# The Effects of Layoffs on Opioid Use and Abuse*

Marius Opstrup Morthorst, David J. Price, and Peter Rønø Thingholm†

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**Abstract**

The opioid epidemic is often associated with economic hardship. We identify their causal relationship by estimating the effect of mass layoffs on opioid use and abuse in Denmark. This paper has three main contributions. First, we find the clearest evidence that economic conditions affect opioid use: individuals increase consumption by 65%, with evidence of abuse. Second, we disentangle indirect effects: spouses consume 40% more opioids. Third, we connect opioid demand (as we study) to the more prominent literature on supply, finding evidence that effects of layoffs are stronger in areas that have a large underlying supply of opioids.

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†Morthorst and Thingholm: Aarhus University; Price: University of Toronto.

# 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, 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 labor market outcomes and increased opioid use and abuse.

In this paper, we provide 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 between 2000 and 2011. Because these layoffs are generally unpredictable a few years 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 job displacement 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 most 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 spouse. This improved identification has important implications for public policy.

We find that a layoff causes a worker to increase their yearly opioid consumption by approximately 0.51 Defined Daily Doses (DDDs) in the 5 years after the layoff. This corresponds to an increase of around 65%.[1] Some of the effect is driven by the extensive margin: a layoff causes a worker to be 0.5 percentage points more likely to take any opioids, an increase of about 19%. We find suggestive evidence of abuse, such as increased chronic opioid use and more frequent prescriptions for drugs used to treat opioid abuse. Increased opioid use is concentrated among workers with greater responsibilities—such as primary breadwinners and those raising children—pointing to a high cost of this use and abuse. The effects we document are economically significant: we estimate that economic conditions account for about 30% of opioid prescriptions in Denmark. These results represent the first major contribution of our research: the best-identified effects of economic conditions on opioid use and abuse.

Our second major contribution is to disentangle direct and indirect effects of economic conditions on opioid use. In addition to the direct effect on laid-off individuals, there are several mechanisms through which layoffs could affect others. Laid off individuals' use of opioids could directly spread to others in their network if laid off individuals communicate about their use, or give those opioids to others (either for free or at a price). Alternatively, the effect could be indirect—for example, if layoffs cause others in their network to experience despair, which then causes increases in use. Although we cannot disentangle these mechanisms, we do find that layoffs also caused increased opioid use and abuse among spouses. When a married individual is laid off, their spouse

[1] Defined daily doses are a measure, defined by the World Health Organization, of how many doses of opioids have been prescribed. Using DDDs helps us to compare the use of many different types of opioids. Similar results on usage are found using Oral Morphine Equivalents (OMEQs), a measure of the analgesic (pain-reducing) strength of the drugs prescribed.

increases their prescription opioid use by 0.98 DDDs, which corresponds to a 40% increase.

In this paper, we focus on how economic conditions might affect demand for opioids. A separate and larger literature, discussed below, examines how the supply of opioids has contributed to the opioid crisis, through the actions of pharmaceutical companies, doctors, and others. Our third major contribution is to link these two literatures: we find evidence that layoffs have the greatest effect on opioid use where there is already a large supply of opioids available. In municipalities in the lowest quartile of opioid prescriptions, layoffs have essentially no effect on opioid use; in municipalities in the highest quartile, effects are over twice as large as our baseline effects.

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 Laird and Nielsen (2016), Thingholm (2019), Harris et al. (2020), and Park and Powell (2021), 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. (2019) 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. Other similar work includes that by Hollingsworth et al. (2017), Betz and Jones (2018), Charles et al. (2019), Venkataramani et al. (2020), and Musse (2024). 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. Kennedy-Hendricks et al. (2016), Barnett et al. (2019), Khan et al. (2019), Nguyen et al. (2020)), physician quality (e.g., Schnell and Currie (2018), Eichmeyer and Zhang (2022), Eichmeyer and Zhang (2023)), the marketing of opioids (e.g. Hadland et al. (2019), Miloucheva (2021)), and local public policy (e.g. Popovici et al. (2018), Alpert et al. (2021), Sacks et al. (2021), Arteaga and Barone (2022), Ahammer and Packham (2024)). In fact, as argued by Musse (2024), the direct effect of job loss could be to decrease opioid use if workers are less likely to be injured; the effect is therefore an empirical question. 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.

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. Consequences of layoffs need not be wholly economic, and a growing literature explores health effects: for example, Sullivan and von Wachter (2009) find that layoffs can lead to increased mortality. Within this literature, our paper is 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. In contrast, some papers find little effect. For example, Roulet (2020) finds no effect on a range of outcomes, including opioid use. Key differences between that manuscript and our work include that we examine a longer time period, during which the opioid epidemic was stronger (effects for the second half of our sample, which Roulet (2020) excludes, are roughly twice as large as the first half); and we restrict attention to establishments with least 50 employees before layoffs (rather than 5 in Roulet (2020)) to prevent reverse causality. 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. Finally, Marcus (2013) explores effects of layoffs on spousal mental health; we complement that by examining opioid use. Other similar work includes that by Eliason and Storrie (2009a,b), Salm (2009), Schmitz (2011), Marcus (2014), Schaller and Stevens (2015), Michaud et al. (2016), and Rocco et al. (2018).

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 explains our main methodology, including how mass layoffs can be used to identify causal effects of job loss and how we estimate these effects. Section 4 describes the Danish register data we use in this paper, defines mass layoffs, and specifies how we select displaced workers and a matched comparison group. Section 5 presents results first on labor market outcomes, next on opioid use and abuse for the laid-off individual, and finally on spillovers on spouses. We extrapolate from our results to the broader context of the opioid crisis and related literature in Section 6. Section 7 concludes.

## 2 Health and economic context

Denmark provides universal healthcare to all residents; this includes access to most health care services free of charge. Primary care physicians (PCPs) operate in small, private practices, outside of hospitals, where they provide primary care and act as gatekeepers to specialists and hospitals. PCPs can write prescriptions for medicines that can be redeemed at community pharmacies. A reimbursement scheme provides standardized partial refunds on most prescription medication, including opioids. The reimbursement is increasing in prescriptions with an annual maximum out-of-pocket cost of approximately 600 euros. Reimbursement is automatically deducted from the price charged at the pharmacy.

It is estimated that 3-4% of the Danish population regularly use opioids (Nissen et al., 2019). In 2017, Tramadol was the most frequently prescribed opioid analgesic with 4.6% of the population receiving a prescription. Comparatively, in 2017 morphine and oxycodone was prescribed to 1.7% and 1.2% respectively. Since the 1990s, prescriptions for opioids have been rising rapidly in Denmark. The increase is driven by an increase in the number of users of strong opioids and an increase in the total consumption of weaker opioids (e.g. Tramadol), which more than doubled. Notably, until 2017, prescriptions of opioids classified as weak opioids were not subject to governmental surveillance. Overall, opioid prescriptions for the non-elderly increased by 43% from 1999 to 2017 (Nissen et al., 2019).

When compared to other countries, Denmark ranks among the highest in per capita opioid prescriptions. Figure 1 depicts the opioids prescribed per capita in selected OECD countries; while the United States occupies a clear first place, the Danish level of prescriptions per capita is near the top (number 6) which is higher than otherwise comparable countries like Norway and Sweden. Jarlbaek (2019) finds that the utilization patterns in other Scandinavian countries are fundamentally different from Denmark. While Denmark has fewer users per 1,000 individuals in the population than both Norway and Sweden, the strength of the opioids prescribed were much higher in Denmark. In 2014, the OMEQs per user in Denmark was 3 times higher than the OMEQs per user in Norway, and notably in other Scandinavian countries the number of DDDs per 1,000 individuals have either fallen (Sweden) or remained stable (Norway) in the period 2006-2014. Ultimately, this positions Denmark as a high-prescribing country with a considerable potential for opioid abuse.

[Figure 1 here.]

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 Denmark (European Monitoring Centre for Drugs and Drug Addiction, 2019). Opioid addictions and abuse are most often treated with substitution therapy, where the monitored use of drugs such as methadone or buprenorphine is a central part of the treatment. Such treatments lessen 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. However, prescription opioid use in Denmark has been associated with addictive behaviors in general (Højsted et al., 2013) and an increased risk of injuries and toxicity/poisoning resulting in hospital inpatient admissions (Ekholm et al., 2014).

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 in much of the world, however, unemployment did not spike during the COVID-19 outbreak; it sat at 4.6% in April, 2020. Given the context of high inequality, poverty, and unemployment in many parts 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 are prescribed substantially more opioids

[2] An 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 (13,500 euros).

[3] See `https://fred.stlouisfed.org/series/LRHUTTTTDKM156N`.

than those in other income brackets. The graph also shows that opioid use in every earnings bracket has been increasing over time.

[Figure 2 here.]

# 3 Identification and empirical strategy

In this paper we want to investigate the causal effect of job displacement on use and abuse of opioids. However, not all changes in employment status are exogenous to the worker. For example, a health shock may cause the worker to become unemployed and subsequent increases in opioid use may be due to the health shock and not the job loss. To overcome this challenge, we rely on job displacements resulting from mass layoffs. A sufficiently large layoff at a sufficiently large establishment is arguably exogenous to the displaced workers' observable as well as unobservable characteristics, especially the displaced worker's propensity to use opioids. The definition of mass layoffs and displaced workers follows Bertheau et al. (2023) and are explained in detail in Section 4. (In an appendix, we show that our results are robust to varying this definition.) To determine the counterfactual, we construct a comparison group using a matched sample from the universe of all Danish workers who have not experienced a mass layoff. The sampling and matching strategy is described in detail in Section 4.

We estimate effect of job displacement on opioid use and abuse using two difference-in-difference estimators. First, we estimate effects in each year relative to displacement by estimating

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

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 comparisons), and $\mathbb{1}(\cdot)$ is an indicator function. This specification includes an individual fixed effect $\alpha_i$ and a year fixed effect $\gamma_t$. Standard errors are clustered at the level of the individual worker and the establishment to account for the fact that outcomes may be correlated by the establishment at which an individual was laid off. Our coefficients of interest are $\delta_\tau$, which we plot in a graph, as in Figure 3.[4] Similar to Bertheau et al. (2023), we omit the indicator for year $t - d = -3$ to ensure that any anticipation of a layoff does not bias our results.

Second, we estimate average effects in all post-displacement years by estimating

$$y_{it} = \alpha_i + \gamma_t + \delta \mathbb{1}\left(t \geq d\right) \times Displaced_i + \epsilon_{it} \tag{2}$$

with terms defined similarly to Equation 1. Here, our coefficient of interest is $\delta$, which we show in a table, as in Table 2.

The key identifying assumption for the difference-in-difference estimator is bias stability (or parallel trends), which implies that the trend in expected outcomes (such as earnings and opioid use) is the same among displaced and comparison workers in the absence of a mass layoff. To further strengthen the identification, we construct our comparison group to consist of individuals who do not experience a mass layoff but who are as similar as possible to displaced workers on a range of predetermined observable characteristics, including earnings; we discuss how we construct this

[4]In these graphs, points for years -2 and -1 are shown in gray because they occur before the layoff (and thus could be part of the pre-trend) but are close enough to the layoff that a worker might anticipate it (and thus could be part of the effect).

comparison group in Section 4.

# 4 Data

The data used in this study stems from several administrative registers, all maintained by Statistics Denmark and the Danish Health Data Authority. We link data from different registers using the unique Danish civil registration number, which allows matching a given sample of workers in Denmark with a wide range of information—for example, labor market attachment and health care utilization.

Information on mass layoffs and workers' labor market outcomes are derived from registers with matched employer-employee data covering the universe of the Danish working age population. All firms and establishments are registered. We can therefore follow establishments over time and identify workers in each establishment.

Information on opioid use and abuse measures are primarily from the National Prescription Register which contains information on all sales and deliveries of medication. The register includes information on medication sold at pharmacy outlets as well as in non-pharmacy outlets, and any medication that is administered by physicians or in a hospital. All prescriptions are coded using the Anatomical Therapeutic Chemical (ATC) Classification System, which allows us to identify prescription opioids, along with all other prescription drugs.

## Sample selection: Establishments and mass layoffs

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 experiences a mass layoff. We follow the approach taken by Bertheau et al. (2023): we focus on establishments in private firms with at least 50 employees in year $t-1$, and we identify plants where employment contracts by at least 30% from year $t-1$ to $t$.[5]

Further, to ensure that the reduction in the number employees is not from switches to another establishment (for example as part of a merger or acquisition), we require that no more than 20% of the displaced workers are moved to the same second establishment.

### Sample selection: Displaced workers and comparison group

As our treated group, we include all individuals working at the establishments that experience a mass layoff in year $t$, who (1) are employed there full time in years $t-3$, $t-2$, and $t-1$; (2) are aged between 20 and 50 in year $t-1$; (3) have never before experienced a mass layoff; and (4) who are not employed at the establishment in year $t$. We sample workers displaced following a mass layoff in the period 2000-2011. Ultimately, we observe 77,019 laid-off individuals satisfying our restrictions. Appendix tables highlight the share of workers displaced in a given year (A.1), different types of municipalities (A.2), and different industries (A.3). Mass layoffs are more pronounced in times of economic downturn (e.g. the global financial crisis, 2008-2009) and in manufacturing.

Potential comparisons consist of the universe of Danish workers who (1) are employed full time in years $t-3$, $t-2$, and $t-1$, and remain at one private establishment in all three years; and (2) are aged between 20 and 50 in year $t-1$. There are over 2 million individual-year observations that

[5] Appendix Figure A.1 shows the trajectory for mean number of employees in an establishment (Panel A) and the probability of an establishment having any employees (Panel B). For establishments experiencing a mass layoff we see a reduction in the number of employees of approximately 70% after 3 years and an approximately 40% reduction in the probability of the establishment having any employees.

are potential comparisons. In order to ensure that the comparison group does not consist of workers who are fundamentally different from the displaced workers, we match each displaced worker to one worker selected from the pool of potential comparison workers. We first exactly match on calendar year of mass layoff, sex, and industry (3 categories: *manufacturing*, *service* or *other*) in year $t-3$. Next, we calculate the propensity to be displaced based on earnings, age, tenure and employer size in year $t-3$. Lastly, for each displaced worker we select a suitable comparison worker based on nearest neighbor matching (on the propensity score) without replacement.

Table 1 shows descriptive statistics for opioid use, labor market attachment and other demographic characteristics across displaced workers and comparison workers. Although there are slight differences between the groups, they are not economically significant.[6] Note that neither prior opioid use nor an indicator for being married or having children are used when matching with comparison workers. However, they are still relatively balanced. In Appendix Table A.6, we add these when matching, and results change little.

[Table 1 here.]

Note that we do not condition on comparison workers being stably employed in the post-period, as do Jacobson et al. (1993). Doing so would bias our results, as some individuals in the comparison group would stop working even absent a layoff (Krolikowski, 2018). We also do not compare laid off individuals to those who will be laid off in the future. Doing so would also bias our results, as any individual who is laid off in year $t$ must be working in year $t-1$; thus individuals who are laid off are often observed to have increasing earnings in the years before their layoff, since 100% are employed the year before it, but fewer are employed in previous years. This would appear as an

[6] In Table 1, descriptive statistics on establishments are based on the sample of workers. Appendix Table A.4 shows similar statistics at the establishment level.

“anticipation” effect if these individuals were included in the comparison group.

# 5 Results

The first step of our empirical analysis is to establish the effect of job displacement on labor market outcomes. In the second step of our analysis, we estimate the effect of job displacement on measures of opioid use and abuse of the displaced worker. Next, we investigate whether job displacement has spillover effects in opioid use among displaced workers’ spouses. Finally, we explore whether the effect of job displacement differs between geographical areas with high versus low underlying opioid use to explore the extent to which supply of and demand for opioids interact to drive opioid use.

## 5.1 Effects on labor market outcomes

We first investigate the effect of experiencing a job displacement following a mass layoff on labor market outcomes. These results replicate a long literature showing that layoffs have both immediate and long-lasting effects on employment status and earnings; see Section 1. In our context, they may be loosely thought of as a “first stage” to the extent that any effect on opioid use and abuse acts through contemporaneous labor market outcomes. (However, as we discuss above, we do not believe that this is the only channel through which layoffs affect opioid use, so the exclusion restriction would be unlikely to hold.)

Panel A in Figure 3 shows the earnings trajectory (in euros) for displaced workers and matched comparison workers. Prior to a mass layoff event, trends in earnings were similar for both displaced workers and the matched comparison group. Displaced workers’ earnings drop sharply from year -1 to year 0 and further in year 1, while earnings for the matched comparison group of non-displaced workers flattens out (since we require the comparison group to work in years -3 to -1, but not thereafter). Panel B in Figure 3 shows the estimated effects of a mass layoff using the event study specification from Equation 1.[7] As has been shown in past literature, layoffs have substantial economic implications for workers. As shown in Table 2, column (1), a layoff causes a worker to earn about 6,500 fewer euros (around 50,000 Danish kroner, or 7,000 US dollars) per year.[8] This corresponds to a 15.5% drop in earnings, and the effect persists even five years after the layoff. Table 2, column (2) shows that total income also drops, as other income, such as unemployment insurance, does not make up this gap.[9] Much of this effect comes from the extensive margin, as shown in Table 2, column (3): layoffs cause people to be about 3.5 percentage points less likely to be working (that is, to earn any money) for the five years after the event.

[Figure 3 here.]

[Table 2 here.]

## 5.2 Effects on opioid use and abuse

We now progress to our outcomes of interest: effects on opioid use. We find that, in addition to economic effects, laid off workers are substantially more likely to use opioids. To investigate the effect of job displacement on opioid use we focus on whether it affected (1) the probability of get-

[7] Appendix Table A.5 presents the corresponding event study point estimates.

[8] All monetary values in this paper are adjusted for inflation to 2015 values.

[9] Eligibility for unemployment insurance is conditional on membership in an unemployment insurance fund and prior labor market participation. However, the benefit is considered generous with a benefit duration of 2 years and an average replacement rate 83% (Kreiner and Svarer, 2022).

ting any prescription for opioids and (2) the usage of opioids measured as Defined Daily Doses (DDDs) of opioids prescribed.[10] Panel A in Figure 4 shows the fraction of displaced and matched comparison workers who get any prescription for opioids. For both groups the trend is generally increasing across time. This increase occurs for two reasons: first, because use is increasing throughout Denmark during this time period; and second, because the population is getting older, and older individuals use more opioids. (This overall increase highlights the importance of finding an appropriate comparison group.) The trend for both groups is similar in years before -3, suggesting that the parallel trends assumption holds in this setting. However, between years -2 and -1, the trends diverge, and there is a sharp increase in the fraction of displaced workers with an opioid prescription. Panel B in Figure 4 shows the corresponding estimated effects. As shown in Table 3, column (1), we find that that laid-off workers are 0.5 percentage points more likely to take any opioids, an increase of about 19%.

[Figure 4 here.]

Panel A in Figure 5 shows the mean Defined Daily Doses (DDDs) for displaced and matched comparison workers. Again, for both groups the trend is generally increasing across time. Around the time of the mass layoff the displaced workers start to increase their consumption of prescription opioids. This change in trajectory for the displaced workers persists for the remaining years. Panel B in Figure 5 shows the corresponding estimated effects. In Table 3, column (2), we find that a layoff causes a worker to take approximately 0.5 more DDDs of opioids per year in the 5 years after the layoff, an increase of around 65%. In Table 3, column (3), we find that there is also a significant effect on Oral Morphine Equivalents (OMEQs), which measures the total analgesic (pain-reducing)

[10] ATC codes N02AA01, N02AA03, N02AA04, N02AA05, N02AA55, N02AB02, N02AB03, N02AC04, N02AE01, N02AG02, N02AJ06, N02AJ07, and N02AX02.

effect of each drug.[11]

[Figure 5 here.]

These results are robust to a variety of changes in our empirical strategy. In Appendix Table A.6, we adjust the variables used to match treated and control workers; this includes matching on opioid use before the layoff.[12] In Appendix Table A.7, we vary our definition of layoffs and our restriction on which employees are included in our results.[13] There is little economically significant difference among any of these results.

[Table 3 here.]

Interestingly, by both measures, opioid use increases in the year before the actual layoff. Previous studies have found that layoffs are often anticipated a year or two before they occur (Wunder and Zeydanli, 2021; Ahammer et al., 2023; Miele and Kai, 2024). Employees can often tell that their employers are experiencing difficulties: wages may stagnate, coworkers may be laid off, managers may discuss problems the firm is experiencing, and workers may even receive notice of impending layoffs before anyone loses their job. If layoffs cause opioid use because of the emotional toll they exact, it should thus not be surprising that this effect begins before the layoff itself.

[11] Appendix Figure A.2 shows the mean OMEQs for displaced and matched comparison workers (Panel A) and the corresponding estimated effects (Panel B).

[12] In Appendix Table A.6 we are particularly interested in testing whether matching on prior opioid use (an outcome of interest) changes the results. Column (6) presents results for a sample matched on changes in prior opioid use, following Stuart et al. (2014) and Daw and Hatfield (2018); column (7) presents results for a sample workers matched on prior opioid use in year $t-5$ and year $t-3$. Results are very similar in both cases.

[13] For example, in our baseline specification, we include establishments that lose at least 30% of their workers. In Appendix Table A.7, we show that point estimates are similar as we increase the threshold to 50%, 70%, and even 90% of workers, though results become less precise.

In Table 4, we show that layoffs increased the use of strong opioids (columns (1) and (3)) in addition to weak ones (columns (2) and (4)), suggesting that the effect could have serious consequences for displaced adults. Column (5) shows that displaced workers are more likely to chronically use opioids,[14] which suggests opioid abuse.

[Table 4 here.]

Table 5 explores effects on related drugs. Benzodiazepines are often prescribed to treat anxiety, but—like opioids—can be addictive and dangerous. Although benzodiazepines are used less frequently, we find that their use increases after layoffs in columns (1) and (2).[15] Antidepressant use (columns (3) and (4)) also increases.[16] Taken together, these results suggest, perhaps unsurprisingly, that laid off workers experience worse mental health, and look to prescription drugs to help manage these symptoms. We also find in columns (5) and (6) an increase in prescriptions for drugs commonly used to treat opioid dependence, such as methadone.[17] This result suggests that increased opioid use by laid off workers also leads to increased abuse. Note that we do not find a significant effect on DDDs for opioid dependence drugs, although the point estimate suggests increased use. This may be due to outliers, as use of these drugs is highly skewed: the average person in our sample at time -3 with any such prescription received 770 DDDs per year, whereas for opioids the statistic is 30 DDDs.

[Table 5 here.]

[14]Chronic opioid use is defined as opioid treatment spells that are longer than six months. If there are fewer than four months between two prescriptions, they are considered part of the same spell (Danish Health Authority, 2016).

[15]ATC codes corresponding to all subcategories in N03AE.

[16]ATC codes corresponding to all subcategories N06A.

[17]ATC codes corresponding to all subcategories in N07BC.

In Table 6, we explore other explanations for the increase in opioid use. If laid off workers experience more pain, or are more likely to seek treatment for pain, we might expect that prescriptions for non-steroidal anti-inflammatory drugs (NSAID) to increase, or expenses for physiotherapy (which is subsidized, usually impartially, by the government). In columns (1) and (2), we see this did not happen.[18] Indeed, use of physiotherapy declined, likely because laid off workers were less able to spend money on such therapy; it is possible that part of the increase in opioid use was to compensate for this decline. Two common reasons for opioid prescriptions are cancer diagnoses and disc prolapses. In column (3), we do not see any evidence for an effect on cancer. There is a marginally statistically significant increase in disc prolapses in column (4), though the point estimate is likely too small to account for much of the increase in opioid use.

[Table 6 here.]

Tables 7, 8, 9, and 10 explore the heterogeneity of our baseline results. Effects are similar between men and women (Table 7, columns (1) and (2)), and between those in and near Copenhagen (by far the largest metropolitan area) and elsewhere (Table 7, columns (3) and (4)). Point estimates of effects are stronger for those who have previously used opioids than those who haven't (Table 7, columns (5) and (6)), though much less precisely estimated. Effects vary by the worker's education and job before a layoff: effects are strongest for those with less education (Table 8, columns (1), (2), and (3)), those in manufacturing (Table 8, columns (4), (5), and (6)), and less knowledge intensive occupations (Table 9, columns (6) and (7)). More research is needed to determine if those workers are most affected due to the jobs they work (for example, manual jobs likely cause more pain), as opposed to a factor related to the worker or their environment. We find evidence that effects

[18] ATC codes corresponding to all subcategories in M01A.

are strongest among those with more responsibilities: workers with children (Table 8, columns (7) and (8)), workers over 30 (Table 9, columns (1), (2), and (3)), and primary breadwinners (Table 9, columns (4) and (5)). The stress of losing a job may weigh particularly heavily on these workers, but they are also the workers for whom opioid use is more likely to damage the lives of others; this result emphasizes the importance of understanding the effect of layoffs on opioid use. Finally, effects are strongest later in our sample period (Table 10, columns (1) and (2)), likely because opioid use was much higher during this time.

[Table 7 here.]

[Table 8 here.]

[Table 9 here.]

[Table 10 here.]

### 5.3 Within-family spillovers

This stronger effect on workers with responsibility for others emphasizes the fact that layoffs do not just affect those laid off; they also affect entire families and communities. To begin to understand this cost, we examine the effect on spouses. Figure 6 shows that, in fact, there is a significant increase in spouses' use. We explore this effect in more detail in Table 11. Panel A replicates our main analysis for individuals who have a spouse; those effects are similar to our baseline results. Panel B estimates the effect on spouses themselves. Layoffs cause spouses to be 0.4 percentage points more likely to use opioids, and they use significantly more over time—the effect on DDDs is stronger than that on the laid off individuals themselves. Note that spouses' pre-layoff opioid use is substantially higher than the use of laid-off individuals; this is likely due to the fact that we do not require spouses to work before the layoff event to be in our sample, and we thus expect them *ceteris paribus* to be in worse health. As discussed above, our ability to examine these spillover effects is a key benefit of our research design. Prior research, which examines effects on whole regions, can only examine effects on the worker and any family put together; based on our results, this elides an important distinction. Considering that there may be effects on other family members or others connected to the laid off individual, it is likely that much of the effect of an economic shock on opioid use operates indirectly.

[Figure 6 here.]

[Table 11 here.]

### 5.4 The role of opioid supply

Much prior research on the causes of the opioid epidemic has focused on supply factors: for example, how actions of pharmaceutical companies and doctors have led to the growth of opioid use and abuse (Maclean et al., 2020). In this paper, we focus on demand factors: how economic factors can cause individuals to use opioids, regardless of the supply. Of course, in any market, quantity is a function of both supply and demand, and that is true in this context as well. To explore this dynamic, we investigate how effects of layoffs vary depending on the background supply of opioids in the area where an individual lives. In particular, we divide the sample into four equally-sized groups by the average DDDs of opioids prescribed to individuals in workers' municipalities. As shown in Figure 7, we find evidence that the effect of layoffs on opioid use is stronger in those municipalities where the underlying use of opioids is already high. As shown in Table 12, point estimates for the effect of layoffs on opioid use in municipalities in the top 25% of opioid use are substantially higher than those in the bottom 25%. The extent to which opioid use and abuse increases following a mass layoff may be related to the environment in which the mass layoff occurs. Thus our results underline the importance of both supply and demand—and, crucially, their interaction—in explaining increasing opioid use.

[Figure 7 here.]

[Table 12 here.]

# 6 Extrapolation and relation to other literature

## 6.1 Extrapolation

With 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. Above, we find that layoffs increase nonemployment by about 3.5 percentage points. Each layoff also caused about 0.007 people (combining effects on laid off individuals and their spouses, among the approximately 50% of individuals who have spouses: $0.005 + 0.5 \times 0.004 = 0.007$) to start using opioids. (This may be an underestimate if there are effects on opioid use by other family members, friends, or neighbors.) Combining these results, nonemployment causes about 0.2 (=0.007 / 0.035) people to start using opioids. Denmark's unemployment rate in June 2024 is 5.9%;[19] thus unemployment would cause 1.2% of people to use opioids, out of around 4% who actually do (so around 30% of total use).

[19] See https://fred.stlouisfed.org/series/LRHUTTTTDKM156S.

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 in January 2024 is 4.3%.[20] With the same methodology, and assuming the same relationships apply in the United States, unemployment would cause about 0.86% of people to use opioids, a small fraction of the total. 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.

### 6.2 Comparison with related literature

With some strong assumptions, we can compare our results to studies in other countries that examine the effect of other economic shocks on opioids. Note that this back-of-the-envelope calculation may be an underestimate; as noted by Bertheau et al. (2023), the effect of layoffs on economic outcomes is smaller in Denmark than in many other countries.

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 $1.7 \times 10^{-5}$ annual opioid-related deaths for every annual DDD prescribed.[21] We

[20] See https://fred.stlouisfed.org/series/UNRATE.

[21] This is calculated by noting that there are approximately 82,000 annual opioid deaths, according to the US Centers for Disease Control and Prevention (https://nida.nih.gov/research-topics/trends-statistics/overdose-death-rates; 337 million people, according the the US Census Bureau (https://www.census.gov/

find that layoffs increase nonemployment by about 0.035, and increase opioid DDDs by around 1.0 (with both laid off individuals and their spouses). Assuming, again, that effects on opioid use operate only through nonemployment, our results would imply that nonemployment causes about 29 (≈1 / 0.035) additional DDDs per year, which would lead to about $5 \times 10^{-4}$ 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 $1.5 \times 10^{-5}$ opioid deaths. By this metric, then, we find a substantially stronger effect of economic conditions of opioid use; however, much caution is necessary, given the strong assumptions necessary for this comparison.

### 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, extrapolating from Thingholm (2019) (Table A.4, column 6), opioid DDDs increasing by 65% (as we see for displaced workers) would cause nonemployment to increase by about 0.069. Based on this calculation, we expect that there may be a modest feedback loop: someone being laid off in one year might cause them to be about 7% less likely to work in future years due to increased opioid use.

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`popclock/`); and 14 DDDs prescribed per person, according to the OECD, as shown in Figure 1. 82,000 / (337 million) / 14 ≈ $1.7 \times 10^{-5}$.

# 7 Conclusions

Opioid use and economic instability are both rising around the world. This paper provides evidence that these two phenomena are causally linked: layoffs can cause laid off individuals and their families to use more opioids. Our results add to the literature in three primary ways. First, we provide the clearest evidence yet that economic conditions can causally affect opioid use and abuse. Second, our results allow us to disentangle direct effects from indirect effects: this paper is the first to note that layoffs can increase opioid use for spouses of those directly affected. This indirect effect is important in understanding how negative economic conditions can cause whole communities to be harmed by opioids. Finally, we link the literature on demand for opioids (via economic conditions, as we study) to the literature on supply of opioids (driven by pharmaceutical companies, doctors, and other actors): as we might expect in any market, consumption is driven by the interaction of these factors, so both factors must be considered in understanding the dynamics of the rise in opioid abuse.

Policymakers should take these results into account when designing unemployment policies: maintaining employment is, perhaps, even more important than previously understood, as it can also help reduce drug use. 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. Furthermore, attempts to rein in the supply of opioids ought to be directed especially at areas where demand is high or increasing—that is, areas where economic conditions are poor, or likely to become worse in the future.

Despite these advances, much research remains. Especially important is to understand the mechanism that links layoffs to opioid use. Based on the patterns observed, it is unlikely that layoffs cause individuals to experience more pain: after all, mass layoffs are broad enough that they are unlikely to be directed at individuals in pain, and we find no evidence of other increased treatment for pain. However, although the literature has focused on despair, other possible explanations remain; for example, laid off individuals could try to get opioid prescriptions in order to sell them. It is also important to understand why spillover effects exist: for example, whether spouses experience despair, which leads them to use opioids, or if the opioid use of laid off individuals directly increases opioid use for their spouses. Further research could help therapists and policymakers consider what tools ought to be deployed to most successfully blunt any effect of layoffs on opioid use and abuse.

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Table 1: Descriptive statistics

| | Comparison workers | Displaced workers | Differences | SDM |
|---|---|---|---|---|
| *Worker characteristics* | | | | |
| Earnings [Euro] | 47,882.891 | 47,685.000 | -197.888 | -0.005 |
| | (27,587.037) | (31,708.225) | (151.444) | |
| Income [Euro] | 50,802.207 | 50,814.059 | 11.855 | 0.000 |
| | (37,739.773) | (40,885.184) | (200.490) | |
| Age | 36.322 | 36.265 | -0.057 | -0.005 |
| | (7.989) | (7.816) | (0.040) | |
| Tenure | 6.071 | 6.043 | -0.028 | -0.006 |
| | (3.581) | (3.615) | (0.018) | |
| Female [0,1] | 0.356 | 0.356 | -0.000 | 0.000 |
| | (0.479) | (0.479) | (0.002) | |
| Full time [0,1] | 0.858 | 0.857 | -0.001 | -0.003 |
| | (0.349) | (0.351) | (0.002) | |
| Married [0,1] | 0.467 | 0.455 | -0.012*** | -0.017 |
| | (0.499) | (0.498) | (0.003) | |
| Parent [0,1] | 0.560 | 0.558 | -0.002 | -0.003 |
| | (0.496) | (0.497) | (0.003) | |
| Any opioid [0,1] | 0.024 | 0.026 | 0.002** | 0.009 |
| | (0.154) | (0.161) | (0.001) | |
| Opioid DDDs | 0.652 | 0.775 | 0.123 | 0.006 |
| | (12.036) | (18.414) | (0.079) | |
| *Establishment characteristics* | | | | |
| Industry: Manufacturing [0,1] | 0.473 | 0.473 | -0.000 | 0.000 |
| | (0.499) | (0.499) | (0.003) | |
| Industry: Services [0,1] | 0.314 | 0.314 | -0.000 | 0.000 |
| | (0.464) | (0.464) | (0.002) | |
| Industry: Other [0,1] | 0.213 | 0.213 | -0.000 | 0.000 |
| | (0.409) | (0.409) | (0.002) | |
| Establishment size | 330.374 | 334.652 | 4.278* | 0.006 |
| | (524.397) | (479.036) | (2.559) | |
| *Sample characteristics* | | | | |
| Number of establishments | 5,430 | 3,014 | | |
| Number of workers | 77,019 | 77,019 | | |
| Number of worker-observations | 847,209 | 847,209 | | |

**Notes:** The table presents descriptive statistics for the sample of displaced workers and the matched comparison workers. Each cell in Columns 1–2 the mean of the corresponding variable in the row with standard deviation in parentheses. Column 3 present differences in means and corresponding p-values. Column 4 present the standardized difference in means (SDM) (or the standardized bias); i.e., the difference in means as a share of the square root of the average sample variances of the displaced and comparison workers. “Earnings” includes only labor earnings; “Income” includes labor earnings and all other income sources, including unemployment insurance. Based on Danish register data.

Table 2: Effect of layoffs on labor market outcomes

| | Earnings [Euro] (1) | Income [Euro] (2) | Earnings>0 [0,1] (3) |
|---|---|---|---|
| Displaced × Post | -6,505.542*** | -4,998.075*** | -0.035*** |
| | (130.808) | (179.264) | (0.001) |
| Mean in Time = -3 | [41912.746] | [44770.755] | [1] |
| Observations | 1,232,304 | 1,232,304 | 1,232,304 |

**Notes:** Values are based on Equation 2. Column (1) estimates the effect of a layoff on annual earnings; column (2) estimates the effect on individual annual income from all sources, including labor earnings and unemployment insurance; and column (3) estimates the effect on an indicator for having any earnings. Based on Danish register data.

Table 3: Effect of layoffs on opioid use

| | Any opioid prescription [0,1] (1) | Opioid DDDs (2) | Opioid OMEQ (3) |
|---|---|---|---|
| Displaced × Post | 0.005*** | 0.506*** | 0.420*** |
| | (0.001) | (0.139) | (0.100) |
| Mean in Time = -3 | [0.026] | [0.775] | [0.419] |
| Observations | 1,232,304 | 1,232,304 | 1,232,304 |

**Notes:** Values are based on Equation 2. Column (1) estimates the effect of a layoff on an indicator for having any opioid prescriptions in a year; column (2) estimates the effect on Defined Daily Doses of opioids; and column (3) estimates the effect on oral morphine equivalent (OMEQ) of opioids. Based on Danish register data.

Table 4: Effect of layoffs on types of opioids used

| | Any strong opioids [0,1] (1) | Any weak opioids [0,1] (2) | Strong opioid DDD (3) | Weak opioid DDD (4) | Chronic treatment [0,1] (5) |
|---|---|---|---|---|---|
| Displaced × Post | 0.0012*** | 0.0041*** | 0.2196*** | 0.2864*** | 0.0020*** |
| | (0.0003) | (0.0007) | (0.0792) | (0.1097) | (0.0004) |
| Mean in Time = -3 | [0.0037] | [0.0237] | [0.1735] | [0.6018] | [0.003] |
| Observations | 1,232,304 | 1,232,304 | 1,232,304 | 1,232,304 | 1,232,304 |

**Notes:** Values are based on Equation 2. Column (1) estimates the effect of a layoff on an indicator for having any *strong* opioid prescriptions in a year; column (2) estimates the effect on any *weak* opioid prescriptions; column (3) estimates the effect on Defined Daily Doses (DDDs) of *strong* opioids; column (4) estimates the effect on DDDs of *weak* opioids; and column (5) estimates the effect on an indicator for having an opioid treatment spell of longer than six months; see Footnote 14. Based on Danish register data.

Table 5: Effect of layoffs on related drugs

| | Any benzodiazepine (1) | Benzodiazepine DDD (2) | Any antidepressants (3) | Antidepressant DDD (4) | Any opioid dependence drug (5) | DDD opioid dependence drugs (6) |
|---|---|---|---|---|---|---|
| Displaced × Post | 0.00027*** | 0.03432*** | 0.00590*** | 2.68446*** | 0.00019** | 0.01893 |
| | (0.00010) | (0.01056) | (0.00087) | (0.39827) | (0.00009) | (0.05784) |
| Mean in Time = -3 | [0.00023] | [0.0086] | [0.02705] | [6.66408] | [0.00021] | [0.16179] |
| Observations | 1,232,304 | 1,232,304 | 1,232,304 | 1,232,304 | 1,232,304 | 1,232,304 |

**Notes:** Values are based on Equation 2. Column (1) estimates the effect of a layoff on an indicator for having any benzodiazepine prescriptions in a year; column (2) estimates the effect on Defined Daily Doses (DDDs) of benzodiazepines; column (3) estimates the effect on having any antidepressant prescriptions; column (4) estimates the effect on DDDs of antidepressants; column (5) estimates the effect on any prescription on drugs for treatment of opioid dependence; and column (6) estimates the effect on DDDs of drugs for treatment of opioid dependence. Based on Danish register data.

Table 6: Effect of layoffs on other pain-related outcomes

| | Any NSAIDs [0,1] (1) | Physiotherapist expenses [Euro] (2) | Any cancer-related hospital admission [0,1] (3) | Any disc prolapse-related hospital admission [0,1] (4) |
|---|---|---|---|---|
| Displaced × Post | -0.00100 | -1.766** | 0.00028 | 0.00040* |
| | (0.00010) | (0.700) | (0.01056) | (0.00023) |
| Mean in Time = -3 | [0.149] | [6.175] | [0.002] | [0.00239] |
| Observations | 1,232,304 | 593,200 | 1,232,304 | 1,232,304 |

**Notes:** Values are based on Equation 2. Column (1) estimates the effect of a layoff on an indicator for having any NSAID prescriptions in a year; column (2) estimates the effect on physiotherapy expenses (based on data from after 2005, the only years for which physiotherapy is available); and column (3) estimates the effect on an indicator of having any cancer-related hospital admissions. Based on Danish register data.

Table 7: Heterogeneity, Part 1

| | Gender | | Place of residence | | Prior opioid use | |
|---|---|---|---|---|---|---|
| | Male | Female | Other | Copenhagen area | Never used | User |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Earnings [Euro]* | | | | | | |
| Displaced x Post | -7,315.150*** | -5,040.076*** | -6,422.263*** | -6,702.350*** | -6,288.851*** | -7,918.429*** |
| | (183.926) | (154.218) | (130.194) | (368.990) | (139.085) | (360.894) |
| Mean in Time = -3 | [45944.051] | [34615.713] | [41067.259] | [44939.215] | [42003.361] | [41113.402] |
| *Panel B: Any opioid prescription [0,1]* | | | | | | |
| Displaced x Post | 0.004*** | 0.006*** | 0.005*** | 0.003*** | 0.004*** | 0.017*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.005) |
| Mean in Time = -3 | [0.025] | [0.028] | [0.028] | [0.02] | [0] | [0.26] |
| *Panel C: Opioid DDD* | | | | | | |
| Displaced x Post | 0.550*** | 0.427* | 0.519*** | 0.441** | 0.343*** | 1.262 |
| | (0.162) | (0.258) | (0.167) | (0.215) | (0.082) | (1.195) |
| Mean in Time = -3 | [0.752] | [0.818] | [0.873] | [0.425] | [0] | [7.615] |
| Observations | 793,776 | 438,528 | 961,224 | 271,080 | 1,112,224 | 120,080 |

**Notes:** Values are based on Equation 1. Panel A presents estimates of the effect of a layoff on earnings; Panel B the effect on having any opioid prescriptions; and Panel C the effect on Defined Daily Doses of opioids. Each column includes results restricting to workers who satisfy the listed condition three years before layoff.

Table 8: Heterogeneity, Part 2

| | Education | | | Sector | | | Family | |
|---|---|---|---|---|---|---|---|---|
| | Basic (1) | Vocational or short (2) | Medium or long (3) | Manufacturing (4) | Services (5) | Other (6) | No children (7) | Children (8) |
| *Panel A: Earnings [Euro]* | | | | | | | | |
| Displaced x Post | -6,458.741*** | -5,670.160*** | -6,681.092*** | -7,287.135*** | -4,961.441*** | -7,050.868*** | -5,939.315*** | -6,966.869*** |
| | (174.814) | (144.808) | (546.555) | (153.250) | (282.274) | (280.129) | (184.839) | (181.990) |
| Mean in Time = -3 | [33839.796] | [41577.944] | [60175.784] | [40663.351] | [44955.87] | [40192.5] | [36607.443] | [46116.182] |
| *Panel B: Any opioid prescription [0,1]* | | | | | | | | |
| Displaced x Post | 0.007*** | 0.003*** | 0.001 | 0.006*** | 0.003** | 0.003** | 0.004*** | 0.005*** |
| | (0.002) | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) | (0.001) | (0.001) |
| Mean in Time = -3 | [0.032] | [0.027] | [0.014] | [0.031] | [0.021] | [0.025] | [0.019] | [0.033] |
| *Panel C: Opioid DDD* | | | | | | | | |
| Displaced x Post | 0.522* | 0.485** | 0.096 | 1.028*** | -0.154 | 0.323*** | 0.367** | 0.620*** |
| | (0.295) | (0.190) | (0.187) | (0.228) | (0.189) | (0.302) | (0.156) | (0.216) |
| Mean in Time = -3 | [1.03] | [0.745] | [0.355] | [0.932] | [0.436] | [0.929] | [0.5] | [0.993] |
| Observations | 350,504 | 690,072 | 191,728 | 582,336 | 387,472 | 262,496 | 543,448 | 688,816 |

**Notes:** Values are based on Equation 1. Panel A presents estimates of the effect of a layoff on earnings; Panel B the effect on having any opioid prescriptions; and Panel C the effect on Defined Daily Doses of opioids. Each column includes results restricting to workers who satisfy the listed condition three years before layoff. For education, "Vocational or short" includes those with a vocational degree or a short-cycle higher education degree; "Medium or long" includes those with a medium- or long-cycle higher education degree.

Table 9: Heterogeneity, Part 3

| | Age | | | Breadwinner | | Occupation | |
|---|---|---|---|---|---|---|---|
| | <30<br>(1) | 30-40<br>(2) | >40<br>(3) | Breadwinner<br>(4) | Not breadwinner<br>(5) | High Knowledge intensive<br>(6) | Low Knowledge intensive<br>(7) |
| *Panel A: Earnings [Euro]* | | | | | | | |
| Displaced x Post | -4,466.292*** | -5,853.207*** | -8,455.550*** | -7,954.340*** | -4,671.025*** | -8,100.422*** | -6,341.556*** |
| | (226.640) | (205.475) | (225.279) | (291.515) | (270.543) | (638.246) | (124.092) |
| Mean in Time = -3 | [26927.849] | [44125.608] | [48167.707] | [53369.606] | [34614.895] | [53369.606] | [34614.895] |
| *Panel B: Any opioid prescription [0,1]* | | | | | | | |
| Displaced x Post | 0.002 | 0.006*** | 0.005*** | 0.005*** | 0.006*** | 0.001 | 0.005*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) | (0.002) | (0.001) |
| Mean in Time = -3 | [0.015] | [0.023] | [0.037] | [0.03] | [0.033] | [0.03] | [0.033] |
| *Panel C: Opioid DDD* | | | | | | | |
| Displaced x Post | 0.375* | 0.553*** | 0.552** | 0.606*** | -0.057 | -0.054 | 0.613*** |
| | (0.204) | (0.203) | (0.269) | (0.222) | (0.479) | (0.234) | (0.205) |
| Mean in Time = -3 | [0.218] | [0.585] | [1.295] | [0.774] | [1.153] | [0.774] | [1.153] |
| Observations | 270,208 | 486,576 | 475,520 | 380,792 | 187,248 | 155,320 | 628,264 |

**Notes:** Values are based on Equation 1. Panel A presents estimates of the effect of a layoff on earnings; Panel B the effect on having any opioid prescriptions; and Panel C the effect on Defined Daily Doses of opioids. Each column includes results restricting to workers who satisfy the listed condition three years before layoff. The "breadwinner" conditions are restricted to married individuals; a breadwinner is the person who earned the most money. Occupation groupings are based on Statistics Denmark (2011); high knowledge intensive occupations are those with codes 1 and 2, while low knowledge intensive occupations are those with codes 4, 5, 6, 7, 8, and 9.

Table 10: Heterogeneity, Part 4

| | Layoff timing | |
|---|---|---|
| | 2000-2005 | 2006-2011 |
| | (1) | (2) |
| *Panel A: Earnings [Euro]* | | |
| Displaced × Post | -4,910.883*** | -8,624.803*** |
| | (174.398) | (215.896) |
| Mean in Time = -3 | [41314.667] | [42743.522] |
| *Panel B: Any opioid* | | |
| Displaced × Post | 0.003*** | 0.007*** |
| | (0.001) | (0.001) |
| Mean in Time = -3 | [0.023] | [0.032] |
| *Panel C: Opioid DDD* | | |
| Displaced × Post | 0.400** | 0.796*** |
| | (0.172) | (0.227) |
| Mean in Time = -3 | [0.567] | [1.065] |
| Observations | 639,104 | 515,808 |

**Notes:** Values are based on Equation 1. Panel A presents estimates of the effect of a layoff on earnings; Panel B the effect on having any opioid prescriptions; and Panel C the effect on Defined Daily Doses of opioids. Each column includes results restricting to workers who satisfy the listed condition three years before layoff.

Table 11: Direct and spillover effects inside the family

| | Any opioid prescription [0,1] (1) | Opioid DDDs (2) |
|---|---|---|
| *Panel A: Direct effect on displaced workers* | | |
| Displaced × Post | 0.007*** | 0.622*** |
| | (0.001) | (0.208) |
| Mean in Time = -3 | [0.031] | [0.898] |
| Observations | 560,832 | 560,832 |
| *Panel B: Spousal spillovers* | | |
| Displaced × Post | 0.004*** | 0.979*** |
| | (0.001) | (0.334) |
| Mean in Time = -3 | [0.043] | [2.44] |
| Observations | 560,832 | 560,832 |

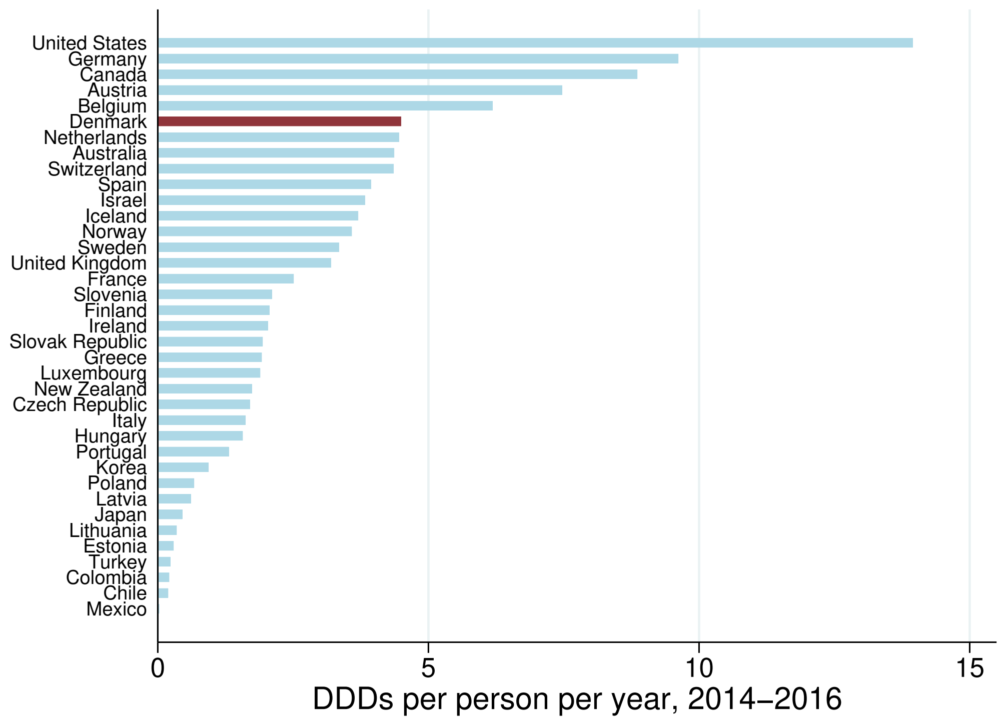
**Notes:** Values are based on Equation 2. In all columns, the sample is restricted to laid off individuals (and those matched to them) who have spouses. Panel A, show effects on an indicator for opioid use and effects on DDDs, respectively, for laid off individuals with spouses. Panel B show effects on an indicator for opioid use and effects on DDDs, respectively, for the spouses themselves. Based on Danish register data.

Table 12: Effects on prescription opioid DDDs used across geographic supply

| | Any opioid prescription [0,1] | | Opioid DDD | |
|---|---|---|---|---|
| | 1st quartile | 4th quartile | 1st quartile | 4th quartile |
| | (1) | (2) | (3) | (4) |
| Displaced × Post | 0.001 | 0.008*** | 0.030 | 1.204*** |
| | (0.001) | (0.002) | (0.205) | (0.320) |
| Observations | 307,691 | 307,690 | 307,691 | 307,690 |

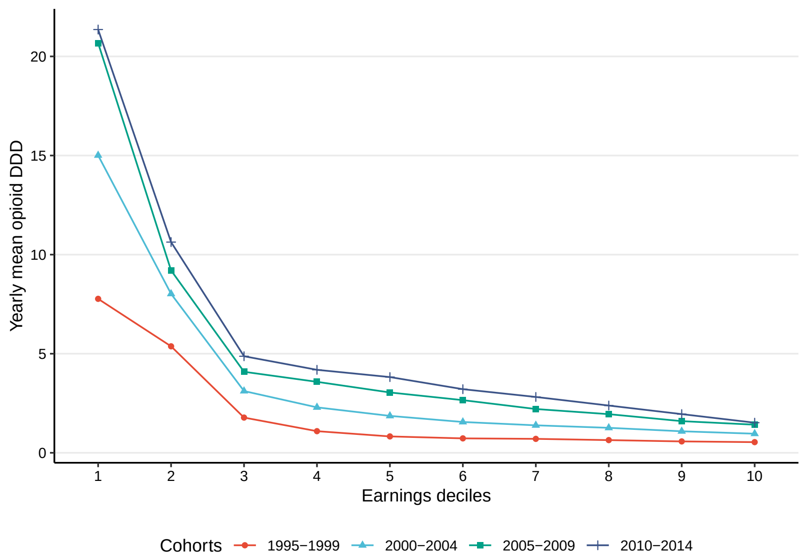
**Notes:** Values are based on Equation 2. Column (1) and (2) show the effects of layoffs on an indicator for having any opioid prescription for those in municipalities in the bottom and top quartiles, respectively, of mean opioids prescribed. Columns (3) and (4) show the effects on opioid DDDs. Based on Danish register data.

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




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

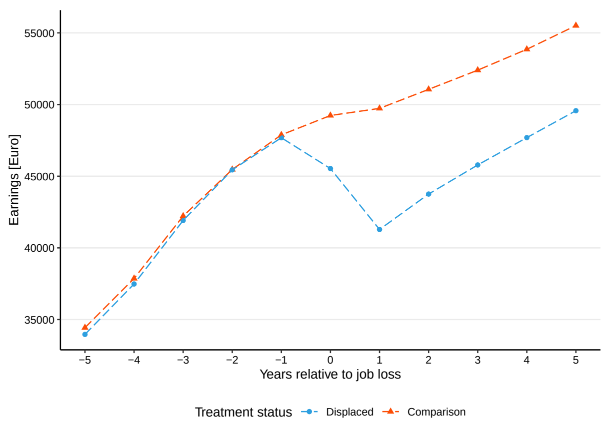
Figure 2: Income and opioid use



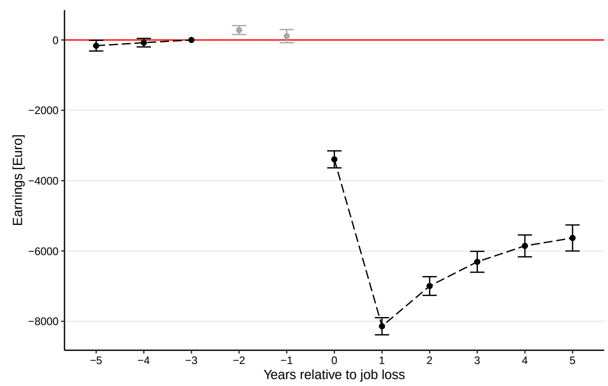

**Notes:** The figure shows prescription opioid use across income and time. The x-axis groups all workers in Denmark (aged 20-60) by their earnings decile in a given year. The y-axis measures average defined daily doses of opioids prescribed in a given year for that cohort.

Figure 3: Labor market trajectories of displaced workers

(a) Mean yearly earnings

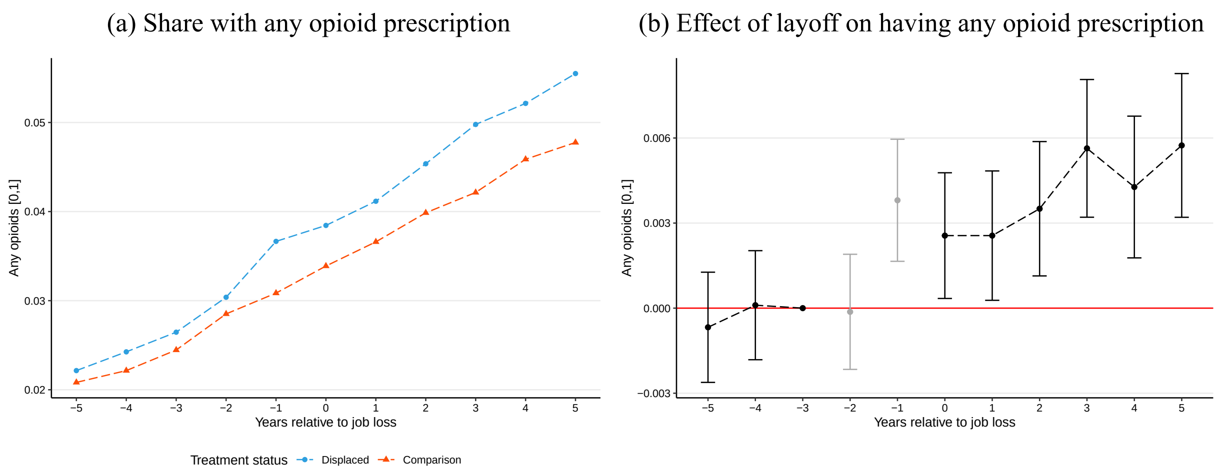



(b) Effect of layoff on earnings




**Notes:** The left panel shows the mean yearly earnings for displaced workers and matched comparison workers as defined in Section 4. The right panel shows estimated effects using the event study specification from Equation 1. Based on Danish register data.

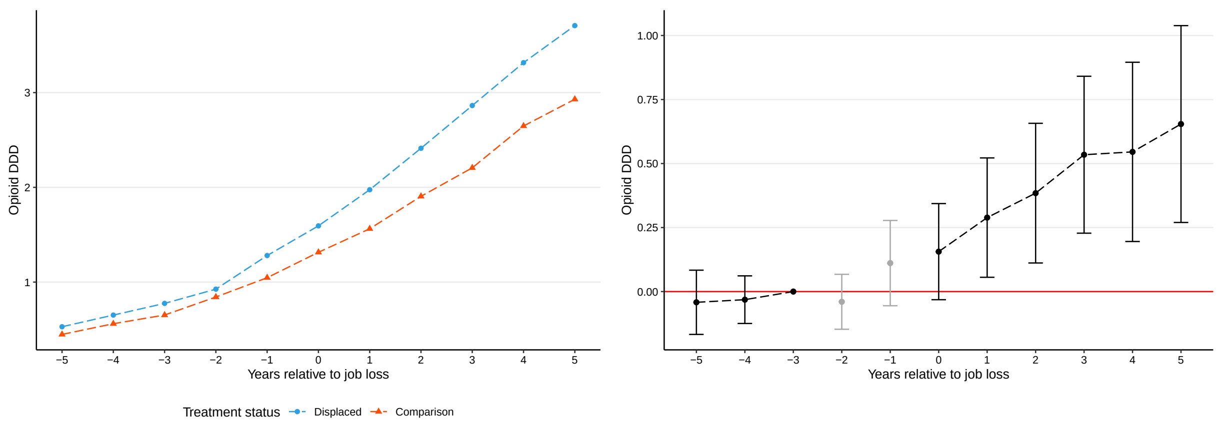
Figure 4: Any opioid prescriptions




**Notes:** The left panel shows the fraction of any opioid prescription among displaced workers and matched comparison workers as defined in Section 4. The right panel shows estimated effects using the event study specification from Equation 1. Based on Danish register data.

Figure 5: Prescription opioid DDDs used

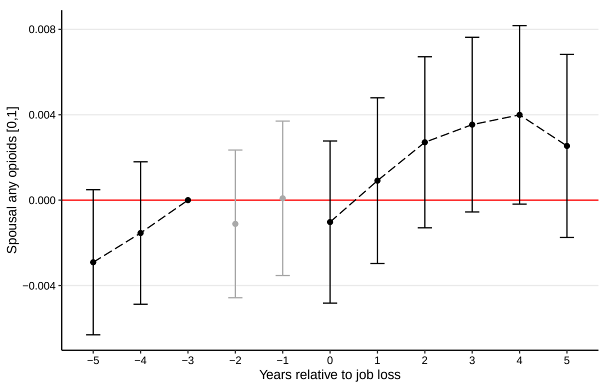
(a) Mean DDD prescription opioid use (b) Effect of layoff on DDDs of opioids prescribed



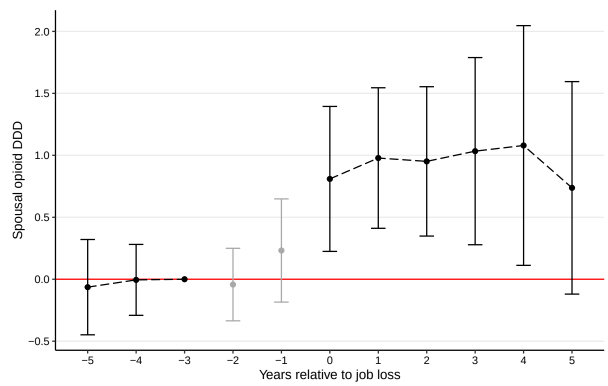

**Notes:** The left panel shows the mean DDD prescription opioid use of spouses of displaced workers and spouses of matched comparison workers as defined in Section 4. The right panel shows estimated effects using the event study specification from Equation 1. Based on Danish register data.

Figure 6: Spillover effect of layoff on spousal opioid use

(a) Spouse having any opioid prescriptions

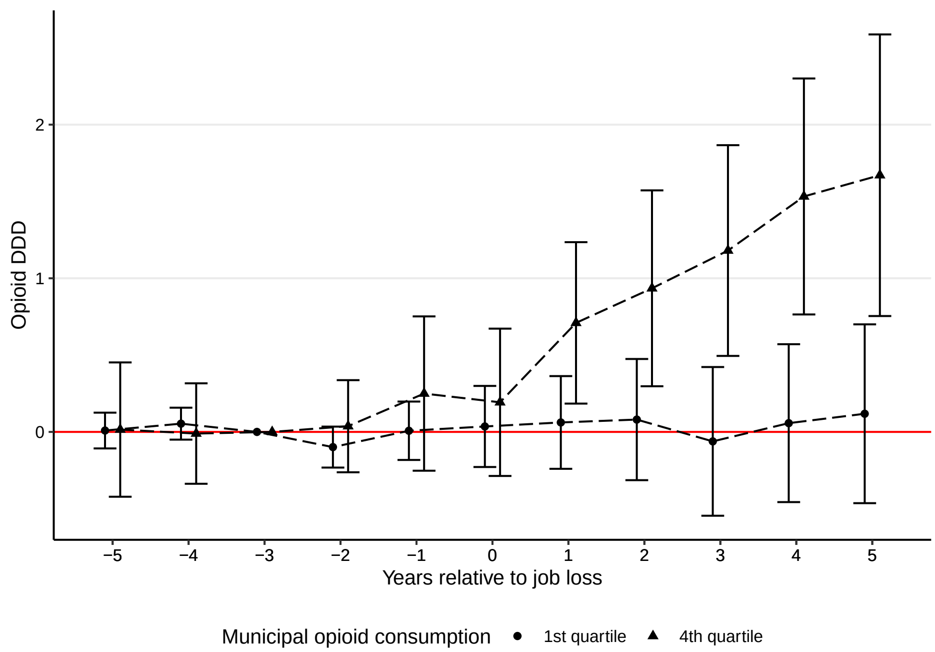



(b) Spousal opioid consumption (DDDs)




**Notes:** The figure plots the spillover effect of a layoff on spousal prescription opioid use. The left panel plots the effect effect on having any opioid prescription in a given year using the event study specification from Equation 1. The right panel plots the effect on Defined Daily Doses of opioids. Based on Danish register data.

Figure 7: Effects on DDDs by DDDs in the municipality




**Notes:** The figure shows estimated effects using the event study specification from Equation 1. The circles show effects for those in a municipality in the lowest quartile of usage, by mean opioids prescribed; the triangles show effects for those in a municipality in the highest quartile. Based on Danish register data.

# ONLINE APPENDIX

The Effects of Layoffs on Opioid Use and Abuse

Marius Opstrup Morthorst, David J. Price, and Peter Rønø Thingholm

January 23, 2025

# A Appendix Tables and Figures

## A.1 Appendix Tables

Table A.1: Share of sample displaced in each year

| Years | Percent layoffs | Percent of laid off workers |
|---|---|---|
| 2000 | 7.1 | 7.3 |
| 2001 | 8.5 | 10.6 |
| 2002 | 9.1 | 10 |
| 2003 | 8.7 | 8.9 |
| 2004 | 6.9 | 7.4 |
| 2005 | 7 | 7.7 |
| 2006 | 5.7 | 6.3 |
| 2007 | 6.8 | 5.8 |
| 2008 | 12.2 | 9.7 |
| 2009 | 13.3 | 10.6 |
| 2010 | 8.1 | 7.6 |
| 2011 | 6.5 | 8.1 |

**Notes:** The table shows the share of layoffs and displaced workers in our sample who are displaced in each year. Based on Danish register data.

Table A.2: Share of sample displaced by type of municipality

| Municipality | Percent layoffs | Percent of laid off workers |
|---|---|---|
| Metropolitan | 28 | 26.3 |
| Urban | 37.2 | 35.7 |
| Rural | 34.8 | 38 |

**Notes:** The table shows the share of layoffs and displaced workers in our sample who are displaced in different types of municipalities. Municipal groupings are based on Statistics Denmark (2018); 'Metropolitan' refers to those with classification 1 and 2, 'urban' to those with classification 3, and 'rural' to those with classification 4 and 5. Based on Danish register data.

Table A.3: Share of sample displaced by industry

| **Industry** | **Percent layoffs** | **Percent of laid off workers** |
|---|---|---|
| Agriculture | 2 | 1.1 |
| Manufacturing | 30.8 | 45.9 |
| Construction | 4.3 | 2.8 |
| Wholesale and retail | 17.1 | 11.4 |
| Information and communication | 15.7 | 11.9 |
| Finance | 7.5 | 9.4 |
| Real estate | 12 | 9.9 |
| Professional services | 9.6 | 7.3 |
| Other | 1 | 0.4 |

**Notes:** The table shows the share of layoffs and displaced workers in our sample who are displaced in different industries. Based on Danish register data.

Table A.4: Descriptive statistics of establishments

| | Comparison workers | Displaced workers | Differences | SDM |
|---|---|---|---|---|
| *Firm characteristics* | | | | |
| Industry: Manufacturing [0,1] | 0.406 | 0.335 | -0.072*** | -0.105 |
| | (0.491) | (0.472) | (0.007) | |
| Industry: Services [0,1] | 0.347 | 0.353 | 0.006 | 0.009 |
| | (0.476) | (0.478) | (0.007) | |
| Industry: Other [0,1] | 0.247 | 0.312 | 0.065*** | 0.103 |
| | (0.431) | (0.463) | (0.007) | |
| Establishment size | 159.717 | 129.301 | -30.416*** | -0.116 |
| | (201.099) | (169.515) | (3.022) | |
| Number of establishments | 5,430 | 3,014 | | |

**Notes:** The table presents descriptive statistics for the mass-layoff establishments and the establishments of comparison workers. Each cell in Columns 1–2 the mean of the corresponding variable in the row with standard deviation in parentheses. Column 3 present differences in means and corresponding p-values. Column 4 present the standardized difference in means (SDM) (or the standardized bias); i.e., the difference in means as a share of the square root of the average sample variances of the lay-off and comparison establishments.

Table A.5: Baseline results of event study regressions

| | Earnings [Euro] (1) | Income [Euro] (2) | Earnings>0 [0,1] (3) | Any opioid prescription [0,1] (4) | Opioid DDDs (5) | Opioid OMEQ (6) |
|---|---|---|---|---|---|---|
| Displaced x Time = -5 | -162.2475** | -87.9231 | -0.0030*** | -0.0007 | -0.0419 | -0.0557 |
| | (77.4005) | (158.4217) | (0.0011) | (0.0010) | (0.0640) | (0.0536) |
| Displaced x Time = -4 | -77.0096 | 49.7863 | -0.0025*** | 0.0001 | -0.0316 | -0.0475 |
| | (61.5233) | (160.3598) | (0.0009) | (0.0010) | (0.0475) | (0.0430) |
| Displaced x Time = -2 | 281.7442*** | 399.2114** | 0.0001 | -0.0001 | -0.0399 | 0.0167 |
| | (64.4246) | (192.8440) | (0.0002) | (0.0010) | (0.0547) | (0.0356) |
| Displaced x Time = -1 | 109.7326 | 365.7444* | 0.0000 | 0.0038*** | 0.1112 | 0.1283** |
| | (95.4987) | (189.1406) | (0.0002) | (0.0011) | (0.0850) | (0.0651) |
| Displaced x Time = 0 | -3,394.5693*** | -282.1343 | -0.0159*** | 0.0026** | 0.1559 | 0.1419** |
| | (123.2743) | (524.1123) | (0.0006) | (0.0011) | (0.0957) | (0.0638) |
| Displaced x Time = 1 | -8,139.2585*** | -5,495.3273*** | -0.0503*** | 0.0026** | 0.2889** | 0.2792*** |
| | (124.2613) | (213.0134) | (0.0011) | (0.0012) | (0.1189) | (0.0785) |
| Displaced x Time = 2 | -6,995.8425*** | -5,191.0274*** | -0.0402*** | 0.0035*** | 0.3844*** | 0.3747*** |
| | (135.8122) | (249.0300) | (0.0012) | (0.0012) | (0.1391) | (0.1002) |
| Displaced x Time = 3 | -6,307.1293*** | -5,096.9058*** | -0.0337*** | 0.0056*** | 0.5344*** | 0.3903*** |
| | (151.3440) | (240.4412) | (0.0012) | (0.0012) | (0.1563) | (0.1123) |
| Displaced x Time = 4 | -5,854.5224*** | -4,543.3311*** | -0.0316*** | 0.0043*** | 0.5455*** | 0.3825*** |
| | (158.6400) | (448.8772) | (0.0012) | (0.0013) | (0.1786) | (0.1299) |
| Displaced x Time = 5 | -5,629.7187*** | -4,727.3471*** | -0.0299*** | 0.0057*** | 0.6542*** | 0.5024*** |
| | (188.9432) | (310.9672) | (0.0013) | (0.0013) | (0.1961) | (0.1489) |
| Mean in Time = -3 | [41912.746] | [44770.755] | [1] | [0.026] | [0.775] | [0.419] |
| Observations | 1,694,418 | 1,694,418 | 1,694,418 | 1,694,418 | 1,694,418 | 1,694,418 |

**Notes:** Significance codes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Values are based on Equation 2. Standard errors in parentheses clustered at the individual, and establishment level. Based on Danish register data.

Table A.6: Robustness across matching strategies

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| *Panel A: Earnings [Euro]* | | | | | | | |
| Displaced x Post | -6,505.542*** | -6,377.545*** | -6,565.078*** | -6,783.830*** | -6,477.750*** | -6,500.906*** | -6,448.568*** |
| | (130.808) | (130.997) | (131.257) | (131.983) | (132.241) | (130.560) | (129.513) |
| Mean in Time = -3 | [41912.746] | [41912.746] | [41913.822] | [41965.982] | [41934.527] | [41912.746] | [41913.822] |
| *Panel B: Any opioids [0,1]* | | | | | | | |
| Displaced x Post | 0.005*** | 0.005*** | 0.004*** | 0.004*** | 0.004*** | 0.004*** | 0.004*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Mean in Time = -3 | [0.026] | [0.026] | [0.026] | [0.026] | [0.026] | [0.026] | [0.026] |
| *Panel C: Opioid DDDs* | | | | | | | |
| Displaced x Post | 0.506*** | 0.630*** | 0.643*** | 0.387*** | 0.608*** | 0.489*** | 0.503*** |
| | (0.139) | (0.138) | (0.137) | (0.148) | (0.133) | (0.140) | (0.140) |
| Mean in Time = -3 | [0.775] | [0.775] | [0.775] | [0.775] | [0.774] | [0.775] | [0.775] |
| Observations | 1,232,304 | 1,232,304 | 1,232,256 | 1,227,776 | 1,230,512 | 1,232,304 | 1,232,256 |
| *Exact matching variables* | | | | | | | |
| Year of layoff | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Gender | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Industry | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Married | | ✓ | ✓ | | | | |
| Have children | | | ✓ | | | | |
| Geographical unemployment rank | | | | ✓ | | | |
| Geographical opioid DDD rank | | | | | ✓ | | |
| *P-score matching variables* | | | | | | | |
| Prior earnings | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Age | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Tenure | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Employer size | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Opioid DDD t=-3 | | | | | | | ✓ |
| Opioid DDD t=-5 | | | | | | | ✓ |
| Change in opioid | | | | | | ✓ | |

**Notes:** Values are based on Equation 1. Panel A presents estimates of the effect of a layoff on earnings; Panel B the effect on having any opioid prescriptions; and Panel C the effect on Defined Daily Doses of opioids.

Table A.7: Robustness across layoff and employee definitions

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| *Panel A: Earnings [Euro]* | | | | | | | | |
| Displaced x Post | -7,788.561*** | -6,528.510*** | -6,688.275*** | -6,155.403*** | -5,869.239*** | -5,802.981*** | -6,550.861*** | -5,881.339*** |
| | (124.329) | (131.154) | (148.685) | (172.286) | (188.871) | (224.990) | (581.430) | (107.079) |
| Mean in Time = -3 | [42559.753] | [41913.976] | [42190.192] | [42186.999] | [42172.761] | [42119.701] | [46619.886] | [36961.205] |
| *Panel B: Any opioids [0,1]* | | | | | | | | |
| Displaced x Post | 0.003*** | 0.004*** | 0.003*** | 0.004*** | 0.004*** | 0.004*** | 0.002** | 0.004*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Mean in Time = -3 | [0.031] | [0.026] | [0.027] | [0.026] | [0.025] | [0.025] | [0.028] | [0.027] |
| *Panel C: Opioid DDDs* | | | | | | | | |
| Displaced x Post | 0.562*** | 0.501*** | 0.484*** | 0.488*** | 0.432** | 0.309 | 0.418** | 0.399*** |
| | (0.149) | (0.139) | (0.144) | (0.169) | (0.186) | (0.214) | (0.193) | (0.126) |
| Mean in Time = -3 | [1.073] | [0.775] | [0.79] | [0.79] | [0.775] | [0.778] | [0.858] | [0.783] |
| Observations | 1,442,944 | 1,232,304 | 1,155,840 | 783.568 | 575.712 | 419,136 | 749,488 | 1,733,600 |
| *Establishment restrictions* | | | | | | | | |
| Establishment size | 30 | 70 | 50 | 50 | 50 | 50 | 50 | 50 |
| Employee reduction | >30% | >30% | >30% | >50% | >70% | >90% | >30% | >30% |
| Absorption | <20% | <20% | <20% | <20% | <20% | <20% | <20% | <20% |
| *Employee restrictions* | | | | | | | | |
| Minimum age | 20 | 20 | 20 | 20 | 20 | 20 | 20 | 20 |
| Maximum age | 50 | 50 | 60 | 50 | 50 | 50 | 50 | 50 |
| Minimum tenure | 3 years | 3 years | 3 years | 3 years | 3 years | 3 years | 5 years | 2 years |

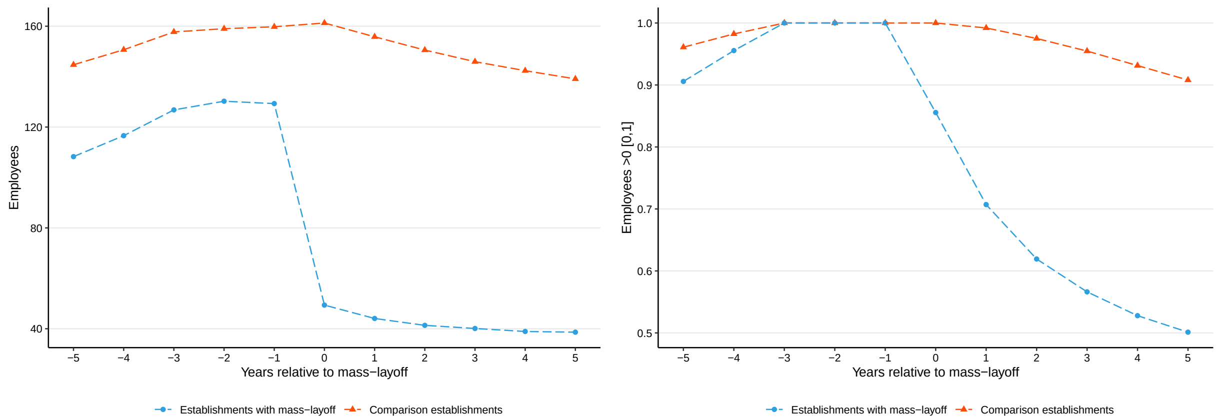
**Notes:** Values are based on Equation 1. Panel A presents estimates of the effect of a layoff on earnings; Panel B the effect on having any opioid prescriptions; and Panel C the effect on Defined Daily Doses of opioids.

## A.2 Appendix Figures

Figure A.1: Establishment trajectories

(a) Number of employees

(b) Probability of having any employees

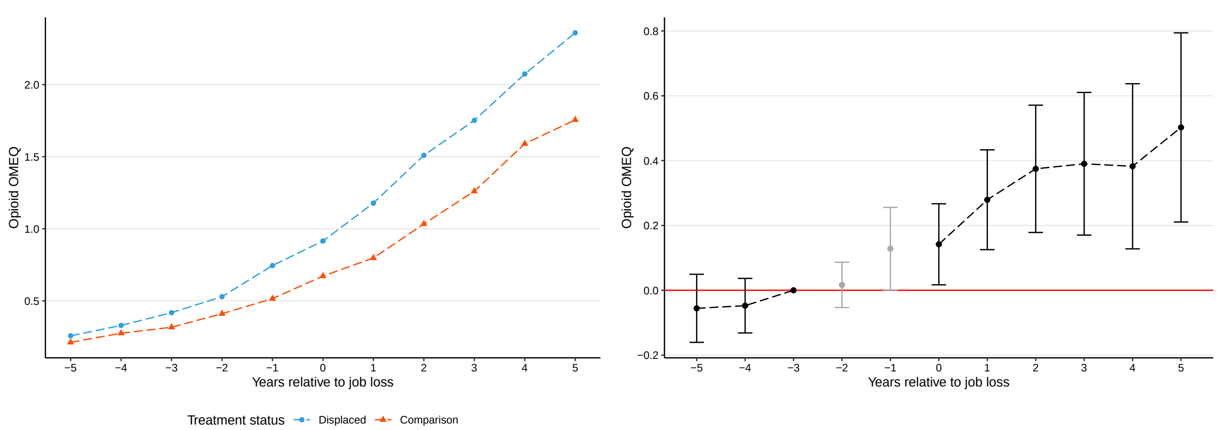



**Notes:** The left panel shows the mean number of employees at an across mass layoff and comparison establishments as defined in Section 4. The right panel shows probability of an establishment having any employees across mass layoff and comparison establishments. Based on Danish register data.

Figure A.2: Prescription opioid OMEQ used

(a) Mean OMEQ prescription opioid use

(b) Effect of layoff on OMEQ opioids prescribed




**Notes:** The left panel shows the mean OMEQ prescription opioid use of spouses of displaced workers and spouses of matched comparison workers as defined in Section 4. The right panel shows estimated effects using the event study specification from Equation 1. Based on Danish register data.