Who Gets Screened Out? The Opioid Crisis and Employer Skill Requirements*

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February 2026

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. 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.

*For helpful comments, we thank Abby Alpert, David Beheshti, Ellora Derenoncourt, John Grigsby, Peter Hull, Gregor Jarosch, Karen Kopecky, Adam Lavecchia, Remy Levin, Lihua Lei, Nicole Maestas, Paolo Martellini, Stephen Ross, Maya Rossin-Slater, David Simon, Youngki Shin, Daniel Waldinger, Jonathan Zhang, Robert Kaestner, Seth Zimmerman, Xuan Zhang, Hanming Fang, and seminar participants at McMaster University, Stanford University, Sogang University, Chung-Ang University, 2024 Econometric Society Asian Meeting, 2024 WEAI Conference, 2024 SEA Conference, and 2025 Society of Labor Economics World Conference. Any errors are our own.

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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 a significant gap in understanding how this crisis affects 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, highlighting both the shift toward illicit opioids and their greater health risks (Alpert et al., 2018; Beheshti, 2019; Evans et al., 2019; Powell et al., 2019; Mallatt, 2022). Importantly, evidence suggests that the reformulation expanded demand in illicit opioid markets and generated spillovers throughout those markets (Powell and Pacula, 2021). As these markets grew and suppliers adapted, exposure to heroin and other potent opioids likely increased more broadly, affecting not only existing prescription opioid users but also individuals who had not previously accessed prescription opioids. Consistent with this interpretation, Powell and Pacula (2021) show that the reformulation led to an increase in overall opioid-related mortality beyond a simple substitution across opioid types.

The expansion of illicit opioid markets may therefore have had important labor market consequences. Increased exposure to illicit opioids can affect worker health, reliability, and productivity and raise uncertainty surrounding hiring decisions, as illicit opioid use—relative to legal prescription use—is associated with greater health risks, legal consequences, and unpredictability, potentially increasing the expected costs to employers of hiring workers at risk of misuse. Building on this expansion of illicit opioid markets and its documented consequences, we investigate how this transition has affected job skill requirements.

To clarify how employers respond to the transition toward illicit drug use and utilize skill requirements as a screening tool, we first present a conceptual framework by adopting a search model inspired by Morgan and Várdy (2009). In this model, an employer seeks to fill a job vacancy for a specific task. While the employer cannot directly identify whether a job candidate misuses opioids, they can observe a skill-based signal that contains some noise, which is correlated with the likelihood of the candidate engaging in opioid misuse. Employers only learn whether a candidate is an opioid addict after hiring them. In this context, the model predicts that an increase in the likelihood of opioid abuse among low-skilled candidates, as a result of the opioid crisis, leads employers to raise their hiring standards.[3] We also show that this hiring strategy reduces the overall share of low-skilled workers, regardless of whether they misuse opioids.

To examine these predictions empirically, we construct unique firm-level data that measure how firms' employment and skill requirements have changed following the transition towards illicit opioids. These firm-level data are constructed from two sources. First, we use online 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 quantitative and qualitative skills. Second, we complement this dataset with firm-level employment data obtained from the Compustat North America Database. For our analysis, we focus on publicly traded companies, whose shares 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).

To estimate the causal effects of the reformulation on job skill requirements and employment, we use a difference-in-difference event study design that compares within-firm changes in outcomes among firms located in counties with higher initial rates of prescription opioid use to outcomes of firms located in low-exposure counties, following the approach proposed by Alpert et al. (2018), which has since been widely adopted in the literature. 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. Specifically, we construct a firm-level exposure measure based on pre-reformulation county-level prescription opioid use by aggregating exposure across a firm's establishments using a weighted average. The key identification assumption is that firms with higher initial exposure to local prescription opioid use would have followed the same outcome path as lower-exposure firms had they experienced the same level of exposure. Because this counterfactual is unobservable, we assess the plausibility of this assumption by examining pre-treatment trends and conducting robustness checks, including estimating an event study specification based on a binary treatment definition.

[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.

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

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 (e.g., Park and Powell (2021)). 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 significant and long-lasting positive 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 6 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 heterogeneity analyses reveal significant effects within specific subgroups.

An important question is whether the observed upskilling is driven by firms posting more high-skilled jobs or by increasing requirements within existing jobs. To distinguish between compositional changes and within-job upskilling, we construct a firm-occupation-year panel and estimate regressions with firm-occupation and occupation-year fixed effects. This specification isolates changes in skill requirements within the same occupation group within a firm over time. We find that the increase in skill requirements at the firm level is largely driven by within-occupation (6-digit SOC) variation. Evidence of within-occupation upskilling therefore suggests that employers have raised skill requirements for the same job, consistent with the idea that firms use skill requirements as a screening tool.

To understand how the effects of reformulation vary based on different characteristics of firms and local labor markets, we examine heterogeneity in relation to factors such as firm size, educational requirements, local employment protection laws, and minimum wage levels. We document the following key findings: First, we find that relatively large firms (specifically, the top one-third among publicly traded companies) focused on raising computer skills, while small and medium-sized firms (the lower two-thirds) emphasized cognitive skills in response to the opioid crisis. This suggests that smaller firms may prioritize cognitive skills in their job postings because they are less costly to highlight compared to more technical skills, like computer proficiency, which may necessitate higher wages. Second, we note that the upskilling effects are more pronounced among firms with lower baseline education requirements. This aligns with previous research indicating that individuals with less education face a higher risk of opioid-related deaths. Finally, we observe that the upskilling effects are especially notable for firms located in areas with higher minimum wage levels or stricter employment protection laws. This indicates that in locations where the costs associated with hiring or firing employees are higher, there is a greater likelihood of screening candidates or an 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

[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.

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 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 are associated with reduced employment and firm value, and firms substitute relatively scarce labor with capital, particularly when they face fewer financial constraints. 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

[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.

[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.

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 (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 job 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. They underscore 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. In addition, 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.

The remainder of the paper proceeds as follows. Section 2 provides background on OxyContin reformulation and its potential impacts on hiring decisions. Section 3 presents a conceptual framework that links changes in opioid-related risk in the local labor market to employers' screening strategies. Section 4 describes the data and key variables. Section 5 outlines the empirical strategy. Section 6 presents the main results, heterogeneity analyses, and robustness checks. Section 7 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) show that areas more exposed to the reformulation experienced disproportionate growth in fatal overdoses involving synthetic opioids such as illicit fentanyl (as well as other substances such as cocaine) and an increase in overall opioid-related mortality.

Figure 2 provides complementary evidence on the timing of the shift from prescription opioids to illicit opioids. Using Google Trends, we plot search intensity for "OxyContin," "Heroin," and "Fentanyl" (relative to January 2008) and mark the 2010 reformulation. The monthly frequency of this series allows us to track changes in opioid-related attention around the reformulation across opioid categories. The figure shows an immediate decline in searches for "OxyContin" following the reformulation. This decline coincides with a sharp rise in searches for "Heroin," which reach a peak in 2017. Searches for "Fentanyl" begin to increase around 2013 and continue to rise thereafter, while heroin searches decline after their 2017 peak. Overall, these patterns are consistent with a post-reformulation shift from OxyContin toward heroin, followed by a growing role of illicit fentanyl in later years. For comparison, Panel (b) plots search intensity for "Marijuana," which does not show a similar change around 2010.

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 account 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).[8] 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 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.

2.3 Transition Towards Illicit Opioids

Impacts of OxyContin reformulation 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;

[8] 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.

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.

2.4 Employer Perspectives on Opioid Use: Concerns and Workplace Responses

Employer concerns around employee opioid use. According to a 2020 survey conducted by the National Safety Council (NSC), 75% of employers report being directly affected by opioids. The survey includes responses from 526 U.S. employer decision-makers at organizations with 50 or more employees.

Figure 3 presents NSC survey results summarizing employer concerns related to employee opioid use and their perceptions of its impact on job performance. Panel (a) displays the share of respondents who view each workplace issue as a "major" or "minor" concern, with the combined percentage shown above each bar. Light blue shading highlights three opioid-related issues. Panel (b) shows employer perceptions of the potential impact of prescribed opioid use on job performance. Respondents were asked to rate their agreement with the statement, "Opioids taken as prescribed can impair job performance." Response options are ordered from strongest to weakest agreement, with the corresponding share of respondents shown in each bar.

Panel (a) of Figure 3 shows that the most commonly cited concern among employers is the ability to hire qualified workers, with 90% reporting it as either a major or minor concern. Other top concerns include employee benefits costs (86%) and worker compensation costs (86%). Opioid-related issues also rank prominently: 83% of employers expressed concern about the misuse of illicit opioids, and 79% and 75% were concerned about legal prescription opioid use and the illicit use or sale of opioids, respectively. These results suggest that while traditional workforce and cost issues remain top of mind, opioid misuse is also perceived as a major workplace challenge.

Panel (b) of Figure 3 highlights employers' perceptions of the impact of prescription opioids on job performance. A combined 86% of respondents either strongly agree (43%) or somewhat agree (43%) that opioids taken as prescribed can impair job performance. In contrast, only a small fraction expressed neutrality (9%) or disagreement (5% in total). These results indicate a broad consensus among employers that even medically appropriate opioid use may negatively affect workplace functioning.

Responses to employee substance use by substance type. The NSC survey also indicates that employers respond more severely to illicit opioid misuse than to the misuse of legal substances such as prescription medications or alcohol. Appendix Table A1 summarizes how employers respond to different types of substance misuse in the workplace. Responses are shown by substance type, including prescription opioids, prescription stimulants, benzodiazepines, alcohol, legal marijuana, illicit marijuana, illicit opioids, heroin/fentanyl, and other illicit drugs.[9]

The table suggests that employers are generally more lenient toward prescription opioid misuse than illicit drug use. For instance, 10% of employers report ignoring prescription opioid misuse, and 41% say they would return the employee to their position after treatment. In contrast, only 2–3% of employers say they would ignore heroin or fentanyl misuse, and just 25% report a return-to-position approach. Instead, a majority of employers indicate they would dismiss employees for heroin (54%), fentanyl (55%), or other illicit opioid use (49%).

[9]The data reflect responses from 526 employer decision makers representing U.S. organizations with 50 or more employees. Respondents were asked: *"Which of the following would you say best reflects your organization's approach to an employee who is found to be misusing...?"* Employers selected one of the following options: ignore the problem, return the employee to their position after treatment, ensure the employee is carefully monitored for the remainder of their employment, relocate the employee to a position of lesser responsibility, or dismiss them.

2.5 Potential Impacts of Reformulation on Firm Skill Requirements

Trends and limitations of drug testing as a hiring screen. In response to rising concerns about illicit opioid use, employers may seek to screen out potential users through pre-employment drug testing. Figure 4 provides suggestive evidence that employer attention to drug screening increased over time. Panel (a) plots Google Trends search intensity for "drug test" and "drug screen," and Panel (b) plots a series we construct from our Lightcast job posting data: the quarterly share of online job ads that mention these terms. The 2010 reformulation is marked by a vertical line. Both measures rose in the years after 2010, suggesting that drug screening became a more salient feature of hiring practices during the period when illicit opioid use intensified.

However, pre-employment drug testing has important limitations that undermine its effectiveness as a screening tool. First, testing is typically conducted only after a conditional offer is made, limiting its deterrent power. Legal restrictions also vary across states—for example, Iowa requires employers to notify applicants of testing in job postings and to list the substances tested. Moreover, drug tests often cannot distinguish between illicit and prescribed opioids, making it difficult to identify heroin use specifically. Moreover, substances like heroin are detectable for only 2 to 8 hours after use, reducing the likelihood of detection. Finally, the rise in drug test cheating further undermines the reliability of these tests in identifying actual substance misuse.[10]

Potential impacts on firm skill requirements. 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

[10]It is also challenging to identify potential legal opioid users once they are employed. According to the NSC 2020 survey, 34% of employers report that employee responsibility to disclose prescription opioid use is not covered in their organization's written policies.

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.[11]

Finally, 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.

3 Conceptual Framework

In this section, we present a theoretical framework to understand how employers adjust their hiring standards in response to the opioid crisis. We adapt the model structure from Morgan and Várdy (2009) to the context of job posting requirements and résumé screening. In our model, an employer seeks to fill a vacancy for a specific occupation. The employer reviews résumés from a pool of job applicants for this position. Each résumé review incurs a fixed cost k, representing the time and resources spent evaluating candidates.

We assume each candidate has two characteristics: skill level (γ) and productivity type (θ). Skill level can be either more-skilled or less-skilled, and is observable to the employer through credentials, education, and experience listed on résumés. In contrast, productivity type—which depends on whether the candidate is a drug user or not—is unobservable to the employer. There are two productivity types: productive (non-drug users) and non-productive (drug users). Non-drug users have high productivity and are able to perform the job effectively, and $\theta = 1$. Conversely, drug users have low productivity and cannot perform the job adequately, so $\theta = 0$. Since this type is unobservable, employers cannot distinguish between drug users and non-users based on observable

[11] 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.

résumé characteristics.

Let m_H and m_L denote the probabilities that a candidate applying for this specific occupation is more-skilled or less-skilled, respectively. For simplicity, we assume $m_H = m_L$. Both more-skilled and less-skilled workers are assumed capable of performing the job once hired; however, their actual productivity depends solely on their drug use status. This simplifying assumption allows us to isolate how increases in local drug use affect employers' hiring decisions when they must rely on imperfect signals to screen candidates. Among more-skilled workers, the probability of being highly productive (i.e., a non-drug user) is p_H, while for less-skilled workers, this probability is p_L. We assume that more-skilled candidates are more likely to be high-productivity (non-drug-using) workers, i.e., $p_H > p_L$.

The timing of the model is illustrated in Figure 5. In the first period, the employer reviews a résumé from a randomly selected applicant for the specific job opening, observing their skill level γ (high-skill or low-skill based on education, certifications, and experience listed). The employer also observes other information in the résumé that serves as signals sent by the candidate about their underlying productivity type θ, represented by $S_\gamma = \theta + \varepsilon_\gamma$. The noise term ε_γ arises because these observable characteristics are imperfect predictors of actual drug use and job performance. The noise term ε_γ is assumed to follow a normal distribution: $\varepsilon_\gamma \sim \mathcal{N}(0, \sigma_\gamma^2)$. Furthermore, we assume that the signal noise is identical for both more-skilled and less-skilled applicants for this occupation, i.e., $\sigma_H = \sigma_L = \sigma$.

After reviewing the résumé and evaluating the signal from the applicant's qualifications, the employer forms a posterior belief about the likelihood that the candidate with skill level γ can perform the specific job effectively. Based on this belief, the employer decides whether to hire the candidate for the position.[12]

In period 2, if the candidate is not hired, the employer continues reviewing résumés for the same position with a new draw from the pool of workers. If the candidate is hired, their true productivity type θ is revealed through their work performance. If the candidate has high productivity (is a non-drug user, $\theta = 1$), the employer receives a net present value payoff of $\nu > 0$, and the worker

[12] As discussed in the Section 2 section, pre-employment drug testing has several limitations: it can typically only be conducted after a conditional job offer is made, has legal restrictions that vary by state, often cannot distinguish between illicit and prescribed opioids, and substances like heroin are detectable for only 2–8 hours after use. Given these constraints, we do not incorporate drug testing into our model and instead focus on how employers use skill requirements as a screening mechanism.

is retained in the position. If the candidate has low productivity (is a drug user, $\theta = 0$) and thus unable to perform the job, the employer dismisses the worker and incurs a cost of c.

3.1 Posterior Beliefs

We first derive the employer's posterior belief that the worker with skill level γ can perform the job productively after receiving signal S Using Bayes' formula, we have the equation below.

$$q_\gamma(S) = \frac{\phi\left(\frac{S-1}{\sigma}\right) \cdot p_\gamma}{(1-p_\gamma) \cdot \phi\left(\frac{S}{\sigma}\right) + p_\gamma \cdot \phi\left(\frac{S-1}{\sigma}\right)}, \tag{1}$$

where the $\phi(\cdot)$ denotes the probability density function of a standard Normal distribution. Equation (1) shows that after receiving a signal S, the employer can form a belief about the probability that a worker of kind γ (high-skill or low-skill) is capable of performing the job. Conversely, by using the inverse function, we can determine the signal S that corresponds to a given posterior belief q about the probability that the worker can perform the job. Importantly, the mapping between the signal and the posterior belief is one-to-one, ensuring a unique signal for each belief and vice versa. The inverse function of Equation (1) can be expressed as follows:

$$S_\gamma(q) = \frac{1}{2} - \sigma^2 \cdot \log\left[\frac{1-q}{q} \cdot \frac{p_\gamma}{1-p_\gamma}\right]. \tag{2}$$

Note that even though the employer knows the candidate's kind γ—that is, whether the worker is a high-skill or low-skill worker—the success probability $Q_\gamma = q_\gamma(S_\gamma)$ remains a random variable prior to the realization of the signal S_γ. Therefore, we can define the cumulative distribution function (CDF) of the success probability $G_\gamma(q)$ and the probability density function (PDF) as below.

$$G_\gamma(q) = p_\gamma \cdot \Phi\left(\frac{S_\gamma(q)-1}{\sigma}\right) + (1-p_\gamma) \cdot \Phi\left(\frac{S_\gamma(q)}{\sigma}\right) \tag{3}$$

$$g_\gamma(q) = \frac{\sigma}{q(1-q)}\left[p_\gamma \cdot \phi\left(\frac{S_\gamma(q)-1}{\sigma}\right) + (1-p_\gamma) \cdot \phi\left(\frac{S_\gamma(q)}{\sigma}\right)\right], \tag{4}$$

where $\Phi(\cdot)$ and $\phi(\cdot)$ are the CDF and PDF of the standard normal distribution, respectively.

Moreover, before the realization of the kind of job applicant γ, the success probability is also a random variable, and its CDF, $G(q)$, is given below.

$$G(q) = m_H G_H(q) + m_L G_L(q). \tag{5}$$

3.2 Value Function and Optimal Hiring Decisions

We next derive the firm's value function and characterize the optimal hiring rule. As explained by Morgan and Várdy (2009), the firm's optimal strategy in this setting takes the form of a threshold rule. Specifically, the firm chooses an optimal threshold $\underline{q}^*$ and hires a job candidate only if the observed signal exceeds this threshold, regardless of the skill level of the job applicant.

Let $V(\underline{q})$ denote the firm's expected payoff under the optimal search strategy, where the firm hires a job candidate only if the received signal exceeds $\underline{q}$. The value function $V(\underline{q})$ can then be expressed recursively as:

$$V(\underline{q}) = \delta \int_0^1 \max\{qv + (1-q)(V(\underline{q}) - c),\ V(\underline{q})\}\, dG(q) - k. \tag{6}$$

Alternatively, using the equation above, the firm's expected payoff $V(\underline{q})$ can be expressed as:

$$V(\underline{q}) = \frac{\delta \int_{\underline{q}}^1 (vq + (1-q)(-c))\, dG(q) - k}{1 - \delta\left(1 - \int_{\underline{q}}^1 q\, dG(q)\right)}. \tag{7}$$

Hence, the firm's problem is to choose the optimal $\underline{q}$, which can maximize $V(\underline{q})$ in Equation (7). The optimal solution to Equation (7) is characterized by Proposition 1.

PROPOSITION 1: *The optimal hiring strategy, $\underline{q}^*$, can be implicitly defined as*

$$\begin{aligned}\underline{q}^* = \frac{c}{c + (1-\delta)v + k} - \frac{c\delta}{c + (1-\delta)v + k}(1 - m_H p_H - m_L p_L) \\ + \frac{c\delta}{c + (1-\delta)v + k}\int_0^{\underline{q}^*} G(q) dq.\end{aligned} \tag{8}$$

POLICY IMPLICATION 1: *When the proportion of low-productivity workers (drug users) increases in the population of less-skilled applicants for this occupation (i.e. p_L decreases), the optimal hiring threshold, $\underline{q}^*$, increases.*

Intuitively, this implies that if the success CDF of low-skilled workers, $G_L(q)$, does not shift too dramatically in response to a decline in the proportion of non-drug users, p_L, firms respond by raising their screening threshold $\underline{q}^*$. In other words, during an opioid crisis, when the share of drug users increases, particularly among low-skilled workers, firms tighten their skill requirements to better screen out drug users. In the appendix, we show that this condition can be supported under reasonable parameter values.

3.3 Permanent Composition

Finally, we characterize the steady-state composition of worker types in the economy. Let r_γ denote the probability that a vacancy is permanently filled by a job applicant of kind γ, given that the firm follows an optimal screening strategy with threshold $\underline{q}$. The value of r_γ can be expressed recursively as:

$$\begin{aligned} r_\gamma = & m_\gamma \left\{ P_r \left[\left(1 - G_\gamma \left(\underline{q} \mid \theta_\gamma = 1\right)\right) + G_\gamma \left(\underline{q} \mid \theta_\gamma = 1\right) r_\gamma \right] + \left(1 - P_\gamma\right) r_\gamma \right\} \\ & + \left(1 - m_\gamma\right) \left\{ r_\gamma \left[1 - P_{-\gamma} \left(1 - G_{-\gamma} \left(\underline{q} \mid \theta_\gamma = 1\right)\right)\right] \right\}. \end{aligned} \tag{9}$$

Let's further define:

$$G_{\gamma\theta} \equiv G_\gamma \left(\underline{q} \mid \theta_\gamma = \theta\right) = \begin{cases} \Phi\left(\frac{S_\gamma(\underline{q}) - 1}{\sigma}\right) & \text{when } \theta = 1 \\ \Phi\left(\frac{S_\gamma(\underline{q})}{\sigma}\right) & \text{when } \theta = 0. \end{cases} \tag{10}$$

Then the Equation (9) can be expressed as:

$$r_\gamma = \frac{m_\gamma P_\gamma \left(1 - G_{\gamma 1}\right)}{m_\gamma P_\gamma \left(1 - G_{\gamma 1}\right) + \left(1 - m_\gamma\right) P_\gamma \left(1 - G_{-\gamma 1}\right)}. \tag{11}$$

POLICY IMPLICATION 2: *As the proportion of drug users among low-skill workers increases (i.e., as P_L decreases), the permanent composition of low-skill workers declines, given a recruiting threshold $\underline{q}$ (i.e. $\frac{\partial \gamma_L}{\partial P_L} > 0$).*

POLICY IMPLICATION 3: *As the recruiting threshold $\underline{q}$ increases, the permanent composition of low-skill workers declines.*

Policy Implication 2 suggests that if the proportion of drug users increases and the recruiting threshold remains unchanged, the permanent share of low-skill workers in the population will decline. Intuitively, this is because as the number of non-drug users in the low-skill population decreases, fewer individuals are able to send signals that meet the employer's hiring threshold.

4 Data

This section outlines the datasets we use in our analysis. First, we describe county-level data on prescription opioid use. 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.

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 5 how we derive firm-level exposure to the reformulation using these data on county-level prescription opioid use in the pre-reformulation period.

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., 2013). 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 A2 lists the skill categories and the corresponding keywords or phrases that belong to each category. For instance, a cognitive skill should include keywords or phrases such as "problem-solving," "research," and "statistics." These keywords are deliberately chosen by Deming and Kahn (2018) to match the “non-routine analytical" job tasks that are classified by Autor et al. (2003) based on the O*NET database. 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. We complement job posting data with employment measures constructed from the Compustat North America Database (Compustat), which collects standardized financial and employment information for all publicly traded U.S. firms. We exclude funds, trusts, and other financial vehicles (NAICS 525) from the sample.

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

[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.

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 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 following outcomes: (1) the average number of required skills in each skill category, such as cognitive, computer, and social skills, (2) the average years of schooling and experience required, and (3) employment level. Our final firm-level sample comprises 2,104 publicly traded companies included in both databases.[17] 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.

[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).

[17]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.

5 Empirical Strategy

To explore the causal impact of the OxyContin reformulation on skill requirement responses, we employ difference-in-difference 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.

5.1 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.

While this broader measure offers a more precise representation of local variation in pre-intervention opioid exposure than state-level aggregates (Evans et al., 2022), we acknowledge important limitations. The CDC measure captures all Schedule II opioid prescriptions rather than OxyContin specifically, and reflects legal distribution rather than misuse. Since the reformulation targeted OxyContin and legal prescriptions may not correlate strongly with actual misuse, this introduces measurement error into our exposure variable. We employ this measure because it provides the county-level geographic variation essential for constructing our firm-level Bartik-style exposure across establishments, an advantage unavailable with state-level misuse data alone.

Despite these limitations, multiple pieces of evidence validate our approach. Appendix Figure A2 shows that when aggregated to the state level, our county-level CDC exposure measure correlates positively ($r = 0.53$) with Alpert et al.'s measure of OxyContin misuse rates from NSDUH data.[18] More importantly, Appendix Figure A3 demonstrates that higher CDC exposure predicts larger subsequent reductions in OxyContin misuse rates from 2008 to 2012 and greater increases in heroin mortality rates from 2008 to 2016,[19] confirming that this measure captures areas more affected by the transition to illicit opioids following reformulation.[20] In Section 6.3, we further assess robustness using the state-level sample with Alpert et al.'s direct measure of OxyContin misuse. Figure 6 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 A4 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

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

[19] 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).

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

prescriptions per capita, with a standard deviation of 0.22.[21]

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.[22] This distinction is particularly important for identifying the mechanisms behind potential changes in the 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 5.2.

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.

5.2 Empirical Specifications

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

$$ln(y_{fgt}) = \sum_{t=2005, t\neq 2009}^{2019} \delta_t Exposure\_Pre_f + \alpha_f + \gamma_{gt} + X'_{ft}\beta + \varepsilon_{fgt}, \tag{12}$$

[21] In addition, Appendix Figure A5 presents the average exposure by industry groups. Note that our analysis focuses on within-industry across-firm comparison rather than across-industry comparison.

[22] 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).

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.[23] Standard errors are clustered at the firm level.

Identification Assumption. The key identification assumption is that firms with higher initial exposure to local prescription opioid use ("higher-dose"firms) would have followed the same outcome path as lower-dose firms had they instead experienced the lower dose—what Callaway et al. (2024) refer to as the *strong parallel trends* assumption. This differs from the standard parallel trends assumption in a binary treatment framework, which requires parallel trends in untreated potential outcomes. Importantly, the strong parallel trends assumption is not strictly stronger than the binary treatment assumption; for example, it does not require parallel trends in untreated potential outcomes for all dose groups.

Because counterfactual outcome paths are unobserved, this assumption cannot be tested directly. We therefore assess its plausibility by examining pre-treatment trends. In addition, we test the robustness of our results by estimating a difference-in-differences event study specification using a binary treatment definition, classifying higher-dose firms as treated and lower-dose firms as the comparison group (see Section 6.3). The resulting estimates are consistent with our baseline specification with continuous treatment, indicating that our findings are not sensitive to

[23] 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.

the choice of treatment definition (continuous versus binary).

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

$$ln(y_{fgt}) = \lambda Exposure\_Pre_f \times Post_t + \rho_f + \tau_{gt} + X'_{ft}\sigma + u_{fgt}, \quad (13)$$

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 (13). 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:

$$\begin{aligned} ln(y_{fgt}) = \lambda_1 Exposure\_Pre_f \times \textit{SR Post}_t + \lambda_2 Exposure\_Pre_f \times \textit{MR Post}_t \\ + \rho_f + \tau_{gt} + X'_{ft}\sigma + u_{fgt}, \quad (14) \end{aligned}$$

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 (13). Standard errors are clustered at the firm level.

6 Results

6.1 Effects on Firm Employment and Skill Requirements

We explore the impacts of OxyContin reformulation on various labor markeffect on fit 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 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 7, we present the results of our event study focusing on firm-level employment. Specifically, we show the coefficients and their associated 95 percent confidence intervals derived from the estimation of equation (12). Our analysis indicates no statistically significant evidence of pre-existing trends prior to the reformulation, which supports the parallel trends assumption. In 2010, the year the reformulation occurred, we observed a decline in firm employment levels. This negative effect continued to grow through 2015 and has remained relatively stable since then. In Column 1 of Panel A in Table 2, we report the average effect over the post-reformulation period based on the estimation of equation (13). Notably, a one standard deviation increase in exposure to the reformulation was linked to a 5.1 percent reduction in employment at the firm level.

Our estimate is larger than the corresponding effects from the literature. For example, 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.[24] However, this discrepancy is consistent with the fact 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. Thus, our results align with existing literature and confirm that publicly traded firms also reduced employment in response to the increased use of illicit opioids in local areas.

Effect on firms' skill requirements. Next, we examine the impact of reformulation on skill requirements using our job posting data. Figure 8 illustrates the dynamic effects of the OxyContin reformulation on firm-level skill requirements, as detailed in equation (12). The outcome variables include 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 work experience (Panel (d)) required in a job posting. Since the Lightcast job posting data are unavailable for 2008 and 2009, we use 2007 as the reference period. The estimates in Panels (a) and (b) indicate that the requirements for cognitive and computer skills increased following the reformulation. The effect size generally

[24] 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.

increased up to 2014 and remained relatively stable through 2019. The rise in the estimated effect size over time in the medium term aligns with the idea that the reformulation of OxyContin contributed to the expansion of illicit heroin markets in areas with higher pre-reformulation rates of prescription opioids (Alpert et al., 2018; Park and Powell, 2021; Evans et al., 2022). Panels (c) and (d) show more muted effects on the years of schooling and work experience.

Columns 2–5 of Table 2 provide the corresponding difference-in-difference estimates for these outcomes. In Panel A, we present estimates summarizing the overall effect from 2010 to 2019 from the estimation of equation (13). 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 (14). 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 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.[25] Note that we do observe statistically significant effects on education and experience requirements in certain subgroups as discussed in Section 6.2.

Occupation-level analysis. So far, we have demonstrated that the shift towards illicit opioids has resulted in an increase in skill requirements at the firm level. A key question is whether this upskilling effect is driven by an increase in skill requirements within an occupation (within-occupation change) or by a shift in the composition of job vacancies, with a greater share of high-skilled positions relative to low-skilled ones (composition change).

An increase in skill requirements within occupations in response to the opioid crisis would provide strong evidence that employers are strategically adjusting job requirements to screen out potential opioid users. Importantly, this analysis addresses concerns that the observed upskilling

[25] In Appendix Figure A6, 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.

reflects changes in labor supply rather than adjustments in employers' job requirements. If upskilling were driven solely by shifts in workforce composition due to the opioid crisis, we would be unlikely to observe increases in skill requirements within the same occupation.

To explore this question, we examine the dynamic effect of OxyContin reformulation on the log of skill requirements at the firm-by-occupation level. To do this, we create a yearly panel dataset of skill requirements for each firm and 6-digit SOC (Standard Occupational Classification) occupation. We then estimate a version of our event study model (equation 12) in which we replace firm fixed effects with firm-by-occupation fixed effects and occupation-by-year fixed effects. All other controls remain unchanged, including industry-by-year fixed effects and time-varying firm-level characteristics. This approach allows us to capture the within-firm-occupation upskilling effects.[26]

Panel (a) of Figure 9 illustrates the within-firm-occupation effects on the number of cognitive skills and confirms that the point estimates are comparable to those observed in the firm-level results (shown in panel (a) of Figure 8). This finding indicates that employers are raising skill requirements for each occupation rather than simply adjusting the composition of occupations with varying skill levels, and that this within-occupation effect drives our baseline firm-level results. Panel (b) reveals a lesser impact on the number of computer skills within firm-occupations compared to the firm-level effect presented in Figure 8.

Panel (c) illustrates the statistically significant within-occupation effects on the requirement for years of schooling, revealing an approximately 7% increase corresponding to a one standard deviation rise in opioid exposure.[27] The estimates for experience requirements shown in panel (d)

[26]This analysis includes firm-by-occupation fixed effects and therefore restricts the sample to firm-occupation pairs observed in more than one year during the sample period (number of firms= 1,961). This restriction is more stringent than in the baseline analysis, which requires firms to be observed in more than one year regardless of occupation (N = 2,104). To address concerns that changes in firm composition may affect the results, we estimate the baseline firm-level event study model (equation 12) using both the full baseline sample and a subsample consisting of firms included in the firm-occupation analysis. Both samples in these regressions are based on firm-level panel data. As shown in Appendix Figure A7, the results are robust across samples, indicating that compositional changes do not drive the findings in the firm-occupation analysis.

[27]One possible explanation for the offsetting compositional effects is that increasing educational requirements can be costly for firms and may not serve purely as a screening tool. As a result, employers might compensate by shifting toward hiring in occupations that require less formal education. These findings, which contrast with the firm-level results depicted in Figure 8, suggest that the within-occupation upskilling effects are tempered by changes in the composition of job postings across occupations with varying schooling and experience requirements. Consistent with this interpretation, our heterogeneity analysis shows that the offsetting effect is more pronounced in states with lower minimum wages (see Figure 12), where the cost of hiring and firing is relatively low. In such environments, firms may be more willing to screen more intensively within certain occupations while simultaneously increasing hiring in

provide no evidence of changes in skill requirements following reformulation, either at the firm level or the firm-occupation level.

6.2 Heterogeneity Analysis

So far, we have established that employers who are more affected by the Opioid Crisis tend to increase the skill requirements for the same job positions disproportionately. Our model indicates that employers adopt this approach to screen out potential opioid users. To evaluate whether the empirical results of upskilling align with the context of the Opioid Crisis and the screening mechanism outlined in our model, we examine the heterogeneous impacts of the OxyContin reformulation on skill requirements across firm and labor market characteristics, including firm size, educational requirements, and local minimum wage levels.

First, we examine heterogeneity by firm size by estimating equation (13) separately for small, medium, and large firms.[28] Given that the costs associated with monitoring and hiring opioid abusers increase with firm size (Garen, 1985; Oi and Idson, 1999), larger firms may have a greater incentive to screen out these individuals during the hiring process. We present the coefficients and associated 95% confidence intervals for the difference-in-differences effects of the OxyContin reformulation on cognitive and computer skill requirements, as well as on years of schooling and experience, for each subgroup in Figure 10. Relatively large firms focused on raising computer skills, while small and medium-sized firms emphasized cognitive skills in response to the opioid crisis. This suggests that smaller firms may prioritize cognitive skills in their job postings because they are less costly to highlight compared to more technical skills. On the contrary, larger firms may be better positioned to raise computer skill requirements, which could require a corresponding pay raise, but are more efficient at screening out unskilled potential opioid abusers.

Second, we explore heterogeneity by firm education requirements. We categorize firms within each sector into two groups based on whether the percentage of job postings requiring a four-year college degree is above or below the median.[29] In Figure 11, we present coefficients and

lower-skilled roles, potentially with higher turnover.

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

[29] Due to missing job posting data for some firms in 2007, we utilize the 2010 data for consistent baseline education requirements. In 2010, the average percentage of job postings requiring a four-year degree was 14.5% for the low-requirement group and 59.6% for the high-requirement group. These figures are weighted according to the number

corresponding 95% confidence intervals for the difference-in-differences effects of the OxyContin reformulation, derived from equation (13). Our findings indicate that the effects of upskilling are significantly more pronounced among firms with a lower percentage of job postings requiring a four-year college degree at baseline. This aligns with previous research indicating that fatal opioid overdoses are more common among less-educated individuals (Case and Deaton, 2015; Altekruse et al., 2020). One possible explanation is that firms that depend on less-educated labor may experience greater increases in illicit opioid use among their employees and job candidates. As a result, these firms may need to raise their skill requirements more significantly to effectively screen job candidates or improve worker productivity.

Third, we explore heterogeneity in effects across firms in areas with low, medium, and high minimum wage levels.[30] It is known that minimum wage raises hiring bars as employers require higher productivity of applicants to ensure that hiring remains profitable (Butschek, 2022). Similarly, a higher minimum wage could raise the cost of hiring an opioid abuser with less productivity, inducing employers to raise hiring standards more in response to the Opioid Crisis. Figure 12 presents coefficients and 95% confidence intervals from the estimation of equation (13) for each group. We observe that the upskilling effects are notably more significant for firms exposed to 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.

Similarly, we analyze heterogeneity in effects on skill requirements across firms in areas with low, medium, and high employment protection levels in Appendix Figure A9.[31] 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

of job postings for each firm in 2010. Notably, we observed similar results when calculating baseline education requirements using averages from both 2007 and 2010 rather than just 2010.

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

[31] 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.

frameworks. These findings are consistent with the idea that in areas where the costs related to recruiting or terminating employees are higher, firms are more inclined to undertake rigorous screening of applicants or exhibit a stronger preference for individuals with higher skill levels.

6.3 Robustness Analysis

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

Continuous treatment. Our baseline specification treats exposure to the OxyContin reformulation as continuous. To assess the sensitivity of our results to this choice, we estimate a version of our event study specification (equation 12) using a binary treatment definition that classifies firms with above-median pre-reformulation exposure as treated and those below the median as the comparison group. Appendix Figure A10 compares the event study coefficients to those from this baseline continuous treatment specification. The two sets of estimates show similar dynamic patterns and lead to similar qualitative conclusions, suggesting that our findings are not driven by the continuous treatment specification.

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 13 presents coefficients and 95% confidence intervals from estimation of a version of equation (12) 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.[32] Most

[32] 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

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 5, 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.[33] 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 accounting for firm-specific time-varying factors.[34] Specifically, we run a version of equation (12) 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

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.

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

[34] 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).

the entity out of the total postings in that firm for that year.

Figure 14 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 Figure 14.

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 6, 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.[35]

This decline in coal production may have contributed to increases in both opioid use and unemployment. We explore how results change when we exclude these regions from our analysis. Appendix Figure A11 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

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

A8, we present event study estimates from equation (12) 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 3 (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 (13) 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).[36] 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 A12 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 A12, 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 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.

[36] 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.

7 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 examine 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 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 analysis of the transition from prescription to illicit opioids provides evidence that remains relevant to the continuing rise in illicit opioid use.

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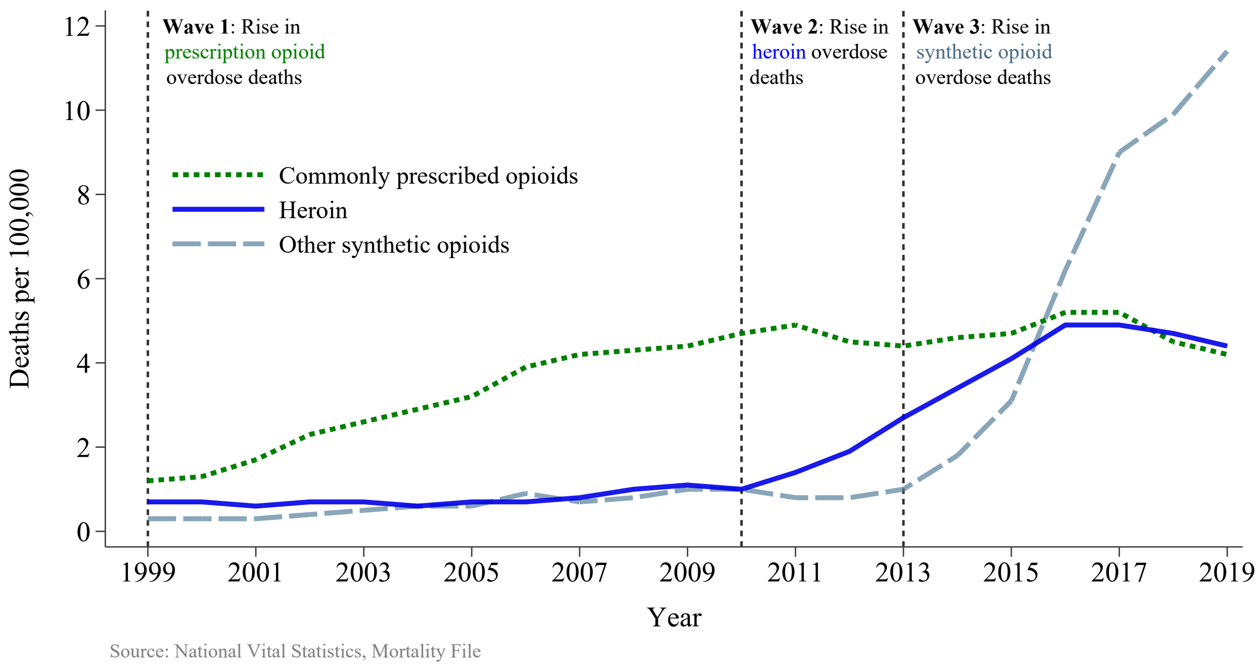
8 Figures and Tables

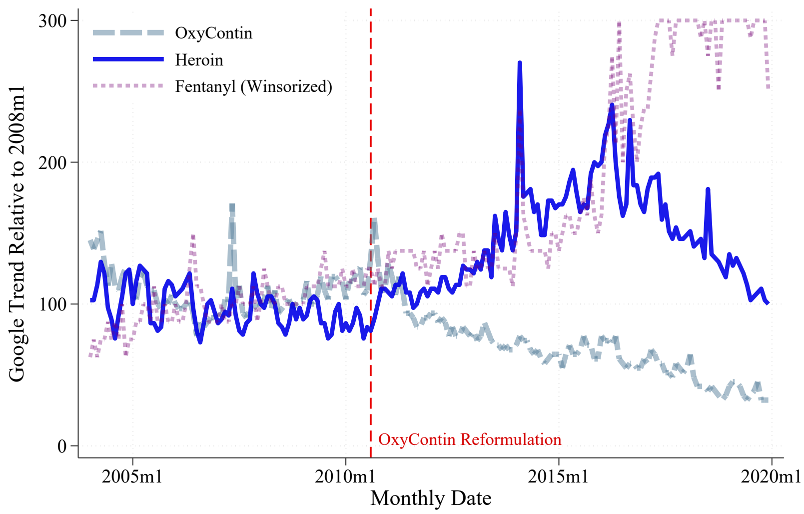
Figure 1: National Trends in Opioid Mortality



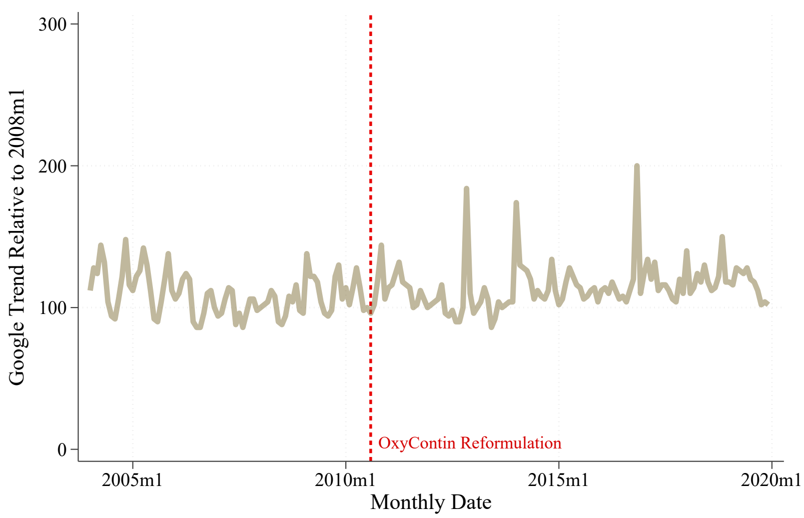
Notes: This figure plots national opioid overdose mortality rates (deaths per 100,000 population) from 1999–2019, separately for overdoses involving commonly prescribed opioids, heroin, and other synthetic opioids. The annotations highlight the three widely discussed "waves" of the opioid epidemic: the rise in prescription-opioid overdose deaths (Wave 1), the rise in heroin overdose deaths (Wave 2), and the rise in synthetic-opioid overdose deaths (Wave 3). Data are from the CDC National Vital Statistics System (NVSS) Mortality File (Multiple Cause of Death).

Figure 2: Interests in Opioids and Marijuana Over Time: Google Trends

(a) Google Trends: Opioids



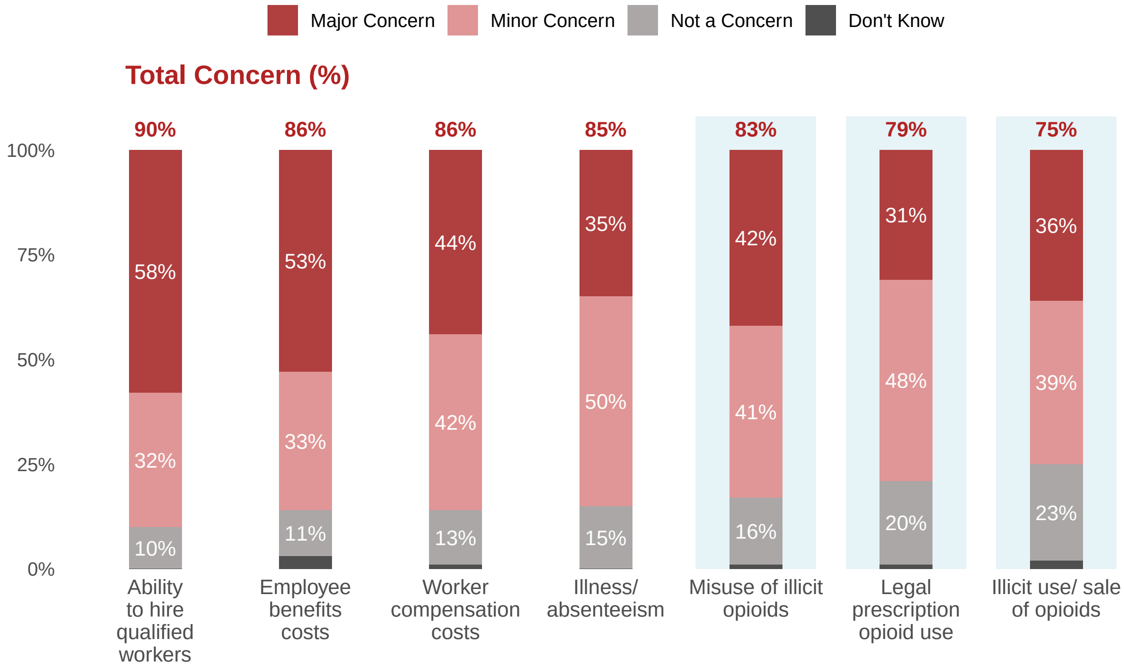
(b) Google Trends: Marijuana



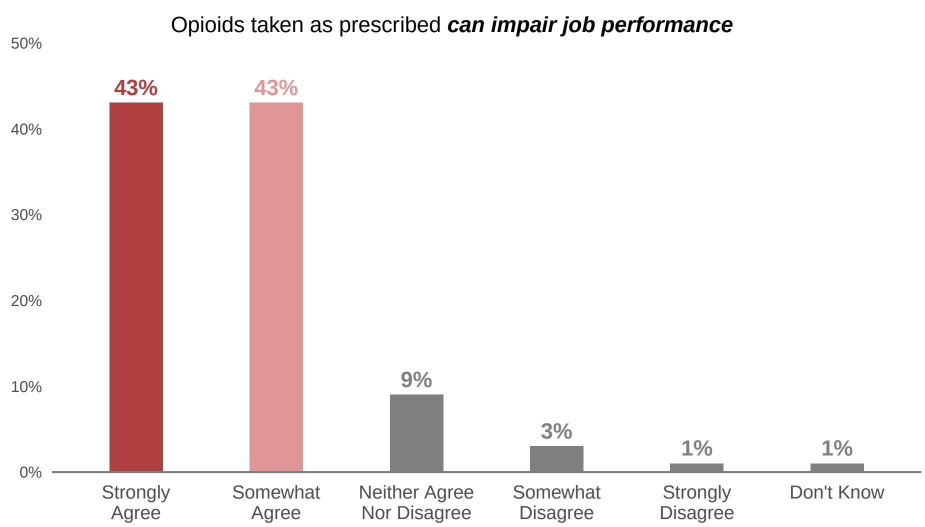
Notes: Panel (a) plots Google Trends search intensity for opioid-related keywords ("OxyContin," "Heroin," and "Fentanyl"; fentanyl is winsorized for readability) from 2005m1–2020m1. Panel (b) plots Google Trends search intensity for "Marijuana" over the same period. Each series is normalized to its value in 2008m1 (i.e., reported relative to 2008m1). The vertical dashed line marks the 2010 OxyContin reformulation.

Figure 3: Employer Workplace Concerns (National Safety Council)

(a) Employer Workplace Concerns



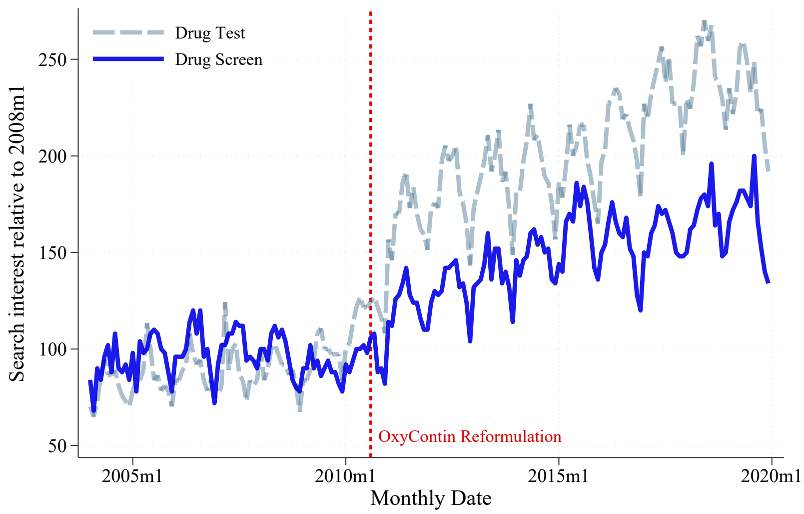
(b) Perceived Impact of Prescription Opioids on Job Performance



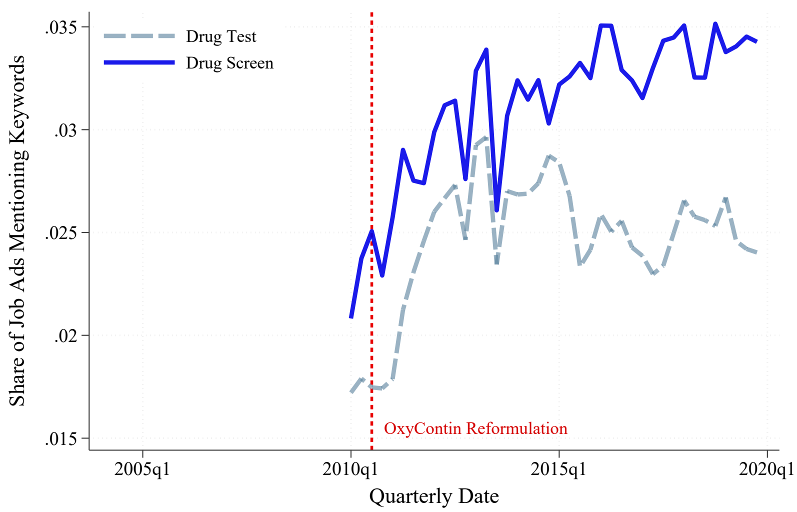
Notes: This figure is based on a 2020 survey conducted by the National Safety Council (NSC), which includes responses from 526 U.S. employer decision-makers working in organizations with 50 or more employees. Panel (a) displays the share of respondents who view each workplace issue as a "major" or "minor" concern, with the total percentage of concern indicated at the top of each bar. Light blue shading highlights three opioid-related issues. Panel (b) reports the perceived impact of prescribed opioid use on job performance. Respondents were asked to rate their level of agreement with the statement "Opioids taken as prescribed can impair job performance." Response options are shown in descending order of agreement, with bars representing the share of respondents selecting each option. Strong agreement responses are highlighted in dark red.

Figure 4: Trends in Drug Testing: Public Searches and Employer Job Ads

(a) Google Trend

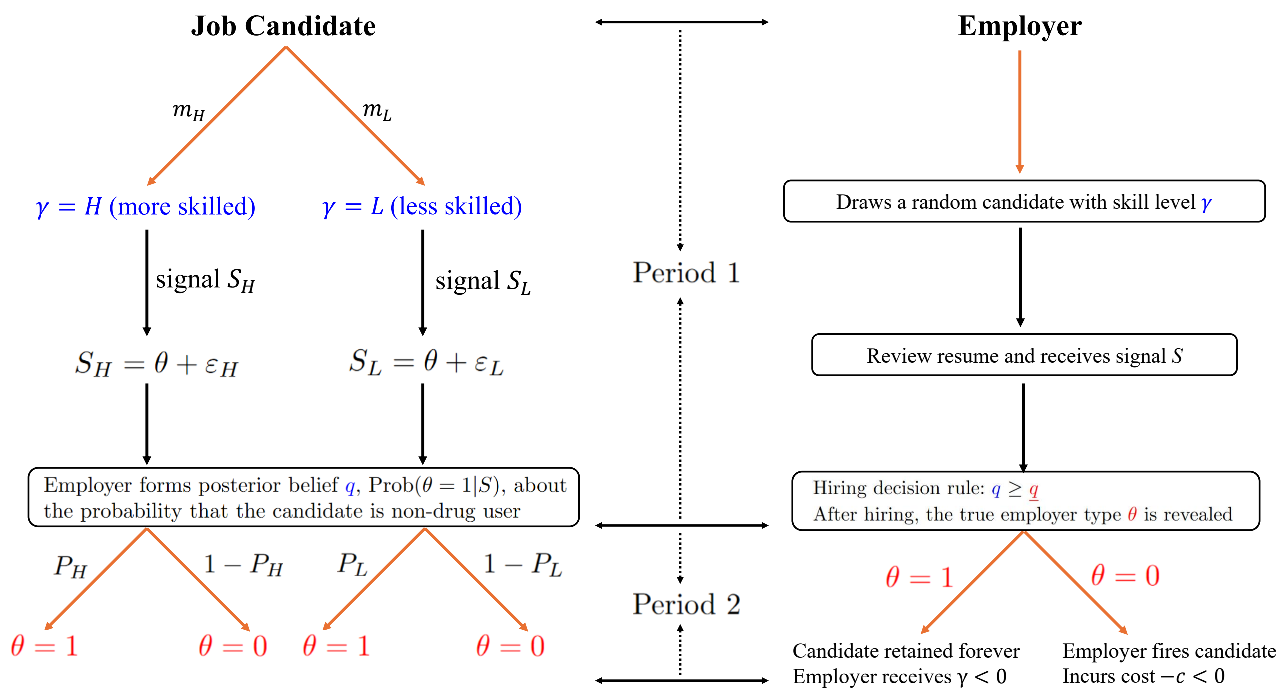


(b) Job Ads Mentioning Drug Screen



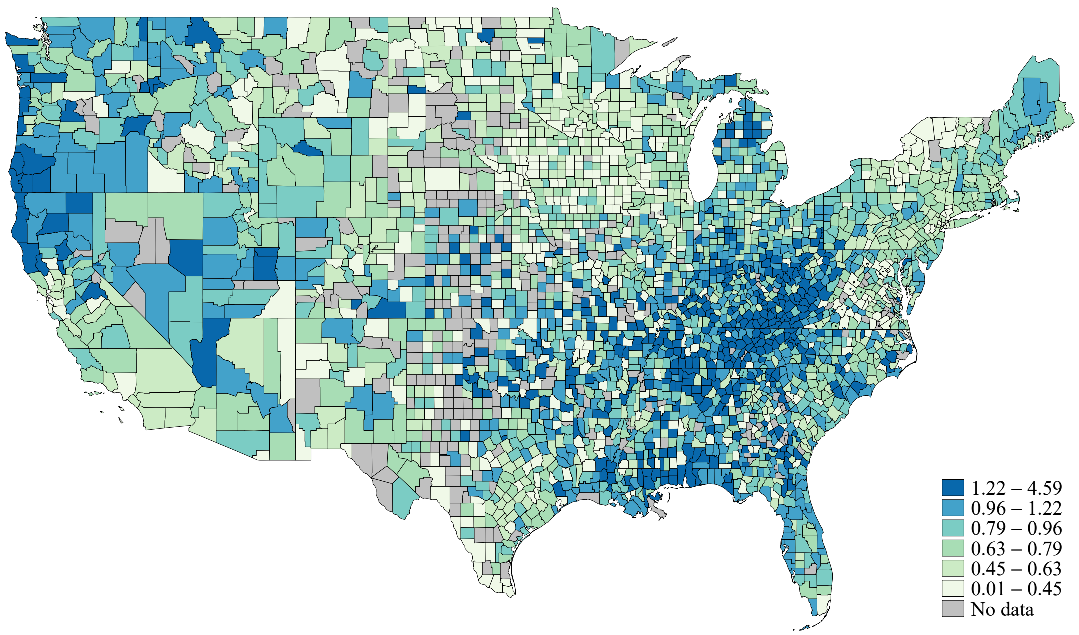
Notes: Panel (a) plots Google Trends search intensity for "Drug Test" and "Drug Screen," normalized to 2008m1, from 2005m1–2020m1. Panel (b) plots the quarterly share of online job postings that mention "drug test" or "drug screen." The vertical dashed line marks the August 2010 OxyContin reformulation.

Figure 5: Timing of the Model



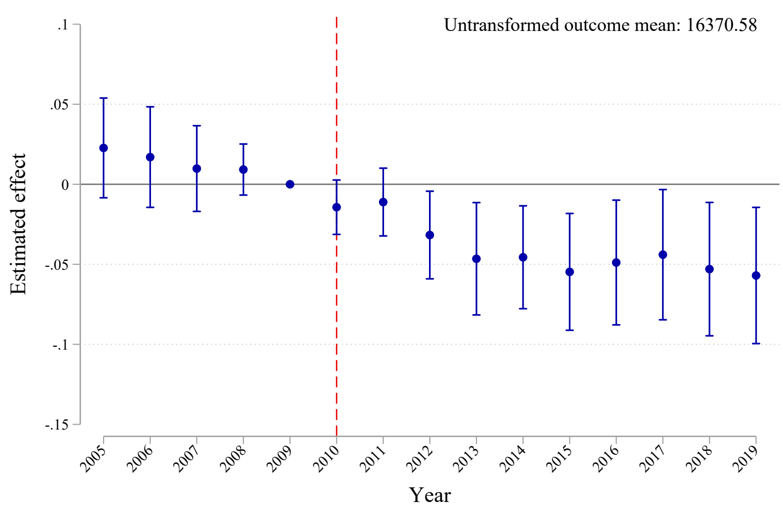
Notes: This figure illustrates the two-period hiring model for a specific occupation. In Period 1, the employer draws a random candidate from the applicant pool and observes their skill level γ (more-skilled H or less-skilled L) from their resume. The employer then reviews the resume and receives a noisy signal S about the candidate's productivity type θ, where $\theta = 1$ indicates high productivity (non-drug user) and $\theta = 0$ indicates low productivity (drug user). Based on the posterior belief q that the candidate is productive, the employer hires a job candidate only if the received signal exceeds the predetermined threshold $\underline{q}$. In Period 2, if hired, the candidate's true productivity type is revealed through work performance. High-productivity workers ($\theta = 1$) are retained and generate payoff $v > 0$, while low-productivity workers ($\theta = 0$) are dismissed, incurring cost c. The values P_H and P_L capture the probability that more-skilled and less-skilled candidates are high-productivity workers (i.e., non-drug users). In our model, we assume that P_H is greater than P_L, indicating that more-skilled candidates are more likely to be high-productivity (non-drug-using) workers.

Figure 6: 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).

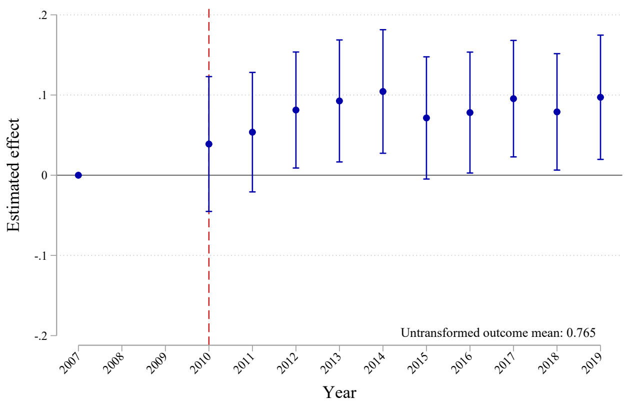
Figure 7: Effects of the OxyContin Reformulation on Firm Employment



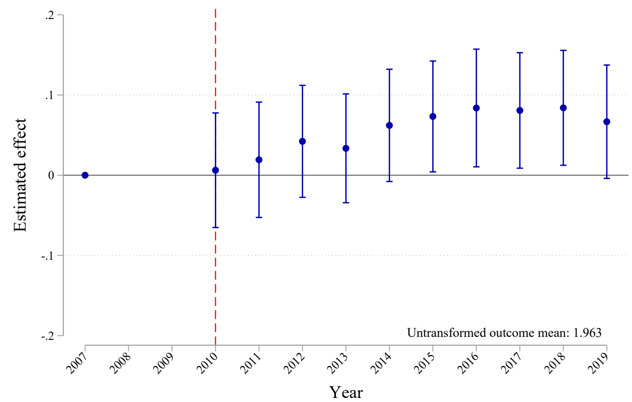
Notes: The figure shows the effect of firm-level exposure to the OxyContin reformulation on the log of employment. The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (12). The year 2009, which is one year prior to the OxyContin 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 8: Effects of the OxyContin Reformulation on Firm Skill Requirements

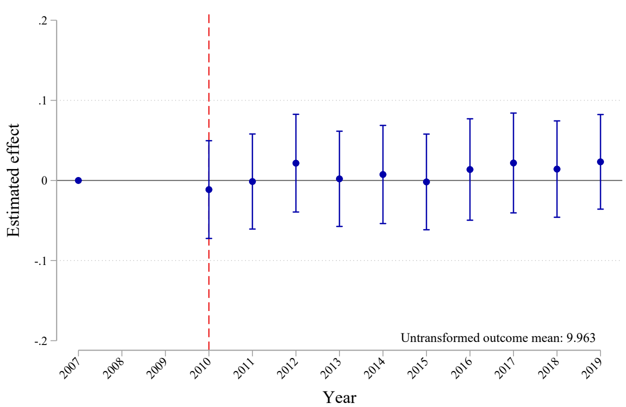
(a) Log(Number of Cognitive Skills)



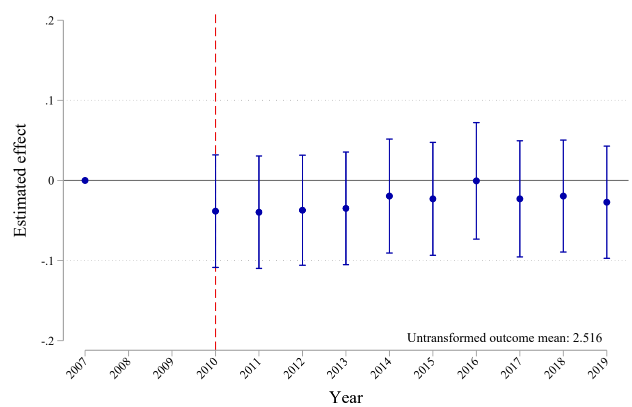
(b) Log(Number of Computer Skills)



(c) Log(Years of Schooling)

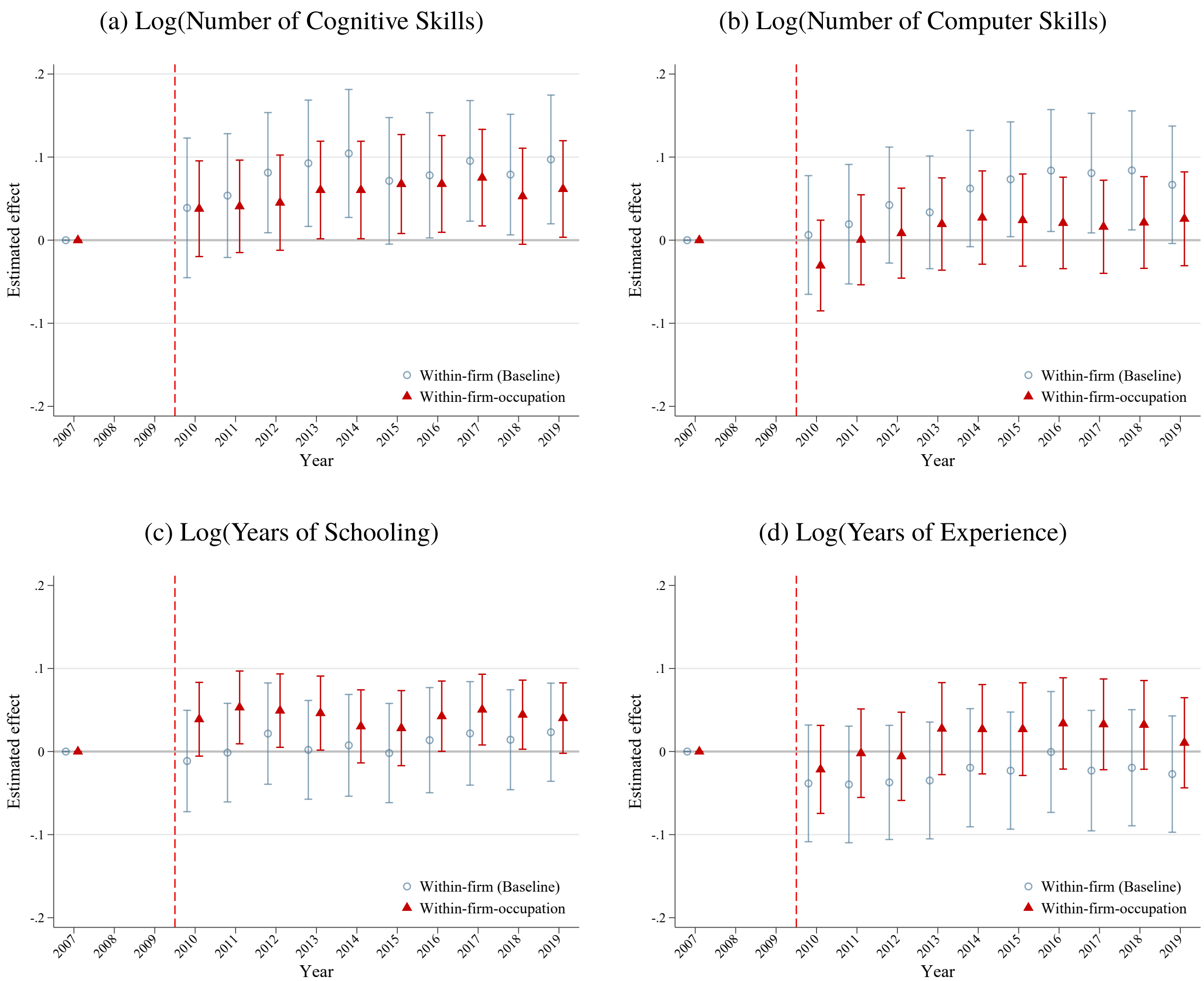


(d) Log(Years of Experience)



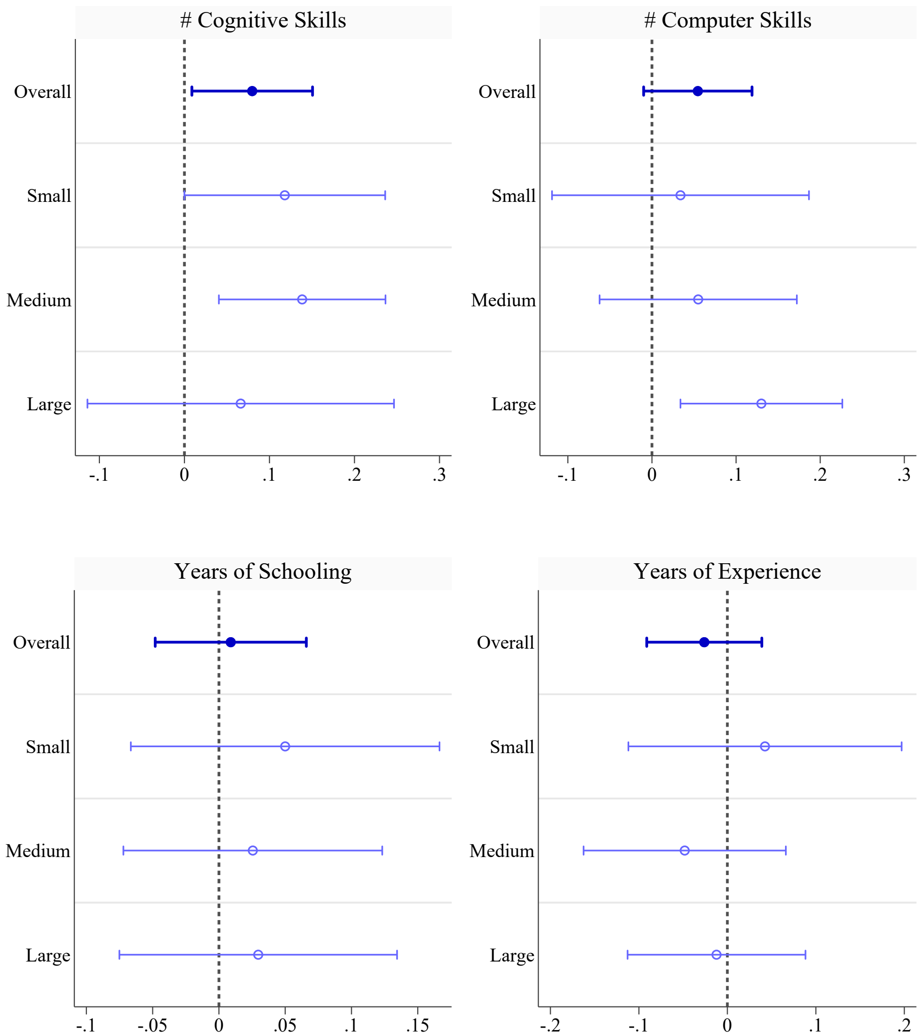
Notes: The figure shows the impact of firm-level exposure to the OxyContin reformulation on the following outcomes: average number of cognitive skills (Panel (a)), average number of computer skills (Panel (b)), average years of schooling (Panel (c)), and average years of experience (Panel (d)) required 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 (12). 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 9: Effects on Within-firm-occupation Changes in Skill Requirements



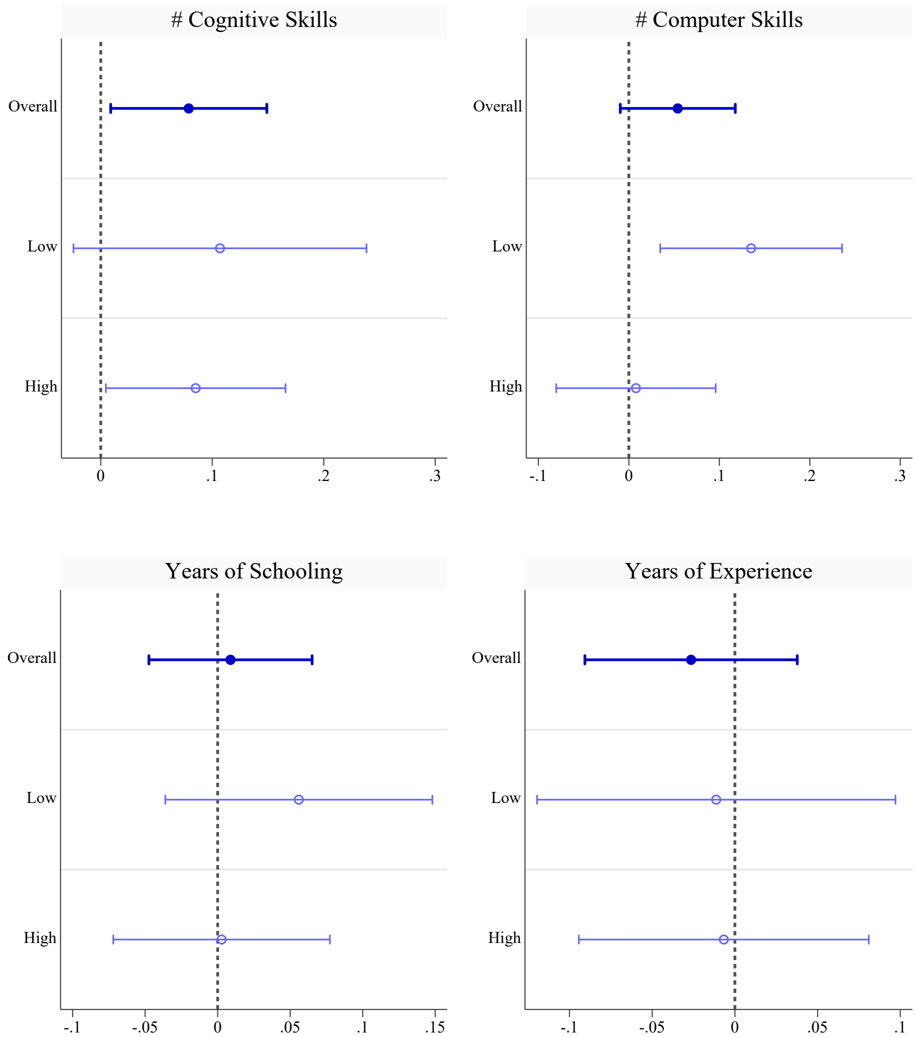
Notes: This figure compares event study estimates of the impact of firm-level exposure to the OxyContin reformulation on job skill requirements, using (i) our baseline firm-level specification ("Within-firm (Baseline)") and (ii) a within-firm-by-occupation specification ("Within-firm-occupation") constructed from a yearly firm $\times$ 6-digit SOC occupation panel. Outcomes are log-transformed measures of skill requirements in job postings: 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)). The figure plots the coefficients and corresponding 95% confidence intervals on the interaction terms between exposure and the full set of year dummies from an event study specification (analogous to equation (12)); year 2007 is the reference period and is normalized to zero. Standard errors are clustered at the firm level. The within-firm-occupation estimates additionally include firm-by-occupation fixed effects and occupation-by-year fixed effects, absorbing occupation-specific time trends.

Figure 10: Effects of the OxyContin Reformulation on Skill Requirements: Heterogeneity by Firm Size



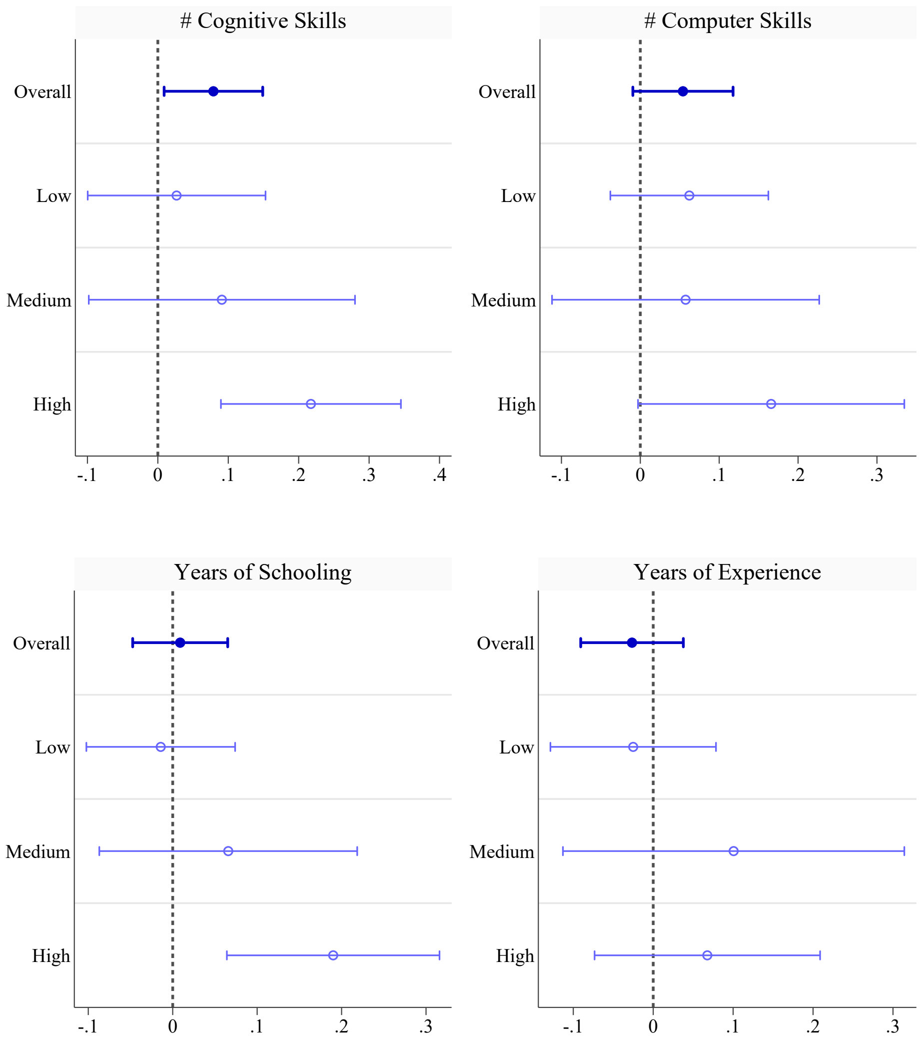
Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (13) separately for the sub-group denoted on the y-axis. Our baseline estimates are displayed at the top of each panel. All outcomes are log-transformed. Standard errors are clustered at the firm level.

Figure 11: 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 (13) 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 12: Effects of the OxyContin Reformulation on Skill Requirements: Heterogeneity by Minimum Wage Levels

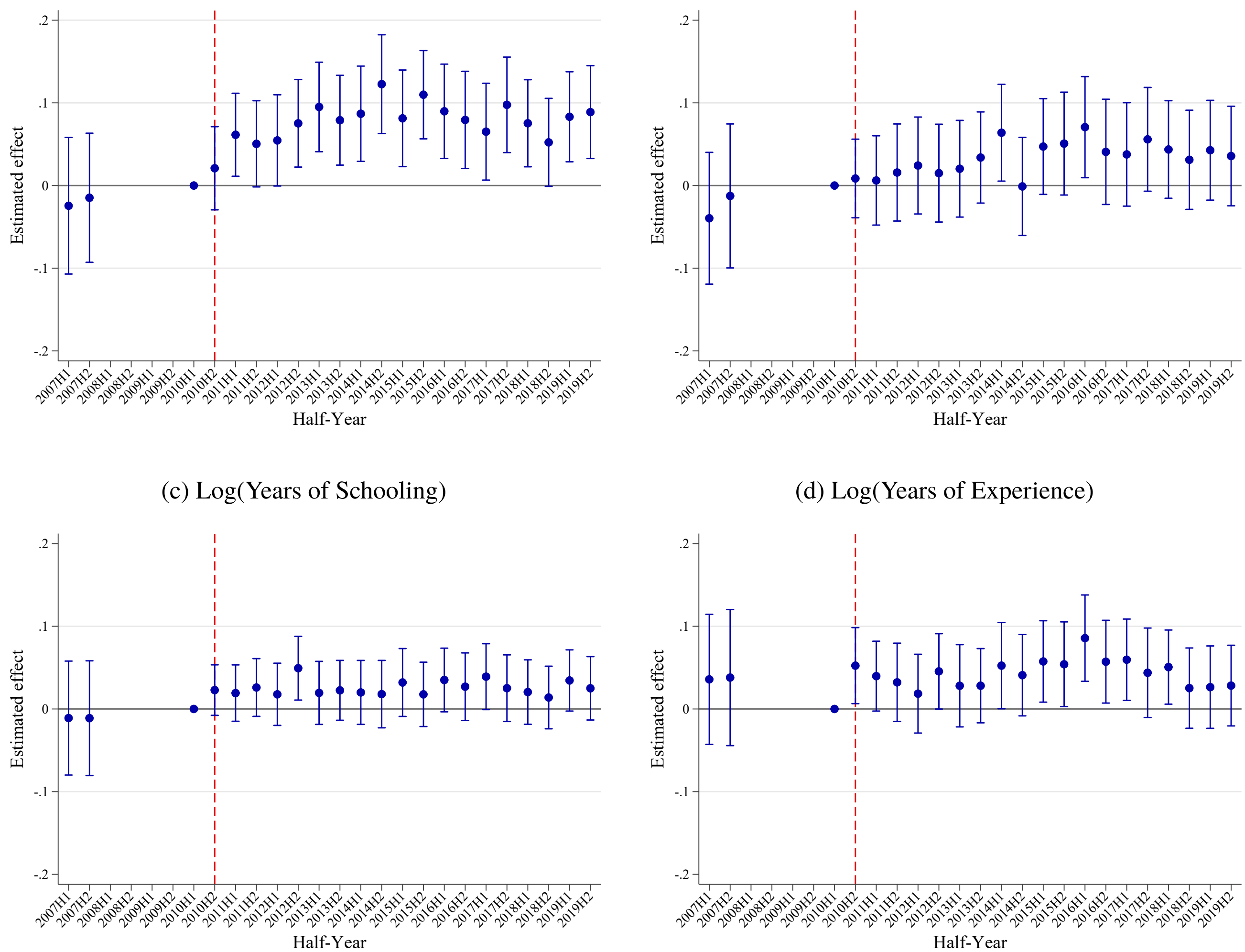


Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (13) separately for the sub-group denoted on the y-axis. Our baseline estimates are displayed at the top of each panel. Standard errors are clustered at the firm level.

Figure 13: Examining Pre-existing Trends Using Half-yearly Data

(a) Log(Number of Cognitive Skills) (b) Log(Number of Computer Skills)

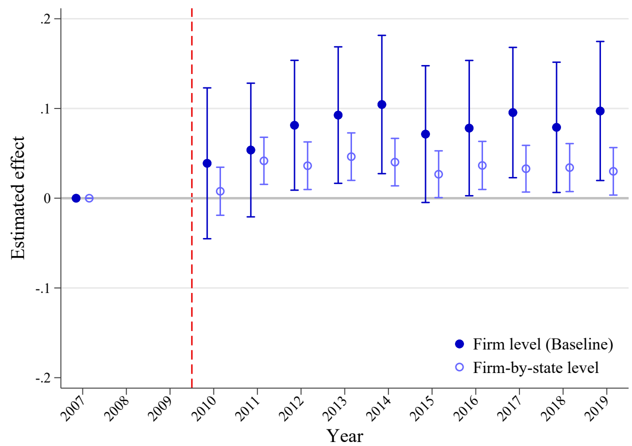
(c) Log(Years of Schooling) (d) Log(Years of Experience)



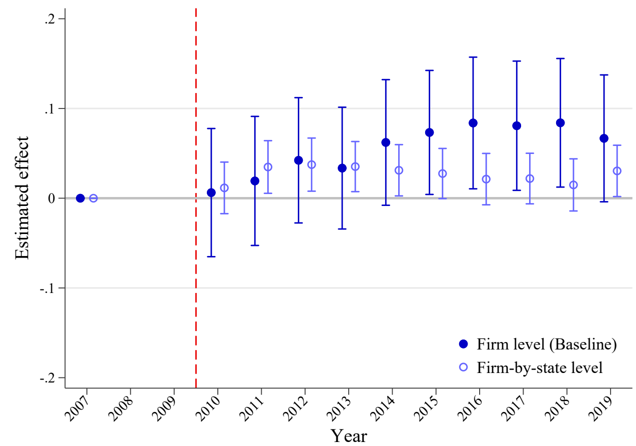
Notes: The figure shows the impact of firm-level exposure to the OxyContin reformulation on job skill requirements using half-yearly data. The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (12) for the following outcomes: average 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.

Figure 14: Effects of the OxyContin Reformulation on Skill Requirements: Firm-by-State Level Analysis

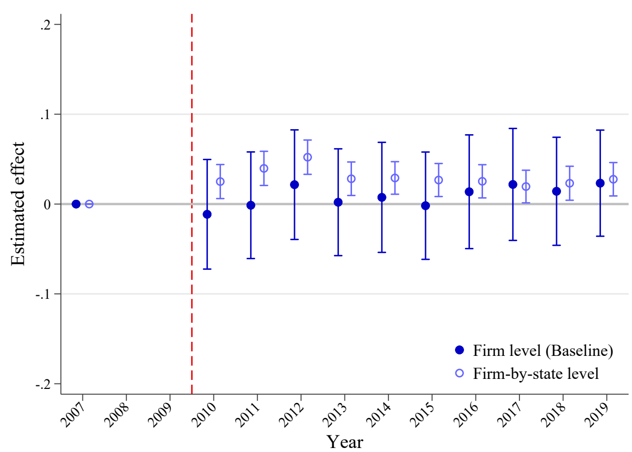
(a) Log(Number of Cognitive Skills)



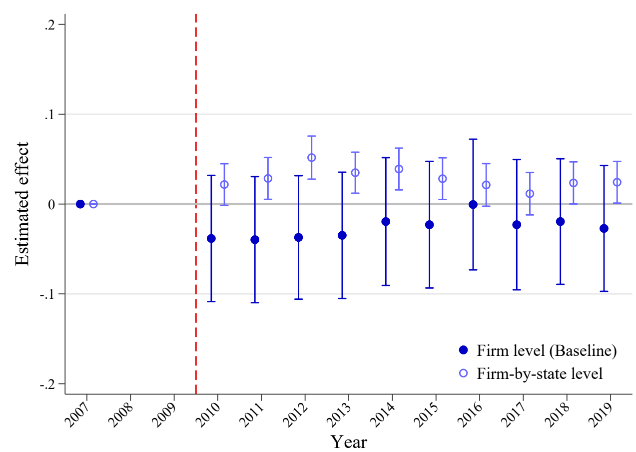
(b) Log(Number of Computer Skills)



(c) Log(Years of Schooling)



(d) Log(Years of Experience)



Notes: The figure presents output from estimation of equation (12) 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.

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	0.703	0.216	0.554	0.816	19870
B. Firm Outcomes, 2005–2019					
Employment	16,894	46,320	442	11,400	19,870
C. Skill Requirements in a Job Posting, 2007, 2010–2019					
Number of job postings	1634	6096	15	747	19,870
Number of cognitive skills	0.705	0.515	0.321	1.000	18,546
Number of computer skills	1.855	1.582	0.770	2.505	18,546
Years of schooling	9.509	4.236	6.894	12.553	18,546
Years of experience	2.394	1.540	1.282	3.326	18,546

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 employment 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	16370.58	0.765	1.963	9.963	2.516
Firm-year observations	26302	16954	17425	17583	17551

Notes: This table presents coefficients, standard errors (in parentheses), and p-values [in brackets] from estimation of equation (13). 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: 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]	[0.002]	[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 (13). 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.

For Online Publication

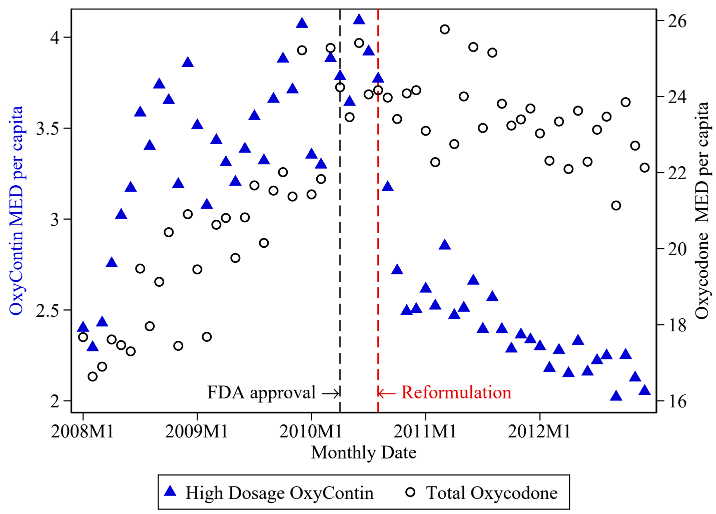
"Who Gets Screened Out? The Opioid Crisis and Employer Skill Requirements"

Kim, Kim, and Park (2026)

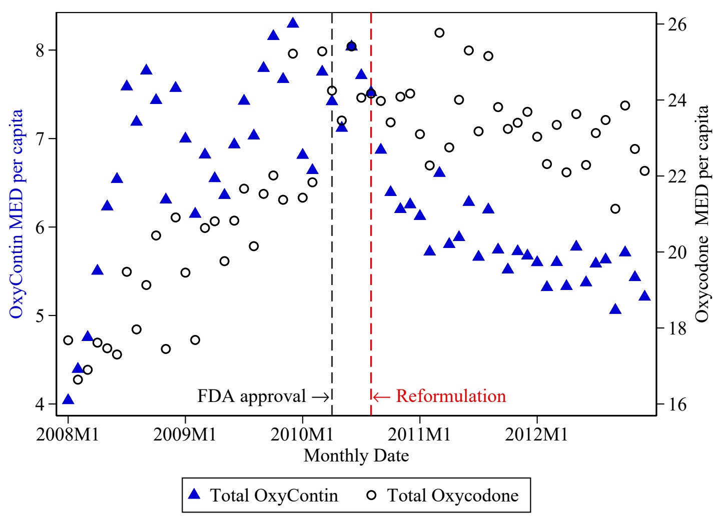
A Supplemental Figures and Tables

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

(a) High Dosage OxyContin and Total Oxycodone

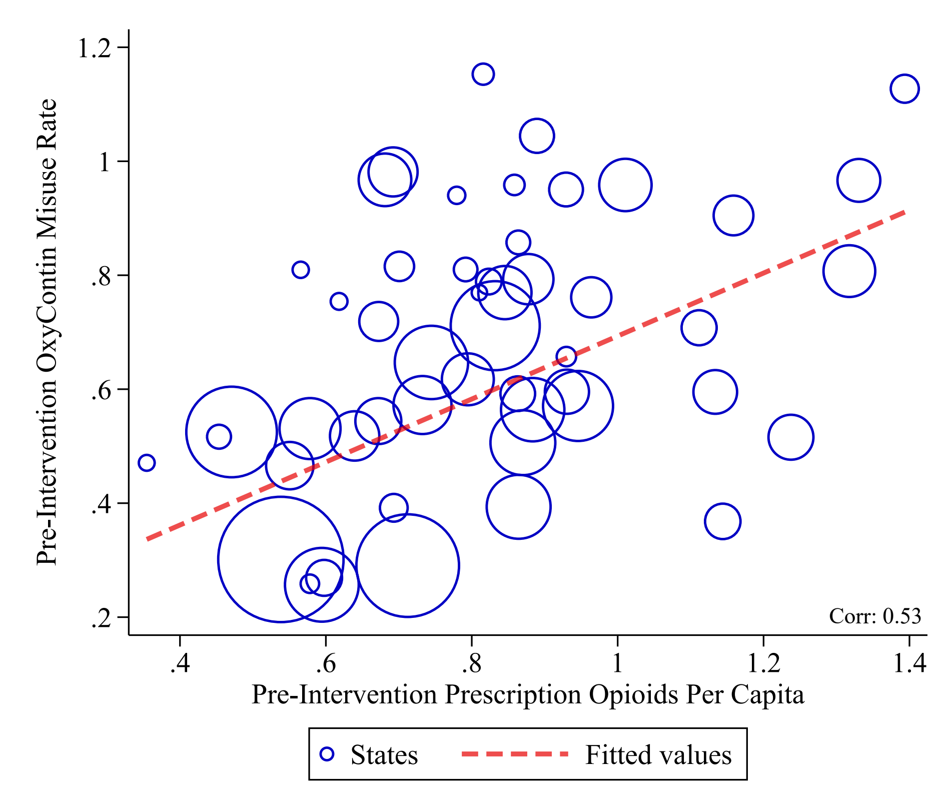


(b) Total OxyContin and Total Oxycodone



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.

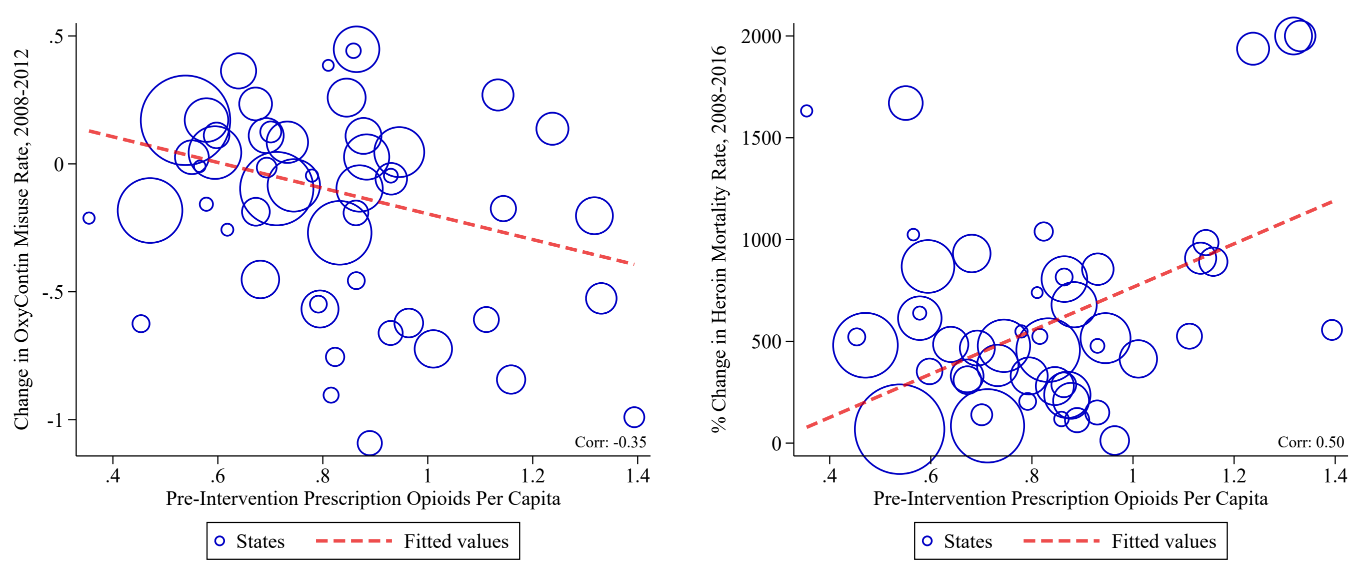
Figure A2: Relationship Between 2006–2009 Prescription Opioid Use Measure (CDC) and 2004–2009 OxyContin Misuse Measure (NSDUH)



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.

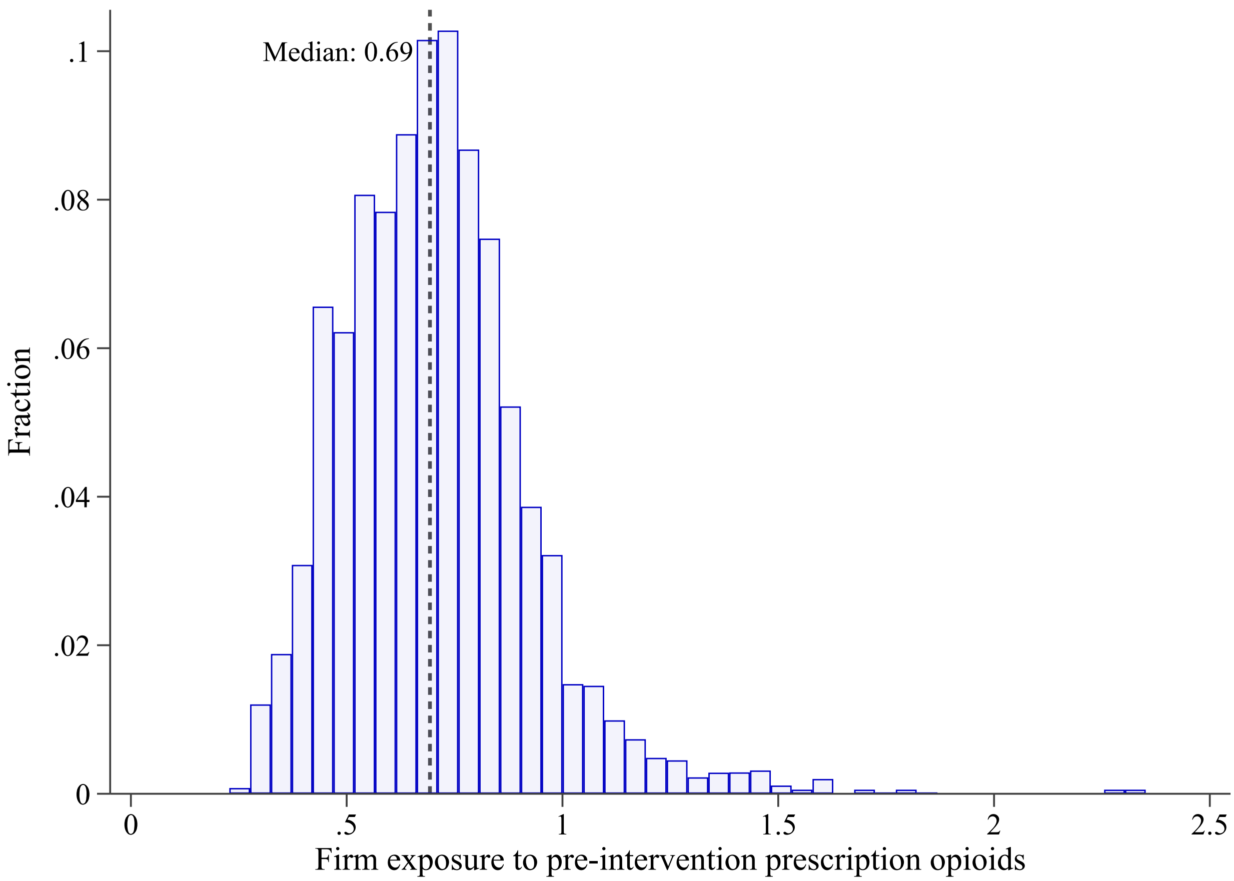
Figure A3: Relationship Between 2006–2009 Prescription Opioid Use Measure (CDC) and Changes in OxyContin Misuse and Changes in Heroin Mortality

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



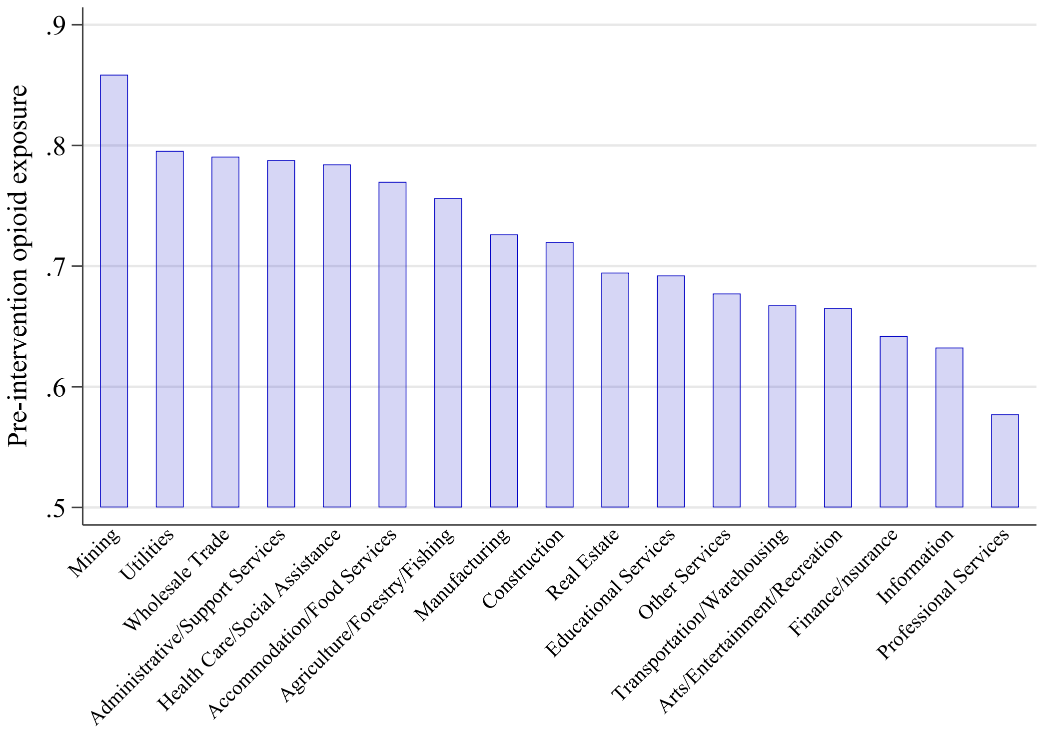
Notes: The figure presents the state-level averages of opioid prescriptions per capita from 2006 to 2009 (CDC data) and subsequent changes. Panel (a) shows the relationship between the CDC measure and level changes in OxyContin misuse rates between 2008 and 2012, and Panel (b) presents the relationship 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 A4: Distribution of Firm Exposure to Pre-Intervention Prescription Opioids



Notes: This figure shows a histogram of the firm-level pre-intervention exposure measure used in the analysis. A firm's exposure is constructed as a Bartik-style weighted average of county-level per-capita Schedule II prescription opioid prescriptions over the pre-intervention period (2006–2009) across the firm's establishments, where weights are based on each establishment's pre-intervention number of job ads (used to proxy establishment size). The vertical marker reports the sample median (0.69).

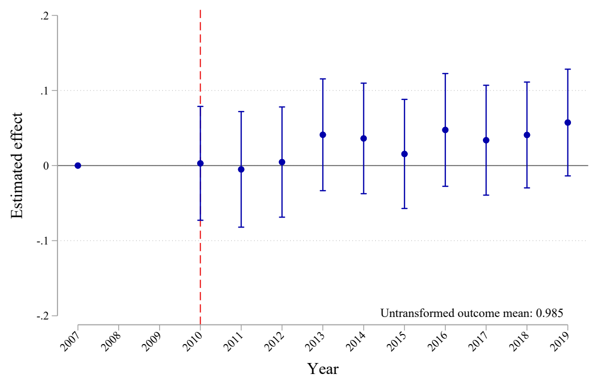
Figure A5: Firm Exposure to Pre-Intervention Prescription Opioids by Industry



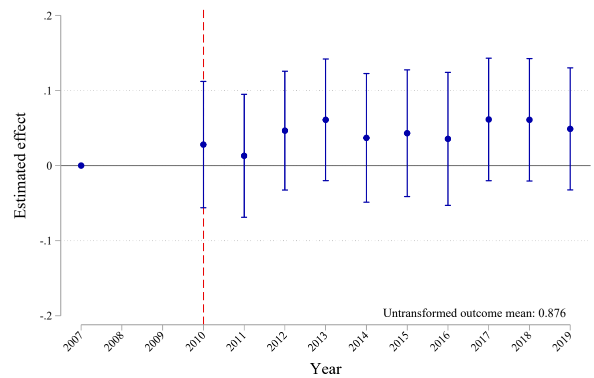
Notes: This figure summarizes firm exposure to pre-intervention prescription opioids by industry group. Firm exposure is the Bartik-style weighted average of county-level per-capita Schedule II prescription opioid prescriptions in 2006–2009 across the firm's establishments, weighted by pre-intervention establishment-level job-ad counts.

Figure A6: Effects of the OxyContin Reformulation on Skill Requirements: Other Skills

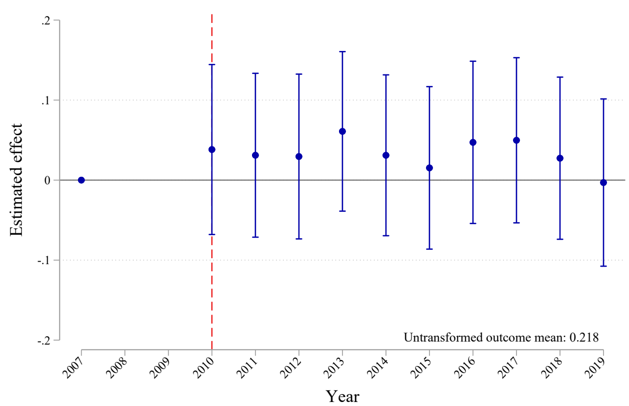
(a) Log(Number of Social Skills)



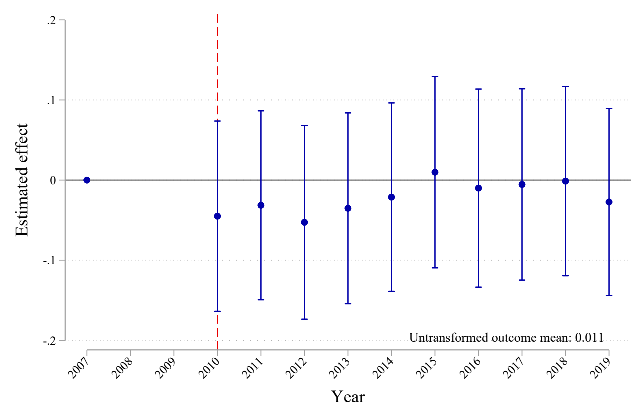
(b) Log(Number of Service Skills)



(c) Log(Number of Management Skills)

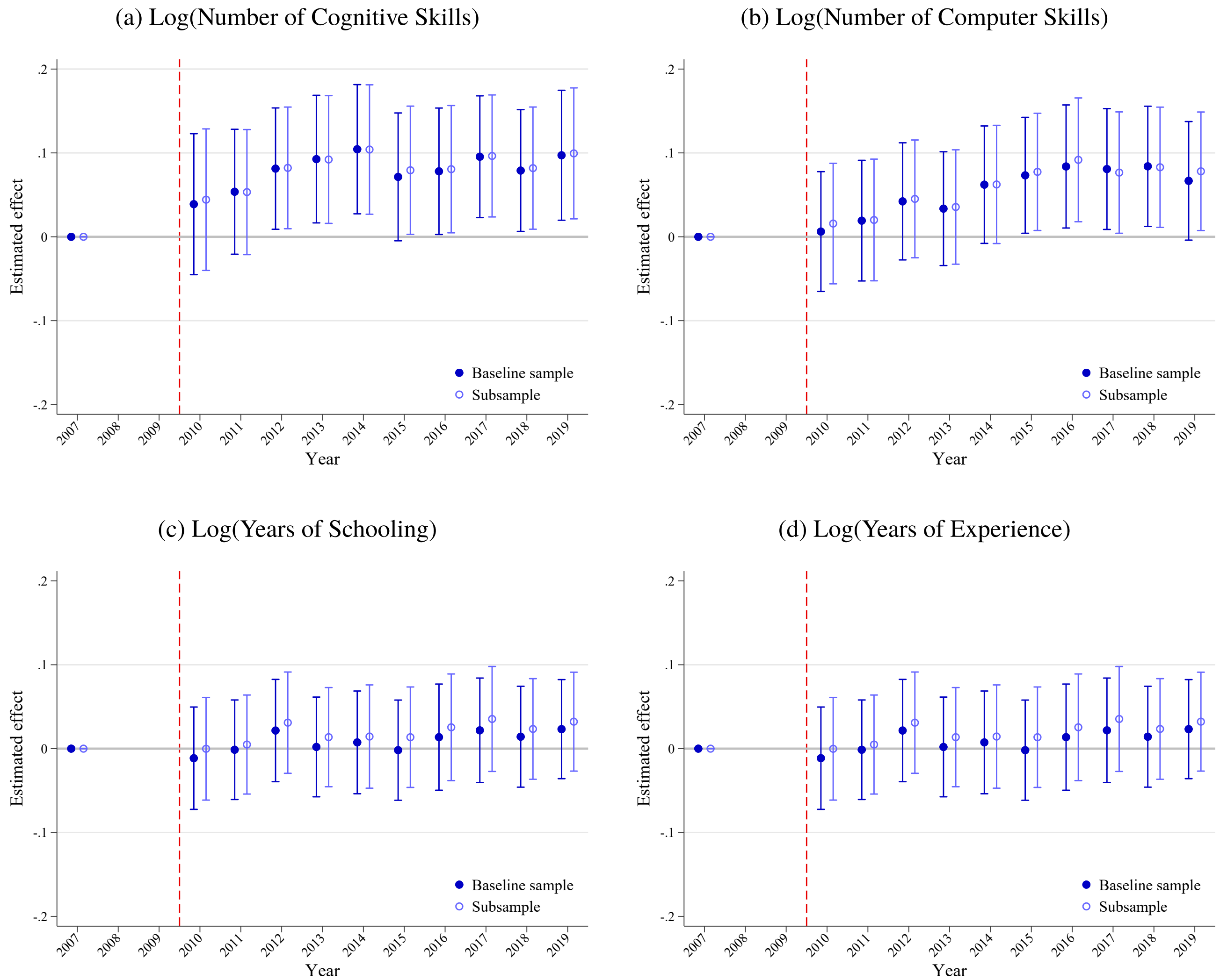


(d) Log(Number of Writing Skills)



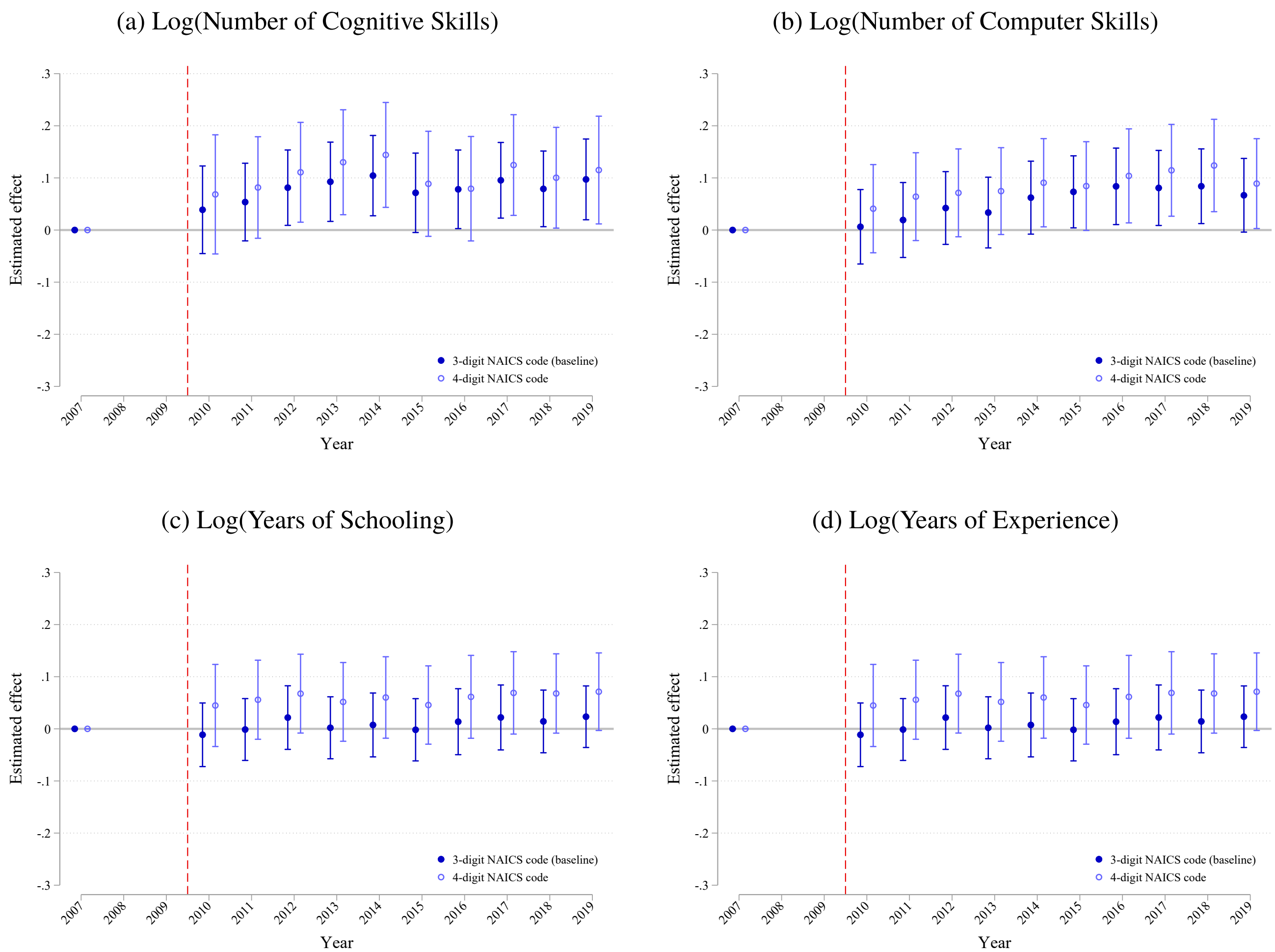
Notes: The figure shows the impact of firm-level exposure to the OxyContin reformulation on the following outcomes: average number of social skills (Panel (a)), average number of service skills (Panel (b)), average number of management skills (Panel (c)), and average number of writing skills (Panel (d)). All outcomes are log-transformed. The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (12). 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 A7: Robustness of Baseline Regression Estimates to Using the Firm-Occupation Analysis Sample



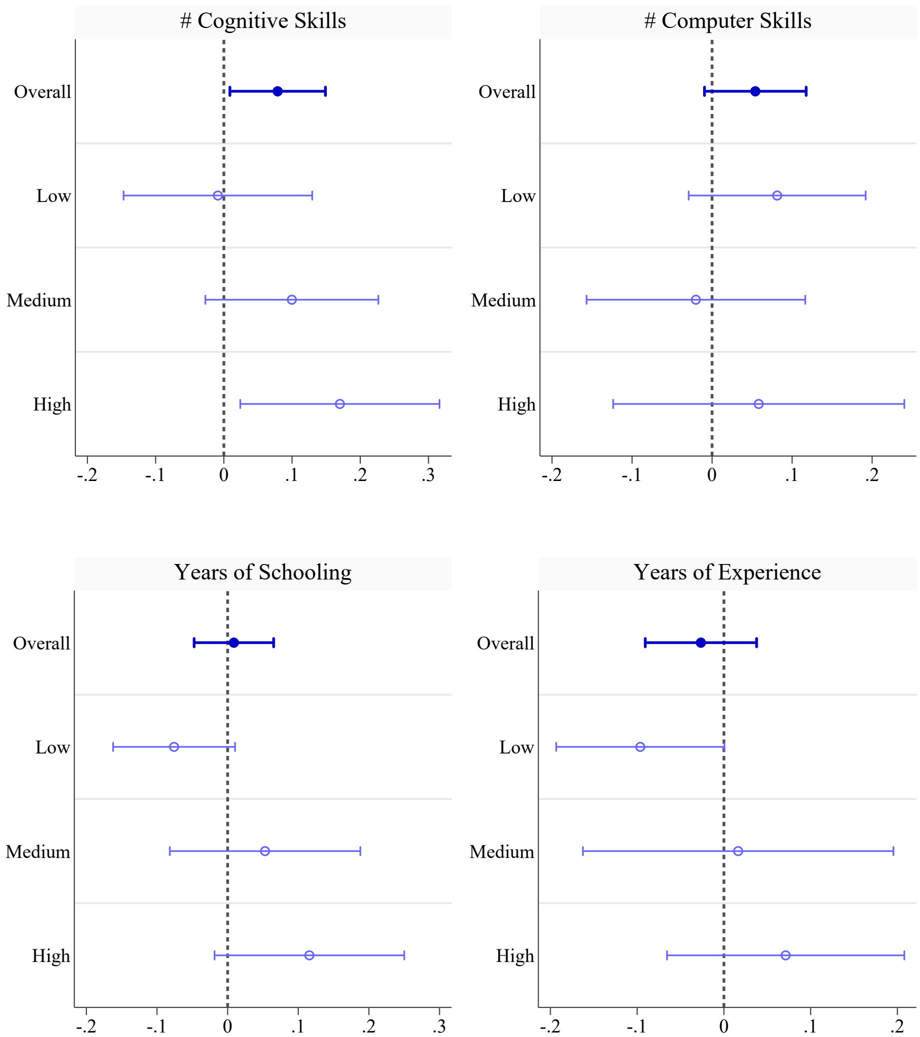
Notes: The figure presents output from the baseline regression model (12) estimated on two samples: the baseline sample (dark blue circles) and a subsample consisting of firms included in the firm-occupation analysis (light blue hollow circles). Both samples are based on firm-level panel data. For each sample, 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 A8: Robustness of the Skill Requirement Estimates to Using a 4-Digit Industry Code



Notes: The figure presents results from equation (12) 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.

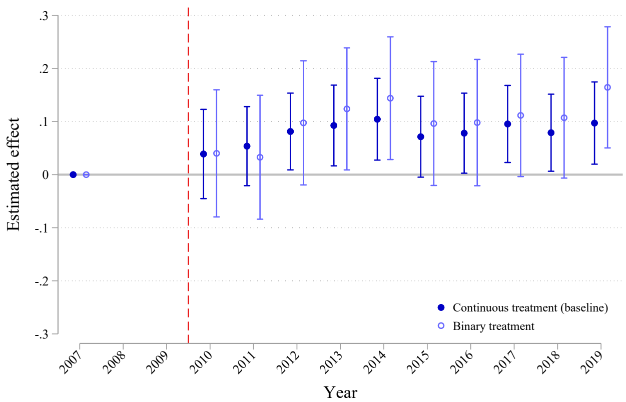
Figure A9: Effects of the OxyContin Reformulation on Skill Requirements: Heterogeneity by Employment Protection Levels



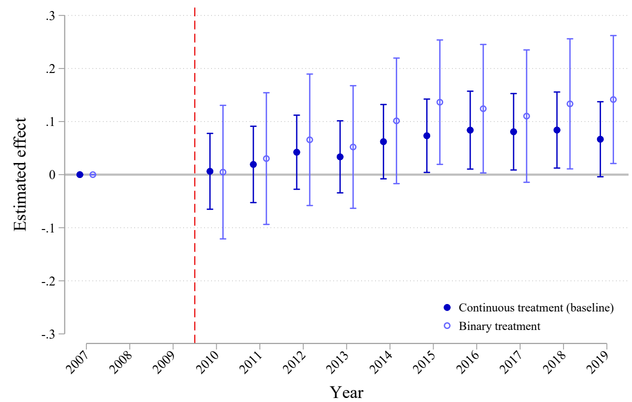
Notes: The figure displays the coefficients and their corresponding 95% confidence intervals on the interaction terms from equation (13) separately for the sub-group denoted on the y-axis. Our baseline estimates are displayed at the top of each panel. Standard errors are clustered at the firm level.

Figure A10: Robustness of Skill Requirement Estimates to a Binary Treatment Measure of Pre-Reformulation Exposure

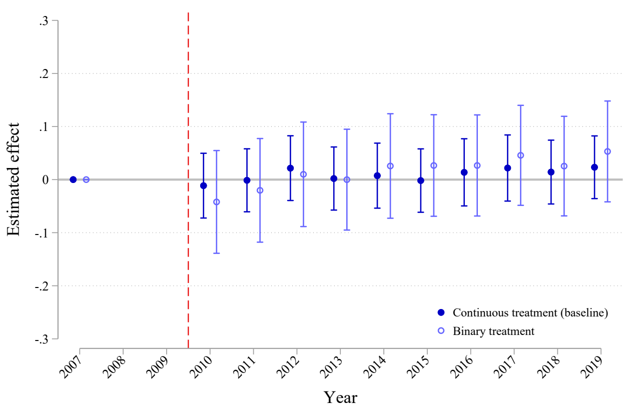
(a) Log(Number of Cognitive Skills)



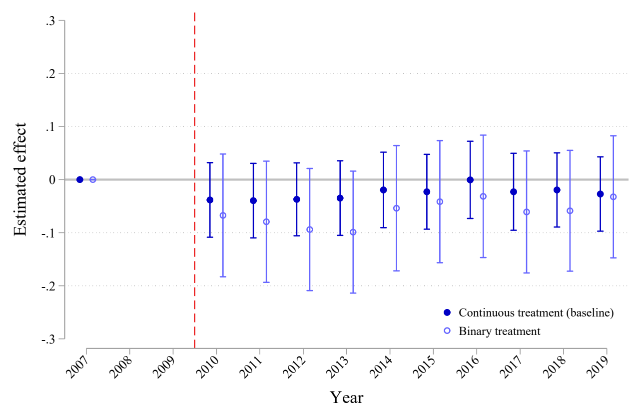
(b) Log(Number of Computer Skills)



(c) Log(Years of Schooling)



(d) Log(Years of Experience)



Notes: The figure presents estimates from two specifications of equation (12). The baseline specification uses the continuous measure of pre-reformulation exposure to local prescription opioid use (solid circles). The alternative specification replaces the continuous exposure measure with a binary treatment indicator (hollow circles), defined as an indicator for firms with above-median pre-reformulation exposure. The figure plots the coefficients and 95% confidence intervals on the interactions between treatment and year indicators for each specification. Outcomes are log-transformed measures of skill requirements in job postings: 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)). The reference year is 2007, normalized to zero. Standard errors are clustered at the firm level.

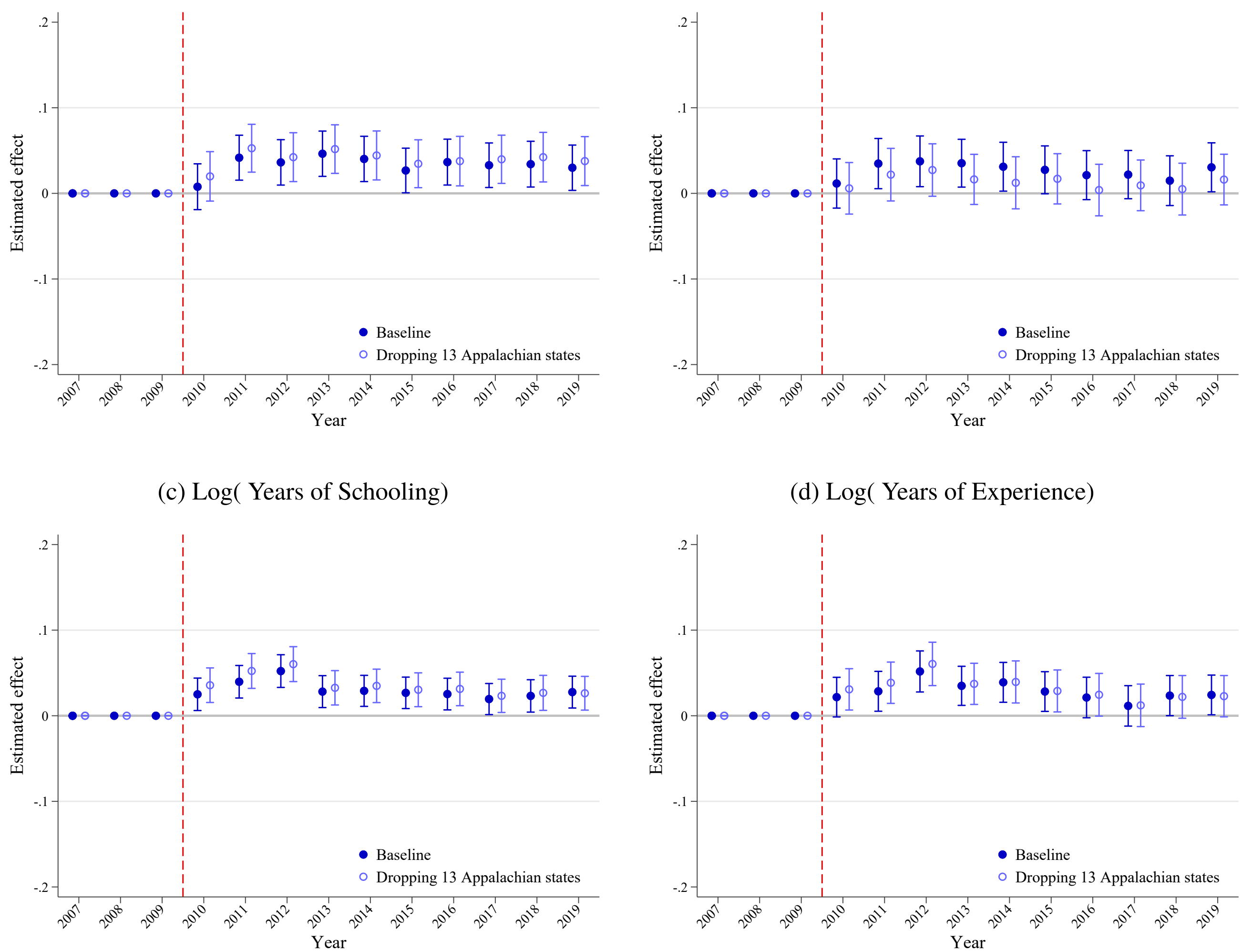
Figure A11: Robustness of the Firm-by-State Analysis Estimates to Dropping Appalachian states

(a) Log(Number of Cognitive Skills)

(b) Log(Number of Computer Skills)

(c) Log(Years of Schooling)

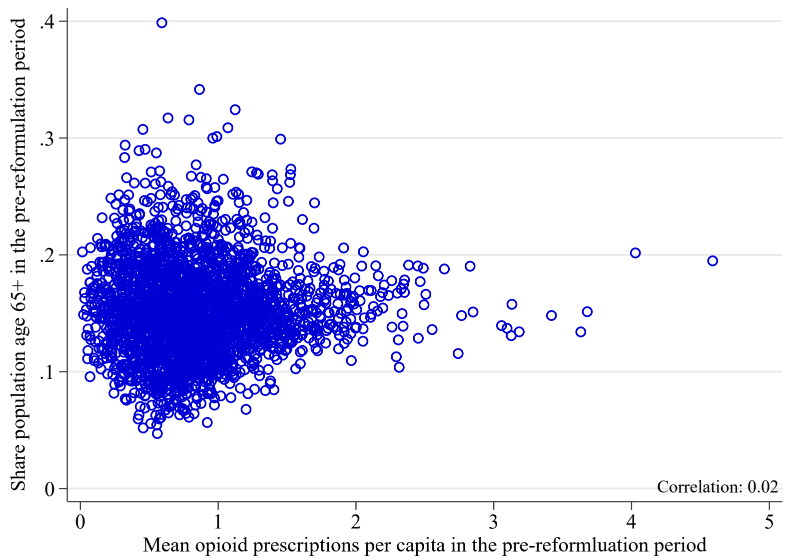
(d) Log(Years of Experience)



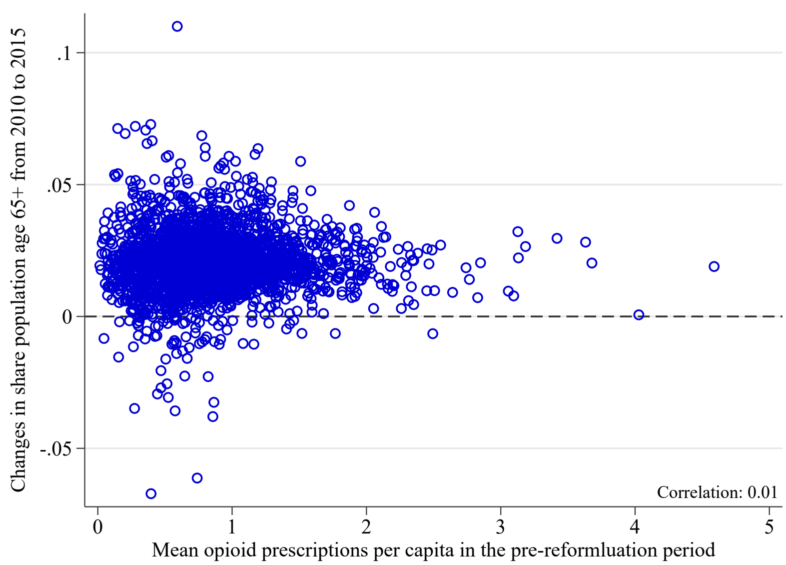
Notes: The figure presents output from estimation of equation (12) using our state-by-year sample (circles in dark blue) and the same sample with the exclusion of 13 Appalachian states (hollow circles in light blue). For both samples, we plot the coefficients and 95% confidence intervals on the interactions 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 A12: 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: Employer Response to Substance Misuse by Substance Type

Response	**Prescrip. Opioids**	Prescrip. Stimulants	Benzodiazepines	Alcohol	Legal Marijuana	Illicit Marijuana	**Illicit Opioids**	**Heroin, Fentanyl**	Other Illicit Drugs
Ignore the problem	**10%**	9%	5%	4%	14%	4%	**3%**	**2%**	1%
Return them to position after treatment	**41%**	39%	39%	44%	27%	31%	**25%**	**25%**	22%
Ensure they are carefully monitored for rest of employment	31%	31%	26%	24%	27%	16%	17%	15%	17%
Relocate to position of lesser responsibility	9%	8%	8%	5%	7%	6%	6%	4%	5%
Dismiss them	9%	13%	21%	23%	24%	43%	49%	54%	55%

Notes: This table summarizes how employers respond to different types of substance misuse in the workplace, based on a survey conducted by the National Safety Council (NSC). The data reflect responses from 526 employer decision makers representing U.S. organizations with 50 or more employees. Respondents were asked: *"Which of the following would you say best reflects your organization's approach to an employee who is found to be misusing...?"* Employers selected one of the following options: ignore the problem, return the employee to their position after treatment, ensure the employee is carefully monitored for the remainder of their employment, relocate the employee to a position of lesser responsibility, or dismiss them. Responses are shown by substance type, including prescription opioids, prescription stimulants, benzodiazepines, alcohol, legal marijuana, illicit marijuana, illicit opioids, heroin/fentanyl, and other illicit drugs.

Table A2: 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 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***	-0.050***	-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 (13). 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.

B Proof of Propositions and Policy Implications

POLICY IMPLICATION 1: *When the proportion of drug users increases in the population of low-skill workers (i.e., p_L decreases), the optimal hiring threshold, $\underline{q}^*$, increases.*

(Proof) Note that:

$$\begin{aligned}\int_{\underline{q}}^{1}(1-q)dG(q) &= \int_{\underline{q}}^{1} 1dG(q) - \int_{\underline{q}}^{1} qdG(q)\\ &= [G(q)]_{q=\underline{q}}^{q=1} - \int_{\underline{q}}^{1} qdG(q)\\ &= 1-G(q) - \int_{q}^{1} qdG(q).\end{aligned}$$

Plug this into the value function of Equation (7), we have:

$$\begin{aligned}V(\underline{q}) &= \frac{\delta\nu\int_{\underline{q}}^{1} qdG(q) - \delta c\left[1-G(\underline{q}) - \int_{\underline{q}}^{1} qdG(q)\right] - k}{1-\delta\left(1-\int_{\underline{q}}^{1} qdG(q)\right)}\\ &= \frac{\delta\nu\int_{\underline{q}}^{1} qdG(q) - \delta c(1-G(\underline{q})) + \delta c\int_{\underline{q}}^{1} qdG(q) - k}{1-\delta\left(1-\int_{\underline{q}}^{1} qdG(q)\right)}.\end{aligned}$$

Now let's define $N \equiv \delta\int_{\underline{q}}^{1}[q\nu + (1-q)(-c)]dG(q) - k$ and $D \equiv 1-\delta\left(1-\int_{\underline{q}}^{1} qdG(q)\right)$. Then we have $V(\underline{q}) \equiv \frac{N}{D}$. Now the F.O.C condition gives:

$$\frac{\partial V(\underline{q})}{\partial \underline{q}} = \frac{N'D - D'N}{D^2} = 0.$$

From the Leibniz Integral Rule, we have:

$$\begin{aligned}N' \equiv \frac{\partial N}{\partial \underline{q}} &= -\delta(\underline{q}\nu + (1-\underline{q})(-c))g(\underline{q})\cdot 1\\ &= -\delta g(\underline{q})[\underline{q}\nu - c + c\underline{q}] = -\delta g(\underline{q})[\underline{q}(\nu+c) - c]\\ D' \equiv \frac{\partial D}{\partial \underline{q}} &= \delta[-\underline{q}g(\underline{q})\cdot 1] = -\delta\underline{q}g(\underline{q}).\end{aligned}$$

From this we have:

$$\begin{aligned}\frac{\partial V(\underline{q})}{\partial \underline{q}} &= \frac{-\delta g(\underline{q})[\underline{q}(\nu+c)-c]D+\delta \underline{q} g(\underline{q})N}{D^2} \\ &= \delta g(\underline{q})\cdot\frac{-D[\underline{q}(\nu+c)-c]+\underline{q}N}{D^2} \\ &= \delta g(\underline{q})\cdot\frac{-D\underline{q}(\nu+c)+Dc+\underline{q}N}{D^2}=0.\end{aligned}$$

Hence, $-D(v+c)q+DC+Nq=0$, so we have

$$\underline{q}^* = \frac{Dc}{D(v+c)-N}.$$

We now substitute $D\&N$ to get:

$$\begin{aligned}\underline{q}^* &= \frac{c\left[1-\delta\left(1-\int_{\underline{q}^*}^1 qdG(q)\right)\right]}{\left[1-\delta\left(1-\int_{\underline{q}^*}^1 qdG(q)\right)\right](\nu+c)-\delta\int_{\underline{q}^*}^1[\nu q+(1-q)(-c)]dG(q)+k} \\ &= \frac{\left(1-\delta\left(1-\int_{\underline{q}^*}^1 qdG(q)\right)\right)c}{\left(1-\delta G\left(\underline{q}^*\right)\right)c+(1-\delta)v+k}.\end{aligned}$$

We further rewrite this condition:

$$\begin{aligned}\underline{q}^*\left[c-\delta cG\left(\underline{q}^*\right)+(1-\delta)\nu+k\right] &= c-c\delta\left[1-\int_{\underline{q}^*}^1 qdG(q)\right] \\ \underline{q}^*(c+(1-\delta)\nu+k)-\delta c\underline{q}^*G\left(\underline{q}^*\right) &= c-c\delta\left[1-\int_{\underline{q}^*}^1 qdG(q)\right] \\ \underline{q}^*(c+(1-\delta)\nu+k) &= c-c\delta\left[1-\int_{\underline{q}^*}^1 qdG(q)\right]+\delta c\underline{q}^*G\left(\underline{q}^*\right).\end{aligned}$$

From Integartion by parts, we get:

$$\begin{aligned}\int_{\underline{q}^*}^1 qdG(q) &= [qG(q)]_{\underline{q}^*}^1-\int_{\underline{q}^*}^1 G(q)dq \\ &= G(1)-\underline{q}^*G\left(q^*\right)-\int_{\underline{q}^*}^1 G(q)dq \\ &= 1-\underline{q}^*G\left(\underline{q}^*\right)-\int_{\underline{q}^*}^1 G(q)dq\end{aligned}$$

$$1-\int_{\underline{q}^*}^{1} q dG(q) = \underline{q}^* G\left(\underline{q}^*\right) + \int_{\underline{q}^*}^{1} G(q)dq.$$

Plug this into the equation above to get:

$$\begin{aligned}\underline{q}^*(c+(1-\delta)\nu+k) &= c - c\delta\left[\underline{q}^* G\left(\underline{q}^*\right) + \int_{\underline{q}^*}^{1} G(q)dq\right] + \delta c \underline{q}^* G(\underline{q}^*) \\ &= c - c\delta \int_{\underline{q}^*}^{1} G(q)dq.\end{aligned}$$

Now adding and subtracting $c\delta \int_0^{\underline{q}^*} G(q)dq$ to RHS to get:

$$\begin{aligned}\underline{q}^*(c+(1-\delta)v+k) =& c - c\delta \int_0^{\underline{q}^*} G(q)dq - c\delta \int_{\underline{q}^*}^{1} G(q)dq + c\delta \int_0^{\underline{q}^*} G(q)dq \\ =& c - c\delta \int_0^{1} G(q)dq + c\delta \int_0^{\underline{q}^*} G(q)dq.\end{aligned}$$

Then we get the expression:

$$\begin{aligned}\underline{q}^* =& \frac{c}{c+(1-\delta)\nu+k} - \frac{c\delta}{c+(1-\sigma)\nu+k}\int_0^1 G(q)dq \\ &+ \frac{c\delta}{c+(1-\delta)\nu+k}\int_0^{\underline{q}^*} G(q)dq \\ \text{where} \quad & G(q) = (1-m_L)\, G_H(q) + m_L G_L(q) \\ & G_H(q) = P_H \cdot \Phi\left(\frac{S_H(q)-1}{\sigma}\right) + (1-P_H)\,\Phi\left(\frac{S_H(q)}{\sigma}\right) \\ & G_L(q) = P_L \cdot \Phi\left(\frac{S_L(q)-1}{\sigma}\right) + (1-P_L)\,\Phi\left(\frac{S_L(q)}{\sigma}\right).\end{aligned}$$

Plug in the equation $\int_0^1 q(q)dq = 1 - E(q) = 1 - m_H P_H - m_L P_L$, we have:

$$\begin{aligned}\underline{q}^* =& \frac{c}{c+(1-\delta)\nu+k} - \frac{c\delta}{c+(1-\delta)\nu+k}(1 - m_H P_H - m_L P_L) \\ &+ \frac{c\delta}{c+(1-\delta)v+k}\int_0^{\underline{q}^*} G(q)dq.\end{aligned}$$

First, when the solution y is given implicitly, we have following relationship from the differentiation of implicit functions:

$$\text{If } F(y(\theta), \theta) = 0 \text{ then } \frac{d}{d\theta} F(y(\theta), \theta) = 0$$
$$\Rightarrow \frac{\partial F}{\partial y} \frac{\partial y}{\partial \theta} + \frac{\partial F}{\partial \theta} = 0 \Rightarrow \frac{\partial y}{\partial \theta} = -\frac{\partial F/\partial \theta}{\partial F/\partial y}.$$

Let's define:

$$\begin{aligned} F\left(\underline{q}^*, \vec{\theta}\right) = \underline{q}^* - \frac{c}{c+(1-\delta)\nu+k} + \frac{c\delta}{c+(1-\delta)\nu+k}\left(1 - m_H P_H - m_L P_L\right) \\ - \frac{c\delta}{c+(1-\delta)\nu+k} \int_0^{\underline{q}^*} G(q) dq. \end{aligned}$$

Then differentiate F with respect to $\underline{q}^*$ gives:

$$\frac{\partial F}{\partial q^*} = -\frac{c\delta}{c+(1-\delta)\nu+k} \frac{d}{d\underline{q}^*}\left[\int_0^{\underline{q}^*} G(q) dq\right] = -\frac{c\delta}{c+(1-\delta)\nu+k} \cdot G\left(\underline{q}^*\right) < 0.$$

We want to check the sign of $\frac{\partial \underline{q}^*}{\partial P_L} = -\frac{\partial F/\partial P_L}{\partial F/\partial \underline{q}^*}$, and since we know that $\frac{\partial F}{\partial \underline{q}^*} < 0$, we only need to check the sign of $\frac{\partial F}{\partial P_L}$.

$$\begin{aligned} \frac{\partial F}{\partial P_L} &= \frac{-c\delta m_L}{c+(1-\delta)\nu+k} - \frac{c\delta}{c+(1-\delta)\nu+k} \int_0^{\underline{q}^*} \frac{\partial G(q)}{\partial P_B} dq \\ &= \frac{-c\delta}{c+(1-\delta)\nu+k}\left[m_L + \int_0^{\underline{q}^*} \frac{\partial G(q)}{\partial P_L} dq\right] \\ &= \frac{-c\delta}{c+(1-\delta)\nu+k}\left[m_B + m_B \int_0^{\underline{q}^*} \frac{\partial G_B(q)}{\partial P_B} dq\right] \\ &= \frac{-c\delta m_B}{c+(1-\delta)\nu+k}\left[1 + \int_0^{\underline{q}^*} \frac{\partial G_B(q)}{\partial P_B} dq\right]. \end{aligned}$$

In the meantime, we have:

$$\begin{aligned} \frac{\partial G_B(q)}{\partial P_B} =& \Phi\left(\frac{S_B(q)-1}{\sigma_B}\right) + P_B \cdot \phi\left(\frac{S_B(q)-1}{\sigma_B}\right) \cdot \frac{1}{\sigma_B} \cdot \frac{\partial S_B(q)}{\partial P_B} \\ &- \Phi\left(\frac{S_B(q)}{\sigma_B}\right) + (1-P_B) \cdot \phi\left(\frac{S_B(q)}{\sigma_B}\right) \cdot \frac{1}{\sigma_B} \cdot \frac{\partial S_B(q)}{\partial P_B} \\ =& \Phi\left(\frac{S_B(q)-1}{\sigma_B}\right) - \Phi\left(\frac{S_B(q)}{\sigma_B}\right) \end{aligned}$$

$$+\frac{1}{\sigma_B}\cdot\frac{\partial S_B(q)}{\partial P_B}\left[P_B\cdot\phi\left(\frac{S_B(q)-1}{\sigma_B}\right)+(1-P_B)\,\phi\left(\frac{S_B(q)}{\sigma_B}\right)\right].$$

Note that since $S_\gamma(q)=\frac{1}{2}-\sigma_\gamma^2*\log\left[\frac{1-q}{q}\cdot\frac{P_r}{1-P_\gamma}\right]$, we have $\frac{\partial S_B(q)}{\partial P_B}=\frac{-\sigma_B^2}{P_B(1-P_B)}$ and this gives:

$$\begin{aligned}\frac{\partial G_B(q)}{\partial P_B}&=\Phi\left(\frac{S_B(q)-1}{\sigma_B}\right)-\Phi\left(\frac{S_B(q)}{\sigma_B}\right)-\frac{\sigma_B}{P_B\,(1-P_B)}\left[P_B\phi\left(\frac{S_B(q)-1}{\sigma_B}\right)+(1-P_B)\,\phi\left(\frac{S_B(q)}{\sigma_B}\right)\right]\\&=\Phi\left(\frac{S_B(q)-1}{\sigma_B}\right)-\Phi\left(\frac{S_B(q)}{\sigma_B}\right)-\frac{\sigma_B}{1-P_B}\phi\left(\frac{S_B(q)-1}{\sigma_B}\right)-\frac{\sigma_B}{P_B}\phi\left(\frac{S_B(q)}{\sigma_B}\right).\end{aligned}$$

Since $\Phi\left(\frac{S_B(q)-1}{\sigma_B}\right)-\Phi\left(\frac{S_B(q)}{\sigma_B}\right)<0$, we have $\frac{\partial G_B(q)}{\partial P_B}<0$ and this gives the inequality below:

$$\begin{aligned}1+\int_0^{\underline{q}^*}\frac{\partial G_B(q)}{\partial P_B}dq&>1+\int_0^1\frac{\partial G_B(q)}{\partial P_B}dq\\&=1+\int_0^1\Phi\left(\frac{S_B(q)-1}{\sigma_B}\right)-\Phi\left(\frac{S_B(q)}{\sigma_B}\right)dq\\&\quad-\frac{\sigma_B}{1-P_B}\int_0^1\phi\left(\frac{S_B(q)-1}{\sigma_B}\right)dq-\frac{\sigma_B}{P_B}\int_0^1\phi\left(\frac{S_B(q)}{\sigma_B}\right)dq\\&=1+\int_0^1\Phi\left(\frac{S_B(q)-1}{\sigma_B}\right)-\Phi\left(\frac{S_B(q)}{\sigma_B}\right)dq-\frac{\sigma_B}{1-P_B}-\frac{\sigma_B}{P_B}\\&=1+\int_0^1\Phi\left(\frac{S_B(q)-1}{\sigma_B}\right)-\Phi\left(\frac{S_B(q)}{\sigma_B}\right)dq-\frac{\sigma_B}{(1-P_B)P_B}\\&=1+\left(\frac{\sigma_B}{(1+P_B)\,P_B}-1\right)-\frac{\sigma_B}{(1-P_B)P_B}=0.\end{aligned}$$

This proves that $\frac{\partial F}{\partial P_B}$ <0, and hence finalizes the proof of $\frac{\partial \underline{q}^*}{\partial P_B}=-\frac{\partial F/\partial P_B}{\partial F/\partial \underline{q}^*}<0$.

POLICY IMPLICATION 2: *As the proportion of drug users among low-skill workers increases (i.e., as P_L decreases), the permanent composition of low-skill workers declines, given a recruiting threshold $\underline{q}$.*

(Proof) When we have $m_A = m_B = \frac{1}{2}$:

$$r_\gamma = \frac{P_\gamma (1 - G_{\gamma 1})}{P_\gamma (1 - G_{\gamma 1}) + P_{-\gamma} (1 - G_{-\gamma 1})}.$$

So we have:

$$\begin{aligned} r_L &= \frac{P_L (1 - G_{L1})}{P_L (1 - G_{L1}) + P_H (1 - G_{H1})} \\ \text{where } \; G_{H1} &= \Phi\left(\frac{S_H(\underline{q}) - 1}{\sigma}\right) = \Phi\left(-\frac{1}{2\sigma} - \sigma * \log\left[\frac{P_H}{1 - P_H} \cdot \frac{1 - \underline{q}}{\underline{q}}\right]\right) \\ G_{L1} &= \Phi\left(\frac{S_L(\underline{q}) - 1}{\sigma}\right) = \Phi\left(-\frac{1}{2\sigma} - \sigma * \log\left[\frac{P_L}{1 - P_L} \cdot \frac{1 - \underline{q}}{\underline{q}}\right]\right). \end{aligned}$$

By differentiating r_L with respect to P_L we get:

$$\begin{aligned} \frac{\partial r_L}{\partial P_L} &= \frac{\frac{\partial \{P_L (1 - G_{L1})\}}{\partial P_L} \cdot [P_L (1 - G_{L1}) + P_H (1 - G_{H1})] - \frac{\partial \{P_L (1 - G_{L1}) + P_H (1 - G_{H1})\}}{\partial P_L} \cdot P_L (1 - G_{L1})}{[P_L (1 - G_{L1}) + P_H (1 - G_{H1})]^2} \\ &= \frac{\frac{\partial \{P_L (1 - G_{L1})\}}{\partial P_L} [P_L (1 - G_{L1}) + P_H (1 - G_{H1}) - P_L (1 - G_{L1})]}{[P_L (1 - G_{L1}) + P_H (1 - G_{H1})]^2} \\ &= \frac{\frac{\partial \{P_L (1 - G_{L1})\}}{\partial P_L} [P_H (1 - G_{H1})]}{[P_L (1 - G_{L1}) + P_H (1 - G_{H1})]^2}. \end{aligned}$$

Note that:

$$\begin{aligned} \frac{\partial \{P_L (1 - G_{L1})\}}{\partial P_L} &= 1 - G_{L1} + P_L \cdot \frac{\partial \{(1 - G_{L1})\}}{\partial P_L} \\ &= 1 - G_{L1} + P_L \cdot \left(-\frac{\partial G_{L1}}{\partial P_L}\right) \\ &= 1 - G_{L1} - P_L \cdot \frac{\partial G_{L1}}{\partial P_L}. \end{aligned}$$

in which

$$\frac{\partial G_{L1}}{\partial P_L} = \frac{\partial \left\{\Phi\left(\frac{S_L(\underline{q}) - 1}{\sigma}\right)\right\}}{\partial P_L} = \partial \left\{\Phi\left(\frac{\frac{1}{2} - \sigma^2 * \log\left[\frac{P_L}{1 - P_L} \cdot \frac{1 - \underline{q}}{\underline{q}}\right] - 1}{\sigma}\right)\right\} / \partial P_L$$

$$
\begin{aligned}
=&\partial\left\{\Phi\left(\frac{-1}{2\sigma}-\sigma*\log\left[\frac{P_L}{1-P_L}\cdot\frac{1-\underline{q}}{\underline{q}}\right]\right)\right\}/\partial P_L\\
=&\phi\left(\frac{-1}{2\sigma}-\sigma*\log\left[\frac{P_L}{1-P_L}\cdot\frac{1-\underline{q}}{\underline{q}}\right]\right)*\frac{\partial\left\{-\sigma*\log\left[\frac{P_L}{1-P_L}\cdot\frac{1-\underline{q}}{\underline{q}}\right]\right\}}{\partial P_L}\\
=&\phi\left(\frac{-1}{2\sigma}-\sigma*\log\left[\frac{P_L}{1-P_L}\cdot\frac{1-\underline{q}}{\underline{q}}\right]\right)*\left[-\sigma*\frac{\frac{1}{(1-P_L)^2}\frac{1-\underline{q}}{\underline{q}}}{\frac{P_L}{1-P_L}\cdot\frac{1-\underline{q}}{\underline{q}}}\right]\\
=&\phi\left(\frac{-1}{2\sigma}-\sigma*\log\left[\frac{P_L}{1-P_L}*\frac{1-\underline{q}}{\underline{q}}\right]\right)*\left[-\sigma*\frac{1}{P_L\left(1-P_L\right)}\right].
\end{aligned}
$$

Combining the equations above, we have:

$$
\begin{aligned}
\frac{\partial\left\{P_L\left(1-G_{L1}\right)\right\}}{\partial P_L}&=1-G_{L1}-P_L\cdot\phi\left(\frac{-1}{2\sigma}-\sigma*\log\left[\frac{P_L}{1-P_L}*\frac{1-\underline{q}}{\underline{q}}\right]\right)*\left[-\sigma*\frac{1}{P_L\left(1-P_L\right)}\right]\\
&=1-G_{L1}+\frac{\sigma}{1-P_L}\phi\left(\frac{-1}{2\sigma}-\sigma*\log\left[\frac{P_L}{1-P_L}*\frac{1-\underline{q}}{\underline{q}}\right]\right)>0.
\end{aligned}
$$

And this proves that $\frac{\partial r_L}{\partial P_L}>0$.

POLICY IMPLICATION 3: *As the recruiting threshold* $\underline{q}$ *increases, the permanent composition of low-skill workers declines.*

(Proof) Note that:

$$
r_\gamma=\frac{m_\gamma P_\gamma\left(1-G_{\gamma1}\right)}{m_\gamma P_\gamma\left(1-G_{\gamma1}\right)+\left(1-m_\gamma\right)P_{-\gamma}\left(1-G_{-\gamma1}\right)}.
$$

Let the denominator:

$$
D\equiv m_LP_L\left(1-G_{L1}\right)+\left(1-m_L\right)P_H\left(1-G_{H1}\right).
$$

Then we have:

$$
\frac{\partial r_L}{\partial\underline{q}}=\frac{-m_LP_L\frac{\partial G_{L1}}{\partial\underline{q}}\cdot N-\frac{\partial N}{\partial\underline{q}}m_LP_L\left(1-G_{L1}\right)}{D^2}.
$$

Note that:

$$
\begin{aligned}
\frac{\partial N}{\partial\underline{q}}&=-m_LP_L\frac{\partial G_{L1}}{\partial\underline{q}}-\left(1-m_L\right)P_H\frac{\partial G_{H1}}{\partial\underline{q}}\\
&=-m_LP_L\frac{\partial G_{L1}}{\partial\underline{q}}-m_HP_H\frac{\partial G_{H1}}{\partial\underline{q}}.
\end{aligned}
$$

If we denote the nominator of $\frac{\partial r_L}{\partial \underline{q}}$ as N', then we have:

$$
\begin{aligned}
N' =& - m_L P_L \frac{\partial G_{L1}}{\partial \underline{q}} \left(m_L P_L \left(1 - G_{L1}\right) + m_H P_H \left(1 - G_{H1}\right)\right) \\
& - \left(-m_L P_L \frac{\partial G_{L1}}{\partial \underline{q}} - m_H P_H \frac{\partial G_{H1}}{\partial \underline{q}} \right) m_L P_L \left(1 - G_{L1}\right) \\
=& - m_L P_L \frac{\partial G_{L1}}{\partial \underline{q}} \left(m_L P_L \left(1 - G_{L1}\right) + m_H P_H \left(1 - G_{H1}\right)\right) \\
& + m_L^2 P_L^2 \frac{\partial G_{L1}}{\partial \underline{q}} \left(1 - G_{L1}\right) + m_H P_H m_L P_L \left(1 - G_{L1}\right) \frac{\partial G_{H1}}{\partial \underline{q}} \\
=& - m_L^2 P_L^2 \left(1 - G_{L1}\right) \frac{\partial G_{L1}}{\partial \underline{q}} - m_H m_L P_H P_L \left(1 - G_{H1}\right) \frac{\partial G_{L1}}{\partial \underline{q}} \\
& + m_L^2 P_L^2 \left(1 - G_{L1}\right) \frac{\partial G_{L1}}{\partial \underline{q}} + m_H m_L P_H P_L \left(1 - G_{L1}\right) \frac{\partial G_{H1}}{\partial \underline{q}} \\
=& m_H m_L P_H P_L \left[\left(1 - G_{L1}\right) \frac{\partial G_{H1}}{\partial \underline{q}} - \left(1 - G_{H1}\right) \frac{\partial G_{L1}}{\partial \underline{q}} \right].
\end{aligned}
$$

Since we know that:

$$
\begin{aligned}
\frac{\partial G_{H1}}{\partial \underline{q}} &= \frac{\sigma}{\underline{q}(1-\underline{q})} \phi \left(\frac{S_H(\underline{q}) - 1}{\sigma} \right) \\
\frac{\partial G_{L1}}{\partial \underline{q}} &= \frac{\sigma}{\underline{q}(1-\underline{q})} \phi \left(\frac{S_L(\underline{q}) - 1}{\sigma} \right).
\end{aligned}
$$

We can plug in these equations above to get:

$$
\begin{aligned}
N' &= m_H m_L P_H P_L \left[\left(1 - G_{L1}\right) \frac{\sigma}{\underline{q}(1-\underline{q})} \phi \left(\frac{S_H(\underline{q}) - 1}{\sigma} \right) - \left(1 - G_{H1}\right) \frac{\sigma}{\underline{q}(1-\underline{q})} \phi \left(\frac{S_L(\underline{q}) - 1}{\sigma} \right) \right] \\
&= m_H m_L P_H P_L \frac{\sigma}{\underline{q}(1-\underline{q})} \left[\left(1 - G_{L1}\right) \phi \left(\frac{S_H(\underline{q}) - 1}{\sigma} \right) - \left(1 - G_{H1}\right) \phi \left(\frac{S_L(\underline{q}) - 1}{\sigma} \right) \right].
\end{aligned}
$$

To prove $\frac{\partial r_L}{\partial \underline{q}} < 0$, it is equivalent to prove $N' < 0$. Hence, we need to prove the distribution G_{H1} dominates G_{L1} in terms of hazard rate, which is equivalent to prove:

$$
\begin{aligned}
\frac{\left(1 - G_{L1}\right) \cdot \phi \left(\frac{S_H(\underline{q}) - 1}{\sigma} \right) \frac{\sigma}{\underline{q}(1-\underline{q})}}{\left(1 - G_{H1}\right) \cdot \phi \left(\frac{S_L(\underline{q}) - 1}{\sigma} \right) \frac{\sigma}{\underline{q}(1-\underline{q})}} &= \frac{\frac{1}{1-G_{H1}} \cdot \phi \left(\frac{S_H(\underline{q}) - 1}{\sigma} \right) \frac{\sigma}{\underline{q}(1-\underline{q})}}{\frac{1}{1-G_{L1}} \cdot \phi \left(\frac{S_L(\underline{q}) - 1}{\sigma} \right) \frac{\sigma}{\underline{q}(1-\underline{q})}} = \frac{\frac{1}{1-G_{H1}(\underline{q})} \, g_{H1}(\underline{q})}{\frac{1}{1-G_{L1}(\underline{q})} \, g_{L1}(\underline{q})} \\
&= \frac{\text{Hazard Rate of Distribution } \ G_{H1}}{\text{Hazard Rate of Distribution } \ G_{L1}} < 1.
\end{aligned}
$$

Before proving the statement above, we first prove that the distribution G_{H1} dominates G_{L1} in terms of likelihood ratio, which is equivalent to proving $\frac{\partial^2 \ln g}{\partial\sigma\partial q} > 0$. This inequality can be proved by:

$$\begin{aligned}
\ln g(q;P) &= \ln\phi\left(\frac{s(q)-1}{\sigma}\right)\frac{\sigma}{q(1-q)} \\
&= \ln\left[\frac{1}{\sqrt{2\pi}}e^{-\frac{1}{2}\left(\frac{s(q)-1}{\sigma}\right)^2}\frac{\sigma}{q(1-q)}\right] = \ln\frac{1}{\sqrt{2\pi}} - \frac{1}{2}\left(\frac{s(q)-1}{\sigma}\right)^2 + \ln\frac{\sigma}{q(1-q)} \\
\frac{\partial \ln g(q;P)}{\partial p} &= -\frac{1}{2p(1-p)} - \frac{\sigma^2}{p(1-p)}\ln\left[\frac{p}{1-p}\frac{1-q}{q}\right] \\
\frac{\partial \ln g(q;P)}{\partial p\partial q} &= \frac{\sigma^2}{p(1-q)}\cdot\frac{1}{(1-p)}\cdot\frac{1}{q} > 0.
\end{aligned}$$

For all $q < q'$, The fact that the distribution G_{H1} dominates G_{L1} in terms of likelihood ratio gives:

$$\begin{aligned}
\frac{g_{L1}(q')}{g_{L1}(q)} < \frac{g_{H1}(q')}{g_{H1}(q)} \Rightarrow \int_q^1 \frac{g_{L1}(t)}{g_{L1}(q)} < \int_q^1 \frac{g_{H1}(t)}{g_{H1}(q)} \Rightarrow \frac{1-G_{L1}(q)}{g_{L1}(q)} < \frac{1-G_{H1}(q)}{g_{H1}(q)} \\
\Rightarrow \frac{g_{H1}(q)}{1-G_{H1}(q)} < \frac{g_{L1}(q)}{1-G_{HL}(q)}.
\end{aligned}$$

This finalizes the proof that $N' < 0$ and $\frac{\partial r_L}{\partial \underline{q}} < 0$.