

The Effects of Layoffs on Opioid Use and Abuse*

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Abstract

Use and abuse of opioids are becoming major problems in many countries around the world. There is speculation that their increase is exacerbated by poor labour market conditions—for example, drug overdoses are categorized by some as “deaths of despair.” If that is true, it is especially relevant now, in a period of economic distress. However, there is little causal evidence to support that claim, and the evidence that does exist is mostly based on regional analyses that cannot distinguish direct effects on individuals from effects that act via peers, mobility, local public policy, or other mechanisms. In this research, we use Danish register data to examine the effect of layoffs on laid-off workers’ use and abuse of opioids. Because these layoffs are likely unrelated to any individual worker’s propensity to use opioids, this strategy allows us to identify the causal effect of a labour market shock on opioid use and abuse. Preliminary results indicate that there are large effects on opioid use for laid-off individuals.

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1 Introduction

Opioid use and abuse are rising rapidly in many parts of the world. At the same time, there has been increasing inequality, with wages stagnating for lower-income individuals in much of the developed world. Use and abuse of opioids appear to be more common among those facing worse economic conditions, leading to much speculation and research that has sought to understand how these facts are related. First—as we document here—poor economic conditions may lead to despair, causing individuals to be more likely to use and abuse opioids to cope. Second, causality may flow in the opposite direction: opioids could lead users, and those connected to them, to do worse work or lose their jobs. Finally, there could be another factor—such as public policy—that could cause both negative labour market outcomes and increased opioid use and abuse.

In this paper, we provide clear evidence documenting how poor economic conditions can directly and indirectly increase opioid use and abuse. We do this by analyzing the effects of layoffs on opioid-related outcomes in Denmark. Because these layoffs are generally unpredictable soon before they occur, we can compare individuals who experienced this negative economic shock to otherwise-similar individuals who did not, thus determining the causal effects of the layoff itself. The rich Danish register data allows us to examine effects on a wide variety of outcomes, and to examine potential causal mechanisms. Because we find these effects in individual-level data rather than the regional data that has been used in past literature, our results allow us to more clearly identify how economic conditions affect individual opioid use and abuse: that economic conditions directly affect opioid use of individuals, and through those individuals affects opioid use by their families and neighbors. This improved identification has important implications for public policy.

We find that a layoff causes a worker to take approximately 0.1 more defined daily doses (DDD) of opioids 5 years after the layoff, an increase of around 8%.¹ This effect is driven by the extensive margin: a layoff causes a worker to be 0.01 percentage points more likely to take any opioids, an increase of about 25%.

In addition to the direct effect on laid-off individuals, there are several mechanisms through which layoffs could affect others. First, individuals who are prescribed additional opioids might be selling some of them. That would lead to increased illicit use of opioids, which could have worse consequences than use overseen by a medical professional. Future versions of this paper will examine the spillover effect of layoffs to opioid use of others in the worker’s social network.

¹Defined daily doses are a measure, defined by the World Health Organization, of how much opioids have been prescribed. Using DDDs helps us to compare the use of many different types of opioids.

These results are consistent with a small but growing literature that sees opioid use and abuse as a consequence of negative economic conditions. [Case and Deaton \(2017\)](#) categorize opioid overdose deaths as one type of “death of despair”, implying that negative conditions—economic or otherwise—can lead to poor mental health outcomes, which in turn increases the chance of opioid abuse. Indeed, [Krueger \(2017\)](#) and [Ruhm \(2018\)](#) find that locations with poor economic conditions do tend to have worse opioid problems. In a related work, [Ahammer and Packham \(2020\)](#) find that longer eligibility for unemployment benefits lead to fewer opioid prescriptions. However, the relationship between opioids and economic conditions may not be so simple. In fact, several recent studies, including those by [Thingholm \(2019\)](#), [Harris et al. \(2019\)](#), [Laird and Nielsen \(2016\)](#), and [Park and Powell \(2019\)](#), present evidence that the causal relationship can go in the opposite direction: opioid use and abuse can lead to deteriorating economic outcomes.

Some recent evidence does attempt to isolate the causal effect of negative economic conditions on opioid use and abuse. For example, [Currie et al. \(2018\)](#) use Bartik-style instruments to determine the effect of local economic conditions on opioid use and abuse, with ambiguous results. [Pierce and Schott \(2020\)](#) examine the effect of trade liberalization with China, finding that counties that were more exposed to that shock—and thus experienced worse economic outcomes—also saw an increase in drug overdose deaths. [Venkataramani et al. \(2020\)](#) find that automotive plant closures lead to increased opioid mortality in the plant’s county. It is important to note that most of this literature explores the effect of local economic conditions rather than individual-level shocks. Thus, even if these papers do correctly identify causal effects, they cannot separately identify direct and indirect effects, as we do.

Indeed, it has been shown or suggested that the use of opioids may be increased by many factors that poor labor market conditions can affect, such as opioid use in family or social networks (e.g. [Nguyen et al. \(2020\)](#), [Kennedy-Hendricks et al. \(2016\)](#), [Barnett et al. \(2019\)](#), [Khan et al. \(2019\)](#)), physician quality (e.g., [Schnell and Currie \(2018\)](#)), the marketing of opioids (e.g. [Hadland et al. \(2019\)](#)), and local public policy (e.g. [Popovici et al. \(2018\)](#), [Alpert et al. \(2019\)](#)). Additionally, the population of an area is endogenously determined, and drug users could be particularly drawn to areas with economic problems, where rent is low. Whether economic conditions affect opioid use directly (for example, through despair) or via any of these other channels is crucially important for determining the optimal policy response. For example, if the effect is direct, then mental health counseling for laid off workers could be an important intervention. If most of the effect measured in other papers is due to the mobility of drug users, then economic conditions may have little effect

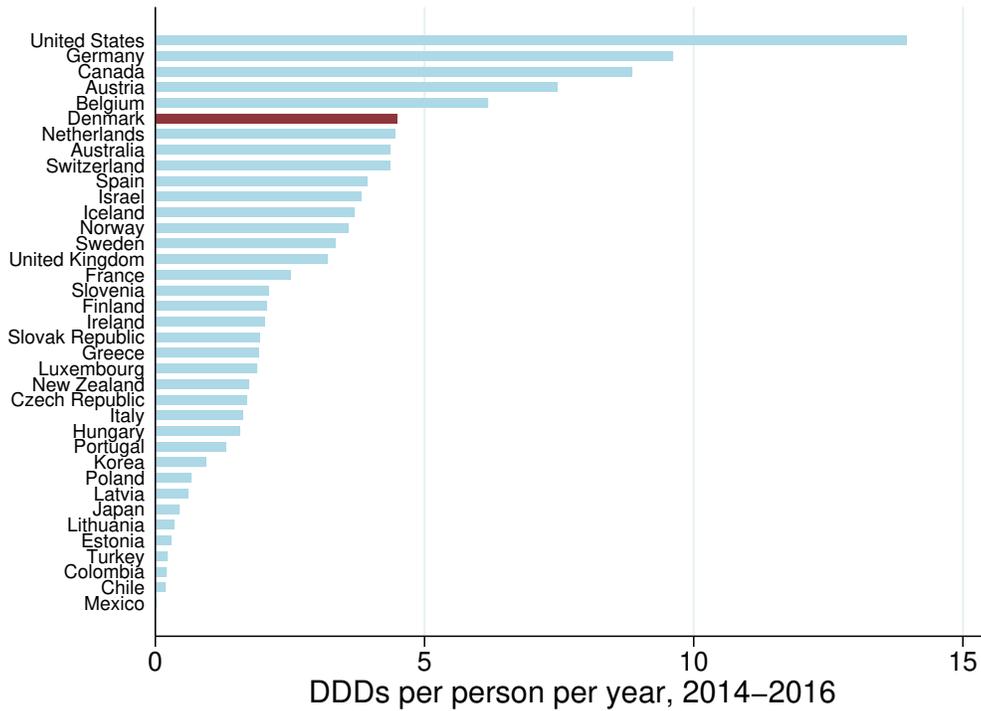
on total opioid use. Other causal pathways suggest different interventions: for example, changing the way opioids are marketed or changing local public policy.

Many of the papers in the preceding paragraph have led to a perception that increased opioid use is driven by opioid supply (such as marketing and physician decisions) rather than opioid demand (as we explore here). We contribute to that literature by showing that opioid use is driven by the interaction between supply and demand: the effect of a layoff is highest for workers whose primary care physician is a heavy prescriber of opioids. (This result is not yet detailed fully in this paper.)

In addition to the literature on economic conditions and opioids, this paper contributes to a long literature, beginning with [Jacobson et al. \(1993\)](#), finding that layoffs can have a variety of negative long-term consequences for individuals and their families. [Oreopoulos et al. \(2008\)](#), for example, show that sons whose fathers were laid off earn less money later in life. Consequences of layoffs need not be wholly economic: for example, [Sullivan and von Wachter \(2009\)](#) find that layoffs can lead to increased mortality. Within this literature, our paper is most closely related to [Browning and Heinesen \(2012\)](#), who use the same Danish register data to examine the effects of layoffs on various measures of health, including alcohol-related disease. Relative to that paper, we examine effects on opioid use and abuse, and expand our focus to include effects on families and others in the worker’s social network. Our paper is also closely related to [Kuhn et al. \(2009\)](#), who examine a broad range of public health outcomes of job loss in Austria. Among other results, they find that job loss leads to no increase in the use of a broad class of “psychosomatic” drugs that could be related to layoffs (such as migraine therapeutics and anti-inflammatory drugs). We expand upon that contribution by focusing on opioids, using data from years during the opioid epidemic, and including results from families and other connected individuals.

The remainder of this paper proceeds as follows. Section 2 discusses the health and economic context in Denmark over the past few decades. Section 3 describes the Danish register data we use in this paper. Section 4 explains our main methodology, including our construction of a matched comparison group to determine the counterfactual: how opioid-related outcomes would have evolved in the absence of a layoff. Section 5 presents results for the laid-off individual. We extrapolate from our results to the broader context of the opioid crisis in Section 6. Section 7 presents next steps and concludes.

Figure 1: Opioids prescribed per capita in Denmark and other countries



Notes: Source: Organisation for Economic Co-operation and Development (OECD).

2 Health and economic context

Prescriptions for opioid use have been rising rapidly in Denmark since the 1990s. According to [Nissen et al. \(2019\)](#)—using a slightly different data source from us—opioid prescriptions increased from approximately 16 DDD per 1,000 people per day (less than 6 DDD per person per year) in 1999, to approximately 21 (more than 7 DDD per person per year) in 2017. Indeed, Danish opioid consumption, while substantially lower than in the United States, is nevertheless much higher than in most other developed countries, as shown in Figure 1.

Despite this high level of use, drug-related mortality has barely changed over the past few decades, unlike in the United States and many other countries. Anecdotally, this appears to be due to the strong focus on harm reduction in the Danish approach to drug abuse ([European Monitoring Centre for Drugs and Drug Addiction \(2019\)](#)). For example, heroin is available by prescription to people diagnosed with drug dependence, lessening the need to buy illegal drugs that can be laced with more dangerous compounds. Additionally, legal drug consumption rooms allow users to be

monitored by staff; these rooms are used hundreds of thousands of times per year without a single death. Drug consumption rooms also reduce the barriers to treatment (Kappel et al. (2016)), which are already low in Denmark: adults who seek treatment are guaranteed free access to it within 14 days.

Economically, Denmark has fairly low levels of inequality and poverty, though both have been growing over the past few decades. As discussed by Causa et al. (2016), both inequality and poverty are lower in Denmark than almost any other member of the Organization for Economic Co-operation and Development (OECD). However—similar to the OECD average—the Gini coefficient on income rose by almost 3 points since the mid-1980s. Poverty in Denmark is defined more restrictively than in many other countries,² so only approximately 0.75% of Danes are considered poor; however, that number has more than doubled between 1999 and 2013. Unemployment in Denmark, like much of the developed world, has been declining since its peak during the Great Recession.³ Unlike that in much of the world, however, unemployment did not spike during the COVID-19 outbreak; it sat at 4.6% in April, 2020. Given this context of rising inequality and poverty, and high unemployment in the rest of the world, it is especially important to understand how these economic shocks can affect opioid use and abuse.

Indeed, low income is highly correlated with opioid use. Figure 2 shows that low-income individuals—including those with no income—are prescribed substantially more opioids than those in other income brackets.

3 Data

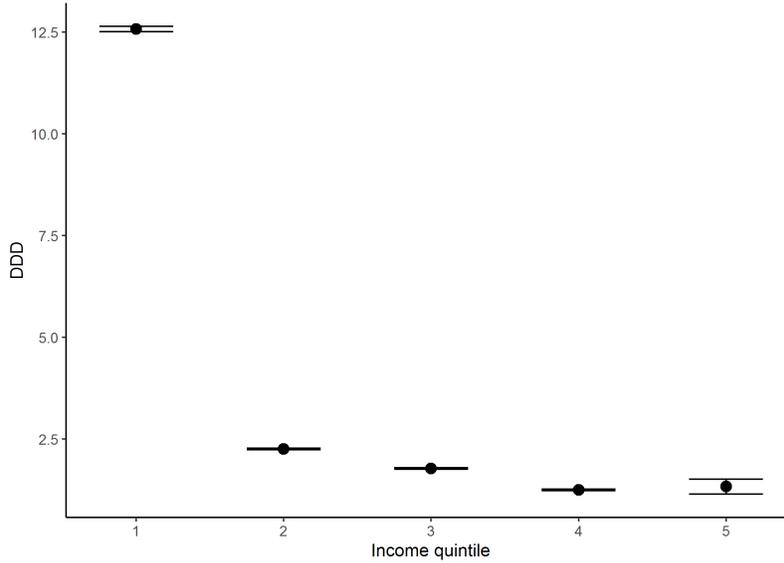
Sample selection: establishments

Our goal is to explore events in which a worker experiences a layoff for exogenous reasons. To do so, we focus on events in which an establishment closes. Identifying establishment closures in administrative data is no trivial task. It comes with several choices that affects the sample size and composition. (Future versions of this paper will show robustness to these choices.) Our sample selection follows Browning and Heinesen (2012) to a large extent. For the years 2000-2011 we identify all single-establishment firms that eventually close down. We focus on single-establishment firms to avoid situations where workers are simply reassigned to a different establishment. We then

²An individual is considered poor if for three straight years, they are not a student and do not live with an adult student; have disposable income less than half of the median; and have net wealth below 100,000 DKK (US\$15,000).

³See <https://fred.stlouisfed.org/series/LRHUTTTDKM156N>.

Figure 2: Income and opioid use



Notes: The X axis groups adults by their income quintile in a given year; the bottom group includes people with no income. The Y axis measures average defined daily doses of opioids prescribed in that year.

identify the closing year t as the year where we see the largest absolute reduction in employment between $t - 1$ and t , where the establishment had at least 5 employees in year $t - 1$, and saw a reduction between $t - 1$ and t of at least 3 workers constituting at least 10% of its workforce. This is to ensure that we get the year of the layoff and not the termination year. For example, if a establishment closes in year t and has a reduction of 95% of its employees in year $t - 3$ we want $t - 3$ to be deemed the closing year. (This may happen in the data because there are legal advantages to keeping an establishment technically open even after shutting down its operations.) We further restrict to establishments that before the closing year had at least 30 full time equivalents employed. This implies that at least 8 individual are laid off at the same time.

Sample selection: workers

As our treated group, we include all workers working at the establishments that close in year t , who (1) are employed there full time in years $t - 1$, $t - 2$, and $t - 3$; (2) are aged between 20 and 60 in year t ; and (3) have never before experienced an establishment closure. Ultimately, we observe 23,369 laid-off individuals.

As described in more detail below, we select a control group using coarsened exact matching. Potential controls are adults who (1) are employed full time in years $t - 1$, $t - 2$, and $t - 3$, and remain

Table 1: Descriptive statistics

| | Age 20-60 | Displaced workers | Potential controls | People w/ opioid Rx |
|-----------------|--------------|----------------------|-----------------------|------------------------|
| Any Opioids | .053 | .036 | .035 | 1 |
| DDD | 4.4 | 1.2 | 1.4 | 82.5 |
| Female | .495 | .303 | .341 | .56 |
| Age | 39.6 | 40.2 | 41.6 | 44.3 |
| Married | .503 | .559 | .604 | .535 |
| Ed: HS | .714 | .725 | .766 | .594 |
| Ed: College | .255 | .177 | .211 | .171 |
| Earnings ('000) | 228.7 | 323.1 | 350.5 | 157.5 |
| N ('000) | 33552.8 | 23.4 | 7449.8 | 1786.4 |

Notes: Statistics based on Danish register data for laid off individuals analyzed in this paper. “Age 20-60” refers to all individual-year observations in that age range; “Displaced workers” refers to the sample of workers whom we include as being laid off, with data on the year before the layoff; “Potential controls” refers to all those individual-year observations that fit the characteristics of controls; “People w/ opioid Rx” refers to individual-year observations where the individual has an opioid prescription. All statistics are averages, except the final row, which is the number of observations in thousands. “Any Opioids” is an indicator for having an opioid prescription; “DDD” is the number of defined daily doses prescribed. “Ed: HS” is an indicator for having at least a high school education; “Ed: College” is an indicator for having at least a college education.

at one establishment in all three years; (2) are aged between 20 and 60 in year t ; and (3) have never experienced an establishment closure. There are over 7 million individual-year observations that are potential controls

Descriptive statistics

Descriptive statistics for our outcome variables are shown in Table 1 for all workers in the age range, all displaced workers, potential controls, and all people who have an opioid prescription. Notably, the year before the layoff, displaced actually less likely than others in their age range to use opioids, likely because of the tenure requirement. For a similar reason, they also have higher earnings. People with opioid prescriptions, on the other hand, have substantially lower earnings than the average adult—further evidence for the strong negative correlation between opioid use and labor market outcomes.

4 Methodology

For each individual, for each outcome we study, our goal is to determine the appropriate counterfactual: what outcome would that individual experience if they had not been laid off? How does that treatment effect vary over time? To determine this counterfactual, we use coarsened exact

matching, as detailed in [Iacus et al. \(2012\)](#). We briefly describe that methodology here.

First, we discretize (or coarsen) each variable on which we hope to achieve balance. We then only match displaced workers to potential control individual-year observations which have the same value for all coarsened variables. For categorical variables, this means we only match to other observations with the same value. For continuous variables, we follow Sturges’ rule: if there are n observations, we create approximately $\log_2(n) + 1$ equally-spaced bins, and only match to observations in the same bin. We match on the following categorical variables: sex (binary), education (4 categories), occupation (17 2-digit categories), living in Copenhagen (binary), and marital status (binary); and the following continuous variables: age, tenure at establishment, yearly earnings, opioid DDDs in year $t - 2$, and opioid DDDs in year $t - 3$. Overall, 89% of displacement workers have at least one potential control that has the same value for all coarsened variables. If there are multiple such matches, we choose the observation with the most similar level of earnings.

To correct for any additional imbalance, we estimate regressions of the following form:

$$y_{it} = \alpha_i + \gamma_t + \sum_{\tau} \delta_{\tau} Displaced_i \times \mathbb{1}(t - d = \tau) + \epsilon_{it} \quad (1)$$

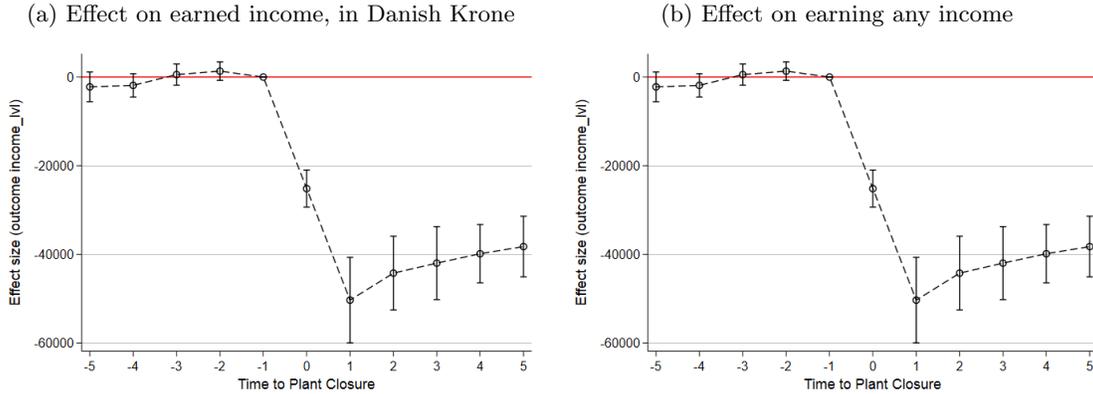
where i indexes people, t indexes years, $Displaced_i$ is an indicator for being displaced, d is the year of displacement (for one’s own displacement, or the displacement of the matched laid off worker in the case of controls), and $\mathbb{1}(\cdot)$ is an indicator function. Standard errors are clustered at the level of the year of the firm’s closing to account for any temporal correlations. Our coefficients of interest are δ_{τ} , which we plot in a graph.

5 Effects on laid-off individuals

As has been shown in past literature, layoffs have substantial economic implications for workers. As shown in [Figure 3a](#), a layoff causes a worker to earn about 40,000 less Danish Kroner (DKK) per year, even five years after the layoff (for reference, DKK 6 \approx US\$1). Much of this effect comes from the extensive margin, as shown in [Figure 3b](#): layoffs cause people to be about 5 percentage points less likely to be working (that is, to earn any money) for the five years after the event.

We find that, in addition to these economic effects, laid off workers are substantially more likely to use opioids: [Figure 4a](#) shows that laid-off workers are about 1% more likely to use opioids. As shown in [Figure 4b](#), layoffs cause workers to get prescriptions for 0.1 more DDD per year. We do not find convincing evidence that these newly-prescribed opioids seem to be abused, though,

Figure 3: Effects on earnings

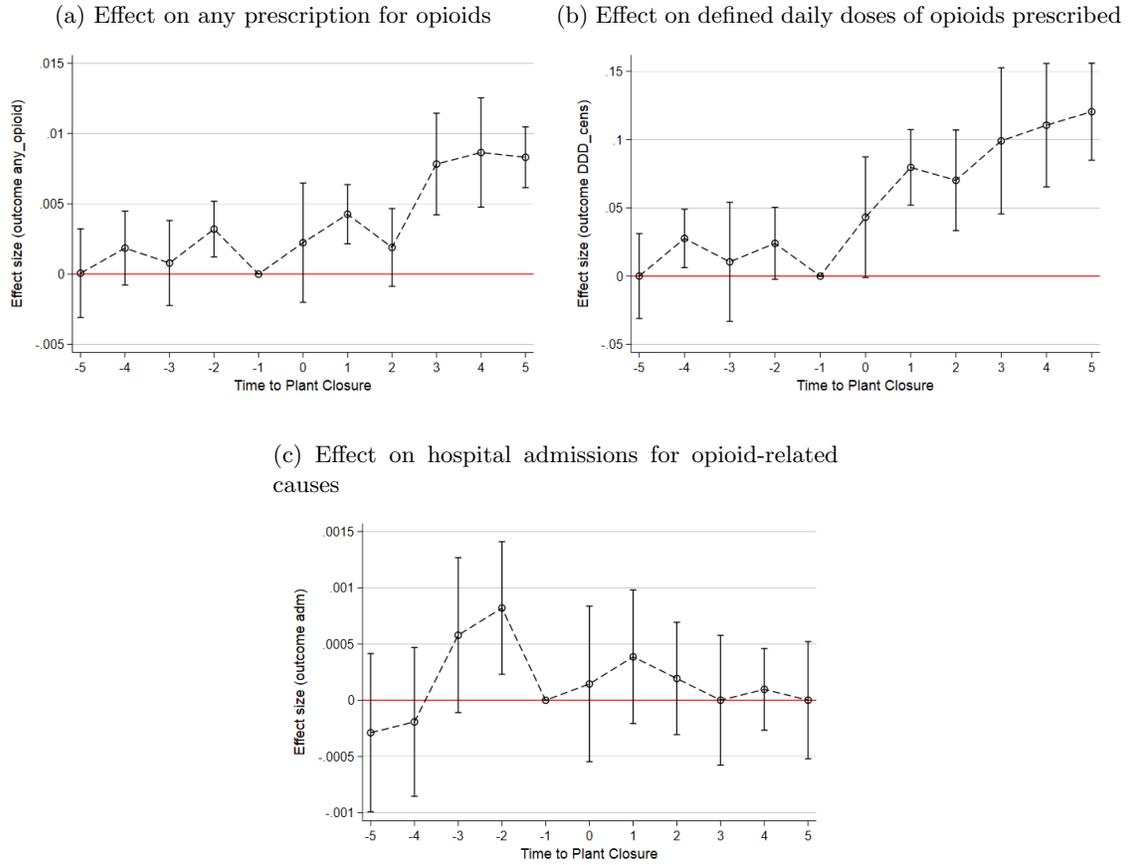


Notes: Based on Danish register data.

with no significant effect on hospital admissions for opioid-related causes, as shown in Figure 4c. However, this measure of abuse is both more narrow than we hope to use (because it does not include all treatment for opioid abuse) and more broad (because it includes treatment for abuse of alcohol and other drugs). Therefore, future versions of this paper will use a more precise measure.

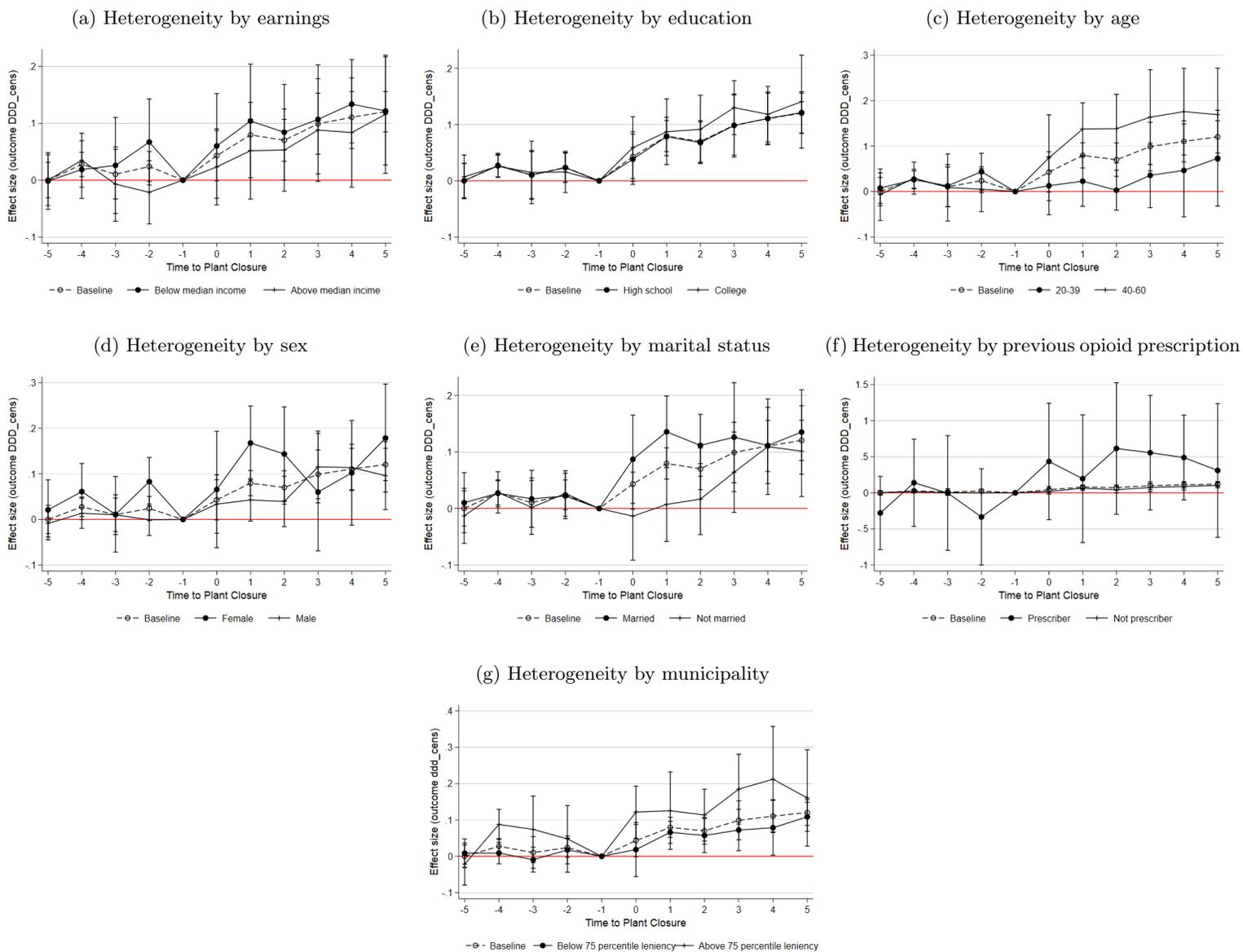
As shown in Figure 5, there is substantial heterogeneity in effects along certain dimensions. Perhaps surprisingly, there is little heterogeneity by income or education; given the strong correlation between opioid use and socioeconomic status, we might have expected the effect to be strongest among low SES individuals, who are more predisposed to use opioids. We do see that older individuals, who generally have more opioids prescribed, account for most of the effect we find. Women are also slightly heavier users of opioids generally, and see a somewhat stronger effect. Intriguingly, the effect is strongest for married workers, perhaps suggesting that anxiety about not being able to provide for a family may contribute to the effect we see. Point estimates of effects are substantially stronger for those who had been prescribed opioids before the layoff, but there are so few of these individuals that we cannot estimate this heterogeneity precisely. Finally, we see that municipalities with higher opioid use also see a stronger effect, suggesting that demand for opioids can lead to opioid use only when there is also a high supply of opioids—and, perhaps, vice versa.

Figure 4: Effects on opioid use and abuse



Notes: Based on Danish register data. Defined daily doses are top-coded at the 99th percentile of that variable. Opioid-related causes includes ICD-10 codes T40 (Poisoning by, adverse effect of and underdosing of narcotics and psychodysleptics [hallucinogens]), and F1 (Mental and behavioral disorders due to psychoactive substance use).

Figure 5: Heterogeneity in effects on DDDs



Notes: Based on Danish register data. All heterogeneity is based on the year before the layoff. “Previous opioid prescription” indicates whether the individual had a prescription in the year before the layoff. “Leniency” measures how prevalent opioids are in the worker’s municipality.

6 Extrapolation and relation to other literature

6.1 Extrapolation

We some assumptions, we can use our results to create a back-of-the-envelope estimate of the extent to which overall opioid use is driven by economic instability.

We begin by assuming that our effects are driven by unemployment: layoffs cause unemployment, which causes opioid use by the individual and their spouse. As shown in Figure 3b, layoffs caused increase the chance that someone is nonemployed by about 5 percentage points. Each layoff also caused about 0.008 people to start using opioids. (This may be an underestimate if there are effects on opioid use by family members, friends, or neighbors; we plan to examine such spillovers in future versions of this paper.) Combining these results, an unemployment causes about 0.16 ($=0.08 / 0.05$) people to start using opioids. Denmark’s unemployment rate is currently around 6%;⁴ thus unemployment would cause 0.9% of people to use opioids, out of 4% who actually do (so around 23% of total use). On the other hand, based on the nonemployment rate of around 20%, nonemployment would cause $\sim 3.2\%$ of people to use opioids (most of the total). Given that economic shocks can cause an individual to leave the labor force, but that many people who are out of the labor force did not experience an economic shock, the true effect of economic shocks likely lies between these estimates.

Although the context varies considerably, these results can help us to understand how much changing economic conditions might have led to the opioid crisis in other countries. For example, in the United States, unemployment was 3.5% at the end of 2019.⁵ With the same methodology, and assuming the same relationships apply in the United States, unemployment would cause about 0.6% of people to use opioids, a small fraction of the total. However, in April 2020, the unemployment rate increased to 14.8%. This increase would cause about 2.4% of Americans to begin using opioids, a substantial increase. Further, the US nonemployment rate is around 40%, which—if that drives opioid use—would cause 6.4% of people to use opioids. For the United States, in particular, these results may represent an underestimate of the true effect of economic conditions. With higher rates of opioid prescriptions, greater illicit opioid use, less affordable access to mental health care, and less access to treatment for opioid abuse, poor economic conditions for lower-income Americans might have contributed even more to the opioid crisis than these numbers indicate.

⁴See <https://fred.stlouisfed.org/series/LRHUTTTDKM156S>.

⁵See <https://fred.stlouisfed.org/series/UNRATE>.

6.2 Relation to other literature

Several other studies that determine the effect of economic shocks on opioid use—such as [Pierce and Schott \(2020\)](#)—do so by calculating the effect of local economic shocks on opioid mortality in the United States. To compare this paper to that literature, we note that, in the US, there are approximately 1.5×10^{-5} annual opioid-related deaths for every annual DDD prescribed.⁶ We find that layoffs increase nonemployment by about 0.05, and increase opioid DDDs by around 0.1; assuming, again, that effects on opioid use operate only through nonemployment, our results would imply that nonemployment causes about 2 ($=0.1 / 0.05$) additional DDDs, which would lead to 3×10^{-5} deaths. Extrapolating from [Pierce and Schott \(2020\)](#) and assuming that their effects on opioid mortality also operate solely through nonemployment, their results imply that nonemployment causes 1.5×10^{-5} opioid deaths—a similar value to what we find. Of course, all caveats above about extrapolating our results to the US context continue to apply.

6.3 Feedback loops

We find that negative labor market outcomes increase opioid use; a separate literature, discussed above, shows that opioid use can cause negative labor market outcomes. We therefore might worry that the labor market and opioid use could lead to a vicious cycle, exacerbating both problems.

To determine if this is the case, we note that our results imply that layoffs cause earnings to decline by approximately 0.2 standard deviations, and opioid DDDs to increase by about 3%. Extrapolating from [Thingholm \(2019\)](#) (which uses a somewhat different measure of income), opioid DDDs increasing by 3% would cause earnings to decline by about 0.005 standard deviations. Based on this calculation, we expect that there is only a modest feedback loop: a direct earnings decline of \$1,000 would be expected to cause \$2.5 of lost earnings through increased opioid use.

7 Conclusion and future work

There are several key steps that remain in this project, in addition to ensuring that all results presented here are robust. First, we will examine spillover effects on laid off workers’ social networks. Preliminary evidence suggests that there are significant effects on workers’ spouses, but further work

⁶This is calculated by noting that there are approximately 70,000 annual opioid deaths, according to the US Centers for Disease Control and Prevention (<https://www.cdc.gov/drugoverdose/data/statedeaths.html>); 330 million people, according to the US Census Bureau (<https://www.census.gov/popclock/>); and 14 DDDs prescribed per person, according to the OECD, as shown in Figure 1. $70,000 / (330 \text{ million}) / 14 \approx 1.5 \times 10^{-5}$.

remains in analyzing those results, as well as spillovers to neighbors and other connections. Second, we will examine how the effect of a layoff varies with characteristics of the worker's primary care physician, which will help us to understand how opioid supply and demand intersect to determine use. This may include estimating physician propensity to prescribe opioids for a disc prolapse before it is officially diagnosed, which gives us a measure of opioid leniency that controls for patients' pain levels but does not allow the physician to know for certain that the patient is actually feeling pain. Third, we will examine whether opioid users endogenously move to areas with worse economic conditions, which could help to explain some of the effects found in papers that examine local economic shocks.

Opioid use and economic instability are both rising around the world. This paper provides clear evidence that these two phenomena are linked: layoffs can cause laid off individuals to use more opioids. Policymakers should take this effect into account when designing unemployment policies. Furthermore, laid off individuals could benefit from public health interventions designed to prevent them from abusing opioids before they start—for example, by making their doctors aware that they are at high risk for opioid abuse.

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