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ORIGINAL ARTICLE

Do pay secrecy policies prevent voluntary turnover?

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Abstract

Pay secrecy (PS) policies, commonly found in the US private sector, restrict pay communication among employees. Previous research suggests that these policies help prevent pay dissatisfaction and attrition among low earners by limiting salary comparisons. However, by exploiting the staggered adoption of PS bans across US states as a negative shock to PS, I find no causal relationship between PS and voluntary turnover. This suggests that workers' reluctance to ask others about their pay renders PS policies irrelevant for retention purposes. This, in turn, highlights the need for organizations to consider alternative approaches to enhance employee retention and commitment.

JEL CLASSIFICATION

J08, J28, J53, J63

INTRODUCTION

Pay secrecy (PS) policies are workplace rules that restrict pay communication among employees. These policies can be communicated in various forms, such as oral and informal communication or formal written statements in employment manuals (Bierman & Gely, 2004; Gely & Bierman, 2003). A formally written PS rule may state that wages are a "confidential matter between the employee and his earnings supervisor", and that any discussions regarding wages among employees "will result in dismissal and/or disciplinary action at the supervisor's discretion" (Gely & Bierman, 2003).[1] Despite the legal challenges described below, many employers continue to implement PS policies. According to a 2017/2018 nationally representative survey by the Institute for Women's Policy Research (IWPR),[2] approximately 60% of US employees in the for-profit private-sector work under some form of PS policy: 44.1% report being subject to informal PS rules, while 15.8% report

[1]According to the National Labor Relations Board, Fredericksburg Glass and Mirror, Inc. included the quoted PS rule in its employment manual during 1992–1995 (National Labor Relations Board, 1997).

[2]The IWPR defines itself as "a nonpartisan, nonprofit organization that engages in research and dissemination to shape public policy and improve the lives and opportunities of women from diverse backgrounds".

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being subject to formal PS arrangements. In contrast, these percentages are only 8.5% and 4% for public sector employees (Sun et al., 2021). The extent to which employers enforce these policies can vary, with some actively monitoring and disciplining employees who violate PS rules (Gely & Bierman, 2003).

PS is commonly believed to help employers during wage negotiations. The underlying rationale is that PS, by restricting employees' access to information about their co-workerss' salaries, enables employers to off lower wages (Burn & Kettler, 2019; Janićijević, 2016; Kim, 2015; Rosenfeld, 2021; Stiglitz, 2021). However, a recent paper by Cullen and Pakzad-Hurson (2023) challenges this notion, arguing and providing evidence consistent with PS actually deteriorating the employer's bargaining position.[3]

An alternative potential explanation for the existence of PS policies is that they help prevent voluntary employee turnover (e.g., Alterman et al., 2021; Colella et al., 2007; O'Neill, 2010).[4] Employees who learn about their peers' higher wages may become dissatisfied with their own salary and more likely to quit the firm (Akerlof & Yellen, 1990; Bellé, 2015; Breza et al., 2018; Card et al., 2012; Cohn et al., 2014; Colella et al., 2007; Cullen & Perez-Truglia, 2022; Dube et al., 2019; Fisman, 2010; Frey et al., 2013; Gely & Bierman, 2003; Lawler, 1966; Nosenzo, 2013; Perez-Truglia, 2020). Employers may, therefore, choose to implement PS policies to prevent low earners from comparing their wages to those of high earners, which could lead to pay dissatisfaction and potential turnover.

Against this backdrop, in this paper, I formally test whether there is a negative causal relationship between PS policies and voluntary turnover. My goal is to assess the effectiveness of these policies in retaining employees and explore the potential consequences of initiatives aimed at eliminating them.[5]

Following Kim (2015), Burn and Kettler (2019) and Cullen and Pakzad-Hurson (2023), my empirical analysis relies on the staggered implementation of the state PS bans as a negative shock to PS. These laws, enacted by several states since the 1980s, punish employers who retaliate against employees who discuss, disclose, or inquire about pay (Cherradi, 2016; Kim, 2015). By combining these laws with recently released state-level data on voluntary turnover from the Bureau of Labor Statistics Job Openings and Labor Turnover Survey (JOLTS), I find no causal relationship between PS and voluntary turnover. This null result continues to hold even after restricting the sample to states with a high fraction of college-educated or non-unionized workers, who seem to be more exposed to PS arrangements (Kim, 2015; Sun et al., 2021). Further analyses using individual-level data from the National Survey of College Graduates reveal no relationship between PS and pay satisfaction either. This provides additional evidence for the lack of a causal relationship between PS and voluntary turnover, as any association between these variables would rely on an underlying connection between PS and pay satisfaction.

According to Oswalt et al. (2023), the PS bans seem to cause employers to merely shift from formal to informal PS rules, leaving employees' overall exposure to PS largely unchanged. This view, along with Oswalt et al.'s (2023) supporting evidence, raises concerns about the validity of my empirical exercise, as it implies that the PS bans might not represent a true negative shock to PS. I address this issue by discussing the limitations of the evidence presented by Oswalt

[3]Cullen and Pakzad-Hurson (2023) posit that, in the absence of PS, employers can credibly argue that raising one employee's salary would require raising everyone else's as well, which diminishes employees' leverage in salary negotiations and strengthens employers' bargaining power.

[4]Voluntary turnover is particularly costly for employers due to disrupted teamwork, loss of organizational memory, and the expenses of hiring and training new employees (e.g., Hom et al., 2017; Li et al., 2022; O'Connell & Kung, 2007; Tziner & Birati, 1996).

[5]There has been a growing movement to eliminate PS policies in the United States, which includes President Obama's 2014 Executive Order 13665 prohibiting federal contractors from maintaining such policies, as well as the state PS bans described below. These initiatives reflect a broader shift toward greater pay transparency in the workplace.

et al. (2023) (details in Section 4.4.1) and by presenting both qualitative and quantitative arguments against the shifting explanation proposed by Oswalt et al. (2023). Qualitatively, when PS rules are banned in a state, employers adopting informal rules to replace formal ones might be seen as circumventing the law, potentially risking legal penalties or regulatory action. This suggests that in states with PS bans, the shift toward informal PS rules might either not occur, or if it does, these rules might be weaker, implying that the PS bans still constitute a negative shock to PS.

Quantitatively, I uncover a novel finding: firms provide more disaggregated financial information in their annual reports following a PS ban.[6] This finding is particularly significant because increased transparency in financial reporting likely conflicts with the maintenance of informal PS arrangements. If firms were substituting formal PS with informal practices, we would expect them to avoid enhancing financial disclosure, as greater financial transparency could undermine the secrecy they wish to maintain. Therefore, the observed increase in financial information disaggregation supports the idea that the PS bans reduce reliance on both formal and informal PS arrangements, reinforcing the notion that these bans represent a genuine negative shock to PS.

Why is there a null relationship between PS and voluntary turnover? Survey evidence suggests that a non-trivial fraction of workers might be willing to tell their salaries to their colleagues (Zippia, 2023). However, workers seem to have a strong aversion to *asking* others about their salaries (e.g., Bierman & Gely, 2004; Cullen & Perez-Truglia, 2023; Edwards, 2005; Gafner, 2024; Trachtman, 1999).[7] As asking for salary information is likely the first requirement for pay information to flow (it is difficult to imagine a worker revealing their salary to colleagues without being asked to do so first), I propose that my results could be mainly explained by workers' reluctance to ask others about their pay. By preventing the first step necessary for salary communication from happening, this reluctance-to-ask among workers might render PS policies irrelevant for retention purposes. This, in turn, suggests that organizations need to explore alternative approaches to enhance employee retention and commitment.

This paper contributes to several strands of literature. First, it contributes to the literature on PS policies and, more broadly, pay transparency. Perhaps unsurprisingly, the literature on PS policies has so far focused mainly on the effect of these policies on wages, finding mixed results.[8] To my knowledge, the only prior paper that examines the relationship between PS and voluntary turnover is that of Alterman et al. (2021). These authors find that the relationship between these two variables is contingent upon perceptions of distributive justice. However, their study may be subject to external validity concerns, as their sample includes only part-time MBA students in a Chinese university and firms registered in Dongguan City's (Guangdong Province, China) Industrial and Commercial Bureau. The Alterman et al. (2021) paper may also be subject to endogeneity issues related to firms' voluntary adoption of PS policies. My paper overcomes such concerns by using JOLTS data and the state PS bans as a natural experiment, allowing for a more robust investigation of the relationship between PS policies and voluntary turnover.[9]

This paper also contributes to the literature on the determinants of voluntary turnover. Economic factors, such as salary and benefits; psychological factors like job satisfaction; sociological aspects such as organizational culture; and work-related factors, including career

[6]I use the Chen et al.'s (2015) disclosure quality index to measure the level of financial information disaggregation in firms' annual reports. I describe this index in Section 4.4.3.

[7]Asking others about their salaries is often seen as rude and intrusive (Bierman & Gely, 2004; Cullen & Perez-Truglia, 2023).

[8]Cullen and Pakzad-Hurson (2023) find a positive relationship between PS and wages, while Kim (2015) and Burn and Kettler (2019) find the opposite.

[9]The provision of qualitative and quantitative arguments supporting the use of the PS bans as a negative shock to PS is another contribution of this paper to the literature on pay transparency.

development opportunities, have all been previously associated with voluntary turnover (e.g., Muchinsky & Morrow, 1980; Mueller & Price, 1990; Nelissen et al., 2017). Recent research has started to demonstrate causal links between some of these factors and voluntary turnover (Card et al., 2012; Cullen & Perez-Truglia, 2022; Dube et al., 2019). My paper highlights the need to continue making progress in this direction, as it illustrates that some variables that have been previously associated with voluntary turnover might have a less significant effect than is commonly assumed.

Finally, by documenting a positive effect of the PS bans on firms' financial information disaggregation, this paper also contributes to a growing literature on the intersection of accounting and labor economics (e.g., Barrios, 2022; Cascino et al., 2021; Gutierrez et al., 2020). Future research could explore whether other types of pay transparency regulations also lead to improvements in financial transparency, as well as the effect of the PS bans on other accounting outcomes.

The remainder of this paper is organized as follows. In Section 2, I provide institutional background on the passage of the state PS bans. In Section 3, I describe the data and the empirical strategy, and in Section 4 I present the main results. Section 5 concludes.

INSTITUTIONAL BACKGROUND ON PAY SECRECY BANS

The federal National Labor Relations Act (NLRA), enacted in 1935, grants private-sector employees the right to engage in "concerted activities for the purpose of collective bargaining or other mutual aid or protection". The NLRA is enforced by the National Labor Relations Board, which has consistently ruled that discussing wages is a form of "protected concerted activity", meaning that PS policies are illegal (Bierman & Gely, 2004; Gely & Bierman, 2003). However, two loopholes in the law have facilitated non-compliance. First, employers can still implement PS policies if they can provide "legitimate and substantial business justifications." Second, managers and supervisors, broadly defined as workers with the power to hire or fire, are not protected by the NLRA (Buchwald, 2021; Malmon, 2022; National Women's Law Center, 2019; Sun et al., 2021). The small penalties to employers for retaliating against an employee who inquires about pay under the NLRA, such as simply rehiring her if the employer fired her, may encourage further non-compliance (Rosenfeld, 2021).

In light of this, specific state laws have been enacted since the 1980s to prevent PS (Buchwald, 2021; Malmon, 2022; National Women's Law Center, 2019; Sun et al., 2021). In 1982, Michigan enacted a law that punishes employers who retaliate against workers who discuss, disclose or inquire about pay (Cherradi, 2016; Kim, 2015). Michigan's "PS ban" became effective in March 1983 (Kim, 2015). Since then, PS bans have become effective in several states (Oswalt et al., 2023). My sample period, which goes from January 2001 to December 2015, includes the following: Illinois (January 2004), Vermont (July 2005), Maine (September 2009), New Jersey (August 2013), Minnesota (July 2014), New Hampshire (January 2015), and Connecticut (July 2015).[10] The state PS bans are similar in that they all protect workers' rights to disclose or discuss wages with co-workers, but they can vary in the potential penalties for breaking the law. For example, while some PS bans allow only for backpay damages, others also allow for the possibility of punitive damages (Oswalt et al., 2023).

Available evidence suggests that the state PS bans have been effective in removing formal PS rules. The Institute for Women's Policy Research (IWPR) surveyed nationally

[10]Colorado passed a PS ban in 2008 that applies only to employees covered by the NLRA, excluding managers and supervisors (Kim, 2015). Louisiana passed a PS ban in 2013, but only for state government workers (Kim, 2015). Massachusetts passed a PS ban in 2016, but it was designed to phase in slowly from 2016 through 2018 (Burn & Kettler, 2019). I do not consider these laws as treatments in my empirical analysis.

representative samples of workers about PS policies in 2010 and 2017/2018 (Hegewisch et al., 2011; Sun et al., 2021). Using data from these two surveys, Cullen & Pakzad-Hurson, 2023 estimate that, in states that enacted PS bans over this time span, the share of private-sector workers subject to formal PS policies declined by 70%, from 33.2% to 10.1%. For states that did not enact PS bans between 2010 and 2018, Cullen & Pakzad-Hurson, 2023 estimate that the percentage of private-sector employees subject to formal PS policies declined by only 23%, from 24.3% to 18.7%, indicating large and statistically significant differences between states with and without PS bans by 2018, as well as significant differences in trajectories (Cullen & Pakzad-Hurson, 2023). Against this backdrop, in my empirical analysis, I rely on the staggered implementation of the PS bans across US states as a negative shock to PS.

EMPIRICAL STRATEGY AND DATA

Data

I use state-level data from the Bureau of Labor Statistics (BLS) Job Openings and Labor Turnover Survey (JOLTS). Each month, the BLS surveys more than 20,000 business establishments to produce estimates of the number and rate of US job openings, hires, and separations. Job separations are subdivided into three categories: quits, or voluntary resignations; layoff and discharges; and other separations, which include retirements and deaths. The BLS began producing state-level JOLTS estimates in 2019 (Skopovi et al., 2021), covering the period from December 2000 onwards.[11]

Additionally, I gather individual-level data on pay satisfaction from the 2003, 2010, 2013, and 2015 waves of the National Survey of College Graduates (NSCG). Sponsored by the National Science Foundation and conducted by the Census Bureau, the NSCG is a repeated cross-sectional survey. It focuses on individuals with education and/or employment in science or engineering, sampling those who live in the United States, has at least a bachelor's degree, and are under 76 years of age. Of particular importance to my study, the NSCG includes a module on job satisfaction, which I use to extract data on pay satisfaction.

I also gather information on state-level macroeconomic and demographic characteristics. I obtain annual per capita personal income data from the Bureau of Economic Analysis and monthly unemployment data from the BLS. I obtain data on private-sector union coverage from the Union Membership and Coverage Database maintained by Barry Hirsch and David Macpherson (www.unionstats.com). Population and educational attainment data come from the Census Bureau.

Finally, I extract financial information for US public firms from Compustat and merge this data with the Chen et al.'s (2015) disclosure quality index.[12]

The sample period begins in 2001, 3 years prior to the effective year of Illinois' PS ban. It ended in 2015 to exclude any potential effects of the Executive Order 13665, which extends the protections of the PS bans to employees of federal contractors and came into effect in 2016. This exclusion follows Cullen and Pakzad-Hurson (2023).

[11]Recent papers that also use JOLTS state-level data include Domash and Summers (2022) and Forsythe (2022).

[12]Compustat is a comprehensive database that provides financial and market data on publicly traded companies, including balance sheet and income statement items. Following the literature, I exclude financial firms (SIC 6000–6999), utilities (SIC 4900–4999), and firm-year observations with zero employees or with negative or missing sales before merging the Compustat database with the Chen et al.'s (2015) disclosure quality index.

Identification strategy

Given the staggered implementation of the PS bans, I use a stacked difference-in-differences (DiD) design to estimate the causal impact of PS on voluntary turnover (e.g., Cengiz et al., 2019; Gormley & Matsa, 2016). For each treated state, a construct a "treatment" cohort. Each treatment cohort consists of the respective treated state and several control states. The states within a cohort are followed from 36 months before to 36 months after the implementation of a PS ban by the cohort's treated state. In other words, for each cohort, I consider an event window of (−36,36) months around the implementation of the treated state's PS ban. Within each cohort, the control group consists of states in the same region as the treated state that have never adopted a PS ban or will adopt one after the event window. This restriction helps ensure a good match between treated and control states, as geographically close states are likely to have similar social and economic conditions.[13] Furthermore, by preventing already-treated states from being used as controls, I avoid the biases arising from the "bad comparisons" problem inherent in traditional two-way fixed DiD regressions (Baker et al., 2022).

After creating a treatment cohort for each treated state, I stack all the treatment cohorts together and estimate the impact of the PS bans on a voluntary turnover by running the following regression on the JOLTS dataset:

$$Ln\left(quits_{csmy}\right) = \alpha_0 + \sum_{k=-36}^{36} \beta_k PSBan_{s,m+k} + \alpha_1 PPIncome_{sy} + \alpha_2 UNEM_{smy} + \alpha_3 LF_{smy} + \theta_{st} + \delta_m + \delta_y + \varepsilon_{csmy} \quad (1)$$

where $Ln\left(quits_{csmy}\right)$ denotes the natural logarithm of the number of quits in state s, cohort c, month m and year y. $PSBan_{s,m+k}$ is a dummy that takes value 1 if a PS ban became effective in state s k months ago ($k = -36, -35 \ldots, 35, 36$). The month at which the PS ban became effective ($k = 0$) serves as the reference month. My main identifying assumption is that treated and control states would have exhibited parallel trends in voluntary turnover in the absence of treatment, which is plausible if the two groups of states follow similar trends in voluntary turnover in the period prior to the implementation of a PS ban, that is, if the coefficients on the $PSBan_{s,m+k}$ dummies with $-36 \leq k \leq -1$ are insignificant. Positive coefficients on the post-ban dummies would be consistent with PS policies decreasing voluntary turnover.

$PPIncome_{sy}$ is the natural logarithm of the real per capita personal income in state s and year y. $UNEM_{smy}$ represents the state-specific unemployment rate in month m of year y, while LF_{smy} denotes the labor force participation rate for the same state and time period. θ_{st} represents state-by-cohort fixed effects, which account for the multiple appearances of observations from the never-treated control states across cohorts (Baker et al., 2022). δ_m and δ_y represent month and year-fixed effects. When estimating Equation (1), observations are weighted by the state's population and standard errors are clustered at the state level to account for potential serial correlation within a state over time (Bertrand et al., 2004).

Description of treatment cohorts

Table 1 describes the treatment cohorts used in the empirical analysis, reflecting two previously mentioned points: each treatment cohort is associated with a single treated state, and most treated states are located in the Northeast region. It also shows that the cohorts in the

[13]PS bans have been adopted mainly by Northeast states. D.C., Maryland, and Delaware are considered to be part of the Northeast region to increase the number of control states.

TABLE 1 Description of treatment cohorts.

	Cohort 1	Cohort 2	Cohort 3	Cohort 4	Cohort 5	Cohort 6	Cohort 7
Name of treated state	Illinois	Vermont	Maine	New Jersey	Minnesota	New Hampshire	Connecticut
Region of treated state	Midwest	Northeast	Northeast	Northeast	Midwest	Northeast	Northeast
Year of treatment	2004	2005	2009	2013	2014	2015	2015
Number of control states	10	11	10	6	9	6	6
Observations	803	876	803	455	540	336	294

Note: This table describes the treatment cohorts used in the empirical analysis.

2010s have a smaller number of control states, and consequently, a smaller number of observations than those in the 2000s. This is because, as time has passed, PS bans have become more common across states, which implies that the set of valid controls has decreased over time.

RESULTS

Figure 1 shows the results for Equation (1). The *y* axis plots the estimated coefficients on each PS ban dummy. The *x* axis shows the time relative to the implementation of the PS ban for −36 to +36 months around its implementation. The vertical lines correspond to the 95% confidence intervals of the coefficient estimates. The coefficients are interpreted with respect to the omitted category, which is the month of the implementation of the ban. The coefficients on the pre-ban dummies are insignificant, supporting the parallel trends assumption. However, the coefficients on the post-ban dummies are also insignificant, indicating that the PS bans did not affect voluntary turnover. Overall, the results in Figure 1 indicate that there is no causal relationship between PS and voluntary turnover.[14]

Robustness checks

In this section, I check the robustness of my results by exploring the relationship between PS policies and workers' pay satisfaction. The rationale for this exercise is that if there is no relationship between PS and voluntary turnover, then there should be no relationship between PS and pay satisfaction either.

For this analysis, I use the NSCG data described above. While the NSCG provides individual-level data on pay satisfaction and a variety of variables that might affect pay satisfaction, it does not report the respondent's state. Instead, it reports the respondent's region. This precludes the possibility of implementing a traditional state-level DiD design, as there might be both treated and untreated states in a given region at the same time.

Hence, in order to explore the relationship between PS and pay satisfaction using the NSCG data, I create a new variable, $PSBansRegion_{rt}$, which measures the fraction of region *r*'s population exposed to PS bans at year *t*. I then estimate the following model:

[14]With regard to the control variables, only the state's unemployment rate is significant, with a negative coefficient. This is consistent with workers being more reluctant to seek new employment opportunities when labor markets are tight (Donoghue & Castle, 2007).

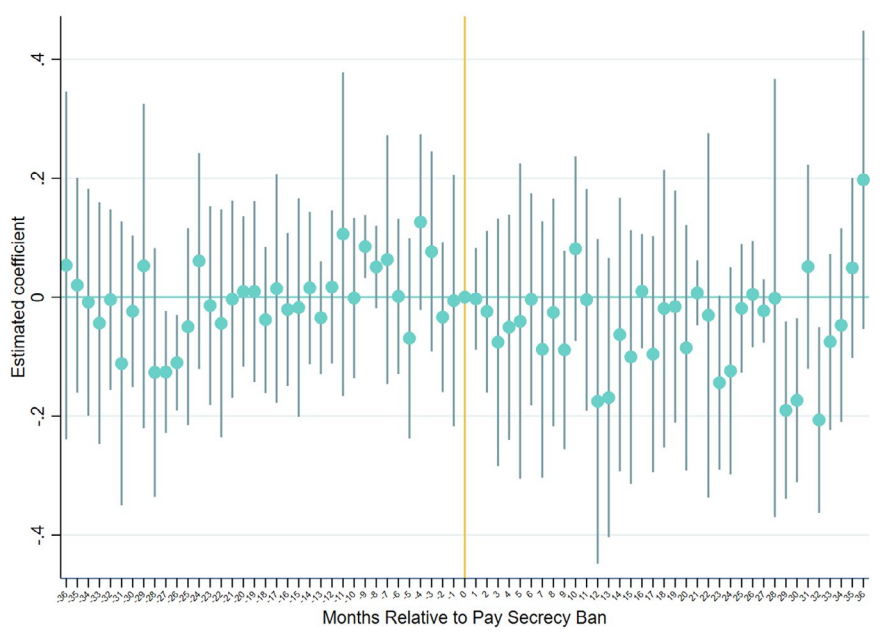

FIGURE 1 Pay secrecy bans and voluntary turnover. This figure shows the effect of the state pay secrecy (PS) bans on voluntary turnover. The *y* axis shows the coefficient estimates resulting from regressing the natural logarithm of the number of quits in the state on state-by-cohort fixed effects (FE), month FE, year FE, the state's unemployment rate, the state's labor force participation rate, the natural logarithm of the state's real per capita personal income, and dummy variables indicating the month relative to the PS ban implementation. The dummies cover up to 36 months before and 36 months after the ban's implementation. The *x* axis represents the time relative to the PS ban implementation. The vertical lines indicate the 95% confidence intervals of the coefficient estimates, calculated from standard errors clustered by state. Observations are weighted by the state's population. [Color figure can be viewed at wileyonlinelibrary.com]

$$SatisfiedWithSalary_{irt} = \beta_0 + \beta_1 PSBansRegion_{rt} + \beta_2 X_{irt} + \delta_r + \delta_t + \varepsilon_{irt} \quad (2)$$

The dependent variable, $SatisfiedWithSalary_{irt}$, is a dummy that takes value 1 if individual *r* who resides in region *r* at year *t* is satisfied with her salary. A negative coefficient on β_1 would suggest that individuals become less satisfied with their salaries after the implementation of a PS ban in their region of residence, indicating a positive relationship between PS and pay satisfaction.

X_{irt} is an individual-level vector that includes a range of control variables potentially correlated with individual *i*'s pay satisfaction. These control variables are a dummy for female individuals (*Female*), the individual's age (*Age*), a dummy for white individuals (*White*), the natural logarithm of the individual's salary (*Log*(*Salary*)), and the individual's occupation (captured through occupation fixed effects).

δ_r and δ_t region and year-fixed effects, respectively. When estimating Equation (2), standard errors are clustered at the region-by-year level because clustering at the region level alone would yield too few clusters. Additionally, observations are weighted by each individual's weight in the NSCG survey.

Column (1) in Table 2 presents the results obtained when estimating Equation (2) in the full NSCG sample.[15] The coefficient on *PSBansRegion* is positive and insignificant. The coefficients on *Age*, *White* and *Log*(*Salary*) are all positive and significant, while the coefficient on

[15]Columns (2) and (3) in Table 2 present the results when Equation (2) is estimated for managers and supervisors, respectively. The rationale for these regressions is explained in Section 4.2 below.

TABLE 2 Pay secrecy bans and pay satisfaction.

	Dependent variable: *SatisfiedWithSalary*		
	Full NSCG sample	**Managers**	**Supervisors**
	(1)	**(2)**	**(3)**
PSBansRegion	0.0215	0.0199	−0.00434
	(0.0174)	(0.0483)	(0.0313)
Female	0.00222	−0.00934	−0.0120*
	(0.00551)	(0.00853)	(0.00607)
Age	0.00147***	−0.0000709	0.000900***
	(0.000146)	(0.000354)	(0.000215)
White	0.0373***	0.0230	0.0386***
	(0.00491)	(0.0165)	(0.00780)
Log(Salary)	0.0978***	0.101***	0.119***
	(0.00265)	(0.00748)	(0.00369)
Occupation FE	Yes	No	No
Region FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations (weighted)	161,875,282	16,247,303	66,393,417
Observations (unweighted)	307,643	25,904	123,532

Note: This table shows the correlation between a dummy variable indicating whether the individual is satisfied with her salary (*SatisfiedWithSalary*) and the fraction of people exposed to pay secrecy (PS) bans in the individual's region of residence (*PSBansRegion*). Individual-level controls include a dummy for female individuals (*Female*), the individual's age (*Age*), a dummy for white individuals (*White*), the natural logarithm of the individual's salary (*Log*(*Salary*)), and the individual's occupation (captured through occupation fixed effects). The model also includes region and year-fixed effects (FE). Standard errors, in parenthesis, are clustered at the region-by-year level. Significant at: *10%, **5% and ***1%. Observations are weighted by each individual's weight in the National Survey of College Graduates (NSCG).

Female is positive but insignificant. The results for these individual-level controls are consistent with previous literature (Judge et al., 2010; Keaveny & Inderrieden, 2000; Loscocco & Spitze, 1991).

The insignificant coefficient on *PSBansRegion* reported in Column (1) of Table 2 suggests that PS policies are irrelevant for workers' pay satisfaction. While this "pay satisfaction-based" robustness check certainly has its limitations—such as the lack of information on whether the worker is employed in an organization with a PS rule and the use of region-level exposure to the PS bans—it helps confirm my previous finding of no significant relationship between PS and voluntary turnover, as any potential link between these variables should be tied to an underlying connection between PS and pay satisfaction.[16]

Heterogeneous treatment effects

In this section, I explore whether a causal relationship between PS and voluntary turnover can be established when heterogeneous treatment effects are taken into account.

[16]I perform two additional untabulated robustness checks using the JOLTS dataset. First, I explore whether the absence of a relationship between PS and voluntary turnover persists when observations are not weighted by the state's population. Second, I examine whether this absence persists when using the interaction-weighted estimator proposed by Sun and Abraham (2021) to estimate dynamic treatment effects in staggered settings. Both robustness checks confirm the main finding of no significant relationship between PS and voluntary turnover.

College-educated workers are more likely to be subject to PS policies, because they often hold jobs that involve salary negotiation, resulting in greater salary disparities among them (Kim, 2015). Therefore, I reestimate Equation (1) in the subsample of states with a high fraction of college-educated workers (states where 35% or more of the population holds a bachelor's degree or higher). The results are presented in Figure 2a. Again, I find no causal relationship between PS and voluntary turnover.

As a second heterogeneous treatment effects analysis, I examine whether there is a causal relationship between PS and voluntary turnover in states with particularly low private-sector unionization rates (states where union coverage in the private sector is below 8%). This exercise is motivated by survey evidence indicating that non-unionized workers are more likely to be

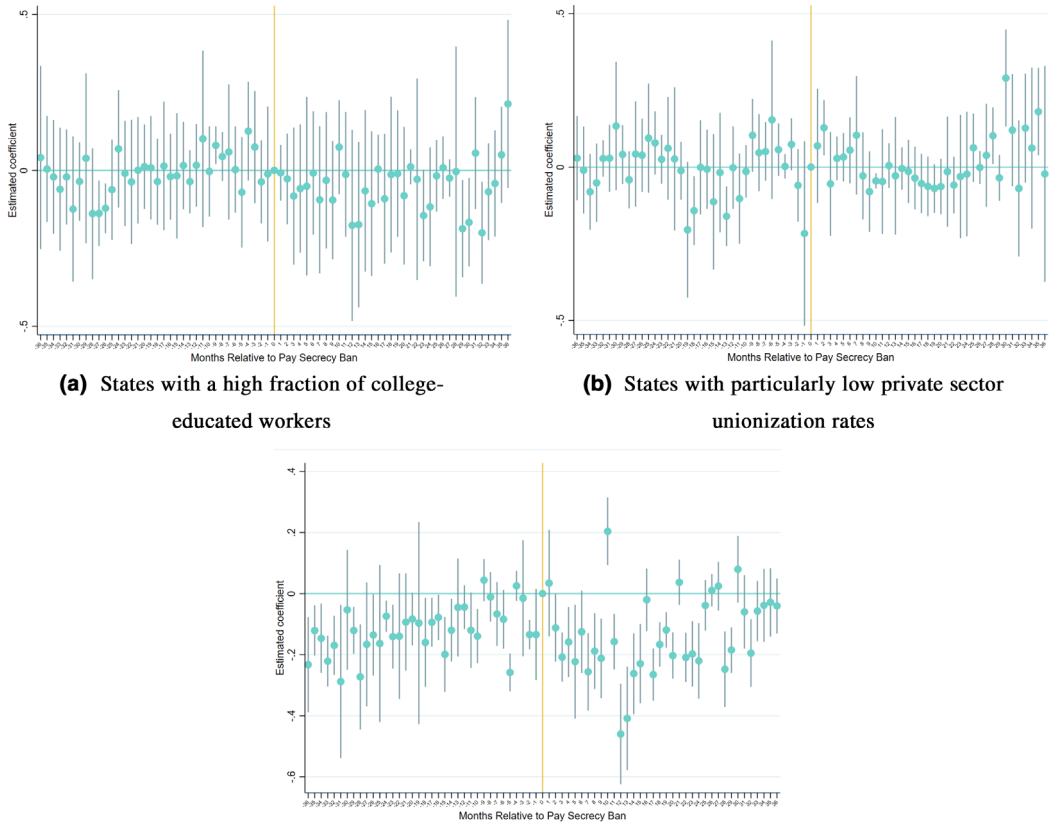

FIGURE 2 Pay secrecy bans and voluntary turnover (Heterogeneous treatment effects). This figure shows the effect of the state pay (PS) bans on voluntary turnover when heterogeneous treatment effects are taken into account. In Figure (a), the sample is restricted to states with a high fraction of college-educated workers. In Figure (b), the sample is restricted to states with particularly low private-sector unionization rates. In Figure (c), the sample is restricted to those treatment cohorts where the event is the implementation of a PS ban that imposes punitive damages. In each subfigure, the *y* axis shows the coefficient estimates resulting from regressing the natural logarithm of the number of quits in the state on state-by-cohort fixed effects (FE), month FE, year FE, the state's unemployment rate, the state's labor force participation rate, the natural logarithm of the state's real per capita personal income, and dummy variables indicating the month relative to the PS ban implementation. The dummies cover up to 36 months before and 36 months after the ban's implementation. The *x* axis represents the time relative to the PS ban implementation. The vertical lines indicate the 95% confidence intervals of the coefficient estimates, calculated from standard errors clustered by state. Observations are weighted by the state's population. [Color figure can be viewed at wileyonlinelibrary.com]

subject to PS policies than their unionized counterparts (Sun et al., 2021). Figure 2b shows no relationship between PS and voluntary turnover within this subset of states.

An implicit assumption in my analysis so far is that all the PS bans have the same treatment intensity. However, some PS bans impose more severe penalties than others (Oswalt et al., 2023). Hence, I test whether a negative causal relationship between PS and voluntary turnover emerges when restricting the sample to those treatment cohorts where the event is the implementation of a PS ban that imposes punitive damages, the most severe type of penalty (Oswalt et al., 2023). Figure 2c shows that even when focusing solely on these cohorts, no relationship between PS and voluntary turnover exists.

As mentioned in Section 2, employers can implement PS policies if they can provide "legitimate and substantial business justifications". This, together with the small penalties to employers implementing PS policies under the NLRA (Rosenfeld, 2021), suggests that one can safely assume that all workers are affected by a PS ban once it is enacted in their state of residence. This approach has been used in previous studies by Kim (2015) and Cullen and Pakzad-Hurson (2023).

Having said that, it is also possible to argue that managers and supervisors are likely the groups of workers most affected by the PS bans (Burn & Kettler, 2019). As also mentioned in Section 2, "managers and supervisors, broadly defined as workers with the power to hire or fire, are not protected by the NLRA (Buchwald, 2021; Malmon, 2022; National Women's Law Center, 2019; Sun et al., 2021)".

Accordingly, I conduct an additional test using the NSCG data to explore the impact of the PS bans on the pay satisfaction of managers and supervisors. The results for these groups of workers, displayed in columns (2) and (3) of Table 2, respectively, show no relationship between PS and pay satisfaction. This, in turn, likely implies a lack of relationship between PS and voluntary turnover for these workers.

Overall, the analysis presented in this section reaffirms the main finding of no causal relationship between PS and voluntary turnover. The lack of a significant negative relationship between these two variables suggests that PS policies are unnecessary to prevent voluntary turnover.

The reluctance-to-ask explanation

According to a 2021 Zippia survey, half of American workers are willing to share their salaries with their co-workerss (Zippia, 2023). Hence, contrary to my results, the Zippia survey suggests that PS policies are indeed necessary to prevent the flow of salary information among workers and subsequent voluntary turnover.

However, it is reasonable to assume that, in most cases, a worker will share her salary with a colleague only if she is asked to do so in the first place (it is difficult to imagine a worker revealing her salary to her colleagues out of nowhere). In this regard, evidence suggests that workers are heavily reluctant to ask others about their pay. In an experiment with a sample of over 700 employees from a large commercial bank in Southeast Asia, Cullen and Perez-Truglia (2023) find that 69% consider it socially unacceptable to ask co-workerss about their salary, and 89% would feel uncomfortable if they had to ask a co-workers about their salary.[17] The findings of Cullen and Perez-Truglia (2023) are consistent with the results of a 2022 Indeed survey of 1500 people living in the United States, in which 63% responded that they have never asked a co-workers about their pay (Gafner, 2024).[18]

[17]By comparison, employees do not find discussions about seniority to be nearly as sensitive as discussions about salary: only 7% find it unacceptable to ask coworkers about their seniority, and only 23% would feel uncomfortable asking a coworker about his or her seniority.

[18]Cullen and Perez-Truglia (2023) also find that 80% of workers would prefer not to share information about their pay with their colleagues. This number, which is much larger than that found in the Zippia survey, provides further support for the role of communication frictions in the form of privacy norms in explaining my findings.

Based on this evidence, I propose that my results could primarily be explained by workers' reluctance to ask others about their pay. This reluctance-to-ask among workers may prevent the initial step necessary for salary communication—asking others about their pay—, potentially rendering PS policies irrelevant for retention purposes. Consequently, organizations might need to explore alternative approaches to enhance employee retention and commitment.

Can the PS bans be considered as a negative shock to PS?

Similar to Cullen and Pakzad-Hurson (2023), Oswalt et al. (2023) use data from two nationally representative surveys on PS policies conducted by the Institute for Women's Policy Research (IWPR) in 2010 and 2017/2018 to explore the evolution of these policies over time. In line with Cullen and Pakzad-Hurson (2023), Oswalt et al. (2023) estimate that, at the national level, the fraction of workers subject to a formal PS policy level declined from 18.1% to 12.8% between 2010 and 2017/2018. However, Oswalt et al. (2023) estimate that the fraction of workers subject to an informal PS policy *increased* from 30.3% to 35.4% during the same period. Hence, according to Oswalt et al. (2023), the national share of workers subject to a PS rule of any kind (either formal or informal) barely changed between 2010 and 2017/2018. Given this time-series evidence, Oswalt et al. (2023) contend that employers might have been shifting from formal to informal PS rules in response to the PS bans. Oswalt et al. (2023) further support their shifting hypothesis with cross-sectional evidence from the 2017/2018 IWPR survey: according to this survey, informal PS rules are slightly more common in states that passed PS bans between 2010 and 2018 than in those that did not (36.8% vs. 35.8%).[19]

The shift from formal to informal PS rules in response to the PS bans would imply that these laws cannot be considered as a negative shock to PS. This, in turn, would challenge the validity of my proposed reluctance-to-ask explanation. If the bans cause employers to switch from formal to informal PS rules, then these informal rules might prevent voluntary turnover from increasing after such laws.

Given this, in the remainder of this section, I point out some limitations of the evidence used by Oswalt et al. (2023) to support their shifting explanation. In addition, I present qualitative and quantitative arguments suggesting that the PS bans do not cause a shift from formal to informal PS rules, thus further supporting their interpretation as a negative shock to PS.

Limitations of the evidence supporting the shifting explanation

The increase in informal PS rules at the national level between 2010 and 2017/2018 reported by Oswalt et al. (2023) is certainly interesting. However, these authors do not provide evidence that this increase comes primarily from states that have adopted PS bans, which likely constitutes the first crucial step in validating their shifting explanation.[20] Without this evidence, it is difficult to ascertain whether the observed increase in informal PS rules is genuinely attributable to the PS bans or if other factors may be at play.

In a similar vein, the fact that informal PS rules are slightly more common in states that passed PS bans between 2010 and 2018 than in those that did not does not necessarily imply

[19]Similar evidence is presented by Sun et al. (2021).

[20]As mentioned in Section 2, Cullen and Pakzad-Hurson (2023) note that the decline in formal PS rules between 2010 and 2017/2018 was driven primarily by treated states.

that the PS bans caused a shift from formal to informal PS rules. Given the cross-sectional nature of this evidence, it is impossible to determine causality. Indeed, it might be the case that the PS bans actually caused a decline in both formal and informal PS rules (as I argue below), but informal PS rules are still more prevalent in treated states due to other unobserved factors.

PS bans as a negative shock to PS: Qualitative arguments

Despite its limitations, the evidence presented by Oswalt et al. (2023) might still cast doubt on the interpretation of the PS bans as a negative shock to PS. Hence, I now proceed to provide arguments that support this interpretation.

On the qualitative front, it is worth noting that if PS rules become illegal in a state, employers adopting informal rules to replace formal ones might be seen as circumventing the law and could be, therefore, at risk of legal penalties. This suggests that in states with PS bans, the shift toward informal PS rules might not be happening, or if it is happening, that the informal rules applied in those states are weaker than those found in states without PS bans. Accordingly, the PS bans could still be considered as a negative shock to PS.

PS bans as a negative shock to PS: Quantitative arguments

Ideally, a differences-in-differences approach would be used to quantitatively rule out the possibility that the PS bans lead to an increase in informal PS rules. In such an approach, the dependent variable would be the presence or intensity of informal PS rules within firms, while the main independent variables would be dummy variables indicating the time relative to the implementation of the PS bans by treated states. Unfortunately, however, the very nature of informal PS rules makes it virtually impossible to conduct such an analysis—these rules are, by definition, difficult to measure and track.

Therefore, as an alternative approach to provide quantitative evidence for the role of the PS bans as a negative shock to PS, I test whether these laws lead to increased workplace transparency more broadly. The rationale is that increased workplace transparency would be inconsistent with firms substituting formal PS policies with informal ones, as greater transparency would undermine the secrecy resulting from such substitution.

I use the Chen et al.'s (2015) disclosure quality index as a proxy for firm transparency. This index measures the level of disaggregation of the financial information disclosed in firms' annual reports, ranging from 0 to 1, where 0 indicates the lowest disaggregation and 1 indicates the highest.[21] The Chen et al.'s (2015) disclosure quality index is particularly appealing as a proxy for firm transparency for at least two reasons. First, unlike other disclosure measures, this index is available for the entire universe of Compustat firms in a panel structure starting from 1973. Such a large panel allows for a comprehensive analysis of the impact of the PS bans on firm transparency. Second, by relying on the disaggregation of financial information, it provides a more objective and consistent assessment of firm transparency compared to measures based on researchers' or managers' judgment (Chen et al., 2015).

I perform one-to-one nearest-neighbor matching with replacement to pair treatment and control firms, using the firm's total assets in the year before a PS ban, as well as the firm's industry (four-digit SIC code) and region as matching variables. I then estimate the following model:

[21]The index is calculated independently for both the income statement and the balance sheet and then averaged at the firm level.

TABLE 3 Pay secrecy bans and disclosure quality.

	Dependent variable: *DisclosureQuality*		
	(1)	**(2)**	**(3)**
PSBan($t-3$)	−0.00327	−0.00327	−0.00330
	(0.00304)	(0.00287)	(0.00333)
PSBan($t-2$)	0.000571	0.000571	0.000206
	(0.00292)	(0.00247)	(0.00297)
PSBan($t-1$)	−0.000101	−0.000101	−0.000704
	(0.00233)	(0.00206)	(0.00220)
PSBan($t+1$)	0.0000824	0.0000824	0.000272
	(0.00291)	(0.00266)	(0.00266)
PSBan($t+2$)	0.00946**	0.00946***	0.0118***
	(0.00376)	(0.00328)	(0.00322)
PSBan($t+3$)	0.00936**	0.00936**	0.00952*
	(0.00441)	(0.00400)	(0.00479)
Firm-cohort FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	2978	2978	2678

Note: This table shows the effect of the state pay secrecy (PS) bans on firms' disclosure quality. In each column, the dependent variable is the Chen et al.'s (2015) disclosure quality index (*DisclosureQuality*), while the independent variables are dummy variables indicating the year relative to the implementation of the PS ban, firm-by-cohort fixed effects (FE), and year FE. Column (1) uses the full sample with standard errors clustered at the state level. Column (2) also uses the full sample but clusters standard errors at the state-cohort level. Column (3) excludes very large firms (those with total assets above $5 billion) and clusters standard errors at the state-cohort level. Significant at: *10%, **5% and ***1%.

$$DisclosureQuality_{icst} = \alpha_0 + \sum_{k=-3}^{3} \beta_k PSBan_{s,t+k} + \alpha_{ic} + \delta_t + \varepsilon_{icst} \quad (3)$$

where $DisclosureQuality_{icst}$ denotes the Chen et al.'s (2015) disclosure quality index for firm i, in cohort c, headquartered in state s in year t.[22] $PSBan_{s,t+k}$ is a dummy that takes value 1 if a PS ban became effective in state s k years ago ($k = -3, -2 \ldots \ldots, 2, 3$). The year at which the PS ban became effective ($k = 0$) serves as the reference year. Positive coefficients on the post-law dummies would be consistent with firms being more transparent after a PS ban, which in turn would be inconsistent with a greater reliance on informal PS rules after such a ban. The model also includes firm-by-cohort fixed effects (α_{ic}) and year-fixed effects (δ_t).

The results are presented in Table 3. Column (1) shows a significant increase in the Chen et al.'s (2015) disclosure quality index starting in the second year following a PS ban, using the full sample with standard errors clustered at the state level. In Column (2), this increase persists when standard errors are clustered at the state-cohort level. Column (3) shows that the result remains robust even after excluding very large firms (those with total assets above $5 billion), whose exposure to the PS bans may not be properly captured by the PS ban dummies, with standard errors clustered at the state-cohort level.

Overall, the results in Table 3 reveal an increase in firm transparency following a PS ban. This finding suggests that informal PS rules do not become more prevalent after such a

[22]The values for the Chen et al.'s (2015) disclosure quality index are winsorized at the 5% level to handle outliers. I obtain Compustat firms' historical headquarters states from Gao et al. (2021).

ban, further supporting the notion that these bans constitute a genuine negative shock to PS practices.

CONCLUSION

Prior research suggests that employers implement pay secrecy (PS) policies to prevent low earners from discovering wage disparities with high earners, which could lead to pay dissatisfaction and ultimately result in their departure from the firm. In this paper, I have formally examined whether there is a negative causal relationship between PS policies and voluntary turnover. Using state-level data on voluntary turnover from the Job Openings and Labor Turnover Survey and exploiting the staggered adoption of the state PS bans as a negative shock to PS, I find no causal relationship between PS and voluntary turnover. The absence of a causal relationship persists even after narrowing down the sample to states with a high fraction of college-educated or non-unionized workers, who are presumed to be more subject to PS arrangements. Using individual-level data from the National Survey of College Graduates, I do not find a relationship between PS and pay satisfaction either. This further supports the finding of a null relationship between PS policies and voluntary turnover, as any link between the two variables should be tied to an underlying link between PS and pay satisfaction.

My results suggest that communication frictions, particularly in the form of social norms that inhibit workers from asking their colleagues about their pay, make PS policies irrelevant for retention purposes. This, in turn, suggests that organizations should explore alternative approaches to enhance employee retention and commitment, as PS policies may not be effective in achieving these goals.

This paper offers multiple avenues for future research. If PS policies do not prevent voluntary turnover and may even reduce employers' bargaining power in salary negotiations (Cullen & Pakzad-Hurson, 2023), why do employers implement them? Are there potential unintended effects of the PS bans, beyond the increase in financial reporting disaggregation documented in this paper, that warrant further exploration? Answering these and related questions could enhance our understanding of the effectiveness of pay transparency initiatives and their impact on organizational culture, employee morale, and overall business performance.

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DATA AVAILABILITY STATEMENT

The JOLTS and NSCG data are publicly available at https://www.bls.gov/jlt/data.htm and https://ncsesdata.nsf.gov/sestat/, respectively. Access to the Compustat database requires a license and is not publicly available without permission.

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