The Real Effect of Smoking Bans: Evidence from Corporate Innovation[*]

Huasheng Gao
Nanyang Business School
Nanyang Technological University
S3-B1A-06, 50 Nanyang Avenue
Singapore 639798
65.6790.4653
hsgao@ntu.edu.sg

Po-Hsuan Hsu
Faculty of Business and Economics
University of Hong Kong
Pokfulam Road
Hong Kong
852.2859.1049
paulhsu@hku.hk

Kai Li
Sauder School of Business
University of British Columbia
2053 Main Mall, Vancouver, BC V6T 1Z2
Canada
604.822.8353
kai.li@sauder.ubc.ca

Jin Zhang
Nanyang Business School
Nanyang Technological University
S3-B2C-117, 50 Nanyang Avenue
Singapore 639798
65.9810.2937
jzhang036@e.ntu.edu.sg

This version: June, 2016

Abstract

We identify a positive causal effect of smoke-free working environments on corporate innovation, using the staggered passage of U.S. state-level smoke-free laws that prohibit smoking in the workplace. We find a significant increase in patents and patent citations for firms headquartered in states that have adopted smoke-free laws relative to firms headquartered in states without such laws. The increase is more pronounced for firms in states with weaker pre-existing tobacco controls and for firms in states with a larger number of employees who have quit smoking in response to smoking bans. Finally, we explore the underlying mechanisms using inventor-level data and uncover suggestive evidence that smoke-free laws affect innovation by attracting more productive inventors and by improving stayer inventors' productivity. We conclude that smoking bans have a real economic impact on corporate innovation.

Keywords: Innovation; Smoking Bans; Patent; Patent Citation; Inventor Productivity; Inventor Relocation

JEL Classification: G30, J28, K32, O31

[*] We are grateful for helpful comments from Neal Ashkanasy, Patrick Bolton, Hui Chen, Tim Folta, Brad Greenwood, Xu Huang, Gur Huberman, Chuan Yang Hwang, Boyan Jovanovic, Dan Li, Feng Li, Chen Lin, TC Lin, Thomas Schmid, Robert Seamans, Wes Sine, Avanidhar Subrahmanyam, Sheridan Titman, Sid Vedula, John Van Reenen, Betty Wu, Hong Yan, Joe Zou, seminar participants at Hong Kong Baptist University, Nanyang Technological University, Shanghai Advanced Institute of Finance, University of Glasgow, and University of Hong Kong, and conference participants at the Darden and Cambridge Judge Entrepreneurship and Innovation Research Conference. We thank Yen-Teik Lee for research assistance. Gao acknowledges financial support from the Singapore Ministry of Education Academic Research Fund Tier 2 (Official Number: MOE2015-T2-1-118). Li acknowledges financial support from the Social Sciences and Humanities Research Council of Canada (SSHRC Grant Number: 435-2013-0023). All errors are our own.

1. Introduction

Smoking is the world's leading preventable cause of death, killing nearly six million people every year. Sixteen million Americans suffer from smoking-related diseases, disabilities, or other serious health problems. Over the last three decades, a large number of U.S. states have adopted smoke-free laws that limit smoking in the workplace. Although these laws are shown to have reduced cigarette consumption in the workplace, their effects on the real economy have not been fully explored. In this paper, we examine the impact of such laws from the perspective of knowledge creation and identify a positive causal effect of smoke-free laws on corporate innovation.

Our tests exploit the staggered passage of smoke-free laws by various U.S. states since 1991, which either completely ban smoking in workplaces or restrict smoking to separately ventilated areas. The setting is highly appealing from an empirical analysis standpoint for two reasons. First, the motivation behind introducing smoke-free laws centers on state legislatures' determination to protect nonsmokers from exposure to secondhand smoke and to reduce cigarette consumption in the workplace. Because smoke-free laws were not introduced with the primary intention of promoting corporate innovation, potential effects on innovation are likely to be an unintended consequence of these laws.[1] Second, the staggered passage of statewide smoke-free laws enables us to identify their effects on corporate innovation in a difference-in-differences framework (see, for example, Bertrand and Mullainathan, 1999; Atanassov, 2013; Acharya, Baghai, and Subramanian, 2014). Because multiple exogenous shocks affect different firms at different points in time, we can avoid a common identification challenge faced by studies with a

[1] Even though the passage of these laws may be subject to firms' or interest groups' lobbying efforts, *a priori*, there is no perceived link between lobbying for a smoke-free working environment and corporate innovation. Further, if innovative firms had wanted to specifically promote a healthy lifestyle, they could have adopted smoke-free policies in the workplace without relying on state legislation, which would bias against us finding any significant effects of smoke-free laws on innovation.

single shock: the potential biases and noise coinciding with the shock that directly affect the dependent variable to be explained (Roberts and Whited, 2013).

We expect smoke-free laws to have a positive effect on corporate innovation through the following two channels. First, according to demographic statistics in the U.S. and around the world, smokers are usually people with poor education, low social status, and low work performance (see, for example, Levine, Gustafson, and Velenchik, 1997; Bunn et al., 2006; Pfizer, 2007; Conference Board of Canada, 2013), and they are also more likely to suffer cognitive decline than nonsmokers (Anstey et al., 2007; Llewellyn et al., 2009; Barnes et al., 2010). After a state adopts smoke-free laws, employees who are smokers tend to relocate out of the state, while employees who are nonsmokers tend to relocate into the state. Because nonsmokers are likely to be more creative than smokers given their education and socio-economic status, corporate innovation is subsequently enhanced. Second, smoking and exposure to secondhand smoke are known to lead to more frequent employee breaks, longer sick leaves, and early retirement, hampering productivity (see, for example, Halpern et al., 2001; Bunn et al., 2006). After a state adopts smoke-free laws, both smokers and their nonsmoking colleagues become healthier and more productive, leading to greater patenting output.

Using a panel data sample of 46,342 U.S. public firm-year observations over the period 1986-2008 and a difference-in-differences specification, we show that the passage of state-level smoke-free laws is associated with a significant increase in corporate innovation output. On average, firms headquartered in states that have introduced smoke-free laws experience an increase in the number of patents by 10% and an increase in the number of patent citations by 19%, relative to firms headquartered in states without such laws. The productivity of individual inventors, measured by the number of patents (citations) per 1,000 employees, also increases by

9% (15%) in firms headquartered in states that have introduced smoke-free laws. It is worth noting that using a number of alternative measures of innovation including patent/R&D, citation/R&D, patent originality, patent generality, and patent value, we continue to find a positive effect of smoke-free laws on innovation.

To make sure that our main results are not purely driven by chance, we run a placebo test where for each legislating state, we randomly pick a pseudo adoption year within the sample period, and we then estimate our baseline model based on those "pseudo" event years. We repeat this procedure for 5,000 times. The results indicate that the effects of smoke-free laws on innovation documented in our main tests are unlikely to be spurious: The coefficient estimate of the true effect from the main test lies well to the right of the empirical distribution of the coefficient estimates from the placebo test.

The identification assumption central to a causal interpretation of the difference-in-differences estimates is that the treated firms (located in states that have introduced smoke-free laws) and the control firms (located in states without such laws) share parallel trends in their innovation output prior to the law changes. Our tests show that the pre-treatment trends in corporate innovation output are indeed indistinguishable between these two groups of firms. Moreover, we show that most of the effects of smoke-free laws on innovation output occur two to three years after the laws' passage, suggesting a causal effect.

It is possible that the passage of state-level smoke-free laws is triggered by some unobservable local economic conditions, which in turn affect corporate innovation (noting that we do control for a host of observable state characteristics including GDP, population, unemployment rate, R&D expenditures, governor's party affiliation, and workforce characteristics, in the regression specification). To mitigate this concern, we exploit the fact that (unobservable) local economic conditions are likely to be similar across neighboring states, whereas the effects of state smoke-free laws stop at the state's border. The discontinuity in smoke-free laws allows us to difference away any unobservable confounding factors as long as they affect both the treated firms and their neighboring control firms. Effectively, cross-border neighboring firms' innovation output serves as the counterfactual response to the unobserved local economic conditions unrelated to the change in smoke-free laws, and this counterfactual response is then subtracted from the treated firms' innovation output in response to the change in laws. By comparing the treated firms to their neighboring control firms, we can better identify how much of the observed change in corporate innovation output is due to state-level smoke-free laws rather than other shocks to local economic conditions. After differencing away changes in local economic conditions using a sample of treated and control firms that are closely located on either side of a state's border, we continue to find a significant increase in the treated firms' innovation output relative to their neighboring control firms. These results suggest that our main findings are unlikely driven by confounding local economic conditions.

To provide further evidence that the effects of state-level smoke-free laws on innovation are indeed tied to restricting smoking in workplaces, we employ a difference-in-difference-in-differences specification to assess heterogeneous treatment effects. We first show that the treatment effects of smoke-free laws are stronger for firms in states with weaker pre-existing tobacco controls measured by state average cost per pack of cigarettes normalized by weekly income in 1990 (the year prior to the first adoption of smoke-free laws). This result suggests that the impact of smoke-free laws on innovation is likely due to restrictions on smoking in workplaces because employees in states with weaker pre-existing tobacco controls are subject to greater incremental restrictions after such laws. We further show that the treatment effects of smoke-free laws are stronger for firms in states with more employees who quit smoking in response to such laws. This result, again, suggests that the treatment effects likely result from a decline in employee smoking. These cross-sectional variations in the treatment effects help establish that the effects of smoke-free laws on innovation are indeed related to restricting smoking in workplaces.

Finally, using inventor-level data, we investigate two possible channels through which smoke-free laws affect innovation (i.e., attracting more productive inventors and improving stayer inventors' productivity). We find that following the passage of state-level smoke-free laws, legislating states experience a significant net inflow of inventors from other states (mainly from states without such laws). Importantly, we find that at the individual-inventor level, newly-arrived inventors are more productive at patenting than departing ones, which is consistent with prior findings that smokers tend to have lower productivity than nonsmokers. We next investigate the patenting activities of stayer inventors (i.e., those who have never moved during the sample period), and we find a significant increase in the number of patents and patent citations for them after the passage of state-level smoke-free laws. This result supports the view that smoke-free laws reduce smoking and exposure to secondhand smoke and thus improve the working conditions of stayer inventors, leading to improved productivity in patenting. In summary, these results help establish the mechanisms through which smoke-free laws affect corporate innovation.

Our paper adds to the growing economics and finance literature that examines the drivers of corporate innovation, which is crucial for sustainable growth and economic development (Solow, 1957; Romer, 1990). Our paper provides suggestive evidence that a healthy working environment is an important factor in knowledge creation in the real economy. Our paper also has important policy implications. Although 25 U.S. states and the District of Columbia had adopted smoke-free laws by the end of 2007, legislators in the remaining states are still debating whether to follow suit, partially because the impact of smoke-free laws on society and the real economy (in particular) is still under-explored.[2] Prior studies on the effects of smoke-free laws typically focus on medical expenses and smoking-related costs such as health and fire insurance premiums, and building maintenance and cleaning costs (see, for example, Javitz et al., 2006; Juster et al., 2007). Extending this strand of research, our paper provides new evidence that this legislation spurs employee productivity with respect to corporate innovation.

The remainder of the paper is organized as follows. Section 2 provides the background on state-level smoke-free laws. Section 3 develops our hypothesis. Section 4 describes our sample and key variable construction. Section 5 presents the effect of smoke-free laws on corporate innovation. Section 6 investigates the channels through which smoke-free laws affect innovation. We conclude in Section 7.

2. Background on State-level Smoke-free Laws

By 2013, nearly 18 out of every 100 American adults aged 18 years old or older (approximately 42 million adults) smoked cigarettes. Cigarette smoking is the leading cause of preventable disease and death in the U.S., accounting for more than 480,000 deaths every year, or one in every five deaths. More than 16 million Americans live with a smoking-related disease (U.S. Department of Health and Human Services, 2014). Smoking is harmful not only to smokers, but also to nonsmokers who are exposed to secondhand smoke. Among adults who

[2] According to Pfizer (2007), 91% of the workforce is employed at establishments that have official smoking restriction policies. Nevertheless, even in workplaces with the most stringent policy—smoking not permitted in any work area, or in any indoor public or common area—the prevalence of smoking is 16%. In establishments with less restrictive smoking policies, or none at all, the prevalence of smoking among employees increases to 24% and 30%, respectively.

have never smoked, secondhand smoke can cause various deceases, including heart problems, lung cancer, and stroke.

In the past, individuals who smoked were free to do so when and where they pleased, and smoking in workplaces was generally unrestricted. Over time, the interests of smokers and nonsmokers started to clash in workplaces. While some employees who smoke adamantly seek to preserve their right to smoke while at work, other nonsmoking employees are pushing for their right to work in a smoke-free environment. Over the last two decades, U.S. state governments have increasingly banned smoking in workplaces as a means of limiting nonsmokers' exposure to secondhand smoke and to discourage smoking. The 2006 report by the U.S. Surgeon General concludes that these smoke-free policies have decreased the number of cigarettes smoked per day, increased the number of attempts to quit smoking, and increased smoking cessation rates (U.S. Department of Health and Human Services, 2006).

Jacobson, Wasserman, and Raube (1993) identify a number of factors that have significantly influenced state-level smoking-control legislation. The first is the presence of key legislators committed to enacting smoking-control legislation. The second factor is the formation of a strong and inclusive anti-smoking coalition (such as the American Lung Association) engaged in an aggressive grassroots and media campaign to elicit public support for smoking restrictions. The third factor is the presence of an active executive branch (such as the State Department of Health) that places additional political pressure on the legislature to act, especially when the executive branch makes such legislation a policy priority. The fourth factor is the enactment of strong local ordinances created by a policy environment that facilitates the enactment of statewide smoking restrictions. The last factor is the absence of tobacco industry opposition. None of the above factors is directly related to firm-specific conditions that include innovative activities. Instead, the primary purpose of smoking bans is to promote public health and reduce cigarette consumption. We therefore conclude that the staggered passage of state smoke-free laws is not triggered by factors that drive corporate innovation.

Although the U.S. does not have any federal legislation that prohibits smoking in workplaces, U.S. states have started to enact laws that either completely ban smoking in workplaces or restrict smoking to separately ventilated areas. In 1991, New Hampshire became the first state to enact such laws. By the end of 2007, 25 states and the District of Columbia had followed suit. Table 1 lists the states and their years of introducing smoke-free laws provided by the Centers for Disease Control and Prevention (CDC).

The scope of smoking-control restrictions has evolved over time. Typically, a state first passes smoke-free laws that only apply to some specific areas, and then expands to other places. For example, Utah passed laws to restrict smoking in restaurants in 1995, expanded the restrictions to private workplaces in 2006, and then expanded them further to include taverns and private clubs in 2009. Because of our focus on laws that restrict smoking in workplaces, we identify 2006 as the year that Utah's smoke-free laws became effective.

The strength of smoking-control restrictions has also evolved over time. The CDC database categorizes workplace smoke-free laws into three categories: "banned", "separately ventilated areas", and "designated areas". Following the CDC's suggestion, we deem workplace smoke-free laws to be effective if they completely ban smoking or only allow smoking in separately ventilated areas. If the state laws allow smoking in designated areas but do not require those areas to be separately ventilated, then they are ineffective, as under such laws, nonsmokers may still be harmed by working adjacent to those areas. A state may pass weak laws first, and then strengthen them incrementally. For example, the 1984 Wisconsin Clean Indoor Air Act permitted smoking in workplaces where the main occupants are smokers or in designated smoking areas; the 2010 Amendment of Wisconsin's Clean Indoor Air Act completely prohibited smoking in workplaces. In this case, we identify 2010 as the year that Wisconsin's smoke-free laws became effective (outside our sample period).

In summary, the effective year of state-level smoke-free laws reported in Table 1 refers to the year when a state for the first time completely banned smoking in workplaces or restricted smoking to separately ventilated areas.

3. Hypothesis Development

We hypothesize that smoke-free laws affect corporate innovation through the following two channels. The first is that smoke-free laws trigger workforce movements by attracting more productive inventors. Two groups of employees are affected by smoke-free laws: those who smoke and those who do not. Smokers derive (short-term) utility from consuming cigarettes, while nonsmokers suffer from exposure to secondhand smoke. Smoke-free laws make smokers worse off by restricting them from smoking at work, and make nonsmokers better off by providing them with a smoke-free working environment. Thus, following a state's adoption of smoke-free laws, we expect that nonsmoking employees will be more likely to relocate into the state, while those employees who smoke will be more likely to relocate out of the state to a place without such laws (or will quit smoking, which will be discussed in the second channel). Supporting this argument, prior studies find that employees who smoke are more likely to switch jobs in response to a smoking ban in workplaces and employees with better health habits (such as not smoking) are more likely to join firms with a smoking ban (Evans, Farrelly, and Montgomery, 1999; Dasley and Park, 2009).

According to the CDC, better-educated people and high-income people are less likely to be smokers.[3] For example, about five out of every 100 adults with a graduate degree are smokers; eight out of every 100 adults with a college degree are smokers. In contrast, about 43 out of 100 adults with a high school equivalency certificate are smokers. In terms of income level, about a quarter of the population below the poverty level are smokers, while only 15% of the population above the poverty level are smokers. Prior studies find that education and income are positively associated with creativity and innovation (Sirgy, 1986; Lucas, 1988). We therefore expect the nonsmoking group to be, on average, more innovative than the group that smokes.

Taken together, the above discussions suggest that one of the channels through which smoke-free laws affect innovation is the relocation of nonsmoking employees who are likely to be more creative and productive into the legislating state and the relocation of smoking employees who are likely to be less creative and productive out of the legislating state.

The second channel is that smoke-free laws help enhance the productivity of stayer inventors. Smoking is known to lead to significant productivity losses because of smokers' frequent breaks, longer sick leaves, and early retirement for smoking-related diseases; and smokers' negative environmental impact due to secondhand smoke on nonsmoking employees (see, for example, Halpern, et al., 2001; Bunn et al., 2006; Weng, Ali, and Leonardi-Bee, 2013). The CDC estimates that the productivity loss resulting from smoking-related health problems was around $92 billion over the period of 1997-2001.[4] After a state adopts smoke-free laws, both employees who smoke and their nonsmoking colleagues become healthier and more productive (Brigham et al., 1994; Alamar and Glantz, 2004; Pickett et al., 2006), leading to more patenting output.

---

[3] http://www.cdc.gov/tobacco/data_statistics/fact_sheets/adult_data/cig_smoking/index.htm#national
[4] http://www.cdc.gov/media/pressrel/r050630.htm

In summary, we predict a positive effect from smoke-free laws on corporate innovation. Moreover, we propose that smoke-free laws affect innovations through the channels of attracting more nonsmoking employees who are likely to be more creative and productive and enhancing existing employees' productivity.

4. Sample Formation and Variable Construction

We start with all U.S. public firms in the Compustat dataset with a book value of total assets exceeding $5 million and with number of employees exceeding five to focus on economically significant firms that are likely to be innovative.[5] We also exclude firms in financial (SIC codes 6000-6999) and utility (SIC codes 4900-4999) industries due to their different regulatory oversight that might have implications on innovation output.

We then obtain information on a firm's headquarters from Compustat and Compact Disclosure (which records changes of a headquarters location), and manually check for any missing information. Following prior literature (see, for example, Pirinsky and Wang, 2006; Hilary and Hui, 2009; Korniotis and Kumar, 2013), we define a firm's location as the location of its headquarters. As noted by Pirinsky and Wang (2006), this approach seems "reasonable given that corporate headquarters are close to corporate core business activities."

Finally, we obtain patent and citation information from the patent database of Kogan et al. (2015), which links each patent and its citations to a Compustat public firm (if the assignee is a public firm) and covers all patents awarded by the U.S. Patent and Trademark Office (USPTO) over the period 1976-2010. The database allows us to better identify the real impact of state-level

[5] Smoke-free laws in several states do not apply to small firms. For example, the smoke-free law in California only applies to firms with more than five employees.

smoke-free laws on corporate innovation, as a large number of states passed such laws in the 2000s (see Table 1).[6]

Following prior work (see, for example, Aghion, Van Reenen, and Zingales, 2013; Bloom, Schankerman, and Van Reenen, 2013), we drop firms that have never applied for a single patent during our entire sample period. We start our sample in 1986, five years prior to the first adoption of state-level smoke-free laws. 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 economics literature (Hall, Jaffe, and Trajtenberg, 2005). Given the typical two- to three-year lag between patent application and approval (Hall et al., 2005), and the Kogan et al. database's coverage ending in 2010, patents applied for in 2009 and 2010 may not be awarded and show up in the database. For this reason, we end our sample of patents applied for in 2008. Our final panel data sample consists of 46,342 firm-year observations over the period 1986-2008.

To assess the performance of corporate innovation, we employ four measures based on patent counts and patent citations.[7] The first is the number of patents applied for (and subsequently awarded) by a firm in a given year. The second is the sum of citation counts received by patents applied for by a firm in a given year, which captures the significance of its patent output. Because citations can be received many years after a patent is awarded, patents awarded near the end of the sample period have less time to accumulate citations. To address this truncation bias, we follow Hall et al. (2005) to adjust for the duration of patent citations by

[6] In contrast, the commonly used NBER Patent Database of Hall, Jaffe, and Trajtenberg (2005) ends its coverage in 2006.

[7] Economists have used firm-level patent records as indicators of corporate innovation performance since Scherer (1965). Although there are limitations in using patent data to measure inventions (Lerner and Seru, 2015), Griliches notes (1990, p. 1702), "*Nothing else even comes close in the quantity of available data, accessibility, and the potential industrial, organizational, and technological detail.*"

technology classes.[8] Given our interest in determining whether smoke-free workplaces affect employee productivity in innovative projects, our last two measures are the number of patents applied for (and subsequently awarded) and the number of citations per 1,000 employees (Acharya et al., 2014). Due to the positive skewness in patent data, we take the natural logarithm of one plus the value of each innovation measure (Lerner, 1994; Aghion et al., 2013).

We control for a number of firm characteristics that may affect corporate innovation including firm size, cash holdings, R&D expenditures, ROA, asset tangibility, leverage, capital expenditures, Tobin's Q, industry concentration (the Herfindahl index based on sales), and firm age. Following Aghion et al. (2005), we also include the squared Herfindahl index in our regressions to account for any possible non-linear effects of product market competition on innovation output.

We also control for a number of state-level variables in our regressions. Since larger and richer states may have more innovative projects, we control for state GDP and population. We include state unemployment rate to control for local business conditions. Further, we control for state expenditures in R&D, political climate (whether or not the state is governed by a Democrat), and workforce characteristics including the percent of college graduates and the percent of smokers, because these variables are likely to be correlated with innovation output and/or the propensity of a state passing smoke-free laws. Data on state GDP is obtained from the Bureau of Economic Analysis, data on population is obtained from the U.S. Census Bureau, data on unemployment rate is from the U.S. Bureau of Labor Statistics Local Area Unemployment Statistics Series, data on state R&D expenditures is from the Division of Science Resources Statistics of the National Science Foundation, data on state governors' party affiliations is via

[8] We obtain similar results when scaling the citation count of each patent by the average citation count of patents that are in the same technology class and were applied for in the same year (Seru, 2014).

web search, and data on college graduates and smokers in the workforce is from the Behavior Risk Factor Surveillance System (BRFSS). To minimize the effect of outliers, we winsorize all continuous variables at the 1st and 99th percentiles. Detailed variable definitions are provided in the Appendix.

Table 2 provides summary statistics. On average, firms in our sample have 11.5 patents applied for (and subsequently awarded) per year and receive 217 citations. After normalizing the number of patents and patent citations by the number of employees, we find that on average, firms in our sample generate 7 patents and 181 citations per 1,000 employees.

The average sample firm hires about 7,480 employees and is 19 years old. The average sample firm holds a sizeable amount of cash, with a cash-to-assets ratio of 22.2%. The sample average R&D and capital expenditures are 8.2% and 5.4% of total assets, respectively. The average sample firm is moderately levered, with a leverage ratio of 20.0%, and its tangible assets (i.e., property, plant, and equipment) account for 23.6% of total assets. In terms of performance, the sample average ROA is 4.6% and the sample average Tobin's Q is 2.2.

5. Results

5.1. Visual Illustration

Figure 1 depicts the impact of smoke-free laws on corporate innovation in states that have introduced those laws relative to states without such laws following the approach in Autor, Donohue, and Schwab (2006) and Acharya et al. (2014). The y-axis shows the logarithm of the number of patents (citations received by patents) applied for in a given year; the x-axis shows the year relative to the passage of smoke-free laws, ranging from five years prior to the passage (year 0) to ten years afterwards.

Figure 1 plots point estimates of the coefficients $\beta_n's$ from running the following regression:

$$Innovation_{ist} = \alpha + \sum_{n=-5}^{10} \beta_n * Eventyear_{st}^{n} + \ Year\ FEs + \varepsilon_{ist}\ , \qquad (1)$$

where $Innovation_{ist}$ is the logarithm of one plus the number of patents (citations received by patents) applied for in year *t* by firm *i* in state *s*. $Eventyear_{st}^{n}$ is an indicator variable flagging the *n*th year relative to the passage of smoke-free laws in state *s* and year *t*. For example, $Eventyear_{st}^{1}$ takes the value of one in the first year after the passage of smoke-free laws in state *s* and year *t*, and zero otherwise. The coefficients β_n's capture the aggregate level of corporate innovation output in legislating states relative to non-legislating states over time (i.e., from five years before to ten years after the passage of smoke-free laws).

The two lines in Figure 1 correspond to the number of patents and the number of citations, respectively, and they show the same pattern. Corporate innovation output increases significantly after the passage of state-level smoke-free laws. For example, in terms of patent counts, in the year when such laws are passed, the coefficient β_0 is approximately 0.028, while in the fifth year after the passage of such laws, the corresponding coefficient β_5 is about eight times as large (0.22). In terms of citation counts, the coefficient β_0 is approximately 0.206, while the corresponding coefficient β_5 is almost three times as large (0.69). Moreover, we show that the greatest increase in innovation output occurs three years after the passage of smoke-free laws, consistent with the notion that corporate innovation practices take time to change, suggesting a persistent long-run effect of such laws.

5.2. Baseline Regression

A large number of U.S. states have adopted smoke-free laws at different points in time during the sample period. Thus, we can examine the before versus after effect of the passage of such laws on corporate innovation in affected states (the treated firms) vis-à-vis the before versus after effect in states without such laws (the control firms). This is a difference-in-differences test design involving multiple groups of the treated firms and multiple periods of the before versus after comparison as employed by Bertrand, Duflo, and Mullainathan (2004), Imbens and Wooldridge (2009), and Acharya et al. (2014). We implement the test by running the following regression:

$$Innovation_{ist} = \alpha + \beta_1 Smokefree_{st} + \beta_2 Other\ Firm\ Characteristics_{ist} + \beta_3 State\ Characteristics_{st} + Firm\ FEs + Region \times Year\ FEs + \varepsilon_{ist}, \quad (2)$$

where $Innovation_{ist}$ is the logarithm of one plus the number of patents (citations received by patents) applied for in year *t* by firm *i* in state *s*, and is scaled by the number of employees (in 1,000s) for the third and fourth innovation measures. $Smokefree_{st}$ is an indicator variable that takes the value of one if smoke-free laws are adopted in state *s* and year *t*, and zero otherwise. That is, for a state that has adopted such laws, the variable *Smokefree* takes the value of one for the period after the adoption (beginning from year *t+1*), and zero for the period leading up to the adoption. For states without such laws during our sample period, the variable *Smokefree* always takes the value of zero. We include a set of control variables that may affect a firm's innovation output, as discussed in Section 4. We also include firm fixed effects to control for time-invariant differences in patenting and citation practices across firms. Finally, we include interaction terms between regional and year indicator variables to control for time-varying differences between geographic regions of the U.S. in corporate innovation and in the passage of smoke-free laws.[9] Controlling for regional time trends helps alleviate potential endogeneity concerns about the passage of smoke-free laws, considering that states in the South and Midwest lagged behind states in the Northeast and West in passing these laws (see Table 1) and that these regions might have different innovation propensities. Given that our treatment is defined at the state level, we cluster standard errors by state.

The coefficient of interest in Equation (2) is the coefficient β_1. As explained by Imbens and Wooldridge (2009), after controlling for all fixed effects, β_1 is the estimate of *within-state* difference between the periods before and after the passage of smoke-free laws relative to a similar difference between those periods before and after in states without such laws.

It is helpful to consider an example. Suppose we want to estimate the effect of smoke-free laws adopted by Oregon in 2002 on innovation output. We can subtract the number of patents (citations) before the passage of such laws from the number of patents (citations) after the passage for firms headquartered in Oregon. However, economy-wide shocks may occur that same year and affect corporate innovation. To difference away such factors, we calculate the same difference in innovation output for firms in a control state without such laws. Finally, we calculate the difference between these two differences, which represents the incremental effect of the passage of smoke-free laws on the corporate innovation outcome of firms in Oregon compared to that of firms in states without such laws.

Table 3 presents the regression results. The coefficient estimates of the effect of the passage of smoke-free laws on corporate innovation are positive and statistically significant in all columns. In column (1) where the dependent variable is Ln (1 + *Patent*), we show that the

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

coefficient estimate on the indicator *Smokefree* is 0.094 and significant at the 1% level, suggesting a positive effect of smoke-free laws on corporate innovation. The economic magnitude of the impact of such laws is also sizeable: The passage of such laws leads to an increase in the number of patents by approximately 10% ($= e^{0.094} - 1$), when compared to firms located in states without such laws.

In column (2) where the dependent variable is Ln (1 + *Citation*), we show that the coefficient on the indicator *Smokefree* is 0.170 and is significant at the 1% level. In terms of economic significance, the passage of smoke-free laws leads to an increase in the number of patent citations by approximately 19% ($= e^{0.170} - 1$).

In columns (3) and (4) where the dependent variables are the number of patents and the number of citations scaled by the number of employees (in 1,000s), we show that the coefficients on the indicator *Smokefree* are 0.088 and 0.141, respectively, and both are significant at the 5% level. These results imply that the number of patents and the number of citations per 1,000 employees increase by approximately 9% and 15%, respectively, in states that have passed smoke-free laws as compared to states without such laws. Our results suggest that employee productivity in innovation increases significantly after the passage of smoke-free laws.

In terms of other control variables in Equation (2), we find that firm size, cash holdings, R&D expenditures, Tobin's Q, and firm age are positively and significantly associated with innovation output, while leverage is negatively and significantly associated with innovation output. These results are broadly consistent with prior findings (see, for example, Aghion et al., 2005). We do not find any consistent association between state-level controls and firm innovation output, possibly because we have controlled for firm fixed effects and region × year fixed effects in the regression. Taken together, the results from Table 3 suggest a strong positive impact of smoke-free laws on innovation output and productivity.[10]

5.3. Placebo Tests

To make sure that our main results are not purely driven by chance, we run a placebo test where for each state that ever adopted the smoke-free laws, we "assign" a pseudo adoption year that is randomly chosen from our sample period 1986-2008, and that is at least either five years before or five years after the actual adoption year so that the "pseudo" adoption year is not confounded with the actual adoption year. We then estimate the baseline regressions in columns (1) and (2) of Table 3 based on those pseudo adoption years and save the coefficient estimates on *Smokefree*. We repeat this procedure for 5,000 times.

Figure 2 plots the histogram of the coefficient estimates on the indicator *Smokefree* based on those pseudo events. Panel A presents the distribution of the coefficient estimates when the dependent variable is Ln (1 + *Patent*). We find that the coefficient estimate of the true effect based on column (1) of Table 3 lies well to the right of the distribution of coefficients estimates from the placebo test. The actual coefficient estimate on *Smokefree* (0.094) is almost three standard deviations (0.032) above the mean (-0.006) of the distribution and is almost as large as the maximum coefficient estimate from the placebo test (0.104). Panel B presents the distribution of the coefficient estimates when the dependent variable is Ln (1 + *Citation*). We find a similar pattern to Panel A: The coefficient estimate of the true effect based on column (2) of Table 3 lies

---

[10] In untabulated analyses, we first examine whether our results are driven by the state of California. We repeat the analysis in Table 3 by excluding California and our inference remains unchanged. For example, after removing firms headquartered in California, the coefficients on *Smokefree* are 0.093 (significant at the 5% level) and 0.162 (significant at the 5% level) when the dependent variables are Ln (1 + *Patent*) and Ln (1 + *Citation*), respectively. We second examine whether our results are driven by the cluster of states passing the smoke-free laws in the period 2005-2007. We repeat the analysis in Table 3 by limiting to the sample period 1986-2004 and our inference remains unchanged. For example, the coefficients on *Smokefree* are 0.104 (significant at the 1% level) and 0.183 (significant at the 5% level) when the dependent variables are Ln (1 + *Patent*) and Ln (1 + *Citation*), respectively.

well to the right of the distribution of coefficient estimates from the placebo test. These results suggest that it is the adoption of smoke-free laws that is behind our main findings.

5.4. The Pre-treatment Trends

The validity of difference-in-differences tests depends on the parallel trends assumption: Without smoke-free laws, the treated firms' innovation output would have evolved in the same way as that of the control firms. To examine pre-treatment trends in innovation output of the treated firms and their control firms, we introduce seven indicator variables, *Year −3*, *Year −2*, *Year −1*, *Year 0* (the year in which such laws are passed), *Year 1*, *Year 2*, and *Year* 3^{+}, to flag the year relative to the passage year. For example, *Year −2* indicates that it is two years before the laws' passage; and *Year* 3^{+} indicates that it is three or more years after the laws' passage. We then re-estimate Equation (2) by replacing the indicator *Smokefree* with the seven indicators as defined above. The coefficients of interest are those on the indicators *Year −3*, *Year −2*, and *Year −1* because their magnitude and significance indicate whether there are parallel trends in innovation output between the treated firms and their control firms prior to the treatment. Table 4 presents the results.

We show that across all four columns, the coefficients on all three indicators are close to zero and not statistically significant, suggesting that the parallel trends assumption of the difference-in-differences tests is likely met.

We further show that across all four columns, the coefficients on the indicators *Year 0* and *Year 1* are small in magnitude and not statistically significant (except that the coefficient on *Year 1* is significant at the 10% level in column (2)). The effects of smoke-free laws show up two years after the laws' passage: The coefficients on the indicator *Year 2* are positive and significant for all four innovation measures, and the coefficients on the indicator *Year* 3^{+} are many times larger than the coefficients on the indicator *Year 0* for all four innovation measures, indicating that it takes several years for smoke-free laws to affect corporate innovation, consistent with the notion that innovation is a long-term process. This finding is also consistent with the pattern illustrated in Figure 1.

In summary, Table 4 shows that the treated firms and their control firms share a similar time trend in innovation output prior to the passage of smoke-free laws, thus supporting the parallel trends assumption necessary for the difference-in-differences tests. Moreover, it also shows that most of the effects of smoke-free laws on innovation occur several years after the passage of such laws, suggesting a causal interpretation.

5.5. Unobservable Confounding Local Economic Conditions

Although we have controlled for *observable* local economic conditions in the regression specification of Equation (2), some *unobservable* local economic conditions may be associated with both the passage of smoke-free laws and corporate innovation. In this subsection, we difference away unobservable local economic conditions by focusing on treated firms that are on one side of a state's border and their neighboring control firms on the other side of the same state's border.

To do so, we exploit the discontinuity in smoke-free laws across the state's border and examine the change in innovation output of the treated firms on one side of the state's border with such laws in effect relative to their neighboring control firms on the other side of the state's border without such laws. The logic for this analysis is as follows. Suppose that smoke-free laws are driven by unobservable changes in local economic conditions, and that it is those changes, rather than smoke-free laws, that spur corporate innovation. Then both the treated firms in states with smoke-free laws and their neighboring control firms in states just across the state's border without such laws would spuriously appear to react to the laws' changes, because local economic conditions, unlike the state-level laws, have a tendency to spread across the state's border (Heider and Ljungqvist, 2015). The change in innovation output of the treated firms should be no different from that of their neighboring control firms.

To examine this possibility, we match each treated firm to a control firm in the same industry (based on the two-digit SIC code), in an adjacent state without smoke-free laws, and closest in total assets in the year of the law adoption. Obviously, a treated firm may not necessarily share the same local economic conditions with its control firm in an adjacent state 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 control firm be within 100 miles.[11] If the distance is more than 100 miles, we drop the pair from our sample, resulting in a sample of 5,274 firm-year observations. By doing so, we increase our confidence that the treated firm and its control firm are truly close to each other geographically and thus face similar local economic shocks.[12] We then re-estimate Equation (2) by using this sample of adjacent firms sharing a common state border. Table 5 presents the results.

We find that by focusing on cross-state border neighboring firms to control for unobservable local economic conditions, the coefficients on the indicator *Smokefree* are positive and significant across all four columns. Under the identifying assumption that neighboring firms are exposed to similar local economic conditions and hence the change in innovation output of the treated firms should be no different from that of their neighboring control firms, our findings suggest that any unobservable confounding local economic conditions cannot be driving the

[11] As robustness checks, we require the distance between the treated firm and its control firm to be within 60, 80, or 120 miles, and our inferences remain unchanged.
[12] The average distance between the treated and control firm is 75 miles, indicating that they are indeed geographically close.

observed impact of smoke-free laws on corporate innovation. It is also worth noting that the coefficients on the indicator *Smokefree* are larger in magnitude than those in the baseline model in Table 3. For example, in column (1) of Table 5, where the dependent variable is Ln (1 + *Patent*), the coefficient on *Smokefree* is 0.143 (significant at the 5% level), which is more than 1.5 times as large as that in column (1) of Table 3 (0.094). Taking column (3) of Table 5 for another example, where the dependent variable is Ln (1 + *Patent per employee*), the coefficient on *Smokefree* is 0.172 (significant at the 1% level), which is about two times as large as that in column (3) of Table 3 (0.088). This result is consistent with our proposed channel of workforce movements triggered by smoke-free laws, considering that employees near state borders are easier to relocate across states and thus treatment effects are greater for firms in these areas, as well as with our identifying assumption that when we difference away unobservable local economic conditions, the standalone treatment effects of smoke-free laws on innovation output become stronger.

5.6. Heterogeneous Treatment Effects

To provide further evidence that the effects of smoke-free laws on innovation are indeed due to (the absence of) smoking in workplaces, in this subsection we implement difference-in-difference-in-differences tests to examine heterogeneous treatment effects. Evidence of heterogeneous treatment effects helps 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 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. We explore two possible sources of heterogeneity in the treatment effect.

First, if the impact of smoke-free laws on innovation output is truly due to restrictions on smoking, we expect the treatment effect to be larger for states with weaker pre-existing tobacco controls. Following Bertrand and Mullainathan (1999) and Atanassov (2013), we use a sticky measure to capture the "pre-existing" levels of tobacco control, i.e., the state average cost per pack of cigarettes normalized by state weekly wages in 1990 (the year prior to the first adoption of smoke-free laws), and we keep those levels constant for the remaining years during our sample period. By doing so, we avoid using future levels of state cigarette costs as the conditioning variable that may be endogenous to the passage of smoke-free laws. The variation in state average cost per pack of cigarettes is largely due to the difference in state and local taxes on cigarettes. High costs per pack of cigarettes suggest stronger tobacco controls. We obtain information on state cigarette costs from the CDC[13] and weekly wages from the Quarterly Census of Employment and Wages Survey provided by the U.S. Bureau of Labor Statistics. The *High (Low) cigarette cost* is an indicator variable that takes the value of one if a state's cost per pack of cigarettes normalized by weekly wages in 1990 are above (below) the sample median, and zero otherwise. We then re-estimate Equation (2) by replacing the indicator *Smokefree* with two interaction terms *Smokefree* × *High cigarette cost* and *Smokefree* × *Low cigarette cost*. Table 6 Panel A presents the results.

We show that across all four columns, the coefficients on *Smokefree* × *Low cigarette cost* are positive and significant, while the coefficients on *Smokefree* × *High cigarette cost* are much smaller in magnitude and not statistically significant. Take column (1) for example, where the dependent variable is the number of patents, we show that the coefficient on *Smokefree* × *Low*

[13] https://chronicdata.cdc.gov/Policy/The-Tax-Burden-on-Tobacco-Volume-49-1970-2014/7nwe-3aj9

cigarette cost is 0.117 and significant at the 1% level, while the coefficient on *Smokefree* × *High cigarette cost* is only 0.015 and is not significantly different from zero. This result indicates that the treatment effect is significant for firms in states with weaker pre-existing tobacco controls, and is virtually absent for firms in states with stronger pre-existing tobacco controls.

Second, if the improved innovation output after the passage of smoke-free laws is due to reduced cigarette consumption in the workplace, we expect this treatment effect to be larger for states with a larger number of employees who have quit smoking in response to such laws, for the following reason. If smoke-free laws influence innovation through improving employee health and productivity, then such a relation should be more pronounced when more employees quit smoking after the passage of such laws. We obtain information about the number of employees who quit smoking in a given state and a given year from BRFSS, which conducts health-related telephone surveys of U.S. residents across states.[14] *More (Few) quit smoking* is an indicator variable that takes the value of one if a state's number of employees who quit smoking normalized by the state's total number of employees is above (below) the sample median, and zero otherwise. We then re-estimate Equation (2) by replacing the indicator *Smokefree* with two interaction terms *Smokefree* × *More quit smoking* and *Smokefree* × *Few quit smoking*. Table 6 Panel B presents the results.

We show that across all four columns, the coefficients on *Smokefree* × *More quit smoking* are positive and significant at the 5% or lower level, while the coefficients on *Smokefree* × *Few quit smoking* are much weaker in terms of both economical and statistical significance. Take column (3) for example, where the dependent variable is the number of patent citations per employee, we show that the coefficient on *Smokefree* × *More quit smoking* is 0.130 and significant at the 1% level, while the coefficient on *Smokefree* × *Few quit smoking* is only 0.031

[14] http://www.cdc.gov/brfss/about/index.htm

and is not significantly different from zero. This result indicates that the treatment effect is significant for firms in states with a large number of employees who have quit smoking, and is virtually absent for firms in states with a small number of employees who have quit smoking.

Taken together, the effects of smoke-free laws on corporate innovation output are stronger for firms in states with weaker pre-existing tobacco controls and for firms in states with a larger number of employees who subsequently quit smoking. These results suggest that the impact of smoke-free laws on innovation is indeed tied to restricting smoking in workplaces.

5.7. Alternative Measures of Innovation

As a robustness check, we employ various alternative measures to examine the effect of smoke-free laws on corporate innovation. Table 7 presents the results. Column (1) shows that when the dependent variable is a firm's R&D expenditures normalized by total assets, the coefficient on *Smokefree* is close to zero and not statistically significant, indicating that firms' R&D expenditures do not increase following the adoption of smoke-free laws. This result suggests that the increased patenting output as shown in Table 3 is likely due to the increase in employee productivity rather than more R&D input. This result also helps rule out an alternative explanation for our main findings that firms spend more in R&D from their savings on healthcare-related expenses.

Columns (2) and (3) show that when the dependent variables are *Patent/R&D* and *Citation/R&D*, respectively, the coefficients on *Smokefree* are positive and significant, indicating that smoke-free laws have a positive effect on employee productivity in innovation (Hirshleifer, Hsu, and Li, 2013). Measuring patent quality using *Originality*, *Generality*, and *Patent value* (Trajtenberg, Henderson, and Jaffe, 1997; Kogan et al., 2015), columns (4)-(6) show that the coefficients on *Smokefree* are positive and significant at or below the 5% level.

Overall, Table 7 shows that the positive effect of smoke-free laws on innovation is robust to various alternative innovation measures and that this positive effect is more likely due to an increase in employee productivity than an increase in R&D expenditures.

6. Channels for Smoke-free Laws to Affect Innovation

6.1. Evidence from Inventor Relocation

In this section, we provide suggestive evidence that a possible channel for smoke-free laws to affect innovation is by attracting more productive inventors. We obtain information on individual inventors from the U.S. Patent Inventor Database (Li et al., 2014). For each patent, the inventor database has the identity and residential address of the inventor(s) (i.e., the individual(s) who creates (create) the patent) and the assignee (i.e., the public firm that owns the patent).

We implement the difference-in-differences tests examining the impact of smoke-free laws on inventor relocation by running the following regression:

$$InventorFlow_{st} = \alpha + \beta_1 Smokefree_{st} + \beta_2 State\ Characteristics_{st} + State\ FEs + Region \times Year\ FEs + \varepsilon_{ist}\ , \quad (3)$$

where $InventorFlow_{st}$ is the logarithm of one plus the number of inventors coming in (moving out) for state *s* in year *t*. Table 8 Panel A presents the results.

In column (1), the dependent variable is Ln (1 + *Inflow from states without smoke-free laws*), capturing the number of newly-arrived inventors who previously worked in a state without smoke-free laws. We show that the coefficient on the indicator *Smokefree* is positive and significant at the 1% level, suggesting that inventors are more likely to move from states without smoke-free laws to states with such laws. In column (2), the dependent variable is Ln (1 + *Outflow to states without smoke-free laws*), capturing the number of inventors who relocate into a state without such laws. We show that the coefficient on the indicator *Smokefree* is positive and significant at the 5% level, suggesting that inventors are also more likely to move out of states after the passage of smoke-free laws into states without such laws.

To capture the net effect of the passage of smoke-free laws on inventor relocation from states without such laws, we define *Net inflow from states without smoke-free laws* = *Inflow from states without smoke-free laws* – *Outflow to states without smoke-free laws*, and column (3) presents the results.[15] We find a significantly positive coefficient on the indicator *Smokefree,* suggesting that the number of newly arrived inventors from states without smoke-free laws significantly exceeds the number of inventors who relocate into states without such laws. This finding is not surprising, considering that about 80% of the U.S. population are nonsmokers and thus there are more nonsmoking inventors likely to relocate to benefit from smoke-free laws.

As a placebo test, we examine the effect of the passage of smoke-free laws on inventor relocation from states with such laws. In column (4), the dependent variable is Ln (1 + *Inflow from states with smoke-free laws*), and we show that the coefficient on the indicator *Smokefree* is not significantly different from zero. In column (5), the dependent variable is Ln (1 + *Outflow to states with smoke-free laws*), and we show that the coefficient on the indicator *Smokefree* is not significantly different from zero. To capture the net effect, we define *Net inflow from states with smoke-free laws* = *Inflow from states with smoke-free laws* – *Outflow to states with smoke-free laws*, and column (6) presents the results. We show that the coefficient on the indicator *Smokefree* is not significantly different from zero, suggesting that among states with smoke-free laws, a similar number of inventors arrive and depart.

[15] If the value of *Net inflow from states without smoke-free laws* is negative, the dependent variable is set as –Ln (1 + the absolute value of *Net inflow from states without smoke-free laws*). For example, when the value of *Net inflow from states without smoke-free laws* is –5, the dependent variable is –Ln (6).

In summary, Table 8 Panel A shows that the passage of smoke-free laws triggers workforce movements: The legislating states experience a greater inflow of inventors from other states (mainly from states without such laws), while simultaneously experiencing a greater outflow of inventors to other states (mainly to states without such laws).[16]

Next, we examine the productivity of departed and newly-arrived inventors. Departed inventors are those who moved to non-legislating states within three years after their home state adopted smoke-free laws, and newly-arrived inventors are those who relocated from non-legislating states within three years after their destination state adopted smoke-free laws. For each inventor, we track her patents applied for (and eventually awarded), and the number of patent citations received by those patents over our sample period. Table 8 Panel B presents the results.

We show that at the median, departed inventors have 7 patents during our sample period, while newly-arrived inventors have 6 patents (or 14% fewer). The difference is significant at the 1% level. In terms of the number of citations, the median departed inventor receives 69.63 citations, while the median newly-arrived inventor receives a significantly larger number of citations (78.32, or 13% more). We obtain similar findings when using the mean values. These results indicate that the productivity of newly-arrived inventors is significantly greater than that of departed inventors, consistent with the observed increase in corporate innovation following the passage of smoke-free laws.

[16] This result could also be driven by the relocation of firms (instead of only some of their employees) to states that have adopted smoke-free laws. However, we find very few cases of firm relocation: Only 46 firms relocated after their home states adopted smoke-free laws, and on average the relocation occurred six years after the laws' passage. We thus conclude that our finding is primarily driven by inventor relocation.

Overall, Table 8 provides supporting evidence that one of the mechanisms through which smoke-free laws affect innovation is the relocation of more productive inventors into states with such laws and the relocation of less productive inventors out of those states.

6.2. Evidence from Stayer Inventors' Productivity

In this subsection, we provide suggestive evidence that a second possible channel for smoke-free laws to affect innovation is to improve stayer inventors' health and thus their productivity. Prior studies have shown that smoke-free laws significantly reduce employees' exposure to secondhand smoke, improve their working environment, cut the productivity loss associated with smoking-related diseases, and thus enhance employees' productivity (Sargent, Shepard, and Glantz, 2004; Bartecchi et al., 2006; WHO, 2007).

We re-estimate Equation (2) by limiting to a sample of inventors who never moved during the sample period, i.e., stayer inventors. Table 9 presents the results. We find that across all four columns, the coefficients on the indicator *Smokefree* are positive and significant at the 5% or lower level. The results seem to suggest that the innovative productivity of inventors who did not relocate improved after the passage of smoke-free laws. However, due to a lack of data on individual inventors' habits, we are unable to pin down whether the productivity change is mainly driven by smoker or nonsmoker inventors, which can be an interesting question for future research.

Taken together, Tables 8 and 9 provide evidence supporting that the two possible channels for smoke-free laws to affect innovation are attracting more productive inventors and enhancing stayer inventors' productivity.

6.3. Further Discussions

Thus far, we have provided evidence on the causal effect of smoke-free laws on corporate innovation. In addition to state-level smoke-free laws, nonsmoking employees may obtain some protection from firm or local municipality smoking-related policies prior to the passage of state-level smoke-free laws. It is worth noting that although state-level smoke-free laws complement those policies, the presence of pre-existing (firm- or municipality-level) smoking-related policies works against us finding a significant effect of such state-level laws on corporate innovation. It is thus likely that we underestimate the real effects of state-level smoke-free laws on innovation in this paper.

On the other hand, it is possible that the observed effects of state-level smoke-free laws on corporate innovation are part of legislating states' general programs to improve business conditions, which couple smoke-free laws with other business-promoting policies that may foster innovation. We believe that the above concern is less likely to be valid for the following reasons.

First, as we discussed in Section 2, a review of the political economy behind the adoption of smoke-free laws yields no evidence that they coincide systematically with other policy changes that possibly affect corporate innovation, instead, their adoption largely depends on the support of political elites, public opinions towards smoking control, and the relative strength of anti-smoking groups and the tobacco industry. To the best of our knowledge, there is no evidence that the above factors are directly related to corporate innovation. Second, throughout our analyses, we have included firm fixed effects, various state characteristics, and regional time trends, which should help account for the effect of "other business-promoting policies" to some extent. Third, the cross-sectional variation in the treatment effects documented in Section 5.6 indicates that the effect of smoke-free laws on corporate innovation is indeed tied to restrictions on smoking in workplaces. This helps alleviate the omitted variable concern, because an omitted variable is more likely to be correlated with the linear term, but less likely to be correlated with the interaction terms (Claessens and Laeven, 2003; Raddatz, 2006). Lastly, our analysis in Section 6.1 shows that the adoption of smoke-free laws leads to a significant outflow of inventors from the legislating states to other states, especially to states without such laws. This finding further suggests that smoke-free laws are unlikely to be correlated with any other statewide policies designed to improve business conditions and foster innovation, because such policies should help retain talent. Nevertheless, as in any research design that uses policy variations, we cannot completely rule out the existence of unexplored confounds whose influence coincides geographically with that of the variation in smoke-free laws we exploit for identification. The readers should bear in mind this possible limitation when deciding how our findings might be generalized.

7. Conclusions

In this paper, we investigate the effect of U.S. state-level smoke-free laws on real economic activities from the perspective of corporate innovation. We find a significant increase in firms' patents and patent citations following the passage of smoke-free laws, relative to firms in states without such laws. We further show that our results are robust to various alternative measures of innovation and that the observed effect of smoke-free laws on innovation is unlikely driven by chance. We then conduct a number of tests in support of a causal interpretation of our findings. Our tests of parallel trends show that there is no time trend difference in innovation output between firms in states that later adopt smoke-free laws and firms in states without such laws, and that the improvement in innovation output occurs several years after the passage of such laws. Our tests employing the treated firms and their neighboring control firms just across the state's border show that our results are unlikely to be driven by unobservable confounding local economic factors that would have affected both the treated and the control firms equally. Further, we present cross-sectional variations in the treatment effects suggesting that those treatment effects are indeed related to smoking: The impact of smoke-free laws on corporate innovation is more pronounced for firms in states with weaker pre-existing tobacco controls and for firms in states with a larger number of employees who have quit smoking. Finally, we provide some suggestive evidence on two underlying mechanisms: (1) workforce rebalancing triggered by the passage of smoke-free laws whereby, following the law change, inventors who are likely to be nonsmokers and more productive relocate into the legislating state while inventors who are likely to be smokers and less productive relocate out of the legislating state, and (2) the productivity increase of stayer inventors who never moved. Overall, our findings are consistent with the notion that a healthy working environment helps spur creativity.

Our paper has important policy implications for curbing smoking. Our results suggest that policies aimed at promoting healthier working environments can have real economic consequences in terms of promoting creative and innovative activities. This finding is particularly timely and relevant because of the ongoing debate on whether to ban smoking in workplaces across the U.S. and the rest of the world.

References:

Acharya, Viral, Ramin Baghai, and Krishnamurthy Subramanian, 2014. Wrongful discharge laws and innovation, *Review of Financial Studies* 27, 301-346.

Aghion, Philippe, Nicholas Bloom, Richard Blundell, Rachel Griffith, and Peter Howitt, 2005. Competition and innovation: An inverted-U relationship, *Quarterly Journal of Economics* 120, 701-728.

Aghion, Philippe, John Van Reenen and Luigi Zingales, 2013. Innovation and institutional ownership, *American Economic Review* 103, 277-304.

Alamar, C. Benjamin, and Stanton A. Glantz, 2004. Smoke-free ordinances increase restaurant profit and value, *Contemporary Economic Policy* 22, 520-525.

Anstey, Kaarin J., Chwee von Sanden, Agus Salim, and Richard O'Kearney, 2007. Smoking as a risk factor for dementia and cognitive decline: A meta-analysis of prospective studies, *American Journal of Epidemiology* 166, 367-378.

Atanassov, Julian, 2013. Do hostile takeovers stifle innovation? Evidence from antitakeover legislation and corporate patenting, *Journal of Finance* 68, 1097-1131.

Autor, David H., John J. Donohue III, and Stewart J. Schwab, 2006. The costs of wrongful-discharge laws, *Review of Economics and Statistics* 88, 211-231.

Barnes, Deborah E., Thaddeus J. Haight, Kala M. Mehta, Michelle C. Carlson, Lewis H. Kuller, and Ira B. Tager, 2010. Secondhand smoke, vascular disease, and dementia incidence: Findings from the Cardiovascular Health Cognition Study, *American Journal of Epidemiology* 171, 292-302.

Bartecchi, Carl, Robert N. Alsever, Christine Nevin-Woods, William M. Thomas, Raymond O. Estacio, Becki B. Bartelson, and Mori J. Krantz, 2006. Reduction in the incidence of acute myocardial infarction associated with a citywide smoking ordinance, *Circulation* 114, 1490-1496.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, 2004. How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics* 119, 249-275.

Bertrand, Marianne, and Sendhil Mullainathan, 1999. Corporate governance and executive pay: Evidence from takeover legislation, Princeton University working paper.

Bloom, Nicholas, Mark Schankerman, and John Van Reenen, 2013. Identifying technology spillovers and product market rivalry, *Econometrica* 81, 1347-1393.

Brigham, Janet, Janet Gross, Maxine L. Stitzer, and Linda J. Felch, 1994, Effects of a restricted work-site smoking policy on employees who smoke, *American Journal of Public Health* 84, 773-778.

Bunn III, William B., Gregg M. Stave, Kristen E. Downs, Jose Ma. J. Alvir, and Riad Dirani, 2006. Effect of smoking status on productivity loss, *Journal of Occupational and Environmental Medicine* 48, 1099-1108.

Claessens, Stijn, and Luc Laeven, 2003. Financial development, property rights, and growth, *Journal of Finance* 58, 2401-2436.

Conference Board of Canada, 2013. Smoking cessation and the workplace: Briefing 1—Profile of tobacco smokers in Canada.

Dalsey, Elizabeth, and Hee Sun Park, 2009. Implication of organizational health policy on organizational attraction, *Health Communication* 24, 71-81.

Evans, William N., Matthew C. Farrelly, and Edward Montgomery, 1999. Do workplace smoking bans reduce smoking? *American Economic Review 89, 728-747.*

Griliches, Zvi, 1990. Patent statistics as economic indicators: A Survey, *Journal of Economic Literature* 28, 1661-1707.

Hall, Bronwyn, Adam Jaffe, and Manuel Trajtenberg. 2005. The NBER patent citation data file: Lessons, insights and methodological tools. In A. Jaffe and M. Trajtenberg (eds.), *Patents, Citations and Innovations: A Window on the Knowledge Economy.* Cambridge, MA: The MIT Press.

Halpern, Michael T., Richard Shikiar, Anne M. Rentz, and Zeba M. Khan, 2001. Impact of smoking status on workplace absenteeism and productivity, *Tobacco Control* 10, 233-238.

Heider, Florian, and Alexander Ljungqvist, 2015. As certain as debt and taxes: Estimating the tax sensitivity of leverage from state tax changes, *Journal of Financial Economics* 118, 684-712.

Hilary, Gilles, and Kai Wai Hui, 2009. Does religion matter in corporate decision making in America? *Journal of Financial Economics* 93, 455-473.

Hirshleifer, David, Po-Hsuan Hsu, and Dongmei Li, 2013. Innovative efficiency and stock returns, *Journal of Financial Economics* 107, 632-654

Imbens, Guido, and Jeffrey M. Wooldridge, 2009. Recent developments in the econometrics of program evaluation, *Journal of Economic Literature* 47, 5-86.

Jacobson, Peter D., Jeffrey Wasserman, and Kristiana Raube, 1993. The politics of antismoking legislation, *Journal of Health Politics, Policy and Law* 18, 789-819.

Javitz, Harold, Susan Zbikowski, Gary Swan, and Lisa Jack, 2006. Financial burden of tobacco use: An employer's perspective, *Clinics in Occupational and Environmental Medicine* 5, 9-29.

Juster, Harlan R., Brett R. Loomis, Theresa M. Hinman, Matthew C. Farrelly, Andrew Hyland, Ursula E. Bauer, and Cuthrie S. Birkhead, 2007. Declines in hospital admissions for acute myocardial infarction in New York State after implementation of a comprehensive smoking ban, *American Journal of Public Health* 97, 2035-2039.

Kogan, Leonid, Dimitris Papanikolaou, Amit Seru, and Noah Stoffman, 2015. Technological innovation, resource allocation and growth, *Quarterly Journal of Economics* forthcoming.

Korniotis, George, and Alok Kumar, 2013. State-level business cycles and local return predictability, *Journal of Finance* 68, 1037-1096.

Lerner, Josh, 1994. The importance of patent scope: An empirical analysis, *RAND Journal of Economics* 25, 319-333.

Lerner, Josh, and Amit Seru, 2015. The use and misuse of patent data: Issues for corporate finance and beyond, Harvard Business School working paper.

Levine, Phillip B., Tara A. Gustafson, and Ann D. Velenchik, 1997. More bad news for smokers? The effects of cigarette smoking on wages, *Industrial and Labor Relations Review* 50, 493-509.

Li, Guan-Cheng, Ronald Lai, Alexander D'Amour, David M. Doolin, Ye Sun, Vetle I. Torvik, Amy Z. Yu, and Lee Fleming, 2014. Disambiguation and co-authorship networks of the U.S. patent inventor database (1975-2010), *Research Policy* 43, 941-955.

Llewellyn, David J., Iain A. Lang, Kenneth M. Lang, Felix Naughton, Fiona E. Matthews, 2009. Exposure to secondhand smoke and cognitive impairment in non-smokers: National cross sectional study with cotinine measurement, *British Medical Journal* 338, 462-467.

Lucas, E. Robert, 1988. On the mechanics of economic development, *Journal of Monetary Economics* 22, 3-42.

Pfizer, 2007. Pfizer facts: Smoking in the United States workforce.

Pickett, Melaine S., Susan E. Schober, Debra J. Brody, Lester R. Curtin, and Gary A. Giovino, 2006. Smoke-free laws and secondhand smoke exposure in US non-smoking adults, 1999-2002, *Tobacco Control* 15, 302-307.

Pirinsky, Christo, and Qinghai Wang, 2006. Does corporate headquarters location matter for stock returns? *Journal of Finance* 61, 1991-2015.

Raddatz, Claudio, 2006. Liquidity needs and vulnerability to financial underdevelopment, *Journal of Financial Economics* 80, 677-722.

Roberts, Michael, and Toni Whited, 2013. Endogeneity in empirical corporate finance, in: George Constantinides, Milton Harris, and Rene Stulz (eds.), *Handbook of the Economics of Finance* Volume 2, Elsevier.

Romer, Paul M., Endogenous technological change, *Journal of Political Economy* 98, S71-S102.

Sargent, Richard P., Robert M. Shepard, and Stanton A. Glantz, 2004. Reduced incidence of admissions for myocardial infarction associated with public smoking ban: Before and after study, *British Medical Journal* 328, 977-980.

Scherer, F. M., 1965. Corporate inventive output, profits, and growth, *Journal of Political Economy* 73, 290-297.

Seru, Amit, 2014. Firm boundaries matter: Evidence from conglomerates and R&D activity, *Journal of Financial Economics* 111, 381-405.

Sirgy, M. Joseph, 1986. A quality-of-life theory derived from Maslow's developmental perspective: 'Quality' is related to progressive satisfaction of a hierarchy of needs, lower order and higher, *American Journal of Economics and Sociology* 45, 329-342.

Solow, Robert, 1957. Technical change and the aggregate production function, *Review of Economics and Statistics*, 312-320.

Trajtenberg, Manuel, Rebecca Henderson, and Adam Jaffe, 1997. University versus corporate patents: A window on the basicness of invention, *Economics of Innovation and New Technology* 5, 19-50.

U.S. Department of Health and Human Services, 2006. The health consequences of involuntary exposure to tobacco smoke: A report of the Surgeon General.

U.S. Department of Health and Human Services, 2014. The health consequences of smoking—50 years of progress: A report of the Surgeon General.

Weng, Stephen F., Shehzad Ali, and Jo Leonardi-Bee, 2013. Smoking and absence from work: Systematic review and meta-analysis of occupational studies, *Addiction* 108, 307-319.

World Health Organization, 2007. Protection from exposure to second-hand tobacco smoke: Policy recommendations.

Appendix: Variable Definitions

Variable	*Definition*
Measures of Innovation Output	
Patent	Number of patents that are applied for (and subsequently awarded) by a firm in a given year.
LnPat	Ln (1 + Patent).
Citation	Number of citations received by a firm's patents. To adjust the citation count for the vintage issue, each patent's number of citations received is adjusted by the factor of Hall, Jaffe, and Trajtenberg (2005) at the technology class level.
LnCit	Ln (1 + Citation).
Patent per employee	Patent scaled by the number of employees (in 1,000s).
LnPat/emp	Ln (1 + Patent per employee).
Citation per employee	Citation scaled by the number of employees (in 1,000s).
LnCit/emp	Ln (1 + Citation per employee).
Patent/R&D	Patent scaled by R&D expenditures (in millions)
Citation/R&D	Citation scaled by R&D expenditures (in millions)
Originality	Originality is the sum of originality scores of patents filed by a firm in a year. The originality score of each patent is defined as one minus the Herfindahl index of the technology category distribution of all patents that have been cited by the designated patent.
Generality	Generality is the sum of generality scores of patents filed by a firm in a year. The generality score of each patent is defined as one minus the Herfindahl index of the technology category distribution of all patents that have cited the designated patent.
Patent value	Patent value is the sum of market values of granted patents to a firm in a year. The market value of each patent is measured by the market capitalization change (benchmarked against the market return) over a three-day window (*t*, *t + 2*) starting on the announcement day of a patent being approved (day *t*), following Kogan et al. (2015).
Firm Characteristics	
Firm size	Natural logarithm of the number of employees.
Cash	Cash and short-term investments normalized by the book value of total assets.
R&D	R&D expenditures normalized by book value of total assets. If R&D expenditures variable is missing, we set the missing value to zero.
ROA	EBITDA normalized by book value of total assets.
PPE	Property, plant & equipment normalized by the book value of total assets.
Leverage	Total debt normalized by the book value of total assets.
Capex	Capital expenditures normalized by book value of total assets.
Tobin's Q	Market value of equity plus book value of total assets minus book value of equity minus balance sheet deferred taxes, normalized by the book value of total assets.
H-index	Sum of squared sales-based market shares of all firms in a two-digit SIC industry.

Firm age	Number of years since a firm's first appearance in Compustat.
State Characteristics	
Smokefree	An indicator variable that takes the value of one if the state (where a firm's headquarter is located) has passed state-level smoke-free laws in a given year, and zero otherwise.
State GDP	Annual GDP of a given state.
State population	Population of a given state.
State unemployment rate	The unemployment rate of a state, calculated as the average unemployment rate over a twelve-month period.
State R&D expenditures	Total R&D expenditures in a given state normalized by state nominal GDP.
Democratic governor	An indicator variable that takes the value of one if the state is governed by a Democrat in a given year, zero otherwise.
College degree	Percentage of the workforce who are college graduates in a given state.
Smoker	Percentage of the workforce who are smokers in a given state.
High cigarette cost	An indicator variable that takes the value of one if the state average cost per pack of cigarettes normalized by weekly wage are above the sample median across all states in 1990, and zero otherwise.
Low cigarette cost	1 − High cigarette cost.
More quit smoking	An indicator variable that takes the value of one if a state's number of employees who quit smoking normalized by the state's total number of employees is above the sample median across all states in that year, and zero otherwise.
Few quit smoking	1 – More quit smoking.
Inflow from states without smoke-free laws	The number of newly-arrived inventors who previously applied for patents in a state that has not adopted smoke-free laws.
Outflow to states without smoke-free laws	The number of departed inventors to a state that has not adopted smoke-free laws.
Net inflow from states without smoke-free laws	Inflow from states without smoke-free laws – Outflow to states without smoke-free laws.
Inflow from states with smoke-free laws	The number of newly-arrived inventors who previously applied for patents in a state that has adopted smoke-free laws.
Outflow to states with smoke-free laws	The number of departed inventors to a state that has adopted smoke-free laws.
Net inflow from states with smoke-free laws	Inflow from states with smoke-free laws – Outflow to states with smoke-free laws.

Figure 1. Effects of State-level Smoke-free Laws on Corporate Innovation

This figure plots the effects of state-level smoke-free laws on corporate innovation, using the difference-in-differences specification in Equation (1), on patent and citation counts in legislating states, relative to non-legislating states, from five years prior to the passage of smoke-free laws (Year 0) to ten years afterwards.

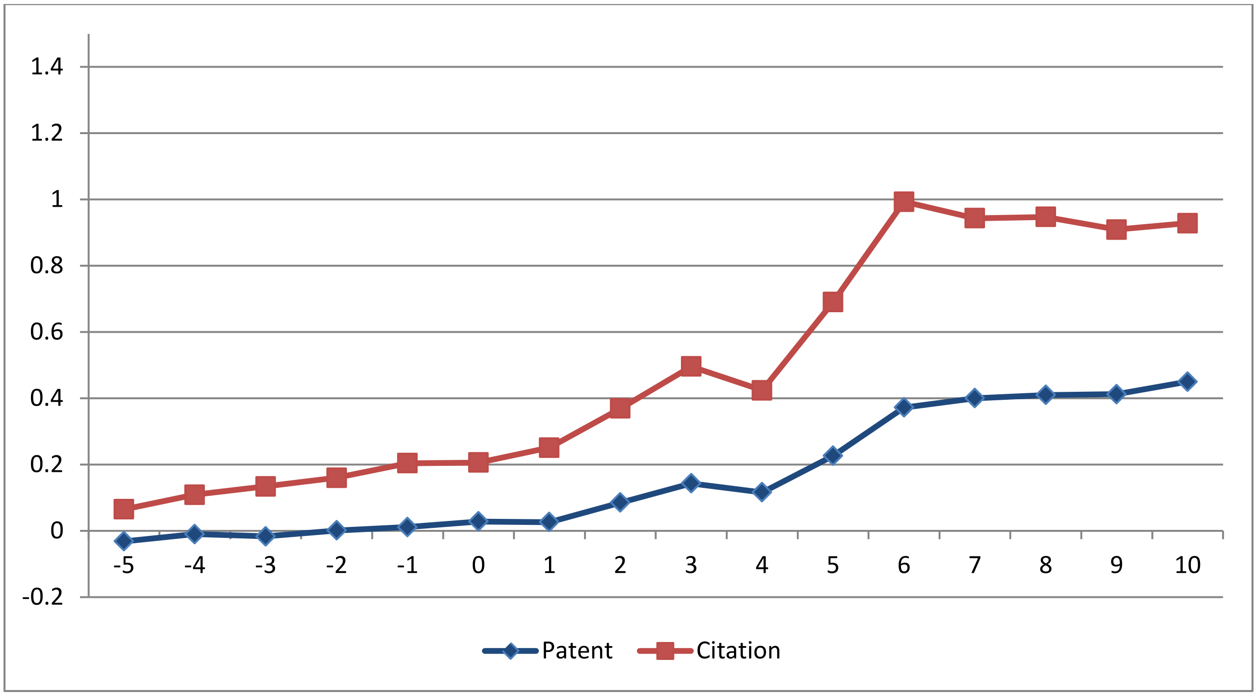
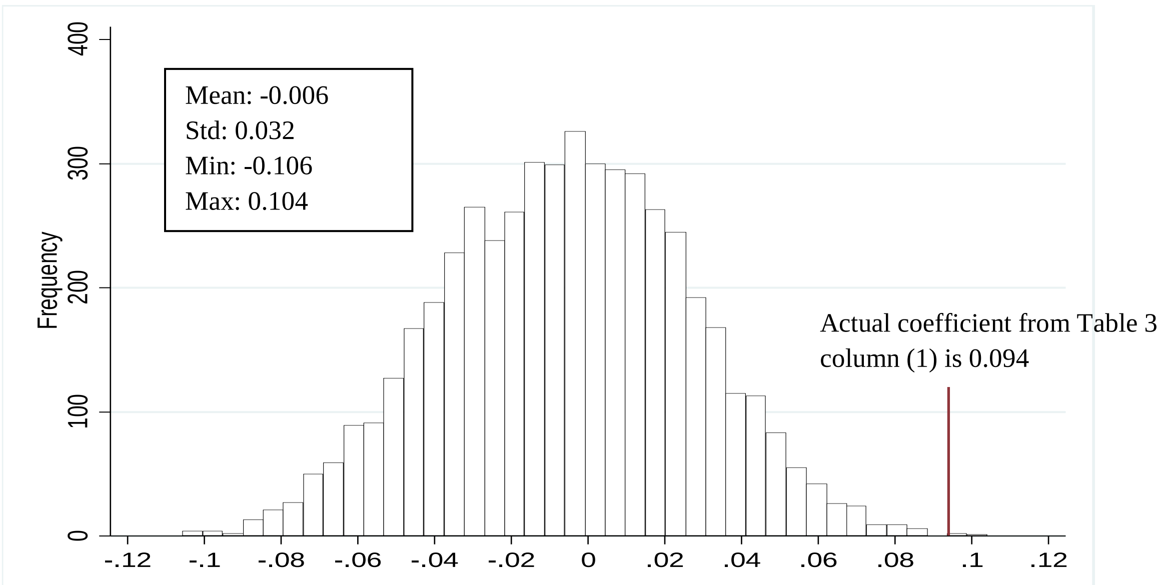


Figure 2. Placebo Tests

This figure plots the histogram of the coefficient estimates on *Smokefree* from 5,000 bootstrap simulations of the baseline model used in Table 3. For each state that ever adopted the smoke-free laws, we "assign" a pseudo adoption year that is randomly chosen from our sample period 1986-2008, and that is at least either five years before or five years after the actual adoption year. We then estimate the baseline regressions in columns (1) and (2) of Table 3 based on those pseudo adoption years and save the coefficient estimates on *Smokefree*. We repeat this procedure for 5,000 times. Panel A reports the distribution of the coefficient estimates when the dependent variable is LnPat. Panel B reports the distribution of the coefficient estimates when the dependent variable is LnCit.

Panel A: The histogram of the coefficient estimates on *Smokefree* when the dependent variable is LnPat



Panel B: The histogram of the coefficient estimates on *Smokefree* when the dependent variable is LnCit

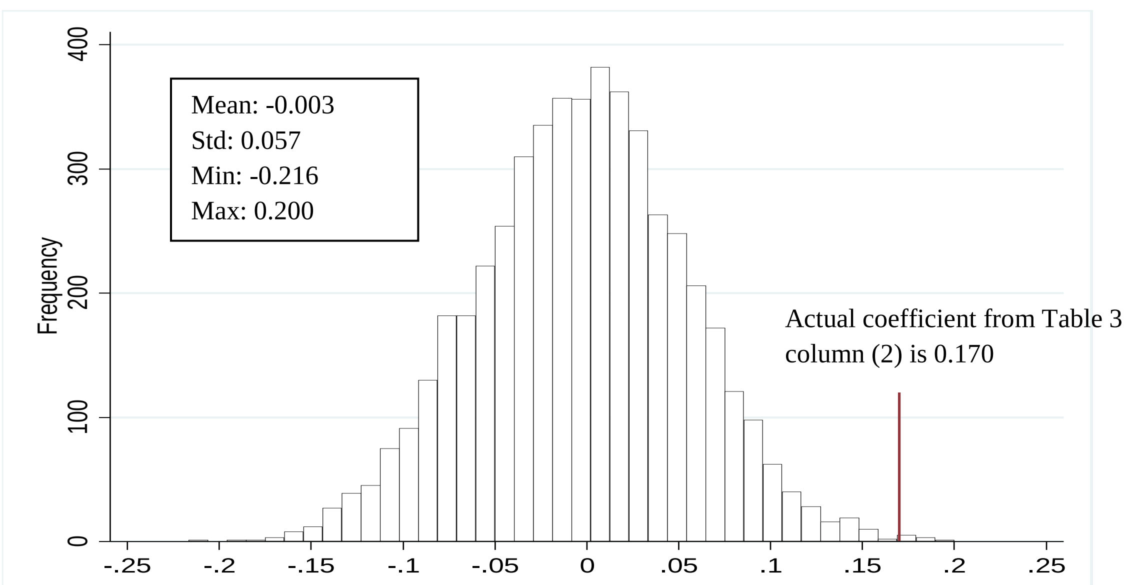


Table 1. List of States Legislating Smoke-free Laws

This table lists the years when different states adopted smoke-free laws that either completely ban smoking in the workplace or restrict smoking to separately ventilated areas.

State	Law	Year of Becoming Effective
New Hampshire	N.H. REV. STAT. ANN. § 155.66	1991
California	CAL. LAB. CODE § 6404.5(d)(13)	1995
Oregon	OR. REV. STAT. § 433.835, and 433.850	2002
Delaware	DEL. CODE ANN. tit. 16 § 2903(e)	2002
South Dakota	S.D. CODIFIED LAWS § 22-36-2 & 22-36-4	2002
Connecticut	CONN. GEN. STAT. ANN. § 31-40q (c)(1) and (c)(3)(A)	2003
Florida	FLA. STAT. ANN. § 386.204	2003
New York	N.Y. PUB. HEALTH LAW §§ 1399-n (2) and (5); N.Y. PUB. HEALTH LAW § 1399-o (1)	2003
Massachusetts	Mass. Gen. Laws 270, § 22 (b)(2)	2004
Rhode Island	R.I. Gen. Laws § 23-20.10-4	2005
Montana	MONT. CODE ANN. § 50-40-104	2005
North Dakota	N.D. CENT. CODE § 23-12-10 (1)	2005
Washington	WASH. REV. CODE §§ 70.160.020, -.030	2005
Arkansas	ARK. CODE ANN. § 20-27-1804 (b)(1)	2006
Colorado	COLO. REV. STAT. ANN. § 25-14-204 (1)(k)(I)	2006
District of Columbia	D.C. CODE ANN. § 7-742 (2)	2006
Hawaii	HAW. REV. STAT. ANN. § 328J-4	2006
New Jersey	N.J. REV. STAT. § 26:3D-58	2006
Nevada	NEV. REV. STAT. ANN. § 202.2483 (1)	2006
Ohio	OHIO REV. CODE ANN. § 3794.02 (a)	2006
Utah	UTAH CODE ANN. § 26-38-8	2006
Arizona	ARIZ. REV. STAT. ANN. § 36-601.01 (b)	2007
Louisiana	LA. REV. STAT. ANN. § 40:1300.256 (a)(3)	2007
Minnesota	MINN. STAT. §§ 144.413 (1)(b) & 144.414 (1)	2007
New Mexico	N.M. STAT. ANN. § 24-16-4 (A)	2007
Tennessee	TENN. CODE ANN. § 39-17-1803 (a)(2)	2007

Table 2. Summary Statistics

The sample consists of 46,342 firm-year observations over the sample period 1986-2008, obtained from merging the Compustat database with the patent database of Kogan et al. (2015). Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles.

	Mean	StdDev	25th Percentile	Median	75th Percentile
Patent	11.50	41.05	0	1	4
Citation	217.16	756.68	0.00	3.25	78.19
Patent per employee	7.06	18.14	0.00	0.27	4.38
Citation per employee	180.65	592.51	0.00	1.01	60.44
Employee (thousand)	7.48	19.52	0.22	0.94	4.44
Cash	22.20%	24.65%	2.93%	11.73%	34.29%
R&D	8.19%	12.77%	0.00%	3.21%	10.72%
ROA	4.60%	23.51%	1.50%	10.98%	17.20%
PPE	23.57%	17.92%	9.55%	19.27%	33.00%
Leverage	20.03%	20.33%	1.71%	15.59%	31.50%
Capex	5.35%	4.84%	2.10%	3.98%	6.99%
Tobin's Q	2.23	1.90	1.13	1.57	2.52
H-index	0.07	0.07	0.04	0.05	0.07
Firm age	18.81	14.20	7.00	14.00	28.00
State GDP (trillion $)	0.53	0.48	0.19	0.36	0.79
State population (million)	14.82	11.08	5.79	11.39	20.94
State unemployment rate	5.54%	1.39%	4.60%	5.37%	6.35%
State R&D expenditures	2.79%	1.25%	1.77%	2.44%	3.85%
Democratic governor	0.37	0.48	0	0	1
College degree	36.24%	6.34%	31.97%	35.96%	40.57%
Smoker	23.08%	4.46%	19.61%	23.57%	26.08%

Table 3. Effects of State-level Smoke-free Laws on Corporate Innovation

This table examines the effects of state-level smoke-free laws on corporate innovation using the difference-in-differences specification in Equation (2). Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) LnPat	(2) LnCit	(3) LnPat/emp	(4) LnCit/emp
Smokefree	0.094***	0.170***	0.088**	0.141**
	(0.028)	(0.059)	(0.033)	(0.069)
Firm size	0.272***	0.474***	0.002	0.162**
	(0.049)	(0.081)	(0.030)	(0.066)
Cash	0.327***	0.729***	0.506***	0.876***
	(0.047)	(0.106)	(0.074)	(0.136)
R&D	0.212***	0.773***	0.637***	1.222***
	(0.079)	(0.192)	(0.098)	(0.211)
ROA	-0.083	-0.110	-0.137**	-0.158*
	(0.052)	(0.088)	(0.056)	(0.094)
PPE	0.047	0.200	0.102	0.224
	(0.084)	(0.180)	(0.092)	(0.188)
Leverage	-0.115***	-0.343***	-0.219***	-0.447***
	(0.034)	(0.075)	(0.042)	(0.086)
Capex	-0.020	0.241	0.174	0.495**
	(0.171)	(0.244)	(0.130)	(0.243)
Tobin's Q	0.005	0.022***	0.011**	0.029***
	(0.003)	(0.005)	(0.004)	(0.006)
H-index	-1.026*	-1.766*	-0.367	-0.461
	(0.556)	(0.940)	(0.540)	(1.036)
H-index^2	1.428	2.725	0.395	0.867
	(1.127)	(2.364)	(0.961)	(2.112)
Ln (firm age)	0.213***	0.542***	0.202***	0.436***
	(0.015)	(0.049)	(0.033)	(0.061)
Ln (state GDP)	-0.106	-0.408	-0.126	-0.563
	(0.165)	(0.325)	(0.165)	(0.340)
Ln (state population)	0.089	0.397	0.097	0.536
	(0.166)	(0.334)	(0.164)	(0.350)
State unemployment rate	0.006	0.009	0.001	0.004
	(0.009)	(0.019)	(0.012)	(0.022)
State R&D expenditures	1.637	1.010	0.970	-0.160
	(1.485)	(2.914)	(1.744)	(3.572)
Democratic governor	-0.003	0.001	-0.012	-0.001
	(0.012)	(0.026)	(0.014)	(0.031)
College degree	0.039	0.140	0.465***	0.607*
	(0.166)	(0.307)	(0.155)	(0.328)
Smoker	-0.244	-0.694	-0.470	-0.995
	(0.276)	(0.745)	(0.327)	(0.786)

Firm FEs	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes
Constant	0.328	-0.232	0.263	-0.740
	(0.753)	(1.516)	(0.676)	(1.553)
Observations	46,342	46,342	46,342	46,342
Adjusted R-squared	0.780	0.657	0.567	0.522

Table 4. Pre-treatment Trends

This table examines whether there are any pre-treatment trends in corporate innovation of firms located in legislating states (the treated group) relative to firms located in non-legislating states (the control group). The indicator variables *Year −3, Year −2*, *Year −1*, *Year 0*, *Year 1*, *Year 2*, and *Year 3*$^+$, indicate the year relative to the year of passage (*Year 0*). For example, the indicator variable, *Year +1*, takes the value of one if it is one year after a state passes such laws, and zero otherwise. All the control variables 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 standard errors clustered by state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) LnPat	(2) LnCit	(3) LnPat/emp	(4) LnCit/emp
Year-3	-0.012	0.045	0.007	0.061
	(0.026)	(0.049)	(0.035)	(0.062)
Year -2	-0.009	0.016	0.039	0.067
	(0.032)	(0.066)	(0.034)	(0.073)
Year -1	-0.014	0.037	-0.020	0.036
	(0.036)	(0.065)	(0.042)	(0.076)
Year 0 (passage year)	0.001	0.013	0.011	0.018
	(0.042)	(0.057)	(0.043)	(0.061)
Year 1	0.028	0.100*	0.041	0.091
	(0.039)	(0.054)	(0.037)	(0.067)
Year 2	0.083*	0.182**	0.117***	0.182**
	(0.042)	(0.070)	(0.041)	(0.074)
Year 3$^+$	0.159***	0.277***	0.133***	0.236***
	(0.043)	(0.076)	(0.040)	(0.082)
Other controls	Same as Table 3			
Firm FEs	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes
Observations	46,342	46,342	46,342	46,342
Adjusted R-squared	0.780	0.657	0.567	0.522

Table 5. Controlling for Unobservable Local Economic Conditions

This table examines whether the effects of state-level smoke-free laws on corporate innovation are confounded by unobservable changes in local economic conditions using a sample of treated firms (located in legislating states) and neighboring control firms (located in non-legislating states) across the state border. For each treated firm, we match to a control firm that is in the same industry, in a neighboring state without such laws, and closest in total assets in the year when the smoking law is adopted. We further require that the distance between the treated and control firms be within 100 miles. All the control variables 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 standard errors clustered by state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) LnPat	(2) LnCit	(3) LnPat/emp	(4) LnCit/emp
Smokefree	0.143**	0.249***	0.172***	0.184*
	(0.053)	(0.087)	(0.048)	(0.102)
Other controls	Same as Table 3			
Firm FEs	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes
Observations	5,274	5,274	5,274	5,274
Adjusted R-squared	0.827	0.703	0.601	0.544

Table 6. Heterogeneous Treatment Effects

This table examines heterogeneous treatment effects of state-level smoke-free laws on corporate innovation by varying a state's pre-existing level of tobacco controls and by varying a state's number of employees who have quit smoking, using a difference-in-difference-in-differences specification. Panel A focuses on state-level tobacco controls. Panel B focuses on state-level number of employees who have quit smoking. All the control variables 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 standard errors clustered by state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Treatment effects by varying pre-existing state-level tobacco controls

	(1)	(2)	(3)	(4)
	LnPat	LnCit	LnPat/emp	LnCit/emp
Smokefree × High cigarette cost	0.015	0.081	0.027	0.087
	(0.044)	(0.093)	(0.054)	(0.097)
Smokefree × Low cigarette cost	0.117***	0.195***	0.105**	0.157*
	(0.036)	(0.072)	(0.042)	(0.083)
Other controls		Same as Table 3		
Firm FEs	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes
Observations	46,342	46,342	46,342	46,342
Adjusted R-squared	0.780	0.657	0.567	0.522

Panel B: Treatment effects by varying state-level number of employees who have quit smoking

	(1)	(2)	(3)	(4)
	LnPat	LnCit	LnPat/emp	LnCit/emp
Smokefree × More quit smoking	0.122***	0.208***	0.130***	0.199**
	(0.027)	(0.061)	(0.039)	(0.077)
Smokefree × Few quit smoking	0.054*	0.128*	0.031	0.080
	(0.032)	(0.070)	(0.033)	(0.073)
Other controls		Same as Table 3		
Firm FEs	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes
Observations	46,342	46,342	46,342	46,342
Adjusted R-squared	0.780	0.657	0.567	0.522

Table 7. Alternative Innovation Measures

This table examines the effects of state-level smoke-free laws on corporate innovation using alternative measures of innovation. All the control variables used in Table 3 are also included in this regression (except that we do not include R&D as a control variable in column (1)) but unreported for brevity. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. 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)	(5)	(6)
	R&D	Ln (1 + Patent/ R&D)	Ln (1 + Citation/ R&D)	Ln (1 + Originality)	Ln (1 + Generality)	Ln (1 + Patent value)
Smokefree	0.003	0.080*	0.156**	0.054***	0.059***	0.294**
	(0.002)	(0.042)	(0.065)	(0.017)	(0.018)	(0.118)
Other controls				Same as Table 3		
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	46,342	46,342	46,342	46,342	46,342	46,342
Adjusted R-squared	0.821	0.695	0.474	0.801	0.790	0.614

Table 8. Inventor Relocation

This table examines the effects of state-level smoke-free laws on inventor relocation and the difference in inventor productivity. Panel A employs a difference-in-differences specification at the state-year level to examine inventor relocation into and out of legislating states. Panel B compares inventor-level productivity between departed and newly-arrived inventors. Departed inventors are those who moved to non-legislating states within three years after their home state adopted smoke-free laws. Newly-arrived inventors are those who came from non-legislating states within three years after their destination state adopted smoke-free laws. Variable definitions are provided in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered by state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: State-level inventor relocation

	(1) Inflow from states without smoke-free laws	(2) Outflow to states without smoke-free laws	(3) Net inflow from states without smoke-free laws	(4) Inflow from states with smoke-free laws	(5) Outflow to states with smoke-free laws	(6) Net inflow from states with smoke-free laws
Smokefree	0.154***	0.149**	0.595*	-0.132	-0.237	0.127
	(0.053)	(0.062)	(0.321)	(0.217)	(0.202)	(0.236)
Ln(state GDP)	-0.222	-0.238	-0.047	-0.549	0.183	0.117
	(0.322)	(0.363)	(1.161)	(1.492)	(1.557)	(1.592)
Ln(state population)	0.857	1.492**	-2.067	5.141**	3.925*	1.183
	(0.689)	(0.616)	(2.283)	(2.005)	(2.111)	(1.822)
State unemployment rate	0.011	0.029	-0.189*	-0.004	-0.006	0.098
	(0.024)	(0.022)	(0.112)	(0.060)	(0.066)	(0.073)
State R&D expenditures	4.792	6.519**	-11.371	-8.306	-11.080	25.230***
	(3.274)	(2.656)	(18.348)	(11.869)	(10.732)	(7.746)
Democratic governor	0.028	0.025	-0.011	-0.046	-0.068	0.098
	(0.028)	(0.036)	(0.149)	(0.087)	(0.090)	(0.112)
College degree	-0.007	-0.041	-1.070	1.502	1.245	-0.297
	(0.349)	(0.483)	(2.317)	(1.168)	(1.191)	(1.346)
Smoker	0.181	-0.839	1.037	-4.354**	-4.507**	2.201
	(0.759)	(0.902)	(4.624)	(1.963)	(2.156)	(2.732)
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-7.844	-17.205**	33.459	-68.752**	-59.026**	-21.085
	(8.497)	(6.899)	(25.378)	(26.385)	(27.135)	(21.220)
Observations	1,067	1,067	1,067	1,067	1,067	1,067
Adjusted R-squared	0.946	0.944	0.458	0.825	0.827	0.366

Panel B: Productivity of departed and newly-arrived inventors

	Newly-Arrived Inventors		Departed Inventors		Test of Differences	
	Mean (1)	Median (2)	Mean (3)	Median (4)	t-test (1) – (3)	Wilcoxon test (2) – (4)
Total # of patents by the inventor over the sample period	14.50	7	13.82	6	0.69**	1***
Total # of patent citations received by the inventor over the sample period	322.77	78.32	286.47	69.63	36.30***	8.69***

Table 9. Productivity of Stayer Inventors

This table examines the effects of state-level smoke-free laws on corporate innovation using the difference-in-differences specification in Equation (2), limiting to a subsample of inventors who never moved over the sample period. All the control variables 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 standard errors clustered by state are in parentheses. Superscripts ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) LnPat	(2) LnCit	(3) LnPat/emp	(4) LnCit/emp
Smokefree	0.113***	0.205***	0.110***	0.177**
	(0.029)	(0.062)	(0.034)	(0.069)
Other controls		Same as Table 3		
Firm FEs	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes
Observations	46,342	46,342	46,342	46,342
Adjusted R-squared	0.741	0.637	0.555	0.515