based on a 2013 nationally representative survey of workers. While the CPS has begun collecting data on licensure rates, Kleiner and Vorotnikov (2017) shows that the larger dataset from the national survey is likely to be more accurate.

Finally, in addition to state fiscal policies and direct regulation, we collect information on general state business attractiveness using the Beacon Hill Institute's composite Business Competitive Index (BCI) for 2012. While a variety of state competitiveness indexes have been produced, we use the BCI index due to its broad coverage including government and fiscal policy, security, infrastructure, human resources, technology, business incubation, openness, and environmental policy. An additional advantage of this index is that it does not include RTW status as an explicit input into the rankings, while many other comparable indices do, as described in Appendix A of Kolko et al. (2011).

6 Border Discontinuity Results

In this section we present the results of our border discontinuity tests. Our identification strategy rests on the comparability of border regions, so we begin by testing whether there are any systematic differences in economic history or population characteristics between the RTW and non-RTW regions. We confirm the result of Holmes (1998) that there is a discontinuous increase in manufacturing employment in RTW regions. We show that manufacturing differences extend to the broader labor market, and show that RTW regions have lower unemployment, higher labor force participation and employment-to-population ratios, and lower rates of disability insurance receipt. Looking at labor compensation, we find mixed results. Contrary to what is typically claimed by unions, workers in RTW locations do not appear to earn lower wages. If anything, on average they appear to earn slightly higher wages but also work slightly longer hours. At least on the extensive margin, aggregate differences in non-wage compensation such as health insurance provision appear small.

Next we examine whether RTW regions differ beyond the labor market. RTW regions have higher in-migration and population growth, possibly in response to the greater job opportunities available in RTW regions. Finally, we show that RTW regions also differ along a range of social dimensions including experiencing higher intergenerational mobility and lower poverty rates.

6.1 Summary statistics and balance tests

As described in Section 4.1 our identification strategy leverages local discontinuities in policy at the state border. The key to credible identification is that economic geography is smooth between the border regions, in other words, border regions resemble each other aside from the policy discontinuity. If locations either side of RTW borders would have equal outcomes in expectation except for differences in policy, observed differences in economic outcomes can be attributed to the policy discontinuity. Therefore, before presenting our border discontinuity results, we establish that local regions are indeed observably similar on non-policy dimensions. To do so, we employ a two-pronged approach.

First, we analyze whether there were important differences in economic outcomes between our treatment and control counties pre-dating RTW laws. While our data does not allow us to perform difference-in-differences analyses for the most part, this addresses concerns that modern differences in economic outcomes (which we observe) could reflect long-standing previously-existing differences between counties. Using the full-count US Censuses of 1930 and 1940, we examine a range of measures and show that the differences we observe today were not present prior to the passage of RTW laws. Second, we verify that the populations demographics of RTW and non-RTW border counties are highly similar in the modern period. Together, these two strands of evidence give credence to our identifying assumption that the RTW and non-RTW border counties are similar on both observable and unobservable dimensions.

6.1.1 Historical border results

A key threat to the identification of the causal effect of RTW laws is that observed differences between border-county pairs may reflect some longstanding non-policy difference between these counties. To investigate this, we can examine differences in the outcome variables between our border county-pairs prior to treatment. If differences in RTW policy are responsible for the observed differences, there should not be any significant differences prior to the passage of RTW laws. To analyze this, we use the full count data for the 1930 and 1940 censuses. While the list of measures we can study in this era is limited relative to the preponderance of modern data, we are consistently unable to detect any statistically significant differences at (subsequent) RTW borders. In fact, none of the resulting estimates are significant under our preferred placebo test at conventional levels of significance.

Table 1 Panel A reports the share of the population employed in different industries in 1930 and 1940. As we are using the full-count census, we split manufacturing into non-durable and durable goods. We see there are no significant differences in employment in manufacturing industries prior to the passage of RTW laws, while we do observe economically significant differences today.

Table 1 Panel B reports employment differentials in 1930 and 1940. Again we see that the border counties looked very similar prior to the passage of RTW laws. We note that the unemployment rate may have been slightly lower in RTW counties, but this result is insignificant in both our placebo tests, and we do not see a significant difference in employment-to-population or labor force participation rates. Nor do we see a meaningful difference in the number of weeks people report working in the previous year.

Finally Table 1 Panel C reports differences in wages and weekly hours worked in 1940 (the 1930 Census did not include wages). Again we do not see meaningful differences between the counties, although the wage estimates in particular are quite imprecise.

We conclude that the border counties looked similar on a range of outcomes prior to the passage of RTW laws. Importantly we observe that the border counties did not differ in their share of manufacturing, a key outcome we observe after the passage of RTW laws. While we are unable to test historical rates of intergenerational mobility, these results provide confidence that the differences observed today do not reflect long-standing non-policy differences between these counties.

Table 1: *Historical Differences at RTW Borders*

Panel A: Industry

	(1) Manufacturing (Non-Durable)	(2) Manufacturing (Durable)
Right to Work	0.0018	0.0001
	(0.0014)	(0.0014)
County Pair FE	Yes	Yes
Year FE	Yes	Yes
Control Mean	0.0163	0.0164
Non-policy Borders p	0.2566	0.4930
Policy Borders p	0.0869	0.4887
Observations	1508	1508

Panel B: Employment

	(1) Unemployment	(2) Employment to Population	(3) Participation	(4) Weeks
Right to Work	-0.0035*	0.0011	-0.0006	0.3497**
	(0.0019)	(0.0025)	(0.0024)	(0.1773)
County Pair FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No
Control Mean	0.0657	0.5032	0.5387	42.1873
Non-policy Borders p	0.1277	0.3965	0.4683	0.1265
Policy Borders p	0.1957	0.4211	0.4704	0.1921
Observations	1506	1506	1506	754

Panel C: Wages

	Full Time Annual Wage			
	(1) (Median)	(2) (P10)	(3) (P90)	(4) Weekly Hours
Right to Work	11.6230	5.0428	0.7807	0.2499
	(14.7161)	(3.4203)	(29.1789)	(0.2811)
County Pair FE	Yes	Yes	Yes	Yes
Control Mean	520.9735	114.1878	1458.7381	48.3221
Non-policy Borders p	0.2679	0.1870	0.4723	0.2841
Policy Borders p	0.2172	0.1162	0.4620	0.2738
Observations	754	754	754	748

Standard errors clustered by county. * p<0.10, ** p<0.05, *** p<0.01. Non-policy Borders p reports the one-sided p-values from the non-policy borders permutation tests, while Policy Borders p reports the one-sided p-values from our alternate policy borders permutation test using our regression sample. Sample includes respondents aged 16 years or older. Panel C and Column 4 of Panel B only includes 1940 Census.

Data source: 1930 and 1940 complete count US Census.

6.1.2 Demographic balance tests

While it is reassuring to see that RTW and non-RTW border areas were similar prior to RTW laws being introduced, this result alone is not sufficient to give credence to our identification strategy. If there are important differences in demographic composition of the two areas in our modern sample period, then this would suggest our assumption of smooth economic geography aside from the policy discontinuity may be violated. This is important for two reasons. Differences in these characteristics could directly affect our outcome variables, but also could be a signal of other unobservable differences between the populations.

A complicating factor here is that RTW policy could impact non-labor market outcomes through selective migration and endogenous investment decision-making.[42] Therefore a difference in outcomes between RTW and non-RTW regions does not necessarily imply a failure of identification as the outcome could be endogenous to the policy regime. However, it would then be important to differentiate between the causal effects of RTW on individuals and population-level effects due to changes in population composition.

Accordingly, in this section we test for observable differences between these counties in demographics, education and marital status using county-level aggregates from the 1970-2010 decennial censuses and ACS. Reassuringly, the breadth of our sample allows us to determine that differences in population characteristics throughout this period are either non-existent or very small in magnitude.

Table 2 Panel A shows there are minimal demographic differences between non-RTW and RTW counties on the border. RTW counties have a 0.3% higher fraction of men, and a slightly lower share of persons aged over 15, but neither of these differences are economically significant. The higher male share in particular may be endogenous to RTW, if RTW induces migration from manufacturing workers. Importantly we see that the natural logarithm of population density is similar between the two locations. As urban and rural areas differ on a wide range of characteristics, it is important there are not systematic differences in density between RTW and non-RTW counties. This also addresses the concern that RTW may be conflated with the underlying rate of unionization being higher in urban areas as described in Ellwood and Fine (1987).

Table 2 Panel B shows there are minimal differences in the racial and ethnic composition of the counties. We note that the border counties between RTW and non-RTW states are predominately

[42]For example, choices regarding education and marriage that consider both current and future expected economic outcomes could plausibly be affected by RTW. Similarly, if RTW induces migration of workers in specific union-exposed industries, then this may alter population demographics slightly.

Table 2: *Balance Tests at RTW Borders*

Panel A: Demographics

	(1) Share Male	(2) Share Age 15+	(3) Share Age 25-54	(4) Log Population Density
Right to Work	0.0029***	-0.0050***	0.0018	0.0423
	(0.0008)	(0.0018)	(0.0017)	(0.0898)
Control Mean	0.4955	0.7723	0.3732	3.0034
Non-policy Borders p	0.0040	0.0360	0.2553	0.3318
County Observations	3778	3778	3778	760

Panel B: Race / Ethnicity

	(1) White	(2) Black	(3) Hispanic	(4) Other
Right to Work	-0.0043	-0.0075**	-0.0010	0.0133**
	(0.0065)	(0.0036)	(0.0042)	(0.0059)
Control Mean	0.8842	0.0363	0.0694	0.0262
Non-policy Borders p	0.3829	0.1216	0.3351	0.1350
County Observations	3784	3784	3032	3032

Panel C: Education

	(1) Less than High School	(2) High School	(3) Some College	(4) College
Right to Work	-0.0012	-0.0063**	0.0044**	0.0031
	(0.0037)	(0.0025)	(0.0017)	(0.0033)
Control Mean	0.3022	0.3801	0.1801	0.1376
Non-policy Borders p	0.4124	0.1462	0.1821	0.2369
County Observations	3784	3784	3784	3784

Panel D: Marital Status

	(1) Single	(2) Married	(3) Divorced	(4) Widowed
Right to Work	-0.0012	0.0068***	-0.0032***	-0.0029***
	(0.0027)	(0.0021)	(0.0009)	(0.0007)
Control Mean	0.2186	0.5814	0.0859	0.0680
Non-policy Borders p	0.3781	0.0573	0.0502	0.0047
County Observations	3024	3024	3024	3024

All panels: Regressions include county-pair and year fixed effects. Standard errors are clustered by county. * p<0.10, ** p<0.05, *** p<0.01. Non-policy Borders p reports the one-sided p-value from the non-policy borders permutation test. Panel A: Column (1) provides total male share of population, column (2) lists the share of population aged 15 or older, column (3) provides the share of population aged 25 to 54, and column (4) reports the log population density calculated as total population divided by total land area in square miles. Panel B: Reports share of population by primary racial / ethnic affiliation. Columns (1) and (2) report non-Hispanic White and non-Hipsanic Black (African-American) population shares. Panel C: Reports share of population age 25 or older by highest educational attainment. Some college refers to attendance at college but without attaining a 4 year degree. Panel D: Reports share of population by current marital status. Married includes couples where spouse is present or absent. Divorced includes persons reporting being separated from spouse as well as divorced.

Data source: 1970 - 2010 Census and 2008-2012 pooled American Community Survey.

non-Hispanic White, reflecting the broader demographics of predominantly rural counties in the middle of the US. None of the small numerical differences in racial composition between RTW and non-RTW regions that we observe are statistically significant under the permutation tests that account for correlation of errors at the state-pair level.

Table 2 Panel C shows differences in education between RTW and non-RTW counties. We see very similar educational attainment in the two groups. RTW counties have slightly fewer high school graduates and correspondingly slightly more individuals with at least some college, but none of these are even close to statistically significant under the permutation tests. We note that educational investment could be an endogenous response to RTW laws, if such laws directly impact the availability of high-quality, low education unionized jobs or indirectly through the broader labor market conditions. In our case any impact on education is small, and these very small differences imply that educational differences are not driving our results. Furthermore an important question would be whether any differences in outcomes like education are due to selection of individuals living in RTW locations through differential migration. We find no evidence this is the case using the ACS data where we can observe state of birth.

Finally Table 2 Panel D shows differences in marital status between non-RTW and RTW counties. We see that people in RTW counties are 0.7% more likely to be married than in non-RTW counties. Like education, it is possible that marital stability could be an outcome of differences in the labor market induced by RTW laws. However, the differences are relatively small and so this channel, if it exists, has a minimal impact.

Across the 16 tests, only 3 are significant under the permutation tests at the standard 5% level of significance,[43] and all of the observed differences are small. We provide similar balance tables for the PUMAs in Table E.1. We find the same overall patterns to the county data, with no economically meaningful differences in any of our balance tests. Therefore we conclude that our border regions are comparable on observable characteristics.

6.2 Manufacturing employment

We begin our border discontinuity results by examining the impact of RTW laws on the extent of manufacturing employment in the local economy. Manufacturing is particularly exposed to the threat of unionization for two reasons. First, manufacturing has historically had higher levels of unionization

[43] Since we report one-sided p-values for the permutation tests, we would expect 10% of tests to meet such a criteria.

than many other industries. Second, the high level of capital investment in manufacturing operations exposes employers to higher potential hold-up costs from unionized employees (and potentially makes unionization more attractive to workers). Accordingly, manufacturing is a natural context in which to test the model's prediction that RTW will lead to higher employment in union-exposed industries. In his seminal study, Holmes (1998) finds that there is a discontinuous increase in manufacturing employment at RTW state borders.

Using our border pairs empirical strategy, we consider two outcomes. First, we confirm the finding of Holmes (1998), and find that there is a 3.23 percentage point increase in the manufacturing share of employment on the RTW side of the border as shown in Table 3. We confirm the significance of this result, finding a p-value of 0.0059 using our preferred non-policy borders placebo test, as shown in Figure 3. To ensure this result is not driven by reduced total employment in the denominator, we show a second specification measure manufacturing employment as a share of the 15+ population, and find a similar sharp increase in manufacturing at the RTW border. Both of these results are consistent with the predictions of the model.

Table 3: *Industry Employment Composition at RTW Borders*

	(1) Manufacturing Share	(2) Manufacturing (p.c.)
Right to Work	0.0323***	0.0158***
	(0.0052)	(0.0026)
County Pair FE	Yes	Yes
Year FE	Yes	Yes
Control Mean	0.1139	0.0493
Non-policy Borders p	0.0059	0.0108
Policy Borders p	0.0094	0.0123
County Observations	16876	16876

Standard errors clustered by county. * p<0.10, ** p<0.05, *** p<0.01. Non-policy Borders p reports the one-sided p-values from the non-policy borders permutation tests, while Policy Borders p reports the one-sided p-values from our alternate policy borders permutation test using our regression sample. Manufacturing employment is defined using NAICS codes 31-33.

Data source: 1990-2017 Quarterly Census of Employment and Wages.

Our 3.2 percentage point increase is equivalent to a 28% increase in RTW border counties relative to non-RTW border counties. This finding is slightly smaller than the finding of Holmes (1998) of an approximately one-third increase. We have not limited our sample to the eastern half of the United States where manufacturing is more concentrated as Holmes (1998) did, and we limit out sample to counties where we observe manufacturing employment consistently across years

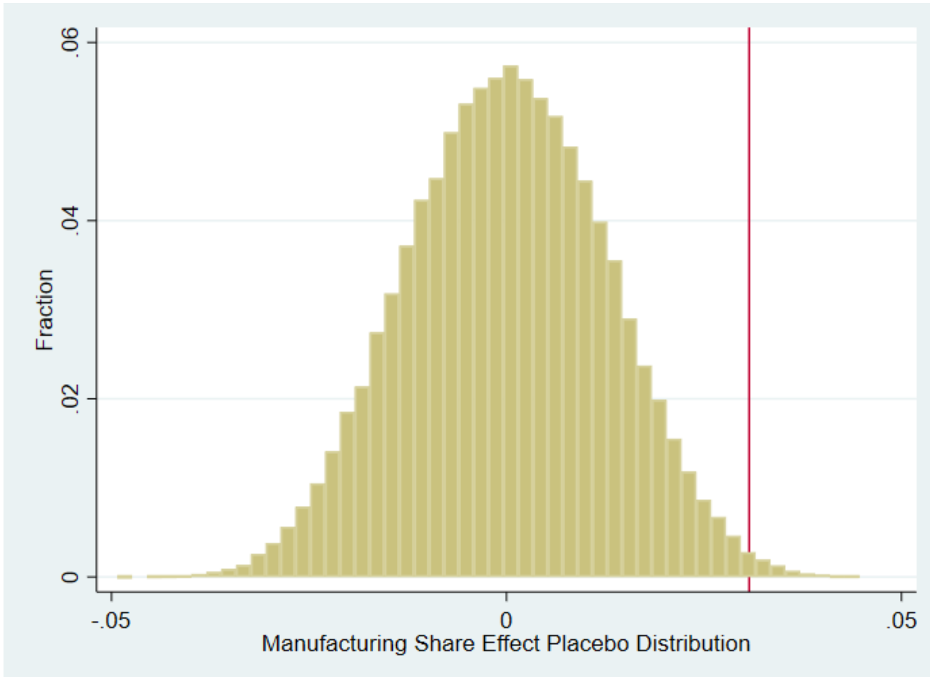

Figure 3: *Manufacturing Placebo Test*

Note: Figure presents estimated coefficient for county-border difference from placebo test, reassigning treatment status at the state-border pair level. The dependent variable is manufacturing share defined as NAICS codes 31-33. Regressions include county-pair and year fixed effects.
Data source: 1990-2017 Quarterly Census of Employment and Wages.

in the QCEW sample.[44] It is worth noting that the share of manufacturing employment has been declining nationally and our more recent sample reflects this decrease. However, the difference in manufacturing employment between RTW and non-RTW regions has been stable over recent decades, as shown in Figure 4.

6.3 Aggregate labor market outcomes

We found in the previous section that manufacturing employment is disproportionately clustered on the RTW side of the state border. In this section, we address whether this shift in manufacturing simply reflects a shift in employment mix or whether, consistent with our prediction, we observe aggregate differences in labor market outcomes.

There are two potential outcomes we could observe. First, we could observe the sorting of local

[44]Our results are robust to limiting our sample to the same counties as Holmes (1998) and including counties with missing data, and doing so increases the estimated coefficient's magnitude and decreases its standard error. It is possible that the majority of the difference from Holmes's simply reflects these differences in methodology.

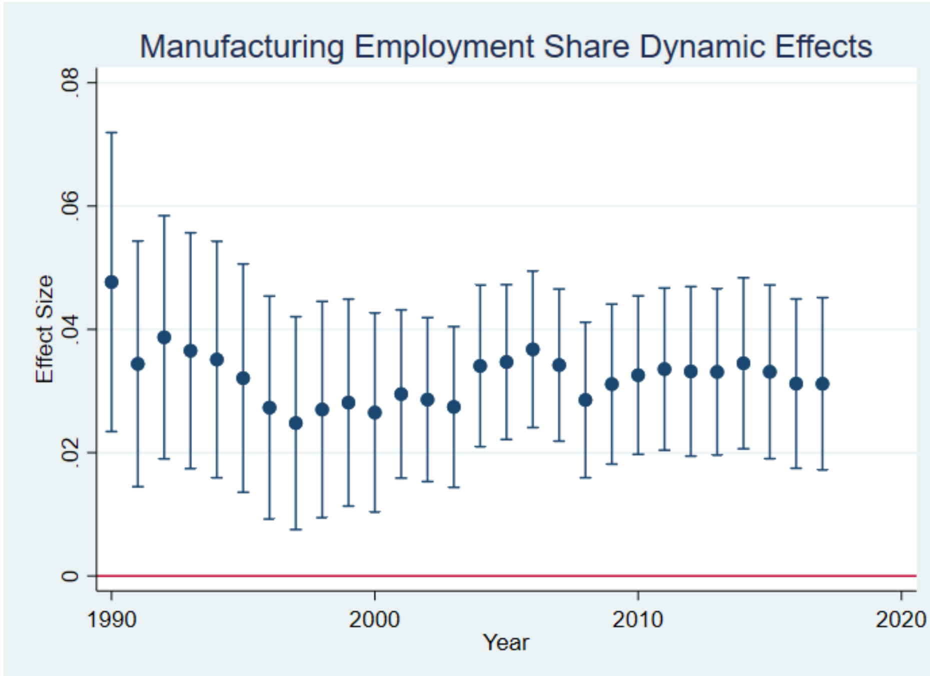

Figure 4: *Manufacturing RTW Border Effects by Year*

Note: Figure presents estimated coefficient for county-border difference in manufacturing share of employment by year. The dependent variable is manufacturing share defined as NAICS codes 31-33. Regressions include county-pair and year fixed effects.
Data source: 1990-2017 Quarterly Census of Employment and Wages.

employers across the state border based on the firm's perceived risk from unionization. Firms who perceive a higher risk of unionization would locate on the RTW side of the border, increasing demand for workers in these industries. This demand could then simply crowd-out firms with a low perceived risk of unionization, who would locate on the non-RTW side. Under these circumstances we would expect to observe a difference in employment composition, but similar aggregate outcomes.

Alternatively, the increased presence of manufacturing could have positive spillovers on the broader economy. For example, manufacturing firms require transportation and business support services, which could create additional local job opportunities. In turn, the increase in workers in these industries could then stimulate additional demand for non-tradable local services, resulting in a net increase in total employment.

Recall that our model of location choice differentiated between location of employment and location of residence. Accordingly, we begin by presenting results in Table 4 that separately analyze the prevalence of employment by location of workplace and residence, using data from the QCEW and LAU respectively. That is, data from the QCEW allow us to see if RTW regions are greater sources of employment, while data from the LAU provides insight on who any such additional jobs accrue to. We find evidence supporting the second hypothesis that RTW laws are associated with increased total employment. Column 1 shows that more jobs are located in RTW areas, with employment-to-population being 3.51 percentage points higher when measured by workplace location.[45] The estimate in Column 2, that employment-to-population (measured by residence location) is 1.58 percentage points higher in the RTW border counties, suggests about half of this increase in job availability accrues to residents of the RTW border county as higher employment. Intuitively, the gap between these two effect sizes should be explained by net commuting patterns. As expected, we find that there is net inter-state commuting into the RTW counties, and the estimated effect closely matches the discrepancy between the two employment results. In addition to being economically significant, all of these key differences are statistically significant under our preferred placebo test. Our results suggest that RTW areas yield more employment in aggregate. Many of these additional jobs are captured by residents, leading to higher employment rates, but some are obtained by residents of non-RTW areas who commute to obtain a better job.

Table 4: *Employment Differentials at RTW Borders*

	(1) Employment (Workplace)	(2) Employment (Residence)	(3) Commuting	(4) Commuting (Cross-state)
Right to Work	0.0351***	0.0158***	0.0247	0.0242**
	(0.0078)	(0.0042)	(0.0177)	(0.0101)
County Pair FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Control Mean	0.4276	0.5760	-0.0321	-0.0149
Non-policy Borders p	0.0251	0.0131	0.0152	0.0073
Policy Borders p	0.0199	0.1037	0.1630	0.0496
County Observations	21280	21280	760	760

Standard errors clustered by county. * p<0.10, ** p<0.05, *** p<0.01. All columns measure outcomes normalised by population aged 15+ in the county. Non-policy Borders p reports the one-sided p-values from the non-policy borders permutation tests, while Policy Borders p reports the one-sided p-values from our alternate policy borders permutation test using our regression sample.

Data source: 1990-2017 Quarterly Census of Employment and Wages; Local Area Unemployment Statistics, 1976-2017; 5-Year ACS Commuting Flows, 2011-15.

Next, we turn to a closer examination of the effects of greater job availability in RTW areas on

[45] While this effect is large, reassuringly it is small relative to the increased employment observed in manufacturing. 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.

residents of these locations. Table 5 compares employment outcomes for RTW and non-RTW areas. If RTW increases job availability and makes it easier to find employment, intuitively we would expect this to have particularly strong effects for people on the margins of the formal job sector who have worse employment prospects. To investigate this, we complement our primary county-based analysis with results using ACS microdata from our sample of PUMAs.

First, however, we compare aggregate outcomes. As above, the employment-to-population ratio is 1.58 percentage points higher in RTW states based on county data, and 2.54 percentage points higher using PUMAs. The labor force participation (LFP) rate is similarly 1.41 percentage points higher in counties and 2.00 percentage points higher in PUMAs. All of these estimates are highly statistically significant under our preferred placebo test. In addition to the effect on employment and participation rates, there is also an effect on the unemployment rate. The mean unemployment rate in RTW counties is 0.39 percentage points lower than in non-RTW counties.[46] The difference is larger in the PUMA sample at 1.11 percentage points.[47] The impact on the unemployment rate could be coming through multiple channels. First, we have seen that there is higher total employment in RTW counties, suggesting more jobs are available. Second, it is possible that RTW laws directly decrease the cost of hiring for firms, decreasing unemployment duration. However, we are unable to verify the mechanism with the data available, and this is an area for potential future research.

Next, in the final column of each the panels of Table 5, we turn to the question we posed above: are the effects of RTW on labor market outcomes particularly strong for people at high risk of non-employment? If unions reduce unemployment by setting above market-clearing wages, these are the individuals most likely to be negatively effected. We consider two specific population subgroups for insight. First, the past several decades have seen a marked decline in employment among prime-aged males, see Austin et al. (2018). Here, we see that long-term joblessness (defined as non-employment for at least 12 months) of prime-aged males is markedly lower in RTW locations. Second, people with disabilities often face greater barriers to holding formal employment. There is a substantial literature establishing a positive correlation between the national unemployment rate and SSDI insurance applications, for example see Autor (2015), with disability insurance being treated as a substitute for unemployment benefits. Therefore we may expect that if RTW counties have a

[46] The reported value includes Local Area Unemployment Statistics data from 1976-1989, which is not consistent with post-1990 data. Table E.2 compares the unemployment differential limiting to the post-1990 period, and finds an almost identical unemployment differential.

[47] The larger value in PUMAs may reflect our sample period of 2012-2017. Examining the time series of unemployment differentials in counties we find the largest differential between RTW and non-RTW counties occurs early in expansionary periods following recessions, which includes our ACS sample period.

Table 5: *Employment Differentials at RTW Borders*

Panel A: County	(1) Unemployment	(2) Employment to Population	(3) Participation	(4) Disability
Right to Work	-0.0039***	0.0158***	0.0141***	-0.0034***
	(0.0011)	(0.0042)	(0.0042)	(0.0007)
County Pair FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Control Mean	0.0661	0.5760	0.6123	0.0427
Non-policy Borders p	0.0723	0.0131	0.0201	0.0343
Policy Borders p	0.0199	0.1037	0.1283	0.0126
Observations	31868	21280	21280	6786

Panel B: PUMA	(1) Unemployment	(2) Employment to Population	(3) Participation	(4) Long Term Jobless (Prime-Aged Male)
Right to Work	-0.0111***	0.0254***	0.0200***	-0.0260***
	(0.0023)	(0.0062)	(0.0058)	(0.0052)
PUMA Pair FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Control Mean	0.0640	0.5626	0.6045	0.1388
Non-policy Borders p	0.0001	0.0000	0.0003	0.0001
Policy Borders p	0.0001	0.0032	0.0184	0.0010
Observations	1944	1944	1944	1944

* p<0.10, ** p<0.05, *** p<0.01

Regressions include county/PUMA border-pair and year fixed effects. Robust spatial standard errors reported.

Source: 1976-2017 Local Area Unemployment Statistics, US Census Bureau intercensal population estimates, and 2009-2018 SSA Disability Data, 2012-2017 American Community Survey.

lower unemployment rate and higher employment rate, they may also have a notably smaller share of SSDI recipients. Indeed, we find there is a 0.34 percentage point lower rate of SSDI receipt in RTW counties. Both of these effects are large in proportional terms; we find a 19% reduction in long-term male joblessness and an 8% reduction in SSDI receipt.

6.4 Labor compensation

The natural counterpart to analysing employment outcomes is to examine how RTW affects wages (and other measures of labor compensation). Recall that theory is not particularly informative regarding aggregate effects on compensation, since higher wages in unionized industries brought about by union wage setting are expected to be offset by negative spillovers on non-union workers.

This is further complicated if unions induce capital hold-up, as higher resulting capital utilization by firms in RTW areas will tend to drive up the marginal product of labor. Accordingly, we could find either that RTW increases, or as commonly argued by RTW opponents, decrease wages on average.

In Table 6 we find that average weekly wages are $27.97 higher for individuals employed in RTW counties overall, using data from the QCEW. Adding industry by year controls to account for differential composition reduces this difference by about a quarter. By comparison, we fail to detect any difference in manufacturing wages by RTW status. Given the potential for workers to commute across state borders, we would expect in equilibrium that any aggregate wage differentials would be relatively small. An important qualifier here is that an effect on mean wages can disguise more complicated patterns within the wage distribution, and in practice unions appear to care about the distribution of wages, not just the mean. For example, there is an extensive literature documenting that unions appear to compress the wage structure and dampen the relationship between skills and income (Stafford (1968), Rosen (1970), Johnson & Youmans (1971), Card (1996)). To study the distribution of wages we turn to the ACS microdata in our PUMA sample. We find that wages are higher at the bottom (10th percentile) and middle (median) of the distribution, but find no clear effect at the top (90th percentile). This is broadly consistent with the theoretical prediction of lower wages in the non-union sector in the non-RTW location due to it having to absorb surplus labor. It also is consistent with our employment results where the biggest effects appear to have occurred at the lower end of the distribution.

Several additional caveats here are necessary. First, labor compensation is broader than wages. Indeed, it is commonly claimed that unions have particularly large direct effects on non-wage compensation, such as generous health insurance and retirement savings programs. While data capturing the intensive margin of non-wage compensation is scarce, we can study the extensive margin of health insurance provision in people aged under 65.[48] We find no meaningful difference in the uninsured share by RTW status, irrespective of whether we look at all individuals under 65 or restrict the analysis to individuals who are ineligible for Medicaid under the Affordable Care Act because they have household income above 138% of the Federal Poverty Line. Second, labor effort and other compensating differentials affect welfare, and are thus also relevant for considering whether jobs in RTW or non-RTW areas are more attractive. Again turning to the additional data available in the ACS for our sample of border-PUMAs, we see that some of the raw difference in

[48]The SAHIE data excludes those 65 and older because there is almost universal coverage through Medicare.

wages can be explained by the additional 0.49 hours worked per week by employed individuals in RTW locations.

Table 6: *Compensation Differentials at RTW Borders*

Panel A: County					
	Mean Weekly Wage			Health Insurance	
	(1)	(2)	(3)	(4)	(5)
	All	Composition Adjusted	Manufacturing Workers	Uninsured	Uninsured (Non-Medicaid)
Right to Work	27.92***	20.30***	2.48	0.0031	0.0026
	(5.41)	(5.14)	(9.94)	(0.0026)	(0.0022)
County Pair FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	Yes	Yes
Industry x Year FE	No	Yes	No	No	No
Control Mean	521.36	519.80	644.98	0.1556	0.1244
Non-policy Borders *p*	0.0054	0.0085	0.2915	0.3299	0.3202
Policy Borders *p*	0.0054	0.0277	0.2074	0.3681	0.3787
Observations	20832	190778	13238	7600	7600

Panel B: PUMA					
	Full Time Annual Wage			Weekly Hours	
	(1)	(2)	(3)	(4)	(5)
	Median	P10	P90	Full Time	Total
Right to Work	1929.87**	761.32***	2291.76	0.2685***	0.4871***
	(752.86)	(229.34)	(2807.58)	(0.0795)	(0.1076)
PUMA Pair FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Control Mean	40695	18264	90461	43.8577	38.8943
Non-policy Borders *p*	0.0037	0.0009	0.0975	0.0007	0.0000
Policy Borders *p*	0.0139	0.0248	0.2158	0.0005	0.0029
Observations	1944	1944	1944	1944	1944

Standard errors clustered by county. * p<0.10, ** p<0.05, *** p<0.01. Non-policy Borders *p* reports the one-sided p-values from the non-policy borders permutation tests, while Policy Borders *p* reports the one-sided p-values from our alternate policy borders permutation test using our regression sample. 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.

Data source: 1990-2017 Quarterly Census of Employment and Wages, 2008-2017 Small Area Health Insurance Estimates (SAHIE), 2012-2017 American Community Survey.

We suggest caution in interpreting these results. Non-wage compensation and compensating differentials such as effort costs are both contextually important when discussing the effects of unions, and despite our efforts, hard to measure. Hence discerning a clear sign for the effect of RTW on aggregate labor compensation is difficult. However, we can be clearer about what we do not find. In particular, RTW opponents often employ cross-sectional analysis to claim that RTW substantially reduces wages and benefits like health insurance for non-unionized 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.

6.5 Migration and population growth

We have found differential labor market outcomes in border counties. Supposing these effects are real, we would expect net migration to RTW states as an endogenous response. If we see higher migration and population growth, this would be consistent with the RTW counties being more attractive for migrants and residents. Alternatively, if we saw net population loss from the RTW counties, we would be concerned that the higher employment rate reflects differential population selection rather than an improved labor market.

Table 7: *Migration and Population Growth at RTW Borders*

	Migration			
	(1)	(2)	(3)	(4)
	Inward	Outward	Net	Population Growth, 15+
Right to Work	0.0045***	0.0034***	0.0011**	0.0021**
	(0.0010)	(0.0009)	(0.0005)	(0.0006)
County Pair FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Control Mean	0.0642	0.0640	0.0002	0.0056
Non-policy Borders p	0.0329	0.0583	0.1705	0.0178
Policy Borders p	0.2462	0.0323	0.2768	0.0643
County Observations	19656	19656	19656	20520

Standard errors clustered by county. * p<0.10, ** p<0.05, *** p<0.01. Non-policy Borders p reports the one-sided p-values from the non-policy borders permutation tests, while Policy Borders p reports the one-sided p-values from our alternate policy borders permutation test using our regression sample.

Source: 1990-2016 IRS Migration Data, 1990-2017 US Census Bureau Intercensal Population Estimates.

We attempt to measure migration in two ways. Firstly, we make use of the migration flows data released by the IRS. Since this data is fairly noisy, we leverage intercensal estimates of population from the Census Bureau as a proxy for net migration. While adjacent RTW and non-RTW counties may see different birth and death rates, meaningful differences in population growth are strongly indicative of net migration flows. Table 7 displays our results. From the IRS data, it appears there is greater migration into (and out of) RTW counties. However, net migration into RTW counties is only

0.11 percentage points (per year) higher than into non-RTW counties, a difference which appears insignificant in the placebo tests. However, in the population estimates data from the Census Bureau, RTW is associated with a statistically significant 0.21 percentage point higher population growth rate per year.[49] The higher net flows into and out of RTW counties may reflect that these counties are overall more dynamic than their comparable non-RTW pairs.

While these two sets of results are suggestive evidence for RTW inducing net migration to RTW counties, the estimated magnitudes are modest and the evidence is relatively weak. One obvious possibility (which is indeed what our model predicts) is that RTW would induce a level shift in population (via migration), not a permanent change in the population growth rate. Of course in practice population adjustment does not occur overnight, but it is unlikely to occur forever either. Since the RTW borders that we study have been in place for many decades on average, it is plausible that population adjustments in response to RTW largely took place prior to the data window of the above analysis. Alternatively put, for many of our outcomes, modern differences in outcomes are sufficient to capture the long run effects of RTW laws, but for migration and population growth, historical data is more important.

Fortunately, unlike most of our measures of interest, an extensive historical time series of population data is available at the county level in the decennial censuses. While this data is only available every decade, this is sufficient for our purposes. Figure 5 presents the results of our difference-in-differences regressions using log population as the dependent variable, plotting the RTW coefficients (and pre-trends) by census year. We set 1940 as the base year, so the coefficients can be interpreted as the excess population growth in RTW counties between year t and 1940. The results are quite clear.[50] Prior to the passage of the Taft-Hartley Act, counties that subsequently obtained RTW status had similar population growth rates to their subsequently non-RTW neighbours. A slight negative pre-trend is possible, although almost exactly no difference in growth occured over the two decades prior to Taft-Hartley. Beginning almost immediately afterwards, the population of RTW counties begins to diverge upward from their non-RTW neighbours. Since the outcome variable is log population, the coefficients trace out the cumulative proportional effect on population over time. Between 1940 and 2010, we observe a 19.1 percent increase in population of RTW counties relative to their non-RTW neighbours.

[49]Note that even if the intercensal population estimates are error prone, they should be largely accurate in decennial census years, so average growth rates over several decades should be well measured.

[50]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.

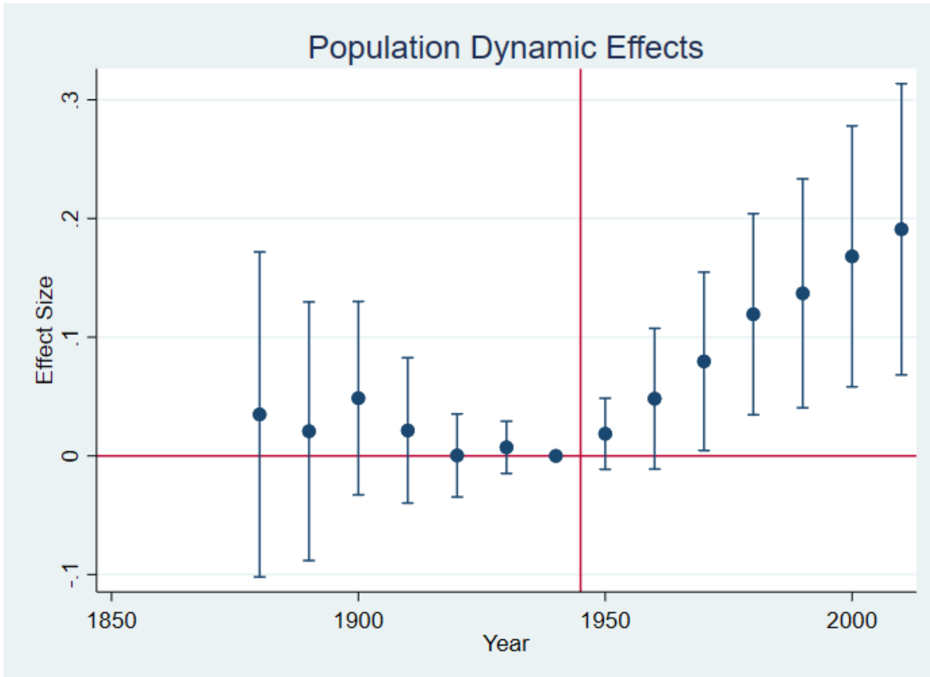

Figure 5: *Population RTW Border Effects by Year*

Note: Figure presents estimated coefficient for RTW status interacted with year, relative to a base year of 1940 (normalised to 0). Regressions include county-pair by year and county-pair-county fixed effects.
Data source: 1880-2010 US Census Bureau Decennial Censuses.

We find very similar results when estimating the event-study specification where the effects of RTW are estimated by years since RTW adoption for each respective county. See Appendix Figure F.3 for details.

6.6 Poverty and intergenerational mobility

We have found that RTW border counties have a higher share of manufacturing employment, improved labor market outcomes including higher employment and lower unemployment rates, and have experienced more rapid population growth, presumably due to higher in-migration. We now turn to the question of whether the combination of these labor market differentials and weaker union presence improve or hinder social outcomes such as poverty, and opportunities for subsequent generations, as reflected in observed intergenerational mobility.

We begin by examining the relative poverty rates between RTW and non-RTW border counties in Table 8. Consistent with the improved employment outcomes and slightly higher wage levels found in RTW counties, the overall poverty rate is 1.41 percentage points lower in RTW counties.[51] The differences are even larger when looking at poverty rates 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 between 5 and 17 years old. Relatively speaking, these are reductions in poverty of approximately 9-12 percent. Again, these amount to large improvements in outcomes for those at the bottom of the socioeconomic distribution.

Table 8: *Poverty Differentials at RTW Borders*

	(1) Poverty Rate	(2) Childhood Poverty	(3) Poverty, Ages 5-17
Right to Work	-0.0141***	-0.0229***	-0.0243***
	(0.0027)	(0.0036)	(0.0034)
County Pair FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Control Mean	0.1577	0.2210	0.2037
Non-policy Borders *p*	0.0014	0.0004	0.0001
Policy Borders *p*	0.0297	0.0077	0.0037
County Observations	15960	15960	15960

Standard errors clustered by county. * p<0.10, ** p<0.05, *** p<0.01. Non-policy Borders *p* reports the one-sided p-values from the non-policy borders permutation tests, while Policy Borders *p* reports the one-sided p-values from our alternate policy borders permutation test using our regression sample.
Data source: 1995-2017 Small Area Income and Poverty Estimates (SAIPE).

Next, we examine how the relative intergenerational mobility rates vary by RTW status, using data from Opportunity Insights. Panel A of Table 9 considers the rate of mobility of children into the top quintile of family income of children born in the same year, as measured during adulthood in 2014-15. Column (1) shows the unweighted mean for all children in the county, and finds a 1.68 percentage point increase in the probability of being in the top quintile, or 8.6% higher than non-RTW border counties. The result is strongly statistically significant with a placebo test p-value of 0.0002 as shown in Figure F.4.

Given we have previously established that RTW counties have different employment outcomes and higher average wages, we may be concerned that this result is driven by the relative income of parents. Columns (2) through (4) report the rate of mobility into the top income quintile conditional on the national percentile rank of household income of parents relative to other parents with children in the same cohort, averaged over 1994-95 and 1998-2000. Importantly we find the effect is

[51] The SAIPE data is not true raw data collected at the county level, but rather model-based estimates of county level poverty using information about broader geographies. It is thus in essence synthetic. While this is perhaps undesirable, there is no obvious reason for this to bias the results, and the placebo tests in particular are reassuring. Additionally, county-level poverty rates calculated from the decennial censuses from 1970-2010 yield a very similar estimate (-1.44 percentage points).

Table 9: *Average Mobility at RTW Borders*

	(1) Unweighted Mean	(2) 25th Percentile Parental Income	(3) 50th Percentile Parental Income	(4) 75th Percentile Parental Income
Panel A: 20th percentile				
Right to Work	0.0168***	0.0166***	0.0114***	0.0037
	(0.0047)	(0.0036)	(0.0038)	(0.0057)
County Pair FE	Yes	Yes	Yes	Yes
Control Mean	0.1961	0.1190	0.1878	0.2880
Non-policy Borders p	0.0002	0.0003	0.0045	0.2258
Policy Borders p	0.0472	0.0108	0.0837	0.3586
Observations	756	756	756	756
Panel B: Mean rank				
Right to Work	0.0151***	0.0131***	0.0098***	0.0067***
	(0.0034)	(0.0031)	(0.0028)	(0.0032)
County Pair FE	Yes	Yes	Yes	Yes
Control Mean	0.5217	0.4515	0.5288	0.6027
Non-policy Borders p	0.0001	0.0008	0.0021	0.0219
Policy Borders p	0.0158	0.0161	0.0611	0.1573
Observations	756	756	756	756
Panel C: Causal impact				
Right to Work		0.0981***		0.0361*
		(0.0312)		(0.0195)
County Pair FE		Yes		Yes
Control Mean		0.4249		0.1559
Non-policy Borders p		0.0170		0.0224
Policy Borders p		0.0472		0.0416
Observations		610		610

Panel D: Education

	(1) High School	(2) Community College	(3) 4 year College	(4) Graduate School
Right to Work	0.0085***	0.0036	0.0015	-0.0037
	(0.0032)	(0.0075)	(0.0070)	(0.0041)
County Pair FE	Yes	Yes	Yes	Yes
Control Mean	0.8664	0.4474	0.3234	0.1080
Non-policy Borders p	0.0931	0.3333	0.4516	0.3462
Policy Borders p	0.1072	0.3863	0.4584	0.3773
Observations	734	694	694	666

Standard errors clustered by count. * p<0.10, ** p<0.05, *** p<0.01. Non-policy Borders p reports the one-sided p-values from the non-policy borders permutation tests, while Policy Borders p reports the one-sided p-values from our alternate policy borders permutation test using our regression sample. Panel A shows the share of children in the top 20 percentiles of national family income. Panel B shows the mean percentile rank of children in the national family income distribution. Panel C reports the causal effect of one additional year of exposure to a county during childhood on percentile rank in national family income distribution at age 26. Panel D reports the fraction of children obtaining at least the stated level of education at age 30+ based on ACS responses.

Data source: Opportunity Insights, Chetty et al. (2018), Chetty and Hendren (2018).

concentrated amongst children from lower parental income levels. Children whose parents were in the 25th percentile experienced a 1.66 percentage point increase, or 13.9% higher than non-RTW border counties. The result is again very significant with a placebo test p-value of 0.00035 as shown in Figure F.5. By contrast, the estimated effect on children born to parents at the 75th percentile is small and insignificant.

Panel B of Table 9 reports the mean national percentile rank of children based on their household income relative to other children born in the same year, again measured in 2014-15. Consistent with our findings above, the unweighted mean is 1.51 percentage points higher for those raised in RTW areas, with a placebo test p-value of 0.0001 shown in Figure F.6. For children whose parents were in the 25th percentile of parental income, the RTW border effect is 1.31 percentage points. As shown in Figure F.7 this is a significant result with a placebo test p-value of 0.0008. Again, the effect sizes are weaker higher up in the parental income distribution.[52]

Using data from Chetty and Hendren (2018) we can examine whether the observed differences in intergenerational mobility reflect causal exposure effects or not. Table 9 Panel C reports the causal effect of an additional year of exposure to the county during childhood on the percentile rank of children in the national income distribution at age 26. The results are consistent with our findings from Panel A and B, and we find the causal impact is positive for RTW counties and higher for children in the 25th percentile of parental income. For this group, on average the causal effect of spending an additional year of childhood in the RTW county rather than the adjacent non-RTW border county is 0.0981 percentiles. The causal estimates are less precisely estimated as they rely on observing migrant children, but the result is still significant with a placebo test p-value of 0.0170.

One potential driver of upward mobility could be differential educational investment by children on the RTW side of the border. Table 9 Panel D reports the results of educational attainment for children at age 30. We see children living on the RTW side of the border are slightly more likely to graduate high school, but otherwise the educational attainment of the two groups is comparable. Therefore it does not seem that educational attainment alone is driving the variation in upward mobility rates. Unfortunately we are not able to observe in our data whether differences are due to local opportunities or differences in migration of children, but this would be an area for future study.

Throughout these analyzes, the effects of RTW on upward mobility consistently appear strongest

[52]While smaller effect magnitudes higher up in the income distribution are reassuring that our results are not spurious, they are not placebo tests per se. An individual who grows up at the top of the income distribution may still benefit as an adult from RTW because they have some (lower) chance of ending up at the lower end of the socioeconomic distribution, where the benefits of RTW appear strongest.

at the lower end of the parental income distribution. This is consistent with both the pattern of employment effects we observe and economic intuition; if unions drive wages higher and produce job queuing, lower-skilled workers are likely to bear any adverse employment effects. Accordingly, the increased employment that RTW appears to produce is likely concentrated among workers who under the counterfactual would be relatively marginally attached to the workforce.

7 Are differences due to RTW laws or other state policies?

In Section 6 we established that RTW border county regions vary substantially from neighboring non-RTW border counties across a range of dimensions including a higher manufacturing share, higher employment and lower unemployment rates, higher migration and population growth, and lower poverty and improved intergenerational mobility particularly for children at the lower end of the parental income distribution. Previously, Section 6.1.1 documented that these regions had similar economic outcomes prior to the passage of RTW laws and Section 6.1.2 showed that RTW and non-RTW areas appear similar on a range of observable demographic characteristics. While reassuring, an obvious concern is that these counties may differ because of other economic policies that discontinuously change at state borders. Indeed, many important labor market policies are set at the state level and therefore discontinuously change at the state border. This section attempts to address this threat to identification and identify whether, and to what extent, the observed border differences are caused by RTW laws or some other set of state policies that are correlated with RTW. Using a double machine learning approach and a wide range of other state policies, we demonstrate that the RTW effects we documented are largely unaffected by controlling for a battery of other policies.

The impact of other state policies is particularly a concern under the hypothesis that RTW laws merely reflect the preexisting preferences of voters. If a RTW policy border reflects differences in political preferences, this could manifest in a range of other policy differences that would be closely correlated with RTW laws. These other policy differences could then explain the observed differences between border counties.

Our goal is therefore to test whether some combination of other state policies can explain the differences we observe. The challenge we face is that there are a large number of potential policies that differ between states, and we do not know which policies are relevant or how such policies interact. Therefore we have a high-dimensional vector of potential controls to consider.

A recent literature has developed methods for identifying the causal effect of treatment in the presence a high-dimensional vector of potential controls. We follow the approach outlined in Belloni et al. (2013), Belloni et al. (2014) and Belloni et al. (2016) and utilize a post-double selection Least Absolute Shrinkage and Selection Operator (LASSO) approach to estimate a sparse high-dimensional linear model.

The post-double selection LASSO model assumes that the true model is:

$$y_{it} = \beta_R RTW_{it} + f(Z_{it}) + \gamma_{i,i'} + \delta_t + \varepsilon_{it} \tag{15}$$

where RTW_{it} is our treatment indicator, and the vector Z_{it} is set of controls that could include a range of other state policies. The functional form $f(\cdot)$ is unknown, but we assume that it can be reasonably approximated by a linear function of a set of variables X_{it} which could include transformations or interactions of the controls Z_{it} with a small remainder term r_{it}.

$$f(Z_{it}) = X_{it}\pi_a + r_{it} \tag{16}$$

Our key assumption is that once we condition on the correct set of controls that treatment is exogenous, so we have that:

$$\begin{aligned} &y_{it} = \beta_R RTW_{it} + X_{it}\pi_a + \gamma_{i,i'} + \delta_t + r_{it} + \varepsilon_{it} \\ &\mathbb{E}[\varepsilon_{it}|RTW_i, X_{it}, r_{it}, \gamma_{i,i'}, \delta_t] = 0 \end{aligned} \tag{17}$$

Using the approach above, we again assume a linear approximation of the relationship between treatment and controls with some small approximation error, and so we can form a two equation system:

$$\begin{aligned} &y_{it} = X_{it}(\beta\pi_b + \pi_a) + \gamma_{i,i'} + \delta_t + r_{it} + \varepsilon_{it} \\ &RTW_{it} = X_{it}\pi_b + \gamma_{i,i'} + \delta_t + q_{it} + \nu_{it} \\ &\mathbb{E}[\varepsilon_{it}|X_{it}, r_{it}, \gamma_{i,i'}, \delta_t] = 0 \end{aligned} \tag{18}$$

To find the appropriate set of controls x_{it} we perform LASSO selection on both of the equations above, keeping the set of controls that are significant in either equation. The LASSO selection process follows the version given in Belloni et al. (2014):

$$\hat{\beta} = \arg\min_{b} \sum_{i=1}^{N} (y_i - \sum_{j=1}^{K} x_{i,j} b_j)^2 + \lambda \sum_{j=1}^{K} \gamma_j |b_j| \tag{19}$$

where λ is an overall penalty term, and γ_j are variable specific penalty loadings that rescale variables. As discussed in Belloni et al. (2016) we allow for cluster-robust standard errors at the state-border level.[53]

As described in Section 5.3 we assemble data on 13 different state policy variables, including the state minimum wage, state unemployment insurance, state government fiscal policies, business conditions, and labor market regulation through licensure and certification requirements. We run the post-double selection LASSO models allowing for these variables to enter the model as an arbitrary second-order polynomial using the code provided by Ahrens et al. (2019). We estimate the model on the full-set of state county-border pairs in the continental United States. This enables us to better estimate the impact of other state policies independently of RTW laws.

Table 10 Panel A reports the result for the share of manufacturing employment reported in Table 3. We find our earlier result is robust to controlling for other state policies, indeed the point estimate is slightly larger when we estimate the model on data from all states with the policy controls.

Table 10 Panel B reports the result for employment differentials at the border. Our point estimates are almost identical to Table 5 although the significance of some of the variables is reduced when including a broad range of controls. If other state policies explained the employment differentials, we would expect the estimated treatment effect of RTW to be close to zero. Alternatively if there was a genuine treatment effect from RTW, we would expect to observe similar point estimates but potentially larger standard errors when we add additional controls. Therefore we conclude that other state policies do not explain the employment differentials observed at RTW borders.

Finally Table 10 Panel C reports the results for poverty rates comparable to Table 8. Again we find the point estimates are almost identical to the results without the controls and statistically significant. Again this is consistent with the difference in poverty rates at RTW borders not being meaningfully due to other state policies.

[53] As we include all continental US states, there are 109 state-borders excluding wide water borders.

Table 10: *RTW Border Differences including State Controls*

Panel A: Industry share	(1) Manufacturing Share
Right to Work	0.0325***
	(0.0091)
County Pair FE	Yes
Year FE	Yes
Control Mean	0.1490
Observations	59855

Panel B: Employment	(1) Unemployment	(2) Employment (Workplace)	(3) Employment (Residence)	(4) Participation
Right to Work	-0.0035*	0.0394***	0.0151	0.0137
	(0.0020)	(0.0137)	(0.0096)	(0.0097)
County Pair FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Control Mean	0.0704	0.4445	0.5786	0.6175
Observations	85275	69957	69941	69941

Panel C: Poverty	(1) Poverty Rate	(2) Childhood Poverty	(3) Poverty Ages 5-17
Right to Work	-0.0131**	-0.0221***	-0.0238***
	(0.0060)	(0.0083)	(0.0078)
County Pair FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Control Mean	0.1375	0.1947	0.1779
Observations	51832	51832	51832

All Panels: The sample includes all county-border pairs across state lines, including states borders where RTW policy is identical. Regressions include county-pair and year fixed effects. Standard errors clustered by state-border pair. * p<0.10, ** p<0.05, *** p<0.01. Panel A: Manufacturing employment share defined as NAICS codes 31-33.

Source: 1990-2017 Quarterly Census of Employment and Wages; 1995-2017 Small Area Income and Poverty Estimates (SAIPE); 1976-2017 Local Area Unemployment Statistics; US Census Bureau intercensal population estimates.

8 The impact of local substitution

In the previous sections, we have established that there are economically meaningful differences between RTW and non-RTW border counties, that these differences did not exist before the introduction of RTW laws, and that these differences cannot be explained by other combinations of state policies. We finally turn our attention to whether these differences appear to be solely due to the impact of local substitution between adjacent counties, or whether these appear to be systematic differences away from the border.

To address this, we examine county-pair differences between adjacent counties away from the policy border. We consider differences up to two counties away from the border, providing us with a set of 5 borders to consider. For this section only, we limit our analysis to the eastern United States as illustrated in Figure 6 given the large size of counties in the western United States would render comparisons two counties away from the state border meaningless (indeed in some cases this would take us across another state border or into the ocean).

As in our specification above, we allow for county-pair fixed effects and estimate a coefficient for the average difference between segments. We label border segments such that segments 1-3 are non-RTW, segments 4-6 are RTW, and the state border lies between the counties in segments 3 and 4. All results are normalized relative to border segment 3 (the non-RTW county at the state border).

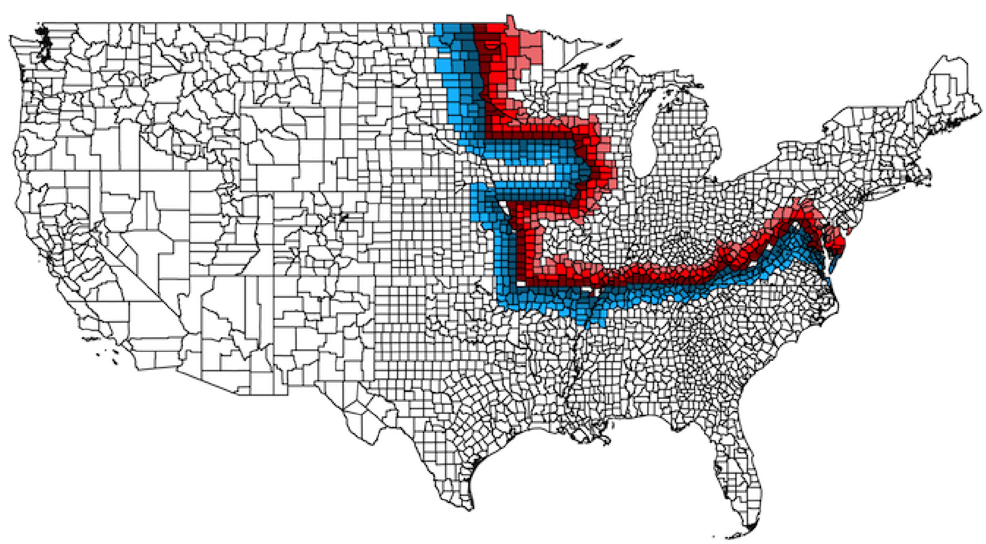

Figure 6: *RTW Border Adjacent Counties*

There are four archetypal patterns of results that are of interest to us. We describe each of these patterns individually even though they are not mutually exclusive (and indeed, we expect to see combinations of them in reality). The first pattern is that associated with a discontinuous policy change that directly causes a change in outcomes without spillovers or substitution. In the absence of other trends, we would expect similar point estimates for segments 1-3, a discontinuous jump at the policy border between segments 3 and 4, and then similar point estimates for segments 4-6. If the policy directly impacted outcomes in counties due to changes in agent behavior, then these differences would persist as we move away from a region of policy discontinuity.

The second archetypal pattern of interest would be associated with a policy that purely causes a local substitution effect but does not impact outcomes away from the border. In the absence of other trends between counties, this pattern is characterized by similar coefficient estimates away from the border (i.e. segments 1 and 6 are comparable) while there could be a discontinuity at the policy border between segments 3 and 4. In this case, the policy does cause local behavioral change between agents otherwise indifferent between two adjacent locations but does not induce changes in locations elsewhere. While such a finding would not literally imply RTW has no effect, it would drastically reduce the importance of the results. If policy effects occur only locally due to local sorting, there would be few implications for the vast majority of the population who reside away from state borders.

The final two archetypal patterns of results would be consistent with no policy impact. The third pattern is characterized by a general linear trend along all border segments, without any discontinuity at the border. This result would be consistent with some other factor, unrelated to the policy border discontinuity, generating differences in outcomes as we move across the country. In this case, by not examining neighboring counties that do not experience a policy discontinuity, we could erroneously conclude the change is due to the policy discontinuity rather than some other geographically varying unobservable characteristic. The fourth pattern is characterized by large, unpredictable changes between all border segments of a similar magnitude to the difference at the policy border. In this case the observed differences may simply be due to random variation between counties rather than the policy discontinuity.[54]

While we have described these four archetypal patterns separately for expositional clarity, in reality we could see multiple patterns in any given set of border segments. For example, we could observe a large policy driven discontinuity while simultaneously observing some general trend along counties due to some other geographically varying unobservable characteristic. In this case, we would see that counties far from the border lie on a similar trend line once we vertically shift for the

[54]This pattern is inconsistent with and thus should be precluded by our standard errors and permutation tests, but we include this possibility for completeness. Further, the county-pair differences between adjacent counties away from the policy border provide an additional robustness check of our results.

policy discontinuity. Alternatively, if the change at the border is purely driven by local substitution, the points away from the border will lie on the same trend without applying a vertical shift. As an additional case, we note that we could observe both a general policy effect and an additional effect from local substitution between border counties.

In Figure 7 we observe the difference in manufacturing share as we move away from the policy border.[55] Similar to the results in Holmes (1998), we find evidence for both a general policy impact on manufacturing and local substitution at the policy border. This result is consistent with manufacturers choosing their location due to a variety of geographic factors including access to suppliers, transportation infrastructure, and the location of their customers. At the policy border these other factors are likely to be similar, so we would expect to see manufacturers disproportionately locating on the RTW side of the border if they have a preference for RTW policies. However, as we move farther away from the border, other variables become important and therefore the difference in manufacturing shares is somewhat smaller.

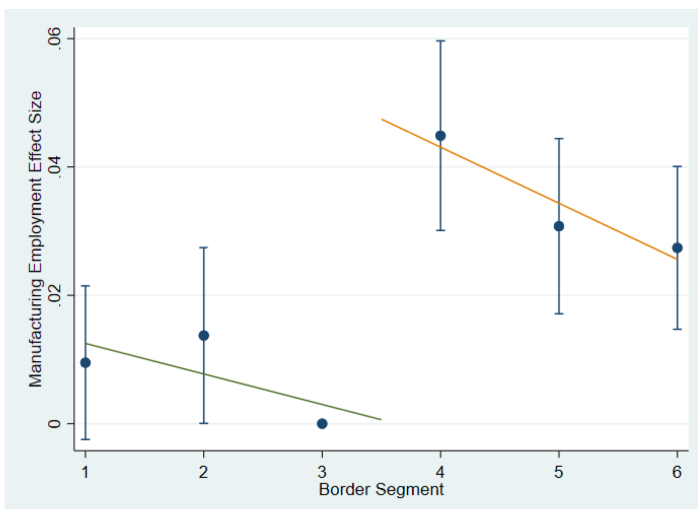

Figure 7: *Manufacturing County Border Differences*

Note: Figure presents estimated average county-difference coefficients and 95% confidence intervals for county border segment waves. The dependent variable is manufacturing share defined as NAICS codes 31-33. The state-border is between segment 3 and 4. Segments 1-3 represent counties in non-RTW state and segments 4-6 represent counties in RTW state. Coefficients normalized such that the non-RTW state border county (segment 3) is zero. Regressions include county-pair and year fixed effects.
Data source: 1990-2017 Quarterly Census of Employment and Wages.

Comparing segments 1 and 6, adjusting for composition by inclusion of county pair FE, the

[55] Notably, manufacturing in the US is heavily concentrated in the eastern half of the country, particularly in the north, and therefore there is a substantial negative trend moving south and west towards RTW states.

manufacturing employment rate remains 1.65 percentage points (s.e. 1.23 percentage points) higher on the RTW side, albeit insignificantly so. Nonetheless, it suggests the causal effect of RTW is sufficiently large to fully offset the negative geographic trend over this window, where a one-county movement towards the RTW side is associated with a 0.75 percentage point (s.e. 0.29 percentage points) decline in manufacturing employment share. This suggests there is still a meaningful differential in manufacturing employment away from the border, providing evidence that RTW laws are not simply inducing local substitution between locations.

Figure 8 shows the same results for the employment-to-population rate (measured by location of residence). Here we see results consistent with a general increase in the employment rate associated with RTW policy. In this case, the difference away from the border is similar to the observed discontinuity at the policy border. This suggests that RTW laws may have a broader impact on labor markets beyond purely local impacts at the policy border.

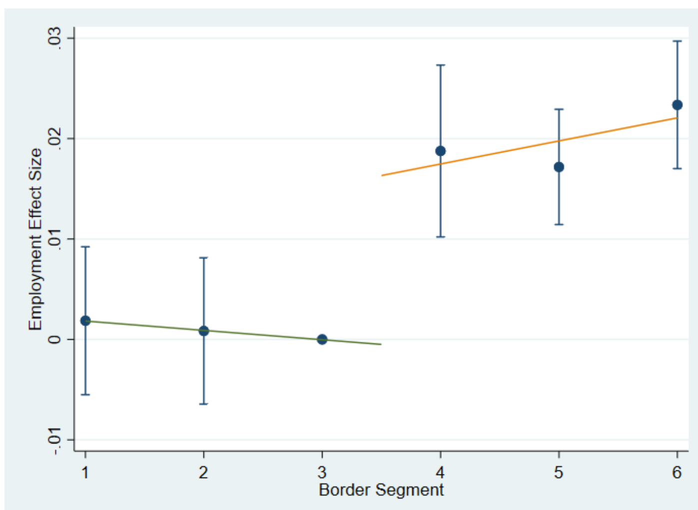

Figure 8: *Employment Rate County Border Differences*

Note: Figure presents estimated average county-difference coefficients and 95% confidence intervals for county border segment waves. The dependent variable is EPOP rate. The state-border is between segment 3 and 4. The state-border is between segment 3 and 4. Segments 1-3 represent counties in non-RTW state and segments 4-6 represent counties in RTW state. Coefficients normalized such that the non-RTW state border county (segment 3) is zero. Regressions include county-pair and year fixed effects.
Data source: 1990-2017 Local Area Unemployment Statistics and US Census Bureau intercensal population estimates.

We next consider differences in poverty levels in Figure 9. Here we again see evidence of a large policy differential at the border associated with RTW laws. There appear to be possible trends either side of the border, which suggests that perhaps some other unobservable leads to higher poverty rates in counties near the state borders relative to counties farther away. This does not change our conclusion that there is a real and substantial discontinuity in poverty rates at the RTW border.

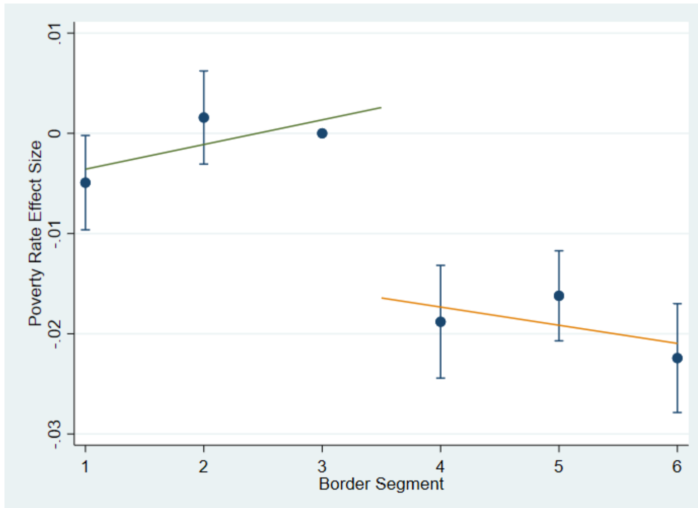

Figure 9: *Poverty County Border Differences*

Note: Figure presents estimated average county-difference coefficients and 95% confidence intervals for county border segment waves. The dependent variable is the proportion of the population below the federal poverty line. The state-border is between segment 3 and 4. Segments 1-3 represent counties in non-RTW state and segments 4-6 represent counties in RTW state. Coefficients normalized such that the non-RTW state border county (segment 3) is zero. Regressions include county-pair and year fixed effects.
Data source: 1995-2017 Small Area Income and Poverty Estimates (SAIPE).

Finally we consider differences in the rate of upward mobility. In Figure 10 we show the results for the impact on the average percentile rank of children who had parental incomes in the 25th percentile of the national income distribution. Similar to the employment rate results we find that there is a clear discontinuity at the policy border, with very similar point estimates as we move away from the border on either side. This is consistent with RTW policy having a general impact on outcomes without any local spillovers or substitution occurring at the border.

In total, our results are consistent with a causal impact of RTW laws. While there is some evidence that the magnitude of effect for manufacturing share could be partially driven by local substitution of firms between neighboring locations, the impact on the aggregate labor market and intergenerational mobility appears consistent with a general causal impact of RTW laws. However, as we described in Section 4.1 our identification strategy is limited to the local impact of RTW laws. We are not able to extrapolate to the general equilibrium impact of RTW laws farther from the state border, although this evidence is suggestive that these laws do not have a purely local impact.

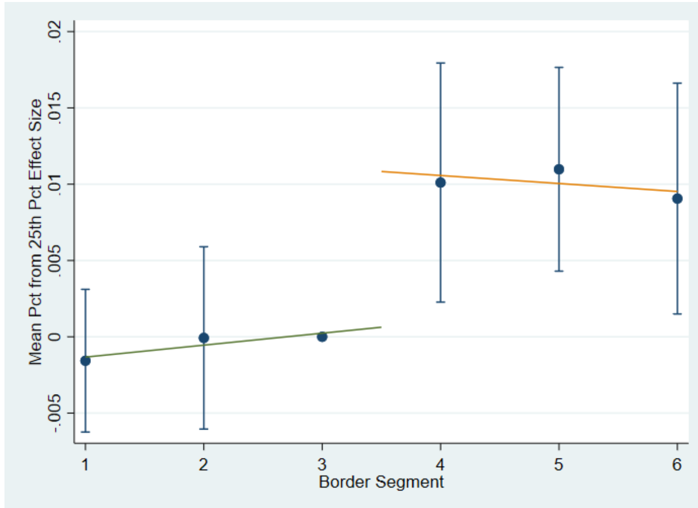

Figure 10: *Mobility of 25th Percentile County Border Differences*

Note: Figure presents estimated average county-difference coefficients and 95% confidence intervals for county border segment waves. The dependent variable is the mean family income rank of children with household parental income at the 25th percentile. The state-border is between segment 3 and 4. Segments 1-3 represent counties in non-RTW state and segments 4-6 represent counties in RTW state. Coefficients normalized such that the non-RTW state border county (segment 3) is zero. Regressions include county-pair fixed effects.
Data source: Opportunity Insights, Chetty et al. (2018).

9 Conclusion

This paper has examined the impact of RTW laws on a range of outcomes including industry location, local labor market conditions, and intergenerational mobility. We document several economically meaningful differences in economic outcomes between neighbouring RTW and non-RTW locations. These differences do not appear to have existed prior to the passage of RTW laws, are not explained by a extensive battery of other state economic policies, and are not purely local in nature.

Specifically, using a county border-pair discontinuity, we find that RTW laws are associated with a 3.2 percentage point increase in the share of manufacturing employment. The impact of RTW laws extend beyond industry mix, and we find that RTW counts have 1.6 percentage points higher employment-to-population, 0.39 percentage points lower unemployment, and 0.34 percentage point lower level of disability receipt. We also find some evidence for higher average wages in RTW locations, although some of this may be driven by higher average hours worked.

These labor market differences in turn yield greater migration to and population growth in RTW areas. There is more total migration into and out of RTW counties, suggesting these may be more dynamic locations with annual net migration 0.11 percentage points higher over recent decades.

Population growth is similarly 0.21 percentage points higher. Evidence on induced migration is particularly strong in our difference-in-differences estimates that uses data prior to the existence of RTW laws, with a notable increase in population growth following the passage of RTW laws.

Finally we examine the impact of RTW laws on poverty and intergenerational mobility. We find that RTW laws are also associated with lower poverty and greater upward mobility, with a 1.7 percentage point increase in the probability of ending up in the top quintile as adults for children who grow up at the 25th percentile of the income distribution. This is a significant difference, and is also reflected in causal exposure estimates, where RTW counties have a 0.10 percentile point higher annual impact than neighboring non-RTW counties per year of additional childhood exposure.

While this paper is unable to determine the general equilibrium effects of RTW policy, our estimated border effects show that RTW laws have a significant impact on local economic outcomes. Future research could focus on the degree to which these local impacts, particularly on intergenerational mobility, extend beyond border regions.

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A Key Model Proofs

A.1 The Monopoly Union Model

Optimal Wage Choice - Local

Proposition 1. *Suppose the union covering j, U_j, sets the wage $w_{uj}(K_j)$ firm j must pay, after observing the firm's capital investment decision. Then, to maximise extraction of rent, wages at unionized firms will be set as a markup over the non-union wage w_n, where the markup is increasing in the capital share of income;* $w_{uj} = \dfrac{w_n}{1-\alpha}$.

Proof. Given the timing structure of the monopoly union model, we proceed by backward induction. Recall that the firm has Cobb-Douglas production, so output is concave in each input, and homogeneous of degree 1 in (K, L).

Given a union set wage w_{uj} and non-reversible capital investment K_j, firm j solves

$$\max_{L_j} \Pi_j = p_g F(K_j, L_j) - w_{uj} L_j \tag{20}$$

The first order condition for optimal labor choice yields

$$L_j^* = L_j : F_L(K_j, L_j) = \frac{w_{uj}}{p_g} \tag{21}$$

Substituting in for Cobb-Douglas production, this yields

$$L = \left(\frac{p_g(1-a)}{w_u}\right)^{\frac{1}{\alpha}} K$$

Knowing this, the union chooses w_{uj} to maximise rents earned by its workers. Substituting for w_{uj} in the union wage setting problem, this is analogous to letting the union choose L_j while requiring that w_{uj} be determined by the firms demand curve.

$$\max_{w_{uj}} R_j = (w_{uj} - w_n).L_j(w_{uj})$$

$$= \max_{L_j} \left(p_g F_L(K_j, L_j) - w_n\right) L_j$$

Differentiating with respect to L_j and rearranging, this yields

$$w_{uj} = w_n - p_g F_{LL}(K_j, L_j) L_j(w_{uj}, K_j) \tag{22}$$

Note that since $F_{LL} < 0$, the union wage always exceeds the non-union wage $w_{uj} > w_n$. The intuition for this is simple: having irreversibly sunk its capital investment, the firm will still choose to hire some labor and produce some output even at relatively high union-set wages. Thus, the union is able to extract positive rent by raising wages above the competitive market price. Of course, in equilibrium, firms are not 'surprised' by the union wage choice, and thus p_g (and the aggregate quantity of output) are such that firms earn zero profits given these union-set wages. (In other words, unions in equilibrium do not take profits from firms, rather they raise prices for consumers and assuming downward sloping product demand, decrease aggregate employment in the industry).

Substituting in for Cobb-Douglas production, this yields

$$w_{uj} = \frac{w_n}{1-\alpha} \tag{23}$$

Substituting the union wage into the firm's labor demand function gives

$$\Rightarrow \frac{L_j}{K_j} = \frac{(1-\alpha)^2}{\alpha} \frac{r}{w_n} \tag{24}$$

The union wage is increasing in α and L^* (normalising K) is falling in α. □

This fits with intuition. Unions set wages once capital is sunk, and because capital is sunk, firms will still employ labor even at very elevated wages. The more important capital is in production, ceteris paribus the higher the marginal product of labor for any given L (and K). The union can accordingly extract more rents.

Capital Investment and Hold-Up

In turn, the firm chooses capital investment, anticipating $w_{uj}(K_j)$ and $L_j(w_{uj}(K_j), K_j)$. The firm solves

$$\max_{K_j, L_j} p_g F(K_j, L_j) - w_{uj}(K_j).L_j - r.K_j \tag{25}$$

The yields first order conditions in K_j and L_j. The latter is as above in Equation 21. The first order condition for capital investment is

$$p_g F_K(K_j, L_j) - w'_{uj}(K_j)L_j - r = 0 \tag{26}$$

This illustrates how union wage setting may potentially create a *hold up* problem that deters firms from investing in capital. Suppose that a union's optimal wage $w_{uj}(K)$ is increasing in K. Anticipating this, the true cost of capital for the firm is not r, but instead $r + w'_{uj}(K)L(w_{uj}, K)$ where $w'_{uj}(K)L(w_{uj}, K)$ is the decrease in profit (by the envelope theorem) due to the marginal increase in labor costs. Where $w'_{uj}(K) > 0$, there is a wedge between the observed cost of capital and the firm's marginal revenue product of capital, and less investment than socially optimal.[56]

Lemma 8. *Let $F(K, L)$ be homogenous of degree 1 in (K, L) and concave in each input. Then $w'_u(K) = 0$ and there is no capital hold-up problem.*

Proof. Since $F(K, L)$ is HOD1 in (K, L), it follows that $F_K(\cdot)$ and $F_L(\cdot)$ are HOD0 in (K, L). The optimal labor choice by the firm is unique (since $F_{LL}(\cdot) < 0$) and satisfies

$$F_L(K, l^*) = \frac{w_u}{p_g} \iff F_L(\lambda K, \lambda l^*) = \frac{w_u}{p_g}$$
$$\Rightarrow L^*(w_u, \lambda K) = \lambda L^*(w_u, K)$$

Recall the rent extracted by the union is $R = (w_u - w_n)L^*(w_u, K)$. Substituting from above,

$$R(w_u, K) = (w_u - w_n)L^*(w_u, K)$$
$$\Rightarrow R(w_u, \lambda K) = (w_u - w_n)\lambda L^*(w_u, K) = \lambda R(w_u, K)$$

Since w_u maximises $R(w, K)$ and is optimal for the union by definition, it must also maximise $R(w, \lambda K)\, \forall \lambda > 0$. Therefore if (K, w_u, l^*) is an equilibrium given (p_g, r), $(\lambda K, w_u, \lambda l^*)$ must be also (and vice versa). Therefore w_u is invariant in K. □

To be clear, this is not to say that capital hold up is not a real phenomenon. Rather, the point

[56] Compare to the choice of labor, which is also socially suboptimal with a higher marginal product of labor in unionized firms than in non-union firms, but where the observed cost of labor and the firm's marginal revenue product of labor are equal.

is that common classes of production functions implicitly allow it no role.[57] In situations where it occurs, capital hold-up leads to inefficiently low capital investment. Thus, by working with a production function that precludes hold-up, the frictions introduced by unionization are potentially underestimated.

The natural corollary of Lemma 8 is that the choice of K_j is not tied down for the firm, since all outcomes scale linearly (and in equilibrium, firms earn zero profit irrespective of scale). Equilibrium requires that p_g is such that firms earn zero profits. Accordingly, p_g must satisfy

$$w_u L^* + rK = pK^\alpha L^{1-\alpha}$$

Substituting for L^* and rearranging yields

$$p_g = \left(\frac{r}{a}\right)^\alpha \left(\frac{w_n}{(1-\alpha)^2}\right)^{1-\alpha}$$

A.2 Union Formation and Right to Work

Proposition 2. *Suppose unionization is costly, with cost $c > 0$. Then there exists some $\alpha^* \in (0, 1)$ such that firms will be unionized if and only if $\alpha \geq \alpha^*$ and the firm operates in a non-RTW location.*

Proof. First consider a firm in a non-RTW location. If a union exists, workers incur the cost c irrespective of whether they themselves are a member of the union. Accordingly, the workplace will be unionized if the workers of the firm benefit from it being unionized. We focus on ex post optimality - a firm will only be unionized if its workers, in the state where it is unionized, would not opt to revoke unionization. The unionized firm has wages w_u and there is a non-union sector where wages are w_n. If the union were revoked, wages would accordingly fall to w_n.[58]

Then, workers will prefer unionization if

$$w_u - c > w_n \tag{27}$$

[57] Hold-up can be introduced by considering production where the scale and quality of capital are considered separately, and an example of this is considered in Appendix B.3.

[58] The firm would hire more labor but those currently employed do not internalise this spillover. Also, if the firm is small, it has no/infinitesimal effect on w_n and any newly-hired workers earn the common non-unionized wage, so the newly-hired workers do not benefit either. Indeed, in this simple example, where workers have a common outside option, *ex post* unionization preference and *ex ante* unionization preference are identical. Ex-ante, at a wage of w_n, workers could unionize to raise wages to w_u. Here, they understand that this will lead the firm to reduce labor hired, but these workers are not harmed compared to non-unionization since they can still obtain wage w_n in the non-union sector.

In the basic Cobb-Douglas model, using the union-set wage in Equation 23 this means that firms will unionize provided that

$$
\begin{aligned}
&\frac{w_n}{1-\alpha} - c > w_n \\
\Rightarrow &\alpha > \frac{c}{w_n + c}
\end{aligned}
\tag{28}
$$

That is, firms where capital is more important in production are more vulnerable to unionization. The monopoly union model's union wage premium is increasing in the capital share α, making unionization more attractive to workers in high α industries.[59]

Next consider a firm in a RTW location.[60] Non-members of unions in RTW states do not have to pay unions any dues or agency fees. If the union opts to act as exclusive representative for all a firm's workers, non-members can obtain w_u without paying c (or at least the monetary portion thereof). Doing so is privately rational, even for workers that prefer the union to operate. Unable to obtain payment for its services, the union collapses, or anticipating lack of dues payment, never forms. Thus in equilibrium, firms are not unionized in RTW locations. □

B Model Derivations and Extensions - Monopoly Union Model

B.1 Union Wage Setting with Cobb-Douglas Production

For analytical tractability, let $F = K^{\alpha} L^{1-\alpha}$.

$$
\begin{aligned}
p_g(1-\alpha)\left(\frac{K}{L}\right)^{\alpha} &= w_u \\
\Rightarrow L &= \left(\frac{p_g(1-a)}{w_u}\right)^{\frac{1}{\alpha}} K
\end{aligned}
$$

The union wage setting problem is

$$
\max_{w_u} (w_u - w_n).L(w_u)
$$

[59]This also is consistent with empirical reality, where the highest rates of private-sector unionization occur in capital-intensive industries such as manufacturing.

[60]On paper, RTW laws have little effect on the legal power held by unions. Unions can still form at majority worker preference and act as exclusive representative of a firm's workers in bargaining over wages, and firms have the same legal obligations to negotiate with the union. That is, conditional on existing, they can negotiate higher wages in an identical manner to firms in non-RTW locations.

$$= \max_L (w_u(L) - w_n).L$$

Since the firm will choose L such that $p_g F_L(K,L) = w_u$, and exploiting $F_L(K,L) = (1-\alpha)\dfrac{F(K,L)}{L}$, this can be rewritten as

$$\begin{aligned}
&\max_L \; p_g(1-\alpha)F(K,L) - w_n L \\
&\Rightarrow p_g(1-\alpha)F_L(K,L) - w_n = 0 \\
&\Rightarrow w_u = \frac{w_n}{1-\alpha}
\end{aligned}$$

Facing factor prices w_u and r, the firm will choose L such that

$$\begin{aligned}
\frac{F_K(K,L)}{F_L(K,L)} &= \frac{(\alpha)}{1-\alpha}\frac{L}{K} = \frac{r}{w_u} \\
\Rightarrow \frac{L}{K} &= \frac{(1-\alpha)^2}{\alpha}\frac{r}{w_n}
\end{aligned}$$

The union wage is increasing in α and L^* (normalising K) is falling in α.

Equilibrium requires that p_g is such that firms earn zero profits. Accordingly, p_g must satisfy

$$w_u L^* + rK = pK^\alpha L^{1-\alpha}$$

Substituting for L^* and rearranging yields

$$p_g = \left(\frac{r}{a}\right)^\alpha \left(\frac{w_n}{(1-\alpha)^2}\right)^{1-\alpha}$$

B.2 Extension: Sectoral Bargaining

The standard model in Section 3.1 considers *enterprise bargaining*, where each unionized firm has a separate union. Further, we assume that each union cares about only the welfare of its own workers; it acts to maximise the surplus extracted from the specific firm which it faces, without considering rents extracted by other unions in the same industry.

For each firm-union pair, the union and firm take p_g and r as given (and the union chooses w_u). While there is a direct mapping between w_u and p_g in general equilibrium (such that the zero profit condition holds),[61] each firm/union is small and thus their individual wage and output decisions have no effect on the equilibrium market price, so they are price-takers with respect to p_g.

An alternate model of union behavior is *sectoral bargaining*, where a single union pertains to an industry and controls wages for all firms in that industry. While enterprise bargagining is predominant in the United States, sectoral bargaining is more common in Europe.

Union Objective Function

By assumption, the union wishes to maximise total rents extracted by workers in a given industry, but only that output good industry.[62] The industry union objective function is accordingly

$$R_g = \sum_j (w_{uj} - w_n).L_j(w_{uj}) \tag{29}$$

In setting the wage level, the union understands the aggregate demand $Q_D(p_g)$ for the product as a function of price, and how changes in the wage (vector) w_u alter the market-clearing p_g via firm's labor demand response $Q_S = \sum_j F_j(K_j, L_j^*(w_{uj}, K_j))$.

As before, firms take prices as given, engage in perfect competition and profit maximise.

Timing

The timing of decisions is as follows.

- At $t = 0$, each firm i chooses K_i, understanding $w_{ui}(K_i)$ and $\Pi(K_i, w_{ui}(K_i))$
- At $t = 1$, the union observes $\{K_i\}$ and chooses $w_{ui}(K_i)$ to maximise total rents, understanding how firms will demand labor in response and how total output will alter the market clearing price p_g.
- At $t = 2$, each firm j chooses optimal $L^*(w_{uj}, K_j)$ taking p_g and K_j as given.

[61] This occurs because firms and their unions are identical, and thus the equilibrium is symmetric across firms.

[62] Having a single union set wages for all firms essentially allows the union to collude with itself- if enterprise unions could collude and punish deviation then this leads to analogous results. Additionally, even if collusion is not possible, if enterprise unions had an objective function of maximising worker rents in the entire industry (hypothetically; it is unclear why they would want to do so), then there exists a (union rent-maximising) Nash Equilibrium where each union identically privately chooses higher w_{uj} than in the basic model, internalising the benefits received by workers at other firms.

Union Wage Choice

In the employer bargaining model, the union extracts maximal rents from the specific firm. This greatly simplifies optimal wage setting. Here, we must consider the rent maximising schedule $w_u(K)$, where the wage for a specific K is chosen to maximise the sum of rents taken from all firms, not just the firm in question.[63]

Lemma 9. *Define the set of ever-chosen K, $K^C = \{K : \exists j \text{ s.t. } K_j = K\}$. Then $w_u(K)$ is constant $\forall K \in K^C$.*

In other words, the union sets a common wage w_u across all capital levels for all firms. This is unsurprising, since the firms are identical. A short sketch of the proof (for the interested reader) is below, followed by a tedious formal proof that is included for completeness but can be safely skipped.

Proof. (Sketch) The basic idea is as follows. Suppose both K' and $\tilde{K}$ are ever chosen. We must have $\Pi(K') = \Pi(\tilde{K})$. Consider w.l.o.g. $w_u(K') > w_u(\tilde{K})$. Consider a firm supobtimally implementing $\left(\tilde{K}, L^*(w_u(K'), \tilde{K})\right)$ when facing $w_u(\tilde{K})$. This is strictly suboptimal so profit is negative. But it in turn strictly dominates $\frac{\tilde{K}}{K'}\Pi(K')$ (same inputs, lower wage costs) so $\Pi(K') < 0$, a contradiction. Thus $w_u(K) = w_u \forall K$ ever chosen. $\square$

Proof. Denote the profit earned by a firm given K to be

$$\Pi(K) = \Pi(K, L^*(w_u(K), K))$$

where

$$L^*(w_u(K), K) = \arg\max_{L \in \mathbb{R}} \Pi(K, L, w_u(K)) = p.F(K, L) - rK - w_u(K)L$$

Then, since firms may freely enter and exit, for any K that could ever be observed in equilibrium, we must have $\Pi(K) = 0$.

Then suppose that both K' and $\tilde{K}$ are ever chosen in equilibrium. We have

$$\Pi(K') = p.F(K', L^*(w_u(K'), K')) - rK' - w_u(\tilde{K}).L^*(w_u(K'), K') = 0$$

$$\Pi(\tilde{K}) = p.F(\tilde{K}, L^*(w_u(\tilde{K}), \tilde{K})) - r\tilde{K} - w_u(\tilde{K}).L^*(w_u(\tilde{K}), \tilde{K}) = 0$$

[63] In the most general form of the problem, the union may set firm-specific schedules $w_{uj}(K)$. However, since general equilibrium requires firms to earn zero profits, if $w_{uj}(K_0) > w_{ui}(K_0)$ (i.e. at $K = K_0$) then firm j must never choose K_0 in equilibrium since it must result in negative profit. We also restrict attention to pure strategies.

Denote off-equilibrium path pseudo-profit $\tilde{\Pi}(K, w_u(K), w_u(K'))$ as the profit earned by a firm with capital K which faces prices $w_u(K)$ but optimises as if it faces prices $w_u(K')$ (and capital K) when choosing L. That is,

$$\tilde{\Pi}(K, w_u(K), w_u(K')) = p.F(K, L^*(w_u(K'), K)) - rK - w_u(K).L^*(w_u(K'), K) = 0$$

Suppose (w.l.o.g.) that $w_u(K') > w_u(\tilde{K})$. Instead of implementing $(\tilde{K}, w_u(\tilde{K}), w_u(\tilde{K}))$ and earning $\Pi(\tilde{K})$, consider implementing $(\tilde{K}, w_u(\tilde{K}), w_u(K'))$ and earning $\tilde{\Pi}(\tilde{K}, w_u(\tilde{K}), w_u(K'))$.

$$\begin{aligned}\tilde{\Pi}(\tilde{K}, w_u(\tilde{K}), w_u(K')) &= p.F(\tilde{K}, L^*(w_u(K'), \tilde{K})) - r\tilde{K} - w_u(\tilde{K}).L^*(w_u(K'), \tilde{K}) \\ &< \Pi(\tilde{K}) = p.F(\tilde{K}, L^*(w_u(\tilde{K}), \tilde{K})) - r\tilde{K} - w_u(\tilde{K}).L^*(w_u(\tilde{K}), \tilde{K}) = 0\end{aligned}$$

because $p.F_L(\tilde{K}, L^*(w_u(K'), \tilde{K})) = w_u(K') > w_u(\tilde{K})$ so money is left on the table.

Recall $F(K, L)$ is HOD1 in (K, L), and thus $F_L(K, L)$ is HOD0 in (K, L). Then

$$\begin{aligned}&p.F_L(\tilde{K}, L^*(w_u(K'), \tilde{K})) = w_u(K') \\ \Rightarrow &p.F_L\left(K', L^*(w_u(K'), \tilde{K}).\frac{K'}{\tilde{K}}\right) = w_u(K') \\ \Rightarrow &L^*(w_u(K'), \tilde{K}).\frac{K'}{\tilde{K}} = L^*(w_u(K'), K') && \text{by definition of } p.F_L(\cdot) = w_u(\cdot)\end{aligned}$$

Thus

$$\begin{aligned}\tilde{\Pi}(\tilde{K}, w_u(\tilde{K}), w_u(K')) &= p.F(\tilde{K}, L^*(w_u(K'), \tilde{K})) - r\tilde{K} - w_u(\tilde{K}).L^*(w_u(K'), \tilde{K}) && < 0 \\ &= p.F\left(\tilde{K}, \frac{\tilde{K}}{K'}L^*(w_u(K'), K')\right) - r\tilde{K} - w_u(\tilde{K}).L^*(w_u(K'), K')\frac{\tilde{K}}{K'} \\ &= \frac{\tilde{K}}{K'}\left(p.F(K', L^*(w_u(K'), K')) - rK' - w_u(\tilde{K})L^*(w_u(K'), K')\right) && \text{as } F \text{ is HOD1} \\ &> \frac{\tilde{K}}{K'}\Pi(K') \text{ since } w_u(\tilde{K}) < w_u(K') \text{ by assumption.} \\ &\Rightarrow \Pi(K') < 0\end{aligned}$$

This is a contradiction of $\Pi(K') = 0$. Thus $w_u(K)$ is constant in $K \in K^C$. $\square$

Using Lemma 9, the union objective function can be simplified to

$$\max_{w_u} R = \sum_i (w_u - w_n)\, L_i(K_i, w_u) \tag{30}$$

Given a union set wage w_u and taking p_g as given, each firm j will solve

$$\max_L p_g F(K_j, L_j) - w_u L_j$$

As before, the first order condition for the optima labor choice yields

$$L_j^* = L_j : F_L(K_j, L_j) = \frac{w_u}{p_g}$$

Knowing this, the union chooses w_u to maximise rents earned by workers in the entire output good industry. Unlike firms (which are small, and thus act as price takers), the union takes into account how its choice of w_u will affect p_g (through total output).

Denote $L = \sum_i Li$ and $K = \sum_i K_i$. Note that $F_L(K, L)$ is HOD0 in (K, L) and thus we have $F_L(K_j, L_j) = F_L\left(1, \frac{L_j}{K_j}\right) = \frac{w_u}{p_g}$, so $\frac{L_j}{K_j}$ is constant $\forall j$.

$$\begin{aligned}
\max_{w_u} R &= \sum_i (w_u - w_n) L_i(K_i, w_u) \\
&= \max_{w_u} (w_u - w_n) L(K) \\
&= \max_L (p_g(Q) F_L(K, L) - w_n) L
\end{aligned}$$

Differentiating with respect to L and rearranging, this yields

$$w_u = w_n - p_g(Q) F_{LL}(K, L) L - p_g'(Q).F_L(K, L)^2.L \tag{31}$$

This exceeds both the non-union wage w_n and the union wage under enterprise bargaining. The intuition for this is simple. In the enterprise bargaining case, locally increasing w_{uj} reduces L_j and thus has an infinitesimal effect on total output and thus p_g.[64] However, in the sectoral bargaining case, more rent can be extracted from each other firm due to the second-order increase in $L_i \forall i$ (which summed across firms, is first-order). The single union is effectively able to internalise these benefits

[64]The relative change in price is second order and has only second order indirect effects on L_j, but the direct effect on L_j is first-order. Thus to maximise rent extracted from firm j, the union acts as a price taker with regard to p_g.

- when w_u is increased for all firms, the price of the output good rises and each firm's demand for labor falls by less than if they had faced unilaterally higher wages. In essence, by seeking to maximise joint rent extraction, the single union avoids each enterprise union competing with each other by reducing wages to increase firm employment (i.e. market share).

Sectoral Bargaining Equilibrium with Cobb-Douglas Production

We continue with the case of Cobb-Douglas production where $F = K^{\alpha}L^{1-\alpha}$. Also let aggregate demand for the output good be governed by $p_g(Q) = Q^{-\frac{1}{\sigma}}$ for $\sigma > 1$. From this,

$$\begin{aligned} p_g'(Q) &= -\frac{1}{\sigma}\frac{p_g(Q)}{Q} \\ \Rightarrow p_g'(Q)\frac{Q}{p_g(Q)} &= -\frac{1}{\sigma} \end{aligned}$$

such that the price elasticity of output demand is $-\sigma$. Firms take prices p_g and wages w_u as given, and with K sunk, they set

$$\begin{aligned} p_g(1-\alpha)\left(\frac{K}{L}\right)^{\alpha} &= w_u \\ \Rightarrow L = \left(\frac{p_g.(1-a)}{w_u}\right)^{\frac{1}{\alpha}} K \end{aligned}$$

The union wage setting problem is

$$\begin{aligned} &\max_{w_u} (w_u - w_n).L(w_u) \\ &= \max_{L} (w_u(L) - w_n).L \end{aligned}$$

Since the firm will choose L such that $p_g(Q)F_L(K,L) = w_u$, and exploiting $F_L(K,L) = (1-\alpha)\frac{F(K,L)}{L}$, this can be rewritten as

$$\begin{aligned} &\max_{L}\ p_g(Q).(1-\alpha)F(K,L) - w_n L \\ &\Rightarrow p_g'(Q).F_L(K,L).(1-\alpha)F(K,L) + p_g(1-\alpha)F_L(K,L) - w_n = 0 \\ &\Rightarrow p_g'(Q)\frac{w_u}{p_g(Q)}.(1-\alpha)F(K,L) + (1-\alpha)w_u - w_n = 0 \end{aligned}$$

$$\Rightarrow w_u(1-\alpha)\left(1+p_g'(Q)\frac{F(K,L)}{p_g(Q)}\right)=w_n$$
$$\Rightarrow w_u=\frac{w_n}{1-\alpha}\left(\frac{\sigma}{\sigma-1}\right)$$

Facing factor prices w_u and r, the firm will choose L such that

$$\frac{F_K(K,L)}{F_L(K,L)}=\frac{(\alpha)}{1-\alpha}\frac{L}{K}=\frac{r}{w_u}$$
$$\Rightarrow \frac{L}{K}=\frac{(1-\alpha)^2}{\alpha}\left(\frac{\sigma-1}{\sigma}\right)\frac{r}{w_n}$$

The union wage is increasing in α (the importance of capital) and falling in the elasticity of product demand σ, and L^* (normalising K) is falling in α and increasing in σ.

Equilibrium requires that p_g is such that firms earn zero profits. Accordingly, p_g must satisfy

$$w_uL^*+rK=pK^{\alpha}L^(1-\alpha)$$

Substituting for L^* and rearranging yields

$$p_g=\left(\frac{r}{a}\right)^{\alpha}\left(\frac{w_n}{(1-\alpha)^2}\frac{\sigma}{\sigma-1}\right)^{1-\alpha}$$

Substituting the market-clearing price into the aggregate product demand yields

$$Q=\left(\frac{r}{a}\right)^{-\alpha\sigma}\left(\frac{w_n}{(1-\alpha)^2}\right)^{-\sigma(1-\alpha)}\left(\frac{\sigma}{\sigma-1}\right)^{-\sigma(1-\alpha)}$$
$$K=\left(\frac{a}{r}\right)^{1+\alpha(\sigma-1)}\left(\frac{(1-\alpha)^2}{w_n}\right)^{(\sigma-1)(1-\alpha)}\left(\frac{\sigma-1}{\sigma}\right)^{(\sigma-1)(1-\alpha)}$$

B.3 Extension: Capital Quality and Hold-Up

In all of these models there is no capital hold-up, that is $w_u'(K)=0$. This follows from output being HOD1 in (K,L). It also relates to there not being any distinction between the quality and scale of capital, or these two dimensions being treated as ex-post fungible. In the base representation, it is as if a firm that has purchased 10 units of capital can ex-post decide (once it knows what wage it faces) whether it possesses 10 basic hammers that it can allot between 10 workers, or 1 extremely high-quality/high-productivity hammer that one worker can use.[65]

Accordingly, consider capital where quality and scale are not fungible. Upon choosing K, both scale S and quality q must be stipulated, with $K = S.q$. Then output is given by $F(S, q, L)$. $F(\cdot)$ is homogenous of degree one in (S, L) - multiplying both the scale of capital and labor used by a common factor holds both the quality of capital each worker uses and the scale of capital per worker fixed, and thus output rises proportionally.

Firm Labor Choice

Consider a firm that has sunk capital investment (S, q) and takes prices (p, w_u) as given. The firm's problem is

$$\max_L p_g.F(S, q, L) - w_u(S, q)L \tag{32}$$

The first order condition yields

$$F_L(S, q, L) = \frac{w_u(S, q)}{p_g} \tag{33}$$

Union Wage Choice

As before, the union chooses a wage schedule to maximise rent.

$$\max_{w_u(S,q)} R = (w_u(S, q) - w_n) .L(S, q, w_u) \tag{34}$$

$$= \max_{L(S,q)} \left(p_g F_L(S, q, L) - w_n\right) .L \tag{35}$$

Differentiating and rearranging, this yields

$$w_u(S, q) = w_n - p_g F_L L(S, q, L).L \tag{36}$$

As in the base model, $F_L(\lambda S, q, \lambda L) = F_L(S, q, L)$ and thus $L^*(\lambda S, q, w_u) = L^*(\lambda S, q, w_u)$. Thus, as in Lemma 8, the wage that maximises the union's rent is constant in S: $\arg\max_{w_u} R(S, q, w_u) = \arg\max_{w_u} R(\lambda S, q, w_u)$. Thus $w_u^*(S, q)$ is constant in S.

[65] An alternate view is that a single worker using one extremely high quality hammer is equally productive as another worker with ten basic hammers.

Accordingly, we can simplify Equation 36 to give

$$w_u(q) = w_n - p_g F_L L\left(1, q, \frac{L}{S}(q, w_u)\right)\frac{L}{S}(q, w_u) \tag{37}$$

Cobb-Douglas Example with Capital Scale and Quality

To proceed further, we need to impose additional structure on the returns to capital scale and quality. Consider a simple model where additional workers are less productive if workers have to share units of capital $(L > S)$, and where additional units of capital are less productive once each worker already has access to one $(L < S)$. In particular, let

$$F(S, q, L) = \begin{cases} \left(\frac{L}{S}\right)^{1-\beta} Sq^{\alpha} = L^{1-\beta}S^{\beta}q^{\alpha} & L \leq S \\ \left(\frac{L}{S}\right)^{1-\gamma} Sq^{\alpha} = L^{1-\gamma}S^{\gamma}q^{\alpha} & L \geq S \end{cases} \tag{38}$$

for $\gamma > \alpha > \beta$.

Since the production function and returns to inputs vary by whether $L > S$, optimal choices depend on whether labor or capital scale is relatively scarce. Proceeding as in the standard model, we have

$$L^*(S, q, w_u) = \begin{cases} \left(\frac{p_g(1-\beta)}{w_u(q)}\right)^{\frac{1}{\beta}} q^{\frac{\alpha}{\beta}} S & L \leq S \\ \left(\frac{p_g(1-\gamma)}{w_u(q)}\right)^{\frac{1}{\gamma}} q^{\frac{\alpha}{\gamma}} S & L \geq S \end{cases} \tag{39}$$

The union's problem in maximising rent is made slightly more complex by the fact that by choosing $w_u(q)$, it can alter whether the firm selects into the $L < S$ or $L > S$ region. There are three cases for the union to consider, from which it then implements the rent-maximising choice.

First, the rent-maximising problem conditional on $L \leq S$ may yield an interior solution that satisfies $L < S$. Second, the rent-maximising problem conditional on $L \geq S$ may yield an interior solution that satisfies $L > S$. Finally, either (or both) segments may not, such that the optimal solution in the respective region occurs at $L = S$. Note that F_L changes discontinuously at $L = S$; accordingly conditional on choosing w_u to induce $L = S$ the union will charge the highest wage that does so, namely $w_u = p_g(1-\beta)q^\alpha$. The possibly optimal wage choices are thus

$$w_u^*(q) = \begin{cases} \dfrac{w_n}{1-\beta} & L \le S \\ p_g(1-\beta)q^\alpha & L = S \\ \dfrac{w_n}{1-\gamma} & L \ge S \end{cases} \tag{40}$$

It is possible to define $q'(p_g)$ and $q''(p_g)$ such that if $q'(p_g) < q < q''(p_g)$, the firm will set $w_u(q)$ to induce $L = S$, while for $q \le q'(p_g)$ and $q \ge q''(p_g)$ it will respectively induce $L < S$ and $L > S$.

Rather than explicitly stipulate these conditions, it is simpler to note that given p_g, the firm will never choose q where $w_u^*(q)$ will induce anything other than $L = S$. The reasoning for this is simple. Taking p_g as given, suppose the firm chooses (q, S) such that that union chooses w_u to induce $L < S$. Then the firm could deviate to $(\lambda q, \frac{S}{\lambda})$ for *some* $\lambda = 1 + \varepsilon$, incurring the same total capital costs, but producing more output since S was in relative surplus. Further, the rent-maximising wage for the union would be unchanged.[66] Thus, if the firm sub-optimally then implemented $L^*(S, q, w_u)$, this would be a profitable deviation overall, and thus implementing $L^*(\frac{S}{\lambda}, \lambda q, w_u)$ is even more so. This yields a contradiction.

Similarly, taking p_g as given, suppose the firm chooses (q, S) such that that union chooses w_u to induce $L > S$, with $w_u^*(q) = \dfrac{w_n}{1-\gamma}$. Consider instead setting $q = \left(\dfrac{w_n}{p_g(1-\beta)^2}\right)^{\frac{1}{\alpha}}$ (with S increased to keep K fixed) to induce the union choosing $w_u = \dfrac{w_n}{1-\beta}$. This deviation involves the same capital costs and a lower wage, such that for fixed L labor costs fall while output rises. Thus the deviation is profitable. Ergo, the firm will always set (S, q) to induce $L = S$ in equilibrium.

Then we have $w_u^*(q) = p_g(1-\beta)q^\alpha$, and $L(w_u^*) = S$. Clearly this yields a hold-up problem for q because $p_g.F_q(S, q, L) - rS - w_u^{*\prime}(q)S = 0$ where $w_u^{*\prime}(q) = p_g(1-\beta)\alpha q^{\alpha-1}$.

C Model Derivations and Extensions - Spatial Model

C.1 Derivation of Choice Probabilities with Fréchet Taste Shocks

The following shows how to derive the location choice probabilities as in Equation 9 from the indirect utility representation with idiosyncratic Fréchet taste shocks. It should be emphasised that this setup

[66]Note that w_u does not jump discontinuously approaching $L = S$ from below, so this holds for all local deviations for $L < S$.

(or close equivalents) is common in urban economics papers involving location choice (Allen and Arkolakis (2014, 2019); Heblich et al. (2020); Ahlfeldt et al. (2015); Eaton and Kortum (2002)) and thus this derivation closely follows materials that can be readily found elsewhere.

Suppose indirect utility is of the form

$$U_{ij}(a) = \frac{\xi_{ij}(a) w_j}{P.T_{ij}} \tag{41}$$

ξ_{ij} is an idiosyncratic taste shock that is i.i.d. Fréchet distributed, with C.D.F. $G(\xi) = e^{-\xi^{-\epsilon}}$. Higher ϵ means the distribution of taste draws is compressed, and thus people are relatively more sensitive to fundamentals than tastes. From above, we can rewrite

$$\xi_{ij}(a) = \frac{U_{ij}(a) P.T_{ij}}{w_j}$$

Since $\xi_{ij}(a) \sim G$, it follows that $\dfrac{U_{ij}(a)P.T_{ij}}{w_j} \sim G$ also.

Denote $X_{ij} = \dfrac{P.T_{ij}}{w_j}$. Then

$$U_{ij}(a) X_{ij} \sim G(U_{ij}(a) X_{ij}) = e^{-X^{-\epsilon} U_{ij}(a)^{-\epsilon}} \tag{42}$$

$$= \exp[-(w_j)^{\epsilon} (P.T_{ij})^{-\epsilon} U_{ij}^{-\epsilon}] \tag{43}$$

That is,

$$\Pr(U_{ij}(a) X_{ij} < U X_{ij}) = G_{u_{ij}(a) X_{ij}}(U X_{ij})$$

Since $\Pr(U_{ij}(a) \leq U) = \Pr(U_{ij}(a) X_{ij} \leq U X_{ij})$,

$$G_{u_{ij}(a)}(U) = \exp[-(w_j)^{\epsilon} (P.T_{ij})^{-\epsilon} U^{-\epsilon}] \tag{44}$$

$$= \exp[-X_{ij}^{-\epsilon} U^{-\epsilon}] \tag{45}$$

Individuals choose the optimal (i, j) pair (accounting for location preferences, commuting costs and wages as part of this), $\arg\max_{m \in \mathbb{L}, n \in \mathbb{L}} U_{mn}(a)$. We thus need to construct the probability distribution of the best option. The probability that $U_{ij} < u$ is $G_{U_{ij}}(u)$ so the probability that the $\max_{j,k} U_{jk} < u$ is the joint probability that each individual draw is below u. Since draws are independent, this takes the form

$$\prod_{m\in\mathbb{L}}\prod_{n\in\mathbb{L}}\Pr(U_{mn}<U)=\prod_{m\in\mathbb{L}}\prod_{n\in\mathbb{L}}G_{U_{mn}}(u) \tag{46}$$

Therefore, the C.D.F. of the probability distribution of the best draw is

$$\begin{aligned} F(u) &= \prod_{m\in\mathbb{L}}\prod_{n\in\mathbb{L}}G_{U_{mn}}(u) && (47)\\ &= \exp\left[\left(-\sum_{m\in\mathbb{L}}\sum_{n\in\mathbb{L}}X_{mn}^{-\epsilon}\right)u^{-\epsilon}\right] && (48)\end{aligned}$$

The distribution of the best option, excluding the specific pair (i,j), denoted $\tilde{F}_{ij}(u)$, can easily be derived by noting that $F(u)=\tilde{F}_{ij}(u).G_{U_{ij}}(u)$

$$\Rightarrow \tilde{F}_{ij}(u)=\exp\left[\left(-\sum_{m\in\mathbb{L}}\sum_{n\in\mathbb{L}}X_{mn}^{-\epsilon}-X_{ij}^{-\epsilon}\right)u^{-\epsilon}\right] \tag{49}$$

The probability that (i,j) is the best option, p_{ij} can thus be obtained by integrating the CDF of the best non-ij option over the distribution of U_{ij}. Denote $g_{ij}=\dfrac{d}{du}e^{-(X_{ij}u)^{-\epsilon}}=X_{ij}.\epsilon.u^{-(\epsilon+1)}e^{-(X_{ij}u)^{-\epsilon}}$

$$\begin{aligned} p_{ij} &= \Pr\left(U_{ij}\geq \max_{k,l}U_{kl}\right)\\ &= \int_0^\infty \tilde{F}_{ij}(u)\; g_{ij}(u)\,du\\ &= \int_0^\infty \exp\left[\left(-\sum_{m\in\mathbb{L}}\sum_{n\in\mathbb{L}}X_{mn}^{-\epsilon}-X_{ij}^{-\epsilon}\right)u^{-\epsilon}\right].\,X_{ij}.\epsilon.u^{-(\epsilon+1)}\exp(-(X_{ij}u)^{-\epsilon})\,du\\ &= \int_0^\infty \exp\left[\left(-\sum_{m\in\mathbb{L}}\sum_{n\in\mathbb{L}}X_{mn}^{-\epsilon}\right)u^{-\epsilon}\right].\,X_{ij}.\epsilon.u^{-(\epsilon+1)}\,du \end{aligned}$$

Note that $\dfrac{d}{du}\left(\dfrac{1}{A}\exp(-Au^{-\epsilon})\right)=\epsilon u^{-(\epsilon+1)}\exp(-Au^{-\epsilon})$. Then we have

$$\begin{aligned} p_{ij} &= \left[\exp\left(-\sum_{m\in\mathbb{L}}\sum_{n\in\mathbb{L}}X_{mn}^{-\epsilon}\,u^{-\epsilon}\right)\right]_{0^+}^{\infty}\;\frac{X_{ij}^{-\epsilon}}{\sum_{m\in\mathbb{L}}\sum_{n\in\mathbb{L}}X_{mn}^{-\epsilon}}\\ &= \frac{X_{ij}^{-\epsilon}}{\sum_{m\in\mathbb{L}}\sum_{n\in\mathbb{L}}X_{mn}^{-\epsilon}} \end{aligned}$$

Thus,

$$\frac{L_{ij}}{L} = \frac{(w_j)^{\epsilon}(T_{ij})^{-\epsilon}}{\sum_{m\in\mathbb{L}}\sum_{n\in\mathbb{L}}(w_n)^{\epsilon}(T_{mn})^{-\epsilon}}\ \forall\, i,j \in \mathbb{L} \tag{50}$$

C.2 Equilibria with Counterfactual Policy Settings

The main analysis considers the equilibria that results from one location ($\mathcal{A}$) having a RTW law and the other ($\mathcal{B}$) being non-RTW. Denote this as the (R,N) equilibria. Here we consider the outcomes that arise if both locations have RTW laws (the (R,R) equiibria) and if neither does (the (N,N) equilibria).

Equilibria if Both Locations are RTW

First, we consider the (R,R) equilibria. Since labor markets are competitive, we immediately have that $w^{RR}_{k\mathcal{A}} = w^{RR}_{l\mathcal{A}} = w^{RR}_{\mathcal{A}}$ and $w^{RR}_{k\mathcal{B}} = w^{RR}_{l\mathcal{B}} = w^{RR}_{\mathcal{B}}$. Furthermore, since the idiosyncratic taste shocks are symmetric, the median individual is indifferent between workplace locations except for differences in wages (and the same holds for residences). Thus $w^{RR}_{\mathcal{A}} = w^{RR}_{\mathcal{B}}$, with common wages across locations and industries.[67] It then follows that $p^{RR}_{k\mathcal{A}} = p^{RR}_{k\mathcal{B}} = p^{RR}_{k}$ and $p^{RR}_{l\mathcal{A}} = p^{RR}_{l\mathcal{B}} = p^{RR}_{l}$, so the two locations produce the exact same quantities of the respective outputs (except that they make their location's variety).

Since we know that $E_A = E_B = \frac{1}{2}$, we can solve for w^{RR} by finding the wage that causes total labor demand across industries in each location to sum to half. Recall from before that product demand for each variety is

$$Q_{vj} = \lambda p_{vj}^{-\sigma}$$

while the minimum cost of production of each variety, achieved by setting $K = \frac{a}{1-a}\frac{w}{r}L$, is

$$p_{vj} = \left(\frac{r}{a}\right)^{\alpha}\left(\frac{w}{1-a}\right)^{1-\alpha} \tag{51}$$

Substituting this price into the the product demand function yields

$$Q_{vj} = \lambda\left(\frac{r}{a}\right)^{-\alpha\sigma}\left(\frac{w}{1-a}\right)^{-(1-\alpha)\sigma} \tag{52}$$

[67]Formally, suppose w.l.o.g. that $w^{RR}_{\mathcal{A}} > w^{RR}_{\mathcal{B}}$. Then a majority of people would elect to work in A, but firm hiring in A would be lower than in B due to the higher wage level; a contradiction.

To derive labor demand L_{vj}^D we then substitute $K = \frac{a}{1-a}\frac{w}{r}L$ into the firm's production function and solve for Q_{vj}, which yields

$$L_{vj}^D = \lambda \left(\frac{r}{a}\right)^{\alpha(1-\sigma)} \left(\frac{w}{1-a}\right)^{\alpha(\sigma-1)-\sigma} \tag{53}$$

We know that $L_{l\mathcal{A}}^D + L_{k\mathcal{A}}^D = \frac{1}{2}$. Remembering that the labor-intensive industry has $\alpha = 0$, the equilibrium wage thus solves

$$w^{-\sigma} + \left(\frac{r}{a}\right)^{\alpha(1-\sigma)} (1-a)^{\sigma-\alpha(\sigma-1)} w^{-\sigma(1-\alpha)-\alpha} = \frac{1}{2\lambda} \tag{54}$$

This yields a unique equilibrium wage w^{RR} because the left-hand side is strictly falling in w (for $\sigma > 1, 0 < \alpha < 1$.

We can then solve for p_{vj}^{RR}, Q_{vj}^{RR} and L_{vj}^{RR} by substituting back into equations 51, 52 and 53. Net commuting is zero because half the population lives in each location and half the population works in each location.

Equilibria if Neither Location is RTW

Next, we consider the (N, N) equilibria. Labor markets are not competitive in either location, with the capital-intensive industry being unionized in both locations. From the union markup rule, we have $w_{k\mathcal{A}}^{NN} = w_{l\mathcal{A}}^{NN}(1-\alpha)^{-1}$ and $w_{k\mathcal{B}}^{RR} = w_{l\mathcal{B}}^{RR}(1-\alpha)^{-1}$. Since wages are higher in both locations in the capital-intensive industry due to unionization, both locations have lotteries for jobs in these industries. As above, the median individual is indifferent between workplace locations except for differences in wages (and the same holds for residences). Thus $w_{k\mathcal{A}}^{NN} = w_{k\mathcal{B}}^{NN} \Rightarrow w_{l\mathcal{A}}^{NN} = w_{l\mathcal{B}}^{NN}$, with a common wage structure across locations.[68] It then follows that $p_{k\mathcal{A}}^{NN} = p_{k\mathcal{B}}^{NN} = p_k^{NN}$ and $p_{l\mathcal{A}}^{NN} = p_{l\mathcal{B}}^{NN} = p_l^{NN}$, so the two locations produce the exact same quantities of the respective outputs (except that they make their location's variety).

Solving for W_l^{NN} and w_k^{NN} follows identically as before, except that $w_k^{NN} = \frac{w_l^{NN}}{1-\alpha}$. We know that $L_{l\mathcal{A}}^D + L_{k\mathcal{A}}^D = \frac{1}{2}$. Remembering that the labor-intensive industry has $\alpha = 0$, the equilibrium wage thus solves

$$w_l^{-\sigma} + \left(\frac{r}{a}\right)^{\alpha(1-\sigma)} (1-a)^{2(\sigma-\alpha(\sigma-1))} w_l^{-\sigma(1-\alpha)-\alpha} = \frac{1}{2\lambda} \tag{55}$$

[68]Formally, suppose w.l.o.g. that $w_{l\mathcal{A}}^{NN} > w_{\mathcal{B}}^{NN}$. Then a majority of people would elect to work in A, but firm hiring in A would be lower than in B due to the higher wage level; a contradiction.

This yields a unique equilibrium wage w_l^{NN} (and thus w_k^{NN} also) because the left-hand side is strictly falling in w_l (for $\sigma > 1, 0 < \alpha < 1$. We can also immediately see that $w_l^{NN} < w^{RR} < w_k^{NN}$.

We can then solve for p_{vj}^{NN}, Q_{vj}^{NN} and L_{vj}^{NN} by substituting back into equations 51, 52 and 53. Net commuting is zero because half the population lives in each location and half the population works in each location.

D Data Appendix

D.1 Construction of ACS variables

We collect annual ACS respondent microdata from IPUMS for 2012 to 2017. We choose to limit our sample to 2012 and beyond to make use of the smaller 2010 Census PUMA areas. Due to substantial revisions to PUMA definitions between the 2000 and 2010 Census, the consistent PUMAs that span both periods are substantially larger than the 2010 Census PUMAs. Given our identifying assumption is that border PUMAs are comparable other than the policy discontinuity, it is important to limit our sample to smaller geographic regions.

We limit our ACS sample to individuals aged 16 years or older, who are not in the Armed Forces (defined as empstatd of 14 or 15) and are not living in institutional group quarters (defined as gq equal to 3).

We define prime-aged as individuals aged 25 to 54. We define individuals as single if marst equals 6, married is marst equals 2 or 3, separated / divorced if marst equals 3 or 4, and widowed if marst equals 5. We define individuals as white, non-Hispanic if race equals 1 and hispan equals 0, Black / African-American if race equals 2 and hispan equals 0, Hispanic if hispan is between 1 and 8, and classify all other persons as "Other". We define someone has holding less than a High School Diploma if educ less than six or educd equals 61, High School Graduate if educ equals six and educd not equal to 61, Some College if educ between 7 and 9, and 4 year College Graduate if educ equals 10 or 11.

We define someone as employed if empstat equals 1, unemployed if empstat equals 2, and not in labor force if empstat equals 3. We define someone as long-term jobless if they are not employed, and report no weeks worked in the past 12 months (wkswork2 equals 0). Weeks worked in the ACS is reported in interval categories, for the purpose of calculating weekly wages, we convert these categories by taking the average number of weeks between the cut-offs: 51 if wkswork2 equals 6, 48.5

if wkswork2 equals 5, 43.5 if wkswork2 equals 4, 33 if wkswork2 equals 3, 20 if wkswork2 equals 2, and 7 if wkswork2 equals 1. We define an employee as full-time if they report working 35 hours per week or more (uhrswork $\geq$ 35 and 50 or more weeks (wkswork2 equals 6). We calculate wage based income using the variable incwage, and exclude missing values (999998) and N/A values (999999). In the ACS values above the 99.5th percentile in the state are coded as the state mean of values above the threshold. This replacement procedure should have minimal impact on our calculated values, and we report percentiles in addition to the mean difference between counties as a robustness check.

Given the limited sample size in each PUMA, we categorize the industry or employment into 12 categories. We categorize these based on IND1990 categories provided by IPUMS. This enables consistent comparisons across ACS and Census data since 1970, and also with Current Population Survey data. Our categories are: Primary Industries (ind1990 $>=$ 010 and ind1990 $<=$ 050), Construction (ind1990 = 60), Non-durable Manufacturing (ind1990 $>=$ 100 and ind1990 $<=$ 229), Durable Manufacturing (ind1990 $>=$ 230 and ind1990 $<=$ 392), Transport (ind1990 $>=$ 400 and ind1990 $<=$ 472), Wholesale Trade (ind1990 $>=$ 500 and ind1990 $<=$ 571), Retail Trade (ind1990 $>=$ 580 and ind1990 $<=$ 691), Finance (ind1990 $>=$ 700 and ind1990 $<=$ 712), Business (ind1990 $>=$ 721 and ind1990 $<=$ 760), Personal Services (ind1990 $>=$ 761 and ind1990 $<=$ 810), Professional Services (ind1990 $>=$ 812 and ind1990 $<=$ 893), and Public Sector (ind1990 $>=$ 900 and ind1990 $<=$ 932).

We define that someone has migrated in the past year if migrate1 is between 2 and 4, and migrated interstate if migrate1 is equal to three. We subdivide interstate migrants into those moving from the adjacent state to the current PUMA, those moving from RTW states as defined by the 2010 RTW Borders, and those moving from non-RTW states. For migration since birth we create the same breakdown based on the bpl variable.

D.2 Construction of Census variables

We collect data from the full-count census from 1930 and 1940. We limit our sample to individuals aged 16 years or older, who are not in the Armed Forces (defined as empstatd equals 13) and are not living in institutional group quarters (defined as gq equal to 3). We adjust county definitions to match the county definitions utilized in defining the 2010 RTW borders. First, countyfips is defined as countyicp divided by 10. Corrections are made for Maryland counties by subtracting 2 from countyfips when countyicp $\geq$ 90 with the exception of countyicp = 5100. Second, in Nevada we set countyfips = 027 if countyicp = 0250 and countyfip = 025 if countyicp = 0510.

We define prime-aged as individuals aged 25 to 54. We define individuals as single if marst equals 6, married is marst equals 2 or 3, separated / divorced if marst equals 3 or 4, and widowed if marst equals 5. We define individuals as white, non-Hispanic if race equals 1 and hispan equals 0, Black / African-American if race equals 2 and hispan equals 0, Hispanic if hispan is between 1 and 8, and classify all other persons as "Other".

Education variables are only available in the 1940 Census. We define someone has holding less than a High School Diploma if educ less than six (in the 1940 census we cannot differentiate grade 12 no diploma from high school graduates), High School Graduate if educ equals six, Some College if educ between 7 and 9, and 4 year College Graduate if educ equals 10 or 11.

We define someone as employed if empstat equals 1, unemployed if empstat equals 2, and not in labor force if empstat equals 3. Wage income is only available in the 1940 Census. We use incwage variable to capture wage based income, and exclude missing values (999998) and N/A values (999999). Wage income is top-coded at $\$5,001$ in the data. Rather than imputing top-coded values, we report percentiles alongside the mean to account for the potential of different levels of top-coding in bordering counties. We calculate mean reported hours worked using hrswork1 for employed individuals excluding missing values, and similarly weeks worked using wkswork1.

Consistent with our approach in the ACS data, we create 12 industry categories from the microdata. As the IND1990 categories are not available for the 1930 and 1940 Census, we utilize the IND1950 categorization from IPUMS. While IND1950 codes differ significantly from IND1990 codes at the detailed level, there are only small differences when aggregated to our twelve major categories. Our categories are: Primary Industries (ind1950 >= 100 and ind1950 <= 239), Construction (ind1950 = 246), Non-durable Manufacturing (ind1950 >= 406 and ind1950 <= 499), Durable Manufacturing (ind1950 >= 306 and ind1950 <= 399), Transport (ind1950 >= 506 and ind1950 <= 598), Wholesale Trade (ind1950 >= 606 and ind1950 <= 627), Retail Trade (ind1950 >= 636 and ind1950 <= 699), Finance (ind1950 >= 716 and ind1950 <= 756), Business (ind1950 >= 806 and ind1950 <= 817), Personal Services (ind1950 >= 826 and ind1950 <= 859), Professional Services (ind1950 >= 868 and ind1950 <= 899), and Public Sector (ind1950 >= 906 and ind1950 <= 946).

E Additional Tables

Table E.1: *Balance Tests at RTW Border*

Panel A: Demographics					
	(1)	(2)	(3)	(4)	(5)
	Age Mean	Age Median	Age 10th Percentile	Age 90th Percentile	Share Male
Right to Work	-0.5390**	-0.8920***	-0.0240	-0.5349**	0.0003
	(0.2247)	(0.3378)	(0.1088)	(0.2127)	(0.0009)
Control Mean	47.6628	48.0442	21.5514	73.0062	0.4903
PUMA Observations	1944	1944	1944	1944	1944

Panel B: Race / Ethnicity			
	(1)	(2)	(3)
	White	Black	Hispanic
Right to Work	0.0050	-0.0051	-0.0099
	(0.0119)	(0.0050)	(0.0088)
Control Mean	0.7934	0.0389	0.1149
PUMA Observations	1944	1944	1944

Panel C: Education				
	(1)	(2)	(3)	(4)
	Less than High School	High School	Some College	College
Right to Work	-0.0113***	-0.0102*	0.0046	0.0169**
	(0.0039)	(0.0058)	(0.0030)	(0.0081)
Control Mean	0.1321	0.3903	0.2391	0.2385
PUMA Observations	1944	1944	1944	1944

Panel D: Marital Status				
	(1)	(2)	(3)	(4)
	Single	Married	Divorced	Widowed
Right to Work	-0.0103**	0.0197***	-0.0055***	-0.0039***
	(0.0048)	(0.0046)	(0.0017)	(0.0011)
Control Mean	0.2695	0.5239	0.1407	0.0659
PUMA Observations	1944	1944	1944	1944

* p<0.10, ** p<0.05, *** p<0.01

All panels: Sample includes respondents aged 16 years or older. Regressions include county-pair and year fixed effects. Robust spatial standard errors reported. Panel B: Reports share of population age 25 or older by highest educational attainment. Some college refers to attendance at college but without attaining a 4 year degree. Panel C: Reports share of population by primary racial / ethnic affiliation. Panel D: Reports share of population by current marital status. Married includes couples where spouse is present or absent. Divorced includes persons reporting being separated from spouse as well as divorced.

Source: 2012-2017 American Community Survey.

Table E.2: *Unemployment Differential at RTW Borders*

	(1)	(2)
	Unemployment 1976-2017	Unemployment 1990-2017
Right to Work	-0.0038***	-0.0037***
	(0.0011)	(0.0011)
County Pair FE	Yes	Yes
Control Mean	0.0656	0.0614
Observations	31584	21280

* p<0.10, ** p<0.05, *** p<0.01

Regression (1) reports the unemployment differential including Local Area Unemployment data from 1976-1989, which is not consistent with post-1990 data. Regression (2) reports the unemployment differential using data from 1990-2017. Regressions include county-pair and year fixed effects. Robust spatial standard errors reported.

Source: 1976-2017 Local Area Unemployment Statistics.

F Additional Figures

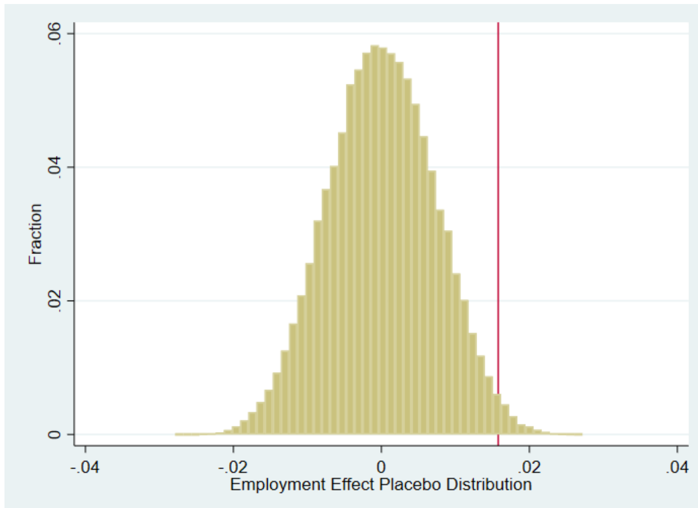

Figure F.1: *Employment-to-Population Ratio Placebo Test*

Note: Figure presents estimated coefficient for county-border difference from placebo test, reassigning treatment status at the state-border pair level. The dependent variable is the employment-to-population rate. Regressions include county-pair and year fixed effects.
Data source: 1990-2017 Local Area Unemployment Statistics and US Census Bureau intercensal population estimates

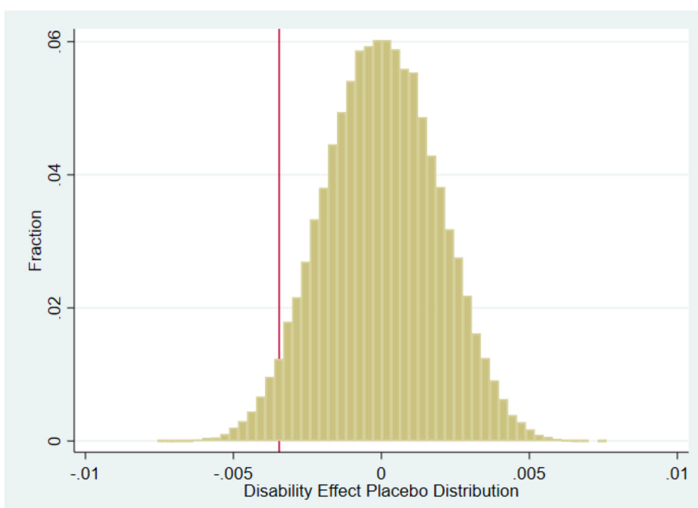

Figure F.2: *Disability Placebo Test*

Note: Figure presents estimated coefficient for county-border difference from placebo test, reassigning treatment status at the state-border pair level. The dependent variable is disability insurance enrollment. Regressions include county-pair and year fixed effects.
Data source: 2009-2018 SSA Disability Data.

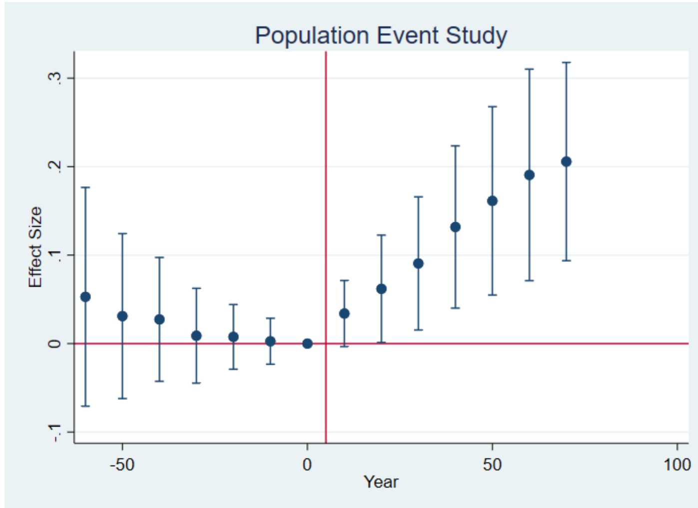

Figure F.3: *Population Event Study*

Note: Figure presents estimated coefficient for RTW status interacted with years since RTW policy adoption. Since population data is only available every 10 years, for policy switches between decennial censuses we formally normalise years since policy adoption to have the base year be the census year prior (inclusive) to the policy change. Regressions include county-pair by year and county-pair-county fixed effects.
Data source: 1880-2010 US Census Bureau Decennial Censuses.

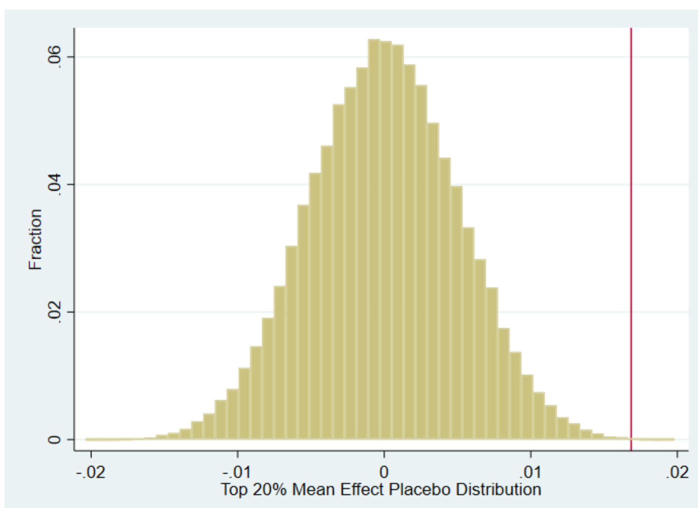

Figure F.4: *Top Quintile Mobility Placebo Test*

Note: Figure presents estimated coefficient for county-border difference from placebo test, reassigning treatment status at the state-border pair level. The dependent variable is the unweighted share of children entering the top quintile of family income. Regressions include county-pair fixed effects.
Data source: Opportunity Insights, Chetty et al. (2018).

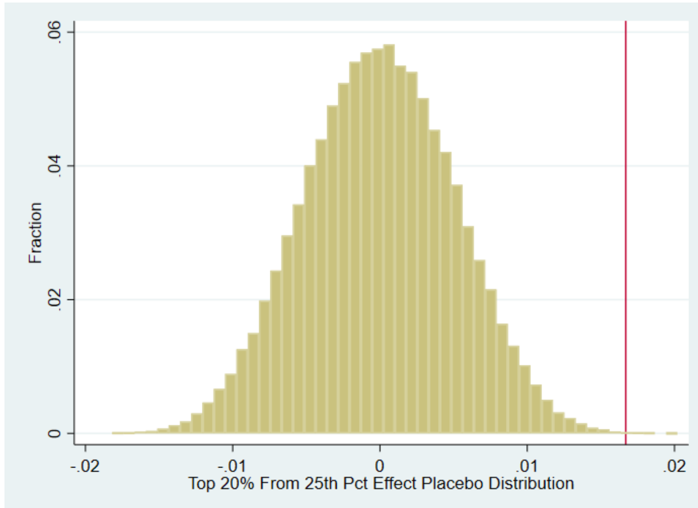

Figure F.5: *Top Quintile Mobility of 25th Percentile Placebo Test*

Note: Figure presents estimated coefficient for county-border difference from placebo test, reassigning treatment status at the state-border pair level. The dependent variable is the share of children entering the top quintile family income rank in households with parental income at the 25th percentile. Regressions include county-pair fixed effects.
Data source: Opportunity Insights, Chetty et al. (2018).

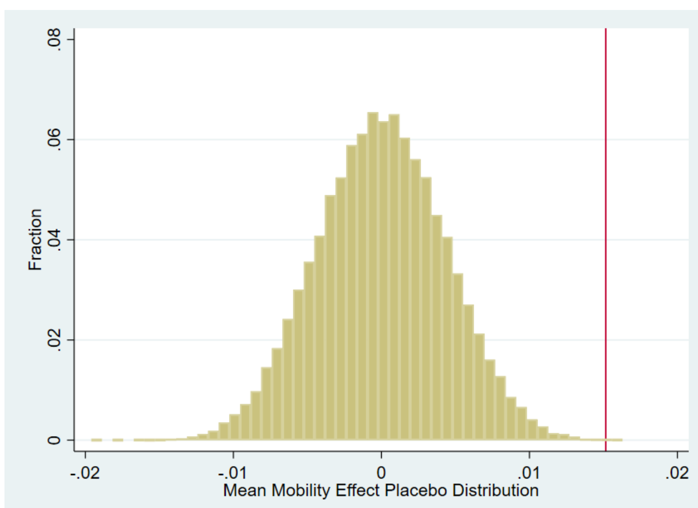

Figure F.6: *Mean Mobility Placebo Test*

Note: Figure presents estimated coefficient for county-border difference from placebo test, reassigning treatment status at the state-border pair level. The dependent variable is the unweighted mean family income rank of children. Regressions include county-pair fixed effects.
Data source: Opportunity Insights, Chetty et al. (2018).

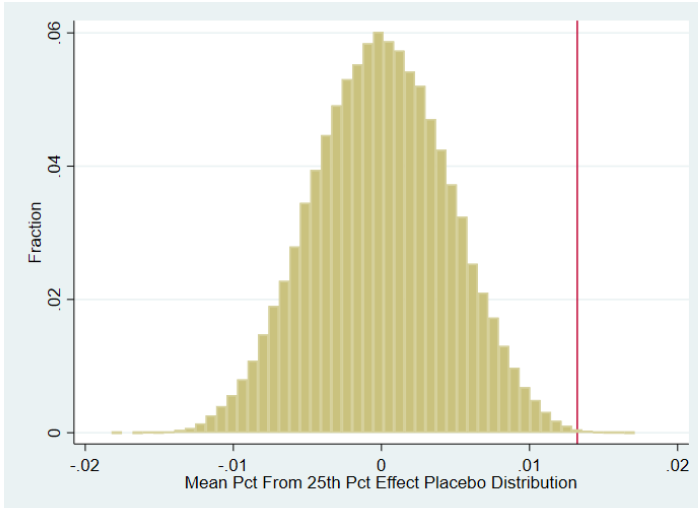

Figure F.7: *Mean Mobility of 25th Percentile Placebo Test*

Note: Figure presents estimated coefficient for county-border difference from placebo test, reassigning treatment status at the state-border pair level. The dependent variable is the mean family income rank of children with household parental income at the 25th percentile. Regressions include county-pair fixed effects.
Data source: Opportunity Insights, Chetty et al. (2018).

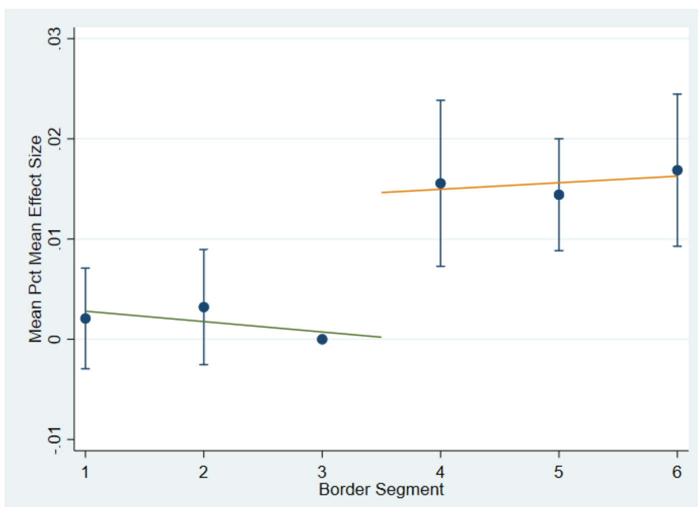

Figure F.8: *Mean Mobility County Border Differences*

Note: Figure presents estimated average county-difference coefficients and 95% confidence intervals for county border segment waves. The dependent variable is the unweighted mean family income rank of children. The state-border is between segment 3 and 4. Segments 1-3 represent counties in non-RTW state and segments 4-6 represent counties in RTW state. Coefficients normalized such that the non-RTW state border county (segment 3) is zero. Regressions include county-pair fixed effects.
Data source: Opportunity Insights, Chetty et al. (2018).

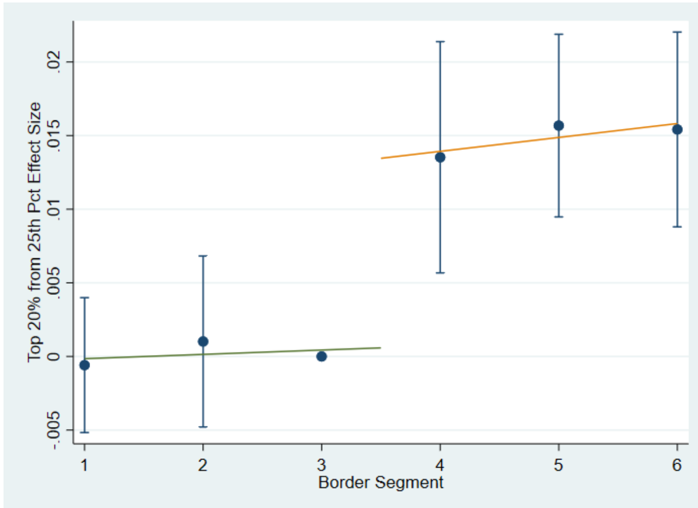

Figure F.9: *Mean Mobility into Top Quartile of 25th Percentile County Border Differences*

Note: Figure presents estimated average county-difference coefficients and 95% confidence intervals for county border segment waves. The dependent variable is the share of children entering the top quintile family income rank in households with parental income at the 25th percentile. The state-border is between segment 3 and 4. Segments 1-3 represent counties in non-RTW state and segments 4-6 represent counties in RTW state. Coefficients normalized such that the non-RTW state border county (segment 3) is zero. Regressions include county-pair fixed effects.
Data source: Opportunity Insights, Chetty et al. (2018).

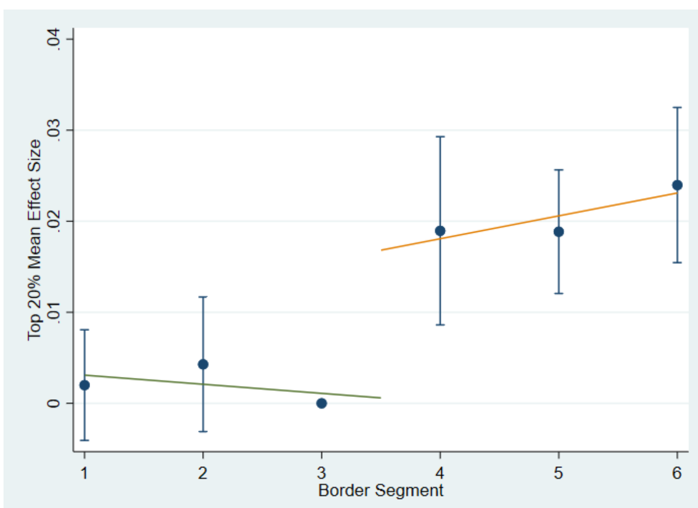

Figure F.10: *Mean Mobility into Top Quartile County Border Differences*

Note: Figure presents estimated average county-difference coefficients and 95% confidence intervals for county border segment waves. The dependent variable is the unweighted share of children entering the top quintile of family income. The state-border is between segment 3 and 4. Segments 1-3 represent counties in non-RTW state and segments 4-6 represent counties in RTW state. Coefficients normalized such that the non-RTW state border county (segment 3) is zero. Regressions include county-pair fixed effects.
Data source: Opportunity Insights, Chetty et al. (2018).