The Impact of the Opioid Crisis on Firm Value and Investment

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Abstract

The opioid crisis has implications for firms which must now contend with a lower supply of workers. Consistent with a labor shortage explanation, we find a negative effect of instrumented opioid prescriptions on subsequent individual employment. Similarly, we show a negative relationship between opioid prescriptions and subsequent establishment growth. Firms respond to the labor shortage by investing more in technology, consistent with an intent to substitute capital for the relatively scarcer labor. We find positive abnormal returns upon the passage of state laws intended to limit opioid prescriptions, especially for those firms that are more reliant on labor.

Keywords: opioid, firm value, labor supply.

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Employee health has a direct impact on worker productivity and the labor supply. Yet its implications on firm outcomes have not been adequately studied in the corporate finance literature. In this paper, we focus on the opioid epidemic, which has large negative effects on health outcomes. Opioid abuse in the U.S. has reached unprecedented levels. The federal government estimates that, as of 2016, 2.1 million Americans were addicted to opioids and 11.4 million Americans (3.5% of the population) misused opioids in the previous year (National Survey on Drug Use and Health Mortality in the United States, 2016). The economic consequences of this health crisis are large,[1] with several economists pointing out that opioid abuse can help explain declining labor force participation (Yellen, 2017; Krueger, 2017). This, in turn, has implications for firms who now contend with a smaller or less productive pool of workers.[2]

To understand the impact of opioid abuse on firms, we consider three main tests and the key findings are as follows. First, we show that instrumented opioid use reduces individual employment. Second, we show a negative relationship between local opioid use and subsequent sales and employment growth, which triggers firms to respond by investing more in automation to substitute for the relatively scarcer labor in harder hit areas. Finally, we show value implications by exploring announcement returns around state initiatives to reduce opioid use. In all these tests, we follow the

[1]The Council of Economic Advisors estimates the monetary cost of the opioid crisis in 2015 to be $504 billion, or 2.8% of GDP that year. (https://www.whitehouse.gov/sites/whitehouse.gov/files/images/The%20Underestimated%20Cost%20of%20the%20Opioid%20Crisis.pdf).

[2]A recent article in the New York Times discusses a high level of job openings in Youngstown Ohio and the difficulty faced by employers to fill in those openings. "It's not that local workers lack the skills for these positions, many of which do not even require a high school diploma but pay $15 to $25 an hour and offer full benefits. Rather, the problem is that too many applicants — nearly half, in some cases — fail a drug test... Each quarter, Columbiana Boiler, a local company, forgoes roughly $200,000 worth of orders for its galvanized containers and kettles because of the manpower shortage, it says, with foreign rivals picking up the slack." (Schwartz, 2017). Another article in the Wall Street Journal describes the severity of the problem in Indiana: "Some 80% of Indiana employers said they have been affected by prescription drug misuse and abuse, facing issues like impaired performance and employee arrests, according to a survey by the National Safety Council and the Indiana Attorney General's Prescription Drug Abuse Prevention Task Force." (Silverman, 2016).

medical literature and use opioid prescriptions as a proxy for opioid abuse (Cicero et al., 2007; Harris et al., 2019). Studies have shown that legal opioid prescriptions are an important driver of opioid addiction (Schnell, 2019).[3] Moreover studies, such as Cicero et al. (2007), Dasgupta et al. (2006), and Wisniewski et al. (2008), have demonstrated a positive correlation between rates of prescriptions in a given geography and subsequent opioid abuse in the area.

We start by documenting the effect of opioids on the labor supply at the individual level using prescription data extracted from individual-level healthcare claims paid by employer-sponsored insurance. For this analysis, we take two individuals who are identified as full-time employees between the ages of 18 and 60, live in the same county, receive the same medical diagnosis and are of the same age and gender but where one individual receives opioid prescription for the first time in a given year and the other does not. To address endogeneity concerns, we instrument for the probability of being prescribed an opioid using the propensity of the doctor they visited to prescribe opioids for their medical condition the prior year. Using the predicted value from the first stage, we then observe whether the individual remains employed within the sample of covered firms five years later.

We find a 3.7% decrease in the probability of being employed five years after the instrumented opioid prescription. These results are estimated with county-year-gender-age-diagnosis fixed effects, thus absorbing any time-varying shocks impacting demographic groups within a county. However, this may be insufficient to address all endogeneity if, for example, drug-seeking individuals visit doctors that have a reputation for easily prescribing opioids. To address this concern, we replicate our analysis using different subsamples where we drop high-opioid prescriber doctors or drop high-income inequality counties where within the same county-year-gender-age-diagnosis there could be sorting of lower-income individuals to lower-quality doctors. In an additional test, we consider only emergency room diagnoses, which arguably

[3] According to the National Institute on Drug Abuse (2020), 80% of heroin users began by misusing prescription opioids.

have to be immediately treated and thus less subject to the "doctor-picking" concern. We construct ER opioid prescriptions based on the top 10 most common emergency room diagnoses, identified by the AAPC.[4,5]

To understand how these negative labor supply effects impact firms, we aggregate opioid prescriptions at the county level. Given the strong persistence in local opioid use, we use two observations per county in a (long) stacked first-differences specification. As such, we measure the change in the rate of opioid prescriptions between 2002-2006 and 2006-2010 on the subsequent change in establishment employment, from 2007 to 2011 and 2011 to 2015, respectively. In our specifications, we control for firm-period fixed effects, thereby comparing the effect of opioids on two establishments in the same firm and during the same time-period but located in counties experiencing different historic opioid prescription growth. We also include industry-period fixed effects to control for differential trends at the industry level and controls for observable economic and demographic characteristics. We find that, on average, establishments in high-opioid growth counties have lower employment five years later, as compared to establishments in low-opioid growth counties. The economic magnitudes are also meaningful: an increase in opioid prescriptions from the 25$^{\text{th}}$ to the 75$^{\text{th}}$ percentile (an increase of 0.3/person) is associated with a 0.6% decline in employment. On the extensive margin, we document a negative and significant effect on establishment exits in counties that experience greater opioid prescription growth.

As labor is a key input of production, opioid use should also be negatively associated with sales growth. Indeed, using the same specification of four-year stacked first-differences, we find that opioid prescriptions are negatively associated with establishment sales. In economic terms, an increase in opioid prescriptions from the 25$^{\text{th}}$ to the 75$^{\text{th}}$ percentile (an increase of 0.3/person) is associated with a 1.7% decrease

[4]AAPC is previously known as the American Academy of Professional Coders.

[5]It is important to emphasize that all individuals in our sample have employer-provided insurance. As such, we are not including individuals who seek treatment at emergency departments due to lack of insurance coverage which limits their alternative treatment options.

in sales, on average. We confirm this result using data on establishment expansions at the county level, where we estimate a negative and significant effect.

Firms appear to respond to this labor shortage by changing their production processes towards more capital. We use data on IT spending and the stock of computers and telecommunication technologies to proxy for investment in automation. As before, we compare establishments within the same firm and time period but located in different counties and control for time-varying firm and local economic and demographic characteristics as well as industry trends. We find that, on average, establishments in high-opioid growth counties spend relatively more on IT five years later, as compared to establishments at the same firm in low-opioid growth counties. The results are economically important: an increase in opioid prescriptions from the 25^{th} to the 75^{th} percentile is associated with a 3.5% increase in IT budget and a 1.9% increase in the count of PCs.

To further provide evidence of a labor channel, we exploit heterogeneity across industries which rely on labor easily replaceable by technology. To this end, we use a measure based on the fraction of each industry's hours spent by workers on tasks that can be performed by industrial robots (Graetz and Michaels, 2018). Consistent with the intuition that our results are driven by a labor shortage mechanism, we find that the decline in firm sales and employment is moderated and the increase in automation is more pronounced in establishments that belong to industries which rely on labor more easily replaceable by technology and where substituting capital for labor is more feasible.

A potential concern with our establishment-level analysis is the endogeneity of opioid abuse. In particular, individuals may be more likely to abuse drugs when they feel that job opportunities are limited. Our key identifying assumption requires that opioid prescriptions written by doctors are independent of economic conditions five years later, after controlling for time-invariant unobservable and time-varying observable county differences. Although there is plethora of evidence in the existing literature (discussed later or see Currie and Schwandt (2020) for a summary) that deteriorating economic conditions are not a significant driver for geographic differences in opioid abuse, we present several pieces of empirical evidence that address the concern that omitted variables are driving our findings.

First, the estimated coefficients of interest are similar when estimated with or without economic and demographic controls as well as industry trends. Likewise, our results are robust to including fixed effects which absorb time-varying firm changes and differences in establishment-specific trends. Second, we estimate similar results when we repeat the analysis just in the tradable sector, which suggests that the results are not driven purely by changes in local demand. Third, results are robust to dropping counties with the worst economic performance before the start of our sample. Fourth, to address concerns that trade shocks such as cheaper Chinese imports contribute to the demise of certain areas which are then more likely to suffer from the opioid epidemic, we drop from the sample all manufacturing industries—since Chinese imports affect specifically the manufacturing sector. Alternatively, we identify counties with the worst exposure to Chinese imports following Autor and Dorn (2013) and exclude those from our analysis. Our results are robust.

Fifth, we instrument opioid prescriptions with opioids prescribed following the most common emergency rooms (ER) diagnoses. Our identifying assumption is that emergency physician visits are driven by an unexpected and sudden deterioration in health which requires immediate treatment, most likely in an emergency department, where doctors are randomly assigned. This immediacy and randomization across doctors reduces the probability that we are picking up intentional behavior seeking opioids. We find quantitatively similar effects of opioid prescription on sales, employment, and investment in technology. These results mitigate the concern that an omitted variable could be driving both drug-seeking behavior and long-term firm outcomes.

Finally, we present further evidence that the opioid crisis hurts firm value using stock price reactions upon the passage of state laws which intend to limit opioid prescriptions. The first law was passed in Massachusetts in 2016 and, since then, another 24 states have passed similar actions to limit opioid abuse. At the passage of such a law in the firms' state of headquarters, we document a statistically significant abnormal return of 20 basis points, on average. Naturally, we show that this effect is driven by those firms with a large fraction of their employees located in the firm's state of headquarters. Consistent with our argument that firms can mitigate some of the costs associated with opioid abuse by investing more heavily in automation, we show that the positive returns upon passage of these laws are more pronounced for the set of firms with low ex-ante capital intensity. On average, these low-capital intensive firms realize a stock price gain of 60 basis points.

Our conclusion that deteriorating economic conditions do not seem to explain our results is consistent with a large literature that studies the determinants of opioid abuse. Currie and Schwandt (2020) summarize the literature and lay out the facts why neither contemporaneous nor long-term economic conditions can explain the opioid epidemic.[6] Instead, they argue that opioids spread in the U.S. due to a combination of three factors: a change in beliefs amongst physicians that pain was not treated adequately, aggressive marketing by pharmaceuticals who were falsely claiming that a new generation of opioids was effective at treating pain with minimal risk of addiction, and little public oversight (until recently) of opioid prescriptions by doctors.

Our paper also contributes to the literature studying the impact of the opioid crisis on the U.S. Case and Deaton (2015) show the impact of opioids on health and longevity. Krueger (2017) and Harris et al. (2019) show the negative impact

[6] "The opioid epidemic first gained a foothold in the prosperous period prior to the recession of 2008. As the epidemic peaked in 2017-2018, unemployment was at its lowest level in decades. And while a great deal of attention has been focused on opioid deaths in depressed areas with persistently high unemployment, the majority of opioid deaths occurred in large states with low unemployment rates (Currie et al., 2019). A final fact that does not fit the popular narrative is that although African-Americans have persistently high unemployment relative to other Americans, the epidemic seemed to start first among non-Hispanic whites, and had a particularly large impact on white women (Singhal et al., 2016)."

of opioid prescriptions on labor supply. Alternatively, Currie et al. (2019) show a weakly positive relation between opioid prescriptions and female labor supply, using short-term variation in lagged opioid prescriptions. Van Hasselt et al. (2015) and Florence et al. (2016) quantify the costs to the U.S. economy due to lost productivity from opioid abuse. Cornaggia et al. (2019) and Li and Zhu (2019) show the impact of opioids on municipal bond rates. Jansen (2019) looks at the impact on opioids on auto loans. We instead show that opioid abuse has an economically important negative impact on firm growth and valuations, prompting firms to respond by automating. Our results thereby speak to the long-term implications for impacted communities which must now struggle with both high rates of drug abuse and job losses through automation.

Finally, our paper builds to the small but important literature on health and finance. Almeida et al. (2020) study the effect of Obamacare on firm employment and performance. Cohn and Wardlaw (2016) show that financing frictions impact negatively investment in workplace safety, as proxied by workers' injury rates. Papanikolaou and Schmidt (2020) show that industries in which a higher fraction of the labor force can work remotely during the Covid-19 pandemic experience less severe disruptions in their business operations. This is the first paper that links the opioid epidemic, that significantly deteriorates health of the labor supply through addiction, with firm outcomes.

I Origins and Determinants of the Opioid Crisis

Starting in the 1980s, the medical community in the United States began to push for a more aggressive approach to treating pain. A view that pain was relatively under treated in the U.S. became prevalent (Weiloo, 2014). Following the arrival of a new generation of prescription opioids, such as the 1995 FDA approval of OxyContin (oxycodone controlled-release), the American Academy of Pain Medicine and the American Pain Society advocated for greater use of opioids. They argued that the long-term risk of addiction from these drugs was minimal. This stance became further institutionalized in 2001 when the Joint Commission on Accreditation of Healthcare Organizations (TJC) determined that the treatment and monitoring of pain should be the fifth vital sign thus, creating a new metric upon which doctors and hospitals would be judged.[7] Even as late as 2011, the Institute of Medicine released a study arguing that pain was being undertreated in America.[8] Concerns about the possible over-use of opioid prescriptions for chronic pain conditions became more common into the 2000s. In 2014, the Agency for Healthcare Research and Quality (AHRQ) concluded that there is limited, if any, evidence-based medicine to support opioids' use in chronic non-terminal pain (Chou et al., 2014). In 2016, the FDA issued a new policy recommendation regarding prescribing opioids with an emphasis on the large public health costs. In 2017, the TJC issued new standards on the treatment of pain.

Pharmaceutical companies, like Purdue Pharma which developed OxyContin, reiterated the same message in their marketing materials. In advertising their new drug, Purdue Pharma made no mention of the addiction potential of OxyContin, relying on two small retrospective studies from the 1980s.[9] According to training materials, Purdue instructed sales representatives to assure doctors—repeatedly and without evidence—that "fewer than one per cent" of patients who took OxyContin became addicted (Keefe, 2017). OxyContin was promoted as safe for chronic pain as well as for simple conditions like wisdom tooth extraction, where alternative pain relief treatments were available (Currie and Schwandt, 2020). The FDA later accused Purdue Pharma of false advertising. In 2007, Purdue Pharma plead guilty to misbranding

[7]https://www.medpagetoday.com/publichealthpolicy/publichealth/57336

[8]In this study, the authors acknowledged concerns about opioid prescriptions being diverted but argued that "when opioids are used as prescribed and are appropriately monitored, they can be safe and effective" (Pizzo and Clark, 2012)

[9]These studies were later criticized of having questionable scientific rigor. Porter (1980) is a one paragraph letter to the editor in the New England Journal of Medicine. Portenoy and Foley (1986) was a study conducted in a sample of 38 patients published in Pain.

of OxyContin, paid a fine of over $600M and agreed to cut its sales force in half.[10] Additional lawsuits are still outstanding arguing that Purdue Pharma intentionally misled doctors and patients about the addiction risks associated with their opioid products.

During this time, the lack of a clear nationwide guidance for doctors and limited public oversight of opioid prescriptions led to a lack of consensus among doctors on best practices and significant heterogeneity in doctor approaches to prescribing opioids (Tamayo-Sarver et al., 2004; Cantrill et al., 2012; Poon and Greenwood-Ericksen, 2014; Paulozzi et al., 2014; Kuo et al., 2016; Jena et al., 2016). Moreover, doctors in the U.S. have discretion in prescribing opioids to patients, while in contrast other countries follow more restrictive policies such as having a lower maximum allowable opioid dosage, requiring doctors to undergo special training or use special prescription pads, or patients to register in order to take opioids (Ho, 2019).

Case and Deaton (2015) brought much needed attention to the opioid crisis when coining the term "deaths of despair" and suggesting economic conditions played a role. However, since then, a number of papers using better data have shown that economic conditions are not a significant driver of regional patterns of opioid use. In fact, most deaths attributed to opioids occur in states with low unemployment rates (Currie and Schwandt, 2020).[11]

Specifically, Currie et al. (2019) find a positive relationship between employment and opioid prescriptions at the county level between 2006 and 2014. This is consistent with the observation that the vast majority of people who are taking opioids are

[10] https://www.washingtonpost.com/national/health-science/oxycontin-maker-purdue-pharma-to-stop-promoting-the-drug-to-doctors/2018/02/10/c59be118-0ea7-11e8-95a5-c396801049ef_story.html%3futm_term%3d.bf485594e8ff?noredirect=on&utm_term=.287306293869

[11] For example, in 2018, Ohio and New Jersey had among the highest rates of opioid deaths per population (35.9 and 33.1 per 100,000) with unemployment rates of 4.4 and 4.6% respectively. While Massachusetts and New Hampshire had similar opioid death rates of 32.8 and 35.8 per 100,000 and yet had relatively low unemployment rates of 3.5 and 2.6%. In the other direction, Texas had an unemployment rate of 4.0% but an opioid death rate of only 10.4 per 100,000. West Virginia does fit the pattern of both high rates of opioid deaths and high unemployment but West Virginia is more the exception than the rule.

employed in their sample. Ruhm (2018) finds economic conditions can predict opioid prescriptions in the cross-section of counties. However, controlling for demographics and persistent county characteristics washes away the explanatory power of the controls for economic conditions. Other papers, using specific economic shocks for better identification, likewise find that economic conditions are not a key driver of opioid use. For example, Pierce and Schott (2020) show that an interquartile increase in trade exposure can only explain one tenth of drug overdose deaths. Schwandt and von Wachter (2020) study whether long-run effects of entering the labor market in a recession could explain the opioid mortality rate. Even under the extreme assumption that all cohorts entered the labor market in a recession, their model could only explain one eight of opioid deaths. Consistent with the argument that doctor practices are responsible for the epidemic, Finkelstein et al. (2018) show that the regional differences in the supply of prescription opioids from doctors is a key contributor to opioid abuse as opposed to patient-specific factors such as mental health or poor economic prospects.

As the epidemic grew and the addictive nature of opioids increasingly occupied the public discussion, a number of states have taken actions to reduce the use of opiods in their states. Some states developed prescription drug monitoring programs (PDMPs), which allow doctors to better identify drug-seeking patients. However, many of these programs relied on voluntary participation of providers with mixed evidence on their effectiveness (Islam and McRae, 2014; Meara et al., 2016; Buchmueller and Carey, 2018). More recently, several states have taken more drastic measures to curb opioid adoption with legislation that explicitly sets limits on opioid prescriptions (with some exceptions such as cancer treatment). In 2016, Massachusetts became the first state to limit opioid prescriptions to a 7-day supply for first time users. As of 2018, 25 states have legislation limiting the quantity of opioids which can be prescribed. In October 2017, the U.S. government declared opioids a public health emergency. In 2019, Medicare adopted a 7-day supply limit for new opioid patients at the federal-level.

II Data

We identify opioid prescriptions at the individual level using data provided by Marketscan from Truven Health Analytics (Adamson et al., 2008). The MarketScan data covers anonymized individual-level health data for 37.8 million privately insured individuals with employment-based insurance through a participating employer.[12] For each individual, we observe all medical expenditures covered by their medical insurance. We observe the date of service and diagnosis code,[13] drug provided and date the prescription was filled. For each individual, we also observe their county of residence and their gender, age and employment status.

We aggregate this data to the county-level to measure local opioid prescription intensity.[14] We use historic county-level opioid prescriptions as a proxy for local opioid abuse. Legal opioids can lead to abuse through two main channels. First, the original consumer of the opioid can end up unintentionally addicted. In a widely-cited meta-analysis, Volkow and McLellan (2016) find that up to 8% of patients who fill an opioid prescription will end up with a diagnosed opioid addiction and 15-26% will misuse opioids. According to the National Institute on Drug Abuse (2020), 21 to 29% of patients prescribed opioids for chronic pain misuse them, between 8 and 12% develop an opioid use disorder, and 4 to 6% of those who misuse prescription opioids start taking heroin. Second, legal opioid prescriptions have been shown to be a major source of diverted opioids, as in Compton et al. (2015) and Shei et al. (2015). These diverted pharmaceuticals are then typically consumed in the local community, leading to a relationship between rates of prescriptions of opioid medications in a

[12]We describe MarketScan and discuss the validity of the data in Internet Appendix A.

[13]There are 12,224 diagnoses codes in our data following the ICD-9-CM classification.

[14]Figure 1 presents the geographic variation of this measure across U.S. counties, averaged over 2001-2010.

given geography and opioid abuse in the area, as in Cicero et al. (2007).[15]

We use rates of historic (five-year lagged) opioid prescriptions to allow for time between the initial prescription and the onset of drug abuse. However, using five-year lagged prescriptions is unlikely to attenuate the relationship between opioid prescriptions and opioid abuse as opioid addiction is a chronic condition. Flynn et al. (2003) find that only 28% of opioid addicts are in recovery five years later.

In Table 1, Panel A, we report summary statistics on county-level opioid prescription rates. We measure opioid prescriptions as the average opioid prescriptions per (Marketscan) enrollee in that county and year. The sample covers 3,145 unique counties between years 2002 and 2010.[16] On average, we report a per enrollee opioid prescription rate of 0.49. Our measure of opioid prescriptions per enrollee is modestly lower than the per capita prescription rates reported by the Centers for Disease Control and Prevention (CDC), likely reflecting healthier demographics of our employed population, as compared to the full adult population in the CDC data.[17,18] We use MarketScan as our baseline data as it is available for a longer time series and specifically provides information on opioid prescriptions among current labor market participants. According to Currie et al. (2019), the majority of opioid prescribers are working-age individuals and prescriptions are paid for by an employer-provided health insurance (precisely the source of our data). Moreover, the richness of Marketscan

[15]Similar results have been reported in Dasgupta et al. (2006) who use national data available through DAWN, Wisniewski et al. (2008), who use four national surveys, and Modarai et al. (2013) who look at North Carolina and use state-specific county level data.

[16]Marketscan data is available until 2015, however, the county identifier is only available until 2010.

[17]We further compare MarketScan with CDC data in Internet Appendix A.

[18]Opioid prescriptions include buprenorphine, codeine, fentanyl, hydrocodone, hydromorphone, methadone, morphine, oxycodone, oxymorphone, propoxyphene, tapentadol, and tramadol. Buprenorphine and Methadone are commonly used to treat opioids abuse and are excluded from our measure.

data allows us to look within county at individual-level outcomes.[19]

In Panel A, we also report summary statistics for key county-level demographic and economic variables. Economic control variables include the median household income. Demographic control variables include total population, distributions by race and age, and neoplasms mortality. All variables are defined in the Appendix.

In Panel B, we report summary statistics of establishment-level data on information technology from the Computer Intelligence Technology Database (CiTDB), a proprietary database that provides information on computers and telecommunication technologies installed in establishments across the U.S. CiTDB generates their data using annual surveys of establishments. The data contains detailed information on IT investment and use, including the stock of existing technology and budgets for new investments. The data also has information on the county of each establishment, a firm-level identifier and establishment-level revenue and employment.[20]

We summarize the data over the 2007-2015 period used in our sample. In our analysis, we drop observations in highly regulated (agriculture, education, and utility) or public sector industries. We also drop all establishments in the health-care sector as the opioid epidemic may increase labor demand in this sector. We also limit our sample to establishments with a minimum of 20 employees in the first year an establishment shows up in the two-period sample (i.e., 2007 or 2011), to ensure that our results are driven by economically important establishments. We end up with 330,000 unique establishments of 126,000 unique firms. The average establishment in our sample has a revenue of $37.6 million, 120.6 employees, invests $0.5 million in IT, and has a stock of 84 PCs.

[19]In Internet Appendix Table B1, we present robustness using CDC as our source for opioid prescriptions. We estimate qualitatively similar results, albeit statistically weaker. This could be due to the fact that CDC data are available only starting in 2006 and cover all Americans, unlike our employer-based Marketscan data.

[20]We describe CiTDB and discuss the validity of the data in Internet Appendix A.

III Opioids and Individual Employment

To identify the effect of the opioid crisis on the labor market, we explore whether individuals who have taken a prescription opioid in a given year have different future employment outcomes, as compared to similar individuals, seeking treatment for the same medical condition, who were not prescribed opioids that year. We then compare an individual who has taken a prescription opioid to an otherwise similar control individual without such a prescription history and measure the marginal difference in employment outcomes five years later.

For this analysis, we require individuals to be employed full-time and be between the ages of 18 and 60, to avoid including individuals who are expected to retire within a five-year window. Moreover, given the addictive nature of opioids, we limit the sample to individuals who have not previously received an opioid in our data. We identify treated individuals as those who receive their first opioid prescription between 2001 and 2010. We start our analysis in 2001 to allow for a window to measure previous opioid use. We stop in 2010 to allow for five years until the end of our data. We randomly pick control individuals among those who did not receive an opioid prescription in that year and, as with the treated sample, did not receive an opioid prescription in the previous years. We also require treated and control individuals to be of the same age, gender, to reside in the same county, and to receive the same medical diagnosis when they visit the doctor that year.

Directly comparing treated and control individuals would raise concerns for endogeneity. To this end, we instrument for the original opioid prescription using doctor propensity to write opioid prescriptions for that given diagnosis. We measure doctor opioid intensity in the prior year as the rate with which a doctor prescribed opioids for the same medical diagnosis the prior year. We estimate our first-stage equation as follows:

$$I\{Opioid\ prescribed\}_{i,d,s,t} = \beta \cdot Doctor\ opioid\ intensity_{d,s,t-1} + FE + \epsilon_{i,d,s,t} \quad (1)$$

where i indexes individuals, d indexes doctors, s indexes diagnosis codes and t indexes year. FE includes fixed effects for county-year-gender-age-diagnosis and insurance plan type.[21] Standard errors are clustered at the county level.

In the second stage, we estimate differences in employment rates five years later, using the instrumented probability of receiving an opioid prescription.

$$I\{Employed\}_{i,d,s,t+5} = \gamma \cdot I\{Opioid\ \widehat{prescribed}\}_{i,d,s,t} + FE + \epsilon_{i,d,s,t+5} \quad (2)$$

where i indexes individuals, d indexes doctors, s indexes diagnosis codes and t indexes year. FE includes fixed effects for county-year-gender-age-diagnosis and insurance plan type. Standard errors are clustered at the county level.

We present the 2SLS results in Table 2. The first-stage results, in column 1, show that doctor opioid intensity significantly predicts whether an individual will receive an opioid. Our instrument is significant at the 1% level with a first stage F-stat of 3,073—well above the 10 threshold.[22] The second-stage result, in column 2, show that individuals who receive an opioid prescription are 3.7% less likely to be employed five years later. In our analysis, we include insurance plan fixed effects, which controls for the fact that individuals with better insurance coverage may have better healthcare access. We also control for interacted county-year-gender-age-diagnosis codes, thereby comparing individuals who live in the same county, are of the same gender and age and who receive the same diagnosis code when they visit the doctor in the same year.

[21] Plan types include basic/major medical, Comprehensive, EPO, HMO, POS, PPO, POS with capitation, CDHP, and HDHP.

[22] These results complement Barnett et al. (2017) who find that whether a patient was randomly assigned to a high-opioid prescribing doctor or not is a significant predictor of long-term use of opioids, using a sample of medicare patients.

This absorbs any time-varying local shocks specific to a given demographic group that could be driving the estimated differences.

One concern for our analysis would be if individuals with worse future job opportunities seek out doctors who are more likely to write opioid prescriptions. To address this concern, we show our results are robust to excluding individuals more likely to exhibit drug-seeking behavior. First, in columns 3-4, we exclude doctors with opioid-prescribing intensity in the top 10% of our sample. A patient seeking medical care for the purpose of drug seeking will try to target those doctors with the highest opioid prescribing tendency. Second, in columns 5-6, we exclude counties with income inequality (measured by Gini index) in the top 10% of our sample. In those high inequality counties, there might be sorting of lower income individuals to lower quality doctors, who might be more likely to prescribe opioids. Although controlling for insurance plan fixed effects alleviates this concern to some extent, dropping counties with large inequalities further addresses the fact that differences in the quality of healthcare access within counties could still be driving the results.

Finally, in columns 7-8, we repeat the analysis using just the top 10 most common emergency room (ER) diagnoses, identified by the AAPC.[23,24] Our assumption is that individuals covered by health insurance receive care in ERs only when facing a health emergency, at which point random assignment to doctors is more likely.

Our results are robust across all these different tests, which alleviates concerns that non-random individual-doctor matching within counties can explain our findings. However, we should point out some caveats to this analysis. In our data, we can

[23]Top 10 most common emergency room diagnoses include chest pain (unspecified), abdominal pain (other specified site), head injury (unspecified), headache, syncope and collapse, open wound of finger without mention of complication, sprains and strains of ankle (unspecified site), pneumonia (organism unspecified), fever (unspecified), and backache (unspecified). https://www.stjhs.org/documents/ICD-10/2014_FastForward_EmergencyDept_Press.pdf

[24]In unreported regressions, we show our results are robust if we exclude pain-related diagnoses (abdominal pain, headache and backache) and instead use the next three most common (non-pain) ER diagnoses of alcohol abuse, vomiting alone, and acute upper respiratory infections of unspecified site.

observe whether an individual who is employed in a given year, remains employed among firms in our sample five years later. While we refer to this as being "employed" for ease of exposition, what we technically observe is being employed at one of the employers which shares their data with MarketScan. To the extent that both treated and control individuals are equally likely to be employed in a firm not covered by MarketScan five years later, this limitation of our data is unlikely to bias our analysis. It is also important to note that changes in individual employment is only one measure of the impact opioids can have on the labor market. Employment status does not capture changes in the quality of the pool of workers. Individuals abusing opioids are presumably more likely to miss work, to be involved in on-the-job injuries, and to be less productive overall. As such, our estimate of the impact on individual employment likely underestimates the impact of the opioid crisis on labor.

Overall, these results suggest a negative association between higher rates of opioid prescriptions and the subsequent supply of labor available to firms. This can have important implications for firms, which we examine in the following section.

IV Opioids and Firm Outcomes

IV.1 Methodology

We next investigate the impact of local opioid use on firm outcomes with a model using two-stacked long differences. We measure the change in establishment-level outcomes between 2007 and 2011 and between 2011 and 2015. We start at 2007 and end in 2015, as this is the first and last year, respectively, of our CiTDB sample. We use historic five-year lagged opioid prescriptions, consistent with the lag structure in the previous analysis, as it takes time for opioid abuse in the community to accumulate. Specifically, we measure opioid prescriptions as the change between 2002-2006 (matched to establishment level outcomes between 2007 and 2011) and as the change between 2006-2010 (matched to establishment level outcomes between

2011 and 2015). We, thus, estimate the following specification:

$$\Delta y_{i,f,c,t} = \beta \cdot \Delta Opioid\ prescriptions_{c,t-5} + \delta \cdot \Delta X_{c,t-5} + FE + \Delta\epsilon_{i,f,c,t} \qquad (3)$$

where Δ denotes the long (four-year) difference operator, c indexes county, i indexes establishments, f indexes firms and t indexes time period. Δ*opioid prescriptions* is the change in opioid prescription per enrollee. Δy is the change in establishment-level outcome variables, including sales, employment and investment in IT. ΔX controls for changes (contemporaneous to the change in opioid prescription rates) in economic and demographic characteristics as well as the underlying cancer rate in the county.[25] Specifically, these controls include the logarithm of population, the logarithm of median household income, the white ratio, the age 20-64 ratio, age over 65 ratio, and the rate of neoplasms mortality. FE include firm-period fixed effects, absorbing time-varying differences in firm quality and industry-period fixed effects, absorbing time-varying differences across industries. We double-cluster standard errors at the county and firm levels.

IV.2 Sales and Employment Growth

We first explore the relation between opioid prescriptions and growth in establishment sales and employment. Table 3 reports the results. In column 1, we find a negative and significant correlation between opioid prescription rates and subsequent establishment sales in the county. In column 2, we estimate similar results after controlling for economic and demographic county characteristics. These results are economically important: an increase in opioid prescriptions from the 25$^{\text{th}}$ to the 75$^{\text{th}}$ percentile (an increase of 0.3 prescriptions/person) is associated with 1.7% decrease in sales in that establishment relative to the firm's average establishment. In column 3, we

[25] Results are robust when we instead control for lagged ΔX by one year to changes in opioid prescription rates.

also find a negative correlation between opioid rates and subsequent establishment employment, a relationship that is robust to including economic and demographic controls in column 4. In economic terms, an increase in opioid prescriptions from the 25^{th} to the 75^{th} percentile is associated with a 0.6% decline in employment in that establishment relative to the firm's average establishment.

Given we include firm-period fixed effects in these regressions, the results should be interpreted as showing within-firm reallocation. However, we find similar results if we exclude firm fixed effects (Internet Appendix Table B2), which suggests that the same patterns identified within firms are also observed across firms. These results also help address potential concerns that an omitted variable associated with firms located in high-opioid counties is driving our results. In addition, we find consistent results on the extensive margin using U.S. Census administrative data. Using counts of establishments at the county-level, we find a statistically positive (negative) relation between opioid prescription rates and subsequent firm deaths (expansions) (Internet Appendix Table B3).

IV.3 Investment

We next examine whether firms respond to the labor shortages attributable to the opioid epidemic by changing their production choices. To the extent that opioids reduce the number and the quality of available workers, firms may choose to substitute capital for labor by investing in automation technologies that can reduce their dependence on labor (Autor et al., 2003). To test this prediction, we use data on IT spending available from CiTDB. Specifically, we use the IT budget and the count of computers (PCs) to proxy for investment in automation and the stock of installed technology, respectively. While investment in IT is not inclusive of all forms of automation, our assumption is that an increase in automation would also be paired with an increase in IT.

In Table 4, we report results using both measures of IT spending in levels (logtransformed) as well as normalized by establishment revenue and employment. We follow Equation (3) and control for firm time-varying trends (firm-period fixed effects), industry time-varying trends (industry-period fixed effects) and economic and demographic controls.[26] We find a positive and significant association between increases in opioid prescriptions in the county and subsequent increases in IT investment across specifications.[27] Thus, firms increase IT investment relatively more at their establishments located in counties with higher growth in past opioid prescription rates as compared to establishments located in counties with lower past opioid prescription growth. In terms of the economic magnitude, an increase in opioid prescriptions from the 25th to the 75th percentile (an increase of 0.3 prescriptions/person) is associated with a 3.5% increase in IT budget and a 1.9% increase in the count of PCs.

Moreover, in Table 5, we show that our results also hold with adding establishment fixed effects. This analysis is estimated using only the establishments observed over both time periods. Establishment fixed effects allow us to further control for differential trends by establishment. The results are quantitatively similar, with the exception that we lose statistical significance on the change in employment (even though the coefficient is of similar magnitude as in Table 3).

These results are consistent with the argument that firms invest in automation to change their production processes and substitute capital for labor as a response to labor shortages in opioid affected areas.

[26]Industry-period fixed effects are not subsumed by firm-period fixed effects as a given firm can operate establishments in multiple industries.

[27]Our results are robust to dropping from the estimation: i) firm-period fixed effects (Internet Appendix Table B2), ii) economic and demographic controls (Internet Appendix Table B4), iii) headquarter establishments (Internet Appendix Table B5), and estimating the analysis iv) at the commuting zone level (Internet Appendix Table B6), and v) separately for the two periods included in our sample (Internet Appendix Table B7). We find stronger magnitudes in the second period, 2011-2015, which may be driven by the tighter labor market nationally in the later period.

IV.4 The Labor Channel

We next present further cross-sectional heterogeneity evidence consistent with the capital deepening mechanism we document in Section IV.3. We consider heterogeneity in labor replaceability rates by industry since investing in automation should be especially relevant in industries where labor can be readily replaced with technology. We use the proxy created by Graetz and Michaels (2018), which measures the fraction of hours spent by workers in a given industry in tasks which can be performed by industrial robots.[28]

In Table 6, we interact Δ*opioid prescriptions* with an indicator variable (*high labor repl.*) that takes a value of one if the establishment is matched to an industry with labor replaceability above the sample median, zero otherwise. We find that the negative relationship between opioid prescriptions and long-term establishment growth is significantly attenuated for high-replaceability industries. These findings are consistent with the intuition that firms in these industries can mitigate the costs of labor shortages through substitution with capital. Indeed, we find a stronger positive relation between opioid prescriptions and IT investment (across five out of six measures) in firms operating in high replaceability industries.

V Identification

V.1 Robustness Tests

A key concern with our analysis is that individuals may be more likely to seek out opioid prescriptions in areas with worse job opportunities. Despite the consensus in the economics literature that medical practices rather than differences in economic

[28]Labor replaceability is measured as of 2000, using the 5% sample available from the American Community Survey in 2000 and based on 4-digit NAICS.

prospects are the primary driver of the opioid epidemic,[29] we perform several tests to mitigate this concern that deteriorating economic conditions may explain our results.

First, we repeat our analysis in a subsample where we drop the most depressed economically counties. In Panel A, Table 7, we exclude from the sample counties at the bottom quartile of the household income distribution as of 2000. In columns 1-2, we present a negative association between opioid prescriptions and subsequent sales and employment growth. In columns 3-6, we continue to find strong positive effects of opioid prescriptions on subsequent establishment investment in technology.[30]

Second, we address a related concern that an increase in Chinese imports might have affected certain geographies in the U.S., dampening local economic conditions (Autor and Dorn, 2013). This might in turn lead the local population in those depressed areas to abuse opioids.[31] To empirically address this concern, in Panel B, Table 7, we drop from the sample establishments in manufacturing industries, namely those industries shown in the literature to have been impacted by the cheaper Chinese imports. We find our results are robust. In Panel C, we instead drop from the sample the top quartile of counties with the highest exposure to Chinese imports as of 2000 and find our results continue to hold.[32]

[29]Survey evidence provides further support that deteriorating economic conditions do not seem to be driving opioid abuse. According to the SHED survey, 54% of adults who know someone addicted to opioids, and are thus directly impacted by the crisis, report that their local economy is good or excellent. Only 38% of this same group of individuals report that the national economy is good or excellent, suggesting a relatively strong local economy even among individuals who are directly impacted by the opioid crisis. https://www.federalreserve.gov/econres/notes/feds-notes/shedding-light-on-our-economic-and-financial-lives-20180522.htm

[30]We present IT budgets and PCs as levels and normalized by sales, for the ease of exposition, but results are similar when we instead normalize these variables by employment.

[31]The evidence in the literature, however, does not support this interpretation. Pierce and Schott (2020) show that an interquartile increase in trade exposure can only explain one tenth of drug overdose deaths. In addition, the opioid abuse has deeply affected regions that were not negatively impacted by trade. As highlighted in Currie et al. (2019), Bloom et al. (2019) show that the West coast and New England benefited from Chinese import competition, but New Hampshire and Massachusetts have still been hit hard by opioids (Stopka et al., 2019).

[32]We measure counties' exposure to China following a similar methodology to Autor and Dorn (2013), where we map Chinese imports to counties based on each industry's share to the county employment.

Third, we consider the concern that declining local demand could be driving the results. The increased use of opioids could be, for example, responsible for dampening demand locally. In this case, opioids could still explain the decline in firm growth, albeit not through a labor channel. Alternatively, a decline in demand could be capturing local economic shocks. To examine whether these alternative interpretations could be driving our results, in Panel D, Table 7, we repeat our baseline analysis using just tradable industries (industries with more than 50% tradable employment as in Delgado et al. (2014)), namely industries that are mostly impacted by local demand, and estimate qualitatively similar results.

V.2 Pill Mills

"Pill mills" have helped seed the opioid crisis in certain areas. A typical pill mill consisted of a store front pain clinic where one or more doctors wrote opioid prescriptions after brief consultations and typically with limited proof of medical appropriateness. These clinics typically provided the prescription (written by a staff doctor) as well as filled the prescription to avoid issues with external pharmacies challenging the legitimacy of these prescriptions. These prescriptions represent opioids that are likely to be misused and, hence, likely to have labor market impacts which can subsequently impact local firm characteristics. However, the identification concern is that some of these pill mills served drug seekers, who often travelled from distance to get easy access to opioids. As such, loading on these counties may introduce noise if the opioids are not consumed locally. Alternatively, if pill mills are endogenously located in areas with weaker labor markets, including them in our data could potentially introduce bias.

To address these concerns, we identify counties most likely to have a pill mill and drop those from the analysis. We identify pill mills using the Automation of Reports and Consolidated Orders System (ARCOS) data.[33] We use this data to identify a pill mill as a pharmacy that dispenses opioid MME in the top 5% of the sample. We then drop from the sample the counties with the highest 25% of these pill mills. In Panel A, Table 8, we show our results are robust to dropping these counties. In Panel B, we also show that our results are similar if we exclude Florida, the state which is known for having the highest concentration of pill mills (Spencer, 2019).

V.3 Instrumental Variable

We next employ an instrumental variable analysis to further address concerns that omitted variables, such as local economic conditions, could be driving the relationship between county-level opioid prescription growth and subsequent establishment outcomes. We use opioids prescribed in emergency rooms as our instrument. In contrast to any doctor visit, emergency room visits are urgent and doctors are randomly assigned, thereby mitigating the concern that our results might be driven by intentional drug-seeking behavior on the part of the patient. One potential concern might still be that individuals without jobs and insurance, such as homeless individuals, may come to the emergency room to seek opioids, given emergency room patients have to be treated. However, this does not seem to be an important concern here since our MarketScan data covers only employed patients.

As described in Section III, we construct ER opioid prescriptions using the top 10 most common emergency room diagnoses. We estimate the following two-stage least square specification:

$$\begin{aligned}\Delta Opioid\ prescriptions_{c,t-5} = \gamma \cdot \Delta ER\ Opioid\ prescriptions_{c,t-5} \\ + \mu \cdot \Delta X_{c,t-5} + FE + \Delta\epsilon_{c,t-5}\end{aligned} \tag{4}$$

[33]This data is collected by the Drug Enforcement Agency (DEA) and was made available to the public following a FOIA lawsuit by the Washington Post. The data only covers the two most common opioid formulations:: OxyContin and Hydrocontin. The ARCOS data provides information on the milligrams of active ingredient (MME) dispensed by pharmacy.

$$\Delta y_{i,f,c,t} = \beta \cdot \Delta Opioid\ \widehat{prescriptions}_{c,t-5} + \delta \cdot \Delta X_{c,t-5} + FE + \Delta\epsilon_{i,f,c,t} \qquad (5)$$

where Δ denotes the long (four-year) difference operator, c indexes county, i indexes establishments, f indexes firms and t indexes time period. ΔER *opioid prescriptions* is the change in ER opioid prescription per enrollee. Δ*opioid prescriptions* is the change in opioid prescription per enrollee. Δy is the change in establishment-level outcome variables (sales, employment and investment in IT). ΔX controls for changes in economic and demographic characteristics as well as the underlying cancer rate in the county. FE include firm-period fixed effects and industry-period fixed effects. We double-cluster standard errors at the county and firm levels.

Table 9, column 1, presents the first-stage results. Changes in ER opioid prescriptions significantly predict changes in overall opioid prescriptions. Columns 2-9, report the second-stage results. Our results are statistically significant across specifications and F-stats are well above the 10 threshold. Notably, the magnitudes are similar to those of the OLS estimation presented in Tables 3 and 4. An increase in opioid prescriptions from the 25$^{\text{th}}$ to the 75$^{\text{th}}$ percentile (an increase of 0.3 prescriptions/person) is associated with 1.5% decrease in sales, 0.5% decrease in employment, 4.7% increase in IT budget and 1.9% increase in PCs. In Internet Appendix Tables B8 and B9, we further show that IV estimates controlling for establishment fixed effects and exploiting cross-sectional industry variation in labor replaceability, analogous to those presented in Tables 5 and 6, are similar to the OLS estimates. These results provide corroborating evidence that our findings are not driven by individuals who have poor future job opportunities and thus seek out opioids.

VI Laws to Limit Opioid Prescriptions and Firm Value

The opioid crisis has prompted states to respond. Massachusetts was the first state that passed a law to limit opioid prescriptions. The law imposed a seven-day limit of opioid prescriptions, with exemptions for cancer pain, chronic pain, and for palliative care. According to the local press, the law "comes as Massachusetts grapples with a deadly drug crisis that claims about 100 lives per month".[34] Several states followed with a total of 25 states having passed laws imposing limits on opioid prescriptions by 2018 (Figure 2). A short description of the state laws passed is included in Internet Appendix A. Consistent with the anecdotal evidence from Massachusetts, Internet Appendix Table B10 shows that the only variable that significantly predicts passage of these laws in the cross section of states is the (age-adjusted) opioid overdose death rate, while economic conditions or political economy do not seem to matter.

Given the timing of these laws, we cannot estimate their long-term effects on labor market outcomes or firm performance. Instead, we estimate firms' stock price reaction at the announcement of their passage. We use firms listed in Compustat, CRSP, and CiTDB to estimate the daily average abnormal return for each event date using the Fama-French three- or four-factor model.[35] The estimation period starts 180 days before each event. We require firms to have return observations during the event window and at least 100 return observations in the estimation period. We then regress three-day cumulative abnormal returns, CAR[-1,1] on *law passage* which is 1 for states where a new legislation is passed in the firms' state of headquarters and 0 otherwise. To assign one date for each law's passage, we use the date the law passed

[34] https://www.bostonglobe.com/metro/2016/03/14/baker-due-sign-opioid-bill-monday/EYWh7oJXvKCRguHErxrWhI/story.html

[35] As in the earlier analysis, we drop health-care industries from our analysis as the prediction for health care companies may be different due to the offsetting effect of the decline in opioid prescriptions on their stock prices. We also drop regulated utilities, education, public sector, and agriculture.

the House or Senate, whichever occurred first.[36]

Since all firms show up at each event, we account for the cross-correlation between firms using the portfolio approach suggested by (Jaffe, 1974). Specifically, for each event, we construct portfolios of firms in our sample, weighting by *law passage*. We conduct a time-series analysis using daily returns for the portfolio between [-219,+1], where zero is the day of the event. We then regress daily returns in excess to risk-free returns on a dummy $I\{[-1,+1]\}$ which is one between [-1,+1],and zero between [-219,-2], and Fama-French factors, controlling for event fixed effects and clustering standard errors at the event level. The coefficient estimate on the dummy $I\{[-1,+1]\}$ times 3 (number of event days) should be same as *law passage* with corrected standard errors.

In Table 10, columns 1-2, we consider all firms and use the Fama-French 3-factor and 4-factor model, respectively. On average, firms exhibit a 20 basis points stock price reaction upon the passage of the legislation, statistically significant at the 5% level. In columns 3-6, we divide all firms into two groups for each event depending on whether the share of their employees located in the headquarter state (*HQ. empl. ratio*) is above the sample median or not. We define *HQ empl. ratio* to be the share of a firm's employment in the state of headquarters, using the CiTDB data to collect information on firm's establishment employment. The first group includes firms with headquarter employment ratio in top 50% and the second group includes firms with headquarter employment ratio in bottom 50%. We anticipate larger effects in the former group, firms with a larger share of their workforce impacted by the law change.

In columns 3 and 5 (4 and 6), we use the Fama-French 3-factor (4-factor) model, respectively. We find the *law passage* to be statistically significant in *high HQ empl.*

[36]If a state passed more than one law, we consider the latest state action as the need for a second state action suggests the first action imposed too few limitations and was deemed ineffective. In Internet Appendix Table B11, we show the results are robust to dropping from the analysis the four states that passed two laws (Connecticut, Main, Pennsylvania and Rhode Island).

firms in columns 3-4, consistent with the fact that firms who employ a large fraction of their workers in the state of the law passage respond to the law's impact on their labor supply. The effect is also economically important as indicated by the 50 basis point stock price reaction. In contrast, *law passage* is not statistically significant when a low fraction of employees is located in the firm's state of headquarters (*low HQ empl.* firms). Overall, these results indicate that firms benefit from state legislation that intended to limit opioid prescriptions. This is in line with our findings in Section IV.2 that the opioid crisis impedes firm growth.

In columns 7-10, Table 10, we further explore heterogeneity in firms' capital intensity. We proxy capital intensity using the count of PCs over employment pre-treatment, as of 2015 (*PC/empl*).[37] We, thus, divide firms by *PCs/empl* ratio, and construct two portfolios for each event: one includes firms with PC/empl in top 50% and the other one includes firms with PCs/empl in bottom 50%. We find positive and significant daily returns for firms with low capital intensity pre-treatment equal to 60 basis points (columns 7-8), and thus a stronger effect among those firms most reliant on labor. In contrast, we find no effect for high capital intensity firms in columns 9-10. This is consistent with our earlier findings that firms invest in automation to mitigate the negative effect of labor market shortages due to opioids. These results indicate that the set of firms which are less capital intensive, and as such are more exposed to the labor shortages brought by the opioid crisis, benefit the most from states' legislation aimed at reducing opioid abuse.

VII Conclusion

The current opioid crisis was fueled, to a large extent, by physician prescriptions. Physicians prescribed opioids in a belief that this drug could improve the well-being of their patients, by reducing pain with minimal risk of addiction. Unfortunately,

[37] We define capital intensity as PCs over employment as both PCs and employment measure the stock of capital and labor, respectively.

it turned out that opioids did indeed pose a significant risk of addiction, ultimately impairing health of those abusing them. This negative health shock, in turn, has implications for the supply of productive workers as evidenced by our finding that individuals that were prescribed opioids are less likely to be employed five years later, as compared to otherwise similar individuals who were not prescribed opioids when they visited a doctor for the same medical condition.

This is the first paper to document the negative effects of opioids on (long-term) firm growth and valuations via this labor supply channel. We show that establishments located in counties that experience a higher growth in opioid prescription rates have lower employment and sales growth, as compared to establishments within the same firm located in low opioid growth counties. We also show that firms respond to labor shortages due to opioids by investing in automation technologies to substitute away from labor towards capital. We show a positive and significant relation between the growth in opioid prescriptions and subsequent IT investments with a more pronounced effect for industries where labor is more easily substitutable by technology.

Our findings imply that firms mitigate some of the costs that would otherwise be anticipated from a reduction in the labor supply. This response, however, changes the production processes at firms, a change which can have lasting negative impacts on the local labor markets for those jobs at risk of technology substitution.

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Appendix: Variable Definitions

Individual-level variables:

Doctor opioid intensity is the count of the doctor's patients who subsequently fill an opioid prescription (within 7 days) following an outpatient service normalized by the doctor's total outpatient services. Source: MarketScan.

Opioid prescribed is one if the individual is prescribed opioids for the first time in our sample in a given year, zero otherwise. Source: MarketScan.

County-level variables:

Opioid prescriptions is the count of total opioid prescriptions normalized by number of enrollees in a given county. Source: MarketScan.

ER opioid prescriptions is calculated by assuming patients with a diagnosis code among the top ten most common emergency room diagnoses were indeed treated in an emergency room. We then use the count of opioid prescriptions filled (within 7 days) of these visits normalized by number of enrollees in a given county. Source: MarketScan.

Income is the median household income in a given county. Source: Census.

Population is the total population in a given county. Source: Census.

White ratio is the white population divided by the total population in a given county. Source: Census.

$Age\ 20-64\ ratio$ is the population between ages 20 and 64 divided by total population in a given county. Source: Census.

$Age\ 65+\ ratio$ is the population at or above 65 years old divided by the total population in a given county. Source: Census.

Neoplasms mortality is the number of deaths due to neoplasms (cancer), normalized by population times 1000 at a given county. Source: CDC (https://wonder.cdc.gov/ucd-icd10.html).

Establishment-level variables:

IT budget is the total IT budget for the establishment. Source: CiTDB.

PC is the total number of personal computers in the establishment. Source: CiTDB.

Sales is the estimated revenue for the establishment. Source: CiTDB.

Employment is the total number of employees in the establishment. Source: CiTDB.

High labor replaceability is an indicator equal to one if an establishment belongs to an industry whose labor replaceability is higher than the sample median, and zero otherwise. Labor replaceability is the fraction of each industry's hours worked in 2000 that was performed by occupations prone to be replaced by robots (Graetz and Michaels, 2018). Source: American Community Surveys.

State legislation analysis variables:

HQEmplratio is the share of a firm's employment (observed in CiTDB) in the given firm's headquarter's state. Source: CiTDB and Compustat.

Low PC Empl is one if the stock of installed PCs at the firm level over the number of employees in the firm, measured in 2015, is below the sample median, and zero otherwise. Source: CiTDB.

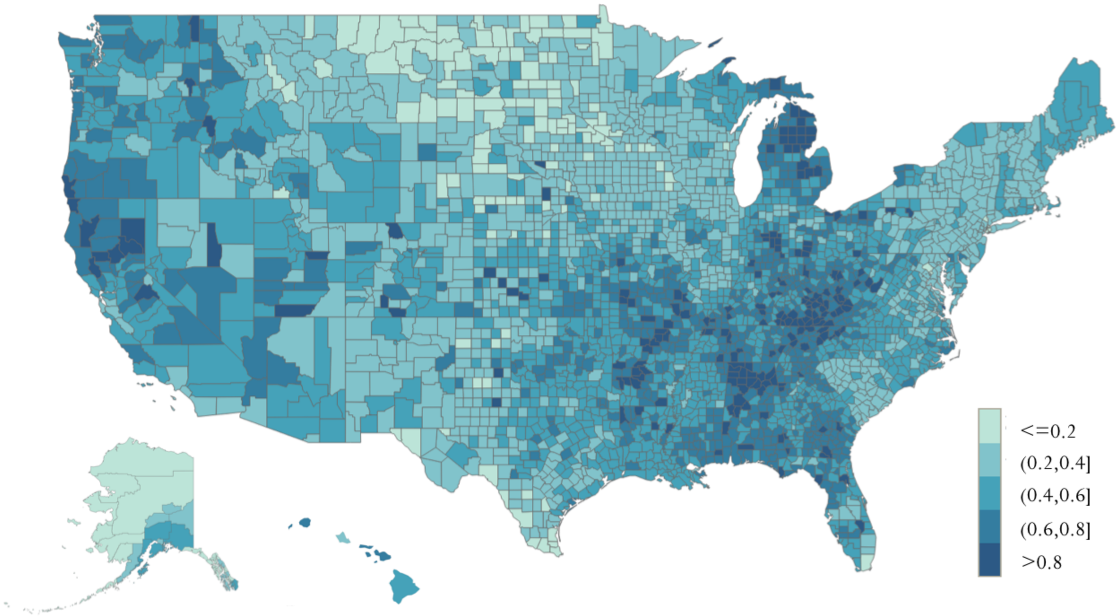

Figure 1: Map of Opioid Prescriptions

This figure plots the distribution of opioid prescriptions per enrollee across U.S counties based on opioid prescription rates from MarketScan over 2001-2010.

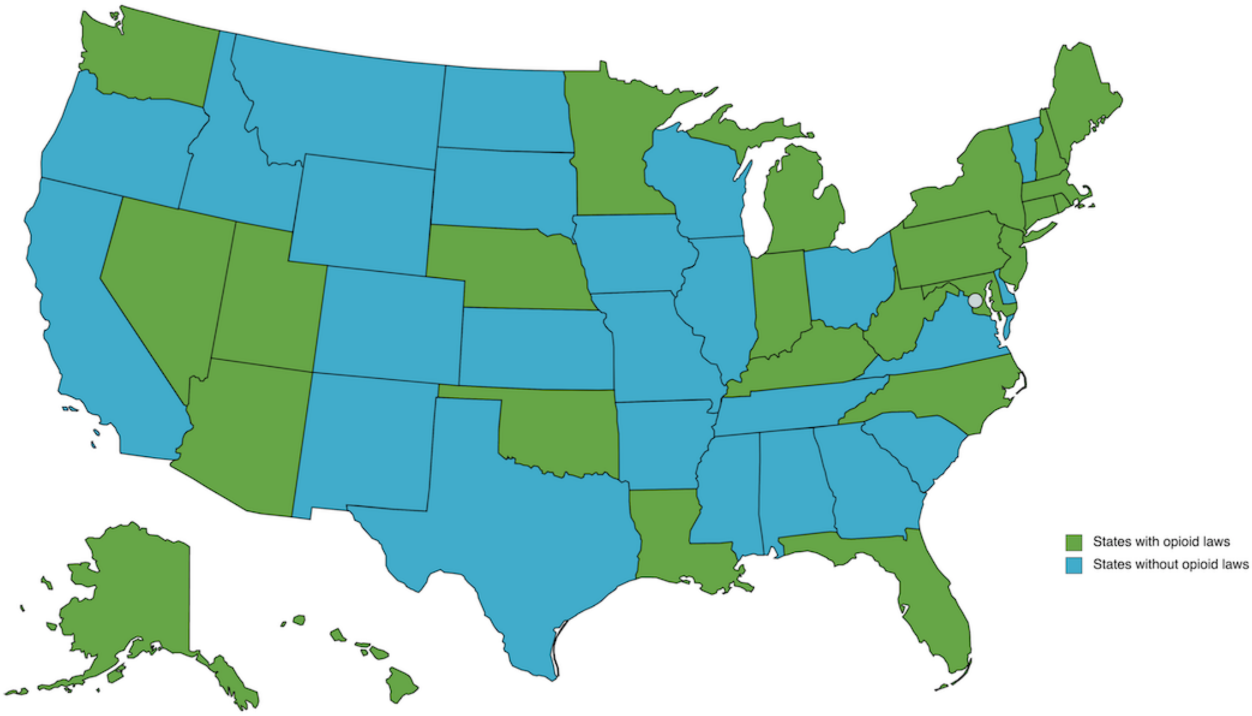

Figure 2: Laws to Limit Opioid Abuse

This figure plots the distribution of laws intended to limit opioid abuse. States that passed at least one law between 2016 and 2018 are in green and states without such legislation are in blue.

Table 1: Summary Statistics

This table reports descriptive statistics. Panel A reports summary statistics on opioid prescriptions, demographic and economic variables at the county level. Panel B reports summary statistics on establishment-level variables. All variables are defined in the Appendix and winsorized at the 1% level.

Variables	N	Mean	Median	Std. Dev.
		Panel A. County − level variables		
Opioid prescriptions (per enrollee)	27,067	0.48	0.47	0.22
ER opioid prescriptions (per enrollee)	27,067	0.03	0.03	0.02
Population (1000)	27,067	88.19	27.27	183.81
Income ($1000)	27,067	40.57	38.82	10.41
White ratio (%)	27,067	86.35	93.22	15.82
Age 20-64 ratio (%)	27,067	57.85	58.01	3.20
Age above 65 ratio (%)	27,067	15.11	14.79	3.96
Neoplasms mortality (per 1000)	27,067	2.31	2.28	0.66
		Panel B. Establishment − level variables		
Sales ($million)	2,176,129	37.63	10.00	228.22
Employment	2,176,142	120.61	45.00	396.16
IT budget ($1000)	2,154,962	525.89	139.72	2627.22
PCs	2,168,489	83.64	31.00	389.16

Table 2: Opioids and Individual Employment

This table presents two-stage least squares estimations examining whether individuals who take opioids between 2001-2010 are more likely to be employed 5 years later (2006-2015). Treated individuals receive their first opioid prescription at year t. For each treated individual, we identify one control individual who lives in the same county, receives the same medical diagnosis when they visit the doctor the same year, and is of the same age and gender. The sample is limited to individuals who are identified as full-time employees between the ages of 18 and 60 and have not received a prior opioid prescription. *Opioid prescribed* indicator is instrumented by *doctor opioid intensity*. The sample in columns 1-2 includes all individuals in our sample. The sample in columns 3-4 excludes doctors with opioid intensity in the top 10% of our sample. The sample in columns 5-6 excludes individuals in counties with Gini index in the top 10% of our sample. The sample in columns 7-8 includes individuals diagnosed with the most common ten emergency room diagnoses. Definition of most common ten emergency room diagnoses follows AAPC. Columns 1, 3, 5 and 7 present first-stage results. Columns 2, 4, 6 and 8 present second-stage results. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are clustered at the county level and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Full sample		Exclude 10% high opioid doctors		Exclude 10% high income inequality counties		ER visits	
	First stage: Opioid prescribed	Second stage: Employed at t+5	First stage: Opioid prescribed	Second stage: Employed at t+5	First stage: Opioid prescribed	Second stage: Employed at t+5	First stage: Opioid prescribed	Second stage: Employed at t+5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Doctor opioid intensity	1.122***		1.877***		1.114***		1.026***	
	(0.018)		(0.036)		(0.020)		(0.040))	
Opioid prescribed		-0.037***		-0.035***		-0.040***		-0.043***
		(0.006)		(0.007)		(0.006)		(0.014)
County-year-gender-age-diagnosis FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Insurance plan FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F statistics		3710		2671		3127		669
Observations	843,809	843,809	715,805	715,805	759,187	759,187	78,994	78,994
R^2	0.057	0.571	0.046	0.573	0.056	0.573	0.052	0.556

Table 3: Opioids and Establishment Growth

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment sales and employment over 2007-2011 and 2011-2015, respectively. Controls are measured as changes over 2002-2006 and 2006-2010. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)		Δln(Emp.)	
	(1)	(2)	(3)	(4)
ΔOpioid prescriptions	-0.048**	-0.056**	-0.019***	-0.019***
	(0.022)	(0.022)	(0.006)	(0.006)
Δln(Income)		0.033		-0.011
		(0.044)		(0.014)
Δln(Population)		0.103***		0.003
		(0.034)		(0.010)
ΔWhite ratio		0.003		-0.000
		(0.002)		(0.001)
ΔAge 20-64 ratio		-0.003		-0.002
		(0.005)		(0.001)
ΔAge above 65 ratio		0.006		-0.000
		(0.005)		(0.001)
ΔNeoplasms mortality		-0.028***		-0.002
		(0.009)		(0.003)
Firm-period FE	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes
Observations	300,658	300,658	300,658	300,658
R^2	0.752	0.752	0.258	0.258

Table 4: Opioids and IT Investment

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment IT investment over 2007-2011 and 2011-2015, respectively. The dependent variables are changes in the logarithm of IT budget in column 1, the logarithm of IT budget by sales in column 2, the logarithm of IT budget by employment in column 3, the logarithm of PCs in column 4, the logarithm of PCs by sales in column 5, and the logarithm of PCs by employment in column 6. Controls are measured as changes over 2002-2006 and 2006-2010. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(IT budget)	Δln(IT budget/ sales)	Δln(IT budget/ emp.)	Δln(PCs)	Δln(PCs/ sales)	Δln(PCs/ emp.)
	(1)	(2)	(3)	(4)	(5)	(6)
ΔOpioid prescriptions	0.116***	0.178***	0.124***	0.063***	0.094***	0.029***
	(0.042)	(0.045)	(0.038)	(0.020)	(0.021)	(0.008)
Δln(Income)	0.200**	0.145*	0.197**	0.091**	-0.006	0.032**
	(0.090)	(0.084)	(0.078)	(0.044)	(0.041)	(0.015)
Δln(Population)	-0.151*	-0.225***	-0.142**	-0.041	-0.054**	0.001
	(0.080)	(0.071)	(0.071)	(0.030)	(0.025)	(0.011)
ΔWhite ratio	0.001	-0.004	0.002	0.003**	0.001	0.002***
	(0.005)	(0.004)	(0.004)	(0.002)	(0.002)	(0.001)
ΔAge 20-64 ratio	0.011	0.023**	0.012	-0.000	0.006	0.002
	(0.010)	(0.009)	(0.009)	(0.004)	(0.003)	(0.002)
ΔAge above 65 ratio	0.006	0.011	0.005	0.001	0.001	0.002
	(0.011)	(0.010)	(0.009)	(0.005)	(0.004)	(0.002)
ΔNeoplasms mortality	-0.017	0.002	-0.011	-0.008	0.011	-0.002
	(0.020)	(0.019)	(0.018)	(0.008)	(0.007)	(0.003)
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	286,073	272,642	286,073	298,288	284,790	298,288
R^2	0.360	0.447	0.407	0.421	0.676	0.592

Table 5: Robustness: Establishment Fixed Effects

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment outcomes over 2007-2011 and 2011-2015, respectively, with establishment fixed effects. The dependent variables are changes of the logarithm of sales in column 1, the logarithm of employment in column 2, the logarithm of IT budget in column 3, the logarithm of IT budget by sales in column 4, the logarithm of IT budget by employment in column 5, the logarithm of PCs in column 6, the logarithm of PCs by sales in column 7, and the logarithm of PCs by employment in column 8. Controls include all additional variables included in Table 4. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(IT budget/ emp.)	Δln(PCs)	Δln(PCs/ sales)	Δln(PCs/ emp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔOpioid prescriptions	-0.030*	-0.016	0.223**	0.275***	0.216***	0.129***	0.119***	0.054***
	(0.017)	(0.012)	(0.092)	(0.090)	(0.080)	(0.034)	(0.027)	(0.013)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-peroid FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	118,716	118,716	110,644	110,312	110,644	117,436	117,078	117,436
R^2	0.860	0.598	0.625	0.619	0.610	0.680	0.759	0.716

Table 6: Heterogeneous Effects: The Labor Channel

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment outcomes over 2007-2011 and 2011-2015, respectively, exploring heteogeneity on industry labor replaceability. The dependent variables are changes of the logarithm of sales in column 1, the logarithm of employment in column 2, the logarithm of IT budget in column 3, the logarithm of IT budget by sales in column 4, the logarithm of IT budget by employment in column 5, the logarithm of PCs in column 6, the logarithm of PCs by sales in column 7, and the logarithm of PCs by employment in column 8. Controls include all additional variables included in Table 4. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(IT budget/ emp.)	Δln(PCs)	Δln(PCs/ sales)	Δln(PCs/ emp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔOpioid prescriptions	-0.127***	-0.031***	-0.115*	0.021	-0.081	-0.023	0.085***	-0.001
	(0.039)	(0.010)	(0.069)	(0.064)	(0.062)	(0.030)	(0.031)	(0.012)
ΔOpioid pres. × high labor repl.	0.139***	0.020*	0.395***	0.250***	0.352***	0.124***	-0.010	0.044***
	(0.043)	(0.012)	(0.089)	(0.084)	(0.078)	(0.032)	(0.033)	(0.011)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	246,190	246,190	233,978	221,235	233,978	244,178	231,378	244,178
R^2	0.670	0.264	0.377	0.416	0.428	0.415	0.607	0.583

Table 7: Robustness: Local Economic Conditions

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment outcomes over 2007-2011 and 2011-2015, respectively, estimated on four different subsamples. In Panel A, we exclude from the sample counties at the bottom quartile of the household income distribution as of 2000. In Panel B, we exclude from the sample establishments in manufacturing industries (NAICS 31-33). In Panel C, we exclude from the sample the top quartile of counties with the most exposure to Chinese imports as of 2000, following Autor et al. (2003). In Panel D, we include in the sample only tradable industries. We define the set of tradable industries following Delgado et al. (2014). The dependent variables are changes of the logarithm of sales in column 1, the logarithm of employment in column 2, the logarithm of IT budget in column 3, the logarithm of IT budget by sales in column 4, the logarithm of PCs in column 5, and the logarithm of PCs by sales in column 6. Controls include all additional variables included in Table 4. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(PCs)	Δln(PCs/ sales)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Exclude counties with lowest household income						
ΔOpioid prescriptions	-0.054*	-0.022***	0.105*	0.183***	0.071**	0.103***
	(0.032)	(0.007)	(0.057)	(0.064)	(0.029)	(0.032)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	219,797	219,797	208,952	198,951	217,996	207,932
R^2	0.760	0.265	0.366	0.452	0.420	0.672
Panel B. Exclude establishments in manufacturing industries						
ΔOpioid prescriptions	-0.122***	-0.029***	0.001	0.136**	0.034	0.120***
	(0.030)	(0.007)	(0.054)	(0.057)	(0.024)	(0.027)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	218,404	218,404	207,624	202,548	216,524	211,409
R^2	0.794	0.259	0.387	0.474	0.465	0.701

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(PCs)	Δln(PCs/ sales)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C. Exclude counties with most exposure to Chinese imports						
ΔOpioid prescriptions	-0.071***	-0.023***	0.092*	0.171***	0.033	0.082***
	(0.025)	(0.006)	(0.050)	(0.049)	(0.022)	(0.022)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	219,853	219,853	209,063	199,449	218,085	208,409
R^2	0.755	0.265	0.367	0.454	0.432	0.685
Panel D. Tradeable industries						
ΔOpioid prescriptions	-0.040	-0.019***	0.134***	0.173***	0.065***	0.078***
	(0.026)	(0.007)	(0.047)	(0.049)	(0.020)	(0.021)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	224,072	224,072	213,133	201,913	222,395	211,118
R^2	0.743	0.258	0.310	0.431	0.387	0.657

Table 8: Robustness: Excluding Pill Mill Counties and Florida

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment outcomes over 2007-2011 and 2011-2015, respectively, estimated on two different subsamples. In Panel A, we exclude from the sample the top quartile of counties with the most pill mill pharmacies. We use ARCOS (available since 2006) and rank all pharmacies by MME of oxycodone and hydrocodone pills received in 2006. We classify the top 5% of pharmacies as pill mill pharmacies. In Panel B, we exclude counties in Florida. The dependent variables are changes of the logarithm of sales in column 1, the logarithm of employment in column 2, the logarithm of IT budget in column 3, the logarithm of IT budget by sales in column 4, the logarithm of PCs in column 5, and the logarithm of PCs by sales in column 6. Controls include all additional variables included in Table 4. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(PCs)	Δln(PCs/ sales)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Exclude pill mill counties						
ΔOpioid prescriptions	-0.058**	-0.019***	0.117**	0.190***	0.063***	0.097***
	(0.023)	(0.007)	(0.048)	(0.050)	(0.020)	(0.022)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	222,910	222,910	212,090	201,966	221,194	211,036
R^2	0.752	0.267	0.364	0.450	0.429	0.682
Panel B. Exclude Florida						
ΔOpioid prescriptions	-0.043**	-0.015***	0.070*	0.116***	0.067***	0.085***
	(0.017)	(0.005)	(0.040)	(0.043)	(0.020)	(0.019)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	348,832	348,832	333,642	321,207	346,497	334,005
R^2	0.738	0.250	0.315	0.385	0.384	0.620

Table 9: 2SLS: Emergency Room Opioids Instrument

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment outcomes over 2007-2011 and 2011-2015, respectively, where changes in opioid prescriptions are instrumented by changes in emergency room opioid prescription rates. Column 1 presents first-stage results. Columns 2-7 present second-stage results. The dependent variables are changes of the logarithm of sales in column 2, the logarithm of employment in column 3, the logarithm of IT budget in column 4, the logarithm of IT budget by sales in column 5, the logarithm of IT budget by employment in column 6, the logarithm of PCs in column 7, the logarithm of PCs by sales in column 8, and the logarithm of PCs by employment in column 9. Controls include all additional variables included in Table 4. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	ΔOpioid prescriptions	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(IT budget/ emp.)	Δln(PCs)	Δln(PCs/ sales)	Δln(PCs/ emp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ΔER opioid prescriptions	11.226***								
	(0.355)								
ΔOpioid prescriptions		-0.049*	-0.018**	0.156***	0.223***	0.166***	0.062**	0.086***	0.029***
		(0.026)	(0.007)	(0.053)	(0.054)	(0.049)	(0.027)	(0.026)	(0.010)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-statistics		1002	1002	1005	994	1005	1000	989	1000
Observations	300,658	300,658	300,658	286,073	272,642	286,073	298,288	284,790	298,288
R^2	0.751	0.752	0.258	0.360	0.447	0.407	0.421	0.676	0.592

Table 10: Abnormal Returns around the Passage of State Laws on Opioids

This table presents firm abnormal returns around the first passage (through the House or the Senate) of laws intended to limit opioid prescriptions. The sample in columns 1-2 includes all U.S. firms listed in both Compustat, and CRSP that can be matched to CiTDB. In columns 3-6, we explore heterogeneity based on the share of employees in the state of firms' headquarters (*HQ empl.*). The sample in columns 3-4 includes firms with headquarter employment ratio in top 50% of our sample. The sample in columns 5-6 includes firms with headquarter employment ratio in bottom 50% of our sample. In columns 7-10, we explore heterogeneity based on pre-treatment firms' capital intensity (*PCs/empl.*). The sample in columns 7-8 includes firms with PCs/empl. ratio in bottom 50% of the sample. The sample in columns 9-10 includes firms with PCs/empl. ratio in top 50% of the sample. Odd columns use the Fama-French three-factor model. Even columns use the Fama-French four-factor model. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the state and event date levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	CAR[-1,1]									
	All firms		*High HQ empl.*		*Low HQ empl.*		*Low PCs/empl.*		*High PCs/empl.*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Law passage	0.002**	0.002**	0.005***	0.005***	-0.001	-0.001	0.006***	0.006***	-0.002	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firms	2695	2695	1348	1348	1347	1347	1356	1356	1339	1339

The Impact of the Opioid Crisis on Firm Value and Investment

Paige Ouimet, Elena Simintzi, and Kailei Ye

INTERNET APPENDIX

Internet Appendix A

Description of MarketScan

The MarketScan database from Truven Health Analytics is one of the largest multi-source healthcare databases. It includes private employment-based insurance benefit plan health data for 37.8 million individuals, consisting of claims and encounter records. The data is provided to MarketScan by employers.[1] The data covers the primary individual, employed at the firm providing coverage, as well as dependents. Although the individuals in the MarketScan data are younger, higher-income and more likely to be employed, as compared to the general U.S. population (Berg and Chattopadhyay, 2004), the MarketScan population is representative of the U.S. population covered by health insurance (Adamson et al., 2008). Aizcorbe et al. (2015) find that the distribution of enrollees in MarketScan is very similar to those in the Medical Expenditure Panel Survey. MarketScan data has been widely used in the health care literature (e.g. Florence et al., 2016; Ali et al., 2019; Roberts et al., 2020; Suda et al., 2020).

In Table A1, we compare MarketScan enrollees to the U.S. population covered by private insurance. Column 1 shows the counts of MarketScan enrollees (in millions) over 2001-2010. On average, MarketScan covers 29 million enrollees each year, including nearly 20 million individuals between 18 and 64 years old, 6.7 million individuals under 18 years old, and 2.6 million individuals over 65 years old. In column 2, we show the U.S. population by age using data obtained from the Bureau of Labor Statistics. MarketScan covers around 10% of the U.S. population, which remains stable across age bins. In columns 3 and 4, we show the population covered by private insurance, and specifically, employment-based private insurance. On average, MarketScan covers around 12% of the insured population and 16% of the population with employment-based private insurance.

[1]No information on the employers is made available to researchers.

Table A1: Distribution of MarketScan Enrollees by Age

This table reports MarketScan enrollees and U.S. population by age. Column 1 shows the number of enrollees covered by MarketScan. Column 2 presents the U.S. population. Column 3 shows the U.S. population covered by private health insurance. Column 4 presents the U.S. population covered by employment-based health insurance. Population data is from the Bureau of Labor Statistics. All columns present averages over 2001-2010.

	MarketScan enrollees (mm)	Population (mm)		
		Total	Private health insurance	Employment based health insurance
	(1)	(2)	(3)	(4)
Total	29.0	294.9	251.2	176.9
Under 18	6.7	74.0	66.4	44.7
18-24	2.8	28.3	20.5	14.0
25-34	3.7	39.9	29.9	24.5
35-44	4.4	42.5	34.8	29.4
45-54	4.9	42.6	36.3	30.4
55-64	4.1	31.4	27.6	21.3
65 and older	2.6	36.2	35.7	12.7

Next, we compare the geographic distribution of MarketScan enrollees to the distribution of employment. We compute the number of MarketScan enrollees, averaged over 2001-2010, by state and rank the states based on the quintiles of this distribution. We present the distribution of MarketScan enrollees across states in Figure A1. States in darker shades are those having more MarketScan enrollees. In Figure A2, we instead present the distribution of U.S. employment by state using data from the Bureau of Labor Statistics. The figures show that the two distributions are similar across states. For example, the states of California, Texas, Florida, Illinois, Michigan, Ohio and New York have most MarketScan enrollees as well as population.

It is important for our analyses to examine the magnitude of employee attrition in MarketScan. To this end, we compute the percentage of employees that remain employed among firms covered by MarketScan. To be consistent with Table 2, we consider full-time employees between 18 and 60 years old. We find that around 95%

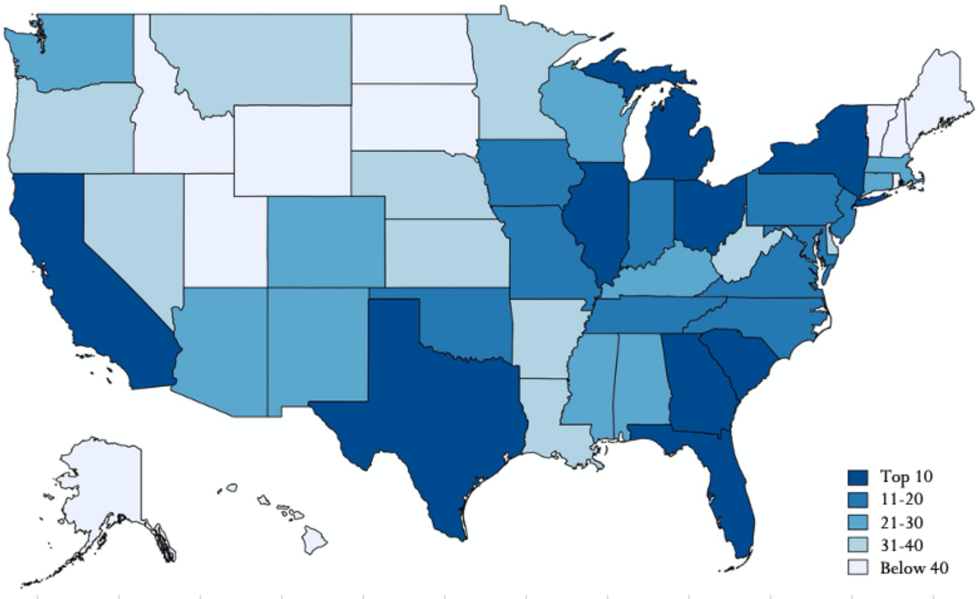

Figure A1: Distribution of MarketScan Enrollees by State

This map plots the quintiles of the average number of MarketScan enrollees by state over 2001-2010. States with darker shades rank at the top and those with lighter shades rank at the bottom. Source: MarketScan.

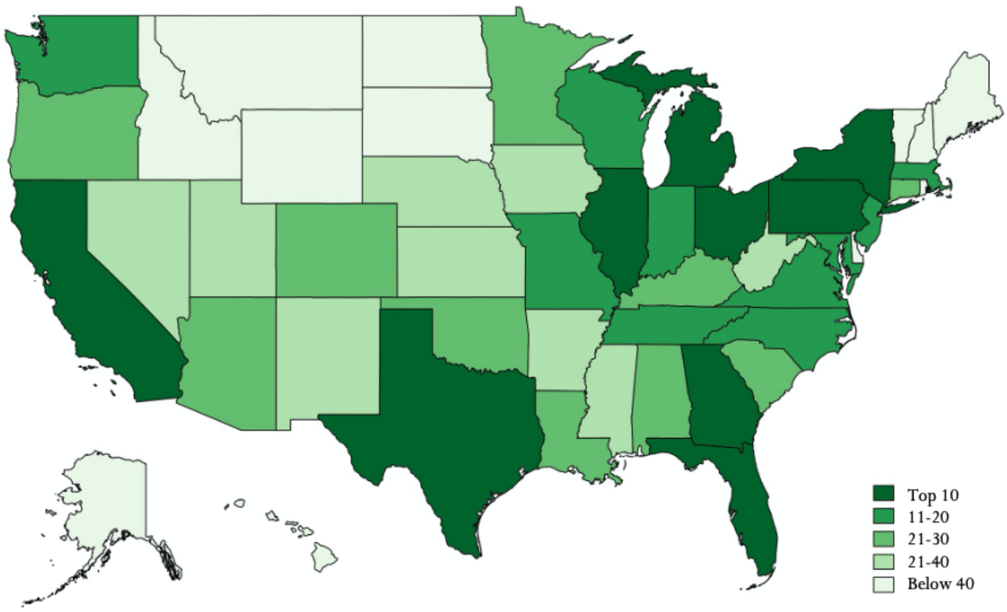

Figure A2: Distribution of U.S. Employment by State

This map plots the quintiles of the average U.S. employment by state over 2001-2010. States with darker shades rank at the top and those with lighter shades at the bottom. Source: Bureau of Labor Statistics.

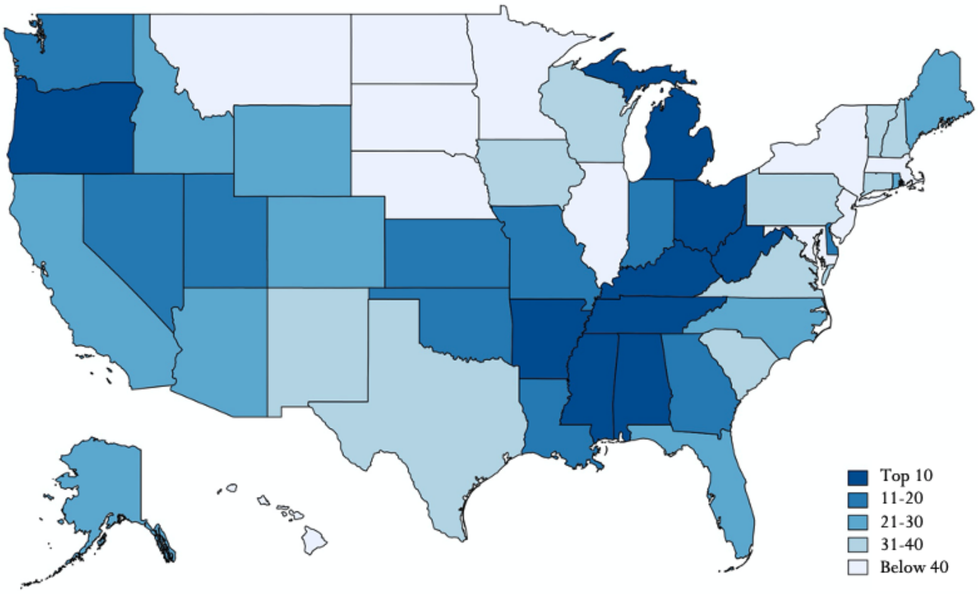

Figure A3: Distribution of MarketScan Opioid Prescriptions by State

This map plots the quintiles of the average opioid prescriptions per 100 enrollees by state over 2006-2010. States with darker shades rank at the top and those with lighter shades rank at the bottom. Source: MarketScan.

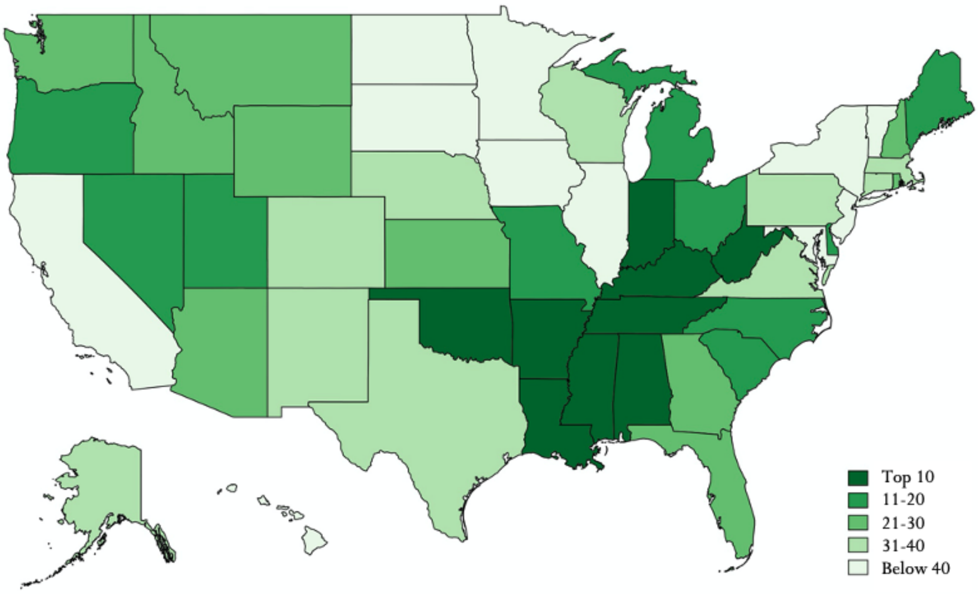

Figure A4: Distribution of CDC Opioid Prescriptions by State

This map plots the quintiles of the average opioid prescriptions per 100 persons by state over 2006-2010. States with darker shades rank at the top and those with lighter shades rank at the bottom. Source: Centers for Disease Control and Prevention.

are still employed as full-time workers one year after the first year they show up in the sample. Babina et al. (2020) analyze worker attrition using data from Current Population Survey (CPS) and find that 83% of the original private sector workers are still reportedly employed in the private sector after one year. This comparison indicates a lower level of job attrition rate in our sample.

Lastly, we compare opioid prescriptions in MarketScan to those reported by the Centers for Disease Control and Prevention. County-level opioid data from MarketScan are available until 2010, while the CDC opioid data are available since 2006. We thus compare the two data sets over the overlapping sample period 2006-2010. We compute the average opioid prescriptions per 100 enrollees by state using data from MarketScan and plot the quintiles in Figure A3. States in darker shades are those having more opioid prescriptions per enrollee. In Figure A4, we plot the quintiles of average opioid prescriptions per 100 persons by state using data from CDC. The figures show the two distributions are similar across states. For example, West Virginia, Kentucky, Tennessee, Alabama, Mississippi, and Arkansas are more heavily impacted by the opioid crisis as measured by both MarketScan and CDC.

Description of CiTDB

Information technology data from the Computer Intelligence Technology Database (CiTDB) has been widely used in the literature to measure technology adoption (see, for example, Brynjolfsson and Hitt, 2003; Bloom et al., 2014; Tambe et al., 2014).

Although CiTDB is a survey of U.S. establishments, we show that it is representative of the aggregate statistics. First, we examine the distribution of CiTDB employment by industry and compare it to the Census U.S. employment by industry. We present the results in Table A2. Column 1 shows CiTDB employment averaged over 2007-2015, by industry. Column 2 presents instead U.S. employment averaged over 2007-2015, by industry, using data from Census Quarterly Workforce Indicators (QWI). Overall, CiTDB covers approximately 72% of U.S. employment. There is

Table A2: Distribution of CiTDB and U.S. Employment by Industry

This table reports CiTDB and U.S. employment and counts of establishments by industry. Column 1 presents employment (in millions) in CiTDB establishments. Column 2 presents employment (in millions) from Census Quarterly Workforce Indicators. Column 3 shows the number of establishments covered by CiTDB (in thousands). Column 4 presents the number of establishments from Census Statistics of U.S. Businesses (in thousands). All numbers are averages over 2007-2015.

	CiTDB employment (mm)	US employment (mm)	CiTDB establishments (1000)	US establishments (1000)
	(1)	(2)	(3)	(4)
Total	81.2	111.9	2040.4	6746.1
Agriculture	0.6	1.1	19.8	18.4
Mining and logging	0.5	0.7	9.8	25.3
Utilities	0.7	0.5	14.2	17
Construction	3.8	5.9	126.6	592.8
Manufacturing	14	12.2	225.8	285.6
Wholesale trade	4.2	5.7	112.5	386.3
Retail trade	1.8	15.5	43.5	1004.1
Transportation and warehousing	3.2	4.3	80.3	191.2
Information	3.2	2.9	85.8	124.2
Financial activities	6.4	7.6	301.9	757.1
Professional and business services	10.6	18	354.7	1122.8
Education and health services	25.8	19.5	501	850.4
Leisure and hospitality	3.9	13.4	72.9	693
Other services	2.5	4.4	91.7	677.8

variation across sectors. For example, CiTDB covers a higher proportion of employment in the manufacturing sector, and a lower proportion in the retail trade sector. Column 3 shows the number of establishments in CiTBD, by industry. Column 4 presents the number of U.S. establishments, by industry, using data from Census Statistics of U.S. Businesses. On average, CiTDB covers 30% of the establishments in the United States. As such, the establishments covered by CiTDB are relatively larger establishments with more employees.

We further show that employment changes in CiTDB are highly correlated with U.S. employment changes. To this end, we compute percentage changes in CiTDB

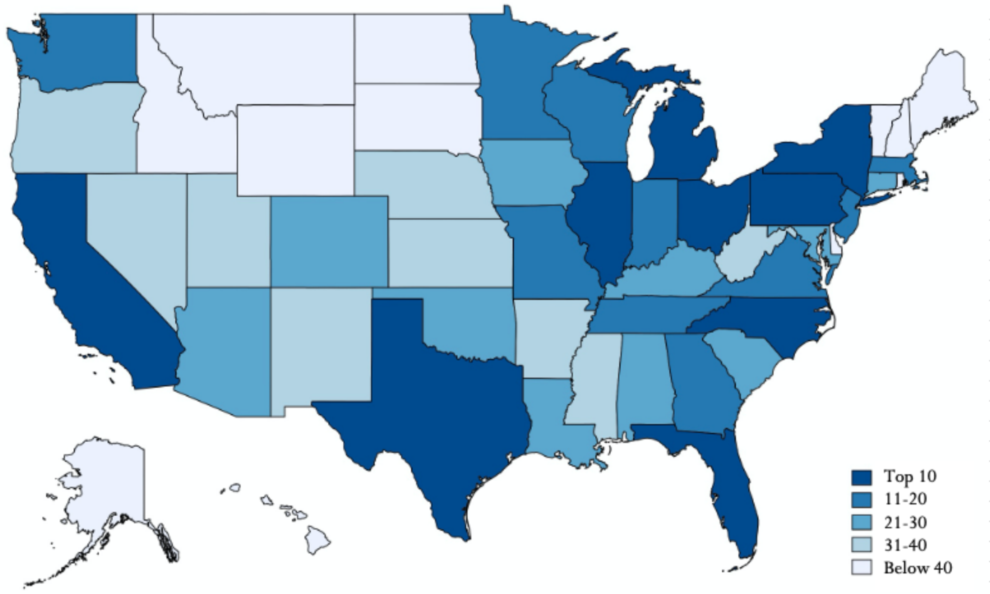

Figure A5: Distribution of CiTDB Employment by State

This map plots the quintiles of the average CiTDB employment by state over 2007-2015. States with darker shades rank at the top and those with lighter shades rank at the bottom. Source: CiTDB.

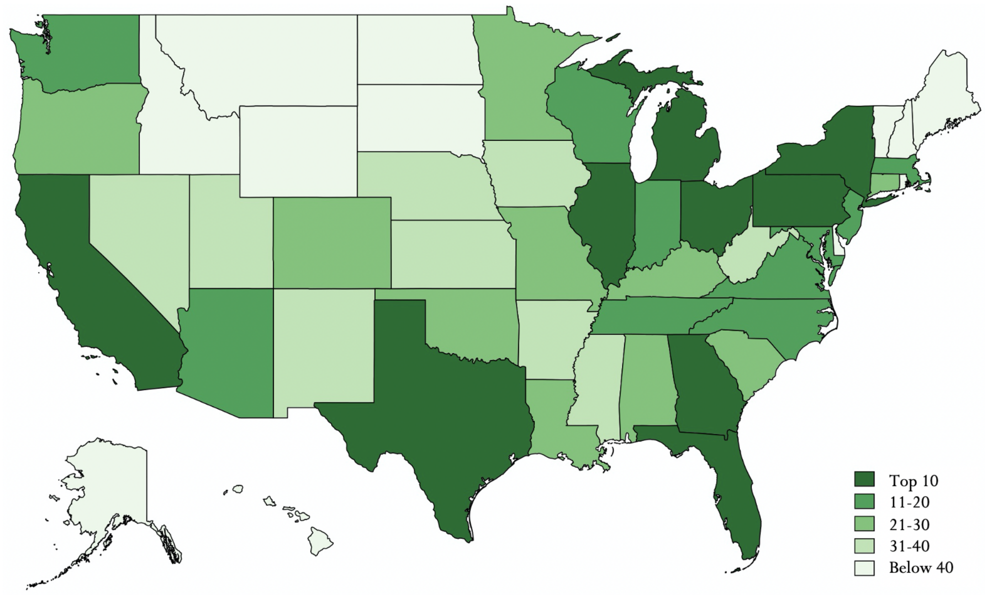

Figure A6: Distribution of U.S. Employment by State

This map plots the quintiles of the average U.S. employment by state over 2007-2015. States with darker shades rank at the top and those with lighter shades rank at the bottom. Source: Bureau of Labor Statistics.

employment in each industry over two periods, 2007-2011 and 2011-2015—following our baseline analysis— using establishments that exist in the sample from the beginning to the end of either period. We then compute percentage changes in U.S. employment, by industry, using QWI data. The correlation estimate between CiTDB employment changes and U.S. employment changes across industries is 0.73 and is statistically significant at the 1% level.

Second, we examine the distribution of CiTDB employment across states and compare it to the U.S. state employment. We compute the average CiTDB employment over 2007-2015 by state and then plot the quintiles in Figure A5. States in darker shades are those that rank higher in terms of CiTDB employment. We plot the quintiles of average U.S. state employment over 2007-2015 in Figure A6. The two distributions look similar. We further estimate a significant correlation of 0.66 at the 1% level between CiTDB employment changes and U.S. employment changes across states over 2007-2011 and 2011-2015.

List of State Laws

By October 2018, 29 laws intended to limit opioid prescriptions have been passed by 25 states in the United States. We list a brief description of these laws below.

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 (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.

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.

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.

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. 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.

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.

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.

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.

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.

Internet Appendix B: Robustness Analysis

Table B1: Robustness: Opioid Prescriptions from CDC

This table presents a first-difference estimation using changes in opioid prescription rates over 2006-2010 and subsequent changes in establishment outcomes over 2011-2015, using opioid prescriptions from CDC. The dependent variables are changes in the logarithm of sales in column 1, the logarithm of employment in column 2, the logarithm of IT budget in column 3, the logarithm of IT budget by sales in column 4, the logarithm of PCs in column 5, the logarithm of PCs by sales in column 6. Controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(PCs)	Δln(PCs/ sales)
	(1)	(2)	(3)	(4)	(5)	(6)
ΔOpioid prescriptions	-0.050***	-0.001	0.012	0.008	0.040**	0.044**
	(0.018)	(0.004)	(0.037)	(0.039)	(0.020)	(0.018)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	231,082	231,082	222,985	209,756	228,772	215,499
R^2	0.780	0.211	0.504	0.627	0.497	0.779

Table B2: Robustness: Fixed Effects

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment outcomes over 2007-2011 and 2011-2015, respectively, estimated with different fixed effects combinations. In Panel A, we include industry-period fixed effects. In Panel B, we add firm fixed effects to industry-period fixed effects. The dependent variables are changes of the logarithm of sales in column 1, the logarithm of employment in column 2, the logarithm of IT budget in column 3, the logarithm of IT budget by sales in column 4, the logarithm of PCs in column 5, the logarithm of PCs by sales in column 6. Controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(PCs)	Δln(PCs/ sales)
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
ΔOpioid prescriptions	-0.047**	-0.017***	0.077*	0.117**	0.058***	0.079***
	(0.021)	(0.005)	(0.047)	(0.050)	(0.020)	(0.020)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	300,659	300,659	287,840	275,789	298,548	286,453
R^2	0.675	0.0612	0.189	0.298	0.251	0.561
			Panel B			
ΔOpioid prescriptions	-0.050**	-0.019***	0.109**	0.169***	0.063***	0.091***
	(0.020)	(0.006)	(0.044)	(0.048)	(0.020)	(0.020)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	300,659	300,659	286,945	273,540	298,325	284,858
R^2	0.738	0.174	0.274	0.369	0.342	0.628

Table B3: Establishment Births and Deaths

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in county-level establishment births and deaths over 2007-2011 and 2011-2015, respectively. Establishment data comes from Census Business Dynamics Statistics. The dependent variables are changes of the logarithm of establishment births in column 1, the logarithm of establishment deaths in column 2, the logarithm of establishment expansions in column 3, and the logarithm of establishment contractions in column 4. *Establishment birth* is the count of establishments that have positive employment in the first quarter of a given year and zero employment in the first quarter of the previous year. *Establishment death* is the count of establishments that have zero employment in the first quarter of a given year and positive employment in the first quarter of the previous year. *Establishment expansion* is the count of establishments that have positive employment in the first quarter of a given year, and an increase in employment in the first quarter of the following year. *Establishment contractions* is the count of establishments that have positive employment in the first quarter of a given year, and a decrease in employment in the first quarter of the following year. Controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are clustered at the county level and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Est. birth)	Δln(Est. death)	Δln(Est. expansion)	Δln(Est. contractions)
	(1)	(2)	(3)	(4)
ΔOpioid prescriptions	0.036	0.094***	-0.056**	0.031
	(0.035)	(0.033)	(0.022)	(0.023)
ΔControls	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes
Observations	6,244	6,244	6,244	6,244
R^2	0.511	0.329	0.352	0.353

Table B4: Robustness: Excluding County-level Controls

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment IT investment over 2007-2011 and 2011-2015, estimated without county-level controls. The dependent variables are changes of the logarithm of IT budget in column 1, the logarithm of IT budget by sales in column 2, the logarithm of IT budget by employment in column 3, the logarithm of PCs in column 4, the logarithm of PCs by sales in column 5, and the logarithm of PCs by employment in column 6. Controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(IT budget)	Δln(IT budget/ sales)	Δln(IT budget/ emp.)	Δln(PCs)	Δln(PCs/ sales)	Δln(PCs/ emp.)
	(1)	(2)	(3)	(4)	(5)	(6)
ΔOpioid prescriptions	0.115***	0.164***	0.124***	0.068***	0.091***	0.032***
	(0.042)	(0.044)	(0.038)	(0.019)	(0.020)	(0.008)
ΔControls	No	No	No	No	No	No
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	286,073	272,642	286,073	298,288	284,790	298,288
R^2	0.360	0.447	0.407	0.421	0.676	0.592

Table B5: Robustness: Excluding Headquarters

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment outcomes over 2007-2011 and 2011-2015, respectively, excluding headquarter establishments. The dependent variables are changes in the logarithm of sales in column 1, the logarithm of employment in column 2, the logarithm of IT budget in column 3, the logarithm of IT budget by sales in column 4, the logarithm of PCs in column 5, the logarithm of PCs by sales in column 6. Controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(PCs)	Δln(PCs/ sales)
	(1)	(2)	(3)	(4)	(5)	(6)
ΔOpioid prescriptions	-0.062***	-0.016**	0.102**	0.175***	0.064***	0.097***
	(0.023)	(0.006)	(0.046)	(0.049)	(0.021)	(0.023)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	227,903	227,903	214,328	204,316	225,646	215,572
R^2	0.747	0.234	0.349	0.435	0.434	0.686

Table B6: Robustness: Commuting Zones

This table presents a first-difference estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment outcomes over 2007-2011 and 2011-2015, respectively, at the commuting zone level. The dependent variables are changes in the logarithm of sales in column 1, the logarithm of employment in column 2, the logarithm of IT budget in column 3, the logarithm of IT budget by sales in column 4, the logarithm of PCs in column 5, the logarithm of PCs by sales in column 6. Controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the commuting zone and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(PCs)	Δln(PCs/ sales)
	(1)	(2)	(3)	(4)	(5)	(6)
ΔOpioid prescriptions	-0.065**	-0.023***	0.100*	0.168**	0.064**	0.098***
	(0.028)	(0.006)	(0.058)	(0.065)	(0.029)	(0.030)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	298,500	298,500	284,016	270,679	296,150	282,748
R^2	0.752	0.259	0.360	0.447	0.421	0.676

Table B7: Robustness: By Periods

This table presents a first-difference estimation using changes in opioid prescription rates and subsequent changes in establishment outcomes, estimated over separate periods. Panel A considers changes in opioid prescription rates over 2002-2006 and subsequent changes in establishment outcomes over 2007-2011. Panel B considers changes in opioid prescription rates over 2006-2010 and subsequent changes in establishment outcomes over 2011-2015. The dependent variables are changes in the logarithm of sales in column 1, the logarithm of employment in column 2, the logarithm of IT budget in column 3, the logarithm of IT budget by sales in column 4, the logarithm of PCs in column 5, the logarithm of PCs by sales in column 6. Controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(PCs)	Δln(PCs/ sales)
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. 2007 − 2011			
ΔOpioid prescriptions	-0.026	-0.030	0.124	0.176	0.112***	0.146***
	(0.017)	(0.019)	(0.113)	(0.107)	(0.040)	(0.029)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	66,039	66,039	59,748	59,742	66,039	66,024
R^2	0.221	0.228	0.263	0.270	0.349	0.422
			Panel B. 2011 − 2015			
ΔOpioid prescriptions	-0.067**	-0.014***	0.106**	0.173***	0.049**	0.075***
	(0.029)	(0.004)	(0.045)	(0.052)	(0.021)	(0.024)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	234,619	234,619	226,325	212,900	232,249	218,766
R^2	0.778	0.209	0.464	0.596	0.499	0.775

Table B8: 2SLS: Establishment Fixed Effects

This table presents a first-difference instrumental variable estimation using instrumented changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment outcomes over 2007-2011 and 2011-2015, respectively, with establishment fixed effects. We use changes in emergency room opioid prescription rates to instrument for changes in opioid prescription rates. All columns present second-stage results. The dependent variables are changes in the logarithm of sales in column 1, the logarithm of employment in column 2, the logarithm of IT budget in column 3, the logarithm of IT budget by sales in column 4, the logarithm of IT budget by employment in column 5, the logarithm of PCs in column 6, the logarithm of PCs by sales in column 7, and the logarithm of PCs by employment in column 8. Controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(IT budget/ emp.)	Δln(PCs)	Δln(PCs/ sales)	Δln(PCs/ emp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔOpioid prescriptions	-0.019	-0.013	0.250	0.294*	0.249*	0.108*	0.091*	0.043*
	(0.031)	(0.022)	(0.153)	(0.151)	(0.135)	(0.065)	(0.049)	(0.024)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-statistics	216	216	216	215	216	217	217	217
Observations	118,716	118,716	110,644	110,312	110,644	117,436	117,078	117,436
R^2	0.861	0.598	0.626	0.620	0.610	0.680	0.759	0.716

Table B9: 2SLS: The Labor Channel

This table presents a first-difference instrumental variable estimation using changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment outcomes over 2007-2011 and 2011-2015, respectively, exploring heteogeneity on industry labor replaceability. We use changes in emergency room opioid prescription rates to instrument for changes in opioid prescription rates. All columns present second-stage results. The dependent variables are changes in the logarithm of sales in column 1, the logarithm of employment in column 2, the logarithm of IT budget in column 3, the logarithm of IT budget by sales in column 4, the logarithm of IT budget by employment in column 5, the logarithm of PCs in column 6, the logarithm of PCs by sales in column 7, and the logarithm of PCs by employment in column 8. Controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the county and firm level and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)	Δln(Emp.)	Δln(IT budget)	Δln(IT budget/ sales)	Δln(IT budget/ emp.)	Δln(PCs)	Δln(PCs/ sales)	Δln(PCs/ emp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔOpioid prescriptions	-0.134***	-0.039***	-0.009	0.137*	0.012	-0.017	0.095**	0.007
	(0.051)	(0.012)	(0.083)	(0.076)	(0.075)	(0.037)	(0.042)	(0.015)
ΔOpioid pres. × high labor repl.	0.180***	0.034**	0.291***	0.116	0.268***	0.112***	-0.050	0.029**
	(0.057)	(0.014)	(0.099)	(0.097)	(0.089)	(0.038)	(0.042)	(0.013)
ΔControls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-statistics	675	675	671	672	671	675	676	675
Observations	246,190	246,190	233,978	221,235	233,978	244,178	231,378	244,178
R^2	0.670	0.264	0.377	0.416	0.428	0.415	0.606	0.583

Table B10: Determinants of Opioids State Laws

This table explores the relation between state opioid-related laws and local economic, demographic, health, and political characteristics. Our sample includes all U.S. states. The dependent variable is an indicator which equals one if a state announces an opioid-related law between 2016 and 2018. Cumulative opioid prescriptions are accumulated between 2001 and 2010. Age-adjusted opioid overdoses death rate is measured by the deaths due to opioid overdoses divided by total population (Source: Centers for Disease Control and Prevention). Unemployment rate is measured by the number of unemployed divided by the sum of employed and unemployed (Source: Bureau of Labor Statistics). Poverty ratio is measured by the number of poverty divided by total population at a given county (Source: Census). Manufacturing rate is measured by the number of employees in manufacturing industries normalized by the number of employees in all industries at a given county (Source: Census Quarterly Workforce Indicators). GSP is measured by the gross domestic product per capita in a given state (Source: Bureau of Economic Analysis). Democratic state is an indicator equal to one if the Democratic Party controls the legislation and the government. Republican state is an indicator equal to one if the Republican Party controls the legislation and the government. All independent variables are defined as of 2015. All variables are winsorized at the 1% level. Standard errors are robust and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Opioid state law indicator			
	(1)	(2)	(3)	(4)
Ln(Cumulative opioid prescriptions)	-0.025	0.041	0.001	0.060
	(0.051)	(0.076)	(0.052)	(0.076)
Age-adjusted opioid overdoses death rate	0.026***	0.020**	0.022**	0.018**
	(0.008)	(0.009)	(0.010)	(0.010)
Unemployment rate		6.235		6.214
		(8.906)		(9.493)
ln(Income)		1.209		1.533
		(1.148)		(1.160)
Poverty ratio		-0.006		-0.000
		(0.065)		(0.061)
Manufacturing ratio		-0.012		-0.011
		(0.022)		(0.023)
ln(GSP per capita)		-0.361		-0.387
		(0.644)		(0.630)
Democratic state			-0.190	-0.304
			(0.214)	(0.197)
Republican state			-0.165	-0.056
			(0.177)	(0.191)
Observations	50	50	50	50
R^2	0.192	0.217	0.151	0.252

Table B11: Abnormal Returns around the Passage of State Laws on Opioids

This table presents firm abnormal returns around the first passage (through the House or the Senate) of laws intended to limit opioid prescriptions. The Table repeats Table 10, except we drop from the analysis the four states that have passed two laws (Connecticut, Maine, Pennsylvania and Rhode Island). All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are double-clustered at the state and event date levels and presented in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	CAR[-1,1]									
	All firms		*High HQ empl.*		*Low HQ empl.*		*Low PCs/empl.*		*High PCs/empl.*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Law passage	0.002*	0.002	0.005***	0.005**	-0.002	-0.002	0.005**	0.005**	-0.002	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firms	2695	2695	1348	1348	1347	1347	1356	1356	1339	1339

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