

A UNION MITIGATION EFFECT ON THE GENDER WAGE GAP:
DO “RIGHT-TO-WORK” LAWS PROVIDE EVIDENCE?

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ABSTRACT

A prominent phenomenon of income inequality, the gender wage gap presents an enduring problem for the pursuit of female economic empowerment as it devalues female labor and consequently reduces economic output. Ultimately dealing with an issue of raising female earnings, unionization is a tried and proven method for increasing worker wages and benefits where it may contribute some mitigating effects towards the gap. This study attempts to quantify such an association by examining states which have enacted right-to-work laws (legislation known to reduce unionization) compared to states without, indicating what changes in wages are a result of changes in unionization. The model was applied to three different samples four times. Each of the four instances concerned one of the following groups: female union members, male union members, females without union membership, and males without union membership. Among all industries, subtle increases in wages for both men and women without union membership and union men were found. Unionized industries yielded wage increases for all males, regardless of membership, and a wage decrease for union women. Individual industries displayed some negative or no wage changes for union members and increases for those without membership. Ultimately, the link between decreased unionization and a stagnating gender wage gap was not so apparent. Right-to-work laws were found to contribute to the gap, but due to the response of a union male wage increase to right-to-work laws, it is unlikely such drops occurred from lowering unionization.

INTRODUCTION & LITERATURE

Since the greater expansion of female labor force participation in the mid-twentieth century, the disparity between male-female wages has always existed and engaged in a stagnantly narrowing trend across the decades. From the late 1950s to around 1980, women made a constant 60% of men's earnings (Blau & Kahn 2017) until 1981 began to see this gender wage gap reduce, seeing full-time working women make 76.5% of what men earned. According to the Census Bureau, the ratio sits at 80% as of 2017, which still conveys a striking as well as costly differential in male-female earnings (Blau & Kahn 2017). The increasing number of women entering male-dominated occupations are linked to said reductions. As described by Blau and Kahn (2000), majority female occupations generally pay less, linking female entry into traditionally male occupations to the increase in female wages overall. Occupational segregation by sex, along with the male-female wage differential, has decreased at a reduced pace since the 1990s, supporting the association. Nevertheless, the integration has not manifested across all occupations evenly (Blau and Kahn 2000). Most female entrances are skewed towards white collar professions rather than blue-collar jobs; a phenomenon resulting from gender differences in qualifications including experience, education, training, etc.

The differences are commonly traced to contrasts in human capital investment and the uneven distribution of home labor assigned to women. Given these assumptions, the consequence is disincentivizing women from entering male-dominated occupations based on demands from the private sphere. Expecting disjointed working careers, women do not pursue higher education or on-the-job training required for more time-consuming professions relative to predominantly female industries. Employers in such particular fields may also discriminate

against women within the labor market, choosing to not hire based on the expectation women are less productive due balancing more home life duties compared to men. Such an attitude towards women's productivity may also adversely affect employed women via their wages. By subscribing to the idea of women as less productive than men on the average, firms could reduce female wages for the supposed loss in possible productivity. Previous studies by Blau and Kahn (2000) and Even and Macpherson (1997) noted that standard wage determinants (e.g., human capital investments), when controlled for the proper demographics, account for nearly two-thirds of the gender wage, leaving approximately 37% unassigned (Blau and Kahn 2000). Such a significant portion may be in part to present discrimination. Labor discrimination of any sorts, however, alters the related labor markets by leaving qualified workers in search of jobs, raising supply and allowing wages to recede for the whole as described by Bergmann (1974).

The gap, however, continues to close, albeit at a reduced rate from the latter twentieth century. According to the Census Bureau, as of 2018, women earn 80% of total male earnings. Strangely, the progression of such a narrowing gap is inconsistent with the trend of income inequality, in general, over the past fifty years (Blau and Kahn 1997). The 1980s saw the beginning of increasing growth in income inequality while simultaneously seeing a relatively rapid decrease in the gender wage gap. Though, the period saw a huge influx of female labor force participation and increased female hours worked. Driven by upswings in female human capital standings, women possibly became more desirable workers in the eyes of the employer, taking on more and more standardly male-filled occupations (Blau and Kahn 1997). Changes in social attitudes and assumptions of women's role in the home reduced discrimination to where more women could devote more time and energy into their professional obligations. A consequence being a change in hiring attitudes as those stereotypes of female low-level

production were disproved. Another possible contributory factor existed in the state of unionization, which at the time mainly benefited blue collar, male-dominated professions. Union membership rates were falling gradually since the 1950s, but the 1980s saw a significant increase in the rate of reduction. Such a consequence had a negative impact on wages for blue collar jobs, thus a wage reduction for a male-dominated industry such as manufacturing (Blau and Kahn 2000). With the 1990s seeing still present stagnation in the rate of closure, the gap still endures, and its persistence asks one to ponder on how to remedy residual wage discrimination.

Unionization holds a notable reputation in its ability to increase wages. Collective bargaining in the form of union-firm negotiations has shown to have some mitigating effects on income inequality, but such studies typically focus on male dominated industries (Blanchflower & Bryson 2003). Regardless, the results found increases in wages due to union presence, a positive union wage effect. It was noted earlier, as presented conversely by Blau and Kahn (1997), that a reduction in union membership possibly contributed to the significant closure of the gender wage gap experienced in the 1980s. However, such an effect is due to de-unionization driving down a considerable number of wages of male-dominated, blue collar occupations, not that union membership rates hold any sort of negative, causal relationship with female wages.

The draw of a union is found in that it acts as a representation of a collective worker voice which communicates grievances and desires of the workplace with the hope of improving wages as well as productivity. Freeman and Medoff in “What Do Unions Do?” (1984) sought to quantify such a union wage effect and found it to average between a 10-15% increase for union member workers. A study revisiting the union wage effect by Blanchflower and Bryson (2003) found a differential of 15.4% via cross-sectional data from the same period. Using data representing the 1970s, both studies captured the union wage effect at the height of union power

in the United States. Unions' ability to raise wages is found in amassed monopoly power related to the "wage sensitivity of the demand for organized labor" (Blanchflower and Bryson 2003). Typical wage increases required a reduction in employment, but if employment levels hold a small response to wage changes, unions may attain greater wages without cost of major reduction in employment. Freeman and Medoff suggest that this effect is positively related to the number of workers organized as well as the scope (e.g., plant, firm, sector) at which the collective agreement is made. The wage sensitivity of the demand for organized labor as the root of union power also denotes union wage premiums as counter-cyclical, moving up and down with the current level of unemployment (Freeman and Medoff 1984).

As previously discussed, the female labor force participation rate has increased considerably over the past seventy years (despite a downtick attributed to the 2008 recession), the same cannot be said for union membership rates (Dunn and Walker 2016). For the past few decades, union membership has declined heavily (Mayer 2004). Peaking in 1979 at nearly twenty-one million members, the rate has fallen considerably to approximately 11.9% as of 2015 (Dunn & Walker 2016). Such a decrease should be noted in examining the present effects union membership holds on wages. The union wage premium has fallen in tandem with the drop in union density since the 1990s as a result of reduction in demand for union labor. Blanchflower and Bryson (2003) attribute this fall in demand to two sources: (1) expanding price competition which caught employers unable to push the premium cost onto consumers and (2) greater domestic and foreign, non-union competition. Workers who were most exposed to the wage premium drop are those who were already the lowest paid in the private sector: youth, least educated, and women (Blanchflower and Bryson 2003). Such risk points to the union capacity

to secure the most vulnerable workers from exploitative labor such as women, who continue to face discrimination within many labor markets.

Moving then to the bigger question at hand, may unions expand on such an ability to protect female workers? According to the Census Bureau, women in 2020, on average, still make 80 to 82 cents to every male dollar earned, conveying the presence of enduring discrimination at the occupational and industry levels (Foster 2020). Previously, it was established that increases in the female labor-force participation rate contributed to a reduced gender wage gap, specifically by women entering what are typically male-dominated industries. The rate, which was on the up since 1950, began to stagnate at the turn of the century, only to start decreasing as a result of the 2008 recession (Toossi & Morisi 2017). Enduring labor discrimination played a factor in this dip. Stigmas regarding women as less productive and riskier human capital investments identified them as the more expendable workers, lining them up to be laid off first. For the majority with families, childcare was forfeit in order to economize, leaving women to take on more home labor and become marginally attached. The rate has yet to recover to its standing, potentially contributing to the continued stagnation of the gender wage gap. Unions, however, may provide an answer.

Women, historically, are far less likely to unionize relative to men (Evan and Macpherson 1993). The phenomena are consequent to enduring occupational segregation and the lack of successful, organizational efforts in industries typically dominated by women (Antos, Chandler, and Mellow 1980). If the theory on union bargaining power, laid out by Blanchflower and Bryson, holds, failures in successful mass organizing would see insignificant union wage effects in female dominated sectors. However, with occupational segregation on the decline

(Blau & Kahn 2016), the number of women entering more organized industries simultaneously increases, resulting in a more potent union wage effect for female workers.

In “How Does Collective Bargaining Affect the Gender Pay Gap?” (2001), Elvira and Saporta lay out a theoretical framework which affirmatively tackles the negative association between unionism and the gender wage gap. Gender discrepancies in wages appear for a variety of differences between measured determinants. Human capital investments account for the extent in which acquired education and experience dictate wages. Gender differences in tenure and educational careers then explain such a manifestation of the gender wage gap (Elvira and Saporta 2001). Additionally, occupational segregation in industries and firms also concurrently contributes to the perpetuation of the gap, as discussed previously. The remaining, unexplained portion of wage disparity is tied to employer discrimination. Elvira and Saporta propose unionization acts as a mitigating factor in respect to wage discrimination, occurring more specifically on a firm-by-firm basis. A wage setting is a common condition found within collective agreements, seeing reductions in wage differentials between workers covered by such agreements, predominantly for those of the same occupations. It follows, then, wage gaps between men and women of the same positions should close when a collective agreement is in place. The notable implications of this effect have even contributed to more direct policies such as pay equity strategies, only increasing female pay to meet male earnings and closing the gender wage gap (Elvira and Saporta 2001).

Evan and Macpherson examined the wage gap from 1973 to 1988, where it declined by 9.3%. Within that decline, 14.3% of the change was attributed to changes in female unionism, substantiating some union effect on the gender wage gap (Evan and Macpherson 1993). Elvira and Saporta suggest such an effect is more potent on gender wage gaps within establishments,

removing the distortion of the effect from occupational segregation (Elvira and Saporta 2001).

The argument is if unionization began to increase and occupational segregation continued to dissipate, the consequence could be closing the gender wage gaps on the industry and sector levels, eventually closing the gap at-large from the bottom up.

If one is to measure the extent to which unionization acts a contributory factor in reducing the gender wage, instances of observable changes in union density, and resulting movements in the gender wage gap, are required. Implementations of policies with a de-unionizing effect fit that requisite. Right-to-work (RTW) legislation is a prime example of a policy with such an outcome. In 1947, the Taft-Hartley amendments to the National Labor Relations Act of 1935 established a state's right to enact laws barring unions from necessitating dues from workers, even if they are covered by a collective agreement (Gould and Shierholz 2011). The next decade saw twelve states, predominantly in the Southern United States, readily pass such legislation, even constitutionally in some cases. More states in the South and Midwest have slowly become RTW states over the years, up until the past decade. Five states (Indiana, Michigan, Wisconsin, West Virginia, Kentucky) have implemented RTW in the span of five years, a rate not previously seen since the 1950s.

Presently, in twenty-seven states, RTW laws enfranchise non-due-paying employees to the same benefits granted to dues-paying members under union security agreements (Gould and Kimball 2015). Higher wages, grievance counsel, negotiated insurance, and all union benefits would apply to all firm workers without exception. Intention behind the law, emphasized in the name, hopes to secure employees from provisions in which they are obligated to financially support unions but still experience the same privileges of a union contract (Moore 1998). The supposed desire is to end non-union worker exclusion from potential employment as well as

stimulate economic growth (Gould and Kimball 2015), however, an underlying outcome persistently exists: RTW laws, in effect, decrease unionization and discourage new organization within their relative states.

Free-riding, consequently, flourishes in RTW states as union benefits are guaranteed to workers without condition of dues. Workers are then financially incentivized to avoid personal cost of union membership, as they already enjoy the advantages. Such lack of incentivization then restricts primary union income, encumbering organizational capabilities and other union resources (Gould and Kimball 2015). Further following of the “Free Rider Hypothesis” sees union members, in a RTW state, paying higher union dues to compensate for the coverage of non-due-paying employees (Moore 1998). The effect, in economic consequence, also sees a fall in union supply. Given the positive association between gains in membership (thus dues as RTW laws are not present) and benefits reaped by members, RTW laws discourage unions from entering areas where they are no longer guaranteed the dues needed to meet initial costs of organizing, driving down union supply (Farber 1983). Diverging from the free-rider problem, “Bargaining-Power Hypothesis” suggests RTW laws decrease union bargaining power in direct consequence. Lacking the ability to implement membership standards within firms, unions exert diminished bargaining-power from a reduced organized workforce, of which diminished membership benefits are a consequence. The depletion of benefits would see a subsequent drop in demand for union membership (Farber 1983). According to Moore, both theories are not mutually exclusive and, in tandem, result in drops in unionization altogether; members opting to de-unionize in response to higher fees for open lower-grade benefits.

Arguments, in contrast, were made in the idea of RTW laws simply being symptomatic of a state’s “taste” towards union presence and, in actuality, hold no real independent effect on

unionization (Farber 1983). Anti-union sentiment is a logical, possible precursor to RTW laws, and unionization, within the hypothetical state, may already face restrictions and unpopularity. Such a “Taste Hypothesis” (Moore 1998), howbeit, assumes a significant amount of anti-union policy predating any RTW laws and realizing a full effect against any unionization outcomes.

Concerning literature on the relationship between RTW laws and wages, there exists very little. Of the limited selection, reports on the RTW wage effects are various and concentrate on two areas of effects: union wage premia and average wages (Mishel 2001). In his overview of existing literature in 1998, Moore notes RTW laws “have little or no influence on average wages, union wages, or nonunion wages” in both private and public sectors with an accompanying caution that such results are incredibly sensitive to the model specification (Moore 1998). The sensitivity appears to be around the treatment of RTW presence as an endogenous or exogenous variable; the former seeing statistically insignificant effects on wages, and the latter showing a notable negative impact (Moore and Newman 1985). Mishel finds a differing effect, seeing an average wage penalty of 6.5% for RTW states, which drops to approximately 4% when cost of living is controlled (Mishel 2001). Multiple, following studies concur with Mishel’s estimation of RTW wage penalty. Gould and Shierholz approximate a 3.2% penalty in average wages and a 3% penalty for non-union workers in RTW states (2011). Chave, Danis, and Hsu suggest RTW laws exert an immediate, negative effect on wage growth as soon as they are implemented (2020). The effect on wage growth, however, is not permanent in the long run, but does corroborate a lasting negative effect on wage levels (Chave, Danis, and Hsu 2020).

By establishing RTW laws may prove detrimental to both unionization and wages, the laws could prove to be an indicator of the effect unionization holds on the gender wage gap or to contribute to the gap itself. This paper will examine the implementation of RTW laws across a

ten-year period and note the relationship with wages within the states in which they are enforced.

METHODOLOGY

I. MODEL

As this paper is attempting to understand the ramifications of a policy action, a difference-in-differences (DID) model appears appropriate to capture the causal impacts of RTW law adoption on real wages at the time of adoption and onward. In order to isolate the varying effects of RTW laws on the categories relevant to this study (sex and union membership), the following model will be applied to four different wage groups (female union members, male union members, female non-union members, and male non-union members) as well as to restricted samples by industry.

$$\ln(RW)_{ist} = \beta_0 + \beta_1 PostRTW_{st} + \beta_2 RTWState_s + \beta_3 Educ_i + \beta_4 Exper_i + \alpha_t + \gamma_i + \varepsilon$$

The response variable, $\ln(RW)$, is the natural log of the individual's real wage in 2019 dollars. Taking a semi-log functional form allows measuring in percent changes of wages as a response to specified variable. As the wage distribution is cut off at zero, absolute wages, for a response variable, may lead to some individuals receiving a negative wage estimation, so the percent change method is more appropriate in capturing reality. Due to its comprehensive amount of data entries, the 2019 real wage is the most consistent wage data available as removes any distortions of inflation.

$PostRTW$ is a dummy variable, when equal to 1, indicates whether an RTW law is present in the corresponding year and state. In the year the state of the individual overserved implements

the law and all subsequent years, $PostRTW$ is equal to 1. All years before implementation, $PostRTW$ will be equal to 0. $RTWState$, when equal to 1, signifies if a state adopts a RTW law at any point in time, 0 if otherwise.

In typical DID models, an interaction term is present in order to denote when a policy takes effect and remains in the treatment groups, but in the case of policy, RTW laws are implemented on various years within their respective states. With then multiple treatment times existing across the data, the $PostRTW$ variable satisfies the need to identify the control states and RTW states at the proper years, removing the need for an interaction variable as it would be completely colinear with $PostRTW$. The estimated effect of the RTW laws is then captured in β_1 .

The $RTWState$ coefficient encapsulates any wage effects emitted from statewide factors that are not RTW laws. Potential wage changes due to other laws or compositions of the RTW state are stated in β_2 , however, it does not communicate the influence in which individual characteristics hold on wages. As the model is measuring percent changes in real wages, there are established, individualistic wage determinants which also must be included. Mincer's "human capital earnings function" (Lemieux 2003) is the longstanding default in estimating wage changes as a result of two human capital factors: education level ($Educ$) and experience ($Exper$). Holding positive effects on wages, both are key in capturing variation in the wage distribution, though experience does increase wages at diminishing rate. Nevertheless, each are integrated into the model.

In order to eliminate possible bias from any outstanding conditions, two fixed effects are incorporated. Since the estimating data spans ten years (2009-2019), the observation year is fixed to acquire said year's impact on real wages, as expressed by α . Race, another significant social determinant of wage, is also fixed, conveying its influence as γ .

II. DATA

The data for the model originates from the Current Population Survey Outgoing Rotation Group (CPS ORG) from the year 2009 through the year 2019. The CPS ORG is a monthly survey conducted in joint effort by the Bureau of Labor Statistics and the U.S. Census Bureau, and the files include numerous variables such as hourly pay or salary, hours/weeks worked, education level, demographics, union records, etc. (Economic Policy Institute 2019). The structure of the ORG is a “rolling panel” (Economic Policy Institute 2019) where household data is recorded for four consecutive months (out of eight in total), and then another four months. The ORG contains a log of responses from those surveyed in their fourth or eighth month and ultimately contains a quarter of the overall CPS data (Economic Policy Institute 2019).

To participate in the survey, respondents must meet the following criteria. Respondents must be at least sixteen years of age or older, employed in either the public or private sectors (self-employed not included), held hours within a range of 1-99 per week, and acquired wages from \$0.50 and up. If salary workers, the hourly wage was calculated by weekly wage divided by weekly number of hours worked (Economic Policy Institute 2019). The 2019 real wage was calculated using the CPI-U-RS and includes entries of overtime, tips, and commission.

RTWState was constructed by identifying all states in the U.S. with existing RTW laws, which is currently numbered at twenty-seven states, and noting them with a value of 1. All states without RTW laws were given a value of 0. *PostRTW* notes the year of RTW adoption, recording a value of 1 for any state observation starting that year and continuing each following year. See Table 1 in the Appendix for a complete list of RTW states and their corresponding year of initiation. This study is primarily concerned with the impacts of the second cohort of states to

adopt RTW legislation, so the state of Oklahoma, adopting in 2001, was removed from the data to stop any possible distortions from its isolated position in years.

Education levels are valued as described in Table 2 (see Appendix), however, within the lower education levels, the CPS grouped grades together, mostly in pairs, under a same value which was unrelated to actual educational level reached. Proving quite difficult to disentangle, the pairs assigned a value corresponding with the higher grade's typical year of education attained. Although not optimal, such recoding grants a better chance in capturing the individuals' actual education level while not forfeit much model integrity as the marginal difference in wage earnings is statistically insignificant at early levels.

Additionally, this manipulation of data is preferred as it more accurately gages individual years of experience. Following an adjusted Mincer's formula for calculating experience (age minus year of schooling minus sixteen) (Lemieux 2003) and using the recoded education year dummy, more approximate values of experience are available to apply as a control. Instances of negative expected experience were generated but simply replaced with a value of zero, making the assumption individuals are not working under the age of sixteen and not working full-time while in school. As previously stated, for participation in the CPS ORG, an individual must be at least sixteen, however there is no cap on age, containing observations of individuals up to ninety years old. After examining the distribution of data by age, the cap was lowered to eighty in order to eliminate some outliers whose pension or post-retirement income ran outside the parameters of study. Summaries of each quantitative variable are found in Table 3. The four different wage groups were created using an interaction between the observed individual's sex and their union membership status which generated four separate dummy variables. Concerning the industry-

specific estimations, the definitions of the relevant industries were provided by the CPS and created using the North American Industry Classification System.

RESULTS & ANALYSIS

Starting off, I estimate the difference-in-difference model for men and women separately and separately by union membership. The results from applying the model to all industries found that an individual's real wage in 2019\$ is estimated to rise in response to the presence of a RTW law in their state if male or a female who is not a union member (see Table 3). *PostRTW*, capturing the estimated wage effect of RTW laws, when tested at a 95% confidence level, proved statistically significant for three groups: men and women without union membership as well as male union members. Regarding female union members, the model found no significant effect on wages from RTW. Education level and experience both proved significant, positive determinants of wages as expected, increasing by 11.6% per additional year and 1% per additional year, respectively.

Results for non-members mimic those of members; females are estimated to gain an approximate 2.2% increase in real wages as a result of RTW presence, and males experience nearly twice the gain with estimated 4.2% growth. The RTW “free-rider” theory discussed earlier suggests such increases for non-union members. The prohibition of security agreements, in effect, allows RTW laws to distribute union benefits to all those in the shop, even non-members. An uptick in non-member wages is a possible consequence of union benefit dissemination. The unexpected outcome, however, is the 2.9% wage increase for male union members. Constituting the majority of union members, males should expect a drop in wages, not a consequent rise. Again, female members did not experience any impact.

Among both union members and non-members, then, it is clear that RTW laws produce more positive income results among males than among females, conveying RTW laws were not successful in reducing the gender wage gap. This is not to say the proposed mitigating effect of unionization is reduced or even existent. Union males observably benefit from the policy, and if the decrease and unionization and union power from RTW was contributing to the stagnation of gap closure, union males, being the majority, should face some drop in wage. To better isolate RTW influence on unionized workers, the model was subsequently administered onto a more limited sample, specifically individuals in industries with average or elevated union membership rates (12.5% or above).

These results hold among unionized industries in particular (Table 4). Again, males showed wage growth, 1.6% for members and 2.7% for non-members. Still, a union male pay benefit is attributed to RTW, rejecting the theory of a RTW negative wage effect on union workers. Female union members, however, now see a 2.1% dip in wages with female non-members witnessing no significant change. In opposition to the previous output, a negative RTW impact towards female union members suggests some gender divergence in the consequences of either unionization or some outstanding effect from RTW laws. The results from this sample, nevertheless, affirm the same claim of RTW laws holding no mitigating effects on the gender wage gap and even go on to support their contribution to gap, itself. The aftermath of RTW implementation consistently sees male wages favored with either gains when females hold none or gains always relatively larger than females.

The presence of the gap already sets female behind to where the disproportionate and inverse interactions of male and female income with the laws makes for an impossible catchup. Whether this is due to decreases in unionization or some unexplained RTW effect remains to be

examined and applying the model to isolated industries could provide insight. Disaggregating the unionized industries sample by individual industry yielded mostly insignificant results pertaining to the impact of RTW laws, except for two instances.

The model shows these results hold even within unionized industries where women are over-represented: educational services and public administration. Applying the model to Educational Services (Table 5) found RTW presence sees pay increases for non-union members, 2% for females and 4.6% for males, and a decrease in union wages, -4.4% for females and -4.7% for males. Educational Services displays some of the highest union member numbers of any industry with a major portion being female. The opposite relationships of members and non-members with RTW law presence supports some level of detrimental influence on union bargaining power by RTW.

The Public Administration concentration (Table 6) found results more consistent with the original sample, seeing male and female non-members increase wages in response to RTW laws (7.8% and 1.4% respectively). Union member wages were shown to be unaffected, however, the gap in wage growth between the sexes of those without membership again affirms no gap mitigation from RTW laws in general.

CONCLUSION

Right-to-works laws seem to exert some contribution to the gender wage gap, except the line of dominos between the two is not so apparent. Following the estimates produced, the laws' negative effect on unionization is not the vehicle between RTW implementation and gap stagnation despite initial theory. The constant increase in male wages across all samples with an

accompanying decrease (or no effect) in female wages, particularly for female union members, shows some disproportionate RTW impacts in regard to gender.

In every sample, males experience the greatest gains, specifically males without membership, conveying RTW laws favor male wages in all industries. Union males experience increases in pay in both aggregate industry samples but at a lesser rate than their non-member counterparts. Even so, wage increases for any union members by RTW law is inconsistent with the presented theory of union mitigation, but an unexplained addition to the gender wage gap from these laws is present due to their favoring of male workers.

The industry specific samples, both relatively high in female employment as well as union membership, corroborate the RTW wage impacts so far. Educational Services sustained a 9% difference in wage changes between union females and non-union males. As Educational Services is a predominantly female industry, it accounts for much of the female labor force, so the apparent reduction in the female union member's earnings and up drive of non-union male earnings ripples out to affect the gap on a macroeconomic scale.

Public Administration, also containing significant amounts of female and unionized workers, saw increases in wages for non-members, especially males, as well. Causing more explicit male gains within a prominent female industry, RTW laws are evidently involved in sustaining the wage gap.

The remaining question is concerned with how RTW laws stagnate or expand the gap if not by reducing union mitigation effect. Further study may concern the effectual differences of these laws on the various distinctions between gender segregated industries. The only observed case of decreased union wages occurred in Educational services, a predominantly female, unionized industry. Another dimension possibly worth exploring are potential disparate effects

based in whether the industry resides predominantly in the public or private sectors. Much of Educational Services lies in the public sector as well as all of Public Administration, and as these two industries were the only to produce some significant results individually, such a common factor might offer explanation.

APPENDIX

Table 1: Right-to-Work States

States	Year Adopted
Alabama	1953
Arizona	1946
Arkansas	1947
Florida	1944
Georgia	1947
Idaho	1985
Indiana	2012
Iowa	1947
Kansas	1958
Kentucky	2017
Louisiana	1976
Michigan	2012
Mississippi	1954
Nebraska	1946
Nevada	1951
North Carolina	1947
North Dakota	1947
Oklahoma	2001 (omitted)
South Carolina	1954
South Dakota	1946
Tennessee	1947
Texas	1947
Utah	1955
Virginia	1947
West Virginia	2016
Wisconsin	2015
Wyoming	1963

Table 2: Education Level Values

Education level, post-1991	Education Level Value.
1st-4th grade	4
5th-6th grade	6
7th-8th grade	8
9th grade	9
10th grade	10
11th grade	11
12th grade-no diploma	12
HS graduate, GED	12
Some college but no degree	14
Associate degree	14
Bachelor's degree	16
Master's degree	18
Professional school	20
Doctorate	21

Table 3: All Industries Sample

	Non-Union Females	Non-Union Males	Union Females	Union Males
	ln(RW)	ln(RW)	ln(RW)	ln(RW)
<i>PostRTW</i>	.0216*** (.0031)	.0419*** (.0033)	-.0058 (.0081)	.0294*** (.0071)
<i>RTWState</i>	-.108*** (.003)	-.1064*** (.0033)	-.0932*** (.0076)	-.0793*** (.0067)
<i>Educ</i>	.1164*** (.0002)	.1098*** (.0002)	.1002*** (.0006)	.0704*** (.0006)
<i>Exper</i>	.0082*** (0)	.0103*** (.0001)	.0055*** (.0001)	.0063*** (.0001)
<i>_cons</i>	1.1753*** (.0042)	1.4784*** (.0041)	1.5967*** (.0116)	2.2106*** (.0104)
2010	-.0051* (.0027)	-.016*** (.0029)	-.0051 (.0067)	-.0091 (.0064)
2011	-.0248*** (.0027)	-.0409*** (.0029)	-.0213*** (.0067)	-.0369*** (.0065)
2012	-.0359*** (.0027)	-.0438*** (.0029)	-.0397*** (.0069)	-.0367*** (.0065)
2013	-.0355*** (.0027)	-.0473*** (.0029)	-.0365*** (.0069)	-.0457*** (.0065)
2014	-.039*** (.0027)	-.0548*** (.0029)	-.0452*** (.0069)	-.0507*** (.0065)
2015	-.0229*** (.0027)	-.0307*** (.0029)	-.038*** (.0069)	-.0385*** (.0066)
2016	-.0079*** (.0027)	-.0114*** (.0029)	-.0189*** (.0071)	-.0292*** (.0067)
2017	.0029 (.0027)	-.0069** (.0029)	-.0196*** (.0071)	-.0101 (.0067)
2018	.0088*** (.0028)	.005* (.0029)	-.0318*** (.0072)	-.0164** (.0068)
2019	.0237*** (.0028)	.0252*** (.003)	-.0108 (.0073)	-.0116* (.0069)
White				
Black	-.0695*** (.0019)	-.2054*** (.0023)	-.0623*** (.0048)	-.1929*** (.0049)
Hispanic	-.0242*** (.0019)	-.0519*** (.0019)	-.014*** (.0052)	-.0577*** (.0047)
Asian	.0213*** (.0026)	-.002 (.0027)	.0119* (.0064)	-.0846*** (.0066)
Native American	-.0626*** (.0065)	-.1309*** (.0076)	-.0518** (.0202)	-.0649*** (.0187)
Mixed	-.0615*** (.0051)	-.108*** (.0057)	-.0634*** (.0127)	-.0188 (.0117)
Observations	788335	793715	92909	106791
R-squared	.2609	.276	.2351	.141

Standard errors are in parentheses

*** $p < .01$, ** $p < .05$, * $p < .1$

Table 4: Unionized Industries Sample

	Non-Union Females	Non-Union Males	Union Females	Union Males
	ln(RW)	ln(RW)	ln(RW)	ln(RW)
<i>PostRTW</i>	-.0042 (.0052)	.0269*** (.0049)	-.0214** (.0089)	.0168** (.0078)
<i>RTWState</i>	-.0846*** (.0051)	-.0933*** (.0049)	-.0915*** (.0084)	-.0817*** (.0074)
<i>Educ</i>	.1035*** (.0004)	.0889*** (.0003)	.091*** (.0007)	.0626*** (.0007)
<i>Exper</i>	.0064*** (.0001)	.009*** (.0001)	.0046*** (.0002)	.0053*** (.0001)
_cons	1.4715*** (.0075)	1.843*** (.0062)	1.7841*** (.0137)	2.3697*** (.0115)
2010	.0036 (.0047)	-.0162*** (.0044)	-.0035 (.0074)	-.0085 (.007)
2011	-.0146*** (.0047)	-.0406*** (.0045)	-.0237*** (.0074)	-.0371*** (.0071)
2012	-.0279*** (.0047)	-.0449*** (.0045)	-.0389*** (.0076)	-.0336*** (.0072)
2013	-.0331*** (.0047)	-.0446*** (.0045)	-.0392*** (.0076)	-.0454*** (.0072)
2014	-.0359*** (.0047)	-.0547*** (.0044)	-.0469*** (.0076)	-.0514*** (.0072)
2015	-.0219*** (.0047)	-.034*** (.0044)	-.0408*** (.0077)	-.0337*** (.0073)
2016	-.0166*** (.0047)	-.0196*** (.0044)	-.0142* (.0078)	-.0297*** (.0074)
2017	-.0068 (.0047)	-.0124*** (.0044)	-.0195** (.0079)	-.0061 (.0074)
2018	-.0071 (.0048)	-.009** (.0044)	-.0335*** (.0079)	-.0128* (.0075)
2019	.0009 (.0048)	.0082* (.0045)	-.0211*** (.008)	-.0107 (.0076)
White				
Black	-.0663*** (.0033)	-.1901*** (.0035)	-.0497*** (.0055)	-.1818*** (.0055)
Hispanic	-.0452*** (.0035)	-.0532*** (.0029)	-.001 (.0061)	-.0412*** (.0054)
Asian	.0047 (.0049)	-.0374*** (.0047)	.0451*** (.0075)	-.0551*** (.0076)
Native American	-.046*** (.0104)	-.1102*** (.0108)	-.0395* (.0225)	-.0562*** (.0204)
Mixed	-.0357*** (.0093)	-.0661*** (.0089)	-.0561*** (.0144)	-.0111 (.0128)
Observations	241250	293237	73476	83811
R-squared	.2304	.2409	.1896	.1197

Standard errors are in parentheses

*** $p < .01$, ** $p < .05$, * $p < .1$

Table 5: Educational Services

	Non-Union Females	Non-Union Males	Union Females	Union Males
	ln(RW)	ln(RW)	ln(RW)	ln(RW)
<i>PostRTW</i>	.0198** (.009)	.0461*** (.0141)	-.0441*** (.0112)	-.0477** (.0196)
<i>RTWState</i>	-.0954*** (.009)	-.1074*** (.014)	-.0816*** (.0106)	-.0697*** (.0183)
<i>Educ</i>	.1133*** (.0007)	.1119*** (.001)	.1101*** (.001)	.0957*** (.0015)
<i>Exper</i>	.0067*** (.0001)	.0111*** (.0002)	.0049*** (.0002)	.0064*** (.0003)
_cons	1.1695*** (.0131)	1.2259*** (.0189)	1.4124*** (.0194)	1.7119*** (.0294)
2010	.0088 (.008)	.0169 (.0128)	.0052 (.0094)	-.0029 (.0162)
2011	-.0116 (.0081)	-.0039 (.0128)	-.009 (.0094)	-.0092 (.0164)
2012	-.0411*** (.0081)	-.0051 (.0129)	-.0308*** (.0096)	-.0118 (.0165)
2013	-.0384*** (.0081)	.0082 (.0129)	-.0267*** (.0097)	-.0211 (.0167)
2014	-.032*** (.0081)	-.0254** (.0128)	-.0431*** (.0097)	-.0337** (.0167)
2015	-.0191** (.008)	.0112 (.0129)	-.0338*** (.0098)	.0144 (.0169)
2016	-.0148* (.008)	.0169 (.0128)	-.0098 (.0099)	.0047 (.0173)
2017	-.0043 (.008)	.0201 (.013)	-.0095 (.01)	.0045 (.0173)
2018	-.0033 (.0081)	.0297** (.013)	-.0265*** (.0101)	.0282 (.0176)
2019	.0011 (.0082)	.0413*** (.0131)	-.0165 (.0101)	.018 (.018)
White				
Black	-.0093 (.006)	-.0754*** (.01)	-.0206** (.0083)	-.0703*** (.0139)
Hispanic	.01 (.0062)	.0042 (.0104)	.0287*** (.0082)	.0225 (.0139)
Asian	-.0374*** (.009)	-.0543*** (.0115)	-.0101 (.0119)	-.0145 (.0179)
Native American	.0282 (.0189)	-.0836*** (.031)	.0069 (.0336)	-.106** (.0517)
Mixed	-.0098 (.0164)	-.0262 (.0238)	-.0538*** (.0208)	.011 (.0326)
Observations	85522	40805	42527	15743
R-squared	.2455	.286	.2205	.2131

Standard errors are in parentheses

*** $p < .01$, ** $p < .05$, * $p < .1$

Table 6: Public Administration

	Non-Union Females	Non-Union Males	Union Females	Union Males
	ln(RW)	ln(RW)	ln(RW)	ln(RW)
<i>PostRTW</i>	.0382*** (.0142)	.0781*** (.0153)	.0096 (.0252)	-.0023 (.0202)
<i>RTWState</i>	-.1622*** (.0142)	-.1835*** (.0153)	-.0747*** (.0234)	-.1062*** (.0192)
<i>Educ</i>	.112*** (.0011)	.1152*** (.0011)	.0944*** (.002)	.0829*** (.0018)
<i>Exper</i>	.0058*** (.0002)	.0073*** (.0002)	.0047*** (.0004)	.0067*** (.0003)
_cons	1.4471*** (.0207)	1.5212*** (.0209)	1.7678*** (.0342)	2.0418*** (.029)
2010.year	.0067 (.0119)	.0004 (.0128)	-.0265 (.0181)	.0126 (.0157)
2011.year	-.0248** (.0121)	-.009 (.0129)	-.0551*** (.0181)	-.0166 (.0157)
2012.year	-.0117 (.0122)	-.0246* (.0131)	-.0498*** (.0184)	-.0162 (.016)
2013.year	-.0179 (.0123)	-.0251* (.013)	-.0527*** (.0185)	-.0376** (.0159)
2014.year	-.0319*** (.0122)	-.0411*** (.013)	-.071*** (.0185)	-.0488*** (.016)
2015.year	-.0182 (.0121)	-.0204 (.013)	-.0463** (.0189)	-.0272* (.0162)
2016.year	-.0232* (.0121)	-.0174 (.0129)	-.0349* (.0194)	-.0234 (.0166)
2017.year	-.0153 (.0121)	-.0074 (.0129)	-.0427** (.0192)	.0021 (.0163)
2018.year	-.0085 (.0121)	-.0194 (.0129)	-.0709*** (.0198)	-.0254 (.0164)
2019.year	-.0138 (.0123)	.0024 (.0132)	-.0256 (.0198)	-.0047 (.0167)
White				
Black	-.0456*** (.0072)	-.1042*** (.0091)	-.0545*** (.0118)	-.1327*** (.012)
Hispanic	-.0476*** (.0095)	-.0262** (.0107)	.0073 (.0139)	-.0204* (.012)
Asian	-.0036 (.0131)	.0082 (.0141)	-.0193 (.0167)	-.0362** (.0168)
Native American	-.0831*** (.0171)	-.0822*** (.0215)	-.0665 (.0433)	-.0204 (.0459)
Mixed	-.022 (.0216)	.0178 (.0241)	-.0499* (.0272)	-.0125 (.0262)
Observations	35594	36253	11865	18433
R-squared	.2479	.2562	.1721	.1323

Standard errors are in parentheses

*** $p < .01$, ** $p < .05$, * $p < .1$

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