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

Bokyung Kim[†] Minseog Kim[‡] Geunyong Park[§]

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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 firms' 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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[†]Stanford University, SIEPR. Email: bokyung@stanford.edu.

[‡]Department of Economics, University of Texas at Austin. Email: minseog.kim@utexas.edu.

[§]Business School, National University of Singapore. Email: park.geunyong@nus.edu.sg.

1 Introduction

The United States is experiencing the worst opioid epidemic in its history. Between 1999 and 2019, opioid-involved overdose deaths increased by over fivefold, resulting in about half a million deaths from opioid overdose (National Institute on Drug Abuse, 2023). In 2019 alone, nearly 50,000 died from opioid overdose, surpassing the numbers resulting from motor vehicle accidents or breast cancer (Centers for Disease Control and Prevention, 2021).[1] Beyond the widely studied health and mortality consequences, recent research has investigated the far-reaching impacts of the opioid crisis on various outcomes, including crime (Deiana and Giua, 2021; Mallatt, 2022), children well-being (Ziedan and Kaestner, 2020; Evans et al., 2022; Buckles et al., 2023), housing market (Custodio et al., 2023; D'Lima and Thibodeau, 2023), and consumer and municipal finance (Cornaggia et al., 2022; 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).[2]

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 online job postings. To estimate the causal impact, we focus on an intervention that inadvertently shifted users from prescription opioids towards riskier and unregulated illicit opioids, including heroin. In 2010, Purdue Pharma introduced a reformulated version of OxyContin in an effort to increase its resistance to abuse. OxyContin was a widely

[1] The opioid crisis worsened during the COVID-19 pandemic (Simha et al., 2023).

[2] 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.

misused prescription medication in 2000s, and the reformulation represents one of the largest reductions in the availability of abusable prescription opioids. Previous studies have documented that this supply-side intervention induced opioid-dependent individuals to switch to illicit opioids with higher addictive potential, such as heroin and other substances. Prior evidence indicates 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. (Alpert et al., 2018; Beheshti, 2019; Evans et al., 2019; Powell et al., 2019; Mallatt, 2022). These findings highlight the profound and wide-ranging impacts of this transition on both individuals and society, extending beyond overdose mortality. Building on this literature, we investigate how this transition has affected job skill requirements.

This transition toward illicit drugs 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 risk of illegal opioid use.[3] Even when only a small percentage of job candidates are at high risk of illicit drug use, certain firms may react excessively to the perceived risks due to factors such as a low tolerance for risk, lack of understanding about substance abuse, concerns about legal liabilities, and uncertainty surrounding a job seeker's future drug use. Second, employers may adjust skill requirements in response to changes in the 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 job positions with higher-skilled workers. Lastly, the increased illicit drug use may reduce workers' productivity, labor supply, and local consumption of non-opioid products, potentially resulting in reduced sales and revenue for firms. These changes in firm performance may eventually impact their financial constraints as well as hiring strategies.

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 constructed using data from two sources. First, we use online

[3] Previous research indicates that opioid use and fatalities are more prevalent among individuals with lower socioeconomic status and lower educational attainment (Case and Deaton, 2015; Altekruse et al., 2020). Employers may use skill levels as a proxy for assessing risks associated with opioid use and overdoses.

job posting data from Lightcast over the years 2007 to 2019.[4] 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 employment, revenue, and capital stock obtained from the Compustat North America Database. For our analysis, we focus on publicly traded companies, the shares of which are available for anyone to buy and sell on the open market. Publicly traded companies account for one-third of U.S. employment in the non-farm business sector (Davis et al., 2006). In addition to the firm-level data, we use 2007–2019 Nielsen Retail Scanner data to measure retail store sales in 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 job skill requirements.

Our findings are as follows. First, we show that the transition toward illicit opioids reduces employment at the firm level, consistent with findings from prior studies looking at aggregate-level local employment. v Our estimates reveal that a one standard deviation increase in firm-level exposure to the reformulation (equivalent to an additional 0.22 per capita opioid prescriptions in the pre-reformulation period) resulted in a 5 percent decrease in employment at the firm level. 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 8 percent increase in the average number of cognitive skills, such as statistical analysis, mathematical capability, and industry knowledge, and a 5 percent increase in the average number of computer skills required in an online job posting. We find little evidence indicating that firms adjust their education and experience requirements following the reformulation, although our findings from heterogeneity analyses suggest a significant impact on these outcomes within specific sub-groups. 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 average store

[4] Note that the Lightcast job posting data are unavailable for 2008 and 2009.

sales in a county and a 4.6 percent decrease in annual firm revenue. We also find a 4.2 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 job positions with that for more-skilled ones, 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 three groups—non-routine manual jobs, routine jobs, and non-routine cognitive jobs, representing low, middle, and high-skilled positions, respectively. If the primary channel is the substitution of less-skilled job positions with more-skilled positions, we would expect to observe an increase in the job posting share for more-skilled positions within a firm following reformulation. In contrast, if screening is the primary channel, we would anticipate observing upskilling within the same occupation group in a firm, indicating that employers are raising skill requirements for similar types of jobs following reformulation. Our findings provide evidence for both channels, ruling out the possibility that one of these channels solely drives the entire results.

To understand how the effects of reformulation vary across different firm and local labor market characteristics, we investigate heterogeneity based on factors such as firm size, firm education requirements, local employment protection levels, and local minimum wage levels. We document the following key findings. First, we find no consistent evidence of heterogeneity in outcomes related to firm size. Second, we document that upskilling effects are more pronounced among firms with lower education requirements at baseline. This aligns with previous research suggesting that less-educated individuals are at higher risk of opioid-related deaths. Finally, 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 higher-skilled workers.

A crucial concern in our analysis is a potential confounder that influences skill requirements and is strongly correlated with baseline local opioid use rates. Of particular concern are shocks that occurred in the pre-reformulation period, especially the Great Recession in 2008. We address this concern in three key ways. First, we directly control for the interaction of the Great Recession shock and the full set of year dummies in all our regressions throughout the paper. Even after controlling for exposure to the 2008 recession, we still uncover statistically significant evidence of upskilling. Second, we report that a firm's exposure to the 2008 Great Recession and its exposure to the OxyContin reformulation have a low correlation rate of 0.08. Third, in our robustness analysis, we estimate our event study regression model using half-yearly data instead of yearly data, considering the first half of 2010 as the reference period. The results show no evidence that higher exposure to reformulation is associated with any pre-existing difference in the 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.[5]

Another concern is that our firm-level exposure to reformulation may be confounded by other concurrent firm-level factors. To address this concern, we conduct a firm-by-state level analysis, aggregating establishments owned by the same firm within a given state as an integrated entity. In this analysis, we include entity fixed effects as well as firm-by-year fixed effects to control for any firm-specific time-varying shocks. The idea is to assess whether entities located in states with higher exposure to reformulation experienced greater changes in skill requirements compared to other entities under the same firm but located in states with lower exposure, even after accounting for firm-specific time-varying factors. Our findings indicate strong evidence of upskilling even when comparing low- and high-exposure entities within the same firm.

Our paper contributes to several strands of literature. First, we add to the literature studying the causal impact of opioid use on the labor market. Prior work uses geographic variation in opioid prescribing and documents that a higher local prescription rate is associated with lower labor market participation rates (Krueger, 2017; Harris et al., 2020; Aliprantis et al., 2023).[6] Other work exploits variation generated by OxyContin reformulation or policies aimed at reducing

[5]While earlier work often suggested that increases in "deaths of despair"—including suicide and poisoning by alcohol and drugs—are attributed to socioeconomic despair (e.g., Case and Deaton, 2015), recent research demonstrates that short-term changes in local economic conditions can explain only a small fraction of such increases (Ruhm, 2019; Deaton and Case, 2020; Currie and Schwandt, 2021). This further mitigates concerns in our study regarding confounding factors.

[6]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.

misuse of prescription opioids and documents that the opioid crisis is associated with lower employment and lower labor force participation rates (Cho et al., 2021; Park and Powell, 2021; Beheshti, 2023).[7] 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 skill requirements 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.[8] 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 navigate the opioid crisis by adapting in another important dimension—adjusting skill requirements when hiring new employees.

Third, our paper also contributes to the literature investigating the factors that influence firms' skill demand. Prior research suggests that firms' skill requirements are influenced by the business cycle (Hershbein and Kahn, 2018), the pool of labor supply (Modestino et al., 2016, 2020), technological progress (Michaels et al., 2014; Alekseeva et al., 2021), international trade (Burstein and Vogel, 2017; Carluccio et al., 2019; He et al., 2021), and labor market institutions

[7] 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.

[8] 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. These results are consistent with our evidence of sales and revenue decreases following reformulation, implying that financial constraints may prevent firms from increasing their technology investments.

(Ballance et al., 2020; Clemens et al., 2021). We add to this literature by examining this question in the unique context of the opioid crisis, which potentially deteriorates the quality of labor supply. Our findings suggest that firms respond to a decrease in labor quality by raising their skill criteria for evaluating job applicants.

Lastly, our study is closely related to the broad literature on statistical discrimination in labor markets. Doleac and Hansen (2020) show that policies that prevent employers from asking questions regarding job applicants' criminal records during the initial application process reduce employment among young Black men without a college degree by 5 percent. In addition, Bartik and Nelson (2024) find that banning employers from accessing credit histories of job applicants reduces job-finding rates for Black job-seekers by 3 percentage points. Another work by Cortés et al. (2022) documents that the ban on using credit reports in hiring decisions leads to reductions in vacancies, especially for positions involving routine tasks. This evidence supports the idea that statistical discrimination may serve as a pathway for our upskilling effect, particularly when employers discriminate against less-skilled worker groups, who are known to have higher opioid use rates compared to more-skilled groups.

Our findings have important policy implications. First, it highlights the *distributional effects* of the opioid crisis on workers. Our findings suggest 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. Second, our research emphasizes the importance of diversifying resources beyond the prevention and treatment of opioid use, highlighting the need for interventions such as occupational training programs to mitigate the negative consequences of the opioid crisis. Finally, our findings indicate that increased local illicit drug use adversely impacts firms' revenue and sales, suggesting that employers may have strong incentives to participate in efforts aimed at mitigating opioid use disorders, not only among their employees but also in their surrounding communities. Policymakers should recognize the vital role employers can play in efforts to prevent and address the opioid crisis, potentially benefiting both employers and local communities in the long run.

The remainder of the paper proceeds as follows. Section 2 provides background on OxyContin 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 the results for the main analysis, 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 can effectively manage pain when used properly, they can be dangerous when used inappropriately due to their potential for recreational and harmful use, along with associated risks of morbidity and mortality. Moreover, opioids 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: (1) prescription opioid pills (labeled as "Commonly prescribed opioids" in Figure 1), (2) heroin, and (3) synthetic opioids excluding methadone (labeled as "Other synthetic opioids" in Figure 1). The first wave, beginning around the year 2000, witnessed deaths from prescription opioids steadily increasing until 2011 and remaining relatively stable thereafter. The second wave was characterized by a sharp increase in heroin-related deaths beginning around 2010. This surge coincided with an intervention in 2010 when Purdue Pharma released a reformulated, abuse-deterrent version of OxyContin. While this reformulation significantly reduced the misuse of the drug, it has also been linked to users switching to alternatives such as heroin. The transition from the second wave to the third, since 2013, has been marked by a surge in fatalities associated with illicitly-made fentanyl. The shift from prescription to illicit opioids, especially noticeable following the reformulation of OxyContin, has resulted in a significant and enduring increase in overdose deaths. 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, introduced by Purdue Pharma in 1996, 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, and 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 U.S. 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).

In response to growing abuse rates, Purdue Pharma introduced an 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 in 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).

Appendix Figure A1 presents national trends in the legal distribution of OxyContin and oxycodone from January 2008 to December 2012, using data from the Drug Enforcement Administration's (DEA) Automated Reports and Consolidated Ordering System (ARCOS).[9] In both panels, blue triangles represent the per capita Morphine Equivalent Dose (MED) of OxyContin, and black hollow circles indicate the per capita MED of oxycodone. In Panel (a), we focus specifically on high-dosage OxyContin (defined as 80 mg OxyContin). In Panel (b), we

[9]ARCOS records certain controlled substances from their point of manufacture through commercial distribution channels to the point of sale or distribution at the dispensing/retail level. This includes hospitals, retail pharmacies, practitioners, mid-level practitioners, and teaching institutions.

present the trends in the total distribution of OxyContin. The figures illustrate that the legal distribution of OxyContin experienced a sudden and significant decline both in levels and trends, immediately following the release of the new formula in August 2010. As shown in Panel (a), this decline was predominantly driven by reductions in high-dosage OxyContin, which is more susceptible to abuse (Janssen and Zhang, 2023). While no immediate shift in levels is observed for oxycodone distribution, there is a noticeable negative trend break following August 2010. These patterns underscore the pronounced and substantial shifts in OxyContin usage following its reformulation.

2.3 Potential Impacts on Existing Users and Firm Skill Demand

Potential impacts on existing users. The reformulation of OxyContin reduced the availability of abusable prescription opioids. However, this reformulation had unintended consequences, leading to a large shift from prescription opioids to 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).

In addition to contributing to the rise in illicit opioid-related fatalities, the shift toward illicit drugs may have substantial impacts on the existing users and the community in various ways. First, increased heroin use may increase the risk of blood-borne infections as heroin users often inject drugs and share needles and syringes. Prior studies have reported increases in hepatitis B and C infections in regions with a history of higher OxyContin use following its reformulation (Beheshti, 2019; Powell et al., 2019). Second, this shift may lead to the expansion of the illicit drug market and associated criminal activities. Recent research by Mallatt (2022) indicates a rise in heroin possession and dealers in areas where OxyContin prescriptions were more widespread. Third, the expansion of the illegal drug trade may expose a new demographic to potent illicit opioids previously unavailable through legal channels, suggesting that the impact of illicit market expansion could reach beyond existing opioid users.

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. Previous research suggests that opioid use and fatalities are more prevalent among individuals with lower levels of education and those from lower socioeconomic backgrounds (Case and Deaton, 2015; Altekruse et al., 2020). Employers may view skill levels as an indicator of potential risks associated with opioid use and overdoses. Even when a small subset of job candidates are at high risk of illicit drug use, certain firms may respond excessively to the perceived risks associated with illegal opioid use among job applicants. This reaction could be attributed to various factors, including uncertainties surrounding an applicant's potential future drug use, low tolerance for risk related to employee drug use, lack of understanding about substance abuse, and concerns about legal liabilities.

Second, employers may adjust skill requirements in response 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-skilled workers.[10] Third, the reformulation may reduce 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 fill positions more quickly, or they may choose to raise skill requirements with the aim of increasing overall productivity with a limited number of newly hired employees.

Finally, 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 changes in firm performance could affect their financial constraints and hiring strategies, potentially influencing the skill requirements for new hires.[11]

[10] The impact of increased illicit opioid use among current employees may also shape expectations for job candidates. For instance, employers could extrapolate the changes in productivity observed among current workers onto job seekers, thereby adjusting their expectations for candidates' future productivity.

[11] One possible explanation for how a firm's performance can affect skill requirements is through "recruitment intensity." Recruitment intensity is defined as a range of recruiting strategies that employers can use to influence the rate

3 Data

This section outlines the datasets we use in our analysis. First, we describe county-level data on prescription opioid use and store sales. Second, we describe our firm-level data sets—Compustat North America Database and Lightcast Online Job Posting Data—and explain how we link these datasets at the firm level.

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, excluding hospitals. We explain in Section 4 how we derive firm-level exposure to the reformulation using these data on county-level prescription opioid use in the pre-reformulation period.

Local store sales. We construct our outcome data on county-level store sales using the 2006–2019 Nielsen Retail Scanner data. These are point-of-sales data from participating retail stores across the U.S.[12] Each data entry contains barcode-level information on a product's price and sales quantities at the weekly frequency. Each entry has a store identifier and product identifier so that the pricing and sales quantities information can be linked to the store where the product is sold and to the product attributes files, which have information on the product's size and brand, among many others. Using these data, we construct county-level longitudinal data from 2006 to 2019, focusing on two outcomes: the average store-level sales within a county and the total sales across all stores

of new hires, such as changes in "advertising expenditures, screening methods, hiring standards, and the attractiveness of compensation packages" (Davis et al., 2013a). For example, in response to a sudden decrease in the quality of job seekers, firms can choose to raise their hiring standards, as they expect higher training costs and reduced returns from this pool of candidates. Recent research shows that firms actively adjust their recruitment intensity in response to economic changes. Faberman (2020) shows that aggregate recruitment intensity sharply declined during the Great Recession and that persistently low recruitment intensity partly explains the jobless recovery following the crisis. Modestino et al. (2020) find that firms lower recruitment intensity by raising education requirements for job positions in response to the expansion in the pool of labor suppliers. In the same manner, the reduction in demand for products and services and firm sales due to the opioid crisis could lower the return to hiring and recruitment intensity.

[12]The data begin in 2006, with approximately 30,000 participating stores each year until 2017. From 2017 onward, Nielsen expanded the number of participating stores to 50,000. The Nielsen data offer a comprehensive view of spending on consumer goods. As noted by Broda and Weinstein (2010) and Jaravel (2019), the product categories included in the Nielsen data align with product categories in the US Consumer Expenditure Surveys by the Bureau of Labor Statistics (BLS), representing about 40% of goods consumption and approximately one-third of total household spending. In addition, Leung (2021) demonstrates that the price indices published by the BLS can be closely replicated using the Nielsen data.

in a county. Our final sample comprises 2,422 counties.

3.2 Firm-level Data

Skill requirements. Our skill 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 claims that its database includes the near universe of online job ads. 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 job 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 that there are some limitations to our job posting data. Job vacancies in Lightcast may not accurately represent the overall employment distribution. Prior studies report that job vacancies in Lightcast are skewed toward certain areas of the economy, although this limitation also applies to other widely used job vacancy databases, such as the Job Openings and Labor Turnover Survey (JOLTS) (Lazear and Spletzer, 2012; Davis et al., 2013b). Despite this limitation in representing the employment distribution, Lightcast is known to be consistent with overall labor market trends. For example, Hershbein and Kahn (2018) show that the national and industry trends in the number of online job postings from Lightcast closely track those of employment from the Current Population Survey (CPS), Occupational Employment and Wage Statistics (OEWS), and job vacancies from JOLTS.

Lightcast uses its algorithm to develop a robust skills taxonomy. We follow Deming and Kahn (2018), who create categories of skill requirements based on the skills taxonomy in a way that the categories are useful for economic research.[13] Appendix Table A1 lists the six skill categories and

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

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. A computer skill should include keywords such as "computer," "spreadsheets," or specialized software such as "Java," "SQL," and "Python."[14] We quantify a firm's demand for a particular skill by calculating the average count of skills within that skill category required in a job posting.[15]

Employment, revenue, and capital stock. We complement job posting data by constructing measures of employment, revenue, and capital stock (Property, Plant and Equipment), obtained from the Compustat North America Database (Compustat hereafter). Compustat gathers financial statements from all 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. Our analyses focus on publicly traded companies.[16] To identify publicly traded companies within our job posting data, we link the Compustat and Lightcast databases at the firm level. Unfortunately, there is no simple way to link these two datasets because there is no common firm identifier between them. 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

[14] Our computer skill category combines general computer skills and specific software skills, categorized separately in Deming and Kahn (2018). Computer skills are basic software, such as Microsoft Excel and PowerPoint, while specific software skills include names of specialized software.

[15] If a job posting does not require such a skill, it is coded as zero. Therefore, our outcomes capture both extensive margin (whether to require at least one skill within that category in a posting) and intensive margin (how many skills to include for that skill category in a posting) responses.

[16] Limiting our focus to publicly traded companies offers two advantages. First, since Compustat data only cover publicly traded companies, focusing on these companies allows us to zoom in on those included in both Compustat and Lightcast datasets. Therefore, we can explore the impact of reformulation on a wide range of firm outcomes within the same group. Second, since job posting data lack a time-invariant company identifier, focusing on publicly traded firms facilitates better identification of the same firm. In the job posting data, there are instances where observations for the same company have varying names across years. However, publicly traded firms, on average, are typically much larger in size and have standardized names, making it easier to merge them into a single entity. As previously noted, publicly traded firms constitute one-third of U.S. employment in the non-farm business sector (Davis et al., 2006).

the Computer Intelligence Technology Database (CiTDB) to overcome this problem. CiTDB has covered 3.2 million establishments since 2010 and provides a firm structure and address for 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. 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.

Using the linked dataset, we construct firm-year level panel data on the logarithm of the following outcomes: (1) the average number of cognitive skills required in a job posting, (2) the average number of computer skills required in a job posting,[17] (3) the average years of schooling required in a job posting, (4) the average years of experience required in a job posting, (5) employment level, (6) revenue, and (7) capital stock, measured by property, plant, and equipment (PP&E). Our final firm-level sample comprises 2,202 publicly traded companies included in both databases.[18] Our sample covers the years 2007 and 2010 through 2019 for job posting outcomes, and the years 2005 through 2019 for firm outcomes from the Compustat database.

Appendix Figure A2 shows raw trends in firm-level skill requirements separately for the average number of cognitive skills, the average number of computer skills, the average years of schooling, and the average years of experience required in a job posting. In particular, we calculate the weighted average of each skill requirement across firms, where the weight is the number of job postings posted by the firm in that year. We then apply a logarithmic transformation to these averages to examine the growth rates over time. We plot the trend for the year 2007 and the years 2010 to 2019. As shown in the figure, the average skill requirement levels exhibit an upward trajectory during the period from 2007 to 2012, and they remain stable from 2013 to 2019.

[17] In addition, we create similar variables for other skills, including social, service, management, and writing skills.

[18] Since our main focus is on the labor market, we exclude firm-by-year observations with missing employment information from the Compustat database. As a result, 6.3% of the total observations are dropped.

4 Empirical Strategy

To explore the causal impact of the OxyContin reformulation on skill 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 describes our empirical models.

4.1 Measuring Exposure to the OxyContin Reformulation

County-level exposure. To quantify the causal impact of the OxyContin reformulation on skill 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 measure based on the population-weighted rate of OxyContin misuse at the state level from 2004 to 2009, calculated using data from the National Survey on Drug Use and Health (NSDUH). Since this measure is only available at the state level, we follow the approach suggested by Evans et al. (2022) to construct a pre-intervention exposure measure at the county level. 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, the county-level CDC measure of pre-intervention exposure covers all uses—both medical and non-medical—of all Schedule II prescription opioids, not limited to OxyContin.

This broader measure offers a more precise representation of local variation 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. Appendix Figure A3 indicates that when our county-level exposure measure is aggregated at the state level, it is positively correlated with Alpert et al.'s measure of initial OxyContin misuse rates.[19] Importantly, Appendix Figure A4 indicates that higher levels of the state-level CDC exposure measure are associated with greater reductions in OxyContin misuse rates from 2008 to 2012 and with larger increases in heroin mortality rates

[19] Data on the NSDUH measure of OxyContin misuse are obtained from Alpert et al. (2018).

from 2008 to 2016.[20] This supports previous evidence and further validates our CDC exposure measure.[21] In Section 5.6, we assess the robustness of our results to using the state-level sample along with Alpert et al.'s state-level measure of OxyContin misuse. Figure 2 displays geographic variation in pre-intervention per capita opioid prescriptions across counties using the CDC exposure measure.

Firm-level exposure. 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. This Bartik-style measure is constructed by calculating a weighted average of the county-level opioid exposure measure across a firm's establishments based on each establishment's physical location. 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 the pre-intervention period number of job ads at the establishment level. One implicit assumption here is that the number of job postings represents the size of establishments, which is widely accepted in the literature. Appendix Figure A5 displays the histogram of the distribution of the firm-level exposure measure in our sample. Summary statistics for this exposure measure are reported in Panel A of Table 1. On average, during the pre-intervention period from 2006 to 2009, a firm is exposed to 0.7 Schedule II opioid prescriptions per capita, with a standard deviation of 0.22.[22]

A firm-level analysis leveraging a firm-level exposure measure offers several advantages over local-level or establishment-level analyses. First, our firm-level approach allows us to distinguish between changes within firms and compositional changes across different firms.[23] This distinction is particularly important for identifying the mechanisms behind potential changes in the

[20] The NSDUH data on OxyContin misuse are obtained from Alpert et al. (2018), and the data on heroin mortality are from the National Vital Statistics System (NVSS).

[21] Similarly, Evans et al. (2022) highlights the validity of the county-level CDC exposure measure by demonstrating that counties with higher CDC opioid exposure experienced larger reductions in the rate of OxyContin misuse between 2008 and 2012, based on Alpert et al.'s state-level measure.

[22] In addition, Appendix Figure A6 presents the average exposure by industry groups. Note that our analysis focuses on within-industry across-firm comparison rather than across-industry comparison.

[23] Many prior studies have attempted to separately identify within-firm effects and compositional changes across firms as potential mechanisms in various contexts (Bloom et al., 2016; Hershbein and Kahn, 2018).

composition of the aggregate labor force. For example, if we observe that the shift towards illicit opioid use has altered the labor force composition at aggregate local levels, investigating whether this is due to within-firm adjustments (such as intentional changes in labor inputs) or shifts in the composition of firms with heterogeneous labor inputs can provide important evidence for understanding the underlying mechanisms.

Second, our firm-level exposure measure is less prone to unobserved local- or industry-level heterogeneity, thus 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 subject to unobserved regional economic shocks, our firm-level shock mitigates this concern by smoothing the exposure to these areas. Note that we also net out industry-specific concurrent shocks by directly controlling for industry-by-time fixed effects in our empirical models, as explained in Section 4.2.

Finally, our firm-level approach captures aggregate decisions related to hiring strategies. Even when a firm operates multiple establishments across regions, it typically makes critical decisions such as hiring collectively (Hazell et al., 2022). Examining changes in employment and skill requirements at the firm level offers insights into possible strategic responses by firms to the opioid crisis.

4.2 Empirical Specifications

In our firm-level analysis, we estimate the following event study specification:

$$ln(y_{fgt}) = \sum_{t=2005, t\neq 2009}^{2019} \delta_t Exposure\_Pre_f + \alpha_f + \gamma_{gt} + X'_{ft}\beta + \varepsilon_{fgt}, \tag{1}$$

where y_{fgt} denotes the outcome for firm f in industry g during year t. All outcomes are log-transformed. $Exposure\_Pre_f$ indicates the standardized pre-intervention exposure to prescription opioids at the firm level. The coefficients of interest are δ's, presenting how the effect of the reformulation on our firm-level outcomes changes over time. Since the Lightcast job posting data are unavailable for 2008 and 2009, we use 2007 as the reference period, and thus the coefficient for this year is normalized to zero. For firm outcomes from the Compustat data, we use 2009, the year prior to the reformulation, as the reference period. The industry classification is based on the three-digit NAICS code. γ_{gt} denotes industry-by-year fixed effects and controls for industry-specific time-varying confounding factors. X_{ft} denotes time-varying firm-level characteristics, including two control variables: the log of the investment tax credit obtained from the Compustat database, and the interaction of firm-level exposure to the 2008 Great Recession shock with the full set of year dummy variables.[24] Standard errors are clustered at the firm level. In our county-level analysis for store sales outcomes, we use a version of equation (1) in which we replace firm with county, omit industry-by-year fixed effects, and exclude the investment tax credit from the control variables.

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

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

$$ln(y_{fgt}) = \lambda Exposure\_Pre_f \times Post_t + \rho_f + \tau_{gt} + X'_{ft}\sigma + u_{fgt}, \tag{2}$$

where $Post_t$ is an indicator denoting 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. All the other variables are defined as in equation (2). Standard errors are clustered at the firm level.

Finally, we use a difference-in-differences specification that separately estimates the short- and medium-run effects, which takes the following form:

$$\begin{aligned} ln(y_{fgt}) = \lambda_1 Exposure\_Pre_f \times \textit{SR Post}_t + \lambda_2 Exposure\_Pre_f \times \textit{MR Post}_t \\ + \rho_f + \tau_{gt} + X'_{ft}\sigma + u_{fgt}, \end{aligned} \tag{3}$$

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

where *SR Post*$_t$ is an indicator for the first five years following reformulation, 2010 to 2014. *MR Post*$_t$ is an indicator for the subsequent years, 2015 to 2019. All the other variables are defined as in equation (2). Standard errors are clustered at the firm level.

5 Results

5.1 Effects on Firm Employment and Skill Requirements

We begin by exploring the impacts of reformulation on various labor market outcomes, including firm employment and their requirements for education, work experience, and other skills that they use to screen candidates for a job vacancy. Throughout 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 pre-reformulation period.

Effect on firms' employment levels. In Figure 3, we present our event study results for firm-level employment. Specifically, we present coefficients and associated 95 percent confidence intervals from estimation of equation (1). We observe no statistically significant evidence of pre-existing trends prior to the reformulation, providing evidence supporting the parallel trends assumption. In 2010, the year of reformulation, we observe that firm employment levels began to decrease. The effect size increased over time through 2013 and has remained stable since then. Column 1 of Panel A in Table 2 reports the average effect over the post-reformulation period from estimation of equation (2). A one standard deviation increase in exposure to the reformulation was associated with a 5.1 percent reduction in employment at the firm level. This estimate is statistically significant at the 1 percent significance level.

Effect on firms' skill requirements. Next, we examine the effect of reformulation on skill requirements using our job posting data. Figure 4 presents the raw trends in within-firm percentage changes in skill requirements in our sample, separately for low-exposure and high-exposure firms.[25] The figure shows raw trends in requirements for cognitive skills (Panel

[25] High-exposure firms are defined as those with an opioid exposure level greater than the median within each industry group, as classified by the NAICS two-digit code. For each firm, for each year, and for each skill category, we calculate the percentage change in the average number of skills required in a job posting relative to its average in 2007. For

(a)), computer skills (Panel (b)), education (Panel (b)), and work experience (Panel (d)). The figure indicates that trends in within-firm changes in the average number of cognitive and computer skill requirements were similar for both high- and low-exposure firms until 2010, but started to sharply diverge from 2011 onward. The level difference in percentage change continued to grow and then remained stable from 2013 onwards. We find suggestive evidence indicating that education and experience requirements also increased following the reformulation, although these patterns are less pronounced than those observed in the trends for cognitive and computer skills. Overall, these trends indicate that higher exposure to initial prescription opioids was closely linked to an increase in skill requirements in the post-reformulation period.

Our event study estimates presented in Figure 5 demonstrate that these patterns remain relatively consistent even with regression adjustments. Figure 5 presents the estimated effect of firm-level exposure to the OxyContin reformulation on skill requirements for each year along with 95 percent confidence intervals from estimation of equation (1). The outcome variables are the log of the number of cognitive skills (Panel (a)), the number of computer skills (Panel (b)), years of schooling (Panel (c)), and years of experience (Panel (d)) required in a job posting. Since the Lightcast job posting data are unavailable for 2008 and 2009, we use the year 2007 as the reference period, as mentioned above. The estimates in Panels (a) and (b) indicate that cognitive and computer skill requirements increased following the reformulation. The effect size generally increased through 2014 and remained relatively stable through 2019.[26]

Columns 2–5 of Table 2 provide summary estimates for these outcomes. In Panel A, we present estimates summarizing the overall effect from 2010 to 2019 from estimation of equation (2). In Panel B, we report estimates separately for the short-run post-reformulation period (2010–2014) and the medium-run period (2015–2019) from equation (3). In Panel A, we observe that a one standard deviation increase in firm-level exposure to reformulation resulted in a 7.9 percent increase in the average number of cognitive skills (Column 2) and a 5.4 percent increase in the average number of computer skills (Column 3) required in an online job posting. In Panel B, our estimates reveal that both the magnitude and statistical significance of these effects increased over time. In

example, if a firm maintains a consistent average number of cognitive skills across all years, it will have values of 100 for all observations. We then calculate the weighted average of these percentage changes across all firms in our sample using the number of job postings in that year as the weight.

[26]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 medium term, cognitive and computer skills increased by 8.3 and 7.8 percent, respectively, and these effects are statistically significant at the 5 percent level. In the last two columns of both panels, we find little evidence that reformulation led to increases or decreases in education and experience requirements.[27] Note that we do observe statistically significant effects on education and experience requirements in certain sub-groups as discussed in Section 5.4.

Why does the reformulation have such substantial effects on cognitive and computer skills, while having little impact on requirements for other skills, including education and experience? There are several plausible explanations. First, cognitive and computer skills might serve as a key screening tool during the hiring process. Prior research suggests that employers often link lower cognitive skills with greater uncertainty regarding workers' characteristics. For example, Cortés et al. (2022) find that the ban on using credit reports of job applicants in hiring decisions led firms to reduce hiring, particularly for routine job positions, which typically require lower levels of cognitive skills.

Second, firms may adjust their requirements for cognitive and computer skills because they perceive these skills as critical determinants of their performance. For instance, Deming and Kahn (2018) demonstrate that cognitive skill requirements have significant explanatory power for firm performance, and Braxton and Taska (2023) show that firms' computer skill requirements are indicative of their technological capabilities. Third, it is noteworthy that cognitive and computer skills are among the most frequently demanded skills by firms, accounting for 42% of the specific skills required in the job postings in our sample, excluding education and experience. Firms might adjust their skill requirements in response to the opioid crisis, particularly focusing on the skills they commonly require. Fourth, if companies choose to raise skill requirements for positions not requiring a college degree, it may be more feasible to adjust skill levels other than education, rather than suddenly imposing a college degree requirement. Finally, if salaries for more-educated or more-experienced job candidates are substantially higher, firms might choose to adjust other skill criteria as a cost-efficient strategy for screening candidates.

[27] In Appendix Figure A7, we also present our event study estimates for the following skills: social skills (Panel (a)), service skills (Panel (b)), management skills (Panel (c)), and writing skills (Panel (d)). Our analysis reveals little evidence suggesting that the reformulation is linked to an increase in requirements for these skills.

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

Next, we examine how the transition toward illicit drugs affected local store sales, firm revenue, and capital stock. We first explore the effect on local store sales. Figure 6 presents the coefficients and 95% confidence intervals on the interactions between county-level initial exposure to prescription opioids and the indicators for each of the years before and after the 2010 OxyContin reformulation from equation (1). The year 2009, which is the 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 being statistically indistinguishable from zero. Starting in 2011, the first fully treated year, there was a gradual decrease in local store sales in higher-exposed counties. The effect size increased over time through 2017, and they remained 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 of Panel A in Table 3 report the average effect on local sales over the post-reformulation period from equation (2). Over the entire post-period from 2010 to 2019, a 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 was 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 1 percent level. Columns 1 and 2 of Panel B report the average effects on local store sales over the short- and medium-run periods from equation (3). Our estimates indicate that these effects were more pronounced in both magnitude and statistical significance in the medium-run, from 2015 to 2019, compared to the short-run effects.

We then investigate how reformulation affected firm revenue and capital stock. Figure 7 presents our event study estimates for the log of firm revenue in Panel (a) and the log of firm capital stock (i.e., property, plant, and equipment) in Panel (b). In Panel (a), there is little evidence of a pre-existing trend in revenue, 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 began to decline in higher-exposed firms. The magnitude of the effect increased through 2013 and then remained stable through 2019.

We find very similar result patterns for capital stock in Panel (b), although there is suggestive evidence of a decreasing trend in capital stock during the last two pre-reformulation years. 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 them as strong evidence against any substantial increase in capital stock resulting from the reformulation. Investigating whether the opioid crisis led to an increase in capital stock is particularly important as it provides insights into understanding firm investment choices. Ouimet et al. (2020) show that firms increase technology investment to replace relatively scarce labor with capital. In contrast, we do not find any evidence of 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.

We report the summary estimates for revenue and capital stock in columns 3 and 4 in Table 3. Our estimates in Panel A indicate that a one standard deviation increase in exposure to reformulation resulted in a 4.6 percent decrease in firm revenue (column 3) and a 4.2 percent decrease in capital stock over the entire post-reformulation period, with both effects significant at the 5 percent level. Panel B suggests that the magnitude of these effects increased over time.

5.3 Composition Analysis

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 positions, or to screening, reflecting increased skill requirements within specific occupational categories. If the composition channel dominates, we would expect to see an increase in demand for higher-skilled positions in the post-reformulation period. Alternatively, if the screening channel dominates, an upskilling trend within the same occupational categories would be observed, suggesting that employers are enhancing skill requirements for similar roles in the post-period.

In Appendix Figure A8, we address this question by conducting a heterogeneity analysis by occupation group. First, we divide occupations into three groups: (1) non-routine manual jobs, (2) routine manual and routine cognitive jobs (routine jobs), and (3) non-routine cognitive jobs, representing low, middle, and high-skilled positions, respectively.[28] Next, we estimate equation (2) for each of these groups separately. The outcome variables include the share of job postings allocated to each occupation group (top left panel), the average number of cognitive skills required in a job posting (top middle panel), the average number of computer skills (top right panel), the average years of schooling (bottom left panel), and the average years of experience required in a job posting (bottom right panel), with each outcome calculated at the firm-by-year level. While many coefficients for these sub-groups are statistically indistinguishable from zero, our analysis reveals no evidence supporting the notion that a single mechanism solely explains our upskilling effect.

5.4 Heterogeneity Analysis

We explore the heterogeneous impacts of the OxyContin reformulation on skill requirements across firm and labor market characteristics, including firm size, firm education requirements, local minimum wage levels, and local employment protection levels. First, we explore heterogeneity by firm size by estimating equation (2) separately for small-, mid-, and large-sized firms.[29] In particular, we present coefficients and associated 95% confidence intervals for each sub-group in Appendix Figure A9. Overall, we find no consistent evidence of differential impacts across middle- and large-sized firms.

Next, we explore heterogeneity by firm education requirements. We divide firms into two groups within each sector based on whether the share of job postings requiring a four-year college degree measured at baseline is larger or smaller than the median.[30] In Appendix Figure A10, we present

[28]Following Acemoglu and Autor (2011), we first classify occupations into non-routine manual, routine manual, routine cognitive, and non-routine cognitive jobs. Along the wage spectrum, non-routine manual jobs are considered low-skilled, routine manual and routine cognitive jobs are considered middle-skilled, and non-routine cognitive jobs are considered high-skilled. Considering that non-routine cognitive jobs account for roughly half of the job ads, we reclassify them into three groups: non-routine manual jobs, routine jobs, and non-routine cognitive jobs.

[29]We categorize firms into three groups within each sector (utility, manufacturing, service, IT, finance, and professional service) based on the average total assets measured in the last three years prior to the reformulation.

[30]Given that job posting outcomes are missing for a subset of our firms in 2007, we use 2010 data to calculate baseline education requirements for consistency. The average share of job postings requiring a four-year college degree in 2010 was 14.5% in the low-requirement group and 59.6% in the high-requirement group, respectively, after weighting observations by the number of job postings for the firm in 2010. We find similar results when calculating baseline education requirements based on the average across 2007 and 2010, rather than exclusively from 2010.

coefficients and associated 95% confidence intervals from equation (2). We find that upskilling effects are much larger among firms with a lower proportion of job postings requiring a four-year college degree at baseline. This is consistent with previous research suggesting that fatal opioid overdoses are more prevalent among less-educated individuals (Case and Deaton, 2015; Altekruse et al., 2020). One potential explanation is that firms relying on less-educated labor experience larger increases in illicit opioid use among employees and job candidates, leading them to raise their skill requirements more substantially to screen job candidates or enhance worker productivity.

Third, we explore heterogeneity in effects across firms in areas with low, medium, and high minimum wage levels.[31] Figure 8 presents coefficients and 95% confidence intervals from estimation of equation (2) for each group. We observe that the upskilling effects are notably larger for firms in states with higher minimum wage levels. Importantly, firms in the high minimum wage group show a large and statistically significant increase in requirements not only for cognitive and computer skills but also for years of schooling. 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 on skill requirements across firms in areas with low, medium, and high employment protection levels in Figure 9.[32] We find similar heterogeneity results as in the analysis by minimum wage levels. We observe that, for most outcomes, the upskilling effects are greater for firms in states with stringent regulatory frameworks.

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 exposure to reformulation led to a 1.6%

[31] We use state minimum wages provided by the US Department of Labor. Using the minimum wages averaged over the last three years prior to the reformulation, we construct the firm-level average minimum wage based on the geographic variation of a firm's establishments.

[32] We use a state-level employment protection score constructed by Oxfarm, a non-governmental organization. The data are available at: `https://www.oxfamamerica.org/explore/countries/united-states/poverty-in-the-us/best-states-to-work/` (accessed May 2024). 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 the weighted average of state-level scores based on the geographic distribution of a firm's establishments. We then classify firms into three groups based on this firm-level score.

reduction in local store sales and a 5.1% 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 the labor market and firm outcomes is beneficial for an 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.[33] Our estimated 5.1 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 potential reallocation of the lowered employment towards self-employed and 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 (moving from the 25th to the 75th percentile) leads to a 1.7 percent reduction in sales at the establishment level. We find that a one standard deviation increase in firm-level exposure to reformulation resulted in an estimated 1.6 percent reduction in local store sales, which is aligned closely with the magnitude of their findings.

5.6 Robustness Analysis

We test the robustness of our results by considering potential alternative explanations, using a different policy exposure measure, and constructing alternative samples.

Half-year level sample. A key concern in our study is a potential confounder that affects both initial opioid use and skill requirements. To mitigate this concern, we conduct our regressions using a more granular, half-yearly panel dataset instead of our yearly panel dataset. Estimates from yearly and half-yearly samples may not be directly comparable because some firms' hiring decisions may be made on an annual basis, making it challenging to compare skill requirements between the first and second halves. Nevertheless, this analysis is useful as it allows for a more rigorous assessment of whether there is any pre-existing trend in our outcomes prior to the reformulation. Figure 10

[33] A one standard deviation difference in their state-level exposure to reformulation, measured by pre-reformulation rate of OxyContin misuse, is 0.23. As explained above, 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, is equal to 0.22.

presents coefficients and 95% confidence intervals from estimation of a version of equation (1) in which the time unit is a half-year. The first half of 2010, the last pre-reformulation period, is used as the reference period. We find that our estimates for cognitive skills remain statistically significant. Although the estimated effects for computer skills are smaller both in size and statistical significance when using half-yearly data, we still observe a comparable pattern of results.[34] Most importantly, we find no evidence of a pre-existing trend using this more granular dataset for any of our skill outcomes. The effect on skill requirements only began to emerge in the latter half of 2010, coinciding with the reformulation period. This reassures us that our results are unlikely to be influenced by other confounding factors that might have impacted skill demand before the reformulation of OxyContin.

Alternative explanation: Great Recession. The 2008 economic downturn, known as the Great Recession, is a key potential confounding factor due to its timing and profound economic consequences. We address this concern in three ways. First, as described in Section 4, we directly control for the dynamic effect of the Grade Recession in our regressions. Second, we observe a small correlation of 0.083 between the recession shock and the reformulation shock, mitigating the concerns regarding this confounder. Lastly, our half-year level analysis indicates no pre-existing trends in the pre-period, suggesting that the Great Recession is unlikely to be the primary driver of our results.

Firm-by-state level sample. One might also argue that our firm-level exposure to reformulation captures other concurrent firm-level confounding factors. To ensure this is not the case, we conduct a firm-by-state level analysis in which we aggregate establishments owned by the same firm within a given state as an integrated entity.[35] For this analysis, we control for firm-by-year fixed effects to account for any firm-specific time-varying shocks. The idea is to investigate whether entities located in states with higher exposure to reformulation experienced greater changes in skill requirements compared to other entities under the same firm but located in states with lower exposure, even after

[34]In contrast, our findings regarding education and experience requirements are less stable across analyses. While our baseline analysis reveals no evidence of increases in requirements for education and experience, we find suggestive evidence of an increase in experience requirements in our half-year analysis. Furthermore, our firm-by-state level analysis indicates evidence of increases in requirements for both education and experience, as discussed below.

[35]In our baseline analysis, we aggregate all establishments operated by the same firm across multiple states into a single firm entity.

accounting for firm-specific time-varying factors.[36] Specifically, we run a version of equation (1) that replaces firm fixed effects with firm-by-state fixed effects (i.e., integrated entity fixed effects) and additionally controls for firm-by-year fixed effects. For opioid exposure measure, we use state-level information on initial OxyContin misuse measure following Alpert et al. (2018), which is further described below. Each entity-year observation is weighted by the share of job postings from the entity out of the total postings in that firm for that year.

Appendix Figure A11 presents the event-study results from our baseline analysis in dark blue circles and the results from this entity-level analysis in light blue hollow circles. Although the point estimates are slightly smaller in magnitude compared to our baseline estimates, we find statistically significant evidence that reformulation led to increases in cognitive and computer skill requirements at the entity level, even after ruling out any potential firm-level concurrent shocks. In contrast to our baseline findings indicating little effect on education and experience requirements at the firm level, this analysis suggests that entities in high-exposure states increase their education and experience requirements following reformulation.

Alternative policy exposure measure. For the robustness analysis using the firm-by-state sample discussed above, 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 for OxyContin misuse. As mentioned above, we observe statistically significant increases in cognitive and computer skill requirements using this alternative measure, as shown in Appendix Figure A11.

Dropping Appalachian states. Another concern might be that shocks associated with the decline of the coal mining industry may confound our estimates. As shown in Figure 2, opioid prescription rates are particularly high in the Appalachian area, comprising 423 counties across 13 states spanning from southern New York to northern Mississippi. Moreover, coal mining began to decline in these areas in 2008, with coal production falling by 50% between 2008 and 2019.[37]

This decline in coal production may have contributed to increases in both opioid use and

[36] 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).

[37] Data are available at: https://www.eia.gov/outlooks/steo/data/browser/ (accessed April 2024).

unemployment. We explore how results change when we exclude these regions from our analysis. Appendix Figure A12 demonstrates that our results from the state-level analysis, as described above, remain relatively stable even after excluding the 13 Appalachian states. This suggests that our results cannot be solely explained by the decline of the mining industry in Appalachian states.

Using a 4-digit industry code. We test the robustness of our upskilling results to replacing 3-digit industry-by-year fixed effects with 4-digit industry-by-year fixed effects. In Appendix Figure A13, we present event study estimates from equation (1) using two specifications: our baseline specification with industry-by-year fixed effects based on 3-digit industry code (dark blue circles), and an alternative specification with industry-by-year fixed effects based on 4-digit industry code (hollow circles in light blue). Overall, our upskilling effects are much larger and stronger when using the 4-digit industry code, suggesting that our estimates provide a more conservative measure of the effect on job skill requirements.

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 OxyContin reformulation. For instance, the supply of less-skilled or young workers, who may be more affected by the transition to illicit opioids, could decline more. Consequently, firms may reduce the number of job ads for less-skilled positions, resulting in increases in equilibrium skill requirements 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 responses to the reformulation. 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 use 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.

In Appendix Table A2 (Appendix Table A3), 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. Although coefficients for computer skills become statistically insignificant in some of the regressions, the point estimates are similar across all these regressions for both cognitive and computer skills. Overall, this analysis suggests that our results are not likely to be entirely driven by labor supply responses to the opioid crisis.

Alternative explanation: Retiring baby boomers. Another possible alternative explanation is the potential confounding impact of the change in the age structure. Around 2011, there was a sharp increase in the growth rate of the share of the population aged over 65 due to the retirement of the large Baby Boom birth cohorts (Maestas et al., 2023).[38] This change in the age structure may have wide-reaching impacts on firm performance, local consumption, and the labor market. For instance, this increase in the retiring population might have reduced local labor supply and employment levels. The reduction in elderly labor supply and the change in the age structure of the labor force could eventually impact companies' hiring strategies.

To address this concern, we investigate the correlation between exposure to the reformulation and two measures: the elderly population share in the pre-reformulation period and changes in the elderly population share between 2010 and 2015. Panel (a) of Appendix Figure A14 presents a scatter plot in which each observation represents the population-weighted average opioid prescriptions per capita in a given county between 2006 and 2009—our measure for county-level exposure to the reformulation—on the x-axis, and the population-weighted average share of the population over age 65 in that county during the same period on the y-axis. We do not observe any notable correlation between these two variables.

In Panel (b) of Appendix Figure A14, we further examine the correlation between exposure to the OxyContin reformulation and the elderly share growth rate from 2010 to 2015. As emphasized in Maestas et al. (2023), we observe that the majority of counties experienced an

[38] Maestas et al. (2023) document an increase in the older population share by more than 20% between 2010 and 2020 and that this was associated with lower capita GDP.

increase in the elderly population during this period, as most observations lie above the horizontal dashed line representing zero change. Once again, we do not witness any meaningful correlation between elderly share growth and exposure to the OxyContin reformulation, assuring that our results are not likely to be driven by the potential confounding effect of the age structure change.

6 Conclusion and Policy Implications

The opioid overdose epidemic is a crisis of both health and economic dimensions in the United States. To measure the 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 bears the costs of the crisis, quantifying the magnitude of the burden, and designing policy interventions to tackle these challenges.

In this study, we study the impact of a large transition toward illicit opioids caused by the OxyContin reformulation on employers' skill requirements for new hires. Using comprehensive firm-level longitudinal data, we find that increased illicit opioid use led to increased requirements for cognitive and computer skills.

Our findings have several 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. Previous research suggests that the toll of the opioid crisis is particularly heavy on individuals with lower socioeconomic status and those with lower levels of educational attainment (Case and Deaton, 2015; Altekruse et al., 2020). Our study highlights that even less-skilled workers without a history of opioid use disorders can be affected by the crisis due to these increased 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 may be 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. Firms might benefit themselves in the longer term by contributing to efforts to combat the opioid crisis. In fact, employers are uniquely positioned to play a pivotal role in preventing and treating opioid use disorder. About 60 percent of adults who report past-year opioid misuse are currently employed (SAMHSA, 2021), and the workplace is a significant part of employees' daily lives. 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—especially illicitly-made fentanyl—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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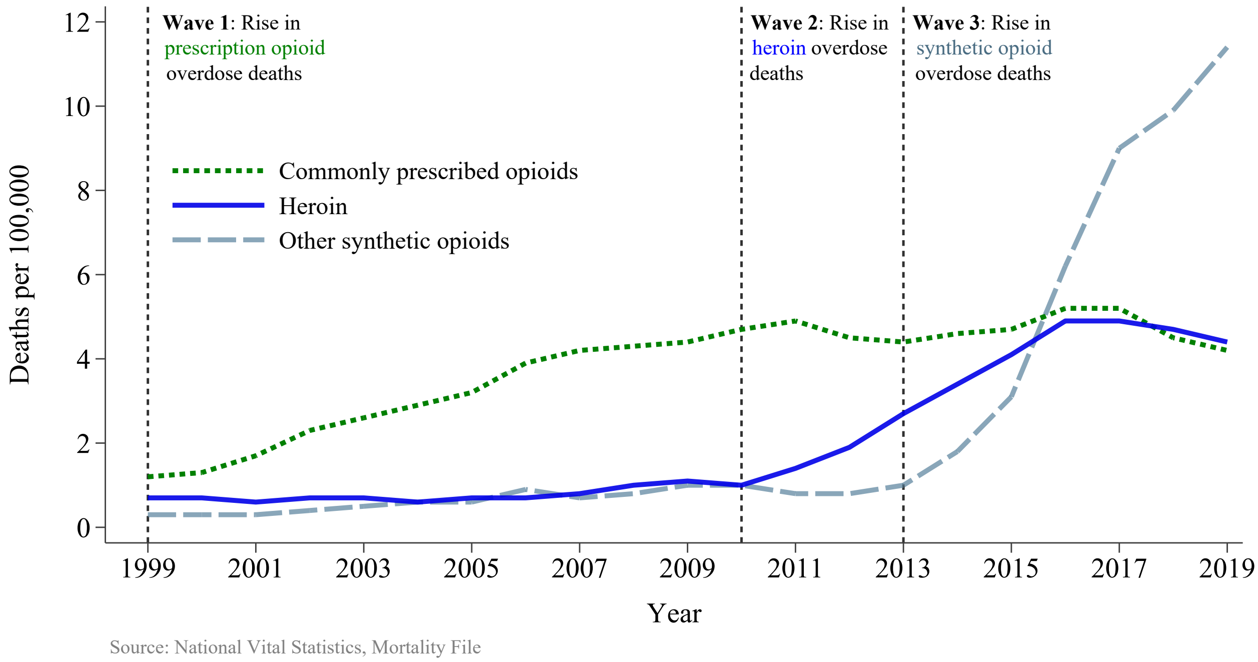
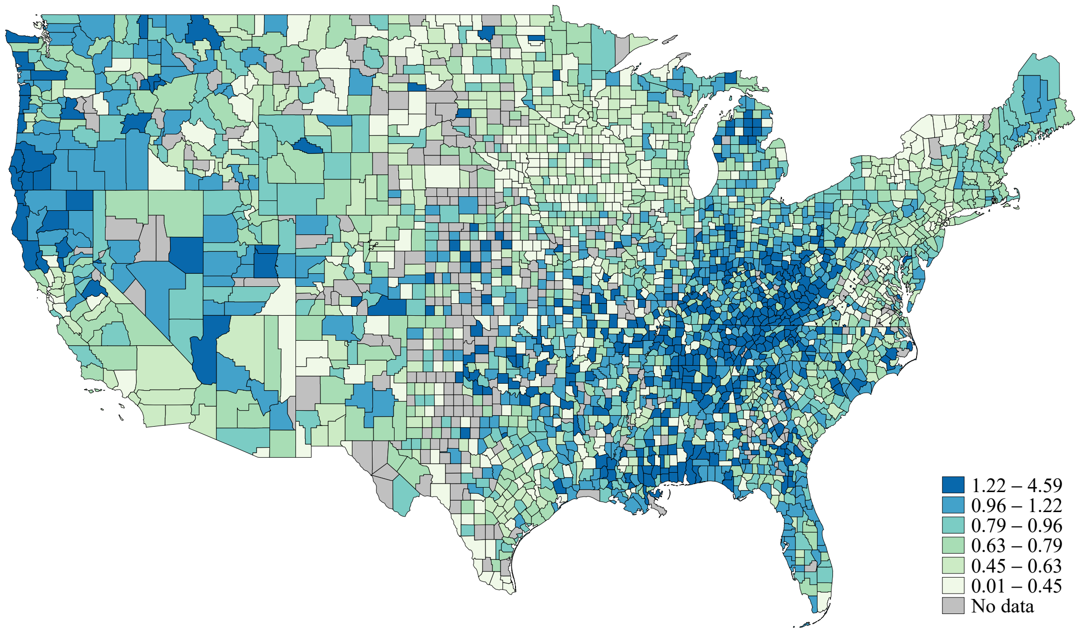
7 Figures and Tables

Figure 1: National Trends in Opioid Mortality



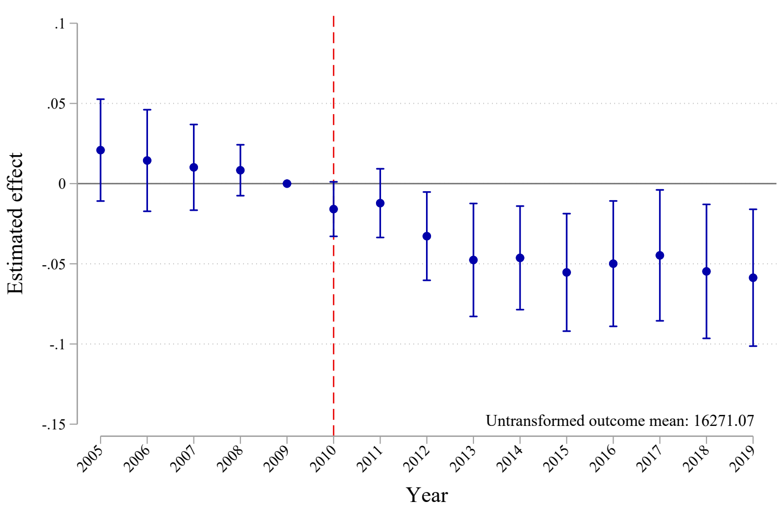
Notes: The figure displays the national trends in opioid overdose deaths per 100,000 from 1999–2019. Data on opioid overdose deaths are from the Centers for Disease Control (CDC).

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



Notes: This figure presents the geographic distribution of pre-intervention per capita opioid prescriptions across US counties. We calculate the population-weighted mean number of all Schedule II opioid prescriptions per capita in each county for the years 2006 to 2009, obtained from the Centers for Disease Control (CDC).

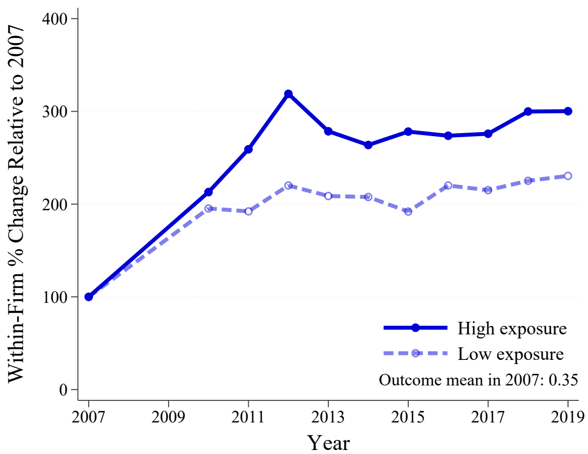
Figure 3: Effects of the OxyContin Reformulation on Firm Employment



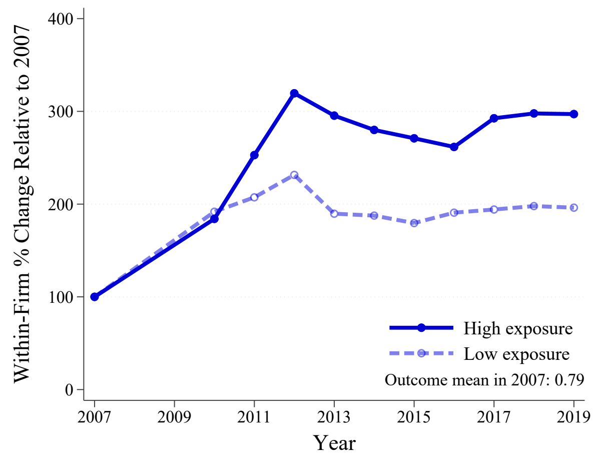
Notes: The figure shows the effect of firm-level exposure to the OxyContin reformulation on the log of employment. The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). The year 2009, which is one year prior to the OxyContin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

Figure 4: Raw Trends in Within-Firm Changes in Skill Requirements Across High- and Low-Exposure Firms

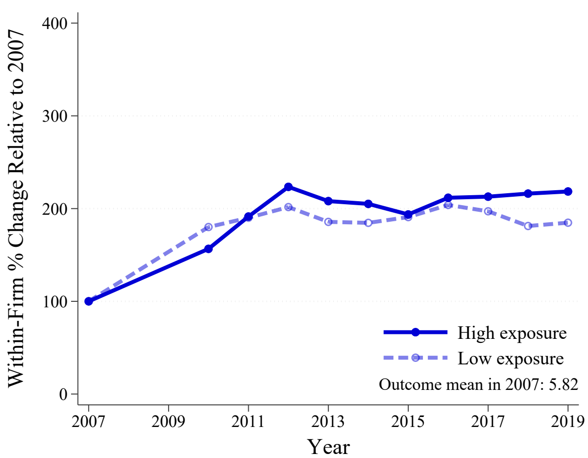
(a) % Change in Number of Cognitive Skills



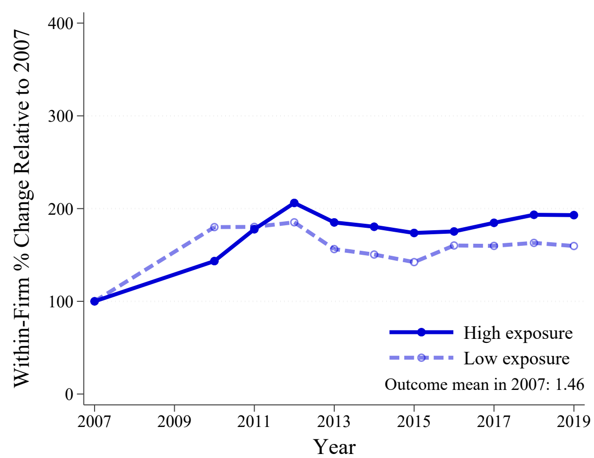
(b) % Change in Number of Computer Skills



(c) % Change in Years of Schooling

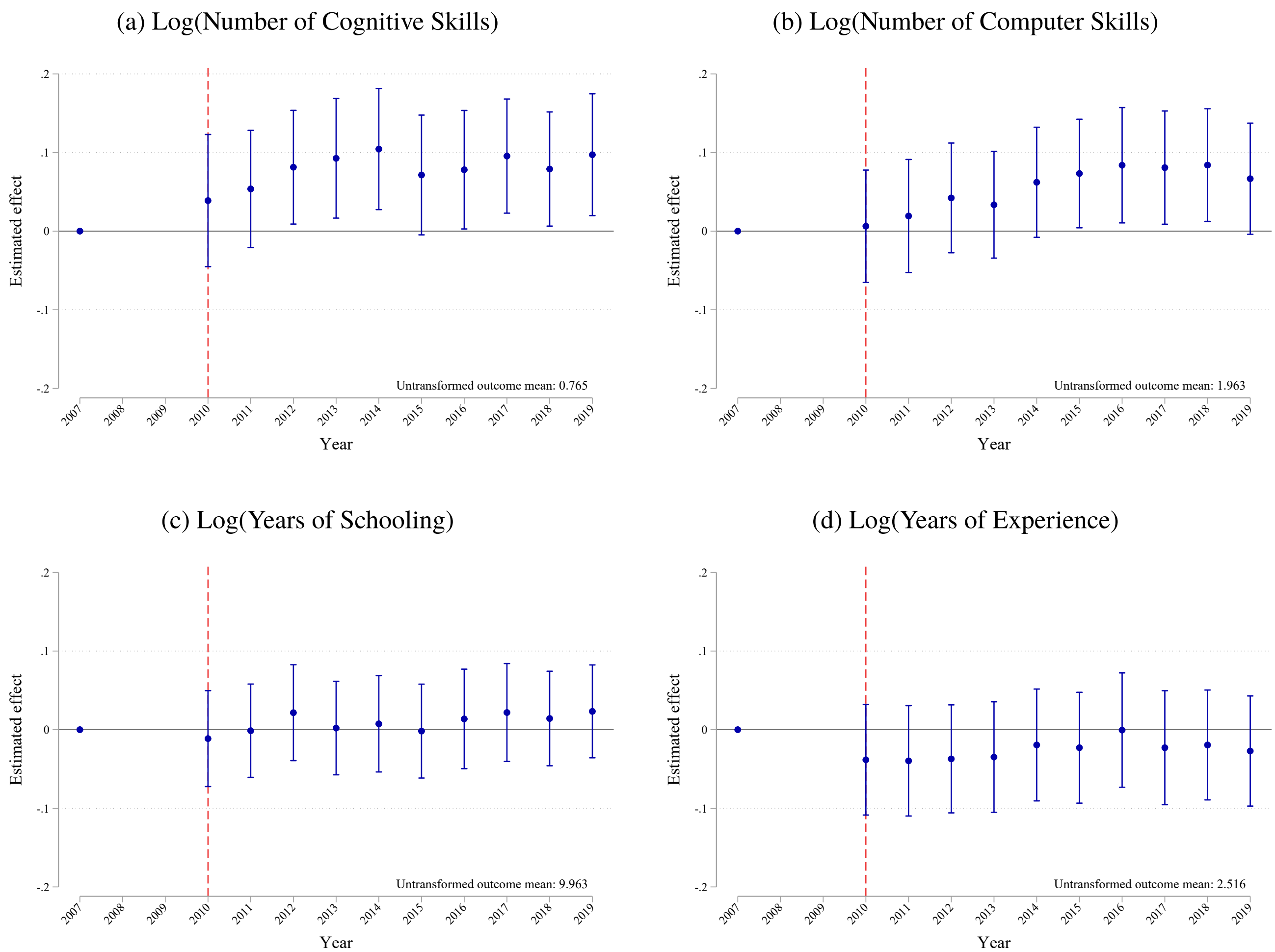


(d) % Change in Years of Experience



Notes: The figure presents raw trends in within-firm changes in skill requirements across high- and low-exposure firms. For each firm, for each year, and for each skill category, we measure the within-firm percentage change in the average number of skills required in a job posting relative to 2007. We then calculate the weighted average of these percentage changes across firms separately for high- and low-exposure groups, using the number of job postings in that year as the weight. High-exposure firms are defined as those with an exposure measure larger than the median in each industry group based on the NAICS 2-digit code. The figure displays trends for cognitive skills (Panel (a)), computer skills (Panel (b)), years of schooling (Panel (c)), and years of experience (Panel (d)).

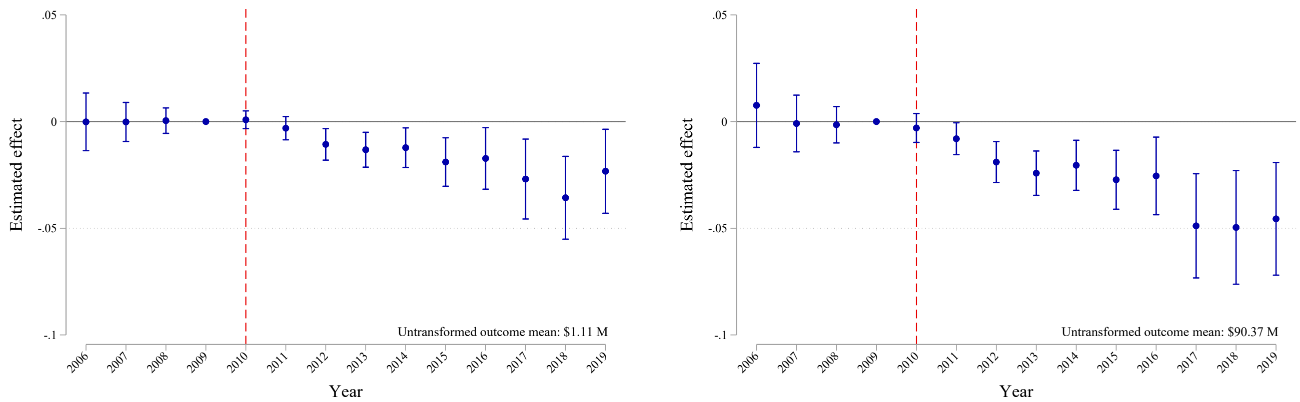
Figure 5: Effects of the OxyContin Reformulation on Firm Skill Requirements



Notes: The figure shows the impact of firm-level exposure to the OxyContin reformulation on the following outcomes: average number of cognitive skills (Panel (a)), average number of computer skills (Panel (b)), average years of schooling (Panel (c)), and average years of experience (Panel (d)) required in a job posting. All outcomes are log-transformed. The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). The year 2007 is set as the reference point and normalized to zero. Standard errors are clustered at the firm level. Untransformed outcome means are calculated based on the pre-reformulation period.

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

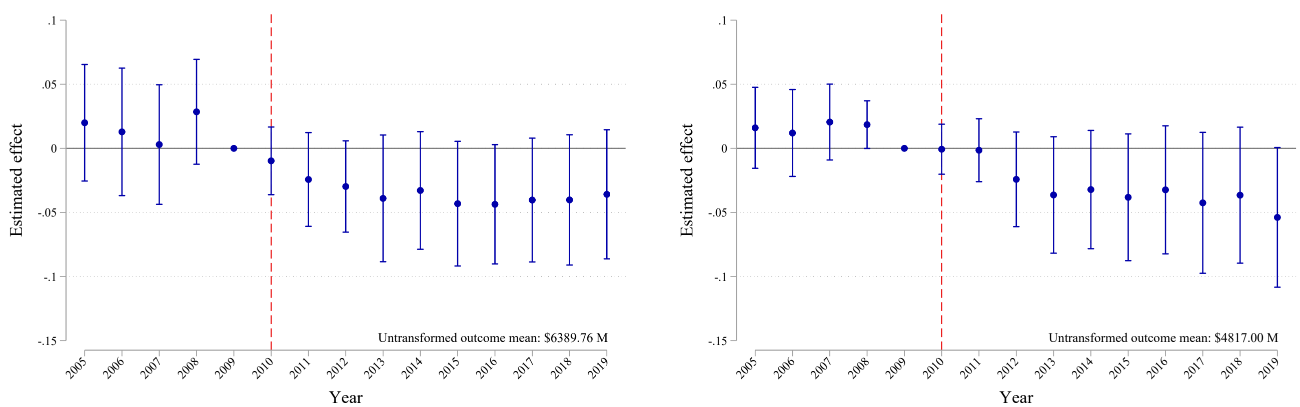
(a) Log(Average Local Store Sales) (b) Log(Total Local Store Sales)



Notes: The figure shows the effect of county-level exposure to the OxyContin reformulation on local store sales measured at the county and year levels. All outcomes are log-transformed. 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 county level. Untransformed outcome means are calculated based on the pre-reformulation period.

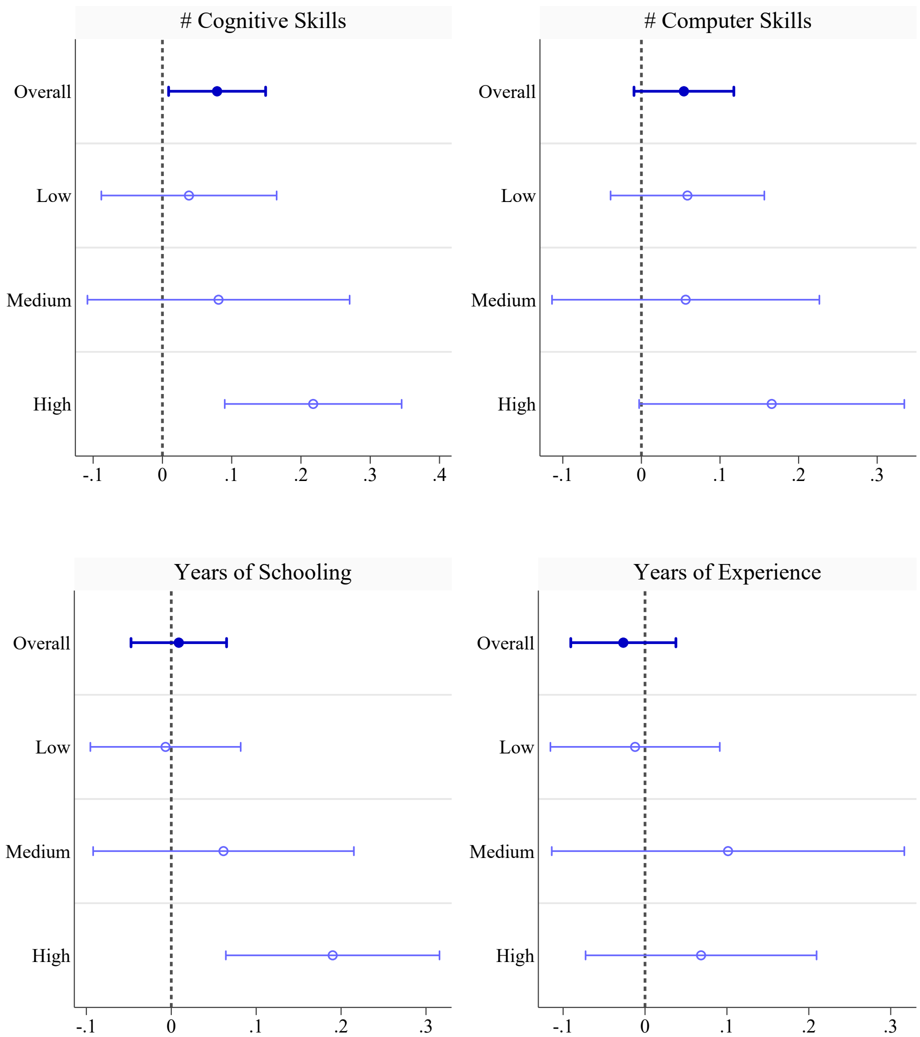
Figure 7: Effects of the OxyContin Reformulation on Revenue and Capital Stock

(a) Log(Revenue) (b) Log(Property, Plant, and Equipment)



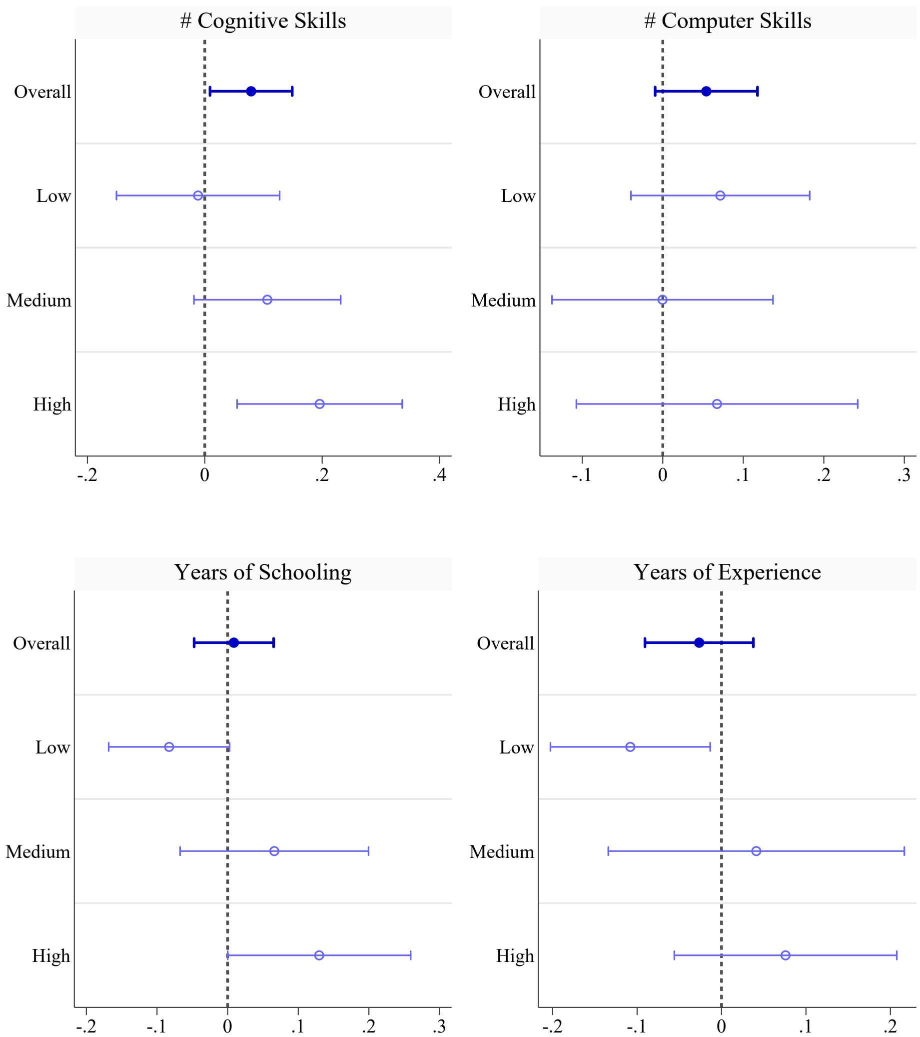
Notes: The figure shows the effect of the OxyContin reformulation on firm revenue and capital stock. All outcomes are log-transformed. The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). The year 2009, which is one year prior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

Figure 8: Effects of the OxyContin Reformulation on Skill Requirements: Heterogeneity by Minimum Wage Levels



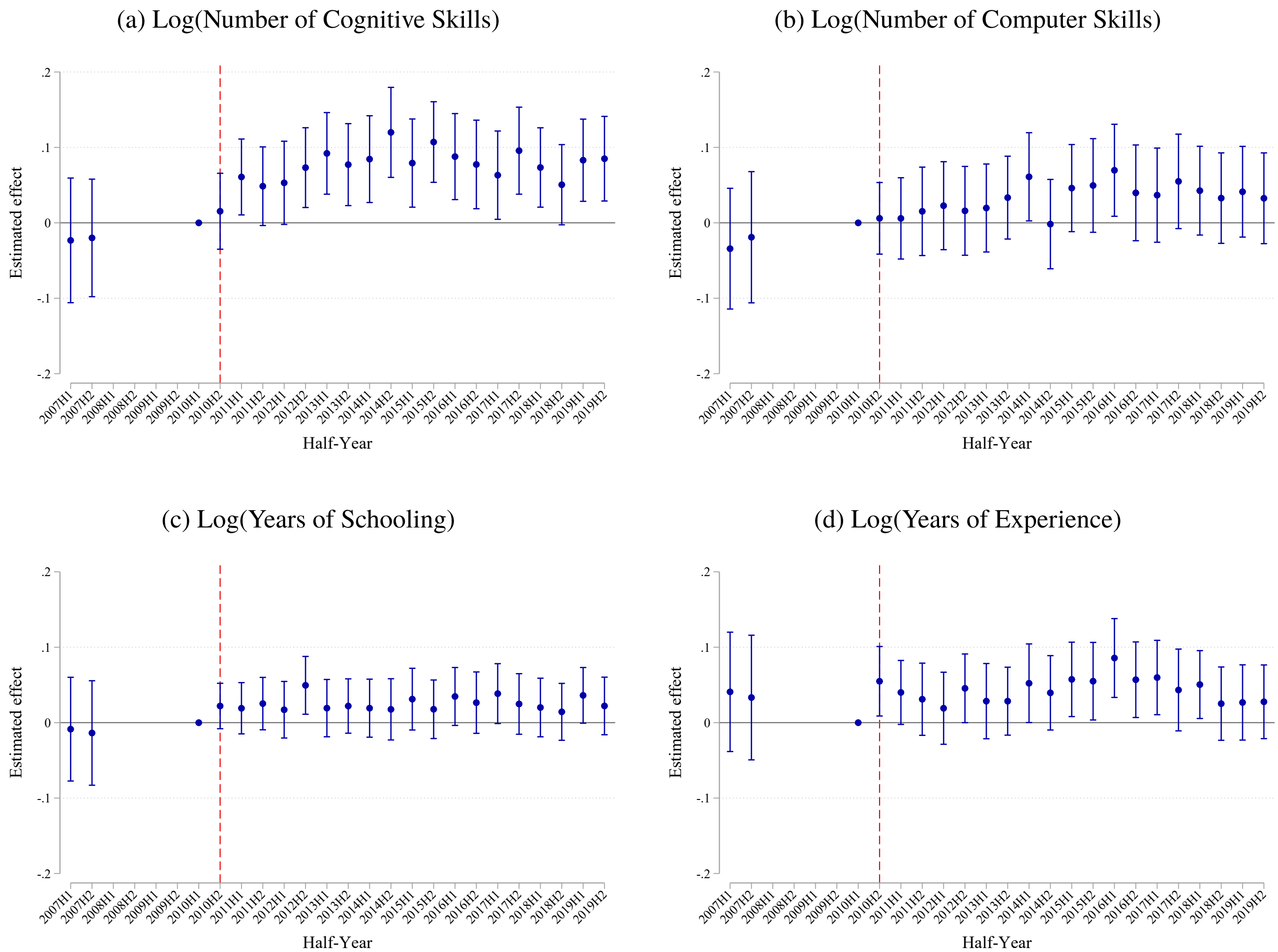
Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (2) separately for the sub-group denoted on the y-axis. Our baseline estimates are displayed at the top of each panel. Standard errors are clustered at the firm level.

Figure 9: Effects of the OxyContin Reformulation on Skill Requirements: Heterogeneity by Employment Protection Levels



Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (2) separately for the sub-group denoted on the y-axis. Our baseline estimates are displayed at the top of each panel. Standard errors are clustered at the firm level.

Figure 10: Examining Pre-existing Trends Using Half-yearly Data



Notes: The figure shows the impact of firm-level exposure to the OxyContin reformulation on job skill requirements using half-yearly data. The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1) for the following outcomes: average number of cognitive skills (Panel (a)), average number of computer skills (Panel (b)), average years of schooling (Panel (c)), and average years of experience (Panel (d)) required in a job posting. All outcomes are log-transformed. The first half of 2010, which is one period 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 for Firm-Level Outcomes

	Mean	SD	25th percentile	75th percentile	Firm-year Observations
A. Pre-Intervention Rate of Prescription Opioid Use, 2006–2009					
Per capita opioid prescriptions	0.706	0.217	0.557	0.819	27,086
B. Firm Outcomes, 2005–2019					
Employment	16,409	45,570	412	10,933	27,086
Revenue (millions)	6,416	21,120	144	3,719	27,086
Property, Plant and Equip. (millions)	4,830	19,837	10	2,008	27,086
C. Firm Skill Requirements in a Job Posting, 2007, 2010–2019					
Number of job postings	1,630	6,087	15	745	19,930
Number of cognitive skills	0.705	0.515	0.321	1.000	18,606
Number of computer skills	1.854	1.581	0.768	2.505	18,606
Years of schooling	9.505	4.239	6.875	12.552	18,606
Years of experience	2.393	1.540	1.279	3.326	18,606

Notes: This table presents the means of the firm-level exposure measure and the main firm outcomes in our analysis. Panel A presents the average firm-level exposure to 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 2010 to 2019.

Table 2: Effects of OxyContin Reformulation on Employment and Skill Requirements

	Employment	Cognitive Skill	Computer Skill	Schooling Years	Experience Years
	(1)	(2)	(3)	(4)	(5)
Panel A: Average Effect					
Post-reformulation (2010–2019)	-0.051***	0.079**	0.054*	0.009	-0.026
	(0.017)	(0.036)	(0.032)	(0.029)	(0.033)
	[0.002]	[0.027]	[0.095]	[0.758]	[0.419]
Panel B: Short- and Medium-Run Effects					
Short-run post-reformulation (2010–2014)	-0.042***	0.075**	0.032	0.003	-0.034
	(0.014)	(0.036)	(0.033)	(0.029)	(0.033)
	[0.004]	[0.040]	[0.322]	[0.915]	[0.301]
Medium-run post-reformulation (2015–2019)	-0.062***	0.083**	0.078**	0.015	-0.018
	(0.021)	(0.036)	(0.034)	(0.029)	(0.034)
	[0.003]	[0.021]	[0.021]	[0.609]	[0.591]
Sample unit	Firm	Firm	Firm	Firm	Firm
Data	Compustat	Lightcast	Lightcast	Lightcast	Lightcast
Outcome	Log	Log	Log	Log	Log
Untransformed outcome mean	16271.07	0.765	1.963	9.963	2.516
Unit-year observations	26,494	16,954	17,425	17,583	17,551

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 3-digit NAICS code. Standard errors are clustered at the firm level. Untransformed outcome means are calculated based on the pre-reformulation period. *p<0.1; **p<0.05; ***p<0.01.

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

	Avg. Sales (1)	Total Sales (2)	Revenue (3)	Capital Stock (4)
Panel A: Average Effect				
Post-reformulation (2010–2019)	-0.016***	-0.028***	-0.046**	-0.042**
	(0.006)	(0.008)	(0.020)	(0.019)
	[0.010]	[0.001]	[0.020]	[0.029]
Panel B: Short- and Medium-Run Effects				
Short-run post-reformulation (2010–2014)	-0.005	-0.014**	-0.040**	-0.033**
	(0.005)	(0.006)	(0.018)	(0.016)
	[0.268]	[0.026]	[0.028]	[0.045]
Medium-run post-reformulation (2015–2019)	-0.027***	-0.042***	-0.053**	-0.053**
	(0.008)	(0.011)	(0.024)	(0.025)
	[0.001]	[0.000]	[0.030]	[0.035]
Sample unit	County	County	Firm	Firm
Data	Nielsen	Nielsen	Compustat	Compustat
Outcome	Log	Log	Log	Log
Untransformed outcome mean	$1.11 M	$90.37 M	$6389.76 M	$4817.00 M
Unit-year observations	32,763	32,763	26,938	26,938

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 3-digit NAICS code. Standard errors are clustered at the firm level. Untransformed outcome means are calculated based on the pre-reformulation period. *p<0.1; **p<0.05; ***p<0.01.

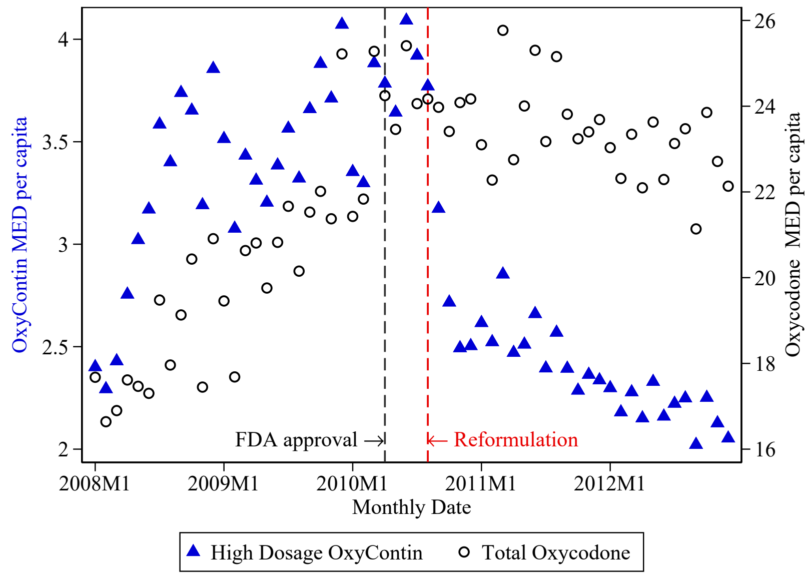
For Online Publication

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

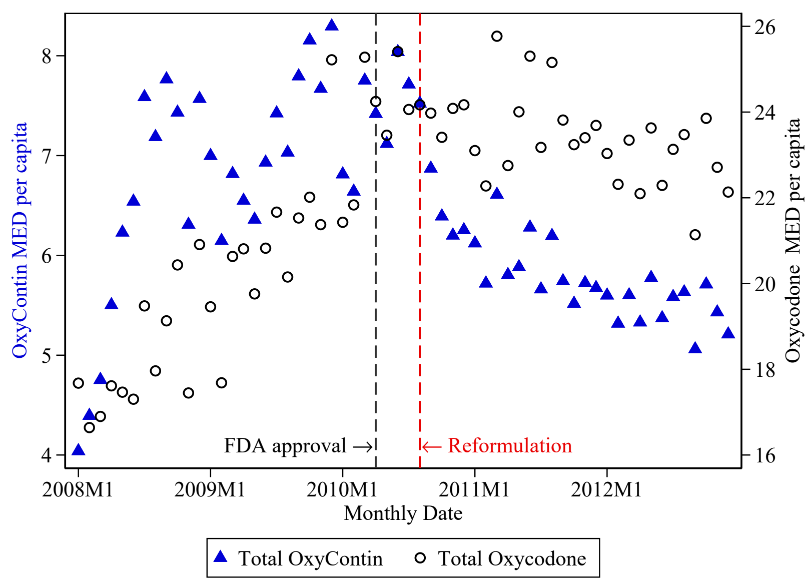
Kim, Kim, and Park (2024)

Figure A1: National Trends in Legal Distribution of OxyContin and Oxycodone

(a) High Dosage OxyContin and Total Oxycodone

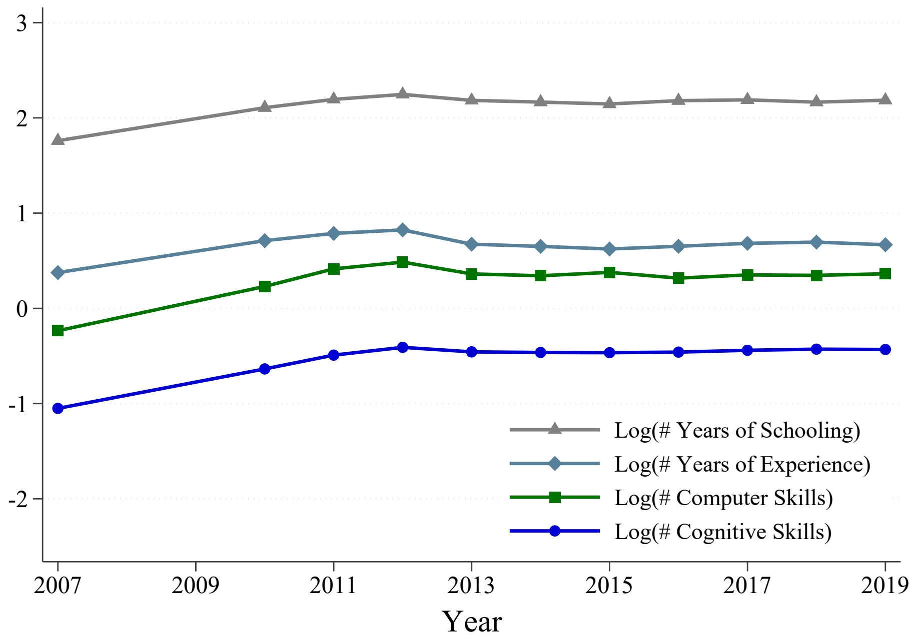


(b) Total OxyContin and Total Oxycodone



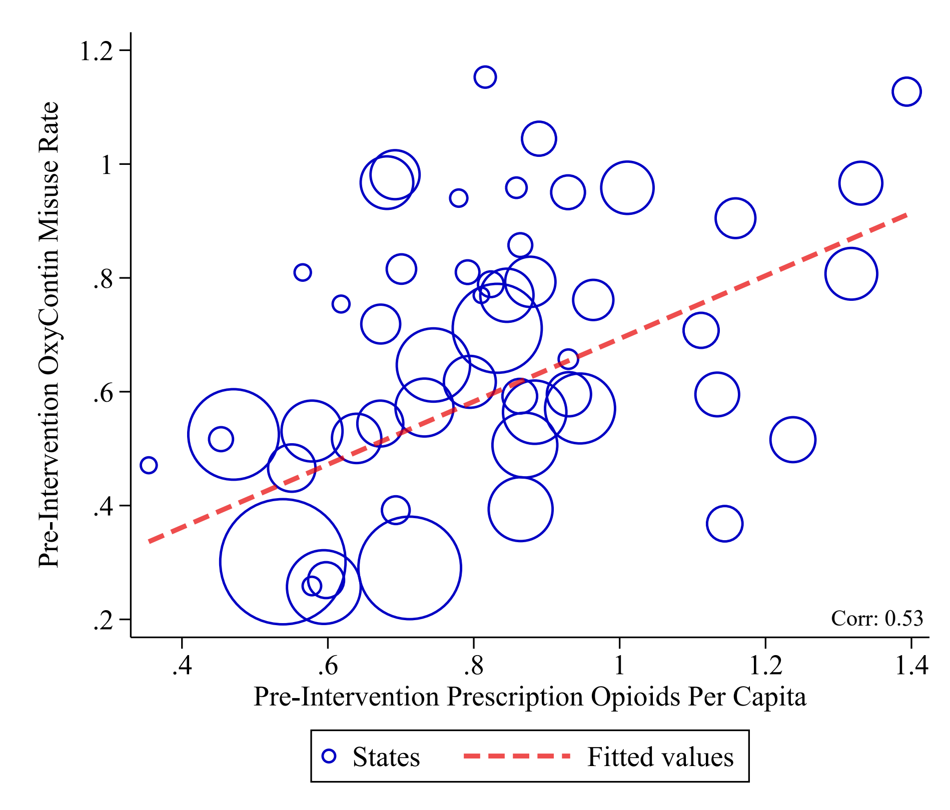
Notes: The figure presents national trends in the legal distribution of OxyContin and oxycodone from 2008 to 2012, based on the DEA's ARCOS data. Blue triangles represent the per capita Morphine Equivalent Dose (MED) of OxyContin, and black hollow circles indicate the per capita MED of oxycodone. In Panel (a), we focus specifically on high-dosage OxyContin (80 mg), which is more susceptible to abuse. In Panel (b), we present the trends in the total supply of OxyContin. The black dashed vertical line indicates April 2010, when the FDA approved the new OxyContin formula. The red dashed vertical line indicates August 2010, when the new formula was released and replaced the original version of OxyContin.

Figure A2: Raw Trends in Skill Requirements



Notes: This figure presents trends in average skill requirements in each job posting in our sample between 2007 and 2010–2019. The trends for education requirements are marked by gray triangles, experience requirements by light blue diamonds, computer skills by green squares, and cognitive skills by blue dots. To compute these, we first construct a firm-by-year panel on the average skill requirements in a job posting. We then calculate the weighted average of skill requirements across firms, using the weight based on the number of job postings by the firm in that year. Lastly, we apply a logarithmic transformation to these averages to examine the growth rates over time.

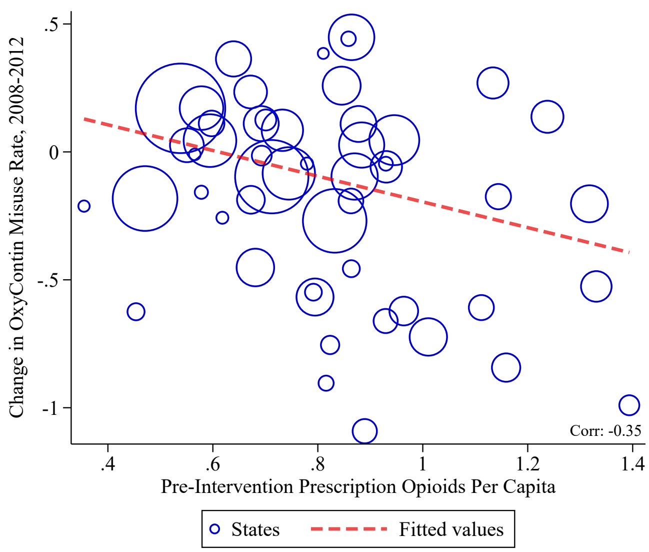
Figure A3: Relationship Between 2006–2009 Prescription Opioid Use Measure (CDC) and 2004–2009 OxyContin Misuse Measure (NSDUH)



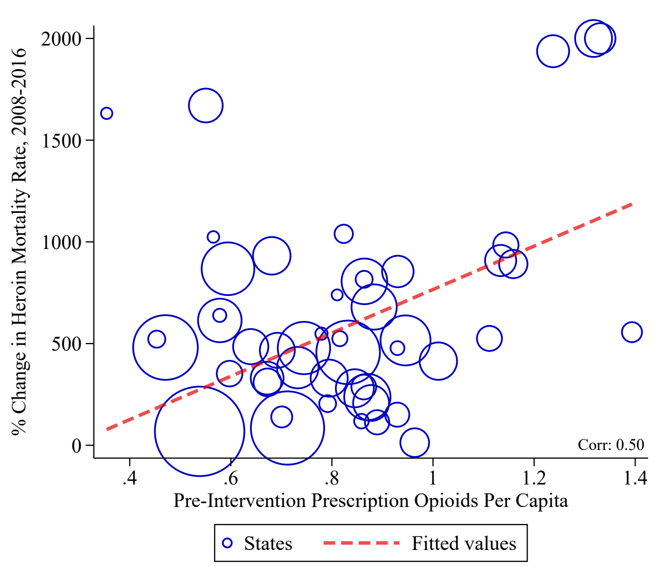
Notes: This figure presents the relationship between the state-level average of opioid prescriptions per capita from 2006 to 2009, obtained using CDC data, and the state-level average of OxyContin misuse rates from 2004 to 2008, calculated using the National Survey on Drug Use and Health (NSDUH) data (Alpert et al.'s measure). Data on the NSDUH measure of OxyContin misuse are obtained from Alpert et al. (2018). To construct the state-level CDC measure of opioid prescription use, we calculate the population-weighted average of our county-level opioid exposure measure. The size of the markers indicates the population size of each state as of 2009.

Figure A4: Relationship Between 2006–2009 Prescription Opioid Use Measure (CDC) and Changes in OxyContin Misuse and Changes in Heroin Mortality

(a) Pre-Intervention Exposure and Changes in OxyContin Misuse

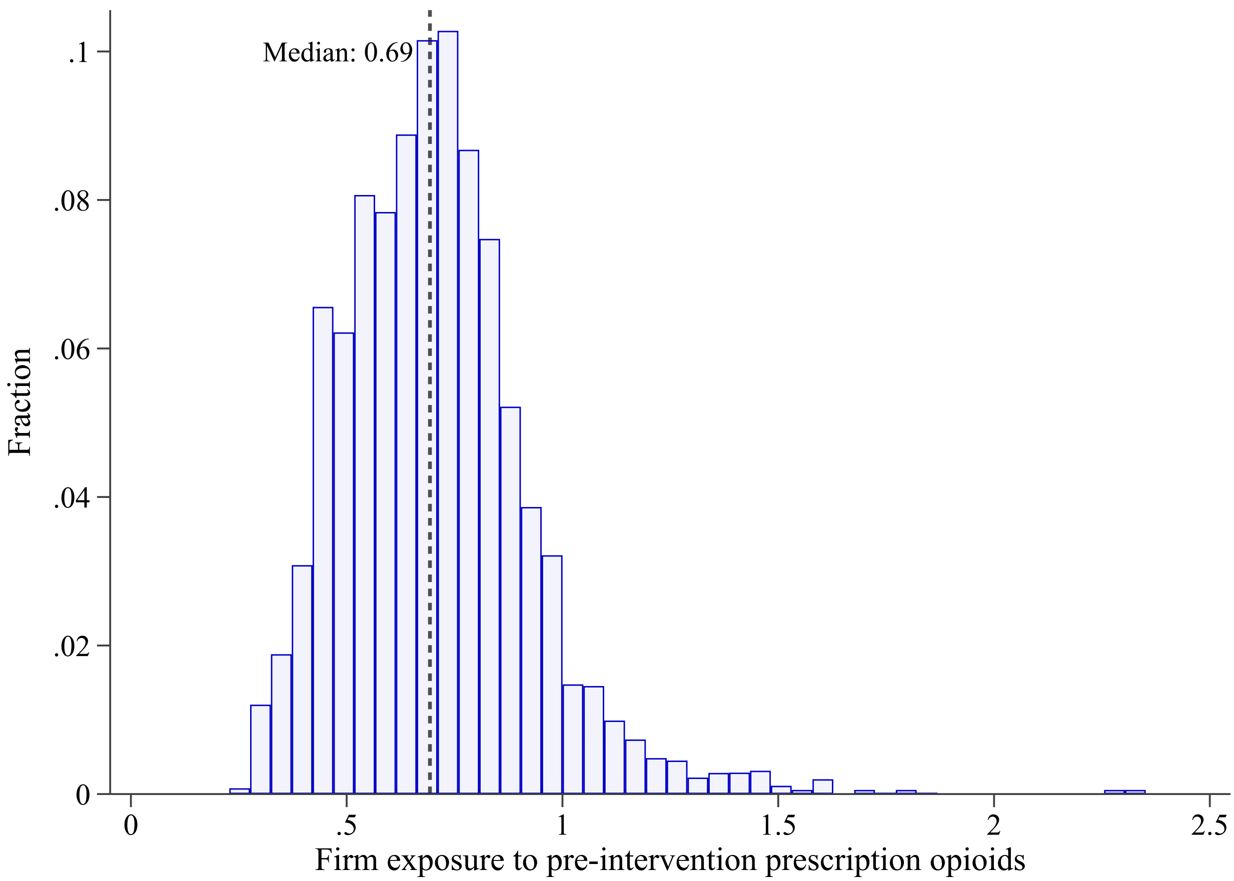


(b) Pre-Intervention Exposure and Changes in Heroin Mortality



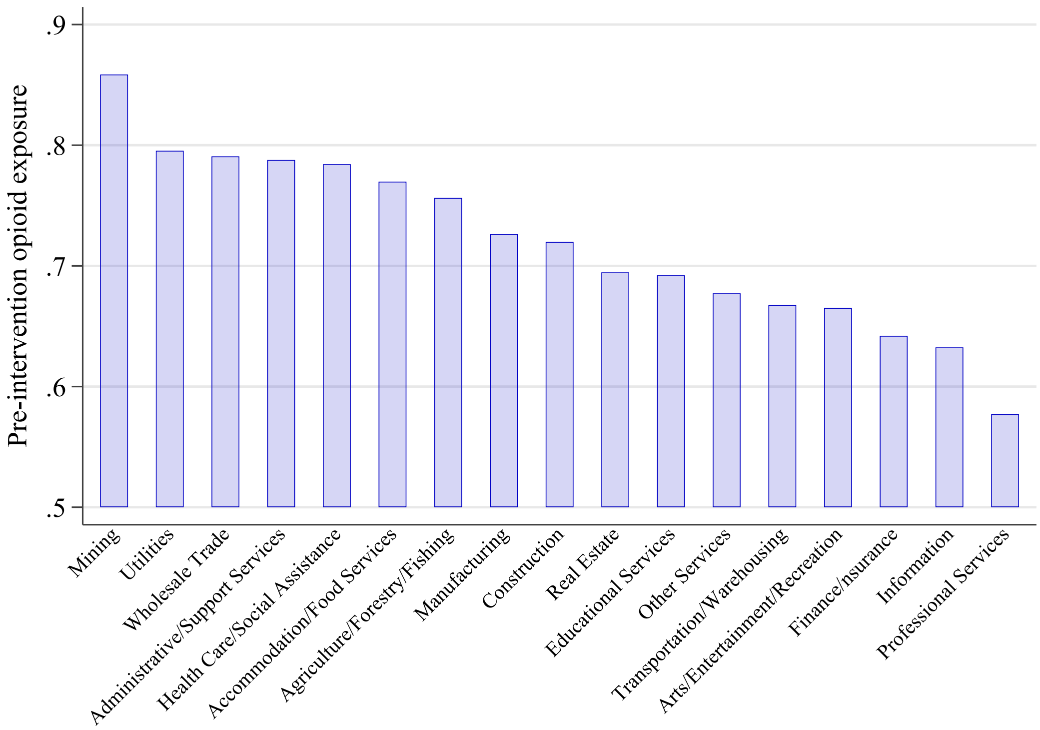
Notes: The figure presents the state-level averages of opioid prescriptions per capita from 2006 to 2009 (CDC data) and subsequent changes. Panel (a) shows the relationship between the CDC measure and level changes in OxyContin misuse rates between 2008 and 2012, and Panel (b) presents the relationship between the same CDC measure and percentage changes in heroin death rates per 100,000 from 2008 to 2016. The NSDUH data on OxyContin misuse is obtained from Alpert et al. (2018), and the data on heroin mortality are obtained from the National Vital Statistics System (NVSS). The percentage change in mortality is winsorized at 2000 percent. The size of the markers indicates the population size of each state as of 2009.

Figure A5: 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.

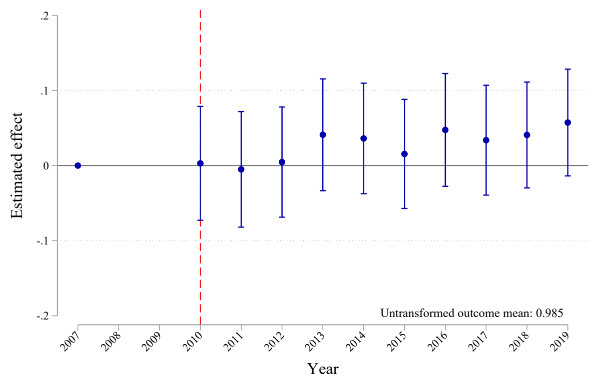
Figure A6: Firm Exposure to Pre-Intervention Prescription Opioids by Industry



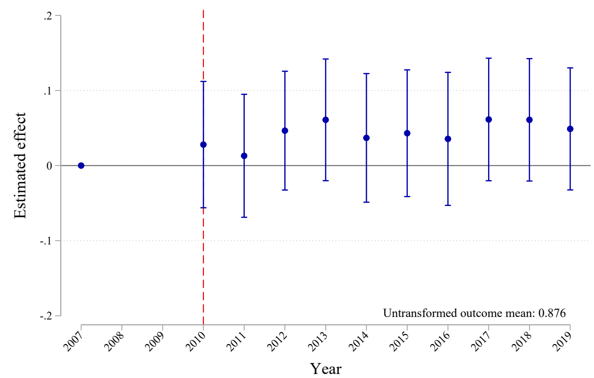
Notes: This figure presents the distribution of a firm's exposure to OxyContin reformulation across industry groups in our analysis sample.

Figure A7: Effects of the OxyContin Reformulation on Skill Requirements: Other Skills

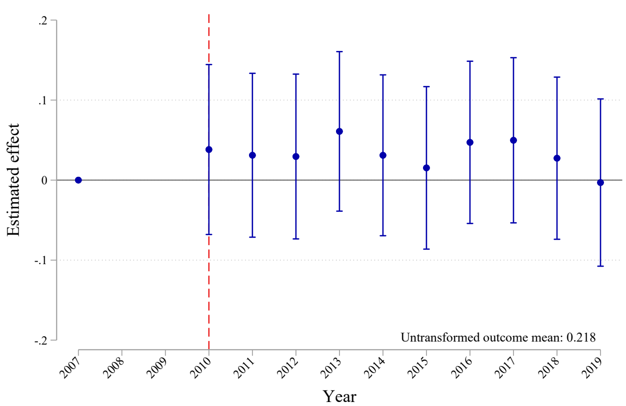
(a) Log(Number of Social Skills)



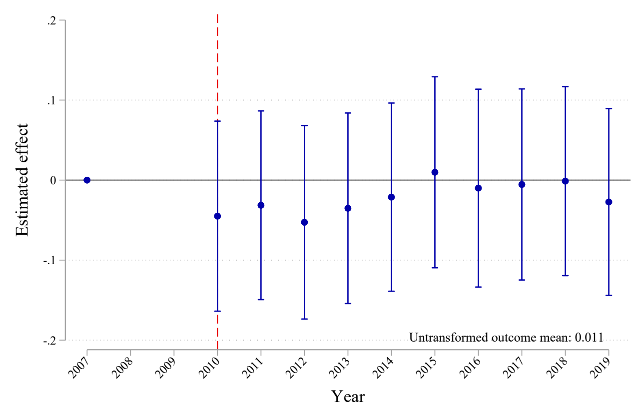
(b) Log(Number of Service Skills)



(c) Log(Number of Management Skills)

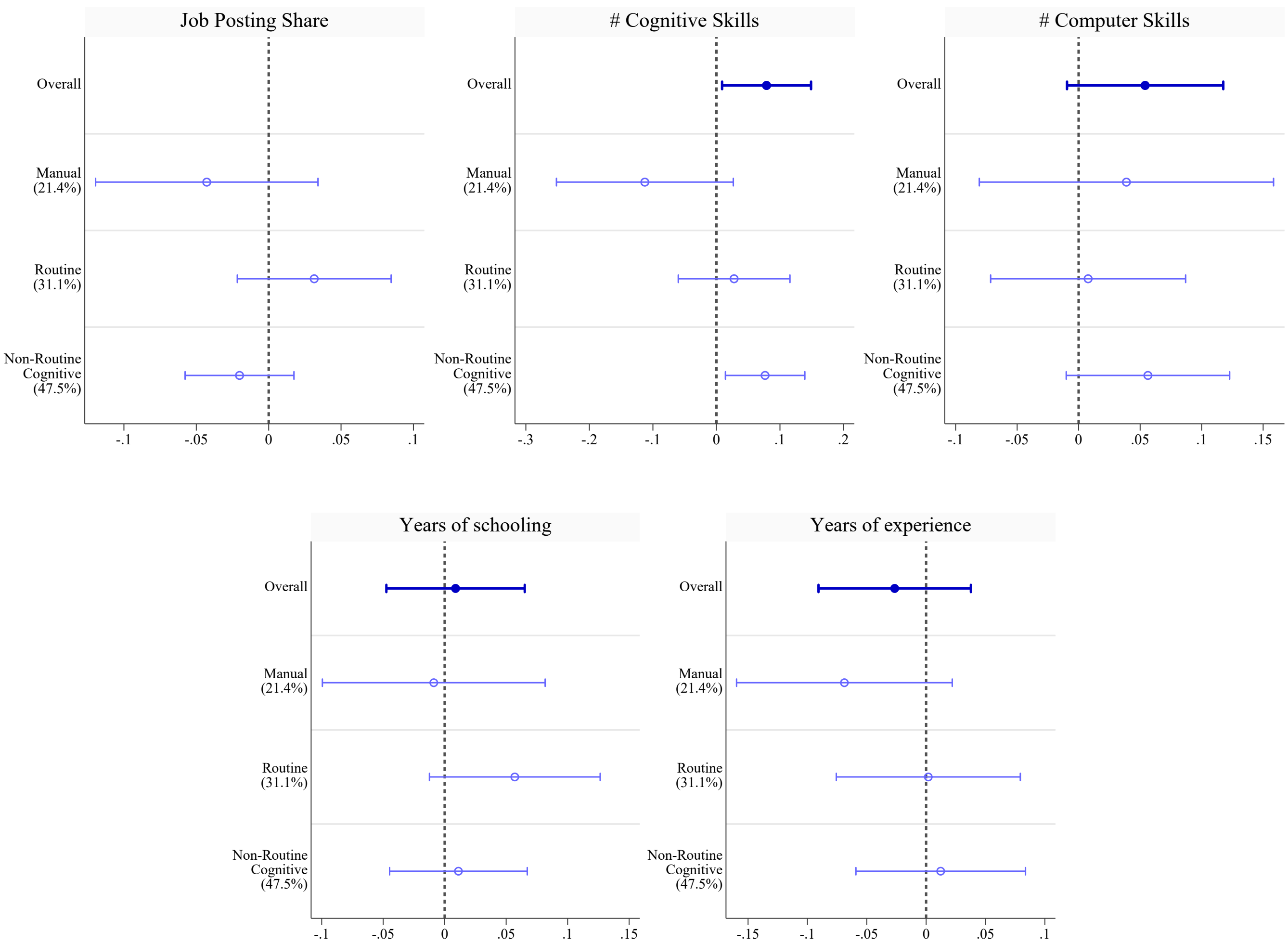


(d) Log(Number of Writing Skills)



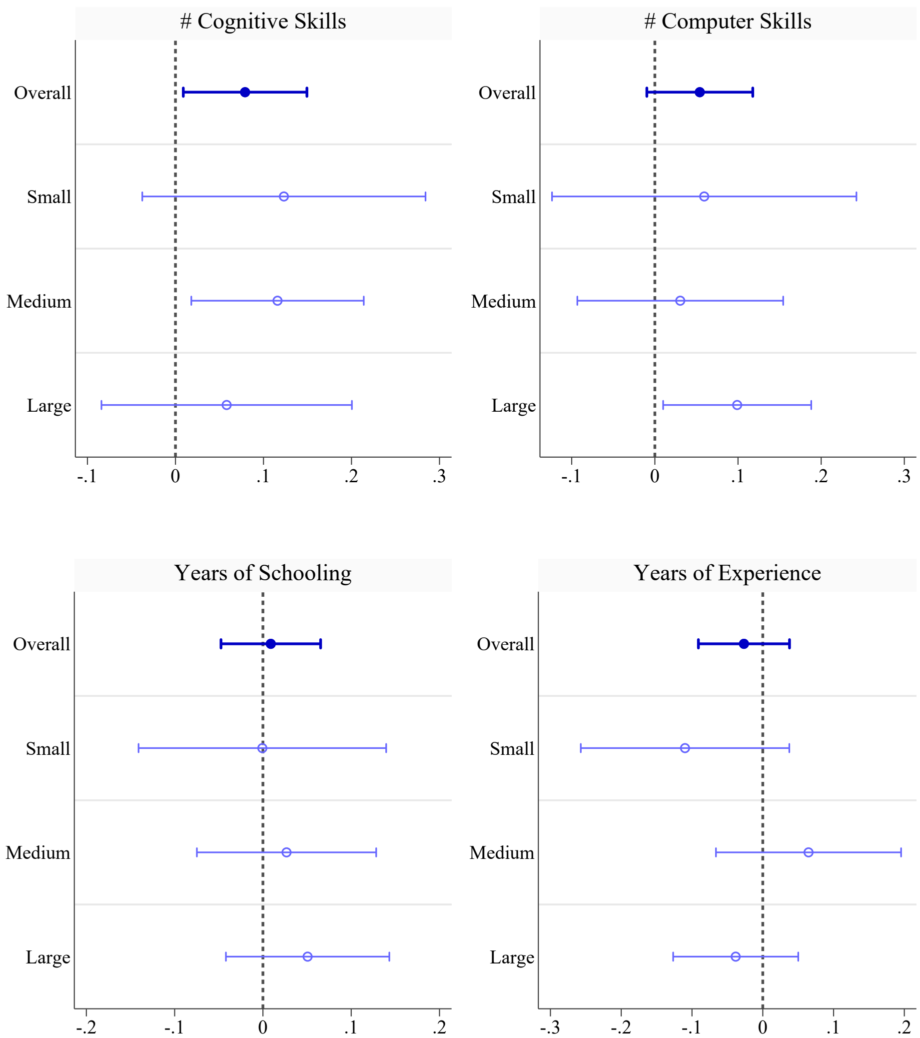
Notes: The figure shows the impact of firm-level exposure to the OxyContin reformulation on the following outcomes: average number of social skills (Panel (a)), average number of service skills (Panel (b)), average number of management skills (Panel (c)), and average number of writing skills (Panel (d)). All outcomes are log-transformed. The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (1). The year 2007 is set as the reference point and normalized to zero. Standard errors are clustered at the firm level. Untransformed outcome means are calculated based on the pre-reformulation period.

Figure A8: Effects of the OxyContin Reformulation on Job Posting Share and Skill Requirements: Heterogeneity by Occupation Group



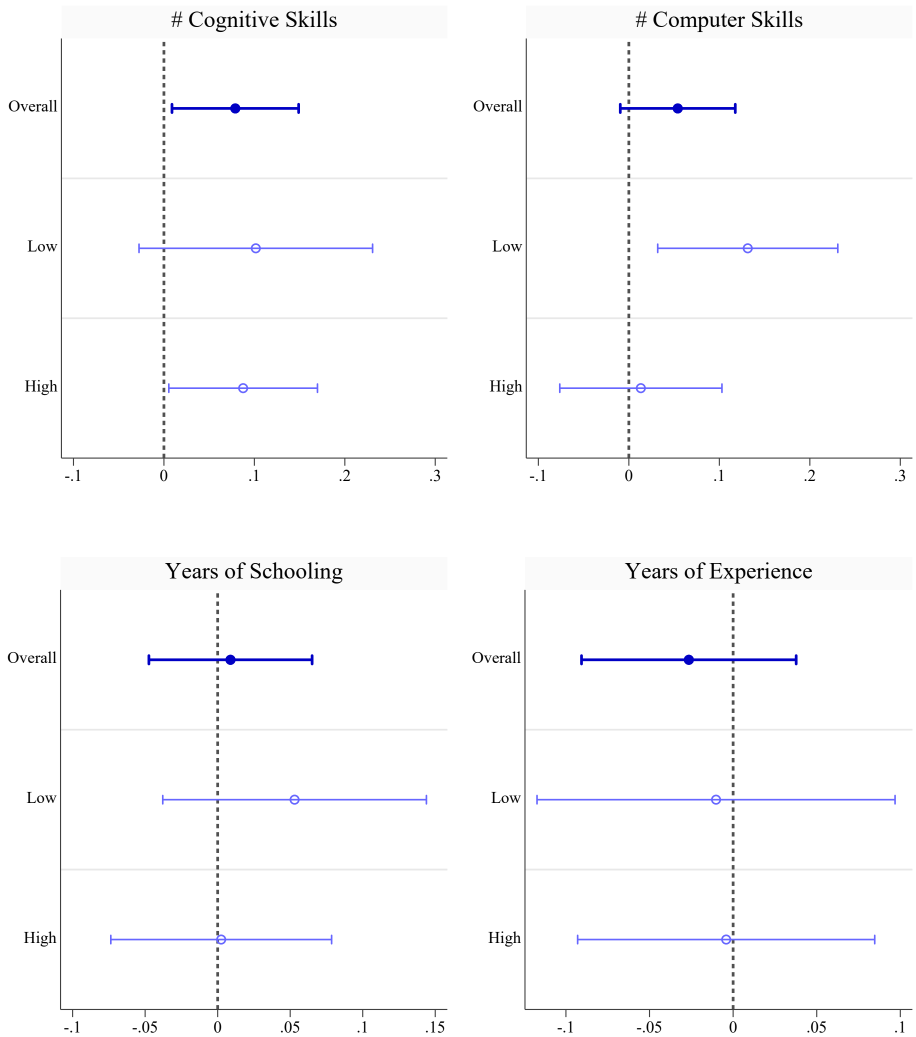
Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (2) separately for the sub-group denoted on the y-axis. Our baseline estimates are displayed at the top of each panel. In the top left panel, the overall effect cannot be estimated since the total job posting share equals 1 for all firm-year observations. All outcomes are log-transformed. The average share of job postings is 21.4% for manual jobs, 31.1% for routine jobs, and 47.4% for non-routine cognitive jobs, respectively, after weighting observations by the number of postings by the firm in that year. Standard errors are clustered at the firm level.

Figure A9: Effects of the OxyContin Reformulation on Skill Requirements: Heterogeneity by Firm Size



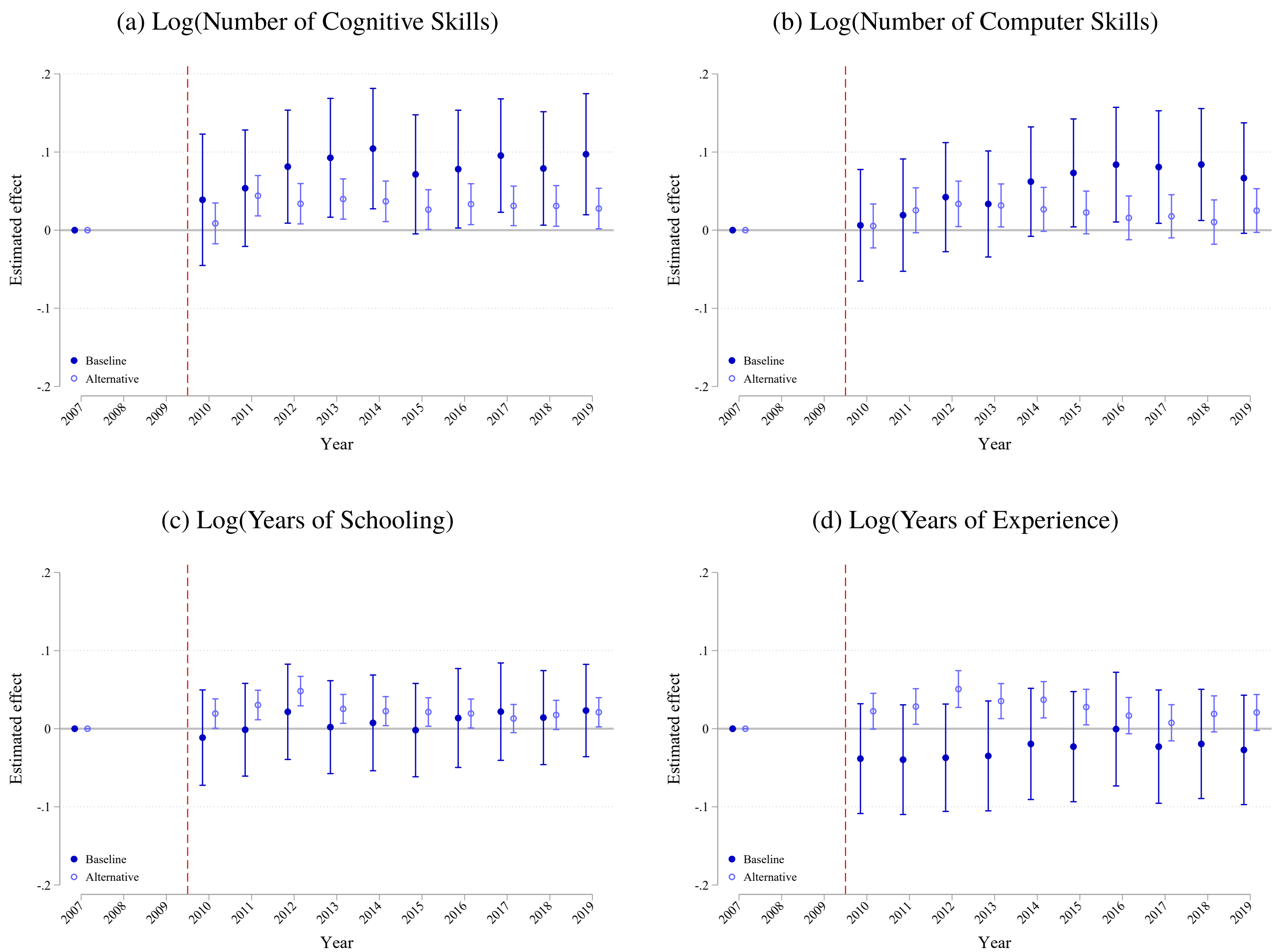
Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (2) separately for the sub-group denoted on the y-axis. Our baseline estimates are displayed at the top of each panel. All outcomes are log-transformed. Standard errors are clustered at the firm level.

Figure A10: Effects of the OxyContin Reformulation on Skill Requirements: Heterogeneity by Firm Education Requirement Levels



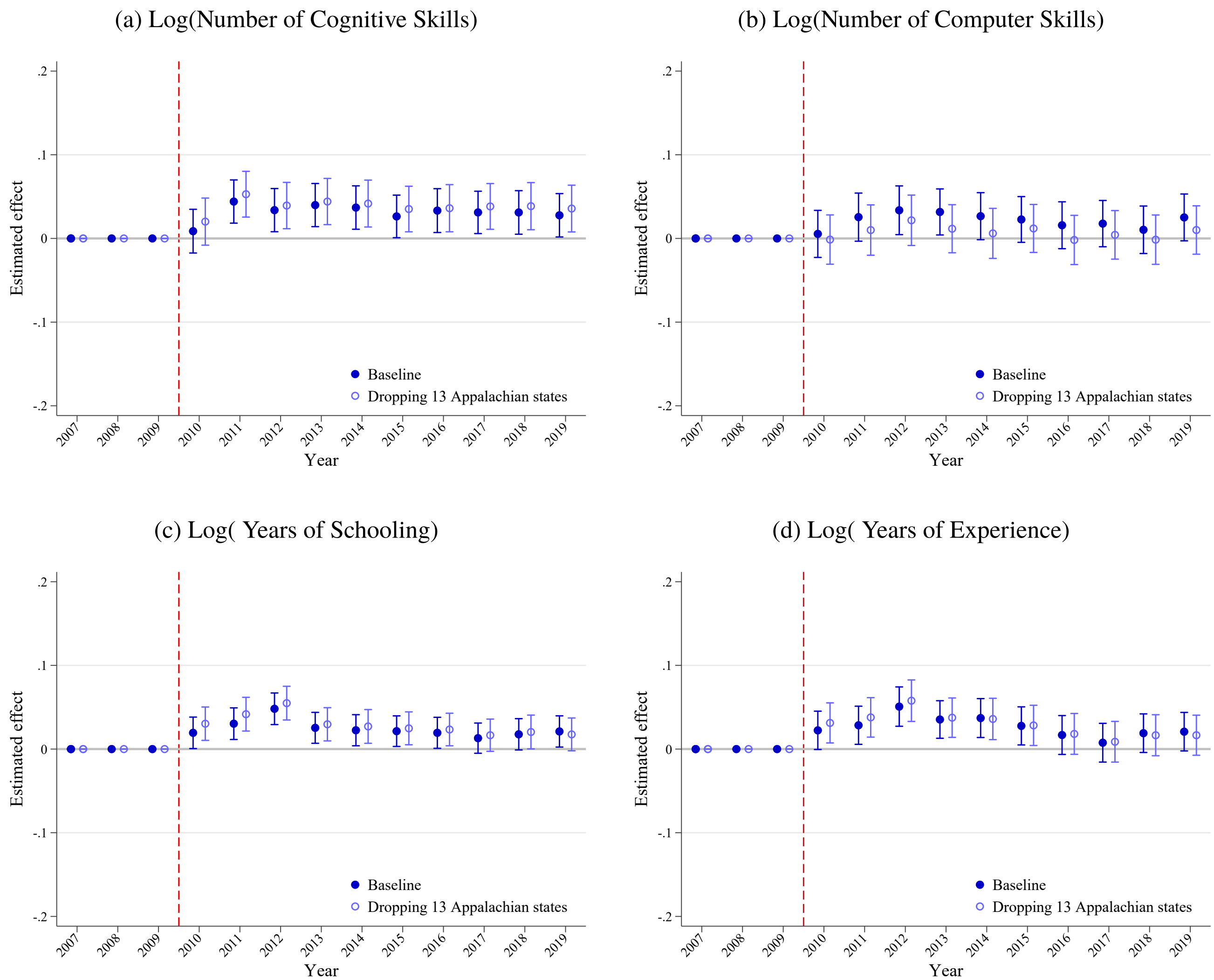
Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (2) separately for the sub-group denoted on the y-axis. Our baseline estimates are displayed at the top of each panel. All outcomes are log-transformed. We divide firms into two groups within each sector (utility, manufacturing, service, IT, finance, and professional service) based on the share of job postings requiring a four-year college degree measured in 2010, a partially treated year. Given that job posting outcomes are missing for a subset of our firms in 2007, we use 2010 data to calculate baseline education requirements for consistency. The average share of job postings requiring a four-year college degree in 2010 is 14.5% in the low-requirement group and 59.6% in the high-requirement group, respectively, after weighting observations by the number of job postings by the firm in 2010. Standard errors are clustered at the firm level.

Figure A11: Effects of the OxyContin Reformulation on Skill Requirements: Firm-by-State Level Analysis



Notes: The figure presents 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 2007 is set as the reference point and normalized to zero. Standard errors are clustered at the firm level.

Figure A12: Robustness of the Firm-by-State Analysis Estimates to Dropping Appalachian states



(a) Log(Number of Cognitive Skills)

(b) Log(Number of Computer Skills)

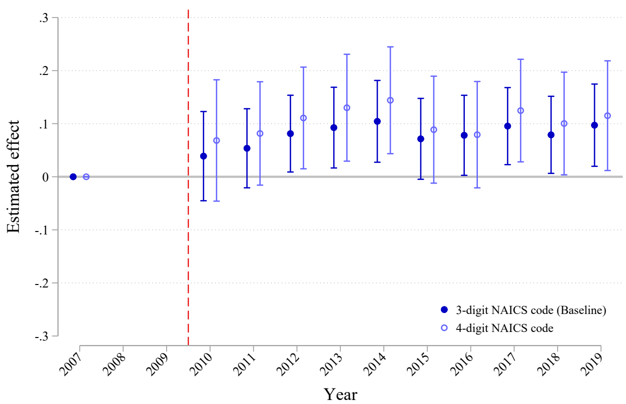
(c) Log(Years of Schooling)

(d) Log(Years of Experience)

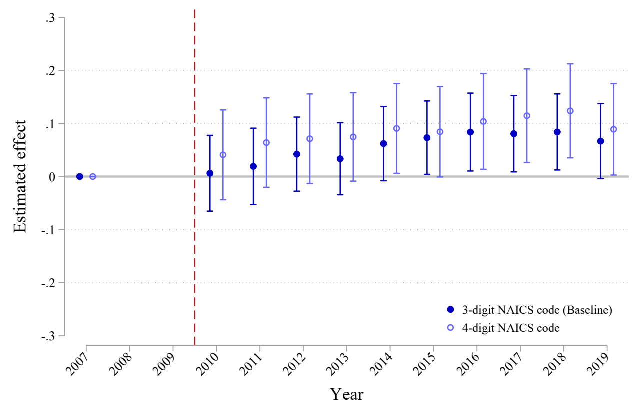
Notes: The figure presents output from estimation of equation (1) using our state-by-year sample (circles in dark blue) and the same sample with the exclusion of 13 Appalachian states (hollow circles in light blue). For both samples, we plot the coefficients and 95% confidence intervals on the interactions between the exposure to reformulation and the full set of year dummies. The year 2007 is set as the reference point and normalized to zero. Standard errors are clustered at the firm level.

Figure A13: Robustness of the Skill Requirement Estimates to Using a 4-Digit Industry Code

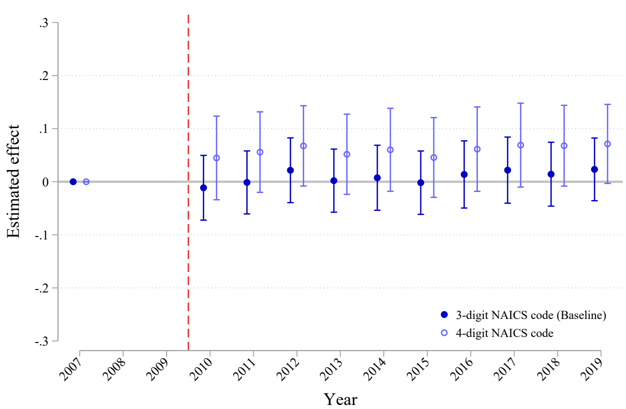
(a) Log(Number of Cognitive Skills)



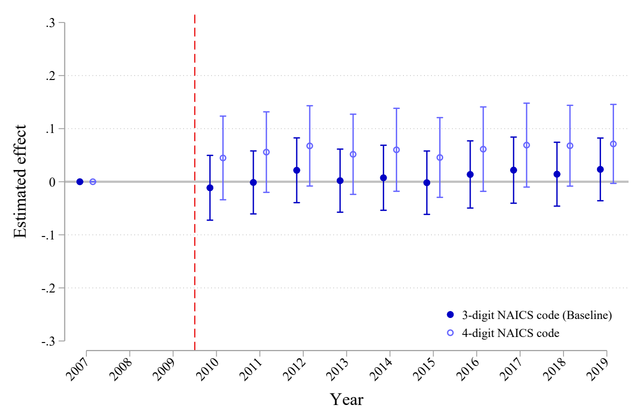
(b) Log(Number of Computer Skills)



(c) Log(Years of Schooling)



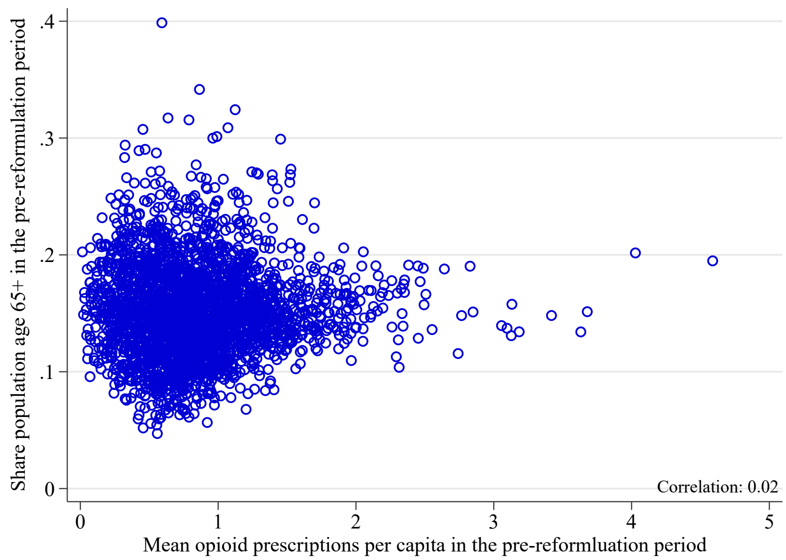
(d) Log(Years of Experience)



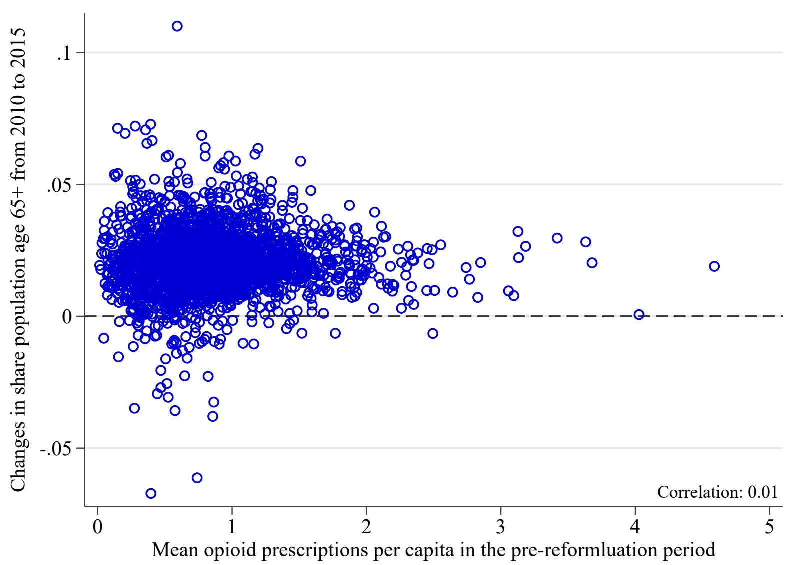
Notes: The figure presents results from equation (1) using two specifications: our baseline specification with industry-by-year fixed effects based on 3-digit industry code (represented by dark blue circles), and an alternative specification with industry-by-year fixed effects based on 4-digit industry code (represented by hollow circles in light blue). For both specifications, 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 2007 is set as the reference point and normalized to zero. Standard errors are clustered at the firm level.

Figure A14: Correlation Between Local Prescription Opioid Use and Elderly Population Share

(a) Baseline Elderly Share



(b) Change in Elderly Share



Notes: This figure presents the correlation between opioid exposure and elderly share. Panel (a) presents a scatter plot in which each observation represents the county-level population-weighted average opioid prescriptions per capita between 2006 and 2009—a measure for county-level exposure to the reformulation—on the x-axis, and the population-weighted average share of the population over age 65 in that county during the same period on the y-axis. In Panel (b), we further examine the correlation between exposure to the OxyContin reformulation and the elderly share growth rate from 2010 to 2015.

Table A1: Skill Categorization

Category	Key words and phrases
Cognitive	Problem Solving, Research, Analytical, Critical Thinking, Math, Statistics
Computer	Computer, Spreadsheets, Common Software (e.g. Microsoft Excel, Powerpoint), Programming language or specialized software (e.g. Java, SQL, Python, etc.)
Social	Communication, Teamwork, Collaboration, Negotiation, Presentation
Customer Service	Customer, Sales, Client, Patient
Management	Project Management, People Management (Supervisory, Leadership, Management, Mentoring, Staff)
Writing	Writing

Notes: Categorization of skill requirements in Lightcast from Deming and Kahn (2018). Our computer skill category combines general computer skills and specific software skills, categorized separately in Deming and Kahn (2018). Computer skills are basic software, such as Microsoft Excel and PowerPoint, while specific software skills include names of specialized software.

Table A2: Robustness of the Labor Market Estimates to Controlling for Weighted Local Labor Force Participation by Worker Subgroups

	(1) Baseline	(2) Add Gender	(3) Add Education	(4) Add Race
Panel A: Employment				
Opioid Exposure × Post	-0.051***	-0.051***	-0.052***	-0.051***
	(0.017)	(0.016)	(0.016)	(0.016)
	[0.002]	[0.002]	[0.001]	[0.002]
Panel B: Cognitive Skill Requirements				
Opioid Exposure × Post	0.079**	0.075**	0.069*	0.069*
	(0.036)	(0.037)	(0.037)	(0.037)
	[0.027]	[0.042]	[0.060]	[0.062]
Panel C: Computer Skill Requirements				
Opioid Exposure × Post	0.054*	0.052	0.044	0.044
	(0.032)	(0.034)	(0.033)	(0.033)
	[0.095]	[0.123]	[0.188]	[0.185]
LFP by Gender	No	Yes	Yes	Yes
LFP by Education	No	No	Yes	Yes
LFP by Race	No	No	No	Yes

Notes: This table reports 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. *p<0.1; **p<0.05; ***p<0.01.

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

	(1) Baseline	(2) Add Gender	(3) Add Education	(4) Add Race
Panel A: Employment				
Opioid Exposure × Post	-0.051***	-0.050***	-0.049***	-0.044**
	(0.017)	(0.017)	(0.017)	(0.017)
	[0.002]	[0.003]	[0.004]	[0.011]
Panel B: Cognitive Skill Requirements				
Opioid Exposure × Post	0.079**	0.084**	0.084**	0.085**
	(0.036)	(0.036)	(0.036)	(0.037)
	[0.027]	[0.019]	[0.021]	[0.021]
Panel C: Computer Skill Requirements				
Opioid Exposure × Post	0.054*	0.065**	0.061*	0.062*
	(0.032)	(0.033)	(0.033)	(0.033)
	[0.095]	[0.048]	[0.062]	[0.062]
Wage by Gender	No	Yes	Yes	Yes
Wage by Education	No	No	Yes	Yes
Wage by Race	No	No	No	Yes

Notes: This table reports 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. *p<0.1; **p<0.05; ***p<0.01.