The Impact of the Opioid Crisis on Firm Value and Investment

Paige Ouimet, Elena Simintzi, and Kailei Ye*

February 1, 2019

Abstract

The increasing rates of opioid abuse has had a significant impact on the United States. This epidemic has implications for firms which must now contend with a lower pool of available and productive workers. To minimize endogeneity, we use historic rates of physician opioid prescriptions to proxy for current opioid abuse. We show a negative relationship between opioids and firm performance, after controlling for local economic and demographic conditions. Firms can mitigate some of the costs due to a decreased labor market by substituting capital for labor. Indeed, we show a positive and significant association between opioid prescriptions in areas hard hit by opioid abuse and investments in technology by local firms. Consistent with causality, we show positive announcement returns following the staggered passage of state laws intended to limit opioid prescriptions. Moreover, the observed increases in shareholder values are concentrated among firms that do not appear to have invested in technology as a means to mitigate the negative effects of opioids on the pool of available workers.

Keywords: opioid, technological change, firm value.

Affiliations: *Kenan-Flagler Business School, University of North Carolina.

e-mails: paige_ouimet@unc.edu, elena_simintzi@kenan-flagler.unc.edu, Kailei_Ye@kenan-flagler.unc.edu

Acknowledgments: We would like to thank seminar participants at Temple University, the University of Kentucky and the University of North Carolina. We would like to thank Sarah Kenyon and Huan Lian for valuable research assistance. We would also like to thank the Frank H. Kenan Institute of Private Enterprise for generous support.

The United States is currently experiencing an unprecedented opioid crisis. As of 2014, the federal government estimates that 2.5 million Americans are addicted to opioids and 11.4 million Americans have misused opioids in the previous year (3.5% of the population).[1] This epidemic of drug use has had large economic effects. 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] More specifically, in her July 2017 Congressional Testimony, Janet Yellen suggested that opioid abuse can help to explain declining labor force participation, a result confirmed in Krueger (2017). This has implications for firms who must now contend with a smaller or less productive pool of workers.[3]

In this paper, we study how the opioid crisis has impacted firms in heavily affected areas. By reducing the supply or productivity of available workers, opioids effectively increase the relative price of labor, thereby reducing firm value. Consistent with this intuition, we show that local opioid abuse is negatively related to firm performance and employment. We also argue that firms will respond to the labor shortage by substituting physical capital for labor. Indeed, we find that opioid abuse is associated with greater investment in IT, investment which likely reflects labor saving automation. These results suggest that the opioid epidemic may have permanent negative effects on local labor markets.

This is the first paper to link the opioid crisis with firm outcomes. In doing so, we need to acknowledge the challenges posed due to the endogeneity of opioid abuse. Individuals may be more likely to abuse drugs when they feel that job opportunities are limited. As a first step to mitigate this potential endogeneity, we use five-year lagged county-level opioid prescription records to proxy for current opioid abuse. 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

[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 are included in a recent article in the New York Times. The article starts by discussing a high level of job openings in Youngstown Ohio. "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)

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. Moreover, using prescription records lagged five years will not 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.

Using prescription records addresses the endogeneity associated with behavior that directly leads to drug abuse but leaves unresolved endogeneity related to the determinants of rates of opioid prescriptions. While this question has been investigated within the medical literature, the key drivers remain unknown. McDonald et al. (2012) document disproportionate geographic variation in opioid prescriptions, as compared to other health care services, with only 1/3rd of variation at the county level explained by known factors. 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.[5] Indeed, when we examine whether lagged economic conditions predict opioid prescriptions, we find significant differences in the cross-section of counties, with higher unemployment and lower household income being positively related to opioid prescriptions. However, these findings are not robust to the inclusion of county fixed effects, which suggests that opioid use is driven by persistent characteristics of the counties rather than changes in their economic conditions. These arguments are supported in Currie et al. (2018) and Ruhm (2018) which find little to no relation between

[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.

[5]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).

economic conditions and opioid prescriptions or opioid deaths.[6] Specifically, Ruhm (2018) argues that changes in local economic conditions can explain at most ten percent of the observed changes in opioid related drug mortality.

Alternatively, we can show more direct evidence of causality using stock price reactions following the passage of state laws which limit opioid prescriptions. The first such law was passed in Massachusetts in 2016 with the intent of reducing opioid abuse and, since then, another 24 states have passed similar legislation. 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 confined to the set of firms assumed to have low previous investments in automation. On average, these firms realize a stock price gain of 50 to 60 basis points. We find similar results if we instead focus on a federal legislation that was passed in 2018 – but only for firms headquartered in states that have not previously passed a law or regulation limiting opioid prescriptions. Taken together, these results show a negative and causal relation between opioid prescriptions and firm value, a relation that is mitigated by investment in automation.

We further strengthen this argument by documenting direct evidence of the changes in local labor markets and firm outcomes following increases in opioid prescriptions. We start by showing a negative correlation between the local labor force participation and historic opioid prescriptions. We identify prescriptions using Centers for Disease Control and Prevention (CDC) data covering nearly 90% of all retail prescriptions in the United States. To minimize concerns regarding autocorrelation in our data, we aggregate our data and use one observation per county. Specifically, we measure the change in the rate of opioid prescriptions between 2006 and 2010 and the change in the labor force participation, by county, from 2011 to 2015. This specification effectively controls for county fixed characteristics that, according to our prior analysis, are key determinants of opioid use, as well as changes in observable economic and demographic characteristics. We find that higher opioid prescriptions are negatively correlated with the local labor supply, as in Krueger (2017) and

[6] Case and Deaton (2015) instead argue that "deaths of despair", a term which includes deaths from opioid abuse, increase following long-term economic distress. In our analysis, we minimize the impact of long-term trends by using county fixed effects.

in Harris et al. (2017). Moreover, the relationship is present when we limit the sample to counties in the 25 states with the highest rates of opioid prescriptions as of 2006. These states represent areas at the core of the opioid crisis, where a percent increase in opioid prescriptions is more meaningful and where the labor market is likely to be already impacted, suggesting additional changes will be particularly costly.

Moreover, using the same specification of four-year first differences, we find that opioid prescriptions are negatively associated with firms' sales and employment, considering a sample of publicly-listed Compustat firms with establishments in areas hard hit by the opioid crisis. This relationship does not hold when we consider states less impacted by the opioid crisis. Similar results hold when we limit the sample to establishments just in the tradeable sector, suggesting the documented negative firm performance is consistent with a labor supply mechanism, as compared to a purely demand-driven mechanism.

To show firms respond to negative labor supply shocks associated with opioid abuse by investing in labor-saving automation, we use data from the Computer Intelligence Technology Database (CiTDB). CiTDB is a proprietary database that provides establishment-level data on planned IT spending and installed computers and telecommunication technologies and has been used to proxy for investments in labor-saving automation in Brynjolfsson and Hitt (2003) among others. Focusing on the 25 high opioid states, we find a positive relation between changes in historic opioid prescriptions and changes in IT budgets and equipment stocks. These results are robust to the addition of controls for local economic and demographic characteristics, as well as industry trends. These findings are also robust to including firm fixed effects. With firm fixed effects, we absorb any time varying differences across firms thereby providing additional support for a causal interpretation of the relationship between opioids and IT spending. We also find the increase in automation, associated with opioid prescriptions, is more pronounced in establishments that belong to industry-geography pairs which rely on a higher share of lower skill-labor, the group of employees most impacted by opioid abuse (Case and Deaton, 2015). We also show more pronounced treatment effects at establishments less likely to be financially constrained, where we proxy for financial constraints with whether or not the establishment is part of a multi-establishment firm.[7] Automation typically comes with relatively higher fixed costs, making it harder for financially constrained firms to adjust. Finally, we also find larger treatment effects in industries with high on-the-job injury rates, industries where opioid-abusing workers are likely to be an especially large liability.

Further, we show that firms located in high opioid states become relatively more reliant on high-skill labor and relatively less reliant on low-skill labor. This is consistent with the literature in economics that suggests that automation is complementary to high-skill labor, thereby increasing its relative productivity compared to low-skill labor (Katz and Autor, 1999).

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.

These results add to the literature on technological change (Katz and Autor, 1999; Goldin and Katz, 2008, 2009; Acemoglu and Autor, 2011; Autor et al., 2003; Autor and Dorn, 2013; Goos et al., 2014). In particular, we add to the literature understanding when firms accelerate adoption of these technologies. Jaimovich and Siu (2015), Zhang (2017), and Hershbein and Kahn (2016) show that technology adoption is accelerated in recessions, when the opportunity cost of investing in technology is lower. Ma et al. (2018) find that M&As act as a catalyst for the adoption of these labor saving technologies. In this paper, we show adoption is accelerated in the presence of a deterioration in the

[7] Hadlock and Pierce (2010) find small firms are more likely to face financial constraints.

labor supply.

Finally, these results also add to the literature understanding 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. We instead show that opioid abuse hurts firm growth and valuations and that some of the costs imposed on firms from the reduced labor supply can be mitigated through greater technology adoption.

I The Opioid Crisis

Starting in the 1980s, there was a push in America to be more aggressive in treating pain. In 1997, The American Academy of Pain Medicine and the American Pain Society encouraged the specific use of opioids, arguing that the long-term risk of addiction was minimal. In 2001, the Joint Commission on Accreditation of Healthcare Organizations (TJC) argued that the treatment and monitoring of pain should be the fifth vital sign, creating a new metric upon which doctors and hospitals would be judged.[8] As late as 2011, the Institute of Medicine argued that pain was being undertreated in America. In this study, the authors acknowledged concerns about opioid prescriptions being diverted but argued "when opioids are used as prescribed and are appropriately monitored, they can be safe and effective" (Pizzo and Clark, 2012).

At the same time, Purdue Pharma, a private pharmaceutical company, was aggressively marketing OxyContin (oxycodone controlled-release), a new prescription opioid approved by the FDA in 1995. In advertising their new drug, Purdue Pharma made no mention of the addiction potential of OxyContin, relying on two small retrospective studies with questionable scientific rigor from the

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

1980s.[9] 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.[10] Additional lawsuits are still outstanding arguing that Purdue Pharma intentionally misled doctors and patients about the addiction risks associated with their opioid products.

Opioids have been shown to be an important and valuable resource for cancer pain and other end of life conditions. Opioids also have demonstrated value in the treatment of acute short-term pain, such as recovery from surgery, although risks do exist. In contrast, the Agency for Healthcare Research and Quality (AHRQ) concluded, as of 2014, that there is limited, if any, evidence-based medicine to support their use in chronic non-terminal pain (Chou et al., 2014).

Concerns about possible over-use of opioid prescriptions became more common in the 2000s. In 2016, the FDC issued a new policy recommendation regarding the prescribing of 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 concerns as well. 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 now have legislation limiting the quantity of opioids which can be prescribed.[11] At the federal level, Medicare also adopted a 7-day supply limit for new opioid patients in 2018. Another key action by states is the development of prescription drug monitoring programs (PDMPs), which allow doctors to better identify drug-seeking patients. Success in these state initiatives were followed with the National All Schedules Prescription Electronic Reporting Act (NASPER). In October of 2017, the US government declared opioids a public health emergency.

[9]Porter (1980) is a one paragraph letter to the editor in the New England Journal of Medicine. Portenoy and Foley (1986) was a study on 38 patients published in Pain.

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

[11]In Appendix Table IA1, we show that rates of deaths attributable to opioids is the main predictor of state-level legislation to curb opioid prescriptions.

II Data and Methodology

II.1 Data

We identify prescriptions using data provided by the Centers for Disease Control and Prevention (CDC), a federal agency under the Department of Health and Human Services.[12] The CDC compiles opioid prescription data from IQVIA Transactional Data Warehouse (TDW), starting in 2006. IQVIA is a multinational company serving clinical research and health information technologies. IQVIA TDW covers 50,000 retail (non-hospital) pharmacies, dispensing nearly 90% of all retail prescriptions in the United States. Opioid prescriptions include buprenorphine, codeine, fentanyl, hydrocodone, hydromorphone, methadone, morphine, oxycodone, oxymorphone, propoxyphene, tapentadol, and tramadol.[13]

In Table 1, we report summary statistics on county-level opioid prescription rates. We measure opioids as the count of total prescriptions per 100 people. The data covers 2,981 unique counties over the years 2006 to 2015, inclusive. On average, we report an opioid prescription rate of 87.63. The quantities are 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 for a month. Prescription rates in our data peak in 2012, then start to decline. A similar pattern is documented in Jones et al. (2018). We also document considerable variation by county as it is also observed in McDonald et al. (2012) and Paulozzi et al. (2014).

In Table 2, we report summary statistics for key county-level demographic and economic variables. Panel A reports summary statistics on demographic variables, inducing total population, distributions by gender, race and age, migration inflows and outflows, density of local physicians and neoplasms mortality. All variables are normalized by population. Data on population and mi-

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

[13]Cough and cold formulations containing opioids and buprenorphine products typically used to treat opioid use disorder are not included. In addition, methadone dispensed through methadone maintenance treatment programs is not included in the IQVIA TDW data.

gration comes from the Census. The count of physicians is from the Health Resources and Services Administration (HRSA).[14] Neoplasms mortality, a measurement of rates of cancer incidents, is from the CDC.[15]

In Table 2, Panel B, we report summary statistics on economic variables, including the labor force participation rate, unemployment rate, median household income, poverty ratio, and manufacturing rate. We use estimates of the labor force participation (LFP) by county, to match the granularity of the prescription data, using data from the BLS and Census.[16] Unemployment rate is from the BLS. Median household income and poverty data comes from the Census.[17] Manufacturing rates are from the Quarterly Workforce indicators (QWI), a part of the Longitudinal Employer Household Dynamics (LEHD) program.[18]

In Table 2, Panel B, we also report summary statistics on three county-level labor share variables, the high-skill, mid-skill and low-skill employee ratio. To calculate these ratios, we retrieve the count of employees by education from QWI. We classify an employee as high skill if they have completed their bachelor's degree or an advanced degree. An employee is classified as mid-skill if they have some college or an associate's degree. An employee is classified as low-skill if they have no college education. We calculate labor shares as the percent of employees in that county within a skill category.

[14]We obtain health data from the Area Health Resources Files (AHRF) (https://data.hrsa.gov/topics/health-workforce/ahrf). AHRF is an information system carried by the National Center for Health Workforce Analysis, Bureau of Health Workforce.

[15]We retrieve death source data from The Underlying Cause of Death database, CDC (https://wonder.cdc.gov/ucd-icd10.html).

[16]We measure labor force participation rate as labor force normalized by population. County-level labor force estimates comes from the Bureau of Labor Statistics (https://www.bls.gov/lau/lauov.htm). County-level population estimates comes from Census (https://www.census.gov). This data is based on the current population survey (CPS) as well as other data sources. The BLS CPS is designed to measure civilian labor force and unemployment. This is estimated using non-institutionalized individuals.

[17]Poverty ratio is normalized by population.

[18]We obtain employee data from Quarterly Workforce indicators (QWI) (https://ledextract.ces.census.gov/static/data.html). Manufacturing rate is measured by the number of employees in manufacturing industries (2-digit NAICS 31-33) scaled by the number of employees in all industries.

In Table 3, Panel A, 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 telecommunications 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), Bloom et al. (2014), and Tuzel and Zhang (2017). 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. This data is used by the sales and marketing teams at large US IT firms, such as IBM and Dell, thereby assuring high data quality, as errors would be quickly picked up by clients in their sales calls.

We report the data for the years 2011-2015, the years used in our later analysis. We drop observations in the agricultural, utility or public sectors. We limit the sample to establishments with a minimum of 10 employees before the start of the sample, in 2010, to ensure that our results are driven by economically important establishments. We end up with 3 million observations. The average establishment in our sample has a revenue of $11.7 million, invests $203.7 thousands in IT, including $27.8 thousand in hardware, $65.3 thousand in software, and $91.5 thousand in services. It has a stock of 57 PCs.

In Table 3, Panel B, we present statistics for our Compustat sample. We match the firms in our IT data to Compustat, matching on firm or subsidiary name.[19] We are able to match to 5,285 unique Compustat firms, approximately 61% of firms in Compustat over this entire period. The average firm in our sample has $11 million in assets, $4 million in sales, 11 thousand employees, and profitability of 5% (as measured by return on assets).

[19]To identify subsidiary names, we scraped the 10-Ks of all US firms and compiled a list of all subsidiary names affiliated with a given firm.

II.2 Methodology

To minimize concerns regarding auto-correlation in our data, we aggregate our data and create one observation per county and measure the change in the rate of opioid prescriptions between 2006 and 2010 and the change in the labor force participation rate from 2011 to 2015.[20] We then run a change on change regression, which negates the need for county fixed effects. Included in the regressions are controls for changes in economic and demographic characteristics as well as the underlying cancer rate in the county and availability of doctors. Standard errors are clustered at the county level. Specifically, we estimate the following specification:

$$\Delta LFP_c = \beta \cdot \Delta ln(historic\ opioid\ prescriptions)_c + \delta \cdot \Delta X_c + \Delta\epsilon_c \tag{1}$$

where Δ denotes the long (four year) difference operator to sweep out county fixed effects. c indexes county. ΔLFP is the change in county labor force participation rate from 2011 to 2015. Δ $ln(historic\ opioid\ prescriptions)$ is the change in the natural logarithm of one plus opioid prescription rates between 2006 and 2010. X are the change in county-level controls, including the logarithm of population, the logarithm of population squared, unemployment rate, the logarithm of median household income, poverty ratio, male ratio, white ratio, black ratio, American Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, manufacturing ratio, migration inflow, migration outflow, the logarithm of physicians and the logarithm of neoplasms mortality. We cluster standard errors at the county level.

To identify the effects of the opioid crisis on firm and establishment level outcomes, including firm level performance and investment in automation, we estimate the following specification:

$$\Delta y_{f,i,j,c} = \beta \cdot \Delta ln(historic\ opioid\ prescriptions)_c + \delta \cdot \Delta X_c + \alpha_j + \Delta\epsilon_{f,i,j,c} \tag{2}$$

where Δ denotes the long (four year) difference operator to sweep out firm (or establishment)

[20]We start with 2006 data on opioid prescriptions as this is the first year of available data. We end in 2015, the last year of available data for CITDB.

fixed effects in the firm (establishment) level analysis. f indexes firm, i indexes establishment (in the establishment-level analysis), j indexes industry and c indexes county. α_j are a full set of industry fixed effects that pick up industry trends as the equation is estimated in long differences. $\Delta ln(\textit{historic opioid prescriptions})$ is the change in the natural logarithm of opioid prescription rate between 2006 and 2010 in a county c. The unit of observation is an establishment in the establishment-level analysis. In the firm-level analysis, we still use establishment-level observations, to more accurately match the relevant labor markets, where each establishment of a Compustat firm has the same outcome variable. Δy is the change in firm performance from 2011 to 2015, including firm sales ($Ln(sales)$), firm employment ($Ln(employment)$), and ROA (defined as operating income before depreciation normalized by assets). ΔX is defined as in Equation (1). We cluster standard errors at the county level.

III Opioids and the Local Labor Markets

We begin by examining how historic opioid prescriptions in a community correlate with the available labor supply by estimating Equation (1). In Table 4, column 1, we observe a negative and statistically significant correlation between the growth in historic opioid prescriptions at the county and the change in the rate of labor force participation.[21] In terms of economic magnitude, an increase in opioid prescriptions from the 25th percentile to the 75th percentile of the distribution, which is equivalent to an additional 50 prescriptions per 100 people, is associated with a reduction in the labor force participation by 0.4% relative to the unconditional mean. In column 2, we add additional controls for demographics and economic conditions and continue to observe the same economic effect that remains statistically significant.

In columns 3 and 4, we sort states based on the rate of opioid prescriptions in 2006 (the start of our sample) and run the regressions looking only at the top 25 states ("high opioid") and bottom 25 states ("low opioid"). We present the two groups of states in Figure 1. Our intuition is that changes in opioid prescriptions should have a greater effect on the local labor market in high opioid states,

[21] We use county-level data and do not aggregate to a commuting zone given the frequency of missing data in some county-years which leaves a small sample of commuting-zone years where all counties are observed.

as compared to low opioid states. For example, prescription rates in 2006 were 44 in New York, versus 130 in West Virginia. As such, a 10% increase in our opioid measure should have less of an impact in New York, where the problem was less severe at the start of our sample, as compared to in West Virginia, one of the hardest hit states. Indeed, we show that the negative effect on labor force participation is driven by rapid increases in opioid prescription rates in high opioid states. In contrast, the relationship is weaker and not statistically significant for those states with lower initial rates of opioid use.

A similar relationship between opioid prescriptions and labor force participation is documented in two other studies. Harris et al. (2017) use opioid prescriptions from ten states measured between 2013 and 2015 and find 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, covering 1999-2001 and 2014-2016, and argues that opioid prescriptions can explain 20% of the decline in the LFP over this time frame. These results are also supported by survey data. In the same paper and 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.[22]

The results suggest a negative relation between opioid prescriptions and labor force participation. However, changes in labor force participation is only one measure of the impact opioids can have on the labor market. First, labor force participation is only measured on non-institutionalized

[22] Alternatively, Currie et al. (2018) finds that one year lagged opioid prescription rates positively predicts employment to population ratios for women. They find no statistical relation between lagged opioid prescription rates and male employment to population ratios. They argue that opioids can help to allow some women to successfully manage pain and, thus, stay employed. The difference between the results may reflect the use of a one year lag in Currie et al. (2018) as compared to our five-year lag. Over a short horizon, opioids may have positive effects. However, as chemical dependency increases as well as the potential for psychological addiction, the negative effects dominate.

individuals.[23] [24] For example, if opioids lead to an increase in prison populations resulting in individuals being removed from the labor market, these changes will not be reflected in our measure of labor force participation. Second, labor force participation does not capture any changes in the quality of the pool of available workers. Individuals abusing opioids are more likely to miss work, to be involved in on-the-job injuries, and to be overall less productive. In sum, our estimate of the impact on labor force participation likely underestimates the impact of the opioid crisis on the supply and productivity of potential employees.

Our results show a negative correlation between historic opioid prescriptions and the local labor supply. The key omitted variable concern is that in areas with worse future job opportunities, individuals are more likely to seek out opioid prescriptions. This concern is minimized in our approach due to the use of first differences, which control for time invariant differences in county economic conditions. Also, the use of the five-year lag between the measurement of opioid prescriptions and economic outcomes, requires that opioid seeking behavior is influenced by economic expectations that will not be realized for a number of years. Moreover, we also directly control for the local unemployment rate, poverty rate and median household income. In addition, in the Appendix Table IA2, we directly investigate the drivers of opioid prescription rates. We find that economic variables cannot predict opioid prescriptions after including county fixed effects.

In sum, while we cannot rule out the possibility of omitted variables, the results suggest that higher historic rates of opioid prescriptions can limit the supply of labor available to firms, effectively increasing the relative price of labor. This can have important implications for firms, a possibility we examine in the following section.

---

[23] https://www.census.gov/topics/income-poverty/poverty/guidance/group-quarters.html; https://www.bls.gov/cps/cps_htgm.pdf

[24] There is also evidence that the CPS, the underlying data in the estimate of the labor force participation undercounts low income individuals. For example, Davern et al. (2009) compares enrollees in Medicaid with CPS records and shows a 42-43% undercount of Medicaid recipients in the CPS. They argue that 12% points of the difference reflects imperfect concept alignment, impacted by segments of the Medicaid sample not being included in the CPS. Given Medicaid is a program specifically for the poor, individuals with opioid addictions and difficulty remaining employed are likely to be covered by this program. The rest of the undercount is explained by individuals in the CPS incorrectly answering the health insurance question.

IV Opioids and Firm Characteristics

In this section, we explore the relation between opioid prescriptions and firm performance. Table 5 reports our findings. We start, in Panel A, from a sample of all 50 states and the District of Columbia and examine the relationship between changes in our opioid prescription measure and changes in firm sales, employment, and profitability, using only publicly traded firms. Each observation reflects an establishment identified in the CiTDB data and matched to a Compustat firm. All establishments associated with the same firm report the same dependent variable but independent variables are estimated based on the location of the establishment. We include a full set of (4-digit NAICS) industry dummies in the specifications to control for differential industry trends. We include all economic and demographic controls used in the labor force participation regressions. We find a weakly negative relationship between opioid prescriptions and firm sales and an insignificant relationship between opioid prescriptions and both employment and ROA.

In Panel B, we repeat our estimation considering just the 25 high opioid states. Consistent with our findings in Table 4, where we observe a significant relation between opioid prescriptions and labor supply in the areas hardest hit by the epidemic, we estimate a negative and statistically significant coefficient on sales and employment in those states. In contrast, in Panel C, we do not find a significant relationship between opioid prescriptions and firm performance in the low opioid states.

These results suggest that labor shortages, associated with the opioid crisis, can harm firm performance. It is important to acknowledge that higher rates of drug use might also dampen local demand in the affected areas. That said, we estimate economically similar results (albeit statistically noisier) when we confine our sample to the tradeable sectors, following Mian and Sufi (2014), suggesting that labor shortage is an important channel through which the opioid crisis can affect firm values.

In Table 6, we repeat our prior analysis using establishment-level revenue. Given, establishment-level revenue is observed in the CiTDB data and available for all establishments, we no longer confine our analysis to establishments matched to Compustat firms. We observe a similar pattern as in the previous table. There is a negative relationship between opioid prescriptions and long-term changes in establishment sales that is statistically significant only for the set of states with high initial rates of prescriptions.

V How do Firms Respond to the Opioid Crisis?

We next examine whether firms respond to opioid associated labor shortages by increasing investment in labor-saving automation. With greater automation, firms will require relatively fewer low- and mid-skill employees (Autor et al., 2003). While we cannot directly observe automation investments specifically meant to reduce labor costs, any such investments will require additional IT spending, investments which are observable using the CiTDB establishment-level data. We focus exclusively on establishments in the 25 high opioid states. In Panel A, Table IA3, we focus on the overall IT budget, as well as its three largest components: hardware, software, and services. We also use the count of computers (PCs) as a proxy for the stock of technology installed at a given establishment. In Panel B, we normalize all five measures by establishment revenue. In all regressions, we include the full set of economic and demographic controls as well as industry fixed effects and cluster standard errors at the county-level.

In both Panels, Table IA3, we observe that an increase in opioid prescriptions is positively associated with IT spending and stocks.[25] The results are statistically and economically important, no matter what definition of IT investment we use.

We next consider heterogeneity starting from the intuition that technology is best at replacing the low- to mid-skill workers impacted by the opioid crisis. Opioid use tends to be more common among low-skill employees (Case and Deaton, 2015). Firms that are more dependent on those types of employees are more likely to benefit the most from technology adoption. To this end, we interact $\Delta ln(historic\ opioidprescriptions)$ with an indicator variable $(low - skill\ high)$ that takes a value of one if the establishment is matched to an industry-state with an above sample median share of employees who do not have a college degree in 2005 (before the start of the sample). Consistent

[25] In the Appendix Table **??**, we show the results for the column 1 dependent variables using different empirical specifications. We show with no controls, just demographic controls and with the full set of controls. Results are qualitatively similar across all specifications.

with this argument, in Table 8, we find stronger correlations between past opioid prescriptions and IT in establishments that belong to industry-states with a higher share of low-skill labor.

We next consider heterogeneity in the ability of a firm to finance investments in automation. Such investments are typically assumed to come with high fixed costs, potentially limiting the ability of financially constrained firms to pursue greater automation. We follow Hadlock and Pierce (2010) and proxy for financial constraints using firm size, measured by whether or not the firm is multi-establishment. In Table 9, we interact $\Delta ln(historic\ opioid\ prescriptions)$ with an indicator variable ($multi\ high$) that takes a value of one if the establishment is matched to a firm with an above sample median number of establishments as of 2010. Consistent with the argument, we generally find stronger correlations in larger firms. In untabulated results, we find qualitatively similar results if we instead measure firm size using establishment revenue.

Finally, we consider heterogeneity in rates of on-the-job injuries by industry. Employees abusing opioids are more likely to be involved in an accident at work, making employers with industrial production which involves high injury risk to be especially wary of hiring such employees and relatively more likely to use automation to reduce labor inputs in high opioid areas. In Table 10, we interact $\Delta ln(historic\ opioidprescriptions)$ with an indicator variable ($injury\ high$) that takes a value of one if the establishment is matched to an industry with an average annual injury rate per worker above 5%, as of 2005. As predicted, we find stronger correlations between opioid prescriptions and IT investment in firms operating in industries with higher on-the-job injury rates.[26]

Although our empirical analysis does not rely on exogenous variation in opioid use, the results are consistent with a causal relation between opioid abuse and greater automation. Our key identifying assumption is that opioid prescriptions written by doctors were unlikely to be determined based on economic conditions 5 years later, after controlling for time invariant county differences, industry trends and a battery of time-varying county demographic and economic characteristics. Such an argument is supported by evidence in Ruhm (2018), which documents little to no negative correlation between economic outcomes and opioid deaths. Likewise, the SHED survey finds that 54% of adults

[26]In untabulated results, we also show higher treatment effects in industries where Graetz and Michaels (2018) argues robots are better suited to replace employees.

who know someone addicted to opioids (i.e. 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.[27]

Furthermore, we provide additional support for a causal interpretation by showing that our results are robust to including firm fixed effects in the differenced equations (i.e., we estimate trend-adjusted difference in differences regressions). As such, firms increase technology investment relatively more at their establishments located in counties with higher growth in past opioid prescription rates as compared to establishments located in counties with lower past opioid prescription growth. Table 11 presents the results and shows a positive and statistically significant relationship at the 1% level in all regressions.

Finally, we show that the firm-specific evidence we have documented so far can be generalized to the county level. The literature on technology adoption has shown that technology disproportionately replaces low- and mid-skill workers (Autor et al., 2003), and is complementary to high-skill workers at the top of the skill distribution (Katz and Autor, 1999). As such, technology adoption is associated in the literature with occupational shifts away from low skill and towards more high-skill workers. To test if this holds in our setting, we compile data of total employment by education from the Quarterly Workforce Indications (QWI). High skill employment is based on employees who have at least a bachelor's degree. Mid-skill employment is defined based on employees with some college or an associate's degree. Low-skill employment is instead based on employees with no college education. We calculate the respective shares of high-, mid- and low-skill employment as the percent of employees in a given county-year.

We estimate specifications as in Equation (1) and report results in Table 12. Column 1 shows a positive correlation between the change in opioid prescriptions and the percent of high skill employees at the county level. In contrast, column 3 shows a negative correlation between the change in opioid prescriptions and the percent of low skill employees. An increase of 50 opioid prescriptions per 100

[27] https://www.federalreserve.gov/econres/notes/feds-notes/shedding-light-on-our-economic-and-financial-lives-20180522.htm

people, predicts a 0.47% increase in the percent of high-skill employees. Following the earlier tests, we estimate these results in the sample of 25 high opioid states.

VI Laws to Limit Opioid Abuse and Firm Values

In response to the opioid crisis, state legislatures have started taking actions. Starting in 2016, Massachusetts passed a law to limit opioid prescriptions. The law imposed a seven-day limit of opioid prescriptions, with exemptions under certain circumstances such as 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".[28] Several states followed with a total of 25 states having passed similar legislation imposing limits on opioid prescriptions by 2018. The list of state laws is presented in the Internet Appendix.

We exploit the staggered adoption of these state laws in a differences-in-differences framework to analyze their effect on firm values. Given the timing of these laws, we cannot estimate their long-term effects on labor market outcomes or firm performance. However, we can estimate firms' stock price reaction at the announcement of the law's passage. To this end, for each firm listed in Compustat and CRSP, we estimate the daily average abnormal return for each event date using the market model, or the Fama-French three- or four-factor model.[29] 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 then regress three-day cumulative abnormal returns, CAR[-1,1], on an indicator variable that is one if a firm's headquarter is located in the state that an opioid-related law passes the House or the Senate or regulation announced, and zero if in other states *law passage*. If a state passed more than one law, the first law is considered. If a state introduced a policy before the first law, the regulation is considered.[30] We consider two dates for each law: the date the law was voted by the House and the date it was voted by the Senate. For each regulation, we consider the date the regulation was

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

[29]We drop observations in the agricultural, utility, and health-care industries.

[30]The list of state regulation is presented in the internet Appendix.

announced.

Consistent with our intuition 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 IT investment. We define *IT budget low* to be one if the 2015 IT budget is below the sample median, and 0 otherwise.[31] We interact *law passage* with *IT budget low* in Table 13. Across specifications, we control for event fixed effects, defined separately for the dates each law passed the house or the Senate. We cluster standard errors at the state level.[32]

In column 1, we use the market model to calculate the three-day cumulative abnormal returns. We find a cumulative abnormal return of 40 basis points that is statistically significant at the 1% level. In column 2, we additionally include firm fixed effects to absorb any time-invariant firm characteristics, firm controls as well as state level economic and demographic controls. We similarly estimate a 50 basis point stock price reaction that is statistically significant. The results are robust in columns 3-4 when we instead consider the Fama-French 3- or 4-factor model to estimate the abnormal returns.[33] These results indicate that the set of firms which have invested relatively less in technology, firms which are more exposed to the labor shortages brought by the opioid crisis, benefit the most from the state legislations.

Given the rising concerns about the opioid epidemic, the federal government also took action and announced a regulation on April 2, 2018. The regulation limits opioid prescriptions for patients with Medicare coverage. According to the regulation, a ceiling of opioid dosage would be established at 90 MME.[34] The pharmacist would have to talk to the prescribing doctor for any prescription at or above that level. The regulation also imposed a limit of an initial 7-day supply of opioids for Medicare beneficiaries.

---

[31]In Table IA4, we show these results are robust to considering instead the count of PCs in 2015.

[32]We get similar results if we use robust standard errors instead of clustering.

[33]In Internet Appendix Table IA5, 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.

[34]According to a non-for-profit news service, about 1.6 million Medicare beneficiaries met or exceeded opioid doses of 90mg MME for at least one day in 2016. https://www.painnewsnetwork.org/stories/2018/4/2/medicare-finalizes-plan-to-reduce-high-dose-opioids

We estimate a three-day cumulative abnormal return as in Table 14. We assign firms to states of headquarters based on whether they have passed a law or regulation limiting opioid prescriptions at an earlier point in time. Thus, *no prior state limits* takes a value of one for the 29 states that have not taken any action to limit opioid prescriptions (regulation or legislation) prior to the federal announcement. We interact this variable with *low IT budget*. In our specifications, we include firm and state controls, as well as industry fixed effects. We find a positive and significant announcement return of 60-70 basis points that is robust across specifications among low-IT firms in states without prior limits.

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 document a negative and significant relation between the growth in historic opioid prescriptions at the county level and the change in labor force participation, supporting the argument that higher rates of opioid prescriptions negatively impacted the supply of workers.

This is the first paper to document the negative effects of opioids on long-term firm performance and valuations. Using historical opioid prescription rates, we show that firm performance deteriorates in the long-run using a number of measures. 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. Consistent with causality, we find a significant and positive stock price reaction following the staggered adoption of state laws passage intended to limit opioid prescription rates.

We also show that firms respond to labor shortages due to opioids by investing in automation technologies. We show a positive and significant relation between the growth in historic opioid prescriptions and IT investments. The effect is concentrated in states hardest hit by the opioid crisis and firms that rely more on low skill labor. In effect, the firms are substituting capital for labor. 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 form of production at firms, a change which can have lasting impacts on the local labor markets.

References

[1] Acemoglu and Autor, 2011. “Skills, Tasks and Technologies: Implications for Employment and Earnings” *Handbook of Labor Economics* 4: 1043-1171.

[2] Acemoglu and Finkelstein, 2008. “Input and Technology Choices in Regulated Industries: Evidence from the Health Care Sector” *Journal of Political Economy*, 116: 837-880.

[3] Autor and Dorn, 2013. “The Growth of Low Skill Service Jobs and the Polarization of the U.S. Labor Market” *American Economic Review*, 103: 1553 -1597.

[4] Autor, Levy and Murnane, 2003. “The Skill Content of Recent Technological Change: An Empirical Exploration” *Quarterly Journal of Economics*, 118: 1279-1333.

[5] Barnett, Olenski, and Jena, 2017. “Opioid-Prescribing Patterns of Emergency Physicians and Risk of Long-Term Use” *New England Journal of Medicine*, 376: 663-673.

[6] Bena Ortiz-Molina, and Simintzi, 2018. “Shielding Firm Value: Employment Protection and Process Innovation” working paper.

[7] Bena and Simintzi, 2018. “Globalization of Work and Innovation: Evidence from Doing Business in China” working paper.

[8] Bloom, Garicano, Sadun and Van Reenen, 2014. “The Distinct Effects of Information Technology and Communication Technology on Firm Organization” *Management Science*, 60: 2859-2885.

[9] Brynjolfsson and Hitt, 2003. “Computing Productivity: Firm-Level Evidence” *Review of Economics and Statistics*, 85: 793–808.

[10] Cantrill, Brown, Carlisle, Delaney, Hays, Nelson, O’Connor, Papa, Sporer, Todd, and Whitson, 2012. “Clinical Policy: Critical Issues in the Prescribing of Opioids for Adult Patients in the Emergency Department” *Annals of Emergency Medicine*, 60: 499-525.

[11] Case and Deaton, 2015. “Rising Morbidity and Mortality in Midlife among White non-Hispanic Americans in the 21st Century” *Proceedings of the National Academy of Sciences of the United States of America*, 112: 15078-15083.

[12] Chou, Turner, Devine, Hansen, Sullivan, Blazina, Bougatsos, Dana, Bougatsos, and Deyo, 2014. “The Effectiveness and Risks of Long-Term Opioid Therapy for Chronic Pain: A Systematic Review for a National Institutes of Health Pathways to Prevention Workshop” *Annals of Internal Medicine*, 162: 276-295.

[13] Cicero, Surratt, Inciardi and Munoz, 2007. “Relationship between Therapeutic Use and Abuse of Opioid Analgesics in Rural, Suburban and Urban Locations in the United States” *Pharmacoepidemiology and Drug Safety*, 16: 827-840.

[14] Compton, Boyle, and Wargo, 2015. “Prescription Opioid Abuse: Problems and Responses” *Preventive Medicine*, 80: 5-9.

[15] Currie, Jin, and Schnell, 2018. "U.S. Employment and Opioids: Is There a Connection?" NBER Working Paper.

[16] Dasgupta, Kramer, Zalman, Carino Jr., Smith, Haddox, and Wright IV, 2006. "Association between Non-Medical and Prescriptive Usage of Opioids" *Drug and Alcohol Dependence*, 82: 135-142.

[17] Davern, Klerman, Baugh, Call and Greenberg, 2009. "An Examination of the Medicaid Undercount in the Current Population Survey: Preliminary Results from Record Linking" *Health Services Research Journal*, 44: 965-987.

[18] Florence, Luo, Xu, and Zhou, 2016. "The Economic Burden of Prescription Opioid Overdose, Abuse and Dependence in the United States, 2013" *Medical Care*, 54: 901-906.

[19] Flynn, Joe, Broome, Simpson, and Brown, 2003. "Recovery from Opioid Addiction in DATOS" *Journal of Substance Abuse Treatment*, 25: 177-186.

[20] Graetz and Michaels, 2018. "Robots at Work" *The Review of Economics and Statistics*, forthcoming.

[21] Goldin and Katz, 2008. "The Race Between Education and Technology" Harvard University Press.

[22] Goldin and Katz, 2009. "The Race Between Education and Technology: The Evolution of U.S. Wage Differential, 1980-2005" NBER working paper.

[23] Goos, Manning and Salomons, 2014. "Explaining Job Polarization: Routine-Biased Technological Change and Offshoring" *American Economic Review*, 104: 2509-2526.

[24] Hadlock and Pierce, 2010. "New Evidence on Measuring Financial Constraints: Moving Beyond the K-Z Index" *Review of Financial Studies*, 23:1909-1940.

[25] Harris, Kessler, Murray and Glenn, 2017. "Prescription Opioids and Labor Market Pains" working paper.

[26] Hershbein and Kahn, 2016. "Do Recessions Accelerate Routine-Biased Technological Change? Evidence from Vacancy Postings" *American Economic Review*, 108: 1737-1772.

[27] International Federation of Robotics, 2012. "World Robotics Industrial Robots 2012".

[28] Jaimovich and Siu, 2015. "The Trend in the Cycle Job Polarization and Jobless Recoveries" working paper.

[29] Jones, Viswanath, Peck, Kaye, Gill, and Simopoulos, 2018. "A Brief History of the Opioid Epidemic and Strategies for Pain Medicine" *Pain and Therapy*, 7: 13-21.

[30] Katz and Autor, 1999. "Changes in the Wage Structure and Earnings Inequality" *Ashenfelter O, Card D Handbook of Labor Economics*, 3: 1463-1555.

[31] Krueger, 2017. "Where Have All the Workers Gone? An Inquiry into the Decline of the U.S. Labor Force Participation Rate" Brookings Paper on Economic Activity, BPEA Conference Drafts, 1-87.

[32] Ma, Ouimet and Simintzi, 2018. "Mergers and Acquisitions, Technological Change and Inequality" working paper.

[33] McDonald, Carlson and Izrael, 2012. "Geographic Variation in Opioid Prescribing in the U.S" *Journal of Pain*, 13: 988-996.

[34] Mian and Sufi, 2014. "What Explains the 2007–2009 Drop in Employment?" *Econometrica*, 82: 2197-2223.

[35] Modarai, Mack, Hicks, Benoit, Park, Jones, Proescholdbell, Ising, and Paulozzi, 2013. "Relationship of Opioid Prescription Sales and Overdoses, North Carolina" *Drug and Alcohol Dependence*, 132: 81-86.

[36] Paulozzi, Mack and Hockenberry. "Vital Signs: Variation Among States in Prescribing of Opioid Pain Relievers and Benzodiazepines — United States, 2012" *Morbidity and Mortality Weekly Report*, 63: 563-568.

[37] Pizzo and Clark, 2012. "Alleviating Suffering 101 — Pain Relief in the United States" *The New England Journal of Medicine*, 366: 197-199.

[38] Poon and Greenwood-Ericksen, 2014. "The Opioid Prescriptions Epidemic and the Role of Emergency Medicine" *Annals of Emergency Medicine*, 64: 490-495.

[39] Portenoy and Foley, 1986. "Chronic Use of Opioid Analgesics in Non-Malignant Pain: Report of 38 Cases" *Pain*, 25: 171-186.

[40] Porter, 1980. "Addiction Rare in Patients Treated with Narcotics" *The New England Journal of Medicine*, 302:123.

[41] Ruhm, 2018. "Deaths of Despair or Drug Problems?" NBER Working Paper.

[42] Schwartz, 2017. "Economy Needs Workers, but Drug Tests Take a Toll" *The New York Times.*

[43] Shei, Rice, Kirson, Bodnar, Birnbaum, Holly and Ben-Joseph, 2015. "Sources of Prescription Opioids among Diagnosed Opioid Abusers" *Current Medical Research and Opinion*, 31: 779-784.

[44] Tamayo-Sarver, Dawson, Cydulka, Wigton, and Baker, 2004. "Variability in Emergency Physician Decisionmaking About Prescribing Opioid Analgesics" *Annals of Emergency Medicine*, 43: 483-493.

[45] Tuzel and Zhang, 2017. "Economic Stimulus at the Expense of Routine-Task Jobs" working paper.

[46] Volkow and McLellan, 2016. "Opioid Abuse in Chronic Pain — Misconceptions and Mitigation Strategies" *New England Journal of Medicine*, 374: 1253-1263.

[47] Shei, Rice, Kirson, Bodnar, Birnbaum, Holly and Ben-Joseph, 2015. "Sources of Prescription Opioids among Diagnosed Opioid Abusers" *Current Medical Research and Opinion*, 31: 779-784.

[48] Wisniewski, Purdy and Blondell, 2008. "The Epidemiologic Association Between Opioid Prescribing, Non-Medical Use, and Emergency Department Visits" *Journal of Addictive Diseases*, 27: 1-11.

[49] Zhang, 2017. "Labor-Technology Substitution: Implications for Asset Pricing" *Journal of Finance*, forthcoming.

Appendix: Variable Definitions

County-level opioid prescriptions, demographic and economic variables:

Opioid prescriptions is measured as the count of total opioid prescriptions per 100 people in a given county.

Male ratio is measured by the male population divided by total population in a given county.

White ratio is measured by the white population divided by total population in a given county.

Black ratio is measured by the black population divided by total population in a given county.

American Indian ratio is measured by the American Indian population divided by total population in a given county.

Hispanic ratio is measured by the Hispanic population divided by total population in a given county.

Age $20 - 64$ *ratio* is measured by the population between age 20 and 64 divided by total population in a given county.

Age over 65 *ratio* is measured by the population over age 65 divided by total population in a given county.

Migration inflow ratio is measured by the population that are current residence in a county but were residence in another county one year ago normalized by total population in a given county.

Migration outflow ratio is measured by the population that were residence in a county one year ago but are current residence in another county normalized by total population in a given county.

Physicians is measured by the number of primary care physicians, excluding hospital residents or age 75 years or over, normalized by population in a given county.

Neoplasms mortality is measured by the number of deaths due to neoplasms (ICD-10 C00-D48), normalized by population in a given county.

Labor force participation rate is measured by labor force normalized by population in a given county.

Unemployment rate is measured by the number of unemployed divided by the sum of employed and unemployed in a given county.

Poverty ratio is measured by the number of poverty divided by total population in a given county.

Manufacturing rate is measured by the number of employees in manufacturing industries normalized by the number of employees in all industries in a given county.

High − skill share is measured by the number of employees who complete a bachelor's degree or advanced degree normalized by the number of all employees in a given county.

Mid−skill share is measured by the number of employees who have some college or an associate's degree normalized by the number of all employees in a given county.

Low−skill share is measured by the number of employees who do not have any college education normalized by the number of all employees in a given county.

Establishment-level variables:

Low−skill high is an indicator equal to one if the establishment is matched to an industry-state with an above sample median share of employees who do not have a college degree in 2005, otherwise zero.

Injury high is an indicator equal to one if the establishment is matched to an industry with an average annual injury rate per worker above 5%, as of 2005, otherwise zero.

Multi high is an indicator equal to one if the establishment is matched to a firm with an above sample median number of establishments as of 2010, otherwise zero.

Firm-level financial variables:

ROA is measured by operation income before depreciation standardized by total assets.

Tobin's Q is measured by market value divided by total assets. Market value is measured by price close multiplied by common shares outstanding.

State-level law-related variables:

Law passage is an indicator equal to one if a firm's headquarter is located in the state that the first opioid-related law passes the House or the Senate or the first opioid-related policy is announcement, and zero if in other states.

Law sign is an indicator equal to one if a firm's headquarter is located in the state that an opioid-related law passes the House or the Senate, and zero if in other states.

No prior state limits is an indicator equal to one if a firm's headquarter state has not passed any opioid-related law or announced opioid-related policy before event date, and zero otherwise.

IT budget low is an indicator equal to one if a firm's IT budget as of 2015 is below the sample median, and zero otherwise.

PCs low is an indicator equal to one if a firm's number of PCs as of 2015 is below the sample median, and zero otherwise.

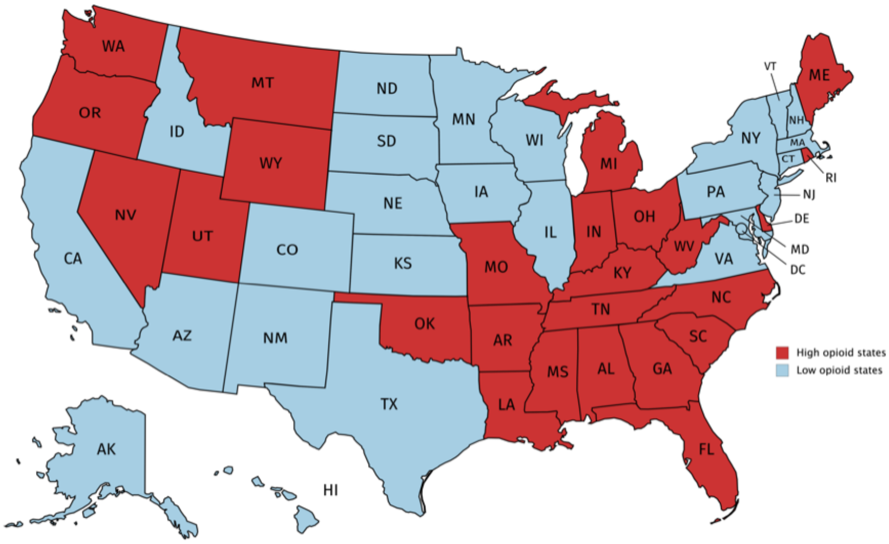

Figure 1: State-Level Distribution of Opioid Prescriptions

This figure plots the distribution of opioids based on state-level opioid prescription rates as of 2006. States with high opioids are in red and states with low opioids are in blue.

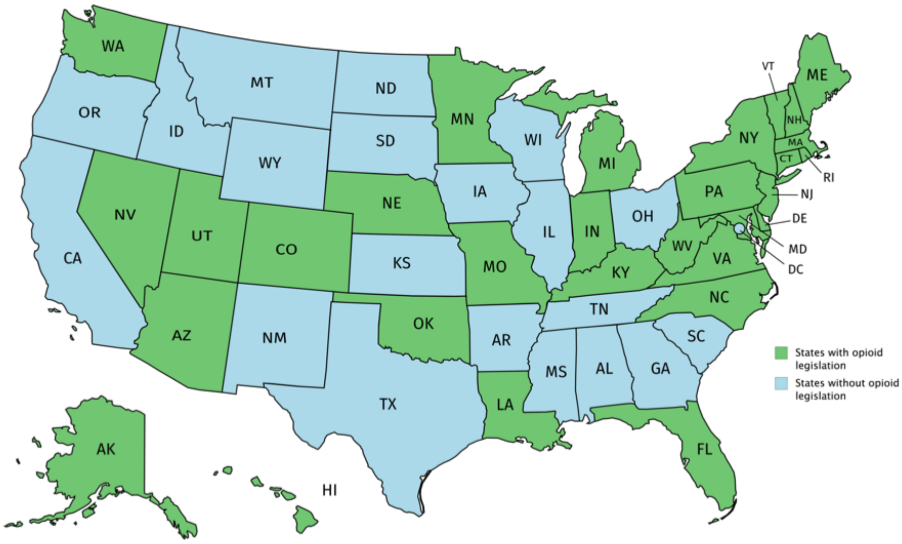

Figure 2: Legislation to Limit Opioid Abuse

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

Table 1: Summary Statistics of Opioid Prescriptions

This table reports descriptive statistics of opioid prescriptions. Opioid prescriptions is defined in the Appendix and winsorized at 1% level.

Year	N	Mean	P25	Median	P75	Std. Dev.
2006	2,754	80.07	51.90	74.50	100.70	41.25
2007	2,746	84.25	55.00	78.35	106.30	43.07
2008	2,758	87.15	56.80	81.50	110.30	44.65
2009	2,750	89.31	58.10	83.10	114.30	45.68
2010	2,741	91.85	60.00	85.70	117.10	46.46
2011	2,745	91.72	59.70	86.20	117.60	46.19
2012	2,736	95.30	63.35	89.85	120.40	46.15
2013	2,753	92.92	62.30	87.50	118.40	44.71
2014	2,960	85.16	54.50	82.15	112.70	46.72
2015	2,963	79.44	51.50	76.80	104.80	43.67
Total	27,906	87.63	57.10	82.30	112.50	45.18

Table 2: Summary Statistics of Demographic and Economic Variables

This table reports descriptive statistics of county-level demographic and economic variables between 2010 and 2014. Panel A reports summary statistics on demographic variables. Panel B reports summary statistics on economic variables. All variables are defined in the Appendix and winsorized at 1% level.

Variables	N	Mean	Median	Std. Dev.
Panel A. Demographic variables				
Population (1000)	13,749	100.00	32.48	197.33
Male ratio (%)	13,749	49.86	49.51	1.84
White ratio (%)	13,749	85.62	91.98	15.26
Black ratio (%)	13,749	9.29	2.59	14.03
American Indian ratio (%)	13,749	1.68	0.52	4.48
Hispanic ratio (%)	13,749	8.23	3.62	11.95
Age 20-64 ratio (%)	13,749	57.91	57.89	3.05
Age over 65 ratio (%)	13,749	16.40	16.14	4.00
Migration inflow (%)	13,749	13.71	13.27	4.07
Migration outflow (%)	13,749	13.40	13.02	3.45
Physicians (per 100,000)	13,749	55.73	51.22	30.46
Neoplasms mortality (per 100,000)	13,749	233.18	230.69	65.05
Panel B. Economic variables				
Labor force participation rate (%)	13,749	47.20	47.49	6.12
Unemployment rate (%)	13,749	8.03	7.80	2.85
Median household income ($1000)	13,749	45.20	43.22	11.10
Poverty ratio (%)	13,749	16.33	15.74	5.76
Manufacturing rate (%)	13,749	12.93	10.70	9.81
High-skill share (%)	13,749	21.83	20.86	4.55
Mid-skill share (%)	13,749	33.04	32.80	2.18
Low-skill share (%)	13,749	45.10	45.45	4.81

Table 3: Summary Statistics on IT Investment and Firm Characteristics

Panel A reports descriptive statistics of establishment-level IT investment variables. Panel B reports summary statistics of Compustat firm-level financial variables. All variables are defined in the Appendix and winsorized at 1% level.

Variables	N	Mean	Median	Std. Dev.
Panel A. IT investment				
Revenue ($million)	3,190,276	11.68	4.00	21.45
IT budget ($1000)	3,123,431	203.68	63.18	384.26
Hardware budget ($1000)	3,123,431	27.78	8.00	50.59
Software budget ($1000)	3,123,431	65.27	18.00	124.64
Services budget ($1000)	3,123,431	91.48	29.00	176.08
PCs	3,168,171	57.20	25.00	75.74
Panel B. Firm characteristics				
Total asset ($million)	18,764	11.33	0.91	83.58
Sales ($million)	18,734	3.87	0.45	13.60
Employee (1000)	17,645	11.09	1.30	36.71
ROA	18,121	0.05	0.08	0.19
Sales/employment ($1000)	17,370	0.54	0.28	0.82

Table 4: Opioid Prescriptions and Labor Force Participation

This table explores the relation between the labor force participation and historic opioid prescriptions. The sample in column (1) and (2) includes all U.S. counties, sample in column (3) includes counties in high opioid states, and sample in column (4) includes counties in low opioid states. All variables are measured as the change over a four-year window. Control variables are lagged by one year and historic opioid prescriptions is lagged by five years. Standard errors are clustered at county level. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	ΔLabor force participation rate			
	All states		High opioid states	Low opioid states
	(1)	(2)	(3)	(4)
ΔLn(Historic opioid prescriptions)	-0.308**	-0.268**	-0.299*	-0.185
	(0.140)	(0.136)	(0.181)	(0.204)
ΔUnemployment rate		-0.107***	-0.133***	-0.167**
		(0.029)	(0.034)	(0.065)
ΔLn(Median household income)		5.328***	5.535***	4.377***
		(0.783)	(1.171)	(1.155)
ΔPoverty ratio		0.003	-0.006	0.022
		(0.023)	(0.027)	(0.041)
ΔManufacturing rate		0.088***	0.069***	0.126***
		(0.018)	(0.021)	(0.032)
ΔLn(Population)		-8.614**	-10.944*	-8.363
		(3.818)	(6.617)	(5.163)
ΔLn(Population) squared		1.065***	1.545**	0.877
		(0.410)	(0.730)	(0.537)
ΔMale ratio		-0.206*	-0.072	-0.330*
		(0.117)	(0.156)	(0.193)
ΔWhite ratio		-0.158	-0.059	-0.090
		(0.112)	(0.241)	(0.118)
ΔBlack ratio		-0.197	-0.177	0.050
		(0.134)	(0.249)	(0.183)
ΔAmerican Indian ratio		-0.025	0.703*	-0.279
		(0.199)	(0.381)	(0.236)
ΔHispanic ratio		-0.220***	-0.153	-0.311***
		(0.079)	(0.125)	(0.107)
ΔAge 20-64 ratio		0.165**	0.149	0.142
		(0.072)	(0.101)	(0.100)
ΔAge over 65 ratio		-0.163**	-0.264***	-0.135
		(0.065)	(0.094)	(0.096)
ΔMigration inflow		-0.051**	-0.057**	-0.040
		(0.023)	(0.029)	(0.038)
ΔMigration outflow		0.024	0.051*	-0.003
		(0.021)	(0.026)	(0.032)
ΔPhysicians		-0.003	0.002	-0.005
		(0.003)	(0.005)	(0.004)
ΔNeoplasms mortality		0.000	0.001	-0.000
		(0.001)	(0.001)	(0.001)
Observations	2,694	2,694	1,450	1,244
R^2	0.002	0.069	0.081	0.069

Table 5: Opioid Prescriptions and Firm Characteristics

This table explores the relation between firm characteristics and historic opioid prescriptions. The sample in Panel A is our baseline sample of establishments observed in the CITDB data. The sample in Panel B includes all baseline observations in high opioid states. The sample in Panel C includes all baseline observations in low opioid states. The sample in Panel D includes all baseline observations in high opioid states from tradable industries. Definition on tradable industries follows Mian and Sufi (2014). The dependent variable is the logarithm of sales in column (1), the logarithm of employment in column (2) and ROA in column (3). Controls include all additional variables included in Table 4. All variables are measured as the change over a four-year window. Control variables are lagged by one year and historic opioid prescriptions is lagged by five years. Standard errors are clustered at county level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	ΔLn(Sales)	ΔLn(Employment)	ΔROA
	(1)	(2)	(3)
	Panel A. All states		
ΔLn(Historic opioid prescriptions)	-0.018**	-0.013	-0.000
	(0.009)	(0.008)	(0.002)
ΔCounty controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	105,375	103,365	103,265
R^2	0.196	0.147	0.232
	Panel B. High opioid states		
ΔLn(Historic opioid prescriptions)	-0.049***	-0.030**	-0.000
	(0.014)	(0.014)	(0.002)
ΔCounty controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	44,314	43,552	43,517
R^2	0.212	0.161	0.234

	ΔLn(Sales)	ΔLn(Employment)	ΔROA
	(1)	(2)	(3)
	Panel C. Low opioid states		
ΔLn(Historic opioid prescriptions)	0.005	0.002	0.001
	(0.011)	(0.011)	(0.002)
ΔCounty controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	61,046	59,797	59,732
R^2	0.193	0.143	0.240
	Panel D. Tradable industries in high opioid states		
ΔLn(Historic opioid prescriptions)	-0.077**	-0.047**	-0.003
	(0.031)	(0.023)	(0.006)
ΔCounty controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	9,296	9,112	9,275
R^2	0.126	0.085	0.195

Table 6: Opioid Prescriptions and Establishment Revenue

This table explores the relation between establishment revenue and historic opioid prescriptions. The sample in column (1) is our baseline sample of establishments observed in the CITDB data. The sample in column (2) includes all baseline observations in high opioid states. The sample in column (3) includes all baseline observations in low opioid states. The dependent variable is the logarithm of establishment revenue. Controls include all additional variables included in Table 4. All variables are measured as the change over a 4 year window. Control variables are lagged by one year and historic opioid prescriptions is lagged by 5 years. Standard errors are clustered at county level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δ Ln(Revenue)		
	All states	High opioid states	Low opioid states
	(1)	(2)	(3)
ΔLn(Historic opioid prescriptions)	-0.013	-0.025**	-0.013
	(0.009)	(0.010)	(0.012)
ΔCounty controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Observations	624,052	258,874	365,177
R^2	0.546	0.555	0.543

Table 7: Opioid Prescriptions and IT Investment

This table explores the relation between information technology and historic opioid prescriptions. Our sample includes all baseline observations in high opioid states. In Panel A, the dependent variables are the logarithm of establishment IT investment. Specifically, we use IT budget in column (1), hardware budget in column (2), software budget in column (3), services budget in column (4), and count of PCs in column (5). In Panel B, the dependent variables are normalized by establishment revenue. Controls include all additional variables included in Table 4. All variables are measured as the change over a four-year window. Control variables are lagged by one year and historic opioid prescriptions is lagged by five years. Standard errors are clustered at county level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	IT budget	Hardware budget	Software budget	Services budget	PCs
	(1)	(2)	(3)	(4)	(5)
Panel A. Δ Ln(IT investment)					
ΔLn(Historic opioid prescriptions)	0.039**	0.041**	0.032*	0.037**	0.019*
	(0.017)	(0.017)	(0.017)	(0.017)	(0.011)
ΔCounty controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	253,134	253,134	253,134	253,134	257,148
R^2	0.534	0.585	0.573	0.533	0.509
Panel B. Δ Ln(IT investment/revenue)					
ΔLn(Historic opioid prescriptions)	0.048***	0.051***	0.044***	0.050***	0.028***
	(0.015)	(0.016)	(0.017)	(0.015)	(0.009)
ΔCounty controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	228,428	228,428	228,428	228,428	232,372
R^2	0.615	0.596	0.668	0.597	0.789

Table 8: Heteogeneity on Low-Skill Labor Shares

This table explores the heteogeneity on low-skill labor share. Our sample includes all baseline observations in high opioid states. In Panel A, the dependent variables are the logarithm of establishment IT investment. Specifically, we use IT budget in column (1), hardware budget in column (2), software budget in column (3), services budget in column (4), and count of PCs in column (5). Controls include all additional variables included in Table 4. All variables are measured as the change over a four-year window. Control variables are lagged by one year and historic opioid prescriptions are lagged by five years. Standard errors are clustered at county level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	IT budget	Hardware budget	Software budget	Services budget	PCs
	(1)	(2)	(3)	(4)	(5)
Panel A. Δ Ln(IT investment)					
ΔLn(Historic opioid prescriptions)	-0.009	-0.008	-0.011	-0.011	0.001
	(0.022)	(0.023)	(0.022)	(0.023)	(0.015)
ΔLn(Historic opioid prescriptions) ·Low-skill high	0.096***	0.100***	0.088***	0.097***	0.035**
	(0.031)	(0.034)	(0.030)	(0.031)	(0.016)
Low-skill high	-0.060***	-0.071***	-0.058***	-0.058***	0.014
	(0.020)	(0.021)	(0.019)	(0.020)	(0.009)
ΔCounty controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	249,794	249,794	249,794	249,794	253,759
R^2	0.536	0.586	0.576	0.534	0.504
Panel B. Δ Ln(IT investment/revenue)					
ΔLn(Historic opioid prescriptions)	0.025	0.026	0.022	0.026	0.032***
	(0.018)	(0.019)	(0.019)	(0.018)	(0.012)
ΔLn(Historic opioid prescriptions) ·Low-skill high	0.041*	0.046*	0.038*	0.040*	-0.011
	(0.022)	(0.025)	(0.023)	(0.022)	(0.013)
Low-skill high	-0.041***	-0.049***	-0.040***	-0.040***	0.038***
	(0.014)	(0.016)	(0.015)	(0.014)	(0.008)
ΔCounty controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	225,299	225,299	225,299	225,299	229,194
R^2	0.618	0.597	0.672	0.600	0.790

Table 9: Heteogeneity on Multi-Establishment Firms

This table explores the heteogeneity on multi-establishment firms. Our sample includes all baseline observations in high opioid states. In Panel A, the dependent variables are the logarithm of establishment IT investment. Specifically, we use IT budget in column (1), hardware budget in column (2), software budget in column (3), services budget in column (4), and count of PCs in column (5). Controls include all additional variables included in Table 4. All variables are measured as the change over a four-year window. Control variables are lagged by one year and historic opioid prescriptions are lagged by five years. Standard errors are clustered at county level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	IT budget	Hardware budget	Software budget	Services budget	PCs
	(1)	(2)	(3)	(4)	(5)
Panel A. Δ *Ln(IT investment)*					
ΔLn(Historic opioid prescriptions)	0.029	0.034*	0.021	0.031*	0.009
	(0.019)	(0.019)	(0.019)	(0.019)	(0.011)
ΔLn(Historic opioid prescriptions) ·Multi high	0.044*	0.039	0.048*	0.037	0.025
	(0.025)	(0.025)	(0.025)	(0.025)	(0.016)
Multi high	0.095***	0.097***	0.098***	0.099***	0.001
	(0.005)	(0.006)	(0.005)	(0.005)	(0.003)
ΔCounty controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	253,134	253,134	253,134	253,134	257,148
R^2	0.535	0.586	0.575	0.535	0.509
Panel B. Δ *Ln(IT investment/revenue)*					
ΔLn(Historic opioid prescriptions)	0.034**	0.038**	0.026	0.035**	0.015
	(0.017)	(0.019)	(0.018)	(0.017)	(0.009)
ΔLn(Historic opioid prescriptions) ·Multi high	0.052***	0.051**	0.063***	0.052***	0.032***
	(0.020)	(0.022)	(0.022)	(0.020)	(0.011)
Multi high	0.085***	0.084***	0.098***	0.086***	0.006***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.002)
ΔCounty controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	228,428	228,428	228,428	228,428	232,372
R^2	0.616	0.597	0.670	0.599	0.789

Table 10: Heteogeneity on On-the-Job Injury

This table explores the heteogeneity on industry injury rate. Our sample includes all baseline observations in high opioid states. In Panel A, the dependent variables are the logarithm of establishment IT investment. Specifically, we use IT budget in column (1), hardware budget in column (2), software budget in column (3), services budget in column (4), and count of PCs in column (5). Controls include all additional variables included in Table 4. All variables are measured as the change over a four-year window. Control variables are lagged by one year and historic opioid prescriptions are lagged by five years. Standard errors are clustered at county level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	IT budget	Hardware budget	Software budget	Services budget	PCs
	(1)	(2)	(3)	(4)	(5)
Panel A. Δ *Ln(IT investment)*					
Δ Ln(Historic opioid prescription rate)	0.021	0.023	0.015	0.019	0.011
	(0.019)	(0.019)	(0.019)	(0.019)	(0.012)
Δ Ln(Historic opioid prescription rate)*Injury high	0.074**	0.072*	0.069**	0.074**	0.036**
	(0.035)	(0.038)	(0.033)	(0.035)	(0.016)
Δ Controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	221,969	221,969	221,969	221,969	225,422
R^2	0.549	0.596	0.590	0.550	0.509
Panel B. Δ *Ln(IT investment/revenue)*					
Δ Ln(Historic opioid prescription rate)	0.039**	0.043**	0.036*	0.040**	0.028***
	(0.018)	(0.018)	(0.020)	(0.018)	(0.010)
Δ Ln(Historic opioid prescription rate)*Injury high	0.044*	0.041	0.038	0.046*	0.005
	(0.026)	(0.029)	(0.029)	(0.025)	(0.013)
Δ Controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Observations	199,173	199,173	199,173	199,173	202,563
R^2	0.648	0.622	0.697	0.633	0.809

Table 11: Opioid Prescriptions and IT Investment with Firm Fixed Effects

This table explores the relation between information technology and historic opioid prescriptions. Our sample includes all baseline observations in high opioid states. In Panel A, the dependent variables are the logarithm of establishment IT investment. Specifically, we use IT budget in column (1), hardware budget in column (2), software budget in column (3), services budget in column (4), and count of PCs in column (5). In Panel B, the dependent variables are normalized by establishment revenue. Controls include all additional variables included in Table 4. All variables are measured as the change over a four-year window. Control variables are lagged by one year and historic opioid prescriptions are lagged by five years. Standard errors are clustered at county level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	IT budget	Hardware budget	Software budget	Services budget	PCs
	(1)	(2)	(3)	(4)	(5)
Panel A. Δ *Ln(IT investment)*					
ΔLn(Historic opioid prescriptions)	0.077***	0.071***	0.074***	0.073***	0.034***
	(0.022)	(0.023)	(0.022)	(0.022)	(0.012)
ΔCounty controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Observations	189,541	189,541	189,541	189,541	193,307
R^2	0.585	0.641	0.603	0.585	0.557
Panel B. Δ *Ln(IT investment/revenue)*					
ΔLn(Historic opioid prescriptions)	0.089***	0.085***	0.092***	0.090***	0.041***
	(0.018)	(0.020)	(0.020)	(0.018)	(0.010)
ΔCounty controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Observations	170,914	170,914	170,914	170,914	174,584
R^2	0.654	0.649	0.688	0.643	0.829

Table 12: Opioid Prescriptions and Labor Skill Shares

This table explores the relation between labor skill share and historic opioid prescriptions. Our sample includes all counties in high opioid states. The dependent variables are the changes in labor shares from 2011 to 2015. Specifically, the dependent variable is high-skill share in column (1), mid-skill share in column (2), and low-skill share in column (3). Controls include all additional variables included in Table 4. All variables are measured as the change over a four-year window. Control variables are lagged by one year and historic opioid prescriptions are lagged by five years. Standard errors are clustered at county level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δ Labor share		
	High-skill	Mid-skill	Low-skill
	(1)	(2)	(3)
ΔLn(Historic opioid prescriptions)	0.290***	0.004	-0.290***
	(0.078)	(0.053)	(0.084)
ΔCounty controls	Yes	Yes	Yes
Observations	1,450	1,450	1,450
R^2	0.126	0.076	0.167

Table 13: Abnormal Return around State Legislation Passages

This table explores the firm abnormal returns around first opioid-related state law passages or regulation announcement. Our sample includes all U.S. firm listed in both Compustat and CRSP. Health care industries are excluded. The dependent variables are three-day cumulative abnormal returns CAR[-1,1]. CAR[-1,1] is measured by market model in column (1) and (2), Fama-French three factor model in column (3) and Fama-French four factor model in column (4). Firm financial controls are lagged by one year and include the logarithm of total asset, ROA, PPE/total asset, Tobin's Q, and the logarithm of age. State-level controls include all additional variables included in Table 4. Standard errors are clustered at state level. All variables are defined in the Appendix and winsorized at 1% level. *** 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	0.001	0.002	0.003*	0.003*
	(0.001)	(0.001)	(0.001)	(0.001)
Firm controls	No	Yes	Yes	Yes
State controls	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Observations	164,221	164,211	164,211	164,211
R^2	0.015	0.048	0.031	0.032
	Panel B. Low IT budget			
Law passage	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.002)	(0.001)	(0.001)
Law passage·IT budget low	0.004**	0.005**	0.006***	0.006**
	(0.002)	(0.002)	(0.002)	(0.002)
IT budget low	-0.001			
	(0.000)			
Firm controls	No	Yes	Yes	Yes
State controls	No	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes
Observations	122,190	122,189	122,189	122,189
R^2	0.018	0.044	0.023	0.023

Table 14: Abnormal Return around Federal Regulation Announcement

This table explores the firm abnormal returns on federal opioid-related regulation announcement at April 2, 2018. Our sample includes all U.S. firm listed in both Compustat and CRSP. Health care industries are excluded. The dependent variables are three-day cumulative abnormal returns CAR[-1,1]. CAR[-1,1] is measured by market model in column (1) and (2), Fama-French three factor model in column (3) and Fama-French four factor model in column (4). Firm financial controls are lagged by one year and include the logarithm of total asset, ROA, PPE/total asset, Tobin's Q, and the logarithm of age. State-level controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at 1% level. Standard errors are clustered at state level. Industries are defined by 4-digit NAICS codes. *** 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. No state regulation				
No prior state limits	0.004	0.005**	0.005**	0.005**
	(0.003)	(0.002)	(0.002)	(0.002)
Firm controls	No	Yes	Yes	Yes
State controls	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Observations	3,304	3,281	3,281	3,281
R^2	0.001	0.014	0.005	0.006
Panel B. Low IT budget				
No state limits	-0.001	-0.000	0.000	0.000
	(0.002)	(0.003)	(0.003)	(0.003)
No state limits·IT budget low	0.007**	0.006**	0.006**	0.006**
	(0.003)	(0.003)	(0.003)	(0.003)
IT budget low	-0.006**	0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)
Firm controls	No	Yes	Yes	Yes
State controls	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Observations	2,337	2,310	2,310	2,310
R^2	0.004	0.043	0.031	0.034

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 Legislation and Regulation

By October 2018, 29 opioid-related laws have been passed by 25 states in the United States. We list the years of passage and a brief description of these laws below.

Alaska (2017): Alaska passed a law that limits first-time opioid prescriptions for no more than a 7-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. This law has various effective dates.

Arizona (2018): Signed into a law, Arizona Senate Bill 1001 limits the first fill prescription of adults to five days and aligned state dosage levels with federal guidelines. This law invested $10 million to assist in improving access to treatment; expanded law enforcement's access to Naloxone, a drug used to reverse overdoses; enacted continuing medical education for opioid prescribers; and required e-prescribing, among other provisions. Most of its provisions went into effect on April 26, 2018.

Connecticut (2016): Connecticut House Bill 5053 has been signed into a law and went into effect on January 1, 2017. This legislation limits opioid prescriptions for new adult patients to 7 days and limits opioid prescriptions to minors to 7 days, with certain exceptions for prescribers' professional medical judgments.

Connecticut (2017): Signed into law and became effective since January 1, 2018, Connecticut House Bill 7052 limits opioid prescription for minors to 5 days and requiring electronic prescribing of controlled substances, .

Florida (2018): Florida's governor has signed House Bill 21 into law, with an effective date of July 1, 2018. This law limits initial opioid prescriptions to 3 days for acute pain, with exceptions for trauma, chronic pain, cancer, or terminal ill patients

Hawaii (2017): Signed into law, Hawaii Senate Bill 505 is intended to limit initial opioid and benzodiazepines prescriptions to 7 days, with exceptions for cancer, chronic pain, trauma, and palliative care patients. The first part of the legislation was effective on July 1, 2017; the second part went into effect on July 1, 2018.

Indiana (2017): Effective since July 1, 2017, pursuant to Indiana Senate Bill 226, a new law limits initial opioid prescriptions for adults to 7 days and limits opioid prescriptions for minors to 7 days, with exceptions for chronic pain, cancer, or palliative care patients.

Kentucky (2017): A new law, Kentucky House Bill 333, took effect on June 29, 2017, and limits opioid prescription to 7 days for new patients with exemptions for cancer patients, diagnosed chronic pain, and end-of-life care.

Louisiana (2017): Effective since August 1, 2017, Louisiana House Bill 192 limits initial opioid prescriptions to 7 days with exceptions for chronic pain, cancer, or palliative care patients.

Maine (2016): Signed into a law, Maine LD1646 is intended to limit opioid prescriptions to 7 days acute pain, 30 days for chronic pain, and opioid amount limit of a maximum of 100 MME per day. This law exempts cancer, hospice and palliative care patients, and patients in treatment for a substance abuse disorder. Maine LD1031 clarifies that chronic pain patients are exempt from the maximum limit of 100 MME per day. Signed into an emergency law, this law became effective on June 16, 2017.

Maryland (2017): Maryland House Bill 1432 has been signed into a law and become effective upon enactment. This legislation limits initial opioid prescriptions for adults to 7 days and limits opioid prescriptions for minors to 7 days, with exceptions for chronic pain, cancer, or palliative care patients.

Massachusetts (2016): Massachusetts Bill H.4056 limits initial opioid prescriptions for adults to 7 days and limits opioid prescriptions for minors to 7 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 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. This law includes a provision that will make most of it effective immediately.

Michigan (2017): Effective since March 27, 2018, Michigan Senate Bill 0274 was signed into a law and limits opioid prescription to 7 days for acute pain patients, with exceptions for chronic pain patients.

Minnesota (2017): A new law in Minnesota limits opioid prescription to 4 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): Nebraska Legislature Bill 931 went into effect on July 19, 2018. This law limits opioid prescriptions to 7 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): A new law, Assembly Bill 474, went into effect on January 1, 2018. This law limits opioid prescription to 90 morphine milligram equivalent (MME) per day and limits initial opioid prescription to 14 days for acute pain. This law requires additional evaluation if patient requires more than 30 days of opioids.

New Hampshire (2016): New Hampshire House Bill 1423 has been signed into a law. It provision prevents medical professionals in an emergency room, urgent care setting, or walk-in clinic from prescribing more than 7 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. The first part of it provision went into effect on June 7, 2016.

New Jersey (2017): Signed into a law, New Jersey Senate Bill 3 limits initial opioid prescriptions to 5 days for acute pain patients. Cancer, hospice care, and long-term care facility patients are exempt. This law does not apply to medications prescribes for treatment of substance abuse.

New York (2016): New York Senate Bill 8139 is intended to limit initial opioid prescriptions to 7 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 emerge treated and increase addiction treatment slots.

North Carolina (2017): Effective since July 1, 2017, North Carolina House Bill 243 is signed in to a law. This law limits initial opioid prescriptions to 5 days for acute pain patients and 7 days for post-operative patients; allows for exemptions for cancer patients, chronic pain, hospice and palliative care, or medications prescribed for the treatment of substance use disorders; increases access to naloxone, requires prescribes and pharmacies to check to prescription data base before prescribing opioids to patients, and strengthens oversight of opioid prescriptions.

Oklahoma (2018): Signed into law and became effective since November 1, 2018, Oklahoma Senate Bill 1446 limits initial opioid prescription to 7 days for new patients with exemptions for cancer, hospice and palliative care patients.

Pennsylvania (2016): Pennsylvania Senate Bill 1367 is signed into a law and intended to limit emergency departments and urgent care centers from prescribing more than a 7 day supply of opioids and from writing refills for opioid prescriptions. Signed into a law, Pennsylvania House Bill 1699 limits opioid prescription to 7 days for minors with acute pain. The legislation provides medical professional 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 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): Signed into a law and went effective since May 9, 2017, Utah House Bill 50 limits initial opioid prescription to 7 days for new acute pain patients with exemptions for cancer, hospice and palliative care patients.

Washington (2017): Signed into a law and went effective since July 23, 2017, Washington House Bill 1427 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): West Virginia Senate Bill 273 has been signed into law and took effect on June 7, 2018. This legislation limits initial opioid prescription to 7 days for acute pain, 4 days for emergency room prescriptions, and 3 day if prescribed by 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): On October 24, 2016, Arizona's governor announced an executive order, which institutes a 7 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): On July 10, 2017, the Department of Health Care Policy and Financing announced a policy that limits initial opioid prescriptions to 7 days with two additional 7-day refills for Medicare patients. This policy requires a consultation with a pain management physician.

Delaware (2017): On February 1, 2017, Delaware state agency unveiled a policy which limits the first fill prescription of opioids to 7 days for adults and limits opioid prescription to minors to 7 days, with certain exceptions for prescribers' professional medical judgments, acute or chronic pain conditions. 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 on March 27, 2017 to limit initial opioid prescriptions to 7 days for Medicare patients.

Nebraska (2016): On June 21, 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): On March 30, 2017, the Ohio governor unveiled a policy that limit opioid prescriptions to 7 days and 30 morphine equivalent dose (MMD) per day for acute pain patients. This policy limits opioid prescription to 5 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): On April 20, 2017, the Vermont Department of Health announced a new policy which limits amounts of opioids able to 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): On February 16, 2017, the Virginia Board of Medicine adopted regulations entitled "Governing Opioid Prescribing for Pain and Prescribing of Buprenorphine". The regulations were adopted under the Board's emergency authority and limit opioid prescription to 7 days for acute pain and for 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 State Legislation and Regulation

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 state legislation and regulation indicator, which equals one if a state announces new opioid-related laws or regulation between 2016 and 2018. Historic opioid prescriptions are cumulative opioid prescriptions between 2006 and 2015. Democratic state is an indicator equal to one if Democratic Party control the legislation and the government. Republican state is an indicator equal to one if Republican Party control the legislation and the government. Other independent variables are as of 2015. Standard errors are clustered at state level. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Δ State Legislation and Regulation Indicator			
	(1)	(2)	(3)	(4)
ΔLn(Historic opioid prescriptions)	-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: Determinants of Opioid Prescriptions

This table explores the relation between opioid prescriptions and local economic, demographic and health characteristics. Our sample is estimated between 2011 and 2015, using all U.S. counties. The dependent variables are the logarithm of opioid prescriptions. Independent variables are measured with a one-year lag. Standard errors are clustered at county level. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	Ln(Opioid prescriptions)	
	(1)	(2)
Unemployment rate	0.031***	0.001
	(0.004)	(0.004)
Ln(Median household income)	-0.887***	-0.111
	(0.111)	(0.070)
Poverty ratio	-0.006	-0.001
	(0.004)	(0.002)
Manufacturing rate	-0.002	0.001
	(0.001)	(0.002)
Ln(Population)	0.384***	-0.381
	(0.045)	(0.764)
Ln(Population) squared	-0.039***	-0.013
	(0.005)	(0.078)
Male ratio	-0.041***	0.006
	(0.007)	(0.017)
White ratio	0.012***	0.034
	(0.003)	(0.023)
Black ratio	0.008**	0.031
	(0.003)	(0.026)
American Indian ratio	-0.001	0.038
	(0.004)	(0.039)
Hispanic ratio	-0.004***	-0.030*
	(0.001)	(0.018)
Age 20-64 ratio	0.000	0.034**
	(0.005)	(0.014)
Age over 65 ratio	-0.018***	0.031**
	(0.004)	(0.014)
Migration inflow	0.014***	-0.002
	(0.004)	(0.004)
Migration outflow	0.005	0.003
	(0.005)	(0.004)
Physicians	0.004***	-0.000
	(0.000)	(0.000)
Neoplasms mortality	0.001***	-0.000
	(0.000)	(0.000)
County FE	No	Yes
Year FE	Yes	Yes
Observations	13,548	13,524
R^2	0.270	0.885

Table IA3: Opioid Prescriptions and IT Investment with Different Controls

This table explores the relation between information technology and historic opioid prescriptions. Our sample includes all baseline observations in high opioid states. In Panel A, the dependent variables are the logarithm of establishment IT budget. In Panel B, the dependent variables are normalized by establishment revenue. Controls in column (2) includes all demographic variables included in Table 4. Controls in column (3) include both demographic and economic variables. All variables are measured as the change over a four-year window. Control variables are lagged by one year and historic opioid prescriptions is lagged by five years. Standard errors are clustered at county level. Industries are defined by 4-digit NAICS codes. All variables are defined in the Appendix and winsorized at 1% level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	(1)	(2)	(3)
	Panel A. Δ *Ln(IT budget)*		
ΔLn(Historic opioid prescriptions)	0.029*	0.043**	0.039**
	(0.017)	(0.018)	(0.017)
ΔCounty demographic controls	No	Yes	Yes
ΔCounty economic controls	No	No	Yes
Industry FE	Yes	Yes	Yes
Observations	253,134	253,134	253,134
R^2	0.533	0.534	0.533
	Panel B. Δ *Ln(IT budget/revenue)*		
ΔLn(Historic opioid prescriptions)	0.044***	0.053***	0.048***
	(0.017)	(0.016)	(0.015)
ΔCounty demographic controls	No	Yes	Yes
ΔCounty economic controls	No	No	Yes
Industry FE	Yes	Yes	Yes
Observations	228,428	228,428	228,428
R^2	0.614	0.614	0.615

Table IA4: Abnormal Return around State Legislation Passage

This table explores the firm abnormal returns around first opioid-related state law passages or regulation announcement. Our sample includes all U.S. firm listed in both Compustat and CRSP. Health care industries are excluded. The dependent variables are three-day cumulative abnormal returns CAR[-1,1]. CAR[-1,1] is measured by market model in column (1) and (2), Fama-French three factor model in column (3) and Fama-French four factor model in column (4). Firm financial controls are lagged by one year and include the logarithm of total asset, ROA, PPE/total asset, Tobin's Q, and the logarithm of age. State-level controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at 1% level. Standard errors are clustered at state level. *** indicates $p < 0.01$, ** indicates $p < 0.05$, and * indicates $p < 0.1$.

	CAR[-1,1]			
	MM		F3	F4
	(1)	(2)	(3)	(4)
Law passage	-0.001	-0.000	-0.001	-0.000
	(0.001)	(0.002)	(0.001)	(0.001)
Law passage·PCs low	0.003*	0.003**	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
PCs low	-0.000			
	(0.000)			
Firm controls	No	Yes	Yes	Yes
State controls	No	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Event FE	Yes	Yes	Yes	Yes
Observations	122,190	122,189	122,189	122,189
R^2	0.018	0.044	0.023	0.023

Table IA5: Abnormal Return around State Legislation Governor Signs

This table explores the firm abnormal returns on the first state opioid-related laws governor signs. Our sample includes all U.S. firm listed in both Compustat and CRSP. Health care industries are excluded. The dependent variables are three-day cumulative abnormal returns CAR[-1,1]. CAR[-1,1] is measured by market model in column (1) and (2), Fama-French three factor model in column (3) and Fama-French four factor model in column (4). Firm financial controls are lagged by one year and include the logarithm of total asset, ROA, PPE/total asset, Tobin's Q, and the logarithm of age. State-level controls include all additional variables included in Table 4. All variables are defined in the Appendix and winsorized at 1% level. Standard errors are clustered at state level. *** 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 sign			
Law sign	-0.000	0.000	-0.000	-0.000
	(0.001)	(0.002)	(0.002)	(0.001)
Firm controls	No	Yes	Yes	Yes
State controls	No	Yes	Yes	Yes
Industry FE	No	Yes	Yes	Yes
Observations	80,667	80,587	80,587	80,587
R^2	0.015	0.047	0.035	0.035
	Panel B. Low IT budget			
Law sign	0.000	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Law sign·IT budget low	-0.002	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
IT budget low	-0.001***			
	(0.000)			
State controls	No	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Event FE	No	Yes	Yes	Yes
Observations	59,794	59,755	59,755	59,755
R^2	0.017	0.055	0.040	0.039