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EQUILIBRIUM EFFECTS OF PAY TRANSPARENCY

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The discourse around pay transparency has focused on partial equilibrium effects: how workers rectify pay inequities through informed renegotiation. We investigate how employers respond in equilibrium. We study a model of bargaining under two-sided incomplete information. Our model predicts that transparency reduces the individual bargaining power of workers, leading to lower average wages. A key insight is that employers credibly refuse to pay high wages to any one worker to avoid costly renegotiations with others. When workers have low individual bargaining power, pay transparency has a muted effect. We test our model with an event-study analysis of U.S. state-level laws protecting the right of private-sector workers to communicate salary information with their coworkers. Consistent with our theoretical predictions, transparency laws empirically lead wages to decline by approximately 2%, and wage declines are smallest in magnitude when workers have low individual bargaining power.

KEYWORDS: Pay Transparency, Bargaining, Privacy, Wage Gap.

1. INTRODUCTION

Most pay transparency initiatives are based on the narrative that transparency gives workers more bargaining power. Pay transparency laws aim to increase workers' knowledge of the pay of their peers to ensure "victims of pay discrimination can effectively challenge unequal pay," equipping them for successful negotiations by revealing their employer's willingness to pay for labor (Phillips, 2009). But this use of salary information to remedy unequal pay for equal work is only half of the story; when salary transparency is anticipated by the employer and employees, optimal wage-setting, bargaining, and employment practices also adjust. Despite a lack of evidence on the indirect effects of pay transparency, 22 U.S. states and 10 EU countries have passed laws to increase pay transparency (Siniscalco et al., 2017, Pender, 2017, Veldman, 2017, International Labour Organization, 2018).

We study how the equilibrium effects of pay transparency on wage negotiation can lead to an unintended outcome. We combine a dynamic wage-bargaining model with an event-study analysis of the enactment of U.S. state level pay transparency laws. Our theory predicts employer adjustments in wage-setting and hiring policies decreases workers' de facto bargaining power when transparency increases, and consequentially lowers average wages. Our empirical analysis corroborates these predictions: the average wage among private-sector employees falls in states enacting transparency laws.

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We study a dynamic bargaining game between workers and a firm, in which neither side observes the match value of the other party. We model pay transparency as the probability of observing peer wages. Mechanically, transparency provides information that workers can exploit in renegotiations, and it can also increase the likelihood a wage renegotiation occurs at all.[1] Both of these alter the de facto bargaining power through two equilibrium effects: a *demand effect* and a *supply effect*. As transparency rises, the firm's maximum willingness to pay for labor falls because information about one worker's pay raise spreads to others, who use that information to renegotiate (demand effect). At the same time, workers make lower initial wage offers to increase their chances of getting hired (supply effect). Because workers expect to learn the wages of others and renegotiate with higher transparency, they are less concerned with securing a high initial wage.

Transparency resembles best-price guarantees which rebate existing customers if prices fall in the future. These agreements are theoretically shown to increase the commitment power of sellers, allowing them to maintain higher prices (Butz, 1990, Cooper and Fries, 1991), and empirical evidence supports these findings (Scott Morton, 1997b,a). However, a point of departure when analyzing the equilibrium effects of pay transparency is that the value of labor is private information of the firm. The presence of two-sided incomplete information leads to novel equilibrium predictions.

We show that increasing transparency has the same equilibrium effect as decreasing worker bargaining power, and results in lower wages. Formally, we show a mapping between the equilibria of our dynamic game and those of static double auctions studied by Chatterjee and Samuelson (1983). The equilibria of our game under high transparency and high worker bargaining power are identical to the equilibria with lower transparency and lower worker bargaining power. Full transparency grants the firm full de facto bargaining power when renegotiations are common, as the firm commits to, and workers observe and adhere to, a maximum wage. By Williams (1987), this maximizes expected firm profits and minimizes expected worker surplus and wages.

In environments with low individual worker bargaining power to begin with, the effects of higher transparency on wages are muted. The reason is that workers, at baseline, are less able to exploit differences in their outside options to secure heterogeneous wages, and as a result there is less scope for upward wage renegotiation. In markets with no individual worker bargaining power, such as markets with posted wages or markets where wages are set by a collective or union, transparency will not affect wages in equilibrium.

Pay transparency has a non-monotonic effect on employment because employment is maximized when bargaining power is shared between workers and the firm. When bargaining power is highly skewed, either workers act like monopolists, making high wage demands that are often rejected; or the firm acts like a monopsonist, committing to low wages that deter high-outside-option workers from considering work at the firm. Granting either the firm or workers all of the bargaining power minimizes expected employment.[2]

Our finding that pay transparency lowers worker bargaining power raises the question of why we do not observe more firms voluntarily selecting high levels of transparency. Indeed,

[1] We do not explicitly model the mechanism by which observing peer wages enables a worker to "bring the firm to the bargaining table." Empirical evidence supports that the observation of peer wages increases the rate of rengotiation; Biasi and Sarsons (2021) find that "knowing the pay of colleagues is associated with a 5.7 percentage point (24 percent) higher chance of having negotiated after the start of the current contract" (page 176).

[2] The potential positive effect of pay transparency on employment is perhaps surprising given results of bargaining models with *one-sided* private information (Brancaccio et al., 2020, Hörner and Vieille, 2009, Bergemann and Hörner, 2018, Kaya and Liu, 2015). All find that transparency decreases the number of (or prevents) transactions. More similarly to our finding, Loertscher and Muir (2021) study a model with one-sided (worker) private information, and show that labor market interventions may lear to non-monotonic employment responses.

evidence suggests that the majority of firms attempt to limit pay transparency (Hegewisch et al., 2011, McCarthy, 2018). Theoretically we find that transparency *laws* serve a critical function in allowing the firm to commit to being transparent. In the absence of a law, a firm would prefer to shirk on its promise of transparency. Consider a firm that promises a worker to post wages on a company message board, to be updated whenever wages adjust. After initial negotiations, the firm would have a profitable deviation to simply neglect to update it, or worse, systematically under-report wages. In equilibrium, our model predicts that when the firm cannot contract on the level of transparency, it will always select full secrecy, and workers (anticipating secrecy) will not alter their bargaining strategies. This points to an important role for transparency legislation, and in particular, laws that promote credible information, such as protections for co-workers to circulate salary information.

We identify a collection of such laws aimed at facilitating pay discussions between coworkers, allowing us to test our equilibrium predictions. Over the past two decades, U.S. states have individually enacted laws imposing punishments for employers that retaliate against workers who disclose their wages or inquire about the wages of coworkers. We refer to these as “Right of Workers To Talk" (ROWTT) laws. The staggered enactment of these state laws allow us to causally identify equilibrium effects of pay transparency in an event study framework.

Nationally representative surveys in 2010 and 2018, conducted by Hegewisch et al. (2011) and Sun et al. (2021), reveal that the share of private-sector employees prohibited from discussing pay with co-workers fell from 33.2% to 10.1% in states that enact ROWTT legislation between survey waves, while the share in all other states fell by a small fraction in comparison.

We examine the effect of ROWTT laws on private-sector workers using data from the American Community Survey (ACS). Our window of analysis runs from the inception of the ACS in 2000 to 2016, when a related federal policy rolled out across all states. During this time period, 13 states enacted ROWTT laws.

Corroborating our main theoretical finding, wages fall as pay transparency rises. In the year following the enactment of the ROWTT, wages fall by 2.2%, and decline 2.7% by the third year after the law. Our conclusions hold across many specifications, including allowing for heterogeneous treatment effects across cohorts.

Wage declines can result from two channels in our model: a direct reduction in wage rates and, under some circumstances, reduced employment of high outside option workers who demand higher wages. Both are a consequence of lower worker bargaining power under higher pay transparency. We find that enactment of ROWTT raises employment levels by 0.5% on average, but this accounts for less than one third of the reduction in wages.

To investigate the interaction between bargaining power and transparency laws, we separately examine labor markets with low and high individual worker bargaining power. In our model, the lowest level of *individual* bargaining power occurs when workers face take-it-or-leave-it (TIOLI) wage offers. Hall and Krueger (2012) find that the two leading predictors of facing a TIOLI wage offer, as opposed to wage bargaining, are low education (those with a 4-year college degree are 1.5 times as likely to bargain as those without), and union membership (non-union members are 2.3 times as likely to bargain as unionized workers). In line with our theoretical predictions, workers with limited individual bargaining power–workers without 4-year college degrees and those in occupations with above-median unionization–experience negligible wage declines following ROWTT. By contrast, the relative wages of workers with a 4-year college degree, and those in occupations with below-median unionization, dip below 3% within the three year window. We caution that there exist differences between these groups that we cannot control for in this analysis. However, as we detail below, these estimates fall in line with similar figures across labor markets in five countries.

Connections to Literature

A key feature of our model is that there is two-sided incomplete information. This leads to both the supply and demand effects arising, which causes simultaneous adjustments of bargaining strategies by workers and the firm in equilibrium as transparency increases. Dynamic games with incomplete information frequently contain analogues of one of these effects, but not, to our knowledge, both. In the well-known chain store game, Kreps and Wilson (1982) and Milgrom and Roberts (1982) show that costly, predatory behavior against early competitors may be optimal in order to create a reputation favorable for later negotiations (demand effect). Kuhn and Gu (1998, 1999) show that unions optimally delay making contract offers to employers so that they can freeride on information gathered from the negotiations of others (supply effect).

Our model of incomplete-information bargaining reconciles heterogeneous impacts of pay transparency policies documented in different labor markets and geographies. A comprehensive review of studies that evaluate the effect of pay transparency on the wages of all employed workers yields nine studies, spanning five countries (Baker et al., 2021, Bennedsen et al., 2019, Blundell, 2021, Böheim and Gust, 2021, Duchini et al., 2020, Gulyas et al., 2021, Mas, 2017, Obloj and Zenger, 2022). These studies are broadly focused on wage compression, but as a secondary outcome, some studies report a significant decrease in wages, while others find a tight null effect on wages. We contextualize these findings by proxying the degree of individual bargaining power workers have in each study based on the share of workers covered by a union or collective bargaining agreements. We find that in markets with high individual bargaining power (low unionization rates), pay transparency laws lower average wages around 2%, as we find in the context of the U.S. private sector. In markets with low individual bargaining power (high unionization rates), pay transparency laws have little to no effect on wages. We conduct a mixed-effects meta-regression analysis and find that a 10 pp decrease in share of labor market unionization is associated with 0.18 pp lower wages following the transparency law.

An important prediction of our model is that wages will be more equal within a firm as low-outside option workers benefit relatively more from transparency. Empirical support for compression within firm can be found in the studies included in our meta-analysis. Six of nine studies find that wages for men decline more than wages for women when evaluating the effect of pay transparecy on within firm wages, consistent with our model's predictions of wage compression when men's outside options are higher than women's.

The remainder of the paper is organized as follows. Section 2 lays out our model and presents our main theoretical findings. Section 3 tests the predictions of our model empirically. We include an event-study analysis of U.S. state ROWTT laws and a meta analysis. Section 4 concludes.

2. MODEL

2.1. *Setup*

There are two time periods $t \in \{1, 2\}$. There is a single firm, and a unit-measure set of workers I. Each worker $i \in I$ has a private outside option $\theta_i \overset{iid}{\sim} G[0,1]$.[3] The firm has a constant-returns-to-scale production function. We assume that the productivity of labor is common

[3]There is a known measurability issue with the assumption of a continuum of i.i.d. random variables (Judd, 1985). A solution is to assume that worker outside options are drawn "almost" i.i.d. in the sense of Sun (2006). This solves the measureability issue and has the intuitive and intended property that the distribution of realized outside options is given by the same function $G(\cdot)$.

across all workers: the firm receives (infinitesimal) payoff $v \sim F[0,1]$ for each hired worker, where v is private information of the firm. F and G are assumed to be twice continuously differentiable with densities f and g, respectively, where $f(x) > 0$ for all $x \in (0,1]$ and $g(y) > 0$ for all $y \in [0,1)$. We also assume $\theta + \frac{G(\theta)}{g(\theta)}$ is strictly increasing in θ and $v - \frac{1-F(v)}{f(v)}$ is strictly increasing in v, that is, agents have strictly increasing virtual values (Myerson, 1981).

Before any workers arrive, the firm commits to a persistent maximum wage $\bar{w}(v) \in [0,1]$; the firm does not individually tailor wage offers to workers, consistent with the empirical findings of Di Addario et al. (2022). We discuss the case in which the firm can set different maximum wages for ex-ante heterogeneous workers in the Online Supplement. $\bar{w}$ is not immediately observed by workers. An initial round of bargaining takes place at $t = 1$. Each worker i commits to a walk-away wage at $t = 1$, which we refer to as her initial offer, $\underline{w}_{i,1}(\theta_i) \in [0,1]$. As in a double auction (Chatterjee and Samuelson, 1983), i is employed if and only if $\underline{w}_{i,1} \leq \bar{w}$. If hired, i's initial wage $w_{i,1}$ is a random variable that equals $\underline{w}_{i,1}$ with probability $1-k$ and equals $\bar{w}$ with probability k (independently across workers), where $k \in [0,1]$ is the known "bargaining weight" of the firm. Let I_1 represent the set of all workers employed at this stage.

We model transparency as the random arrival of information about current wages. At time $t = 2$ each employed worker $i \in I_1$ observes the set of negotiated wages, $\{w_{i,1}\}_{i \in I_1}$, with independent probability $\tau \in [0,1]$. Therefore, τ defines the level of pay transparency. Each employed worker $i \in I_1$ who observes the wages of her peers renegotiates with the firm with probability $\rho \in [0,1]$ by making a new offer $\underline{w}_{i,2}$. Let $I_2 \subset I_1$ be the set of initially employed workers who remain employed, either by not renegotiating with the firm or by making second-round offers weakly less than $\bar{w}$ (i.e. $\underline{w}_{i,2} \leq \bar{w}$). For each $i \in I_2$, the final wage w_i is determined as follows: if i did not renegotiate her wage, $w_i = w_{i,1}$. If i did renegotiate her wage, w_i equals $\underline{w}_{i,2}$ with probability $1-k$ and $\bar{w}$ with probability k, independently across workers.

All agents are risk neutral and wish to maximize their payoff, where a worker's payoff is w_i for all employed workers $i \in I_2$ and θ_i otherwise, and the firm's (infinitesimal) payoff is $v - w_i$ from each employed worker $i \in I_2$ and 0 from each unemployed worker $j \in I \setminus I_2$.

2.2. *Effect of transparency on bargaining power*

We investigate pure strategy perfect Bayesian equilibria (PBE) which satisfy the following regularity conditions:

A1 $0 \leq \bar{w} \leq v$ for all v. If $v \leq \underline{w}_{i,1}$ for every worker i according to equilibrium strategies then $\bar{w} = v$.

A2 $\theta_i \leq \underline{w}_{i,1} \leq 1$ for all i. If there is no v such that $\theta_i \leq \bar{w}$ according equilibrium strategies then $\underline{w}_{i,1} = \theta_i$.

A3 $\bar{w}$ and $\underline{w}_{i,1}$ are strictly increasing functions of v and θ_i, respectively. Moreover, $\bar{w}$ is continuously differentiable for $v \in (\underline{w}_{i,1}(0), 1)$ and $\underline{w}_{i,1}$ is continuously differentiable for $\theta \in (0, \bar{w}(1))$.

A1 and **A2** restrict actions of agents who never match in equilibrium, because either the firm's value for labor is too low or the worker's outside option is too high. These assumptions rule out pathological equilibria in which, for example, $\bar{w} = 0$ for all v and all workers offer $\underline{w}_{i,1} = 1$. **A3** assists in tractability. It also eliminates equilibria in which workers and the firm pool on a predetermined wage from consideration.[4]

[4] Leininger et al. (1989) suggest similarities between the set of continuous equilibria and a set of discontinuous equilibria in static double auctions, and so we do not believe this to be a conceptually limiting constraint. We discuss the connection of our game to static double auctions below.

There always exists an equilibrium satisfying **A1-3**. In equilibrium, an increase in transparency lowers de facto worker bargaining power in any equilibrium. The following result and proof demonstrates the connection between transparency and bargaining power.

PROPOSITION 1: *The set of equilibria is non-empty. In any equilibrium, $I_1 = I_2$ and each worker i receives $w_i = \bar{w}$ upon renegotiating.*

PROOF: In any equilibrium satisfying **A1-3**, each worker receives a final wage equal to $\bar{w}$ upon renegotiating; by **A3** workers trace out the set $[a, 1]$ for some $a \in [0, 1]$ with their initial wage offers. Therefore, there is some worker j who receives initial wage $w_{j,1} = \bar{w}$ for any $k \in [0, 1]$ (assuming the firm hires a positive measure of workers, i.e. $\bar{w} \geq a$), and any worker i who observes peer wages and renegotiates will offer and receive $\underline{w}_{i,2} = w_{j,1} = \bar{w}$.

Therefore, any worker i who offers $\underline{w}_{i,1} < \bar{w}$ will receive final wage $w_i = \bar{w}$ if either: $w_{i,1} = \bar{w}$ (which occurs with probability k), or if she observes peer wages and renegotiates (which occurs with probability $\tau\rho$). With the complementary probability, i receives final wage $w_i = \underline{w}_{i,1}$. Let $\bar{F}(x) = Pr(\bar{w} \leq x)$, and $\bar{G}(x) = Pr(\underline{w}_{i,1} \leq x)$ for $x \in [0, 1]$ with densities $\bar{f}(\cdot)$ and $\bar{g}(\cdot)$, respectively. We prove the following useful lemma in Appendix A.

LEMMA 1: *In any equilibrium satisfying **A1-3**, each worker i who matches to the firm with positive probability and each firm type v that hires a positive measure of workers respectively solve*

$$\underline{w}_{i,1} = \underset{w}{argmax} \int_{w}^{1} \left((1-\Omega)\, w + \Omega x - \theta_i\right) \bar{f}(x) dx, \tag{1}$$

$$\bar{w} = \underset{w}{argmax} \int_{0}^{w} \left(v - (1-\Omega)\, y - \Omega w\right) \bar{g}(y) dy \tag{2}$$

where

$$\Omega = k + (1-k)\tau\rho. \tag{3}$$

Equations 1 and 2 are the same objective functions as those in a static double auction between a single worker whose type is drawn according to G, and a single firm whose type is drawn according to F, with a bargaining weight of Ω on the firm's offer. Therefore, the set of equilibria of this static double auction corresponds to the set of equilibria of our game, and the set of such equilibria satisfying **A1-3** is nonempty (Satterthwaite and Williams, 1989, Theorem 3.2).

In equilibrium, for any worker with a positive probability of matching with the firm, and any firm type which matches with a positive measure set of workers, the first order conditions are, respectively:

$$\underline{w}_{i,1} - \theta_i = \overbrace{(1-\Omega)}^{\text{direct effect}} \cdot \underbrace{\frac{1 - \bar{F}(\underline{w}_{i,1})}{\bar{f}(\underline{w}_{i,1})}}_{\text{indirect effect}} \tag{4}$$

$$v - \bar{w} = \overbrace{\Omega}^{\text{direct effect}} \cdot \underbrace{\frac{\bar{G}(\bar{w})}{\bar{g}(\bar{w})}}_{\text{indirect effect}} \tag{5}$$

It is both necessary and sufficient for any equilibrium satisfying **A1-3** to also satisfy these two first order conditions (Satterthwaite and Williams, 1989, Theorem 3.1), as the assumed monotonicity of the virtual value functions ensures the quasi-concavity of the worker and firm objective functions in Equations 1 and 2, respectively. *Q.E.D.*

Equation 3 reveals that increasing the level of transparency τ has a similar effect as increasing the firm's bargaining weight k: both increase Ω, the *de facto bargaining power* of the firm, for any $\rho > 0$. Moreover, Ω is submodular in τ and k, implying that a fixed increase in transparency is more impactful the smaller is k, the nominal bargaining power of the firm. When $\tau\rho = 1$, $\Omega = 1$, implying that when workers always observe peer wages and renegotiate, the nominal bargaining power k does not affect the equilibrium outcome. Similarly, when the firm has all of the nominal bargaining power, i.e. $k = 1$, the equilibrium outcome is constant in the level of transparency τ. This matches our earlier description: under full transparency with common renegotiations, all workers learn $\bar{w}$ immediately and secure this wage if it is higher than their outside option, regardless of k. The firm therefore "posts" $\bar{w}$ knowing that all employed workers will receive this wage. When $k = 1$ the firm makes an initial TIOLI offer $\bar{w}$ to each worker, and all workers with a lower outside option will be employed by the firm at this wage. Wages are "transparent" to workers in that all workers know the firm pays a common wage.

REMARK 1: Ω is (continuously) increasing in k, τ and ρ, and is submodular in k and τ, submodular in k and ρ, and supermodular in τ and ρ.

The proof of this result is contained in Appendix A.

These descriptions indicate that increasing any of k, ρ, and τ lowers workers' de factor bargaining power, however, k and τ are substitutes, k and ρ are substitutes, and τ and ρ are complements.

Decoupling transparency and renegotiation

Our analysis thus far assumes that workers are not able to renegotiate unless they have observed the wages of their peers. We extend our model to a continuous-time setting and decouple the timing of wage information and renegotiations in the Online Supplement.[5]

Decoupling the arrival of information and the timing of renegotiation results in the same key connection between transparency and bargaining power: raising transparency lowers worker bargaining power. We show that analogous first-order conditions governing equilibria in our two-period model (Equations 4 and 5) hold, and the comparative statics of firm bargaining power, transparency, and renegotiation frequency have similar qualitative effects on equilibrium bargaining power. Therefore, our predictions on the effects of pay transparency on labor market outcomes are not limited by our assumption that workers are unable to negotiate without first observing peer wages.

[5]Our two-period model suffers from an unrealistic lack of stationarity if workers can renegotiate without first observing peer wages: workers who do not observe peer wages before $t = 2$ but are able to renegotiate will do so more aggressively knowing that there is no future possibility of observing peer wages.

We also use this continuous time model to study an extension in which workers search for work across multiple firms in the presence of transparency.

2.3. *Effect of Transparency on Labor Market Outcomes*

To make predictions about labor market outcomes that we can take to the data, we select a relevant class of equilibria. We then study the effects of increasing transparency within this class of equilibria.

The monotonicity of the virtual value functions leads to a unique equilibrium when $\Omega \in \{0,1\}$ (Williams, 1987), but there exists a continuum of equilibria for $\Omega \in (0,1)$ (Satterthwaite and Williams, 1989). This multiplicity arises because the equilibrium bargaining strategies of workers and the firm are interdependent for any $\Omega \in (0,1)$; workers decide how aggressively to make initial offers depending on how the firm sets $\bar{w}$, while the firm sets $\bar{w}$ as a function of how aggressively the workers make initial offers. The set of equilibria for $\Omega \in (0,1)$ lacks natural ordering. However, experimental evidence in Radner and Schotter (1989) suggests that equilibria in which $\underline{w}_{i,1}$ and $\bar{w}$ are linear functions of θ_i and v, respectively, are focal and most likely to be selected in practice.

To focus on linear equilibria, we restrict attention to a two-parameter family of power law distributions of worker outside options and firm values, and we show that this family admits a unique linear equilibrium for any $\Omega \in [0,1]$.[6]

$$\begin{aligned} F(v) &= 1-(1-v)^r, r>0 \\ G(\theta) &= \theta^s, \qquad\qquad s>0 \end{aligned} \tag{6}$$

As r increases, v is on average lower and as s increases, θ is on average higher. Therefore, increasing r or s reduces the average surplus from employment.

A4 $\bar{w}$ is a linear function of v if there exists a worker i such that $\underline{w}_{i,1} \leq v$ according to equilibrium strategies, and $\underline{w}_{i,1}$ is a linear function of θ_i for any worker i such that there exists a firm type v with $\theta_i \leq \bar{w}$ according to equilibrium strategies.

We refer to a pure strategy PBE satisfying **A1-4** as a *linear equilibrium*. The following result extends work by Chatterjee and Samuelson (1983) who show existence of a linear equilibrium when F and G are uniform, corresponding to the case in which $r = s = 1$.

PROPOSITION 2: *For any pair of distributions within the family described in Equation 6 there exists a unique linear equilibrium for any* $\Omega \in (0,1)$.

The proof of this result is contained in Appendix A.

For the remainder of Section 2.3, we consider a *marketplace* characterized by $r > 0,\ s > 0$ and study the impact of pay transparency in the unique linear equilibrium.

Outcomes of interest are affected by supply and demand effects as transparency rises. Workers initially offer premia over their outside options, $\underline{w}_{i,1} - \theta_i \geq 0$. Similarly, the firm sets a markdown below its value for labor, $v - \bar{w} \geq 0$. We show that both $\bar{w}$ and $\underline{w}_{i,1}$ are decreasing in the level of transparency: with increased transparency the firm reduces the highest wage offer

[6]The approach of making parametric assumptions to ensure linear equilibrium is common. One recent example on CEO pay is Edmans et al. (2012). Power law distributions are commonly observed in economic situations such as ours, including worker incomes and firm productivities. See Gabaix (2009, 2016) for details. We note that these distributional restrictions are unnecessary for our analysis if one is only interested in comparing $\Omega = 0$ to $\Omega = 1$ (i.e. full secrecy with $k = 0$ to full transparency and renegotiation).

it is willing to accept in order to mitigate information spillovers across workers (which we call the **demand effect**), and workers make more conservative initial offers as they anticipate they will be able to risklessly renegotiate and receive $\bar{w}$ in short order (which we call the **supply effect**). In an abuse of notation, let $\bar{w}_\Omega(\cdot)$ denote the maximum wage function of the firm and $\underline{w}_{i,1,\Omega}(\cdot)$ the initial offer function of worker i for given de facto firm bargaining power Ω. The following result formalizes these effects.

PROPOSITION 3: *Consider the unique linear equilibrium given the family of distributions in Equation 6. $\bar{w}_\Omega(v)$ and $\underline{w}_{i,1,\Omega}(\theta_i)$ are weakly decreasing functions of Ω for all v and θ_i. As $\Omega \to 0$, $\bar{w}_\Omega(v) \to v$ for all v. As $\Omega \to 1$, $\underline{w}_{i,1,\Omega}(\theta_i) \to \theta_i$ for all θ_i.*

The proof of this result is contained in Appendix A.

Prediction 1: Transparency lowers average wages. When transparency increases, the demand and supply effects both place downward pressure on wages. The demand effect causes the firm to reduce its maximum wage, similar to the pricing strategy of a monopsonist. This restricts the extensive margin of labor (the proportion of workers it hires) and increases the intensive margin (profit per worker hired). Simultaneously, the supply effect reduces worker initial offers. The decline in average wages results in higher expected profit for the firm, and lower average worker surplus.

Although raising transparency increases the share of workers who receive wage $\bar{w}$, it lowers both $\underline{w}_{i,1}$ and $\bar{w}$ in equilibrium. The overall effect is to shift de facto bargaining power to the firm, benefiting the firm at the expense of workers. For clear intuition, consider the extreme cases of full privacy ($\tau\rho = 0$, $k = 0$) and full transparency ($\tau\rho = 1$). In the former, each worker makes a single TIOLI offer to the firm as no worker ever learns the wages of her coworkers, and hence, no worker will ever renegotiate. In the latter, each worker learns the wages of others within the firm and renegotiates. Therefore, every employed worker will demand and receive exactly $\bar{w}$, which is equivalent to the firm making a single TIOLI offer to all workers. The main result of Williams (1987) implies that each party prefers to be the one making the once-and-for-all offer to the other, as that party maximizes their expected surplus.

THEOREM 1: *Consider the unique linear equilibrium given the family of distributions in Equation 6. In expectation (over firm types), the equilibrium profit of the firm is strictly increasing in Ω, and the average equilibrium surplus of workers and average wages conditional on employment are strictly decreasing in Ω.*

The proof of this result is contained in Appendix A.

Prediction 2: Partial transparency maximizes employment when workers have sufficiently high individual bargaining power. When transparency increases, raising the de facto firm bargaining power from Ω' to Ω'', the demand effect lowers the equilibrium level of employment while the supply effect raises it. $\bar{w}_{\Omega''}(v) \leq \bar{w}_{\Omega'}(v)$ for all v implies there are fewer workers with $\theta_i \leq \bar{w}_{\Omega''}(v)$, and therefore fewer workers who are eligible for employment. Meanwhile, $\underline{w}_{i,1,\Omega''}(\theta) \leq \underline{w}_{i,1,\Omega'}(\theta)$ for all θ, implying that fewer workers over-negotiate by initially offering $\underline{w}_{i,1,\Omega''}(\theta) > \bar{w}_{\Omega'}(v)$. The primary cause of unemployment when Ω is low is workers act too much like monopolists in initial negotiations, and when Ω is high, the firm acts too much like a monopsonist. We show that employment is single-peaked in the level of transparency; similarly to the surplus-increasing effect of equal bargaining power studied in Loertscher and Marx (2022), a more even split of the de facto bargaining power is employment maximizing in our model, and either full privacy or full transparency is employment minimizing.

THEOREM 2: *Consider the unique linear equilibrium given the family of distributions in Equation 6. The expected proportion of workers hired in equilibrium is concave in Ω and is maximized at $\Omega^* = \frac{r+1}{r+s+2}$. Moreover, the ex-post employment level is submodular in v and Ω for the set of firm types that hire a positive measure of workers.*

The proof of this result is contained in Appendix A.

One implication that is useful for our empirical analysis is the connection between the employment level and the composition of the workforce. For any pair (Ω, v) there exists a cutoff $\theta(\Omega, v)$ such that a worker i is employed by the firm (i.e. $i \in I_2$) if and only if $\theta_i \leq \theta(\Omega, v)$. Therefore, if the level of employment is the same under Ω'' and Ω' with $\Omega'' > \Omega'$, then it must be the case that $\theta(\Omega', v) = \theta(\Omega'', v)$. In other words, marginally employed workers always have higher outside options than inframarginal workers. Therefore, if employment rises (falls, remains constant) in transparency, high outside option workers join (leave, do not transition in or out of) the workforce.

We offer a comment on social surplus: due to the cutoff structure of employment, the ex-post maximizer of the employment level also maximizes ex-post social surplus. Because each employed worker earns a wage weakly greater than her outside option, in equilibrium almost every employed worker increases social surplus by $v - \theta_i > 0$, implying that social surplus is strictly increasing in the level of employment. Therefore, Ω'' results in increased ex-post social surplus compared to Ω' if and only if v is below some threshold, due to the submodularity of employment in v and Ω.

Prediction 3: Transparency's effects are muted when workers have low individual bargaining power. Consider two bargaining weights $k_H > k_L$ and two levels of transparency $\tau_H > \tau_L$. For any fixed $\rho > 0$, take the four resulting combinations of de facto bargaining power from Equation 3: $\Omega_{LL} = k_L + (1-k_L)\tau_L\rho$, $\Omega_{LH} = k_L + (1-k_L)\tau_H\rho$, $\Omega_{HL} = k_H + (1-k_H)\tau_L\rho$, and $\Omega_{HH} = k_H + (1-k_H)\tau_H\rho$. Remark 1 implies that $\Omega_{LH} - \Omega_{LL} > \Omega_{HH} - \Omega_{HL} \geq 0$; de facto firm bargaining power is more responsive to an increase in transparency under k_L, than k_H. Combining this with Theorem 1 yields the following conclusion:

COROLLARY 1: *Consider the unique linear equilibrium given the family of distributions in Equation 6. For any $\rho > 0$, an increase in transparency from τ' to $\tau'' > \tau'$ will result in smaller declines in expected average wages the larger is k.*

If workers have no bargaining power ($k = 1$), transparency will have no effect on the equilibrium outcome. Note that our parameter k captures *individual* bargaining power, not overall worker bargaining power. In the Online Supplement we formally model how collective bargaining agreements may similarly strip workers of *individual* bargaining power even if (collectively) workers have high bargaining power. We show that the level of pay transparency will have no effect on average wages in our collective bargaining model, analogous to the case in our base model where $k = 1$.

Prediction 4: Transparency compresses wages within marketplaces, but not necessarily across marketplaces. To understand the effects of transparency on compression within a marketplace, we consider the *relative* effect of transparency on different workers. To fix intuition, assume $k = 0$ and $\rho > 0$, and take two levels of transparency τ' and τ'', where $\tau' < \tau''$, corresponding to Ω' and Ω'', respectively, where $\Omega' < \Omega''$. Consider the difference in earnings of two workers i and j, with outside options $\theta_i > \theta_j$ who are hired under both Ω' and Ω''. There are two effects when moving from Ω' to Ω''. First, the supply effect incentivizes both workers to reduce initial wage offers. We find that in equilibrium, since j has a lower outside option than i, j reduces her initial offer more than i, increasing the initial wage gap between i and j ($\underline{w}_{i,1,\Omega''} - \underline{w}_{j,1,\Omega''} > \underline{w}_{i,1,\Omega'} - \underline{w}_{j,1,\Omega'}$ for $\Omega'' > \Omega'$). Second, higher transparency increases the probability that both workers renegotiate, reducing dispersion of their earnings as $\bar{w}_\Omega - \underline{w}_{j,1,\Omega} > \bar{w}_\Omega - \underline{w}_{i,1,\Omega}$ for all $\Omega < 1$. The compressing effect of renegotiation overwhelms the increased dispersion from the supply effect, leading to more equal wages between i and j. The following statement formalizes this point for any k.

THEOREM 3: *Consider the unique linear equilibrium given the family of distributions in Equation 6. Let $\theta_i > \theta_j$, $\Omega'' > \Omega'$, and fix any $v \in [0,1]$. For almost all workers i and j hired in equilibrium under both Ω' and Ω'', the difference in expected wages between i and j is smaller under Ω'' than Ω', and converges to zero as $\Omega'' \to 1$.*

The proof of this result is contained in Appendix A.

Theorem 3 can also be used to study transparency's effect on wages across groups of workers *within* a marketplace. In the Online Supplement, we derive a condition under which transparency closes the gender pay gap. However, Theorem 3 cannot be applied to workers (or groups of workers) *across* marketplaces. These effects do not aggregate across marketplaces because parameters r and s mediate the degree of wage compression within each marketplace. For example, suppose that in the market for nurses s is very large (i.e. workers typically have high reservation wages) and r is large (i.e. a firm typically has low marginal revenue product from labor), while in the market for miners s is small and r is large. We expect transparency to compress wages within each marketplace. However, under these parameterizations, the scope for wage reduction is smaller among the workers who start out with higher wages, nurses. As a result, when aggregating these two markets, we could observe greater dispersion in wages under higher transparency.

2.4. *Endogenous transparency and the role of legislation*

We have thus far assumed transparency is exogenously set at a common level for all firm types. In reality, a firm may have the ability to select its own level of transparency. In Appendix B, we discuss a game in which the firm selects the level of transparency after observing its value v. The firm's selected level of transparency is unobserved by workers.[7]

In equilibrium, no firm type will pick a level of transparency that is higher than the minimum level allowed by law, because unobservability of the selected level of transparency removes commitment power a firm obtains from higher transparency, as in Bagwell (1995). The implication of this result is that a law raising the minimum-allowable transparency from $\underline{\tau}$ to $\underline{\tau}'$ will have the same effect as increasing the exogenous level of transparency from $\underline{\tau}$ to $\underline{\tau}'$ in our base model. In Section 3.1 we present evidence of the impact of U.S. state laws which prohibit employers from retaliating against workers who discuss their own wages with peers, or inquire as to the wages of coworkers.

[7]Due to "cheap talk" forces, any non-binding initial announcements by the firm regarding its level of transparency will not be credible. While firms may benefit from making an announcement of high transparency in $t = 1$ to secure the lowest wage offers, in $t = 2$ all firms types have an incentive to break their promise in order to avoid costly renegotiations. Because of this undercutting incentive, workers will ignore such announcements. One real-world company that "credibly" promises high levels of transparency is Buffer. Buffer has built a salary-formula app into its website that allows employees to "test" their own salaries and discover those of others as a function of observables. Of course, such technology may come with other costs (e.g. a rigid wage structure, and broadcasting wages to competing firms) which may make it infeasible for most firms.

3. EMPIRICAL EVIDENCE

We empirically test our model predictions that transparency lowers average wages (Theorem 1), and that the magnitude of wage reduction is increasing in the level of individual bargaining power (Corollary 1). Our model makes ambiguous predictions about the role of transparency on employment: Theorem 2 finds that increasing transparency can increase employment (in which case it also raises the average outside options of employed workers) or it can decrease employment (in which case it also lowers the outside options of employed workers). We investigate this question empirically.

3.1. *"Right of Workers to Talk" (ROWTT) U.S. State Laws*

We study the enactment of legislation setting a minimum level of transparency through strong protections for coworkers who discuss pay with each other, which we refer to as "Right of Workers to Talk" (ROWTT) laws. As early as 1935, a clause in the National Labor Relations Act (NLRA) established worker rights to discuss pay, a central part of labor organizing; however, these protections were described in very general terms–"protecting concerted activity"– and violators did not face punitive damages, which led to the critique of the NLRA as a "toothless tiger" (Green, 2014). Stansbury (2021) finds the NLRA did not create sufficient incentives for firms to comply. More recently, individual U.S. states have enacted ROWTT laws, purportedly to combat discriminatory pay. While ROWTT laws vary in their precise language, all legislation included in our analysis protect the right of workers to disclose their own salary and inquire about the salaries of others, and apply to all workers, except HR representatives in certain cases. All ROWTT laws studied impose financial penalties on employers who violate the provisions.

In 2016, Executive Order 13665 came into effect, extending ROWTT protections to employees of federal contractors (Federal Register, 2015), which collectively employ roughly 25% of U.S. workers.[8] We follow Donohue III and Heckman (1991) and use neither event data nor outcome data after 2016, due to the complementary federal policy. There are 13 state ROWTTs enacted in our study window (2000-2016), spanning the West, Northeast, Mid-Atlantic, and Midwest regions of the country. In Figure 1, we provide a timeline and geographical depiction of the enactment of each state law. Additional information about the collection of data on ROWTT polices is included in the Online Supplement.

3.1.1. *Transparency laws' effect on perceived rights to discuss pay*

Our theory defines transparency as the likelihood that employees learn about the pay of their coworkers. Ideally we would have a direct measure of how communication about pay between coworkers changed after ROWTT protections were enacted. While we cannot directly observe this, we gained access to a repeated survey conducted by the Institute for Women's Policy Research, along with Jake Rosenfeld at Washington University in St. Louis. The two waves of this survey (2010, 2017/8) included a question about whether the employer prohibited discussions about pay, offering two snapshots of the share of U.S. workers in the private sector who believe their employers prohibited discussions with other co-workers about pay.[9] Between these two waves, 11 states enacted ROWTT. Figure 2 illustrates the share of employees who claimed

[8]See https://uhr.umd.edu/wp-content/uploads/Workplace_Rights_JRF_QA_508c.pdf for a discussion on the share of workers employed by federal contractors.

[9]See Hegewisch et al. (2011) and Sun et al. (2021) for details about these survey waves. In the first survey, respondents with jobs were asked, "In some workplaces, information on co-worker wages and salaries is publically [*sic*] available, while in others a policy may state that wage and salary information is private, and that employees

FIGURE 1.—Year Right of Workers to Talk (ROWTT) Law Takes Effect

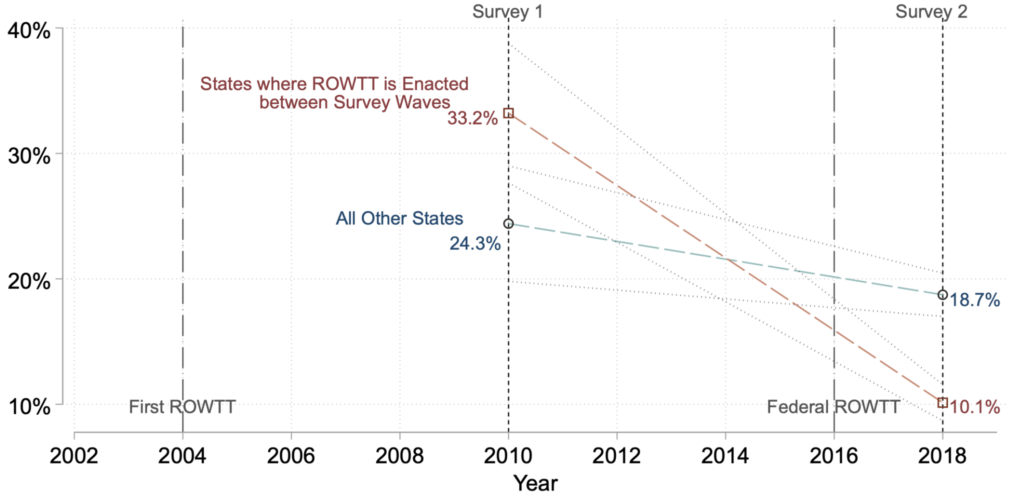
Note: This figure displays the set of states enacting Right of Workers to Talk (ROWTT) laws prior to and including 2016. Documentation of each state's ROWTT is provided below:

State	Effective Date	Documentation
Illinois	1/1/2004	820 ILCS 112
Maine	9/1/2009	Maine Revised Statues 26.628
Vermont	7/1/2013	Act 31, H.99
New Jersey	8/29/2013	P.L.2013, Chapter 154
Minnesota	10/5/2014	Chapter 239–H.F.No. 2536
New Hampshire	1/1/2015	RSA 275:38
Washington, DC	3/11/2015	D.C. Act 20-531
Connecticut	7/1/2015	Public Act No. 15-196
California	1/1/2016	California Labor Code Section 1197.5
Oregon	1/1/2016	ORS 659.A
New York	1/19/2016	NY Labor Law Section 194
Delaware	6/30/2016	Delaware Code, Section 711 Title 19
Maryland	10/1/2016	Annotated Code of Maryland Section 3–304.1

their employer prohibited them from discussing pay in each survey wave, across states that did and did not enact an ROWTT between waves. In states that enacted an ROWTT between survey waves, the share fell from 33.2% to 10.1%, while in the remaining states the share fell

can be punished for discussing pay with each other. Where would you say your workplace fits here?" We codify a response as "1" if the employee indicated "discussion is formally prohibited, and/or employees caught discussing wage and salary information could be punished" and "0" otherwise. In the second survey, respondents with jobs were asked, "Do you know the wages or salary levels of at least some of your co-workers?" We codify a response as "1" if the employee indicated "discussion of wage and salary information is formally prohibited, and/or employees caught discussing wage and salary information can be punished" and "0" otherwise.

FIGURE 2.—Percent of Employees Prohibited from Discussing Pay



Note: This figure displays the results of a survey tool administered in 2010 and 2017/2018 sampling a cross-section of U.S. employees. We restrict our sample to private sector employees. Survey 1 (2010) N=613; Survey 2 (2017/18) N=3,785. Data from the 2010 sample was collected by the Institute for Women's Policy Research, and results were published in Hegewisch et al. (2011). Data from the 2017/2018 Pay Secrecy Survey was collected by Dr. Jake Rosenfeld at the Washington University in St. Louis, in partnership with the survey research firm GfK Knowledge Networks (now Ipsos), and results were published in Sun et al. (2021). Responses in the early wave were weighted to match the distribution of responses across states in the later wave. Long-dashed lines offer a linear approximation of the evolution between survey waves. Light grey dotted lines represent 95% confidence intervals.

from 24.3% to 18.7%, indicating large and statistically significant differences between states with and without protections by 2017 (p-value$<$0.001), as well as significant differences in trajectories (p-value$<$0.001). Anecdotally, these additional protections from ROWTTs increased the sharing of salary information. The prevalence of salary spreadsheets shared among museum professionals, baristas, journalists, ad agency staffers, and public interest lawyers led to the newspaper headline: "The Google spreadsheet was the most powerful labor tool in 2019" (Reyes, 2019).

3.1.2. *Outcome data and sources*

We use data from the American Community Survey (ACS) to track wages and employment between 2000 and 2016. Starting in 2000, the ACS surveyed more than 3 million individuals annually, allowing us to identify 5,452,711 prime working-age individuals, those ages 25 to 54, in states that enact ROWTT laws during our window of analysis. The ACS contains information on hours worked per week, weeks worked per year, sector, occupation, industry, U.S. state of work, and demographic characteristics, in addition to annual earnings information with a cap equal to the 99.5th percentile in each state.[10] We complement the main ACS sample with a measure of union coverage from the Current Population Survey (CPS). We merge in unionization at the occupation level using the standardized 1990 occupation codes provided by Flood

[10]See https://usa.ipums.org/usa-action/variables/INCWAGE#codes_sec.

et al. (2020) and Ruggles et al. (2021). We provide summary statistics on our combined sample in Appendix Table C.1.

3.1.3. *Empirical Strategy*

We carry out a multi-period difference-in-difference design, often referred to as an event-study analysis. Our key identifying assumption is that the precise timing of ROWTT enactment during our 17-year window, among states that eventually pass ROWTT laws, is uncorrelated with underlying wage and employment dynamics. We empirically test this assumption by examining how wages, employment and additional labor market features evolve in each state leading up to the enactment of ROWTT. Under the presumption that the 13 states that pass ROWTT laws during our window of analysis are more similar along unobservables than states that do not pass these laws, our baseline specification excludes states that do not enact ROWTT during our window.

In our baseline specification, we also assume that the effect of transparency is homogeneous across cohorts. As a robustness check we relax our assumption about homogeneous treatment effects and estimate the effect of each cohort separately before calculating the weighted average to determine the average treatment effect.

Across all specifications we restrict our sample to prime working-age individuals employed full-time in the private sector. We define a worker as being employed full-time if they self-report that they typically work at least 35 hours per week and work for at least 48 weeks in the last year. We consider only workers in the private sector because local laws made salaries of many government workers public information before ROWTT protections were enacted state wide (see, for example, Mas, 2017). We discuss the effect of ROWTT in the public sector further in the Online Supplement.

We estimate the dynamic effect of ROWTT laws over the three years following their enactment. We also estimate the dynamic effect of ROWTT laws over the six years prior to their enactment as a test of whether enactment was precipitated by any underlying events that could co-move with our outcomes of interest, such as a rise in pro-business sentiment and related policies.

We estimate the following multi-period difference-in-differences specification:

$$\begin{aligned} y_{ist} =& \alpha_s + \sum_{\ell=-6}^{-2} \beta_\ell \mathbf{1}\{t - E_s = \ell\} + \sum_{\ell=0}^{3} \beta^*_\ell \mathbf{1}\{t - E_s = \ell\}\mathbf{1}\{E_s \leq 2013\} + \\ & \gamma \mathbf{1}\{t - E_s < -6\} + \delta \mathbf{1}\{t - E_s > 3\} + \boldsymbol{\lambda} \boldsymbol{X}_{ist} + \epsilon_{ist} \end{aligned} \tag{7}$$

where i indexes individuals, s indexes state of employment, and t indexes year. In our main specification, y_{ist} is the logarithm of annual wage income or an indicator for worker i's full-time employment status in the private sector. α_s is a state fixed effect and E_s is the year when state s enacts the ROWTT law. Thus, $t - E_s$ indexes years relative to the event. $\ell = -1$ is the omitted reference period, γ and δ are indicators for periods outside the event window. $\boldsymbol{X}_{ist}$ is a vector of controls that include age (quadratic), education, year-by-industry (NAICS 3-digit) and year-by-occupation (SOC 3-digit) indicators. We allow for interactions between available demographic characteristics, namely marital status, race and gender, and we allow region-by-industry effects to differ by gender. In our baseline specification, we report the results from a balanced composition of states in the years following the enactment of the law. We implement this through the addition of an indicator equal to 1 for states with events through 2013 and zero for those states with events after 2013. Standard errors are two-way clustered by state and by year to allow for both serial correlation within states over time, and cross-sectional correlation across states within a given year. We additionally report p-values using wild cluster bootstrap with randomization inference (WBRI) proposed by MacKinnon and Webb (2017), as cluster robust standard errors may overreject when the number of clusters is small. We provide additional details on this specification in the Online Supplement.

In a series of robustness tests we include year-by-Census-division fixed effects α_{tr}, and we weight our sample to estimate a counterfactual where the composition of workers in each education-gender-state cell remains fixed throughout the post period.[11] To relax our assumption of homogeneous treatment effects across cohorts, we allow treatment effects to vary depending on the year ROWTT is enacted, and calculate the weighted average of each year-specific treatment effect. We implement this using the interaction-weighted estimator, first proposed by Gibbons et al. (2019) and refined by Sun and Abraham (2020). This estimator is designed to recover average treatment effects even in the presence of underlying heterogeneity across years. We include the full specification and estimation details in the Online Supplement.

We estimate the heterogeneous effects of ROWTT separately for workers who have a relatively high versus low degree of individual bargaining power. We identify the two univariate factors closely associated with TIOLI job offers, our theoretical marker of the lowest level of individual bargaining power. Those with 4-year college degrees are two-thirds as likely to receive TIOLI offers as those without, and non-union members are less than half as likely to receive TIOLI offers as unionized workers (Hall and Krueger, 2012, Table 3). Equation 8 includes additional interaction terms to test the effect of low individual worker bargaining power ("low BP"), proxied by whether the individual does not have a 4-year college degree, and by whether the individual's occupation has above the median unionization rate of 7%.

$$\begin{aligned} y_{ist} =& \alpha_s + \sum_{\ell=-6}^{-2} \beta_\ell \mathbf{1}\{t-E_s=\ell\} + \sum_{\ell=-6}^{-2} \beta'_\ell \mathbf{1}\{t-E_s=\ell\}\mathbf{1}\{\text{low BP}\}+ \\ & \sum_{\ell=0}^{3} \beta_\ell \mathbf{1}\{t-E_s=w\}\mathbf{1}\{E_s \le 2013\} + \sum_{\ell=0}^{3} \beta'_\ell \mathbf{1}\{t-E_s=w\}\mathbf{1}\{E_s \le 2013\}\mathbf{1}\{\text{low BP}\}+ \\ & \gamma \mathbf{1}\{t-E_s<-6\} + \gamma' \mathbf{1}\{t-E_s<-6\}\mathbf{1}\{\text{low BP}\}+ \\ & \delta \mathbf{1}\{t-E_s>3\} + \delta' \mathbf{1}\{t-E_s>3\}\mathbf{1}\{\text{low BP}\}+ \\ & \boldsymbol{\lambda} \boldsymbol{X}_{ist} + \epsilon_{ist} \end{aligned} \tag{8}$$

3.1.4. *Results*

Figure 3 graphically presents our estimates of β_ℓ from Equation 7. The event-study graph shows the evolution of log wages (Panel A) and private sector employment (Panel B) in each of the six years leading up to the enactment of ROWTT and three years after enactment. The year before the event (-1) corresponds to the omitted category, and thus the corresponding coefficient is always zero by construction. The range along the y-axis has been set to approximately +/- 1 standard deviation in average wages over time, within state.

Coefficient estimates in Figure 3, Panel A represent wage differences relative to the period prior to ROWTT enactment, and calendar-year fixed effects absorb time trends. As a result, a

[11] We take the year before the law is enacted as the reference year and estimate the educational distribution of each state separately for men and women. Within each state, we then reweight the sample in every other year to match the education-by-gender distribution in the reference year.

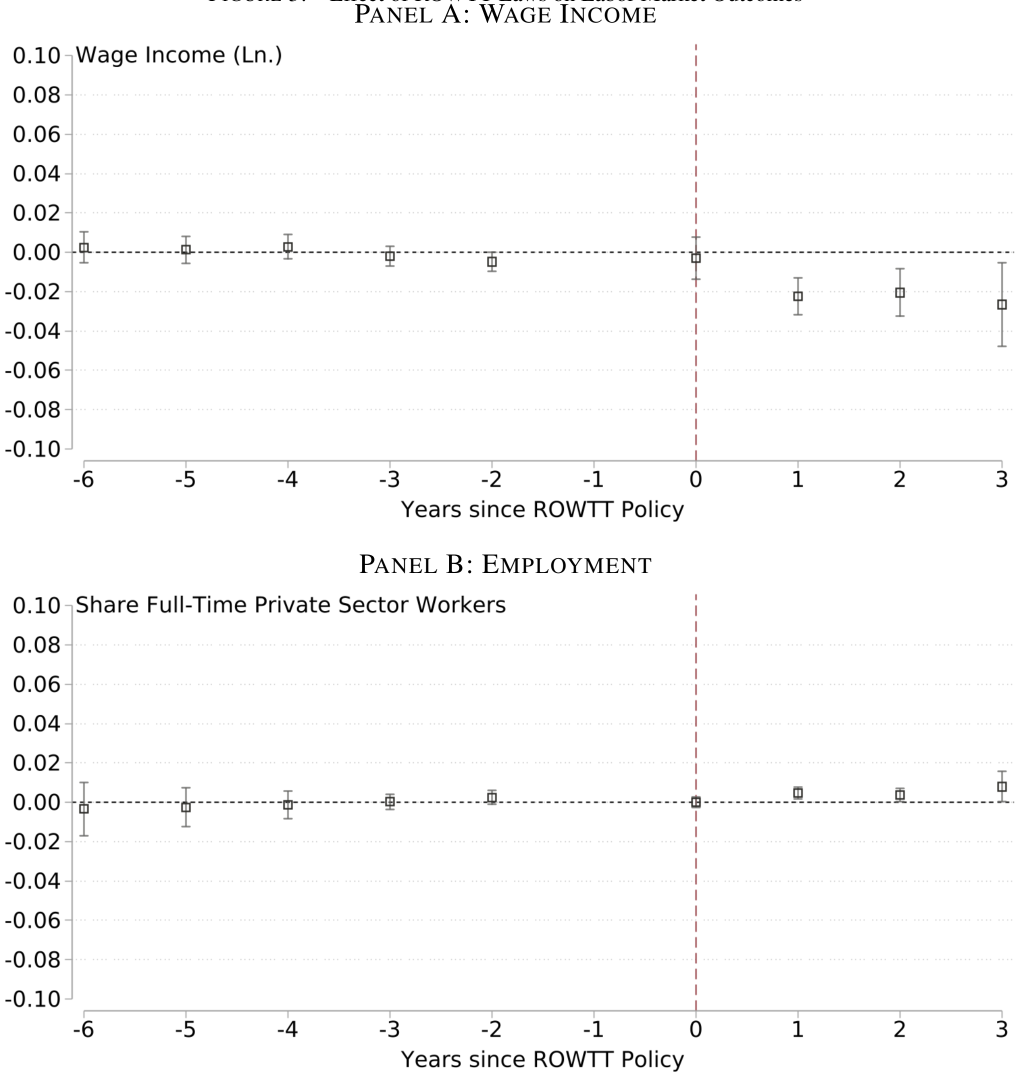
coefficient estimate of zero does not imply that wages remain stagnant in nominal terms; rather, it indicates similar growth rates of wages in states leading up to the enactment of ROWTT. In the six years leading up to the enactment of ROWTT, coefficients are precisely estimated and statistically indistinguishable from zero, suggesting that our assumption of parallel trends in wages holds.

By contrast to the period before enactment, the post-ROWTT evolution of wages diverges from the wage path of states that have yet to enact ROWTT. One year after enactment, wages are 2.2% lower (p-value<0.001) or roughly a fifth of a standard deviation in state-level average wages over the years. By three years after enactment, wages are 2.7% lower (p-value=0.019) than the period prior to enactment. Over the entire post-treatment period, including the year of enactment, wages decline by 1.8% (p-value=0.003). The decline in real wages we observe is consistent with a slowing of nominal wage growth after enactment of ROWTT as nominal wages rise on average by approximately 2.8% per year from 2004-2016 (SSA, 2021).

In Table I, we report the results of alternative specifications to our baseline model of wage effects. The stability of estimates across specifications corroborate the baseline estimate of the wage decline. In Col. 1, we present our baseline results, the multiperiod difference-in-differences estimator with a fixed composition of states in the post period. In Col. 2, we drop our restriction for a balanced composition of states and allow all cohorts (2004-2016) to contribute to all periods for which the data are available. The post event coefficients exhibit nearly identical results as our baseline, and average -1.6% (p-value=0.019) in the post-treatment period. In Col. 3 we include region-by-year fixed effects using detailed Census divisions, effectively restricting comparisons between states to neighboring states. The average post-treatment effect is -1.7% (p-value = 0.079). In Col. 4, we re-weight observations to maintain a fixed composition of workers within education-by-gender cells over time, illuminating whether the exit or entry of different types of workers drives wage reductions. The average post-treatment effect remains largely unchanged at -1.6% (p-value = 0.003). Finally, we compute the Sun-Abraham interaction-weighted estimator which relaxes our assumption of homogeneous treatment effects across cohorts. The average post-treatment point estimate is -2.3% (p-value<0.001), suggesting a slightly larger decline in wages but still statistically indistinguishable from our baseline estimate. Across all specifications, wages appear to follow parallel trajectories leading up to ROWTT enactment. For each of these specifications, we report the p-values associated with our average treatment effect, taking the mean post-treatment effect and subtracting the mean pre-treatment effect. WBRI p-values broadly confirm statistical significance of our treatment effects, but offer a more conservative account of the precision achieved across our specifications. The WBRI results are presented graphically in the Online Supplement.

Wage declines could partially stem from a change in employment resulting from ROWTT. If high-paid workers disproportionately leave the private sector after ROWTT, or if low-paid workers disproportionately join the private sector, average wages would fall even if no wages are renegotiated. We present estimates of the effect of ROWTT on private sector employment in Panel B of Figure 3. We plot estimated coefficients from the same event study specification as Panel A, replacing our dependent variable with an indicator for full-time employment in the private sector. The range of our y-axis is set to match Panel A, +/- 1 standard deviation in average wages over time, to visually aid comparison in the assessment of employment's role in wage changes. Our point estimates suggest that employment in the private sector remains constant leading up to ROWTT enactment, and subsequently rises modestly with an average treatment effect of 0.5% (p-value =0.108). Table II reveals that these point estimates are stable across specifications, and only marginally statistically distinguishable from zero. When we apply the wild cluster bootstrap with randomization inference, we cannot reject that ROWTT laws have zero effect on private sector employment. Nonetheless, an effect of 0.5% would

FIGURE 3.—Effect of ROWTT Laws on Labor Market Outcomes



Note: In this figure, we present our baseline multiperiod difference-in-difference estimates. In this baseline specification, we report the results from a balanced composition of states following the enactment of the law. Thus, we estimate the dynamic post period effects for states with events through 2013 separately and report these in periods 0 to +3. See Equation 7 for more information on this specification. The standard deviation of the state-level mean from 2000 to 2016 is 0.103 for the natural logarithm of wage income and 0.017 for the share of full-time private sector workers.

represent a 0.29 standard deviation change in private sector employment within state over this period. For that reason, we take further steps to bound the potential role of composition changes in lowering average wages.

TABLE I

DYNAMIC EFFECT ESTIMATES: WAGE INCOME

	Balanced	Unbalanced	Add Reg. $\times$ Yr. FE	Fix Ed. $\times$ Sex Dist.	Sun-Abraham IW Estimator
Mean Pre-Treatment Estimate	-0.000	0.000	-0.002	0.001	-0.005
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)
Dynamic Post Treatment Effect Estimates					
$t = 0$	-0.003	-0.005	-0.003	-0.001	-0.018
	(0.005)	(0.003)	(0.006)	(0.004)	(0.001)
$t = 1$	-0.022	-0.017	-0.020	-0.019	-0.023
	(0.004)	(0.006)	(0.007)	(0.004)	(0.002)
$t = 2$	-0.021	-0.015	-0.020	-0.019	-0.022
	(0.006)	(0.008)	(0.008)	(0.004)	(0.002)
$t = 3$	-0.027	-0.027	-0.024	-0.025	-0.029
	(0.010)	(0.012)	(0.015)	(0.009)	(0.004)
Mean Effect, t $\geq$ 0	-0.018	-0.016	-0.017	-0.016	-0.023
	(0.005)	(0.006)	(0.009)	(0.004)	(0.002)
Mean Difference: Post $-$ Pre	-0.018	-0.016	-0.014	-0.017	-0.018
	(0.005)	(0.005)	(0.010)	(0.004)	(0.001)
P-Value (CRVE)	0.002	0.011	0.170	0.002	$<$ 0.001
P-Value (WBRI-β)	0.022	0.020	0.105	0.025	0.052
Balanced Post-Period	Yes	No	Yes	Yes	Yes
Year-by-Region FE	No	No	Yes	No	No
Fix Gender-by-Ed. Composition	No	No	No	Yes	No

Note: Obs=2,341,955; Clusters: 13 states $\times$ 17 years = 221. The mean of the outcome is 10.77 (0.10 standard deviation). In Cols. 1-4, we use the standard multiperiod DID estimator to recover the dynamic effect of state-level ROWTT legislation on wage income. We restrict the sample to workers in full-time private sector employment. In Col. 1, we present the baseline model, balancing the set of states identifying the post-treatment dynamic effects by absorbing post-treatment dynamic effect estimates for cohorts with events after 2013. In Col. 2 our estimates includes all cohorts from 2000 to 2016. In Col. 3, we add year-by-region fixed effects to our baseline specification. We pool together the "West North Central" and "East North Central" divisions to form the "Midwest" Census region to ensure that no divisions contain only a single treated state. In Col. 4, we reweight our sample by education-by-gender within each state. We take the year before the law is enacted as the reference year and estimate the educational distribution of each state separately for men and women. Within each state, we then reweight the sample in each year to match the education-by-gender distribution in that state's reference year. In Col. 5, we use the Sun and Abraham (2020) interaction-weighted (IW) estimator to allow for heterogeneous treatment effects across cohorts. The IW estimator requires that the last-treated cohort be used as a control group in the absence of never-treated units. Thus, in this specification, the 2016 cohort does not contribute to dynamic effect estimates. We balance the post-treatment estimate by estimating the full set of cohort-specific dynamic effects, but excluding the 2014 and 2015 cohorts from the post treatment interaction-weighted estimates. In the final rows, we report p-values associated with the mean difference between the post-treatment effects and pre-treatment effects calculated using our two-way cluster-robust variance estimator (CRVE). Additionally, we report finite sample valid p-values testing the alternative null hypothesis of zero treatment effects using the wild cluster bootstrap with randomization inference (WBRI-β), described in the Online Supplement.

In the spirit of Lee (2009), we liberally bound the effect that additional workers could have on lowering average wages by assuming an employment shift equal to the upper 95% confidence interval of our estimates, and attributing the entirety of the employment shift to new workers who earn zero wages. Under these assumptions, employment changes account for less than one-third the adjustment to wages following ROWTT.[12] This finding is consistent with the re-weighting exercise we carry out in Table II, Col. 4 which finds that differential exit and entry

[12]Let ϵ be the employment share increase and let A be the average wages prior to ROWTT. We bound the (log point) decrease in wages due to employment increases by $\frac{\frac{A+0\cdot\epsilon}{(1+\epsilon)}-A}{A} = -\frac{\epsilon}{1+\epsilon}$. Table II Col. 1 reports the upper 95% confidence interval for employment share increases to be 0.006. Picking $\epsilon = 0.006$ to maximize the absolute value of this bound leads to an effect size of -0.0059 log points, which accounts for less than one-third of the effect size

TABLE II

DYNAMIC EFFECT ESTIMATES: EMPLOYMENT

	Balanced	Unbalanced	Add Reg. × Yr. FE	Fix Ed. × Sex Dist.	Sun-Abraham IW Estimator
Mean Pre-Treatment Estimate	-0.001	-0.001	-0.000	-0.003	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)
Dynamic Post Treatment Effect Estimates					
$t = 0$	0.000	-0.000	0.000	-0.001	0.000
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
$t = 1$	0.005	0.002	0.002	0.004	0.003
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
$t = 2$	0.004	0.005	0.004	0.004	0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)
$t = 3$	0.008	0.007	0.006	0.007	0.007
	(0.004)	(0.004)	(0.003)	(0.003)	(0.001)
Mean Effect, t $\geq$ 0	0.004	0.004	0.003	0.004	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mean Difference: Post $-$ Pre	0.005	0.005	0.003	0.007	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)
P-Value (CRVE)	0.108	0.133	0.331	0.064	0.224
P-Value (WBRI-β)	0.168	0.148	0.210	0.111	0.726
Balanced Post-Period	Yes	No	Yes	Yes	Yes
Year-by-Region FE	No	No	Yes	No	No
Fix Gender-by-Educ. Composition	No	No	No	Yes	No

Note: Note: Obs=5,452,696; Clusters: 13 states $\times$ 17 years = 221. The mean of the outcome is 0.43 (0.02 standard deviation). In Cols. 1-4, we use the standard multiperiod DID estimator to recover the dynamic effect of state-level ROWTT legislation on the share of workers employed full-time in the private sector. In Col. 1, we present the baseline model, balancing the set of states identifying the post-treatment dynamic effects by absorbing post-treatment dynamic effect estimates for cohorts with events after 2013. In Col. 2 our estimates includes all cohorts from 2000 to 2016. In Col. 3, we add year-by-region fixed effects to our baseline specification. We pool together the "West North Central" and "East North Central" divisions to form the "Midwest" Census region to ensure that no divisions contain only a single treated state. In Col. 4, we reweight our sample by education-by-gender within each state. We take the year before the law is enacted as the reference year and estimate the educational distribution of each state separately for men and women. Within each state, we then reweight the sample in each year to match the education-by-gender distribution in that state's reference year. In Col. 5, we use the Sun and Abraham (2020) interaction-weighted (IW) estimator to allow for heterogeneous treatment effects across cohorts. The IW estimator requires that the last-treated cohort be used as a control group in the absence of never-treated units. Thus, in this specification, the 2016 cohort does not contribute to dynamic effect estimates. We balance the post-treatment estimate by estimating the full set of cohort-specific dynamic effects, but excluding the 2014 and 2015 cohorts from the post treatment interaction-weighted estimates. In the final rows, we report p-values associated with the mean difference between the post-treatment effects and pre-treatment effects calculated using our two-way cluster-robust variance estimator (CRVE). Additionally, we report finite sample valid p-values testing the alternative null hypothesis of zero treatment effects using the wild cluster bootstrap with randomization inference (WBRI-β), described in the Online Supplement.

of workers across gender-by-education cells do not drive wage declines. We conclude that composition changes do not account for the entirety of our estimated wage effects.[13]

In Figure 4, Panel A, we report the dynamic wage effects for private sector employees with, and without, a 4-year college degree separately, following Equation 8. In Panel B we plot the difference between the effects for those with and without a 4-year college degree. Leading up to the enactment of ROWTT, wages follow the same trajectory irrespective of educational

of ROWTT on wages. In summary, our exercise reveals that a 1 pp increase in employment can account for no more than a 1 pp decrease in average wages.

[13]In Section 2.3, we describe theoretical reasons why the observed composition changes would lead us to underestimate, rather than overestimate, the wage decline.

attainment. Starting the year that ROWTT laws are enacted, wages between these groups diverge. Among those without a college degree, wages stay relatively constant throughout the post-period, falling on average 1.0% (p-value=0.213) over the entire post period. By one year after enactment, wages fall by 3.1 pp more among those with a college degree than those without (p-value=0.108), and the gap persists. Over the post period, those with a college degree experience a wage decline on average of 3.2% (p-value=0.019). We show the same pattern across more granular education bins in the Online Supplement. There, we also show that the private sector employment of the different education groups follow the same trajectory following ROWTT enactment. These results offer suggestive evidence that individuals with higher education, and thus likely higher individual bargaining power, face steeper effects of pay transparency on wages.

In Figure 5, we report the wage effect for above- and below-median rates of unionization at the occupation level. In Figure 5 Panel A we plot the dynamic effects of ROWTT for occupations with above and below the median share of unionized workers, also estimated following Equation 8. In Panel B we plot the difference between the effects for occupations with low and high rates of unionization. Wages in high and low unionized occupations follow the same path until the year that ROWTT laws are enacted. Among relatively unionized occupations, wages fall by 1.4% (p-value=0.085) one year after enactment and remain at 1.5% (p-value=0.264) three years after enactment. For occupations with relatively low rates of unionization, wages decline nearly twice as much, an additional 1.8 pp (p-value=0.001) over the post-period window, and experience wage declines of 3.4% (p-value = 0.005) three years after enactment. In the Online Supplement we show the average post treatment effect gradually rises as unionization rates fall from 20% in the upper quartile down to 2% in the bottom quartile. There, we also show that employment trajectories do not diverge post ROWTT. These results suggest that collective bargaining agreements that reduce individual bargaining power also mitigate the effects of pay transparency on the bargaining position of workers.

When interpreting our heterogeneous treatment effects, it is important to consider alternative interpretations to the causal relationship we present. While our theory predicts a causal relationship, our empirical test does not rule out the possibility that individuals with a college degree are different from those without one in ways that affect wage negotiations, and mediate the impact of transparency, yet are unrelated to relative bargaining power. Similarly, occupations with higher rates of unionization are different along dimensions that could mute the effects of ROWTT but are orthogonal to individual bargaining power. In Section 3.2, we strengthen our empirical test of the role of bargaining power by exploring the relationship between unionization and transparency's effect across a wide range of labor markets, with distinct institutions for collective bargaining.

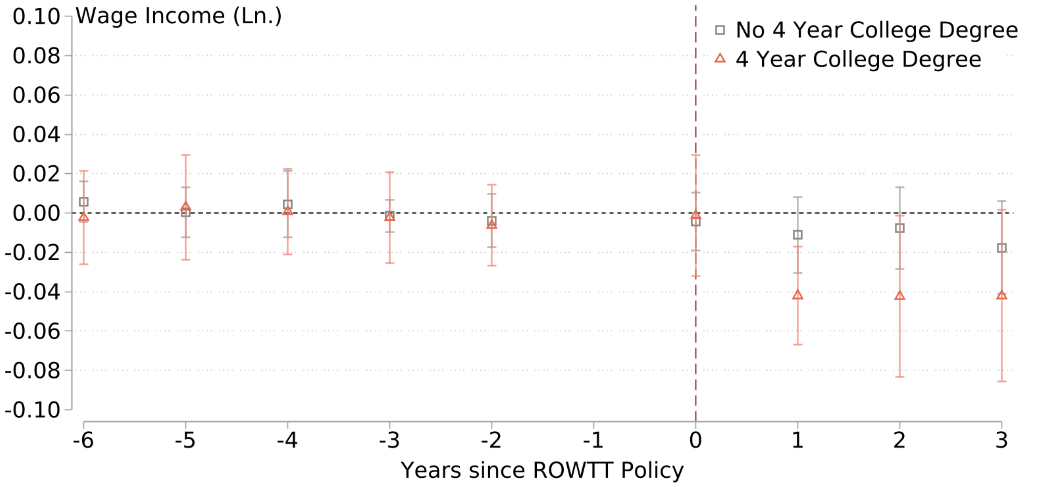
3.1.5. *Main Threats to Internal Validity*

Inherent in our empirical strategy are several assumptions. The first is that ROWTT laws are enacted in isolation; in other words, these laws are not coupled with additional legislation or timed around another noteworthy event. We have reason to believe this is the case. While nearly all ROWTT legislation are amendments to existing equal pay laws, in only four cases is there arguably related legislation enacted around the same time.[14] Our results are robust to excluding these four events.

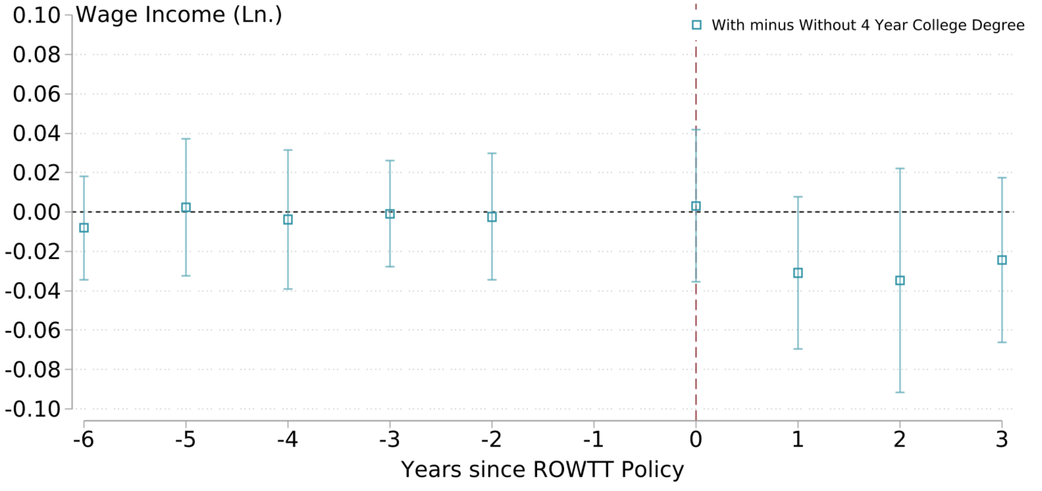
[14]VT enacts a new law about working mothers in the workplace, and new guidelines supporting flexible working arrangements. MN sets aside money for grants to create programs to hire women in different workplaces. NH creates additional anti-retaliation laws. DE creates new provisions and protections regarding reproductive health. Salary history bans, salary range posting laws and wage gap disclosure laws are not coupled with ROWTT laws nor are they enacted within several years of any ROWTT law that we study in the window 2004 to 2016.

FIGURE 4.—Heterogeneous Effects of ROWTT Laws on Wages, By Education

PANEL A: WAGE INCOME, WITH VS. WITHOUT COLLEGE EDUCATION



PANEL B: WAGE INCOME DIFFERENCE, WITH VS. WITHOUT COLLEGE EDUCATION

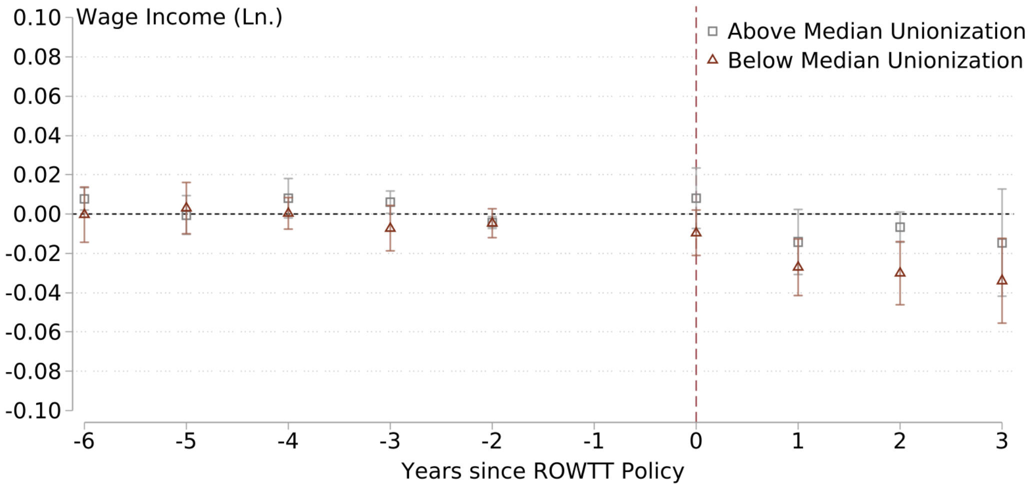


Note: In this figure, we present our baseline multi-period difference-in-difference estimates from a balanced composition of states following the enactment of the law. Thus, we estimate the dynamic post period effects for states with events through 2013 separately and report these in periods 0 to +3. See Equation 8 for more information on this specification. The standard deviation of the state-level mean from 2000 to 2016 is 0.103 for the natural logarithm of wage income and 0.017 for the share of full-time private sector workers. We use self-reported education from the ACS to classify workers as having a college degree or not.

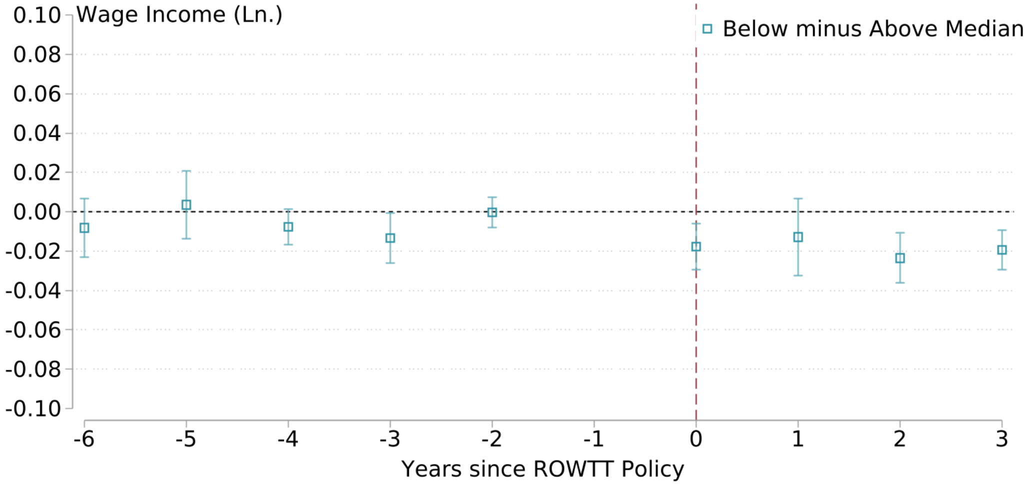
Relatedly, we assume the choice to enact ROWTT is not driven by changes that are already underway, in essence a story of reverse causality whereby declining wages leads to the enactment of ROWTT, rather than the other way around. Reverse causality is typically less of a concern when effects are discontinuous and occur after the law is enacted; nevertheless, we also collect facts about the motivation for the passage of ROWTT laws. More than three-quarters

FIGURE 5.—Heterogeneous Effects of ROWTT Laws on Wages, By Unionization

PANEL A: WAGE INCOME, BELOW- VS. ABOVE-MEDIAN UNIONIZATION RATES



PANEL B: WAGE INCOME DIFFERENCE, BELOW- VS. ABOVE-MEDIAN UNIONIZATION RATES



Note: In this figure, we present our baseline multi-period difference-in-difference estimates from a balanced composition of states following the enactment of the law. Thus, we estimate the dynamic post period effects for states with events through 2013 separately and report these in periods 0 to +3. See Equation 8 for more information on this specification. The standard deviation of the state-level mean from 2000 to 2016 is 0.103 for the natural logarithm of wage income and 0.017 for the share of full-time private sector workers. We use data from the Current Population Survey to estimate the share of workers covered by a union or collective bargaining agreement at the occupation level each year and split at the median occupation.

of the ROWTT laws refer to pay discrimination in the title or preamble describing the law (the partial equilibrium narrative), and nowhere is there mention of wage *levels*.

In theory a third factor could lead to both declining wages and the enactment of ROWTT. For instance, we could be detecting a rapid shift in sentiment in favor of businesses that, either through policies or atmosphere, effectively shifts bargaining power towards firms and simultaneously leads to the enactment of ROWTT. However, any positive co-movement of ROWTT enactment and pro-business policies are likely purely coincidental and not systematic across states because the public discourse about ROWTT has centered on the benefits of the partial effect of ROWTT, renegotiating higher pay for underpaid workers.

3.2. *Examining other Transparency Laws through a Bargaining Framework*

Our model predicts similar equilibrium labor market outcomes across a wide range of pay transparency policies, as we discuss in the Online Supplement. This allows us to extend our empirical analysis to include recent pay transparency policies evaluated in other contexts, including laws requiring employers to share information about wage gaps between men and women, as in Austria, the United Kingdom, and Denmark.

We compile results from studies that evaluate these policies to test the equilibrium effects. We refer to this empirical exercise as a meta-analysis, because we examine average wage declines across settings and combine the data from these studies in a mixed-effects meta-regression to test our comparative static prediction that wages decline less when unionization rates are higher.

3.2.1. *Criteria for selecting pay transparency studies*

We aim to include the universe of pay transparency studies, subject to certain criteria. First, the study must investigate a policy referred to as “pay transparency" or a related term. Second, the study must evaluate the effect of a pay transparency policy in a real-world labor market. Second, the study must evaluate the effect of pay transparency on the wages of all employees in that labor market. We include details on our study selection process and each selected study in the Online Supplement.

While we take steps to identify the universe of studies that meet these criteria, one concern with meta-analyses is that publication bias results in studies skewed toward finding a significant effect (Andrews and Kasy, 2019). In our case, this is a relatively minor concern. Overall wage levels are only a secondary outcome in all of these studies; one study (Mas, 2017) primarily focuses on wage compression between high- and low-paid workers, and the remainder focus first and foremost on the gender wage gap, consistent with the stated goal of pay transparency policies to close the wage gap between men and women and other minorities.

3.2.2. *Overview of studies & results extraction*

Our search results in eight independently-conducted papers. Seven of these papers each include one study (Bennedsen et al., 2019, Blundell, 2021, Böheim and Gust, 2021, Duchini et al., 2020, Gulyas et al., 2021, Mas, 2017, Obloj and Zenger, 2022), while Baker et al. (2021) contains two relevant studies, one based on unionized workers and one on non-unionized workers. In total, these papers evaluate six distinct pay transparency policies spanning five countries. In four of these studies, policies mandate disclosure of individual employee salaries, and in the remaining five, wage gaps between men and women.

We extract information about overall wage effects and labor market unionization from each study. We select the author’s preferred specification when clear, as is the case for six of the nine studies. When not specified, we select the specification closest to our theoretical framework, i.e. examining wage spillovers within position. For three of the nine studies, the authors do not report a single post-treatment effect. To minimize assumptions about the covariance between estimates, we do not aggregate over annual estimates when authors do not report a single post-treatment effect; rather, we choose the final period in the window reported.

All but three studies specifically report the effect of transparency policies on men's wages, and then provide the differential effect of the policy on women's wages. We impute the overall wage effect of transparency by weighing the changes in men's and women's wages by the share of men in the industry, and the pre-transparency ratio of female to male wages.

3.2.3. *Results*

In Figure 6, we graphically present the relationship between the share of the workforce covered by a collective bargaining agreement in each study (x-axis), and the estimated effect of the pay transparency policy on log wages (y-axis). For each study we include two points. The first is an effect size directly reported in the paper and refers to the effect of pay transparency on men's wages. For these point estimates we also plot the reported 95% confidence interval. We include a second point, lighter in color, to indicate our imputed estimate of transparency's effect on the overall population.

The results of these studies match our theoretical predictions. Observations generally fall below the x-axis, indicating a negative impact of pay transparency on wages (Theorem 1), and follow an upward-sloping line (Corollary 1), indicating that the effect on wages is smaller in magnitude as a higher share of the workforce has wages set by a collective bargaining agreement. The resulting slope on the effect of transparency on men's wages is 0.018 (p-value=0.008), implying that a 10 pp reduction in the share of workforce under a collective bargaining agreement is associated with a 0.18 pp larger decrease in men's wages following a transparency intervention. Studies with nearly full coverage by a collective bargaining agreement see no statistically significant change in wages following the transparency intervention. Our imputed point estimates for all workers (the lighter point) reveal a similar pattern.

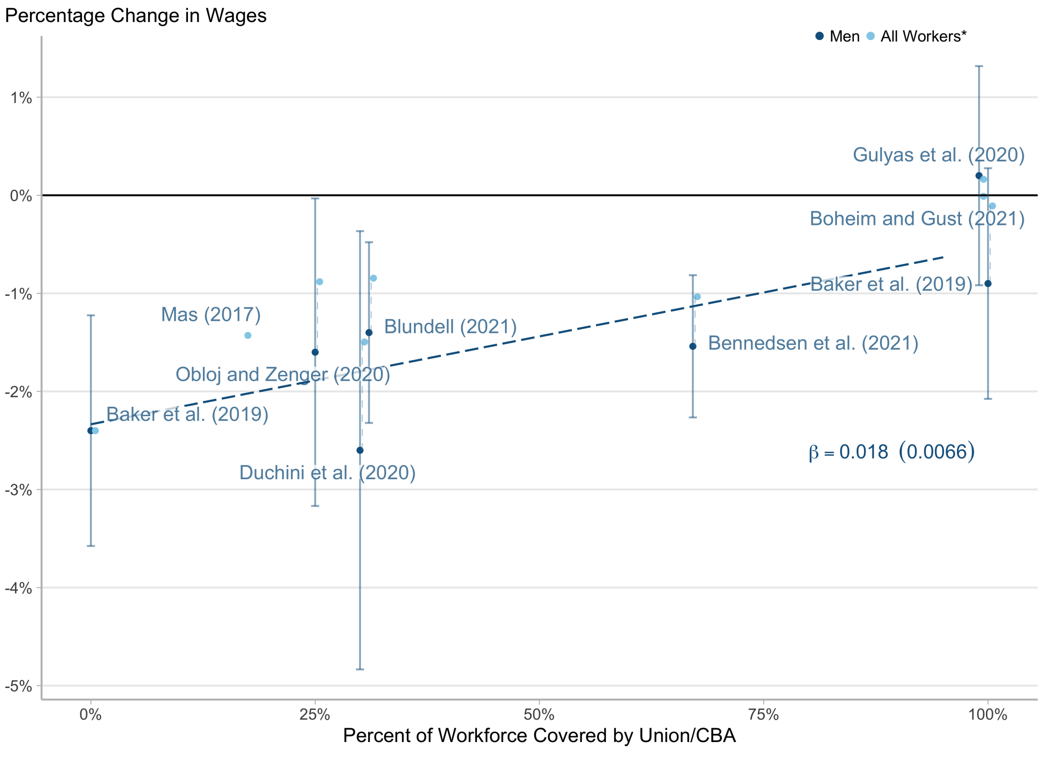
Finally, these prior studies provide empirical tests of our Theorem 3 and Corollary 2 (the latter can be found in the Online Supplement), stating that higher transparency leads to more equal wages within firm and thus, more equal wages between men and women. Each one of these studies includes panel observations at the level of the firm, allowing the authors to include firm fixed effects and track wages over time. Six studies find that wages between men and women converge, and the remaining three find no statistically significant change. We capture this pattern in Figure 6. The darker point estimates, reflecting men's wages only, generally fall below the lighter point estimates for the full population, showing wage declines are generally largest for male workers, whose wages have been shown to start out higher than women's.

4. CONCLUSION

Pay transparency has been in the political and popular spotlights as a way to combat pay discrimination by improving workers' ability to renegotiate low pay. We present a model of bargaining, under incomplete information, that corroborates the intuition that transparency leads to more equal pay between co-workers. However, we also find an unintended, and countervailing equilibrium effect of increasing pay transparency: workers' de facto bargaining power decreases as employers credibly refuse to pay high wages in order to avoid costly renegotiations with other workers.

Our model predicts that increasing transparency leads to lower wages. Wage declines in transparency can result from both changes to who is hired and direct changes to wages. In an

FIGURE 6.—Effect of Transparency on Wages by Individual Bargaining Power, Existing Studies



Note: In this figure, we graphically present estimates from the related literature. For the majority of studies, we plot two observations, one for the effect of transparency on the wages of men (dark blue series), and one for the imputed effect of transparency on the wages of all workers (light blue series). Mas (2017) presents the wage effects for “managers" and “non-managers" and we therefore present only an imputed observation for this study. The x-axis represents the share of workers covered by a union/collective bargaining agreement, and the y-axis the percentage change in wages. We report the estimated effect of the unionization rate on the impact of pay transparency recovered from a mixed-effects meta-regression model (Schwarzer, 2007, Viechtbauer, 2010). Since the estimates for all workers are imputed for some studies, we only report the meta-regression results for the male series for which standard errors are known and displayed (we do not include Mas (2017) because wage results for men are not reported, nor do we include Böheim and Gust (2021) because the authors' specifications show the change in male wages, not the natural logarithm of change; we display the imputed percent change in male wages by dividing the change in average male wages by the average male wage, as detailed in the Online Supplement).

empirical examination of the enactment of 13 U.S. state-level pay transparency laws between 2004 and 2016, we find that relative wages fall 2% in states that enact transparency reforms, predominantly due to direct wage adjustments. As predicted in our model, downward pressure on wages is especially pronounced in markets with high individual worker bargaining power, where workers would have otherwise been able to make high wage demands.

Our framework helps explain the wide array of results from prior evaluations of pay transparency policies. A meta-analysis of these studies reveals that wages typically decline following transparency laws, and by varying degrees. In line with our model predictions, wages decline sharply where workers have high individual bargaining power. These studies also confirm our prediction that pay transparency compresses wages among comparable workers in the same firm. In so doing, transparency may contribute to the emergence of stable firm wage effects, as studied in the literature on firm-level drivers of wage inequality and reviewed by Card et al. (2018).

Our model sheds light on why few firms adopt pay transparency in the absence of a government law. Firms face commitment issues in implementing pay transparency policies. After hiring a worker and setting initial wages, a firm finds it profitable to renege on promises of high pay transparency in order to minimize costly renegotiations. Empirically, this bears out: previous studies document that the majority of U.S. firms adopt limited levels of pay transparency (Hegewisch et al., 2011, McCarthy, 2018, Sun et al., 2021). Other barriers may also lead to low levels of transparency despite (or because of) its impact on wages, such as agency issues wherein a manager personally stands to lose from adopting transparency. In Appendix Remark 2, we additionally show that high value “superstar" firms may earn lower profits from additional pay transparency.

The central message of our paper is that the equilibrium effects of pay transparency may differ from its intended effects. Without an equilibrium response in a bargaining framework, we would expect wages to rise after transparency is introduced, as transparency’s partial effect of revealing pay disparities allows low-wage workers to negotiate higher pay. There are other equilibrium channels to consider which may also impact wage setting practices in the presence of pay transparency. First, high transparency could lead to public scrutiny, and demand for accountability (Mas, 2017). We would expect this channel to play a large role in public-sector jobs where wages are supported by tax dollars, and where wages are highly visible to the public. Second, transparency could lead workers to experience low morale, and reduce effort or quit their jobs upon learning peers make more money (Akerlof and Yellen, 1990, Card et al., 2012, Cullen and Perez-Truglia, 2022, Perez-Truglia, 2020, Breza et al., 2018, Dube et al., 2019, Cohn et al., 2014, Bracha et al., 2015). In the presence of morale concerns, we would expect an employer to equalize wages only if the productivity consequences from transparency were larger than the additional wage bill incurred, otherwise the employer would rationally allow pay differences to continue (Eliaz and Spiegler, 2013). In follow-up work, we discuss how bargaining forces may subsume morale concerns, leading wages to be equalized even when productivity consequences are small in comparison to the wage gap (Cullen and Pakzad-Hurson, 2022). Continued study of transparency laws, as policies evolve and spread to new labor markets, will be important to fully elucidate the roles of each mechanism.

APPENDIX A: PROOFS

This appendix contains the proofs of results omitted from the main text.

PROOF OF LEMMA 1: We begin by examining workers. From the proof of Proposition 1 presented in the main text, each worker $i \in I$ who matches with the firm with positive probability (i.e. $\theta_i \leq \bar{w}(1)$ negotiates at time $t = 1$ to solve:

$$\underline{w}_{i,1} \in \underset{w}{\operatorname{argmax}} \; \left[(k+(1-k)\rho\tau)\mathbb{E}\left(\bar{w}|\bar{w} \geq w\right) + (1-(k+(1-k)\rho\tau))w\right](1-\bar{F}(w)) + \theta_i \bar{F}(w) \tag{9}$$

where the first term represents the expected wage the worker receives, if matched with the firm: she receives $\bar{w}$ if $w_{i,1} = \bar{w}$, which happens with probability k, or if she observes the wages of her peers and has the ability to renegotiate, which happens with probability $\tau\rho$. Otherwise, she receives $\underline{w}_{i,1}$. The second term represents the earnings of the worker if she exceeds $\bar{w}$ with her initial offer, in which case she consumes her outside option. In what follows, let $\Omega := k + (1-k)\tau\rho$, i.e. Ω is the probability that an employed worker receives $\bar{w}$.

In a series of steps, we modify the objective function without affecting the maximizer.

$$\begin{aligned}
&\underline{w}_{i,1} \in \underset{w}{\operatorname{argmax}} \; \left[\Omega\mathbb{E}\left(\bar{w}|\bar{w} \geq w\right) + (1-\Omega)w\right](1-\bar{F}(w)) + \theta_i \bar{F}(w) \\
\iff &\underline{w}_{i,1} \in \underset{w}{\operatorname{argmax}} \; \left[\Omega\mathbb{E}\left(\bar{w}|\bar{w} \geq w\right) + (1-\Omega)w\right](1-\bar{F}(w)) + \theta_i \bar{F}(w) - \theta_i \\
\iff &\underline{w}_{i,1} \in \underset{w}{\operatorname{argmax}} \; \left[\Omega\mathbb{E}\left(\bar{w}|\bar{w} \geq w\right) + (1-\Omega)w - \theta_i\right](1-\bar{F}(w)) \\
\iff &\underline{w}_{i,1} \in \underset{w}{\operatorname{argmax}} \int_w^1 \left(\Omega x + (1-\Omega)\, w - \theta_i\right) \bar{f}(x) dx
\end{aligned} \tag{10}$$

where the first equivalence follows because subtracting θ_i from the objective function does not change the set of maximizers, and the last equivalence follows from Assumption **A3** which implies that $\underline{w}_{i,1}$ and $\bar{w}$ are absolutely continuous. This completes the argument for the worker objective function.

The argument for the firm objective function is similar, as each hired worker $i \in I_1$ with initial offer strictly less than $\bar{w}$ receives final wage $\bar{w}$ with probability Ω. *Q.E.D.*

PROOF OF REMARK 1: Recalling that $\Omega = k + (1-k)\tau\rho$, it is easy to see that $\Omega \in [0,1]$ for any $(k,\tau,\rho) \in [0,1]^3$. Also, for any $(k,\tau,\rho) \in (0,1)^3$, Ω is twice differentiable in all three variables.

Ω is increasing in k, τ, and ρ: $\frac{\partial\Omega}{\partial k} = 1 - \rho\tau \geq 0$ since $\rho\tau \leq 1$ (the inequality is strict unless $\rho = \tau = 1$). $\frac{\partial\Omega}{\partial\rho} = (1-k)\tau \geq 0$ since $\tau \leq 1$ and $k \geq 0$ (the inequality is strict unless $k = \tau = 0$). $\frac{\partial\Omega}{\partial\tau} = (1-k)\rho \geq 0$ since $\rho \leq 1$ and $k \geq 0$ (the inequality is strict unless $k = \rho = 0$).

Ω is submodular in τ and k: $\frac{\partial^2\Omega}{\partial\tau\partial k} = -\rho \leq 0$ since $\rho \geq 0$ (inequality is strict unless $\rho = 0$). Ω is supermodular in τ and ρ: $\frac{\partial^2\Omega}{\partial\tau\partial\rho} = 1 - k \geq 0$ since $k \leq 1$ (the inequality is strict unless $k = 1$). Ω is submodular in ρ and k: $\frac{\partial^2\Omega}{\partial\rho\partial k} = -\tau \leq 0$ since $\tau \geq 0$ (the inequality is strict unless $\tau = 0$). *Q.E.D.*

PROOF OF PROPOSITION 2: Let $\bar{w} = \beta(v)$ and let $\underline{w}_{i,1} = \gamma(\theta_i)$ for each i and assume that a linear equilibrium exists. Workers are hired at initial wages in some range $[a,h]$ where $0 \leq a \leq h \leq 1$. By the linearity hypothesis, it must be the case that

$$\bar{w} = \begin{cases} v & 0 \leq v < a \\ a + \frac{h-a}{1-a}(v-a) & a \leq v \leq 1 \end{cases}, \qquad \underline{w}_{i,1} = \begin{cases} a + \frac{h-a}{h}\theta_i & 0 \leq \theta_i \leq h \\ \theta_i & h < \theta_i \leq 1 \end{cases} \tag{11}$$

Furthermore, by definition $\bar{F}(x) = Pr(\beta(v) \leq x) = F(\beta^{-1}(x))$, and similarly $\bar{G}(x) = G(\gamma^{-1}(x))$. Inverting the functions in Equation 11 and plugging in to the distributions in Equation 6 yields that for all $a \leq x \leq h$

$$\bar{F}(x) = 1 - \left(1 - a - \tfrac{(x-a)(1-a)}{h-a}\right)^r, \quad \bar{G}(x) = \left(\tfrac{(x-a)h}{h-a}\right)^s \quad a \leq x \leq h \tag{12}$$

Equations 4 and 5 give another set of equations for $\gamma^{-1}(\cdot)$ and $\beta^{-1}(\cdot)$. Plugging these in to the distributions in Equation 6 yields that for all $a \leq x \leq h$

$$\bar{F}(x) = 1 - \left(1 - x - \Omega\tfrac{\bar{G}(x)}{\bar{g}(x)}\right)^r, \quad \bar{G}(x) = \left(x - (1-\Omega)\tfrac{1-\bar{F}(x)}{\bar{f}(x)}\right)^s \tag{13}$$

Solving Equations 12 and 13 simultaneously results in a unique solution in which

$$a = \tfrac{(1-\Omega)s}{(s+\Omega)r+(1-\Omega)s}, \quad h = \tfrac{(1-\Omega)s+rs}{(s+\Omega)r+(1-\Omega)s} \tag{14}$$

As $\bar{w}$ and $\underline{w}_{i,1}$ are pinned down by a and h due to linearity, there is a unique linear equilibrium. *Q.E.D.*

PROOF OF PROPOSITION 3: We first show $\bar{w}_\Omega(v)$ is strictly decreasing in Ω for all $v \in [a, 1]$. Using Equations 11 and 14, we see that $\bar{w}_\Omega(v) = a + \frac{s}{s+\Omega}(v-a)$ for all $v \in [a, 1]$. Differentiating with respect to Ω yields

$$\frac{\partial \bar{w}_\Omega(v)}{\partial \Omega} = \frac{\partial a}{\partial \Omega}\left(1 - \frac{s}{s+\Omega}\right) - \frac{s}{(s+\Omega)^2}(v-a) \tag{15}$$

Noting that $\frac{s}{s+\Omega} \in (0,1]$ and that from Equation 14, $\frac{\partial a}{\partial \Omega} \overset{\text{sign}}{=} -r(s+1) < 0$ implies that $\frac{\partial \bar{w}_\Omega(v)}{\partial \Omega} < 0$ for all $v \in [a, 1]$. From Equation 12 we see that $\frac{\bar{G}(x)}{\bar{g}(x)} = \frac{x-a}{s}$ for all $x \in [a, h]$. Therefore, from Equation 5 we see that $\bar{w}_\Omega(v) \to v$ for all $v \in [0, 1]$ as $\Omega \to 0$.

By virtue of the fact that $\bar{w}_\Omega(v)$ is decreasing in Ω for all v, it must also be the case that h is decreasing in Ω. (It is possible to directly verify this by computing $\frac{\partial h}{\partial \Omega}$.) From Equation 12 we calculate $\frac{1-\bar{F}(x)}{\bar{f}(x)} = \frac{h-x}{r}$ for all $x \in [a, h]$. Since h is decreasing in Ω, $\frac{1-\bar{F}(x)}{\bar{f}(x)}$ is also decreasing in Ω over this range. Therefore, from Equation 4 we see that $\underline{w}_{i,1,\Omega}(\theta_i)$ is strictly decreasing in Ω for all $\theta_i \in [0, h]$, and $\underline{w}_{i,1,\Omega}(\theta_i) \to \theta_i$ for all $\theta_i \in [0, 1]$ as $\Omega \to 1$. *Q.E.D.*

PROOF OF THEOREM 2: We calculate the probability that a worker is hired by the firm ex-ante. Let $\mathbb{H}(r, s, \Omega)$ be the expected equilibrium employment level in a market with distribution parameters r and s and transparency Ω. Then

$$\begin{aligned}
\mathbb{H}(r, s, \Omega) &:= \textstyle\int_0^h Pr\left(\bar{w} \geq \underline{w}_{i,1}(\theta)\right) g(\theta) d\theta \\
&= \textstyle\int_0^h Pr\left(v \geq a + \frac{1-a}{h}\theta\right) g(\theta) d\theta \\
&= s \cdot (1-a)^r \textstyle\int_0^h \left(1 - \frac{1}{h}\theta\right)^r \theta^{s-1} d\theta \\
&= s\,(1-a)^r\, h^s \tfrac{\Gamma(r+1)\Gamma(s)}{\Gamma(r+s+1)}
\end{aligned}$$

where the first equality comes from substituting in Equation 11, the second equality comes from substituting in the distribution of outside options from Equation 6, and the third from the definition of the Gamma Function, i.e. $\Gamma(x) \equiv \int_0^\infty y^{x-1} e^{-y} dy$. As we see, transparency affects the hiring rate through changing a and h. Because all of the terms not involving a and h are strictly positive in this equation, it is the case that

$$\underset{\Omega}{\text{argmax}}\,\mathbb{H}(r,s,\Omega) = \underset{\Omega}{\text{argmax}}\ (1-a)^r\, h^s \tag{16}$$

Substituting in from Equation 14 and taking the first order condition with respect to Ω yields

$$\Omega^* = \frac{r+1}{r+s+2} \tag{17}$$

It remains to show that the maximization problem in Equation 16 is concave in Ω over [0,1]. Taking the first order condition of Equation 16 we see that

$$\frac{\partial(1-a)^r h^s}{\partial\Omega} = -\frac{r^2 s^2 (1-a)^{r-1} h^{s-1}(r(\Omega-1)+(2+s)\Omega-1)}{(s(1+r-\Omega)+r\Omega)^3} \tag{18}$$

From this, since $r,s>0$ and $a<1$ we see that the first order condition in Equation 17 holds. Substituting in from Equation 6 gives us the particular form of Ω^* in the theorem. We further can calculate

$$\begin{aligned}\frac{\partial^2(1-a)^r h^s}{\partial\Omega^2} \overset{\text{sign}}{=} & -s^3(r^2+r\left(2-\Omega^2\right)+(1-\Omega^2)) \\ & -r\Omega\left(r^2(2-\Omega)+2r\left(\Omega^2-3\Omega+2\right)+\left(4\Omega^2-5\Omega+2\right)\right) \\ & -s^2\left(r^3+r^2\left(-2\Omega^2+2\Omega+2\right)+r\left(-2\Omega^2+4\Omega+1\right)+2\Omega\left(1-\Omega^2\right)\right) \\ & -s\left(r^3\left(-\Omega^2+2\Omega+1\right)+r^2\left(3-2\Omega^2\right)\right) \\ & -s\left(r(6\Omega^2-6\Omega+3)+\left(-4\Omega^3+7\Omega^2-4\Omega+1\right)\right)\end{aligned}$$

A sufficient condition for $\frac{\partial^2(1-a)^r h^s}{\partial\Omega^2}<0$ for all $\Omega\in(0,1)$ is that each of the parenthetical polynomial terms involving Ω be strictly positive for $\Omega\in(0,1)$. It is easy to check each of these polynomials separately to see that this sufficient condition is indeed satisfied. Therefore, extreme point Ω^* is the global maximizer of expected employment. To see the second point, note that in equilibrium, there is an outside option cutoff for employment $\theta(\Omega,v)$ such that all workers with outside options weakly less than $\theta(\Omega,v)$ negotiate wages that are acceptable to the firm. Then the hiring rate is equal to $G(\theta(\Omega,v))$. Noting that a worker i with outside option $\theta(\Omega,v)$ sets $\underline{w}_{i,1}=\bar{w}$ it must be the case that $G(\theta(\Omega,v))=\bar{G}(\bar{w})$. From Equations 11 and 12 it is the case that for all $v\geq a$

$$\bar{G}(\bar{w}) = \left(\frac{h}{1-a}(v-a)\right)^s \tag{19}$$

We can use a monotonic transformation of $\bar{G}(\bar{w})$ to complete the claim, that is, we show submodularity of $\frac{h}{1-a}(v-a)$ in v and Ω :

$$\frac{\partial\dfrac{h}{1-a}(v-a)}{\partial v} = \frac{h}{1-a} = \frac{(1-\Omega)s+rs}{(s+\Omega)r} \tag{20}$$

which is clearly decreasing in Ω. Therefore, $\bar{G}(\bar{w})$ is submodular in v and Ω for a firm of type $v\geq a(\Omega)$. *Q.E.D.*

PROOF OF THEOREM 1: We show that the expected equilibrium profit of the firm is strictly increasing in Ω. That the worker expected equilibrium surplus is strictly decreasing in Ω follows a similar calculation. We invoke the law of iterated expectations by first finding the firm's profit for a particular draw $v > a$ which we denote by $\pi(v, \Omega)$.

$$\begin{aligned}\pi(v,\Omega) &:= \int_a^{\bar{w}} (v-(1-\Omega)\,y-\Omega\bar{w})\,\bar{g}(y)dy \\ &= \int_a^{\bar{w}} (v-(1-\Omega)\,y-\Omega\bar{w})\,s\left(\frac{h}{h-a}\right)^s (y-a)^{s-1}dy \\ &= \frac{(\bar{w}-a)^s}{s+1}\left(\frac{h}{h-a}\right)^s (a\,(\Omega-1)-\bar{w}(\Omega+s)+sv+v) \qquad (21)\end{aligned}$$

where the second equality comes by using Equation 12. The ex-ante expected profit of the firm can be expressed as $\pi(\Omega)=\int_a^1 \pi(v,\Omega)f(v)dv$. A tedious, but straightforward calculation shows that $\frac{\partial \pi(\Omega)}{\partial \Omega} > 0$ for all $r, s > 0$ as desired.

The proof that expected discounted wages are decreasing in Ω follows from Theorem 2 and the earlier part of the current proof. Let Ω^* be the expected employment maximizing level of transparency as defined in Equation 17. From Theorem 2 we know that the expected hiring rate is increasing in Ω on $[0,\Omega^*]$ and we have just shown that expected worker surplus is decreasing in Ω on $[0,\Omega^*]$. Therefore, it must be the case that expected discounted wages, conditional on employment, must be decreasing in Ω on $[0,\Omega^*]$. Similarly, from Theorem 2 we know that the expected hiring rate is decreasing in Ω on $[\Omega^*,1]$ and we have just shown that firm surplus is increasing in Ω on $[\Omega^*,1]$. Therefore, it must be the case that expected discounted wages, conditional on employment, must be decreasing in Ω on $[\Omega^*,1]$. Combining these two arguments, we see that expected discounted wages, conditional on employment, are decreasing in Ω on $[0,1]$, as desired. *Q.E.D.*

REMARK 2: Increasing transparency does not increase profits for all firm types: Let $v = 1$ and let $r = s = 1$. We can calculate the profit $\pi(v,\Omega)$ of the firm using Equation 21. We see that $\pi(1,1)=\frac{1}{4}$ while $\pi(1,\frac{1}{2})=\frac{9}{32}$. Notice that by symmetry of our model, this example implies that increasing transparency can strictly increase the expected earnings of workers with very low outside options. This observation was first made in Yilankaya (1999).

PROOF OF THEOREM 3: Recall from Equation 10 that the expected wage of a worker with outside option θ_i at a firm with value v is $Z(\Omega,v,\theta_i) := (1-\Omega)\,\underline{w}_{i,1}+\Omega\bar{w}$. A sufficient condition for $Z(\cdot,v,\theta_i)-Z(\cdot,v,\theta_j)$ being strictly decreasing in Ω is that $\frac{\partial^2 Z(\Omega,\theta)}{\partial\theta\partial\Omega} < 0$ for all $\Omega,\theta \in [0,1)$ and all $v \in [0,1]$. From Equations 10 and 11 we see that

$$\frac{\partial^2 Z(\Omega,v,\theta)}{\partial\theta\partial\Omega} = \frac{\partial(1-\Omega)\dfrac{h-a}{h}}{\partial\Omega} = \frac{\partial(1-\Omega)\dfrac{r}{r+(1-\Omega)}}{\partial\Omega} = \frac{-r^2}{(r+1-\Omega)^2} \qquad (22)$$

where the second equality comes from Equation 14. Since $r > 0$ and $\Omega \leq 1$ we have $\frac{\partial^2 Z(\Omega,v,\theta)}{\partial\theta\partial\Omega} < 0$ as desired. To show $Z(\cdot,v,\theta_i)-Z(\cdot,v,\theta_j)\to 0$ as $\Omega\to 1$, we note that $Z(\cdot,v,\theta_i)=(1-\Omega)\,\underline{w}_{i,1}+\Omega\bar{w}$. Since $\underline{w}_{i,1}$ is bounded below by θ_i then $Z(\cdot,v,\theta_i)$ converges to $\bar{w}$ for any θ_i. *Q.E.D.*

APPENDIX B: ENDOGENOUS TRANSPARENCY

This appendix presents an extension of our model in which the firm endogenously selects the level of transparency. It illuminates that in the absence of transparency laws, the firm endogenously selects pay secrecy.

The *endogenous transparency* game proceeds as follows. Prior to workers arriving at $t = 1$, the firm observes v and simultaneously selects its maximum wage $\bar{w} \in [0, 1]$ and its level of transparency $\tau \in [\underline{\tau}, 1]$. The minimum allowable level of transparency by law is $\underline{\tau} \in [0, 1)$, and is common knowledge between the firm and all workers. However, workers do not observe the firm's selected level of transparency τ at the time of the initial negotiation. The game then proceeds as in our base model.

PROPOSITION 4: *Let $k < 1$ and $\rho > 0$. $\tau = \underline{\tau}$ in any equilibrium of the endogenous transparency game, regardless of the value of the firm.*

PROOF: In any candidate equilibrium in which the firm selects $(\tau, \bar{w})$ where $\tau \in (\underline{\tau}, 1]$, the firm has a profitable deviation to selecting pair $(\underline{\tau}, \bar{w})$. All workers make initial offers as if $\tau = \underline{\tau}$ as the level of transparency is (initially) unobserved. The firm will therefore employ the same set of workers at the same initial wages. And, by selecting $\underline{\tau}$, the firm avoids costly renegotiations with $(1 - k)\rho(\tau - \underline{\tau})$ fraction of workers that it employs, thus increasing its profits.[15] *Q.E.D.*

This result extends even if workers receive a partially-informative signal of the firm's chosen level of transparency (possibly from a third-party source or the firm's reputation from unmodeled previous generations of workers). Let each worker receive a signal of the firm's choice of transparency at $t = 1$ prior to initial negotiations, where the signal is drawn with full support over $[0, 1]$ and the distribution from which the signal is drawn (potentially) varies based on the chosen τ. For $k < 1$ and $\rho > 0$, there remains an equilibrium in which the firm selects $\tau = \underline{\tau}$ for all v. Moreover, similar reasoning implies this is the unique pooling equilibrium in which the firm selects the same level of transparency with probability one for all v.

[15]We have not formally modeled the choice of workers to "bury their heads in the sand" and ignore wage information. Nevertheless, a richer model that allows each worker to ignore information would lead each worker to seek out wage information to the fullest extent allowed by the firm: for fixed $\bar{w}$, higher transparency helps workers at the point of (re)negotiation. Because each worker has zero measure, no single worker will affect the equilibrium payoff, and therefore actions, of the firm.

TABLE C.1

SUMMARY STATISTICS: COMBINED AMERICAN COMMUNITY SURVEY (ACS) AND CURRENT POPULATION SURVEY (CPS) SAMPLE, 2000-2016

PANEL A: Prime-Age Full-Time Private Sector Employees

	Median	25th P'tile	75th P'tile	Min./Max.	Mean	Std. Dev.	Obs.
Wage Income (Ln.)	10.65	10.20	11.16	5.70–13.48	10.69	0.72	2,341,981
Share Full-Time Private Sector Workers	1.00	1.00	1.00	1.00–1.00	1.00	0.00	2,341,981
Occ. Unionization	0.05	0.03	0.11	0.00–1.00	0.08	0.09	2,339,694
Male	1.00	0.00	1.00	0.00–1.00	0.59	0.49	2,341,981
Age	39.00	32.00	46.00	25.00–54.00	39.22	8.47	2,341,981
Sex					*Share*		*Count*
Male					0.59		1,344,480
Female					0.41		997,501
State							
California					0.35		790,564
New York					0.20		456,690
Illinois					0.13		317,049
Education Level							
4 Year College+					0.37		929,797
High School Only					0.40		872,363
Some College					0.23		539,821
Race							
White					0.69		1,700,862
Other					0.09		173,746
Black					0.09		172,071

TABLE C.1 (CONTINUED)

SUMMARY STATISTICS: COMBINED AMERICAN COMMUNITY SURVEY (ACS) AND CURRENT POPULATION SURVEY (CPS) SAMPLE, 2000-2016

PANEL B: Full Prime-Age Sample

	Median	25th P'tile	75th P'tile	Min./Max.	Mean	Std. Dev.	Obs.
Wage Income (Ln.)	10.49	9.90	11.00	5.30–13.48	10.37	1.04	4,261,749
Share Full-Time Private Sector Workers	0.00	0.00	1.00	0.00–1.00	0.44	0.50	5,452,711
Occ. Unionization	0.05	0.03	0.12	0.00–1.00	0.09	0.09	4,838,993
Male	0.00	0.00	1.00	0.00–1.00	0.50	0.50	5,452,711
Age	40.00	32.00	47.00	25.00–54.00	39.56	8.58	5,452,711
Sex					*Share*		*Count*
Female					0.50		2,785,968
Male					0.50		2,666,743
State							
California					0.37		1,955,753
New York					0.20		1,055,036
Illinois					0.13		681,978
Education Level							
High School Only					0.43		2,229,524
4 Year College+					0.34		1,979,486
Some College					0.23		1,243,701
Race							
White					0.68		3,886,332
Black					0.11		491,974
Other					0.09		403,438

Note: Panel A reports statistics for the sub-sample of the ACS-CPS sample that is employed full-time and is prime working age, Panel B reports statistics for the full sample of working age individuals. Information about individual demographics, earnings, employment and geography are captured by the ACS. The ACS caps recorded earnings at the 99.5th percentile within each state from 2003 onwards, and top codes in earlier years (https://usa.ipums.org/usa-action/variables/INCWAGE#codes_section). Occupation unionization rates are taken from the CPS data. The merge between data sets uses the standardized 1990 occupation codes provided by Ruggles et al. (2021) and Flood et al. (2020). As a result, the distribution of occupation unionization rates in this table reflects the weighted distribution of individuals across occupations. The median unionization rate across occupations, unweighted by population, is 7%. For state of work, education level and race, we report summary statistics about the three largest categories.

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