The Opioid Crisis and Firm Skill Demand: Evidence from Job Posting Data*

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Abstract

While growing evidence suggests that the opioid crisis has reduced employment levels, little is known about how the crisis has affected job skill requirements—tools that employers use to screen job candidates. Using data on the near universe of US job vacancies, this paper studies the impact of the opioid crisis on employers' job skill requirements. Specifically, we investigate the effect of the reformulation of OxyContin, which represents one of the most substantial reductions in the availability of abusable prescription opioids. Prior studies have documented that the reformulation resulted in a large transition from prescription opioids to more dangerous illicit opioids. Using a difference-in-differences event study design that exploits firm-level variation in exposure to reformulation, we show that this transition toward illicit opioids has reduced employment at the firm level. Furthermore, we find that firms have increased requirements for cognitive and computer skills in response to this crisis. Finally, we find that the reformulation has resulted in reductions in local store sales, firm revenue, and firm capital stock, highlighting how the opioid crisis may impact firms' hiring decisions by affecting various aspects of the firm's constraints and considerations. Our findings emphasize the distributional consequences of this crisis: less-skilled workers may experience a disproportionate impact from the increased skill requirements, even among workers without a history of opioid use disorders.

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1 Introduction

The United States is experiencing the worst opioid epidemic in its history. From 1999 to 2021, nearly 645,000 Americans died of an opioid overdose, and the number of fatal opioid overdoses increased more than tenfold during this period (CDC, 2022). In 2019 alone, nearly 50,000 died from opioid overdose, surpassing the numbers resulting from motor vehicle accidents or breast cancer (National Center for Health Statistics, 2019). Beyond the widely studied health and mortality consequences, recent research has investigated far-reaching impacts of the opioid crisis on various outcomes, including crime (Deiana and Giua, 2021; Mallatt, 2022), child maltreatment (Evans et al., 2022), infant health (Ziedan and Kaestner, 2020), housing market (Custodio et al., 2023), and consumer credit (Jansen, 2023). Of particular interest to researchers and policymakers have been its implications for the labor market, given its potential to have substantial impacts on both workers and employers. A recent survey highlighted the widespread influence of opioid use in the workplace, with 75% of employers reporting that their workplace has been directly affected by employee use of opioids (National Safety Council, 2017).[1]

A growing literature has investigated the causal impact of the opioid crisis on the labor market, where most studies focus on the equilibrium employment effects and labor supply consequences (Krueger, 2017; Currie et al., 2019; Savych et al., 2019; Harris et al., 2020; Cho et al., 2021; Park and Powell, 2021; Aliprantis et al., 2023; Beheshti, 2023). However, there remains an important gap in understanding how this crisis impacts employers in their recruitment and screening decisions. One crucial strategy that employers may use to adapt to the challenges posed by the opioid crisis is to adjust job skill requirements—tools that employers use to screen job candidates.

In this paper, we study the impact of the opioid crisis on job skill requirements for new hires using data on the near universe of US job postings. To estimate the causal impact, we focus on a policy intervention that inadvertently shifted users from prescription opioids towards riskier and unregulated illicit opioids, including heroin. In 2010, Purdue Pharma altered the formulation of OxyContin, a widely misused prescription medication, in an effort to increase its resistance to abuse. This represents one of the largest reductions in the availability of abusable prescription opioids. Prior studies document that this supply-side intervention induced individuals using prescription opioids to switch to illicit opioids with higher addictive potential, such as heroin (Alpert et al., 2018; Evans et al., 2019; Powell et al., 2019; Beheshti, 2019). The existing literature has demonstrated

[1] Of all the respondents, 38% have experienced absenteeism or impaired worker performance, and 31% have had an overdose, arrest, a near-miss or an injury because of employee opioid use.

that the reformulation of OxyContin was associated with the growth of heroin markets, increased rates of heroin-related crime, heroin-involved overdose deaths, and hepatitis B and C infections. (Buchmueller and Carey, 2018; Alpert et al., 2018; Evans et al., 2019; Powell et al., 2019; Beheshti, 2019). These findings highlight the profound and wide-ranging impacts of this transition on both individuals and society, extending beyond mortality. Building on this literature, we investigate how this transition towards illicit drugs affected job skill requirements.

This transition toward illicit opioids could affect job skill requirements through various channels. First, employers may adjust hiring standards as a strategy to screen out individuals they believe are at higher risks of illegal opioid use. Considering the uncertainties related to a job applicant's potential future drug use, certain firms may respond excessively by substantially altering their hiring criteria for screening purposes. Second, employers may adjust skill requirements in respond to changes in quality of job seekers. Increased illicit opioid use may reduce anticipated future productivity among job seekers as individuals become more susceptible to involvement in criminal activities and confront health issues, such as heroin overdose and blood-borne diseases. In response, firms may enhance skill requirements to mitigate the risk of productivity loss by filling in job positions with higher-skilled workers. Lastly, the increased illicit drug use has the potential to reduce workers' productivity, labor supply and local consumption of non-opioid products, potentially leading to reduced sales and revenue for firms. These changes might also impact hiring and screening processes. Moreover, employers might project observed productivity changes in their current workforce onto job applicants, influencing their expectations for future productivity of new hires, which in turn would affect screening strategies and skill requirements.

To measure how firms' skill requirements and their performance have changed following the transition towards illicit opioids, we construct unique firm-level data that follow each firm over a decade. These firm-level data are derived using data from two sources. First, we use online job posting data from Lightcast over the years 2007 to 2019.[2] The data provide detailed information on job positions and specific skill requirements, covering both qualitative and software skills. Second, we complement this dataset with firm-level data on revenue, employment, and capital stock (Property, Plant, and Equipment) obtained from the Compustat North America Database, which collects financial information from publicly traded U.S. companies. In addition to the firm-level data, we use 2007–2019 Nielsen Retail Scanner data to measure retail store sales in

[2]Note that 2008 and 2009 data are not available.

each county.

We use a difference-in-differences event study design that compares within-firm changes in outcomes among firms that were located in counties with higher initial rates of prescription opioid use to outcomes among firms located in low-exposure counties, following the approach suggested by Alpert et al. (2018). The idea is to investigate whether firms in counties with higher initial prescription opioid use—which are likely more affected by the shift towards illegal substances resulting from reformulation—experienced larger changes in their financial outcomes and job skill requirements.

Our findings are as follows. First, we show that the transition toward illicit opioids reduce employment at the firm-level, consistent with findings from prior studies looking at the aggregate-level employment. Second, we then show that the transition toward illicit opioids has large and long-lasting impacts on skill requirements. Our results indicate that a one standard deviation increase in firm-level exposure to the reformulation led to an 11 percent increase in the average number of cognitive skills, such as statistical analysis, mathematical capability, and industry knowledge, and an eight percent increase in the average number of computer skills required in each online job position posted by each firm. Third, we demonstrate that the opioid crisis adversely impacts firm performance. On average, a one standard deviation increase in exposure to the reformulation led to a 1.6 percent decrease in the average annual sales in each retail store and a 4.3 percent decrease in annual firm revenue. We also find a 5 percent decrease in capital stock following reformulation. Our results emphasize how the opioid crisis can impact firms' hiring and screening decisions by affecting various aspects of the firm's constraints and considerations.

An essential question regarding mechanisms is whether the upskilling effect is driven by *composition changes*, involving the substitution of demand for less-skilled workers with that for more-skilled workers, or by *screening*, indicating an increase in skill requirements within a given occupation group. Although it is difficult to disentangle these channels, we conduct a simple heterogeneity analysis to explore the relative importance of both channels. We categorize occupations into two groups—manual and routine jobs, representing less-skilled, and non-routine jobs, representing more-skilled positions. If the primary channel is composition change by substituting less-skilled individuals with more-skilled workers, we would expect to observe a higher job posting share for more-skilled workers within each firm in the post-reformulation period. In contrast, if screening is the primary channel, we would anticipate observing upskilling within the same occupation group in a given firm, indicating that employers are raising skill requirements for similar types of jobs following the reformulation. Our findings provide some evidence for both channels, ruling out the possibility that one of these channels solely drives the entire results.

We investigate heterogeneity in the impacts of reformulation based on firm size, minimum wage levels, and employment protection levels. We find no consistent evidence of heterogeneity in outcomes related to firm size. Notably, we observe that the upskilling effects are particularly pronounced for firms located in areas with higher minimum wage levels or stricter employment protection levels. This suggests that in locations where the costs associated with hiring or firing employees are higher, there is a greater likelihood of screening candidates or increased demand for more-skilled workers.

A crucial concern in our analysis is the potential correlation between opioid exposure and other factors associated with both skill requirements and baseline opioid use. Of particular concern are shocks that occurred in the pre-reformulation period, especially the Great Recession in 2008. We address this concern using three approaches. First, we directly control for the interaction of the recession shock and the full set of year dummies in all our regressions throughout the paper. Even controlling for the recession shock, we still uncover statistically significant evidence of upskilling. Second, when dividing firms into two groups with high- and low-exposure to reformulation, we observe no average difference in the size of the recession shock. Third, in our robustness analysis, we construct half-yearly data instead of yearly data, considering the first half year of 2010 as the reference period. The results show no evidence that an increase in exposure to reformulation is correlated with any pre-existing difference in trends in our outcomes, reassuring that our results are not driven by the Great Recession or other earlier shocks that are correlated with baseline opioid use.

Our paper contributes to three strands of literature. First, we add to the literature studying the causal impact of opioid use on the labor market. Prior work use geographic variation in opioid prescribing and document that higher local prescription rate is associated with lower labor market participation rates (Krueger, 2017; Harris et al., 2020; Aliprantis et al., 2023).[3] Other work exploit variation generated by OxyContin reformulation or policies aimed at reducing misuse of prescription opioids and document that an increase in opioid use is associated with lower

[3] In contrast, Currie et al. (2019) find a small and positive impact of opioid prescribing on women's employment with no effect on men's employment. They use the prescribing rates for adults aged 65 and older in a county as an instrument for the prescribing rates among younger adults.

employment and lower labor force participation rates.[4] Recent studies reveal that beyond affecting labor supply along the extensive margin, increased opioid use contributes to higher employee absenteeism (Armando et al., 2019) and reduced on-the-job productivity within the Military (Alpert et al., 2022). While previous work has primarily focused on estimating the impacts on the equilibrium employment effects and labor supply outcomes, our work provides new evidence on how employers' job skill requirements are affected by the transition toward illicit opioids. To the best of our knowledge, this is the first paper to examine the impact of the opioid crisis on hiring decisions using the US job posting data.

Second, our study contributes to the extensive literature examining the causes and consequences of the opioid crisis. This literature has studied the roles of opioid policies, physicians, manufacturers, insurers, and pharmacists in contributing to the opioid crisis, as thoroughly reviewed in Maclean et al. (2020). A small but growing literature has looked at the effect of the crisis on firms and their responses. Ouimet et al. (2020) find that increased opioid prescriptions is associated with reduced employment and firm value, and firms substitute relatively scarce labor with capital, particularly when they face fewer financial constraints.[5] Chen et al. (2021) document that the opioid epidemic adversely affects local firms' innovation. We focus on understanding firms' responses in the labor market. Our results suggest that firms are navigating the opioid crisis by adapting in another crucial dimension—adjusting skill requirements when hiring new employees.

Third, our paper also contributes to the literature examining the factors influencing firms' hiring decisions. Prior research largely focused on how changes in the quantity of labor supply influence hiring and screening, particularly in the context of Great Recession (e.g., Modestino et al., 2020). Our contribution extends to exploring this issue in the unique context of the opioid crisis, a factor that potentially deteriorates the quality of labor supply as measured by expected productivity. Our findings indicate that firms react to a decline in labor quality by raising their skill criteria for evaluating job applicants.

The remainder of the paper proceeds as follows. Section 2 provides backgrounds on OxyContin

[4] For instance, Park and Powell (2021) and Cho et al. (2021) show that the transition from prescription opioids to illicit opioids triggered by the OxyContin reformulation led to declines in employment, labor force participation rates, hours worked, and earnings. Beheshti (2023) documents that the rescheduling of hydrocodone, which reduced access to it, resulted in higher labor force participation rates.

[5] In contrast, our results rule out the possibility of an increase in capital stock, implying that firms are unlikely to substitute capital for labor in our context. This results are consistent with with our evidence of sales and revenue decreases following reformulation, implying that financial constraints may prevent firms from increasing their technology investments.

reformulation and its potential impacts on hiring decisions. Section 3 provides details on the data, and Section 4 outlines our empirical strategies. Section 5 provides main results, heterogeneity analyses, and robustness exercises. Section 6 discusses policy implications and concludes.

2 Background

2.1 The Three Waves of the Opioid Crisis

Opioids are a class of drugs that includes both natural and synthetic substances used to reduce pain. Opioids range from common prescription medications such as oxycodone and morphine to illicit substances such as heroin and fentanyl. While opioids are useful and important medications, their potential for recreational and harmful use is high because because they produce a sense of euphoria. Moreover, opioid are often used in combination with alcohol or other depressant medications like benzodiazepines or tranquilizers, which can greatly increase the risk of overdose.

The United States has been experiencing a devastating opioid crisis, unfolding across three waves of overdose deaths. As illustrated in the overdose trends in Figure 1, these three waves are linked to different types of opioids: prescription opioid pills (labeled as "Commonly prescribed opioids" in Figure 1), heroin, and synthetic opioids, excluding methadone (labeled as "Other synthetic opioids" in Figure 1). The first wave, beginning around the year 2000, involved deaths from prescription opioid pills, showing a steady increase through to 2016. The second wave was marked by a significant rise in heroin-related deaths since 2010. The turning point came in 2010 with the introduction of a reformulated, abuse-deterrent version of OxyContin by Purdue Pharma, which substantially decreased the misuse of this previously favored drug, pushing users towards alternatives like heroin. The second wave was transitioned in 2013 into the third wave since 2013, which has been driven by fatalities associated with illicitly-made fentanyl. The transition from prescription to illicit opioids, especially noticeable following the reformulation of OxyContin, has led to a dramatic spike in overdose deaths, highlighting the ongoing and escalating nature of the opioid epidemic in the United States. Powell and Pacula (2021) find evidence of disproportionate increases in fatal overdoses involving synthetic opioids such as illicit fentanyl and non-opioid substances like cocaine in regions more significantly affected by the reformulation, underscoring the lasting consequences of this change.

2.2 The Reformulation of OxyContin

OxyContin. OxyContin, introduced by Purdue Pharma in 1996. OxyContin is a brand-name version of the extended-release form of oxycodone that acts for 12 hours. Purdue Pharma aggressively marketed OxyContin targeting primary care providers for the treatment of non-cancer chronic pain, pushing for more lenient prescribing standards (Van Zee, 2009). This marketing strategy led to OxyContin being prescribed to a broader population. As a result, OxyContin's sales skyrocketed from $48 million in 1996 to nearly $1.1 billion by 2000 (Van Zee, 2009). The widespread availability of OxyContin was associated with a rise in its misuse, diversion, and addiction rates, making it one of the most abused drugs in the United States by 2004 (Cicero et al., 2005). Recent studies have indicated that its introduction and promotional targeting significantly accounts for the increases in the supply of prescription opioids and overdose incidents since 1996 (Alpert et al., 2022; Arteaga and Barone, 2022).

OxyContin Reformulation. In response to growing abuse rates, Purdue Pharma introduced a abuse-deterrent formulation of OxyContin tablets in April 2010. The abuse-deterrent formulation was designed to make the pill difficult to break, crush, or dissolve. Reformulated OxyContin became commercially available on August 2010, with the distribution of the original formulation ending within the same month. By December of 2010 and 2011, the reformulated OxyContin constituted 90% and 99% of all OxyContin prescriptions dispensed, respectively (Beachler et al., 2022).

However, this reformulation had unintended consequences, leading to a large shift from prescription opioids to illicit opioids. It also contributed to the expansion of the illicit drug market and introduced a new population to potent illicit opioids. A study by Cicero and Ellis (2015) surveyed 153 recreational OxyContin users, finding that 33% of them switched to other substances due to the reformulation, with 70% of this group moving to heroin. Consistent with these survey results, subsequent research suggests that while the reformulation reduced OxyContin abuse, it inadvertently led to an increase in fatal overdoses involving heroin and other illicit drugs and crimes related to illicit opioids (Alpert et al., 2018; Evans et al., 2019; Powell and Pacula, 2021; Mallatt, 2022).

2.3 Potential Impacts on Existing Users and Firm Skill Demand

Potential impacts on existing users. The reformulation of OxyContin reduced the availability and increased the cost of obtaining divertible oxycodone and other prescription opioids. Consequently, this intervention had two substantial impacts on people who were dependent on opioids. First, some individuals turned to heroin and other unregulated substances as alternatives, which resulted in more severe withdrawals, less reliable supply, and higher variability in dose concentrations compared to prescription opioids. Second, along with the shift to these more potent substances, workers transitioned from the market for regulated to unregulated substances. Prior studies have reported increases in overdoses, hepatitis B and C infections, and other adverse outcomes in regions with a history of heavy OxyContin use following its reformulation (Beheshti, 2019). Additionally, recent research by Mallatt (2022) and Powell and Pacula (2021) has indicated a rise in heroin-related arrests and the development of more robust heroin markets in areas where oxycodone use was prevalent, suggesting that some substitution from prescription opioids to unregulated substances occurred.

Potential impacts on firm skill demand. The shift toward illicit opioids could affect job skill requirements through various channels. First, employers may modify hiring criteria to avoid hiring candidates perceived to be at greater risk of illegal opioid use. Even when only a small percentage of job candidates are directly impacted by the increase in illicit opioid use, firms could considerably modify their screening processes because of uncertainties surrounding an applicant's potential future drug use.

Second, employers may adjust skill requirements in respond to shifts in the quality of job applicants. The rise in illicit opioid use could reduce the expected future productivity of job seekers, as they become more prone to engaging in criminal activities and facing health issues, including illicit drug overdoses and blood-borne diseases. To offset the potential decline in future productivity among job applicants, employers may raise skill requirements, seeking to hire more highly skilled workers.

Third, the reformulation may also lead to a reduction in labor supply, thereby narrowing the pool of job applicants. How firms adapt to this labor shortage is ex-ante ambiguous. Firms may choose to relax hiring standards to more quickly fill positions, or they may choose to raise skill requirements with the aim to increase overall productivity with a limited number of newly hired employees.

Lastly, the increased illicit drug use has the potential to reduce workers' productivity, labor supply and local consumption of non-opioid products, which may result in decreased firm sales and revenue. These shifts may also influence screening decisions. Tighter financial conditions could decrease the number of vacancies companies aim to fill. In addition, employers may extrapolate the changes in productivity among current workers onto job seekers when shaping their expectations for candidates' future productivity.

3 Data

This section describes the datasets used in our analysis. First, we describe county-level on prescription opioid use. Second, we describe our firm-level data sets: Compustat North America Database; and Lightcast Online Job Posting Data. Finally, we describe our data on local store sales.

3.1 County-level data

Prescription opioid use. Our data on county-level prescription opioid use are from the Centers for Disease Control (CDC). This dataset comprises an 85 percent sample of retail pharmacy providers while notably excluding hospitals. Within our dataset, a compelling pattern emerges: median per capita opioid prescriptions show a steady increase until they reach a peak in the year 2012, after which they undergo a noticeable decline. It's worth noting that other researchers, such as Alpert et al. (2018) and Evans et al. (2019) have explored national trends in OxyContin abuse using distinct measures. Their findings reveal a peak in 2010, a peak that coincides with the drug's reformulation, occurring approximately two years earlier than the peak observed in the median opioid prescriptions within our dataset.

Figure 2 displays geographic variation in pre-intervention per capital opioid prescriptions across counties. Panel A of Table 1 reports the population-weighted average number of Schedule II opioid prescriptions per capita in the pre-intervention period from 2006 to 2009. On average, each individual received 0.7 Schedule II opioid prescriptions during this period, with a standard deviation of 0.22.

3.2 Local Sales Data

To investigate how the opioid crisis affects firms' sales, To construct total store sales in each county, we rely on Nielsen Retail Scanner data by Nielsen Company (US) through Kilts Center for Marketing Data Center at Chicago Booth School of Business. It is a point-of-sales data from participating retail stores across the U.S.. The data starts from 2006 and there are around 30,000 participating stores each year in the data before 2017, and since 2017 Nielsen has expanded their number of participating stores to 50,000. Each data entry in the data is a barcode level information on a product's price, sales quatities at the weekly frequency. Each data entry has store identifier and product identifier so that the pricing and sales quantities information can be linked to the store information where the product is sold and product attributes files which has the information on the product's size, brand among many others. Mostly importantly, since it is a point-of-sales data and each unique product has its unique barcode, we can use this information to identify the manufacturer of the product (which is missing in the Nielsen Retail Scanner data) through the link provided by GS1 U.S..

A manufacturer can request for the UPC barcode of their products through the GS1 U.S. and then a unique UPC barcode code is assgined to each of the product. The UPC barcodes consists of 12 numbers and it is composed of company prefix and product number. The company prefix can range from 6 digits to 9 digits and this allows us to identify the manufacturer of the product through the GS1 data.

Once the manufacturer is identified in the Nielsen Retail data, we can aggregate the data to construct quarter-by-store-by-company level of total sales. Also, since the Nielsen Retail data contains the store level location information in the county level, we can then further aggregate the data to construct qurter-by-county-by-company level of total sales.

3.3 Firm-level Data

Our labor demand measures are from the online job posting data provided by Lightcast, an employment analytic firm. Lightcast collects job ads from about 40,000 online job boards and uses its own machine-learning algorithm to unify duplicate job ads and parse the postings into a systematic form. Lightcast argues that its database includes the near universe of online job ads and the database has widely been used in academia and industries. It provides detailed information on each job posting, including job title, standard occupation classification (SOC), employer name, location, and employer industry. More importantly, Lightcast collects detailed skill requirements of each job ad, such as education, work experience, and a list of skill requirements. There are more than 10,000 unique skill keywords in the skill requirements. The list of skills includes general skills (such as communication skills, teamwork, critical thinking, quality control, etc.), specific skills (such as foreign languages, legal compliance, computer numerical control, revenue projections, etc.), and specific software names (such as SAP, Python, Java, SQL, Tensor Flow, ND4J, etc.).

It is worth noting some limitations of online job posting data. Job vacancies posted online may not well represent the overall employment distributions. This problem is shared by the other sources of job vacancies, such as the Job Openings and Labor Turnover Survey (JOLTS), and some studies report that job vacancies are skewed toward certain areas of the economy (Davis et al., 2013; Lazear and Spletzer, 2012). However, Lightcast is known to be consistent with these survey-based job vacancies. Hershbein and Kahn (2018a) show that the national and industry trends of the number of online job postings from Lightcast closely track those of employment from the Current Population Survey (CPS) and Occupational Employment and Wage Statistics (OEWS), and job vacancies from JOLTS.

Skill Requirements in Job Posting Data. We restrict our attention to skill requirements among various information in the Lightcast database. Lightcast uses its algorithm to develop a robust skills taxonomy. We follow Deming and Kahn (2018) who create categories of skill requirements in Lightcast that could be useful for economic research.[6] Appendix Table A1 lists the ten skill categories and the corresponding keywords or phrases that belong to each category. For instance, a cognitive skill should include keywords or phrases such as "problem-solving," "research," and "statistics." These keywords are deliberately chosen by Deming and Kahn (2018) to match the “non-routine analytical" job tasks that are classified by Autor et al. (2003) based on the O*NET database. Deming and Kahn (2018) also classify “software skills" and “computer skills," separately. Software skills include names of specialized software, while computer skills are common software, such as Microsoft Excel and PowerPoint. We use the numbers of skill requirements in the categories as the measures of firms' skill demand.

Employment, revenue, and capital stock. We complement job posting data by constructing measures of revenue, employment, and capital stock (Property, Plant and Equipment), obtained

[6]Deming and Kahn (2018) also ensure that the skills in each category are prevalent in the job ads of relevant occupations.

from Compustat North America Database (Compustat hereafter). Compustat gathers financial statements from publicly traded firms in the U.S. and provides a standardized set of asset, revenue, capital expenditures, employment, tax reports, and supplementary data items. We exclude all funds, trusts, and other financial vehicles (NAICS 525) from our sample.

Linkage process. Unfortunately, there is no simple way to link Compustat and Lightcast because there is no common firm identifier between the two datasets. Moreover, many firm names in the job posting data contain abbreviations or misspellings. For instance, a firm name "Micron Technology" can be expressed as "Micron," "Micron Tech," "Micron Incorporation," or "Micron Technology, Inc." in the job posting data. We use a fuzzy matching algorithm and employ another proprietary database called the Computer Intelligence Technology Database (CiTDB) to overcome this problem. CiTDB covers 3.2 million establishments since 2010 and provides a firm structure and address of each establishment. Specifically, there is a unique firm ID, and one can identify a firm's headquarters, branches, and addresses. We first standardize company names using the algorithm provided by Wasi and Flaaen (2015). As an additional input for matching, we also construct a measure of industry linkage based on the input-output table. And then, we link Compustat and Lightcast using a fuzzy match based on the standardized names and industry linkage. Lastly, we compare the name and address of a matched establishment from Lightcast and that from CiTDB to ensure that the matched establishment is a branch of the matched firm from Compustat.

4 Empirical Strategy

To explore the causal impact of the OxyContin reformulation on labor demand responses, we employ difference-in-differences and event study designs that exploit pre-reformulation exposure to prescription opioids. This section explains how we construct the measure of exposure to the OxyContin reformulation and our empirical models.

Measuring exposure to the opioid crisis at the county level. To quantify the causal impact of the OxyContin reformulation on labor demand, we leverage geographic variations in pre-intervention exposure to prescription opioids, the approach that has been suggested by Alpert et al. (2018) and Evans et al. (2019), and widely adopted in the literature. Alpert et al. (2018) use state-level variation by constructing a pre-intervention exposure metric based on the population-weighted rate of OxyContin misuse at the state level from 2004 to 2009. This measure is only available at the state level, and to construct a pre-intervention exposure measure at the county level, we follow Evans et al. (2022). Specifically, we use the population-weighted mean number of all Schedule II opioid prescriptions per capita in each county for the years 2006 to 2009, obtained from CDC data. While Alpert et al.'s measure focuses exclusively on OxyContin misuse, our county-level measure of pre-intervention exposure covers both prescribed and misuse of all Schedule II prescription opioids, extending beyond just OxyContin. This broader measure offers a more precise representation of local variations in pre-intervention opioid exposure (Evans et al., 2022), even though it includes a wider range of prescription opioids than the specific target of the intervention, OxyContin. Evans et al. (2022) highlights the validity of this county-level exposure measure by showing that the reformulation had the most substantial impact on reducing OxyContin misuse and potentially creating adverse substitution patterns in areas where pre-reformulation opioid use was most prevalent the county-level exposure measure.

Measuring exposure to the opioid crisis at the firm level. To investigate the impact of the intervention on firm-level outcomes, we create a firm-level exposure measure based on our county-level exposure measure. One challenge with this approach is that a single firm often has multiple establishments dispersed across different counties. To address this, we construct a firm-level exposure that combines exposure across the various establishments owned by the same firm, akin to the Bartik measure. Specifically, we compute a weighted average of the county-level opioid exposure measure, with each county's weight based on the location of establishments operated by the firm. It is often challenging to obtain suitable weights for aggregating establishment-level outcomes into the firm level. To address this, we create weights based on pre-intervention period job posting data at the establishment level. One implicit assumption here is that the number of job postings represents the size of establishments, which is widely used in the literature.

A firm-level analysis leveraging a firm-level exposure measure offers advantages in several aspects for our research. First, we can better identify the mechanisms of labor demand responses to the opioid crisis through employers' behavior. Even if a firm runs multiple establishments across regions, the firm makes important decisions like hiring as a whole (Hazell et al., 2022). So, how a firm's sales, revenue, and other inputs respond to the opioid shock helps understand how the crisis influences labor demand. Second, we can separate within-firm changes in labor demand and composition changes across firms. Many studies try to separate out firm-level effects to distinguish among possible mechanisms in various contexts (Bloom et al., 2016; Hershbein and Kahn, 2018b). Even if the transition to illicit opioids lowers labor demand at some aggregate levels, it does not necessarily mean that firms intentionally reduce employment because the shock could also affect the composition of more or less labor-intensive firms. Third, it is less prone to unobserved local- or industry-specific heterogeneity, thereby implying fewer confounding effects related to the opioid shock. While the geographic variation in the opioid shock is concentrated in some local areas, which could also be affected by unobserved regional economic shocks, our firm-level shock mitigates this concern by smoothing the exposure to these areas.

Event study specification. We estimate the following event study specification:

$$y_{fgt} = \sum_{t=2005, t\neq 2007}^{2020} \delta_t Exposure\_Pre_f + \alpha_f + \gamma_{gt} + X'_{ft}\beta + \varepsilon_{fgt}, \tag{1}$$

where y_{fgt} denotes the outcome for firm f in industry g during year t. The industry classification is based on the 4-digit NAICS code. γ_{gt} denotes industry-by-year fixed effects. X_{ft} denotes time-varying county-level characteristics, including county-level recession shock interaction with year-specific dummy variables.[7] The coefficients of interest are δ's, presenting how the effect of the reformulation on our firm- or county-level outcomes change over time. Standard errors are clustered at the firm level.

Our key identification assumption is parallel trends assumption. That said, we assume that outcomes would have evolved similarly among firms with different baseline exposure to prescription opioids. Note that our firm-level longitudinal data allow us to include firm fixed effects and industry-by-year fixed effects in our empirical model, accounting for any firm-specific characteristics that remain constant over time or any industry-year-specific characteristics.

[7] Following Hershbein and Kahn (2018b), we use unemployment rates during the Great Recession (2007–2009) as a measure of the recession shock. We construct a weighted average of unemployment rates for each firm in a manner similar to how we aggregated the firm-level opioid shock. This control mitigates the concern that the recession influences the outcomes through the increased use of illicit opioids as a scarring effect.

Difference-in-differences specification. To summarize the average effect in the post-period, we estimate the following difference-in-differences specification:

$$y_{fgt} = \lambda Exposure\_Pre_f \times Post_t + \rho_f + \tau_t + X'_{ft}\sigma + u_{icgt}, \quad (2)$$

where $Post_t$ denotes the post-reformulation period covering years from 2010 to 2019. The coefficient of interest is λ, representing the effect of the reformulation on our firm- or county-level outcomes. Standard errors are clustered at the firm level.

5 Results

5.1 Effects on Firm Employment and Skill Requirements

We explore the impact of reformulation on various labor market outcomes—firm employment levels and requirements for education, work experience, and other skills that employer use to screen candidates for a job vacancy.

Effects on firms' employment. In Figure 4, we present our event study results for firm employment. Specifically, we present coefficients and associated 95 percent confidence intervals from estimation of equation (1). We do not observe any evidence of pre-existing trend in the pre-intervention period. In 2010, the year of reformulation, we observe that firm employment level begins to decrease. The effect size increases over time through 2013 and remain stable since then. Column 1 of Table 2 reports the average effect over the post-reformulation period from equation 2. In our analyses, we will report the impact of a one standard deviation increase in firm-level exposure to the reformulation, equal to an additional 0.22 per capita opioid prescriptions in the period prior to reformulation. One standard deviation increase in exposure to the reformulation was associated with a 5.7 percent reduction in employment at the firm level. This estimate is statistically significant at the 1 percent significance level.

Main Results: Effects on firms' skill requirements. In Figure A2, we present raw trends in average number of specific skill requirements required in each job posting, measured as the firm-level percentage increase relative to 2007, separately for low-exposure and high-exposure firms. High-exposure firms are defined as those with an exposure measure larger than the median in each industry group based on the NAICS two-digit code. Panel (a) shows trends for cognitive skills and Panel (b) for software skills, respectively. Observations are firm by year level and weighted by the number of job postings in that year. The figure indicates that trends in the average number of cognitive and skill requirements within firms were similar for both high- and low-exposure firms until 2010, but began to significantly diverge from 2011 onwards. The difference in percentage change continued to grow until 2012, after which the gap stabilized starting in 2013. This observed trend offers evidence that exposure to the reformulation is closely linked to changes in skill requirement patterns.

These patterns persist even after making regression adjustments. In Figure 5, we present our event study results from estimation of equation (1) firms' skill requirements. The figures plot the estimated impact of the reformulation of OxyContin on skill requirements for each year, relative to 2007, as well as 95 percent confidence intervals. The outcome variable is the log of the number of specific skills required in a job posting for each of the following skill categories: cognitive skills (Panel (a)) and computer skills (Panel (b)).

Panel (a) presents evidence that cognitive skill requirements increased following the reformulation. Starting in 2011, the first fully treated year, we observe a gradual increase in cognitive skills in each job posting in higher-exposed firms. The estimated event study coefficients generally increase through 2014, and they remain relatively stable through 2019. The rise in the estimated effect size over time aligns with the idea that the reformulation of OxyContin contributed to the expansion of illicit heroin markets in areas with higher pre-reformulation rate of prescription opioids, as discussed in Park and Powell (2021) and Powell and Pacula (2021).[8]

Panel (b) presents the event study results for computer skills. This panel shows a generally similar pattern in magnitudes, although certain coefficients are statistically indistinguishable from zero. It is worth highlighting the positive and statistically significant impact of reformulation on computer skills within several sub-groups, which will be discussed in Section 5.4.

Columns 2 and 3 of Table 2 provide summary estimates for these outcomes. We observe that a one standard deviation increase in exposure to reformulation resulted in an 11 percent rise in the average number of cognitive skills and an 8 percent increase in the average number of computer skills required for each online job position posted by firms.

The timing of this decreasing trend coincides with the period of reformulation, suggesting that

[8]The increasing pattern of the estimated effect size has also been observed in prior studies examining the reformulation's impact on various outcomes.(Alpert et al., 2018; Park and Powell, 2021; Evans et al., 2022).

the effects are unlikely to be driven by confounding factors associated with the Great Recession. As mentioned in Section 4, our econometric models explicitly account for the impact of Great Recession by including Recession shock interacted with the full set of year dummies.

Effects on education and experience requirements. Figure A4 reports our event study results for education requirements (Panel (a)) and experience requirements (Panel (b)). We find no evidence that reformulation led to increases in the years of schooling or years of experience required in a job posting, although the point estimates in both regressions are positive. Columns 4 and 5 of Table 2 show that one standard deviation increase in exposure to reformulation was associated with a 2.8 percent increase in years of schooling (column 3) and 1 percent increase in experience years, although these outcomes are statistically insignificant at the 10 percent significance level.

5.2 Effects on Local Store Sales, Revenue, and Capital Stock

Effect on local store sales. We first explore the effect of OxyContin reformulation on local store sales. Figure 6 presents the coefficients and 95% confidence intervals on the on the interactions between standardized pre-intervention opioid prescription rate and the indicators for each of the years before and after the 2010 OxyContin reformulation from equation (1). The year 2009, which is one year prior to the reformulation, is normalized to zero. In Panel (a) (Panel (b)), the outcome variable is the log of the average (total) store sales in a county in that year.

In both panels, we do not observe any pre-existing trend in local store sales prior to the reformulation, with the estimated coefficients in the pre-reformulation period are statistically indistinguishable from zero. Starting in 2011, the first fully treated year, we observe a gradual increase in local store sales in higher-exposed counties. The estimated event study coefficients generally increase through 2017, and they remain relatively stable through 2019. All the estimates for the period 2012 to 2019 are statistically significant at the 5 percent significance level.

Columns 1 and 2 in Table 3 report the average effect over the post-reformulation period from equation 2. Over the entire post-period spanning from 2010 to 2019, one standard deviation increase in exposure to reformulation was associated with a 1.6 percent decrease in the average store sales in a county, as shown in column 1. There is a larger impact on the total store sales in a county, with a 2.8 percent decrease throughout the entire post-period. In both columns, coefficients are statistically significant at the 5 percent level.

Effect on revenue and capital stock. We then investigate how reformulation affected firm revenue and capital stock. Figure 7 presents our event study estimates for the log of firm capital stock (i.e., property, plant, and equipment) in Panel (a) and the log of firm revenue in Panel (b). There is little evidence of pre-existing trend in these outcomes, as all of the coefficients in the pre-reformulation period are statistically indistinguishable from zero at the 5 percent significance level. Starting in 2011, revenue begins to decline. The size of the effect increases over time through 2013 and then remain stable through 2019. This is consistent with our findings of of decreasing sales following reformulation. We observe a similar pattern for capital stock in the post period.

We find very similar results pattern for capital stock in Panel (b). There is some evidence of a decreasing trend in capital stock during the last three pre-reformulation periods. If this downward trend would have persisted, our capital stock estimates would imply no impact on capital stock. Therefore, our capital stock results are suggestive, and we interpret it as strong evidence against any substantial increase in capital stock resulting from the reformulation.

We summarize these effects in columns 3 and 4 in Table 3. One standard increase in exposure to reformulation led to a 4.3 percent decrease in firm revenue (column 3) and 5.1 percent decrease in capital stock. In contrast to findings from Ouimet et al. (2020), who find that firms increase technology investment to replace relatively scarce labor with capital, we do not find any evidence on an increase in capital stock. This aligns with our findings of declines in sales and revenue following reformulation, suggesting that financial constraints could inhibit firms from enhancing their technology investment.

5.3 Compositional Changes

In Section 5.1, we show that the transition toward illicit opioids led to an increase in skill requirements. A central question is whether this upskilling effect is attributed to changes in occupational composition, characterized by a shift from demand for lower-skilled to higher-skilled labor, or to screening, reflecting increased skill requirements within specific occupational categories. If compositional shifts, substituting lower-skilled with higher-skilled labor, predominate, we would expect to see an increase in demand for higher-skilled labor in the subsequent period. Alternatively, if screening channel predominates, an upskilling trend within the same occupational categories would be observed, denoting that employers are enhancing skill requirements for similar roles in the post-reformulation period.

In Figure 8, we address this question by conducting a heterogeneity analysis. First, we divide occupations into two segments—manual and routine positions, and non-routine positions. Second, we estimate equation 2 for each of these groups separately. The outcome variables include the share of job postings allocated to each occupation group (left panel), the average number of cognitive skills required in each job posting (middle panel), and the number of computer skills required in each job posting (right panel), with each outcome calculated at the firm-by-year level. We find no evidence that a single mechanism solely accounts for the observed upskilling effect.

5.4 Heterogeneity Analysis

We explore heterogeneous impacts of reformulation on firm outcomes, employment, and skill requirements by firm size, employment protection levels, and minimum wage levels.

Heterogeneity by firm size. To explore heterogeneity in effects by firm size, we estimate equation (2) separately for small-, mid-, and large-sized firms.[9] Figure 9 and Appendix Figure A7 present coefficients and associated 95% confidence intervals for our employment and skill requirements in Figure 9 and firm financial outcomes in Appendix Figure A7, respectively. Overall, the effects on small-sized firms are less pronounced for most of our outcomes, while we find no consistent evidence of differential impacts across middle- and large-sized firms.

Heterogeneity by employment protection levels. Second, we explore heterogeneity in effects on employment and skill requirements across firms in areas with low, medium, and high employment protection levels. For doing this, we use a state-level employment protection score constructed by Oxfarm, a non-governmental organization.[10] The index is based on state policies related to protections around paid sick leave, advance notice, flexible scheduling, sexual harassment, equal pay, etc. We construct a firm-level exposure to employment protection by taking a weighted average of state-level scores with the geographic variation of a firm's establishments. Then, we classify firms into three groups based on this firm-level score.

[9]We classify firms into three groups within each sector (utility, manufacturing, service, IT, finance, and professional service) based on the average total asset for three years before the reformulation.

[10]The data are available at: `https://www.oxfamamerica.org/explore/countries/united-states/poverty-in-the-us/best-states-to-work/`.

Figure 10 presents coefficients and 95% confidence intervals from estimation of equation (2) for each group. We observe that the estimates for both employment and skill requisites are notably higher for firms in jurisdictions with stringent regulatory frameworks. We find that the estimated effect for employment and skill requirements are particularly large in magnitude among firms in high regulation areas. This finding is consistent with the idea that in areas where the costs related to recruiting or terminating employees are higher, firms may be more inclined to undertake rigorous screening of applicants or exhibit a stronger preference for individuals with higher skill levels.

Similarly, we analyze heterogeneity in effects across firms in areas with low, medium, and high minimum wage levels in Appendix Figure A8. We use state minimum wages provided by the U.S. Department of Labor. Using the minimum wages averaged over the three years before the reformulation, we construct the firm-level average minimum wage based on the geographic variation of a firm's establishments as the other measures. We observe similar results to those found in the analysis of heterogeneity based on employment protection levels. The largest effect is observed among firms highly exposed to the states with high minimum wage levels.

5.5 Comparison to the literature

Our estimates indicate that the impact of the opioid crisis is substantial in magnitude. Specifically, a one standard deviation increase in reformulation exposure leads to a substantial 1.6% reduction in local store sales and a 5.7% decline in firm employment at the county level during the first ten years following the reformulation. Comparing our findings to recent research on the impact of opioid use on labor market and firm outcomes is beneficial for a understanding of the size of our results.

Park and Powell (2021) find that a one standard deviation increase in reformulation exposure resulted in a 1.2 percent reduction in per capita employment after five years following reformulation.[11] Our estimated 5.7 percent decrease in firm employment is larger than their estimate. This is consistent with the idea that our firm-level results do not take into account the reallocation of the lowered employment towards self-employed and the other small firms, which are not included in our sample.

Furthermore, Ouimet et al. (2020) find that an increase of 0.3 opioid prescriptions per person

[11] A one standard deviation difference in their state-level exposure to reformulation, measured by pre-reformulation rate of OxyContin misuse, is 0.23. This is comparable in magnitude to a one standard deviation difference in our firm-level exposure to reformulation, calculated by pre-reformulation per-capita opioid prescriptions in the counties where a firm is located, which is equal to 0.22.

(moving from the 25th to the 75th percentile) leads to a 1.7 percent reduction in sales at the establishment level. We find that one standard deviation increase in firm-level exposure to reformulation resulted in an estimated 1.6 percent reduction in local store sales, which is align closely in magnitude their findings.

5.6 Robustness Analysis

We test the robustness of our results to alternative policy exposure measures, different sample constructions, and alternative explanations.

Half-year level sample. A key concern in our study is whether opioid exposure might be intertwined with other variables that affect both skill demands and initial opioid usage. Lastly, our thoroughness checks involve a more granular, semi-annual dataset, treating the first half of 2010 as a baseline. Although the effects estimated are slightly smaller in size, they remain statistically significant across most years following the intervention. More importantly, we observe the impacts on skill requirements emerging only in the latter half of 2010, coinciding with the reformulation period. This timing provides assurance that our results are not confounded by other variables that may have affected these outcomes before the reformulation of OxyContin.

Confounding effect of Great Recession. The economic downturn of 2008, known as the Great Recession, is one of the key potential confounding factors due to its timing, closely preceding the opioid reformulation, as well as its profound economic consequences. We address this concern in three ways. First, as described in Section 4, we directly control for a term in our regression models that interacts the recession's impact with annual indicators. Second, in categorizing firms by their level of exposure to the reformulation, we find no significant disparity in how the recession affected both high- and low-exposure groups. Lastly, as demonstrated above, we observe no effect of reformulation even a half-year before implementation in our half-year level analysis, implying that the Great Recession is not likely the main driver of our results.

Firm-by-state level sample. One might also argue that our firm-level shock captures other concurrent firm-level confounding factors. To ensure this is not the case, we conduct a firm-state-level analysis after controlling for firm-by-year fixed effects. The idea is that the establishments of the same firm should show consistent effects of the reformulation of OxyContin based on the pre-intervention exposure in the locations of the establishments after controlling for any firm-level time-varying shocks. To reduce noise, we aggregate job postings at the firm-state-year level rather than using firm-county-year observations following Giroud and Mueller (2019). We construct the parallel firm-state-level measure of opioid exposure based on the job posting shares of counties within the state and conduct the firm-state-level version of Equation (1) with firm-by-year fixed effects controlled. Appendix Figure A9 presents the event-study results from this analysis. The point estimates are slightly smaller in magnitude for firm-by-state level samples compared to our baseline results. However, the figures show the persistent and statistically significant effects of the reformulation of OxyContin on skill requirements at this granular level after any potential firm-level concurrent shocks are ruled out.

Alternative policy exposure measure. For the robustness analysis using the firm-by-state sample, we use an alternative measure of exposure—state-level misuse of OxyContin from (Alpert et al., 2018). Alpert et al.'s measure captures misuse of OxyContin in the state during the pre-reformulation period, and may provide a more accurate state-level exposure measure. As mentioned above, our skill requirements results are similar with this alternative measure in this analysis (Appendix Figure A9).

Dropping one industry category. We also test the robustness of our skill and employment results to dropping each of industry category: (1) mining, (2) utilities, (3) wholesale trade, (4) health care/social assistance, (5) accomodation/food services, (6) agriculture/forestry/fishing, (7) manufacturing, (8) real estate, and (9) educational services. In Appendix Figures A10 and A11, we present presents coefficients and 95% confidence intervals from estimation of equation (2) where we drop each industry group. The dependent variables are skill requirements and employment in Appendix Figures A11, and firm financial outcomes in Appendix Figure A10, respectively. One notable finding is that the effects on revenue and capital stock become smaller in magnitude and statistically less significant when dropping manufacturing. This may reflect that manufacturing firms were the most affected by increased illicit opioid use among employees as a result of reformulation. Overall, our results are robust to dropping one category for all of our main outcomes.

Controlling for labor supply measures. Another concern is that our results may solely reflect the change in the local labor pool in response to the Opioid Crisis. For instance, the supply of low-skilled or young workers, who may be more affected by the prevalence of opioid use, could decline more, and firms could subsequently reduce the number of job ads for low-skilled positions, raising the average skill levels per job posting.

While there is no way to test this directly, we conduct an exercise where we explicitly control for labor supply measures for worker sub-groups, which reflects the labor supply response to the Opioid Crisis. The idea is that if our results are entirely driven by factors that predict labor supply responses, our estimates would be sensitive if we control for labor supply measures. We employ the state-year-level labor force participation rates and average wages by gender, education level, and race from the National Historical Geographic Information System (NHGIS) and construct the corresponding firm-level labor supply measures based on the geographic variation of a firm's establishments across states as the other measures.

In Appendix Table A3 (Appendix Table A4), we report the sensitivity of our results when controlling for labor force participation rates (wage levels) for the subgroup of workers. In both tables, we reproduce our baseline estimates from the estimation of equation (2) in column 1. In column 2, we add female and male workers' labor force participation rates (wage levels). In column 3, we add measures of education sub-groups—college graduates and non-college graduates. Finally, column 4 adds measures for race sub-groups—non-Hispanic White, non-Hispanic Black, and Hispanic. The tables show that the employment impact of the reformulation of OxyContin is robust to the inclusion of the measures reflecting concurrent labor supply changes. Though the impacts on skill requirements are statistically insignificant after controlling for some of the measures, the point estimates are economically significant and stable, indicating that our results are not driven by the impacts of the Opioid Crisis on local labor supply.

6 Conclusion and Policy Implications

The opioid overdose epidemic represents a dual crisis of health and economy in the United States. To measure economic consequences of the opioid crisis, it is crucial to understand the economic impacts of the opioid crisis on employers and their responses in the labor market. This is particularly important in understanding who is bearing the costs of the crisis, quantifying the magnitude of the burden, and designing policy interventions to address these issues.

In this study, we study the impact of a large transition toward illicit opioids, caused by OxyContin reformulation, focusing on skill requirements for new hires. Our estimates suggest that increased illicit opioid use resulted in higher skill level requirements when firms higher new employees.

Our findings have important policy implications. First, it underscores the *distributional effects* of the opioid crisis on workers. Our findings reveal that employers increase their skill requirements for new hires in response to the crisis, disproportionately affecting less-skilled workers. Importantly, even those less-skilled workers without a history of opioid use disorders can also be impacted by these changes in skill requirements. Therefore, interventions aimed at addressing the adverse impact of the opioid crisis, such as those designed to improve employment outcomes, should not be limited to individuals with opioid use disorders.

Second, our study highlights the need for diverse types of resources tailored to targeted populations. Policy discussions surrounding the opioid crisis have largely concentrated on health outcomes and resources for the prevention and treatment of opioid use disorders. However, our results suggest that providing occupational training programs to enhance the skills of less-skilled workers could be a meaningful approach to mitigating the adverse impact of the opioid crisis on this group.

Third, our study implies that employers may have strong incentives to prevent and address opioid use disorders not only among their employees but also within their communities. Our findings suggest that employers are adversely affected by the opioid crisis not just in terms of employee productivity but also through local opioid use, as an increase in local drug abuse can result in reductions in local consumption and the number of qualified job candidates. Policymakers may consider that employers can play a critical role in preventing and addressing the opioid crisis.

Lastly, the fact that our study focuses on the transition from prescription to illicit opioids, which occurred in 2010, carries important implications and relevance. Since then, illicit opioids have been a major driver of the escalating overdose mortality rates. Our investigation into this pivotal shift from legal to illegal opioids in 2010 yields a unique set of results that bears relevance to the ongoing epidemic of illicit opioids.

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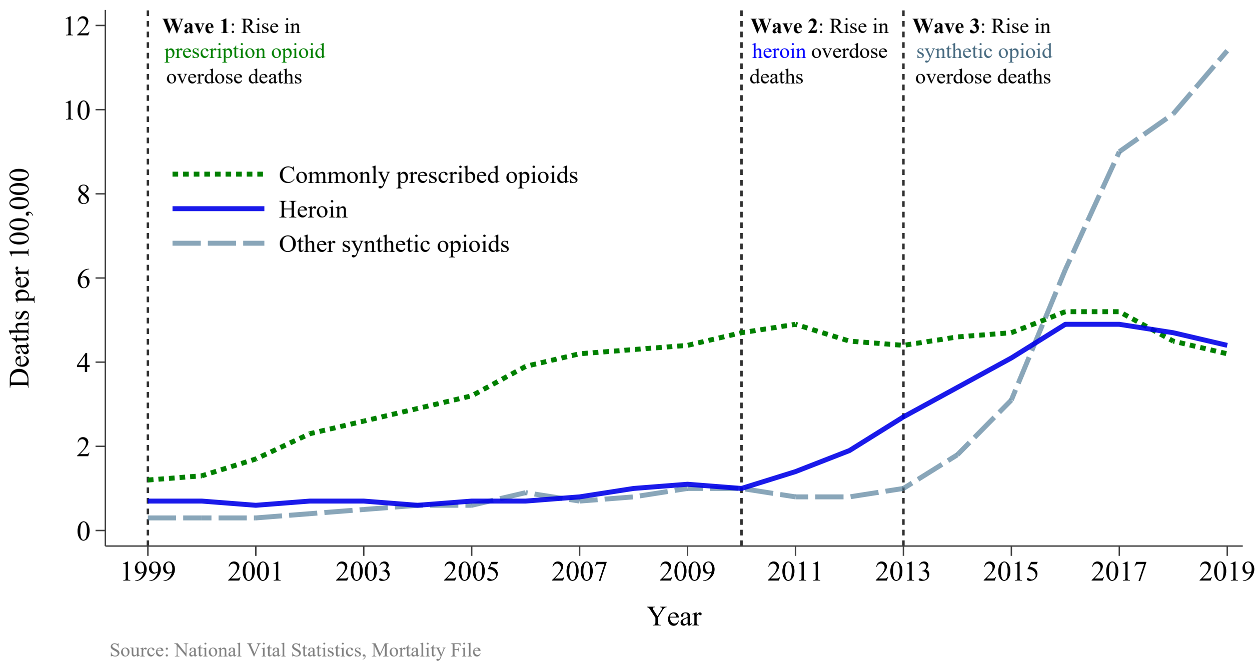
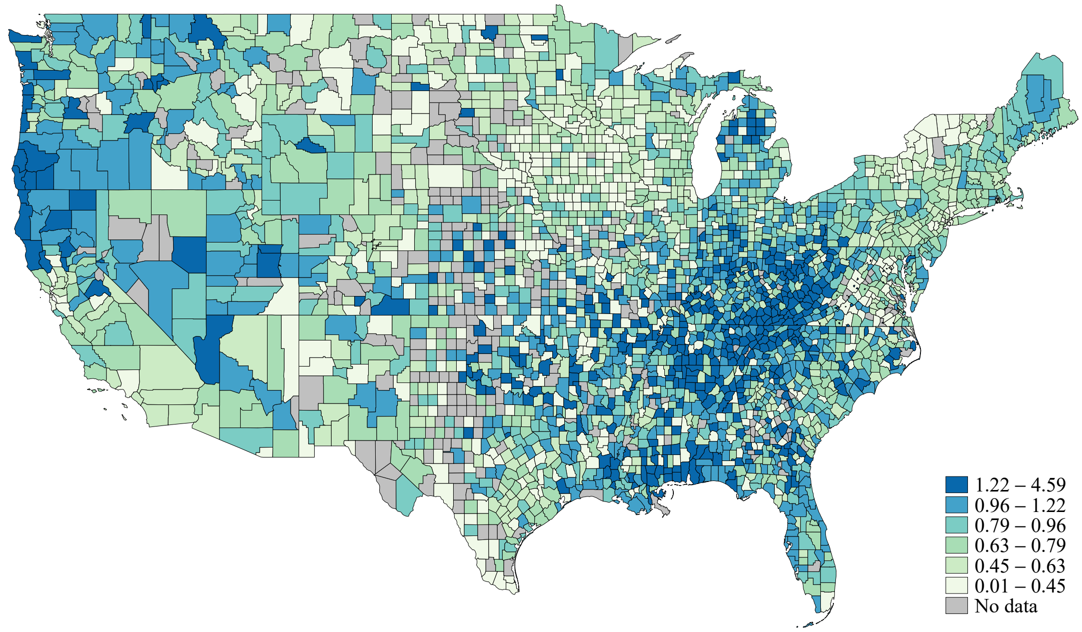
7 Figures and Tables

Figure 1: National Trends in Opioid Mortality



Notes: The figure displays the national trends in opioid overdose deaths per 100,000 from 1999–2019. The opioid overdose deaths were adapted from the Centers for Disease Control (CDC).

Figure 2: Geographic Variation in Exposure to Pre-Intervention Prescription Opioids Across Counties

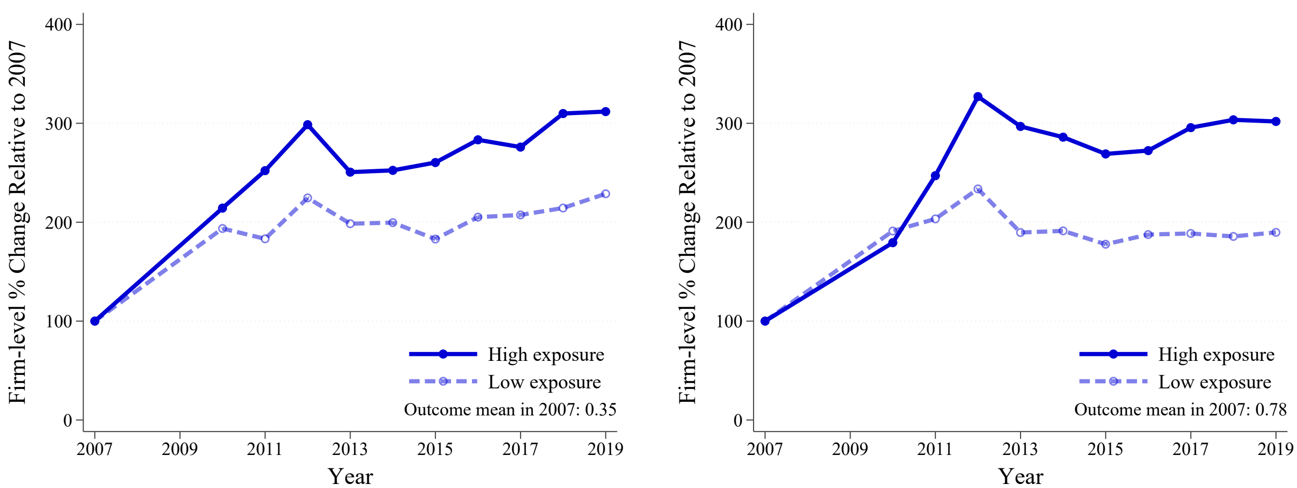


Notes: This figure presents the distribution of opioid prescriptions per capita across U.S counties based on opioid prescription rates from the Centers for Disease Control (CDC). The CDC data represent an 85 percent sample of retail pharmacy providers but exclude hospitals.

Figure 3: Raw Trends in Firm-level Changes in Skill Requirements Over Time

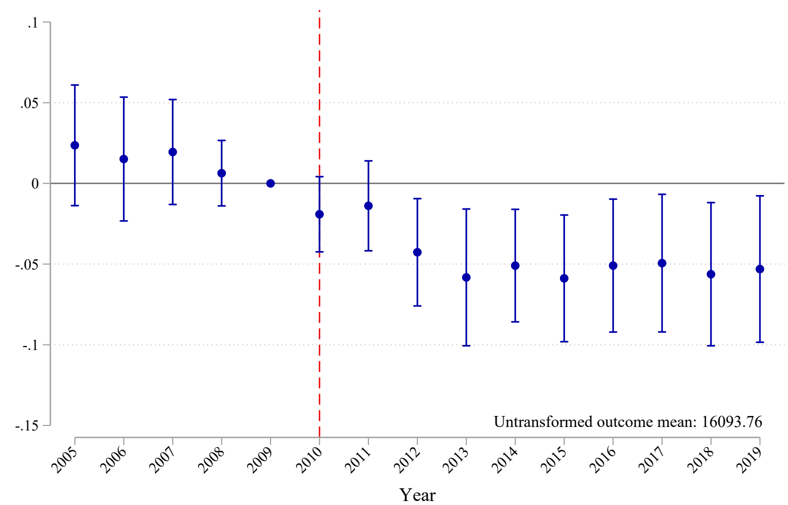
(a) % Change in Number of Cognitive Skills

(b) % Change in Number of Computer Skills



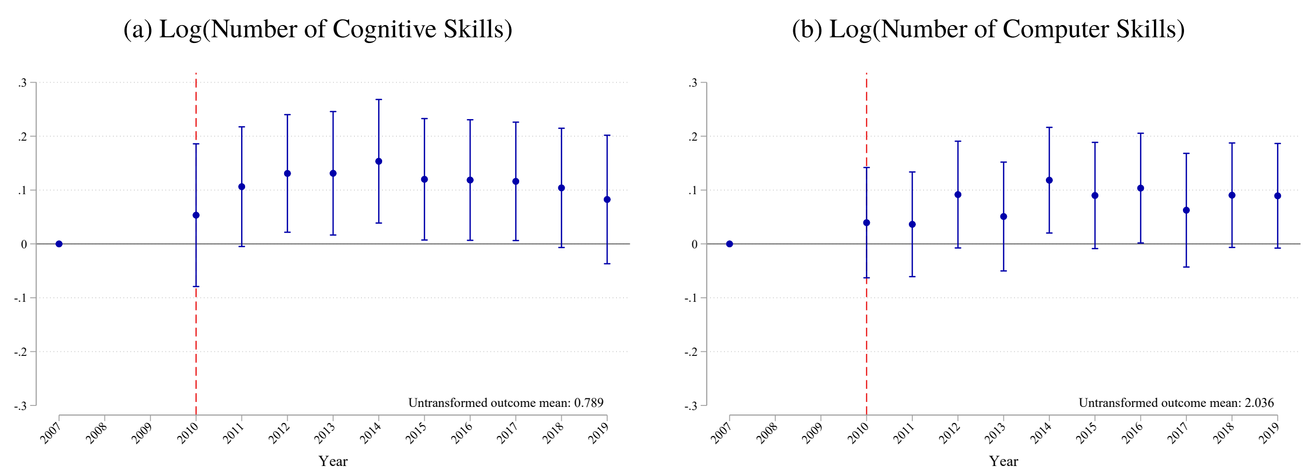
Notes: The figure illustrates raw trends in average number of specific skill requirements required in each job posting, measured as the firm-level percentage increase relative to 2007, separately for low-exposure and high-exposure firms. High-exposure firms are defined as those with an exposure measure larger than the median in each industry group based on the NAICS two-digit code. Panel (a) shows trends for cognitive skills and Panel (b) for software skills, respectively. Observations are firm by year level and weighted by the number of job postings in that year.

Figure 4: Effects of the OxyContin Reformulation on Log(Firm Employment Levels)



Notes: The figure displays the effect of the OxyContin reformulation on firm employment. The coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). The year 2009, which is one year prior to the OxyContin reformulation, is set as the reference point and normalized to zero. Standard errors are clustered at the firm level.

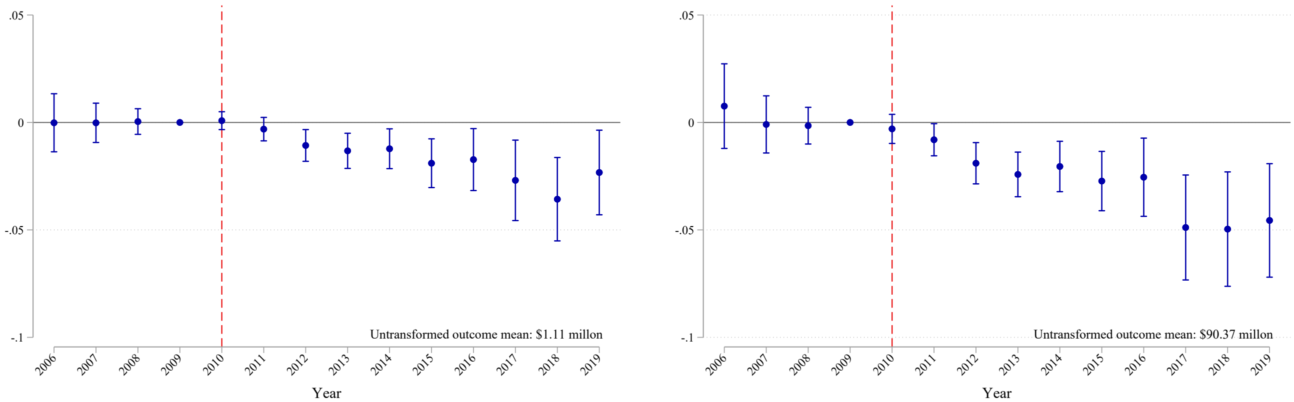
Figure 5: Effect of the OxyContin Reformulation on Firm Skill Demand



Notes: The figure illustrates the impact of the OxyContin reformulation on the number of particular skills listed in a job posting across cognitive skills (Panel (a)) and computer skills (Panel (b)). The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). The year 2009, which is one year prior to the OxyContin reformulation, is set as the reference point and normalized to zero. Standard errors are clustered at the firm level.

Figure 6: Effects of the OxyContin Reformulation on Local Store Sales

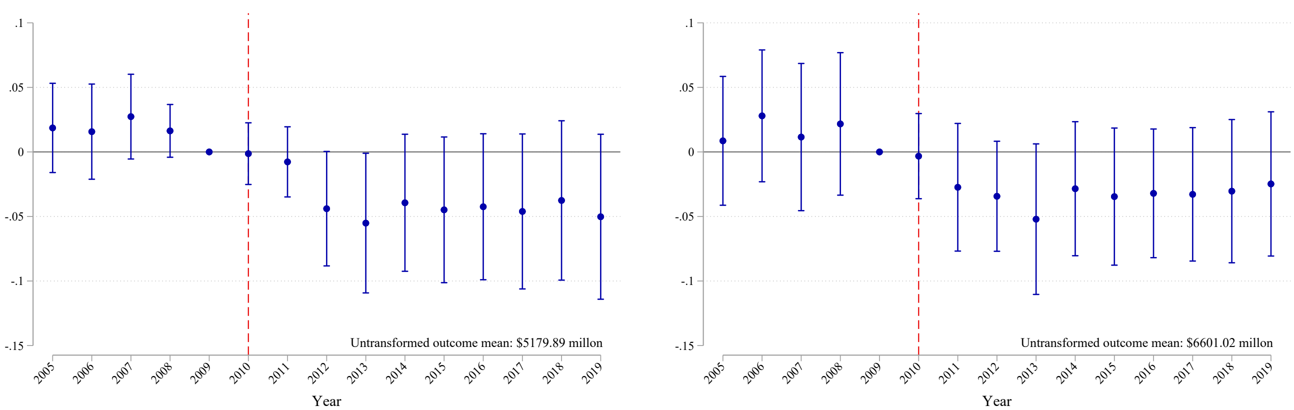
(a) Log(Average Local Store Sales) (b) Log(Total Local Store Sales)



Notes: The figure shows the effect of the OxyContin reformulation on local store sales measured at the county and year level. The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). The year 2009, which is one year prior to the OxyContin reformulation, is set as the reference point and normalized to zero. Standard errors are clustered at the firm level.

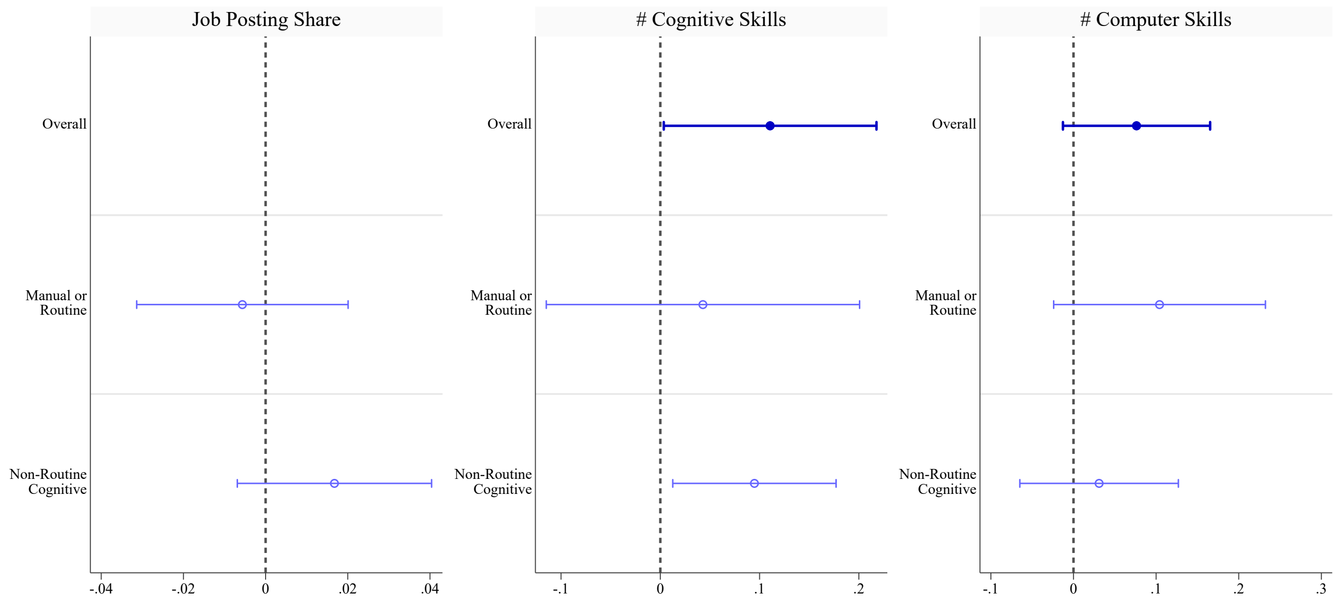
Figure 7: Effects of the OxyContin Reformulation on Revenue and Capital Stock

(a) Log(Property, Plant, and Equipment (PP&E)) (b) Log(Revenue)



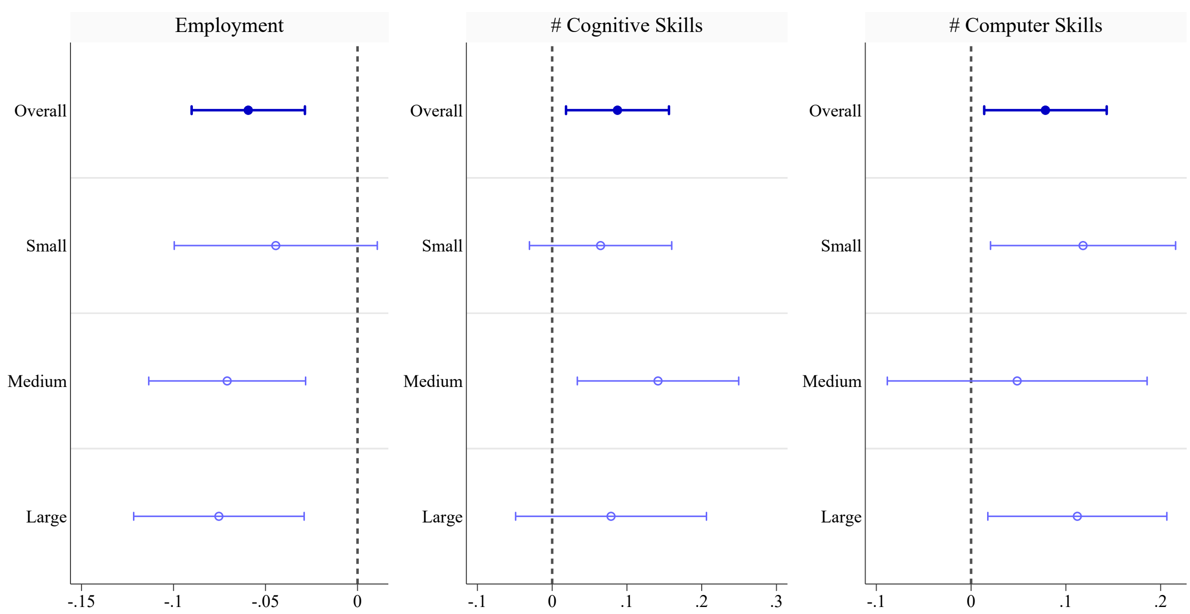
Notes: The figure shows the effect of the OxyContin reformulation on firm revenue and capital stock. The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). The year 2009, which is one year prior to the OxyContin reformulation, is set as the reference point and normalized to zero. Standard errors are clustered at the firm level.

Figure 8: Effect of the OxyContin Reformulation on Job Posting Share Labor Demand: Heterogeneity by Occupation Group



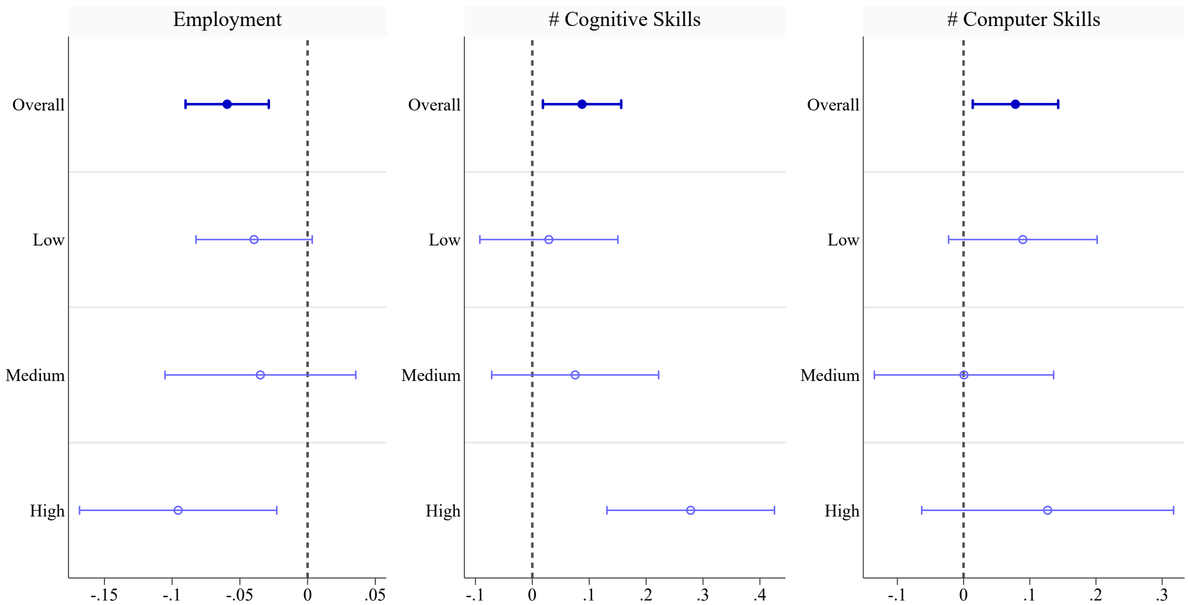
Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). Standard errors are clustered at the firm level. In the left panel, the overall effect cannot be estimated since the total job posting share equals 1 for all firm-year observations.

Figure 9: Effect of the OxyContin Reformulation on Labor Demand: Heterogeneity by Firm Size



Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). Standard errors are clustered at the firm level.

Figure 10: Effect of the OxyContin Reformulation on Labor Demand: Heterogeneity by Employment Protection Levels

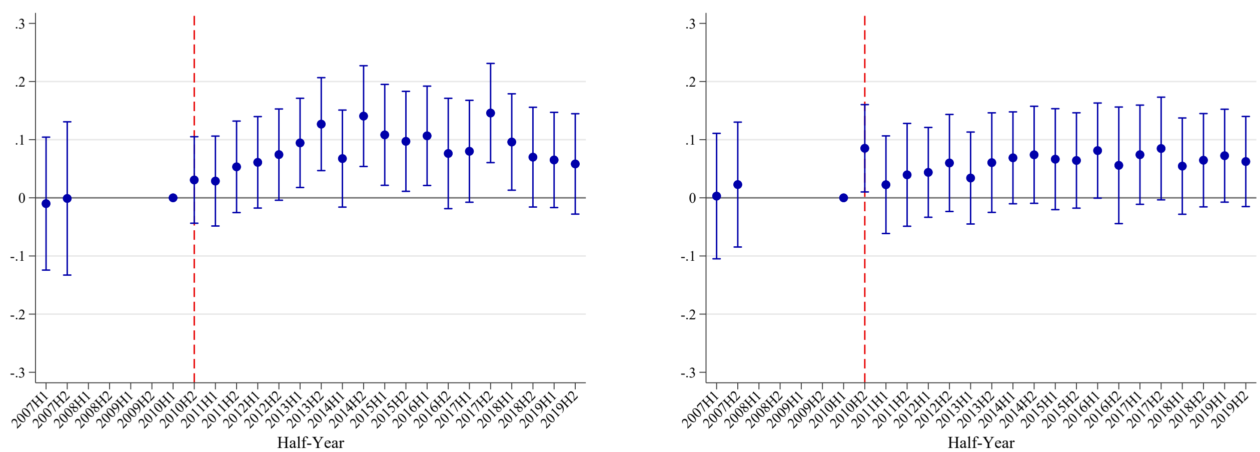


Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). Standard errors are clustered at the firm level.

Figure 11: Robustness of the skill results to using half-yearly data

(a) Log(Number of Cognitive Skills)

(b) Log(Change in Number of Computer Skills)



Notes: The figure shows the impact of the OxyContin reformulation on the number of particular skills listed in a job posting across cognitive skills (Panel (a)) and computer skills (Panel (b)). The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). The year 2009, which is one year prior to the OxyContin reformulation, is set as the reference point and normalized to zero. Standard errors are clustered at the firm level.

Table 1: Summary Statistics

	Mean	SD	25th percentile	75th percentile	Observations
A. County-level Pre-Intervention Prescription Opioid Use, 2006–2009					
Per capita opioid prescriptions	0.706	0.218	0.557	0.821	29,321
B. Firm-level Outcomes, 2005–2019					
Revenue (millions)	6,362	21,003	142	3,671	29,321
Property, Plant and Equip. (millions)	4873	20,050	10	2,030	29,321
Employment	16,381	46,091	406	10,900	29,321
C. Skill Requirements in a Job Posting, 2007, 2010–2019					
Number of Job Postings	1591	6016	13	745	15355
Number of Cognitive Skills	0.733	0.533	0.336	1.000	13952
Years of schooling	9.763	4.241	7.257	12.748	13989
Years of experience	2.480	1.549	1.366	3.427	13989

Notes: This table presents means of the main outcomes in our analysis. Panel A presents the county-level population-weighted average per capita opioid prescriptions from 2006 to 2009. Panel B reports averages of firm-level financial statements over the years 2005–2019. Panel C presents averages of the number of specific skills required in job postings during the years 2007 and 2008 to 2019.

Table 2: Effect of OxyContin Reformulation on Employment and Skill Requirement

	Employment	Cognitive Skill	Computer Skill	Schooling Years	Experience Years
	(1)	(2)	(3)	(4)	(5)
Opioid Exposure $\times$ Post	-0.057***	0.111**	0.077*	0.028	0.010
	(0.019)	(0.055)	(0.046)	(0.039)	(0.043)
	[0.002]	[0.042]	[0.092]	[0.464]	[0.811]
Sample	Firm	Firm	Firm	Firm	Firm
Data	Compustat	Lightcast	Lightcast	Lightcast	Lightcast
Outcome	Log	Log	Log	Log	Log
Untransformed outcome mean	16093.76	0.789	2.036	10.193	2.601
Number of observations	23,018	10576	10842	10915	10929

Notes: This table presents coefficients, standard errors (in parentheses), and p-values [in brackets] from estimation of equation (2). The regressions include firm and industry-by-year fixed effects, where industry is defined by the 4-digit NAICS code. Standard errors are clustered at the firm level. Untransformed outcome means are calculated based on the pre-reformulation period.

Table 3: Effect of OxyContin Reformulation on Local Store Sales, Firm Revenue, and Capital Stock

	Avg. Sales (1)	Total Sales (2)	Revenue (3)	Capital Stock (4)
Opioid Exposure × Post	-0.016***	-0.028***	-0.043**	-0.051**
	(0.006)	(0.008)	(0.021)	(0.022)
	[0.010]	[0.001]	[0.041]	[0.019]
Sample	County	County	Firm	Firm
Data	Nielsen	Nielsen	Compustat	Compustat
Outcome	Log	Log	Log	Log
Untransformed outcome mean	$1.11 M	$90.37 M	$6601.02 M	$5179.89 M
Number of observations	32,763	32,763	23,018	23,018

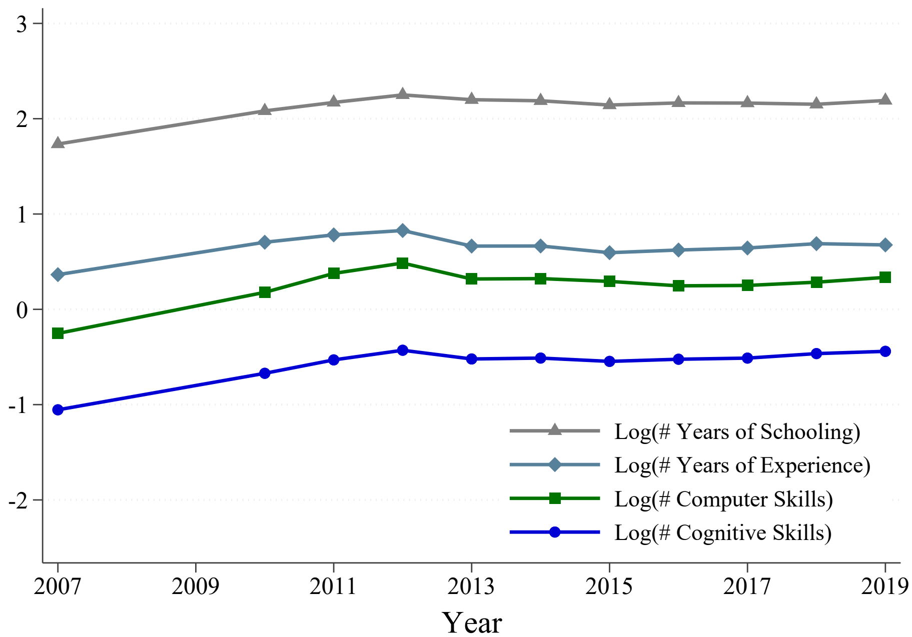
Notes: This table presents coefficients, standard errors (in parentheses), and p-values [in brackets] from estimation of equation (2). The regressions include firm and industry-by-year fixed effects, where industry is defined by the 4-digit NAICS code. Standard errors are clustered at the firm level. Untransformed outcome means are calculated based on the pre-reformulation period.

For Online Publication

"The Opioid Crisis and Labor Demand: Evidence from Job Posting Data"

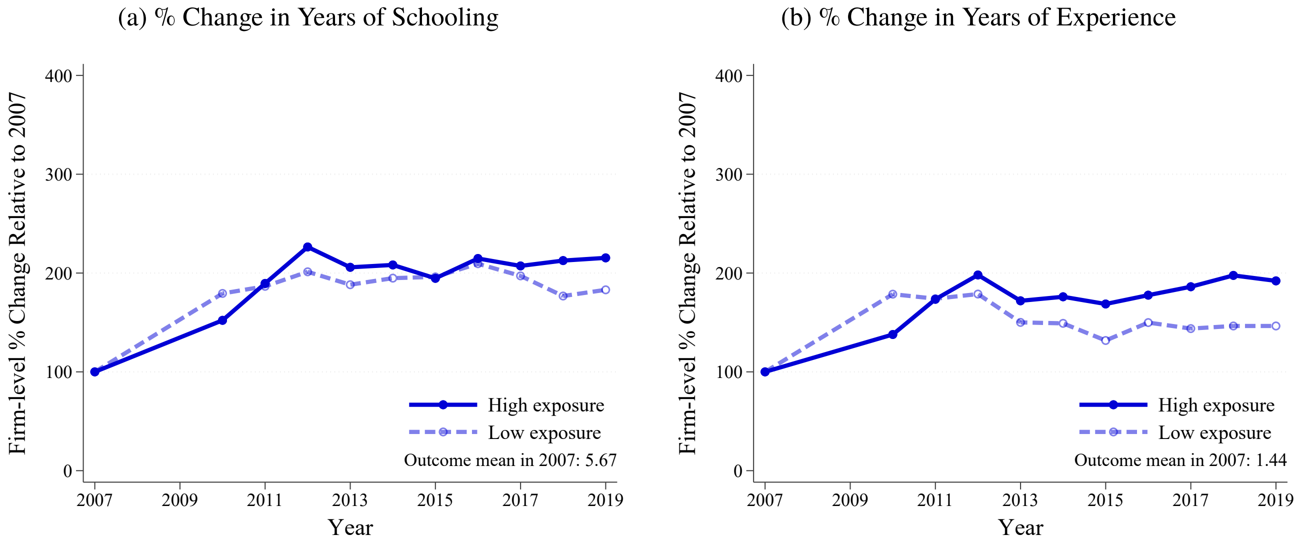
Kim, Kim, and Park (2024)

Figure A1: Raw Trends in Skill Requirements



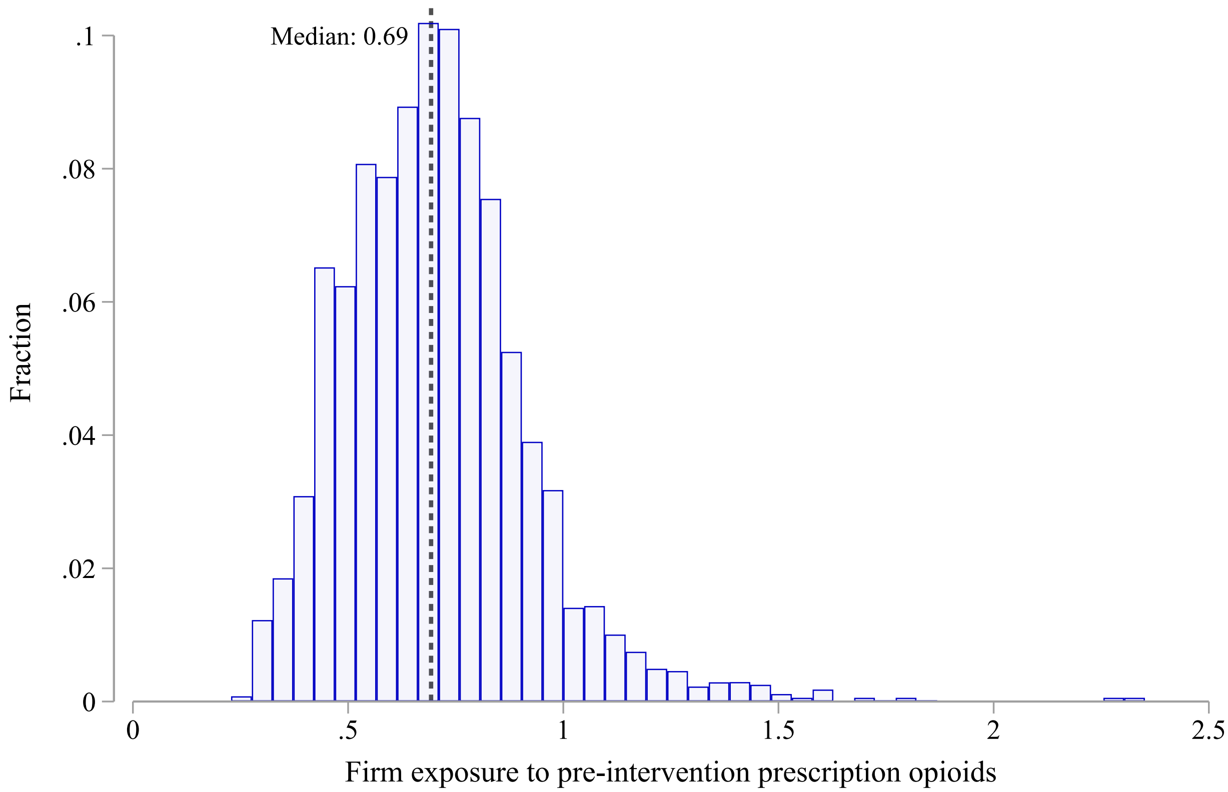
Notes: This figure presents trends in average skill requirements in each job posting in our sample between 2007 and 2009–2019. The trends for Log(years of schooling) are marked by gray triangles, Log(years of experience) by green diamonds, Log(number of computer skills) by orange squares, and Log(number of cognitive skills) by blue dots. To compute these, we first construct firm-by-year panel on the average skill requirement for each job posting. We then calculate the weighted average of skill requirements for each job posting, applying weights based on the number of job postings posted by that firm in the pre-reformulation period (2007 in our data set). Lastly, we apply a logarithmic transformation to these averages to examine the growth rates over time.

Figure A2: Raw Trends in Firm-level Changes in Skill Requirements Over Time



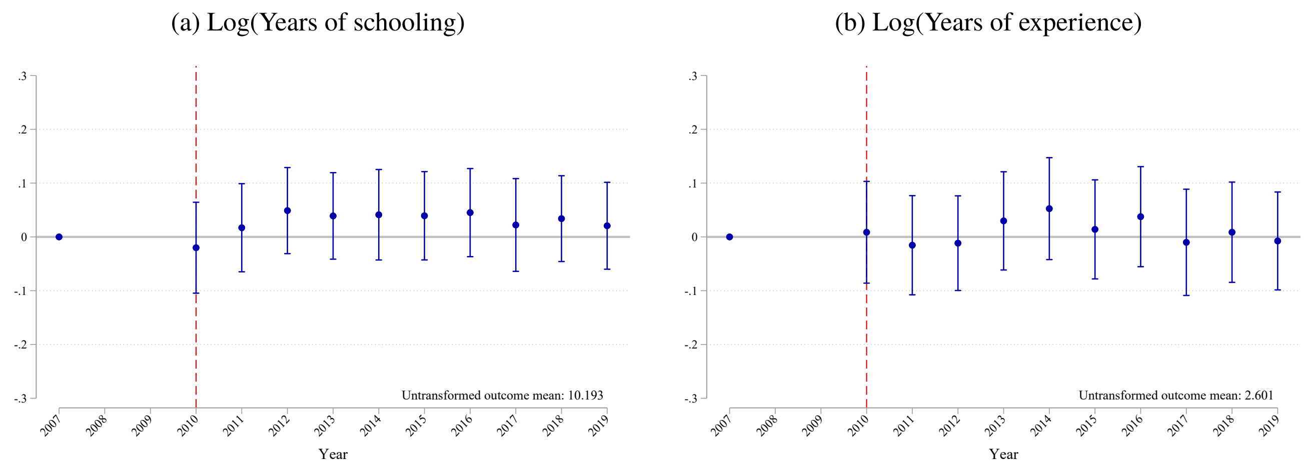
Notes: The figure illustrates raw trends in average number of specific skill requirements required in each job posting, measured as the firm-level percentage increase relative to 2007, separately for low-exposure and high-exposure firms. High-exposure firms are defined as those with an exposure measure larger than the median in each industry group based on the NAICS two-digit code. Panel (a) shows trends for years of schooling, Panel (b) for years of experience, respectively. Observations are firm by year level and weighted by the number of job postings in that year.

Figure A3: Distribution of Firm Exposure to Pre-Intervention Prescription Opioids



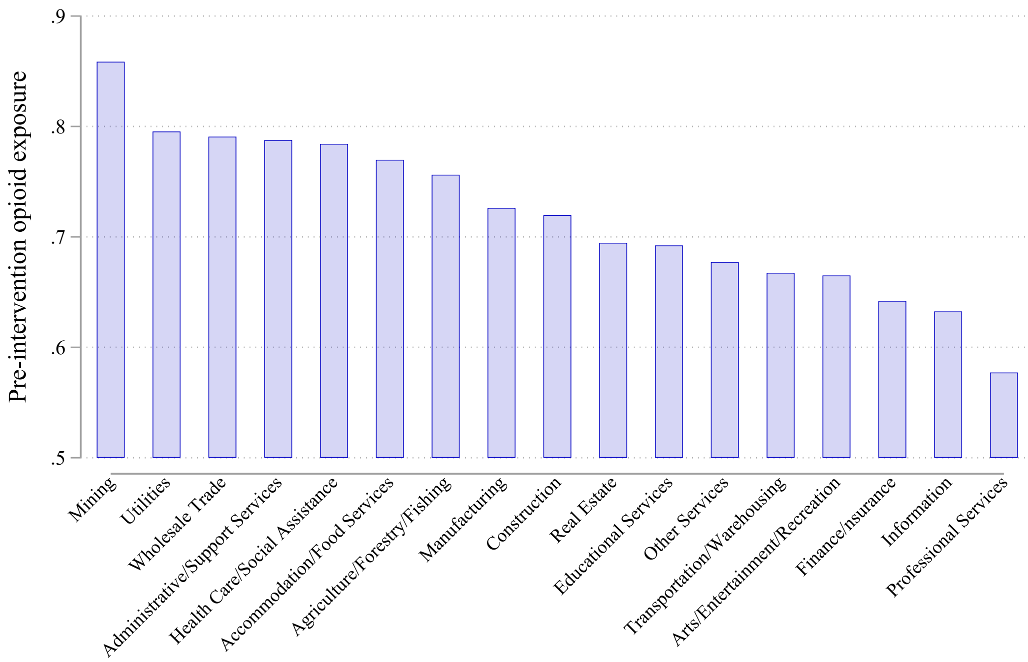
Notes: This figure presents the distribution of opioid prescriptions per capita across firms in our analysis sample.

Figure A4: Effect of the OxyContin Reformulation on Education and Work Experience Requirements



Notes: The figure illustrates the impact of the OxyContin reformulation on the number of particular skills listed in a job posting across years of schooling (Panel (a)) and years of experience (Panel (b)). The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). The year 2009, which is one year prior to the OxyContin reformulation, is set as the reference point and normalized to zero. Standard errors are clustered at the firm level.

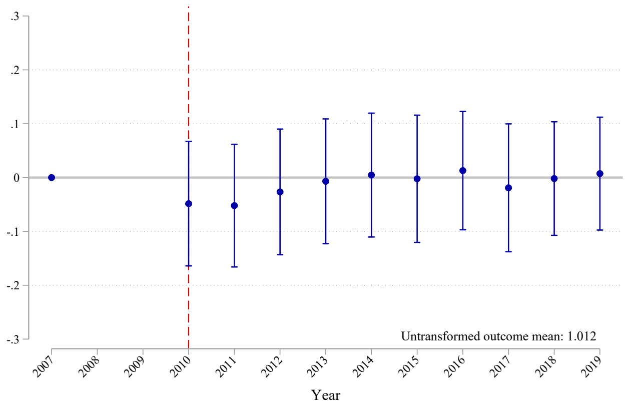
Figure A5: Firm Exposure to Pre-Intervention Prescription Opioids by Industry



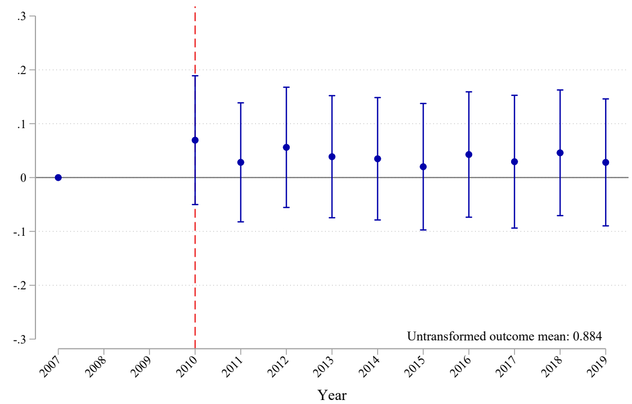
Notes: This figure presents the distribution of a firm's exposure to OxyContin reformulation across industry groups in our analysis sample.

Figure A6: Effect of the OxyContin Reformulation on Firm Skill Demand: Other Skills

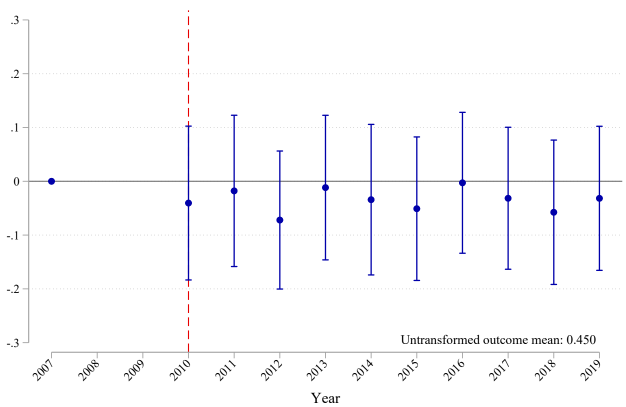
(a) Log(Number of Social Skills)



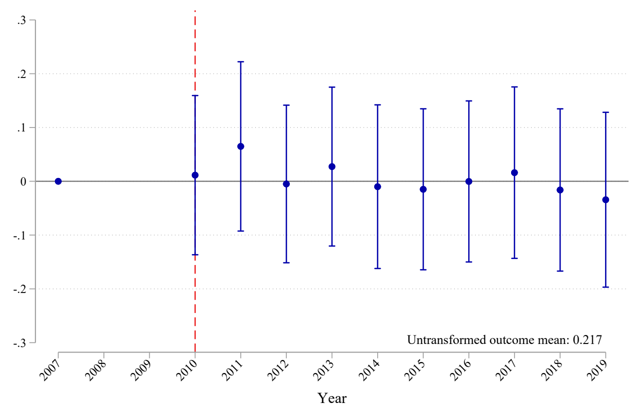
(b) Log(Number of Service Skills)



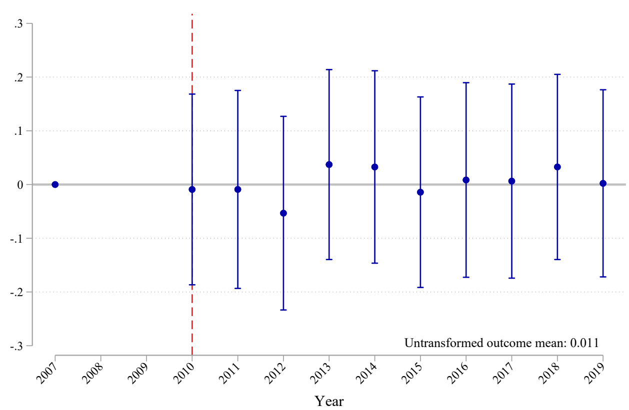
(c) Log(Number of Finance Skills)



(d) Log(Number of Management Skills)

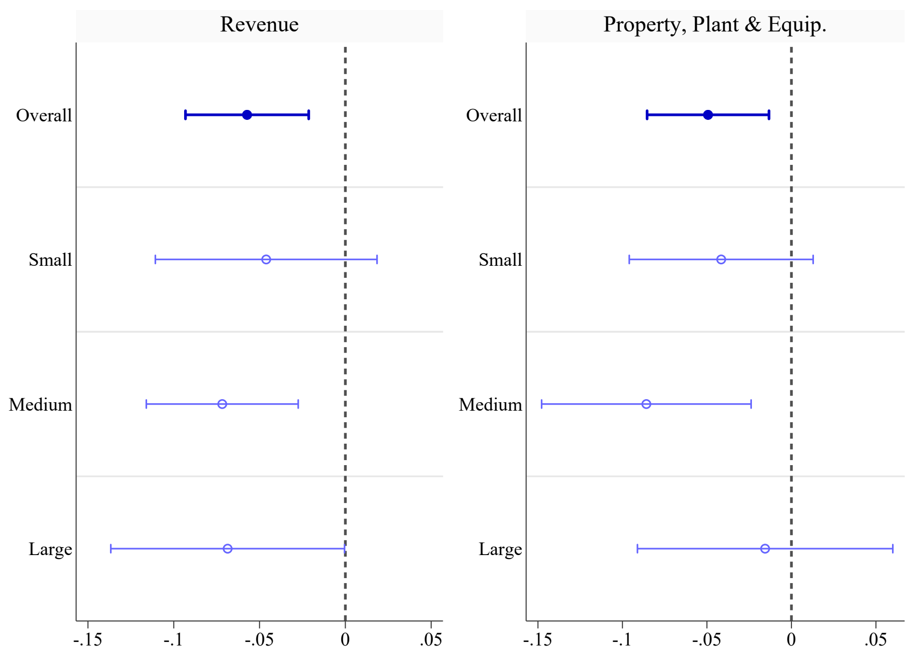


(e) Log(Number of Writing Skills)



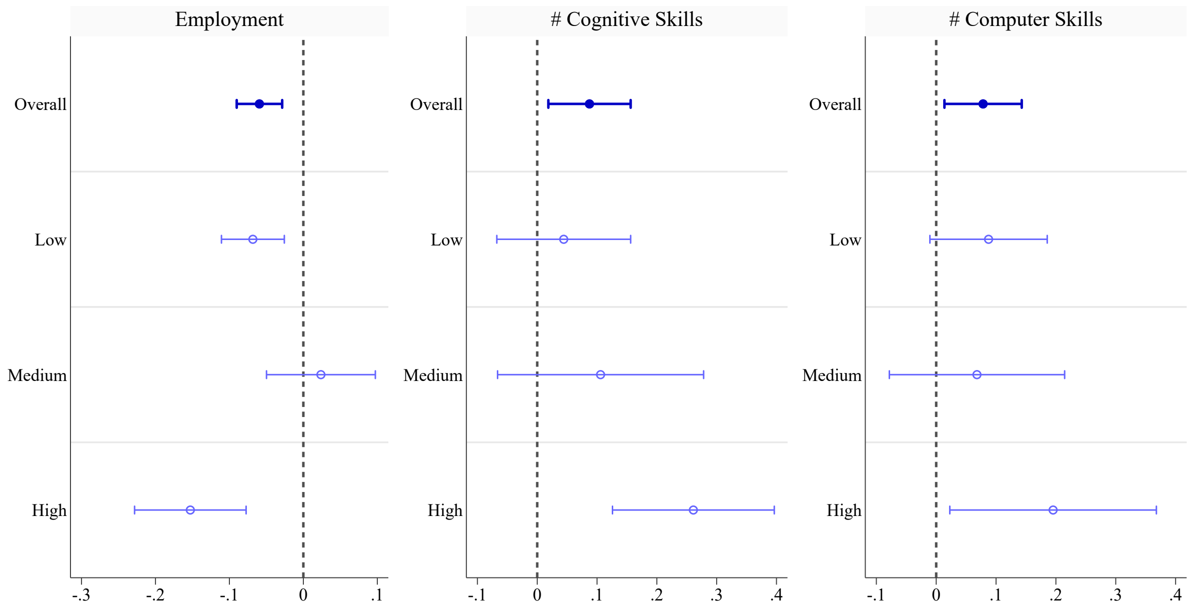
Notes: The figure illustrates the impact of the OxyContin reformulation on the number of particular skills listed in a job posting across five categories: social skills (Panel (a)), service skills (Panel (b)), finance skills (Panel (c)), and management skills (Panel (d)), and writing skills (Panel (e)). The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). The year 2009, which is one year prior to the OxyContin reformulation, is set as the reference point and normalized to zero. Standard errors are clustered at the firm level.

Figure A7: Effect of the OxyContin Reformulation on Capital and Revenue: Heterogeneity by Firm Size



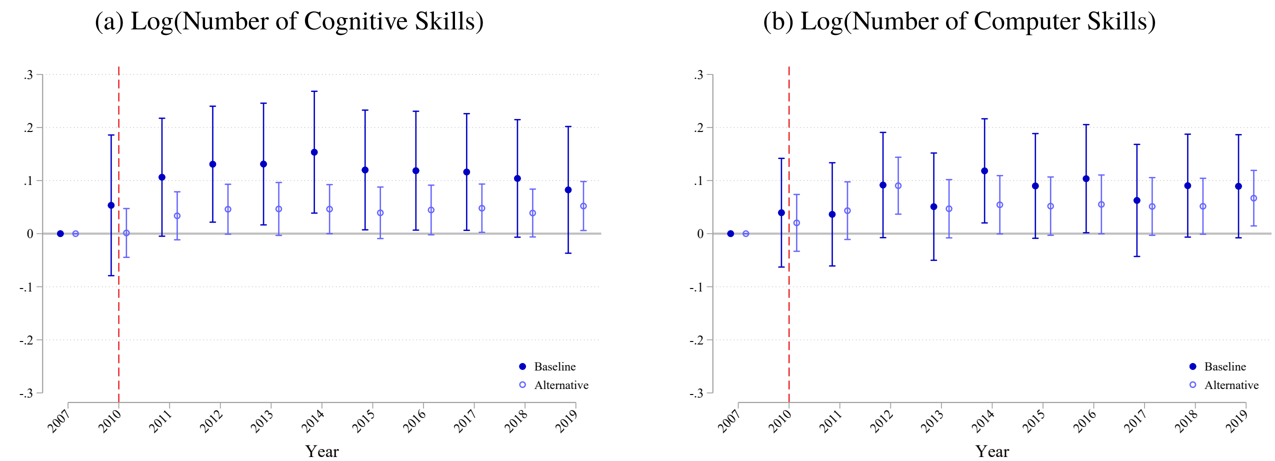
Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). Standard errors are clustered at the firm level.

Figure A8: Effect of the OxyContin Reformulation on Labor Demand: Heterogeneity by Minimum Wage Levels



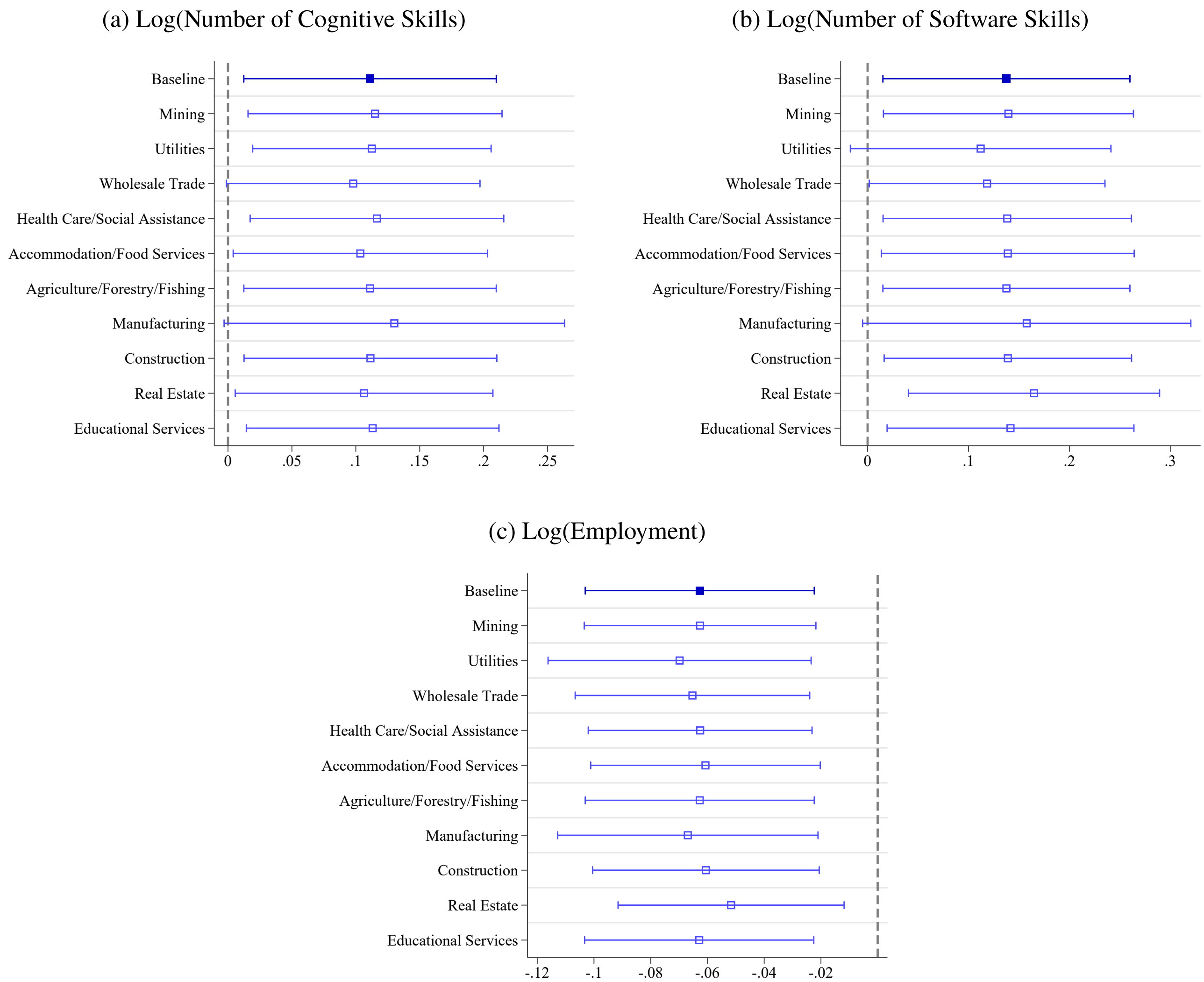
Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). Standard errors are clustered at the firm level.

Figure A9: Effect of the OxyContin Reformulation on Firm Skill Demand: Firm-by-State Level Analysis



Notes: These figures present output from estimation of equation (1) using our baseline sample (circles in dark blue) and our alternative sample based on state-by-year observations (hollow circles in light blue). For both samples, we plot the coefficients and 95% confidence intervals on the interactions between the exposure to reformulation and the full set of year dummies. The year 2009, which is one year prior to the OxyContin reformulation, is set as the reference point and normalized to zero. Standard errors are clustered at the firm level.

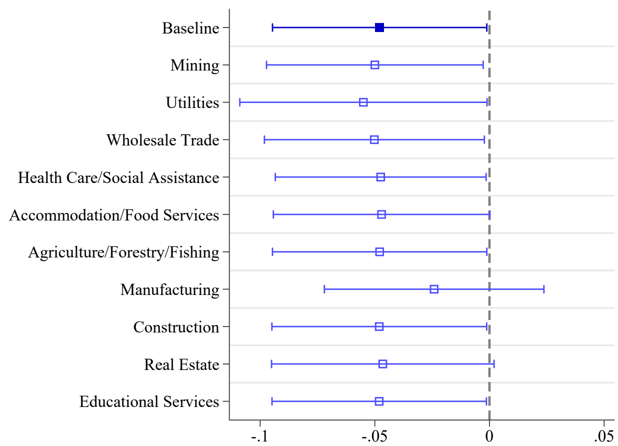
Figure A10: Robustness to Dropping One Industry Category: Skill Demand Outcomes



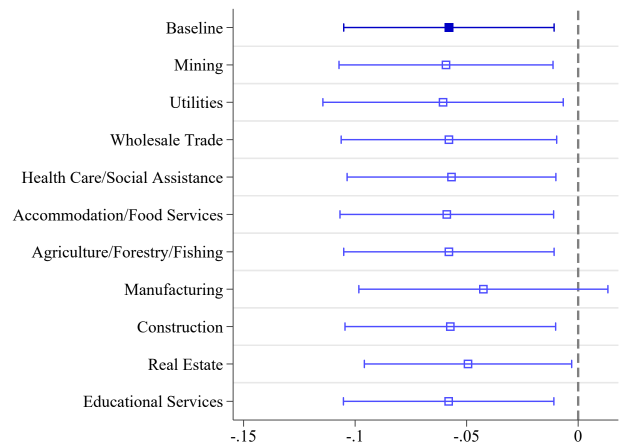
Notes: We present presents coefficients and 95% confidence intervals from estimation of equation (2) where we drop each of the following industry groups from our sample: (1) mining, (2) utilities, (3) wholesale trade, (4) health care/social assistance, (5) accomodation/food services, (6) agriculture/forestry/fishing, (7) manufacturing, (8) real estate, and (9) educational services.

Figure A11: Robustness to Dropping One Industry Category: Firm Financial Outcomes

(a) Revenue



(b) Property, Plant, and Equipment (PP&E)



Notes: We present presents coefficients and 95% confidence intervals from estimation of equation (2) where we drop each of the following industry groups from our sample: (1) mining, (2) utilities, (3) wholesale trade, (4) health care/social assistance, (5) accomodation/food services, (6) agriculture/forestry/fishing, (7) manufacturing, (8) real estate, and (9) educational services.

Table A1: Skill Categorization

Category	Key words and phrases
Cognitive	Problem Solving, Research, Analytical, Critical Thinking, Math, Statistics
Software (specific)	Programming language or specialized software (e.g. Java, SQL, Python, etc.)
Social	Communication, Teamwork, Collaboration, Negotiation, Presentation
Customer Service	Customer, Sales, Client, Patient
Computer (general)	Computer, Spreadsheets, Common Software (e.g. Microsoft Excel, Powerpoint)
Financial	Budgeting, Accounting, Finance, Cost
Management	Project Management, People Management (Supervisory, Leadership, Management, Mentoring, Staff)
Writing	Writing
Relationships	Organized, Detail-oriented, Multi-tasking, Time Management, Meeting Deadlines, Energetic

Notes: Categorization of skill requirements in Lightcast from Deming and Kahn (2018).

Table A2: Effect of the OxyContin Reformulation on the Number of Job Postings

	Number of Job Postings (1)
Opioid Exposure × Post	-0.096
	(0.096)
Sample	Firm
Data	Lightcast
Outcome	Log
Untransformed outcome mean	1,610.119
Number of observations	12,834

Notes: This table presents coefficients, standard errors (in parentheses), and p-values [in brackets] from estimation of equation (2). The regressions include firm and industry-by-year fixed effects, where industry is defined by the 4-digit NAICS code. Standard errors are clustered at the firm level. Untransformed outcome means are calculated based on the pre-reformulation period.

Table A3: Robustness of the Labor Market Estimates to Controlling for Weighted Local Labor Force Participation by Worker Subgroups

	(1)	(2)	(3)	(4)
Panel A: Employment				
Opioid Exposure × Post	-0.058***	-0.055***	-0.059***	-0.058***
	(0.019)	(0.019)	(0.019)	(0.019)
	[0.002]	[0.003]	[0.002]	[0.002]
Panel B: Cognitive Skill Requirements				
Opioid Exposure × Post	0.099*	0.072	0.072	0.072
	(0.057)	(0.058)	(0.058)	(0.058)
	[0.080]	[0.215]	[0.219]	[0.218]
Panel C: Software Skill Requirements				
Opioid Exposure × Post	0.138**	0.113*	0.109*	0.110*
	(0.063)	(0.064)	(0.065)	(0.065)
	[0.111]	[0.178]	[0.216]	[0.221]
LFP by Gender	No	Yes	Yes	Yes
LFP by Education	No	No	Yes	Yes
LFP by Race	No	No	No	Yes

Notes: In this table, we report the sensitivity of our results when controlling for labor force participation rates for the subgroup of workers. In column 1, we reproduce our baseline estimates from the estimation of equation (2). In column 2, we add female and male workers' labor force participation rates. In column 3, we add measures of education sub-groups—college graduates and non-college graduates. Finally, column 4 adds measures for race sub-groups—Non-Hispanic White, Non-Hispanic Black, and Hispanic.

Table A4: Robustness of the Labor Market Estimates to Controlling for Weighted Local Wage Level by Worker Subgroups

	(1)	(2)	(3)	(4)
Panel A: Employment				
Opioid Exposure × Post	-0.058***	-0.055***	-0.056***	-0.050***
	(0.019)	(0.019)	(0.019)	(0.020)
	[0.002]	[0.004]	[0.004]	[0.010]
Panel B: Cognitive Skill Requirements				
Opioid Exposure × Post	0.099*	0.108*	0.107*	0.111*
	(0.057)	(0.056)	(0.056)	(0.058)
	[0.080]	[0.052]	[0.055]	[0.055]
Panel C: Software Skill Requirements				
Opioid Exposure × Post	0.078	0.083*	0.081*	0.078
	(0.049)	(0.048)	(0.048)	(0.049)
	[0.111]	[0.087]	[0.090]	[0.110]
Wage by Gender	No	Yes	Yes	Yes
Wage by Education	No	No	Yes	Yes
Wage by Race	No	No	No	Yes

Notes: In this table, we report the sensitivity of our results when controlling for wage levels for the subgroup of workers. In column 1, we reproduce our baseline estimates from the estimation of equation (2). In column 2, we add female and male workers' wage levels. In column 3, we add measures of education sub-groups—college graduates and non-college graduates. Finally, column 4 adds measures for race sub-groups—Non-Hispanic White, Non-Hispanic Black, and Hispanic.