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Employment Non-Discrimination Acts and Corporate Innovation*

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Abstract

We show that U.S. state-level Employment Non-Discrimination Acts (ENDAs)—laws that prohibit discrimination based on sexual orientation and gender identity—spur innovation. We find a significant increase in patents and patent citations for firms headquartered in states that have passed ENDAs relative to firms headquartered in states that have not. This result is more pronounced for firms that previously have not implemented pro-gay non-discrimination policies, for firms in states with a large homosexual population, and for firms in human capital-intensive industries. Lastly, we provide evidence suggesting that ENDAs affect innovation by matching pro-gay employees, who are generally more creative than anti-gay employees, with innovative firms.

Keywords: Innovation; Patents; Employment Non-Discrimination Acts; ENDAs; Sexual Orientation

JEL Classification: G38; J24; K31; M14; O31

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1. Introduction

Over the last three decades, several U.S. states have adopted the Employment Non-Discrimination Acts (ENDAs) that prohibit discrimination based on sexual orientation and gender identity. However, there is limited research investigating the real effects of these laws on business activities. In this paper, we examine this question from the perspective of corporate innovation and reveal a positive effect of ENDAs on innovation.

We expect ENDAs to foster corporate innovation through the mechanism of the matching process between firms and their employees. Currently, the U.S. population is divided in their attitudes on homosexuals. In general, those who are more likely to be pro-gay are younger, better educated, more tolerant, more open-minded, more risk-taking, have diverse backgrounds, and exhibit a stronger ideological liberalism. Further, these types of people also tend to be more creative. In contrast, those who are older, are more conservative, and exhibit a stronger religiosity tend to be anti-gay (Barth and Overby, 2003; Herek, 1994; Lewis and Gossett, 2008; Wilcox and Wolpert, 2000). After a state adopts an ENDA, pro-gay employees are more likely to join innovative firms in the adopting state since these firms cannot pursue discriminatory employment policies towards homosexuals. Because pro-gay individuals are likely to be more creative than anti-gay individuals, corporate innovation is subsequently enhanced.

Using a panel of 58,009 U.S. public firms from 1976 to 2008 and a difference-in-differences approach, we show that the adoption of ENDAs subsequently leads to a significant increase in innovation output. On average, firms headquartered in states that passed ENDAs experienced an increase in the number of patents by 8% and an increase in the number of patent citations by 11%, relative to firms headquartered in states that did not pass such a law.

The identifying assumption central to the difference-in-differences estimation is that treated and control firms share parallel trends prior to the change in law. Our tests show that their pre-treatment trends are indeed indistinguishable. Moreover, most of the impact of ENDAs on innovation occurs three years after the law's enactment.

However, it is possible that the adoption of ENDAs is triggered by local business conditions that in turn increase firms' innovation. To mitigate this concern, we additionally control for local business conditions such as state GDP, population, education, and political balance. Our inferences are largely unchanged. In further tests, we exploit the fact that economic conditions are likely to be similar in neighboring states, whereas the effects of ENDAs stop at state borders. This discontinuity in ENDAs allows us to difference away any unobserved confounding factors as long as they affect both the treated state and its neighbors. By comparing treated firms to their immediate neighbors, we can better identify how much of the observed innovation change is due to ENDAs rather than other shocks to local business conditions. When we difference away changes in local business conditions by focusing on treated and control firms closely located on either side of a state border, we continue to find a significant increase in firms' innovation after their states pass ENDAs, relative to their neighboring firms. These results suggest that the observed increase in innovation following the enactment of ENDAs is not driven by local economic shocks.

To provide further evidence that the effects of ENDAs on innovation are indeed tied to sexual orientation discrimination in the workplace, we apply a double difference-in-differences approach to examine heterogeneous treatment effects. We find that the treatment effects are stronger for firms that previously did not implement pro-gay non-discrimination policies, for firms that are in states with a large homosexual population, and for firms that operate in human capital-intensive industries. These cross-sectional variations in the treatment effects further increase our confidence that the impact of ENDAs on innovation is indeed related to workplace discrimination with respect to sexual orientation.

Finally, we examine the relocation decisions of individual employees, and investigate whether the matching process between firms and employees is indeed the mechanism through which ENDAs affect innovation. We find that, following the adoption of an ENDA, the adopting state experiences a greater inflow of inventors from other states while simultaneously experiencing a greater outflow of existing inventors to other states. We further find that the inventors who move out of the state mainly move to states that have not adopted ENDAs and that the inventors who move into the state are from all over the country, regardless of whether their states of origin adopted an ENDA or not. These results suggest that the enactment of ENDAs triggers workforce rebalancing: pro-gay employees are more likely to match with innovative firms following the adoption of ENDAs. We further show that, at the individual level, the inventors who move in are more productive at patenting than the inventors who move out, which is consistent with the view that pro-gay individuals tend to be more creative than anti-gay individuals.

Our setting of employing the staggered passage of ENDAs is highly appealing from an empirical standpoint for two reasons. First, the motivation behind adopting ENDAs centers around state courts' determination to address a persistent, widespread pattern of discrimination on the basis of sexual orientation and gender identity, and to reinforce the commitment to fairness and equal opportunity in the workplace. As ENDAs were not passed with the intention of promoting innovation, potential effects on innovation are likely to be an unintended consequence of these laws. Second, the staggered adoption of ENDAs in several U.S. states enables us to identify their effects in a difference-in-differences framework. Because multiple shocks affect different firms at different times, we can avoid the common identification difficulty faced by studies with a single shock: the potential biases and noise coinciding with the shock that directly affects corporate innovation (Roberts and Whited, 2012).

Our paper adds to studies examining the drivers of corporate innovation. This strand of literature is important for the economy, as Lerner and Seru (2015) point out (p. 2): "Not only is innovation critical in many cases to firm survival—witness the fates of firms which failed to successfully innovate, such as Kodak, Motorola, and Xerox—but it illustrates the critical issues that motivate corporate finance theory more generally. Topics such as uncertainty, information asymmetries, and the intangibility of assets are central when it comes to financing innovative firms and projects." Contributing to this strand of literature, our paper shows that laws that limit firms' ability to discriminate homosexual employees are an important driver of innovation.

The matching mechanism proposed in this paper differ from the well-known contract schemes to motive innovation in the existing literature (see, e.g., Ederer and Manso, 2013; Holmstrom, 1989; Manso, 2011). These principal-agent models usually treat a firm's employee base to be static and focus on incentivizing employees to be more innovative. Complementing these models, our matching mechanism treats a firm's employee base to be dynamic and focuses on attracting more creative employees from the external labor market.

Our paper has important policy implications. Though Title VII of the U.S. Civil Rights Act of 1964 provides comprehensive nationwide protection from discrimination based on race, color, national origin, gender, and religion, adding sexual minorities to that list remains a controversial topic across the U.S. While more than 20 of the 50 U.S. states have offered full legal protection, legislators in the remaining states are still debating whether or not to follow suit, partially because the impacts of these anti-discrimination laws on society and the economy are still unclear. Our paper provides evidence that the passage of this legislation fosters creativity in the workplace.

2. Hypothesis Development

Individual workers relocate for many reasons including employment, education, and economic opportunities, as well as feelings of safety, security, and a sense of belonging (Hagelskamp et al., 2010). Anti-gay prejudice often creates an environment where homosexuals lack a sense of belonging, frequently resulting in a desire for them to relocate to gay-friendly communities (McCallum and McLaren, 2011; Weston, 1995). Gorman-Murray (2007) and Rosenfeld and Kim (2005) find that sexual orientation is an important factor for homosexuals' decision to relocate, which usually involves such individuals moving from a location with greater sexual orientation discrimination to a location with less sexual orientation discrimination. Therefore, a state that passes an ENDA can become more attractive to homosexuals.

Suppose that two types of workers are affected by ENDAs: the pro-gay workers and the anti-gay workers. The pro-gay workers include homosexuals and their heterosexual allies. The anti-gay workers include those who hold a prejudice against homosexuals and derive disutility when working with them.

ENDAs promote gay rights and enhance homosexuals' employment opportunities in the workplace. Thus, we expect that more pro-gay workers will relocate to the state after the state adopts the ENDA. In contrast, for anti-gay workers, greater gay rights and more pro-gay employees decrease their utility and make them more likely to relocate to a state that has not adopted such a law. In summary, following the state's adoption of an ENDA, we expect that more pro-gay employees relocate to that state and more anti-gay employees relocate out of that state. Supporting this argument, Klawitter and Flatt (1998) provide evidence that homosexuals tend to work in states that have adopted ENDAs, and Florida (2011) documents a negative association between the concentration of gay and anti-gay populations.

Based on U.S. census data, Black et al. (2000) investigate the demographics of the homosexual population in the U.S. and find that, on average, homosexuals are better educated than heterosexuals. Kanazawa (2012) provides evidence that more intelligent individuals are more likely to engage in homosexual behavior. Moreover, among heterosexuals, those more likely to be pro-gay are younger, better educated, more tolerant, more open-minded, more risk-taking, have more diverse backgrounds, and exhibit a stronger ideological liberalism than those more likely to be anti-gay, which includes older people, more conservative people, and people with stronger religiosity (see, for example, Barth and Overby, 2003; Herek, 1994; Lewis and Gossett, 2008; Wilcox and Wolpert, 2000). Existing literature also finds that the personal attributes of pro-gay individuals listed above positively contribute to creativity and that the attributes listed above for anti-gay individuals are usually negatively associated with creativity (Bénabou et al., 2015; Florida, 2003; Jacoby, 1967; Oldham and Cummings, 1996). Therefore, we expect the pro-gay group to be, on average, more innovative than the anti-gay group.

In summary, the above discussion leads to our prediction that corporate innovation increases after the adoption of ENDAs. Moreover, we propose that ENDAs affect innovation through the mechanism of matching pro-gay employees (who are likely to be more creative) with innovative firms.

3. Institutional Details

3.1 Background on Sexual Orientation Discrimination

People who have a homosexual orientation account for a nontrivial part of the U.S. population. According to a review conducted by the Williams Institute at UCLA School of Law (Gates, 2011), approximately 3.5% (9 million) of American adults identify themselves as lesbian, gay, or bisexual. Moreover, 8.2% (19 million) of American adults have engaged in same-sex sexual activities at least once, and 11% (25.6 million) of American adults acknowledge at least some same-sex sexual attraction.

Discrimination on the basis of sexual orientation and gender identity is a widespread problem in the American workplace. Badgett et al. (2007) document a variety of studies showing that homosexuals experience various forms of discrimination, including denial of employment, workplace harassment, negative performance evaluations, denial of promotion, job termination, etc. In a June 2013 Pew Research Center survey of the American homosexual population, more than 20% of homosexual employees reported experiencing discrimination in the workplace.[1]

It is worth pointing out that, for sexual orientation discrimination to occur, one does not need to be open about her sexual orientation. This discrimination can be based on *perceived* sexual orientation or gender non-conformity. Barber (2002) and Diefenbach (2007) describe a variety of cases in which individuals are sexually harassed based on *perceived* sexual orientation (which is not necessarily the same as their actual sexual orientation). Sirin et al. (2004) and Wood-Nartker et al. (2007) provide examples of the degree to which individuals use cues like job titles and gender non-conformity as a way to determine an individual's sexual orientation. As a result, sexual orientation discrimination in the workplace is applicable to not only homosexuals but also heterosexuals who are *perceived* by others as homosexuals.

Although the U.S. does not have any federal legislation that prohibits sexual orientation discrimination in the labor market, American homosexuals have sought and won legal protections against employment discrimination at the state level in the last few decades. In 1977, the District of Columbia

[1] Pew Research Social & Demographic Trends, "A Survey of LGBT Americans," June 13, 2013. http://www.pewsocialtrends.org/2013/06/13/a-survey-of-lgbt-americans/#fn-17196-1.

became the first U.S. district to pass an anti-discrimination law that prohibits employment discrimination on the basis of sexual orientation (Human Rights Act of 1977). By the end of 2007, 20 states had followed suit. Table 1 presents a detailed list of statewide ENDAs provided by the Human Rights Campaign.[2]

The employment protections for homosexual employees provided in ENDAs have usually mirrored the earlier protections against workplace discrimination on the basis of race, gender, religion, national origin, and physical disability, and have allowed advocates to frame the sexual orientation protections as incremental additions to existing policies (Klawitter and Flatt, 1998). The existing literature shows that ENDAs have significantly increased awareness of sexual orientation discrimination, improved the living and working environment for homosexuals, and helped to create a level playing field for them (Button et al., 1995; Klawitter and Flatt, 1998). Martell (2014) finds that ENDAs increase the labor supply of homosexuals by 12 to 20 hours per week. Gates (2009) finds a significant wage increase of same-sex couples following the passage of ENDAs (especially among those with a higher level of education), which suggests that ENDAs have noticeably reduced workplace discrimination against homosexuals.

3.2 Adopting Process of ENDAs

The enactment of ENDAs reflects the sustained efforts of gay rights activists, who campaigned for the laws despite years of legislative defeats. For example, New York did not adopt such laws until more than 30 years after they were first introduced into the legislative debate (Sears et al., 2009). These defeats were due to opposition from conservative political and religious groups. These opponents regularly advance two conflicting arguments in debates: (1) protection for homosexuals is not necessary as they do not face pejorative treatment in the labor market, and (2) there will be a flood of complaints that will overwhelm state agencies following the laws' enactment. In contrast, proponents of such laws have insisted that there

[2] The list of statewide employment laws is obtained from http://hrc-assets.s3-website-us-east-1.amazonaws.com//files/assets/resources/statewide_employment_10-2014.pdf.

is a need for protection of homosexuals from discrimination and that any discrimination is unjust and should be remedied (Rubenstein, 2002).

Haider-Markel and Meier (2003) provide a case study on how Wisconsin passed an ENDA. Wisconsin established its anti-discrimination policies in 1982 and was the first U.S. state to adopt such a law (following the District of Columbia). The policy entrepreneur behind this legislation was David Clarenbach, a liberal state legislator. His strategy had four parts: (1) frame the legislation as an incremental change that extends current civil rights coverage to homosexuals, (2) defuse the religious issue by seeking support from mainline religious organizations, (3) gather bipartisan support for the bill, and (4) use the gay activists to do the ground work in the building of political support. Clarenbach introduced the legislation in every session from 1975 to 1981 but did not bring the issue to a vote, considering that the process required some long-term education of political elites on this issue. Clarenbach decided to push for passage of the bill in 1981 when his legislation to repeal Wisconsin's "sodomy" law lost by a single vote. With the help of gay activists who contacted individuals for support, Clarenbach managed to obtain endorsements from the Catholic archbishop of Milwaukee and most mainstream Protestant denominations, including Baptists. The effort was designed to isolate the Moral Majority as the sole religious group opposing the legislation. Clarenbach also worked together with the National Gay and Lesbian Task Force to gather support from major professional organizations such as the American Medical Association. Furthermore, in order to avoid having the legislation designated as a "Democratic bill" that could be used as a future campaign issue, Clarenbach encouraged several prominent Republican legislators to support the bill using libertarian arguments that the government should not regulate sexual preferences. This legislation was successfully passed as Republican Governor Lee Dreyfus signed the bill into law on February 25, 1982.

It is worth noting that Clarenbach's strategy was a classic one, which was widely followed by other regions in the U.S. to adopt ENDAs (Button et al., 1995). As summarized in Haider-Markel and Meier (2003), the successful adoption of ENDAs largely depends on skillful work by political elites, well-organized gay rights groups, and the absence of significant opposition groups. None of these factors seem to be directly related to an individual firm's patenting activities. Moreover, the primary purpose of ENDAs is to promote gays' rights of fairness and equality, rather than stimulate economic growth. Therefore, the staggered adoption of ENDAs seems not to be triggered by factors that drive corporate innovation.

4. Sample Formation and Variable Construction

4.1 The Sample

We retrieve patent and patent citation data from the worldwide Patent Statistical Database (PATSTAT, April 2012) and financial information from Compustat.[3] We then obtain the firm's headquarter information from Compustat, Compact Disclosure (which records headquarters' changes), and manually check any missing information.

We assume that firms produce zero patents if they are not matched with PATSTAT. Patents are included in the database only if they are eventually granted. Given the average of a two-year lag between patent application and patent grant, and that the latest year in the database is 2011, patents applied for from 2009 to 2010 may not appear in the database. Following the suggestion by Hall et al. (2001), we end our sample period in 2008.

We exclude firms incorporated outside the U.S., and firms in the financial industry (SIC codes 6000-6999) and utility industry (SIC codes 4900-4999) due to the differences in regulatory oversight for these industries. Following Bloom et al. (2013), we further exclude firms that never filed a single patent during our entire sample period. Our final sample consists of 58,009 firm-year observations (4,915 unique firms) from 1976 to 2008.

It is worth mentioning that the quality of the PATSTAT database is at least as good as that of the NBER patent database (which has been widely used in the innovation literature). Moshirian et al. (2015)

[3] We merge PATSTAT with Compustat using both PERL matching of company names and manual checks. We first use a PERL script to clean the company names by removing punctuation marks (such as dashes and commas) and suffixes (such as "inc" and "ltd"). Then we match the two databases based on these cleaned company names. Finally, we manually check whether the two firms matched by PERL are in fact the same firm.

compare the U.S. patent coverage in both databases, and find that they are generally comparable, except for a large decline in the number of patents from the NBER database over the 2002-2006 period. This difference is because many patent applications filed during this period were still under review and had not been granted by 2006 when the NBER database ended. However, the PATSTAT database does not suffer from this problem because it continues to include granted patents up to 2011.[4]

4.2 Innovation Variables

To assess the success of long-term investment in corporate innovation, we employ five innovation measures based on patent counts and patent citations. The use of patenting to measure a firm's innovativeness has been widely used in the literature since Scherer (1965) and Griliches (1981).

The first measure of innovation is the number of patents filed (and subsequently granted) by a firm in a given year. Our second measure of innovation is the sum of citation counts across all patents filed by the firm in a given year, which captures the significance of the patent outputs. Because citations are received for many years after a patent is created, patents created near the end of the sample period have less time to accumulate citations. To address this truncation bias, we follow the recommendations of Hall et al. (2001, 2005) and scale the citation count of each patent by the average citation count of all firms' patents that are filed in the same year.

Moreover, we employ citations per patent as the third measure of innovation to capture the patent's quality. Lastly, given that we are interested in determining whether or not workplace discrimination affects employees' productivity in innovative projects, we use patents and citations per employee as our last two innovation measures. Due to the high level of skewness of patent data, we use natural logarithms of the innovation variables.

[4] As a robustness check, we repeat our analysis based on the NBER patent database over a shorter period from 1976 to 2003 (the suggested ending year of using NBER patent database by Hall et al. (2001)), and find that our inference is unchanged.

4.3 Other Control Variables

We control for a vector of firm and industry characteristics that may affect a firm's innovation productivity, and these controls are motivated by prior literature (e.g., Fang et al., 2014; He and Tian, 2013). These variables include firm size, firm age, asset tangibility, leverage, cash holding, R&D expenditures, capital expenditures, ROA, Tobin's *Q*, and industry concentration (the Herfindahl index based on sales). Following Aghion et al. (2005), we also include the squared Herfindahl index in our regressions to mitigate non-linear effects of product market competition on innovation outputs. All explanatory variables are lagged by one year.[5] To minimize the effect of outliers, we winsorize all variables at the 1st and 99th percentiles. Detailed variable definitions are provided in the Appendix.

4.4 Summary Statistics

Table 2 provides summary statistics. On average, firms in our sample have 11 patents filed (and subsequently granted) per year and receive 22 total citations and 0.76 citations per patent. After normalizing patents and patent citations by number of employees, we find that an average firm generates 5.89 patents and 17 citations per 1000 employees.

Our average sample firms have book value assets of $2.49 billion, hire more than 9,000 employees, and are 16 years old. They hold a sizeable amount of cash with a cash ratio of 20% of total assets. The average R&D and capital expenditure account for 7% and 6% of total assets, respectively. The average firms are moderately levered with a book leverage ratio of 20%, and tangible assets account for 26% of total assets in the average firms. In terms of performance, sample firms perform well with an average ROA of 7% and Tobin's *Q* of 2.13.

[5] Aghion et al. (2013), Fang et al. (2014), and He and Tian (2013) find that analyst coverage, stock liquidity, and institutional ownership are also important determinants of corporate innovation. In an untabulated analysis, we include these variables in the regression based on a smaller sample with available information. We find a similar effect of ENDAs on corporate innovation. Moreover, in this paper, we use Ln (employees) to control for firm size largely because our paper focuses on how ENDAs influence employees' innovation performance. As a robustness check, we also use Ln (total assets) to control for firm size, and our inference is unchanged.

5. Empirical Results

5.1 Visual Illustration

Figure 1 depicts the effects of ENDAs on innovation in states that adopt the policy change relative to states that do not adopt the policy change. We follow Autor et al. (2006) and Acharya et al. (2014) in constructing this graph. The y-axis shows the logarithm of the number of patents or citations received to patents filed in a given year; the x-axis shows the time relative to the adoption of the anti-discrimination laws, ranging from five years prior to the adoption year (year 0) until ten years afterwards.

The plots demonstrate the point estimates of the coefficients β_n from the following regression:

$$Innovation_{i,t} = \alpha + \sum_{n=-5}^{10} \beta_n * Pass\_year_{s,t+n} + Year\ FE + \varepsilon_{i,t} \ , \qquad (1)$$

where *i* indexes firm, *s* indexes the state in which the firms' headquarters are located, and *t* indexes the year. $Pass\_year_{s,t+n}$ is a dummy variable indicating the year relative to the adoption of the ENDA in state *s* and year *t*.[6] The two plots in Figure 1 correspond to the number of patents and citations, respectively, and they show the same pattern. Innovation increases significantly after the adoption of ENDAs. For example, in the year prior to the law adoption, the β_{-1} coefficient is approximately 0.02 for patents, while in five years after the law adoption, the corresponding β_5 coefficient is more than six times as large (0.013). In terms of patent citations, the β_{-1} coefficient is approximately 0.08; in contrast, the corresponding β_5 coefficient is three times as large (0.25). Moreover, we observe that the greatest increase in innovation appears several years after the law adoption, suggesting that the passage of ENDAs has a persistent long-run effect.

5.2 Baseline Regression

Several U.S. state courts adopted the ENDAs in different years during the sample period. Thus, we can examine the before-after effect of the change in ENDAs in affected states (the treatment group) compared

[6] For example, $Pass\_year_{s,\,t+1}$ takes the value of one in the first year after the adoption of the ENDA in state *s*, and zero otherwise.

to the before-after effect in states in which such a change was not effected (the control group). This is a difference-in-differences test design in multiple treatment groups and multiple time periods as employed by Acharya et al. (2014), Atanassov (2013), Bertrand et al. (2004), and Imbens and Wooldridge (2009). We implement this test through the following regression:

$$Innovation_{i,t} = \alpha + \beta_1 Pass_{s,t-1} + \beta_2 Other\ Firm\ Characteristics_{i,t-1} + Firm\ FE + Year\ FE + Region \times Year\ FE + \varepsilon_{i,t}\ , \qquad (2)$$

where *i* indexes firm, *s* indexes the state in which the firms' headquarters are located, and *t* indexes the year. The dependent variable is a proxy for innovation performance. The variable *Pass* is a dummy variable that equals one if the ENDA is in place in state *s* in a given year, and zero otherwise. We include a set of control variables that may affect a firm's innovation output, as discussed in Section 4. The year fixed effects enable us to control for intertemporal technological shocks, as well as the fact that citations to patents applied for in later years would be, on average, lower than those in earlier years. Similarly, the firm fixed effects also allow us to control for time-invariant differences in patenting and citation practices across firms. Following Acharya et al. (2014), we also control for regional time trends through the interaction of region dummies with year dummies.[7] These interactions enable us to nonparametrically account for time-varying differences between geographic regions of the U.S. in corporate innovation and in the enactment of ENDAs. Given that our treatment is defined at the state level, we cluster standard errors by state.

The coefficient of interest in this model is the β_1 coefficient. As explained by Imbens and Wooldridge (2009), the employed fixed effects lead to β_1 being estimated as the *within-state* differences before and after the anti-discrimination law change as opposed to similar before-after differences in states that did not experience such a change during the same period.

[7] Following Acharya et al. (2014), we distinguish four U.S. regions based on the classification of U.S. Census Bureau: Northeast, South, Midwest, and West.

It is helpful to consider an example. Suppose we want to estimate the effect of the ENDA passed in Minnesota in 1993 on innovation. We can subtract the number of innovations before the law adoption from the number of innovations after the law adoption for firms headquartered in Minnesota. However, economy-wide shocks may occur at the same time and affect corporate innovations in 1993. To difference away such factors, we calculate the same difference in innovations for firms in a control state that does not adopt such a law. Finally, we calculate the difference between these two differences, which represents the incremental effect of the law change on firms in Minnesota compared to firms in the control state.

Table 3 presents the regression results. The coefficient estimates on the passage of ENDAs are positive and statistically significant in all columns. The dependent variable in column (1) is Ln(1+patents) and we find that the coefficient estimate on the *Pass* indicator is 0.076 and significant at the 5% level, suggesting a positive effect of the law change on corporate innovation. The economic magnitude is also sizeable: the adoption of ENDAs leads to an increase in the number of patents by approximately 7.9% ($= e^{0.076} - 1$).

Examining Ln(1+citations) as the dependent variable in column (2), we find that the coefficient on the *Pass* indicator is 0.106 and is significant at the 5% level, which implies that the adoption of ENDAs leads to an increase in the number of patent citations by approximately 11.2% ($= e^{0.106} - 1$).

The positive effects of ENDAs on the number of citations could be driven by either more patents or more citations per patent. To further examine the impact of each patent, we examine the number of citations per patent in column (3) and find that the law adoption has a significant and positive effect on citations per patent. These results indicate that ENDAs lead to an increase in not only the number of patents but also the impact of the patents.

In columns (4)-(5), we repeat our test using patents and citations scaled by the number of employees. The coefficients on the *Pass* indicator are 0.095 and 0.117, respectively, and both are significant at the 1% level. These results indicate that patents and citations per 1000 employees increase by approximately 9.96% and 12.41%, respectively, in states that adopt the ENDAs as compared to states that do not. Therefore, employees' productivity in innovation increases significantly after the ENDAs are adopted.

With regards to control variables, large firms, firms with large cash holdings, firms with high R&D expenditures, and firms with higher growth potential are more innovative. These results are broadly consistent with prior literature (e.g., Fang et al., 2014; He and Tian, 2013).

Taken together, these results indicate a positive effect of ENDAs on innovation outputs in terms of both quantity and quality.

5.3 The Pre-treatment Trends

The validity of a difference-in-differences estimation depends on the parallel trends assumption: absent the ENDAs, treated firms' innovation would have evolved in the same way as that of control firms. Table 4 investigates the pre-trend between the treated group and control group. In particular, we define seven dummies, *Year −2*, *Year −1*, *Year 0*, *Year +1*, *Year +2*, *Year +3*, and *Year +4 and afterwards* to indicate the year relative to the enactment of ENDAs. For example, year 0 indicates the year in which the law is enacted; year −2 indicates that it is 2 years before the law enactment; and year +2 indicates that it is 2 years after the law enactment. Then, we re-estimate Equation (2) by replacing the *Pass* indicator with the seven indicators above.

The coefficients on *Year −2* and *Year −1* indicators are especially important because their significance and magnitude indicate whether there is any difference in innovation between the treatment group and the control group prior to the adoption of ENDAs. The coefficients on both indicators are close to zero and not statistically significant across all five columns, suggesting that the parallel trend assumption of the difference-in-differences approach is not violated.

The coefficients on *Year 0* and *Year +1* indicators are also small in magnitude and insignificant in all five columns. The impact of ENDAs starts to show up two years after the enactment: the coefficients on the *Year +2* indicator become significantly positive for patents per 1000 employees (column (4)) and citations per 1000 employees (column (5)). The coefficients on *Year +4 and afterwards* are more than twice as large as the coefficients on the *Year 0* indicator for all five innovation measures, indicating that it takes a few years to reveal the full impact of ENDAs on corporate innovation. This is understandable given that innovation is usually a long-term process. This result is also consistent with the pattern illustrated in Figure 1.

Overall, Table 4 shows that the treated group and the control group share a similar trend in innovation prior to the law changes, thus supporting the parallel trends assumption associated with the difference-in-differences estimation. Moreover, Table 4 also indicates that most of the impact of ENDAs on innovation occurs three years *after* they are passed.

5.4 Confounding Local Business Conditions

Location is one important common factor that likely induces an association between the passing of anti-discrimination laws and corporate innovation. Specifically, corporations with the strongest innovation performance are concentrated in California, New York, New Jersey, Massachusetts, Connecticut, Illinois, and Texas. Of these seven states, six (all but Texas) are "liberal" states, where the combination of general attitudes and state policies are much more likely to give rise to active anti-discrimination policies than in more conservative states. This geographic effect would tend to induce correlations between the passing of ENDAs and corporate innovation.

In this section, we implement two tests to address this issue. In our first test, we additionally control for a set of observable state characteristics in the regression. In our second test, we difference away unobservable local business conditions by focusing on treatment firms that are on one side of a state border and their neighboring control firms on the other side of the state border. In both tests, we continue to find a significant increase in innovation after the adoption of ENDAs.

Table 5 presents our first test. In addition to our usual set of explanatory variables used in Table 3, we also account for various time-varying, state-level variables in our regressions. We control for the political balance in a given state (measured as the ratio of Republican to Democrat state representatives in the House of Representatives). Further, since richer and larger states may have the resources to provide a higher level of innovation and may also be more likely to pass anti-discrimination legislation, we include the logarithm of real GDP in a state. We additionally control the logarithm of annual state population. Further, investment in education is another factor that may lead to differences in patenting. Therefore, we also control for a state's intellectual resources using the number of degree-granting institutions of higher education, as well as the enrollment in institutions of higher education. Data on both state GDP and population are collected from the U.S. Bureau of Economic Analysis. Information regarding the number of colleges, college enrollment, and political balance is obtained from the annual Statistical Abstracts from the U.S. Census Bureau.

We find that the adoption of ENDAs continues to have a positive and (statistically and economically) significant impact on corporate innovation. Compared to Table 3, the coefficient on the *Pass* dummy becomes a little smaller in columns (1) and (2), but gets bigger in columns (3)-(5). Also, we find that more colleges in a state are (weakly) positively associated with innovation quality in column (3). Other state-level variables have no significant impact on corporate innovation, probably because we have already controlled for *Region* × *Year FE* in the regression.

Although the above test accounts for *observable* local business conditions, some unobservable local economic shocks may be associated with both the passage of ENDAs and corporate innovation. In our second test, we exploit the discontinuity of ENDAs and examine the innovation change in the treatment firms on the state border relative to their neighboring control firms. The logic is as follows. Suppose that ENDAs are driven by unobserved changes in local business conditions, and that it is these changes, not the ENDAs, that spur corporate innovation. Then both firms in treated states and their neighbors in untreated states just across the state border would spuriously appear to react to the law changes, because economic conditions, unlike state laws, have a tendency to spill across state borders (Heider and Ljungqvist, 2015). In this case, the change in innovation in treated firms should be no different from that in the neighboring control firms that are located just across the state border.

To examine this possibility, we match each treated firm to a control firm that is in the same industry (three-digit SIC), is in an adjacent state without passing the ENDA, and is closest to the treated firm in distance. Obviously, treated firms may not necessarily share the same local economic conditions with its "closest" control firm if the treated firm is in the middle of a large state. To alleviate this concern, we further require that the distance between the treated firm and its matched untreated firm be within 50 miles.[8] If the distance between the treated firm and its closest control firm is more than 50 miles, we drop this pair from our sample. By doing so, we increase our confidence that our treated firm and control firm are truly close to each other geographically and thus face similar local economic shocks. Then, we re-estimate Equation (2) by focusing on this sub-sample of firms across the state border. We also include a pair fixed effect for each pair of treated firms and neighboring control firms.

Table 6 presents the results. Restricting our sample to the pairs of neighboring treated and control firms reduces the sample to 7,617 firm-year observations; yet, we still find positive and significant coefficients (at the 1% level) on the *Pass* indicator in all five columns (except for column (1)). Taking column (2) for example (where patent citations is the dependent variable), the coefficient on *Pass* is 0.140 and is significant at the 1% level, indicating that the number of patent citations increases by approximately 15% in the treated firms relative to untreated firms in the same industry located just on the other side of the state border. The point estimate is even slightly larger than that reported in our baseline regression in column (2) of Table 3. Overall, these results suggest that unobserved local confounds seem unlikely to drive our results.

5.5 Double Difference-in-differences Tests

To provide further evidence that the effects of ENDAs on innovation are indeed tied to sexual orientation discrimination in the workplace, in this subsection we implement double difference-in-differences tests to examine the heterogeneous treatment effects. Examining heterogeneous treatment effects can further help

[8] As a robustness check, we also require the distance between the treated firm and control firm to be within 10, 20, 30, or 100 miles, and our inferences are unchanged.

to alleviate the concern that some omitted firm or state variables are driving our results, because such variables would have to be uncorrelated with all the control variables we include in the regression model and they would also have to explain the cross-sectional variation of the treatment effects. As pointed out by Claessens and Laeven (2003) and Raddatz (2006), it is less likely to have an omitted variable correlated with the interaction term than with the linear term.

First, if the enhanced innovation after the law enactment is due to reduced sexual orientation-related discrimination in the workplace, we expect this treatment effect to be stronger in firms that do not have pro-gay non-discrimination policies prior to the treatment. We obtain the information on firm-level pro-gay non-discrimination policies from the Kinder, Lydenberg, Domini Research & Analytics (KLD) ratings database, which covers approximately 650 companies that have comprised the Domini 400 Social SM Index and the S&P 500 index since 1991 and more than 3,000 companies that have comprised the Russell 3000 index since 2003. The KLD database provides an indicator variable to flag whether or not a firm has a pro-gay non-discrimination policy in a given year, and this policy indicates that the company has implemented notably progressive policies toward its gay and lesbian employees.

We re-estimate Equation (2) by replacing the *Pass* indicator with *Pass* × *Pro-gay* and *Pass* × *Non-pro-gay* indicators. The *Pro-gay* indicator takes the value of one if the firm has a pro-gay non-discrimination policy prior to the passage of ENDAs, and zero otherwise. Similarly, the *Non-pro-gay* indicator is defined as (1− *Pro-gay*). Table 7 presents the results.[9]

The coefficients on *Pass* × *Pro-gay* are not significantly different from zero, while the coefficients on *Pass* × *Non-pro-gay* are positive and significant across all five columns. This result indicates that the effect of ENDAs on corporate innovation is significant for firms that previously had not adopted pro-gay non-discrimination policies, whereas it is virtually absent in firms that had done so.

Furthermore, the impact of ENDAs on corporate innovation may also depend on the state's homosexual population. *Ex ante*, there could be two different views on how a state's homosexual population is related to the treatment effect. On one hand, if there is a large homosexual population in a

[9] Because the KLD database starts in 1991, the sample in Table 7 is smaller than that in Table 3.

state, then homosexual employees are likely to account for a sizeable part of a firm's workforce and, thus, the treatment effect is likely to be more pronounced. On the other hand, a large homosexual population in a state indicates friendliness of the state towards homosexual people in the first place; for this reason, the treatment effect should be less pronounced. We empirically examine these two views by obtaining the information on a state's homosexual population from the 2005 American Community Survey.[10] We define the *High gay population state* indicator as taking the value of one for the top ten states with the largest percentage of population that is gay, lesbian, or bisexual, and zero otherwise.[11] Then, *Low gay population state* is defined as (1− *High gay population state*). We re-estimate Equation (2) by replacing the *Pass* indicator with *Pass* × *High gay population state* and *Pass* × *Low gay population state* indicators.

As reported in Table 8, the coefficients on *Pass* × *High gay population state* are positive and significant at the 1% level across all five columns, while the coefficients on *Pass* × *Low gay population state* are usually not significant and much smaller in magnitude. For example, in column (1) (where the dependent variable is the patent number), the coefficient on *Pass* × *High gay population state* is 0.116 and significant at the 1% level; in contrast, the coefficient on *Pass* × *Low gay population state* is only 0.034 and not significantly different from zero.

Lastly, considering that ENDAs affect productivity associated with human capital, not physical capital, the treatment effects should be stronger for firms that rely more on human capital. Following Coff (2002), we measure human capital intensity as the number of knowledge workers as a proportion of all workers. We obtain data on employment levels from the Occupational Employment Statistics (OES) survey from the Bureau of Labor Statistics. Based on the OES occupational codebook, we define knowledge workers to be those with an occupational code below 50,000. This definition includes occupations such as managers, scientists, engineers, computer programmers, IT professionals, and so forth. The OES provides data on the breakdown of the total number of workers employed in each three-

[10] Earlier survey information on the homosexual population is also available in the Census 1990 and Census 2000 data. The 2005 American Community Survey may provide a more accurate estimation of the homosexual population because more homosexual people have been willing to identify themselves in recent years (Gates, 2006).
[11] The top ten states (and district) with the largest percentage of homosexual people are District of Columbia, New Hampshire, Massachusetts, Maine, California, Colorado, Vermont, New Mexico, Minnesota, and Florida.

digit SIC industry. From the OES data, we calculate the proportion of the total workforce being knowledge workers for a given three-digit SIC industry, and then assign that measure to each focal firm in our sample. We then define the *High human capital intensity* indicator as one if the proportion of knowledge workers among all workers is above the sample median, and zero otherwise. Then, the *Low human capital intensity* indicator is defined as (1− *High human capital intensity*). We re-estimate Equation (2) by replacing the *Pass* indicator with *Pass* × *High human capital intensity* and *Pass* × *Low human capital intensity* indicators.

Table 9 presents the results. The coefficients on *Pass* × *High human capital intensity* are positive and significant at the 1% level across all five columns, while the coefficients on *Pass* × *Low human capital intensity* are virtually zero. For example, in column (2) (where the dependent variable is patent citations), the coefficient on *Pass* × *High human capital intensity* is 0.237 and significant at the 1% level, while the coefficient on *Pass* × *Low human capital intensity* is only -0.048 and not significantly different from zero.

Taken together, the effects of ENDAs on corporate innovation are much stronger for firms that previously have not had pro-gay non-discrimination policies, for firms in a state with a large homosexual population, and for firms in human capital-intensive industries. These results suggest that the impact of ENDAs on corporate innovation is indeed tied to sexual orientation discrimination and seems not to be spuriously driven by unobserved heterogeneity.

5.6 Evidence on Inventors' Relocation

As discussed in Section 2, we expect that ENDAs affect innovation through the firm-employee matching channel. That is, pro-gay employees move into ENDA adopting states, while anti-gay employees move out. This matching process fosters innovation because pro-gay employees are usually younger, better-educated, more tolerant, open-minded, and risk-taking (and thus more innovative) than anti-gay employees. In this section, we investigate this channel by exploiting inventors' relocation. We collect individual inventor data from the Harvard Business School Patent Dataverse, which provides information on both inventors (i.e., the persons who produce the patents) and assignees (i.e., companies that own the patents). We can thus track the mobility records of inventors across different firms.

We define an inventor as a new hire for firm *i* in year *t* if she files for her first patent in firm *i* in year *t* after filing a patent in a different firm. Similarly, we define an inventor as a leaver for firm *i* in year *t* if she previously filed patents for firm *i* but starts to file patents for another firm in year *t*. Then, we compute the number of leavers and the number of new hires for each firm in a given year.

Table 10 reports the difference-in-differences tests that examine the impacts of ENDAs on inventors' relocation. The regression specification is the same as Equation (2), except that we use the measures of inventors' relocation as the dependent variables. In column (1), the dependent variable is Ln (1+ number of leaver) and the coefficient on the *Pass* indicator is positive and significant at the 1% level. In column (2), the dependent variable is Ln (1+ number of new hire) and the coefficient on the *Pass* indicator is also positive and significant at the 1% level. This result indicates that after the adoption of ENDAs, local firms experience more inventors moving in while more existing inventors move out. We examine Ln (1+ number of net new hire) as the dependent variable in column (3), where the number of net new hires is the difference between the number of new hires and the number of leavers.[12] We find a significantly positive coefficient on the *Pass* indicator, suggesting that newly hired inventors still outnumber the leaving inventors. This result is broadly consistent with Martell (2014), who finds that ENDAs significantly increase the labor supply of homosexuals.

In columns (4) and (5), we further investigate the locations of new firms for the leaving inventors. In column (4), we investigate the leavers whose new firm is located in a different state that has also adopted an ENDA. We find that the coefficient on the *Pass* indicator is not significantly different from zero. However, in column (5), we examine the leavers whose new firm is located in a different state that has not adopted an ENDA, and we find a positive and significant coefficient on the *Pass* indicator. Thus, the outflow of inventors following the enactment of ENDAs is mainly driven by those who move to a

[12] If the *net new hire* is negative, the dependent variable is set as –Ln(1+absolute value of net new hire). For example, when the *net new hire* is –5, the dependent variable is –Ln(6).

state without such a law. This result seems to suggest that the leavers are not comfortable with the state's adoption of the ENDA, so they relocate to avoid this law.

In columns (6) and (7), we investigate the new hire's previous location. We separately examine the new hires from the states that have adopted ENDAs and from the states that haven't, respectively. We find that the coefficients on the *Pass* indicator are positive and significant in both columns, suggesting that the adoption of ENDAs helps local firms hire inventors from both gay-friendly and gay-unfriendly states.

Finally, we investigate the productivity of leaving and newly hired inventors in Table 11. We track patents filed (and eventually granted) by each inventor, and the patent citations received by these patents over our sample period. On average, the leaving inventors have 10.56 patents in total during our sample period, while the newly hired inventors have 13.32 patents. The difference is significant at the 1% level. In terms of citations, the average leaving inventor receives 13.07 patent citations, while the average newly hired inventor has a significantly larger number (16.94). We obtain the same inference when comparing the median number of patents and patent citations. These results indicate that the individual productivity of the newly hired inventors is greater than that of leaving inventors, consistent with the conjecture that pro-gay employees are likely to be more creative than anti-gay employees.

Overall, Tables 10 and 11 provide evidence supporting that ENDAs affect innovation by triggering workforce rebalancing, with more pro-gay employees (who tend to be more creative than anti-gay employees) matched with innovative firms.[13]

5.7 Limitations

So far, our evidence suggests that ENDAs have a positive effect on corporate innovation. However, our study has two limitations. First, the adoption of ENDAs might not be random and could be an endogenous

[13] This matching may also occur if firms (instead of employees) choose to relocate into the states that passed the ENDAs. However, we find very few cases of firm relocation. In our sample, only 165 firms relocate to a different state after their previous states adopt the ENDAs. On average, their relocations occur 8 to 9 years after the adoption of ENDAs. Thus, this suggests that the matching mechanism is more about employee relocation rather than firm relocation.

decision by the states. It may be that forward-thinking, progressive business leaders press states to pass such laws, and also such leaders invest in innovation. Thus, the mindset of business leaders (which is difficult to control in the regression) could be the true driving force. Even if the enactment of ENDAs is not prompted by any pressure from business leaders, it is possible that the state is doing so as part of a general program to improve business conditions, and is thus accompanying ENDAs with other forward-thinking policies that may foster innovation. Although we implement several analyses to address this concern (such as controlling for various state characteristics, and using control firms from neighboring states just across the border), we acknowledge that these analyses may not fully address this non-randomness issue associated with ENDAs.[14]

Second, although we provide some evidence in Tables 10 and 11 that ENDAs affect corporate innovation through the mechanism of workforce rebalancing, we are unable to directly identify pro-gay or anti-gay employees due to a lack of data on employees' sexual orientation. Thus, this piece of evidence is only suggestive. In summary, the readers should bear in mind these two limitations when deciding how our findings might be generalized.

6. Conclusions

In this paper, we investigate the effect of ENDAs on corporate innovation. Using a difference-in-differences approach, we find a significant increase in firms' patents and patent citations following the law changes, relative to firms in states that do not pass such laws. We further find that the impact of ENDAs on corporate innovation is more pronounced when the firms previously did not have a pro-gay non-discrimination policy in place, when the state has a large homosexual population, and when the firms rely more on human capital. Finally, based on individual inventor information, we provide evidence suggesting that ENDAs spur innovation by triggering workforce rebalancing and matching pro-gay employees (who are likely to be more creative) with innovative firms.

[14] We thank an anonymous referee for pointing out this limitation for us.

Our paper provides important implications not only for technology firms' hiring strategies, but also for public policies aimed at fostering innovation. Our results suggest that policies aimed to promote equal employment can have real economic effects in terms of improving corporate innovation. This finding is particularly timely and relevant because of the ongoing consideration of federal legislation to ban sexual orientation discrimination in the workplace across the nation.

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Appendix: Variable Definitions

Variable	*Definition*
Measures of Innovation Output	
LnPat	Natural logarithm of one plus firm's total number of patents filed (and subsequently granted).
LnCit	Natural logarithm of one plus firm's total number of citations received on the firm's patents filed. To adjust the citation count, each patent's number of citations is divided by the average citation count of all patents applied in the same year.
LnCit/pat	Natural logarithm of one plus firm's average number of citations received on the firm's patents filed. If the firm filed no patents in that year, the missing value of average citation counts is set to zero.
LnPat/emp	Natural logarithm of one plus firm's total number of patents filed (and subsequently granted), scaled by the number of the firm's employees.
LnCit/emp	Natural logarithm of one plus firm's total number of citations received on the firm's patents filed (and subsequently granted), scaled by the number of the firm's employees.
Firm Characteristics	
Cash	Cash and marketable securities normalized by the book value of total assets.
Firm size	Natural logarithm of the number of employees.
Leverage	Total debt normalized by the book value of total asset.
R&D	R&D expenditures normalized by the book value of total assets. If R&D expenditures variable is missing, we set the missing value to zero.
Capex	Capital expenditures normalized by the book value of total assets.
ROA	Return on assets, measured as operating income normalized by the book value of total assets.
Firm age	Number of years since the firm's first appearance in CRSP.
Tobin's Q	Market value of equity plus book value of assets minus book value of equity minus balance sheet deferred taxes, normalize by the book value of total assets.
Tangible	Property, plant & equipment normalized by the book value of total assets.
Hindex	Herfindahl index is the sum of squared sales-based market shares of all firms in a three-digit SIC industry.

Pro-gay	An indicator variable that takes the value of one if the company has implemented notably progressive policies toward its gay and lesbian employees, and zero otherwise.
Non-pro-gay	1−Pro-gay.
High human capital intensity	An indicator variable that takes the value of one for the firm whose proportion of knowledge workers among all workers is above the sample median, and zero otherwise.
Low human capital intensity	1− High human capital intensity.
State Characteristics	
Pass	An indicator variable that takes the value of one if a state has adopted the ENDA which prohibits discrimination based on sexual orientation and gender identity in a given year, and zero otherwise.
Ln(State GDP)	Natural logarithm of annual state GDP.
Ln(Population)	Natural logarithm of a state's population.
Ln(Colleges)	Natural logarithm of the number of degree-granting institutions of higher education in a given state.
Ln(Enrollment)	Natural logarithm of enrollment in institutions of higher education in a given state.
Political balance	The ratio of Republican-to-Democrat representatives in the Lower House (House of Representatives) for a given state.
High gay population state	An indicator variable that takes the value of one for the top ten states with the largest percentage of population that is gay, lesbian, or bisexual, and zero otherwise.
Low gay population state	1− High gay population state.

Table 1: List of the Passages of State Employment Non-Discrimination Acts (ENDAs)

This table reports the year when each state adopted state-level Employment Non-Discrimination Acts that prohibit discrimination based on sexual orientation and gender identity, from 1976 to 2007.

State	Law	Year
District of Columbia	D.C. CODE 1-2512	1977
Wisconsin	WIS. STAT. 111.32 (13m)	1982
Massachusetts	MASS. GEN. LAWS ch. 151B, 3 (6)	1989
Connecticut	CONN. GEN. STAT. 46a-81a	1991
Hawaii	HAW. REV.STAT. 378-1	1991
Vermont	1 VT. STAT.ANN. 143	1991
California	CAL. GOV. CODE 12940	1992
New Jersey	N.J. STAT. 10:5-5 (hh)	1992
Minnesota	MINN.STAT. 363A.03 subd. 44	1993
Rhode Island	R.I. GEN. LAWS 28-5-6 (7)	1995
New Hampshire	N.H. REV. STAT. ANN. 354-A:2 (XIV-c)	1998
Nevada	NEV.REV.STAT. 613.310 (6)	1999
Maryland	MD CODE, STATE GOV'T, 20-606	2001
New Mexico	N.M. STAT. 28-1-2 (P)	2003
New York	N.Y. EXEC. LAW 292 (27)	2003
Maine	ME. REV. STAT. ANN. Tit. 5 4553 (9-C)	2005
Illinois	775 ILCS 5/1-102 (O-1)	2006
Washington	WASH.REV.CODE 49.60.040 (15)	2006
Colorado	COLO.REV.STAT. 24-34-401 (7.5)	2007
Iowa	IOWA CODE 216.2 (14)	2007

Table 2: Summary Statistics

The sample consists of 58,009 firm-year observations from 1976-2008. We obtain patent information from PATSTAT and financial information from Compustat. Definitions of all variables are provided in the Appendix. All dollar values are in 2008 dollars. All continuous variables are winsorized at the 1st and 99th percentiles.

Variable	N	Mean	SD	P1	Median	P99
Patents	58009	11.05	87.64	0	0	183
Citations	58009	22.42	193.97	0	0	360
Citations per patent	58009	0.76	1.45	0	0	6.45
Patents per 1000 employees	58009	5.89	24.43	0	0	100
Citations per 1000 employees	58009	17.03	92.89	0	0	317.8
Cash	58009	0.20	0.23	0.001	0.10	0.90
Firm assets ($b)	58009	2.49	15.54	0.003	0.19	39.13
Number of employees in 1000s	58009	9.48	41.17	0.01	1.06	127.8
Firm age	58009	16.37	15.67	1	11	72
Tobin's Q	58009	2.13	1.97	0.433	1.45	11.64
ROA	58009	0.07	0.22	-0.926	0.12	0.40
Leverage	58009	0.20	0.18	0	0.17	0.84
Tangible	58009	0.26	0.18	0.01	0.22	0.80
R&D	58009	0.07	0.11	0	0.03	0.52
Capex	58009	0.06	0.06	0.002	0.05	0.29
H-index	58009	0.18	0.15	0.04	0.14	0.79

Table 3: Effect of ENDAs on Innovation

This table reports the difference-in-differences tests that examine the impacts of ENDAs on corporate innovation. The indicator variable *Pass* takes the value of one if a state has adopted the ENDA which prohibits discrimination based on sexual orientation and gender identity in a given year, and zero otherwise. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust t-statistics based on standard errors clustered by state are in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) LnPat	(2) LnCit	(3) LnCit/pat	(4) LnPat/emp	(5) LnCit/emp
Pass	0.076**	0.106**	0.025**	0.095***	0.117***
	(2.25)	(2.60)	(2.40)	(3.46)	(3.69)
Cash	0.224***	0.330***	0.142***	0.405***	0.562***
	(3.97)	(5.69)	(7.35)	(6.41)	(8.40)
Firm size	0.186***	0.214***	0.039***	-0.029**	-0.019
	(7.90)	(7.94)	(6.41)	(-2.14)	(-1.14)
Ln(Firm age)	-0.015	-0.011	0.014**	-0.003	0.015
	(-0.84)	(-0.57)	(2.46)	(-0.18)	(0.95)
Tobin's Q	0.016***	0.018***	0.004**	0.006	0.008
	(5.75)	(5.33)	(2.64)	(1.19)	(1.41)
Leverage	-0.022	0.055	0.015	-0.067*	0.025
	(-0.55)	(1.02)	(0.74)	(-1.71)	(0.42)
R&D	0.227*	0.360***	0.172***	0.729***	0.921***
	(1.88)	(2.71)	(4.78)	(5.33)	(6.08)
Capex	0.042	0.107	0.092*	0.189*	0.196
	(0.49)	(1.04)	(1.75)	(1.72)	(1.62)
Tangible	-0.158*	-0.251**	-0.027	-0.059	-0.108
	(-1.85)	(-2.62)	(-0.64)	(-0.53)	(-0.98)
ROA	-0.008	0.018	0.027	0.040	0.046
	(-0.21)	(0.36)	(1.14)	(0.77)	(0.66)
H-index	-0.282	-0.465	-0.182**	-0.234	-0.378*
	(-0.92)	(-1.35)	(-2.23)	(-1.33)	(-1.91)
H-index^2	0.639*	0.861**	0.220**	0.351*	0.495**
	(1.84)	(2.25)	(2.49)	(1.75)	(2.54)
Constant	0.765***	0.884***	0.381***	0.698***	0.797***
	(6.91)	(8.41)	(12.90)	(8.41)	(9.85)
Observations	58009	58009	58009	58009	58009
Year FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.751	0.726	0.405	0.571	0.620

Table 4: Testing for Pre-treatment Trends and Reversals

This table investigates the pre-treatment trends between the treated group and control group. The indicator variables *Year −2*, *Year −1*, *Year 0*, *Year +1*, *Year +2*, *Year +3*, and *Year +4 and afterwards*, indicate the year relative to the adoption of ENDAs that prohibit discrimination based on sexual orientation and gender identity. For example, the *Year +1* indicator takes the value of one if it is one year after a state adopts the ENDA, and zero otherwise. All the firm characteristics used in Table 3 are also included in this regression but unreported for brevity. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust t-statistics based on standard errors clustered by state are in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) LnPat	(2) LnCit	(3) LnCit/pat	(4) LnPat/emp	(5) LnCit/emp
Year −2	0.034	0.055	0.007	0.022	0.037
	(1.15)	(1.27)	(0.33)	(0.77)	(1.00)
Year −1	0.027	0.059	0.012	0.031	0.052
	(0.86)	(1.47)	(0.68)	(0.94)	(1.53)
Year 0 (event year)	0.055	0.054	0.020	0.068	0.054
	(1.33)	(1.01)	(0.83)	(1.58)	(1.09)
Year +1	0.051	0.064	0.013	0.068*	0.078
	(1.32)	(1.26)	(0.74)	(1.74)	(1.58)
Year +2	0.043	0.076	0.017	0.090***	0.104***
	(1.00)	(1.65)	(0.86)	(2.93)	(2.90)
Year +3	0.070	0.082*	0.024	0.104***	0.124***
	(1.67)	(1.68)	(1.51)	(3.38)	(3.58)
Year +4 and afterwards	0.122***	0.172***	0.041***	0.133***	0.166***
	(2.72)	(3.12)	(3.02)	(3.86)	(4.32)
Other controls			Same as Table 3		
Observations	58009	58009	58009	58009	58009
Year FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.751	0.727	0.405	0.571	0.620

Table 5: Controlling for State-level Characteristics

This table reports the difference-in-differences tests that examine the impacts of ENDAs on corporate innovation, controlling for state-level characteristics. The indicator variable *Pass* takes the value of one if a state has adopted the ENDA which prohibits discrimination based on sexual orientation and gender identity in a given year, and zero otherwise. Ln(State GDP) is the natural logarithm of annual real state GDP. Ln(Population) is the natural logarithm of a state's population. Ln(Colleges) is the natural logarithm of the number of degree-granting institutions of higher education in a given state. Ln(Enrollment) is the natural logarithm of enrollment in institutions of higher education in a given state. Political balance is the ratio of Democrat-to-Republican representatives in the Lower House (House of Representatives) for a given state. All the firm characteristics used in Table 3 are also included in this regression but unreported for brevity. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust t-statistics based on standard errors clustered by state are in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) LnPat	(2) LnCit	(3) LnCit/pat	(4) LnPat/emp	(5) LnCit/emp
Pass	0.072**	0.100**	0.027**	0.098***	0.122***
	(2.24)	(2.55)	(2.32)	(3.51)	(3.65)
Ln(State GDP)	-0.042	0.002	-0.008	-0.119	-0.073
	(-0.42)	(0.01)	(-0.17)	(-1.31)	(-0.64)
Ln(Population)	0.138	0.090	-0.082	0.099	0.028
	(1.06)	(0.46)	(-1.18)	(0.87)	(0.16)
Ln(Colleges)	0.007	0.037	0.043*	0.016	0.064
	(0.11)	(0.51)	(1.78)	(0.23)	(0.82)
Ln(Enrollment)	-0.104	-0.133	0.044	-0.003	-0.029
	(-0.83)	(-0.91)	(0.98)	(-0.03)	(-0.23)
Political balance	-0.022	-0.031	-0.001	0.007	0.011
	(-0.91)	(-1.00)	(-0.12)	(0.35)	(0.45)
Other controls			Same as Table 3		
Observations	58009	58009	58009	58009	58009
Year FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.752	0.727	0.405	0.570	0.620

Table 6: Treated Firms and Neighboring Control Firms across State Borders

This table examines whether the effect of ENDAs on innovation is confounded by unobserved changes in local business conditions. For each treated firm, we match to a control firm that is in the same industry, in a neighboring state without adopting the ENDA, and closest in distance. The indicator variable *Pass* takes the value of one if a state has adopted the ENDA which prohibits discrimination based on sexual orientation and gender identity in a given year, and zero otherwise. All the firm characteristics used in Table 3 are also included in this regression but unreported for brevity. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust t-statistics based on standard errors clustered by state are in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) LnPat	(2) LnCit	(3) LnCit/pat	(4) LnPat/emp	(5) LnCit/emp
Pass	0.047	0.140***	0.077***	0.125***	0.206***
	(1.60)	(4.11)	(5.34)	(3.83)	(5.37)
Other controls			Same as Table 3		
Observations	7617	7617	7617	7617	7617
Year FEs	Yes	Yes	Yes	Yes	Yes
Pair FEs	Yes	Yes	Yes	Yes	Yes
Region × year FEs	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.698	0.666	0.312	0.427	0.466

Table 7: Heterogeneous Treatment Effects based on Firms' Pro-gay Policy

This table reports the double difference-in-differences tests that examine the relative impacts of ENDAs on innovation in firms with and without pre-existing firm-level pro-gay policies. The indicator variable *Pro-gay* takes the value of one if a firm has a pro-gay policy prior to the enactment of the ENDA, and zero otherwise. *Non-pro-gay* is (1− *Pro-gay*). The indicator variable *Pass* takes the value of one if a state has adopted the ENDA which prohibits discrimination based on sexual orientation and gender identity in a given year, and zero otherwise. All the firm characteristics used in Table 3 are also included in this regression but unreported for brevity. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust t-statistics based on standard errors clustered by state are in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) LnPat	(2) LnCit	(3) LnCit/pat	(4) LnPat/emp	(5) LnCit/emp
Pass × Pro-gay	0.116	0.165	0.047	0.053	0.079
	(0.72)	(1.25)	(1.22)	(1.57)	(0.89)
Pass × Non-pro-gay	0.118**	0.357**	0.129**	0.084**	0.129*
	(2.23)	(2.50)	(2.02)	(2.54)	(1.80)
Other controls			Same as Table 3		
Observations	8613	8613	8613	8613	8613
Year FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.870	0.848	0.510	0.850	0.846

Table 8: Heterogeneous Treatment Effects based on States' Gay Population

This table reports the double difference-in-differences tests that examine the relative effects of ENDAs on corporate innovation in different states based on the gay, lesbian, and bisexual population. The indicator variable *High gay population state* takes the value of one for the top ten states with the largest percentage of population that is gay, lesbian, or bisexual, and zero otherwise. *Low gay population state* is (1− *High gay population state*). The indicator variable *Pass* takes the value of one if a state has adopted the ENDA which prohibits discrimination based on sexual orientation and gender identity in a given year, and zero otherwise. All the firm characteristics used in Table 3 are also included in this regression but unreported for brevity. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust t-statistics based on standard errors clustered by state are in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) LnPat	(2) LnCit	(3) LnCit/pat	(4) LnPat/emp	(5) LnCit/emp
Pass × High gay population state	0.116*** (2.77)	0.164*** (3.17)	0.035** (2.65)	0.130*** (3.53)	0.174*** (3.72)
Pass × Low gay population state	0.034 (0.77)	0.048 (0.79)	0.014 (0.97)	0.057** (2.24)	0.062* (1.72)
Other controls			Same as Table 3		
Observations	58009	58009	58009	58009	58009
Year FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.751	0.726	0.405	0.571	0.620

Table 9: Heterogeneous Treatment Effects based on Human Capital Intensity

This table reports the double difference-in-differences tests that examine the relative effects of ENDAs on corporate innovation in different industries based on the human capital intensity. The indicator variable *High human capital intensity* takes the value of one for the firms whose proportion of knowledge workers among all workers is above the sample median, and zero otherwise. *Low human capital intensity* is (1− *High human capital intensity*). The indicator variable *Pass* takes the value of one if a state has adopted the ENDA which prohibits discrimination based on sexual orientation and gender identity in a given year, and zero otherwise. All the firm characteristics used in Table 3 are also included in this regression but unreported for brevity. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust t-statistics based on standard errors clustered by state are in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) LnPat	(2) LnCit	(3) LnCit/pat	(4) LnPat/emp	(5) LnCit/emp
Pass × High human capital intensity	0.151*** (3.28)	0.237*** (4.64)	0.065*** (5.04)	0.154*** (3.15)	0.227*** (4.70)
Pass × Low human capital intensity	-0.016 (-0.44)	-0.048 (-1.12)	-0.020 (-1.51)	0.023 (0.82)	-0.010 (-0.34)
Other controls			Same as Table 3		
Observations	58009	58009	58009	58009	58009
Year FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.746	0.723	0.404	0.570	0.620

Table 10: Inventors' Relocation

This table reports the difference-in-differences tests that examine the impacts of ENDAs on inventors' relocation. The regression specification is the same as Equation (2), except that we use the measures of inventors' relocation as the dependent variables. In column (1), the dependent variable is *Ln (1+Leaver)*, where *Leaver* refers to the number of a firm's inventors who leave for other firms in a given year. In column (2), the dependent variable is *Ln (1+New hire)*, where *New hire* refers to the number of a firm's newly hired inventors in a given year. In column (3), the dependent variable is *Ln (1+Net new hire)*, where *Net new hire* refers to the difference between the number of newly hired inventors and the number of leaving inventors. In column (4), the dependent variable is *Ln (1+ Leaver to states with ENDAs)*, where *Leaver to states with ENDAs* refers to the number of inventors who move to another firm in a different state which has already adopted the ENDA. In column (5), the dependent variable is *Ln (1+ Leaver to states without ENDAs)*, where *Leaver to states without ENDAs* refers to the number of inventors who move to another firm in a different state which has not adopted the ENDA. In column (6), the dependent variable is *Ln (1+ New hire from states with ENDAs)*, where *New hire from states with ENDAs* refers to the number of newly hired inventors who previously worked in another firm in a different state which has already adopted the ENDA. In column (7), the dependent variable is *Ln (1+ New hire from states without ENDAs)*, where *New hire from states without ENDAs* refers to the number of newly hired inventors who previously worked in another firm in a different state which has not adopted the ENDA. The indicator variable *Pass* takes the value of one if a state has adopted the ENDA which prohibits discrimination based on sexual orientation and gender identity in a given year, and zero otherwise. All the firm characteristics used in Table 3 are also included in this regression but unreported for brevity. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust t-statistics based on standard errors clustered by state are in parentheses. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) Leaver	(2) New hire	(3) Net new hire	(4) Leaver to states with ENDAs	(5) Leaver to states without ENDAs	(6) New hire from states with ENDAs	(7) New hire from states without ENDAs
Pass	0.084***	0.087***	0.065**	0.025	0.053***	0.041**	0.050**
	(3.47)	(3.10)	(2.07)	(1.48)	(2.78)	(2.41)	(2.21)
Other controls				Same as Table 3			
Observations	58009	58009	58009	58009	58009	58009	58009
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R2	0.738	0.696	0.477	0.513	0.721	0.482	0.656

Table 11: Comparison of Productivity between Leaving Inventors and Newly Hired Inventors

This table compares the person-level productivity between the leaving inventors and newly hired inventors. Leaving inventors are the inventors who move to other states within three years after the state adopts the ENDA. Newly hired inventors are the inventors who move into the state from other states within three years after the state adopts the ENDA. All continuous variables are winsorized at the 1st and 99th percentiles. The superscript ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Leaving Inventors		Newly Hired Inventors		Test of Differences	
	Mean (1)	Median (2)	Mean (3)	Median (4)	t-test (3) – (1)	Wilcoxon test (4) – (2)
Total # of patents by the inventor over our sample period	10.56	6	13.32	7	2.76***	1***
Total # of patent citations received by the inventor over our sample period	13.07	5.13	16.94	5.84	3.87**	0.71***

Figure 1: Effect of the Passage of ENDAs on Innovation

This figure shows a visual difference-in-differences examining the effects of ENDAs on patent and citation counts in adopting states, relative to non-adopting states, from 5 years prior to the laws' passage (Year 0) to 10 years afterwards.