The More You Know, the Better You're Paid? Evidence from Pay Secrecy Bans for Managers

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Abstract

Approximately half of Americans are employed at firms where employees are forbidden or discouraged from discussing their pay with coworkers. Employees who violate these rules may be subject to punishment or dismissal. While many employees are legally protected from reprisal under the National Labor Rights Act, the law exempts managers from these protections. Eleven states have passed laws banning pay secrecy policies for managers. In this paper, we explore what effect these state laws had on the wages and employment of managers. We find pay secrecy bans increased the wages of managers by 3.5% but had no effect on the gender wage gap, job tenure, or labor supply. The effects are heterogeneous along a number of dimensions. Below the median wage, female managers experienced a 2.9% increase in their wages relative to male managers. Above the median wage, male managers experienced a 2.7% increase in their wages relative to female managers. The wage gains were concentrated among managers employed at firms with fewer than 500 employees.

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1 Introduction

Corporate pay secrecy policies restrict employees' ability to discuss issues regarding pay and compensation with their coworkers. Pay secrecy may be a formal rule, but more often it is enforced as a cultural norm in the workplace. According to a survey by the Institute for Women's Policy Research, 48% of employees work at firms where discussions of pay are either prohibited or discouraged (Hegewisch, Williams and Drago 2011). Two-thirds of employees in the private sector report their company has a pay secrecy policy in place, compared to just 15% of public sector employees (Hegewisch et al. 2011).

Firms implement pay secrecy policies because they believe it reduces worker dissatisfaction by restricting the ability to compare wages (Colella, Paetzold, Zardkoohi and Wesson 2007, Kim 2015).[1] Firms often think wage comparisons between workers are uninformative because there are many variables that can determine wages which are hard for employees to quantify (Colella et al. 2007, Gely and Bierman 2003). Openness about pay and compensation may also lead employees to look for other opportunities after they learn they are paid less than their coworkers (Gely and Bierman 2003). Pay secrecy may be detrimental to employees since it reduces workers' ability to bargain for higher wages due to asymmetric information about salaries at the firm (Kim 2015). These policies may also exacerbate wage discrimination against women and minorities (Colella et al. 2007, Kim 2015). Without the ability to discover their coworkers' earnings, underpaid individuals at a firm cannot gather the necessary information to file a complaint with their firm's human resources department or the Equal Employment Opportunity Commission.

[1] The evidence from the experimental economics literature regarding pay comparisons' effects on worker effort is mixed (Charness, Cobo-Reyes, Lacomba, Lagos and Perez 2016). The evidence from laboratory experiments designed to study pay secrecy shows that worker effort is higher under pay secrecy than it is under pay disclosure (Bamberger and Belogolovsky 2010, Nosenzo 2013). Pay secrecy has also been found to promote participants' willingness to help each other in experiments (Bamberger and Belogolovsky 2017). In an experiment designed to test wage inequality between managers and non-managers, Hesse and Rivas (2015) find a significant decline in non-managers' effort choices when the wage gap between managers and non-managers increases, suggesting a possible reason for pay secrecy among managers.

The federal government in the United States has long recognized the important role discussions of pay and compensation play in promoting bargaining. Since 1935, the right to discuss pay and compensation has been codified in the National Labor Rights Act (NLRA). Because the NLRA exempts managers and supervisors, states have filled this gap in the federal law with state-level pay secrecy bans. Managers in states which pass a pay secrecy ban gain the right to discuss pay and compensation with coworkers without fear of reprisal. Legal protections for non-managers remain unchanged by the passage of state-level pay secrecy bans.[2]

In this paper, we evaluate the effect of state-level pay secrecy bans on the wages and employment of managers. Using data from the 1977 through 2016 Current Population Survey, we construct a repeated cross-section of American workers and use a differences-in-differences-in-differences (DDD) methodology to identify the effect of pay secrecy bans on the labor market outcomes of managers. The DDD methodology identifies changes in the outcomes of managers due to the passage of a pay secrecy ban by comparing differences between managers and non-managers in states that pass a pay secrecy ban to differences between managers and non-managers in states that did not pass a law. We find the passage of a pay secrecy ban increased the wages of managers by 3.5%. We find no evidence pay secrecy bans increased the labor force participation, hours worked per week, or job turnover of managers. Our results are robust to the inclusion of state time trends, correcting for selection into the labor force using both parametric and nonparametric estimation techniques, and correcting the standard errors to account for the small number of treated states.

The primary contribution of this paper is to provide a better estimation of the effect of pay secrecy bans. The evidence from this paper suggests a more cautious interpretation

[2]State laws may provide a new avenue of complaints from non-managers, but given that state enforcement agencies are not funded as well as the national Equal Employment Opportunity Commission, and pursue fewer instances of litigation (Kim 2015), this does not represent a meaningful increase in the protections for such individuals.

of the effect than earlier work investigating these policies. Only Kim (2015) has studied these policies, using full-time workers as the treatment group. In contrast, we define our treatment group as workers who report being employed in management occupations. While Kim (2015) focuses on the effect of pay secrecy bans on the gender wage gap, we take a more comprehensive view and investigate a larger number of outcomes. We provide additional evidence of the heterogeneity in the effects of state-level pay secrecy bans. When we test for differential impacts of the law on high- and low-paid managers, we find high-paid male managers experienced statistically significant increases in wages, and low-paid managers do not. The laws' effect on the gender wage gap varies over the wage distribution as well. Pay secrecy bans reduce the gender wage gap among the low-paid managers but increase the gender wage gap among high-paid managers. We also find significant effects by firm size. Managers at large firms experienced reductions in wages, while managers at small and medium-sized firms experienced increases in wages.

Our work provides important evidence of the effect of a common policy promoted to improve pay equality at a time when many state governments are seeking to address gender wage gaps. Pay secrecy bans are closely related to employment non-discrimination laws. Both policies aim to increase the wages of marginalized groups: pay secrecy bans by promoting wage bargaining by workers and non-discrimination laws by discouraging discriminatory wages offered by employers. Our finding that pay secrecy bans do not reduce the gender wage gap is in-line with previous work finding that gender equality laws are associated with reductions in average wages for women (Neumark and Stock 2006), that such laws have little effect on the wage gap for older women (Lahey 2008, Song McLaughlin 2017), and that additional worker protections had no effect on the wages of lesbians (Burn 2018, Klawitter and Flatt 1998, Klawitter 2011).

2 Pay Secrecy Policies in the United States

In the United States, pay secrecy policies are common.[3] These policies either take the form of explicitly written rules in the employee handbook or as unwritten cultural norms (Gely and Bierman 2003). A typical rule may state that wages are a "confidential matter between the employee and his earnings supervisor...[discussions of earnings] will result in dismissal and/or disciplinary action at the supervisor's discretion" (Gely and Bierman 2003).[4] Half of all employees work for firms that have pay secrecy policies (Hegewisch et al. 2011).

2.1 Protections under the National Labor Rights Act

Section 7 of the NLRA protects the rights of employees to bargain for higher wages. Federal courts and the National Labor Relations Board (the government agency in charge of enforcing the NLRA) have repeatedly ruled that pay secrecy policies are a violation of the law (Colella et al. 2007, Gely and Bierman 2003). Though, pay secrecy policies may be legal if they are designed to protect a legitimate business interest or specific proprietary information (Gely and Bierman 2003).

When deciding if a policy violates the NLRA, the court applies a balancing test that weighs the rights of workers to engage in collective action against the rights of employers to protect their legitimate business interests. The most common legitimate business interest that employers cite is that pay secrecy policies are put in place to reduce worker discontentment and jealousy (Kim 2015). They argue that allowing discussions about wages when employees do not have information about all factors involved in wage-setting

[3] The IWPR/Rockefeller survey conducted in 2010 asked workers whether they were subject to a pay secrecy policy at work. Respondents could respond either "a) wage and salary information is public; b) wage and salary information can be discussed in the workplace; c) discussion of wage and salary information is discouraged by managers; d) discussion is formally prohibited, and/or employees caught discussing wage and salary information could be punished" (Hegewisch et al. 2011).

[4] The quoted pay secrecy policy comes from Fredericksburg Glass and Mirror, Inc., and resulted in the 1997 National Labor Relations Board decision that these types of rules violated the National Labor Rights Act (Gely and Bierman 2003).

is counterproductive and reduces the productivity of the firm. This reason has not swayed the courts. The Supreme Court has found that so long as the activities in question were aimed "to improve terms and conditions of employment or otherwise improve their lot as employees," discussions of pay are considered protected activity (Colella et al. 2007, Gely and Bierman 2003, Kim 2015).

Courts' suspicions of pay secrecy policies have been tempered in recent years as policies have shifted towards banning discussion of how someone earns their wages (rather than the amount of wages they earn) under the theory that compensation schemes are a protected business secret (Gely and Bierman 2003). As compensation practices become more algorithmic, the discussion of the algorithms may be restricted to avoid the details being revealed to outside parties. Gely and Bierman (2003) argue that the downside of these business intelligence exemptions for pay secrecy policies is that they may result in curbs being placed on the discussion of pay by employees because such discussions reference the algorithm that determines pay as essential context to pay differentials. A movement towards more broad exemptions of pay secrecy policies would reduce the types of policies covered by the law and dilute the effect of both federal and state pay secrecy regulations.

2.2 State-Level Pay Secrecy Policies

While the NLRA protects the rights of non-supervisory employees to discuss their wages and earnings with their coworkers, the law does not extend these protections to supervisory employees or contractors. This gap in the NLRA has led to many managers being underpaid and unable to bargain for higher wages because they do not know how much their colleagues are making. Famous examples of this scenario include Lily Ledbetter and Jennifer Lawrence (Boot 2014, Gely and Bierman 2003).[5] These women were only able to bargain for higher

[5]https://www.thedailybeast.com/exclusive-sony-hack-reveals-jennifer-lawrence-is-paid-less-than-her-male-co-stars, accessed January 22, 2018.

wages after their colleagues' wages were revealed to them by others. In Ledbetter's case, information about her peers' wages came via an anonymous note from a colleague (Gely and Bierman 2003). The pay differential for Jennifer Lawrence was publicized by an email hack of Sony Entertainment. The email made clear that the producers of the film *American Hustle* knew she was paid less than her male co-stars and wanted to keep Lawrence's share of the revenue lower (Boot 2014).

Table 1: List of States and Years Law was Passed

State	Year
California	1984
Colorado	2008
Connecticut	2015
Illinois	2003
Maine	2009
Michigan	1982
Minnesota	2014
New York	2015
New Hampshire	2014
New Jersey	2013
Vermont	2005

Source is the U.S. Department of Labor. www.dol.gov/wb/resources/WB_PaySecrecy-June16-F-508.pdf. Massachusetts passed a pay secrecy ban in 2016, but it was designed to phase in slowly between 2016 and 2018. Therefore, we exclude it from the analysis. Accessed: January 25, 2018.

Eleven states have passed laws that make pay secrecy illegal to address the gap in coverage of the NLRA. Table 1 provides a list of states that have passed a law between 1977 and 2016. The earliest adopters of these policies were in the 1980s (Michigan and California). There were no pay secrecy bans passed in the 1990s, but these policies saw a renewed interest during the Great Recession. State-level pay secrecy bans cover every employee in a state, regardless of supervisory status.[6] They make pay secrecy policies

[6]Some employers and workers may be exempt from these policies if they are exempt from all fair labor and employment laws (e.g., small employers with only a handful of employees or family members).

illegal and subject to the same penalties as other fair labor violations.[7]

2.3 Mechanisms for Pay Secrecy Bans

The proposed mechanism by which pay secrecy bans reduce the gender wage gap often appeals to the information asymmetry between female employees and firms (Kim 2015). Managers who discover they are paid less than comparable coworkers will be able to bargain for higher wages to reduce this gap. At firms where female managers are systematically paid less than male managers, this bargaining will benefit women more than men and reduce the gender wage gap at the firm.

Opponents of pay secrecy bans make three arguments about the cost of these policies to firms. First, workers who are unpleasantly surprised by the comparison between their wage and their coworkers' might complain or quit (Colella et al. 2007). Second, employees who earn less than their coworkers might push for a raise regardless of how the company's normal compensation scheme has rated each worker's productivity (Kim 2015). Third, they believed that pay secrecy bans would result in an increase in the number of pay discrimination complaints, which would be costly to defend even if they were found innocent of discrimination in the end (Kim 2015).[8]

Finally, policymakers cannot predict *a priori* how pay secrecy bans will affect managers' wages. The negotiation between the firm and different employees will involve power differentials across employees and across firms. Where employees have more negotiating power, total expenditure on wages might rise. But when firms have more negotiating power, there is a second contest that occurs among workers. The bargaining behavior of managers will

[7] These penalties are often small compared to the benefits perceived by employers (Gely and Bierman 2003, Kim 2015). Therefore, the potential effect of these policies is likely to be small. Where they are likely to be felt is if they result in information that leads to class action lawsuits of discrimination (Kim 2015).

[8] We report evidence of the number of pay discrimination cases filed with the EEOC after the passage of a pay secrecy ban in Appendix Table A11. We find no significant effect of pay secrecy bans on the number of cases filed with the EEOC. After the passage of a pay secrecy ban, pay equality complaints do not make up a larger share of discrimination complaints either.

then play a pivotal role. Male managers may be more willing to initiate negotiations for higher wages, as seen in the experimental literature on wage bargaining (Dittrich, Knabe and Leipold 2014, Leibbrandt and List 2015, Small, Gelfand, Babcock and Gettman 2007). If gender differences exist in who is able to obtain the "insider information" needed to bargain for wages, then male managers who are better connected may be able to use information about their coworkers' wages to negotiate more effectively on their own behalf.

3 Data

To study the effect of pay secrecy bans on labor market outcomes, we use the Current Population Survey (CPS). Public use micro samples are obtained from IPUMS for the years 1977 to 2016 (Ruggles, Genadek, Goeken, Grover and Sobek 2017). We restrict the sample to adults between the ages of 25 and 65 to exclude individuals still in school and those who are past retirement age. In addition to the age restriction, we drop anyone whose reported hourly wages are below \$2 or above \$15,000 from our analysis. Our final sample consists of 2,089,402 individuals, 327,787 managers and 1,761,615 non-managers.[9]

Data collected in the Current Population Survey is not longitudinal. Therefore, we only observe a respondent's primary occupation at the time of the survey and a respondent's total income from wages for the previous year. A respondent's primary occupation is defined as the job at which they worked the most hours. Unemployed persons and those not currently in the labor force report their most recent occupation.[10] Because state pay secrecy bans only extend new rights to managers and supervisors, we utilize the Census occupation codes from IPUMS to identify all respondents whose primary occupation is

[9] When we focus on the selection of workers into the labor force, we use all individuals between the ages of 25 and 65. For those analyses, our sample size is 2,961,477.

[10] If someone is currently unemployed but was a manager the previous year, we are able to include them in the estimation sample since we observe both income and occupation. We are also able to observe the number of managers who are not in the labor force and test whether the pay secrecy bans had any effect along that margin (see Table A7).

classified as management related (Ruggles et al. 2017).

Table 2 highlights how the observable characteristics of managers differ from non-managers in 2016. Managers earn $39.10 an hour in 2012 dollars, which is about 60% higher than non-managers. Approximately 1 in 3 managers live in states that have a pay secrecy ban. Managers are much more likely than non-managers to be working full time (82% vs. 49%).

We use the Current Population Survey Job Tenure Supplement to study the effect of pay secrecy bans on job tenure. The supplement is collected every two years beginning in 1996.[11] The Job Tenure Supplement contains the employment information of all CPS respondents 15 years and older who were employed in the reference week. Data collected includes occupation and industry from one year ago, and how long the respondent has worked for their current employer. On average, managers had been in their current positions for 6.9 years. This is about half a year longer than the average job tenure for non-managers. Non-managers interviewed in the tenure supplement have been at their job for 6.4 years.

4 Methodology

This paper uses the previous work by Kim (2015) as a branching off point to provide a more robust estimation of the effect of a pay secrecy ban. Kim (2015) uses CPS data on workers between 1977 and 2012 to study the effects of the passage of state-level pay secrecy bans. The author restricts their estimation sample to only full-time workers who were employed year-round. In this sample, the author finds pay secrecy bans had no significant effect on wages, but this was due to pay secrecy bans having a positive and significant effect on the wages of women and having a negative but not significant effect on the wages of men. The

[11]Note that California and Michigan passed pay secrecy bans before the Current Population Survey began collecting this information. Therefore, the estimated effect of pay secrecy bans is not identified from observations in those states. The $PaySecrecyBan$ indicator variable is always equal to one for these states when Job Tenure is used as an outcome variable.

Table 2: Average Worker Characteristics in 2016

	Managers		Non-managers	
	Mean	SD	Mean	Sd
Pay secrecy ban	0.31	0.46	0.28	0.45
Hourly wage	39.10	95.13	25.41	87.03
Full time work	0.82	0.39	0.49	0.50
Controls				
Female	0.46	0.50	0.52	0.50
Black	0.09	0.28	0.13	0.34
Other race	0.09	0.29	0.10	0.29
High school graduate	0.14	0.34	0.30	0.46
Some college	0.23	0.42	0.28	0.45
Bachelor's degree	0.40	0.49	0.19	0.40
Master's degree or Doctorate	0.22	0.41	0.12	0.32
Experience = Age - Years schooling - 5	24.38	10.89	25.63	11.72
Metro area	0.92	0.28	0.86	0.35
Married	0.68	0.47	0.59	0.49
Separated/widowed/divorced	0.14	0.35	0.17	0.38
Manager occupation	1.00	0.00	0.00	0.00
Professional occupation	0.00	0.00	0.18	0.38
Technical occupation	0.00	0.00	0.22	0.42
Service occupation	0.00	0.00	0.13	0.33
Farming occupation	0.00	0.00	0.02	0.15
Production occupation	0.00	0.00	0.08	0.28
Agriculture industry	0.00	0.07	0.02	0.15
Mining industry	0.01	0.09	0.00	0.07
Construction industry	0.08	0.27	0.05	0.22
Manufacturing industry	0.10	0.31	0.08	0.27
Utilities industry	0.06	0.23	0.06	0.23
Wholesale retail industry	0.02	0.15	0.02	0.14
Retail industry	0.09	0.28	0.11	0.31
Finance industry	0.18	0.38	0.04	0.19
Business services industry	0.10	0.29	0.06	0.23
Personal services industry	0.02	0.13	0.02	0.15
Entertainment services industry	0.02	0.15	0.01	0.11
Professional services industry	0.26	0.44	0.23	0.42
Observations	11,868		81,152	

Note: Data come from the 2016 Current Population Survey. Wages are inflation-adjusted to 2012 dollars.

author assumes that since men saw no significant change in their wages after the passage of a pay secrecy ban that they were a good control group in a triple difference setting. Kim (2015) then estimates the triple difference by college education. The author finds that among college-educated workers employed full-time, the effect of pay secrecy bans was a positive and significant increase in female wages. Among non-college educated workers, these policies had a positive but not significant effect. The effect of pay secrecy bans was about twice as large on college-educated workers than it was on non-college educated workers.[12]

For our analysis, we rely on the fact that under the NLRA, all non-managers in the United States enjoy the protections of the pay secrecy bans described in the preceding sections. The stability of worker protections for non-managers when a state-level pay secrecy ban is implemented suggests a clear control group for a quasi-experimental research design. In contrast to Kim (2015), we construct our sample of treated managers and control group non-managers using CPS occupation codes. While there is overlap between the sample used in Kim (2015) and our sample, Kim (2015) excludes a significant portion of the true treated group. Approximately 20% of managers are not employed full-time, and women make up 58% of managers working part-time. As Kim (2015) adds more restrictions to the analysis sample, the number of excluded managers grows. In the most restrictive sample of college-educated workers employed full-time, 59% of managers are excluded because they are either not college educated (38%) or they are not working full-time (21%).

[12] Appendix B discusses the replication of the results from Kim (2015). In the appendix, we show how the results from Kim (2015) change when we include our additional controls (focusing on the initial estimation of workers employed full-time and for a full year).

4.1 Differences-in-Differences-in-Differences Model

Our preferred estimation strategy in this paper is a DDD methodology. We define our treatment group as managers in each state that passed a pay secrecy ban. Non-managers in each state that passed a pay secrecy ban are the within state control group that allows us to control for state-specific shocks. The triple difference methodology identifies changes in the outcomes of managers due to the passage of a pay secrecy ban by comparing the differences between managers and non-managers in states that pass a pay secrecy ban to the differences between managers and non-managers in states that did not pass a law. We estimate the effect of pay secrecy bans on four different outcomes: labor force participation, hours worked per week, wages, and job tenure. In this section, we describe the methodology for estimating the effect of pay secrecy bans on wages. We use the same methodology and equations to estimate the effect on other outcomes.

$$\begin{aligned}\ln Y_{i,s,t} = \alpha + \beta_1(PSB_{s,t} \times Manager_{i,s,t}) + \beta_2(Female_{i,s,t} \times PSB_{s,t} \times Manager_{i,s,t}) \\ +\gamma_1 Female_{i,s,t} + \gamma_2(Female_{i,s,t} \times Manager_{i,s,t}) + \gamma_3(I_t \times Manager_{i,s,t}) \\ +\gamma_4(I_s \times Manager_{i,s,t}) + \gamma_5(I_s \times Female_{i,s,t}) + \gamma_6(I_t \times Female_{i,s,t}) \\ +\gamma_7 I_s + \gamma_8(t \times I_s) + \mathbf{X'}_{i,s,t}\delta + \omega f(L\hat{F}P) + \epsilon_{i,s,t}\end{aligned} \tag{1}$$

The coefficients of interest are β_1, the effect of a ban on the wages of all managers, and β_2, the differential effect of a ban on the wages of female managers. If β_1 is positive, then the wages of all managers increase after the passage of a ban. If β_2 is positive, then the wage gap for female managers shrank after the passage of a pay secrecy ban.

The other terms in the equation control for the differences in wages across states and over time and differences in demographics. We control for the average differences in female wages relative to men (γ_1) and for the difference in the average gender wage gap among managers (γ_2). We control for shocks to the national average wage for managers in each year (γ_3) and state level differences in managers' wages (γ_4). The term γ_5 controls for state-level differences in the wage gap for women, and γ_6 controls for the yearly national wage gap for women. Finally, γ_7 reports state fixed-effects and γ_8 is the coefficient on a linear state-year time trend in wages.

We include a vector of worker characteristics $\mathbf{X'}_{i,s,t}$. We control for differences due to race, potential experience, education, urban residence, marital status, occupation, and industry. See Table 2 for a list of the controls included. The error term, $\epsilon_{i,s,t}$ is associated with worker i in state s during year t. The standard errors are clustered at the state level. When estimating this equation, we weight the observations using the Annual Social and Economic Supplement (ASEC) weights provided by Ruggles et al. (2017).

Our estimates are biased if we ignore the effects of selection into the labor force. Workers that choose to enter the labor force are likely to have different characteristics and outcomes than workers who do not participate. The selection into the labor force may not be random, introducing bias into our estimation. Our preferred nonparametric specification to correct for selection into the labor market involves the quadratic term, $f(L\hat{F}P)$, a polynomial of the predicted probability that a respondent is in the labor force.

4.2 Selection Corrections

Many of our outcomes are only observed for workers who participate in the labor market. For our analysis of the effect of pay secrecy bans on the average outcomes of managers, selection into the labor force does not play a large role. So long as the selection into the labor market is unaffected by the passage of the law, the selection bias will be the same before and after the policy has passed and therefore will be differenced out of our estimate. Selection into the labor market may be affected by the passage of a pay secrecy ban if the knowledge of coworkers wages changes the decisions of individuals to either enter the labor force or to leave it. Managers currently out of the labor force, such as those on maternity leave, may gain new information about wages that affect the decision to re-enter the labor force. If wages are higher than previously thought, individuals may be more likely to re-enter the labor force rather than specialize in home production. Managers in the labor force may be discouraged by how underpaid they are relative to their coworkers and decide to exit the labor force rather than continue to be discriminated against.

Because the CPS occupation categorization reports an individual's most recent occupation (even if they are out of the labor force), we are able to estimate the effect of pay secrecy bans on the selection of managers into the labor force. In Table 3, we present evidence that managers' labor force participation was unchanged on average by the implementation of pay secrecy bans. There is evidence of a differential effect for female managers relative to male managers, but the total effect for female managers is not significantly different from zero. The negative differential effect on female managers provides evidence that pay secrecy bans may affect the selection into the labor force. Therefore, we need to address selection to obtain a valid estimate of the change in the gender wage gap among managers.

We implement a nonparametric two-step estimator to address differential selection into the labor market based on worker characteristics, following the existing literature (Newey 1999, Newey 2009, Depalo and Pereda-Fernandez 2018, Vella 1998).[13] Our preferred nonparametric approach is more robust to misspecification than the Heckman procedure. For both nonparametric and the Heckman corrections, we suggest two potential excluded variables to include in the first stage. Our preferred instrument is a vector indicating the presence of children under the age of five in the household and the number of the worker's own children in the household. This instrument is widely used in the literature (e.g., Mulligan and Rubinstein (2008), Chi and Li (2014), and Chzhen and Mumford (2011)). Children under the age of five may increase the chances a woman is a stay-at-home

[13] Table A9 reports the coefficients from first stage of our nonparametric estimator using our preferred instrument, as well as with the additional instruments we use for robustness checks.

mom since childcare in the United States is privately paid for. After the age of five, children may attend school which allows the mother to re-enter the labor force. This exclusion restriction may be violated if children have an effect on the wages that employers offer mothers and fathers.[14] We show that our results are robust to choosing another commonly used instrument for selection into the labor market, how much of the worker's household income does not come from the worker's own wages (Chzhen and Mumford 2011, Fisher and Houseworth 2012, Blau and Kahn 2017, Jackle and Himmler 2010). As the amount of non-own labor household income increases, an individual may feel less of a need to work. Since employers do not observe non-own labor household income, in theory, it has no effect on the wages of workers.

In our nonparametric two-step procedure, the first stage is estimated as a linear probability model where the outcome of interest is each worker's participation in the labor force. The linear probability model includes the same set of variables as our DDD (see Equation 1). Our first stage estimation also includes an instrument: either children under the age of 5, other household income, or both. We interact the instrument in the first stage with the worker's gender to address the concern that children (or household income) might

[14]There is evidence that the motherhood penalty is a combination of many factors that lead to mothers having lower wages. Kleven, Landais and Søgaard (2018) shows that the decline in female wages after giving birth is due to changes in occupation, promotion to manager, sector, and the family friendliness of the firm for women relative to men. Results from the US also provide suggestive evidence that the decline in wages for mothers is due primarily to the labor supply disruptions of children, rather than the children themselves (Herr 2016). Women who had children before entering the labor force have higher wages than women who have children while in the labor force.

differentially affect labor force participation by gender.

$$
\begin{aligned}
LFP_{i,s,t} = \alpha + \omega_1 instrument_{i,s,t} + \omega_2(female_{i,s,t} \times instrument_{i,s,t}) \\
+\beta_1(PSB_{s,t} \times Manager_{i,s,t}) + \beta_2(Female_{i,s,t} \times PSB_{s,t} \times Manager_{i,s,t}) \\
+\gamma_1 Female_{i,s,t} + \gamma_2(Female_{i,s,t} \times Manager_{i,s,t}) + \gamma_3(I_t \times Manager_{i,s,t}) \\
+\gamma_4(I_s \times Manager_{i,s,t}) + \gamma_5(I_s \times Female_{i,s,t}) + \gamma_6(I_t \times Female_{i,s,t}) \\
+\gamma_7 I_s + \gamma_8(t \times I_s) + \mathbf{X'}_{i,s,t}\delta + \epsilon_{i,s,t}
\end{aligned} \tag{2}
$$

We obtain the fitted values for the probability of selecting into the labor force: $\hat{p}_{i,s,t} = L\hat{F}P_{i,s,t}$. Our selection correction term is quadratic in the probability of being in the labor force: $\omega f(L\hat{F}P) = \omega_1\hat{p} + \omega_2\hat{p}^2$.[15]

The Heckman correction differs from our preferred probability specification by relying on a probit estimation for the first stage. Rather than $\hat{p}$, the Heckman procedure uses the inverse Mills ratio $(\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)})$ in the second stage wage equation. The Heckman procedure does not require an exclusion restriction, but a first stage that includes all variables present in the second stage is identified only using nonlinearities in the probit regression (Wooldridge 2002).

4.3 Pre-Trends & Event Studies

The DDD research design relies on the same parallel trends assumption required by other difference-in-differences analyses. Before estimating treatment effects, we demonstrate that treated states do not differ from untreated states in the years preceding the laws. If pay secrecy bans are passed only in states where the wages of managers are growing at a different rate than states that do not pass a law, the difference-in-difference estimator will not consistently estimate the treatment effect. Similarly, the estimator is not consistent if

[15] The results are identical if we use first, third, fourth, or fifth order polynomials of the predicted probability.

the passage of pay secrecy bans is a response to growing gender wage gaps.

To address these concerns, we conduct event studies on two variables: wages of managers and wage gaps for women. Each event study involves a regression of the form

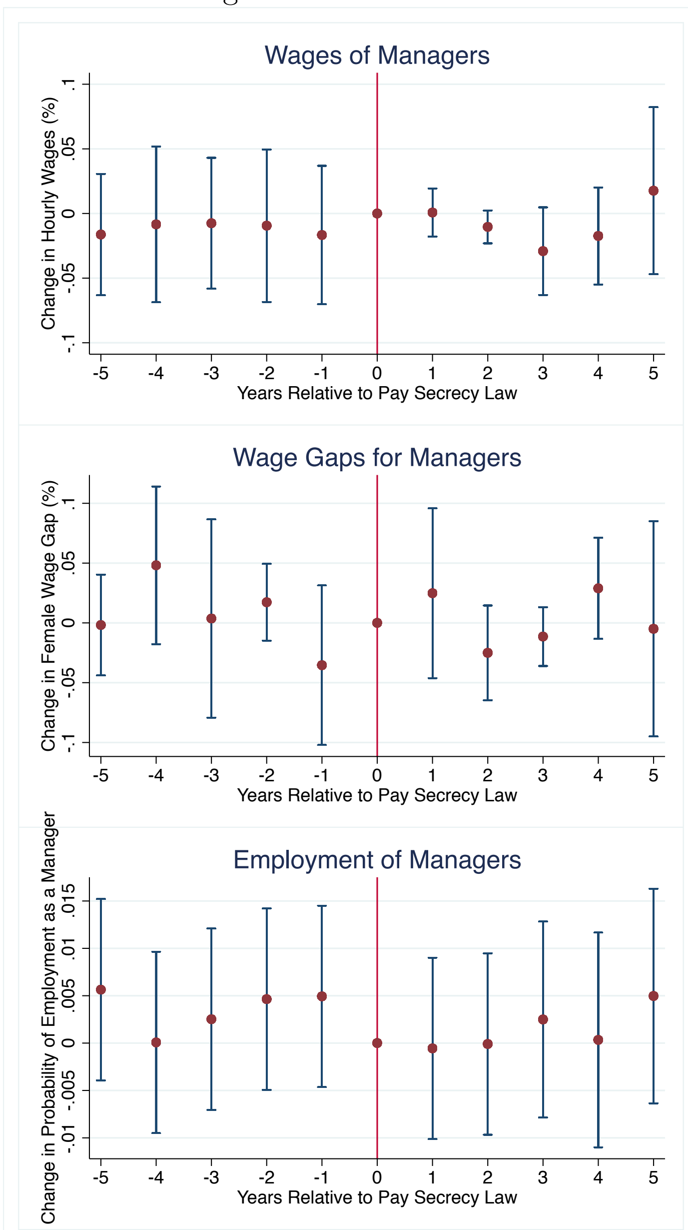
$$Y_{i,s,t} = \sum_{j=-5}^{-1} \beta_j TTL_{j,s,t} + \sum_{j=1}^{5} \beta_j TTL_{j,s,t} + \Omega X + \gamma_s + \gamma_t + \gamma_{s,t} + \varepsilon_{i,s,t} \tag{3}$$

where $TTL_{j,s,t}$ is an indicator variable that takes a value of 1 for state s in year t if there are j years until that state passes a pay secrecy ban (i.e., $TTL_{-1,s,t}$ is equal to 1 the year before a pay secrecy ban is passed). A vector of worker demographics $\mathbf{X}$ controls for individual characteristics that may affect labor market outcomes. The coefficients β_i report whether eventually treated states have statistically significant differences from other states in the years immediately before and after the passage of a pay secrecy ban. The terms γ_s, γ_t, and $\gamma_{s,t}$ represent state, year, and state-year fixed effects.

Figure 1 reports the coefficients from these event studies. Panel A shows that there is no differential trend in wages for the states that pass a law in the lead up to the passage of pay secrecy bans. Similarly, Panel B shows that passing a law is not associated with a trend in the wage gaps for women. Taken together, these event studies suggest that the DDD research design assumptions are satisfied subject to controls.

The DDD estimator additionally relies on the assumption that the composition of the treatment group does not vary with the treatment. Pay secrecy bans could affect the composition of who is a manager in two important ways. First, firms might have promoted workers to manager before the passage of state-level pay secrecy bans to circumvent NLRA requirements. After the passage of a pay secrecy ban, firms may no longer promote as many individuals to managers. We analyze this using an event study to identify if there is a differential trend in the share of individuals employed as managers before and after a law. Panel C of Figure 1 shows that there is no change in the employment of managers

Figure 1: Event Studies



Note: Data come from the 1977 through 2016 Current Population Survey. The horizontal axes are the years relative to the passage of a pay secrecy ban. The first graph is the event study for the average wages of managers. The second graph is the event study for the gender wage gap among managers. The third graph reports the event study for the probability that a worker is employed as a manager. In the first and second graphs, the sample is restricted to all individuals in the labor force working as managers. The third graph uses a sample of all individuals in the labor force.

around the passing of a law.

Second, the composition of who is a manager could be affected if firms or workers change their job search behavior after the passage of a law. Managers may learn more about the wages of their coworkers and seek different opportunities, obtain more education, or leave management altogether. In the appendix, we present evidence that the composition of managers is unchanged by testing whether the demographics of managers in treated states differ the year before and the year after a law. Using both a simple difference of means in the year preceding the passage of the law and the year after the passage (Table A3) and a regression framework incorporating all years of data (Table A2), we find only small changes in the demographics of managers before and after the law.

Finding no pre-trends in wages, wage gaps, or probability of employment as a manager in the lead up to the implementation of pay secrecy bans, we conclude that the passage of state laws is not endogenous to changes in the labor market outcomes featured in this analysis.

5 Results

Recall that proponents of pay secrecy bans argue that information provision to workers is likely to result in a reduction of wage inequality, without an increase in average wages. This is consistent with a scenario where the earnings of high-income managers fall while the wages of low-income managers increase or stay the same. Opponents of pay secrecy bans warn that firms will be faced with giving raises to their entire workforce, increasing overall wage expenditures. The actual effect of these policies is an empirical question that we seek to answer. In this section, we focus on the average treatment effect for managers.

We first investigate the effect of pay secrecy policies on labor supply by estimating the laws' effects on labor force participation and hours worked per week. In column 1 of Table

3, we estimate Equation 1 with the outcome variable being a dummy variable equal to one if the individual is in the labor force.[16] We find no significant effect of the pay secrecy bans on labor force participation. Female managers saw a decline in their labor force participation of 0.6 percentage points relative to male managers, but the total effect of pay secrecy bans on female managers' labor force participation is not significantly different from zero. In column 2, we report the effect of pay secrecy bans along the intensive margin of labor supply. We find no significant effect of pay secrecy bans on the hours worked per week by managers. Based on this evidence, it does not appear these policies impacted the decision of managers to work. This may be because 82% of managers are employed in full-time positions, so workers may lack the flexibility to adjust their hours in response to changes in pay.

Table 3: Effect of Pay Secrecy Bans on Labor Market Outcomes

	(1) LFP	(2) Hours worked	(3) Hourly wages	(4) Job tenure
Pay Secrecy Ban × Managers	0.001	0.879	0.035***	-0.087
	(0.004)	(0.935)	(0.010)	(0.321)
Pay Secrecy Ban × Managers × Female	-0.006***	-0.347	-0.005	0.252
	(0.002)	(1.074)	(0.008)	(0.164)
Observations	2,961,477	2,089,402	2,089,402	588,165
adj R^2	0.812	0.015	0.330	0.289

Data come from the 1977 through 2016 Current Population Survey (Ruggles et al. 2017). Labor force participation is a dummy variable that takes the value of one if the respondent is classified as being in the labor force when surveyed. Hours worked per week are the usual hours per week that an individual worked the past year. Log hourly wages are reported in constant 2012 dollars. Data on job tenure come from the Job Tenure Supplements to the CPS, which were collected in 1983, 1987, and every two years beginning in 1996. Job tenure is measured as the length in time respondents have been employed at their current job. The regressions contain all the controls reported in Equation 1, except where their inclusion would be inappropriate (e.g., controlling for selection in column 1). A list of demographic controls can be found in Table 2. For evidence of the effect inclusion of these controls has on the estimates, please see Tables A7 and A8 in the Appendix.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The main effects of the policy were predicted to be on the wages and job tenure of

[16] These results are similar to those found when we estimate our first stage equation of the selection correction. See Table A9 for coefficients of Equation 2.

managers. Column 3 reports the estimated effect of pay secrecy bans on log hourly wages.[17] We find the passage of a pay secrecy ban increased the average wages of managers by 3.5 percent relative to non-managers. Contrary to Kim (2015), we find no evidence these laws affected male and female managers differently. The coefficient on $PaySecrecyBan \times Manager \times Female$ is -0.005, suggesting that the wages of female managers fell a small amount relative to male managers after the passage of a pay secrecy ban, increasing the wage gap. This change is not significantly different from zero, leading us to conclude that pay secrecy bans did not reduce the wage gap for female managers.

Column 4 reports the results on job tenure, which firms predicted would decrease after the passage of a pay secrecy ban. We find the passage of pay secrecy bans had no significant effect on the job tenure of managers. The estimated effect of a pay secrecy ban was to decrease average job tenure by 0.087 years, approximately one month. We also observe an increase in the gap in job tenure between male and female managers because female managers increased their job tenure after the passage of a pay secrecy ban. But neither of these two coefficients were significantly different from zero.

Taken together, the evidence in Table 3 casts doubt on the ability of pay secrecy bans to achieve policymakers' stated goals. The evidence suggests that on average managers received a raise after wages become an open topic of conversation, but average wage gaps between male and female managers remained unchanged. On the other hand, the objection of companies that the laws would raise wages appear have been least partially empirically validated. We find pay secrecy bans increased the wages of all managers on average.

[17] The appendix contains tables that provide more detail about the baseline estimates. In Table A5, we report the effect of pay secrecy bans on all individuals, all managers, and all non-managers using a differences-in-differences setting for wages (Table A5) and job tenure (Table A6). We also show how the inclusion of controls affects the estimates for labor force participation (Table A7), hours worked per week (Table A7), wages (Table A8), and job tenure (Table A8). The appendix also contains a number of additional robustness checks. Most importantly, Table A4 tests how robust this result is to excluding the 2.6% of managers and the 2.7% of non-managers who have moved between states in the past year. We find the coefficients are qualitatively similar, but it has shrunk slightly from 3.5 percent to 3.3 percent. It remains significant at the 5% level.

However, we find no evidence that these laws increased labor mobility or job turnover of managers. These results suggest that the wage gains that managers bargained for were enough to keep managers from becoming dissatisfied with their current employer.

6 Robustness Checks

The results in Table 3 indicated significant increases in the wages of managers after the passage of pay secrecy bans. In Table 4, we investigate whether the effect of pay secrecy bans on our two primary outcomes of interest is robust to correcting for the small number of treated states we have in the sample. The literature has shown when the number of treated clusters is small that the standard errors can be biased (Bertrand, Duflo and Mullainathan 2004). To account for this, Conley and Taber (2011) showed how one could correct the standard errors when the number of treated clusters is small. Button (2018) extended this methodology from the original DD to cover the DDD model. In Kim (2015), there were only eight treated states, raising the concern that the standard errors are biased in this way. While our sample includes additional treated states, the results may remain sensitive to the small number of treated states.

When we estimate the results using the method from Button (2018), we find the point estimate has grown to 5.7 percent. The width of the 95% confidence interval has increased, and we are unable to rule out large effects of up to a 15 percent increase in the wages of managers. The results remain significantly different from zero at the 5% level. The laws' effect on job tenure is still not statistically significant, but again the confidence intervals have grown quite large. The results show that the small number of states merits a cautious interpretation of the results. In the case of wages of managers relative to non-managers, it does not appear that the small number of treated states is biasing our results enough for the conclusions of the analysis to be qualitatively different. But, properly correcting the standard errors for the small number of treated states results in much larger confidence intervals.

Table 4: Inference Using Conley-Taber Confidence Intervals

	(1) Wages	(2) Tenure
Pay Secrecy Ban × Manager	0.057	-0.444
95% Confidence Interval	[0.028, 0.152]	[-2.863, 1.862]
Observations	2,089,402	588,165

Data come from the 1977 through 2016 Current Population Survey. Outcome variable is log hourly wages in constant 2012 dollars. A list of demographic controls can be found in Table 2. They include controls for age, race, marital status, urban residence, education, occupation, and industry. The confidence intervals for the DDD were constructed using code from Button (2018) following the method devised by Conley and Taber (2011).

Next, we investigate how sensitive the results are to the selection corrections. We report pay secrecy bans' effect when using different approaches to correcting for selection in Table 5. Regardless of the instruments chosen, the effect of the law on wages when using the nonparametric selection correction is estimated as an increase of 3.5 or 3.7 percent. Under the Heckman correction procedure, laws are found to increase managers' wages between 2.7 and 2.9 percent.[18] These effects are statistically significant at the 1% level. Only when we use other household income as an instrument in our nonparametric method do we find a differential effect on female managers. Using this instrument, we find that wages of female managers fall by 2.1 percent relative to men.

Panel B of Table 5 reports that the laws' effect on job tenure for managers is not statistically different from zero. However, the results for women vary according to the method of correction applied. Under the Heckman selection correction for labor force participation, female managers stay in their jobs slightly longer in states with pay secrecy

[18]It is possible to estimate the Heckman correction without including an excluded variable. The standard errors are slightly larger when we estimate it in this manner, but the point estimate remains 0.027 and is significant at the 1% level.

bans. The coefficient is marginally significant regardless of the instrument chosen, and all three instruments predict a three-month increase in average job tenure for female managers.

Table 5: Effect of Using Different Selection Methods

	Young children		Other HH income		Both	
	NP	Heckman	NP	Heckman	NP	Heckman
	(1)	(2)	(3)	(4)	(5)	(6)
Pay Secrecy Ban × Manager	0.035***	0.029***	0.037***	0.027***	0.035***	0.027***
	(0.010)	(0.009)	(0.010)	(0.008)	(0.010)	(0.008)
Pay Secrecy Ban × Manager × Female	-0.005	0.013	-0.021**	0.015	-0.010	0.015
	(0.008)	(0.011)	(0.008)	(0.011)	(0.008)	(0.011)
Observations	2,089,402	2,089,402	2,079,510	2,079,510	2,079,510	2079510
adj R^2	0.330		0.330		0.330	
	Young children		Other HH income		Both	
	NP	Heckman	NP	Heckman	NP	Heckman
	(1)	(2)	(3)	(4)	(5)	(6)
Pay Secrecy Ban × Manager	-0.076	-0.136	-0.077	-0.135	-0.079	-0.135
	(0.209)	(0.241)	(0.213)	(0.242)	(0.212)	(0.242)
Pay Secrecy Ban × Manager × Female	0.131	0.307*	0.122	0.312*	0.134	0.312*
	(0.107)	(0.166)	(0.114)	(0.167)	(0.108)	(0.167)
Observations	444,224	444,224	442,315	442,315	442,315	442,315
adj R^2	0.221		0.221		0.221	

Data on wages come from the 1977 through 2016 Current Population Survey. The outcome variable is log hourly wages in constant 2012 dollars. Data come from the Job Tenure Supplements to the CPS, which were collected in 1983, 1987, and every two years beginning in 1996. Columns 1, 3, and 5 report the results using parametric methods, while columns 2, 4, and 6 report the results of using the Heckman correction. The nonparametric method controls for the probability of being in the labor force using a quadratic function. Columns 1 and 2 use children under the age of 6 as the instrument, columns 3 and 4 use other household income, and columns 5 and 6 use both. The 'other household income' instrument is the difference between the household's income and the individual's income from wages. Here we define household income as the total amount of income reported for each CPS respondent's family.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

7 Heterogeneous Effects

Based on our evidence that pay secrecy bans increased the wages of managers relative to non-managers, the question arises whether employers are increasing wages for all managers or if firms are reallocating wages among different groups. We focus on three different axes of heterogeneous treatment effects: employee income, firm size, and when a law was passed.

We compare the effect of pay secrecy bans for individuals with above and below median hourly wages in Table 6.[19] Median hourly wages are determined separately for managers and non-managers. This analysis reveals that the effect of these laws was not evenly applied across the income distribution.[20] Columns 1 and 2 report the effects for those earning less than the median wage. We find that these laws had significant effects on both wages and job tenure. Male managers experienced a small, but not significant, decline in their wages after the passage of a pay secrecy ban. After the passage of a pay secrecy ban, female managers below the median saw their wages increase 2.9 percent relative to their male colleagues below the median. The wage gap for female managers earning below the median wage is 4.6 percentage points.[21] Therefore, this increase in wages shrinks the wage gap between male and female managers by 60 percent (2.9%/4.6%).

The change in wages observed in column 1 is potentially related to the effect of pay secrecy bans on job tenure for managers below the median. In column 2, we see male managers earning less than the median experience a decline in job tenure of about 0.9 years after the passage of a law. On the other hand, female managers below the median experience an increase in their job tenure relative to men (though the net effect of pay

[19] The results are similar if we do not control for linear state time trends or selection. Similar to what is observed in Table A8, as we add more controls the coefficients fall slightly.

[20] In the appendix, Figure A1 provides a graphical illustration of the quantile effects at every decile. We see that the effect of pay secrecy bans is more positive as we move higher into the wage distribution. For female managers, these wage gains grow smaller the higher in the wage distribution one goes. Above the 75^{th} percentile of wages, the effect of pay secrecy bans for female managers is a net negative. Note that as one goes higher in the wage distribution, the number of women falls. This means that one should interpret the results in the upper tail with caution since the results may be driven by a small number of observations.

[21] See Table A10 for the gender wage gap across the various estimation samples.

secrecy bans on female managers is not significantly different from zero). It appears that these policies induce low-wage male managers to seek different positions, but starting a new job reduces their wages slightly. For women, it is the opposite effect. They are more likely to stay at their current jobs and see their wages increase.

Columns 3 and 4 of Table 6 report the results for those earning above the median wage. Among managers making more than the median hourly wage, we find that the laws had more mixed effects. Male managers experienced a 2.7 percent increase in their wages after the passage of these laws. This wage increase came at the expense of female managers who experienced a 3.1 percent decline in their wages relative to male managers. The wage gap among managers earning above the median was 12.7 percentage points. Therefore, the wage gap for female managers earning above the median wage grew by 24 percent (3.1%/12.7%).[22] These wage changes were not accompanied by any changes in average job tenure. Taken together, the results suggest that pay secrecy bans increased the wage gap for high-income women, despite being promoted by policymakers as a tool for reducing it.

The second dimension we investigate is firm size. Given that many individuals who are covered by the NLRA are unaware of their legal protections, there is a role for human resource management in ensuring that both employers and workers are aware of workers' rights. Larger firms are likely to have more expansive human resource departments. Additionally, as the firm size increases, so do the odds that there are multiple workers employed in the same position. This may increase the probability that individuals utilize pay secrecy bans to bargain for higher wages, compared to smaller firms where no coworker is directly comparable. Beginning in 1990, the Current Population Survey collected data on the number of persons who worked for the respondent's employer during the preceding calendar

[22] The results of this analysis would be biased if there was a composition change in who was above or below the median wage, especially if more men were earning above the median wage due to the law. We find no evidence of a change in the gender composition of individuals earning above the median. In the year before a law was passed 29% of managers earning above the median manager wage were female. The year after a pay secrecy ban was passed, 31% of managers earning above the median wage were female.

Table 6: Effect of Pay Secrecy Policies Above and Below the Median Wage

	(1)	(2)	(3)	(4)
	Below median income		Above median income	
	Wages	Job tenure	Wages	Job tenure
Pay Secrecy Ban × Manager	-0.010	-0.894**	0.027***	0.273
	(0.010)	(0.341)	(0.010)	(0.286)
Pay Secrecy Ban × Managers × Female	0.029***	0.525**	-0.031***	0.097
	(0.010)	(0.208)	(0.009)	(0.148)
Observations	1,003,675	187,279	1,085,727	256,575
adj R^2	0.216	0.177	0.249	0.241

Data on wages come from the 1977 through 2016 Current Population Survey. The outcome variable is log hourly wages in constant 2012 dollars. Data come from the Job Tenure Supplements to the CPS, which were collected in 1983, 1987, and every two years beginning in 1996. The outcome variable is the length in time respondents have been employed at their current job.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

year.[23] The variable aggregates over all of the employer's locations.[24]

When we look at the effect of pay secrecy bans in Table 7, we find that for managers at small firms (less than 100 employees), these policies increased the relative wages of managers by 8.8 percent. The wage gains for employees at firms with more than 100 employees were smaller than the gains at firms with fewer than 100 employees. In larger firms with more than 500 employees, the net effect of the passage of a pay secrecy ban was a decline in wages of between 0.05 and 2.9 percent. For most firm sizes, we find no change in the wage gap for female managers. Only for firms with between 500 and 999 employees do we observe a decline in the female wage gap in states that pass a pay secrecy ban.

This decline in wages may be related to changes in labor mobility for managers. When wages increased for managers at smaller firms, we find the job tenure of managers increased after the passage of a law by 0.415 years. At the larger firms where wages declined, we find the net effect of the passage of these laws was a reduction in the job tenure of managers

[23]Note that California and Michigan passed pay secrecy bans before the Current Population Survey began asking about firm size in 1990. Therefore, the estimated effect of pay secrecy bans by firm size is not identified using observations from those states.

[24]If the respondent is self-employed, then their response to the question on firm size reports how many employees they were employing in the previous calendar year.

Table 7: Effect by Firm Size

	(1) Log hourly wages	(2) Job tenure
Pay Secrecy Ban × Manager	0.088***	0.415**
	(0.015)	(0.189)
Pay Secrecy Ban × Manager × Firm size 100-499	-0.055***	0.113
	(0.013)	(0.239)
Pay Secrecy Ban × Manager × Firm size 500-999	-0.117**	-0.939**
	(0.019)	(0.454)
Pay Secrecy Ban × Manager × Firm size 1000+	-0.083***	-0.968***
	(0.022)	(0.199)
Pay Secrecy Ban × Manager × Female	-0.009	0.113
	(0.009)	(0.239)
Pay Secrecy Ban × Manager × Female × Firm size 100-499	-0.007	-0.273
	(0.012)	(0.307)
Pay Secrecy Ban × Manager × Female × Firm size 500-999	0.045**	0.630
	(0.018)	(0.527)
Pay Secrecy Ban × Manager × Female × Firm size 1000+	-0.008	0.130
	(0.012)	(0.606)
Observations	1,751,084	443,854
adj R^2	0.333	0.226

Data come from the 1990 through 2016 Current Population Survey, when the firm size variable became available. The definition of firm size has also changed over time, so categories were collapsed down to be mutually exclusive. Outcome variable is log hourly wages in constant 2012 dollars. A list of demographic controls can be found in Table 2. They include controls for age, race, marital status, urban residence, education, occupation, and industry. Standard errors are reported in parentheses, clustered at state level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

of between 0.524 and 0.553 years.[25] Women did not experience differential effects of pay secrecy bans on their job tenure.

The third dimension of heterogeneity we explore is the timing of adoption of a pay secrecy ban. Pay secrecy bans were passed in two phases. Michigan and California were early adopters in the early 1980s, but the remaining states that passed a pay secrecy ban did so post 2000. Therefore, we estimate the effect of pay secrecy bans focusing on these two distinct phases. Table 8 reports the effects of a pay secrecy ban on wages when we restrict the sample to cover the early adoption period in the 1980s and the later adoption period post 2000. We find the effect of pay secrecy bans in the early adoption period is not significant at the 5% level. The point estimate on the effect of the law on the wages of managers on average is very close to zero. We find female managers experienced a marginally significant increase in their wages relative to male managers. The laws passed in the 1980s did not have an effect on job tenure. The effect of pay secrecy bans passed in the 2000s is an increase in wages, but no effect on job tenure. These bans raised the wages of managers by 1.8 percent relative to non-managers. There was no differential effect on female managers. The effect sizes estimated here are smaller than the estimates we obtain when we use the full sample.

Next, we restrict our sample to only consider a limited number of years after the passage of a law to avoid over-weighting the results from California and Michigan (who have post-periods stretching for over 30 years). When we restrict the window to just five years after the passage of a law, we find the passage of a pay secrecy ban increased the wages of managers by 2.5 percent, and there was no differential effect by gender. Pay secrecy bans did not have a significant effect on the job tenure of managers. As we expand the window to ten years after the passage of a ban, the effect on wages grows even larger to 3.2 percent.

[25]Because managers at larger firms earn more than managers at smaller firms, these wage changes may decrease wage inequality across firms. The cross-firm increase in equality serves as a potential counter-balance to the cross-individual increase in inequality found in Table 6.

Table 8: Effect of Pay Secrecy Bans By Time Period

Panel A. Log hourly wages	(1) 1977-1989	(2) 2000-2016	(3) Age of law≤5	(4) Age of law≤10
Pay Secrecy Ban × Manager	-0.005	0.018**	0.025***	0.032***
	(0.014)	(0.008)	(0.009)	(0.009)
Pay Secrecy Ban × Manager × Female	0.030*	-0.015	-0.004	0.007
	(0.017)	(0.012)	(0.016)	(0.015)
N	401,313	1,174,596	1,879,822	1,921,073
r2	0.369	0.321	0.329	0.329

Panel B. Job tenure	(2) 2000-2016	(3) Age of law≤5	(4) Age of law≤10
Pay Secrecy Ban × Manager	-0.060	-0.200	-0.124
	(0.247)	(0.214)	(0.207)
Pay Secrecy Ban × Manager × Female	0.123	0.282*	0.118
	(0.110)	(0.151)	(0.144)
N	306,662	398,299	395,105
r2	0.219	0.223	0.223

Data on wages come from the 1977 through 2016 Current Population Survey. The outcome variable is log hourly wages in constant 2012 dollars. Data come from the Job Tenure Supplements to the CPS, which were collected in 1983, 1987, and every two years beginning in 1996. Therefore, we do not test the effect of the early adopters on job tenure. The outcome variable is the length in time respondents have been employed at their current job.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These results suggest that the effect of pay secrecy bans is a large increase in the early years, followed by a slower increase over time.

8 Conclusion

Policymakers promoted pay secrecy bans as a tool to increase wages, reduce wage inequality, and reduce wage gaps for women. Firms opposed the laws because they feared they would increase the wages of employees and increase employee turnover. The results of this paper indicate that, in most cases, policymakers' goals were not achieved and the fears of firms were realized. We find that pay secrecy bans increased the wages of managers by 3.5 percent on average. In contrast to the results from Kim (2015), we find limited evidence that these laws reduced the wage gap between men and women. On average, there is no evidence that the wages of female managers rose relative to male managers because they bargained for higher wages after the passage of a law.

When exploring the heterogeneous effects of pay secrecy bans, we draw a more nuanced conclusion. In general, pay secrecy bans appeared to help some managers, while hurting others. When comparing managers across firm sizes, we find that the wage gains were concentrated among managers working for firms with fewer than 500 employees. Managers at firms with more than 500 employees say declines in their wages. When we investigate the effect of pay secrecy bans for low paid and high paid managers, we find no wage gains for male managers below the median wage but significant wage increases above the median. Among female managers, the effects were the opposite. Female managers below the median saw their wages increase relative to male managers below the median, and female managers above the median saw their wages decline relative to male managers. These wage gains were more prominent in the states that most recently passed a pay secrecy ban. In the two earliest adopters in the 1980s, we find no evidence that pay secrecy bans increased the wages of managers.

Our results suggest that pay secrecy bans had no effect along the extensive or intensive margins of labor supply. When investigating the average effect on managers, we find no evidence that pay secrecy bans led to changes in labor force participation, hours worked per week, or job tenure. But again, these null effects on average mask interesting heterogeneity along a number of dimensions. When we investigate pay secrecy bans effect on heterogeneous sub-groups, we find that falling or stagnant wages are associated with reductions in average job tenure. Some managers appear to have left their previous positions, trading off the wage gains due to tenure for a new position. These results suggest that firms that were willing to increase wages did not see increases in job turnover, but those firms that did not increase wages saw a decline in the job tenure of managers.

The results of this paper highlight how the average effect of these laws can obscure the zero-sum nature of wage bargaining. In many of our analyses, we find evidence that the pay secrecy bans seemed to reinforce existing inequality. Among men, managers below the median experienced no change in their wages, while managers earning above the median experienced a 2.7 percent increase in their wages. Female managers earning more than the median wage experienced a decline in their wages relative to male managers. Female managers earning less than the median experienced a wage increase relative to male managers. But this is not as positive as it may first appear since their wage gap only declined because male wages fell. They still lost ground to high-income male managers.

It is an open question as to why these policies affect managers differently. In terms of income, high-income managers may have larger or more forthcoming professional networks and therefore greater access to information. Employers may also be more cognizant of the litigation risk that high-income managers pose since the damages awarded to successful plaintiffs are determined based on wages. In terms of gender, differences in bargaining may explain the lack of a decline in the gender wage gap. There is significant evidence from the experimental literature that women initiate negotiations less often (Dittrich et al. 2014, Leibbrandt and List 2015) and that when they do they may be punished more than men (Bowles, Babcock and Lai 2007). Even if there are no gender differences in how firms renegotiate wages with managers after the passage of a pay secrecy ban, a lower rate of female managers initiating negotiations would explain why we observe wage growth for male managers, but little evidence of raises for female managers. Female access to insider information about wages may also prove to be a key limitation to their ability to obtain the knowledge and confidence needed to initiate wage negotiations.

A key limitation of this paper has been our inability to explore the longitudinal impact of pay secrecy bans on wage growth due to the data limitations of the CPS. Following labor market outcomes for individual workers impacted by pay secrecy policies over a number of years is a potentially fruitful strand of further research. More detailed information about the content of an employee's pay and compensation package would also allow a more nuanced understanding of these negotiations. Currently, the CPS only allows us to investigate the effect of pay secrecy bans on wages. If managers are receiving increases in non-wage compensation, such as equity or vacation days, then we may be underestimating the effect of these policies on equality in the workplace.

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A Appendix Tables

Table A1: Probability of Being A Manager

	(1)	(2)	(3)
Pay Secrecy Ban	-0.004	0.002	-0.001
	(0.004)	(0.003)	(0.002)
State & Year FE	X	X	X
Demographic controls		X	X
Region time trends			X
Observations	2,978,427	2,978,427	2,978,427
adj R^2	0.003	0.103	0.103

Note: The dependent variable is a dummy variable equal to one if the individual's primary occupation is a manager.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Change in Demographics of Managers After Law

	Law
Female	0.006
	(0.032)
Black	0.003
	(0.002)
Other	0.011*
	(0.007)
HS graduate	-0.022
	(0.027)
Some college	-0.014
	(0.022)
College graduate	0.002
	(0.012)
Advanced degree	0.009*
	(0.005)
Central city resident	0.012
	(0.029)
Married	0.002
	(0.017)
Separated/Divorced	-0.001
	(0.012)
Age	-0.150
	(0.575)

Note: Data come from the 1977 through 2016 Current Population Survey. Each row represents a separate regression of the change in the pay secrecy ban on the demographics of managers. State fixed effects and region specific time trends are included. The data has been weighted using the ASEC supplement weights provided by Ruggles et al. (2017). Standard errors are reported in parentheses, clustered at state level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Manager Characteristics In the Year Before and the Year After Passing Law

	Year t-1		Year t+1		Difference	
	mean	sd	mean	sd	difference	t statistic
Wages	40.24	83.83	37.68	32.42	0.25	(0.58)
Full time	0.76	0.43	0.78	0.42	-0.00	(-0.77)
Female	0.39	0.49	0.39	0.49	0.00	(0.30)
Black	0.06	0.23	0.05	0.23	-0.00	(-0.07)
Other race	0.07	0.25	0.08	0.27	-0.00	(-1.56)
High school graduate	0.09	0.28	0.09	0.28	0.01**	(2.71)
Some college	0.23	0.42	0.22	0.42	-0.00	(-0.36)
Bachelor's degree	0.42	0.49	0.44	0.50	-0.00	(-1.15)
Master's degree or Doctorate	0.12	0.33	0.15	0.36	-0.01*	(-2.02)
Experience = Age - Years schooling - 5	23.29	10.24	23.75	10.55	-0.03	(-0.23)
Metro area	0.92	0.28	0.93	0.25	-0.01**	(-2.85)
Married	0.72	0.45	0.73	0.44	0.01*	(2.34)
Separated/Widowed/Divorced	0.14	0.35	0.13	0.34	-0.00	(-1.11)
Observations	2612		2417			

Note: Data come from the 1977 through 2016 Current Population Survey.

Table A4: Effect Controlling for Migration

	(1) Log hourly wages	(2) Job tenure
Pay Secrecy Ban × Manager	0.033**	-0.066
	(0.013)	(0.193)
Pay Secrecy Ban × Manager × Female	-0.014	0.154
	(0.010)	(0.107)
Observations	1,843,077	437,277
adj R^2	0.330	0.220

See Table A8 for a description of the data used. Here, we exclude all individuals who have migrated across state lines in the past year.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A5: DD Results: Wages

Panel A. All individuals	(1)	(2)	(3)	(4)
Pay Secrecy Ban	-0.067***	-0.027**	-0.032**	-0.030*
	(0.014)	(0.012)	(0.014)	(0.015)
Pay Secrecy Ban × Female	0.071**	0.032*	0.034*	0.031*
	(0.029)	(0.017)	(0.017)	(0.018)
Observations	2,089,402	2,089,402	2,089,402	2,089,402
adj R^2	0.074	0.296	0.294	0.298
State & Year Fixed Effects	X	X	X	X
Demographic Controls		X	X	X
Linear State Time Trend			X	X
Selection				X
Panel B. Managers	(1)	(2)	(3)	(4)
Pay Secrecy Ban	0.002	0.013	-0.005	-0.002
	(0.008)	(0.014)	(0.012)	(0.012)
Pay Secrecy Ban × Female	0.031**	0.014	0.016	0.010
	(0.015)	(0.011)	(0.011)	(0.011)
Observations	327,787	327,787	327,787	327,787
adj R^2	0.101	0.246	0.242	0.243
State & Year Fixed Effects	X	X	X	X
Demographic Controls		X	X	X
Linear State Time Trend			X	X
Selection				X
Panel C. Non-Managers	(1)	(2)	(3)	(4)
Pay Secrecy Ban	-0.076***	-0.037***	-0.037**	-0.034**
	(0.015)	(0.009)	(0.015)	(0.016)
Pay Secrecy Ban × Female	0.069**	0.033*	0.035*	0.028
	(0.029)	(0.018)	(0.018)	(0.018)
Observations	1,761,615	1,761,615	1,761,615	1,761,615
adj R^2	0.069	0.302	0.300	0.301
State & Year Fixed Effects	X	X	X	X
Demographic Controls		X	X	X
Linear State Time Trend			X	X
Selection				X

Note: Data come from the 1977 through 2016 Current Population Survey. Outcome variable is log hourly wages in constant 2012 dollars. A list of demographic controls can be found in Table 2. They include controls for age, race, marital status, urban residence, education, occupation, and industry.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A6: DD Results: Job Tenure

Panel A. All individuals	(1)	(2)	(3)	(4)
Pay Secrecy Ban	-0.285*	-0.302**	-0.262*	-0.253*
	(0.157)	(0.130)	(0.131)	(0.132)
Pay Secrecy Ban × Female	0.235	0.159	0.159	0.158
	(0.192)	(0.150)	(0.148)	(0.153)
Observations	444,224	444,224	444,224	444,224
adj R^2	0.016	0.215	0.215	0.216
State & Year Fixed Effects	X	X	X	X
Demographic Controls		X	X	X
Linear State Time Trend			X	X
Selection				X
Panel B. Managers	(1)	(2)	(3)	(4)
Pay Secrecy Ban	-0.029	-0.276	-0.336	-0.341
	(0.192)	(0.193)	(0.209)	(0.207)
Pay Secrecy Ban × Female	0.302***	0.263***	0.259***	0.277***
	(0.112)	(0.094)	(0.094)	(0.091)
Observations	75,390	75,390	75,390	75,390
adj R^2	0.015	0.211	0.211	0.211
State & Year Fixed Effects	X	X	X	X
Demographic Controls		X	X	X
Linear State Time Trend			X	X
Selection				X
Panel C. Non-Managers	(1)	(2)	(3)	(4)
Pay Secrecy Ban	-0.335*	-0.303**	-0.241	-0.244*
	(0.169)	(0.140)	(0.169)	(0.141)
Pay Secrecy Ban × Female	0.200	0.119	0.119	0.128
	(0.210)	(0.162)	(0.161)	(0.147)
Observations	368,834	368,834	368,834	368,834
adj R^2	0.017	0.222	0.222	0.228
State & Year Fixed Effects	X	X	X	X
Demographic Controls		X	X	X
Linear State Time Trend			X	X
Selection				X

Note: Data come from the Job Tenure Supplements to the CPS, which were collected in 1983, 1987, and every two years beginning in 1996. The outcome variable is the length in time respondents have been employed at their current job. A list of demographic controls can be found in Table 2. They include controls for age, race, marital status, urban residence, education, occupation, and industry.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Effect of Pay Secrecy Bans on Labor Supply

Panel A. Labor Force Participation	(1)	(2)	(3)
Pay Secrecy Ban × Manager	0.004	0.002	0.001
	(0.004)	(0.004)	(0.004)
Pay Secrecy Ban × Manager × Female	0.001	-0.006***	-0.006***
	(0.012)	(0.002)	(0.002)
State & Year Fixed Effects	X	X	X
Demographic Controls		X	X
Linear State Time Trend			X
Observations	2,961,477	2,961,477	2,961,477
adj R^2	0.092	0.812	0.812
Panel B. Hours worked per week	(1)	(2)	(3)
Pay Secrecy Ban × Manager	-0.478	0.994	0.879
	(1.078)	(0.746)	(0.935)
Pay Secrecy Ban × Manager × Female	-0.169	-0.348	-0.347
	(1.128)	(1.104)	(1.074)
Observations	2,328,618	2,328,618	2,328,618
adj R^2	0.006	0.017	0.015
State & Year Fixed Effects	X	X	X
Demographic Controls		X	X
Linear State Time Trend			X

Data come from the 1977 through 2016 Current Population Survey (Ruggles et al. 2017). Labor force participation is a dummy variable that takes the value of one if the respondent is classified as being in the labor force when surveyed. Hours worked per week are the usual hours per week that a individual worked the past year. The regressions contain all the controls reported in Equation 1, except where their inclusion would be inappropriate (e.g., controlling for selection in column 1). A list of demographic controls can be found in Table 2. Standard errors are reported in parentheses, clustered at state level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Effect of Pay Secrecy Bans on Wages and Tenure

Panel A. Log hourly wages	(1)	(2)	(3)	(4)
Pay Secrecy Ban × Managers	0.066***	0.044***	0.037***	0.035***
	(0.014)	(0.008)	(0.010)	(0.010)
Pay Secrecy Ban × Managers × Female	-0.026*	-0.013	-0.012	-0.005
	(0.015)	(0.008)	(0.008)	(0.008)
Observations	2,089,402	2,089,402	2,089,402	2,089,402
adj R^2	0.127	0.332	0.329	0.330
State & Year Fixed Effects	X	X	X	X
Demographic Controls		X	X	X
Linear State Time Trend			X	X
Selection				X
Panel B. Job tenure	(1)	(2)	(3)	(4)
Pay Secrecy Ban × Manager	0.318	0.006	-0.091	-0.087
	(0.525)	(0.321)	(0.324)	(0.321)
Pay Secrecy Ban × Managers × Female	0.313	0.276	0.260	0.252
	(0.267)	(0.169)	(0.162)	(0.164)
Observations	588,165	588,165	588,165	588,165
adj R^2	0.019	0.289	0.288	0.289
State & Year Fixed Effects	X	X	X	X
Demographic Controls		X	X	X
Linear State Time Trend			X	X
Selection				X

Data on wages come from the 1977 through 2016 Current Population Survey. The outcome variable is log hourly wages in constant 2012 dollars. Data on job tenure come from the Job Tenure Supplements to the CPS, which were collected in 1983, 1987, and every two years beginning in 1996. The outcome variable is the length in time respondents have been employed at their current job. We show the results from the basic triple difference in Column 1, which does not include control variables. Column 2 adds demographic controls, column 3 adds linear time trends at the state level, and column 4 controls for selection. A list of demographic controls can be found in Table 2. Standard errors are reported in parentheses, clustered at state level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A9: First Stage of Nonparametric Selection

	(1)	(2)	(3)
Children under 5	-0.001		-0.002**
	(0.001)		(0.001)
Number of children	0.006***		0.005***
	(0.000)		(0.000)
Female × Number of children	-0.013***		-0.012***
	(0.000)		(0.000)
Female × Children under 5	-0.032***		-0.032***
	(0.002)		(0.002)
Other HH income		-0.001***	-0.001***
		(0.000)	(0.000)
Female × Other HH income		-0.000	0.000
		(0.000)	(0.000)
Pay Secrecy Ban × Manager	0.001	0.001	0.001
	(0.004)	(0.004)	(0.004)
Pay Secrecy Ban × Manager × Female	-0.007**	-0.006**	-0.007**
	(0.002)	(0.002)	(0.002)
Female	-0.094***	-0.109***	-0.093***
	(0.003)	(0.003)	(0.003)
High school graduate	0.015***	0.017***	0.017***
	(0.002)	(0.002)	(0.002)
Some college	0.009***	0.013***	0.012***
	(0.002)	(0.002)	(0.002)
College	-0.003	0.003	0.001
	(0.002)	(0.002)	(0.002)
Advanced degree	0.000	0.009***	0.006*
	(0.002)	(0.002)	(0.002)
Black	0.014***	0.012***	0.013***
	(0.002)	(0.002)	(0.002)
Other	-0.006***	-0.006**	-0.006***
	(0.001)	(0.002)	(0.002)
Experience	0.004***	0.005***	0.005***
	(0.000)	(0.000)	(0.000)
Experience^2	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)
Metro area	-0.007***	-0.006***	-0.006***
	(0.001)	(0.001)	(0.001)
Central city	-0.001	-0.002*	-0.002*
	(0.001)	(0.001)	(0.001)
Married	0.008***	0.009***	0.012***
	(0.001)	(0.001)	(0.001)
Separated/Divorced/Widowed	0.013***	0.009***	0.011***
	(0.001)	(0.001)	(0.001)
Observations	2,961,477	2,946,958	2,946,958
adj R^2	0.813	0.813	0.813

Note: Data come from the 1977 through 2016 Current Population Survey. The outcome variable is a dummy variable for being in the labor force. Standard errors are reported in parentheses, clustered at state level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10: Wage Gaps

	(1) Below median	(2) Above median	(3) 2000-2016	(4) 1977-1989	(5) All years
Manager	0.165***	0.449***	0.168***	-0.171***	0.035
	(0.014)	(0.012)	(0.022)	(0.039)	(0.021)
Female	-0.014***	-0.151***	-0.203***	-0.150***	-0.179***
	(0.003)	(0.004)	(0.005)	(0.011)	(0.005)
Manager × Female	-0.059***	0.029***	-0.048***	-0.182***	-0.098***
	(0.008)	(0.004)	(0.008)	(0.013)	(0.007)
Wage gap for female managers	-0.073***	-0.122***	-0.252***	-0.331***	-0.277***
	(0.007)	(0.004)	(0.005)	(0.008)	(0.005)

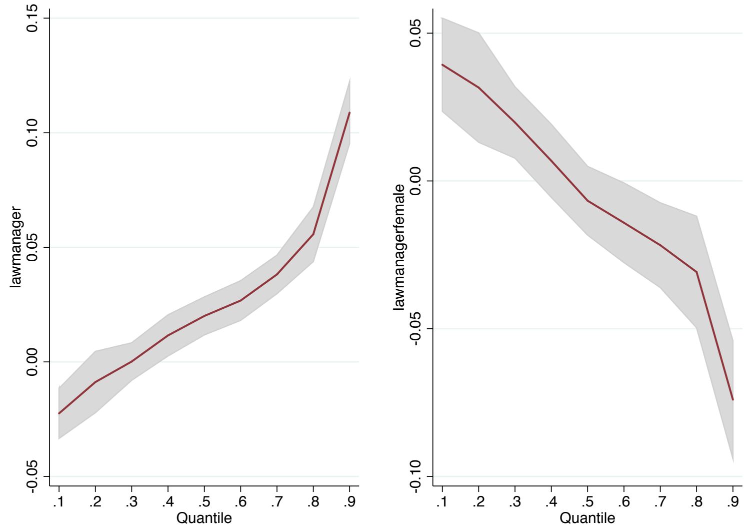
Data come from the 1977 through 2016 Current Population Survey (Ruggles et al. 2017). Log hourly wages are reported in constant 2012 dollars. A list of demographic controls can be found in Table 2.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Effect of Pay Secrecy Bans on Discrimination Complaints

	(1)	(2)
Panel A. Number of complaints	Poisson	Negatuve Binomial
Pay Secrecy Ban	-0.051	0.064
	(0.155)	(0.256)

Note: Data come from the 2009 through 2016 charges by state from the EEOC. Source: https://www.eeoc.gov/eeoc/statistics. The outcome variable is the number of discrimination complaints the EEOC received in a state for violation of pay equality. The regressions include state and year fixed effects. Standard errors are reported in parentheses and clustered at the state level. In panel A, the data is a count variable. The models are estimated using a Poisson model and a negative binomial.

Figure A1: Quantile Regression



Data come from the 1977 through 2016 Current Population Survey. The outcome variable is log hourly wages in constant 2012 dollars. A list of demographic controls can be found in Table 2. The shaded area represents the 95% confidence intervals.

B Replication of Kim (2015)

In this section, we recreate the analysis conducted by Kim (2015). The data used in Kim's original data set includes all workers from age 25 to age 65 interviewed as part of the CPS between 1977 and 2012. In the original analysis, the laws' effect on wages is estimated separately for men and women. Control variables account for workers' characteristics including age and experience as well as state- and year-fixed effects. Kim (2015) also conducts a more complex difference in differences analysis that includes state- and year-fixed effects by gender. The gender-specific model is estimated on the entire sample of year-round full-time workers, as well as on sub-samples chosen according to the educational attainment of workers in the CPS.

Table B1 reports the results of our replication exercise. In order to keep the analysis as simple and direct as possible, we replicate the baseline results of Kim (2015), where the author shows that there is an effect of pay secrecy bans on the wages of women working full-time but not men. To construct the analysis sample, we include all workers age 25-65 surveyed as part of the CPS between the years 1977 and 2016. Workers with reported wages between \$2 and \$15,000 are excluded as outliers. Column 1 reports the simple DD including both a treatment indicator (for men) and a treatment indicator interacted with a gender indicator variable (for women). We find that our point estimates are similar to those found in Kim (2015), but the standard errors are very different which leads to stark differences in the significance of the results. Where Kim (2015) finds that the pay secrecy bans had a negative but not significant effect on men who worked full-time, our results are significant at the 5% level. (Kim 2015) found that pay secrecy bans increased the wages of women working full-time by 4% and that this effect was significant at the 5% level. Our estimated increase in female wages is 3% and only significant at the 10% level.

In the remaining columns of Table B1, we show how the results change when we add in additional controls. The addition of time-trends and selection controls result in a more cautious interpretation of the effect of pay secrecy bans than featured in Kim (2015). The key takeaway is that the significance of the results is very sensitive to the inclusion of controls for selection and time-trends, but the point estimates themselves change very little. Where Kim (2015) used the result that pay secrecy bans had no effect on men working full-time to motivate the use of a triple-difference estimation methodology with women as the treatment group, we would argue that in the absence of theoretical grounds to assume that men would be unaffected by pay secrecy bans, the control group should be defined based on occupation.

When we replicate additional results from Table 4 in Kim (2015), the pattern of results remains the same. We note significant differences in the point estimates and significance of the estimates. Always our estimates closer to zero than the original and are not significant at the 5% level. These estimates are available upon request from the authors. We have tried to identify the causes of the differences in our estimates by varying the hours threshold for full-time work, by allowing the laws to go into effect in year t rather than year t+1, varying the controls used, and by clustering the results at different levels. None of these changes have moved our estimates closer to the originals. Attempts to obtain the original code used to construct the data set from Kim (2015) were unsuccessful. The author has not saved the data and do files.

Our second concern with the results from Kim (2015) is that the author does not control for selection. In the regression models, there are controls for children under the age of five and total number of children, but the author does not use these to address selection into the labor force only to address differences in wages. Given that the author estimates results focusing on individuals working full-time, selection into full-time work is potentially a large problem that is going unmentioned. In Table B2, we show that pay secrecy policies lead to large increases in the percentage of women working full-time. After the passage of a pay secrecy law, approximately 2 percent more women were employed full-time. This is true across all groups, regardless of education.

Table B1: Kim Replication

	(1)	(2)	(3)	(4)	(5)
Pay Secrecy Ban	-0.025***	-0.024	-0.025***	-0.024	-0.031**
	(0.009)	(0.014)	(0.009)	(0.015)	(0.015)
Pay Secrecy Ban × Female	0.030*	0.033*	0.030	0.033*	0.031*
	(0.017)	(0.017)	(0.018)	(0.017)	(0.016)
Observations	1,908,886	1,908,886	1,908,886	1,908,886	1,908,886
adj R^2	0.347	0.344	0.347	0.344	0.341
Time-trends		X		X	X
Selection			X	X	X
Weighted					X

Note: This table replicates the results of model 2 in Table 4 from Kim (2015), titled "Regression Results: Difference-in-Difference and Difference-in-Difference-in-Difference". It is a DD analysis comparing full-time workers before and after the passage of a pay secrecy ban.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B2: Change in Full-Time Work by Educational Subsamples in Kim

	(1) All	(2) College	(3) Non-college
Pay Secrecy Ban	-0.009	-0.008	-0.008
	(0.013)	(0.006)	(0.007)
Pay Secrecy Ban × Female	0.022***	0.019***	0.023***
	(0.006)	(0.007)	(0.006)
Observations	3,427,415	909,426	2,517,989
adj R^2	0.322	0.276	0.323

Note: The outcome variable is a dummy variable for being employed full-time and for a full year.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$