The Impact of the Opioid Crisis on Firm Value and Investment

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Abstract

High rates of opioid abuse have had a significant impact on the United States including implications for firms which must now contend with a lower pool of available and productive workers. This paper documents a negative effect of instrumented opioid prescriptions and subsequent individual employment outcomes. In turn, this impacts firms as we show a negative relationship between opioid prescriptions and subsequent firm growth. We also show that firms respond to the labor shortage by investing more in technology, substituting capital for labor to mitigate some of the costs otherwise expected due to the decline in labor supply. Moreover, we establish a causal link between opioids and firm values using the staggered passage of state laws intended to limit opioid prescriptions. Following the passage of these laws, we find a 40 basis point increase in the cumulative abnormal return of the average firm and a 70 basis point increase for firms that are less capital intensive pre-treatment and thus more dependent on labor inputs.

Keywords: opioid, technological change, firm value.

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Opioid abuse in the US 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.[1] The economic effects of the opioid epidemic are estimated to be large. 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.[2] One large implication of the opioid abuse is the negative impact on the quantity and quality of the labor supply. In her July 2017 Congressional Testimony, Janet Yellen suggested that opioid abuse can help to explain declining labor force participation, a result also argued in Krueger (2017). This adverse shock to the labor supply has implications for firms who must now contend with a smaller or less productive pool of workers.[3]

This is the first paper to study how the opioid crisis has impacted firms. By reducing the number or productivity of available workers, opioids effectively make labor costlier with several implications for firms. Consistent with a negative shock to labor supply, we find that higher growth in opioid abuse locally is negatively related to establishment growth. Moreover, firms may engage in capital deepening as labor shortage provides them with the incentive to substitute from labor to capital (Autor et al., 2003). Indeed, we find that opioid abuse is associated with greater investment in IT in establishments located in areas most impacted by the opioid epidemic. Such investment likely reflects labor saving automation, suggesting that the opioid epidemic may have permanent negative effects on local labor markets.

[1] National Survey on Drug Use and Health Mortality in the United States, 2016

[2] https://www.whitehouse.gov/sites/whitehouse.gov/files/images/The%20Underestimated%20Cost%20of%20the%20Opioid%20Crisis.pdf

[3] Examples of concerns raised by employers abound in the media. For example, 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.” (WSJ, 2016)

To measure opioid abuse, we use information extracted from individual-level healthcare claims for covered individuals. Specifically, we observe opioid prescriptions which were billed to their insurance and distributed by a retail pharmacy. To measure the effect of the epidemic at the local level, we aggregate opioid prescriptions at the county level. Physician prescriptions represent a major source of diverted opioids (Compton et al., 2015; 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 given geography and opioid abuse in the area (Cicero et al., 2007).[4] In addition, some of the original patients will end up addicted to opioids. Extended use of opioids leads to changes in the reward circuitry within the brain and attempts to stop taking opioids are typically met with severe withdrawal syndromes. Volkow and McLellan (2016) estimate that about 15% to 26% of patients prescribed opioids misuse them and up to 8% develop an opioid use disorder.

We start our analysis by providing evidence of the changes in local labor markets following increases in opioid prescriptions. As measuring the overall effect of opioids on the quality of existing workers is challenging, we instead focus on the quantity of labor. First, we provide evidence at the individual level. We take two individuals who are identified as full time employees between the ages of 18 and 60 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 control for the obvious endogeneity, 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 individuals remains employed five years later within a sample of covered firms. We find that the probability of being employed five years after the opioid prescription is lower by 5.8%.

Second, we show negative effects on establishment employment. To minimize concerns regarding auto-correlation, we aggregate our data and use two observations per county in a (long) stacked first-differences approach. As such, we measure the change in the rate of opioid prescriptions between 2002-2006 and 2006-2010 on the subsequent change in employment, from 2007 to 2011 and 2011 to

[4]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.

2015, respectively. In our specification, we control for firm-period fixed effects, thereby comparing the effect of opioids on two establishments in the same firm-period 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 changes in observable economic and demographic characteristics. We find that higher opioid prescriptions are negatively correlated with establishment employment, consistent with evidence by Krueger (2017) and Harris et al. (2017). The economic magnitudes are also meaningful: an increase in opioid prescriptions from the 25th to the 75th percentile is associated with a 0.5% decline in employment. On the extensive margin, we find a negative and significant effect on firm exits in counties that experience greater opioid prescription growth.

As labor is a key input of production, it is intuitive to expect that opioids will be negatively associated with the firm's bottom line. Using the same specification of four-year stacked first differences, we find that opioid prescriptions are negatively associated with firm growth proxied by establishment sales. We confirm this result using data on establishment deaths at the county level, where we estimate a positive and significant effect.

Interestingly, we find that firms respond to the crisis by changing their production processes towards more capital. To this end, we use data on IT spending and the stock of computers and telecommunication technologies to proxy for investment in automation. Specifically, we compare establishments within the same firm and period but located in different counties. The establishment in the high-opioid growth county spends relatively more in IT five years later, as compared to the establishment in the low-opioid growth county. The results are economically important: an increase in opioid prescriptions from the 25th to the 75th percentile is associated with a 3.4% increase in IT budget and a 2% increase in the count of PCs. Besides time-varying firm characteristics, these results control for local economic and demographic controls as well as industry trends. These findings are also robust to including establishment fixed effects.

Consistent with the idea that a shortage in labor supply is the channel explaining our results, we find that the decline in firm growth and the increase in automation is more pronounced in establishments that belong to industries which rely on labor more 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). We find larger treatment effects for those industries where replacing labor with capital is easier.

The key empirical challenge 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. The following pieces of evidence support this assumption. First, the estimated coefficients of interest are similar when estimated with or without a variety of 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 establishment time-invariant characteristics. Second, we estimate similar results when we repeat the analysis just in the tradable sector, which suggest that the results are not driven purely by changes in the local demand. Third, results are robust to dropping the sample counties with the worst economic performance pre-treatment. 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 (i.e. those impacted by Chinese imports). In addition, we specifically identify counties with the worst exposure to Chinese imports pre-treatment following Autor and Dorn (2013) and exclude those from our analysis. Our results are robust.

Our results are also robust to an instrumental variables identification approach using opioids prescribed following the most common emergency rooms (ER) diagnoses. This directly gets at the concern that our results could be driven by an omitted variable that drives both drug seeking behavior and future economic outcomes. 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.[5] We construct ER opioid prescriptions based on top 10 most common emergency room diagnoses, identified by the

[5]In the context of emergency room visits, 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.

AAPC, and instrument for total opioids in a given community using only these opioids prescribed in emergency rooms.[6] Using this approach, we find quantitatively similar effects of opioid prescription on sales, employment, IT budgets and PCs.

Moreover, we also employ a difference-in-differences setting and show direct evidence of causality using stock price reactions following 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 legislation. We find a positive cumulative abnormal return for firms located in states that passed such legislation (treated) as compared to firms in states that did not (controls). Consistent with our argument that firms can mitigate some of the costs associated with opioid abuse by investing more heavily in automation, as in Autor et al. (2003) and Autor and Dorn (2013), we show that the positive returns upon the announcement of these laws are more pronounced for the set of firms with low ex-ante capital intensity. On average, these firms realize a stock price gain of 70 basis points. Taken together, these results are consistent with a negative and causal relation between opioid prescriptions and firm value, a relation that is mitigated by investment in automation.

Our conclusion that our results do not seem to be driven by deteriorating economic conditions is consistent with a growing literature that studies the determinants of opioid abuse. Finkelstein et al. (2018) show that the 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. Fernandez and Zejcirovic (2018) show that counties, where sales representatives of opioid drugs reach more doctors, have higher opioid overdose mortality rates. Paulozzi et al. (2014) conclude that rates of opioid prescriptions cannot be explained by variation in the underlying health of the population and instead suggest that the patterns reflect the lack of a consensus among doctors on best practices when prescribing opioids. Ruhm (2018) finds a modest relation between economic conditions and opioid deaths.[7]

[6] AAPC is previously known by full title the American Academy of Professional Coders.

[7] The lack of clinical guidelines for the appropriate use of opioids is regularly used to explain the heterogeneity in prescribing patterns as well as the occurrence of overprescribing. For example, see Tamayo-Sarver et al. (2004), Cantril et al. (2012), Poon and Greenwood-Ericksen (2014), and Barnett et al. (2017). Such heterogeneity even exists when comparing rates of prescribing opioids for emergency room doctors within a given hospital, as shown in Barnett et al. (2017).

Our paper adds to the literature that examines how frictions in labor markets affect corporate policies and valuations. Specifically, there is a literature that shows that firms respond to labor market frictions which increase costs by substituting with capital. Acemoglu and Finkelstein (2008) show how regulatory changes in the U.S. healthcare sector that disproportionately increased the price of labor affect the capital-labor mix and technology adoption in hospitals. Bena and Simintzi (2018) show that U.S. firms respond to access to cheap offshore labor by reducing their investment in labor-saving technologies at home. Bena et al. (2018) show that employment protections that effectively increase the price of labor stimulate labor-saving innovation, allowing firms to reduce their reliance on the domestic labor and to mitigate the negative effects of labor rigidities on their valuations. We add to this work by highlighting the negative effects of the opioid epidemic on labor markets and firm values, prompting firms to invest in technology as a response to the reduced pool of available workers.

Finally, our paper also contributes to the literature studying the impact of the opioid crisis on the US. Case and Deaton (2015) show the impact of opioids on health and longevity. Krueger (2017) and Harris et al. (2017) show the negative impact of opioid prescriptions on labor supply. Alternatively, Currie et al (2018) shows 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 US economy due to lost productivity from opioid abuse. Cornaggia et al. (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. These results also speak to the long-term implications for these impacted communities which must struggle with both high rates of drug abuse but also lost jobs through automation.

I 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. Following the 1995 FDA approval of OxyContin (oxycodone controlled-release), a new prescription opioid, the American Academy of Pain Medicine and the

American Pain Society advocated for greater use of opioids, arguing 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.[8] Moreover, even as late as 2011, the Institute of Medicine released a study arguing that pain was being undertreated in America. 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).

Purdue Pharma, the pharmaceutical company 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 (The New Yorker, 2017).[10] The FDA later accused Purdue Pharma of false advertising. In 2007, Purdue Pharma plead guilty to misbranding of OxyContin, paid a fine of over $600M and agreed to cut its sales force in half.[11] Additional lawsuits are still outstanding arguing that Purdue Pharma intentionally misled doctors and patients about the addiction risks associated with their opioid products.

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

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

[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.

[10] https://www.newyorker.com/magazine/2017/10/30/the-family-that-built-an-empire-of-pain

[11]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

non-terminal pain (Chou et al., 2014). In 2016, the FDC 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.

A number of states have also taken action to address the opioid epidemic. Early actions involved the development of 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 and they were not welcomed by physicians with at best mixed evidence on their effectiveness (Buchmueller and Carey, 2018; Meara et al., 2016; Islam and McRae, 2014). Indeed, we also tested whether the different PDMP policies relate to opioid prescriptions in our setting and found no significant effects.[12]

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. These laws seem to be more likely to pass in states that suffer from high rates of deaths related to opioids, as shown in Appendix Table IA1, while other determinants such as local economic, demographic, health and political characteristics do not seem to matter. In October of 2017, the US government declared opioids a public health emergency. At the federal level, Medicare also adopted a 7-day supply limit for new opioid patients in 2018.

II Data

For our analysis, 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. 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).

[12] These results are available upon request.

Consistent with these arguments, the medical literature has shown a positive correlation between rates of prescriptions in a given geography and subsequent opioid abuse in the area (Cicero et al., 2007; Dasgupta et al., 2006; Wisniewski et al., 2008).

We use rates of historic (five-year lagged) opioid prescriptions to minimize the endogeneity of opioid prescriptions which could be correlated with current economic conditions. This lag also allows for time between the initial prescription and the onset of drug abuse. Moreover, 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.

While using opioid prescriptions abstracts away from the obvious endogeneity of rates of drug abuse and economic conditions, it introduces a new endogeneity: What drives patterns of opioid prescriptions? This remains an open question in the health economics literature. A growing literature, such as Tamayo-Sarver et al. (2004), Poon et al. (2014), Paulozzi et al. (2014), Kuo et al. (2016) and Jena et al. (2016) argue that the large heterogeneity in doctor approaches to prescribing opioids reflects a lack of consensus among doctors on best practices during our sample period. The FDA did not issue guidance until 2014. Until that time, different doctors interpreted the evidence differently, with some doctors prescribing opioids more readily.[13] Presumably these doctors believed the claims that these new opioids were unlikely to be habit forming and, thus, could be used to address a wide range of real pain felt by their patients. While other doctors remained more skeptical and were more reluctant to prescribe opioids. Regional patterns arose due to local training and information sources, as well as the presence of sales representatives of opioid drug makers (Fernandez and Zejcirovic, 2018). Key to our analysis is the fact that there is little to no indication that economics are a key driver of opioid prescriptions. Finkelstein et al. (2018), looking at social security disability claimants, find strong regional differences in opioid prescribing habits which are unlikely to be explained by economic conditions. Likewise, Ruhm (2017) finds economic conditions can explain little of the geographic variation in opioid abuse. Specifically, he argues economic conditions can explain

[13] Department of Health and Human Services Behavioral Health Coordinating Committee. Addressing prescription drug abuse in the United States: current activities and future opportunities. 2014. https://www.cdc.gov/drugoverdose/pdf/hhs_prescription_drug_abuse_report_09.2013.pdf

no more than $1/10^{\text{th}}$ of the total variation.

We identify opioid prescriptions at the individual level using data provided by Marketscan from Truven Health Analytics. The Marketscan data covers anonymized individual-level health data data for 37.8 million patients. For each individual, we observe all medical expenditures covered by their medical insurance, including all inpatient and outpatient services as well as outpatient prescriptions. We observe date of service and diagnosis code, the drug provided and the 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.

In Table 1 Panel A, we report summary statistics on county-level opioid prescription rates. We measure opioid prescriptions as the mean number of opioid prescriptions per enrollee observed in our data in that county, in that year. The data covers 2,981 unique counties between years 2000 and 2010. On average, we report a per capita opioid prescription rate of 0.49. These rates of prescriptions per capita are similar to those reported by the the Centers for Disease Control and Prevention (CDC), a federal agency under the Department of Health and Human Services.[14][15]

We use the Marketscan as our baseline data as it is available for a longer time series. The CDC data is only available starting in 2006. Moreover, the richness of the data allows us to consider additional robustness tests, such as restricting the data to just working age employees, as well as to look within county at individual level outcomes. However, in untabulated results, we repeat our baseline analysis using the CDC data and find qualitatively similar results.

These quantities are also similar to what is reported in Harris et al. (2017), who use data from the Controlled Substance Monitoring Database (CSMD) or PDMP database in 10 states. The high rate of prescriptions is also consistent with Cantrill et al. (2012), who report that in 2010, there were enough opioid prescriptions written to give every American adult 5 mg of hydrocodone every 4 hours

[14]https://www.cdc.gov/drugoverdose/index.html.

[15]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.

for a month. Prescription rates in our data peak in 2012 as is also shown by Jones et al. (2018). We also document considerable variation by county as in McDonald et al. (2012) and Paulozzi et al. (2014).

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 normalized by population. All variables and their sources are defined in the Appendix.[16]

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 is a key resource for data on IT investments at US firms and has been used in a number of papers exploring technology spending, including Brynjolfsson and Hitt (2003) and Bloom et al. (2014). 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. This data is used by the sales and marketing teams at large US IT firms, thereby assuring high data quality, as errors would be quickly picked up by clients in their sales calls. We summarize the data over the 2007-2015 period used in our sample. In our analysis, we drop observations in highly regulated (agriculture and utility) or public sectors (including education). We also drop all establishments in the health care sector as the opioid epidemic may increase demand in this sector. We also limit our sample to establishments with a minimum of 20 employees in the first year an establishment showing up in 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 associated with 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. It has a stock of 84 PCs.

[16]We drop regulated and public sector industries, specifically agriculture, utility, health care and social assistance, education and public sector.

III Opioids and Individual Employment

To identify the effect of the opioid crisis on the labor market, we make use of the richness of our individual level data to explore whether individuals who have taken a prescription opioid have different future labor market outcomes. While we cannot observe whether an individual is employed across all jobs, we can observe whether an individual observed at one point in time in our data, remains covered as an active employee five years later. For ease of exposition, we refer to this as being "employed" but technically reflects being employed at one of the employers which shares their data with MarketScan. We then compare this five year ex-post labor market outcome for individuals who have taken a prescription opioid as compared to an otherwise similar control individual without such a prescription history and measure the marginal difference in employment outcomes.

For this analysis, we start by identifying a set of treated individuals, as people between the ages of 18-60, to avoid including individuals who are expected to retire within the five-year window. We use only those individuals who are currently employed. Moreover, given the inherent dependency that can occur with opioids, we limit the sample of treated individuals to people who have not received an opioid previously. We identify treated individuals as those who receive their first opioid prescription between 2001 and 2010. We start in 2001 to observe a pre-window to measure previous opioid use. We stop in 2010 to allow for five years before the end of our data. Control individuals are picked randomly from the set of individuals who did not receive an opioid prescription in that year (and similar to the treated sample) did not receive an opioid prescription in the previous years. We also match on age, gender, county of residence and medical diagnosis.

Directly comparing treated and control individuals would raise concerns for endogeneity. Treated individuals who receive an opioid prescription, even if presenting with the same diagnosis as the matched control sample, may be more ill and this difference in future underlying health may lead to differences in employment outcomes. As such, we instead instrument for the original opioid prescription using doctor proclivity to write opioid prescriptions for that given diagnosis. We measure doctor opioid intensity in the prior year and then estimate our first stage equation as follows:

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

where i indexes individuals, d indexes doctors, s indexes diagnosis codes and t indexes year.

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

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

In Table 2 we report the 2SLS results. In column 1, we report the first-stage result using all individuals between 18 and 60. Past doctor opioid intensity is a significant predictor of whether or not an individual will receive an opioid, even with fixed effects for gender, age, year, county diagnosis code and insurance plan type. In column 2, we report the second stage result. Individuals who receive an opioid prescription are 5.8% less likely to be employed five years later.

The key identification concern with this approach is that individuals with worse future job opportunities may seek out doctors who are more likely to write opioid prescriptions. To address this concern, we show our results are robust to excluding a number of subsamples where drug seeking behavior will be concentrated. First, in columns 3-4, we repeat the analysis after exclude lumbago-related diagnoses. Given lumbago is a diagnosis related to pain but not verifiable with medical tests, it is commonly used by opioid seekers. Results are consistent. Second, in columns 5-6, we exclude the 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. Again, we find similar results. Finally, in columns 7-8, we repeat the analysis with a subsample of top 10 most common emergency room diagnoses, identified by AAPC, since these prescriptions are less likely to be misused.[17]. Results are again robust.

[17]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

These results compliment results in Barnett et al. (2017) which uses a sample of medicare patients treated in emergency rooms and find that whether a patient was randomly assigned to a high opioid prescribing doctor or not is a significant predictor of later long-term use of opioids. Within the field of economics, a similar relationship between opioid prescriptions and medium-term labor force participation is documented in two other studies. Harris et al. (2017) use opioid prescriptions in a sample of ten states measured between 2013 and 2015 and find that a one standard deviation increase in opioid prescriptions by population leads to a contemporaneous 6.4 percentage point drop in the labor force participation rate. Krueger (2017) uses opioid prescriptions adjusted to morphine milligram equivalents (MME) pooled over two windows, 1999-2001 and 2014-2016, and argues that opioid prescriptions can explain 20% of the decline in the labor force participation over this time frame. These results are also supported by survey data. Using data compiled from the Princeton Pain Survey, Krueger (2017) finds that 31% of non-labor force participant prime age men report they took prescription pain medication the previous day, a rate that is likely underestimated given the stigma associated with illegal drug use. Alternatively, Currie et al. (2018) finds a small but positive correlation between opioid prescriptions and shorter-term women labor force participation.

It is 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 any changes in the quality of the pool of workers. Individuals abusing opioids are more likely to miss work, to be involved in on-the-job injuries, and to be overall less productive. As such, our estimate of the impact on individual employment likely underestimates the impact of the opioid crisis on the supply and productivity of potential employees.

In sum, 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

To investigate the impact of local opioid use on firm outcomes, we run regressions using two stacked long differences. Specifically, we measure the change in establishment-level outcomes between 2007 and 2011 and between 2011 and 2015 (inclusive). We start at 2007, as this is the first year the CiTDB data is available and end in 2015, the last year of our CiTDB data. Following the approach in the individual labor market analysis, we use historic five-year lagged opioid prescriptions. Specifically, in the first period, we measure opioid prescriptions as the change between 2002-2006 (matched to establishment level outcomes between 2007 and 2011) and in the second period we measure opioid prescriptions as the change between 2006-2011 (matched to establishment level outcomes between 2011 and 2015).

We, thus, estimate the following specification:

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

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 capita. Δy is the change in establishment-level outcome variables, including 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 and availability of doctors. 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. All changes in control variables are measured contemporaneous to the change in opioid prescription rates. Using a sample of multi-establishment firms, we are able to control for firm-period fixed effects in all regressions, hence, absorbing time varying differences in firm quality. We also control for industry-period fixed effects, absorbing time varying differences across industries. We double cluster standard errors at the county and firm level.

The identifying assumption required to establish causality is that opioid prescriptions written by doctors were unlikely to be determined based on economic conditions 5 years later, after controlling for time invariant and observable time varying county differences. In the absence of an instrument, we are not able to argue this assumption unequivocally. However, as discussed in the data section, evidence suggests that opioid prescriptions are unlikely to be driven (in an economically meaningful way) by economics. Second, in all regressions we include firm-period fixed effects. This controls for any concern that firms in high opioid regions are inherently differently. Moreover, as discussed later in the paper, we consider a number of robustness tests which exclude the obvious endogeneity interpretations.

IV.2 Establishment Growth

In this section, we explore the relation between opioid prescriptions and establishment growth measured by establishment sales and employment. Table 3 reports the results. In the absence of controls, we find a negative and significant correlation between historic opioid prescription rates and the sales of establishments located in that county (column 1). Similar results hold in column 2 if we include the full set of controls. An increase in opioid prescriptions from the 25^{th} to the 75^{th} percentile (an increase of 0.3/person) is associated with 1.7% decrease in sales. Given the presence of firm-period fixed effects, these results should be interpreted as within firm reallocation, measured on the intensive margin. In the Internet Appendix Table, we show that similar results hold across firm, as measured using the same sample but excluding the firm-period fixed effects. Moreover, we find consistent results on the extensive margin. Using counts of establishments at the county-level, we find a statistically positive relation between the rate of firm deaths and historic opioid prescription rates, as presented in Internet Appendix Table IA2. Finally, we also find a negative correlation between establishment employment rates and historic opioid rates, as measured with and without controls and reported in Table 3, columns 3 and 4.

IV.3 Firm Investment

Our analysis, so far, suggests that higher rates of opioid prescriptions reduce the labor supply and are negatively associated with firm growth. We next evaluate to what extent these labor shortages impact firms' production choices. To the extent that opioids reduce the number and the quality of available workers, firms may choose to substitute from labor to capital by investing in automation that can reduce dependence on labor. Automation is labor-saving and it tends to substitute low- and mid-skill employees (Autor et al., 2003).

To examine whether firms engage in capital deepening to respond to labor shortages, we use data on IT spending available from CiTDB. Specifically, we use 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. The assumption is that any increase in automation would also be paired with a parallel increase in automation.

In Table 4, we report results using both measures of IT spending as levels as well as normalized by establishment revenue and employment. We follow Equation (3) which controls for firm time varying trends (firm-period fixed effects) as well as industry time varying trends (industry-period fixed effects) and a battery of economic and demographic controls as detailed in the previous section. Irrespective of the measure we use, we find a positive and significant association between increases in opioid prescriptions in the county and subsequent increases in IT investment.[18] In terms of the economic magnitude, an increase in opioid prescriptions from the 25^{th} to the 75^{th} percentile (an increase of 0.3/person) is associated with a 3.4% increase in IT budget and a 2% increase in the count of PCs.

Although our analysis does not rely on exogenous variation in opioid prescriptions, it is reassuring our results remain robust to including firm-period fixed effects. We show that 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

[18]In Internet Appendix Table IA3, we report the results without controls and show similar magnitudes. Internet Appendix Table IA4 presents results excluding headquarters and shows similar magnitudes.

prescription growth. Moreover, in Table 5, we show that our results also hold with adding establishment fixed effects. This analysis is estimated over a subset of our sample as we do not observe a sufficient time series to create two stacked long-differences for all establishments. However, for those establishments for which we do observe the complete time series, we can include establishment fixed effects, thereby controlling for differential mean trends by establishment. The results are quantitatively similar, with the exception that we lose significance on the change in employment. In Internet Appendix Table IA5, we show the results for the first period and the second period separately. Overall, we find stronger magnitudes in the second period, 2011-2015. This may be driven by the tighter labor market nationally in the later period.

IV.4 The Labor Channel

Our argument is that firms invest in automation to change their production processes and substitute from labor to capital as a response to labor shortages in the opioid affected areas. We next provide more direct evidence that the observed capital deepening is due to the labor channel.

We consider heterogeneity in labor replaceability rates by industry. Our argument that firms invest in automation should be especially relevant in industries where labor can be readily replaced with technology. We measure this using the definition created by Graetz and Michaels (2018) and used in Acemoglu and Restrepo (2019) which measures the fraction of hours spent by workers in a given industry in tasks which can be performed by industrial robots. 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. Example of low replaceable industries: Finance, Real estate, Management of Companies and Enterprises, Arts and Entertainment, Accommodation and Food Services. Example of high replaceable industries: Manufacturing, Mining, Construction, Transportation and Warehousing.

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 in the top ½ of our sample. As predicted, we find the negative correlation between sales and past opioids is significantly weakened in high replaceability industries, as firms in these industries can better mitigate the costs of the diminished labor supply through substitution. In addition, we find a stronger positive relation between opioid prescriptions and IT investment (across five out of six measures) in firms operating in industries with high replaceability.

V Identification

V.1 Concerns and Robustness

Although our analysis so far does not rely on exogenous variation in opioid use, in this section we explain why our results do not seem to be endogenously explained. In the next section, we will additionally exploit exogenous variation in opioid prescriptions and more directly provide causal evidence consistent with our prior analysis.

The key omitted variable concern is that in areas with worse future job opportunities, individuals are more likely to seek out opioid prescriptions. We start from discussing what findings in the existing literature on opioids can help explain opioid abuse, and thereby the geographic variation of opioid addictions. In general, the existing literature has settled on geographic variation in medical practices and not differences in economic prospects as the main driver of heterogeneity in opioid use. For example, Paulozzi et al. (2014) concludes that rates of opioid prescriptions cannot be explained by variation in the underlying health of the population and instead suggest that the patterns reflect the lack of a consensus among doctors on best practices when prescribing opioids. 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. Finkelstein et al. (2018) follows individuals who move across counties and concludes that factors such as variation in doctor practices across counties rather than individuals economic prospects explain the observed prescription patterns across counties. Fernandez and Zejcirovic (2018) reaches a similar conclusion in a different setting where they show opioid sales representatives tapping different doctors predicts higher overdose mortality rates.[19]

Moreover, it is important to note that for our identifying assumption to be violated, it should be that opioid prescriptions written by doctors were determined based on economic conditions five years later, after controlling for time invariant county differences, industry trends, firm trends and a battery of time-varying county demographic and economic characteristics. Although this seems unlikely, we further address the concern that deteriorating economic conditions may be driving our findings by repeating our analysis in subsamples where we drop the most depressed economically counties.

In Panel A, Table 7, we exclude from our sample counties at the bottom quartile of the household income distribution as of 2000. We focus on sales, employment, IT budgets, and PCs (as levels and normalized by sales) for the ease of exposition but results are similar when we consider normalizing by employment. In column 1, we repeat the analysis on establishment growth and continue to report a negative association between opioid prescriptions and sales growth. While we lose significance, the point estimate of the coefficient is of similar magnitudes to the baseline results. Moreover, we find a significant negative correlation between past opioid prescription rates and establishment employment, as in the baseline. In columns 3-6, we repeat the analysis on establishment investment in automation and similarly continue to find strong positive effects.

In Panel B, Table 7, we instead repeat the analysis for tradable industries alone (industries with more than 50% tradable employment as defined in Delgado et al., 2014) and estimate qualitatively similar results. These results further suggest that a demand channel either because of deteriorating economic conditions, or because the opioid crisis itself has dampened demand in the local areas which would impact mostly the non-tradeable sector, cannot be driving our findings.

A related concern may be that an increase in trade and import competition from China has

[19] 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

increasingly affected certain areas in the US dampening economic conditions, including high unemployment and reduced wages (Autor et al., 2013). This might in turn lead the local population in those depressed areas to abuse opioids. This alternative explanation is not consistent with the more recent findings in the literature that the impact of Chinese imports is confined to the 2000-2007 period and seems to have disappeared in the 2007-2015 period (Bloom et al., 2019). To empirically address this concern, in Panel C, Table 7, we drop from the sample establishments in manufacturing industries, namely precisely those industries that are impacted by the cheaper Chinese imports. We continue to find strong significant effects across all specifications. In Panel D, we present an alternative test where instead we drop from the sample the top quartile of counties with the most exposure to Chinese imports as of 2000 and find robust results.[20]

V.2 Pill Mills

One way through which opioid prescriptions entered a community was through so called "pill mills". 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 both the prescription (written by a staff doctor) as well as filled the prescription to avoid issues with more reputable 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 subsequently impact local firm characteristics. However, the identification concern is that some of these pill mills served drug seekers who 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.

As a robustness check, we first identify, then exclude counties most likely to have a pill mill. We identify pill mills using the Automation of Reports and Consolidated Orders System (ARCOS) data. 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

[20]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.

common forms of opioid prescriptions: OxyContin and Hydrocontin. The ARCOS data provides information on the milligrams of active ingredient (MME) dispensed by pharmacy. 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 take the counties with the highest 25% of these pill mills and drop them from our sample. As reported in Table 8, the results are not dependent on these pill mill counties.[21]

V.3 Instrument Variable

In this section, we construct an instrument variable based on opioids prescribed in emergency rooms. Compared to doctor visits, emergency room visits are urgent and doctors are randomly assigned, reducing the potential our results are driven by intentional drug seeking behavior on the part of the patient. One potential concern is that given emergency room patients have to be treated, individuals without jobs, such as homeless, may come to emergency room to seek opioids. However, this concern is unlikely to drive our results, because our data from MarketScan covers only employed patients. We construct ER opioid prescriptions using the top 10 most common emergency room diagnoses as described earlier. We estimate the following two-stage least square specification:

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

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

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 capita. Other specifications are the same as in Equation (2). Table 9 column 1 presents the first stage results. Change in ER opioid prescriptions is a significant predictor of changes in overall opioid prescriptions, even with controls and fixed effects of firm-year and industry-period. In columns 2-9, we report the second stage results. Results are strong and the magnitudes

[21]In untablulated results, we also show that our results are similar if we exclude Florida, the state which was known for having the highest concentration of pill mills.

are similar to findings in Table 3 and 4. An increase in opioid prescriptions from the 25^{th} to the 75^{th} percentile (an increase of 0.3/person) is associated with 1.4% decrease in sales, 0.5% decrease in employment, 4.7% increase in IT budget and 1.8% increase in PCs. These results further suggest that our results are not driven by individuals who have less future job opportunities and seek out opioids.

VI Laws to Limit Opioid Prescriptions and Firm Value

In response to the opioid crisis, state legislatures have started taking actions. 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.[22] Several states followed with a total of 25 states having passed similar legislation imposing limits on opioid prescriptions by 2018 (Figure 2). A short description of the state laws and regulations is included in the Internet Appendix. Consistent with the anecdotal evidence from Massachusetts, Internet Appendix Table IA1 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.

The staggered adoption of these laws provides quasi-exogenous variation that allows to causally link the opioid crisis to firm values. Given the timing of these laws, we cannot estimate their long-term effects on labor market outcomes or firm performance. Instead, we use a differences-in-differences framework to focus on firms' stock price reaction at the announcement of the law's passage. We predict that the passage of these laws should increase firm value consistent with our findings in the prior section that the opioid crisis impedes firm growth. Moreover, this should be more pronounced for the least capital intensive firms, or in other words, those firms that have not substituted from labor to capital pre-treatment.

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

We use each firm listed in Compustat and CRSP to estimate the daily average abnormal return for each event date using the market model, or the Fama-French three- or four-factor model.[23] The estimation period starts 250 days before each event and ends 30 days before the event day. We require firms to have return observations during the event window and at least 100 return observations in the estimation period. 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.[24] We then regress three-day cumulative abnormal returns, CAR[-1,1] on this variable which captures the intensity with which legislation in a firm's state of headquarter impacts each firm in our sample, interacted with *law passage* which is 1 for states where legislation is passed and 0 otherwise. If a state passed more than one law, the latest law is considered as the former law typically imposes few limitations and as such is not effective.[25] If a state has not passed a law but introduced a regulation intended to limit opioid prescriptions, the regulation is considered.[26] We consider one date for each law: the earlier date the law was voted by the House or the date it was voted by the Senate. For each regulation, we consider the date the regulation was announced.

We present the results in Table 10. In column 1, Panel A, we use the market model to calculate the three-day cumulative abnormal returns and control for event fixed effects defined separately for the dates each law passed the house or the Senate. We find a cumulative abnormal return of 20 basis points that is not statistically significant. In column 2, we additionally include firm fixed effects to absorb any time-invariant firm characteristics and firm controls. We similarly find a 30 basis point stock price reaction that is not statistically significant. In columns 3-4, we consider the Fama-French 3- or 4-factor model to estimate the abnormal returns. We estimate a 40 basis points increase in the stock price that is statistically significant. Overall, these results indicate that firms benefit from

[23]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 and agriculture.

[24]The results are robust to replacing HQ empl. ratio as 0 if less than 0.25 as shown in Internet Appendix Table IA6.

[25]The results are robust to dropping from the analysis the four states that passed two laws (Connecticut, Main, Pennsylvania and Rhode Island) and the two states that passed a regulation and subsequently a law (Arizona and Nebraska) as shown in Internet Appendix Table IA7.

[26]There are two states that have passed regulations that were subsequently followed by laws and six states that have only passed regulations.

headquarters state legislations that intend to limit opioid prescriptions.

Consistent with our earlier findings that firms invest in automation to mitigate the negative effect of labor market shortages due to opioids, we find positive announcement returns for firms with low capital intensity pre-treatment. Specifically, we define *low PC/empl.* to be one if the 2015 stock of installed PCs over the number of employees is below the sample median, and 0 otherwise.[27] In Panel B, Table 10 we interact *HQ empl. ratio* $\times$ *law passage* with *low PC/empl.*.

In column 1, we control for event fixed effects and find a cumulative abnormal return of 90 basis points that is statistically significant at the 1% level. In column 2 we additionally control for firm fixed effects and firm controls, and similarly estimate a 90 basis point stock price reaction that is statistically significant. The results remain qualitatively unchanged in columns 3-4 when we instead consider the Fama-French 3- or 4-factor model to estimate the abnormal returns.[28] 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 the state legislations.

VII Conclusion

The current opioid crisis was fueled, in part, 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, it turned out that opioids did indeed pose a significant risk of addiction and large societal costs. One of these costs is a reduction in the supply of productive workers. We show that individuals that are 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 medican condition. We also document this negative relation at the establishment level: establishments located in counties that experience a higher growth in opioid precription rates have lower employment growth as compared to establishments within the same

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

[28]In unreported regressions, we repeat our estimation for the dates the governor signed the legislation. We cannot identify significant effects consistent with the fact that the market anticipated the legislation to be signed into law after passing the House and the Senate.

firm located in low opioid growth counties.

This is the first paper to document the negative effects of opioids on (long-term) firm growth and valuations. Although we do not observe random variation in opioid prescription rates, our analysis suggests that deteriorating economic conditions in areas where the opioid crisis is more acute do not seem to explain the patterns we observe. We furthermore establish a causal link between opioids and firm values when we use the staggered adoption of state laws passage intended to limit opioid prescription rates and find a significant and positive stock price reaction.

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. The effect is concentrated in industries where labor is easily substitutable by technology. This response by firms mitigates some of the costs that would otherwise be anticipated from a reduction in the labor supply. However, it also changes the production processes at firms, a change which can have lasting impacts on the local labor markets.

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Appendix: Variable Definitions

Individual-level variables: *Opioid prescribed indicator* is 1 if the individual is prescribed with opioids in a given year and have never been prescribed with opioids before, and 0 if the individual has never been prescribed with opioids in and before a given year. Source: MarketScan.

Doctor opioid intensity is measured by the fraction of each doctor's outpatient service records that can be linked to opioid prescriptions in seven days. For each individual, *Doctor opioid intensity* is measured based on outpatient service records of other patients. Source: MarketScan.

County-level variables:

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

ER opioid prescriptions is measured as the count of total opioid prescriptions related to top 10 common emergency room diagnoses normalized by number of enrollees at a given county.

Source: MarketScan.

$Non-Cancer$ *opioid prescriptions* is measured as the count of opioid prescriptions unrelated to cancer diagnosis normalized by number of enrollees at a given county. If an opioid prescription can be linked to outpatient service records in seven days, and outpatient service records do not include any cancer diagnosis ICD-9 code (Sherry et al., 2018), it is classified as cancer-unrelated. Source: MarketScan.

$Work-age$ *opioid prescriptions* is measured as the count of opioid prescriptions prescribed to patients between age 18 and 65 normalized by number of enrollees between age 18 and 65 at a given county. Source: MarketScan.

CDC opioid prescriptions is measured as the count of total opioid prescriptions normalized by population at a given county level. Source: Centers for Disease Control and Prevention (CDC) (https://www.cdc.gov/drugoverdose/maps/rxrate-maps.html).

$White$ *ratio* is measured by the white population divided by total population at a county. Source: Census.

Age $20-64$ *ratio* is measured by the population between ages 20 and 64 divided by total population at a county. Source: Census.

Age over 65 *ratio* is measured by the population over age 65 divided by total population at a county. Source: Census.

Neoplasms mortality is measured by the number of deaths due to neoplasms (ICD-10 C00-D48), 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 in a given establishment. Source: CiTDB.

PCs is the total number of personal computers in a given establishment. Source: CiTDB.

Sales is the estimated revenue in a given establishment. Source: CiTDB.

Employment is the total number of employees in a given 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 is zero otherwise. Labor replaceability is the fraction of each industry's hours worked in 2000 that was performed by occupations that subsequently pron to be replaced by robots (Graetz and Michaels, 2018). Source: American Community Surveys.

State law related variables:

HQempl.ratio is the share of a firm's employment in the state of headquarter's. Source: CiTDB and Compustat.

Law passage is an indicator equal to one if a firm's headquarter is located in the state with the opioid related law/regulation, and zero if in other states.

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

No prior law is an indicator equal to one if a firm's headquarter state has not passed an opioid law intended to limit opioid prescriptions before, and 0 otherwise.

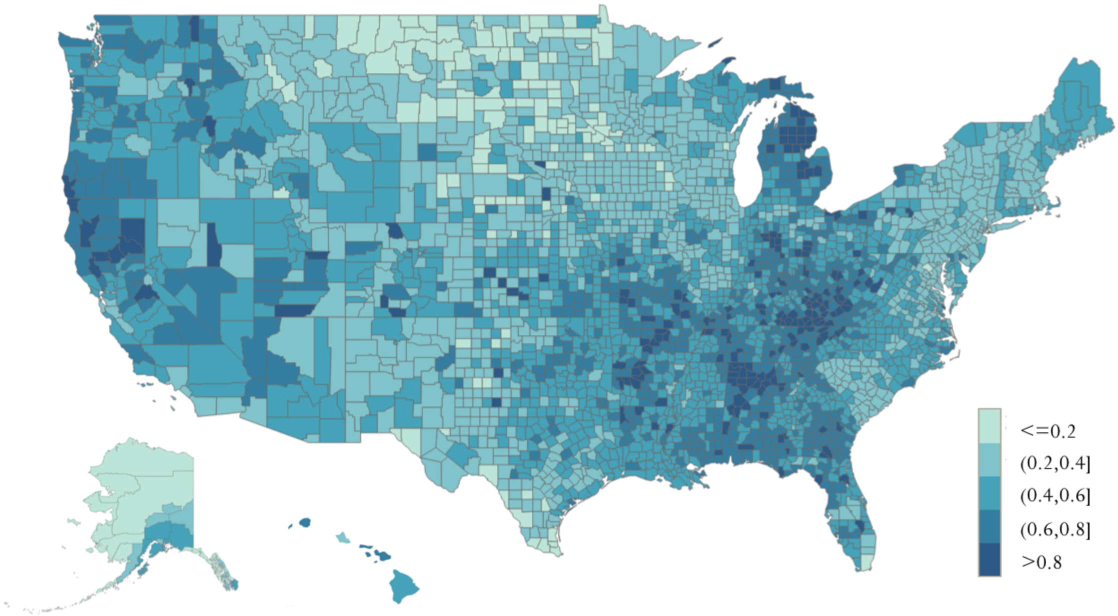

Figure 1: Map of Opioid Prescriptions

This figure plots the distribution of opioids by county based on opioid prescription rates over 2001-2010.

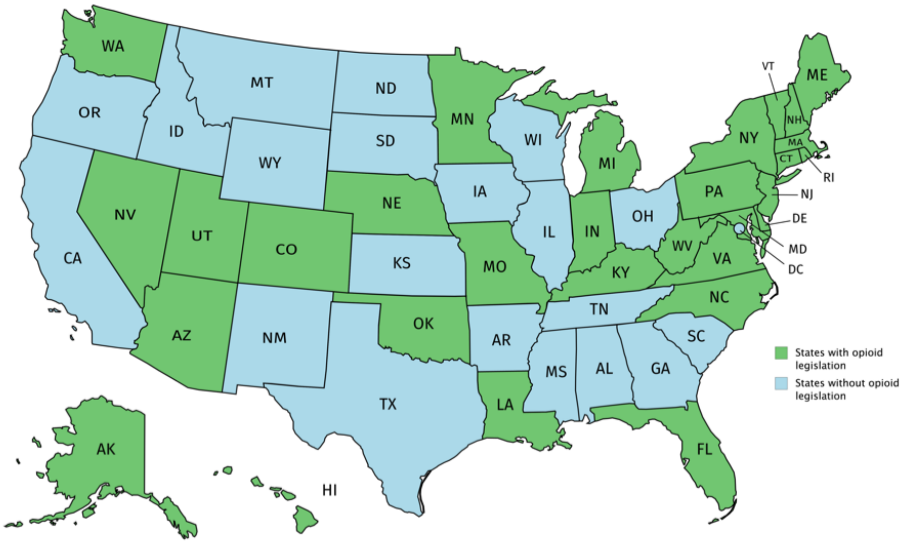

Figure 2: Legislation to Limit Opioid Abuse

This figure plots the distribution of laws and regulations intended to limit opioid abuse. States that passed at least one law or regulation 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 county-level opioid prescriptions, and demographic and economic variables that we use as controls in our analysis. Panel B reports summary statistics on establishment-level IT investment 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 capita)	27,067	0.49	0.47	0.22
ER opioid prescriptions (per capita)	27,067	0.03	0.03	0.02
Population (1000)	27,067	88.19	27.27	183.81
Income ($1000)	27,067	40.57	38.82	10.41
White ratio (%)	27,067	86.35	93.22	15.82
Age 20-64 ratio (%)	27,067	57.85	58.01	3.20
Age above 65 ratio (%)	27,067	15.11	14.79	3.96
Neoplasms mortality (per 1000)	27,067	1.50	1.89	1.31
		Panel B. Establishment − level IT variables		
Sales ($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 a two-stage least squares estimation between whether individuals take (or not) opioids between 2001-2010 and whether they are employed 5 years later (2006-2015). We take two individuals who are identified as full time employees between the ages of 18 and 60 in the same county, receive the same medical diagnosis and are of the same age and gender but where one individual receives first opioid prescription at year and the other does not. Opioid prescribed indicator is instrumented by doctor opioid intensity. The sample in columns 1 and 2 includes all individuals between age 18 and 60. The sample in columns 3 and 4 exclude lumbago-related diagnosis. The sample in columns 5 and 6 exclude doctors with opioid intensity in the top 10% of our sample. The sample in columns 7 and 8 include individuals diagnosed as top 10 emergency room diagnoses. Definition of top 10 emergency room diagnoses follows (AAPC, 2014). Columns 1, 3, 5 and 7 present first stage results. Columns 2, 4, 6 and 8 present second stage results. Standard errors are clustered at the county level. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	All		Exclude Lumbago		Exclude 10% high opioid doctors		Only top 10 ER visits	
	Opioid prescribed	Employed at t+5	Opioid prescribed	Employed at t+5	Opioid prescribed	Employed at t+5	Opioid prescribed	Employed at t+5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Doctor opioid intensity	0.517***		0.508***		0.807***		0.443***	
	(0.009)		(0.010)		(0.017)		(0.022)	
Opioid prescribed		-0.058***		-0.056***		-0.067***		-0.057**
		(0.013)		(0.014)		(0.015)		(0.024)
Gender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Diagnosis code FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Insurance plan FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F statistic		3073		2838		2176		411.9
Observations	802,731	802,731	757,396	757,396	721,939	721,939	99,069	99,069

Table 3: Opioids and Establishment Growth

This table presents a first difference estimation between 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. The dependent variable is the logarithm of establishment sales in columns 1 and 2, and the logarithm of establishment employment in columns 3 and 4. Controls are changes in 2002-2006 and 2006-2010. All variables are measured as the change over a four-year window. Standard errors are clustered at the county and firm level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δln(Sales)		Δln(Emp.)	
	(1)	(2)	(3)	(4)
ΔOpioid pres.	-0.048**	-0.056**	-0.019***	-0.018***
	(0.022)	(0.022)	(0.006)	(0.006)
Δln(Income)		0.028		-0.015
		(0.043)		(0.013)
Δln(Population)		0.111***		0.006
		(0.034)		(0.010)
ΔWhite ratio		0.004		-0.000
		(0.002)		(0.001)
ΔAge 20-64 ratio		-0.003		-0.002
		(0.005)		(0.001)
ΔAge above 65 ratio		0.005		0.000
		(0.004)		(0.001)
ΔNeoplasms mortality		-0.021***		-0.008**
		(0.006)		(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 between changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment IT investment over 2007-2011 and 2011-2015. The dependent variable is 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 changes in 2002-2006 and 2006-2010. All variables are measured as the change over a four-year window. Standard errors are clustered at the county and firm level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are 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 pres.	0.117***	0.180***	0.125***	0.061***	0.092***	0.028***
	(0.043)	(0.045)	(0.038)	(0.020)	(0.021)	(0.008)
Δln(Income)	0.190**	0.138	0.191**	0.101**	0.008	0.038**
	(0.091)	(0.085)	(0.079)	(0.044)	(0.041)	(0.015)
Δln(Population)	-0.143*	-0.222***	-0.137*	-0.043	-0.063**	-0.001
	(0.080)	(0.070)	(0.072)	(0.030)	(0.025)	(0.011)
ΔWhite ratio	0.001	-0.004	0.003	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***	0.013	-0.000	0.005	0.002
	(0.010)	(0.009)	(0.009)	(0.004)	(0.003)	(0.002)
ΔAge above 65 ratio	0.007	0.013	0.005	-0.001	-0.001	0.001
	(0.011)	(0.010)	(0.009)	(0.005)	(0.004)	(0.002)
ΔNeoplasms mortality	-0.027	-0.012	-0.017	0.015*	0.031***	0.010***
	(0.019)	(0.018)	(0.015)	(0.008)	(0.006)	(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: Establishment Fixed Effects

This table presents a first difference estimation between 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 within establishments. The dependent variable is 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 and are changes in 2002-2006 and 2006-2010. All variables are measured as the change over a four-year window. Standard errors are clustered at the county and firm level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are 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 pres.	-0.030*	-0.017	0.224**	0.278***	0.218***	0.129***	0.119***	0.053***
	(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 between changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment IT investment over 2007-2011 and 2011-2015, exploring heteogeneity on industry labor replaceability. The dependent variable is 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 and are changes in 2002-2006 and 2006-2010. All variables are measured as the change over a four-year window. Standard errors are clustered at the county and firm level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are 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 pres.	-0.127***	-0.031***	-0.114*	0.022	-0.080	-0.025	0.083***	-0.002
	(0.039)	(0.010)	(0.069)	(0.064)	(0.062)	(0.030)	(0.031)	(0.012)
ΔOpioid pres. $\times$ high labor repl.	0.140***	0.020*	0.396***	0.251***	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

This table presents a first difference estimation between changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment IT investment over 2007-2011 and 2011-2015, considering four different samples. In Panel A, we exclude from the sample counties at the bottom quartile of the household income distribution as of 2005. In Panel B, we include in the sample only tradable industries. We define the set of tradable industries following Delgado et al. (2014). In Panel C, we exclude from the sample establishments in manufacturing industries (NAICS 31-33). In Panel D, we exclude from the sample the top quartile of counties with the most exposure to Chinese imports as of 2000, following Autor and Dorn (2013). The dependent variable is 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 and are changes in 2002-2006 and 2006-2010. All variables are measured as the change over a four-year window. Standard errors are clustered at the county and firm level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are 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 pres.	-0.056*	-0.022***	0.103*	0.182***	0.070**	0.102***
	(0.031)	(0.007)	(0.057)	(0.065)	(0.030)	(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. Tradeable industries						
ΔOpioid pres.	-0.040	-0.019***	0.136***	0.174***	0.064***	0.077***
	(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

	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 establishments in manufacturing industries						
ΔOpioid pres.	-0.123***	-0.028***	0.001	0.137**	0.032	0.119***
	(0.030)	(0.007)	(0.055)	(0.057)	(0.024)	(0.026)
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
Panel D. Exclude counties with most exposure to China imports						
ΔOpioid pres.	-0.071***	-0.022***	0.094*	0.173***	0.031	0.081***
	(0.025)	(0.006)	(0.050)	(0.049)	(0.022)	(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	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

Table 8: Robustness: Excluding Pill Mill Counties

This table presents a first difference estimation between changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment IT investment over 2007-2011 and 2011-2015, 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. The dependent variable is 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 and are changes in 2002-2006 and 2006-2010. All variables are measured as the change over a four-year window. Standard errors are clustered at the county and firm level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are 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 pres.	-0.057**	-0.018***	0.121**	0.194***	0.061***	0.094***
	(0.023)	(0.006)	(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

Table 9: 2SLS: Emergency Room Opioids Instrument

This table presents a first difference estimation between changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment IT investment over 2007-2011 and 2011-2015, using changes in emergency room opioid prescription rates to instrument changes in opioid prescription rates. Column 1 presents first stage results. Columns 2-7 present second stage results. The dependent variable is 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 and are changes in 2002-2006 and 2006-2010. All variables are measured as the change over a four-year window. Standard errors are clustered at the county and firm level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	ΔOpioid pres.	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 pres.	11.231***								
	(0.354)								
ΔOpioid pres.		-0.048*	-0.017**	0.158***	0.224***	0.168***	0.060**	0.082***	0.027***
		(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
Weakid F		1007	1007	1009	998.7	1009	1005	994.3	1005
Observations	300,658	300,658	300,658	286,073	272,642	286,073	298,288	284,790	298,288
R^2	0.750	0.00051	0.00015	0.00018	0.00028	0.00027	0.00030	0.00085	0.00083

Table 10: Abnormal Returns around the Passage of Opioids State Legislation

This table presents firm abnormal returns around the first passage through the House or the Senate of laws or regulations intended to limit opioid prescriptions (Panel A). In Panel B, we further explore heterogeneity based on pre-treatment firms' capital intensity. The sample includes all U.S. firms listed in both Compustat and CRSP that can be matched to CiTDB. The dependent variables are three-day cumulative abnormal returns CAR[-1,1], measured using the market model in columns 1 and 2, the Fama-French three factor model in column 3, and the Fama-French four factor model in column 4. Firm financial controls are lagged by one year and include the logarithm of total assets, ROA, PPE, Tobin's Q, the logarithm of age and no prior law dummy. Standard errors are clustered at state level. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	CAR[-1,1]			
	MM		F3	F4
	(1)	(2)	(3)	(4)
Panel A. Law passage				
Law passage × HQ empl. ratio	0.002	0.003	0.004*	0.004**
	(0.002)	(0.003)	(0.002)	(0.002)
Firm controls	No	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes
Observations	68,068	68,052	68,052	68,052
R^2	0.029	0.042	0.014	0.013
Panel B. Interaction with low capital intensity				
Law passage × HQ empl. ratio	-0.002	-0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Law passage × HQ empl. ratio × low PCs/emp.	0.009***	0.009***	0.007***	0.007***
	(0.002)	(0.002)	(0.002)	(0.002)
PCs/empl. low	-0.001***			
	(0.000)			
Firm controls	No	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes
Observations	66,035	66,021	66,021	66,021
R^2	0.030	0.042	0.014	0.013

The Impact of the Opioid Crisis on Firm Value and Investment

Paige Ouimet, Elena Simintzi, and Kailei Ye

INTERNET APPENDIX

Internet Appendix I: List of State Laws and Regulations

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 provisons 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 health care 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 treatement 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.

By October 2018, 8 states have announced opioid-related policies or executive order. We list the years of regulation and a brief description of these regulations below.

Arizona (2016): An executive order, which institutes a seven day opioid limit for first time prescriptions for anyone insured under Arizona's Medicaid program or state employee insurance program with exceptions for cancer patients, chronic disease/pain patients, and traumatic injury patients. This executive order removes the pre-approval to be prescribed Vivitrol for those with state-provided insurance.

Colorado (2017): The Department of Health Care Policy and Financing announced a policy that limits initial opioid prescriptions to seven days with two additional 7-day refills for Medicare patients. This policy requires a consultation with a pain management physician.

Delaware (2017): The Delaware state agency unveiled a policy which limits the first fill prescriptions of opioids to seven days for adults and limits opioid prescription to minors to seven days, with certain exceptions for acute, chronic pain conditions, or according to prescribers' professional medical judgments, . If the doctor deems that a larger supply is necessary, the patient must undergo a physical exam, be educated about the dangers of opioid abuse, and the doctor must examine the patient's prescription history.

Missouri (2017): Missouri's Medicaid program adopted a new policy to limit initial opioid prescriptions to seven days for Medicare patients.

Nebraska (2016): The Nebraska Department of Health and Human Services announced a policy which limits opioid prescriptions to 150 tablets per 30 days for Medicare patients, excluding cancer patients.

Ohio (2017): The Ohio governor unveiled a policy that limits opioid prescriptions to seven days and 30 morphine equivalent dose (MMD) per day for acute pain patients. This policy limits opioid prescriptions to five days for minors with written consent by a parent or guardian. Cancer patients, chronic pain, hospice and palliative care, or medications prescribed for the treatment of substance use disorders are exempt.

Vermont (2017): The Vermont Department of Health announced a new policy which limits amounts of opioids that can be prescribed. The policy established four prescribing categories: minor, moderate, severe, and extreme pain. Moderate pain patients are allowed an average of 24 MME per day. Severe pain patients are allowed an average of 32 MME per day. Minors suffering from moderate to serve pain are allowed an average of 24 MME per day.

Virginia (2017): The Virginia Board of Medicine adopted regulations to limit opioid prescriptions to seven days for acute pain and to 14-days for post-surgical pain. In addition, this policy requires medical professionals to document reasons for prescribing more than 50 morphine milligram equivalents per day and either consult with or refer patients who are prescribed more than 120 morphine milligram equivalents per day to a pain management specialist.

Table IA1: Determinants of Opioids State Legislation

This table explores the relation between state opioid-related regulation 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 or regulation between 2016 and 2018. Cumulative opioid prescriptions are cumulative opioid prescriptions between 2006 and 2015. GSP per capital is measured by the gross domestic product per capital in a given state. 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 as of 2015. Standard errors are clustered at state level. All remaining variables are defined in the Appendix and winsorized at the 1% level. Standard errors are in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	State Legislation and Regulation Indicator			
	(1)	(2)	(3)	(4)
Ln(Cumulative opioid pres.)	-0.364	0.298	-0.168	0.390
	(0.238)	(0.436)	(0.269)	(0.453)
Age-adjusted opioid overdoes death rate	0.034***	0.025***	0.028***	0.021**
	(0.007)	(0.009)	(0.009)	(0.010)
Unemployment rate		0.625		-0.268
		(8.300)		(8.604)
Ln(Median household income)		1.090		1.107
		(1.139)		(1.155)
Poverty ratio		-0.022		-0.016
		(0.061)		(0.062)
Manufacturing ratio		-0.018		-0.016
		(0.023)		(0.024)
Ln(GSP per capita)		-0.279		-0.280
		(0.571)		(0.562)
Democratic State			-0.061	-0.080
			(0.174)	(0.167)
Republican State			-0.214	-0.155
			(0.157)	(0.172)
Observations	50	50	50	50
R^2	0.192	0.198	0.188	0.177

Table IA2: Establishment Births and Deaths

This table presents a first difference estimation between 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. The dependent variable is the logarithm of establishment births in column 1, the logarithm of establishment deaths by sales in column 2, the logarithm of establishment expansion in column 3, and the logarithm of establishment contractions in column 4. *Establishment births* is measured by number 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 deaths* is measured by number 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 measured by number of establishments that have positive employment in the first quarter of previous year, and positive and increase employment in the first quarter of a given year. *Establishment contractions* is measured by number of establishments that have positive employment in the first quarter of previous year, and positive and decrease employment in the first quarter of a given year. Controls are changes in 2002-2006 and 2006-2010. All variables are measured as the change over a four-year window. Standard errors are clustered at the county and firm level. Industries are defined by 4-digit NAICS codes. Other variables are defined in the Appendix and winsorized at the 1% level. Standard errors are 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 pres.	0.036	0.093**	-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	0.332	0.352	0.353

Table IA3: Robustness: Opioids and IT Investment without Controls

This table presents a first difference estimation between 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 controls. The dependent variable is 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 and are changes in 2002-2006 and 2006-2010. All variables are measured as the change over a four-year window. Standard errors are clustered at the county and firm level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are 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 pres.	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 IA4: Robustness: Excluding Headquarters

This table presents a first difference estimation between changes in opioid prescription rates over 2002-2006 and 2006-2010 and subsequent changes in establishment IT investment over 2007-2011 and 2011-2015, exclude from headquarter establishments. The dependent variable is 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 and are changes in 2002-2006 and 2006-2010. All variables are measured as the change over a four-year window. Standard errors are clustered at the county and firm level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are 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 pres.	-0.062***	-0.015**	0.104**	0.177***	0.063***	0.095***
	(0.023)	(0.006)	(0.046)	(0.049)	(0.022)	(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 IA5: Robustness: By Periods

This table presents a first difference estimation between changes in opioid prescription rates and subsequent changes in establishment IT investment, estimated by separate periods. Panel A presents estimation between changes in opioid prescription rates over 2002-2006 and subsequent changes in establishment IT investment over 2007-2011. Panel B presents estimation between changes in opioid prescription rates over 2006-2010 and subsequent changes in establishment IT investment over 2011-2015. The dependent variable is 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 and are changes in 2002-2006 and 2006-2010. All variables are measured as the change over a four-year window. Standard errors are clustered at the county and firm level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at the 1% level. Standard errors are 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 pres.	-0.025	-0.028	0.128	0.180*	0.103***	0.136***
	(0.016)	(0.018)	(0.112)	(0.107)	(0.039)	(0.028)
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 pres.	-0.067**	-0.014***	0.106**	0.174***	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 IA6: Robustness: Firms with Small Ratio of Employment in Headquarter

This table presents firm abnormal returns around the passage of laws or regulations intended to limit opioid prescriptions (Panel A). In Panel B, we further explore heterogeneity based on pre-treatment firms' capital intensity. The Table repeats Panels A and B, Table 10, except we replace HQ empl. ratio as 0 if less than 0.25. Standard errors are in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	CAR[-1,1]			
	MM		F3	F4
	(1)	(2)	(3)	(4)
Panel A. Law passage				
Law passage × HQ empl. ratio	0.002	0.003	0.004*	0.004**
	(0.002)	(0.003)	(0.002)	(0.002)
Firm controls	No	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes
Observations	68,068	68,052	68,052	68,052
R^2	0.029	0.042	0.014	0.013
Panel B. Interaction with low capital intensity				
Law passage × HQ empl. ratio	-0.002	-0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Law passage × HQ empl. ratio × low PCs/emp.	0.009***	0.009***	0.007***	0.007***
	(0.002)	(0.002)	(0.002)	(0.002)
PCs/empl. low	-0.001***			
	(0.000)			
Firm controls	No	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes
Observations	66,035	66,021	66,021	66,021
R^2	0.030	0.042	0.014	0.013

Table IA7: Robustness: Excluding States Passing More than One Regulation

This table presents firm abnormal returns around the first passage through the House or the Senate of laws or regulations intended to limit opioid prescriptions (Panel A). In Panel B, we further explore heterogeneity based on pre-treatment firms' capital intensity. The Table repeats Panels A and B, Table 10, except we drop from the analysis the four states that have passed two laws. Standard errors are in parentheses. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	CAR[-1,1]			
	MM		F3	F4
	(1)	(2)	(3)	(4)
Panel A. Law passage				
Law passage × HQ empl. ratio	0.003	0.004	0.005**	0.006***
	(0.003)	(0.003)	(0.002)	(0.002)
Firm controls	No	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes
Observations	53,817	53,790	53,790	53,790
R^2	0.026	0.043	0.014	0.014
Panel B. Interaction with low capital intensity				
Law passage × HQ empl. ratio	-0.001	0.001	0.003	0.003
	(0.002)	(0.003)	(0.002)	(0.002)
Law passage × HQ empl. ratio × low PCs/emp.	0.009***	0.008***	0.007***	0.007***
	(0.002)	(0.002)	(0.002)	(0.002)
PCs/empl. low	-0.001***			
	(0.000)			
Firm controls	No	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes
Observations	52,222	52,197	52,197	52,197
R^2	0.026	0.043	0.014	0.013