# Spillover Effects of the Opioid Epidemic on Labor and Innovation

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**Abstract**

Using two independent exogenous shocks, we investigate whether the opioid epidemic affects labor and innovation even when personal addiction is not the main driver. Exploiting a setting of corporate innovation where production is mostly generated by white-collar individuals - unlikely addicts - we first find that county-level increases in opioid abuse cause declines in innovation. Second, legislations limiting opioid prescriptions lead to increases in innovation. A plausible channel is that star-young inventors relocate to counties that suffer less from the epidemic. Finally, the opioid epidemic is more detrimental for firms when employees' input to innovation and firm growth are important.

***JEL Classification*:** G18, I10, I15, I18, O30, R2

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## 1. Introduction

Opioid abuse is a global epidemic which has reached unprecedented levels in the U.S., with an estimated 10.1 million people (or 3.7% of the U.S. population) misusing opioids (National Survey on Drug Use and Health, 2019). Such a phenomenon has led to a dramatic rise in emergency department visits (Centers for Disease Control and Prevention, 2020) and a record number of deaths.[1] Beyond such tragic effects on health and mortality rates, the opioid epidemic also has enormous *direct* – i.e., through personal addiction – economic consequences (Florence, Luo, and Rice, 2021). Among other consequences, addiction to opioids leads to declining labor force participation, mostly concentrated among less educated, blue-collar individuals (Krueger, 2017). This, in turn, has adverse effects for firm value and investments (Ouimet, Simintzi, and Ye, 2021), the housing market (Custodio, Cvijanovic, and Wiedemann, 2022) and access to finance of affected municipalities (Cornaggia, Hund, Nguyen, and Ye, 2022). Less is known, however, whether opioid abuse have significant spillover effects on non-addicted participants in the labor market and, if so, what the potential implications are for firms.

Spillover effects on non-addicted individuals could arise because of actions of the addicted individuals. For instance, due to lower productivity or even job loss, addicted individuals are exposed to greater economic difficulties; not only their compensation is reduced, but they may also face increasing needs to secure finances to procure substances, something that may lead to illicit market activities and crime (e.g., Maclean, Mallatt, Ruhm and Simon, 2022). In addition, due to intoxication, they become more vulnerable to potential offenders (Becker, 1968). Such safety and security problems make an area less appealing to live and work at, and adversely affect the quality

[1] See, e.g., https://newsnetwork.mayoclinic.org/discussion/emergency-department-visits-related-to-opioid-overdoses-up-significantly-during-covid-19-pandemic/ and the article in the Wall Street Journal (November 17, 2021): https://www.wsj.com/articles/drug-overdose-deaths-fueled-by-fentanyl-hit-record-high-in-u-s-11637161200.

of life of individuals who are not addicted, which may prompt them to move away and live elsewhere.[2]

In this study, we investigate whether the opioid epidemic affects firms in a setting where outcomes are unlikely to be driven by personal addiction: corporate innovation. This setting has two main advantages. First, innovation is typically generated by creative and well-educated individuals. Such individuals are unlikely to make use of opioids (Case and Deaton, 2020). In this respect, any effect of the opioid epidemic on corporate innovation should be indirect, and thus, distinct from prior literature which focuses on firm implications arising from the direct effect of addicted individuals. Second, innovation inputs, such as inventors, and outcomes, such as patents, are externally observable and important to firms' long run growth (e.g., Fich, Nguyen, and Petmezas, 2022). Thus, we can zoom-in on inventors by tracking them over time; this is helpful to understand spillover effects of the opioid epidemic on firms, such as those reflected in corporate innovation and inventor mobility.

To establish the causal effect of the opioid epidemic on corporate innovation we employ two separate and independent methods. First, we use an exogenous positive supply shock in opioid distribution to instrument opioid mortality (Cornaggia et al., 2022a). We find that firms in counties associated with opioid abuse experience a *decrease* in corporate innovation driven by migration of high skilled labor.

Second, we complement this analysis by investigating the impact of the staggered passage of state-level opioid-limiting legislations and find that the adoption of these laws leads to an *increase* in corporate innovation. The setting of these two experiments is not only independent but also allows us to test whether the effect is symmetric, since the state legislation is expected to have a

[2] Consistent with this idea, Custodio et al. (2022) find that opioid prescriptions cause declines in residential real estate prices.

negative impact on opioid consumption and the resulting supply shock is expected to lead to an increase in corporate innovation – which is what we find.

We measure corporate innovation using patents, citations and innovation value (Seru, 2014; Kogan, Papanikolaou, Seru, and Stoffman, 2017; Mukherjee, Singh, and Zaldokas, 2017). For the instrumental variable approach, the instrument is based on the supply of a variety of opioids with the highest (i) addictive potential and (ii) desirability to addict, through distribution channels that exhibit little oversight. Cornaggia et al. (2022a) show that this *supply chain* instrument is highly correlated with the opioid mortality rates.[3] At the same time, the exclusion restriction is likely to hold as it is unlikely to directly correlate with corporate innovation.[4] The results show that county-level increases in opioid abuse cause declines in corporate innovation. Economically, the effect of opioid abuse on corporate innovation is substantial. For example, moving from the 25$^{th}$ to the 75$^{th}$ percentile of the instrumented opioid mortality leads to 2.27 fewer patents. Given that an average firm generates 21.07 patents per year, our results imply an annual reduction of about -10.8% in the number of patents.

These results are robust to alternative measures of corporate innovation, such as patent originality and generality, patent successfulness and employees' innovation productivity. In addition, the results remain robust after excluding (i) firms that engage in mergers and acquisitions (M&As), which would allow them to acquire innovation (Sevilir and Tian, 2012), rather than develop innovation in-house; and (ii) the financial crisis period (2008-2009), which may have affected the physical and mental well-being (Ruckert and Labonte, 2017) that in turn increased the likelihood of opioid abuse and firms' willingness to invest in innovation (Archibugi, Filippetti, and

[3] The recent opioid epidemic started in 1995, when Purdue Pharma developed the prescription painkiller OxyContin (see, e.g., Alpert, Evans, Lieber, and Powel, 2022). OxyContin was actively promoted through individual physicians and became a blockbuster. However, it was highly addictive, and thus, areas with higher prescription rates (i.e., more *supply*) were associated with relatively more residents addicted to opioids and greater mortality rates.

[4] Because individual physicians, who were responsible for the prescription of OxyContin, vary in opioid prescribing behavior, heterogeneity across addicted residents is more likely to be a supply effect rather than the outcome of local economic conditions (Barnett, Olenski, and Jena, 2017; Currie and Schwandt, 2020; Maclean, Mallatt, Ruhm, and Simon, 2022).

Frenz, 2013). Finally, although prior literature suggests that deteriorating economic conditions have little effect on opioid mortality (see, e.g., Ruhm, 2018) and the opioid epidemic more broadly (see, e.g., Currie and Schwandt, 2020), we further alleviate concerns that the results might be driven by economic conditions. First, we exploit variation in opioid abuse within adjacent counties, which likely exhibit similar economic conditions, to re-examine its impact on corporate innovation. The results continue to show a negative relationship between opioid abuse and corporate innovation. Second, we use a sub-sample of firms where a potential relationship between economic conditions and opioid abuse is less likely to prevail. In particular, we focus on firms with headquarters located in counties that belong to the top tercile in terms of the average per capita income during our sample period. We still find a negative relationship between opioid abuse and corporate innovation. Overall, these results support the view that the local economic environment does not drive our results.

We complement the instrumental variable analysis using a quasi-natural experiment that exploits the adoption of state-level legislation which aims at limiting opioid prescriptions.[5] The passage of these laws is not related to the economic environment and the political economy with the probable exception of opioid overdose death rate (Ouimet et al., 2021). Therefore, the timing of these laws is plausibly exogenous to corporate innovation, creating an effective setting to examine whether the passage of these laws affects corporate innovation. Using a difference-in-differences analysis, we find a positive impact (i.e., the mirror image of our previous findings), providing further support for the causal interpretation of opioid epidemic effects on corporate innovation. In economic terms, the effect of changes in state regulations on corporate innovation is considerable and symmetrically comparable to the results of the instrumental variable analysis. For instance, after the passage of the laws, firms in the affected states experience a 15.5% increase in

[5] Specifically, in response to public outcry about health and economic implications arising from the opioid epidemic, state regulators passed laws to limit opioid prescription, aiming to address the over-prescription of powerful opioid pain relievers. Massachusetts, in March 2016, was the first state that enacted legislation to limit the supply of opioids prescribed by doctors to seven days for adults and minors. Since then, another 37 states have passed similar laws.

the number of firm patents relative to the innovation of rival firms in unaffected states. Importantly, this result is consistent with Ouimet et al. (2021), who demonstrate a positive market reaction to the announcement of the laws and highlight that not only investors but also inventors value the decisiveness of several states to fight the opioid epidemic.

Next, we explore the inventor mobility channel as a plausible mechanism in explaining the results. Particularly, we investigate whether the opioid epidemic *indirectly* affects the "industrial organization" of innovation by impeding a firm's ability to retain existing (productive) inventors and attract new ones. To understand the merit of this explanation, we use inventor-level data, which track individual inventors over time across two successive patent applications assigned to different firms. The opioid epidemic can lead to reallocation of human capital from firms located in high opioid epidemic counties to firms that are less affected by the opioid epidemic. We investigate such reallocation implications by focusing on out-of-county moves. We obtain a positive relation between the instrumented opioid mortality rates and out-of-county moves, suggesting a causal link between opioid abuse and inventor mobility. In line with these results, we also find a negative relationship between instrumented opioid mortality rates and within-county moves, which suggests that if inventors move to another firm, it is less likely to move to firms within the same opioid-affected county.

Additionally, we show that moving inventors are more (less) likely to move to a lower (higher) crisis county. Concentrating particularly on the characteristics of relocating inventors, we find that more productive (so-called "star") and younger inventors who face lower adjustment costs are more likely to relocate to a lower crisis county. Hence, firms located in counties that are affected more by the opioid epidemic lose both their most productive inventors and youth creativity. Overall, these results support the inventor mobility as a plausible explanation for the negative impact of opioid abuse on corporate innovation.

Finally, to shed further light on the impact of the inventor mobility explanation on corporate innovation, we exploit the fact that our main analysis is at firm level and analyze heterogeneous effects. First, we find that the decline in corporate innovation concentrates among firms with high R&D per employee. In such firms, employees' input to innovation plays a more crucial role, thus, they are more dependent on human capital. Second, building on the idea that inventor mobility should have greater consequences in the presence of growth opportunities, we show that the opioid epidemic effect is more pronounced among high growth firms. Third, the effect concentrates among firms that are mostly exposed to the challenges arising from inventor mobility, namely larger firms. Larger firms, consistent with an optimal matching equilibrium (Gabaix and Landier, 2008), are matched with more talented inventors. Thus, talented inventor mobility problems, combined with the fact that larger firms can handle bigger and more complicated projects, worsen the impact of the opioid epidemic on innovation.

Our study is related to the growing literature on causes and consequences of the opioid epidemic. Alpert et al. (2022) emphasize the role of the introduction of OxyContin and the related marketing efforts in the development of the epidemic. Maeckle and Ruenzi (2022) use county-to-county friendship links and examine whether social proximity relates to the propagation of the opioid epidemic. Most relevant to our work are studies that examine real effects of the opioid epidemic. Florence et al. (2021) estimate the costs for opioid use disorder and fatal opioid overdoses to the U.S. economy. Krueger (2017) examines the impact of the opioid epidemic on labor markets and finds a negative relation between opioid prescriptions and labor force participation rates. Lower labor participation rates have implications for both capital and municipal markets. Ouimet et al. (2021) establish a causal link between opioid prescriptions and firm value and investment, which they attribute to a lower supply of workers. Custodio et al. (2022) find that mortgage default and foreclosures due to opioid addiction increase housing supply, resulting in lower real estate prices. Finally, Cornaggia et al. (2022a) and Li and Zhu (2019) show that opioid abuse negatively affects

municipalities' cash flows, leading to lower access to finance. All these studies show negative effects of opioid abuse on economic outcomes through a *direct* use of opioids. We add to this literature by documenting an economically significant *indirect* effect of the opioid epidemic on corporate innovation driven by inventor mobility. Understanding the dynamics of the opioid epidemic on corporate innovation is of first-order importance given that innovation is vital for sustaining economic growth.

Second, we contribute to the literature on the mobility of individuals' talent (Almeida and Kogut, 1999; Moretti and Wilson, 2015; Hombert and Matray, 2017) and its impact on knowledge diffusion (Marx, Strumsky, and Fleming, 2009) and economic growth (Docquier and Rapoport, 2012). Our results show that the opioid epidemic influences the mobility of individuals' talent.

Finally, our work is related to the literature that explores how the geographical area where a firm resides affects corporate innovation. Innovation relates to (i) market characteristics, such as competition (Aghion, Bloom, Blundell, Griffith, and Howitt, 2005); (ii) regulations, such as laws that protect employees against unjust dismissals (Acharya, Baghai, and Subramanian, 2014), employment non-discrimination (Gao and Zhang, 2017) and smoking bans (Gao, Hsu, Li, and Zhang, 2020); and (iii) other local characteristics, such as county religious adherence (Chen, Podolski, Rhee, and Veeraraghavan, 2014) and terrorist attacks (Fich, Nguyen, and Petmezas, 2022). We contribute to this literature by showing that the opioid epidemic curtails corporate innovation.

Closest to our work are the concurrent studies by Jansen (2022) and Cornaggia, Hund, Pisciotta, and Ye (2022b). Jansen (2022) finds that opioid abuse negatively affects the pricing of non-addicted consumer finance products because traditional credit scoring attributes fail to separate addicted from non-addicted borrowers. Thus, like ours, the opioid epidemic consequences are indirect. Cornaggia et al. (2022b), in a similar spirit to our study, show that opioid abuse reduces the quantity and value of firm innovation driven by the migration of high-income employees from

opioid affected areas. There are, however, important differences between their study and ours. First, their analysis is based on *aggregate* – at the county level – innovation output and migration flows. Instead, we focus on *disaggregated* firm and inventor-level analyses, which allows us to examine the opioid epidemic implications at a more granular level. In this respect, we exploit *heterogeneous* implications of the opioid epidemic and provide results on which specific firms (and inventors) are most likely affected by the opioid epidemic, as well as why and how such firms are affected. Second, to establish causality, Cornaggia et al. (2022b) use a difference-in-differences framework around Purdue's 2010 reformulation of OxyContin, and an instrumental variable approach in which they instrument opioid abuse exposure with two instruments that rely on Purdue's marketing strategy for OxyContin. We also use an instrumental variable approach, but, instead, we instrument opioid mortality using the supply of a variety of opioids with the highest (i) addictive potential and (ii) desirability to addict, through distribution channels that exhibit little oversight. Their results thus provide independent support for our conclusions. In addition, we employ an additional exogenous shock whereby we exploit recently passed state laws which aimed at limiting opioid over-prescription. We find that these regulatory efforts helped to reverse the negative implications of the opioid epidemic on corporate innovation. This result provides further causal evidence of the effects of the opioid epidemic on innovation, this time from the opposite side, i.e., limiting opioid prescriptions increases corporate innovation. Importantly, it reveals that inventors respond to actions against the opioid epidemic, something that is relevant for policy makers who attempt to mitigate both direct and indirect effects of the opioid epidemic.

The remainder of the paper is organized as follows. Section 2 describes the data and research design. Section 3 presents the results for the impact of the opioid epidemic on corporate innovation. Section 4 presents the channel underlying the opioid epidemic-corporate innovation relationship. Section 5 presents the results on heterogenous effects. Finally, Section 6 concludes the study.

## 2. Data and Research Design

To construct the sample, we combine data from several sources. We use (i) the National Center for Injury Prevention and Control (NCIPC) of the Centers for Disease Control (CDC) to obtain county-level related mortality rates; (ii) the Drug Enforcement Administration (DEA) to obtain information about opioid prescriptions and the distribution channels; (iii) the U.S. Census Bureau, U.S. Bureau of Economic Analysis (BEA) and the U.S. Association of Religion Data Archives (ARDA) to obtain county-level variables; (iv) Standard and Poor's Compustat database to obtain financial information; (v) the CSRP database to obtain stock information; and (vi) data for patents and citations from Stoffman, Woeppel, and Yavuz (2022).[6] Appendix A provides detailed information about the definitions of the variables used in our analysis and the data sources.

### 2.1. Opioid epidemic data

#### 2.1.1. County-level opioid mortality

Our main analysis utilizes data from the Multiple Cause of Death data found in the NCIPC of the CDC (https://wonder.cdc.gov/controller/datarequest/D76). Specifically, following the 10th Revision (ICD-10) classifications by the International Classification of Diseases, we define drug overdose (or poisoning) deaths as those with ICD-10 underlying cause-of-death codes X40-X44 (unintentional overdose), X60-X64 (suicide by drug self-poisoning), X85 (homicide by drug poisoning) or Y10-Y14 (undetermined intent). Our data do not suffer from the left-censoring problem discussed in Cornaggia et al. (2022a). Nevertheless, it is worth noting that drug poisoning deaths include deaths caused – apart from opioids – by other types of non-opioid drugs with abuse potential (e.g., cocaine, methamphetamine, amphetamine, prescription stimulants). This is unlikely to affect our results for two reasons: First, most individuals using these other non-opioid types of

[6] See https://www.mikewoeppel.com/data.

drugs also use opioids. For instance, Lowder, Ray, Huynh, Ballew, and Watson (2018) examine actual toxicology reports in Marion County of Indiana and conclude that 86.8% of all unspecified drug deaths were tested positive for an opioid. Second, while opioid deaths show a large increase during our sample period, deaths due to non-opioid drugs remain relatively stable (Buchanich, Balmert, Williams, and Burke, 2018). Given that our empirical analysis exploits across-county changes in overdose death rates, our results are more likely to capture opioid overdoses than non-opioid drugs overdoses.

The main variable of interest, *Opioid Death*, is defined as the county drug overdose mortality rate per 100,000 residents. Figure 1 presents a choropleth map of opioid mortality in 2014 (the last year of our sample period). Areas with dark blue color exhibit higher opioid mortality rates. First, the map shows that opioid abuse concentrates in parts of the Midwest and South (e.g., West Virginia), parts of the Southwest (e.g., Oklahoma and New Mexico) and Northeast (e.g., Maine). Second, consistent with absent (or weak) relationship between economic activity and opioid mortality (e.g., Ruhm, 2018; Currie and Schwandt, 2020), there are economically flourishing counties which have high opioid mortality rates, and others which have few or no opioid deaths. Likewise, there are economically depressed counties which have low mortality rates. Finally, there is substantial geographic heterogeneity in opioid mortality rates even within states. For instance, the standard deviation of opioid mortality rates in California and Pennsylvania, the two most innovative states, is 13.77 and 8.07, respectively, highlighting the importance of a county-level measurement which preserves the granularity of the opioid epidemic.

### 2.1.2. Instrument: Opioid prescriptions and distribution channels

Building on the supply-side perspective of opioid epidemic, which emphasizes the role of individual physicians in promoting opioid analgesics by pharmaceutical companies, we instrument mortality rates using a supply chain variable that is based on information about prescriptions and distribution

channels of opioids by type. Specifically, we obtain the relevant information from the DEA, which collects data from manufacturers and distributors (https://www.slcg.com/opioid-data/) through the Automated Reports and Consolidated Ordering System (ARCOS). The data are comprehensive and contain information about every opioid pill sold in the U.S. during the period 2006-2014 for every retail distributor, including pharmacies, hospitals, etc. Because each opioid pill may exhibit different strength, following the public health literature, we convert their opioid strength using the Morphine Milligram Equivalent (MME) value. Then, we use the retail distributor's zip codes to geolocate pills to counties. Our instrument, *MME*, is defined as the county MME value per 100,000 residents.

## 2.2. Innovation data

### 2.2.1. Corporate innovation

To measure innovation, we use the patent dataset created by Stoffman et al. (2022). The dataset covers all patent applications filed with (and subsequently granted by) the U.S. Patent and Trademark Office (USPTO). Due to data limitations in our instrument, our final sample covers the period 2007-2015 and includes 2,951 firms with 15,178 firm-year observations.

For this sample, we construct three main measures of corporate innovation: First, the natural logarithm of one plus the number of patents filed (and subsequently granted) by a firm, in a given year.[7] To make the calculation, we focus on the patent application year rather than grant year because, according to Griliches, Pakes, and Hall (1987), the application year better captures the actual time of innovation. In addition, using the application year prevents anomalies that may arise due to the time lag until the granting year, which is, on average, two years.

[7] We use natural logarithms of the innovation variables due to the high level of right skewness of patent data.

A disadvantage of this patent output measure, however, is that it does not inform whether the patent is associated with a revolutionary innovation or an incremental discovery (e.g., Griliches, 1990). Therefore, to capture the importance of the patent outputs, we also use the natural logarithm of one plus the number of citations across all patents filed by a firm, in a given year.

Finally, as in Kogan et al. (2017), we use a measure of innovation quality, defined as the natural logarithm of one plus the cumulative economic value (in millions of nominal U.S. dollars) of patents that a firm applies for each year. The variable represents the market value of a new patent as implied by the market reaction to the patent approval. Specifically, a patent's value is the firm's market adjusted return over the three-day window around the date of patent approval ($t$, $t$+2), multiplied by the firm's market capitalization on the day prior to the announcement of the patent issuance.

In robustness analyses, we also use the following innovation measures: First, following Trajtenberg, Henderson, and Jaffe (1997), we evaluate the importance of innovation using patents that start a citation stream (i.e., patents that have higher originality value) and patents that affect a wider set of succeeding patent classes (i.e., patents that have higher generality value). In addition, as in Atanassov (2013), we assess how successful a firm's patents are using the natural logarithm of one plus the number of citations across all patents filed by a firm scaled by the number of patents filed (and subsequently granted) by a firm, in a given year. This measure is motivated by the fact that a simple count of patents might not distinguish breakthrough innovations from less significant technological discoveries. Lastly, following Acharya et al. (2014) and Mukherjee et al. (2017), we evaluate employees' innovation productivity using the natural logarithm of one plus the number of patents filed (and subsequently granted) by a firm scaled by the number of employees, in a given year.

### 2.2.2. Inventor mobility

To identify when inventors change their employer, we follow Marx, Strumsky, and Fleming (2009) and check whether an inventor files two successive patent applications which are assigned to different firms. The data include the names of the inventors for every patent as well as the identity of their employer, but they do not have consistent listings of inventor names or unique inventor identifiers. Lai, D'Amour, and Fleming (2009) and Li et al. (2014) generate, through a disambiguation algorithm, unique inventor identifiers, which we use to track individual inventors over time.

Initially, we keep one observation per inventor-employer-year (hence, we drop $n-1$ observations whenever the same inventor files $n$ patents during the same year with the same employer). Whenever an inventor files two successive patent applications assigned to different firms, the inventor-employer-year is based on the midpoint between the year in which the first patent is filed and the year in which the subsequent patent is filed. Then, we use this information to create various indicator inventor-level variables, including (i) an out-of-county move, (ii) a within-county move, (iii) a move to a lower crisis county, and (iv) a move to a higher crisis county. As in Acharya et al. (2014), we identify county moves using the location of a firm's headquarters; when this information is missing we use the location of inventors.[8] Our final sample includes 274,627 inventors with 629,763 inventor-employer-year observations. About 4.1% of observations are associated with a move; 3.4% are out-of-county moves, whereas 0.7% are within-county moves. Moves to a lower/higher crisis county are based on the difference in drug overdose mortality rate per 100,000 residents of the county where the new employer is located relative to the county where the previous employer is located. Specifically, we define a move as being to a lower (higher) crisis county when the difference in mortality rates is above (below) the 75$^{th}$ (25$^{th}$) percentile.

[8] The results remain qualitatively similar if we identify county moves using the location of inventors throughout, instead.

### 2.3. Sample overview and statistics

To form our main sample, we merge the opioid epidemic data and the innovation data with county characteristics from the U.S. Census, BEA and ARDA, and financial and accounting information from the CRSP and Compustat databases. Our firm-level sample excludes all firms without headquarters in the U.S. and with missing ZIP codes. In addition, as in Brav, Jiang, Ma, and Tian (2018), we focus on innovative firms and, thus, exclude firm-years without positive R&D expenditures. Likewise, our inventor-level sample measures mobility focusing on active inventors, defined as inventors that filed at least one patent over the previous three years.

Table 1 reports the mean, standard deviation, $10^{th}$, $50^{th}$ and $90^{th}$ percentiles for the variables used in our analysis. Panel A reports summary statistics for the firm-level sample and Panel B provides summary statistics for the inventor-level sample. Patent-data-based samples like ours have been extensively used in previous studies, so we refrain from discussing descriptive statistics but verify that they are in line with prior studies (see, for example, Chang, Fu, Low, and Zhang, 2015; Cornaggia, Mao, Tian, and Wolfe, 2015; Balsmeier, Flemming, and Manso, 2017; Mukherjee et al., 2017; Li and Zhu, 2019; Fich et al. 2022).

### 2.4. Research design

Empirically, it is challenging to identify the causal impact of the opioid epidemic on corporate innovation because opioid abuse and corporate innovation might be jointly affected by other variables, such as the local economic environment. To alleviate such endogeneity concerns, we employ an instrumental variable (IV) regression approach and use a supply chain instrument that is highly correlated with the opioid mortality rates, but unlikely to correlate with local economic environment and innovation. Specifically, we evaluate how the opioid epidemic affects innovation using the following two-stage least squares regressions:

$$Ln(Opioid\ Deaths + 1)_{l,t} = a_0 + a_1 MME_{l,t} + \beta' X_{i,l,t} + \gamma' Z_{l,t} + a_h + a_t + u_{i,l,t} \quad (1)$$

$$Ln(Y + 1)_{i,l,t+1} = \delta_0 + \delta_1 Ln(\widehat{Opioid\ Deaths} + 1)_{l,t} + \zeta' X_{i,l,t} + \eta' Z_{i,l,t} + a_h + a_t + \varepsilon_{i,l,t} \quad (2)$$

where $Y_{i,l,t+1}$ are the dependent variables of interest – the corporate innovation measures – for firm *i*, located in county *l* in year *t+1*. [9] The independent variable of interest is the instrumented $\widehat{Opioid\ Deaths}_{l,t}$ which captures the exposure to opioid epidemic in county *l* in year *t*. The coefficient $\delta_1$ is the elasticity of corporate innovation with respect to opioid abuse; thus, it measures the average difference in corporate innovation attributable to opioid abuse. The instrument is the supply chain *MME* for county *l* in year *t*.

We include firm-level, $X_{i,l,t}$, and county-level, $Z_{l,t}$, control variables that could affect corporate innovation. Following the extant innovation literature (e.g., Atanassov, 2013; Acharya et al., 2014), we control for Ln (size), Tobin's Q, cash holdings, leverage, ROA, tangible assets, capital expenditures, Ln (firm age), Ln (county population), Ln (per capital personal income), educational attainment, religiosity and local seniors. All the variables are winsorized at the 1st and 99th percentiles.

Finally, we also include industry, $a_h$, and year, $a_t$, fixed effects to control for time-invariant unobservable differences in innovation across industries and time trends, and macroeconomic characteristics that might affect corporate innovation, respectively. The standard errors are adjusted for heteroscedasticity and are clustered at the county level.

[9] Within our conceptual framework, the location of a firm's headquarters matters for corporation innovation. Although firms sometimes have inventors in many jurisdictions, data limitations preclude us from identifying the location of a firm's R&D facilities. This, however, is unlikely to be a problem for our analysis because Howells (1990) and Breschi (2008) suggest that large firms locate their R&D facilities close to the company's headquarters and do not disperse them geographically.

## 3. Empirical analysis

### 3.1. The impact of the opioid epidemic on patents, citations and innovation value

To investigate the impact of the opioid epidemic on corporate innovation, we begin by plotting in Figure (2) the average corporate innovation across opioid epidemic quartiles. The opioid epidemic quartiles are estimated on a yearly basis using the county's opioid mortality rate per 100,000 residents. Across all the measures of corporate innovation, the results show that innovation declines (almost) monotonically from the low to high opioid epidemic quartile groups. For instance, the average firm patent in the lowest group is 29.22 and declines to 15.71 in the highest group. Importantly, a Chi-square test indicates that the difference between low and high groups is statistically significant.

To formalize this evidence in a multivariate setting, we present in Table 2 our instrumental variable (IV) regression results. Model (1) reports the first-stage results. The coefficient estimate of the instrument is positive and significant at the 1% level, suggesting that the supply chain instrument strongly correlates with local opioid abuse. Importantly, we find that the Cragg-Donald Wald F statistic for the weak identification test is comfortably higher (692.612) than the critical value prescribed by Stock and Yogo (2010) (i.e., LIML Size of Nominal 10% Wald, which is 16.38 in our case) and satisfies the relevance condition, allowing us to reject the null of weak identification.

Models (2)-(4) report the second-stage results. The dependent variables are the natural logarithm of the number of patents plus one, the natural logarithm of the number of citations plus one, and the natural logarithm of the innovation's value plus one, respectively. The results show a negative relation between the instrumented opioid mortality rates and corporate innovation, suggesting a causal link between opioid abuse and lower corporate innovation. Economically, the effect of opioid abuse on corporate innovation is substantial. For example, the coefficient estimate

of the instrumented $Ln(Opioid\ Deaths + 1)$ in Model (1) is -0.94%; this implies that moving from the 25th to the 75th percentile of the instrumented opioid mortality leads to 2.27 fewer patents (that is, the difference in the instrumented opioid mortality between 25th and 75th percentiles, which equals 0.242 x (-0.94%) deflated by 0.01). Given the unconditional average number of firm patents, which equals 21.07, the decline in the number of patents for firms in the high group is -10.8%.

Turning to the control variables, the results are generally consistent with prior literature. For instance, firms that are larger, are older, are less profitable, exhibit higher growth opportunities, hold more cash, have more tangible assets and spend more on capital expenditures are more innovative.

We also perform additional tests to ensure our findings are robust to alternative model specifications and variable definitions. Table 3 tabulates the results. Specifically, (i) in Panel A we use alternative measures of corporate innovation, including patent originality, patent generality, the natural logarithm of citations scaled by patents plus one, and the natural logarithm of the number of patents scaled by the number of employees, plus one; (ii) in Panel B we exclude firms that engage in mergers and acquisitions during our sample period, to alleviate concerns that our results are driven by acquired-innovation (Sevilir and Tian, 2012) rather than internally developed in-house innovation; and (iii) in Panel C we exclude the financial crisis period (2008-2009), where neoliberal policies eroded physical and mental well-being (Ruckert and Labonte, 2017) and thus heightened opioid abuse, to avoid the effect of short-term unwillingness of firms to invest in innovation (Archibugi, Filippetti, and Frenz, 2013). None of these tests affects our main results.

Taken together, the results indicate a negative effect of the opioid epidemic on innovation outputs, both in terms of quantity and quality.

### 3.2 Ruling out the Economic Conditions Explanation

While our results do indicate a strong negative relation between opioid abuse and corporate innovation, they could potentially be spurious if our model omits variables that may generate an association between the opioid epidemic and corporate innovation. One such variable that particularly needs further attention is the local economic environment. Specifically, if individuals are more likely to abuse opioids when economic conditions are deteriorating, then our results could be an artefact of the economic environment. Although prior literature supports the view that deteriorating economic conditions have little effect on opioid mortality (see, e.g., Ruhm, 2018) and the opioid epidemic more broadly (see, e.g., Currie and Schwandt, 2020), we further address these concerns as follows.

First, we use adjacent counties as controls, and we employ within-adjacent county variation in opioid abuse. The rationale of this approach rests on the idea that the economic environment within adjacent counties is qualitatively similar. In this respect, there is a plethora of evidence about the importance of geography on knowledge spillovers (Ellison, Glaesser, and Kerr, 2010) and more broadly on the economic environment (e.g., Almeida and Kogut, 1999; Belenzon and Schankerman, 2013). Panel A of Table 4 reports the results. The second-stage results remain qualitatively similar and continue to show a negative and statistically significant relation between the instrumented opioid mortality rates and corporate innovation.

Second, we use the sub-sample of firms with headquarters located in counties that belong to the top tercile of the average per capita income during our sample period. Flourishing economic conditions make a relationship with opioid abuse less plausible. Panel B of Table 4 reports the results. Like the previous results, we find a negative and statistically significant relation between the instrumented opioid mortality rates and corporate innovation; thus, the local economic environment is less likely to explain our results.

### 3.3 Difference-in-differences approach

Thus far, using a supply shock in opioid distribution to instrument opioid mortality, we provide evidence that opioid abuse leads to lower corporate innovation. In this section, we exploit a regulatory supply-side intervention, to provide complementary support to a causal interpretation of the opioid epidemic effects on corporate innovation - this time from the opposite side. Specifically, in response to the unprecedented levels of the opioid epidemic and the severe implications on health and the economy, state regulators enacted tough laws and regulations that substantially limit opioid prescriptions. In March 2016, Massachusetts was the first state to enact legislation limiting the supply of opioids prescribed by doctors to seven days for adults. The legislation also limits opioid prescriptions for minors to seven days. Exceptions include certain circumstances, such as cancer pain, chronic pain and palliative care. Since then, another 37 states have passed similar laws, aiming to limit opioid abuse.[10] To the extent that inventors value the decisiveness of states to fight the opioid epidemic, the passage of these regulations should have a positive impact on corporate innovation.

We further investigate this idea using a difference-in-differences approach to examine how the laws that impose prescription limits affect corporate innovation in affected states. To ensure we have balanced observations, both before and after the state laws, our sample covers the period 2014-2020. If limiting opioid prescriptions leads to increases in corporate innovation, this will be a mirror image of our previous findings, providing further evidence that the opioid epidemic curtails innovation.

We employ pooled ordinary least square regression models with various corporate innovation measures as the dependent variables (akin to Table 2). Table 5 presents the results. In Panel A, the main variable of interest is the *After Law Passage*, which is an indicator variable that equals one when the firm's state of headquarters passed the law, and zero otherwise. In Panel B, the main

[10] Appendix B provides a brief description of the laws.

variables of interest are the *Law Passage*$_{t=-2^-}$, *Law Passage*$_{t=-1}$, *Law Passage*$_{t=0}$, *Law Passage*$_{t=+1}$, *and Law Passage*$_{t=+2^+}$. These are indicator variables that equal one if a firm is headquartered in a state that will pass the law in two years or more, will pass the law in one year, passes the law in the current year, passed the law one year ago, or passed the law two years ago or more, respectively, and zero otherwise. In all regressions, we include firm fixed effects and industry-by-year fixed effects. Because the Law Passage variables are state-level variables, we cluster standard errors at the state level. The coefficient on After Law Passage gauges the effect of changes in state regulation on corporate innovation relative to the innovation of rival firms in unaffected states (i.e., the within firm and industry by year effect). Likewise, the coefficients of Law Passage indicators gauge the evolution of corporate innovation around the passage of the law relative to the innovation of rival firms in unaffected states.

Our identification strategy rests on the assumption that the laws which limit prescriptions provide an exogenous source of variation in the effect of opioid abuse on corporate innovation. Two reasons make us believe that this assumption is valid. First, by enacting such laws, the states do not aim to directly affect corporate innovation. Instead, the states aim to address the problem of over-prescription of powerful opioid pain relievers. Along this vein, at the Massachusetts bill's ceremony, Gov. Baker said "…*The Commonwealth stands in solidarity to fight opioid and heroin epidemic…I am proud to sign this legislation marking a remarkable statewide effort to strengthen prescribing laws…*" Second, the passage of these laws is unrelated to state-level variables that likely affect corporate innovation. Specifically, Ouimet et al. (2021) find that the passage of the laws relates neither to economic environment variables such as unemployment rate, income, poverty ratio, manufacturing ratio and gross domestic product per capita, nor to political economy variables such as whether the Democratic or Republican Party controls the legislation and the government. Therefore, the timing of these laws is plausibly exogenous to corporate innovation.

The results, in Panel A, show that the coefficient of *After Law Passage* is positive and statistically significant. In economic terms, the effect of the passage of laws on corporate innovation is considerable. For instance, the coefficient estimate of *After Law Passage* in Model (1) is -0.144; this represents a 15.5% increase in the number of firm patents. The magnitude is comparable to the -10.8% decline in the number of patents shown in Table 2 (the IV analysis), when the supply shock was positive.

In Panel B, the coefficients on $Law\ Passage_{t=-2^{-}}$ and $Law\ Passage_{t=-1}$ are largely statistically insignificant, rendering a reverse causality explanation unlikely. By contrast, the coefficients on $\text{Law Passage}_{t=0}$, $\text{Law Passage}_{t=+1}$ and $\text{Law Passage}_{t=+2^{+}}$ are positive and generally statistically significant. Therefore, the results suggest that the passage of state laws that aim to limit opioid prescriptions is associated with an increase in corporate innovation. Interestingly, the effect becomes obvious immediately, with the passage of the state laws. Given that opioid addiction recovery is a time consuming and tedious procedure (Hser, Evans, Grella, Ling, and Anglin, 2015), these results make an inventor opioid addiction explanation less plausible.

Overall, the results further support a causal interpretation of opioid epidemic effects on corporate innovation.

## 4. Channel: Inventor mobility

In this section, we investigate a plausible explanation for our results. In particular, we propose an inventor mobility explanation whereby the opioid epidemic can affect the "industrial organization" of innovation by impeding a firm's ability to retain existing (good) inventors and attract new ones. We use the inventor-level data described in Section 2.2.2, which track individual inventors over time across two successive patent applications assigned to different firms. The unit of analysis is at the inventor-employer-year.

Table 6 reports the second-stage results. The dependent variables in models (1) and (2) are an out-of-county move and a within-county move, respectively. As in our baseline analysis, the independent variable of interest is the instrumented opioid deaths, and the instrument is the supply chain MME. Control variables include firm and county characteristics, and industry and year fixed effects. The standard errors are adjusted for heteroscedasticity and are clustered at the county level.

Interestingly, we find a positive relation between the instrumented opioid mortality rates and out-of-county moves, suggesting a causal link between opioid abuse and inventor mobility. In corroboration, we also find a negative relationship between the instrumented opioid mortality rates and within-county moves. Economically, a 1% change in instrumented opioid mortality rates leads to a 4.6% increase in out-of-county moves and a 3.3% decrease in within-county moves.

We next investigate where inventors move in terms of the difference in mortality rates of the county where the new employer is located relative to the county where the previous employer is located. We focus on the subsample of inventor-employer-year that corresponds only to a change in employer, either out-of-county or within-county. In models (3) and (5) the dependent variable reflects moves to a lower crisis county, while models (4) and (6) reflect moves to a higher crisis county. Conditional on moving, the results show that inventors are more (less) likely to move to a lower (higher) crisis county. These results are consistent with an inventor shortage explanation.

Finally, we examine the heterogeneity of these results across inventors. In models (5) and (6) we interact the instrumented opioid mortality rates with a Star & Young inventor indicator. The rationale of this analysis is that, if unsatisfied with their current job, star inventors can find a job more easily than non-star inventors and move elsewhere. Likewise, younger inventors may have lower adjustment costs, including commitments and bonds, to tie them to a specific location and are thus more likely to move elsewhere. Accordingly, the Star & Young inventor variable takes the value of one if (i) the cumulative number of patents filed in the past by an inventor since inception belongs in the top 20% relative to all inventors in that year, and (ii) the time elapsed since the

inventor first filed a patent within a firm is less than five years. The results show that the interaction term of the instrumented opioid mortality rates is positive and significant. Thus, the positive effect of opioid abuse on inventor moves to a lower crisis county is accentuated when inventors are Star and Young. Furthermore, as reflected by the negative sign in the Star & Young variable, in the absence of opioid abuse, Star and Young inventors are less likely to move to a lower crisis county. More broadly, it appears that firms not only lose inventors because of opioid abuse, but they lose productive inventors and youth creativity.

On the contrary, we do not find evidence that Star and Young inventors are associated with a likelihood of moving to a higher crisis county. Thus, attracting inventors remains challenging for firms located in higher crisis counties, and this challenge is unrelated to whether inventors are Star and Young.

In sum, we draw the following conclusions from the previous analyses: (i) the opioid epidemic prompts inventors to move away from firms located in areas with high opioid mortality, (ii) inventors prefer to move to areas which are less affected by the opioid epidemic, and (iii) Star and Young inventors are more likely to be the ones to leave an opioid-affected firm.

## 5. Heterogeneous effects

To shed further light on whether inventor mobility is the channel through which opioid abuse affects corporate innovation, in this section we exploit cross-sectional variation through interaction term variables to investigate how the results vary across firms. In particular, we focus on heterogeneous effects of specific firms in which inventor moves would have smaller (or larger) consequences on innovation and investigate the relationship between opioid mortality rates and corporate innovation.

To perform this analysis, it is necessary to re-estimate the first-stage results. Beyond equation (1), we must also estimate the interaction term of opioid mortality with our moderating variable.

This is because interactions with an endogenous variable, such as opioid mortality, are themselves endogenous (Murnane and Willett, 2011). In order to satisfy the rank condition, following Wooldridge (2002), we add as a new instrument the corresponding interaction between the original instrument (i.e., the supply chain MME) and the moderating variable.[11] Table 7 reports the results.

### 5.1. Importance of employees' input to innovation

The skills and efforts of employees are fundamental inputs of corporate innovation. Thus, inventor mobility is likely to distort the availability of inventors' skills and efforts, curtailing corporate innovation. This problem should be more (less) severe when employees' input to innovation is higher (lower). To investigate this idea, we use the per employee R&D expenses variable to measure the importance of employees' input to innovation. Human capital and labor have an expanding role in entrepreneurship, especially in R&D-intensive industries (Ouimet and Zarutskie, 2014). Thus, per employee R&D expenses should positively correlate to the importance of an employee's input to innovation. Following Chang et al. (2015), we classify firms as having *Low Employee Input* if the ratio of their per employee R&D expenses belongs to the bottom tercile of the sample in a year.

Panel A presents the second-stage regression results. The main variables of interest are *Instrumented Opioid Deaths* and *Instrumented Opioid Deaths x Low Employee Input*. The coefficient on the interaction term gauges the change effect of opioid abuse for firms with *Low Employee Input* relative to the remaining firms (i.e., the coefficient on *Instrumented Opioid Deaths*). The results show a negative relation between the instrumented opioid mortality rates and corporate innovation. Interestingly, the coefficient of the interaction term is positive and statistically

[11] The rank condition states that for every endogenous predictor included in the second stage, there must be at least one instrument included in the first stage.

significant, supporting the view that the negative impact of the opioid epidemic on corporate innovation is lower when the importance of an employee's input to innovation is low.

### 5.2 High growth opportunities

Growth opportunities positively relate to corporate innovation, and inventor mobility could distort this relationship. Specifically, we would expect that inventor mobility will have a weaker (stronger) impact on innovation in firms with low (high) growth opportunities. To explore this idea, we use a firm's growth per employee, defined as the difference between market capitalization and common shareholder's capital, scaled by the number of employees (Chang et al., 2015). We classify firms as having *Low Growth* opportunities if their growth per employee belongs to the bottom tercile of the sample in a year.

Panel B presents the second-stage regression results. The main variables of interest are *Instrumented Opioid Deaths* and *Instrumented Opioid Deaths x Low Growth*. The coefficient on the interaction term gauges the change effect of opioid abuse for firms with *Low Growth* relative to the remaining firms (i.e., the coefficient on *Instrumented Opioid Death*s). Consistent with our expectations, the results show a negative relationship between the instrumented opioid mortality rates and innovation measures, and a positive relationship between the interaction term and innovation measures. These results support the view that opioid abuse has a lower impact on firms with lower growth opportunities.

### 5.3 Project size/complexity

Innovation projects involve a wide range of tasks and processes and are thus rarely handled by one individual. Instead, to enhance innovation, projects are often handled by teams, which may even work together with teams from other departments. Inventor mobility could have important implications for the efficiency of teamwork. For instance, for smaller, less complicated projects

that involve lower levels of technical novelty, less uncertainty and less complexity, mobility should have milder implications for teamwork efficiency. We examine this idea using firm size as a proxy of a firm's ability to handle projects of differing size and complexity.[12] We classify firms as having *Low Ability* to handle larger, more complicated projects when their total assets belong to the bottom tercile of the sample in a year.

Panel C presents the second-stage regression results. The main variables of interest are the *Instrumented Opioid Deaths* and *Instrumented Opioid Deaths x Low Ability*. The coefficient on the interaction term gauges the change effect of opioid abuse for firms with *Low Ability* relative to the remaining firms (i.e., the coefficient on *Instrumented Opioid Death*s). As expected, we find a negative relation between the instrumented opioid mortality rates and corporate innovation, which becomes weaker when firms have lower ability to handle larger, more complicated projects.

Overall, the results in this section, reinforce the conclusion that inventor mobility is a plausible channel for the negative relationship between opioid abuse and corporate innovation.

## 6. Conclusions

Using a corporate innovation setting, this study provides new evidence of spillover effects of the opioid epidemic on firms (i.e., when personal addiction is unlikely to be the main driver). By employing a positive supply shock in opioid distribution to instrument opioid mortality, we show that firms in counties associated with opioid abuse experience declines in patenting, citations and innovation value. Along this vein, using a difference-in-differences analysis which exploits the staggered passage of state-level opioid-limiting legislations, we show that states which passed such laws experience an increase in corporate innovation. Both results support causal effects of opioid abuse on corporate innovation.

[12] Assuming an optimal matching equilibrium, as in Gabaix and Landier (2008), more talented inventors are matched to larger firms. At the same time, in section 4 we find that Star and Young inventors are more likely to move to a lower opioid crisis county. Thus, firm size has the additional advantage of positively correlating with inventor mobility problems.

Consistent with an inventor mobility explanation, we find a positive (negative) relation between opioid abuse and out-of-county (within-county) moves. Conditional on moving, inventors, particularly the star and young ones, move to counties that suffer less from the opioid epidemic. Hence, the opioid epidemic impedes firms' ability to retain existing (good) inventors and attract new ones, resulting in deteriorating corporate innovation.

Finally, heterogeneous analysis reveals that the opioid epidemic is more detrimental for firms when employees' input to innovation is higher, when firms exhibit higher growth opportunities, and when firms handle larger and more complex projects.

Overall, our evidence on the effects of opioid abuse on inventor productivity and mobility is particularly important to firms and policy makers given the centrality of innovation activity for sustaining long-run economic growth and the growing trends of opioid abuse. Therefore, our results have implications related to the measures companies should take to lessen the effects of the opioid epidemic on their employees, and to the economic benefits of implementing public policies that improve the environment in which innovation activity takes place.

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## Appendix A. Variable Definitions

***Dependent variables***

***Ln (#Patents+1)*** - The natural logarithm of the total number of patents that a firm applies for (and are subsequently granted) each year plus one. This variable is created using data from Stoffman, Woeppel, and Yavuz (2022). (see, https://www.mikewoeppel.com/data).

***Ln (#Citations+1)*** - The natural logarithm of the total number of citations obtained on all patents that a firm applies for (and are subsequently granted) each year plus one. This variable is created using data from Stoffman, Woeppel, and Yavuz (2022). (see, https://www.mikewoeppel.com/data).

***Ln (Innovation Value+1)*** - The natural logarithm of the cumulative dollar value of patents (in millions of 2005 nominal U.S. dollars) that a firm applies for each year plus one. A patent's value is measured as the firm stock return in excess of the market over the three-day window around the date of patent approval (*t, t+2*), multiplied by the firm's market capitalization on the day prior to the announcement of the patent issuance. The dollar value of each patent is obtained from Stoffman, Woeppel, and Yavuz (2022). (see, https://www.mikewoeppel.com/data).

***Originality*** - One minus the Herfindahl concentration index of the number of cited patents across technological classes. We use the bias correction of the Herfindahl measures, described in Jaffe and Trajtenberg (2002), to account for cases with a small number of patents within technological categories. This variable is created using data from the NBER patent database *(https://www.nber.org/patents/)* and Bhaven Sampat's United States Patent and Trademark Office (USPTO) patent and citation database. (see, http://thedata.harvard.edu/dvn/dv/boffindata).

***Generality*** - One minus the Herfindahl concentration index of the number of patents citing across technological classes. We use the bias correction of the Herfindahl measures, described in Jaffe and Trajtenberg (2002), to account for cases with a small number of patents within technological categories. This variable is created using data from the NBER patent database *(https://www.nber.org/patents/)* and Bhaven Sampat's United States Patent and Trademark Office (USPTO) patent and citation database. (see, http://thedata.harvard.edu/dvn/dv/boffindata).

***Ln (#Citations/ #Patents+1)*** - The natural logarithm of citations obtained on all patents that a firm applies for (and are subsequently granted) scaled by the total number of patents that a firm applies for (and are subsequently granted) each year plus one. This variable is created using data from Stoffman, Woeppel, and Yavuz (2022). (see, https://www.mikewoeppel.com/data).

***Ln (#Patents/ Employees+1)*** - The natural logarithm of the number of patents per 1,000 firm employees (EMP) plus one. This variable is created using data from Mike Woeppel's Website and Compustat.

***Out-of-County Move*** - An indicator variable that takes the value of one if an inventor moves to another employer located in a different county, and zero otherwise. This variable is created using data from Stoffman, Woeppel, and Yavuz (2022). (see, https://www.mikewoeppel.com/data).

***Within County Move*** - An indicator variable that takes the value of one if an inventor moves to another employer located in the same county as her previous employer, and zero otherwise. This variable is created using data from Stoffman, Woeppel, and Yavuz (2022). (see, https://www.mikewoeppel.com/data).

***Move to a Lower Crisis County*** – It is based on the difference in drug overdose mortality rate per 100,000 residents of the county where the former employer is located relative to the county where the previous employer is located. An indicator variable that takes the value of one when the difference in mortality rates is above the 75$^{th}$ percentile of this difference for the moving inventors. The data are from NCIPC.

***Move to a Higher Crisis County*** - It is based on the difference in drug overdose mortality rate per 100,000 residents of the county where the former employer is located relative to the county where the previous employer is located. An indicator variable that takes the value of one when the difference in mortality rates is below the $25^{th}$ percentile of this difference for the moving inventors. The data are from NCIPC.

## ***Instrument***

***Ln (MME)*** **-** The natural logarithm of the per-capita Morphine Milligram Equivalent (MME) of strong opioids (i.e., those with MME > 1) dispensed by non-chain pharmacies in a given county-year per 100,000 residents. The data are from DEA.

## ***Firm variables***

***Opioid Deaths*** - The number of deaths caused by drug overdose per 100,000 county residents. The data are from NCIPC.

***Ln (Size)*** - The natural logarithm of total assets (AT). This variable is created using data from Compustat.

***Tobin's Q*** - The market value of equity (CSHO*PRCC_F) plus book value of assets (AT) minus book value of equity (CEQ) minus balance sheet deferred taxes (TXDB), scaled by total assets (AT). This variable is created using data from Compustat.

***Cash Holdings*** - Cash and short–term investments (CHE) scaled by total assets (AT). This variable is created using data from Compustat.

***Leverage*** - The sum of long-term debt (DLTT) and debt in current liabilities (DLC) scaled by total assets (AT). This variable is created using data from Compustat.

***ROA*** **-** Income before extraordinary items (IB) plus interest expense (item XINT) plus income taxes (item XINT), divided by total assets (item AT). This variable is created using data from Compustat.

***Tangible Assets*** - Property, plant, and equipment (PPEGT) scaled by total assets (AT). This variable is created using data from Compustat.

***Capital Expenditures*** - Capital expenditures (CAPX) scaled by total assets (AT). This variable is created using data from Compustat.

***Ln (Firm Age)*** - The natural logarithm of one plus the number of years since the firm's first appearance in the Center for Research in Security Prices (CRSP). This variable is created using data from CRSP.

***Low Employee Input*** – An indicator variable that takes the value of one if the firm's ratio of R&D per employee belongs to the bottom tercile of the sample in a year, and zero otherwise. This variable is created using data from Compustat.

***Low Ability –*** An indicator variable that takes the value of one if the firm's size measured by total assets belongs to the bottom tercile of the sample in a year, and zero otherwise. This variable is created using data from Compustat.

***Low Growth –*** An indicator variable that takes the value of one if the firm's market capitalization minus common shareholder's interest in the firm, scaled by the number of employees, belongs to the bottom tercile of the sample in a year, and zero otherwise. This variable is created using data from Compustat.

## ***County Variables***

***Ln (Population)*** **-** The natural logarithm of the county-level population. This variable is created using data from the Census Bureau.

***Ln (Income Per Capita)*** **–** The natural logarithm of the county-level income per capita. This variable is created using data from the U.S. Bureau of Economic Analysis (BEA).

***Education*** **–** The ratio of people with a bachelor's degree or higher to the population aged 25 years or older in one county. This variable is created using data from the Census Bureau.

***Religion*** **-** The portion of a county's population whose residents adhere to any religion in the county where the firm is headquartered. This variable is created using the "Churches and Church Membership" files from the U.S. Association of Religion Data Archives (ARDA).

***Seniors*** **–** The portion of residents of a county's population who are aged 65 or older. This variable is created using data from the Census Bureau.

## ***Inventor variable***

***Star & Young Inventor*** - An indicator variable that takes the value of one (a) if the cumulative number of patents filed in the past by an inventor since inception belongs in the top 20% relative to all inventors in that year, and (b) the time elapsed since the inventor first filed a patent within a firm is less than 5 years, and zero otherwise. This variable is created using data from Stoffman, Woeppel, and Yavuz (2022). (see, https://www.mikewoeppel.com/data).

## Appendix B. List of State Laws

As of January 2022, 38 states had implemented policies or guidelines setting limits on the supply of opioids that can be prescribed by doctors. The opioid limits in five of those states only apply to Medicaid recipients. Two of those states have no set pill or day limit for opioid prescriptions but require doctors to prescribe the lowest effective dose. We list a brief description of these laws below.

***Alabama (2018):*** Law that limits ***s***hort-acting prescription opioids for patients without an opioid claim history in the past 180 days. The policy established a maximum supply of seven days for adults and five days for children, with a 50 morphine milligram equivalent (MME) limit per day. The AMA implemented a policy on August 1, 2019, to decrease the daily cumulative MME limit by 50 MME every four months until it reached 90 MME per day.

***Alaska (2017):*** Law that limits first-time opioid prescriptions to a maximum of a seven-day supply with exceptions for chronic pain patients, cancer patients, palliative care patients, and patients that are unable to access a practitioner to obtain a prescription refill due to travel or logistic barriers.

***Arizona (2016):*** *Law that sets* opioid prescription limits for anyone insured under the state's Medicaid program or the state's employee insurance plan. The order limits the first fill prescription of opioids for adults to seven days. No opioid prescriptions are permitted for children unless they have cancer, a chronic disease, or suffer a traumatic injury.

***Arizona (2018):*** Law that limits the first-fill prescription of adults to five days and aligned state dosage levels with federal guidelines. Other measures taken by the law include a $10 million investment to assist in improving access to treatment, an expanded law enforcement's access to Naloxone, a drug used to reverse overdoses, the continuing medical education for opioid prescribers, and the requirement for e-prescribing.

***Colorado (2017):*** Law that reduced opioid prescriptions in the state's Medicaid program. The policy was rolled out in two phases. The first phase went into effect on August 1, 2017, and limits the supply of opioids a Medicaid recipient can receive if they did not have an opioid prescription in the past 12 months. The policy limits the first fill prescription of opioids to a seven day supply. Patients can get two more seven-day prescriptions, but a fourth prescription would have to be approved by the department. Medicaid recipients that already had an opioid prescription before the new policy will still be allowed up to a 30-day prescription. The second phase went into effect on October 1, 2017, and reduced the morphine milligram equivalents (MME) from Medicaid patients from 300 MME per day to 250 MME per day. The new MME limit does not apply to patients being treated for cancer or in palliative or hospice care.

***Colorado (2018):*** Law that implemented a seven-day initial limit on opioid prescriptions, with prescribers allowed to write another seven-day prescription if they consider it necessary. The bill included exemptions for patients experiencing chronic pain, cancer patients, patients under hospice care, and patients experiencing post-surgical pain expected to last longer than 14 days.

***Connecticut (2016):*** Law that limits opioid prescriptions for new adult patients to seven days and limits opioid prescriptions to minors to seven days, with certain exceptions for prescribers' professional medical judgments.

***Connecticut (2017):*** Law that limits opioid prescription for minors to five days and requires electronic prescribing of controlled substances.

***Delaware (2017):*** Law that limits the first fill prescription of opioids to seven days for adults. It also limits opioid prescriptions for minors to seven days. Prescriptions can exceed seven days if a doctor determines that the patient requires it. If the doctor deems that a larger supply is necessary,

the patient must undergo a physical exam, be educated about the dangers of opioid abuse, and the doctor must examine the patients prescription history.

***Florida (2018):*** Law that limits initial opioid prescriptions to three days for acute pain, with exceptions for trauma, chronic pain, cancer, or terminal ill patients.

***Hawaii (2017):*** Law that limits initial opioid and benzodiazepines prescriptions to seven days, with exceptions for cancer, chronic pain, trauma, and palliative care patients.

***Indiana (2017):*** Law that limits initial opioid prescriptions for adults to seven days and limits opioid prescriptions for minors to seven days, with exceptions for chronic pain, cancer, or palliative care patients.

***Iowa (2017):*** Law that requires doctors to register for and use prescription monitoring program, establishes criteria for recognizing patients at high risk for abuse and addiction, requires electronic prescribing, and allows licensing boards to evaluated penalties for healthcare providers who overprescribe.

***Kentucky (2017):*** Law that limits opioid prescription to seven days for new patients with exemptions for cancer patients, diagnosed chronic pain, and end-of-life care.

***Louisiana (2017):*** Law that limits initial opioid prescriptions to seven days with exceptions for chronic pain, cancer, or palliative care patients.

***Maine (2016):*** Law LD1646 that limits opioid prescriptions to seven days for acute pain, 30 days for chronic pain, and sets an opioid amount limit of a maximum of 100 MME per day. This law exempts cancer, hospice and palliative care patients, and patients in treatment for a substance abuse disorder.

***Maine (2017)*****:** Law LD1031 clarifies that chronic pain patients are exempt from the maximum limit of 100 MME per day.

***Maryland (2017):*** Law that limits initial opioid prescriptions for adults to seven days and limits opioid prescriptions for minors to seven days, with exceptions for chronic pain, cancer, or palliative care patients.

***Massachusetts (2016):*** Law that limits initial opioid prescriptions for adults to seven days and limits opioid prescriptions for minors to seven days, with exceptions for chronic pain, cancer, or palliative care patients. This law includes other provisions such as requiring information on opiate-use and misuse be disseminated at the annual head injury safety programs for high-school athletes, doctors to check the Prescription Monitoring Program (PMP) database before writing a prescription for a Schedule 2 or Schedule 3 narcotic, and continuing education requirements for prescribers.

***Michigan (2017):*** Law that limits opioid prescription to seven days for acute pain patients, with exceptions for chronic pain patients.

***Minnesota (2017):*** Law that limits opioid prescriptions to four days for acute pain due to dental or ophthalmic pain and allows healthcare providers to use their judgment if a larger opioid quantity is needed.

***Missouri (2017):*** Law that limits initial opioid prescriptions to seven days.

***Nebraska (2016):*** Law that limits Medicaid recipients to 150 doses of short-acting opioids in 30 days.

***Nebraska (2018):*** Law that limits opioid prescriptions to seven days for those under the age of 19, directs physicians to discuss risk of addiction with patients, and requires a photo ID for persons receiving dispensed opiates.

***Nevada (2017):*** Law that limits opioid prescriptions to 90 morphine milligram equivalent (MME) per day and limits initial opioid prescriptions to 14 days for acute pain. This law requires additional evaluation if patient requires more than 30 days of opioids.

***New Hampshire (2016):*** Law that prevents medical professionals in an emergency room, urgent care setting, or walk-in clinic from prescribing more than seven days of opioids and requires pain patients be prescribed the lowest effective dose of pain medications. The law requires the state Board of Medicine, the state Board of Dental Examiners, the state Board of Nursing, the state Board of Registration in Optometry, the state Board of Podiatry, the state Naturopathic Board of Examiners, and the state Board of Veterinary Medicine to adopt rules for prescribing controlled drugs.

***New Jersey (2017):*** Law that limits initial opioid prescriptions to five days for acute pain patients. Cancer, hospice care, and long-term care facility patients are exempt. This law does not apply to medications prescribed for treatment of substance abuse.

***New York (2016):*** Law that limits initial opioid prescriptions to seven days for acute pain patients. Cancer, chronic pain, hospice care, and palliative care patients are exempt. This law requires insurers to cover initial inpatient drug treatment without prior approval, extend the time to 72 hours a person can be held for emergency treatment and increase addiction treatment slots.

***North Carolina (2017):*** Law that limits initial opioid prescriptions to five days for acute pain patients and seven days for post-operative patients. It allows for exemptions for cancer patients, chronic pain, hospice and palliative care, or medications prescribed for the treatment of substance use disorders. It increases access to naloxone, requires prescribers and pharmacies to check the prescription database before prescribing opioids to patients, and strengthens oversight of opioid prescriptions.

***North Dakota (2021):*** Law that imposes a seven-day limit, and 90 morphine milligram equivalents (MME) during the first 30-days of treatment.

***Ohio (2017):*** Law that limits opioid prescriptions to no more than a seven-day prescription for adults with acute pain. Minors may be prescribed no more than a five-day prescription with written consent by a parent or guardian. The rules also limit patients with acute pain to an average of 30 morphine equivalent dose (MMD) per day.

***Oklahoma (2018):*** Law that limits initial opioid prescription to seven days for new patients with exemptions for cancer, hospice and palliative care patients.

***Pennsylvania (2016):*** Pennsylvania Senate Bill 1367 is signed into a law that limits emergency departments and urgent care centers from prescribing more than a seven-day supply of opioids and from writing refills for opioid prescriptions. Signed into a law, Pennsylvania House Bill 1699 limits opioid prescriptions to seven days for minors with acute pain. The legislation provides medical professionals with flexibility to prescribe more if needed to stabilize acute pain. Cancer, chronic pain, hospice and palliative care patients are exempt.

***Rhode Island (2016):*** Rhode Island Senate Bill 2823 and House Bill 8224 that limit initial opioid prescriptions for acute pain to 30 morphine milligram equivalents per day, for a maximum of 20 doses. Cancer, chronic pain, long term, hospice and palliative care patients are exempt.

***Rhode Island (2019):*** Law that limits first-time opioid prescriptions for adults to a seven-day supply and puts the same limit on all prescriptions for minors.

***South Carolina (2018):*** Law that limits initial opioid prescriptions for acute pain or post-operative pain to the lowest effective dose. The state limited the supply to five days or 90 morphine milligran equivalents (MMEs) per day, except for cases of chronic pain, cancer pain, sickle cell disease-related pain, pallliative care, or medicatio-assisted treatment for substance abuse disorder.

***Tennessee (2018):*** Law that established a three-day opioid supply limit.

***Texas (2019):*** Law that limits opioid prescriptions for acute pain to a ten-day supply, as well as prohibiting refill of opioid prescriptions.

***Utah (2017):*** Law that limits initial opioid prescriptions to seven days for new acute pain patients with exemptions for cancer, hospice and palliative care patients.

***Vermont (2017):*** Law that requires doctors must assess non-opioid and non-drug treatments before prescribing opioids.

***Virginia (2017):*** Law that breaks down the prescribing limits for opiates between acute pain and chronic pain. The regulations define acute pain as pain that lasts less than three months. If a medical professional determines that opioids are required for acute pain, the regulations limit the provider from prescribing more than a seven-day supply. Patients prescribed opioids for post-surgical pain are limited to a 14-day supply. For acute pain and pain related to surgery, medical professional may not prescribe a longer supply unless "extenuating circumstances are clearly documented in the medical record.

***Washington (2017):*** Law that limits opioid prescriptions to 42 tablets for Medicaid patients and 18 tablets for Medicaid patients under the age of 20. Cancer, chronic pain, hospice and palliative care patients are exempt.

***West Virginia (2018):*** Law that limits initial opioid prescriptions to seven days for acute pain, four days for emergency room prescriptions, and three days if prescribed by a dentist or optometrist. Cancer, hospice, long-term care and palliative care patients are exempt.

**Figure 1. Geographic Distribution of Opioid Mortality by County**

This figure plots the opioid mortality rates per 100,000 across counties in 2014. The opioid mortality rates are from the Multiple Cause of Death data found in the NCIPC of the CDC. Mortality rates are identified using the ICD-10 underlying cause-of-death codes X40-X44 (unintentional overdose), X60-X64 (suicide by drug self-poisoning), X85 (homicide by drug poisoning), or Y10-Y14 (undetermined intent).

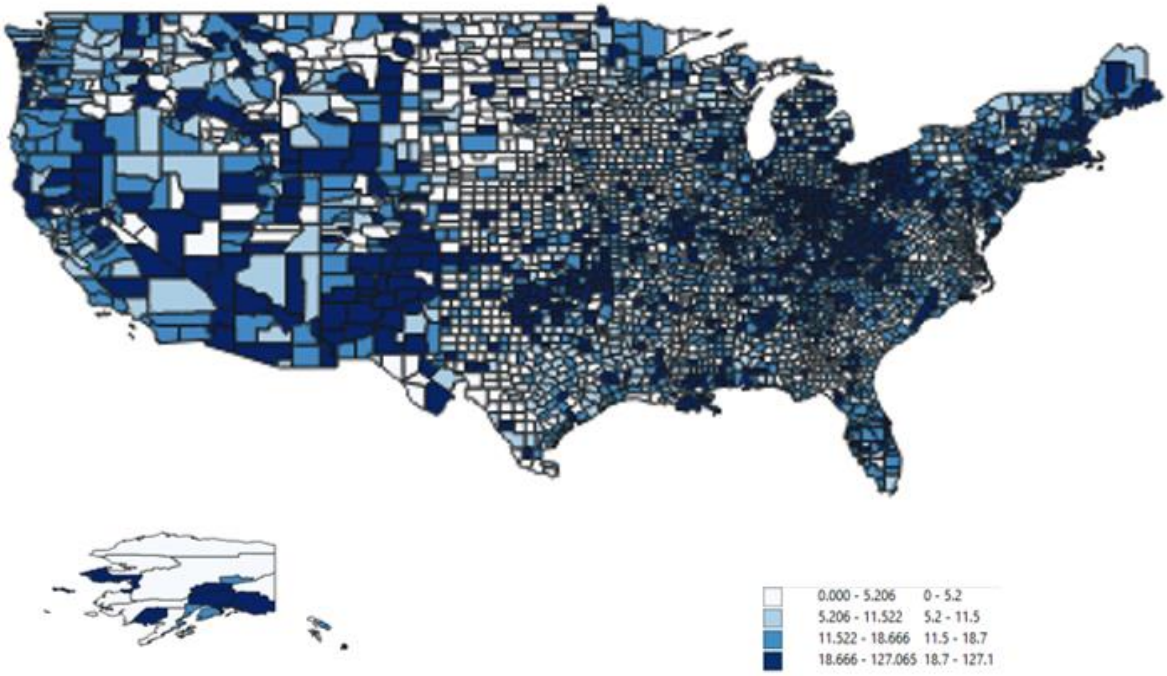
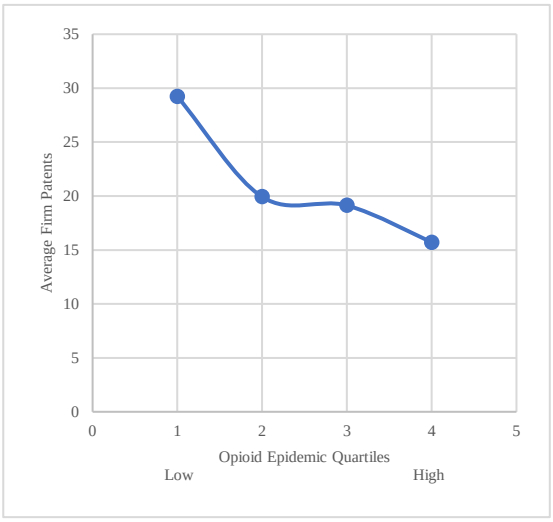
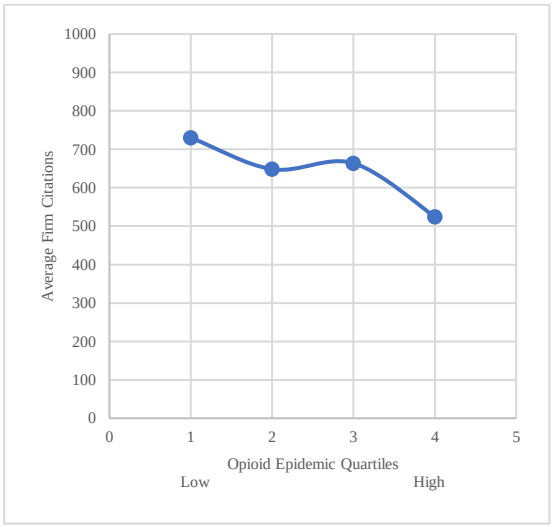


**Figure 2. Average Corporate Innovation by Opioid Epidemic Quartiles**

This figure plots the average number of firm patents, average number of firm citations, and average patent value across opioid epidemic quartiles. The opioid epidemic quartiles are estimated on a yearly basis using the county's opioid mortality rate per 100,000 residents. The sample consists of U.S. innovative firms and covers the period 2007-2015. Appendix A provides detailed definitions of the corporate innovation variables.



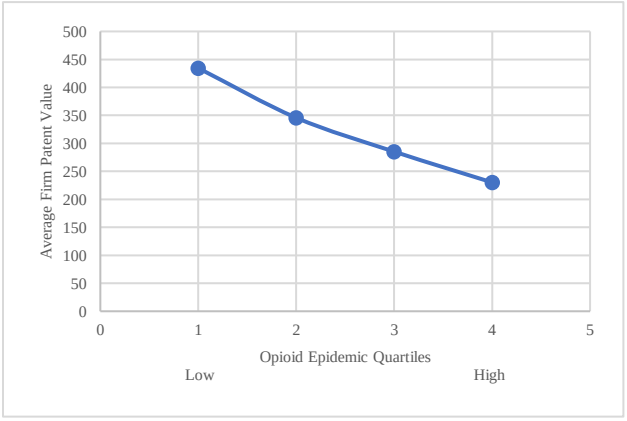



**Table 1. Summary Statistics**

This table reports summary statistics for our sample of U.S. innovative firms over the period 2007-2015. Panel A presents summary statistics for the firm level sample. Panel B presents summary statistics for the inventor-level sample. Appendix A provides detailed definitions of all variables.

| | Obs. | Mean | Std. dev. | p10 | p50 | p90 |
|---|---|---|---|---|---|---|
| **Panel A: Firm-Level Sample** | | | | | | |
| | | | *Dependent Variables* | | | |
| #Patents | 15,178 | 21.071 | 83.472 | 0.000 | 0.000 | 34.000 |
| #Citations | 15,178 | 642.930 | 2,529.963 | 0.000 | 0.000 | 993.000 |
| Innovation Value ($M) | 15,178 | 324.337 | 1,435.306 | 0.000 | 0.000 | 327.806 |
| | | | *Firm-Level Variables* | | | |
| Size | 15,178 | 5.119 | 2.433 | 1.870 | 5.061 | 8.334 |
| Tobin's Q | 15,178 | 5.177 | 14.231 | 0.997 | 1.928 | 7.227 |
| Cash Holdings | 15,178 | 0.330 | 0.277 | 0.032 | 0.250 | 0.781 |
| Leverage | 15,178 | 0.351 | 1.108 | 0.000 | 0.098 | 0.553 |
| ROA | 15,178 | -0.548 | 2.077 | -1.069 | -0.001 | 0.156 |
| Tangible Assets | 15,178 | 0.375 | 0.359 | 0.053 | 0.269 | 0.826 |
| Capital Expenditures | 15,178 | 0.033 | 0.039 | 0.003 | 0.022 | 0.074 |
| Firm Age | 15,178 | 16.284 | 12.800 | 2.000 | 13.000 | 37.000 |
| | | | *County-Level Variables* | | | |
| Population | 15,178 | 1,575,816 | 1,775,471 | 299,134 | 1,048,985 | 3,114,363 |
| Income Per Capita ($) | 15,178 | 56,187.43 | 19,553.38 | 38,031 | 51,460 | 75,455 |
| Education | 15,178 | 0.385 | 0.097 | 0.272 | 0.383 | 0.508 |
| Religion | 15,178 | 0.557 | 0.129 | 0.409 | 0.545 | 0.750 |
| Seniors | 15,178 | 0.124 | 0.025 | 0.094 | 0.122 | 0.152 |
| **Panel B: Inventor-Level Sample** | | | | | | |
| | | | *Dependent Variables* | | | |
| #Patents | 25,808 | 2.556 | 3.036 | 1.000 | 2.000 | 5.000 |
| #Citations | 25,808 | 108.616 | 478.086 | 2.000 | 20.000 | 202.000 |
| Innovation Value ($M) | 25,808 | 52.730 | 125.148 | 3.067 | 18.710 | 119.926 |

**Table 2. Opioid Epidemic and Corporate Innovation**

This table presents two-stage instrumental variables (IV) regression results on the effect of the opioid epidemic on corporate innovation. Column (1) presents first-stage regression results, while columns (2) to (4) present second-stage regression results. In the first stage, we regress the variable Opioid Death on a Supply Chain instrument as well as on firm and county control variables. The variable, Opioid Deaths, is the number of deaths caused by drug overdose per 100,000 county residents (source: NCIPC data). The Supply Chain instrument is the per-capita Morphine Milligram Equivalent (MME) of strong opioids (i.e., those with MME > 1) dispensed by non-chain pharmacies in a given county-year. The dependent variables in columns (2)-(4) are the natural logarithm of the number of patent counts plus one, the natural logarithm of citation counts plus one, and the natural logarithm of the cumulative dollar value (in millions of 2005 nominal U.S. dollars) of patents that a firm applies for in a given year plus one, respectively. All control variables are lagged by one year. Appendix A provides detailed definitions of all variables. All models include year and industry fixed effects, whose coefficients are suppressed, and are based on calendar year and 2-digit SIC industry classification, respectively. Standard errors are adjusted for heteroscedasticity and are clustered at the county level. P-values are reported in parentheses below the coefficient estimates. The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

| | **First Stage** | **Second Stage** | | |
|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** |
| | **Ln (Opioid Deaths)** | **Ln (#Patents+1)** | **Ln (#Citations+1)** | **Ln (Innovation Value+1)** |
| Ln (MME) | 0.042*** | | | |
| | (0.003) | | | |
| Instrumented Opioid Deaths | | -0.940** | -2.057** | -1.426** |
| | | (0.040) | (0.021) | (0.042) |
| Ln (Size) | -0.002 | 0.486*** | 0.839*** | 0.815*** |
| | (0.672) | (0.000) | (0.000) | (0.000) |
| Tobin's Q | 0.001*** | 0.009*** | 0.015*** | 0.018*** |
| | (0.009) | (0.000) | (0.000) | (0.000) |
| Cash Holdings | -0.036 | 0.738*** | 1.653*** | 0.932*** |
| | (0.238) | (0.000) | (0.000) | (0.000) |
| Leverage | 0.000 | 0.021 | 0.018 | 0.041** |
| | (0.939) | (0.112) | (0.432) | (0.042) |
| ROA | 0.008** | -0.057*** | -0.062*** | -0.082*** |
| | (0.011) | (0.000) | (0.000) | (0.000) |
| Tangible Assets | -0.005 | 0.178*** | 0.298*** | 0.219** |
| | (0.741) | (0.006) | (0.010) | (0.024) |
| Capital Expenditures | 0.265* | 0.937** | 2.516*** | 1.896*** |
| | (0.063) | (0.029) | (0.002) | (0.006) |
| Ln (Firm Age) | -0.005 | 0.134*** | 0.203*** | 0.205*** |
| | (0.536) | (0.000) | (0.000) | (0.000) |
| Ln (Population) | 0.007 | 0.116* | 0.207* | 0.176* |
| | (0.862) | (0.068) | (0.09) | (0.060) |
| Ln (Income Per Capita) | -0.334** | -0.384 | -1.018* | -0.446 |
| | (0.018) | (0.177) | (0.057) | (0.277) |
| Education | -0.228 | -0.073 | -0.117 | -0.231 |
| | (0.650) | (0.922) | (0.938) | (0.835) |
| Religion | -0.085 | -1.127*** | -2.018*** | -1.682*** |
| | (0.669) | (0.000) | (0.000) | (0.000) |
| Seniors | 4.108*** | 4.615* | 9.662* | 7.721* |
| | (0.000) | (0.080) | (0.078) | (0.065) |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Observations | 15,178 | 15,178 | 15,178 | 15,178 |
| *Adjusted* $R^2$ | 0.268 | 0.497 | 0.449 | 0.548 |

**Table 3. Opioid Epidemic and Corporate Innovation: Robustness Checks**

This table presents two-stage instrumental variable (IV) regression results on the effect of the opioid epidemic on corporate innovation. For brevity, the table reports only the second-stage regression results. Panel A presents results using alternative measures of innovation. Panel B presents results after excluding firms which engage in an M&A during our sample period. Panel C presents results after excluding the financial crisis (2008-2009) period. Control variables are the same as in Table 2. All control variables are lagged by one year. Appendix A provides detailed definitions of all variables. All models include year and industry fixed effects, whose coefficients are suppressed, and are based on calendar year and 2-digit SIC industry classification, respectively. Standard errors are adjusted for heteroscedasticity and are clustered at the county level. *P*-values are reported in parentheses below the coefficient estimates. The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

| **Panel A: Alternative measures of innovation** | | | | |
|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** |
| | **Originality** | **Generality** | **Ln (#Citations/(#Patents+1)** | **Ln (#Patents/Employees+1)** |
| Instrumented Opioid Deaths | -0.213** | -0.140** | -1.187** | -1.108** |
| | (0.016) | (0.035) | (0.026) | (0.012) |
| Controls | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Observations | 15,178 | 15,178 | 15,178 | 14,928 |
| *Adjusted* $R^2$ | 0.257 | 0.179 | 0.310 | 0.248 |

| **Panel B: Excluding M&A firms** | | | |
|---|---|---|---|
| | **(1)** | **(2)** | **(3)** |
| | **Ln (#Patents+1)** | **Ln (#Citations+1)** | **Ln (Innovation Value+1)** |
| Instrumented Opioid Deaths | -0.975** | -2.093*** | -1.405** |
| | (0.045) | (0.007) | (0.039) |
| Controls | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Observations | 6,831 | 6,831 | 6,831 |
| *Adjusted* $R^2$ | 0.291 | 0.245 | 0.347 |

| **Panel C: Excluding financial crisis period** | | | |
|---|---|---|---|
| | **(1)** | **(2)** | **(3)** |
| | **Ln (#Patents+1)** | **Ln (#Citations+1)** | **Ln (Innovation Value+1)** |
| Instrumented Opioid Deaths | -0.965** | -2.104** | -1.414** |
| | (0.037) | (0.017) | (0.039) |
| Controls | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Observations | 11,711 | 11,711 | 11,711 |
| *Adjusted* $R^2$ | 0.524 | 0.400 | 0.524 |

**Table 4. Opioid Epidemic and Corporate Innovation: Ruling out the Economic Conditions Explanation**

This table presents two-stage instrumental variable (IV) regression results on the effect of the opioid epidemic on corporate innovation. For brevity, the table reports only the second-stage regression results. Panel A presents results by utilizing variation of the opioid epidemic within adjacent counties. Panel B presents results using firms located in counties ranked in the top tercile of per capital income. Control variables are the same as in Table 2. All control variables are lagged by one year. Appendix A provides detailed definitions of all variables. All models include year and industry fixed effects, whose coefficients are suppressed, and are based on calendar year and 2-digit SIC industry classification, respectively. Standard errors are adjusted for heteroscedasticity and are clustered at the county level. *P*-values are reported in parentheses below the coefficient estimates. The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

| **Panel A: Within adjacent counties** | | | |
|---|---|---|---|
| | **(1)** | **(2)** | **(3)** |
| | **Ln (#Patents+1)** | **Ln (#Citations+1)** | **Ln (Innovation Value+1)** |
| Instrumented Opioid Deaths | -0.925** | -1.802** | -1.511** |
| | (0.044) | (0.029) | (0.028) |
| Controls | Yes | Yes | Yes |
| Adjacent County Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Observations | 101,056 | 101,056 | 101,056 |
| *Adjusted* $R^2$ | 0.541 | 0.491 | 0.586 |
| **Panel B: Top tercile counties based on per capita income** | | | |
| | **(1)** | **(2)** | **(3)** |
| | **Ln (#Patents+1)** | **Ln (#Citations+1)** | **Ln (Innovation Value+1)** |
| Instrumented Opioid Deaths | -1.551** | -2.713** | -2.443** |
| | (0.020) | (0.032) | (0.015) |
| Controls | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Observations | 11,734 | 11,734 | 11,734 |
| *Adjusted* $R^2$ | 0.454 | 0.398 | 0.503 |

**Table 5. Opioid Epidemic and Corporate Innovation: Limiting Opioid Prescriptions**

This table presents diff-in-diff results on the effect of state-level limiting opioid prescriptions regulations on corporate innovation. The dependent variables in columns (1) to (3) are the natural logarithm of the number of patent counts plus one, the natural logarithm of citation counts plus one, and the natural logarithm of the cumulative dollar value (in millions of 2005 nominal U.S. dollars) of patents that a firm applies for in a given year plus one, respectively. Panel A reports results after the passage of the regulations. The variable After Law Passage is an indicator variable which equals 1 after the passage of the regulation, and zero otherwise. Panel B reports results about the evolution of the effect around the passage of the regulation. The variable Law Passage is an indicator variable that equals one for certain years before or after a states passes a regulation that limits opioid prescription, and zero otherwise. The control variables are the same as in Table 2. All control variables are lagged by one year. The data cover the period 2014-2020. Appendix A provides detailed definitions of all variables. All models include industry-by-year fixed effects, whose coefficients are suppressed, and are based on calendar year and 2-digit SIC industry classification, respectively. Standard errors are adjusted for heteroscedasticity and are clustered at the state level. *P*-values are reported in parentheses below the coefficient estimates. The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

| **Panel A: After law passage effect** | | | |
|---|---|---|---|
| | **(1)** | **(2)** | **(3)** |
| | **Ln (#Patents+1)** | **Ln (#Citations+1)** | **Ln (Innovation Value+1)** |
| After Law Passage | 0.144** | 0.232*** | 0.196*** |
| | (0.023) | (0.007) | (0.001) |
| Controls | Yes | Yes | Yes |
| Firm Fixed Effects | Yes | Yes | Yes |
| Industry x Year Fixed Effects | Yes | Yes | Yes |
| Observations | 9,724 | 9,724 | 9,724 |
| *Adjusted* $R^2$ | 0.872 | 0.819 | 0.877 |

| **Panel B: Evolution of the law passage effect** | | | |
|---|---|---|---|
| | **(1)** | **(2)** | **(3)** |
| | **Ln (#Patents+1)** | **Ln (#Citations+1)** | **Ln (Innovation Value+1)** |
| Law Passage $_{t = -2^-}$ | -0.049 | 0.158 | 0.073 |
| | (0.518) | (0.342) | (0.553) |
| Law Passage $_{t = -1}$ | 0.041 | 0.365** | 0.157 |
| | (0.610) | (0.035) | (0.180) |
| Law Passage $_{t = 0}$ | 0.097 | 0.407** | 0.239** |
| | (0.226) | (0.010) | (0.033) |
| Law Passage $_{t= +1}$ | 0.157* | 0.505*** | 0.327*** |
| | (0.065) | (0.002) | (0.004) |
| Law Passage $_{t = +2^+}$ | 0.248* | 0.670*** | 0.448*** |
| | (0.054) | (0.001) | (0.003) |
| Controls | Yes | Yes | Yes |
| Firm Fixed Effects | Yes | Yes | Yes |
| Industry x Year Fixed Effects | Yes | Yes | Yes |
| Observations | 9,724 | 9,724 | 9,724 |
| *Adjusted* $R^2$ | 0.873 | 0.819 | 0.878 |

**Table 6. Opioid Epidemic and Inventor Mobility: Inventor Level Analysis**

This table presents two-stage instrumental variable (IV) regression results on the effect of the opioid epidemic on inventor mobility using inventor-level data. For brevity, the table reports only the second-stage regression results. The dependent variables are: an indicator variable that takes the value of one if an inventor moves to another employer located in a different county, and zero otherwise (in column (1)); an indicator variable that takes the value of one if an inventor moves to another employer located in the same county with the previous employer, and zero otherwise (in column (2)); an indicator variable that takes the value of one if an inventor moves to an employer located in a lower-crisis county (the top 25% moves, based on the difference in the number of deaths caused by drug overdose per 100,000 county residents of the current vs the previous county of the inventor), and zero otherwise (in columns (3) and (5)); an indicator variable that takes the value of one if an inventor moves to an employer located in a higher-crisis county (the bottom 25% moves, based on the difference in the number of deaths caused by drug overdose per 100,000 county residents of the current vs the previous county of the inventor), and zero otherwise (in columns (4) and (6)). Star & Young inventors is an indicator variable that takes the value of one (a) if the cumulative number of patents filed in the past by an inventor since inception belongs in the top 20% relative to all inventors in that year, and (b) the time elapsed since the inventor first filed a patent within a firm is less than 5 years, and zero otherwise. Control variables the same as in Table 2. All control variables are lagged by one year. Appendix A provides detailed definitions of all variables. All models include year and industry fixed effects, whose coefficients are suppressed, and are based on calendar year and 2-digit SIC industry classification, respectively. Standard errors are adjusted for heteroscedasticity and are clustered at the county level. P-values are reported in parentheses below the coefficient estimates. The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

| | **All Inventors** | | | **Only Moving Inventors** | | |
|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| | **Out-of-County Move** | **Within County Move** | **Move to a Lower Crisis County** | **Move to a Higher Crisis County** | **Move to a Lower Crisis County** | **Move to a Higher Crisis County** |
| Instrumented Opioid Deaths | 0.046* | -0.033** | 0.963*** | -0.548*** | 0.952*** | -0.550*** |
| | (0.053) | (0.016) | (0.001) | (0.004) | (0.001) | (0.003) |
| Star &Young Inventors | | | | | -0.410* | -0.075 |
| | | | | | (0.056) | (0.706) |
| Instrumented Opioid Deaths * Star & Young Inventors | | | | | 0.178** | 0.024 |
| | | | | | (0.047) | (0.776) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 629,763 | 629,763 | 25,807 | 25,807 | 25,807 | 25,807 |
| *Adjusted* $R^2$ | 0.006 | 0.008 | 0.159 | 0.061 | 0.159 | 0.061 |

**Table 7. Opioid Epidemic and Corporate Innovation: Heterogeneous Effects**

This table presents two-stage instrumental variable (IV) regression results on the effect of the opioid epidemic on corporate innovation. For brevity, the table reports only the second-stage regression results. In Panel A, a firm is classified as having low employee importance if its ratio of R&D per employee belongs to the bottom tercile of the sample in a year, and zero otherwise. In Panel B, a firm is classified as having low growth opportunities if its market capitalization minus common shareholder's interest in the firm, scaled by the number of employees, belongs to the bottom tercile of the sample in a year, and zero otherwise. In Panel C, a firm is classified as having low ability to handle larger, more complex projects if its size measured by total assets belongs to the bottom tercile of the sample in a year, and zero otherwise. The control variables are the same as in Table 2. All control variables are lagged by one year. Appendix A provides detailed definitions of all variables. All models include year and industry fixed effects, whose coefficients are suppressed, and are based on calendar year and 2-digit SIC industry classification, respectively. Standard errors are adjusted for heteroscedasticity and are clustered at the county level. *P*-values are reported in parentheses below the coefficient estimates. The symbols ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels, respectively.

| **Panel A: Importance of Employee Input (R&D/Employee)** | | | |
|---|---|---|---|
| | **(1)** | **(2)** | **(3)** |
| | **Ln (#Patents+1)** | **Ln (#Citations+1)** | **Ln (Innovation Value+1)** |
| Instrumented Opioid Deaths | -1.149*** | -2.368*** | -1.604*** |
| | (0.005) | (0.003) | (0.008) |
| Instrumented Opioid Deaths x Low Employee Input | 1.137** | 1.977** | 1.456** |
| | (0.024) | (0.033) | (0.044) |
| Low Employee Input | -3.341*** | -5.800** | -4.348** |
| | (0.007) | (0.011) | (0.014) |
| Controls | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Observations | 15178 | 15178 | 15178 |
| *Adjusted* $R^2$ | 0.446 | 0.377 | 0.510 |

| **Panel B: Growth Opportunities** | | | |
|---|---|---|---|
| | **(1)** | **(2)** | **(3)** |
| | **Ln (#Patents+1)** | **Ln (#Citations+1)** | **Ln (Innovation Value+1)** |
| Instrumented Opioid Deaths | -1.073** | -2.090*** | -1.693*** |
| | (0.015) | (0.010) | (0.010) |
| Instrumented Opioid Deaths x Low Growth | 0.775** | 1.037* | 1.419*** |
| | (0.029) | (0.098) | (0.007) |
| Low Growth | -2.260*** | -3.237** | -4.173*** |
| | (0.010) | (0.036) | (0.001) |
| Controls | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Observations | 15178 | 15178 | 15178 |
| *Adjusted* $R^2$ | 0.442 | 0.376 | 0.506 |

| **Panel C: Project Size/Complexity** | | | |
|---|---|---|---|
| | **(1)** | **(2)** | **(3)** |
| | **Ln (#Patents+1)** | **Ln (#Citations+1)** | **Ln (Innovation Value+1)** |
| Instrumented Opioid Deaths | -1.416*** | -2.785*** | -2.213*** |
| | (0.007) | (0.005) | (0.005) |
| Instrumented Opioid Deaths x Low Ability | 1.023** | 1.590** | 1.709** |
| | (0.035) | (0.043) | (0.017) |
| Low Ability | -1.596 | -2.908 | -2.773 |
| | (0.162) | (0.127) | (0.101) |
| Controls | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Observations | 15178 | 15178 | 15178 |
| *Adjusted* $R^2$ | 0.426 | 0.339 | 0.474 |