Labor Market Institutions and the Distribution of Wages:

The Role of Spillover Effects[*]

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ABSTRACT:

This paper updates and extends the DiNardo, Fortin, and Lemieux (1996) study of the links between labor market institutions and wage inequality in the United States. A notable extension quantifies the magnitude and shape of spillover effects from minimum wages and unions, providing multiple sources of evidence for the later. A distribution regression framework is used to estimate both types of spillover effects jointly. Accounting for spillover effects double the explanatory power of de-unionization towards male wage inequality, and raise that of minimum wages to two-thirds of the increase in bottom wage inequality for women.

1. Introduction

A vast literature has investigated the causes of the substantial and continuing growth in wage and earnings inequality in the United States. Although most studies suggest that various forms of technological change are a leading explanation for these changes (see, e.g., Acemoglu and Autor 2011), other explanations such as changes in labor market institutions have been implicated. For instance, DiNardo, Fortin, and Lemieux (1996, DFL from thereafter) show that the decline in the real value of the minimum wage during the 1980s helps accounts for a significant fraction of the growth in wage inequality at the bottom of the distribution during this period. Card (1996), Freeman (1993) and DFL also show that the decline in the rate of unionization contributed to the rise in male wage inequality over the same period. Card, Lemieux, and Riddell (2004) and Firpo, Fortin, and Lemieux (2018) find that the continuing decline in unionization after the late 1980s accounts for some of the continuing growth in inequality, while Farber, Herbst, Kuziemko, and Naidu (2018) reach a similar conclusion using data going back to the 1940s.

One important limitation of the earlier literature is that it typically ignored potential spillover effects of institutional changes. These could magnify the impact of such changes on the wage distribution. In an influential study, Lee (1999) indeed shows that accounting for spillover or "ripple" effects of the minimum wage on the wage of workers earnings slightly above the minimum substantially increases the impact of the minimum wage on the wage distribution. For instance, Lee (1999) finds that about half of the increase in the standard deviation of log wages, and almost all of the increase in the 50-10 differential between 1979 and 1989 can be explained by the decline in the minimum wage once spillover effects are taken into account. Lee's estimates of the contribution of the minimum wage to inequality growth are substantially larger than those found by DFL who ignore spillover effects; although they have been recently challenged by Autor, Manning, and Smith (2016). DFL find that the decline in the minimum wage explains about a quarter of the increase in the standard deviation of log wages between 1979 and 1988 (25% for men and 30% for women), and about 60% of the increase in the 50-10 differential.

On the other hand, existing studies of the impact of de-unionization on wage inequality, ignore possible spillovers effects of unionization. The existing decompositions typically assume that the observed non-union wage structure provides a valid counterfactual for how union workers would be paid in the absence of unionization. It has long been recognized, however, that union power as measured by the unionization rate (or related indicators) may also influence wage

setting in the non-union sector (e.g., Lewis, 1963). In particular, non-union employers may seek to emulate the union wage structure to discourage workers from supporting unionization. This "threat effect" (Rosen, 1969) likely increases the equalizing effects of unionization by making non-union wages more similar to the more equally distributed ones observed in the union sector. Based on cross-country evidence, Freeman (1996) conjectures that existing estimates of the effect of de-unionization are biased down for failing to take account of threat effects. Taschereau-Dumouchel (2017) reaches a similar conclusion by calibrating a search model of the U.S. economy.

Empirical evidence on the distributional impact of threat effects is limited by the challenge of finding exogenous sources of variation in the rate of unionization rate (the conventional measure of threat effects) across labor markets. Older studies such as Freeman and Medoff (1981), Moore et al. (1985), and Podbursky (1986) estimate threat effects by including the unionization rate in the relevant market (defined by industry, occupation, geography, and so on) in a standard wage regression, but only make limited attempts at controlling for possible confounding factors.

One exception is Farber (2005) who uses the passage of "right-to-work" laws in the states of Idaho (1985) and Oklahoma (2001) as an arguably exogenous source of variation in union power. Unfortunately, Farber's results based on Current Population Survey (CPS) data are inconclusive because of a lack of statistical power linked to the small samples available in these two states.

The contribution of this paper is twofold. First, we update DFL's analysis all the way to 2017 to see whether changes in labor market institutions have remained an important source of inequality change over the last 25 years. Second, we extend DFL by taking account of spillover effects linked to ripple effects of the minimum wage and threat effects of unionization. In the case of the minimum wage, we depart from Lee (1999) and Autor, Manning, and Smith (2016) by estimating a rich model of the wage distribution using distribution regressions (Foresi and Peracchi, 1995, Fortin and Lemieux, 1998, Chernozhukov et al., 2013). The model can be thought of as a distributional difference-in-differences approach that yields estimates of spillover effects regardless of whether the minimum wage varies at the state or federal level. We consider several estimation strategies in the case of the union threat effects. We first extend Farber (2005)'s approach by studying the impact of the introduction of right-to-work laws in

three large Midwestern states (Indiana, Michigan, and Wisconsin) since 2011. We then use a difference-in-differences strategy where the effect of the unionization rate by state and industry on wages is estimated using models that also include rich sets of controls for state, industry, year and state and industry trends. We also consider an alternative strategy where the rate of success of union organizing drives captures the threat effect. The identification in these models is mostly driven by variation in the rate of decline in the unionization rate by state-industry categories. We then estimate the distributional impact of de-unionization by combining this estimation strategy with the distributional regression approach developed in the case of the minimum wage.

Our key findings are as follows. First, we estimate minimum wage spillovers effects that are roughly as large as those found by Lee (1999) for the 1980s, though the magnitude of spillover effects are smaller in subsequent years. These differences partly reconcile the divergence in results between Lee (1999) and Autor, Manning, and Smith (2016) who found smaller spillover effects using data from more recent years. Second, we find that changes in the minimum wage accounts for most of the substantial growth in lower tail inequality (50-10) in the 1980s, and its relative stability since then. Our main finding concerning the impact of unions is that spillover effects of unionization on non-union wages are similar in shape and magnitude to the direct impact of unionization linked to differences in union and non-union wage structures. The effects are largest in the lower middle of the distribution, but negative at the top. Adding spillover effects roughly doubles the contribution of de-unionization to the growth in wage inequality. For instance, in the case of men, the contribution of unions to the steady growth in the 90-50 gap over the entire 1979-2017 period goes from 20% to 40% when spillover effects are also taken into account. Overall, we find that changes in labor market institutions account for 53% and 28% of the 1979-2017 growth in the standard deviation of log wages for men and women, respectively.

The remainder of the paper is organized as follows. In Section 2, we propose a distribution regression approach to estimate the spillover effects of the minimum wage. Several estimation strategies for union threat effects, including one also based on distribution regressions, are presented in Section 3. We present the data and estimation results in Section 4 and use decompositions to compute the contribution of changing institutions to the wage distribution in Section 5. We conclude in Section 6.

2. Estimating spillover effects of the minimum wage

A key contribution of DFL was to present visual evidence based on kernel density estimates to illustrate the role of the decline in the real value of the minimum wage in the growth of wage inequality between 1979 and 1988. DFL then made two main assumptions to quantify the contribution of the minimum wage to inequality growth. First, they assumed that the changes in the minimum wage had no effect on employment. At the time, the recent work of Card and Krueger (1995) was used in support of the assumption of no employment effect. DFL also showed that allowing for modest employment effects had little impact on the findings. Recent work by Brochu et al. (2018) based on Canadian data show substantial spillover effects even after controlling for employment effects using a hazard rate estimation approach. Cengiz et al. (2018) also find evidence of spillovers and no employment effects using a distributional event study approach.

More importantly, they also assumed there were that minimum wages had no spillover effects. This assumption allowed DFL to use a simple "tail pasting" approach where the bottom end of the distribution in a low minimum wage year (1988) is replaced by the bottom end of the distribution in a high minimum wage year (1979).

Lee (1999) relaxed the assumption of no spillover effects by exploiting the fact that a common federal minimum wage is relatively higher in low-wage than high-wage states. The basic estimation approach consists of running flexible regressions of selected wages percentiles relative to the median on the relative value of the minimum wage by state and year. This involves running regressions of $w_{st}^q - w_{st}^{.5}$ on a polynomial function in $mw_{st} - w_{st}^{.5}$, where w_{st}^q is the qth percentile of log wages in state s at time t, while mw_{st} is the corresponding value of the minimum wage. The term $mw_{st} - w_{st}^{.5}$ can be thought of as the relative "bite" of the minimum wage in different states. The minimum wage "bites" more in low-wage states where $mw_{st} - w_{st}^{.5}$ is larger than in high-wage states where it is lower

Using this approach, Lee finds that that the minimum wage had an impact on wage percentiles above and beyond the corresponding value of the minimum wage. He concludes that most of the change in inequality in the lower tail of the distribution between 1979 and 1989 can be explained by changes in the minimum wage once spillover effects are taken into account.

This finding has been challenged by Autor, Manning and Smith (2016) who point out that sampling error in the estimated median wage $w_{st}^{.5}$ can positively bias estimates of Lee-type regressions as the noisily measured median is included on both sides of the regression. They suggest correcting for this problem by instrumenting the right-hand side variable $mw_{st} - w_{st}^{.5}$ with the value of the minimum wage mw_{st}. As Lee-type regressions also include year dummies, this strategy can only work in periods where there is substantial variation in the state minimum wage (variation in the federal minimum wage is fully absorbed by time dummies). Autor, Manning and Smith (2016) take advantage of the large variation in state minimum wages after the 1980s (see Figure 1) to revisit Lee's estimates, and find substantially smaller spillover effects.

One alternative interpretation of these findings is that Lee's estimates of spillover effects were not substantially biased, but that spillover effects have become smaller over time. It is indeed unclear that the more frequent and smaller changes in state minimum wages of the post-1980s period have a comparable impact to the large (over 30%) and permanent decline in the real value of the federal minimum wage that took place during the 1980s. For example, a large and permanent change in the minimum wage may affect the composition of firms at the lower end of the wage distribution. Butcher et al. (2012) show that when firms have monopsony power, spillover effects can arise as unproductive firms shut down when the minimum wage increases, and workers who used to work for those firms move to more productive, and higher-paying, firms. Such a reallocation channel is unlikely to take place for smaller and more transitory changes in the minimum wages. Spillover effects may still arise because of internal wage considerations (Grossman, 1983, Dube et al., 2018), but the magnitude of the spillover effects may be smaller than when longer term labor re-allocation effects are involved too.[1]

In what follows we propose a new estimation approach based on distribution regressions that make it possible to estimate minimum wage spillover effects regardless of whether the minimum wage varies at the state or federal level. Intuitively, Lee (1999) uses a two-step procedure by estimating features of the distribution like the median in a first step and plugging it in a regression model for wage percentiles in a second step. Autor, Manning and Smith (2016) then propose an IV procedure to correct the bias linked to the fact a noisy measure is plugged

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[1] See Brochu et al. (2018) for a more thorough discussion of possible economic explanations for minimum wage spillover effects.

into the second step estimation. By contrast, in our approach we jointly estimate the wage distribution and the impact of the minimum wage in a single step. As a result, our approach does not yield biased results because of estimated regressors.

3.1 Distribution regressions

Following Foresi and Peracchi (1995) and Chernozhukov et al. (2013), we use a distribution regression approach to model the whole wage distribution and the effects of the minimum wage at different points of the distribution. The logic is very simple. The probability of an outcome variable *y* being above (or below) a given cut point y_k is modeled as a flexible function of covariates *X*, and estimated using a probit, logit, or linear probability model. For example, in the case of a probit model we have:

$$\text{Prob}(Y \geq y_k) = \Phi(X\beta_k) \text{ for } k=1,2,...,K. \tag{1}$$

The y_k cutoffs can either be chosen using a fine grid, or as percentiles (*k=1,2,...,99*) of the unconditional wage distribution. The method is quite flexible as rich functions of the covariates (including state and year dummies) can be included as regressors, and no restrictions are imposed on how β_k varies across cutoff values. Once the series of distribution regressions have been estimated, various counterfactual scenarios can be computed by either changing the distribution of the covariates or some the β_k coefficients.

The flexibility of distribution regressions come at a cost, however, as there is no guarantee to get positive counterfactual probabilities, especially when the set of covariates is large. More importantly, having completely unrestricted coefficients across each cutoff y_k means that different effects of the minimum wage need to be estimated at each point of the distribution. As we discuss below, the effect of the minimum wage will be modelled using a set of dummy variables indicating where the minimum wage stands (at, below, or above) relative to a given cutoff point y_k. Allowing for separate minimum wage effects at each cutoff would likely be an overly flexible approach yielding identification challenges.

In light of these issues, we impose some restrictions on the β_k coefficients by letting them evolve in a smooth way over the wage distribution. Doing so also helps provide an economic interpretation to distribution regressions. To see this, consider the special case where

the β_k's are fixed across the distribution. This corresponds to the "rank regression" model proposed by Fortin and Lemieux (1998) that can easily be estimated using an ordered probit model. Consider a latent wage or skill index $Y^* = X\beta + \varepsilon$, where $\varepsilon \sim N(0,1)$. The observed wage is assumed to be a monotonic transformation $Y = g(X\beta + \varepsilon)$ of the skill index. Fortin and Lemieux (1998) call this a "rank regression" model as the only restriction being imposed is that the rank of an observation in the wage and skill distribution are the same.

The model is flexibly estimated by dividing the wage range into a fine grid. Fortin and Lemieux (1998) use about 200 cutoff point y_k. The corresponding cutoff points in the skill distribution, c_k, are defined as $c_k = g^{-1}(y_k)$. It follows that:

$$\text{Prob}(Y \geq y_k) = \Phi(X\beta - c_k).$$

This corresponds to a standard ordered probit model where the probability of observing wages in a wage category $[y_{k,}y_{k+1}]$ is given by:

$$\text{Prob}(y_k \leq Y < y_{k+1}) = \Phi(X\beta - c_{k+1}) - \Phi(X\beta - c_k).$$

When the transformation function g(.) is linear, it follows that:

$$Y = \sigma \cdot (X\beta + \varepsilon) = X\beta' + u,$$

where $\beta' = \sigma\beta$ and $u = \sigma\varepsilon$ is a homoskedastic normal error term with a standard deviation of σ. It also follows that the cutoff points in the ordered probit model, c_k, are a linear function $c_k = y_k/\sigma$ of the wage cutoffs y_k. Fortin and Lemieux (1998) find that the relationship between c_k and y_k is reasonably linear for values above the minimum wage, but is clearly non-linear around the value of the minimum wage.[2]

While log normality may not be a bad approximation of the conditional wage distribution, the homoskedasticity assumption is strong and clearly violated in wage data (see,

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[2] Bunching of wages at the minimum wage means that a substantial fraction of observations lie in a narrow wage interval. Say the minimum wage is in the wage interval $[y_m, y_{m+1}]$. To fit the data, we need a much larger gap between c_m and c_{m+1} than between other values of the c_k's , which generates a local flat spot in the relationship between y_k and c_k.

e.g. Lemieux, 2006). For the rank regression model to fit reasonably well the data, it is thus essential to allow for heteroscedasticity in the error term ε. To see how this changes the probability model, consider a simple case where individual belong to two possible groups, for instance high school ($X = 0$) and college ($X = 1$) graduates. Assume that wages are log normally distributed with a different mean and variance for the two groups:

$$Y = \beta_0 + \varepsilon \text{ with } \varepsilon \sim N(0, \sigma_0) \text{ for } X = 0, \text{ and}$$
$$Y = \beta_1 + \varepsilon \text{ with } \varepsilon \sim N(0, \sigma_1) \text{ for } X = 1.$$

It follows that

$$\text{Prob}(Y \geq y_k | X) = \begin{cases} \Phi\left(\dfrac{\beta_0}{\sigma_0} - \dfrac{y_k}{\sigma_0}\right) & \text{if } X = 0 \\ \Phi\left(\dfrac{\beta_1}{\sigma_1} - \dfrac{y_k}{\sigma_1}\right) & \text{if } X = 1 \end{cases}$$

$$= \Phi\left[\beta_0' + X\beta + c_k + X\left(\frac{1}{\sigma_1} - \frac{1}{\sigma_0}\right)y_k\right] \tag{2}$$

where $c_k = y_k/\sigma_0$, $\beta_j' = \frac{\beta_j}{\sigma_j}$, $\beta = \beta_1' - \beta_0'$ is the main effect of education, and $\left(\frac{1}{\sigma_1} - \frac{1}{\sigma_0}\right)$ is the coefficient on the interaction between X and y_k. In other words, introducing heteroskedasticity leads to a specification where the effect of education varies in a smooth (linear) way over the wage distribution.[3]

The heteroskedastic model provides a middle ground between distribution regressions where β_k is allowed to vary in a completely unrestricted way, and the rank regression model where β_k is constrained to be the same (except for the intercept) at each cutoff point y_k. Although we only use linear interactions in the empirical applications used here, one could also imagine including a more flexible set of interaction between X and polynomial functions in y_k. This could help accommodate further departures from log-normality such as skewness in the wage distribution.

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[3] An alternative interpretation is that the cutoff points in the ordered probit model are now $c_k + X\left(\frac{1}{\sigma_1} - \frac{1}{\sigma_0}\right)y_k$, and depend on the value of the covariate X.

3.2 Empirical implementation

After various experimentations, we settled on an empirical model where the wage distribution is divided into 58 intervals of width 0.05.[4] As we are constraining the coefficients to change smoothly across wage cutoffs, the model is estimated by jointly fitting 57 "stacked" probit regressions. The covariates used in the estimation consists of a set of state and year effects, state specific trends as well as a rich set of individual characteristics similar to those use by DFL. They include years of education, a quartic in potential experience, experience-education interactions (16 categories plus experience times education), race and marital status, public sector, part-time status, 11 industry categories and 4 occupation categories. In light of the above discussion, we also include interactions between the covariates and the cutoff points y_k.

The minimum wage effects are captured by a set of dummy variables indicating where the prevailing minimum wage stands relative to a given wage cutoff. For each cutoff value y_k, we first create an "at the minimum" dummy D_{ist}^0 that is equal to 1 when the minimum wage faced by workers i in state s at time t is between y_k and y_{k+1}. We also create a set of up to six dummies to capture possible spillover effects of the minimum wage. For example, the dummy for being at one wage bin above the minimum wage, D_{ist}^1, is set to one whenever the minimum wage is between y_{k-1} and y_k.

Likewise, we create a set of three "below the minimum" dummies to capture the large decline in the probability below the minimum wage. D_{ist}^{-1} is set to one whenever the minimum wage is between y_{k+1} and y_{k+2}, D_{ist}^{-2} is set to one whenever the minimum wage is between y_{k+2} and y_{k+3}, and D_{ist}^{-3} is set to one for all observations for which the minimum wage is above y_{k+3}. The resulting probit models being estimated are:

$$\text{Prob}(Y_{ist} \geq y_k) = \Phi(Z_{ist}\beta + y_k Z_{ist}\lambda + \sum_{m=-3}^{6} D_{ist}^m \varphi_m - c_k), \text{ for } k = 1,..,57, \quad (3)$$

where $Z_{ist}\beta = X_{ist}\beta_x + \theta_s + \gamma_t + t \cdot \pi_s$ ($Z_{ist}\lambda$ is similarly defined). Standard errors are clustered at the state level to allow for correlation across the 57 probit models, and for autocorrelation over time.

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[4] For over 99% of observations the years 1979 to 2017, the log wage falls in the range going from 1.6 ($4.95) to 4.4 ($81.50). All wages were converted into dollars of 2017. There are 56 intervals of width 0.05 going from 1.6 to 4.4, plus two intervals for log wages below 1.6 or above 4.4.

3.3 Identification:

As mentioned earlier, the distribution regression model is identified regardless of whether the prevailing minimum wage is set at the federal or state level. This may be surprising at first glance since the model in equation (3) includes a full set of state and time dummies, where the latter absorbs all the variation in the federal minimum wage. As it turns out, only allowing for a smooth change in the probit coefficient across wage cutoffs plays an essential role in the identification when the minimum wage only varies at the federal level. The reason is that for a probit model at a given cutoff point y_k, the time effects capture all the variation in the federal minimum wage. Allowing for an unrestricted set of time effects γ_t^k for each cutoff point y_k would indeed make it impossible to identify the distributional effects of the federal minimum wage.

That said, such an approach would be overly flexible in light of the above discussion on the economic interpretation of the coefficients in the distribution regression. Going back to the example in equation (2), if X was a time instead of an education dummy, the main effect β would capture a shift in mean wages over time, while the coefficient $(\frac{1}{\sigma_1} - \frac{1}{\sigma_0})$ on the interaction between X and y_k would capture changes in the variance over time. One could also go further by including interaction terms between X and polynomial functions of y_k that would capture changes in moments of the wage distribution besides the mean and the variance. The implication would remain that time effects should only smoothly vary across the various cut points y_k of the distribution.

Identification of minimum wage effects is now possible as the minimum wage "bites" at different points of the distribution at different times, a feature of the wage distribution that cannot be captured by smoothly varying time effects. Intuitively, the minimum wage creates a sharp discontinuity in the probability of being just above and just below the value of the minimum. As in Doyle (2006) and Jales (2018), identification can be achieved as in a regression discontinuity design provided that the underlying latent wage distribution is smooth around the value of the minimum wage. Constraining the coefficient of the distribution regression to change smoothly across the various cut points y_k implies that the latent distribution is also smooth.

Having established that the model is identified even in the case where the minimum wage only varies at the federal level, we illustrate in Figure 2 how the effect of the minimum wage on the wage distribution maps into the parameters φ_m of the model. Consider a latent normal wage

distribution in Figure 2a (blue line). We now add a minimum wage (red line) that creates a large spike at the minimum, adds some mass slightly above the minimum wage (spillover effects), and greatly reduces the probability of being at values below the minimum wage. Figure 2a shows that the probability of being in the "spillover zone" just about the minimum increases from A to A+C, while the probability of being at the spike increases from B to B+D. In this simple example, the parameters φ_1 and φ_0 are the horizontal values (illustrated by arrows in Figure 2b) by which the cutoff points have to be move to increase the two probabilities by an amount of C and D, respectively.

Next, Figures 2c and 2d illustrate a case with two states that differ in terms of mean wages. If we use the dummy variable *X* in equation (2) to indicate if an observation comes from the high-wage state, the parameter β will capture the mean wage difference between the two states. The three key parameters to be estimated in this example are β (the difference in means) along with the minimum wage parameters φ_1 and φ_0. As discussed at the beginning of this section, these parameters are jointly estimated in our estimation approach, while corresponding parameters are estimated in two separate steps in Lee (1999) and Autor, Manning, and Smith (2016).[5]

The better understand how φ_1 and φ_0 are estimated in the two states example, Figure 2d shows the recentered densities obtained using the parameter – or adjustment factor – β. The recentering clearly shows how the same federal minimum wage bites at different points of the distribution in the two states. An exactly similar graph would be obtained if the two states had the same latent wage distribution but different state wage minimum wages. Thus, from an identification perspective, it does not matter whether the variation in the relative minimum wage is driven by differences in mean wages across states (as in Lee, 1999), or difference in state minimum wages (as in Autor, Manning and Smith, 2016). The parameters φ_1 and φ_0 correspond again to horizontal moves in cutoff values (arrows in Figure 2d) required to fit the change in probabilities induced by the minimum wage. Interestingly, the same horizontal move has a larger impact on probabilities when the minimum wage is relatively higher up in the distribution (low-

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[5] The model parameters are quite different in the two approaches since we are modelling the probability distribution, while Lee (1999) and Autor, Manning, and Smith (2016) are modelling quantiles of the wage distribution. The β parameters in equation (2) are, nonetheless, closely connected to the "first-step" median used in these two papers to compute the relative value of the minimum wage. The measurement error linked to plugging in estimates of the medians doesn't apply as we are jointly estimating similar centrality parameters and minimum wage effects.

wage state case in Figure 2d). This convenient property is linked to the well-known fact that marginal effects in a probit model are directly proportional to the density at the point where the marginal effects are being computed. As in Lee (1999), the relative bite of the minimum --its distance relative to the median-- also plays a central role in the estimation in the distribution regression model.

3. Union threat effects

We use two approaches to assess the importance of union threat effects. The first approach relies on an event-study design to look at the impact of states introducing "right-to-work" laws (Farber, 2005). We focus on the case of three relatively large Midwestern states, Indiana, Michigan, and Wisconsin, that did so in 2011, 2013, and 2015, respectively.[6][7]

The second and main approach uses the unionization rate at the state-industry-year level as a measure of the (declining) threat of unionization. An important advantage of this approach is that it can easily be incorporated in the distribution regression approach proposed in Section 2 by adding the unionization rate at the state-industry-year level to the list of covariates included in the model.

3.1 Event study of the introduction of right-to-work laws

Under the 1935 National Labor Relations Act, all U.S. workers covered by a collective bargaining receive the same benefits from unionization including compensation, benefits, and access to grievance procedures regardless of whether they are members of the union. In most states, workers covered by a collective agreement have to pay union dues (typically withheld from paychecks by employers) regardless of whether they decide to become members of their union.

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[6] For public workers in the state of Wisconsin we use 2011 as the date of the introduction of right-to-work laws. Under Governor Scott Walker, the State introduced a law (Bill 10) in June 2011 that suspended collective bargaining and made union dues contribution voluntary in the public sector. However, it took several years for the law to have a full impact as provisions only started binding upon expiration of existing collective bargaining agreements.

[7] We also investigate right-to-work policy changes in Oklahoma (2001), West Virginia (2016), and Kentucky (2017). However, the geographical proximity and timing of the Indiana, Michigan, and Wisconsin policy changes provide a better setting for our study.

However, following the passage of the Taft-Hartley Act in 1947, it became possible for States to introduce so-called "right-to-work" (RTW) laws making it no longer compulsory for workers covered under a collective bargaining agreement to pay their union dues. As shown in Figure 3, several (mostly Southern) states quickly adopted RTW around that time. A few states then adopted RTW laws in 1950s, 1960s, and 1970s. The impact of these RTW adoptions cannot be studied using micro data on union status and wages that only became available with a full set of state indicators in the late 1970s.

The next two RTW adopters, Idaho (1985) and Oklahoma (2001), were studied by Farber (2005) who could not draw informative conclusions because of the statistical imprecision linked to the small CPS sample sizes in these two small states.[8] In this paper, we take advantage of the introduction of RTW laws in the three relatively larger Midwestern states of Indiana, Michigan, and Wisconsin. Although two other states, Tennessee and West Virginia, have also adopted RTW laws very recently, we don't consider these two states in our analysis due to the very short time span available for the post-adoption analysis. For the same reason, it is not yet possible to study the impact of a recent Supreme Court decision (Janus case, June 2018) that has imposed RTW to the entire U.S. public sector.

RTW laws weaken union by allowing free riding by workers covered under a union contract. For instance, recent work by Feigenbaum, Hertel-Fernandez, and Williamson (2018) shows that the passage of RTW laws had an adverse impact on union finances and campaign contributions.[9] As in Farber (2005), we expect that by reducing union power, RTW laws should have a negative impact on unionization rate and non-union wages due to declining threat effects. Indeed, Figure 4 shows that unionization rates are much lower in RTW relative to non-RTW states. As these differences could simply reflect cross-states differences in confounding factors, we adopt an event-study approach in Section 4 to isolate the impact of RTW laws on state unionization rates and on the wages of union and non-union workers.

In principle, RTW laws could also be used as an instrumental variable in a regression of non-union (and union) wage on state unionization rate. Resulting estimates could then be used to compute the contribution of declining unionization rates to change in the distribution of wages of

[8] Some studies include a change to the RTW laws in Texas in 1993 as an additional source of policy variation (Taschereau-Dumouchel, 2017). Texas's original RTW legislation was introduce in 1947 and we group the state with earlier adopters.
[9] Ellwood and Fine (1987) show that RTW laws have an adverse impact on union organizing activities.

non-union workers.[10] This approach could potentially provide a way of quantifying the role of declining threat effects on the wage distribution.

As we show in Section 4, statistical imprecision makes it challenging to use the event-study estimates to compute the contribution of threat effects to changes in wage inequality. The main contribution of that part of the paper is thus to provide some evidence supporting the view that threat effects are a significant factor in wage setting, as opposed to a spurious consequence of the fact unionization rates at the state-year level may be correlated with omitted factors.

3.2 Measuring threat effects in a distributional context using the unionization rates

A more traditional way of estimating union threat effects is to run wage regressions where the unionization rate in the relevant labor market is used as a proxy for threat effects. Older studies based on cross-sectional data or short repeated cross sections have generally found that the unionization was positively correlated with the wages of non-union workers.[11] An important advantage of this approach is that it can be readily adapted to a distributional context by including the rate of unionization as a regressor in the distribution regressions introduced in Section 2. Separate impacts for union and non-union workers can be obtained by estimating separate distribution regressions for these two groups of workers.

However, a major challenge with the approach is that the unionization rate may be correlated with other factors that have a direct impact on wages. For instance, states with more profitable ("high rent") industries may pay higher wage and have higher unionization rates. We address this important challenge in several ways. First, we define the relevant labor market at the state-industry level and include a rich set of controls to capture potential confounding factors. These include state fixed effects, industry fixed effects, and state and industry trends. The main source of identifying information left is state-industry specific trends in unionization rates and wages.

For example, consider the case of two industries (manufacturing and services) in two states (Michigan and South Carolina). Including state and industry trends and fixed effects controls for the fact that, for instance, wages and unionization rates may be declining faster in

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[10] One would actually need to go beyond simple regressions to look at distributional impacts. This could be done, for instance, by adapting the distribution regression approach to the case where there is an endogenous regressor.
[11] See, for instance, Freeman and Medoff (1981) and Podgursky (1986).

Michigan than in South Carolina because of negative shocks in the manufacturing sector that account for a large share of employment in Michigan. Thus, our empirical strategy relies on variation linked to the fact that unionization in the manufacturing sector may be declining faster in Michigan than in South Carolina. We then look at whether this faster decline in the unionization rate is linked to a faster decline in the wages of non-union workers in the Michigan manufacturing sector.

Of course, it is possible that there are state-industry specific shocks that affect both wages and unionization rates. If so, there are no particular reasons to believe these shocks would have very different impacts at different points of the distribution. By contrast, the union wage effects literature (e.g. Card, 1996) indicates that unions have a relative large impact on the wages of workers in the middle (or bottom) of the distribution, but little or even a negative impact on workers at the top of the distribution. Based on this evidence, it is natural to expect that union threat effects should be much larger in the middle or bottom of the distribution than at the top. Finding such a pattern would be more supportive of a story based on threat effects of unions than unmodelled state-industry shocks.

An additional way of addressing these concerns is to use information about a state's RTW status to help predict the evolution of unionization across states and industries. More specifically, Figure 4 shows that the unionization rate is very low in some industries (e.g. services and trade) regardless of whether a state is RTW. By contrast, there is a much larger gap between RTW and non-RTW states in high-unionization rate industries like manufacturing, construction, transportation, education and public administration. Figure 4 suggests that unionization rates by industry and RTW status in the base period (1979) is an excellent predictor of the decline in the rate of unionization by state and industry. This suggests using the interaction of time, RTW status and industry as an instrument for state-industry trends in unionization. Preliminary results suggest that IV estimates based on this strategy are not statistically different from the OLS estimates, which further supports the validity of our research design.

We also present results obtained when the unionization rate is replaced by the success rate of union organizing campaigns as a measure of the threat effect of unionization. The idea is that regardless of the rate of unionization, non-union firms will not worry about the threat of unionization if no unions in their relevant labor market (defined by state and industry here) are able to organize workers.

4. Data and estimation results

4.1 Data

Data from the 1979-2017 MORG CPS are used to estimate the distribution regressions. Sample selection criteria and variable definitions are similar to those used in DFL. Note that the union status of workers is only available from 1983 on. As in DFL, we use union status information from the 1979 May CPS matched with the May-August MORG to extend the analysis back to 1979. One difference relative to DFL is that we impute topcoded wages using a stochastic Pareto distribution (see Firpo, Fortin, and Lemieux, 2018). This helps obtain a smother wage density in the upper end of the distribution. In the case of workers paid by the hour, our wage measure is the hourly wage directly reported by the worker. The wage measure is average hourly earnings (usual earnings divided by usual hours of work) for workers not paid by the hour. Wages are deflated into constant dollars of 2017 using the CPI-U. See Lemieux (2006) for more information about data processing.

We use union coverage as our measure of unionization throughout. Only observations with unallocated wages are used throughout to avoid the large attenuation bias linked to the fact union status is not used to impute wages in the CPS (Hirsch and Schumacher, 2003). The value of the minimum wage used in the estimations is the maximum of the federal and state minimum computed at the quarterly level.

Summary statistics are reported in Table 1. These statistics and econometric models are all weighted using CPS sample weights.

4.2 Minimum wage effects

We separately estimate the distribution regression models for men and women over the 1979-88, 1988-2000, and 2000-17 periods. After some experimentation we settled on specifications that allow for spillover effects up to 30 log points above the minimum wage in 1979-88, and 20 log points above the minimum wage in subsequent periods.[12] Besides the minimum wage variables, other variables included in the models consist of a set of state and year effects, state specific trends, years of education, a quartic in potential experience, experience-education interactions (16 categories plus experience times education), race and marital status, public sector, part-time

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[12] Spillover effects above these levels were not found to be significant.

status, 11 industry categories and 4 occupation categories. As discussed in Section 2, we also include interactions between these covariates and the cut points y_k.

As is well known, there is a substantial amount of heaping at integer values of hourly wages in the CPS data, especially at $5 (in earlier years) and $10. This can have an important impact on estimated probabilities depending on whether a given cutoff point y_k is just below (or above) an integer value. Heaping can also affect the estimated effect of the minimum if some observations with a true wage equal to the minimum are rounded off to the nearest integer. This type of measurement error could create spurious spillover effects when the minimum wage is slightly below an integer value. For instance, if workers earning a $9.80 minimum wage report a $10 wage in the CPS, this will increase the mass just above the minimum wage and give a false impression about the importance of spillover effects. This is an important issue in the literature as Autor, Manning, and Smith (2016) present calculations suggesting that minimum wage spillovers effects may be a spurious consequences of measurement error.

One advantage of the distribution regression approach is that heaping can be controlled for by including dummy variables indicating whether an integer value (re-expressed in nominal terms) lies into a specific wage interval $[y_{k,}y_{k+1}]$. As the heaping problem is most important for values of wages up to $10, we create dummies for heaping at $5, $10, and any other integer value up to $10. These dummies are included as additional covariates in all the estimated models.

Table 2 reports the estimated minimum effects for each of the six specifications (men and women for three time periods). The estimated coefficients for the large set of other covariates are not reported for the sake of brevity, and standard errors are clustered at the state level. The estimated coefficient for being right at the minimum wage (φ_0) is large and significant in all specifications, though it tends to decline over time. The coefficients linked to spillover effects are also precisely estimated, and tend to decline as we move further away from the minimum wage.

There is also clear evidence that minimum wage effects are substantially larger in 1979-88 than in subsequent years. Unlike Lee (1999) and Autor, Manning, and Smith (2016) who use different estimation methods for different years, our method yields estimates based on the same method for different years. The results suggest that Autor, Manning, and Smith (2016)'s

conclusion that Lee overstated the importance of spillover effects is at least in part due to the fact their estimates are based on more recent data.

As it is always difficult to interpret the magnitude of coefficients estimated using probit models, we transform the results into marginal effects that are reported in Figure 5. The marginal effects are computed as the difference between the fitted probabilities estimated using the model in equation (3):

$$\widehat{P}_{ist}^{k} = \Phi\left(Z_{ist}\hat{\beta} + y_k Z_{ist}\hat{\lambda} + \sum_{m=-3}^{6} D_{ist}^{m}\hat{\varphi}_m - \hat{c}_k\right),$$

and counterfactual probabilities obtained by setting the minimum wage coefficients, φ_m, to zero:

$$\widehat{P}_{ist}^{k,c} = \Phi\left(Z_{ist}\hat{\beta} + y_k Z_{ist}\hat{\lambda} - \hat{c}_k\right).$$

Since distribution regression yield estimates of cumulative probabilities, the probability of being in a given interval $[y_k, y_{k+1}]$ is simply the difference between two predicted cumulative probabilities, e.g. $\widehat{P}_{ist}^{k} - \widehat{P}_{ist}^{k+1}$. Thus, for a given interval $[y_k, y_{k+1}]$, the marginal effects reported in Figure 5 are the difference in the average value of $\widehat{P}_{ist}^{k} - \widehat{P}_{ist}^{k+1}$ and $\widehat{P}_{ist}^{k,c} - \widehat{P}_{ist}^{k+1,c}$.

Figure 5 shows that the minimum wage spike is quite large. Depending on years and gender, it increases by a factor of 150 to 300% the probability of being at a given wage interval. Spillovers in the first interval to the right of the minimum wage are also quite large, but decline as we move further above the minimum. Visually speaking, Figure 5 shows that spillover effects are substantially more important in 1979-88 than in subsequent periods.

4.3 Union threat effects: RTW laws

Figure 6 presents the main event study estimates for the impact of the introduction of RTW laws in Indiana, Michigan, and Wisconsin, on the rate of unionization using MORG CPS data for the 2000-17 period. Other Rust-belt states are used as controls. Although the estimates are quite noisy (standard errors clustered at the state level), there is some evidence that unionization rates drop following the introduction of RTW laws. Supporting the validity of the research design, there is little evidence of pre-trends.

To help with precision, we next estimate a difference-in-differences specification where the effects plotted in the event-study graph are constrained to be the same in the before and after periods. The results reported in Table 3 are robust to the choice of control groups (Rustbelt vs. all states) and control variables. The models in column 1 and 4 only include state and year dummies. The covariates discussed in Section 3 are added in column 2 and 5, while the state unemployment rate is added in columns 3 and 6. Controlling for the unemployment rate is important as RTW laws were introduced in the years following the Great Recession.

For both men and women, most specifications indicate a negative and significant effect of about 2 percentage points. We also report difference-in-difference estimates of the impact of RTW laws on the wages of non-union workers in the second panel of Table 3. Interestingly, the estimated effects are similar in magnitude (negative 2 log points) to the ones for the coverage rates, and are statistically significant in most cases. This suggests that RTW laws have reduced threat effects by weakening the power of unions, leading to a decline in the wages of non-union workers.

We next report in Figure 7 difference-in-differences estimates at various percentiles of the wage distribution. The estimates are based on the same specification as in columns 2 and 5 of Table 3, but are estimated using Firpo, Fortin and Lemieux (2009) RIF-regressions instead of OLS regressions. Although there is some evidence that RTW laws have a more negative impact in the lower middle of the wage distribution, the lack of statistical precision makes it hard to draw strong conclusions.

As discussed earlier, it is not clear how these event study estimates could be used to assess the contribution of declining threat effects to changes in the distribution of wages. If we were to use RTW as an instrument for the rate of unionization, the implied estimates in Table 3 would be implausibly large (a 1 percent increase in the rate of unionization leading to a 1 percent increase in wages). A possible challenge is that RTW may have an immediate effect on wages because of an abrupt decline in threat effects, while the decline in the unionization would only fully materialize in the long run.

Another challenge is that event study estimates for non-union wages reported in Appendix Figure A4 are quite imprecise and suggest there may be an issue with pre-trends. So while, on balance, the results are consistent with the view that RTW laws have a negative impact

on non-union wages because of reduced threat effects, we now switch to an alternative approach to quantify the contribution of declining union threat effects to changes in the wage distribution.

4.4 Unionization rates as a proxy for threat effects

As discussed in Section 3.2, our main estimates of union threat effects are based on estimates of the distribution regressions where the unionization rate at the state-industry-year level is included as an additional regressor. But before presenting these results, we present simpler estimates based on OLS and RIF-regression models in which it is easier to see the effects of the unionization rate at different points of the distribution. The results from these simple regressions are reported in Figure 8. To compare our results with earlier studies, we report in the first panel estimates of the effect of the union status on wages. The OLS estimates yield the conventional union wage premium, while the RIF-regression coefficients indicate how the union effect varies at different point of the distribution.[13] We use the same set of covariates as before and estimate the models over the 2000-17 period.

Consistent with the existing literature, Panel A of Figure 8 shows that the union wage premium (horizontal red line) is about 18% for men, but smaller for women.[14] Consistent with Firpo, Fortin, and Lemieux (2009), the union effect estimates obtained using RIF-regressions are hump shaped. For both men and women, they peak around the 30th and 40th percentile and then steadily decline to become negative above the 80th percentile.

Intuitively, the fact that union wage effects are positive on average but declining over most of the wage distribution are consistent with other evidence on the effect of unions on the wage structure. For instance, Card (1996) shows that the union wage premium is positive on average, but declines over the skill distribution.

It is not as intuitive, however, to see why the RIF-regression estimates first grow before reaching a peak around the 30th-40th percentile. Part of the story is that changes in the rate of unionization have little impact at the bottom of the distribution where wages mostly depend on

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[13] As discussed in Firpo, Fortin, and Lemieux (2009), RIF-regression estimates can be interpreted as the impact of a small change in the probability of unionization on the unconditional quantiles of the wage distribution. As such, RIF-regressions are one among several possible ways of computing the counterfactual distribution obtained by changing the probability of unionization. An alternative approach would be to reweight the data to slightly increase the weight put on union relative to non-union workers (as in DFL), and see how it affects the various wage quantiles.

[14] For instance, Card, Lemieux, and Riddell (2018) find a union wage premium of 0.16 for men and 0.09 for women in 2015.

the minimum wage. Another part of the story is that very few workers are unionized at the bottom of the distribution. The issue is discussed in more detail using an example with uniform distributions in the Appendix. Note that the hump-shaped distribution of RIF-regression coefficients has important implications on how de-unionization affects the shape of the wage distribution. Panel A of Figure 8 indeed indicates that unionization substantially reduces the 90-50 gap, but slight increases the 50-10 gap. Interestingly, DFL reach a similar conclusion using a reweighting approach, as we do using the distribution regression method (see below).

Panel B next shows corresponding estimates of the effect of the state-industry-year unionization rate on the wages of non-union workers. Interestingly, in the case of men the shape and magnitude of the estimated effects are qualitatively similar to those for the union status reported in Panel A. In the case of women, the OLS estimate is substantially smaller, and the RIF-regression estimates are unstable across the various percentiles of the distribution.

We next show in Panel C estimates from models where the proxy for union threat effects is the success rate of union organizing elections (by state-industry-year) instead of the rate of unionization. The estimated effects are positive on average, and generally decline over the wage distribution. More information about the union election data is provided in the Appendix.

Taken together, the results reported in Figure 8 support the view that the threat of unionization has a positive effect on the wages of non-union workers. Although the shape of the RIF-regression coefficients varies across the specifications reported in Panels B and C, the estimates tend to be small and often negative at the top of the distribution. As discussed in Section 3, this supports the view that declining unionization rates (or success rates of union elections) capture declining threat effects instead of spurious state-industry shocks that both reduce wages and unionization rates.

4.4 Distribution regression estimates of the effect of unionization

Table 4 reports estimates from the distribution regression models in which the state-industry-year rate of unionization has been added as an explanatory variable. We capture the fact that impacts may vary over the wage distribution by interacting the unionization rate with a quartic function in y_k (normalized to zero at the midpoint of the y_k range). All models include a set of industry trends in addition to the other explanatory variables listed at the beginning of Section 4.2.

The models are estimated separately for union and non-union workers for two reasons. First, we want to allow for different effects of the unionization rate for these two groups of workers.[15] Second, as we discuss in Section 5, estimating separate models for union and non-union workers is essential for computing standard counterfactual experiments illustrating the contribution of de-unionization to changes in the wage structure.

Panel A of Table 4 shows the estimated effect of the unionization rate for non-union workers. The main effect of the unionization rate is large and statistically significant in all three time periods. Consistent with the evidence reported in Panel B of Figure 8, the estimated effect of the unionization rate is substantially smaller for women, especially in the earlier periods. Panel B shows that the unionization has a larger effect for union workers, suggesting that the union wage gap increases with the unionization rate.[16]

While most of the interactions between the unionization rate and the polynomials in y_k are statistically significant, it is difficult to infer the shape of the estimated effects from the results reported in Table 4. To facilitate interpretation, we translate the estimated parameters for non-union workers into wage impacts at different points of the distribution by considering the effect of a 10% increase in the unionization rate for the 2000-17 period. The wage effects are obtained by first comparing the CDF computed from the distribution regressions --using the observed rates of unionization-- to the counterfactual CDF that would prevail if the unionization rate was 10 percentage points higher. The horizontal distance between the two CDFs indicates by how much wages change at each percentile of the distribution under this counterfactual experiment. The results of this exercise are reported in Figure 9.

Interestingly, the wage effects reported Figure 9 are again hump shaped, suggesting that the threat of unionization has the largest effect in the lower middle of the distribution. It then turns negative above the 80th percentile. The effects are remarkably similar to the RIF-regression estimates of the effect of the union status reported in Panel A of Figure 8.[17] The estimates are

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[15] When the threat of unionization is limited and non-union employers don't attempt to match union wage increases, these high union wages are more likely to put union employers out of business. As a result, we expect the unionization rate (proxy for threat effects) to have a positive effect on both union and non-union wages, though it is difficult to predict how the magnitude of the two effects compares.

[16] While the effect of the unionization rate on the wage premium cannot be inferred directly from the distribution regression results, OLS estimates like those reported for non-union workers in Panel B of Figure 8 show the that the effect of the unionization rate is larger for union than non-union workers. In other works, the unionization rate has a positive effect on the union wage premium.

[17] Note also that estimating a richer distribution regression model helps smooth out the unstable results reported for women using the simpler RIF-regression approach (Panel B of Figure 8)

consistent with the view that when non-union employers try to emulate the union wage structure in response to the threat of unionization, we should find small or even negative impacts at the top of the distribution. This supports the view that the effects of the unionization rate at the state-industry-year level captures union threat effects, as opposed to unmodelled state-industry shocks that both affect wages and unionization.

5. Decomposition results

We are now in a position to estimate how much of the change in the wage distribution over the 1979-2007 period can be accounted for by changes in the rate of unionization and the minimum wage in the presence of spillover effects. In the case of the minimum wage, we first compute counterfactual probabilities by replacing the observed minimum wages in the end period (say 1988) by the minimum wage in the base period (say 1979). For example, for each individual i in year 1988, the predicted probabilities estimated using the distribution regressions are:

$$\widehat{P}_{is88}^k = \Phi\big(Z_{is88}\hat{\beta} + y_k Z_{is88}\hat{\lambda} + \sum_{m=-3}^{6} D_{is88}^m \hat{\varphi}_m - \hat{c}_k\big),$$

while the counterfactual probabilities are:

$$\widehat{P}_{is88}^{k,c} = \Phi\big(Z_{is88}\hat{\beta} + y_k Z_{is88}\hat{\lambda} + \sum_{m=-3}^{6} D_{is79}^m \hat{\varphi}_m - \hat{c}_k\big).$$

Call the $\hat{Q}_{ist}^k$ the predicted probability that individual i is in a given interval $[y_k, y_{k+1}]$, where $\hat{Q}_{ist}^k = \widehat{P}_{ist}^k - \widehat{P}_{ist}^{k+1}$. Averaging these probabilities over all individuals in 1988 yields the predicted probability $\hat{Q}_{88}^k$, and its counterfactual counterpart $\hat{Q}_{88}^{k,c}$. We can then compute the various counterfactual statistics of interest in 1988 by reweighting observations using the reweighting factor $\hat{Q}_{88}^{k,c}/\hat{Q}_{88}^k$.[18] We use the same procedure for the periods 1988-2000 and 2000-17.

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[18] To be more specific, for each worker i with a wage Y_{i88} in 1988, we first find the interval $k(i)$ in which the observation belongs. The relevant reweighting factor is then $\hat{Q}_{88}^{k(i),c}/\hat{Q}_{88}^{k(i)}$.

To isolate the contribution of spillover effects, we also use DFL's "tail pasting" procedure where the distribution in the end year with a lower minimum wage (say 1988) is replaced by the distribution in the base year with a higher minimum wage (say 1979) for wages at or below the higher minimum. The opposite procedure is used when the minimum wage is higher in the end year than in the base year.

In the case of the decline of the rate of unionization, we also compare the predicted probabilities obtained using observed values of the unionization rates in the end period (say 1988) to the counterfactual probabilities obtained using the unionization rate in the base period. As such, the procedure is very similar to the one described above in the case of the minimum wage. And as in the case of the minimum wage, for the sake of comparison with DFL we first compute the contribution of de-unionization without spillover effects using DFL's reweighting procedure. More specifically, we first reweight data in the end period (say 1988) to have the same distribution of unionization as in the base period conditional on covariates, and then add spillover effects to the reweighted distribution using the procedure we just described.

Figures 10-12 report the actual and counterfactual distributions corresponding to the three periods of analysis 1979-1988, 1988-2000, and 2000-2017. In each figure, panel A shows the counterfactual distribution corresponding to a model where the minimum wage is held constant at the base period level, and spillovers are accounted for. Panel B then shows the counterfactual corresponding to the base period's minimum wage and unionization rate, accounting for spillovers from each. Thus, a comparison of the two panels highlights the interaction between these two forms of spillovers. The inequality measures corresponding to these distributions can be found in Table 5, along with additional models including counterfactuals without spillover effects.

As in Lee (1999) adding spillover effects substantially increases the contribution of the decline in the real minimum wage over the 1979-1988 period (see Figure 10). Comparing our results with spillovers to DFL's `tail pasting' methodology, we predict a counterfactual with far greater mass above the 1979 minimum wage level, and less mass at the minimum wage. As discussed, this occurs because the model accounts for the fact that with spillover effects some of the observed 1988 mass below the 1979 minimum wage level is the result of lower spillover effects and in the counterfactual belongs above the 1979 minimum wage level. For women,

accounting for these spillovers is particularly important: doubling the explained increase the standard deviation of log wages and Gini coefficient.

For men, the decline in union representation explains a large share of the declining wage density in the middle of the distribution between 1979 and 1988 (Figure 10, panel B). Moreover, because the unionization rate can explain some of the increasing mass in the lower tail of the 1988 distribution, including unionization (and its spillovers) in the model reduces the share of the mass explained by the minimum wage. The model with only minimum wage spillovers may therefore over fit the 1979 distribution in the counterfactual. For women, the minimum wage effect still dominates. Combined, changes in these two institutions account for 101% (74%) of the change in the 50-10 wage gap for men (women) between 1979 and 1988.

Between 1988 and 2000 real minimum wages remain relatively constant (see Figure 1). The minimum wage cannot therefore explain the decline in inequality at bottom of the wage distribution (the decline in the 50-10 gap). The decline in union representation does however explain some of the changing mass in the middle of the distribution and can account for a large share of increase in the 90-50 gap. Accounting for union spillovers doubles the share of the decline in the 90-50 wage gap explained by unions (Figure 11). This is consistent with the hump-shaped union threat effects discussed earlier.

Minimum wages rise across a number of states between 2000 and 2017.[19] Here our model shows that some of the wage gains above the 2017 minimum wage level can be explained as a spillover of these increases. For men, declining union representation continues to explain a share of the declining mass in the middle of the distribution, and taken together both institutions explain 99% of the decline in 50-10 wage gap over this period. Women experience almost no change in the 50-10 gap over this period.

As in DFL de-unionization has a modest impact on the female wage distribution, in large part because union representation declines much less among women than men. Table 1 shows a relatively modest 6 percentage point decline in the rate of unionization among women, compared to 21 percentage points in the case of men. For men we see the largest effects of declining union representation; in particular between 1979 and 1988. Moreover, as union representation declines,

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[19] The vertical lines in Figures 10-12 depict the 95th-percentile of the population weighted minimum wage distribution. It is therefore closer to the maximum state minimum wage, not the mean. For this reason, the graphically-depicted spillover effect from a minimum wage increase may appear below the vertical line. This matters more in later periods when there is increasing variation in state minimum wages.

so does the impact of union coverage on the wage-distribution; a component of which represents a decline in the threat effect of unions. Of the two institutions discussed here, declining union representation explains almost all of the explained (39%) increase in the 90-50 wage gap for men. Half of which is explained by a decline in union spillovers. Overall our model explains 53% (28%) of the increase in the standard deviation of log wages for men (women), and 49% (27%) of the increase in the Gini coefficient.

6. Conclusion

This paper uses an estimation strategy based on distribution regressions to quantify the role of union and minimum wage spillover effects in the growth in U.S. wage inequality over the 1979-2017 period. A first important finding is that the continuing decline in the rate of unionization has contributed to the growth in wage inequality after 1988. A second important finding is that accounting for spillover effects substantially increases the contribution of institutional change in the growth in inequality. These findings confirm and strengthen DFL's conclusion that labor market instructions have played a central role in the dramatic growth in U.S. wage inequality since the late 1970s.

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APPENDIX (to be completed)

Appendix A: Union Election Data

As a second proxy for the union threat level within a local labor market we construct a measure of the union activity within each state-industry pair using union election data. The original source of this union election data is the National Labor Relations Board, but our sample is derived from three sources: (1) 1977-1999 is sourced from Henry Farber; (2) 1999-2010 from Thomas J. Holmes[20]; (3) publicly available NLRB monthly and annual reports. Source (2) extends source (1) without overlap, and we compile our own data to extend the series to 2017. Unfortunately, from 2011 onwards the annual reports no longer provide industry information. For this reason, our measure of state-industry activity only extends to 2010.

Our measure of union activity is the net union certification rate within a state-industry: the number of newly certified workers less the number of newly decertified workers. To make the measure is a rate, we divide by total employment in that state-industry. Given the irregular nature of union elections, and dispersion of firm sizes, we also smooth the series over a 2 year period. The total level of new certifications (number of eligible workers in winning certification elections), and decertifications (number of eligible workers in winning decertification elections) is shown in Figure A2. The sharp drop in the union certification in 1981 corresponds to the Reagan administration's victory over the PATCO strikers. Union activity does not recover following 1981, and from 2000 declines even further. This downward trend is largely explained by a decline in the number of elections and the average number of participating workers, not the probability of winning an election (see also Farber, 2015). In fact, the probability that a union wins a certification election increases from the late 1990s (although, there is a decline from 2010) suggesting that unions have become more selective in their campaigns.

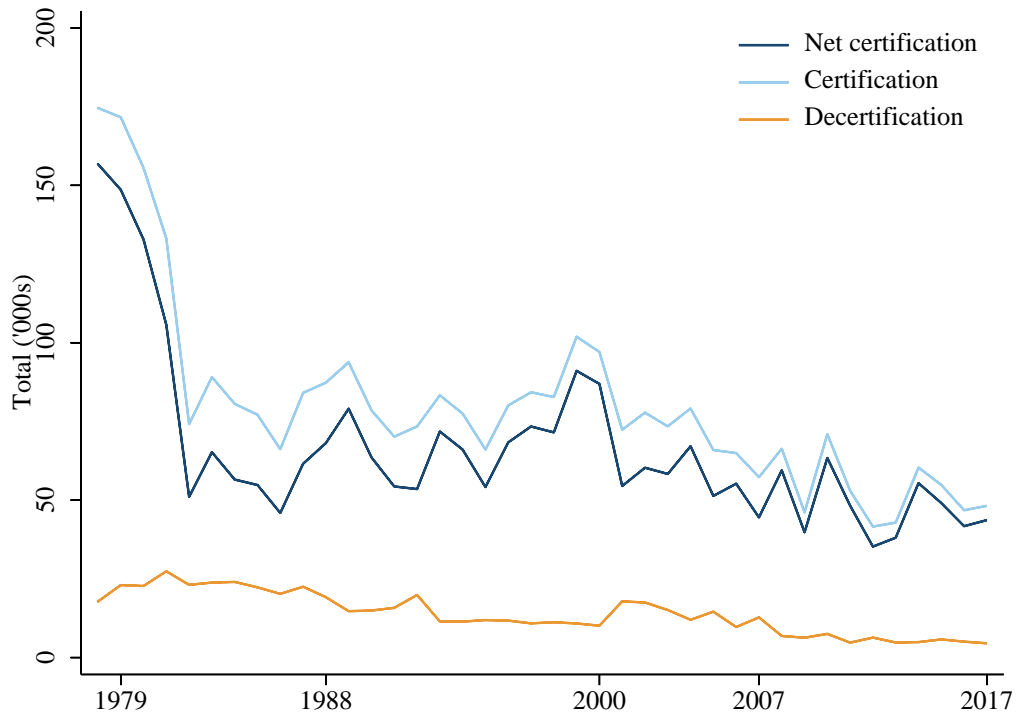[20]Publicly available at https://www.thomas-holmes.com/data

Figure A2: Total union activity: number of eligible workers certified and decertified (1978-2017; 2 year average)

In relation to the right-to-work law changes in the Midwest, we do not find strong evidence that the law hampered union activity. A confounding factor is that union activity is in decline across all of the Midwestern region from 2000. We do find evidence that in states where right-to-work laws were implemented the incidence of decertification elections declines sharply, and weaker evidence that the probability of losing a decertification election increases. This is consistent with the adoption of a more defensive strategy among unions following the law change.

We apply this proxy alongside the industry-state unionization rate in our empirical strategy (see Figure 7). Here the source of identifying variation is the within industry-state differences in trend. For example, certain states did see a sustained level of net certification in specific industries, that exceeded the declining industry or state trend. Both of these proxies are constructed using the pooled male and female samples. In the case of the election data, we do not have the data to construct a gender-specific measure. However, this does not negate gender differences, as the weight applied to each industry-state specific series will be proportional to the

share of women/men employed in that industry-state pair. For example, the unionization rate declines fastest in the male dominated manufacturing sectors of the Midwest; while the net certification rate remains high in the service sectors of education and health, in specific states. For this reason, we should expect to see that the two proxies have different explanatory power for men and women. Indeed, we see evidence of this in Figure 7.

Figure 7 graphs both the OLS and RIF coefficients for these two proxies as they relate to non-union wages. The sample includes the years 1983-2007: the PATCO strike variation is excluded, as is the 2008 recessionary period. The RIF coefficients demonstrate how the effect changes along the wage distribution, and for both proxies non-linearities are evident. For men, a higher coverage rate is associated with an increase in non-union wages in the middle of the wage distribution; reflecting the estimated union wage premium. For women, the coverage rate coefficients are noisier, especially at the top end of the distribution. However, the certification rate captures the same inverted-U shape as the union-wage premium. We take these estimates as further evidence that the magnitude of the union threat effect reflects that of the union-wage premium.

Appendix B: Understanding the hump shaped effect of unionization on the wage dsitribution
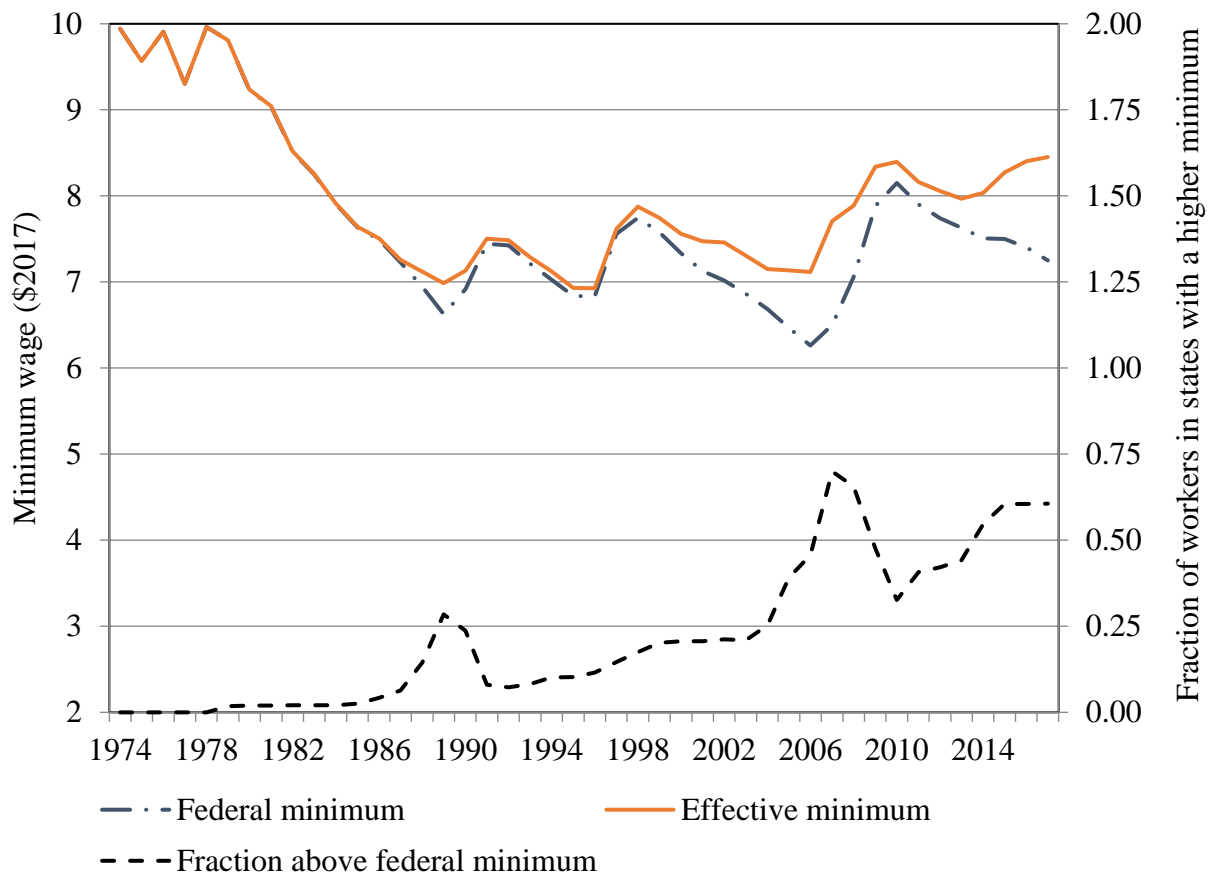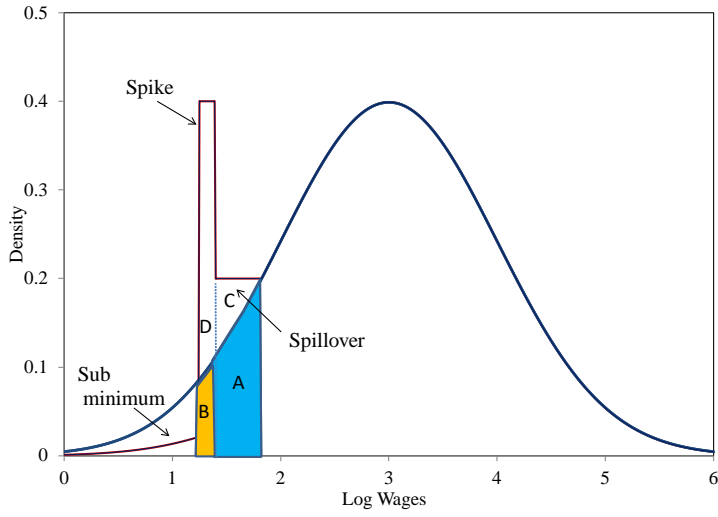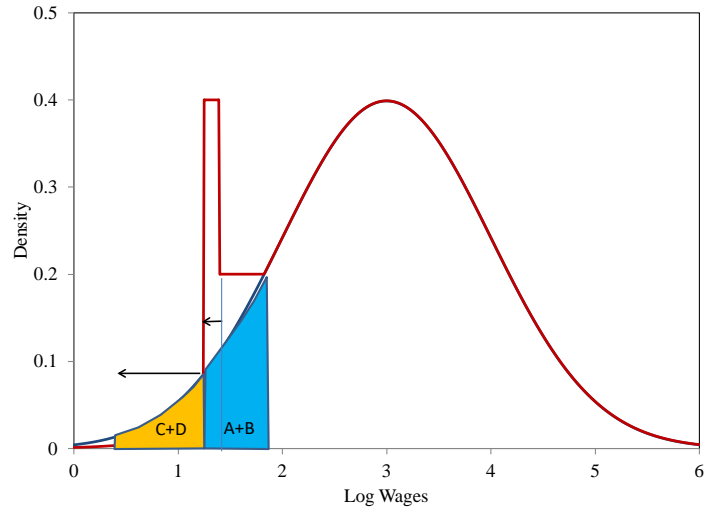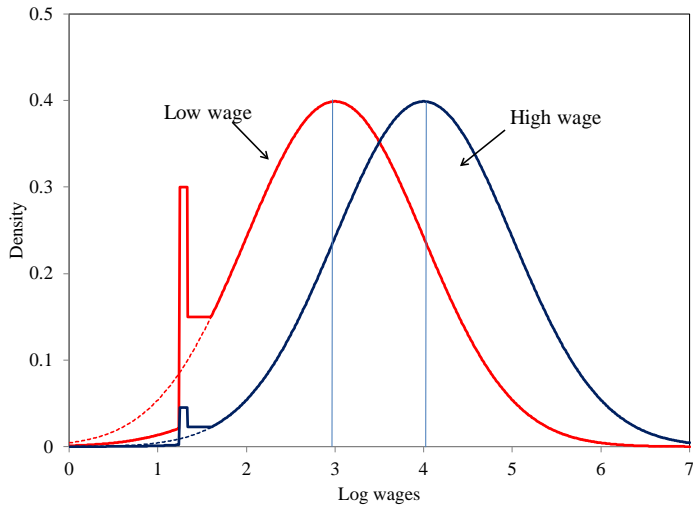
[TO BE COMPLETED]

Figure 1. Real Value ($2017) of the Minimum Wage and Fraction of Workers in States with a Higher Minimum
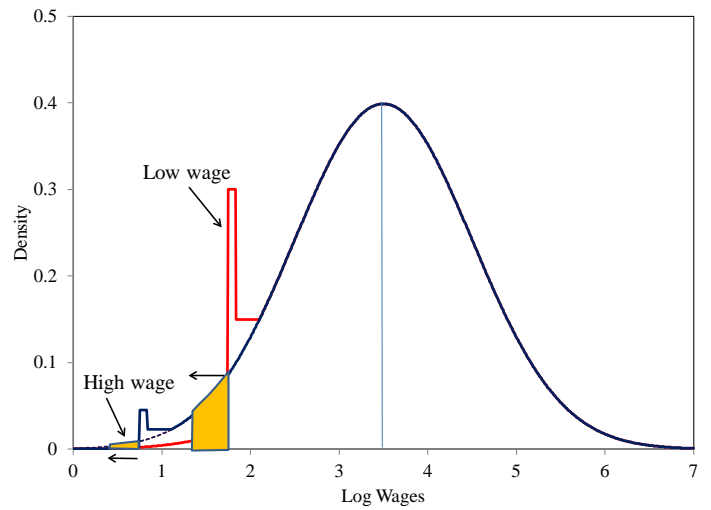
A. Probabilities of wage bins with a minimum wage

B. Changes in estimated cut-off points

C. Minimum wage in high- and low-wage states

D. Minimum wage in high- and low-wage states: recentered

Figure 2. Illustrative Example of the Identification of Minimum Wages Effects

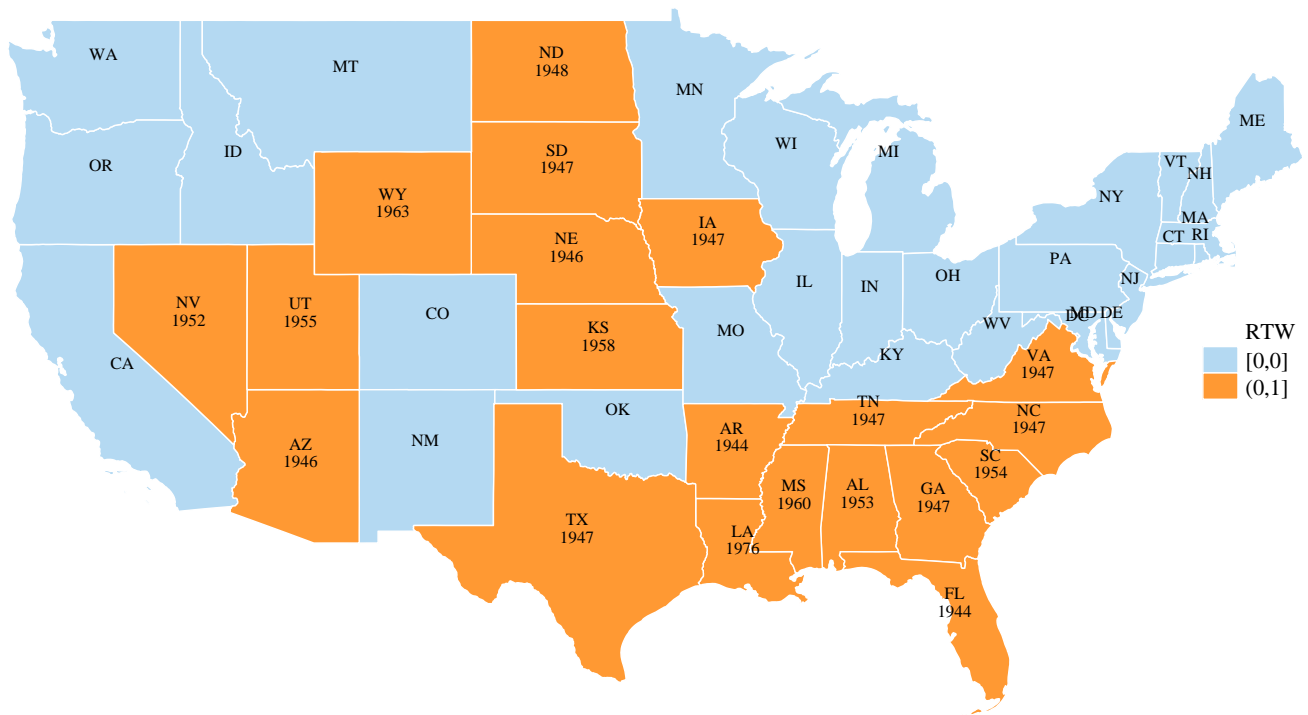Figure 3. Evolution of Right-to-Work Legislations across US States

A. High Unionization Industries

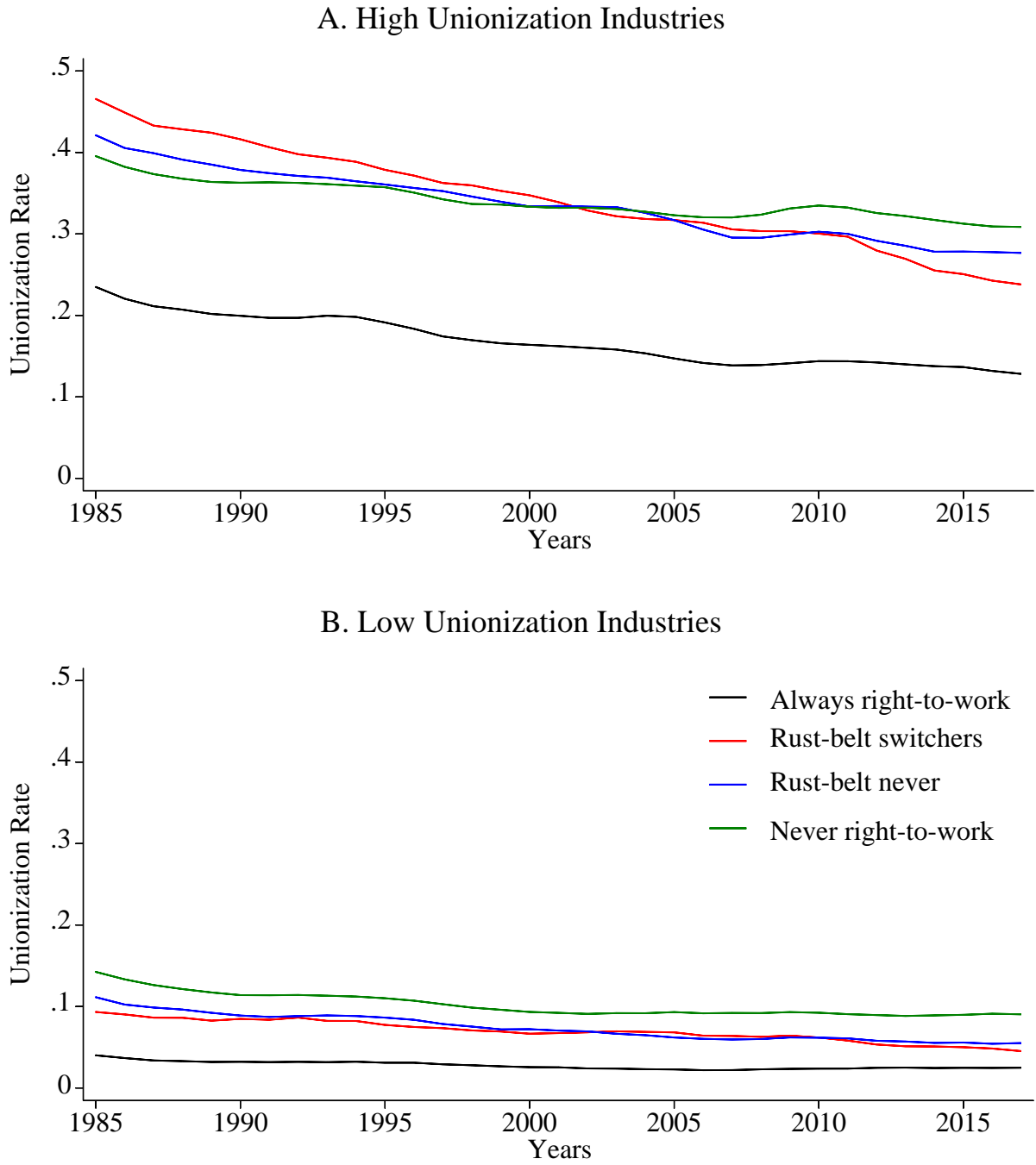## B. Low Unionization Industries

Figure 4. Trends in Unionization Rates across Low and High Unionization Industries by Right-to-Work Status - Men and Women Combined
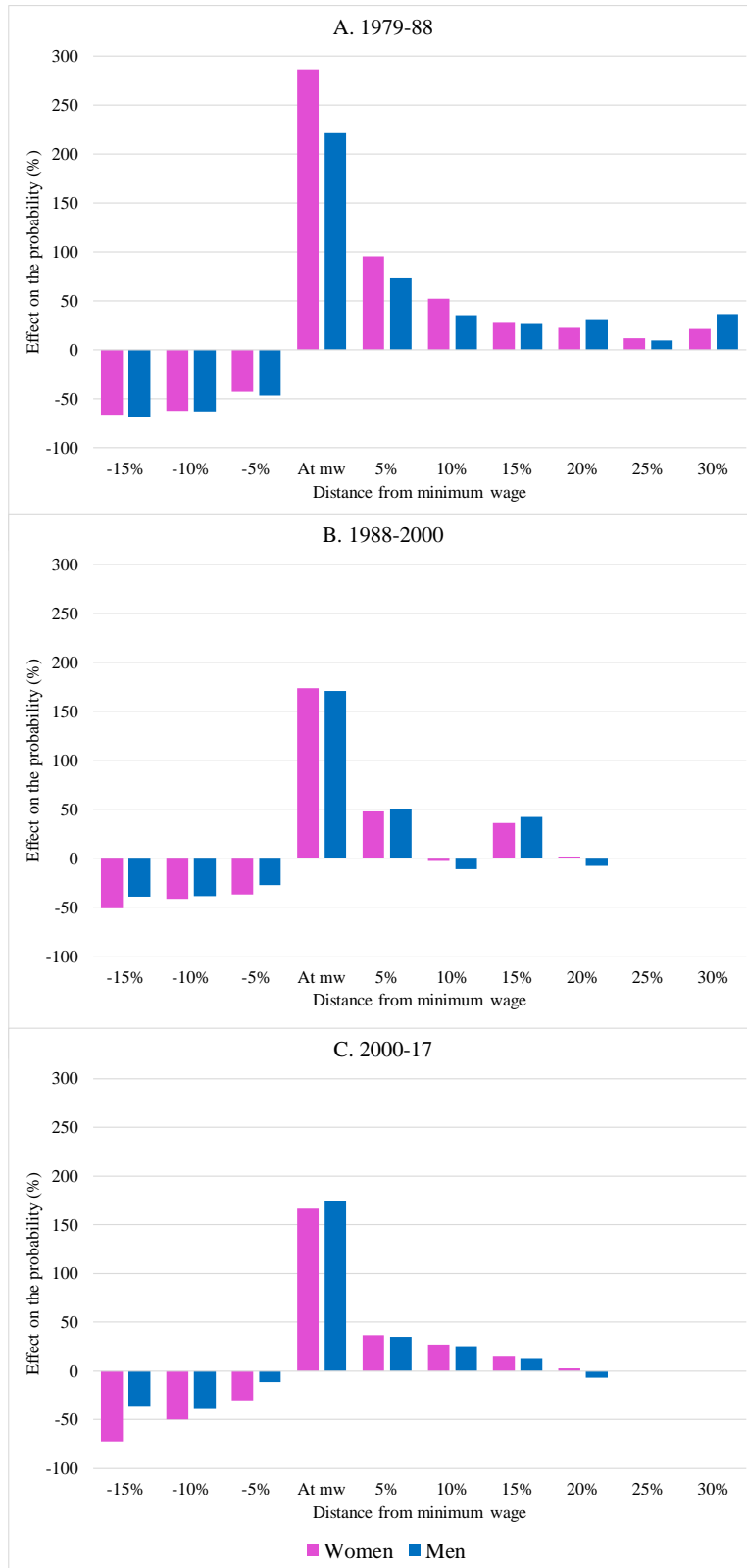
Figure 5. Marginal Effects of Minimum Wages across Bins
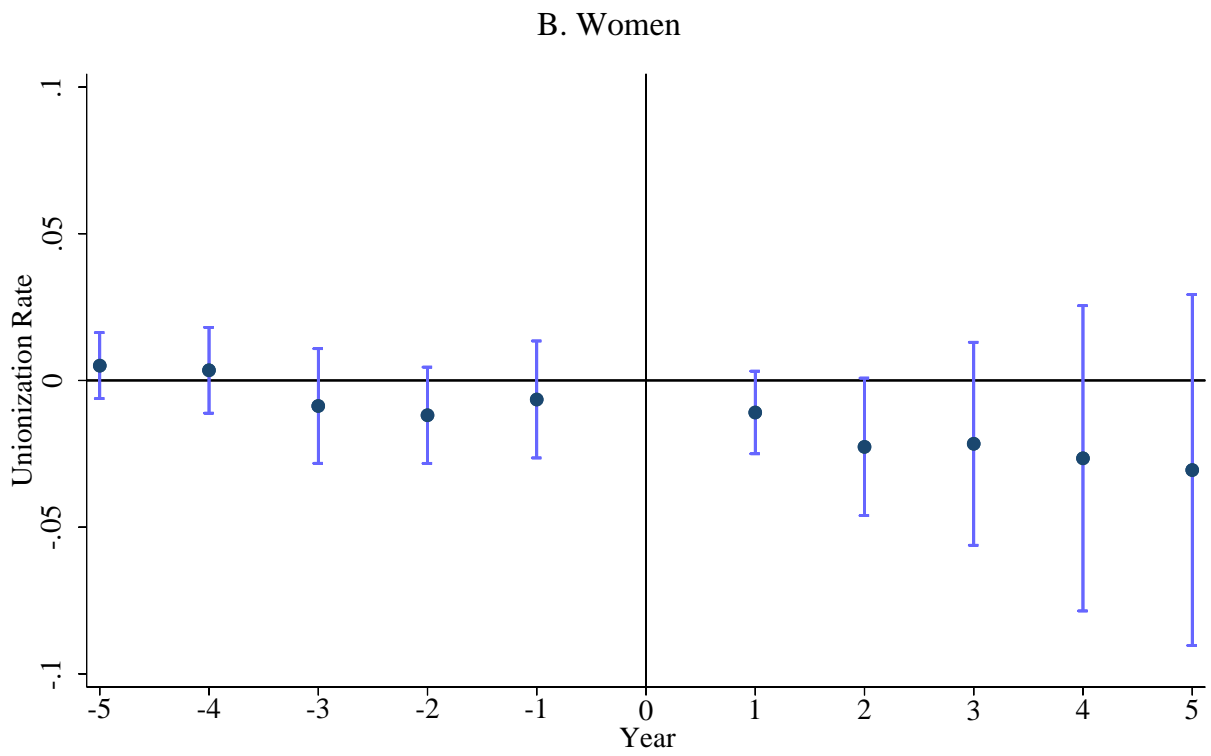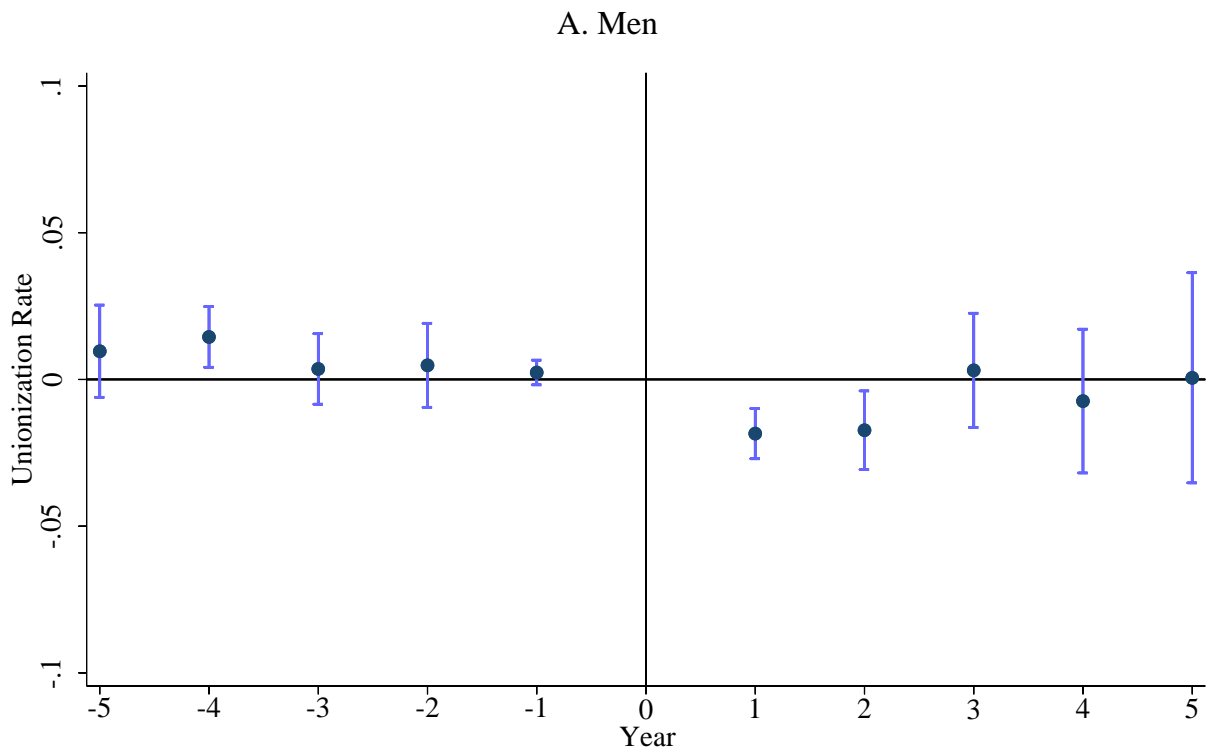
Figure 6. Event Study (Years to) of Right-to-Work Legislation on Unionization Rates

A. Men

B. Women
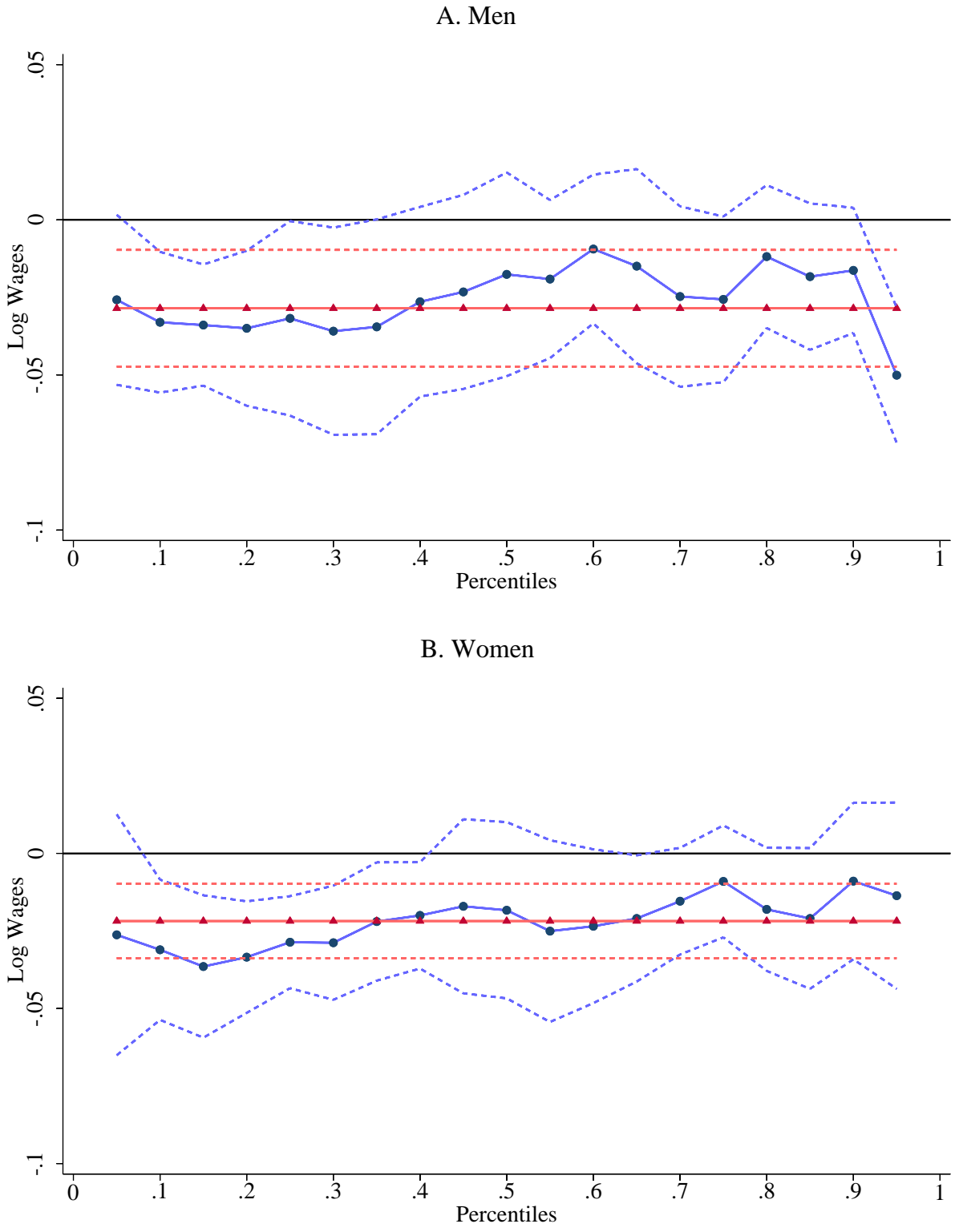
Figure 7. Effects (RIF-regressions) of Right-to-Work Legislations on the Log Wages on Non-unionized Workers

A. Union premium

Men
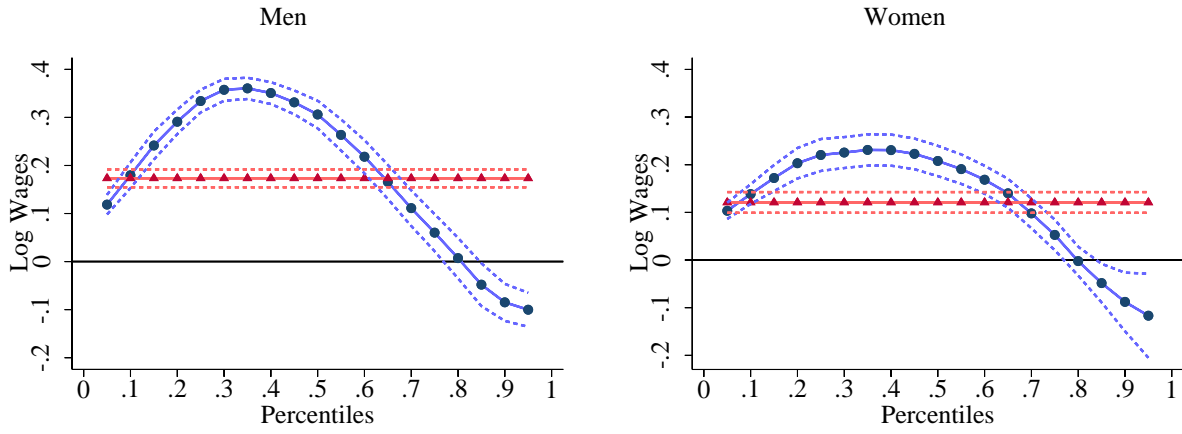
Women

B. Unionization rate

Men

Women

C. Election

Men
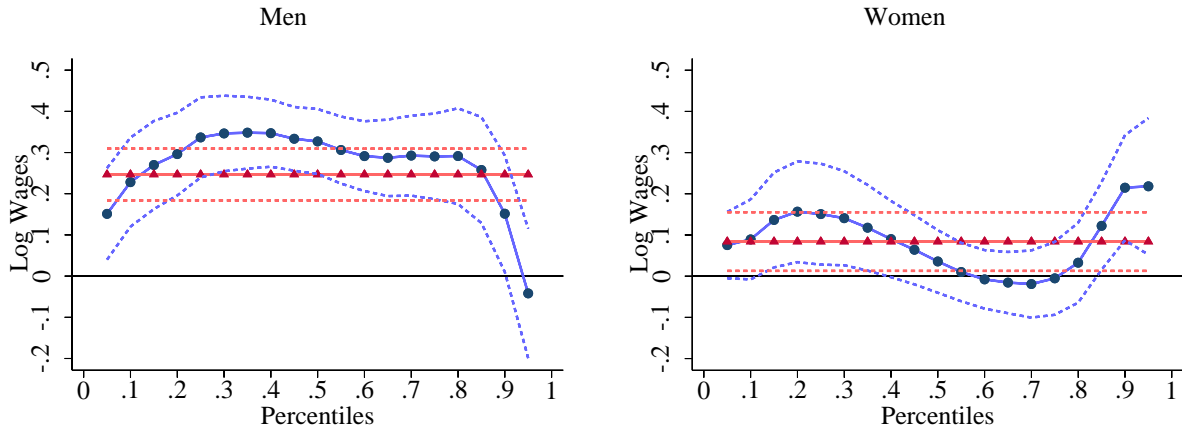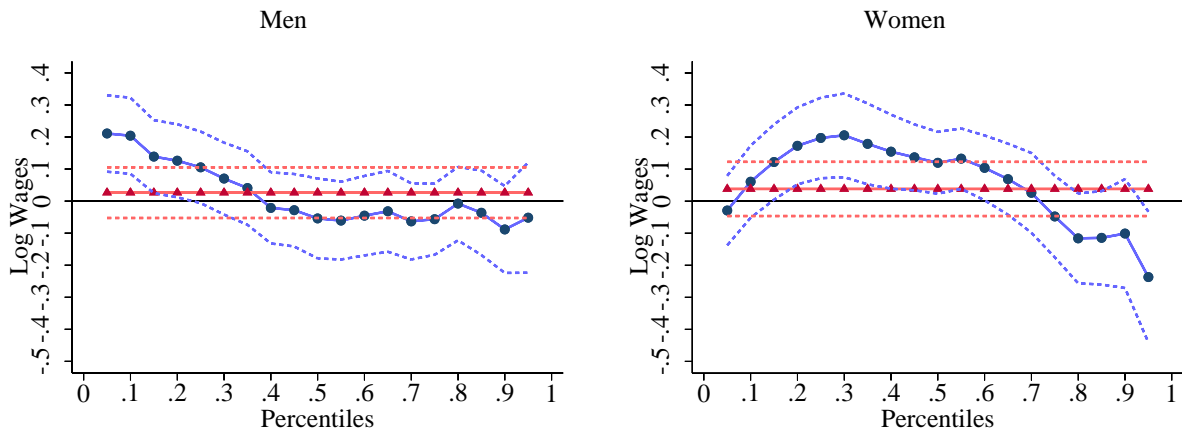
Women

Figure 8. Spillover Effects Based on Unionization Rates and Union Elections
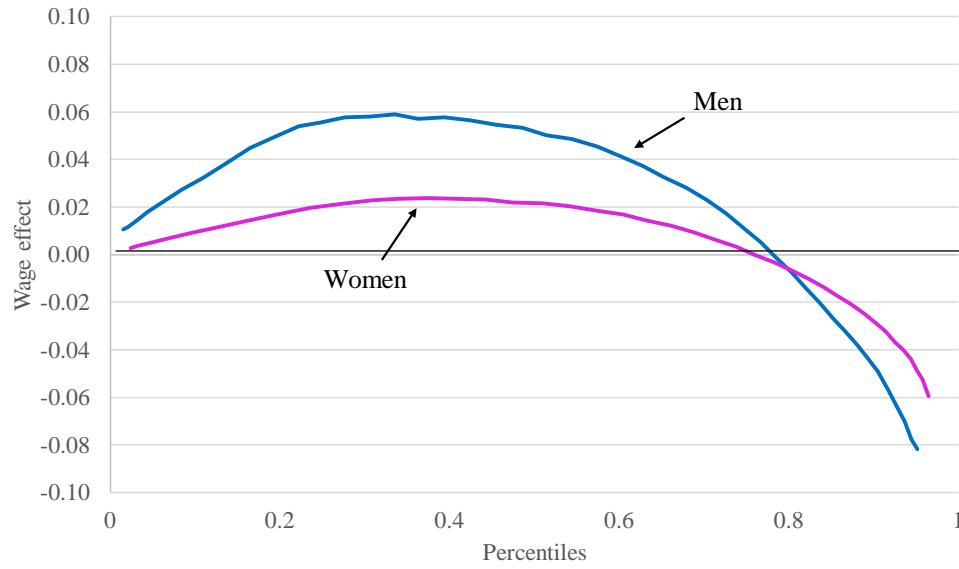
Figure 9. Effect of a 10% increase in the unionization rate on wages of non-union men and women, 2000-2017

A. Minimum Wages Effects with Spillovers

Men

Women

B. Adding Union Wages Effects with Spillovers

Men

Women

Figure 10. Counterfactual Densities 1979-1988

A. Minimum Wages Effects with Spillovers

Men

Women

B. Adding Union Wages Effects with Spillovers

Men

Women
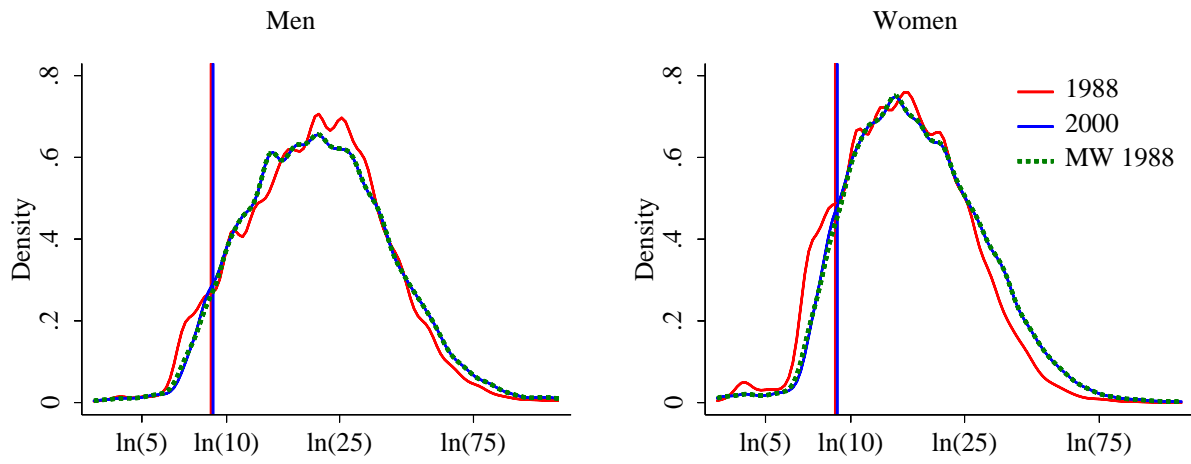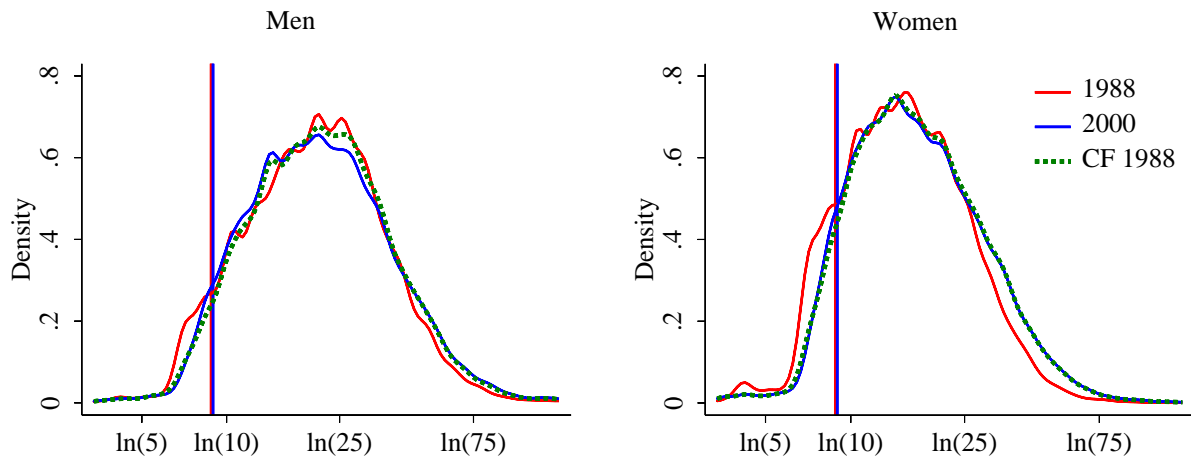
Figure 11. Counterfactual Densities 1988-2000

A. Minimum Wages Effects with Spillovers

Men

Women

B. Adding Union Wages Effects with Spillovers

Men

Women
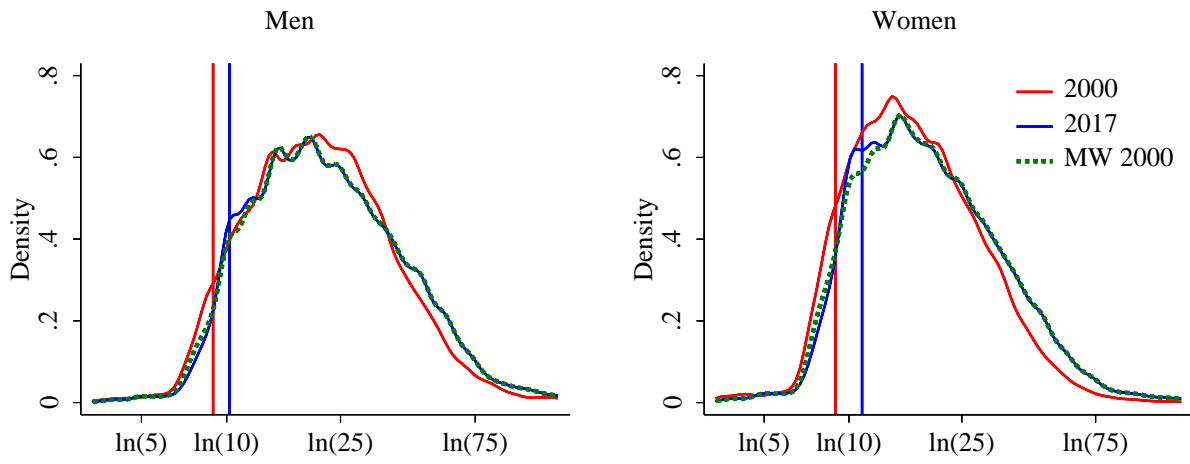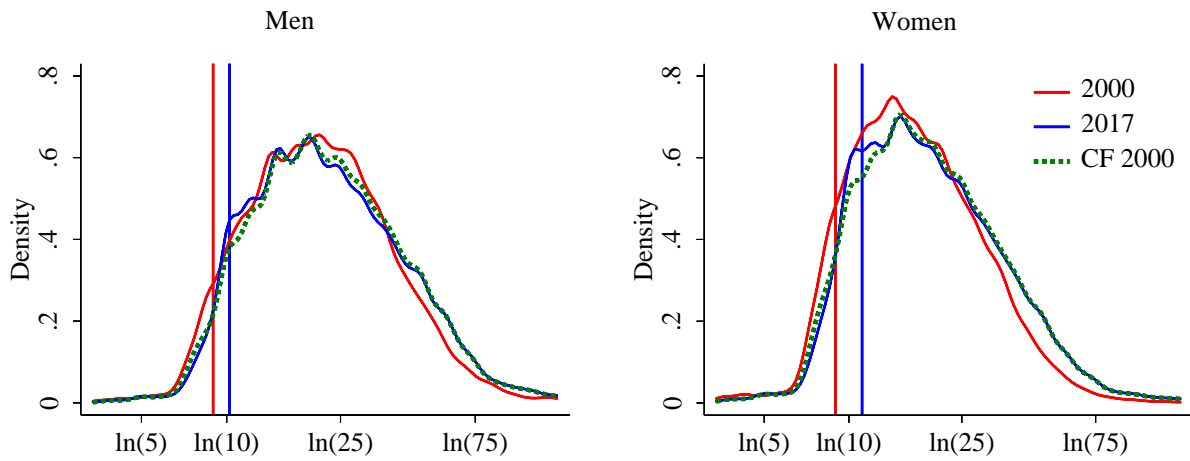
Figure 12. Counterfactual Densities 2000-2017

Table 1 - Inequality Measures and Descriptive Statistics

Year	1979	1988	2000	2017
A: Men				
90-10	1.281	1.452	1.521	1.608
90-50	0.588	0.693	0.793	0.901
50-10	0.693	0.759	0.728	0.707
Std(log wages)	0.249	0.326	0.357	0.413
Gini	0.279	0.324	0.355	0.392
Unemployment Rate	0.051	0.055	0.040	0.045
Union Density	0.337	0.229	0.168	0.127
No. of Obs.	76213	74020	53037	46342
B: Women				
90-10	0.950	1.286	1.357	1.452
90-50	0.568	0.667	0.746	0.865
50-10	0.382	0.619	0.611	0.588
Std(log wages)	0.172	0.255	0.288	0.357
Gini	0.236	0.287	0.317	0.363
Unemployment Rate	0.070	0.057	0.042	0.044
Union Density	0.176	0.153	0.134	0.115
No. of Obs.	62281	69292	52171	45382

Note: 90-10, 90-50, and 50-10 denote corresponding log wage differentials.
Number of observations corresponds to the unallocated sample used to compute
inequality measures. Unemployment rate and union density based off full sample.
For 1979 union-density sample is derived from the May MORG sample.

Table 2: Minimum Wage Effects Estimated from Distribution Regression Models

Wage Bins	(1)	(2)	(3)	(4)	(5)	(6)
Years:	1979-88		1988-2000		2000-2017	
Wages at	Women	Men	Women	Men	Women	Men
At minimum	0.557	0.494	0.341	0.324	0.329	0.293
	(0.017)	(0.014)	(0.025)	(0.029)	(0.035)	(0.040)
0-5% above	0.152	0.122	0.095	0.092	0.074	0.059
	(0.003)	(0.004)	(0.007)	(0.005)	(0.005)	(0.005)
5-10% above	0.077	0.052	0.003	-0.011	0.053	0.042
	(0.002)	(0.003)	(0.006)	(0.004)	(0.004)	(0.003)
10-15% above	0.038	0.033	0.057	0.061	0.024	0.016
	(0.002)	(0.004)	(0.007)	(0.005)	(0.005)	(0.004)
15-20% above	0.028	0.031	0.003	-0.011	0.004	-0.010
	(0.002)	(0.003)	(0.010)	(0.007)	(0.013)	(0.009)
20-25% above	0.016	0.012				
	(0.002)	(0.003)				
25-30% above	0.026	0.034				
	(0.002)	(0.003)				

Notes: Standard errors (clustered at the state level) in parentheses.

Table 3 - Effect of Right-to-Work Laws on Unionization Rates and Log Wages of Uncovered Workers

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All States			Rust-belt States		
Unionization Rate						
Men:	-0.017**	-0.024***	-0.024***	-0.003	-0.018*	-0.017*
	(0.008)	(0.006)	(0.006)	(0.010)	(0.008)	(0.008)
Women:	-0.012	-0.022**	-0.022**	-0.003	-0.021	-0.021
	(0.009)	(0.011)	(0.010)	(0.014)	(0.016)	(0.016)
Log Wages of						
Non-Union Workers						
Men:	-0.044***	-0.026***	-0.029***	-0.030*	-0.019***	-0.022**
	(0.015)	(0.006)	(0.007)	(0.014)	(0.006)	(0.007)
Women:	-0.017	-0.022***	-0.022***	-0.014	-0.020***	-0.021***
	(0.011)	(0.004)	(0.004)	(0.014)	(0.005)	(0.006)
Covariates	No	Yes	Yes	No	Yes	Yes
State Unemployment Rate	No	No	Yes	No	No	Yes

Note: Each entry corresponds to a different regression with the indicated dependent variable and sample. Other included variables are experience (quartic), education, education-experience, race, marital status, part-time, public sector, industry (11), occupation (12), and CMSA. The number of observations in the unionization rate (log wages of non-union workers) regressions are 1475798 (813901) for men and 1431090 (818458) for women in all states, and 313699 (165692) for men and 307908 (175594) women those in rust-belt states. The rust-belt state sample includes Pennsylvania, Ohio, Indiana, Illinois, Michigan, Wisconsin, Minnesota, Iowa, and Missouri.

Table 4: Unionization Rate Effects Estimated from Distribution Regression Models

Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)
Years:	1979-88		1988-2000		2000-2017	
A. Non-Union Workers	Women	Men	Women	Men	Women	Men
Unionization Rate (UR)	0.060	0.887	0.133	0.870	0.237	0.850
	(0.135)	(0.217)	(0.109)	(0.190)	(0.084)	(0.187)
UR*y_k	-0.139	-0.374	-0.293	-0.528	-0.352	-0.309
	(0.036)	(0.110)	(0.045)	(0.098)	(0.057)	(0.064)
UR*$(y_k)^2$	0.002	0.296	-0.047	0.101	-0.096	-0.178
	(0.026)	(0.053)	(0.027)	(0.039)	(0.018)	(0.055)
UR*$(y_k)^3$	-0.004	0.065	0.018	0.098	0.044	0.057
	(0.008)	(0.022)	(0.009)	(0.016)	(0.012)	(0.013)
UR*$(y_k)^4$	-0.006	-0.064	-0.007	-0.049	-0.006	-0.005
	(0.004)	(0.007)	(0.004)	(0.005)	(0.004)	(0.007)
B. Union Workers	Women	Men	Women	Men	Women	Men
Unionization Rate (UR)	0.887	1.367	0.724	1.396	0.771	1.399
	(0.217)	(0.149)	(0.101)	(0.128)	(0.103)	(0.129)
UR*y_k	-0.374	-0.558	-0.487	-0.487	-0.498	-0.344
	(0.110)	(0.063)	(0.047)	(0.085)	(0.071)	(0.057)
UR*$(y_k)^2$	0.296	-0.072	-0.190	-0.043	-0.193	-0.072
	(0.053)	(0.062)	(0.024)	(0.041)	(0.016)	(0.051)
UR*$(y_k)^3$	0.065	0.092	0.037	0.088	0.053	0.063
	(0.022)	(0.017)	(0.007)	(0.019)	(0.009)	(0.011)
UR*$(y_k)^4$	-0.064	-0.029	-0.003	-0.044	0.000	-0.029
	(0.007)	(0.007)	(0.003)	(0.005)	(0.003)	(0.005)

Notes: Standard errors (clustered at the state level) in parentheses.

Table 5a. Decomposition Results - Men

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Inequality	Raw	Minimum Wages		Unions		Together	Total
Measures	Changes	no spill.	w/spill.	no spill.	w/spill.	w/spill.	Explained
A: 1979-1988							
90-10	0.213	0.089	0.126	0.027	0.050	0.159	75%
90-50	0.119	0.006	0.004	0.036	0.062	0.065	55%
50-10	0.094	0.083	0.122	-0.009	-0.012	0.094	101%
Std(log wages)	0.073	0.019	0.032	0.009	0.017	0.050	69%
Gini	0.041	0.007	0.011	0.008	0.016	0.026	64%
B: 1988-2000							
90-10	0.015	0.004	0.004	0.011	0.026	0.031	212%
90-50	0.090	0.001	0.001	0.018	0.037	0.039	43%
50-10	-0.075	0.003	0.002	-0.007	-0.011	-0.008	10%
Std(log wages)	0.013	0.001	0.000	0.004	0.011	0.011	81%
Gini	0.018	0.001	0.000	0.003	0.009	0.009	52%
C: 2000-2017							
90-10	0.095	-0.007	-0.013	0.004	0.012	-0.001	-1%
90-50	0.121	0.001	0.001	0.011	0.024	0.025	21%
50-10	-0.027	-0.008	-0.014	-0.007	-0.011	-0.026	99%
Std(log wages)	0.032	-0.002	-0.004	0.002	0.005	0.002	5%
Gini	0.020	0.000	-0.001	0.002	0.004	0.004	18%
D: 1979-2017							
90-10	0.322	0.086	0.117	0.043	0.088	0.189	59%
90-50	0.330	0.008	0.006	0.065	0.123	0.129	39%
50-10	-0.008	0.078	0.111	-0.023	-0.034	0.060	—
Std(log wages)	0.118	0.018	0.028	0.014	0.034	0.063	53%
Gini	0.079	0.007	0.011	0.013	0.029	0.039	49%

Note: Column (1) shows the raw changes without decomposition. Each subsequent column corresponds to a different decomposition model. Columns (2) and (3) show the results of the minimum wage model estimated without (`tail-pasting') and with spill-over effects. Likewise, columns (4) and (5) show the results of the decomposition results based on a model without and then with union spill-over (`threat effects'); and without minimum wages. Column (6) corresponds to a model estimated with minimum wage effects (incl. spill-overs), and union effects (incl. spill-overs). Column (7) corresponds to column (6)'s results.

Table 5b. Decomposition Results - Women

Inequality Measures	(1) Raw Changes	(2) Minimum Wages no spill.	(3) Minimum Wages w/spill.	(4) Unions no spill.	(5) Unions w/spill.	(6) Together w/spill.	(7) Total Explained
A: 1979-1988							
90-10	0.333	0.141	0.195	0.007	0.007	0.201	60%
90-50	0.087	0.008	0.007	0.011	0.014	0.020	23%
50-10	0.246	0.133	0.188	-0.004	-0.007	0.181	74%
Std(log wages)	0.093	0.017	0.039	0.003	0.004	0.045	48%
Gini	0.050	0.011	0.020	0.002	0.003	0.024	48%
B: 1988-2000							
90-10	0.045	0.003	-0.003	0.003	0.007	0.004	9%
90-50	0.087	0.002	0.002	0.005	0.010	0.012	14%
50-10	-0.042	0.001	-0.004	-0.002	-0.003	-0.008	20%
Std(log wages)	0.024	0.001	-0.001	0.001	0.003	0.003	11%
Gini	0.021	0.001	0.000	0.001	0.003	0.003	13%
C: 2000-2017							
90-10	0.110	-0.014	-0.026	0.003	0.007	-0.022	-20%
90-50	0.102	0.001	0.002	0.005	0.011	0.013	13%
50-10	0.008	-0.015	-0.028	-0.002	-0.004	-0.035	—
Std(log wages)	0.047	-0.002	-0.005	0.001	0.003	-0.002	-5%
Gini	0.030	-0.001	-0.002	0.001	0.002	0.001	2%
D: 1979-2017							
90-10	0.488	0.130	0.166	0.013	0.021	0.184	38%
90-50	0.276	0.011	0.010	0.021	0.036	0.046	17%
50-10	0.212	0.119	0.156	-0.008	-0.014	0.138	65%
Std(log wages)	0.163	0.017	0.034	0.005	0.011	0.045	28%
Gini	0.102	0.011	0.019	0.004	0.009	0.027	27%

Note: Column (1) shows the raw changes without decomposition. Each subsequent column corresponds to a different decomposition model. Columns (2) and (3) show the results of the minimum wage model estimated without (`tail-pasting') and with spill-over effects. Likewise, columns (4) and (5) show the results of the decomposition results based on a model without and then with union spill-over (`threat effects'); and without minimum wages. Column (6) corresponds to a model estimated with minimum wage effects (incl. spill-overs), and union effects (incl. spill-overs). Column (7) corresponds to column (6)'s results.