Pay Transparency and Inventor Productivity:

Evidence from State-level Pay Secrecy Laws*

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Abstract

We examine the role of pay transparency in the productivity of firms' and inventors' innovation activities. Our test exploits the staggered adoption of state-level pay secrecy laws, which enhance pay transparency in the workplace. We find a significant increase in inventor productivity of firms located in states that have passed such laws relative to firms elsewhere. This relation is more pronounced for firms in states with lower levels of pre-existing pay transparency. We further show that pay secrecy laws promote inventor productivity by motivating inventors—especially minority inventors—to exert more effort, enhancing the diversity of inventor teams, and encouraging all inventors to pursue promotions.

Keywords: pay transparency; pay secrecy laws; innovation; inventor productivity; patents

JEL Classification: G30, J28, K32, O31

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1. Introduction

The recent legal and regulatory environment of the U.S. is increasingly committing to pay transparency — making each employee's salary more observable to other employees within the company (Trotter et al., 2017; Heisler, 2021).[1] Although there is a growing literature on the impact of pay transparency on reducing wage gaps (e.g., Kim, 2015; Baker et al., 2021), little is known about the real effects of pay transparency on productivity. Analyzing these effects is important, because there are concerns that enhanced pay transparency can reduce job satisfaction, increase turnover among star employees, and thus decrease productivity (Card et al., 2012; Mas, 2017). In this paper, we fill this gap in the literature by proposing an identification strategy to examine the effect of pay transparency on productivity with respect to technological innovation.

It is well-known that firms commonly use "pay secrecy rules and practices" — contracts and internal rules prohibiting or strongly discouraging employees from disclosing their wages to coworkers (Gely and Bierman, 2003; Bierman and Gely, 2004; Edwards, 2005).[2] Such pay secrecy rules and practices have been criticized for their effect on pay discrimination (Kim, 2013, 2015; Baker et al., 2021; Cullen and Perez-Truglia, 2022) and have resulted in many prominent lawsuits.[3] Our tests exploit the staggered passage of pay secrecy laws by various U.S. states since the 1980s, which has enhanced pay transparency by prohibiting firms from implementing pay secrecy rules and practices. We find that the adoption of pay secrecy laws indeed mitigates pay differentials between minority employees and their counterparts, which supports the effectiveness of these laws.

[1] In the U.S., several states have adopted pay transparency laws that require employers to disclose pay ranges for job candidates (California in 2016, Washington in 2019, and Colorado, Connecticut, Maryland, Nevada, and Rhode Island in 2021). Outside the U.S., Iceland's government announced on International Women's Day (March 8, 2017) that every Icelandic company with 25 or more employees would be required to earn a certificate to prove that they offer men and women equal compensation for work of equal value.

[2] According to the Institute for Women's Policy Research/Rockefeller Survey of Economic Security conducted in 2010, about half of all workers report that discussions of salary information are either discouraged or prohibited and could even lead to punishment (Hayes and Hartmann, 2011; U.S. Department of Labor, 2016).

[3] For instance, the U.S. Department of Labor filed a lawsuit against Google on Wednesday, January 4th, 2017 because Google repeatedly refused to release employee compensation records to the Department (http://money.cnn.com/2017/01/04/technology/google-labor-department-lawsuit/index.html). In another case, a former female teacher working in Google's childcare center, Heidi Lamar, filed a complaint in San Francisco Superior Court and alleged that female teachers were paid less than men with fewer qualifications who nonetheless performed the same job (https://www.usatoday.com/story/tech/2018/01/03/former-google-preschool-teacher-alleges-gender-pay-discrimination/1000424001/ and https://www.theguardian.com/us-news/2018/jan/03/google-childcare-center-teachers-women-pay-pay-discrimination).

Our literature review suggests that enhanced pay transparency may encourage or discourage inventors, an important class of employees in today's knowledge-based economy, for different reasons. Several arguments support a positive effect. First, pay transparency reduces the uncertainty of compensation that inventors expect from their efforts, which could motivate them to invest more in human capital and to work harder (Hsieh et al., 2019; Cullen and Perez-Truglia, 2022). This incentive could be stronger among minority inventors (Kim, 2015). Second, pay secrecy laws could increase the diversity of a firm's inventor teams and thus enhance their productivity, considering that the combination of diversified backgrounds and knowledge sources usually results in higher-quality innovation (Drach-Zahavy and Somech, 2001; Hong and Page, 2001; Berliant and Fujita, 2011; Yang et al., 2022). Finally, when senior colleagues' compensation becomes more visible, inventors working for firms with higher expected promotion raises may exert more effort to pursue promotions.

On the other hand, there are also arguments suggesting a negative effect of pay transparency associated with pay secrecy laws. The potential disclosure of inventors' compensation may increase their chances of being recruited by competing firms, which reduces affected firms' incentives in innovation investment (Kim and Marschke, 2005; Hitz and Werner, 2012). Moreover, the enactment of pay secrecy laws may result in a large number of renegotiations or lawsuits, which requires management teams' time as well as energy and likely distracts them from innovation activities (Gao and Zhang, 2019; Bennedsen et al, 2022). Finally, once some inventors find that they are under-paid, they may decide to exert less effort ("quiet quitting"), or they may work hard to attract external offers and eventually leave their current firms (Akerlof and Yellen, 1990; Danziger and Katz, 1997).

Using a panel of U.S. public firms with patent records from 1976 to 2017 and employing a staggered difference-in-differences approach, we show that the adoption of pay secrecy laws is associated with a significant increase in firms' inventor output scaled by their number of inventors, even when controlling for R&D input. We use patents' forward citations to capture the quality of innovation output so as to avoid weak patent concerns (Lemley and Sampat, 2012; Picard and van Pottelsberghe de la Potterie, 2013; Lei and Wright, 2017). On average, firms headquartered in states that have adopted pay secrecy laws increase their numbers of patents and corresponding forward citations per inventor by 1.5% and 2.1%, respectively, relative to firms headquartered in other states. More importantly, we find that such increases occur two years after the passage of pay secrecy laws, which mitigates concerns of reverse causality. We also find similar increases in these firms' patent value, generality, and originality (Trajtenberg et al., 1997; Hsu et al., 2014; Kogan et al., 2017). On the other hand, we do not find a significant increase in R&D input after the adoption of pay secrecy laws, which confirms that our findings are due to enhanced productivity rather than increased investment in technological innovation.

Our use of the staggered passage of pay secrecy laws is appealing for three reasons. First, the motivation behind promoting pay secrecy laws is to reduce the wage gender/race gap. Given that these laws were not passed with the primary intention to enhance innovation, their effect on inventor productivity is likely an unintended consequence. Second, because multiple exogenous shocks affect different firms at different times, we can better avoid a common identification challenge faced by studies with a single shock: the potential noise coinciding with the shock that directly affects the explained variable. Finally, due to staggered policy changes, a state can be in both the treatment group and the control group at different times, which helps mitigate concerns about any large differences between the two groups.

We also implement an extensive list of tests to investigate the validity of our difference-in-differences analysis, and we find that a state's passage of pay secrecy laws is unrelated to pre-existing levels and trends of local firms' inventor productivity, which mitigates the concern about omitted variables. Additionally, an important assumption behind a causal interpretation of the difference-in-differences estimates is that the treated firms and control firms share parallel trends with respect to inventor productivity before the adoption of these laws; that said, we show that the pre-treatment trends in inventor productivity are indeed indistinguishable between these two groups of firms. Moreover, we move the passage years from the actual event years to 3 to 5 years before and do not find a significant coefficient in a sample of treated firms in the pre-event years and control firms across all years. Furthermore, when we implement stacked difference-in-differences estimates following Gormley and Matsa (2011), Deshpande and Li (2019), and Cengiz et al. (2019), we find consistent results. More importantly, we also find consistent difference-in-differences estimates using a propensity score-matched sample to ensure homogeneous treated and control firms. In sum, these collective analyses support a causal interpretation of our main results.

We perform several robustness checks on our main findings. We exclude firms headquartered in California and Michigan—two states that adopted pay secrecy laws around the beginning of our sample period—as well as firms headquartered in New Hampshire, Connecticut, and Oregon, for these three latter states adopted pay secrecy laws around the end of our sample period. Moreover, we control for not-to-compete covenants, as well as control for state-specific pre-trends that reflect state-level progressiveness or openness and may affect inventor productivity. Finally, we use a number of alternative measures of innovation output in our analysis. When we perform these robustness checks, the positive relation between pay secrecy laws and inventor productivity remains.

To provide further evidence that the effect of pay secrecy laws on inventor productivity is indeed tied to pay transparency, we show that the effect of pay secrecy laws is stronger for states with higher pre-existing pay secrecy in the workplace and for firms with higher ratios of minority inventors, who are more vulnerable to pay secrecy practices. These results further increase our confidence that the positive impact of pay secrecy laws on innovation is likely due to pay transparency.

In the second part of this paper, we investigate possible mechanisms through which pay secrecy laws enhance inventor productivity. We first show that, in an inventor-year panel, inventors' productivity significantly increases with pay secrecy laws, which confirms our firm-level results and highlights the role of inventors' motivation. More importantly, such a relation is stronger among minority inventors. Second, we show that inventor teams become more diversified after pay secrecy laws have been adopted and that patents produced by more diversified teams are of higher quality. Finally, we find that the positive relation between pay secrecy laws and inventor productivity is more pronounced in firms with higher expected promotion raises. In summary, our mechanism tests provide further evidence for the effect of pay secrecy laws on inventor productivity.

Our empirical analysis responds to increasing concerns about pay discrimination and productivity.[4] While pay discrimination occurs in workplaces globally, it is empirically challenging to establish the causality between pay discrimination and employee performance, even

[4] Pay gaps exist among knowledge workers in high-tech firms and research institutes. The literature has reported that female scientists overall have earned about 11 percent less than male scientists over the past several decades (Goyette and Xie, 1999; Prokos and Padavic, 2005). Moreover, in a sample of 10,585 R&D staff members, Sauermann (2018) finds that male employees' salaries are 8% higher than those of female employees. Such a gap may have implications for productivity because female life scientists patent much less than their male peers, even though there is no difference in patent quality between the two groups of scientists (Ding et al., 2006; Azoulay et al., 2007).

when compensation data is available (Heckman, 1998). In this study, we propose a new difference-in-differences framework to draw a causal inference for the negative impact of discriminatory compensation on productivity in innovation activities. Our empirical evidence thus has implications for both economic growth and human capital.

This study also adds to the growing literature that examines the real effects of pay secrecy laws and, more broadly, pay transparency. While there exists an extensive list of studies showing that pay secrecy laws or laws related to pay transparency effectively reduce pay gaps,[5] the real effects of pay transparency on productivity are still under-explored. Our use of patent data enables us to appropriately measure productivity at both firm and inventor levels, and our novel evidence shows that pay transparency fosters inventors' productivity and teamwork. In addition, our findings that diversified inventor teams are more likely to be formed after the passage of pay secrecy laws and that such teams produce more valuable patents offer new insights to the literature on workplace diversity.[6]

Moreover, this study is related to the literature that analyzes compensation design and its impact on firm-level innovation. This literature stream has focused on aspects such as pay-for-performance sensitivity, subjective vs. objective performance evaluations, and long-term compensation plans (e.g., Lerner and Wulf, 2007; Hall and Lerner, 2010; Ederer and Manso, 2011; Manso, 2011). Extending this stream, our paper provides evidence that the transparency of compensation schemes—in addition to the compensation schemes themselves—is an important driver of corporate innovation and inventor productivity.

2. Pay Secrecy Laws

2.1. Institutional Background

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[5] Kim (2015) finds that the adoption of pay secrecy laws helps to reduce gender pay gaps, especially among women with college or graduate degrees. Also, Baker et al. (2021) examine the impact of public sector salary disclosure laws on university faculty salaries in Canada, and show that these laws help to reduce gender pay gaps. Bennedsen et al. (2022) study a 2006 legislative change in Denmark that requires firms to provide disaggregated wage information and show that this law significantly reduces gender pay gaps.

[6] Empirical studies on group decision-making also find that groups consisting of more diverse individuals produce higher quality and more innovative decisions than groups of homogenous individuals (Watson et al., 1993; Amason, 1996). Qian et al. (2012) show that top management teams comprised of more diverse executives are more likely to propose and pursue novel projects. In addition, Niebuhr (2010), Singh and Fleming (2010), Parrotta et al. (2014), Nathan (2015), and Yang et al. (2022) all show that team diversity positively influences innovation quality.

The National Labor Relations Act (NLRA) of 1935 provides the earliest legal protection on pay secrecy matters. In Section 7, the Act protects non-supervisory employees who are covered by the Act from employer retaliation if they discuss their wages or working conditions with their colleagues as part of a concerted activity to improve them (U.S. Department of Labor, 2016). Nevertheless, in the same document, the U.S. Department of Labor also highlighted that the NLRA did *not* solve the pay transparency issue because it did not address all situations in which employers prohibit or discourage employees from discussing their wages with their colleagues. Pay secrecy rules and practices appear frequently in the workplace as firms may use contracts and internal rules to prohibit employees from disclosing their wages to coworkers (Gely and Bierman, 2003; Bierman and Gely, 2004; Edwards, 2005).[7]

As a reaction to the insufficient protection and enforcement of transparent compensation, specific state laws have been introduced since the 1980s to mitigate pay secrecy rules and to enhance pay transparency. For example, Michigan passed a law in 1982 that prohibited employers from: 1) requiring as a condition of employment non-disclosure by an employee of his or her wages; 2) requiring an employee to sign a waiver or other document that purports to deny an employee the right to disclose his or her wages; or 3) discharging, formally disciplining, or otherwise discriminating against an employee for job advancement on the basis of having disclosed his or her wages. In 1984, California passed a similar law. More recently, the following seven states passed similar laws between 2000 and 2016 (U.S. Department of Labor, 2016): Illinois (2003), Vermont (2005), Colorado (2009), Maine (2009), Louisiana (2013), New Jersey (2013), Minnesota (2014), New Hampshire (2015), Connecticut (2015), and Oregon (2016). We map these states in Figure 1 and provide the details of these laws in Appendix 1. These laws are known as pay secrecy laws because they aim to eliminate or mitigate pay secrecy rules and practices. These laws have been effective. For example, Kim (2015) reports that the adoption of pay secrecy laws increases female workers' total compensation by 3% and reduces gender pay gaps by more than 5% for female workers with a college education. Our later analysis in Section 2.2 also suggests that these laws mitigate pay gaps by 3% in terms of the hourly wages of scientists and engineers.

[7] For example, a 2017 survey conducted by the Institute for Women's Policy Research shows that 25 percent of private sector employees work in environments that formally prohibit them from discussing salaries, and another 41 percent work in environments that discourage them from discussing salaries (Hayes and Hartmann, 2011).

As pointed out by Kim (2013, 2015), the political motivation for passing these state laws is to close the wage gap. Feminist activists and legislators claim that once pay is no longer a secret, women will be able to discover gender pay gaps for themselves and subsequently take action to reduce such gaps.[8] Opponents of such legislation argue that such laws may result in three types of costs. First, it may cause some social discomfort, inasmuch as the legislation challenges social norms in the U.S. regarding pay secrecy. Second, it could increase costs to employers if more employees (falsely) file suits. Third, after they learn what their co-workers earn, employees who are paid below the average may become disgruntled, while employees paid above the average may not necessarily become more satisfied (Card et al., 2012). The passage of state pay secrecy laws depends on the relative power between the two sides in each state at a given time. For example, after several failed attempts, Maine successfully passed its pay secrecy law in December 2009 because (i) Congress passed the Lilly Ledbetter Fair Pay Act in early 2009; (ii) Ledbetter's legal case caused great public outrage regarding the gender pay gap; and (iii) Republican female senators Susan Collins and Olympia Snowe, both from Maine, were important supporters of this act (Ramachandran, 2012; Kim, 2015). In summary, a state's adoption of pay secrecy laws certainly depends on some political factors, such as a legislator's support, the existence of influential decision-makers, and public opinion towards pay secrecy itself.

To the best of our knowledge, these factors seem largely unrelated to inventors or innovative firms. Moreover, it is unlikely that the majority of firms will lobby or influence the passage of pay secrecy laws because managers are generally unwilling to adopt a transparent pay policy before the passage of state-level pay secrecy laws for the following reasons. First, pay secrecy policies that maintain a lack of transparency and prevent disclosure of salary information help reinforce the power of managers (Lawler, 1992; Rosenfeld, 2017). Second, as discussed in Gely and Bierman (2003) and Bierman and Gely (2004), pay secrecy practices are legally feasible and have established a social norm for firms and managers; as a result, managers might follow such social norms and peer practices because they may be risk averse and wish to avoid peer pressure. For example, when all firms practice pay secrecy, they may prevent employees from leaving for better paying jobs (Danziger and Katz, 1997; Colella et al., 2007). Finally, while pay

[8] Ledbetter vs. Goodyear & Rubber Co. is a classic legal case in which pay information facilitates efforts to combat pay discrepancy. In this case, Lilly Ledbetter worked at Goodyear Tire for nineteen years. During that period, she consistently received wages lower than her male colleagues, but she was not aware of this fact (Ramachandran, 2012). When she became aware of this pay disparity, she initiated a lawsuit immediately.

secrecy policies may benefit firms' profits and operations, they also come with costs in other dimensions, such as adding administrative costs because employees may feel unfairly treated and frequently request reevaluation.

On the other hand, we do not rule out the possibility that some managers and firms may be aware of the benefits of transparency in pay and thus may voluntarily adopt transparent pay policies to encourage innovative performance. In fact, the existence of such firms would make it difficult for us to find any relation between pay secrecy laws and innovation in our empirical analysis.

2.2. Evidence on the Effectiveness of Pay Secrecy Laws

In this section, we examine the effectiveness of pay secrecy laws. Considering that these laws were initially adopted to reduce wage gaps between minority employees and their counterparts, we expect that, if these laws are effective, we will observe a reduction in wage gaps following the adoption of these laws.

Given that our paper focuses on corporate innovation and inventors, we examine the salaries of scientists and engineers. We use the 1990 Census Bureau occupational classification system to categorize scientists and engineers as follows: engineers, mathematical and computer scientists, natural scientists, engineering and related technologists and technicians, and science technicians.[9] We use their hourly salaries and other variables from the IPUMS-CPS-ASEC (Flood et al., 2018) database to estimate the following ordinary least squares regression for the following employee-year panel:

$$Ln(Hourly\ wage)_{jst} = \alpha_0 + \alpha_1 Transparency_{st} \times Minority_j + \alpha_2 Transparency_{st} + \alpha_3 Minority_j + \alpha_4 X_{jst} + \delta_s + \gamma_l + \theta_t + \varepsilon_{jst}, \quad (1)$$

in which $Ln(Hourly\ wage)_{jst}$ denotes the natural logarithm of hourly wages received by employee j in industry/occupation l in state s in year t. The indicator variable $Transparency_{st}$ takes the value of one if pay secrecy laws are passed in state s in year t, and zero otherwise. The indicator variable $Minority_j$ takes the value of one if employee j is not a white male, and zero

[9] The full list of occupations can be found at https://cps.ipums.org/cps-action/variables/OCC90LY#codes_section

otherwise. X_{jst} denotes the set of time-varying control variables,[10] δ_s denotes state fixed effects, γ_l denotes industry fixed effects (in Table 2 column (1)) or occupation fixed effects (in Table 2 column (2)), and θ_t denotes year fixed effects. Given that our treatment is defined at the state level, we cluster standard errors by location state (Atanassov, 2013; Acharya et al., 2014; Png, 2017a, 2017b). We provide the summary statistics of related variables in Panel A of Table 1.

The coefficient of interest in Equation (1) is α_1 for the interaction term, which captures the relation between the passage of pay secrecy laws and minority employees' salaries (as compared to majority employees' salaries). As reported in Table 2, the coefficients on the interaction terms $Transparency \times Minority$ are positive and significant in both columns, which is consistent with the expectation that pay secrecy laws significantly increase salaries for minority scientists and engineers relative to other scientists and engineers.[11] This finding suggests that these laws mitigate pay gaps by 3 percentage points in terms of the hourly wages of scientists and engineers. The economic magnitude is also sizeable: considering that the coefficient on $Minority$ is around -0.12, pay secrecy laws reduce pay gaps by 25% (=0.03/0.12). This finding, which is consistent with the findings of Kim (2015) and Baker et al. (2021), provides supporting evidence that pay secrecy laws are indeed effective in mitigating pay gaps of intellectual workers in the workplace.

3. Inventor Productivity in the Firm Level

3.1. Data

Following the literature, we use patent data to capture the performance and productivity of firms' and individuals' innovative activities (e.g., Kamien and Schwartz, 1975; Griliches, 1990). We collect information about all patents granted by the U.S. Patent and Trademark Office (USPTO) from the PatentsView database.[12] This database includes information about each inventor, assignee, technology group, filing date, grant date, and references (backward citations) for each utility patent. We then identify each patent granted to U.S. public firms in the Compustat database

[10] We include each person's age, annual working hours, whether s/he has completed a college degree, and whether s/he has a postgraduate degree.

[11] We also test the sum of the coefficients on $Transparency \times Minority$ and $Transparency$, and find that their sum is insignificantly different from zero. One interpretation for this finding is that while minorities' wages increase, the majority experience no significant changes in their wages.

[12] The USPTO PatentsView database is derived from its bulk data files and is supported by the USPTO Office of the Chief Economist, with additional support from the U.S. Department of Agriculture.

using the Kogan et al. (2017) and Stoffman et al. (2022).[13] We use the application year of a patent as the time of its invention to measure a firm's innovation output, which is common in the literature (Hall et al., 2005a, 2005b). Our sample starts in 1976; since it takes two to three years for the USPTO to approve a patent, we end our sample in 2017 to ensure that the majority of the patents applied for before 2019 have been granted by the USPTO and, thus, exist in our database. Following prior studies (e.g., Lerner et al., 2011; Aghion et al., 2013; Bloom et al., 2013), we drop firms that never applied for a single patent during our entire sample period. Our final panel data sample consists of 67,685 firm-year observations over the period 1976-2017.

We focus on two primary measures for inventor productivity. Our first measure is patent count per inventor (*Pat/Inventor*), defined as the number of patents applied for by a firm in year t, scaled by the number of unique inventors in the most recent 10 years (t-9 to t).[14] This measure captures the quantity of innovation output of a firm in year t. Despite its simplicity, the number of patents has been widely used in the economics literature to capture firm-level innovation output since the seminal work of Griliches (1981). Another measure is the sum of forward citation counts received by these patents scaled by the number of unique inventors in the most recent 10 years (*Cit/Inventor*). The number of forward citations received by a patent (i.e., the number of references the patent receives by subsequent patents) reflects the importance of the patent (Trajtenberg et al., 1997; Hall et al., 2005b).[15] We then add the forward citation counts of all patents applied for by a firm in year t to obtain the citation count of the firm in year t, as this citation count takes into account patent quality (Hall et al., 2005b; Aghion et al., 2013) and mitigates concerns about "weak patents" (Lemley and Sampat, 2012; Picard and de la Potterie, 2013; Lei and Wright, 2017).

3.2. Main Results

[13] The data are available from the website of Noah Stoffman: https://kelley.iu.edu/nstoffma/. We only include companies that are headquartered in the U.S. We exclude firms in financial industries (SIC codes 6000-6999) and utility industries (SIC codes 4900-4999) because they are under different regulatory oversights. Finally, we exclude all firm-year observations with book values of assets below $5 million to ensure that extremely small firms will not drive our empirical results.

[14] Since not every inventor files a patent each year, we focus on the number of unique inventors in most recent 10 years to provide a more stable estimation of the size of a firm's inventor team. Nevertheless, we find consistent results in a robustness check using the number of unique inventors in year t as the denominator.

[15] Patents granted near the end of our sample period have less time to receive citations than patents granted earlier. Therefore, to adjust for the duration of patent citations, we only consider a patent's forward citations that happen in a five-year window since the patent's grant year (Lerner et al., 2011; Bernstein, 2015). In addition, we scale a patent's forward citation count by the average forward citation count of all patents in the same technology group and the same grant year (Seru, 2014).

Since 12 states adopted pay secrecy laws at different time points during the sample period, we implement difference-in-differences tests following Bertrand and Mullainathan (2003). That is, we compare changes in inventor productivity for firms headquartered in the states that adopt pay secrecy laws with changes in inventor productivity for firms headquartered in other states.[16] We estimate the following ordinary least squares regression in a firm-year panel:

$$Ln(Y_{ijst} + 1) = \alpha_0 + \alpha_1 Transparency_{st} + \alpha_2 X_{ijst} + \gamma_i + \theta_{jt} + \varepsilon_{jst} , \qquad (2)$$

The dependent variable Y_{ist} is the patent count (*Pat/Inventor*) and citation count (*Cit/Inventor*) per inventor of firm *i* that is in industry *j* (SIC 2-digit codes) and is headquartered in state *s* in year *t*.[17] Similar to Equation (1), the indicator variable $Transparency_{st}$ takes the value of one if pay secrecy laws are passed in state *s* in year *t*, and zero otherwise. We also consider an extensive list of control variables X_{ijst}, and provide their detailed variable definitions in Appendix 2.[18] γ_i and θ_{jt} denote firm and industry×year fixed effects, respectively. Robust standard errors clustered by headquarters state are in parentheses. The summary statistics of all variables are provided in Table 1 Panel B.

We present our results in Table 3. In column (1), the dependent variable is *Pat/Inventor*; we show that the coefficient estimate on *Transparency* is 0.013 and significant at the 1% level, suggesting that inventor productivity drops after the passage of pay secrecy laws. In terms of economic significance, a firm's patent output per inventor increases by 1.5% after the passage of

[16] We use a firm's headquarters location for law changes because top executives and R&D facilities tend to be geographically close to a firm's headquarters (Acharya et al., 2014). It is also common in prior studies to use a headquarters location to analyze the behaviors of managers, analysts, and investors because a firm's headquarters is comprised of high-level managers, scientists, and researchers, and thus serves as an information hub and operational center (Coval and Moskowitz, 1999, 2001; Pirinsky and Wang, 2006).

[17] We obtain historical headquarters data from Bai et al. (2020). The data is publicly available at https://sites.google.com/site/johnbaijianqiu/data

[18] Specifically, in our baseline regressions, we control for firm size, cash holdings, R&D intensity, R&D missing dummy, ROA, asset tangibility, leverage, capital expenditures, Tobin's Q, and firm age. Following Bebchuk et al. (2011), we set missing R&D as zero and include a dummy variable R&D missing. We also control for a vector of state-level variables in our regressions. We control for state GDP, personal income per capita, and population, as these factors may affect firm- and inventor-level innovation performance. In addition, we control for business combination laws and good-faith exceptions of wrongful discharge laws that affect competitive situations and job security. The literature documents the impact of these two laws on corporate innovation (Atanassov, 2013; Acharya et al., 2014). We collect data on business combination laws from Bertrand and Mullainathan (2003) and data on good-faith exceptions from Autor et al. (2006). We obtain the data on GDP, per capita income, and population from the Bureau of Economic Analysis. To minimize the effect of outliers, we winsorize all continuous variables at the 1st and 99th percentiles. In an unreported robustness check, we find consistent results if we do not include any control variable or if we do not include R&D intensity and the R&D missing dummy.

pay secrecy laws, which corresponds to an increase of 10.07% of the sample mean.[19] In column (2), the dependent variable is *Cit/Inventor*; we show that the coefficient on *Transparency* is 0.018 and significant at the 1% level. The magnitude of economic significance is also sizable: a firm's citation count per inventor increases by 2.1% after the passage of pay secrecy laws, which corresponds to an increase of 13.29% to the sample mean. This magnitude is economically significant, considering that one additional citation could increase a patent's value by one million U.S. dollars (Harhoff et al., 1999).

In terms of other control variables, we find that total assets, cash level, R&D intensity, ROA, profitability, and Tobin's Q are generally both positively and significantly related with inventor productivity, while age, asset tangibility, and leverage are negatively and significantly related with inventor productivity.

3.3. The Pre-treatment Trends Assumption

The validity of our difference-in-differences approach relies on the parallel trends assumption: absent pay secrecy laws, treated firms' innovation output would have evolved in the same way as that of control firms. To test the pre-treatment trends in innovation output for both the treated firms and control firms, we re-estimate Equation (2) after replacing *Transparency* with fifteen indicator variables ($Year^{-7}$ to $Year^{7+}$) as the following:

$$Ln(Y_{ijst} + 1) = \alpha_0 + \sum_{\tau=-7}^{7+} \alpha_\tau \times Year_{st}^{\tau} + \alpha_8 X_{ijst} + \gamma_i + \theta_{jt} + \varepsilon_{jst}\ , \qquad (3)$$

in which each *Year* variable indicates the time relative to the passage year. For example, $Year^{-7}$ indicates that the sample year is seven years before a state passes pay secrecy laws, $Year^{0}$ indicates the year in which pay secrecy laws are passed, and $Year^{7+}$ indicates that the sample year is seven or more years after the passage of pay secrecy laws. We focus on the coefficients on the indicators $Year^{-7}$ to $Year^{-1}$ because their magnitude and significance indicate whether there are differences in innovation output between treated firms and their control firms prior to the adoption of pay secrecy laws. We present our results in columns (3) and (4) in Table 3 as well as Figure IA1 in the Internet Appendix. We find that the coefficients on $Year^{-7}$ to $Year^{-1}$ are not significantly different from

[19] Considering that the average number of patents per inventor is 0.149 in treated firms before the treatment, treated firms experience an increase in their number of patents per inventor by 0.015 (= (1 + 0.149)*(exp(0.013) − 1)), which corresponds to a relative increase of 10.07% (=0.015/0.149).

zero, suggesting that the parallel trends assumption of our difference-in-differences approach is not violated.

Further, we show that the coefficients on the indicators $Year^0$ and $Year^1$ are small in magnitude and not statistically significant for both innovation measures. The effect of pay secrecy laws emerges two years after the laws' adoption: the coefficients on $Year^2$ to $Year^{7+}$ are significantly positive in many cases, which is consistent with the intuition that it may take a longer time for pay secrecy laws to impose an effect on inventor productivity, if any.[20] This finding further mitigates concerns of reverse causality and also supports a causal effect of pay secrecy laws.

While Table 3 and Figure IA1 in the Internet Appendix do not reveal significant pre-trends, we follow Moser and Voena (2012) and re-estimate Equation (2) by additionally controlling for state-specific pre-trends.[21] The results are presented in Table IA1 in the Internet Appendix. In columns (1) and (2) of Table IA1, we control for state-specific linear pre-trends; in columns (3) and (4), we further control for state-specific quadratic pre-trends.[22] We find that the coefficient estimates on *Transparency* remain significantly positive. These results suggest that, even if we include pre-existing time trends (if any), our main findings nonetheless remain robust.

As a further check for the existence of any pre-trends, we consider another test: we keep only the observations of a treated state before the passage of pay secrecy laws and the observations in a never-treated state throughout the sample period. We then assign pseudo-event years as 3, 4, or 5 years before the true event years. As shown in Table IA2 in the Internet Appendix, we do not find any relation between pay secrecy laws and inventor productivity when we conduct this placebo test.

3.4. The Timing of Adopting Pay Secrecy Laws

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[20] Prior empirical studies have posited that it takes less than one year for increases in R&D input to generate increases in patent applications (Hausman, Hall, and Griliches, 1984; Hall, Griliches, and Hausman, 1986; Lerner and Wulf, 2007). Using a survey of 497 observations at the EPO conducted in 2006 (de Rassenfosse, 2012), de Rassenfosse and Jaffe (2018) argue that roughly 80% of patents are filed within one year of the start of corresponding R&D projects. As long as a firm's policies with respect to R&D, human capital, and inventors respond to pay secrecy laws shortly, our regression setting is reasonably acceptable.

[21] We acknowledge that the evidence of Table 3 and Figure IA1 is only suggestive. Thus, our subsequent robustness checks as well as mechanism tests help strength a causal interpretation of our results.

[22] In columns (1) and (2), we additionally include $\sum_1^{51} \beta_s \times Pre - trend_s$, in which *Pre-trend* is equal to the number of years from the current year to the law passage year before a state passes a pay secrecy law, and is equal to 0 in the years after or in states that never pass a pay secrecy law. For instance, if a state passes pay secrecy laws in 1990, we set *Pre-trend* to be 1, 2, 3, … for firms in that state in 1989, 1988, 1987, …. . In columns (3) and (4), we additionally include $\sum_1^{51} \beta_s \times (Pre - trend_s\ )$^2.

In conducting our difference-in-differences tests, we assume that a state's adoption of pay secrecy laws is not correlated with the pre-existing inventor productivity of firms headquartered in that state. To validate this assumption, we follow Acharya et al. (2014) and Png (2017b) and estimate a Weibull hazard model in which the "failure event" is the adoption of pay secrecy laws in a given state. The sample consists of all states over our sample period with treated states being dropped from the sample once they have adopted pay secrecy laws. The independent variables of interest are the levels, changes, and growth rates of *Ln(Average Pat/Inventor)* and *Ln(Average Cit/Inventor),* which are the average number of patent counts or citation counts per inventor of all public firms headquartered in a state in a given year. We also control for the levels, changes, and growth rates of the following state-level variables: state pay gaps, state GDP, population characteristics, unemployment rates, and political climate (i.e., whether or not a state is governed by a Republican), as well as state-level business combination laws and wrongful discharge laws.[23] We provide variable definitions in Appendix 2.

In Table IA3 in the Internet Appendix, we present our results from this estimated hazard model. Panel A is based on the levels of explanatory variables, Panel B is based on the three-year average changes for each of the explanatory variables, and Panel C is based on the three-year average of the growth rates of each of the explanatory variables.[24] We show that the coefficients on the levels, changes, and growth rates of *Ln(Average Pat/Inventor)* and *Ln(Average Cit/Inventor)* are not statistically significant across all columns. These results indicate that a state's passage of pay secrecy laws is unrelated to the pre-existing inventor productivity of local firms, which supports the exogeneity of pay secrecy laws to local firms' inventor productivity and mitigates the concern that some omitted variables influence both the passage of pay secrecy laws and inventor productivity.

3.5. Additional Tests for Staggered Difference-in-Differences Estimates

We are aware of potential bias with staggered difference-in-differences estimates and thus implement two robustness checks.

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[23] We obtain the data on GDP, per capita income, and population from the Bureau of Economic Analysis, and we obtain the data on state governors' party affiliation from the National Governors Association. We also obtain the data on state unemployment rates as well as the data on state-level college graduates, males, and whites in the labor force at the state level from the IPUMS-CPS-ASEC database.

[24] When we consider growth rates, we do not include explanatory variables that are dummies.

Our first check is to implement the stacked difference-in-differences estimates following Gormley and Matsa (2011), Deshpande and Li (2019), and Cengiz et al. (2019). This method allows us to mitigate the influence of heterogeneous treatment effects and avoids potential negative weights of specific treatments. For each treatment event (i.e., the event when a state adopted pay secrecy laws), we collect a cohort set that includes all firm-year observations in a window [-10,10] that ranges from 10 years before the event to 10 years after the event. The control group includes all firm-year observations in the same window in never-treated states. Lastly, we stack all cohort sets together and estimate the following ordinary least squares regression using the stacked panel:

$$ln\left(1 + Y_{icj\tau}\right) = \alpha_0 + \alpha_1 Treatment_{ic\tau} + \alpha_2 X_{icj\tau} + \gamma_i + \delta_c + \theta_{j\tau} + e_{icj\tau}. \quad (4)$$

The dependent variable $Y_{icj\tau}$ is the patent count (*Pat/Inventor*) or citation count (*Cit/Inventor*) per inventor for firm *i* that is in industry *j* and belongs to cohort *c* in event year τ (which is the calendar year minus the treatment year in cohort *c*). The indicator variable $Treatment_{ic\tau}$ takes the value of one if firm *i* is treated in an event year (i.e., $\tau >= 0$) in cohort *c*, and zero otherwise. $X_{ijc\tau}$ denotes the set of time-varying control variables. γ_i denotes the firm fixed effects, δ_c denotes the cohort fixed effects, and $\theta_{j\tau}$ denotes industry-event year joint fixed effects. The coefficient, α_1, on $Treatment_{ic\tau}$ represents our difference-in-differences estimates. The standard errors are clustered by cohort-state.

We present the summary statistics of all variables in the stacked panel in Table IA4 in the Internet Appendix. We present the estimation of Equation (4) in Panel A of Table 4. The difference-in-differences estimates are 0.016, which are statistically significant. These results confirm that our difference-in-differences estimates are reasonably robust and are not driven by some early- or late-adopting states.

Our second check is to conduct a propensity score matching (PSM) procedure to generate matched pairs of firms (one treated and one matched control) that are homogenous for all firm characteristics.[25] We present the firm characteristics of treated and matched control firms before the treatment in Table IA5 in the Internet Appendix, which shows that our matching procedure

[25] For each firm in the stacked sample, we use the value of all the firm-level characteristics in the year -1 (i.e., τ=-1). We then run a Probit model with all these variables. For each treatment firm-cohort, we identify a control firm-cohort with the closest propensity score, without replacement.

indeed generates reasonably homogeneous firms before treatment. We then estimate Equation (4) using the matched sample and present the results in Panel B of Table 4. These results suggest that, even if we start with similar firms across treated and untreated states, we are still able to obtain consistent results.

3.6. Robustness Check of Our Baseline Regression

In this section, we present a large number of robustness checks on our main findings. First, we consider the standard errors based on regular wild bootstrap and wild cluster bootstrap (Roodman et al., 2019) and cluster jackknife for standard errors (MacKinnon et al., 2022a, 2022b) to mitigate the estimation errors related to our cluster structures (i.e., only a few treated clusters that may make asymptotic inference unreliable). As shown in Table IA6 in the Internet Appendix, we find that the coefficients on *Transparency* remain significantly positive.

Second, we estimate the following Quasi-Maximum Likelihood Poisson estimation in a firm-year panel:

$$E[Y_{ijst}|\mathcal{X}] = \ exp\big(\alpha_0 + \alpha_1 Transparency_{st} + \alpha_2 X_{ijst} + \ \gamma_i + \theta_{jt}\ \big). \tag{5}$$

We apply the Quasi-Maximum Likelihood Poisson estimation for patents and citations as dependent variables following Amore et al. (2013), Chava et al. (2013), and Bernstein (2015), because doing so can not only treat any non-negative dependent variables, whether integer or continuous (Santos Silva and Tenreyro, 2006), but its standard errors are also robust to arbitrary patterns of serial correlation (Wooldridge, 1999). As shown in Table IA7 in the Internet Appendix, the coefficients on $Transparency_{st}$ are significantly positive across all measures of inventor productivity.

Third, we exclude firms headquartered in California and Michigan—two states that adopted pay secrecy laws around the beginning of our sample period—and find a consistent result in Panel A of Table IA8 in the Internet Appendix. Fourth, in Panel B, we examine the relation between state-level pay secrecy laws and inventor productivity after we exclude companies headquartered in three states that adopted those laws around the end of our sample period: New Hampshire, Connecticut, and Oregon. We continue to find positive coefficients on *Transparency*. These results mitigate concerns that our results are driven by early or late treatment effects (Goodman-Bacon, 2021). In Panel C, we consider only states that eventually pass pay secrecy laws and find consistent results.

Fifth, because there is a concern that the not-to-compete covenant may affect corporate innovation (Marx et al., 2009; Png, 2017a, 2017b), we additionally control for the existence and enforcement of not-to-compete covenants.[26] Because our not-to-compete covenant data is available only up to 2011, the sample period is 1976-2011. We report these results in Table IA9 in the Internet Appendix. The coefficients on *Transparency* remain positive and significant at the 1% level.

Sixth, in Table IA10 in the Internet Appendix, we re-estimate Equation (2) by using several alternative measures of inventor productivity. In Panel A, we scale innovation output by the number of unique inventors who file patents with a firm in the same year, and find consistent results. In Panel B, we consider alternative measures of innovation output, including patent value, unadjusted citation counts (i.e., raw citation counts without any adjustment for the duration of patent citations), generality scores, and originality scores, all scaled by inventor number.[27] As reported in columns (1)-(4) of Table IA10 Panel B, the coefficients on *Transparency* are positive and significant at the 1% level. Taking column (1) as an example (in which the dependent variable represents patent value per inventor), the coefficient on *Transparency* is 0.047, which is higher than its counterparts in Table 3 (0.013 and 0.018 for patent count per inventor and forward citations per inventor, respectively), and is significant at the 1% level. Finally, in column (5), we re-estimate Equation (2) by using R&D expenditure scaled by the number of inventors as the dependent variable, and find an insignificant coefficient on *Transparency*. These results suggest that the relation between pay secrecy laws and inventor productivity is not driven by an increase in R&D investment.

Seventh, to further address the potential confounding effects of the creation of the Court of Appeals for the Federal Circuit (CAFC) and the enactment of the Bayh–Dole Act in the early

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[26] The dummy for the existence of not-to-compete covenants is one if a state enacted such covenants, and zero otherwise. The dummy for the enforcement of such covenants is one if a state indeed enforced them, and zero otherwise.

[27] Our patent value is the sum of values of all patents applied for by a firm in year *t* scaled by the number of inventors, and each patent's value is the stock market reaction to its grant news (Kogan et al., 2017). Based on the market efficiency hypothesis, stock investors assess the value of granted patents and adjust stock prices accordingly. The generality score of a patent is defined as one minus the Herfindahl index of the technology group distribution of all subsequent patents citing the patent (Hall et al., 2005a). The originality score of a patent is defined as one minus the Herfindahl index of the technology group distribution of all prior patents being cited by the patent (Hall et al., 2005a). The generality score of a patent reflects the breadth of purposes to which it can be applied, and the originality score of a patent reflects the breadth of knowledge from which it draws. A firm's originality (generality) score in a year is defined as the sum of the originality (generality) scores of all patents filed by a firm in a given year (Hsu et al., 2014).

1980s (Henry and Turner, 2006), we have conducted the following two tests. First, we start our sample in 1991 and find consistent results. Second, we drop patents that cite any university patents from our sample so that we can remove the effect of spillovers from universities' patents due to the Bayh–Dole Act.[28] These results are presented in Table IA11 in the Internet Appendix and are consistent with our baseline results, suggesting that our results cannot be entirely attributed to these confounding events.

Finally, we acknowledge that inventors in the PatentsView database may not be officially affiliated with the assignee firms because some inventors may be independent inventors who can sell their patents to firms. To mitigate this concern, we only consider an inventor as employed by a firm only after s/he has filed at least three patents that are assigned to the firm. Table IA12 in the Internet Appendix present consistent results, suggesting that our baseline finding is not subject to the issue of independent inventors.

3.7. Heterogeneous Effects

To further strengthen our argument that the effect of pay secrecy laws on inventor productivity is related to pay secrecy practices and rules in the workplace, we explore possible heterogeneous treatment effects. If improved inventor productivity after the passage of pay secrecy laws is due to reduced pay secrecy practices and rules in the workplace, then we expect this treatment effect to be stronger (i) in states with stronger *ex ante* pay secrecy practices and (ii) in firms with a higher ratio of minority inventors, who are more vulnerable to pay secrecy practices. Evidence from these tests helps alleviate concerns that our results are driven by omitted variables because it is quite unlikely that an omitted variable is correlated with the interaction terms (Claessens and Laeven, 2003; Raddatz, 2006).

For the first test, we measure the severity of pay secrecy practices and rules using ex ante unexplained pay differentials between white males and other groups. Ex ante unexplained pay differentials refer to the differences in hourly salaries that cannot be explained by observable characteristics (e.g., age, education, working hours) in 1980. In particular, we estimate the following ordinary least squares regression for all individuals in each state-industry (2-digit SIC industry) combination in 1980:

[28] By dropping patents that cite university patents, the number of patents covered in our sample drops by 12%.

$$Ln(Hourly\ wage)_j = \beta_0 + \beta_1 White\ male_j + \beta_2 Ln(Employee\ age)_j + \beta_3 College\ education_j + \beta_4 Ln(Annual\ working\ hour)_j + \varepsilon_j, \quad (6)$$

in which $Ln(Hourly\ wage)_{jt}$ is the natural logarithm of hourly wage by employee j. $White\ male_j$ is an indicator that represents white male employees.[29] All other variables have been defined in Appendix 2. Also, we obtain data from the IPUMS-CPS-ASEC database. The coefficient estimate of β_1 is our measure of the severity of state-industry-level unexplained pay differentials in state s in industry l in 1980. A larger value of β_1 indicates a greater difference in salary between white males and other groups that cannot be explained by observable characteristics and thus supports greater pay secrecy.

We first examine if existing pay gaps are reduced upon the passage of pay secrecy laws, especially for states with higher ex ante unexplained pay differentials. In particular, we estimate the following ordinary least squares regressions for a state-year panel:

$$Pay\ gap_{st} = \beta_0 + \beta_1 Transparency_{st} \times Larger\ pay\ secrecy_s + \beta_2 Transparency_{st} + \beta_3 Larger\ pay\ secrecy_{st} + \beta_4 X_{st} + \delta_s + \lambda_t + e_{st}, \quad (7)$$

in which $Pay\ gap_{st}$ denotes the unexplained pay differentials in state s in year t.[30] $Larger\ pay\ secrecy_s$ is an indicator that equals one if a firm is located in a state where the state-level average unexplained pay differential in 1980 is larger than the median of all observations, and zero otherwise.[31] X_{st} includes state-level control variables, such as GDP, personal income per capita, population, business combination laws, and wrongful discharge laws; δ_s denotes state fixed effects; and λ_t denotes year fixed effects. We provide the summary statistics of related variables in Panel C of Table 1. As reported in Table IA13 in the Internet Appendix, the coefficients on

[29] According to the instructions from the U.S. Census Bureau, white, black, and Asian are race categories, while Hispanic is an ethnicity category. In this paper, we do not distinguish between the concepts of race and ethnicity. For example, if an employee is both white and Hispanic, then that employee is categorized as Hispanic, and is also treated as non-white.

[30] We estimate Equation (6) in each state-industry for every year. We then use the average β_1 in each state-year as our measure of state-specific pay gap.

[31] We use the state-level average unexplained pay differential in 1980 to mitigate two endogeneity concerns. First, contemporaneous pay differentials may be related to local job market competitiveness that affects employees' work incentives and productivity. Second, contemporaneous pay differentials may reflect technological opportunities: firms observing new ways to employ technology may hire employees with specific skills or increase their compensation, both of which could lead to pay differentials.

$Transparency_{st} \times Larger\ pay\ secrecy_s$ are negative and significant in all columns, which is consistent with the expectation that pay secrecy laws close pay gaps.

We then further examine the heterogeneous treatment effects of pay secrecy laws on inventor productivity. In particular, we estimate the following regression that adds an interaction term to Equation (2):

$$Ln(Y_{ijst} + 1) = \alpha_0 + \alpha_1 Transparency_{st} \times Larger\ pay\ secrecy_{js} + \alpha_2 Transparency_{st} + \alpha_3 Larger\ pay\ secrecy_{js} + \alpha_4 X_{ijst} + \gamma_i + \theta_{jt} + \varepsilon_{jst}, \quad (8)$$

in which $Larger\ pay\ secrecy_{js}$ is an indicator that equals one if the state-industry level unexplained pay differentials in 1980 of a firm in industry *j* in state *s* is larger than the median of all observations, and zero otherwise. We report our results in Table IA14 in the Internet Appendix. We find that across both columns, the coefficients on *Transparency* × *Larger pay secrecy* are positive and significant. This finding supports heterogeneous treatment effects of pay secrecy laws: when a state is subject to greater pay secrecy practices before the passage of pay secrecy laws, the inventor productivity of firms in that state increases more after the passage of pay secrecy laws.

For the second test, we estimate Equation (8) by replacing *Larger pay secrecy* with *Higher minority ratio*, which equals one if a firm's ratio of minority inventors is above the sample median in the year, and zero otherwise.[32] We report our results in Table IA15 in the Internet Appendix. We find that the coefficients on *Transparency* × *Higher minority ratio* are positive and significant across all columns.

Overall, our interaction regression results suggest that the positive relation between pay secrecy laws and inventor productivity is indeed tied to pay secrecy practices and rules in the workplace; thus, such a relation is unlikely spuriously driven by unobserved heterogeneity and thus has a causal interpretation.

3.8. Alternative Explanations

We also acknowledge the following alternative explanations that are not related to pay secrecy practices and/or minority inventors but may explain our baseline findings. First, under pay

[32] We will define whether an inventor is a minority or not in Section 4.1.

secrecy laws, firms may disclose employees' base salaries but not other forms of compensation that may be tied to employees' performance indicators, such as patents. Thus, inventors may be incentivized to produce more patents after the passage of pay secrecy laws. If this explains our main finding, then we would expect inventors to increase patent output by "piecemeal patenting" (i.e., splitting big patents into several smaller ones). However, we do not find a change in the average number of claims, which is a common measure for piecemeal patenting (Tong and Frame, 1994; Dang and Motohashi, 2015), related to pay secrecy laws, as shown in columns (1) and (2) in Table IA16 in the Internet Appendix.

Second, we acknowledge that the passage of pay secrecy laws may increase the overall bargaining power of employees as well as the level of average wages. Thus, inventors may be motivated to exert more effort and produce more patents. To address this possibility, we examine whether the average employee compensation level increases after the adoption of the laws. We use a firm's selling, general, and administrative (SG&A) expenses scaled by the number of employees to measure its average compensation level, following Eisfeldt and Papanikolaou (2013). It is noteworthy that total wage and labor expenses are often missing in the Compustat database. On the other hand, a large part of SG&A consists of expenses related to human capital (e.g., white collar wages and training); it thus serves as a reasonable proxy for inventors' compensation level. As shown in column (3) in Table IA16 in the Internet Appendix, we do not find a significant increase in compensation after the passage of pay secrecy laws.

Third, as firms can no longer under-pay some inventors after the passage of pay secrecy laws, they face higher labor costs and may thus develop more labor-saving technologies, which will also result in more patent output. To address this issue, we use the new data of Ganglmair et al. (2021) in which each independent claim of a patent is classified as product-related or process-related. We calculate the average ratio of process claims of all patents filed by a firm in a year to measure firms' inclination toward labor-saving technologies that are more likely related to "process patents." As shown in column (4) in Table IA16 in the Internet Appendix, we do not find a significant increase in the ratio of process patents after the passage of pay secrecy laws.

4. Mechanisms

4.1. Motivating Inventors

The enactment of pay secrecy laws can motivate inventors to work harder and perform better from the perspectives of monetary incentives and morale. With respect to monetary incentives, pay transparency has been modelled as a way to reduce labor market frictions in Hsieh et al. (2019). In their framework, when pay is not transparent, employees may tend to believe that their salaries are subject to higher uncertainty, which discourages them from investing in human capital. In contrast, when pay secrecy laws enhance pay transparency, employees' payoffs for their effort become less ambiguous, which increases their motivation to work harder. With respect to morale, Cullen and Perez-Truglia (2022) show that when pay is secretive, employees may assume that other factors are at play, such as unconscious bias, wage compression, playing favorites, or discrimination. As these negative factors usually lead to employee disengagement, enhanced pay transparency associated with pay secrecy laws could strengthen inventors' morale and, in turn, increase their productivity.

To examine this mechanism, we identify inventors using the unique inventor ID based on a disambiguation algorithm in the "inventor" file from the PatentsView database. In our sample, we have 655,653 unique inventors. This sample includes all inventors working for our sample firms. We identify inventors' gender and ethnicity based on their first and last names in the "inventor" file in the PatentsView database. Also, we use each inventor's state information, which could be different from firms' headquarters states, and which is also more related to labor laws (including labor union laws and anti-discrimination laws).[33] The detailed procedure is provided in Appendix 3. We consider inventors working for firms that have "assignee_ID" in the database.

We implement a difference-in-differences analysis by estimating the following ordinary least squares regression for an inventor-year panel:[34]

$$Ln(Innovation_{kist} + 1) = \alpha_0 + \alpha_1 Transparency_{st} + \alpha_2 X_{ist} + FEs + \varepsilon_{kist}\ , \quad (9)$$

[33] Because 48% of inventor-year observations are in the same state as a firm's headquarters, we are unable to fully separate the effects from inventors' residential state and headquarters state in the same regression. Prior studies that connect labor laws to residential states include Gao and Zhang (2017) and Bloom et al. (2019).

[34] The use of inventor ID based on disambiguation algorithms can be traced back to Li et al. (2014) and has been widely used in the literature (e.g., Bernstein, 2015; Gao and Zhang, 2017; Galasso and Schankerman, 2018). However, we acknowledge that it is subject to errors in disambiguation algorithms, such as abbreviated first names and different inventors sharing similar or even identical names (Bernstein, 2015, page 1388). We use the first-time appearance of an inventor in the database as her first year, and use the last-time appearance of the inventor in the database as her last year. When the inventor does not have any patent records in a year between the first and the last year, we set her patent count and citation count to be zero in that year. For these inventors with only one patent (34% in our sample), they only exist in our sample in the year when their patent is filed.

in which $Innovation_{kist}$ is one of our inventor-level productivity measures, *InvPat* and *InvCit*. These measures denote the patent count and adjusted citation count of inventor k located in state *s* and employed by firm i in year *t*, respectively. For each inventor, we measure her contribution to a patent as 1 divided by the number of inventors for the patent. We then calculate that inventor's patent count (*InvPat*) by adding up her contributions to the patents that list her as an (co-)inventor and are applied for in a given year. The inventor's citation count (*InvCit*) is calculated as her contribution to each patent times the number of forward citations received by each of these patents. In Table 1 Panel D, we provide summary statistics of these innovation measures of inventor-year observations over the period 1976-2017. We control for firm characteristics and state characteristics (X_{ist}) as in Equation (2) (if our sample only includes public firms), as well as control for different combinations of fixed effects (FEs): firm fixed effects, year fixed effects, and inventor fixed effects. In some regressions, we also additionally control for the number of patents filed by firm i in year *t* to capture firms' innovation capability that likely positively influences individual inventors' performance (e.g., clustering of capable inventors and spillovers, internal competition pressure). The coefficient of interest in Equation (9) is α_1, which measures the effect of pay secrecy laws on inventor productivity.

We report our estimation results for Equation (9) in Table 5. We first consider public firms and include all firm characteristics and state characteristics, as well as firm and year fixed effects. We consider all inventors in columns (1) and (2); to avoid any possible confounding effects from inventor relocation, we only consider inventors who stay in the same state in all years (i.e., they have never moved) in columns (3) and (4).[35] We find that the coefficients on *Transparency* range between 0.0010 and 0.015, all of which are significant at least at the 1% level. In terms of economic significance, the coefficient estimates in columns (1) and (2) suggest that an inventor's patent count and citation count increase by 4.3% and 5.8%, respectively, after the passage of pay secrecy laws. These counts correspond to 0.013 more patents and 0.017 more citations for an average inventor in our sample who invents 0.300 patents per year that receive 0.292 forward citations before the passage of pay secrecy laws in her respective state.

[35] Hoisl (2007) provides empirical evidence that inventor productivity and relocation simultaneously affect each other. In addition, the literature suggests that more productive inventors tend to be more mobile (Stark and Bloom, 1985; Autor and Dorn, 2013; Gao and Zhang, 2017) and that inventors tend to relocate to places with more favorable compensation schemes (Akcigit et al., 2016). In the sample of inventor-year observations in public firms, 90.4% of unique inventors never moved states.

This positive relation holds in many different regression specifications and samples, as shown in the Internet Appendix. First, the positive relation remains even when we exclude firms' patent/citation counts, as we still obtain consistent results, which we provide in Table IA17 in the Internet Appendix. Second, we have also included inventor fixed effects and find consistent results in Table IA18 in the Internet Appendix. We do not include inventor fixed effects in our baseline results because (i) 34% of inventors have only one patent in our sample and (ii) a large portion of (53%) of inventor-year observations do not have any patent records (and thus their output is set to zero).[36] Thus, including inventor fixed effects may give us a lower power. Finally, when we implement similar tests to include firms that are *not* publicly listed, we find consistent results, which we provide in Table IA19 in the Internet Appendix. Overall, our results support the mechanism that inventors are motivated to work harder after pay secrecy laws.

4.2. Motivating Minority Inventors

Although pay transparency could improve all employees' motivation, it could have a particularly stronger effect on minority employees, as such employees are traditionally disadvantaged in the workplace and face a higher likelihood of being underpaid (Kim, 2015). We thus implement a difference-in-differences analysis by estimating the following ordinary least squares regression for an inventor-year panel:

$$Ln\left(Innovation'_{kist} + 1\right) = \alpha_0 + \alpha_1 Transparency_{st} \times Minority_{kist} + \alpha_2 Transparency_{st} + \alpha_3 X_{ist} + FEs + \varepsilon_{ijst}, \qquad (10)$$

in which all independent variables have been defined as in Equation (9) except $Minority_{kist}$, which takes the value of one if the inventor is not a white male, and zero otherwise. We require that sample inventors have identifiable gender and ethnicity information. Among unique inventors, 18.91% are minority inventors. For the dependent variables, $Innovation'_{kist}$, we only consider the values corresponding to patents that are coinvented by either all minority inventors or all non-minority inventors. This way, we can clearly separate the effect of pay secrecy laws on these two types of inventors and circumvent the issue that the post-event increase of inventor productivity

[36] Controlling for inventor fixed effects would allow us to disentangle whether our main findings are driven by the same pool of inventors becoming more productive over time (i.e., the treatment effect) or by the firm's composition of inventors changing over time (i.e., the composition effect). The coefficient estimates on *Transparency* in Table IA18 are of similar magnitudes as those in Table 5, suggesting that the results we report in the inventor level are more attributable to the former effect.

results from either slightly more efforts from non-minority inventors or better collaboration within teams (e.g., Koning et al., 2021).

We present the results in Table 6 for all inventors and stayer inventors. Specifically, the coefficients on *Transparency* × *Minority* are significantly positive in all but one column. These results indicate that the positive relation between pay secrecy laws and inventor productivity is stronger for minority inventors than their counterparts. On the other hand, the coefficients on *Transparency* remain positive and largely significant, suggesting that even non-minority inventors' productivity increases after pay secrecy laws. Thus, our results in Table 6 support the mechanism that inventors, especially minority inventors, are motivated to exert more effort because of less discrimination and clearer expected payoffs. This finding is also consistent with our firm-level evidence of a stronger treatment effect in firms with a larger proportion of minority inventors (see Table IA15 of the Internet Appendix).

To further examine the motivation mechanism, we estimate Equation (10) in subsamples of high and low average employee compensation (measured by SG&A expenses scaled by the number of employees) and present the results in Table IA20 in the Internet Appendix. We find that the coefficients on *Transparency* × *Minority* are significantly positive (insignificant) in the subsample of high (low) average employee compensation. These results suggest that minority inventors are motivated to exert more effort and produce more patents when they observe their peers' higher compensation. In addition, we notice that the coefficients on *Transparency* are significantly positive (insignificant) in the subsample of high (low) average employee compensation. This finding is intuitive because, for firms with lower compensation, the disclosure of peers' salary will not motivate any inventors to work harder.

4.3. Diversity in Inventor Teamwork

Our second mechanism posits that pay secrecy laws contribute to more diversified inventor teams, which produce more high-quality innovation output. We expect that pay secrecy laws will increase the diversity of a firm's inventor teams and thus enhance output quality. Employees with a variety of backgrounds may provide diverse perspectives, valuable ideas, and problem-solving abilities, which all facilitate optimal, creative solutions and innovation (Drach-Zahavy and Somech, 2001; Berliant and Fujita, 2011). Hong and Page (2001) construct a model of heterogeneous agents of bounded ability and analyze their individual and collective efforts to find solutions to difficult problems (e.g., searching for new cancer treatments, developing new software). Their model predicts that diverse perspectives and heuristics among these individuals help generate optimal solutions to address these problems. Empirical studies on group decision-making also find that groups consisting of more diverse individuals produce higher quality and more innovative decisions than groups of homogenous individuals (Watson et al., 1993; Amason, 1996). As this literature stream predicts that firms with greater workforce diversity are more innovative, another channel for pay secrecy laws to enhance inventor productivity is to diversify inventor teams. After a state passes pay secrecy laws, we expect that inventor teams in that state become more diversified, as minority inventors are more willing/able to cooperate with others.

To test this mechanism, we measure diversity within the inventor team for each patent by using the Shannon Index of Diversity (Shannon, 1948) based on the distribution of inventors' ethnicity and gender (all details are provided in Appendix 3). We provide the summary statistics of both diversity measures in Panel E of Table 1.

To examine how pay secrecy laws enhance inventor productivity through increasing inventor diversity, we estimate the following two ordinary least squares regressions at the patent level:

$$Diversity_{kst} = \alpha_0 + \alpha_1 Transparency_{st} + \gamma_i + \pi_h + \theta_t + \varepsilon_{kst}, \tag{11}$$

$$Ln(1 + PatentCitation\ or\ PatentValue)_{kst} = \beta_0 + \beta_1 Diversity_{kst} + \lambda_i + \Phi_h + \mu_t + e_{kst}, \tag{12}$$

in which $Diversity_{kst}$ is racial and gender diversity for the inventor team of patent k that is in technology group h and is filed by firm i located in state s in year t.[37] $PatentCitation$ and $PatentValue$ are the number of forward citations received by patent k and its patent value, respectively. γ_i, π_h, and θ_t denote firm, group, and year fixed effects, respectively, in Equation (11), and λ_i, $\Phi_{h,}$ and μ_t denote firm, group, and year fixed effects, respectively, in Equation (12).

Equation (11) estimates how a patent's inventor diversity changes after the passage of pay secrecy laws. Equation (12) examines how patent citations and values are associated with team diversity. Table 7 presents our results. In Panel A, we focus on racial diversity. As we show in column (1), the coefficient on *Transparency* is positive and significant at the 1% level, indicating

[37] We use the majority of co-inventors to define the location of an inventor team.

that the racial diversity of inventor teams significantly increases after the passage of pay secrecy laws.[38] In column (2), we find that diversity has a positive and significant coefficient (at the 1% level), which supports the proposition that diversity in inventor teams helps to enhance patent quality. These findings are consistent with those of prior studies that suggest groups with more diversified members solve problems more creatively and efficiently (e.g., Wiersema and Bantel 1992; Watson et al., 1993; Hambrick et al., 1996; Hong and Page, 2001). In Panel B, we focus on gender diversity and find a consistent result, which echoes Yang et al. (2022) that gender-diverse teams produce more novel and higher-impact publications. Our results from Table 7 support the view that one mechanism for pay secrecy laws to stimulate inventor productivity is to increase the diversity of inventor teams.

4.4. Expected Promotion Raise

Our third mechanism suggests that, after the passage of pay secrecy laws, compensation for senior colleagues becomes more visible. Thus, for companies with a larger increase in promotion raises, all inventors should be encouraged to exert more effort to pursue promotions. To test this mechanism, we measure expected promotion raises associated with promotions in a firm by using the ratio of CEO compensation to average employee compensation. We then implement a difference-in-differences analysis by estimating the following ordinary least squares regression for firm-year observations:

$$Ln\left(Y_{ijst}+1\right) = \alpha_0 + \alpha_1 Transparency_{st} \times Higher\ promotion\ raises_{it} + \alpha_2 Transparency_{st} + \alpha_3 Higher\ promotion\ raise_{it} + \alpha_4 X_{ijst} + \gamma_i + \theta_{jt} + \varepsilon_{ijst}\ , \qquad (13)$$

in which all variables have been defined as in Equation (2) except $Higher\ promotion\ raises_{it}$, which equals one if a firm's ratio of CEO compensation to average employee compensation is above the median of all the firms in the year, and zero otherwise. The ratio of CEO compensation to average employee compensation is measured as the total compensation of the CEO divided by average selling, general, and administrative (SG&A) expenses per employee.[39] We obtain CEO

[38] Another interpretation to explain the increase in team member diversity could be that minority inventors now receive more credit for their work and are thus listed as co-inventors in documents.

[39] As discussed earlier, we cannot use the wage in the Compustat data because a large portion of firms do not report that accounting item. Instead, we use SG&A expenses following the literature (Eisfeldt and Papanikolaou, 2013) as it includes most human capital expenses (e.g., white collar wage and training costs). We do not control for business

compensation data from the ExecuComp database, because it only covers S&P 1500 firms since 1992, the sample size is much smaller.

As shown in Table 8, the coefficients on *Transparency* × *Higher promotion raises* are significantly positive in all columns, confirming our third mechanism that inventors are encouraged to pursue promotions when they observe their senior colleagues' salaries. On the other hand, the coefficients on *Transparency* are insignificant. These results indicate that the positive relation between pay secrecy laws and inventor productivity is only concentrated in firms with high expected promotion raises. This finding is intuitive because pay transparency should not matter for firms with fairly flat compensation structure.

5. Conclusion

Does pay transparency affect productivity? In this paper, we propose and empirically test the effect of pay transparency on corporate innovation and inventor productivity by exploiting the staggered adoption of pay secrecy laws in different U.S. states. We find a significant increase in firms' inventor productivity following the passage of pay secrecy laws, relative to firms in states without such laws. These results are robust to various alternative specifications. We also show that the adoption of pay secrecy laws mitigates the pay differentials between minority employees and their counterparts, supporting the relevance of these laws. We find that the adoption of pay secrecy laws is unrelated to the pre-existing innovation of local firms, mitigating the concern of reverse causality. Various tests indicate that there is no time trend difference in inventor productivity between the treated group and the control group prior to the passage of pay secrecy laws, and that the improvement in inventor productivity occurs several years after the passage of such laws. Further, we present heterogeneous treatment effects suggesting that these treatment effects are indeed related to pay secrecy practices and rules in the workplace. All these results collectively support a positive effect of pay transparency on productivity in terms of innovation activities.

Moreover, we provide some suggestive evidence to support the three underlying mechanisms for pay secrecy laws to promote inventor productivity: (i) motivating inventors (especially minority inventors) to exert more effort; (ii) increasing the diversity of inventor teams;

combination laws in this test because although CEO pay is known since 1992, all business combination laws passed before 1992.

and (iii) encouraging inventors to pursue promotions. Overall, our findings suggest that a transparent pay system helps spur corporate innovation and individual productivity.

We acknowledge that a causal interpretation of our analysis presumes that these laws are exogenous to firms' innovation activities. While we can neither claim that such legislation is entirely exogeneous nor rule out all endogeneity concerns, we believe that the political factors that underpin these laws and our explanation of firms' and managers' opposition to such laws, as well as all of our empirical analyses (including additional checks for staggered difference-in-differences, state-specific pre-trends, the dynamic effect analysis, hazard model estimation, analysis of heterogeneous treatment effects, and all mechanism tests), collectively mitigate endogeneity concerns to a certain extent.

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Appendix 1: List of States Legislating Pay Secrecy Laws

Information is provided by the U.S. Department of Labor (Permanent Link: https://hdl.handle.net/1813/78735)

State	Pass year	Details
Michigan	1982	Mich. Comp. Laws Section 408.483a Prohibited conduct. Sec. 13a. (1) An employer shall not do any of the following: (a) Require as a condition of employment nondisclosure by an employee of his or her wages. (b) Require an employee to sign a waiver or other document which purports to deny an employee the right to disclose his or her wages. (c) Discharge, formally discipline, or otherwise discriminate against for job advancement an employee who discloses his or her wages. This provision was added to Act 390 of 1978, Payment of Wages and Fringe Benefits, by Act 524 of 1982, effective March 30, 1983.
California	1984	Labor Code, Section 232 "No employer may do any of the following: a. Require, as a condition of employment, that an employee refrain from disclosing the amount of his or her wages. b. Require an employee to sign a waiver or other document that purports to deny the employee the right to disclose the amount of his or her wages. c. Discharge, formally discipline, or otherwise discriminate against an employee who discloses the amount of his or her wages."
Illinois	2003	ST CH 820 § 112/10 Sec. 10. Prohibited Acts. (b) It is unlawful for any employer to interfere with, restrain, or deny the exercise of or the attempt to exercise any right provided under this Act [Equal Pay Act of 2003]. It is unlawful for any employer to discharge or in any other manner discriminate against any individual for inquiring about, disclosing, comparing, or otherwise discussing the employee's wages or the wages of any other employee, or aiding or encouraging any person to exercise his or her rights under this Act.
Vermont	2005	Title 21 (Labor), Chapter 5 (Employment Practices), Sec. 495 (Unlawful Employment Practices). Sec. 495(a) It shall be unlawful employment practice, except where a bona fide occupational qualification requires persons of a particular race, color, religion, national origin, sex, sexual orientation, gender identity, ancestry, place of birth, age, or physical or mental condition: (7)(B)(i) No employer may do any of the following: (I) Require, as a condition of employment, that an employee refrain from disclosing the amount of his or her wages or from inquiring about or discussing the wages of other employees. (II) Require an employee to sign a waiver or other document that purports to deny the employee the right to disclose the amount of his or her wages or to inquire about or discuss the wages of other employees. (ii) Unless otherwise required by law, an employer may prohibit a human resources manager from disclosing the wages of other employees. (8) Retaliation prohibited. An employer, employment agency, or labor organization shall not discharge or in any other manner discriminate against any employee because the employee: (D) has disclosed his or her wages or has inquired about or discussed the wages of other employees.
Maine	2009	Chapter 29, S.P. 33 – L.D. 84, An Act to Ensure Fair Pay, effective 9/12/09

		Sec.1. 26 MRSA Sec. 628, first paragraph, as amended by PL 2001, c. 304, Sec. 2, is further amended to read: "An employer may not discriminate between employees in the same establishment on the basis of sex by paying wages to any employee in any occupation in this State at a rate less than the rate at which the employer pays any employee of the opposite sex for comparable work on jobs that have comparable requirements relating to skill, effort and responsibility. Differentials that are paid pursuant to established seniority systems or merit increase systems or difference in the shift or time of the day worked that do not discriminate on the basis of sex are not within this prohibition. An employer may not discharge or discriminate against any employee by reason of any action taken by such employee to invoke or assist in any manner the enforcement of this section. An employer may not prohibit an employee from disclosing the employee's own wages or from inquiring about another employee's wages if the purpose of the disclosure or inquiry is to enforce the rights granted by this section. Nothing in this section creates an obligation to disclose wages."
Colorado	2009	Senate Bill 08-122, approved 4/17/08 Sec. 1. 24-34-402(1), Colorado Revised Statutes, is amended BY THE ADDITION OF A NEW PARAGRAPH to read: 24-34-402.Discriminatory or unfair employment practices. (1) It shall be a discriminatory or unfair employment practice: (i) unless otherwise permitted by federal law, for an employer to discharge, discipline, discriminate against, coerce, intimidate, threaten, or interfere with any employee or other person because the employee inquired about, disclosed, compared, or otherwise discussed the employee's wages; to require as a condition of employment nondisclosure by an employee of his or her wages; or to require an employee to sign a waiver or other document that purports to deny an employee the right to disclose his or her wage information. this paragraph (i) shall not apply to employers who are exempt from the provisions of the 'national labor relations act,' 29 u.s.c. sec. 151 et seq.
Louisiana	2013	Chapter 6-A (Louisiana Equal Pay for Women Act) of Title 23 of the Louisiana Revised Statutes of 1950 § 664. Prohibited acts D. It shall be unlawful for an employer to interfere with, restrain, or deny the exercise of, or attempt to exercise, any right provided under this Chapter. It shall be unlawful for any employer to discriminate, retaliate, or take any adverse employment action, including but not limited to termination or in any other manner discriminate against any employee for inquiring about, disclosing, comparing, or otherwise discussing the employee's wages or the wages of any other employee, or aiding or encouraging any other employee to exercise his or her rights under this Chapter. Note: This Act applies only to any department, office, division, agency, commission, board, committee or other organizational unit of the state.
New Jersey	2013	Title 10. Civil Rights Sec. 10:5-12. Unlawful employment practices, discrimination. 11. It shall be an unlawful employment practice, or, as the case may be, an unlawful discrimination: r. For any employer to take reprisals against any employee for requesting from any other employee or former employee of the employer information regarding the job title, occupational category, and rate of compensation, including benefits, of any employee or former employee of the employer, or the gender, race, ethnicity, military status, or national origin of any employee or former employee of the employer, regardless of whether the request was responded to, if the purpose of the request for the information was to assist in investigating the possibility of

		the occurrence of, or in taking of legal action regarding, potential discriminatory treatment concerning pay, compensation, bonuses, other compensation, or benefits. Nothing in this subsection shall be construed to require an employee to disclose such information about the employee herself to any other employee or former employee of the employer or to any authorized representative of the other employee or former employee.
Minnesota	2014	Ch. 239—H.F. No. 2536 Article 3. Labor Standards and Wages Sec. 2. [181.172] WAGE DISCLOSURE PROTECTION. (a) An employer shall not: (1) require nondisclosure by an employee of his or her wages as a condition of employment; (2) require an employee to sign a waiver or other document which purports to deny an employee the right to disclose the employee's wages; or (3) take any adverse employment action against an employee for disclosing the employee's own wages or discussing another employee's wages which have been disclosed voluntarily. (b) Nothing in this section shall be construed to: (1) create an obligation on any employer or employee to disclose wages; (2) permit an employee, without the written consent of the employer, to disclose proprietary information, trade secret information, or information that is otherwise subject to a legal privilege or protected by law; (3) diminish any existing rights under the National Labor Relations Act under United States Code, title 29; or (4) permit the employee to disclose wage information of other employees to a competitor of their employer. (c) An employer that provides an employee handbook to its employees must include in the handbook notice of employee rights and remedies under this section. (d) An employer may not retaliate against an employee for asserting rights or remedies under this section. (e) An employee may bring a civil action against an employer for a violation of paragraph (a) or (d). If a court finds that an employer has violated paragraph (a) or (d), the court may order reinstatement, back pay, restoration of lost service credit, if appropriate, and the expungement of any related adverse records of an employee who was the subject of the violation.
Connecticut	2015	Conn. Gen. Stat. § 31-40z(b)-(c) (b) No employer shall: (1) Prohibit an employee from disclosing or discussing the amount of his or her wages or the wages of another employee of such employer that have been disclosed voluntarily by such other employee; (2) Prohibit an employee from inquiring about the wages of another employee of such employer; (3) Require an employee to sign a waiver or other document that denies the employee his or her right to disclose or discuss the amount of his or her wages or the wages of another employee of such employer that have been disclosed voluntarily by such other employee; (4) Require an employee to sign a waiver or other document that denies the employee his or her right to inquire about the wages of another employee of such employer; (5) Discharge, discipline, discriminate against, retaliate against or otherwise penalize any employee who discloses or discusses the amount of his or her wages or the wages of another employee of such employer that have been disclosed voluntarily by such other employee; or

		(6) Discharge, discipline, discriminate against, retaliate against or otherwise penalize any employee who inquires about the wages of another employee of such employer. (c) Nothing in this section shall be construed to require any employer or employee to disclose the amount of wages paid to any employee.
New Hampshire	2015	N.H. Rev. Stat. § 275:38-a(I)(b)-(II) I. No employer shall discharge or in any other manner discriminate against any employee because he or she: (b) Inquired about, discussed, or disclosed his or her wages or those of another employee. II. This section shall not apply to any employee who has access to the wage information of other employees as a part of such employee's essential job functions who discloses the wages of such other employees to individuals who do not otherwise have access to such information, unless such disclosure is in response to a complaint or charge or in furtherance of an investigation, proceeding, hearing, or action under RSA 275:41-a including an investigation conducted by the employer. Nothing in this section shall be construed to limit the rights of an employee provided under any other provision of law. N.H. Rev. Stat. § 275:41-b(I)-(II) I. No employer shall require the following as a condition of employment: (a) That an employee refrain from disclosing the amount of his or her wages. (b) That an employee sign a waiver or other document that purports to deny the employee the right to disclose the amount of his or her wages, salary, or paid benefits. II. No employer shall discharge, formally discipline, or otherwise discriminate against an employee who discloses the amount of his or her wages, salary, or paid benefits.
Oregon	2016	Or. Rev. Stat. § 659A.355(1)-(2) (1) It is an unlawful employment practice for an employer to discharge, demote or suspend, or to discriminate or retaliate against, an employee with regard to promotion, compensation or other terms, conditions or privileges of employment because the employee has: (a) Inquired about, discussed or disclosed in any manner the wages of the employee or of another employee; or (b) Made a charge, filed a complaint or instituted, or caused to be instituted, an investigation, proceeding, hearing or action based on the disclosure of wage information by the employee. (2) This section does not apply to an employee who has access to wage information of employees as part of the job functions of the employee's position and discloses the wages of those employees to individuals not authorized access to the information, unless the disclosure is in response to a charge or complaint or is in furtherance of an investigation, proceeding, hearing or action, including but not limited to an investigation conducted by the employer.

Appendix 2: Variable Definitions

Variable	*Definition*
Measures of Innovation Output	
Pat/Inventor	Number of patents filed (and subsequently awarded) by a firm in a year scaled by the number of unique inventors working for the firm in a ten-year rolling window. We only have comprehensive coverage of patents awarded since 1976 due to the truncation issue in available patent data. Thus, for each firm-year observation in the 1976-1983 period, we proportionately adjust its denominator, which is the number of unique inventors by year t multiplied by 10/(t-1974).
Cit/Inventor	Number of adjusted forward citations received by all the patents that are applied for (and subsequently awarded) by a firm in a given year scaled by the number of unique inventors working for the firm in a ten-year rolling window. The number of adjusted forward citations of each patent is defined as the number of forward citations received by the patent (within a five-year window from its grant year) scaled by the average forward citations received by all patents in the same CPC 4-digit group.
Pat/Inventor1	Number of patents filed (and subsequently awarded) by a firm in a year scaled by the number of unique inventors who file patents with the firm in the same year.
Cit/Inventor1	Number of adjusted forward citations received by all the patents that are applied for (and subsequently awarded) by a firm in a given year scaled by the number of unique inventors who file patents with the firm in the same year.
Val/Inventor	Sum of values of all patents applied for by a firm in year *t* scaled by the number of inventors; each patent's value is the stock market reaction to its grant news (Kogan et al., 2017).
Ucit/Inventor	Number of all forward citations received by all the patents that are applied for (and subsequently awarded) by a firm in a given year scaled by the number of unique inventors working for the firm in a ten-year rolling window.
Gen/Inventor	A firm's generality score in a given year scaled by the number of unique inventors working for the firm in a ten-year rolling window. The generality score of a patent is defined as one minus the Herfindahl index of the technology group distribution of all subsequent patents citing the patent. A firm's generality score in a year is defined as the sum of the generality scores of all patents filed by a firm in a given year.
Ori/Inventor	A firm's originality score in a given year scaled by the number of unique inventors working for the firm in a ten-year rolling window. The originality score of a patent is defined as one minus the Herfindahl index of the technology group distribution of all prior patents being cited by the patent. A firm's originality score in a year is defined as the sum of the originality scores of all patents filed by a firm in a given year.
R&D/Inventor	R&D expenditures by a firm in a given year scaled by the number of unique inventors working for the firm in a ten-year rolling window.
Claims	The average number of claims of all the patents that are filed (and subsequently awarded) by a firm in a year.
LnPat/Inventor	Natural logarithm of one plus Pat/Inventor.
LnCit/Inventor	Natural logarithm of one plus Cit/Inventor.
LnPat/Inventor1	Natural logarithm of one plus Pat/Inventor1.
LnCit/Inventor1	Natural logarithm of one plus Cit/Inventor1.
LnClaims	Natural logarithm of Claims.

Process claims	The average ratio of process claims of all the patents that are filed (and subsequently awarded) by a firm in a year.
LnVal/Inventor	Natural logarithm of one plus Val/Inventor.
LnUcit/Inventor	Natural logarithm of one plus UCit/Inventor.
LnGen/Inventor	Natural logarithm of one plus Gen/Inventor.
LnOri/Inventor	Natural logarithm of one plus Ori/Inventor.
LnR&D/Inventor	Natural logarithm of one plus R&D/Inventor.

Firm Characteristics

Assets	Total assets.
Cash	Cash and short-term investments normalized by total assets.
R&D	R&D expenditures normalized by total assets. If the R&D expenditures variable is missing, we set the missing value to zero.
R&D missing	An indicator variable that equals one if the R&D expenditures variable is missing, and zero otherwise.
ROA	Net income normalized by total assets.
PPE	Gross property, plant, & equipment normalized by total assets.
Leverage	Total debt normalized by total assets.
Capex	Capital expenditures normalized by total assets. If the capital expenditures variable is missing, we set the missing value to zero.
Tobin's Q	Market value of equity plus the book value of total assets minus the book value of equity minus balance sheet deferred taxes, normalized by the book value of total assets.
Age	Number of years since a firm's first appearance in the Compustat database.
	In Table IA19 for which the sample includes both public firms and private firms, Age is defined as the number of years since a firm's first appearance in the innovation database.
Higher minority ratio	An indicator variable that equals one if the percentage of minority inventors in a firm is larger than the median of all the firms in the year, and zero otherwise. The percentage of minority inventors is calculated as the number of minority inventors/(number of minority inventors + number of majority inventors), all measured in a ten-year rolling window.
Higher promotion raises	An indicator variable that equals one if a firm's promotion raise is larger than the median of all the firms in the year, and zero otherwise. CEO-employee pay disparity is measured as the total compensation of a CEO divided by average selling, general, and administrative expenses per employee.
Average compensation	Selling, general, and administrative expenses divided by the number of employees.
Larger pay secrecy (state-industry)	An indicator variable that equals one if the state-industry group level unexplained pay differentials in 1980 of a firm is larger than the median of all observations, and zero otherwise.

State Characteristics

Transparency	An indicator variable that equals one if the state has adopted pay secrecy laws in a given year, and zero otherwise.
Pay gap	We first estimate β_1 from the following regression of all individuals within each state-industry-year combination: $Ln(Hourly\ wage)_{jt} = \beta_0 + \beta_1 White\ male_j + \beta_2 Ln(Employee\ age)_{jt} + \beta_3 College\ education_{jt} + \beta_4 Ln(Annual\ working\ hour)_{jt} + \varepsilon_{jt}$. We then use the average β_1 in each state-year as our measure of state-year level pay gap.
State GDP	Annual GDP of a given state.
Per capita income	Annual personal income per capita in a given state.
State population	Population of a given state.
State unemployment rate	The unemployment rate of a state.
Republican governor	An indicator variable that equals one if the state is governed by a Republican in a given year, and zero otherwise. The value is always zero for companies located in D.C.
State education	Percentage of the labor force who finish 4-years' college education in a given state.
Percentage of males	Percentage of males in the labor force in a given state.
Percentage of whites	Percentage of whites in the labor force in a given state.
Business combination	An indicator variable that equals one if the state adopts business combination laws, following Bertrand and Mullainathan (2003).
Good faith	An indicator variable that equals one if the state adopts the good-faith exception, following Autor et al. (2006).
Ln(Average Pat/Inventor)	Natural logarithm of one plus the average Pat/Inventor across all public firms headquartered in a state.
Ln(Average Cit/Inventor)	Natural logarithm of one plus the average Cit/Inventor across all public firms headquartered in a state.
Larger pay secrecy (state)	An indicator variable that equals one if the state-level unexplained pay differentials in 1980 of a firm is larger than the median of all observations, and zero otherwise.
Existence of CNC	An indicator variable for the existence of not-to-compete covenants that equals one if a state enacted such covenants, and zero otherwise.
Enforcement of CNC	An indicator variable for the enforcement of not-to-compete covenants that equals one if a state indeed enforced them, and zero otherwise.
Inventor Characteristics	
LnInvPat	Natural logarithm of one plus the number of patents filed by an inventor in a year. If X inventors file a patent together, we deem each inventor filed 1/X patents.
LnInvCIt	Natural logarithm of one plus the number of forward citations of the patents filed by an inventor in a year. If X inventors file a patent together, we normalize the total forward citations by inventor number.
LnInvPat'	Natural logarithm of one plus the number of patents filed by an inventor in a year. If X inventors file a patent together, we deem each inventor filed 1/X patents. We only include patents for which all the inventors are majority or minority. However, the inventor team may include inventors whose gender or race are not identifiable.
LnInvCIt'	Natural logarithm of one plus the number of forward citations of the patents filed by an inventor in a year. If X inventors file a patent together, we normalize the total forward citations by inventor number. We only include patents for which all the

	inventors are majority or minority. However, the inventor team may include inventors whose gender or race are not identifiable.
LnInvClaims	Natural logarithm of one plus the average number of claims per patent filed by an inventor in a year.
Minority	An indicator variable that equals one if an inventor is not a white male, and zero otherwise.
Ln(Number Patent)	Natural logarithm of one plus the number of patents that are applied for (and subsequently awarded) by a firm in a given year.
Ln(Number Citation)	Natural logarithm of one plus the number of adjusted forward citations received by all patents that are applied for (and subsequently awarded) by a firm in a given year. The number of adjusted forward citations of each patent is defined as the number of forward citations received by the patent (within a five-year window from its grant year) scaled by the average forward citations received by all patents in the same CPC 4-digit group.
Patent/Team Characteristics	
Inventor diversity (race)	$\sum_{i=1}^{5} Proportion\ of\ the\ race_i * Ln(\frac{1}{Proportion\ of\ the\ race_i})$ in which *Proportion of the race* is the number of inventors from a given ethnic group divided by the total number of inventors in the team. The detailed procedure for ethnicity identification is provided in Appendix 3.
Inventor diversity (gender)	$\sum_{i=1}^{2} Proportion\ of\ the\ gender_i * Ln(\frac{1}{Proportion\ of\ the\ gender_i})$ in which *Proportion of the gender* is the number of people from a given gender group divided by the total number of inventors in the team. The detailed procedure for gender identification is provided in Appendix 3.
PatentCitation	The number of adjusted forward citations received by a patent. The number of adjusted forward citations of each patent is defined as the number of forward citations received by the patent (within a five-year window from its grant year) scaled by the average forward citations received by all patents in the same CPC 4-digit group.
PatentValue	A patent's value is the stock market reaction to its grant news as defined in Kogan et al. (2017).
Scientist/Engineer Characteristics	
Minority	An indicator variable that equals one if an inventor is not a white male, and zero otherwise.
Employee age	Age of the person.
Annual working hours	Number of working weeks in a calendar year times usual working hours per week.
Hourly wage	Annual wage divided by annual working hours, adjusted to 1999 dollars.
College education	An indicator variable that equals one if the person has completed a college education, and zero otherwise.
Postgraduate	An indicator variable that equals one if the person has a postgraduate degree, and zero otherwise.

Appendix 3: Identification of Each Inventor's Ethnicity/Gender

We collect names data from the PatentsView database, which includes every inventor's last name, first name, and middle name (if available).

1. Ethnicity

We use the last name database provided by the U.S. Census Bureau to determine the ethnic group of an inventor by her last name. The database is available at http://www2.census.gov/topics/genealogy/2000surnames/names.zip. The U.S. Census Bureau constructed the last name database based on the name responses from almost 270 million people in the 2000 Census. The data file covers all 151,671 surnames that occur 100 or more times. The database presents the distribution of last names in five ethnic groups: White, Black, Asian and Pacific Islander, American Indian and Alaskan Native, and Hispanic.

We then assign an inventor to an ethnic group if more than 50% of people who use the same surname belong to that ethnic group. For example, as shown in the following example, 96.34% people using the last name "Lefebvre" are white, and 90.27% people using the last name "Batiste" are black. Thus, we assume that an inventor with the last name "Lefebvre" is white, and that an inventor using the last name "Batiste" is black.

A	B	C	D	E	F	G	H	I	J	K
name	rank	count	prop100k	cum_prop100k	pctwhite	pctblack	pctapi	pctaian	pct2prace	pcthispanic
LEFEBVRE	5354	5991	2.22	60919.71	96.34	0.35	0.48	0.47	1.12	1.24
BATISTE	5355	5990	2.22	60921.93	4.41	90.27	0.28	1.02	2.47	1.55

For an inventor using a surname that is not used by at least 50% people of any ethnic group, we cannot determine the person's ethnic group. For example, among all the people using the last name "Lee," 40.09% are white, and 37.83% are Asian and Pacific Islander. Thus, we do not assign inventors with the last name "Lee" to any ethnic group. For inventors not using any of the 151,671 surnames, we cannot determine their ethnicity groups.

2. Gender

We use the first name databases provided by the U.S. Census Bureau to determine the gender of an inventor by her first name. In rare cases in which an inventor's first name is not available but her middle name is available, we use the middle name to determine gender. For example, there are four inventors listed in the following example. We use the name "Fernand" for the first person and use the name "Jenny" for the third and fourth person, but we cannot determine the gender of the second person.

name_first	name_last
Fernand	Chetelat
F. A.	Klasek
Jenny L.	Gilles
J. Jenny	Yuan

The U.S. Census Bureau constructed the first name databases based on the name responses from 7.2 million people in the 1990 Census. The male first name database covers the 1,219 most popular first names that represent 90% of the male population; these data can be downloaded at http://www2.census.gov/topics/genealogy/1990surnames/dist.male.first. The female first name database covers the 4,275 most popular first names that represent 90% of the female population; these data can be downloaded at http://www2.census.gov/topics/genealogy/1990surnames/dist.female.first. Both databases show the distribution of each first name for each gender. For example, as shown in the following example from the female first name database, the name "Mary" is used by 2.629% females, and the name "Patricia" is used by 1.073% females.

```
MARY            2.629
PATRICIA        1.073
LINDA           1.035
BARBARA         0.980
ELIZABETH       0.937
JENNIFER        0.932
```

If an inventor's first name is covered by the male first name database, we assign this person as male. If an inventor's first name is covered by the female first name database, we assign this person as female. More importantly, when an inventor's first name is covered by both databases, we assume the inventor is male (female) if the percentage of males with this first name is larger (smaller) than the percentage of females with this first name. For example, the first name "Robin" is used by 0.208% females, and is used by 0.032% males. Thus, for an inventor using the first name "Robin," we assume this person is female. For an inventor whose first name is used by males and females with similar probabilities, we cannot determine that person's gender. For example, the first name "Ariel" is used by 0.007% of males and 0.007% of females. Thus, we cannot determine the gender of inventors with the first name "Ariel." For inventors whose first names are not covered by the first name databases, we also cannot determine their genders.

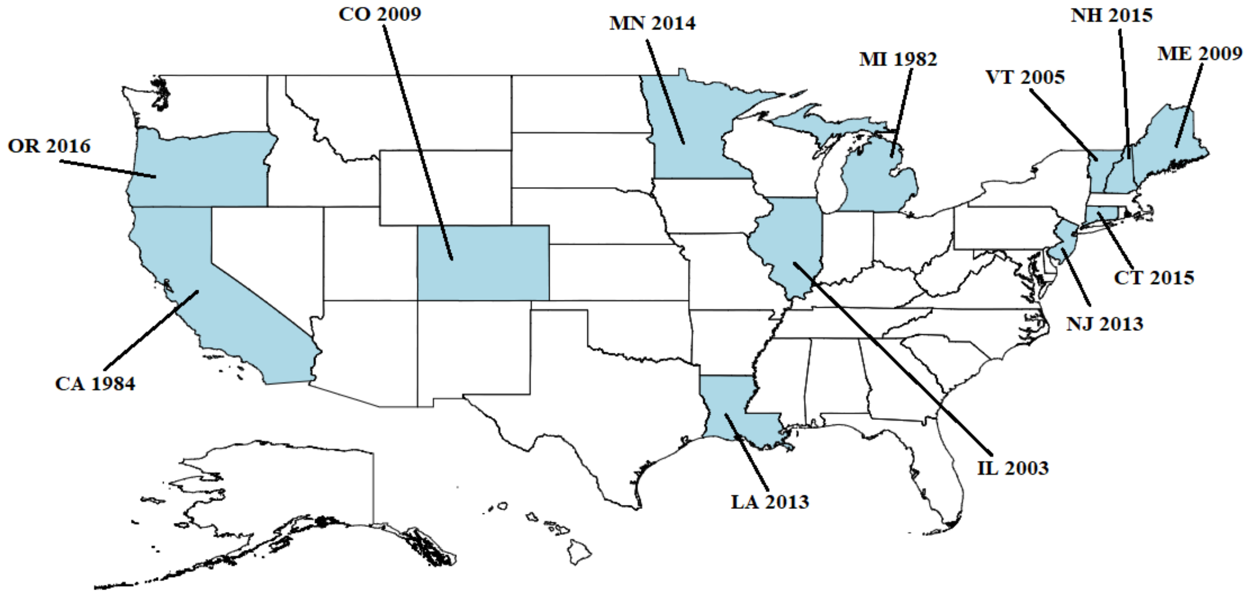


Figure 1 A Map of States Legislating Pay Secrecy Laws

This figure presents the states that passed pay secrecy laws (in shaded areas) and their passage years. The legislating states include Michigan (1982), California (1984), Illinois (2003), Vermont (2005), Colorado (2009), Maine (2009), Louisiana (2013), New Jersey (2013), Minnesota (2014), Connecticut (2015), New Hampshire (2015), and Oregon (2016). The details of pay secrecy laws in each state are provided in Appendix 1.

Table 1. Summary Statistics

This table reports summary statistics for the 1976-2017 period. Panel A reports the descriptive statistics for the scientist/engineer analysis. Panel B reports the descriptive statistics of the 67,685 firm-year observations for the firm-level analysis. Panel C reports the descriptive statistics for the pay gap analysis. Panel D reports the descriptive statistics for the inventor-level analysis. Panel E reports the descriptive statistics for the patent-level analysis. Variable definitions are provided in Appendix 2. All continuous variables are winsorized at the 1st and 99th percentiles.

Panel A: Scientist/Engineer data

	N	Mean	SD	P25	P50	P75
Hourly wage	132,581	26.56	65.67	16.82	23.52	31.71
Transparency	132,581	0.16	0.36	0.00	0.00	0.00
Minority	132,581	0.39	0.49	0.00	0.00	1.00
Employee age	132,581	38.83	10.87	30.00	38.00	47.00
Annual working hours	132,581	2112.10	492.89	2080.00	2080.00	2340.00
College education	132,581	0.63	0.48	0.00	1.00	1.00
Postgraduate	132,581	0.17	0.38	0.00	0.00	0.00

Panel B: Firm-level data

	N	Mean	SD	P25	P50	P75
Pat/Inventor	67,685	0.16	0.21	0.00	0.10	0.21
Cit/Inventor	67,685	0.17	0.32	0.00	0.05	0.19
Val/Inventor	67,685	0.89	2.39	0.00	0.10	0.63
UCit/Inventor	67,685	3.90	9.45	0.00	0.48	3.11
Gen/Inventor	67,685	0.06	0.11	0.00	0.02	0.08
Ori/Inventor	67,685	0.06	0.10	0.00	0.02	0.08
R&D/Inventor	67,685	1.10	2.48	0.03	0.31	0.95
Claims	43,678	2.90	1.47	2.00	2.67	3.50
Average compensation ($1,000)	61,662	76.01	96.45	17.85	40.33	96.48
Process claims	43,678	0.31	0.28	0.04	0.29	0.49
Transparency	67,685	0.24	0.42	0.00	0.00	0.00
Existence of CNC	60,112	0.40	0.49	0.00	0.00	1.00
Enforcement of CNC	60,112	0.18	0.38	0.00	0.00	0.00
Larger pay secrecy (state-industry)	61,716	0.50	0.50	0.00	0.00	1.00
Higher minority ratio	65,531	0.47	0.50	0.00	0.00	1.00
Higher promotion raise	19,231	0.50	0.50	0.00	0.00	1.00
Assets ($million)	67,685	2264.65	7036.25	49.81	190.83	1017.31
Cash	67,685	21.58%	24.21%	3.37%	11.42%	31.75%
R&D	67,685	7.91%	12.64%	0.32%	3.09%	9.77%
R&D Missing	67,685	0.21	0.41	0.00	0.00	0.00
ROA	67,685	-3.92%	24.85%	-4.43%	3.89%	8.12%
PPE	67,685	47.81%	32.15%	23.18%	41.38%	65.59%
Leverage	67,685	19.98%	18.84%	2.71%	16.98%	30.71%
Capital expenditure	67,685	5.44%	4.83%	2.12%	4.10%	7.19%
Tobin's Q	67,685	2.08	1.73	1.07	1.48	2.34

Age	67,685	20.17	14.18	9.00	17.00	29.00
State GDP ($ million)	67,685	531515.15	561505.58	156749.95	310240.03	683577.81
Per capita income ($10,000)	67,685	2.88	1.33	1.81	2.74	3.86
State population (million)	67,685	14.43	10.92	5.80	11.31	19.74
Business combination	67,685	0.69	0.46	0.00	1.00	1.00
Good faith	67,685	0.32	0.47	0.00	0.00	1.00

Panel C: Pay gap analysis

	N	Mean	SD	P25	P50	P75
Pay gap	2,142	0.25	0.09	0.19	0.25	0.31
Larger pay secrecy (state)	2142	0.49	0.50	0.00	0.00	1.00
State GDP ($ million)	2,142	174445.54	243628.50	37033.00	84757.49	212056.98
Per capita income ($10,000)	2,142	2.64	1.37	1.47	2.44	3.67
State population (million)	2,142	5.27	5.73	1.33	3.61	6.23
Business combination	2,142	0.42	0.49	0.00	0.00	1.00
Good faith	2,142	0.17	0.37	0.00	0.00	0.00

Panel D: Inventor-level data

D1. The sample for which we do not require inventors' gender and race to be identifiable.

	N	Mean	SD	P25	P50	P75
InvPat	3,867,396	0.31	0.55	0.00	0.00	0.39
InvCit	3,867,396	0.31	0.78	0.00	0.00	0.22
Number Patent	3,867,396	0.27	0.44	0.00	0.00	1.00
Number Citation	3,867,396	686.23	1221.40	52.00	235.00	730.00
Transparency	3,867,396	651.72	1004.13	51.26	231.57	753.85
Assets ($million)	3,867,396	52641.11	101654.86	2940.00	14685.00	57048.02
Cash	3,867,396	16.22%	16.92%	4.23%	10.05%	21.75%
R&D	3,867,396	6.29%	5.96%	2.43%	4.90%	8.49%
R&D Missing	3,867,396	0.04	0.19	0.00	0.00	0.00
ROA	3,867,396	4.95%	10.61%	2.48%	6.30%	9.93%
PPE	3,867,396	51.10%	31.39%	25.49%	44.44%	71.61%
Leverage	3,867,396	21.86%	15.18%	10.95%	20.59%	29.78%
Capital expenditure	3,867,396	5.68%	4.04%	2.62%	4.52%	7.71%
Tobin's Q	3,867,396	2.09	1.31	1.23	1.69	2.46
Age	3,867,396	37.08	17.25	24.00	38.00	51.00
State GDP ($ million)	3,867,396	676681.73	638102.50	220581.98	408941.19	964185.88
Per capita income ($10,000)	3,867,396	3.33	1.33	2.30	3.40	4.32
State population (million)	3,867,396	15.82	11.25	6.52	11.89	20.19
Business combination	3,867,396	0.82	0.39	1.00	1.00	1.00
Good faith	3,867,396	0.33	0.47	0.00	0.00	1.00
InvClaims	1,803,648	2.89	1.65	2.00	3.00	3.25

D2. The sample for which we require inventors' gender and race to be identifiable.

	N	Mean	SD	P25	P50	P75
InvPat'	2,763,721	0.23	0.48	0.00	0.00	0.33
InvCit'	2,763,721	0.22	0.63	0.00	0.00	0.05
Number Patent	2,763,721	0.24	0.43	0.00	0.00	0.00

Number Citation	2,763,721	625.99	1110.42	47.00	213.00	664.00
Transparency	2,763,721	605.41	967.56	46.51	210.42	694.03
Minority	2,763,721	0.15	0.36	0.00	0.00	0.00
Assets ($million)	2,763,721	50305.52	100468.87	2806.56	13700.00	49539.00
Cash	2,763,721	14.85%	16.04%	3.88%	9.29%	19.27%
R&D	2,763,721	5.94%	5.63%	2.31%	4.70%	8.02%
R&D Missing	2,763,721	0.04	0.19	0.00	0.00	0.00
ROA	2,763,721	4.92%	10.04%	2.48%	6.11%	9.63%
PPE	2,763,721	52.54%	31.23%	27.26%	46.52%	73.21%
Leverage	2,763,721	22.22%	15.09%	11.45%	20.88%	30.11%
Capital expenditure	2,763,721	5.81%	4.06%	2.75%	4.72%	7.85%
Tobin's Q	2,763,721	2.03	1.27	1.21	1.64	2.38
Age	2,763,721	37.50	17.02	25.00	39.00	51.00
State GDP ($ million)	2,763,721	614415.19	588866.52	202910.06	385698.06	833305.69
Per capita income ($10,000)	2,763,721	3.21	1.32	2.19	3.24	4.17
State population (million)	2,763,721	14.99	10.72	6.42	11.69	19.31
Business combination	2,763,721	0.80	0.40	1.00	1.00	1.00
Good faith	2,763,721	0.31	0.46	0.00	0.00	1.00

Panel E: Patent-level data

	N	Mean	SD	P25	P50	P75
For racial diversity analysis						
Inventor diversity (race)	1,430,670	0.16	0.28	0.00	0.00	0.00
Transparency	1,430,670	0.31	0.46	0.00	0.00	1.00
Patent Citation	1,430,670	1.04	1.57	0.00	0.51	1.30
Patent Value	1,430,670	13.81	23.14	2.43	6.03	14.28
For gender diversity analysis						
Inventor diversity (gender)	1,376,761	0.09	0.22	0.00	0.00	0.00
Transparency	1,376,761	0.30	0.46	0.00	0.00	1.00
Patent Citation	1,376,761	1.04	1.56	0.00	0.52	1.31
Patent Value	1,376,761	13.95	23.41	2.45	6.08	14.41

Table 2. Evidence on the Effectiveness of Pay Secrecy Laws

This table reports OLS regression results examining the effect of state-level pay secrecy laws on the salaries of scientists and engineers. Variable definitions are provided in Appendix 2. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by location state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) Ln(Hourly wage)	(2) Ln(Hourly wage)
Transparency × Minority	0.032***	0.033***
	(0.009)	(0.011)
Transparency	-0.009	-0.006
	(0.013)	(0.014)
Minority	-0.125***	-0.116***
	(0.005)	(0.004)
Ln (Employee age)	0.539***	0.530***
	(0.008)	(0.009)
Ln (Annual working hours)	0.025***	0.050***
	(0.008)	(0.007)
College education	0.270***	0.196***
	(0.005)	(0.004)
Postgraduate	0.137***	0.143***
	(0.005)	(0.006)
State fixed effects	Yes	Yes
Industry fixed effects	Yes	
Occupation fixed effects		Yes
Year fixed effects	Yes	Yes
Observations	132,581	132,581

Table 3. Baseline Results

This table reports OLS regression results. In columns (1) and (2), we examine the effect of pay secrecy laws on inventor productivity using a difference-in-differences specification in Equation (2). In columns (3) and (4), we examine the effect of pay secrecy laws on inventor productivity using a difference-in-differences specification in Equation (3). We include firm and industry×year fixed effects. Our industry is defined by SIC 2-digit codes. Variable definitions are provided in Appendix 2. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) LnPat/Inventor	(2) LnCit/Inventor	(3) LnPat/Inventor	(4) LnCit/Inventor
Transparency	0.013***	0.018***		
	(0.003)	(0.005)		
Year -7			0.007	0.011
			(0.007)	(0.007)
Year -6			0.005	0.008
			(0.004)	(0.006)
Year -5			-0.008	0.007
			(0.007)	(0.009)
Year -4			-0.006	0.003
			(0.006)	(0.008)
Year -3			-0.000	0.012
			(0.007)	(0.009)
Year -2			-0.005	-0.002
			(0.006)	(0.009)
Year -1			0.003	0.008
			(0.006)	(0.008)
Year 0			0.002	0.016
			(0.007)	(0.011)
Year 1			0.009	0.013
			(0.008)	(0.009)
Year 2			0.012*	0.020**
			(0.006)	(0.009)
Year 3			0.016**	0.021**
			(0.007)	(0.009)
Year 4			0.008	0.021**
			(0.010)	(0.009)
Year 5			0.013**	0.008
			(0.006)	(0.006)
Year 6			0.005	0.020*
			(0.009)	(0.012)
Year 7^{+}			0.019***	0.029***
			(0.006)	(0.007)
Ln(Assets)	0.023***	0.021***	0.023***	0.021***
	(0.003)	(0.003)	(0.003)	(0.003)
Cash	0.025***	0.039***	0.025***	0.039***
	(0.009)	(0.014)	(0.009)	(0.014)
R&D	0.072***	0.060***	0.072***	0.060***
	(0.008)	(0.019)	(0.008)	(0.019)

R&D missing	0.003	0.004	0.003	0.004
	(0.007)	(0.006)	(0.007)	(0.006)
ROA	0.032***	0.033***	0.032***	0.033***
	(0.004)	(0.006)	(0.004)	(0.006)
PPE	-0.012**	-0.021***	-0.012**	-0.021***
	(0.005)	(0.007)	(0.005)	(0.007)
Leverage	-0.028***	-0.034***	-0.028***	-0.034***
	(0.005)	(0.008)	(0.005)	(0.008)
Capex	0.161***	0.215***	0.162***	0.216***
	(0.016)	(0.032)	(0.016)	(0.032)
Tobin's Q	0.007***	0.010***	0.006***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)
Ln(Age)	-0.113***	-0.117***	-0.113***	-0.117***
	(0.005)	(0.012)	(0.005)	(0.012)
Ln(State GDP)	-0.023	-0.030	-0.022	-0.028
	(0.022)	(0.027)	(0.021)	(0.026)
Per capita income	-0.000	-0.003	0.000	-0.004
	(0.007)	(0.008)	(0.007)	(0.009)
Ln(State population)	0.020	0.025	0.017	0.020
	(0.022)	(0.027)	(0.021)	(0.026)
Business combination	0.006	0.004	0.006	0.004
	(0.007)	(0.009)	(0.007)	(0.009)
Good faith	-0.000	0.000	-0.001	-0.002
	(0.005)	(0.005)	(0.005)	(0.006)
Firm fixed effects	Yes	Yes	Yes	Yes
Industry×year fixed effects	Yes	Yes	Yes	Yes
Constant	0.527**	0.638**	0.514**	0.621**
	(0.214)	(0.272)	(0.209)	(0.266)
Observations	67,222	67,222	67,222	67,222

Table 4. Additional Tests for Staggered Difference-in-Differences Estimates

This table reports two additional tests on the staggered difference-in-differences estimates. In Panel A, we focus on a window that contains the ten years before and after the adoption of pay secrecy laws. In Panel B, we use a matched sample. For each treatment event (i.e., the event when a state adopted pay secrecy laws), we collect a cohort set that includes all firm-year observations in a window [-10,10] that ranges from 10 years before the event to 10 years after the event. In this cohort set, we have two groups of firms. The control group includes firm-year observations from the never-treated states. We then implement propensity score matching for treated and matched control firms based on all control variables in baseline regressions. Variable definitions are provided in Appendix 2. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Stacked difference-in-differences estimates

VARIABLES	(1) LnPat/Inventor	(2) LnCit/Inventor
Transparency	0.016**	0.016**
	(0.008)	(0.008)
Other controls	Same as those in Table 3	
Firm fixed effects	Yes	Yes
Cohort fixed effects	Yes	Yes
Industry×event year fixed effects	Yes	Yes
Observations	183,346	183,346

Panel B: Stacked difference-in-differences estimates with matched sample

VARIABLES	(1) LnPat/Inventor	(2) LnCit/Inventor
Transparency	0.025***	0.022*
	(0.008)	(0.011)
Other controls	Same as those in Table 3	
Firm fixed effects	Yes	Yes
Cohort fixed effects	Yes	Yes
Industry×event year fixed effects	Yes	Yes
Observations	12,779	12,779

Table 5. The Effect of Pay Secrecy Laws on Inventors' Innovation Output

This table examines the effect of state-level pay secrecy laws on inventors' innovation output using the difference-in-differences specification in Equation (9). The unit of analysis is an inventor-year observation. In columns (1) and (2), we include all the inventors. In columns (3) and (4), we focus on inventors who have never changed their states. We include firm and year fixed effects. Variable definitions are provided in Appendix 2. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by location state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) LnInvPat	(2) LnInvCit	(3) LnInvPat	(4) LnInvCit
	All the Inventors		Stayer Inventors	
Transparency	0.010***	0.013***	0.010***	0.015***
	(0.003)	(0.003)	(0.003)	(0.003)
Ln(Number Patent)	0.079***		0.077***	
	(0.002)		(0.002)	
Ln(Number Citation)		0.075***		0.073***
		(0.003)		(0.003)
Other controls	Same as those in Table 3			
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	3,867,131	3,867,131	3,064,408	3,064,408

Table 6. Motivation of Minority Inventors

This table examines the effect of state-level pay secrecy laws on minority inventors' innovation output using the difference-in-differences specification in Equation (10). The unit of analysis is an inventor-year observation. In columns (1) and (2), we include all the inventors whose race and gender are identifiable. In columns (3) and (4), we focus on inventors who have never changed their states and whose race and gender are identifiable. Variable definitions are provided in Appendix 2. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by location state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) LnInvPat'	(2) LnInvCit'	(3) LnInvPat'	(4) LnInvCit'
	All the Inventors		Stayer Inventors	
Transparency × Minority	0.010*	0.006	0.013**	0.008**
	(0.006)	(0.004)	(0.006)	(0.004)
Transparency	0.006*	0.008***	0.006	0.009***
	(0.003)	(0.002)	(0.004)	(0.003)
Minority	-0.081***	-0.069***	-0.083***	-0.071***
	(0.002)	(0.002)	(0.002)	(0.002)
Ln(Number Patent)	0.063***		0.061***	
	(0.001)		(0.001)	
Ln(Number Citation)		0.057***		0.055***
		(0.001)		(0.001)
Other controls	Same as those in Table 3			
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	2,763,437	2,763,437	2,150,901	2,150,901

Table 7. Inventor Diversity and Patent Quality

This table reports OLS regression results for two separate regressions. The unit of analysis is a patent observation. Panel A focuses on the race diversity of an inventor team. Panel B focuses on the gender diversity of an inventor team. In column (1), we examine the relation between inventor diversity and the passage of pay secrecy laws. In column (2), we examine the relation between inventor team diversity on patent citation and patent value. Variable definitions are provided in Appendix 2. Robust standard errors clustered by technology group are in parentheses. All continuous variables are winsorized at the 1st and 99th percentiles. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A Racial diversity

	(1)	(2)	
	Inventor diversity (race)	Ln(1+PatentCitation)	Ln(1+PatentValue)
Inventor diversity (race)		0.056***	0.014**
		(0.005)	(0.007)
Transparency	0.028***		
	(0.003)		
Firm fixed effects	Yes	Yes	Yes
Patent group fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	1,429,465	1,429,465	1,429,465

Panel B Gender diversity

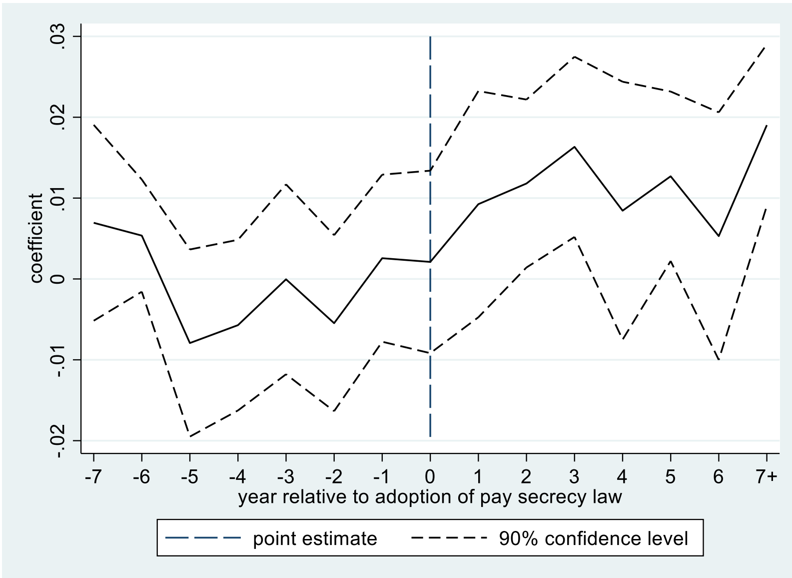
	(1)	(2)	
	Inventor diversity (gender)	Ln(1+PatentCitation)	Ln(1+PatentValue)
Inventor diversity (gender)		0.036***	0.026***
		(0.004)	(0.009)
Transparency	0.009***		
	(0.001)		
Firm fixed effects	Yes	Yes	Yes
Patent group fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	1,375,554	1,375,554	1,375,554

Table 8. Heterogeneous Treatment Effects Based on Expected Promotion Raises

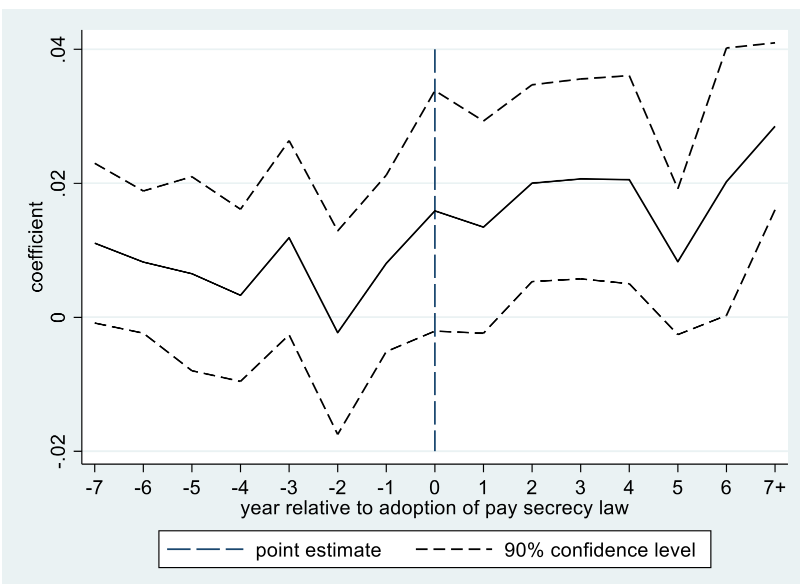
This table examines heterogeneous treatment effects of state-level pay secrecy laws on inventor productivity conditional on expected promotion raises in a firm. Variable definitions are provided in Appendix 2. We include firm and industry×year fixed effects. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) LnPat/Inventor	(2) LnCit/Inventor
Transparency × Higher promotion raises	0.010***	0.013***
	(0.004)	(0.004)
Transparency	0.003	0.002
	(0.007)	(0.012)
Higher promotion raises	-0.003	-0.001
	(0.003)	(0.004)
Other controls	Same as those in Table 3	
Firm fixed effects	Yes	Yes
Industry×year fixed effects	Yes	Yes
Observations	18,931	18,931

Internet Appendix for "Pay Transparency and Inventor Productivity: Evidence from State-level Pay Secrecy Laws"



Graph A. Patent per Inventor



Graph B. Citation per Inventor

Figure IA1. Pre-treatment Trend

This figure illustrates the pre-treatment trend between the treated group and the control group. On the y-axis, the graph plots regression coefficients when the dependent variables are *LnPat/Inventor* and *LnCit/Inventor*, respectively. The x-axis shows the year relative to the year of adoption (ranging from seven years prior to the adoption until seven years after the adoption of the pay secrecy laws). The solid lines correspond to the coefficient estimates of $Year^{-7}$ to $Year\ 7^{+}$ as specified in Equation (3). The dashed lines correspond to the 90% confidence intervals of the coefficient estimates; the confidence intervals are based on robust standard errors clustered by headquarters state.

Table IA1. State-specific Pre-trend

This table reports ordinary least squares regression results after controlling for state-level pre-trends and squared pre-trends. Variable definitions are provided in Appendix 2. We include firm and industry×year fixed effects. Industry is defined by SIC 2-digit codes. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) LnPat/Inventor	(2) LnCit/Inventor	(3) LnPat/Inventor	(4) LnCit/Inventor
Transparency	0.011**	0.016***	0.010**	0.018***
	(0.004)	(0.006)	(0.004)	(0.006)
Ln(Assets)	0.023***	0.020***	0.023***	0.020***
	(0.003)	(0.003)	(0.003)	(0.003)
Cash	0.025***	0.039***	0.025***	0.039***
	(0.009)	(0.014)	(0.009)	(0.014)
R&D	0.072***	0.060***	0.073***	0.060***
	(0.008)	(0.019)	(0.008)	(0.019)
R&D missing	0.003	0.003	0.003	0.003
	(0.007)	(0.006)	(0.007)	(0.007)
ROA	0.032***	0.034***	0.032***	0.034***
	(0.004)	(0.006)	(0.004)	(0.006)
PPE	-0.013**	-0.022***	-0.012**	-0.022***
	(0.005)	(0.007)	(0.005)	(0.007)
Leverage	-0.028***	-0.034***	-0.028***	-0.035***
	(0.006)	(0.008)	(0.006)	(0.008)
Capex	0.161***	0.215***	0.161***	0.215***
	(0.016)	(0.032)	(0.016)	(0.032)
Tobin's Q	0.006***	0.010***	0.006***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)
Ln(Age)	-0.113***	-0.117***	-0.113***	-0.117***
	(0.005)	(0.012)	(0.005)	(0.012)
Ln(State GDP)	-0.026	-0.040	-0.033	-0.053**
	(0.022)	(0.025)	(0.021)	(0.024)
Per capita income	0.001	0.000	0.000	-0.001
	(0.006)	(0.008)	(0.007)	(0.009)
Ln(State population)	0.024	0.037	0.032	0.051**
	(0.022)	(0.025)	(0.021)	(0.023)
Business combination	0.006	0.004	0.005	0.004
	(0.007)	(0.009)	(0.007)	(0.009)
Good faith	-0.001	-0.001	-0.005	-0.006
	(0.005)	(0.006)	(0.004)	(0.006)
Time trend	Yes	Yes	Yes	Yes
Squared time trend			Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry×year fixed effects	Yes	Yes	Yes	Yes
Observations	67,222	67,222	67,222	67,222

Table IA2. Placebo Tests

This table reports ordinary least squares regression results. For each treated state, we set pseudo-treated years as t-3, t-4, and t-5 from the true event year t. We drop the observations of treated states since the passage year. Variable definitions are provided in Appendix 2. We include firm and industry×year fixed effects. Industry is defined by SIC 2-digit codes. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) LnPat /Inventor	(2) LnCit /Inventor	(3) LnPat /Inventor	(4) LnCit /Inventor	(5) LnPat /Inventor	(6) LnCit /Inventor
	t-3		t-4		t-5	
Transparency	-0.001	0.001	-0.002	-0.001	-0.004	-0.002
	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Ln(Assets)	0.020***	0.018***	0.020***	0.018***	0.020***	0.018***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Cash	0.016**	0.019*	0.016**	0.019*	0.016**	0.019*
	(0.007)	(0.009)	(0.007)	(0.009)	(0.007)	(0.009)
R&D	0.143***	0.118***	0.143***	0.118***	0.143***	0.118***
	(0.017)	(0.031)	(0.017)	(0.031)	(0.017)	(0.031)
R&D missing	0.008	0.006	0.008	0.007	0.008	0.007
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
ROA	0.034***	0.030***	0.034***	0.030***	0.034***	0.030***
	(0.007)	(0.010)	(0.007)	(0.010)	(0.007)	(0.010)
PPE	-0.018***	-0.023***	-0.018***	-0.023***	-0.018***	-0.023***
	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)
Leverage	-0.025***	-0.024***	-0.025***	-0.024***	-0.025***	-0.024***
	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)
Capex	0.173***	0.204***	0.173***	0.204***	0.173***	0.204***
	(0.019)	(0.027)	(0.019)	(0.027)	(0.019)	(0.027)
Tobin's Q	0.008***	0.010***	0.008***	0.010***	0.008***	0.010***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ln(Age)	-0.094***	-0.086***	-0.094***	-0.086***	-0.094***	-0.086***
	(0.007)	(0.008)	(0.007)	(0.008)	(0.006)	(0.008)
Ln(State GDP)	0.002	0.002	0.002	0.002	0.003	0.002
	(0.010)	(0.012)	(0.010)	(0.012)	(0.010)	(0.012)
Per capita income	-0.002	0.001	-0.002	0.001	-0.002	0.001
	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)
Ln(State population)	-0.004	-0.005	-0.004	-0.005	-0.005	-0.005
	(0.011)	(0.013)	(0.011)	(0.013)	(0.011)	(0.013)
Business combination	0.002	0.004	0.002	0.004	0.002	0.004
	(0.008)	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)
Good faith	-0.005	-0.005	-0.004	-0.005	-0.004	-0.004
	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry×year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	51,229	51,229	51,229	51,229	51,229	51,229

Table IA3. The Timing of Pay Secrecy Law Adoption

This table reports estimates from a Weibull hazard model in which the "failure event" is the adoption of the pay secrecy law in a state. States drop from the sample once they have adopted pay secrecy laws. *Ln(Average Pat/Inventor)* is the natural logarithm of 1 plus the average *Pat/Inventor* across all public firms headquartered in a state. *Ln(Average Cit/Inventor)* is the natural logarithm of 1 plus the average *Cit/Inventor* across all public firms headquartered in a state. In Panel A, the sample period is 1976-2017. In Panels B and C, the sample period is 1979-2017. We use the three-year average of annual changes for all explanatory variables in Panel B, and use the three-year average of annual growth rates of all explanatory variables in Panel C. We do not include dummy variables in Panel C. All independent variables are at the state level. Variable definitions are provided in Appendix 2. Robust standard errors clustered by state are in parentheses. The superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Level

	(1)	(2)
Ln(Average Pat/Inventor)	1.567	
	(3.438)	
Ln(Average Cit/Inventor)		2.893
		(2.484)
Pay gap	-1.590	-2.014
	(4.800)	(5.068)
Ln (State GDP)	-2.686	-2.681
	(1.846)	(1.996)
Per capita income	0.775	0.896
	(0.593)	(0.639)
Ln (State population)	3.337**	3.358*
	(1.698)	(1.901)
State unemployment rate	23.275	22.136
	(14.151)	(14.387)
Republican governor	-0.038	-0.042
	(0.636)	(0.632)
State education	18.619*	17.472
	(11.212)	(11.374)
Percentage of males	21.276	20.511
	(15.432)	(16.319)
Percentage of whites	3.719	4.049*
	(2.300)	(2.307)
Business combination	-1.771*	-1.767*
	(0.949)	(0.950)
Good faith	0.207	0.130
	(1.191)	(1.201)
Constant	-6.666	-6.024
	(17.701)	(18.328)
Observations	1,831	1,831

Panel B. Changes

	(1)	(2)
Ln(Average Pat/Inventor)_Change	7.352	
	(5.900)	
Ln(Average Cit/Inventor) Change		7.321
		(5.269)
Pay gap Change	-4.972	-5.276
	(8.669)	(8.501)
Ln (State GDP) Change	-32.229**	-33.802**
	(16.228)	(16.572)
Per capita income Change	2.540	2.483
	(6.308)	(6.342)
Ln (State population) Change	-33.005	-34.450
	(54.216)	(55.312)
State unemployment rate Change	-2.457	-4.570
	(40.987)	(40.642)
Republican governor Change	-0.356	-0.470
	(1.864)	(1.829)
State education Change	55.806	57.066
	(37.311)	(38.191)
Percentage of males Change	-20.207	-20.167
	(40.202)	(40.099)
Percentage of whites Change	31.249	32.499
	(30.303)	(30.914)
Business combination Change	-41.891***	-43.420***
	(2.077)	(1.987)
Good faith Change	-3.050	-3.237*
	(1.875)	(1.927)
Constant	-5.655*	-5.535*
	(3.157)	(3.126)
Observations	1,662	1,662

Panel C. Growth rates

	(1)	(2)
Ln(Average Pat/Inventor)_Growth	-0.300	
	(0.932)	
Ln(Average Cit/Inventor) Growth		0.193
		(0.251)
Pay gap Growth	-0.985	-0.949
	(1.024)	(0.970)
Ln (State GDP) _Growth	-584.992**	-584.012**
	(240.738)	(235.398)
Per capita income Growth	5.155	-0.752
	(36.634)	(34.428)
Ln (State population) _Growth	1.817	1.170
	(2.020)	(1.757)

State unemployment rate Growth	-1.763	-2.071
	(2.504)	(2.424)
State education Growth	4.238	4.824
	(6.913)	(7.045)
Percentage of males Growth	-24.333	-23.476
	(23.210)	(22.977)
Percentage of whites Growth	30.485*	29.552*
	(16.701)	(17.286)
Constant	-3.781	-3.041
	(3.226)	(2.727)
Observations	1,523	1,504

Table IA4. Summary Statistics for the Stacked Difference-in-Differences Estimates

This table report the summary statistics of the treated and control samples for the stacked difference-in-differences estimates. Panel A reports the summary statistics one year before pay secrecy laws. Panel B reports the summary statistics one year after pay secrecy laws.

Panel A: Treated vs control one year before the event

Two-sample	Control		Treated		
Variables	Observations	Mean	Observations	Mean	Mean Diff.
LnPat/Inventor	10,026	0.114	520	0.125	-0.010
LnCit/Inventor	10,026	0.116	520	0.128	-0.013
Ln(Assets)	10,026	6.153	520	5.508	0.645***
Cash	10,026	0.236	520	0.198	0.038***
R&D	10,026	0.089	520	0.070	0.019**
R&D missing	10,026	0.222	520	0.219	0.002
ROA	10,026	-0.075	520	-0.016	-0.059***
PPE	10,026	0.469	520	0.488	-0.019
Leverage	10,026	0.210	520	0.193	0.018
Capex	10,026	0.046	520	0.0610	-0.014***
Tobin's Q	10,026	2.083	520	1.958	0.125
Ln(Age)	10,026	2.917	520	2.780	0.137***
Ln(State GDP)	10,026	12.82	520	12.73	0.095**
Per capita income	10,026	3.807	520	2.975	0.832***
Ln(State population)	10,026	2.210	520	2.469	-0.259***
Business combination	10,026	0.777	520	0.467	0.310***
Good faith	10,026	0.186	520	0.438	-0.253***

Panel B: Treated vs control one year after the event

Two-sample	Control		Treated		
Variables	Observations	Mean	Observations	Mean	Mean Diff.
LnPat/Inventor	9,545	0.113	536	0.131	-0.018***
LnCit/Inventor	9,545	0.108	536	0.128	-0.020**
Ln(Assets)	9,545	6.258	536	5.480	0.778***
Cash	9,545	0.247	536	0.198	0.049***
R&D	9,545	0.089	536	0.075	0.014*
R&D missing	9,545	0.214	536	0.203	0.011
ROA	9,545	-0.059	536	-0.028	-0.031**
PPE	9,545	0.463	536	0.492	-0.030*
Leverage	9,545	0.208	536	0.201	0.007
Capex	9,545	0.043	536	0.057	-0.014***
Tobin's Q	9,545	2.309	536	2.035	0.273***
Ln(Age)	9,545	2.954	536	2.758	0.197***
Ln(State GDP)	9,545	12.88	536	12.88	0.003
Per capita income	9,545	3.978	536	3.127	0.851***
Ln(State population)	9,545	2.216	536	2.533	-0.317***
Business combination	9,545	0.771	536	0.459	0.312***
Good faith	9,545	0.193	536	0.459	-0.266***

Table IA5. Summary Statistics for the Stacked Difference-in-Differences Estimates with a Matched Sample

This table reports the summary statistics for the sample used for the stacked difference-indifferences estimates with a matched sample. Panel A reports the summary statistics one year before pay secrecy laws. Panel B reports the summary statistics one year after pay secrecy laws.

Panel A: Treated vs control one year before the event

	Control		Treated		
Variables	Observations	Mean	Observations	Observations	Mean
LnPat/inventor	520	0.129	520	0.125	0.004
LnCit/Inventor	520	0.121	520	0.128	-0.007
Ln(Assets)	520	5.426	520	5.508	-0.082
Cash	520	0.191	520	0.198	-0.007
R&D	520	0.076	520	0.0700	0.005
R&D missing	520	0.219	520	0.219	0
ROA	520	-0.024	520	-0.0160	-0.008
PPE	520	0.494	520	0.488	0.006
Leverage	520	0.207	520	0.193	0.014
Capex	520	0.058	520	0.0610	-0.003
Tobin's Q	520	2.076	520	1.958	0.118
Ln(Age)	520	2.806	520	2.780	0.027
Ln(State GDP)	520	12.58	520	12.73	-0.149***
Per capita income	520	3.270	520	2.975	0.295***
Ln(State population)	520	2.164	520	2.469	-0.305***
Business combination	520	0.648	520	0.467	0.181***
Good faith	520	0.177	520	0.438	-0.262***

Panel B: Treated vs control one year after the event

	Control		Treated		
Variables	Observations	Mean	Observations	Observations	Mean
LnPat/inventor	444	0.104	448	0.108	-0.004
LnCit/Inventor	444	0.098	448	0.118	-0.020
Ln(Assets)	444	5.630	448	5.721	-0.091
Cash	444	0.177	448	0.182	-0.005
R&D	444	0.076	448	0.065	0.011
R&D missing	444	0.191	448	0.210	-0.018
ROA	444	-0.051	448	-0.010	-0.041**
PPE	444	0.520	448	0.519	0.001
Leverage	444	0.212	448	0.207	0.005
Capex	444	0.051	448	0.056	-0.005
Tobin's Q	444	2.080	448	1.887	0.194**
Ln(Age)	444	2.950	448	2.951	-0.001
Ln(State GDP)	444	12.68	448	12.82	-0.137**
Per capita income	444	3.494	448	3.187	0.307***
Ln(State population)	444	2.187	448	2.458	-0.271***
Business combination	444	0.662	448	0.475	0.187***
Good faith	444	0.178	448	0.429	-0.251***

Table IA6. Standard Errors Based on Wild Cluster Bootstrap and Cluster-Jackknife

This table reports the t-statistics of standard errors based on wild cluster bootstrap (WCB) and cluster-jackknife. WCB includes standard errors from Roodman et al. (2019). CV3 and CV3J are two versions of cluster-jackknife standard errors of MacKinnon et al. (2022b). We estimate a difference-in-differences specification in Equation (2). Variable definitions are provided in Appendix 2. We include firm and industry×year fixed effects. Industry is defined by SIC 2-digit codes. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
VARIABLES	LnPat/Inventor	LnCit/Inventor
Transparency	0.013	0.018
t-stat (WCB)	(3.889)**	(3.804)**
t-stat (CV3)	(2.372)**	(2.198)**
t-stat (CV3J)	(2.373)**	(2.202)**
Control variables	Yes	Yes
Firm fixed effects	Yes	Yes
Industry×year fixed effects	Yes	Yes
Observations	67,222	67,222

Table IA7. Poisson Regression

This table reports Poisson regression results that we use to examine the relation between state-level pay secrecy laws and inventor productivity using a difference-in-differences specification. Variable definitions are provided in Appendix 2. We include firm and industry×year fixed effects. Industry is defined by SIC 2-digit codes. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) Pat/Inventor	(2) Cit/Inventor
Transparency	0.068**	0.109***
	(0.031)	(0.039)
Ln(Assets)	0.238***	0.224***
	(0.021)	(0.024)
Cash	0.162***	0.234**
	(0.057)	(0.099)
R&D	0.857***	0.885***
	(0.066)	(0.126)
R&D missing	0.036	0.056
	(0.065)	(0.069)
ROA	0.284***	0.306***
	(0.045)	(0.064)
PPE	-0.095**	-0.157**
	(0.047)	(0.078)
Leverage	-0.230***	-0.282***
	(0.043)	(0.047)
Capex	1.082***	1.413***
	(0.096)	(0.194)
Tobin's Q	0.039***	0.051***
	(0.004)	(0.006)
Ln(Age)	-0.720***	-0.687***
	(0.031)	(0.047)
Ln(State GDP)	-0.260	-0.447
	(0.195)	(0.282)
Per capita income	0.046	0.096
	(0.051)	(0.079)
Ln(State population)	0.227	0.384
	(0.193)	(0.282)
Business combination	0.081	0.100
	(0.062)	(0.073)
Good faith	-0.001	-0.004
	(0.037)	(0.048)
Firm fixed effects	Yes	Yes
Industry×year fixed effects	Yes	Yes
Observations	66,684	65,161

Table IA8. Excluding Specific States

This table reports ordinary least squares regression results after excluding specific states. In Panel A, we drop firms headquartered in California and Michigan. In Panel B, we drop firms headquartered in New Hampshire, Connecticut, and Oregon. In Panel C, we only include states that eventually pass pay secrecy laws. Variable definitions are provided in Appendix 2. We include firm and industry×year fixed effects. Industry is defined by SIC 2-digit codes. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Excluding California and Michigan

VARIABLES	(1) LnPat/Inventor	(2) LnCit/Inventor
Transparency	0.011**	0.012*
	(0.005)	(0.006)
Ln(Assets)	0.021***	0.017***
	(0.002)	(0.003)
Cash	0.015*	0.020*
	(0.008)	(0.011)
R&D	0.080***	0.055**
	(0.012)	(0.026)
R&D missing	0.009	0.008
	(0.006)	(0.006)
ROA	0.034***	0.031***
	(0.006)	(0.010)
PPE	-0.012*	-0.023***
	(0.007)	(0.008)
Leverage	-0.024***	-0.025***
	(0.007)	(0.007)
Capex	0.139***	0.181***
	(0.015)	(0.030)
Tobin’s Q	0.007***	0.010***
	(0.001)	(0.001)
Ln(Age)	-0.114***	-0.104***
	(0.007)	(0.009)
Ln(State GDP)	-0.015	-0.016
	(0.024)	(0.028)
Per capita income	0.004	0.002
	(0.008)	(0.009)
Ln(State population)	0.014	0.015
	(0.023)	(0.028)
Business combination	0.005	0.006
	(0.009)	(0.009)
Good faith	-0.003	-0.002
	(0.008)	(0.009)
Firm fixed effects	Yes	Yes
Industry×year fixed effects	Yes	Yes
Observations	51,583	51,583

Panel B: Excluding New Hampshire, Connecticut, and Oregon

VARIABLES	(1) LnPat/Inventor	(2) LnCit/Inventor
Transparency	0.017***	0.022***
	(0.002)	(0.005)
Ln(Assets)	0.023***	0.020***
	(0.003)	(0.004)
Cash	0.027***	0.040***
	(0.009)	(0.014)
R&D	0.075***	0.065***
	(0.007)	(0.017)
R&D missing	0.001	0.003
	(0.007)	(0.007)
ROA	0.032***	0.034***
	(0.004)	(0.006)
PPE	-0.011*	-0.022***
	(0.006)	(0.007)
Leverage	-0.027***	-0.035***
	(0.006)	(0.008)
Capex	0.154***	0.202***
	(0.017)	(0.033)
Tobin's Q	0.006***	0.009***
	(0.001)	(0.001)
Ln(Age)	-0.113***	-0.118***
	(0.006)	(0.012)
Ln(State GDP)	-0.044**	-0.061***
	(0.019)	(0.022)
Per capita income	0.005	0.003
	(0.007)	(0.009)
Ln(State population)	0.041**	0.057**
	(0.019)	(0.022)
Business combination	0.008	0.006
	(0.008)	(0.010)
Good faith	-0.005	-0.005
	(0.004)	(0.005)
Firm fixed effects	Yes	Yes
Industry×year fixed effects	Yes	Yes
Observations	63,737	63,737

Panel C: Only including states that eventually pass pay secrecy laws

VARIABLES	(1) LnPat/Inventor	(2) LnCit/Inventor
Transparency	0.009**	0.015***
	(0.003)	(0.005)
Ln(Assets)	0.031***	0.030***
	(0.001)	(0.002)
Cash	0.023	0.045**
	(0.013)	(0.018)
R&D	0.087***	0.090***
	(0.009)	(0.022)
R&D missing	-0.005	-0.002
	(0.012)	(0.010)
ROA	0.030***	0.035***
	(0.004)	(0.004)
PPE	-0.015*	-0.012
	(0.008)	(0.012)
Leverage	-0.023**	-0.038***
	(0.008)	(0.008)
Capex	0.201***	0.293***
	(0.017)	(0.036)
Tobin's Q	0.006***	0.010***
	(0.000)	(0.001)
Ln(Age)	-0.114***	-0.135***
	(0.005)	(0.017)
Ln(State GDP)	-0.001	-0.025
	(0.052)	(0.070)
Per capita income	-0.019	-0.014
	(0.011)	(0.016)
Ln(State population)	-0.009	0.005
	(0.055)	(0.074)
Business combination	0.015**	0.002
	(0.007)	(0.012)
Good faith	0.020**	0.016*
	(0.008)	(0.008)
Firm fixed effects	Yes	Yes
Industry×year fixed effects	Yes	Yes
Observations	29,516	29,516

Table IA9. Not-to-Compete Covenants

This table reports ordinary least squares regression results after controlling for not-to-compete covenants. Variable definitions are provided in Appendix 2. We include firm and industry×year fixed effects. Industry is defined by SIC 2-digit codes. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) LnPat/Inventor	(2) LnCit/Inventor
Transparency	0.015***	0.019***
	(0.003)	(0.003)
Existence of CNC	-0.011	-0.008
	(0.008)	(0.008)
Enforcement of CNC	0.001	-0.001
	(0.007)	(0.007)
Ln(Assets)	0.026***	0.022***
	(0.003)	(0.004)
Cash	0.024***	0.036***
	(0.007)	(0.013)
R&D	0.086***	0.074***
	(0.010)	(0.019)
R&D missing	0.003	0.005
	(0.007)	(0.007)
ROA	0.034***	0.033***
	(0.004)	(0.005)
PPE	-0.008*	-0.016**
	(0.005)	(0.007)
Leverage	-0.032***	-0.036***
	(0.006)	(0.007)
Capex	0.150***	0.198***
	(0.015)	(0.027)
Tobin's Q	0.007***	0.009***
	(0.001)	(0.001)
Ln(Age)	-0.120***	-0.122***
	(0.006)	(0.013)
Ln(State GDP)	-0.027	-0.034
	(0.022)	(0.028)
Per capita income	-0.003	-0.004
	(0.008)	(0.010)
Ln(State population)	0.024	0.029
	(0.022)	(0.029)
Business combination	0.006	0.004
	(0.007)	(0.009)
Good faith	0.004	0.003
	(0.005)	(0.005)
Firm fixed effects	Yes	Yes
Industry×year fixed effects	Yes	Yes
Observations	59,657	59,657

Table IA10. Alternative Measures of Inventor Productivity

This table reports ordinary least squares regression results when we use alternative measures of inventor productivity. In Panel A, we scale innovation output by the number of inventors who file patents with a firm in a year. In Panel B, we use alternative measures of innovation output. Variable definitions are provided in Appendix 2. We include firm and industry×year fixed effects. Industry is defined by SIC 2-digit codes. T-statistics based on robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Alternative Measures of the Number of Inventors

VARIABLES	(1) LnPat /Inventor1	(2) LnCit /Inventor1
Transparency	0.009**	0.021**
	(0.004)	(0.009)
Ln(Assets)	0.005	0.015**
	(0.005)	(0.007)
Cash	-0.000	0.032*
	(0.009)	(0.018)
R&D	-0.020	0.042
	(0.021)	(0.037)
R&D missing	0.011	0.018
	(0.008)	(0.015)
ROA	0.017**	0.047***
	(0.007)	(0.014)
PPE	0.010	-0.004
	(0.010)	(0.017)
Leverage	-0.020**	-0.026
	(0.010)	(0.020)
Capex	0.060*	0.230***
	(0.033)	(0.078)
Tobin's Q	0.002***	0.010***
	(0.001)	(0.001)
Ln(Age)	-0.011	-0.052**
	(0.008)	(0.021)
Ln(State GDP)	-0.032*	-0.038
	(0.019)	(0.033)
Per capita income	-0.003	-0.014
	(0.007)	(0.016)
Ln(State population)	0.036*	0.031
	(0.020)	(0.035)
Business combination	-0.003	0.000
	(0.008)	(0.019)
Good faith	0.002	0.006
	(0.006)	(0.007)
Firm fixed effects	Yes	Yes
Industry×year fixed effects	Yes	Yes
Observations	42,074	42,074

Panel B: Alternative Measures of Innovation

VARIABLES	(1) LnVal/Inventor	(2) LnUCit/Inventor	(3) LnGen/Inventor	(4) LnOri/Inventor	(5) LnR&D/Inventor
Transparency	0.047***	0.071**	0.008***	0.007***	0.017
	(0.011)	(0.028)	(0.002)	(0.002)	(0.011)
Ln(Assets)	0.120***	0.097***	0.008***	0.011***	0.152***
	(0.008)	(0.012)	(0.001)	(0.001)	(0.010)
Cash	-0.060**	0.193**	0.013***	0.018***	-0.140***
	(0.027)	(0.073)	(0.005)	(0.005)	(0.018)
R&D	0.212***	0.396***	0.025***	0.025***	
	(0.027)	(0.069)	(0.005)	(0.005)	
R&D missing	0.010	-0.004	0.004	0.002	
	(0.019)	(0.026)	(0.003)	(0.003)	
ROA	0.054***	0.124***	0.014***	0.012***	-0.217***
	(0.011)	(0.024)	(0.003)	(0.003)	(0.017)
PPE	-0.032*	-0.093***	-0.008***	-0.004	-0.001
	(0.017)	(0.031)	(0.003)	(0.003)	(0.023)
Leverage	-0.149***	-0.195***	-0.013***	-0.013***	-0.088***
	(0.017)	(0.020)	(0.003)	(0.003)	(0.021)
Capex	0.252**	0.880***	0.086***	0.057***	0.472***
	(0.106)	(0.085)	(0.010)	(0.009)	(0.140)
Tobin's Q	0.056***	0.036***	0.003***	0.003***	0.008***
	(0.002)	(0.003)	(0.000)	(0.000)	(0.003)
Ln(Age)	-0.136***	-0.515***	-0.061***	-0.050***	-0.108***
	(0.012)	(0.041)	(0.003)	(0.003)	(0.013)
Ln(State GDP)	-0.068	-0.078	-0.010	-0.008	0.024
	(0.054)	(0.109)	(0.011)	(0.008)	(0.072)
Per capita income	0.026	-0.075**	-0.001	-0.002	0.069*
	(0.023)	(0.037)	(0.003)	(0.003)	(0.037)
Ln(State population)	0.053	0.050	0.008	0.007	-0.017
	(0.056)	(0.112)	(0.011)	(0.008)	(0.071)
Business combination	0.000	0.011	0.003	0.001	-0.013
	(0.016)	(0.041)	(0.004)	(0.003)	(0.015)
Good faith	-0.022	-0.002	-0.002	-0.000	0.014
	(0.016)	(0.024)	(0.002)	(0.002)	(0.014)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Industry×year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	67,222	67,222	67,222	67,222	67,222

Table IA11. Court of Appeals for the Federal Circuit (CAFC) and Bayh–Dole Act

This table reports ordinary least squares regression results when we address the potential confounding effects of the creation of the Court of Appeals for the Federal Circuit (CAFC) and the enactment of the Bayh–Dole Act in the early 1980s. In columns (1) and (2), we start our sample in 1991. In columns (3) and (4), we drop patents that cite university patents. Variable definitions are provided in Appendix 2. We include firm and industry×year fixed effects. Industry is defined by SIC 2-digit codes. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) LnPat/Inventor	(2) LnCit/Inventor	(3) LnPat/Inventor	(4) LnCit/Inventor
	Start in 1991		Remove patents that cite university patents	
Transparency	0.009*	0.017**	0.007**	0.010**
	(0.005)	(0.007)	(0.003)	(0.005)
Ln(Assets)	0.023***	0.018***	0.018***	0.015***
	(0.003)	(0.003)	(0.002)	(0.003)
Cash	0.028**	0.038**	0.008	0.018**
	(0.012)	(0.015)	(0.006)	(0.008)
R&D	0.049***	0.042**	0.068***	0.061***
	(0.010)	(0.019)	(0.006)	(0.010)
R&D missing	-0.001	-0.003	0.002	0.004
	(0.010)	(0.011)	(0.007)	(0.006)
ROA	0.026***	0.030***	0.029***	0.027***
	(0.005)	(0.007)	(0.003)	(0.005)
PPE	-0.012*	-0.029***	-0.011**	-0.020***
	(0.007)	(0.008)	(0.004)	(0.006)
Leverage	-0.025***	-0.037***	-0.024***	-0.025***
	(0.007)	(0.012)	(0.005)	(0.006)
Capex	0.182***	0.244***	0.145***	0.192***
	(0.023)	(0.034)	(0.019)	(0.033)
Tobin's Q	0.006***	0.009***	0.005***	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
Ln(Age)	-0.117***	-0.126***	-0.094***	-0.091***
	(0.006)	(0.016)	(0.006)	(0.009)
Ln(State GDP)	-0.031	-0.050	-0.026	-0.032
	(0.028)	(0.042)	(0.022)	(0.026)
Per capita income	0.002	0.002	0.004	-0.001
	(0.009)	(0.010)	(0.007)	(0.008)
Ln(State population)	0.029	0.045	0.025	0.029
	(0.029)	(0.041)	(0.022)	(0.026)
Business combination	0.061***	0.007	0.007	0.005
	(0.008)	(0.013)	(0.007)	(0.007)
Good faith	0.002	0.004	0.003	0.002
	(0.008)	(0.010)	(0.005)	(0.005)
Firm fixed effects	Yes	Yes	Yes	Yes
Industry×year fixed effects	Yes	Yes	Yes	Yes
Observations	46,606	46,606	67,222	67,222

Table IA12. Analyses for Insider/Employed Inventors

This table reports OLS regression results when we focus on employed inventors by only counting the patents from inventors who started to file their third consecutive patent in a firm. In columns (1) and (2), the unit of analysis is a firm-year observation. We include firm and industry×year fixed effects. In columns (3) and (4), the unit of analysis is an inventor-year observation. We include firm and year fixed effects. Variable definitions are provided in Appendix 2. Robust standard errors clustered by headquarters/location state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	LnPat/Inventor	LnCit/Inventor	LnInvPat	LnInvCit
	Firm level		Inventor level	
Transparency	0.008***	0.011***	0.011***	0.016***
	(0.003)	(0.003)	(0.002)	(0.003)
Ln(Number Patent)			0.114***	
			(0.003)	
Ln(Number Citation)				0.107***
				(0.004)
Ln(Assets)	0.017***	0.018***	-0.047***	-0.044***
	(0.002)	(0.003)	(0.003)	(0.005)
Cash	0.028***	0.038***	0.022	0.048**
	(0.007)	(0.012)	(0.023)	(0.021)
R&D	0.050***	0.053***	-0.355***	-0.390***
	(0.009)	(0.014)	(0.058)	(0.067)
R&D missing	0.003	0.002	0.012	0.031*
	(0.003)	(0.003)	(0.007)	(0.018)
ROA	0.011**	0.011**	-0.022**	-0.038**
	(0.004)	(0.005)	(0.009)	(0.015)
PPE	0.002	-0.001	-0.092***	-0.079***
	(0.004)	(0.005)	(0.012)	(0.015)
Leverage	-0.020***	-0.027***	-0.011	-0.004
	(0.004)	(0.004)	(0.012)	(0.015)
Capex	0.084***	0.118***	0.306***	0.421***
	(0.016)	(0.028)	(0.057)	(0.068)
Tobin's Q	0.003***	0.005***	0.006***	0.008***
	(0.001)	(0.001)	(0.002)	(0.002)
Ln(Age)	-0.020***	-0.025***	-0.132***	-0.157***
	(0.005)	(0.008)	(0.014)	(0.023)
Ln(State GDP)	-0.006	-0.011	-0.032	-0.037
	(0.012)	(0.016)	(0.024)	(0.027)
Per capita income	-0.005	-0.007	-0.001	-0.010
	(0.004)	(0.006)	(0.011)	(0.011)
Ln(State population)	0.008	0.011	0.032	0.031
	(0.012)	(0.016)	(0.022)	(0.026)
Business combination	0.002	-0.002	-0.004	-0.009
	(0.004)	(0.003)	(0.009)	(0.012)
Good faith	-0.001	-0.002	0.013	0.014

	(0.004)	(0.005)	(0.009)	(0.011)
Firm fixed effects	Yes	Yes	Yes	Yes
Industry×year fixed effects	Yes	Yes		
Year fixed effects			Yes	Yes
Observations	67,222	67,222	1,442,613	1,442,613

Table IA13. Impact of Pay Transparency on State-level Pay Gaps

This table reports OLS regression results when we examine the effect of pay secrecy laws on state-level pay gaps between male whites and others conditional on a state's ex ante pay secrecy practices and rules in 1980. The unit of analysis is a state-year observation. Variable definitions are provided in Appendix 2. We include state and year fixed effects. Robust standard errors clustered by state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) Pay gap	(2) Pay gap
Transparency × Larger pay secrecy (state)	-0.056**	-0.040*
	(0.024)	(0.021)
Transparency	0.025	0.002
	(0.017)	(0.010)
Larger pay secrecy (state)	0.013	0.023***
	(0.012)	(0.008)
Ln(State GDP)	-0.018	-0.037**
	(0.026)	(0.014)
Per capita income	0.010	0.001
	(0.015)	(0.006)
Ln(State population)	0.058	0.042***
	(0.039)	(0.013)
Business combination	-0.021*	-0.014**
	(0.012)	(0.007)
Good faith	-0.017	0.014*
	(0.015)	(0.008)
State fixed effects	Yes	No
Year fixed effects	Yes	No
Observations	2,142	2,142

Table IA14. Heterogeneous Treatment Effects Based on *ex ante* Pay Secrecy

This table examines heterogeneous treatment effects of state-level pay secrecy laws on inventor productivity conditional on a state-industry's *ex ante* pay secrecy practices and rules in 1980. Variable definitions are provided in Appendix 2. We include firm and industry×year fixed effects. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) LnPat/Inventor	(2) LnCit/Inventor
Transparency × Larger pay secrecy (state-industry)	0.013***	0.016*
	(0.003)	(0.008)
Transparency	0.007*	0.011**
	(0.004)	(0.005)
Larger pay secrecy (state-industry)	-0.002	-0.005
	(0.004)	(0.006)
Ln(Assets)	0.024***	0.021***
	(0.003)	(0.003)
Cash	0.023**	0.039**
	(0.010)	(0.015)
R&D	0.069***	0.061***
	(0.008)	(0.019)
R&D missing	0.003	0.005
	(0.008)	(0.008)
ROA	0.032***	0.034***
	(0.004)	(0.006)
PPE	-0.012**	-0.022***
	(0.005)	(0.007)
Leverage	-0.026***	-0.034***
	(0.006)	(0.009)
Capex	0.159***	0.208***
	(0.018)	(0.033)
Tobin's Q	0.006***	0.010***
	(0.001)	(0.001)
Ln(Age)	-0.112***	-0.115***
	(0.006)	(0.014)
Ln(State GDP)	-0.021	-0.027
	(0.026)	(0.032)
Per capita income	-0.003	-0.006
	(0.008)	(0.009)
Ln(State population)	0.015	0.017
	(0.027)	(0.031)
Business combination	0.004	0.003
	(0.007)	(0.009)
Good faith	0.003	0.007
	(0.005)	(0.006)
Firm fixed effects	Yes	Yes
Industry×year fixed effects	Yes	Yes
Observations	61,276	61,276

Table IA15. Heterogeneous Treatment Effects Based on the Ratio of Minority Inventors

This table examines heterogeneous treatment effects of state-level pay secrecy laws on inventor productivity conditional on the percentage of minority inventors in a firm. Variable definitions are provided in Appendix 2. We include firm and industry×year fixed effects. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
VARIABLES	LnPat/Inventor	LnCit/Inventor
Transparency × Higher minority ratio	0.010***	0.014***
	(0.003)	(0.005)
Transparency	0.007*	0.011***
	(0.004)	(0.004)
Higher minority ratio	0.006**	0.007**
	(0.003)	(0.003)
Ln(Assets)	0.023***	0.020***
	(0.003)	(0.003)
Cash	0.023***	0.034***
	(0.008)	(0.012)
R&D	0.077***	0.068***
	(0.009)	(0.019)
R&D missing	0.005	0.005
	(0.006)	(0.006)
ROA	0.031***	0.033***
	(0.004)	(0.006)
PPE	-0.015***	-0.025***
	(0.006)	(0.006)
Leverage	-0.026***	-0.033***
	(0.005)	(0.008)
Capex	0.161***	0.216***
	(0.019)	(0.033)
Tobin's Q	0.006***	0.009***
	(0.001)	(0.001)
Ln(Age)	-0.113***	-0.118***
	(0.006)	(0.013)
Ln(State GDP)	-0.021	-0.032
	(0.023)	(0.027)
Per capita income	-0.002	-0.005
	(0.006)	(0.008)
Ln(State population)	0.019	0.026
	(0.022)	(0.027)
Business combination	0.003	0.004
	(0.007)	(0.009)
Good faith	0.000	-0.001
	(0.005)	(0.005)
Firm fixed effects	Yes	Yes
Industry×year fixed effects	Yes	Yes
Observations	65,042	65,042

Table IA16. Alternative Explanations

This table reports OLS regression results when we test for alternative explanations. In column (1), we examine the effect of pay secrecy laws on the average number of claims per patent. The unit of analysis is a firm-year observation. In column (2), we examine the effect of pay secrecy laws on the average number of claims per patent. The unit of analysis is an inventor-year observation. In column (3), we examine the effect of pay secrecy laws on the average compensation per employee. In column (4), we examine the effect of pay secrecy laws on the average percentage of process claims in a firm. We include firm and industry×year fixed effects in columns (1), (3), and (4). We include firm and year fixed effects in column (2). Variable definitions are provided in Appendix 2. Robust standard errors clustered by headquarters state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	LnClaims	LnInvClaims	Average compensation	Process claims
Transparency	0.035	0.005	-0.116	0.008
	(0.031)	(0.006)	(1.406)	(0.009)
Ln(Assets)	0.016	0.007	-0.058	-0.002
	(0.022)	(0.005)	(1.403)	(0.003)
Cash	0.083	0.040***	36.465***	0.021*
	(0.050)	(0.014)	(3.065)	(0.013)
R&D	-0.013	0.080	140.101***	-0.005
	(0.171)	(0.060)	(21.666)	(0.018)
R&D missing	-0.056	-0.017	7.321***	-0.001
	(0.059)	(0.016)	(1.771)	(0.009)
ROA	0.046	-0.013	-31.514***	-0.005
	(0.102)	(0.013)	(4.790)	(0.012)
PPE	-0.082	-0.043**	-21.880***	0.003
	(0.062)	(0.017)	(4.055)	(0.013)
Leverage	-0.244***	-0.071***	-2.215	0.005
	(0.057)	(0.020)	(2.104)	(0.015)
Capex	-0.120	0.103*	-7.289	0.053
	(0.302)	(0.056)	(6.882)	(0.033)
Tobin's Q	0.016**	0.008***	-1.807***	0.000
	(0.007)	(0.002)	(0.417)	(0.002)
Ln(Age)	-0.110***	-0.051***	13.657***	0.010*
	(0.040)	(0.017)	(1.726)	(0.005)
Ln(State GDP)	-0.289	-0.120***	-10.445	-0.047
	(0.192)	(0.037)	(6.708)	(0.030)
Per capita income	0.067	0.026**	14.221***	0.014
	(0.073)	(0.012)	(3.827)	(0.009)
Ln(State population)	0.302	0.137***	10.877	0.042
	(0.197)	(0.038)	(7.194)	(0.031)
Business Combination	0.138**	-0.017	0.729	-0.000
	(0.056)	(0.013)	(1.463)	(0.010)
Good faith	0.045	0.038***	-4.136**	0.011
	(0.056)	(0.010)	(1.883)	(0.010)
Firm fixed effects	Yes	Yes	Yes	Yes

Industry×year fixed effects	Yes		Yes	Yes
Year fixed effects		Yes		
Observations	42,080	1,803,185	61,100	42,080

Table IA17. The Effect of Pay Secrecy Laws on Inventors' Innovation Output—Without Controlling for Firm-level Innovation Output

This table reports ordinary least squares regression results when we examine the effect of state-level pay secrecy laws on inventors' innovation output without controlling for firm-level innovation output. The unit of analysis is an inventor-year observation. Variable definitions are provided in Appendix 2. We include firm and year fixed effects. Robust standard errors clustered by state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	LnInvPat	LnInvCit	LnInvPat	LnInvCit
	All the Inventors		Stayer Inventors	
Transparency	0.008**	0.011***	0.008**	0.012***
	(0.003)	(0.003)	(0.003)	(0.003)
Ln(Assets)	0.007	0.011**	0.007	0.010**
	(0.006)	(0.005)	(0.006)	(0.005)
Cash	-0.038**	-0.008	-0.034**	-0.003
	(0.016)	(0.016)	(0.016)	(0.015)
R&D	0.053	0.042	0.048	0.036
	(0.041)	(0.043)	(0.039)	(0.042)
R&D missing	-0.009	0.002	-0.008	0.001
	(0.006)	(0.005)	(0.006)	(0.006)
ROA	0.025***	0.012*	0.025***	0.013*
	(0.009)	(0.007)	(0.008)	(0.007)
PPE	-0.055***	-0.042***	-0.052***	-0.039***
	(0.013)	(0.013)	(0.014)	(0.014)
Leverage	-0.026**	-0.027**	-0.028**	-0.029**
	(0.011)	(0.011)	(0.010)	(0.011)
Capex	0.101**	0.216***	0.099**	0.212***
	(0.041)	(0.049)	(0.042)	(0.050)
Tobin's Q	0.005***	0.009***	0.005***	0.009***
	(0.001)	(0.001)	(0.001)	(0.001)
Ln(Age)	-0.041***	-0.059***	-0.043***	-0.060***
	(0.004)	(0.009)	(0.004)	(0.009)
Ln(State GDP)	-0.061***	-0.059***	-0.058***	-0.059***
	(0.015)	(0.020)	(0.015)	(0.021)
Per capita income	0.021*	0.003	0.021*	0.003
	(0.010)	(0.010)	(0.011)	(0.011)
Ln(State population)	0.064***	0.055**	0.062***	0.056**
	(0.015)	(0.021)	(0.015)	(0.022)
Business combination	-0.015***	-0.015***	-0.013***	-0.014***
	(0.005)	(0.003)	(0.005)	(0.004)
Good faith	-0.003	-0.001	-0.002	0.001
	(0.007)	(0.007)	(0.007)	(0.007)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	3,867,131	3,867,131	3,064,408	3,064,408

Table IA18. The Effect of Pay Secrecy Laws on Inventors' Innovation Output—With Inventor Fixed Effects

This table reports ordinary least squares regression results when we examine the effect of state-level pay secrecy laws on inventors' innovation output after additionally controlling for inventor fixed effects. The unit of analysis is an inventor-year observation. Variable definitions are provided in Appendix 2. We include firm and year fixed effects. Industry is defined by SIC 2-digit codes. Robust standard errors clustered by location state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) LnInvPat	(2) LnInvCit	(3) LnInvPat	(4) LnInvCit
	All the Inventors		Stayer Inventors	
Transparency	0.009***	0.010***	0.011**	0.013*
	(0.003)	(0.003)	(0.005)	(0.007)
Ln(Number Patent)	0.085***		0.085***	
	(0.002)		(0.002)	
Ln(Number Citation)		0.081***		0.081***
		(0.003)		(0.003)
Ln(Assets)	-0.033***	-0.036***	-0.034***	-0.038***
	(0.002)	(0.003)	(0.002)	(0.003)
Cash	-0.006	0.004	-0.005	0.006
	(0.013)	(0.012)	(0.012)	(0.012)
R&D	-0.139***	-0.165***	-0.139***	-0.171***
	(0.025)	(0.022)	(0.023)	(0.020)
R&D missing	0.010**	0.025***	0.012*	0.025**
	(0.004)	(0.009)	(0.006)	(0.011)
ROA	-0.001	-0.009	-0.001	-0.008
	(0.006)	(0.008)	(0.006)	(0.008)
PPE	-0.059***	-0.063***	-0.059***	-0.065***
	(0.008)	(0.010)	(0.008)	(0.011)
Leverage	0.003	0.004	-0.001	-0.000
	(0.005)	(0.007)	(0.005)	(0.006)
Capex	0.152***	0.195***	0.138***	0.174***
	(0.030)	(0.027)	(0.029)	(0.024)
Tobin's Q	0.003***	0.004***	0.003***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
Ln(Age)	-0.039***	-0.057***	-0.041***	-0.061***
	(0.006)	(0.012)	(0.006)	(0.011)
Ln(State GDP)	-0.039***	-0.040***	-0.049***	-0.056***
	(0.011)	(0.013)	(0.013)	(0.013)
Per capita income	-0.001	-0.002	0.001	0.002
	(0.008)	(0.008)	(0.010)	(0.009)
Ln(State population)	0.039***	0.041***	0.048***	0.058***
	(0.011)	(0.013)	(0.013)	(0.013)
Business combination	-0.000	-0.002	-0.001	-0.003
	(0.004)	(0.004)	(0.003)	(0.004)
Good faith	0.014***	0.015***	0.016***	0.018***
	(0.004)	(0.005)	(0.004)	(0.005)
Firm fixed effects	Yes	Yes	Yes	Yes

Year fixed effects	Yes	Yes	Yes	Yes
Inventor fixed effects	Yes	Yes	Yes	Yes
Observations	3,644,110	3,644,110	2,841,469	2,841,469

Table IA19. The Effect of Pay Secrecy Laws on Inventors' Innovation Output— Inventors Employed by Public or Private Firms

This table reports ordinary least squares regression results when we examine the effect of state-level pay secrecy laws on inventors' innovation output. The sample include all inventors employed by public or private firms. The unit of analysis is an inventor-year observation. Variable definitions are provided in Appendix 2. We include firm and year fixed effects. Robust standard errors clustered by state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) LnInvPat	(2) LnInvCit	(3) LnInvPat	(4) LnInvCit
	All the Inventors		Stayer Inventors	
Transparency	0.006**	0.009***	0.006*	0.010***
	(0.003)	(0.003)	(0.003)	(0.003)
Ln(Number Patent)	0.093***		0.094***	
	(0.002)		(0.002)	
Ln(Number Citation)		0.100***		0.101***
		(0.003)		(0.004)
Ln(Age)	-0.074***	-0.080***	-0.070***	-0.079***
	(0.002)	(0.005)	(0.003)	(0.005)
Ln(State GDP)	0.008	0.016*	0.006	0.015
	(0.007)	(0.009)	(0.008)	(0.010)
Per capita income	0.001	-0.000	-0.002	-0.003
	(0.004)	(0.005)	(0.004)	(0.005)
Ln(State population)	-0.006	-0.014	-0.004	-0.013
	(0.006)	(0.009)	(0.008)	(0.010)
Business combination	0.005*	0.007**	0.004	0.007*
	(0.002)	(0.003)	(0.003)	(0.004)
Good faith	-0.002	0.001	-0.002	0.002
	(0.003)	(0.004)	(0.003)	(0.004)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	8,567,185	8,567,185	5,829,225	5,829,225

Table IA20. The Effect of Pay Secrecy Laws on Inventors' Innovation Output—Heterogeneous Treatment Effects Based on Average Compensation

This table examines the effect of state-level pay secrecy laws on minority inventors' innovation output conditional on the average compensation per employee in a firm. The unit of analysis is an inventor-year observation. An observation is in the high (low) average compensation sub-sample if the selling, general, and administrative expenses per employee is higher (lower) than the sample median in a year. Variable definitions are provided in Appendix 2. We include firm and year fixed effects. Robust standard errors clustered by state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A All Inventors

VARIABLES	(1) LnInvPat'	(2) LnInvPat'	(3) LnInvCit'	(4) LnInvCit'
	High Average Compensation	Low Average Compensation	High Average Compensation	Low Average Compensation
Transparency × Minority	0.012***	0.006	0.007**	0.004
	(0.003)	(0.011)	(0.003)	(0.009)
Transparency	0.008***	0.003	0.012***	0.004
	(0.002)	(0.004)	(0.002)	(0.004)
Minority	-0.077***	-0.084***	-0.069***	-0.069***
	(0.003)	(0.003)	(0.003)	(0.003)
Ln(Number Patent)	0.067***	0.066***		
	(0.002)	(0.002)		
Ln(Number Citation)			0.058***	0.060***
			(0.002)	(0.002)
Ln(Assets)	-0.031***	-0.029***	-0.025***	-0.029***
	(0.002)	(0.004)	(0.003)	(0.004)
Cash	-0.011	0.025	0.002	0.016
	(0.010)	(0.019)	(0.012)	(0.018)
R&D	-0.148***	-0.140***	-0.114***	-0.164***
	(0.035)	(0.049)	(0.040)	(0.053)
R&D missing	0.006	0.013**	0.007	0.013**
	(0.007)	(0.005)	(0.008)	(0.005)
ROA	0.005	-0.007	-0.003	-0.003
	(0.008)	(0.011)	(0.010)	(0.008)
PPE	-0.058***	-0.041***	-0.046***	-0.044***
	(0.010)	(0.008)	(0.010)	(0.013)
Leverage	-0.014*	0.015*	-0.013	0.016*
	(0.007)	(0.009)	(0.008)	(0.009)
Capex	0.142***	0.136***	0.197***	0.165***
	(0.041)	(0.029)	(0.031)	(0.018)
Tobin's Q	0.001	-0.003*	0.002	-0.001
	(0.001)	(0.002)	(0.001)	(0.002)
Ln(Age)	-0.017***	-0.013***	-0.038***	-0.007*
	(0.006)	(0.005)	(0.007)	(0.004)
Ln(State GDP)	-0.042**	-0.014	-0.059**	-0.015
	(0.019)	(0.012)	(0.023)	(0.009)
Per capita income	-0.001	0.011*	0.003	0.013**
	(0.009)	(0.006)	(0.009)	(0.005)

Ln(State population)	0.038**	0.006	0.055**	0.006
	(0.017)	(0.010)	(0.022)	(0.007)
Business combination	-0.001	0.006	-0.002	0.009
	(0.006)	(0.005)	(0.006)	(0.006)
Good faith	0.003	-0.008*	-0.001	-0.013***
	(0.005)	(0.004)	(0.008)	(0.004)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	1,277,659	1,311,781	1,277,659	1,311,781

Panel B Stayer Inventors

VARIABLES	(1) LnInvPat'	(2) LnInvPat'	(3) LnInvCit'	(4) LnInvCit'
	High Average Compensation	Low Average Compensation	High Average Compensation	Low Average Compensation
Transparency × Minority	0.014***	0.008	0.009**	0.005
	(0.003)	(0.011)	(0.004)	(0.010)
Transparency	0.009***	0.003	0.013***	0.004
	(0.003)	(0.005)	(0.002)	(0.004)
Minority	-0.079***	-0.087***	-0.070***	-0.071***
	(0.003)	(0.002)	(0.003)	(0.003)
Ln(Number Patent)	0.065***	0.065***		
	(0.002)	(0.002)		
Ln(Number Citation)			0.057***	0.059***
			(0.002)	(0.002)
Ln(Assets)	-0.030***	-0.028***	-0.024***	-0.029***
	(0.002)	(0.004)	(0.003)	(0.004)
Cash	-0.009	0.025	0.004	0.016
	(0.009)	(0.020)	(0.011)	(0.018)
R&D	-0.146***	-0.146***	-0.118***	-0.152***
	(0.029)	(0.054)	(0.037)	(0.053)
R&D missing	0.006	0.013**	0.007	0.013**
	(0.007)	(0.006)	(0.009)	(0.006)
ROA	0.009	-0.007	0.003	-0.000
	(0.009)	(0.010)	(0.011)	(0.009)
PPE	-0.056***	-0.038***	-0.045***	-0.041***
	(0.011)	(0.009)	(0.011)	(0.013)
Leverage	-0.017**	0.016*	-0.016**	0.019*
	(0.007)	(0.009)	(0.007)	(0.010)
Capex	0.123***	0.136***	0.174***	0.154***
	(0.042)	(0.032)	(0.032)	(0.018)
Tobin's Q	0.001	-0.002	0.002	0.000
	(0.002)	(0.002)	(0.002)	(0.002)
Ln(Age)	-0.019***	-0.010**	-0.040***	-0.006
	(0.007)	(0.005)	(0.008)	(0.004)
Ln(State GDP)	-0.042**	-0.015	-0.063***	-0.018**
	(0.018)	(0.012)	(0.023)	(0.008)
Per capita income	0.004	0.012**	0.006	0.014***

	(0.008)	(0.006)	(0.009)	(0.005)
Ln(State population)	0.040**	0.007	0.062***	0.009
	(0.017)	(0.010)	(0.022)	(0.006)
Business combination	-0.001	0.004	-0.002	0.005
	(0.006)	(0.004)	(0.007)	(0.005)
Good faith	0.002	-0.007	-0.000	-0.013***
	(0.006)	(0.005)	(0.008)	(0.004)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	998,713	1,031,023	998,713	1,031,023