Effects of Job Displacement on Prescription Opioid Demand: Evidence from the Medical Expenditure Panel Survey*

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PRELIMINARY AND IN PROGRESS

Abstract

I study whether labor market hardship makes prime-age workers in the United States more likely to use opioids. I find that job displacement is not associated with changes in workers' likelihood of opioid use, except for at high thresholds: displacement makes workers in my sample two tenths of a percentage point less likely to receive 12 or more opioid prescriptions, or four tenths of a percentage point less likely to receive a prescription for 120 or more morphine milligrams equivalent per day. To the extent that poor local labor market conditions cause increases in opioid use, my results suggest that these increases are mediated by increased opioid supply associated with economic hardship rather than increased opioid demand among affected workers.

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JEL: H51, I12, I18

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1 Introduction

In recent years, scholars, pundits, and policymakers have pronounced in unison that opioid abuse in the United States has reached crisis proportions. Estimates from federal government agencies suggest that 68% of the 70,200 drug overdose deaths in 2017 involved opioids, amounting to over 130 opioid overdose deaths per day (Centers for Disease Control and Prevention, 2018*a*; National Institute on Drug Abuse, 2019). Poorer, whiter regions of the country are among the hardest-hit by upticks in opioid deaths; for instance, opioid overdoses increased 70% from July 2016 to September 2017 in the Midwest (CDC, 2018*a*).

The relationship between local economic distress and drug deaths has led some scholars to propose that poor local labor market conditions may increase demand for opioids. Case and Deaton (2017; 2020) in particular argue that poor local labor market conditions have been an important driver of increases in midlife mortality for non-Hispanic whites in the United States since the late 1990s. This suggestion has spurred a flurry of research investigating the relationship between labor market conditions and opioid abuse (Aliprantis and Schweitzer, 2018; Charles, Hurst, and Schwartz, 2018; Currie, Jin, and Schnell, 2018; Harris et al., 2018; Hollingsworth, Ruhm, and Simon, 2017; Krueger, 2017; Laird and Nielsen, 2017; Metcalf and Wang, 2019; Pierce and Schott, 2020; Ruhm, 2019; Venkataramani et al., 2019). There is no consensus among the subset of studies concerned with whether labor market-induced despair affects opioid use.

A critical feature of Case and Deaton's (2017; 2020) argument is that labor market hardship causes individuals to demand more opioids. Because Case and Deaton's narrative hinges on workers' demand for opioids, existing studies' reliance on county-level data is an insuperable weakness. At the county level, it is impossible to distinguish person-specific responses to poor labor market conditions from place-specific responses; one only observes an equilibrium. Ultimately, microdata on economic dislocation and opioid use are likely to provide the clearest insights on demand effects of the former on the latter, but thus far no study has used such data in the United States context.

The primary project of this study is to address this gap using Medical Expenditure Panel Survey (MEPS) data, in which I observe both job displacement and opioid use over time at the individual level. I provide the first demand-side estimates of the effect of labor market hardship on opioid use in the United States. I focus on prime-age individuals who lose their job because their place of employment dissolves or is sold or their term of employment ends (e.g. their contract expires). I dub these individuals "non-layoff displaced." My sample consists of individuals who were employed during the first period of their survey participation and who did not use opioids during this period. I estimate a linear probability model in which my independent variable is an indicator for experiencing non-layoff job displacement and my dependent variables are indicators for exceeding various opioid use thresholds in terms of number of opioid prescriptions and morphine milligram equivalent (MME) dosage per day. The detailed health information collected by the MEPS allows me to condition richly on health conditions correlated with opioid use as well as demographic characteristics and pre-displacement industry and occupation. Simultaneity and omitted variables bias under my main specification, if present, are likely positive, meaning that the associations I measure are likely upper bounds on causal effects.

I find little relationship between displacement and opioid abuse conditional on industry, occupation, health status, and demographic characteristics. For outcomes representing lower thresholds of opioid use, I am generally not able to reject null effects of displacement on opioid use. When considering higher levels of opioid use, I find statistically significant negative results: experiencing non-layoff displacement makes workers in my sample two tenths of a percentage point less likely to receive 12 or more opioid prescriptions, or four tenths of a percentage point less likely to receive a prescription for 120 or more MME per day. I do not find evidence that non-layoff displacement is associated with increases in likelihood of opioid use among non-Hispanic whites or

individuals working in blue-collar occupations during their first reference period of survey participation. This casts some doubt on the idea that labor market dislocations are particularly important drivers of increasing mortality among blue-collar non-Hispanic whites.

Under my sample restrictions, the effects I measure approximate the effect of displacement on non-abusers' propensity to begin using opioids. My sample restrictions allow me to credibly estimate causal effects at the expense of shedding light on the effects of labor market dislocations among typical opioid abusers, who are less likely to be employed and are not necessarily opioid naïves (Krueger, 2017). Nevertheless, my study is informative because workers' transition from non-abuse to opioid abuse is not well-understood. Moreover, my results generally hold when I relax sample restrictions.

Displacement-related income reductions appear to be responsible for reductions in probability of high-threshold opioid use associated with displacement.[1] Among individuals whose displacement is less likely to have caused financial hardship – individuals who were employed for their entire survey participation, individuals whose labor income did not make up the majority of their dwelling unit's income in their first year of participation, and individuals whose dwelling units earned business or trust income in the first year of participation – displacement is not associated with changes in likelihood of high-threshold opioid use. On the other hand, displacement is associated with reductions in the probability of high-threshold opioid use among individuals who spent at least one reference period without working, individuals whose labor income made up the majority of their dwelling unit's income, and individuals whose dwelling unit did not report business or trust income.

My findings suggest that increased demand for opioids associated with short-term labor market dislocation among prime-age workers is unlikely to be a driver of increasing opioid deaths in recent years. To the extent that labor market dislocations are to blame for increasing opioid abuse, these effects may be driven by increases in opioid supply associated with labor market dislocations. This explanation is consistent with evidence from Currie and Schnell (2018) and Finkelstein, Gentzkow, and Williams (2018) which establishes supply as a strong determinant of opioid abuse.

2 Background

Recent studies of opioid abuse in economics can be traced in large part to Case and Deaton's (2015) finding that midlife mortality among non-Hispanic whites has increased in the United States over the past two decades, which they attribute to "poisonings," a term they use to characterize drug or alcohol overdose deaths. In subsequent work, Case and Deaton (2017; 2020) suggest that long-term deterioration of middle-aged non-Hispanic whites' labor market outlook has driven rising poisoning deaths. They specifically highlight the growing scarcity of labor market opportunities for less-educated and blue-collar workers from generation to generation. Following their lead, several studies have examined whether labor market dislocations induce opioid use (Charles, Hurst, and Schwartz, 2018; Currie, Jin, and Schnell, 2018; Hollingsworth, Ruhm, and Simon, 2017; Metcalf and Wang, 2019; Pierce and Schott (2020); Roulet, 2017; Ruhm, 2019; Venkataramani et al., 2019), whether opioid use induces labor market inactivity (Aliprantis and Schweitzer, 2018; Currie, Jin, and Schnell, 2018; Harris et al., 2018; Krueger, 2017; Torbin and Nielsen, 2017), and the social determinants of opioid use in general (e.g. Finkelstein, Gentzkow, and Williams, 2018).

Of these studies, only Roulet (2017) measures the effects of job displacement on individual opioid demand. She uses Danish administrative employment and healthcare utilization data to study whether job dis-

[1] Case and Deaton (2020) mention the possibility that job loss can reduce mortality-inducing behavior (for instance, driving and drinking) by reducing income.

placement induces greater prescription opioid use. She finds no effect of job displacement on opioid use, though there is reason to believe that the United States context would differ from the Danish context. First, Roulet argues that in Denmark, unemployment is not so despair-inducing or stigmatized as in the United States, as evidenced by generous unemployment insurance policies. Second, Roulet finds that generous unemployment insurance policies prevent large reductions in healthcare spending associated with job displacement. Thus the two most obvious determinants of post-displacement prescription opioid use or abuse – despair and financial hardship – are likely much weaker in Denmark than in the United States. Therefore, we would not expect *a priori* that Roulet's (2017) finding would generalize to the United States.

The studies which most resemble my own using U.S. data are Charles, Hurst and Schwartz (2018), Currie, Jin, and Schnell (2018), Metcalf and Wang (2019), Pierce and Schott (2020), Ruhm (2019), and Venkataramani et al. (2019), all of which use county-level data. These studies paint conflicting pictures. Ruhm (2019) finds that worsening economic conditions over the medium term may increase the drug death rate, though economic decline accounts for no more than one tenth of the change in the drug death rate and, per Ruhm, "a small amount of remaining omitted variables bias would be sufficient to completely eliminate the contributions of economic factors." Venkataramani et al. (2019) and Pierce and Schott (2020) find that auto plant closures and trade liberalization, respectively, increase drug deaths. Metcalf and Wang (2019) find that decreases in the coal employment share actually decrease the prevalence of opioid overdose deaths. Currie, Jin, and Schnell's (2018) results suggest no relationship between employment-to-population ratios and opioid prescribing rates. Charles, Hurst, and Schwartz (2018), on the other hand, shows strong positive relationships between declining manufacturing share of employment and opioid use metrics.[2]

Reconciling the results of these papers is beyond the scope of my study. More pertinent is the inability of these papers to identify and measure separate supply and demand effects of economic shocks on opioid use. This distinction is most clearly drawn in Finkelstein, Gentzkow, and Williams (2018), who explain that "person-specific factors generally correspond to what we would think of demand and place-specific factors to what we would think of as supply." Since all existing analysis is conducted at the county level, the existing literature is unable to determine whether any effects of economic conditions on prescription opioid use are related to person-specific or place-specific responses to labor market shocks.

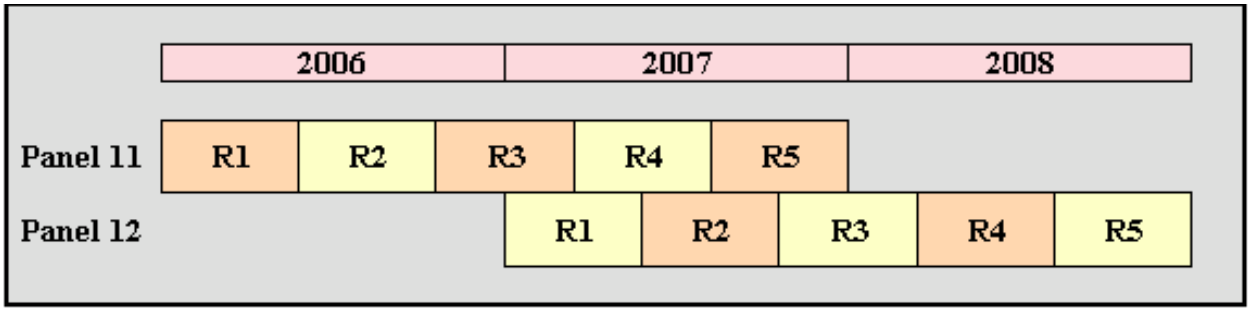
3 Data and Descriptive Results

3.1 Data source

My project uses data collected through the MEPS, a nationally representative survey covering the United States civilian non-institutional population. The MEPS interviews survey participants five times over two years. Each respondent's participation is partitioned into five reference periods of roughly equal length, each corresponding to a round of interviews. A new panel of survey participants is added each year so that, in any given year, two different panels participate in the survey. Figure 1 illustrates the mechanics of this overlapping panel design with panel number 11, whose participants enter the survey at the beginning of 2006 and exit at the end of 2007, and panel 12, whose participants enter the survey at the beginning of 2007 and leave the survey at the

[2]Of these studies, all but Currie, Jin, and Schnell use drug deaths outcomes as opposed to opioid prescribing outcomes. The extent to which prescription opioid abuse leads to illicit opioid abuse is an open research question, but correlation between the two is strong. For instance, estimates from the National Institute on Drug Abuse indicate that "nearly 80% of Americans using heroin (including those in treatment) reported misusing prescription opioids prior to using heroin," which suggests that prescription opioid abuse may act as a gateway to more dangerous substance abuse. One clear advantage of using drug deaths outcomes is that they are more likely to encompass illicit as well as medically sanctioned opioid use. My inability to observe use of illicit opioids is an important limitation of my study.

Figure 1: Diagram of the MEPS' overlapping panel design



Notes: Diagram sourced from Chowdhury (2011).

end of 2008. In total, the MEPS data from 1996 to 2017 covers 21 panels, numbered 1-21.

My analysis primarily relies on two types of data files – person-level longitudinal files and prescription-level prescribed medicines files – which I link using person-level identifiers and panel numbers. Information in longitudinal files is primarily obtained through interviews. The MEPS obtains prescription information from interviews with MEPS participants and obtains permission from participants to follow up with pharmacies they list as having provided medicines to them. The prescribed medicines files contain records for all prescriptions received by MEPS participants in an outpatient setting in a given year. Prescriptions received in a hospital, clinic, or physician's office are all excluded from prescribed medicines files (Stagnitti, 2015).[3]

3.2 Sample selection

I impose a handful of sample restrictions to minimize threats to identification under my research design. First, I restrict my analysis sample to include only individuals for whom I measure a full battery of health conditions, which excludes MEPS participants who were members of panels 1, 2, or 3. Second, to combat the threat of selection into job displacement on unobserved dimensions correlated with propensity to use opioids, I restrict my sample to prime-age individuals, who are less likely to be marginal workers than their younger and older counterparts. For the same reason, I exclude individuals who received opioid prescriptions during the first period in the MEPS. Finally, I restrict my analysis to individuals who report being employed in the first round of interviews. I impose this last restriction primarily because my definition of job displacement requires that a displaced individual be employed in the pre-displacement period. Secondarily, restricting my analysis sample to include individuals who do not use prescription opioids during a reference period in which they work helps screen individuals against negative selection into displacement on unobservable characteristics associated with future opioid use.

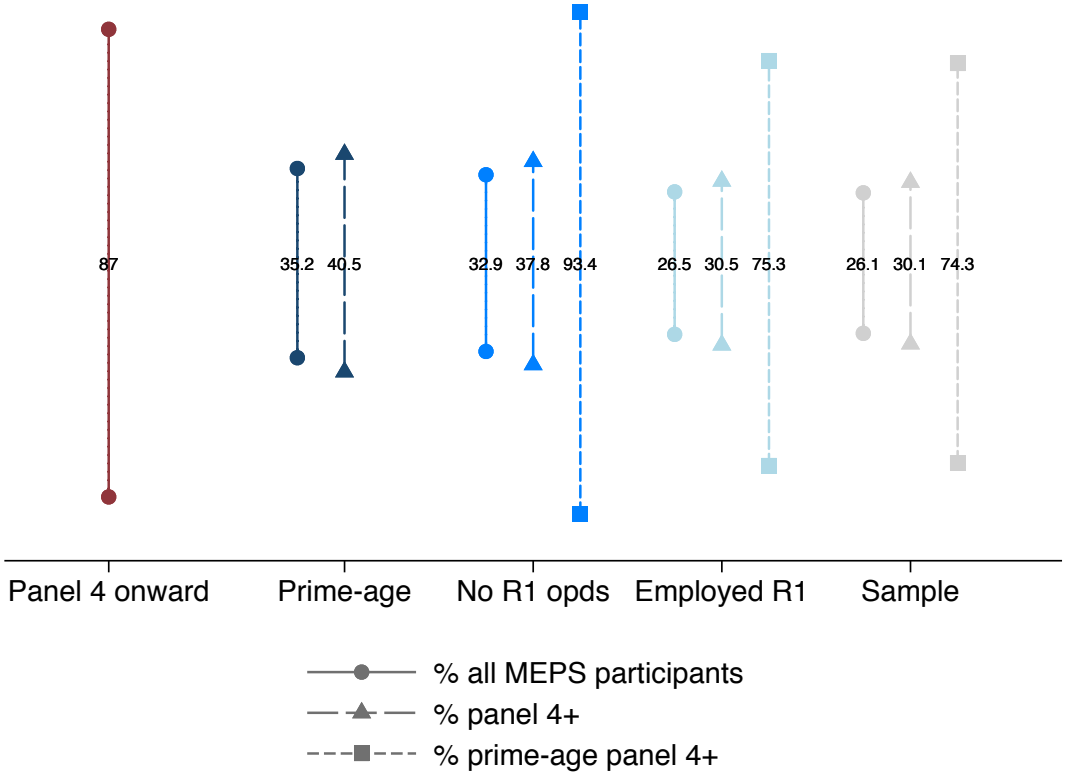
Figure 2 shows how successive restrictions reduce the size of my analysis sample relative to (1) the full set of MEPS participants from 1996-2017 (2) the set of MEPS participants in rounds 4-onward and (3) the set of prime-age MEPS participants from panel 4-onward. Restricting my sample to MEPS participants in panels 4 and beyond shrinks my sample to 87% of all participants. Further restricting to prime-age participants shrinks this proportion to 35.2% of all participants, or 40.5% of all participants in panels 4 or greater. Excluding individuals who received opioid prescriptions in round one further shrinks my subsample to 32.9% of all MEPS

[3] It is not clear what proportion of all opioid prescriptions are received in an outpatient setting as opposed to a hospital, clinic, or physician's office. Regardless, prescription opioids intended for outpatient use are likely the most valuable subject of study, since they are more likely to be abused than opioids prescribed for use under physician supervision.

participants, 37.8% of participants in panel 4 or greater, or 93.4% of prime-age MEPS participants in panel 4 or greater. Applying my full set of restrictions yields an analysis sample of 26.1% of all MEPS participants, 30.1% of panel 4-onwards participants, or 74.3% of prime-age participants from panel 4 onwards.

Attrition is low among my analysis sample: only 0.6% of individuals therein become institutionalized or unreachable by the end of their survey participation.[4] I present demographic characteristics of MEPS participants pooling all years of data in section 1 of table 1. Demographic characteristics of individuals in my analysis sample resemble all prime-age individuals in the MEPS, though analysis sample individuals are slightly more likely to be male and hold at least a college degree.

Figure 2: Size of analysis subsample after successive sample restrictions



Notes: This diagram shows the proportion of individuals in the MEPS made up by different subsamples, where subsamples apply successively more sample restrictions. Solid lines show the proportion of all MEPS participants in panels 1-21 in each subsample. Dashed lines with triangle markers show the proportion of individuals in panels 4-onwards represented by each subsample. Dashed lines with square markers show the proportion of prime-age individuals in panels 4-onwards represented by each subsample.

To be clear, the population my analysis sample represents – individuals with nontrivial attachment to the labor market who do not start their survey participation using opioids – is not representative of opioid users in the United States. Previous studies such as Krueger (2017) have established that the typical individual struggling with opioid addiction has a more tenuous relationship with the labor market and may experience periods of opioid addiction which are interspersed among or concurrent with periods of employment. I also observe these patterns: as appendix figures A.1 and A.2 show, I detect much more prescription opioid receipt among individuals who never worked during their survey participation than individuals who did. Among participants

[4] In computations not shown here, I verify that dropping individuals in my analysis sample who attrited does not change my results.

whose primary year of MEPS participation was 1996, individuals who never worked were nearly eight times as likely as individuals who always worked to receive six or more opioid prescriptions (8.2% versus 1.1%) and ten times as likely to receive twelve or more opioid prescriptions (4% versus 0.4%). Similarly, roughly double the share of never-working individuals receive high-MME per day opioid prescriptions relative to always-working individuals.

Two features of my study make it informative despite my sample's restrictiveness. First, all opioid abusers transitioned from non-abuse to abuse at some point. If labor market hardship speeds along this transition, it is informative to start with non-abusers and study whether displacement has an effect on the extensive margin. Second, appendix tables A.1 and A.2 show that displacement reduces likelihood of opioid abuse even when I relax sample restrictions and include (1) all MEPS participants or (2) all prime-age MEPS participants from panel 4 onwards. I do not regard these estimates as cleanly approximating causal effects in the same way I do my main results, but they do lend support to the idea that my findings are not driven by excessively narrow sample selection. Nevertheless, I concede that my analysis primarily sheds light on the transition from non-use to opioid abuse. I cannot rigorously study intensive-margin feedback loops between labor market hardship and opioid use among individuals already abusing opioids.

3.3 Measuring job displacement

While the MEPS collects round-by-round information on participants' labor market activities, it does not record whether they experience job displacement. As a starting point, I follow Schaller and Stevens (2015) in classifying an individual as displaced in a round if they report during that round that they switched their current main job because (1) they were laid off (2) the business where they worked was dissolved or sold or (3) their job ended.[5] Individuals can "switch" current main jobs into unemployment; they need not work in the post-displacement period. I focus on displacement due to these latter two causes because displacement for these reasons (henceforth "non-layoff displacement") is less likely than layoffs to be correlated with workers' productivity and, relatedly, their propensity to use opioids. I defend this assertion in greater detail in subsection 4.2.

Section 1 of table 2 shows the prevalence of job displacement, both overall and disaggregated by displacement type. Approximately 9% of my analysis sample (or 7,100 individuals) experience any displacement; layoffs account for slightly less than half of all job displacements and non-layoff displacement accounts for slightly more than half. A small portion of my analysis sample, roughly three tenths of a percent, are both laid off and non-layoff displaced.

3.4 Measuring prescription opioid use

As I discuss in subsection 3.1, the MEPS obtains prescription information from in-person interviews with MEPS participants and from pharmacies to which participants refer it. Each prescription record includes a National Drug Code (NDC), a drug name, and a generic drug name. I observe which rounds participants receive prescriptions and drugs in, but not exact dates. I classify a prescription as being an opioid prescription according to criteria I outline in detail in appendix B, following Soni (2018), Moriya and Miller (2018*a*), Moriya and Miller (2018*b*), Stagnitti (2017), Groenewald et al. (2016), Zhan et al. (2001), and Zhou, Florence, and Dowell (2016). My methods essentially amount to a prescription meeting two of the three following criteria:

[5] Job ending is distinct from voluntarily leaving a job. An individual's job might end, for instance, if they were employed under a two-year contract which their employer allowed to expire.

Table 1: Characteristics of MEPS participants

	Proportions of MEPS participants (%)		
	All prime-age in MEPS	Analysis sample	MME analysis sample (2010+)
Section 1. Demographic characteristics			
U.S. Census Region			
Northeast	18.4	18.4	17.5
Midwest	22.0	22.5	21.4
South	35.9	36.0	37.1
West	23.5	23.2	24.0
Ten-Year Age Group			
25-34	32.7	32.3	33.4
35-44	34.2	34.2	32.2
45-54	33.2	33.6	34.4
Sex			
Female	50.8	46.4	46.9
Male	49.2	53.6	53.1
Race			
White	80.0	80.7	78.9
Black	12.5	11.5	11.5
American Indian/Alaska Native	0.8	0.7	0.7
Asian/Pacific Islander	5.5	5.7	7.0
Multiple races	1.2	1.3	1.9
Ethnicity			
Not Hispanic	84.8	85.0	82.6
Hispanic	15.2	15.0	17.4
Marital status			
Married	59.4	60.4	58.8
Widowed/divorced/separated	15.4	14.8	13.6
Never married	24.9	24.8	27.5
Educational attainment			
No degree	11.4	8.8	7.6
GED or HS diploma	49.2	47.6	47.4
Four-year degree	21.1	23.6	25.4
Master's, doctoral, or professional degree	9.9	11.9	13.9
Other degree	7.8	8.1	5.8
Section 2. Industry of round one employment			
Natural Resources	1.2	1.3	1.1
Mining	0.4	0.4	0.5
Construction	5.8	7.3	6.6
Manufacturing	10.5	12.2	10.8
Wholesale And Retail Trade	10.6	12.7	11.6
Transportation And Utilities	4.6	5.3	4.9
Information	1.4	2.1	2.4
Financial Activities	5.4	6.9	6.7
Professional And Business Services	12.9	14.7	12.7
Education, Health, And Social Services	13.3	19.3	24.6
Leisure And Hospitality	4.4	6.2	7.5
Other Services	5.2	5.8	4.8
Public Administration	4.5	5.5	5.5
Military	0.1	0.2	0.2
Unclassifiable Industry	0.2	0.1	0.1
Section 3. Occupation of round one employment			
Management, Business, And Financial Oper	13.5	16.9	17.2
Professional And Related Occupations	18.9	24.4	26.3
Service Occupations	11.4	14.5	15.8
Sales And Related Occupations	7.1	8.7	8.2
Office And Administrative Support	9.9	12.2	11.3
Farming, Fishing, And Forestry	0.8	0.8	0.6
Construction, Extraction, And Maintenanc	8.0	9.8	8.4
Production, Trnsportation, Matrl Moving	10.2	12.5	11.9
Military Specific Occupations	0.1	0.2	0.2
Section 4. Health status			
Reported fair/poor mental health in R1	5.8	3.2	3.2
Rcvd. presc. for antidepressant/antipsychotic in R1	7.1	5.5	5.5
Ever reported limitations climbing stairs	7.9	4.4	4.0
Ever reported difficulty performing moderate activities	7.2	3.8	3.4
Ever reported experiencing illness/inj. requiring immed. care	37.9	35.0	32.9
Ever reported illness/inj. requiring specialist attention	39.2	37.1	35.2
Ever reported more likely to take risks than average	30.7	31.1	30.5
Ever reported health impeding social life	23.1	18.0	17.1
Ever reported taking aspirin daily	11.6	10.8	9.9
Ever reported undergoing hysterectomy	6.7	5.6	5.3
Ever reported using assistive device	3.0	1.1	1.3
Ever reported complete inability to do activity	5.7	1.2	1.1
Ever reported general phys. difficulty	12.4	8.1	7.8
Ever reported phys. difficulty impeding work	9.0	3.4	3.1
Ever reported joint pain	43.5	41.4	43.7
Ever reported difficulty bending/stooping	9.2	5.4	5.1
Ever reported difficulty grasping w/ fingers	4.1	1.9	1.6
Ever reported difficulty walking mile	9.6	5.5	5.3
Ever reported difficulty reaching overhead	6.3	3.1	2.9
Ever reported difficulty standing 20 mins	8.1	4.3	4.0
Ever reported difficulty walking 3 blks	8.7	4.7	4.5
Ever spent night inpatient in hospital	11.0	8.4	7.8
Ever missed work b/c illness/inj.	50.6	57.3	54.9
Observations	129,911	78,819	30,859

Notes: MEPS age groups reflect age in first year of survey participation. Race, educational attainment, industry, and occupation variables all harmonize different variable codings used from 1996 to 2017. Marital status variables reflect round one marital status; they are also recoded from more granular marital status categories. Industry and occupation schemas align roughly with 1-digit NAICS and SOC schemas. Health status variables are derived directly from MEPS health status variables in raw data. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. MME analysis sample is composed of all individuals in the analysis sample who entered the sample on or after 2010.

Table 2: Job displacement and opioid use among MEPS participants

	Proportions of MEPS participants (%)		
	All prime-age in MEPS	Analysis sample	MME analysis sample (2010+)
Section 1. Displacement			
Displaced	8.2	8.8	8.0
Laid off	3.7	4.1	3.5
Displaced b/c bus. diss. or sold/job ended	4.8	5.1	4.7
Section 2. Opioid use			
High MME per day prescriptions			
Ever had a prescription for greater than 60 MME per day	4.4	2.8	2.8
Ever had a prescription for greater than 90 MME per day	2.2	1.3	1.3
Ever had a prescription for greater than 120 MME per day	1.0	0.5	0.5
Prescription counts			
Accumulated one or more opioid prescriptions	24.1	18.2	16.9
Accumulated 6 or more opioid prescriptions	3.6	1.1	1.0
Accumulated 12 or more opioid prescriptions	2.0	0.3	0.3
Observations	129,911	78,819	30,859

Notes: I designate an individual as having been displaced if they report changing their current main job for one of the following three reasons: (1) they were laid off (2) their business dissolved or was sold or (3) their current main job ended. Opioid receipt variables are constructed as detailed in appendix B. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. MME analysis sample is composed of all individuals in the analysis sample who entered the sample on or after 2010.

1. The drug matches based on its NDC to a list of opioid drugs compiled by the CDC.[6]
2. The drug's nonproprietary name is that of an opioid.
3. The drug's proprietary name is that of an opioid.

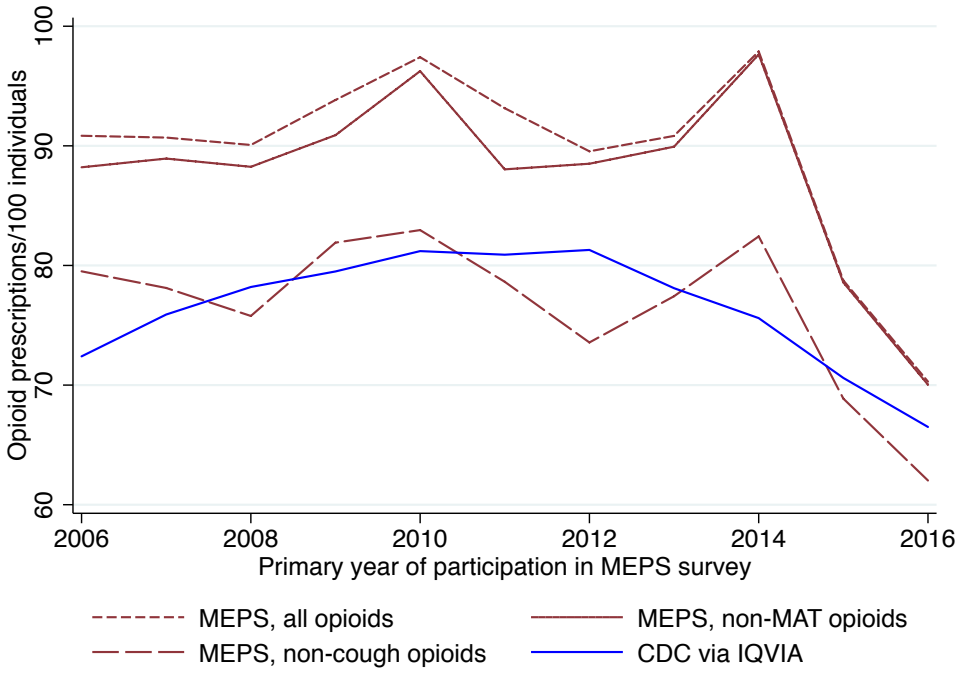
Unless otherwise indicated, I do not count opioids used in medication-assisted treatment (MAT) for substance abuse disorder, namely buprenorphine and methadone, as I am only interested in workers beginning to use opioids. Furthermore, coverage of methadone and buprenorphine prescriptions in the MEPS is poor because most MAT drugs are required by law to be dispensed by a physician in special clinics or a physician's office (Substance Abuse and Mental Health Services Administration, 2020*a*; Substance Abuse and Mental Health Services Administration, 2020*b*). Prescriptions dispensed in these settings are not covered by the MEPS.

To verify that the MEPS data is a reasonable measure of Americans' opioid use, I compare it with retail (non-hospital) opioid prescribing data reported by CDC and obtained by IQVIA.[7] Figure 3 plots opioid prescriptions per 100 individuals in the United States as computed in the MEPS and the CDC. I estimate that the prescribing rate for all opioid medications as well as all medications not used in MAT is higher in the MEPS than reported by the CDC. However, when I exclude opioid cough medicines (which are excluded in the CDC's prescribing rate statistics), the MEPS prescribing rates are close to the CDC prescribing rates. Figure 3 justifies drawing conclusions about opioid use from MEPS data.

The metrics I construct to proxy opioid abuse are (1) indicators for exceeding various thresholds of opioid prescriptions over the course of their survey participation and (2) indicators for exceeding various thresholds of morphine milligram equivalent (MME) dosage per day. I can only compute prescriptions' MME per day for individuals who entered the MEPS on or after 2010 (panel 15-onward), as the days' supply variable in prescribed medicines data files only became available in that year's release. Furthermore, computing MME per day for a prescription requires that I know the strength of the opioid component of the drug, which is often missing. To overcome this obstacle, I compile a list of possible drug strengths for opioids using the IBM Micromedex Red Book Drug Database based on their drug components to conservatively impute the opioid component strength

[6]This list can be accessed at `https://www.cdc.gov/drugoverdose/resources/data.html` within the Data Files box (CDC, 2018*c*).
[7]Retail pharmacy coverage would, like the MEPS, cover few methadone and buprenorphine prescriptions.

Figure 3: Opioid prescribing rate over time, by data source



Notes: CDC IQVIA prescribing rates are taken from CDC Opioid Overdose Data: U.S. Opioid Prescribing Rate Maps, last updated October 2018 (CDC 2018*b*). CDC IQVIA prescribing rates only take into account opioids which are not cough medicines. "MAT" is an abbreviation for "medication-assisted treatment." Non-cough opioids are all non-MAT opioids (as classified in appendix subsection B.2) except those which contain the components “phenylephrine", “guaifenesin", “promethazine", “chlorpheniramine", “homatropine", “triprolidine", “diphenhydramine", “potassium guaiacolsulfonate", “brompheniramine", or “bromododiphenhydramine."

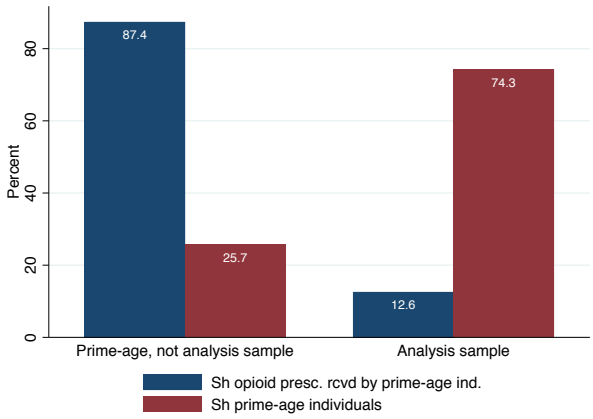
(IBM Red Book, 2019). For a discussion of my imputation methods, see appendix subsection B.3.

It is difficult to specify how many opioid prescriptions correspond to opioid abuse, though estimates exist in the public health literature. Rice et al. (2012) show that diagnosed opioid abusers in a sample of 12 million employer-insured United States patients accumulated 13.3 opioid prescriptions per year on average. However, this likely overstates the number of prescriptions corresponding to abuse because Rice et al. (2013) are unable to observe the number of prescriptions received by undiagnosed opioid abusers. Morden et al. (2014) designate a much lower threshold, six or more prescriptions per year, for potentially problematic “chronic" prescription opioid use. For transparency’s sake, I show indicators for accumulating one, six, and twelve prescriptions over the twenty-four month period of MEPS participation, and show regression results corresponding to each of these indicators in the main text of paper. I present summary statistics regarding the prevalence of prescription opioid use by these metrics in section 2 of table 2. By my estimates, 24.1% of Americans received a non-MAT opioid prescription between 1996 and 2017, with 3.6% receiving six or more prescriptions and 2% receiving twelve or more prescriptions. Restricting to my analysis sample shrinks these proportions to 18.2%, 1.1%, and 0.3%, respectively.

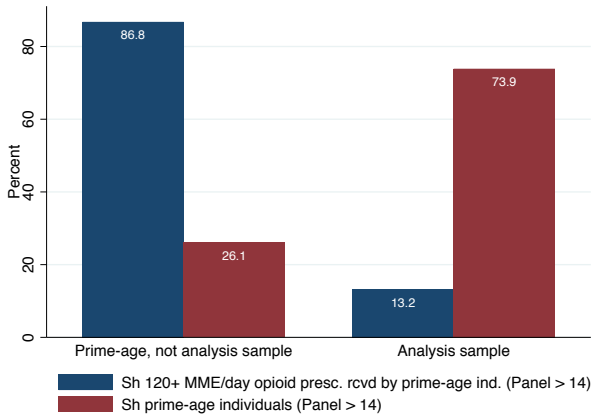
Correspondency between MME per day and opioid abuse is well-established. A patient is “opioid tolerant" if they use more than 60 MME per day. According to the CDC, physicians should “avoid or carefully justify increasing dosage to ≥90 MME per day" (Dowell, Haegerich, and Chou, 2016). Finkelstein, Gentzkow, and Williams (2018) set their threshold for abuse at 120 MME per day. I report regression results for all three of these MME per day outcomes, though my inability to compute MME for individuals who entered the MEPS prior to 2010 reduces my power to rule out small displacement effect sizes for this outcome. I also present summary statistics regarding the prevalence of prescription opioid use by these metrics in section 2 of table 2. I estimate that 4.4% of Americans ever received a prescription for greater than 60 MME per day, 2.2% of Americans ever received a prescription for over 90 MME per day, and 1% of prime-age Americans ever received a prescription for over 120 MME per day. These proportions are lower in my analysis sample at 2.8%, 1.3%, and 0.5%, respectively.

Figure 4: Share prescriptions linked to prime-age individuals by inclusion in analysis sample, 1999-2017

(a) All opioid prescriptions



(b) 120+ MME per day opioid prescriptions



Notes: I classify opioid prescriptions and compute MME per day according to the methods outlined in appendix B. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. MME analysis sample are individuals in my analysis sample who began their survey participation in or after 2010 (e.g. individuals in panel 14 or subsequent panels). Estimates are pooled over the years 1999-2017. Data from 1996-1998 is excluded because no individuals who began their survey participation in these years are in the analysis sample.

My analysis sample accounts for fairly little of the opioid use I measure among prime-age individuals in the MEPS. As figure 4 shows, individuals in the analysis sample and MME analysis sample make up the vast majority – 74.3% and 73.9%, respectively – of prime-age MEPS participants from 1999-2017 but only account for 12.6% of both total opioid prescriptions and of 120+ MME per day opioid prescriptions linked to prime-age MEPS participants. I link the lion's share of both overall opioid prescriptions and 120+ MME per day prescriptions – 87% of both – to individuals I exclude from my analysis sample because they were not employed during the first round of survey participation, because they received one or more opioid prescriptions in the first round of survey participation, or both. These facts suggest that analyses of individuals transitioning to opioid abuse on the extensive margin may explain a small proportion of opioid abuse in the United States at any given time. Nevertheless, the transition from "opioid naïveté" to opioid abuse is a worthwhile research subject because every opioid user was once a non-user.

3.5 Other relevant data: health status, industry, and occupation

The MEPS' rich health data allows me to condition on health status when considering the impact of displacement on likelihood of opioid abuse. I show summary statistics of health characteristics in section 4 of table 1. I construct most of the variables therein using round-specific health status variables, setting each indicator to one if, in any round of interviews, a survey participant reports experiencing the health issue in question. The exceptions are the indicator for reporting "fair" or "poor" mental health in round one and the indicator for receiving an antidepressant or anti-psychotic in round one, which I use because conditioning on post-displacement mental health very likely "controls for the treatment." Some would argue that controlling for any health condition throughout the course of the survey, instead of prior to displacement, is "controlling for the treatment" because I control for negative health effects of displacement (see Schaller and Stevens, 2015). For this reason, I reproduce my main results controlling only for round-one health status in appendix C.

Few of the MEPS' health status variables are available for all five reference periods; the vast majority are available for either rounds 1, 3, and 5 or 2 and 4. As I discuss in subsection 4.1, this renders individual fixed effects and event study specifications much less informative for estimating the effect of displacement on opioid use in the MEPS. Failing to control for the full vector of health status variables in examining the effect of displacement on opioid use will yield highly upward-biased estimates, since health challenges are likely to be highly correlated with both displacement and opioid use.

In addition to health status, I condition on pre-displacement industry and occupation, as an individual's propensity to use opioids following job displacement may be related to the degree to which their job induces pain as well as their attachment to their job, both of which might vary according to industry and occupation. The MEPS industry and occupation schemas, shown in sections 2 and 3 of table 1 alongside proportions of analysis sample survey participants working in each of them during round one, roughly map onto two-digit North American Industry Classification System and Standard Occupation Classification schemas, respectively.

4 Regression Analysis

4.1 Main specifications

My main empirical specifications are linear probability models in which I regress an indicator Y_i for a participant exceeding an opioid use threshold by the end of her two years of survey participation on a constant, an indicator for non-layoff job displacement, a vector of panel fixed effects, and the demographic, industry,

occupation, and health status variables enumerated in table 1. Written out formally, this amounts to

$$Y_i = \alpha + \beta \text{Non-layoff displaced}_i + X_i'\gamma + \text{PANEL}_i'\rho + \epsilon_i \tag{1}$$

where Non-layoff displaced$_i$ is an indicator for having experienced non-layoff job displacement during survey participation due to business dissolution, establishment sale, or job ending; X_i is a vector containing the demographic, industry, occupation, and health status variables in table 1; PANEL$_i$ is a vector consisting of indicators for being in each panel; and ϵ_i is an error term.[8] I use Non-layoff displaced$_i$ as the explanatory variable in the regression results I report in the main text because I see it as more plausibly exogenous than being laid off or displaced overall. However, I report estimates of β in equation 1 as estimated using all displacement and displacement due to layoffs in appendix tables A.3 and A.4, respectively.

My outcomes of interest Y_i can be written as:

$$\mathbb{1}\left(\sum_{\text{round}=1}^{5} \text{Opioid prescription count}_{\text{round}} \geq k \right) \text{ for } k \in \{1, 6, 12\} \tag{2}$$

$$\mathbb{1}\left(\max \left(\text{MME per day}_{\text{prescription}} \middle| \text{prescription} \in \text{All prescriptions} \right) \geq t \right) \text{ for } t \in \{60, 90, 120\} \tag{3}$$

which are indicators for individual i accumulating k or more opioid prescriptions across their five rounds of survey participation for $k \in \{1, 6, 12\}$ and individual i having a max of $\geq t$ MME per day at any point in their survey participation, respectively. I show results with Y_i defined as outcome 2 for $k \in \{1, 2, 3, ..., 15\}$ in appendix figure A.3.

4.2 Identification in main specification

I am interested in using equation 1 to understand the effect of job displacement on the probability that an individual begins using opioids. My specification of choice has two key weaknesses: first, the possibility of negative selection into displacement on productivity-related characteristics correlated with propensity to use opioids, and second, my imprecise measurement of the relative timing of opioid use and displacement under equation 1.

I have already hinted at the strategies I employ to limit negative selection into displacement. To reiterate, I strategically define “displacement" to capture only termination in which managers have less discretion to displace workers selectively on productivity (Gibbons and Katz, 1991; Eliason and Storrie, 2006) and control for observable correlates of opioid use and displacement. I hope that measuring the effect of non-layoff displacement and conditioning on race, age, ethnicity, and health status will bring β in equation 1 closer to the causal effect of displacement. To the extent that bias remains, it is likely positive. As Eliason and Storrie (2006) note, workers who remain at establishments until the bitter end (as opposed to quitting upon announcement of establishment closure to take other jobs) are likely negatively selected. Furthermore, the potential bias introduced by insufficiency of my battery of health status controls is also likely positive. If unobserved health conditions affect workers' likelihood of experiencing displacement, they probably make workers more likely to experience displacement. Such conditions almost certainly make workers more likely to receive opioid prescriptions.

A thornier inadequacy of my main specification is the question of relative timing of displacement and opioid prescription receipt. Clearly, specification 1 is a second-best specification relative to an individual fixed-

[8] My results are not sensitive to whether I include individuals whose job ended as being non-layoff displaced. Appendix table A.5 shows my main results under specifications excluding these inidviduals. The results closely resemble those in the main text.

effects specification:

$$Y_{i,\text{round}} = \alpha_i + \beta \text{Displaced}_{i,\text{round}} + X'_{i,\text{round}}\gamma + \epsilon_{i,\text{round}} \tag{4}$$

in which I can cleanly identify the relative timing of displacement $\text{Displaced}_{i,\text{round}}$ and opioid use measure $Y_{i,\text{round}}$ and compare individuals before and after displacement conditional on other factors. To understand why, on its face, equation 1 is inferior to specification 4, consider two individuals A and B in my analysis sample, where individual A is displaced after round three and receives 12 opioid prescriptions in round two and individual B is also displaced in round three but receives her 12 opioid prescriptions in round four. Both individuals A and B are "treated compliers" under equation 1, but it is not possible that individual A's job displacement caused her opioid use because her opioid use preceded her displacement. Indeed, in order for β to yield the causal effect of displacement, not only would $\{\text{Non-layoff displaced}_i, \epsilon_i\}$ need to satisfy $\text{cov}(\text{Non-layoff displaced}_i, \epsilon_i | X_i, \text{PANEL}_i) = 0$, but the "treated" analysis sample would need to be free of any individuals such as individual A. Clearly, this is not a reasonable assumption.

However, two shortcomings of the MEPS data prevent equation 4 from being an improvement on equation 1 in my setting. First, using specification 4 prevents me from using a full battery of health controls. As I discuss in subsection 3.5, most health status variables are available only for a subset of rounds, either 1, 3, and 5 or 2 and 4. For this reason, I am unable to control for nearly all of my health condition variables under equation 4 (I can still control for whether individuals missed work due to illness or injury). Failing to control for workers' health status will introduce strong upward bias in estimates of the effect of displacement on opioid use.

Second is that I do not observe exact dates of prescription receipt and displacement in the MEPS, but only the round in which they occur, which generally spans three to five months. Hence equation 4 suffers from the same relative timing issue as equation 1. To see this, consider a survey participant who received 12 opioid prescriptions during round three. Assume she told her MEPS interviewer in her third round interview that she had switched her current main job due to business closure or her job ending, meaning that she was displaced in round three. I will count this individual as a treated complier in equation 4, though her opioid use may well have preceded her displacement, in which case it would be incorrect to attribute her opioid use to her displacement.

To the extent that the relative timing issue under equation 1 pushes the β coefficient I measure away from the causal effect of displacement, the bias is likely upward. Bias would be introduced by workers using opioids before displacement, and using opioids likely reduces workers' productivity, thereby making them more likely to be displaced to the extent that managers have discretion. Because the potential bias introduced by both identification issues I have raised is positive, I expect estimates of β to be upper bounds on the effect of displacement on opioid use among individuals in my sample.

4.3 Effect heterogeneity

In addition to estimating my baseline specification, equation 1, I estimate a number of simple interaction models. All of these models are of the form:

$$Y_i = \varphi_0 + \varphi_1 \text{Non-layoff displaced}_i + \varphi_2 W_i + \varphi_3 \text{Non-layoff displaced}_i \times W_i + (X_i \setminus W_i)'\varphi_4 + \text{PANEL}'_i\rho + \psi_i \tag{5}$$

where W_i is the interaction category. In the main text of the paper, I estimate two interaction models which allow the effect of displacement to differ across subgroups, namely non-Hispanic whites and blue-collar workers. I call a worker a "blue-collar worker" if their round 1 occupation is one of the last four occupations in section 3 of

table 1. Recall that part of this study's goal is to give evidence pertaining to the image of the white, working-class individual who resorts to opioid abuse in response to a lack of opportunity in the labor market. If this depiction were accurate, I would expect to observe positive φ_2 in both of these interaction models. However, my results do not support this hypothesis. In appendix tables A.6-A.9, I also show results from simple interaction models which allow for differential impacts of displacement on workers based on whether they experienced pain in the first reference period, experienced depression in the first reference period, or did not hold a bachelor's degree as of the first reference period. The evidence from these interaction models does not support the idea that displacement makes individuals from these groups disproportionately more likely to use opioids.

I also estimate three regressions which enable me to test whether the effect of displacement on opioid use is mediated by the extent to which displacement poses a financial challenge for the displaced worker. If displacement makes individuals less likely to use opioids only to the extent that it makes them less able to afford opioids, I would expect to see more strongly negative estimates of the effect of displacement among individuals who experienced a considerable reduction in income as a result of displacement, such as displaced individuals who reported not working for at least one period, whose wages constituted the majority of income in their households, or individuals in families without significant non-labor (business or trust) income. To test this, I estimate equation 5 setting W_i equal to an indicator for (1) reporting at least one period of no work, (2) reporting wage income for the first year in the MEPS exceeding half of the total dwelling-unit income reported, and (3) anyone in the survey participant's dwelling unit having reported having business or trust income in their first year in the MEPS.

4.4 Regression estimates

I estimate equations 1 and 5 using ordinary least squares for both the opioid count and MME per day opioid use metrics and report coefficients on non-layoff displacement indicators in table 3.[9] Section 1 shows baseline results from regressing indicators for exceeding opioid abuse indicators on indicators for experiencing displacement, controlling for the demographic, health status, and industry and occupation variables outlined in table 1. Sections 2 through 6 show sums of coefficients on displacement and interaction terms (e.g. $\varphi_2 + \varphi_3$ in equation 5), which can be thought of roughly as marginal effects for the following partitions of the sample: (1) blue-collar vs. white-collar workers (2) non-Hispanic white vs. Hispanic or nonwhite workers (3) workers who went at least one reference period without working vs. workers who worked all reference periods (4) workers whose dwelling units reported having business or trust income in their first year in the MEPS vs. workers who did not and (5) workers whose wage income constituted the majority of their dwelling unit's year one income vs. those whose did not. The sums of coefficients I report in sections 2 and 3 are meant to shed light on Case and Deaton's "deaths of despair" hypothesis, and the sums of coefficients I report in sections 4 through 6 are meant to shed light on the mechanism by which displacement is associated with reduced likelihood of opioid use.

Section 1 of table 3 shows that non-layoff displacement is associated with a significantly lower likelihood of ever receiving an opioid prescription, receiving twelve or more opioid prescriptions, and ever receiving an opioid prescription with 120 or more MME per day. The first of these effects is larger in absolute terms, at 1.3 percentage points or 5 percent of the non-layoff displaced individuals' baseline probability of ever receiving an opioid prescription. The reductions in probability of receiving twelve prescriptions or a 120+ MME per day prescription, while smaller in absolute terms at 0.2 and 0.4 percentage points respectively, are much larger in

[9] Tables A.1 and A.2 report the same set of results relaxing the sample restrictions to include all MEPS participants and all prime-age participants from panel 4 onwards, as discussed in subsection 3.2. Tables A.3, A.4, and A.5 report the same set of coefficients where I use displacement, layoff, and displacement due to establishment dissolution or sale, respectively, as independent variables.

relative terms: they amount to 20 and 80 percent reductions in non-layoff displaced individuals' baseline probability of achieving these thresholds of opioid use. At lower MME-per-day thresholds and at the six prescription threshold, non-layoff displacement is not associated with significant changes in likelihood of opioid use, and point estimates are modest in both absolute and in relative terms.

The results shown in sections 2 and 3 of table 3 do not support the hypothesis that labor market hardship makes blue-collar workers and non-Hispanic whites particularly likely to abuse opioids. Point estimates of the "marginal effects" of non-layoff displacement for workers in these sub-groups are generally lower than the corresponding estimates for workers not in these sub-groups, though only about half of the point estimates among either set of subgroups are statistically distinguishable from zero. Most of the statistically significant point estimates are similar in size to their corresponding baseline results. The exceptions are associations between non-layoff displacement and likelihood of ever receiving an opioid prescription, which are double the size of the baseline associations for blue-collar workers and non-Hispanic whites and roughly half the size of the baseline associations for white-collar workers and Hispanics and nonwhites.

Table 3: Main regression results

	Panel A: Opioid Count Outcomes			Panel B: MME per Day Outcomes		
	Ever used opds (1)	Rcvd. ≥ 6 opd. prsc. (2)	Rcvd. ≥ 12 opd. prsc. (3)	Ever 60+ MME/day (4)	Ever 90+ MME/day (5)	Ever 120+ MME/day (6)
Section 1. Baseline						
Ever non-layoff displaced	-0.013**	-0.002	-0.002**	0.002	0.001	-0.004***
	(0.007)	(0.002)	(0.001)	(0.006)	(0.004)	(0.001)
Mean of outcome	0.197	0.020	0.009	0.026	0.013	0.005
Section 2. Heterogeneity by occupation						
Blue-collar	-0.027**	-0.005	-0.002	-0.009	-0.001	-0.001
	(0.011)	(0.003)	(0.002)	(0.007)	(0.005)	(0.003)
Mean of outcome (blue-collar)	0.167	0.017	0.011	0.025	0.016	0.005
White-collar	-0.007	-0.001	-0.002***	0.006	0.003	-0.005***
	(0.008)	(0.002)	(0.001)	(0.008)	(0.006)	(0.001)
Mean of outcome (white-collar)	0.197	0.020	0.009	0.026	0.013	0.005
Section 3. Heterogeneity by race/ethnicity						
Non-Hisp. white	-0.022**	-0.001	-0.002*	0.003	0.002	-0.006***
	(0.009)	(0.003)	(0.001)	(0.011)	(0.007)	(0.002)
Mean of outcome (non-hisp. white)	0.244	0.029	0.015	0.043	0.021	0.006
Not non-hisp. white	0.002	-0.004***	-0.002**	-0.001	0.000	-0.002
	(0.008)	(0.002)	(0.001)	(0.004)	(0.003)	(0.001)
Mean of outcome (not non-hisp. white)	0.197	0.020	0.009	0.026	0.013	0.005
Section 4. Heterogeneity by whether individual did not work for at least one period						
At lst 1 pd did not work	-0.025*	-0.012**	-0.006**	-0.009	-0.007	-0.007
	(0.015)	(0.006)	(0.003)	(0.012)	(0.009)	(0.005)
Mean of outcome (at lst 1 pd did not work)	0.199	0.022	0.011	0.026	0.011	0.005
Worked all ref pds	-0.011	-0.001	-0.001	0.005	0.003	-0.004***
	(0.007)	(0.002)	(0.001)	(0.008)	(0.005)	(0.001)
Mean of outcome (worked all ref pds)	0.197	0.020	0.009	0.026	0.013	0.005
Section 5. Heterogeneity by whether dwelling unit has business/trust income						
Family has biz/trust inc.	-0.025*	-0.004*	-0.001	-0.010	-0.002	-0.004***
	(0.013)	(0.002)	(0.001)	(0.008)	(0.007)	(0.001)
Mean of outcome (family has biz/trust inc.)	0.202	0.018	0.009	0.027	0.012	0.002
No fam biz/trust inc.	-0.009	-0.001	-0.003**	0.005	0.002	-0.004***
	(0.008)	(0.002)	(0.001)	(0.008)	(0.005)	(0.001)
Mean of outcome (no fam biz/trust inc.)	0.197	0.020	0.009	0.026	0.013	0.005
Section 6. Heterogeneity by share of dwelling unit income from individuals' wage income						
Wage inc. was majority of Y1 family income	-0.002	-0.001	-0.003**	0.028	0.017	-0.006***
	(0.013)	(0.004)	(0.001)	(0.019)	(0.014)	(0.001)
Mean of outcome (wage inc. was majority of y1 family income)	0.211	0.020	0.009	0.029	0.014	0.002
Wage inc. not majority of Y1 family inc.	-0.017**	-0.003	-0.002*	-0.007	-0.004	-0.003**
	(0.007)	(0.002)	(0.001)	(0.006)	(0.003)	(0.001)
Mean of outcome (wage inc. not majority of y1 family inc.)	0.197	0.020	0.009	0.026	0.013	0.005
Observations	78,819	78,819	78,819	30,859	30,859	30,859

Notes: Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. Regression is estimated using pooled data from 1996-2017.

Finally, results in sections 4 through 6 of table 3 support the hypothesis that reductions in likelihood of high-threshold opioid use associated with non-layoff displacement are mediated by the extent to which affected workers suffer financially. Results are less consistent at lower thresholds of opioid use. At the highest thresholds of opioid use – ever receiving 12 or more prescriptions, or ever receiving a 120+ MME per day prescription, point estimates of the association between non-layoff displacement and opioid use among workers who suffer more

financially – workers who did not work for at least one reference period, who did not have dwelling unit business or trust income in year one, and whose wage income made up the majority of their dwelling unit's total year one income – are lower and more statistically significant than point estimates among individuals who likely suffer less financially as a result of their displacement.

Estimating these regressions among broader samples yield coefficients of the same sign, but of smaller magnitude, as shown in tables A.1 and A.2. Coefficients on indicators for ever experiencing a layoff, reported in table A.4, are typically larger than the coefficients reported in table 3. Coefficients on indicators for ever experiencing displacement, reported in appendix table A.3 lie between the two. In general, coefficients in these two appendix tables are less statistically significant than their counterparts in table 3, though the same patterns between sections are discernible. Regression results reported in table A.5, on the other hand, are generally more strongly negative and statistically significant than in table 3, likely reflecting the fact that individuals displaced because their businesses dissolved or were sold would experience more financial hardship than individuals displaced due to job ending, as the latter may have anticipated their job ending. I do not observe patterns in any of these tables which would suggest that displacement induces blue-collar workers or non-Hispanic whites to abuse opioids. Furthermore, I observe patterns in all of these tables which suggest that the extent to which displacement reduces the likelihood of opioid use is mediated by the financial hardship displacement imposes on workers, though these differences are smaller and sometimes wrong-signed in table A.5, possibly reflecting the fact that unexpected displacement due to business dissolution or sale is especially financially taxing regardless of workers' pre-displacement situations.

5 Discussion

This paper provides the first estimates from micro data on the effects of job displacement on individuals' demand for prescription opioids. While I observe the strong cross-sectional relationship between hardship in the labor market and opioid use noted by Case and Deaton (2015; 2017; 2020) and Krueger (2017), my regression analysis tends to cast doubt on the idea that short-term labor market dislocations cause opioid abuse. Conservatively, I can interpret the primary relationships I observe in subsection 4.4 as bounds on the causal effect of displacement on likelihood of opioid abuse. My findings imply, then, that the causal effect of displacement on likelihood of opioid use at all thresholds – one prescription, six or more prescriptions, twelve or more prescriptions, or 60, 90, or 120 MME per day – is at most zero, or slightly negative at high thresholds. This would appear at first glance to be at odds with Case and Deaton (2015; 2017; 2020) and Krueger (2017), both of which argue that labor market dislocation has advanced the opioid epidemic.

How might I reconcile my findings with the "deaths of despair" narrative around opioid use? One possibility is that the idea of the individual who experiences labor market dislocation I have used in this paper differs from the idea of the labor-market afflicted individual advanced in Case and Deaton (2015; 2017) and Krueger (2017). In particular, my paper focuses on individuals who are employed during at least one reference period of their survey participation (the first), who do not use opioids during this reference period, and who experience a short-term dislocation by way of displacement. The interplay between opioids and labor market activity documented by Case and Deaton (2015; 2017) and Krueger (2017) may relate to individuals who experience adversity in the labor market over longer periods of time, and for reasons that might be more related to their productivity. While tables A.1 and A.2 show that my key findings hold up in broader samples, data limitations prevent me from investigating this question rigorously. Perhaps most importantly, I only observe individuals in the MEPS for two years, scarcely a sufficient time frame to investigate individuals' long-term labor market difficulties.

Further addressing the narratives advanced by Case and Deaton (2015; 2017; 2020) will require further research. Researchers should study individuals' labor market and opioid abuse behavior over longer periods of time: such studies could provide evidence related to the effect of longer-term labor market dislocations on individuals' likelihood of opioid abuse. Furthermore, physician-level data on opioid prescribing for specific geographies could help researchers measure the supply-side response of physicians to county-level labor market dislocations and help illuminate whether the findings of papers using county-level data are dictated in part by supply-side changes as opposed to individual opioid demand responses to changes in the labor market. For instance, Currie and Schnell (2018) show that physicians from lower-ranked medical schools prescribe considerably more opioid drugs than physicians trained at higher-ranked institutions. If this result is reliable, and if physicians with prestigious credentials are less likely to locate in regions experiencing economic downturns, inhabitants of poor regions may rely on poorly trained physicians who overprescribe opioids. Both of these are lofty goals for research insofar as they may require use of administrative data sets, but could provide critical information for policymakers looking to address the opioid epidemic.

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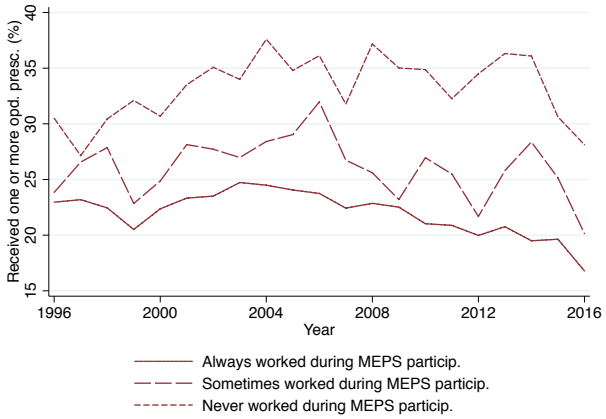
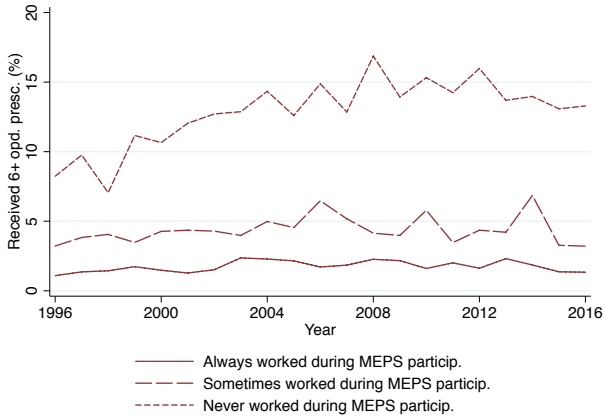
A Supplemental exhibits

Figure A.1: Non-MAT opioid prescription receipt among prime-age individuals by employment

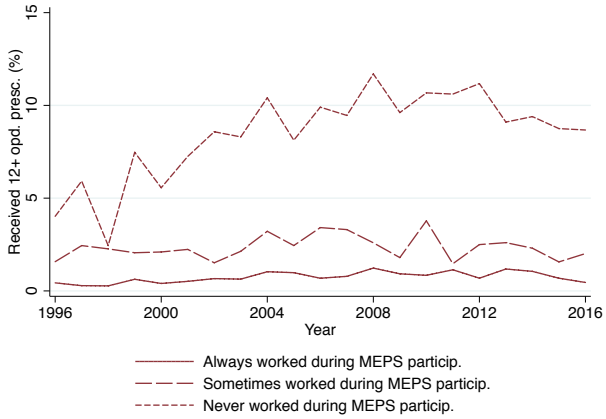
(a) At least one prescription



(b) Six or more prescriptions



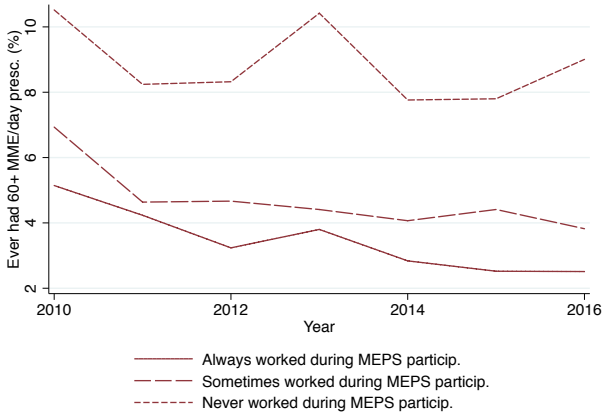
(c) Twelve or more prescriptions



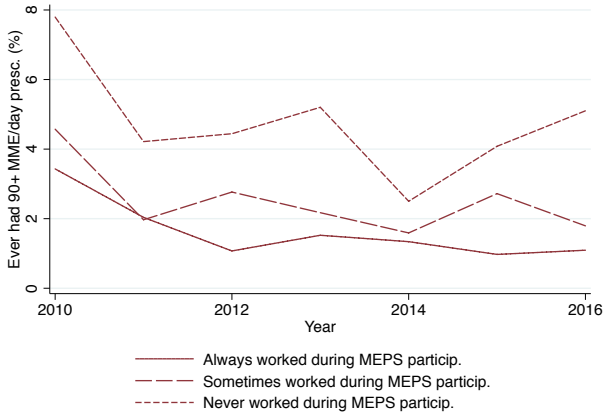
Notes: Statistics presented here are computed among prime-age individuals using survey weights. I compute these statistics by assigning each individual in the MEPS a primary year of participation in the survey, which is always their first year of participation except for individuals for which I have no data in the first year of their participation but for whom I have data in the second year of participation. Then I determine whether the individual worked for some, all, or none of their participation in the MEPS. For each year shown, then, I present means of indicators for individuals exceeding each threshold of opioid use depending on their employment category. In this graph, rates of individuals exceeding opioid prescribing thresholds are computed without opioid drugs used in medication-assisted treatment for opioid addiction, namely buprenorphine and methadone.

Figure A.2: MME per day opioid prescription receipt among prime-age individuals by employment

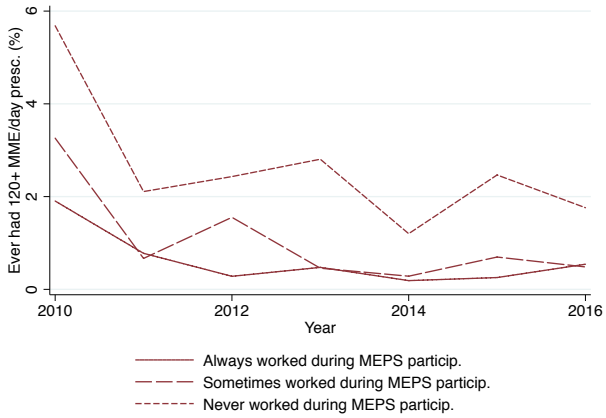
(a) Had presc. with ≥ 60 MME per day



(b) Had presc. with ≥ 90 MME per day



(c) Had presc. with ≥ 120 MME per day



Notes: Statistics presented here are computed among prime-age individuals using survey weights. I compute these statistics by assigning each individual in the MEPS a primary year of participation in the survey, which is always their first year of participation except for individuals for which I have no data in the first year of their participation but for whom I have data in the second year of participation. Then I determine whether the individual worked for some, all, or none of their participation in the MEPS. For each year shown, then, I present means of indicators for individuals exceeding each threshold of opioid use depending on their employment category. In this graph, rates of individuals exceeding opioid prescribing thresholds are computed without opioid drugs used in medication-assisted treatment for opioid addiction, namely buprenorphine and methadone.

Table A.1: Main regression results: Sample = all panel 4+ participants

	Panel A: Opioid Count Outcomes			Panel B: MME per Day Outcomes		
	Ever used opds (1)	Rcvd. ≥ 6 opd. prsc. (2)	Rcvd. ≥ 12 opd. prsc. (3)	Ever 60+ MME/day (4)	Ever 90+ MME/day (5)	Ever 120+ MME/day (6)
Section 1. Baseline						
Ever non-layoff displaced	-0.004	-0.002	-0.001	-0.002	-0.000	-0.003**
	(0.006)	(0.002)	(0.002)	(0.005)	(0.004)	(0.002)
Mean of outcome	0.197	0.020	0.009	0.026	0.013	0.005
Section 2. Heterogeneity by occupation						
Blue-collar	-0.014	-0.006*	0.000	-0.008	-0.001	-0.002
	(0.010)	(0.004)	(0.003)	(0.007)	(0.005)	(0.003)
Mean of outcome (blue-collar)	0.167	0.017	0.011	0.025	0.016	0.005
White-collar	-0.001	-0.001	-0.002	0.000	-0.000	-0.004**
	(0.007)	(0.003)	(0.002)	(0.006)	(0.004)	(0.002)
Mean of outcome (white-collar)	0.197	0.020	0.009	0.026	0.013	0.005
Section 3. Heterogeneity by race/ethnicity						
Non-Hisp. white	-0.007	-0.001	-0.001	-0.001	-0.000	-0.004**
	(0.008)	(0.003)	(0.002)	(0.008)	(0.006)	(0.002)
Mean of outcome (non-hisp. white)	0.244	0.029	0.015	0.043	0.021	0.006
Not non-hisp. white	0.000	-0.005**	-0.003*	-0.004	-0.000	-0.001
	(0.007)	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)
Mean of outcome (not non-hisp. white)	0.197	0.020	0.009	0.026	0.013	0.005
Section 4. Heterogeneity by whether individual did not work for at least one period						
At lst 1 pd did not work	-0.006	-0.007	-0.005	-0.005	-0.005	-0.005
	(0.011)	(0.005)	(0.004)	(0.010)	(0.007)	(0.004)
Mean of outcome (at lst 1 pd did not work)	0.199	0.022	0.011	0.026	0.011	0.005
Worked all ref pds	-0.008	-0.002	-0.001	-0.003	0.001	-0.003
	(0.007)	(0.002)	(0.002)	(0.006)	(0.005)	(0.002)
Mean of outcome (worked all ref pds)	0.197	0.020	0.009	0.026	0.013	0.005
Section 5. Heterogeneity by whether dwelling unit has business/trust income						
Family has biz/trust inc.	-0.013	-0.002	-0.000	-0.001	0.001	-0.002
	(0.011)	(0.004)	(0.003)	(0.010)	(0.008)	(0.003)
Mean of outcome (family has biz/trust inc.)	0.202	0.018	0.009	0.027	0.012	0.002
No fam biz/trust inc.	-0.001	-0.003	-0.002	-0.003	-0.001	-0.004**
	(0.007)	(0.003)	(0.002)	(0.006)	(0.004)	(0.002)
Mean of outcome (no fam biz/trust inc.)	0.197	0.020	0.009	0.026	0.013	0.005
Section 6. Heterogeneity by share of dwelling unit income from individuals' wage income						
Wage inc. was majority of Y1 family income	0.005	-0.003	-0.003	0.012	0.010	-0.007***
	(0.012)	(0.005)	(0.003)	(0.015)	(0.012)	(0.002)
Mean of outcome (wage inc. was majority of y1 family income)	0.211	0.020	0.009	0.029	0.014	0.002
Wage inc. not majority of Y1 family inc.	-0.007	-0.002	-0.001	-0.006	-0.003	-0.002
	(0.006)	(0.002)	(0.002)	(0.005)	(0.003)	(0.002)
Mean of outcome (wage inc. not majority of y1 family inc.)	0.197	0.020	0.009	0.026	0.013	0.005
Observations	120,299	120,299	120,299	47,523	47,523	47,523

Notes: Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Age groups in regressions above are those enumerated in section 1 of table 1, plus “under age 25" and “over age 55." Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis sample is all MEPS participants in panels 4-onward. Regression is estimated using pooled data from 1996-2017.

Table A.2: Main regression results: Sample = all prime-age panel 4+ participants

	Panel A: Opioid Count Outcomes			Panel B: MME per Day Outcomes		
	Ever used opds	Rcvd. ≥ 6 opd. prsc.	Rcvd. ≥ 12 opd. prsc.	Ever 60+ MME/day	Ever 90+ MME/day	Ever 120+ MME/day
	(1)	(2)	(3)	(4)	(5)	(6)
Section 1. Baseline						
Ever non-layoff displaced	-0.010	-0.002	-0.001	-0.004	-0.001	-0.005***
	(0.007)	(0.003)	(0.002)	(0.006)	(0.005)	(0.002)
Mean of outcome	0.197	0.020	0.009	0.026	0.013	0.005
Section 2. Heterogeneity by occupation						
Blue-collar	-0.022*	-0.006	-0.001	-0.011	0.001	-0.003
	(0.012)	(0.005)	(0.004)	(0.008)	(0.006)	(0.003)
Mean of outcome (blue-collar)	0.167	0.017	0.011	0.025	0.016	0.005
White-collar	-0.005	-0.000	-0.002	-0.001	-0.001	-0.005***
	(0.008)	(0.003)	(0.002)	(0.008)	(0.006)	(0.002)
Mean of outcome (white-collar)	0.197	0.020	0.009	0.026	0.013	0.005
Section 3. Heterogeneity by race/ethnicity						
Non-Hisp. white	-0.014	0.001	-0.001	-0.004	-0.001	-0.007***
	(0.010)	(0.004)	(0.003)	(0.011)	(0.007)	(0.003)
Mean of outcome (non-hisp. white)	0.244	0.029	0.015	0.043	0.021	0.006
Not non-hisp. white	-0.003	-0.006***	-0.003	-0.004	-0.000	-0.001
	(0.008)	(0.002)	(0.002)	(0.005)	(0.004)	(0.002)
Mean of outcome (not non-hisp. white)	0.197	0.020	0.009	0.026	0.013	0.005
Section 4. Heterogeneity by whether individual did not work for at least one period						
At lst 1 pd did not work	-0.017	-0.018**	-0.015***	-0.020	-0.010	-0.009
	(0.015)	(0.007)	(0.005)	(0.013)	(0.009)	(0.005)
Mean of outcome (at lst 1 pd did not work)	0.199	0.022	0.011	0.026	0.011	0.005
Worked all ref pds	-0.010	0.000	0.001	-0.000	0.002	-0.004**
	(0.008)	(0.003)	(0.002)	(0.008)	(0.005)	(0.002)
Mean of outcome (worked all ref pds)	0.197	0.020	0.009	0.026	0.013	0.005
Section 5. Heterogeneity by whether dwelling unit has business/trust income						
Family has biz/trust inc.	-0.018	-0.002	0.001	-0.019**	-0.008	-0.006***
	(0.013)	(0.004)	(0.003)	(0.008)	(0.006)	(0.002)
Mean of outcome (family has biz/trust inc.)	0.202	0.018	0.009	0.027	0.012	0.002
No fam biz/trust inc.	-0.007	-0.002	-0.002	0.001	0.001	-0.004**
	(0.008)	(0.003)	(0.002)	(0.008)	(0.006)	(0.002)
Mean of outcome (no fam biz/trust inc.)	0.197	0.020	0.009	0.026	0.013	0.005
Section 6. Heterogeneity by share of dwelling unit income from individuals' wage income						
Wage inc. was majority of Y1 family income	0.002	-0.000	0.000	0.024	0.013	-0.009***
	(0.014)	(0.006)	(0.004)	(0.018)	(0.014)	(0.002)
Mean of outcome (wage inc. was majority of y1 family income)	0.211	0.020	0.009	0.029	0.014	0.002
Wage inc. not majority of Y1 family inc.	-0.014*	-0.002	-0.002	-0.013**	-0.005	-0.003
	(0.008)	(0.003)	(0.002)	(0.006)	(0.004)	(0.002)
Mean of outcome (wage inc. not majority of y1 family inc.)	0.197	0.020	0.009	0.026	0.013	0.005
Observations	83,908	83,908	83,908	32,502	32,502	32,502

Notes: Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis sample is prime-age all MEPS participants in panels 4-onward. Regression is estimated using pooled data from 1996-2017.

Table A.3: Main regression results: Independent variable = ever displaced

	Panel A: Opioid Count Outcomes			Panel B: MME per Day Outcomes		
	Ever used opds (1)	Rcvd. ≥ 6 opd. prsc. (2)	Rcvd. ≥ 12 opd. prsc. (3)	Ever 60+ MME/day (4)	Ever 90+ MME/day (5)	Ever 120+ MME/day (6)
Section 1. Baseline						
Ever displaced	-0.008	-0.001	-0.002**	0.004	0.004	-0.003*
	(0.005)	(0.002)	(0.001)	(0.005)	(0.004)	(0.001)
Mean of outcome	0.207	0.022	0.010	0.031	0.016	0.006
Section 2. Heterogeneity by occupation						
Blue-collar	-0.020**	-0.000	-0.001	0.010	0.017*	0.001
	(0.009)	(0.003)	(0.002)	(0.009)	(0.009)	(0.003)
Mean of outcome (blue-collar)	0.184	0.022	0.011	0.030	0.020	0.007
White-collar	-0.003	-0.001	-0.002**	0.002	-0.000	-0.004**
	(0.007)	(0.002)	(0.001)	(0.006)	(0.004)	(0.002)
Mean of outcome (white-collar)	0.207	0.022	0.010	0.031	0.016	0.006
Section 3. Heterogeneity by race/ethnicity						
Non-Hisp. white	-0.018**	-0.001	-0.001	0.006	0.005	-0.003
	(0.008)	(0.002)	(0.001)	(0.008)	(0.006)	(0.002)
Mean of outcome (non-hisp. white)	0.257	0.033	0.016	0.048	0.024	0.009
Not non-hisp. white	0.009	-0.000	-0.002**	0.001	0.002	-0.002
	(0.006)	(0.002)	(0.001)	(0.004)	(0.003)	(0.001)
Mean of outcome (not non-hisp. white)	0.207	0.022	0.010	0.031	0.016	0.006
Section 4. Heterogeneity by whether individual did not work for at least one period						
At lst 1 pd did not work	-0.011	-0.008	-0.006**	0.007	-0.003	-0.008
	(0.013)	(0.005)	(0.003)	(0.011)	(0.008)	(0.005)
Mean of outcome (at lst 1 pd did not work)	0.211	0.026	0.012	0.036	0.016	0.007
Worked all ref pds	-0.009	-0.001	-0.001	0.004	0.006	-0.003*
	(0.006)	(0.002)	(0.001)	(0.006)	(0.005)	(0.002)
Mean of outcome (worked all ref pds)	0.207	0.022	0.010	0.031	0.016	0.006
Section 5. Heterogeneity by whether dwelling unit has business/trust income						
Family has biz/trust inc.	-0.011	-0.001	0.002	0.005	0.008	0.003
	(0.011)	(0.003)	(0.002)	(0.009)	(0.007)	(0.004)
Mean of outcome (family has biz/trust inc.)	0.211	0.017	0.009	0.033	0.018	0.005
No fam biz/trust inc.	-0.007	-0.000	-0.003***	0.004	0.003	-0.004***
	(0.006)	(0.002)	(0.001)	(0.006)	(0.004)	(0.001)
Mean of outcome (no fam biz/trust inc.)	0.207	0.022	0.010	0.031	0.016	0.006
Section 6. Heterogeneity by share of dwelling unit income from individuals' wage income						
Wage inc. was majority of Y1 family income	-0.005	-0.003	-0.004***	0.011	0.008	-0.004
	(0.010)	(0.003)	(0.001)	(0.012)	(0.009)	(0.003)
Mean of outcome (wage inc. was majority of y1 family income)	0.221	0.025	0.011	0.029	0.015	0.004
Wage inc. not majority of Y1 family inc.	-0.009	0.000	-0.001	0.002	0.003	-0.002
	(0.006)	(0.002)	(0.001)	(0.005)	(0.004)	(0.002)
Mean of outcome (wage inc. not majority of y1 family inc.)	0.207	0.022	0.010	0.031	0.016	0.006
Observations	78,819	78,819	78,819	30,859	30,859	30,859

Notes: Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. Regression is estimated using pooled data from 1996-2017.

Table A.4: Main regression results: Independent variable = ever laid off

	Panel A: Opioid Count Outcomes			Panel B: MME per Day Outcomes		
	Ever used opds (1)	Rcvd. ≥ 6 opd. prsc. (2)	Rcvd. ≥ 12 opd. prsc. (3)	Ever 60+ MME/day (4)	Ever 90+ MME/day (5)	Ever 120+ MME/day (6)
Section 1. Baseline						
Ever laid off	0.000	0.001	-0.001	0.009	0.010	-0.001
	(0.008)	(0.003)	(0.001)	(0.008)	(0.007)	(0.003)
Mean of outcome	0.221	0.024	0.010	0.040	0.021	0.008
Section 2. Heterogeneity by occupation						
Blue-collar	-0.012	0.004	0.001	0.030*	0.036**	0.005
	(0.012)	(0.004)	(0.002)	(0.018)	(0.018)	(0.005)
Mean of outcome (blue-collar)	0.203	0.026	0.011	0.037	0.026	0.010
White-collar	0.007	-0.001	-0.002	0.001	0.001	-0.003
	(0.010)	(0.003)	(0.002)	(0.009)	(0.006)	(0.003)
Mean of outcome (white-collar)	0.221	0.024	0.010	0.040	0.021	0.008
Section 3. Heterogeneity by race/ethnicity						
Non-Hisp. white	-0.007	-0.001	-0.000	0.013	0.014	-0.000
	(0.011)	(0.003)	(0.002)	(0.013)	(0.011)	(0.004)
Mean of outcome (non-hisp. white)	0.275	0.037	0.018	0.058	0.031	0.014
Not non-hisp. white	0.014	0.004	-0.001	0.004	0.005	-0.002
	(0.010)	(0.004)	(0.001)	(0.006)	(0.005)	(0.002)
Mean of outcome (not non-hisp. white)	0.221	0.024	0.010	0.040	0.021	0.008
Section 4. Heterogeneity by whether individual did not work for at least one period						
At lst 1 pd did not work	0.004	-0.003	-0.004	0.019	0.004	-0.005
	(0.015)	(0.006)	(0.003)	(0.015)	(0.010)	(0.005)
Mean of outcome (at lst 1 pd did not work)	0.227	0.030	0.013	0.049	0.025	0.010
Worked all ref pds	-0.003	-0.001	0.000	0.006	0.014	-0.001
	(0.010)	(0.003)	(0.002)	(0.010)	(0.009)	(0.004)
Mean of outcome (worked all ref pds)	0.221	0.024	0.010	0.040	0.021	0.008
Section 5. Heterogeneity by whether dwelling unit has business/trust income						
Family has biz/trust inc.	0.013	0.003	0.005	0.027	0.022	0.013
	(0.017)	(0.005)	(0.004)	(0.018)	(0.014)	(0.011)
Mean of outcome (family has biz/trust inc.)	0.222	0.015	0.007	0.042	0.029	0.011
No fam biz/trust inc.	-0.004	0.000	-0.002*	0.005	0.007	-0.005**
	(0.009)	(0.003)	(0.001)	(0.009)	(0.008)	(0.002)
Mean of outcome (no fam biz/trust inc.)	0.221	0.024	0.010	0.040	0.021	0.008
Section 6. Heterogeneity by share of dwelling unit income from individuals' wage income						
Wage inc. was majority of Y1 family income	-0.011	-0.007*	-0.005***	-0.002	0.006	-0.002
	(0.015)	(0.004)	(0.002)	(0.016)	(0.014)	(0.006)
Mean of outcome (wage inc. was majority of y1 family income)	0.230	0.030	0.012	0.032	0.019	0.006
Wage inc. not majority of Y1 family inc.	0.005	0.004	0.001	0.014	0.012	-0.001
	(0.009)	(0.003)	(0.002)	(0.009)	(0.008)	(0.003)
Mean of outcome (wage inc. not majority of y1 family inc.)	0.221	0.024	0.010	0.040	0.021	0.008
Observations	78,819	78,819	78,819	30,859	30,859	30,859

Notes: Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. Regression is estimated using pooled data from 1996-2017.

Table A.5: Main regression results: Independent variable = ever displaced because business dissolved or sold

	Panel A: Opioid Count Outcomes			Panel B: MME per Day Outcomes		
	Ever used opds (1)	Rcvd. ≥ 6 opd. prsc. (2)	Rcvd. ≥ 12 opd. prsc. (3)	Ever 60+ MME/day (4)	Ever 90+ MME/day (5)	Ever 120+ MME/day (6)
Section 1. Baseline						
Displaced b/c biz disslvd/sold	-0.016	-0.004	-0.003***	-0.021***	-0.014***	-0.006***
	(0.010)	(0.003)	(0.001)	(0.006)	(0.002)	(0.001)
Mean of outcome	0.211	0.021	0.010	0.025	0.011	0.006
Section 2. Heterogeneity by occupation						
Blue-collar	-0.029	-0.003	-0.005***	-0.019**	-0.009*	-0.006***
	(0.018)	(0.006)	(0.001)	(0.009)	(0.006)	(0.002)
Mean of outcome (blue-collar)	0.201	0.021	0.010	0.036	0.020	0.010
White-collar	-0.011	-0.004	-0.002	-0.021***	-0.016***	-0.006***
	(0.012)	(0.003)	(0.001)	(0.007)	(0.002)	(0.001)
Mean of outcome (white-collar)	0.211	0.021	0.010	0.025	0.011	0.006
Section 3. Heterogeneity by race/ethnicity						
Non-Hisp. white	-0.021	-0.003	-0.003**	-0.024***	-0.016***	-0.008***
	(0.013)	(0.004)	(0.001)	(0.008)	(0.003)	(0.002)
Mean of outcome (non-hisp. white)	0.253	0.033	0.014	0.041	0.019	0.008
Not non-hisp. white	-0.006	-0.006***	-0.002	-0.016***	-0.011***	-0.004***
	(0.013)	(0.002)	(0.002)	(0.004)	(0.002)	(0.001)
Mean of outcome (not non-hisp. white)	0.211	0.021	0.010	0.025	0.011	0.006
Section 4. Heterogeneity by whether individual did not work for at least one period						
At lst 1 pd did not work	-0.027	-0.020***	-0.006*	-0.032**	-0.028***	-0.014***
	(0.024)	(0.007)	(0.004)	(0.015)	(0.008)	(0.005)
Mean of outcome (at lst 1 pd did not work)	0.213	0.027	0.015	0.031	0.003	0.003
Worked all ref pds	-0.014	-0.002	-0.002**	-0.019***	-0.012***	-0.005***
	(0.011)	(0.003)	(0.001)	(0.006)	(0.002)	(0.001)
Mean of outcome (worked all ref pds)	0.211	0.021	0.010	0.025	0.011	0.006
Section 5. Heterogeneity by whether dwelling unit has business/trust income						
Family has biz/trust inc.	-0.025	-0.004	-0.002***	-0.021***	-0.012***	-0.006***
	(0.019)	(0.004)	(0.001)	(0.008)	(0.004)	(0.002)
Mean of outcome (family has biz/trust inc.)	0.218	0.025	0.011	0.017	0.008	0.000
No fam biz/trust inc.	-0.012	-0.004	-0.003**	-0.021***	-0.015***	-0.006***
	(0.012)	(0.003)	(0.001)	(0.007)	(0.003)	(0.001)
Mean of outcome (no fam biz/trust inc.)	0.211	0.021	0.010	0.025	0.011	0.006
Section 6. Heterogeneity by share of dwelling unit income from individuals' wage income						
Wage inc. was majority of Y1 family income	-0.027	-0.004	-0.005***	-0.011	-0.017***	-0.007***
	(0.018)	(0.005)	(0.001)	(0.017)	(0.004)	(0.002)
Mean of outcome (wage inc. was majority of y1 family income)	0.200	0.015	0.003	0.022	0.006	0.006
Wage inc. not majority of Y1 family inc.	-0.011	-0.004	-0.002	-0.024***	-0.013***	-0.006***
	(0.012)	(0.003)	(0.001)	(0.004)	(0.003)	(0.001)
Mean of outcome (wage inc. not majority of y1 family inc.)	0.211	0.021	0.010	0.025	0.011	0.006
Observations	78,819	78,819	78,819	30,859	30,859	30,859

Notes: Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. Regression is estimated using pooled data from 1996-2017.

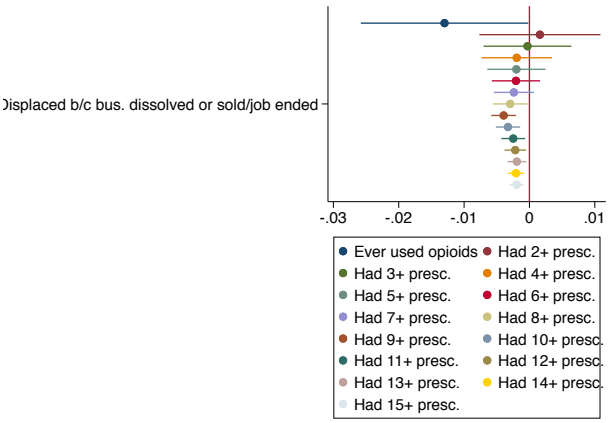
Table A.6: Heterogeneity by round one pain, all displacement types

	Panel A: Opioid Count Outcomes			Panel B: MME per Day Outcomes		
	Ever used opds (1)	Rcvd. ≥ 6 opd. prsc. (2)	Rcvd. ≥ 12 opd. prsc. (3)	Ever 60+ MME/day (4)	Ever 90+ MME/day (5)	Ever 120+ MME/day (6)
Section 1. Independent variable = individual ever non-layoff displaced						
Fair/poor R1 m. hlth	-0.032	-0.015	-0.013***	-0.035	-0.014	-0.010**
	(0.032)	(0.013)	(0.003)	(0.023)	(0.021)	(0.005)
Mean of outcome (fair/poor r1 m. hlth)	0.279	0.051	0.028	0.013	0.004	0.000
Good or better R1 m. hlth	-0.012*	-0.001	-0.002*	0.004	0.002	-0.004***
	(0.007)	(0.002)	(0.001)	(0.007)	(0.005)	(0.001)
Mean of outcome (good or better r1 m. hlth)	0.197	0.020	0.009	0.026	0.013	0.005
Section 2. Independent variable = individual ever displaced						
Fair/poor R1 m. hlth	-0.043*	-0.016*	-0.011***	-0.039**	-0.012	-0.011***
	(0.026)	(0.010)	(0.004)	(0.018)	(0.016)	(0.004)
Mean of outcome (fair/poor r1 m. hlth)	0.281	0.053	0.030	0.030	0.017	0.003
Good or better R1 m. hlth	-0.006	0.000	-0.001	0.006	0.005	-0.002
	(0.005)	(0.002)	(0.001)	(0.005)	(0.004)	(0.002)
Mean of outcome (good or better r1 m. hlth)	0.207	0.022	0.010	0.031	0.016	0.006
Section 3. Independent variable = individual ever laid off						
Fair/poor R1 m. hlth	-0.051	-0.018	-0.007	-0.038	-0.008	-0.011**
	(0.036)	(0.011)	(0.006)	(0.024)	(0.021)	(0.005)
Mean of outcome (fair/poor r1 m. hlth)	0.278	0.051	0.030	0.057	0.036	0.007
Good or better R1 m. hlth	0.003	0.002	-0.000	0.012	0.011	-0.001
	(0.008)	(0.003)	(0.001)	(0.008)	(0.007)	(0.003)
Mean of outcome (good or better r1 m. hlth)	0.221	0.024	0.010	0.040	0.021	0.008
Observations	78,819	78,819	78,819	30,859	30,859	30,859

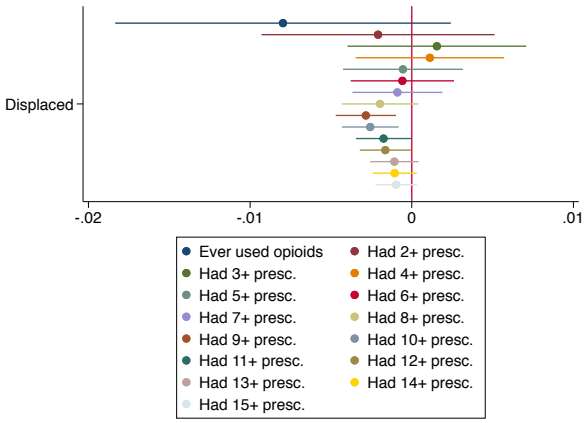
Notes: Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. Regression is estimated using pooled data from 1996-2017.

Figure A.3: Baseline Regression Results of Regression of All Prescription Count Indicators on Displacement

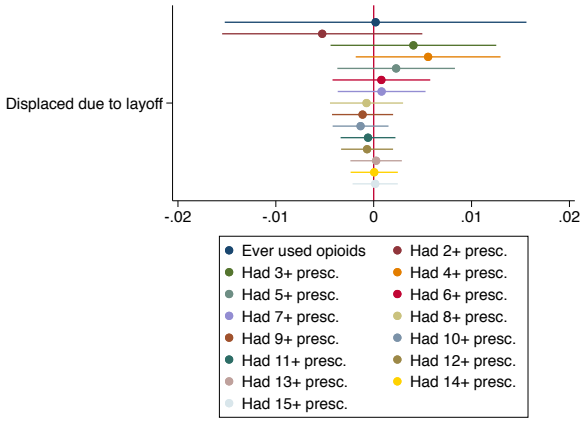
(a) Indicator for experiencing non-layoff displacement



(b) Indicator for experiencing displacement



(c) Indicator for experiencing a layoff



Notes: These figures plot regression estimates and 95% confidence intervals from the displacement coefficient in equation 1. Standard errors are robust to heteroskedasticity. Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Estimates are computed using survey weights. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. Regression is estimated using pooled data from 1996-2017.

Table A.7: Heterogeneity by self-reported round one mental health status, all displacement types

	Panel A: Opioid Count Outcomes			Panel B: MME per Day Outcomes		
	Ever used opds (1)	Rcvd. ≥ 6 opd. prsc. (2)	Rcvd. ≥ 12 opd. prsc. (3)	Ever 60+ MME/day (4)	Ever 90+ MME/day (5)	Ever 120+ MME/day (6)
Section 1. Independent variable = individual ever non-layoff displaced						
Rcvd. presc. for antidepressant/antipsychotic in R1	-0.006	-0.004	-0.009**	0.013	-0.021	-0.013***
	(0.032)	(0.013)	(0.004)	(0.034)	(0.015)	(0.004)
Mean of outcome (rcvd. presc. for antidepressant/antipsychotic in r1)	0.355	0.082	0.034	0.059	0.022	0.005
No R1 antidep./antipsy. presc.	-0.013**	-0.002	-0.002*	0.001	0.003	-0.003***
	(0.007)	(0.002)	(0.001)	(0.006)	(0.005)	(0.001)
Mean of outcome (no r1 antidep./antipsy. presc.)	0.197	0.020	0.009	0.026	0.013	0.005
Section 2. Independent variable = individual ever displaced						
Rcvd. presc. for antidepressant/antipsychotic in R1	0.007	0.002	-0.008*	0.013	-0.021	-0.006
	(0.026)	(0.011)	(0.004)	(0.028)	(0.014)	(0.010)
Mean of outcome (rcvd. presc. for antidepressant/antipsychotic in r1)	0.371	0.090	0.038	0.072	0.031	0.010
No R1 antidep./antipsy. presc.	-0.009*	-0.001	-0.001	0.003	0.006	-0.002*
	(0.005)	(0.002)	(0.001)	(0.005)	(0.004)	(0.001)
Mean of outcome (no r1 antidep./antipsy. presc.)	0.207	0.022	0.010	0.031	0.016	0.006
Section 3. Independent variable = individual ever laid off						
Rcvd. presc. for antidepressant/antipsychotic in R1	0.020	0.010	-0.006	0.007	-0.020	0.005
	(0.039)	(0.018)	(0.007)	(0.044)	(0.023)	(0.023)
Mean of outcome (rcvd. presc. for antidepressant/antipsychotic in r1)	0.382	0.098	0.042	0.096	0.053	0.018
No R1 antidep./antipsy. presc.	-0.001	0.000	-0.000	0.009	0.013*	-0.002
	(0.008)	(0.002)	(0.001)	(0.008)	(0.007)	(0.002)
Mean of outcome (no r1 antidep./antipsy. presc.)	0.221	0.024	0.010	0.040	0.021	0.008
Observations	78,819	78,819	78,819	30,859	30,859	30,859

Notes: Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. Regression is estimated using pooled data from 1996-2017.

Table A.8: Heterogeneity by receipt of antidepressant/antipsychotic in round one, all displacement types

	Panel A: Opioid Count Outcomes			Panel B: MME per Day Outcomes		
	Ever used opds (1)	Rcvd. ≥ 6 opd. prsc. (2)	Rcvd. ≥ 12 opd. prsc. (3)	Ever 60+ MME/day (4)	Ever 90+ MME/day (5)	Ever 120+ MME/day (6)
Section 1. Independent variable = individual ever non-layoff displaced						
Had R1 pain	-0.047***	-0.023***	-0.012***	0.010	-0.003	-0.006**
	(0.018)	(0.005)	(0.002)	(0.016)	(0.010)	(0.003)
Mean of outcome (had r1 pain)	0.303	0.062	0.033	0.041	0.018	0.006
No R1 pain	-0.005	0.003	0.000	-0.002	0.003	-0.003***
	(0.007)	(0.002)	(0.001)	(0.006)	(0.005)	(0.001)
Mean of outcome (no r1 pain)	0.197	0.020	0.009	0.026	0.013	0.005
Section 2. Independent variable = individual ever displaced						
Had R1 pain	-0.023	-0.017***	-0.007***	0.011	0.006	-0.006**
	(0.014)	(0.004)	(0.002)	(0.013)	(0.009)	(0.003)
Mean of outcome (had r1 pain)	0.329	0.066	0.035	0.052	0.025	0.008
No R1 pain	-0.004	0.003*	-0.000	0.001	0.003	-0.001
	(0.006)	(0.002)	(0.001)	(0.005)	(0.004)	(0.002)
Mean of outcome (no r1 pain)	0.207	0.022	0.010	0.031	0.016	0.006
Section 3. Independent variable = individual ever laid off						
Had R1 pain	0.010	-0.010	-0.001	0.019	0.028	-0.005
	(0.021)	(0.007)	(0.005)	(0.021)	(0.020)	(0.004)
Mean of outcome (had r1 pain)	0.365	0.071	0.035	0.068	0.038	0.012
No R1 pain	-0.002	0.003	-0.000	0.005	0.003	0.001
	(0.008)	(0.003)	(0.001)	(0.007)	(0.005)	(0.003)
Mean of outcome (no r1 pain)	0.221	0.024	0.010	0.040	0.021	0.008
Observations	78,819	78,819	78,819	30,859	30,859	30,859

Notes: Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. Regression is estimated using pooled data from 1996-2017.

Table A.9: Heterogeneity by bachelor's degree receipt

	Panel A: Opioid Count Outcomes			Panel B: MME per Day Outcomes		
	Ever used opds (1)	Rcvd. ≥ 6 opd. prsc. (2)	Rcvd. ≥ 12 opd. prsc. (3)	Ever 60+ MME/day (4)	Ever 90+ MME/day (5)	Ever 120+ MME/day (6)
Section 1. Independent variable = individual ever non-layoff displaced						
Bachelor's degree+	-0.020*	0.002	-0.001	-0.005	-0.008**	-0.005***
	(0.011)	(0.003)	(0.001)	(0.009)	(0.004)	(0.001)
Mean of outcome (bachelor's degree+)	0.207	0.020	0.008	0.029	0.015	0.009
No bachelors' degree	-0.009	-0.004*	-0.003**	0.006	0.007	-0.003*
	(0.008)	(0.002)	(0.001)	(0.009)	(0.007)	(0.002)
Mean of outcome (no bachelors' degree)	0.197	0.020	0.009	0.026	0.013	0.005
Section 2. Independent variable = individual ever displaced						
Bachelor's degree+	-0.005	0.004	0.000	0.004	-0.001	-0.000
	(0.009)	(0.003)	(0.001)	(0.008)	(0.005)	(0.003)
Mean of outcome (bachelor's degree+)	0.212	0.022	0.010	0.031	0.014	0.008
No bachelors' degree	-0.009	-0.003	-0.003***	0.004	0.007	-0.004***
	(0.006)	(0.002)	(0.001)	(0.007)	(0.005)	(0.001)
Mean of outcome (no bachelors' degree)	0.207	0.022	0.010	0.031	0.016	0.006
Section 3. Independent variable = individual ever laid off						
Bachelor's degree+	0.023	0.007	0.003	0.017	0.010	0.007
	(0.015)	(0.005)	(0.003)	(0.015)	(0.010)	(0.008)
Mean of outcome (bachelor's degree+)	0.223	0.026	0.014	0.031	0.013	0.007
No bachelors' degree	-0.010	-0.002	-0.002*	0.006	0.010	-0.005***
	(0.009)	(0.003)	(0.001)	(0.010)	(0.009)	(0.002)
Mean of outcome (no bachelors' degree)	0.221	0.024	0.010	0.040	0.021	0.008
Observations	78,819	78,819	78,819	30,859	30,859	30,859

Notes: Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. Regression is estimated using pooled data from 1996-2017.

B Classifying opioid prescriptions in the MEPS prescribed medicines files

B.1 Previous efforts to classify opioids in the MEPS

A variety of papers have attempted to classify prescriptions in the MEPS prescribed medicines files. Prescriptions might be classified as opioid prescriptions by three criteria, namely (1) the non-proprietary name of the drug prescribed (Soni, 2018; Zhan et al., 2001), (2) the therapeutic class variable associated with the prescription (Soni, 2018; Moriya and Miller, 2018*a*; Moriya and Miller, 2018*b*; Stagnitti, 2017; Groenewald et al., 2016), or (3) using National Drug Codes to match prescription records in the MEPS to a CDC database listing National Drug Codes for all prescription opioids available in the United States (Soni, 2018; Zhou, Florence, and Dowell, 2016). The first approach amounts to testing whether each non-proprietary name contains any of the strings butorphanol, codeine, dihydrocodeine, fentanyl, hydrocodone, hydromorphone, levorphanol, meperidine, morphine, nalbuphine, opium, oxycodone, oxymorphone, pentazocine, propoxyphene, tapentadol, or tramadol (note the omission of methadone and buprenorphine, which are used in drug-assisted therapy to wean individuals off illicit opioids). The second approach amounts to using variables imputed by Multum Lexicon for all prescription records in the MEPS prescribed medicines files to check whether the therapeutic class associated with a prescription is “narcotic analgesic" or “narcotic analgesic combination." The third approach amounts to merging MEPS prescribed medicines files with a CDC database of National Drug Codes (and other information) associated with prescription opioids currently available in the United States and counting prescriptions as opioids if the National Drug Codes given for them in the MEPS prescribed medicines files match to National Drug Codes in the CDC database (CDC, 2018*c*).[10]

For a variety of reasons, none of the above methods are foolproof. Counting opioid prescriptions based on their non-proprietary names is faulty insofar as the names associated with prescription records in the MEPS prescribed medicines files are rife with misspellings and proprietary names.[11] Classifying opioids based on therapeutic class variables is unreliable because some prescription records whose non-proprietary names would suggest them being opioids are classified under therapeutic categories other than “narcotic analgesic" or “narcotic analgesic combination" and, correspondingly, some prescription records whose therapeutic class is "narcotic analgesic" or "narcotic analgesic combination" have names which suggest that they are not opioid prescriptions. Finally, counting opioid prescriptions using National Drug Codes is unreliable because many prescriptions in the MEPS files whose names would indicate that they are opioid prescriptions do not merge with the aforementioned CDC database, suggesting data entry errors in National Drug Code variables in the MEPS.

All of these shortcomings of the data are noted by Soni (2018), who I follow in using a combination of all three measures to classify opioid prescriptions.

B.2 My strategy for classifying opioids in the MEPS

My process for classifying drugs as opioids is as follows:

1. Using Multum-Lexicon (ML) drug name variables, classify a prescription as being a potential opioid if the capitalized ML drug name contains any of the following strings: “BUTORPHANOL", “CODEINE", “DIHYDROCODEINE", “FENTANYL", “HYDROCODONE", “HYDROMORPHONE", “LEVORPHANOL", “MEPERIDINE", “MORPHINE", “NALBUPHINE", “OPIUM", “OXYCODONE", “OXYMORPHONE", “PENTAZOCINE",

[10] This list can be accessed in spreadsheet at `https://www.cdc.gov/drugoverdose/resources/data.html` within the Data Files box.
[11] As Soni (2018) notes, “the drug name ‘Acetaminophen’ is spelled almost 70 different ways in the MEPS files.”

"PROPOXYPHENE", "TAPENTADOL", or "TRAMADOL."[12] Using this strategy, I classify 269,549 prescriptions (or 4.29% of all drug prescriptions in the Prescribed Medicines files from 1996-2017) as potential opioids.

2. Clean national drug codes (NDCs) in Prescribed Medicines records by removing non-numeric characters. Match prescriptions in dataset to CDC spreadsheet of opioids using NDC, and classify drug as a potential opioid if it matches to CDC successfully. Exclude matches whose non-proprietary name contains one of the strings "BUPRENORPHINE" or "METHADONE." Using this strategy, I classify 223,250 prescriptions (or 3.55% of all drug prescriptions in the Prescribed Medicines files from 1996-2017) as potential opioids.

3. Create a list of misspellings of opioid drug names and proprietary names in the main drug name variable in the Prescribed Medicines file. Compile a spreadsheet of these incorrect or proprietary names, and add two fields: one for correct proprietary name and another for opioid component. Then merge in this spreadsheet, creating a corrected version of opioid names, and classify as a potential opioid if the correct name matches any of the strings enumerated in step 1. Using this strategy, I classify 267,698 prescriptions (or 4.26% of all drug prescriptions in the Prescribed Medicines files from 1996-2017) as potential opioids.

4. Classify a prescription as an opioid if it is classified as a potential opioid under at least two of the three schemas above. Count 268,644 opioid prescriptions, or 4.28% of all prescriptions in the Prescribed Medicines files from 1996-2017.

B.3 My strategy for computing MME per day for prescriptions in the MEPS

For some of my analysis, I am interested in computing the strengths of opioid prescriptions in morphine milligrams equivalent (MME) per day. The advantage of computing MME per day for prescriptions is that it allows for comparing individuals' opioid use in apples to apples terms. MME per day is computed as:

$$\text{MME per day} = \frac{\text{Opioid component strength} \times \text{MME conversion factor} \times \text{Quantity of medication prescribed}}{\text{Days supply of medication}}$$

where MME conversion factors are well-known quantities published by the CDC, generally specific to each type of drug but sometimes specific to the form of the drug. These are shown for each drug in table B.1

The main challenge for computing MME per day for each prescription is finding an accurate opioid component strength associated with each prescription. While relatively few observations lack strength measurements, the data in the strength field is often messy: for instance, there may be more strength measurements in the prescription strength variable than there are components of the drug, the drug strength may be coded as "999999" or "9999" in place of missing, or drug component strengths may be appended together. For drugs I classify as opioid based on their having matched to the CDC catalogue (these make up 83.1% of drugs I classify as opioids), I use the opioid component strength associated with that drug as listed in the CDC catalogue. For drugs which do not match to the CDC catalogue, I use the following strategy to find an accurate opioid strength for the prescription:

1. Clean drug name and drug strength fields as much as possible, so that missing values for drug strengths are all coded as blanks, abbreviations for drug components are replaced with full names (e.g. "APAP" becomes

[12] I take care, however, not to count prescriptions whose drug names contain the string "TROPIUM" as opioids, given that a relatively common asthma inhaler medication, ipratropium bromide, contains the string "OPIUM". I also check whether using the therapeutic class variables added by Multum Lexicon add any additional information, but I am not able to classify any prescriptions as potential opioids using the therapeutic class variables that I had not already caught using Multum Lexicon drug names.

Table B.1: MME Conversion Factors

Opiate component	Drug form	Conversion factor	Converting from
Butorphanol	–	7	Milligrams
Codeine	–	0.15	Milligrams
Dihydrocodeine	–	0.25	Milligrams
Fentanyl	Tablets	0.13	Micrograms
Fentanyl	Lozenge	0.13	Micrograms
Fentanyl	Oral Spray	0.18	Micrograms
Fentanyl	Film	0.18	Micrograms
Fentanyl	Nasal Spray	0.16	Micrograms
Fentanyl	Patch	7.2	Micrograms/hour
Fentanyl	Injection	300	Milligrams
Hydrocodone	–	1	Milligrams
Hydromorphone	–	4	Milligrams
Levorphanol	–	11	Milligrams
Meperidine	–	0.1	Milligrams
Morphine	–	1	Milligrams
Nalbuphine	–	–	Milligrams
Opium	–	1	Milligrams
Oxycodone	–	1.5	Milligrams
Oxymorphone	–	3	Milligrams
Propoxyphene	–	0.23	Milligrams
Pentazocine	–	0.37	Milligrams
Tapentadol	–	0.4	Milligrams
Tramadol	–	0.1	Milligrams

Conversion factors are sourced from the Oral MME - Excel Data File Summary Table sheet, retrieved from `https://www.cdc.gov/drugoverdose/resources/data.html` within the Data Files box (CDC, 2018*c*).

“ACETAMINOPHEN"), and drug names of different components of a drug are separated by a slash (e.g. “ACETAMINOPHEN-CODEINE" becomes “ACETAMINOPHEN/CODEINE").

2. Using IBM Micromedex drug database and the FDA Orangebook drug database, I make a list of every possible opioid drug strength associated with each drug for the list of prescription records which I am unable to match to the CDC opioid catalogue based on NDCs (IBM Red Book, 2019; United States Food and Drug Food and Drug Administration, 2019).

3. Split the drug name and strength variables association with each prescription by component, if it is possible to separate these fields. For instance, a prescription record with drug name “ACETAMINOPHEN/CODEINE" and strength “120/12.5" now has drug name #1 “ACETAMINOPHEN", drug strength #1 “120", drug name #2 “CODEINE" and drug strength #2 “12.5"

4. Match prescription records in the MEPS to the IBM Micromedex Red Book/FDA Orange Book list of all possible opioid strengths by drug component.

5. Cycle through the drug strength variables created in step 3 and create a list of “exact matches," namely instances in which one of the split drug strength variables matches a possible opioid strength according to Micromedex and/or the FDA Orange Book. No drug has more than three “exact matches."

   (a) For drugs which are liquids (as determined by the form variable in the prescribed medicines files), eliminate an exact match to a 5 MG strength, as many drug strengths for liquids in the MEPS are reported as drug strength in milligrams per 5 ML. If there is only one remaining exact match for drug strength, assign this value as the drug’s opioid component strength. If there are two remaining exact matches after cancelling the 5 MG exact match, use the smaller of the two measurements.

   (b) For non-liquid drugs, there are at most two exact matches. Use the smaller of these two.

6. Cycle through drug strength variables created in step 3 and create a list of “partial matches", namely instances in which one of the possible opioid strengths according to Micromedex and/or the FDA Orange Book is a substring of one of the split prescription strength variables. No drug has more than two exact matches. If there are two matches, take the lesser of the two. If there is one, assign that partial-matched drug strength to be the opioid component strength of the drug. After this step previous two steps, I will have imputed opioid component strengths for roughly 58% of the prescription records which did not match to the CDC opioid catalogue.

7. For the remaining opioids without an imputed opioid strength, assign the lowest possible opioid component strength for that drug combination. After this step, I am able to assign an opioid strength to 99.98% of the opioids I identify in the Prescribed Medicines files. The prescription records which I am unable to assign an imputed opioid strength have the following components:

   - Chlorpheniramine, codeine, phenylephrine, and potassium iodide
   - Codeine, diphenhydramine, and phenylephrine
   - Dexbrompheniramine, hydrocodone, and phenylephrine
   - Hydrocodone, pheniramine, phenylephrine, phenylpropanolamine, and pyrilamine

   I am unable to find drug strengths for drugs made up of these component combinations in either the IBM Micromedex database or the FDA Orange Book (IBM Red Book, 2019; United States Food and Drug Food and Drug Administration, 2019).

C Regression results on full set of indicators for exceeding opioid count thresholds, conditioning only on round one health status

The table and figures in this section reproduce my main results in specifications which condition on round one health status only, as opposed to the indicators enumerated in section 4 of table 1, which are indicators for survey participants ever experiencing a health issue over the course of their survey participation. As referenced in subsection 3.5, not all health status questions are asked in every round of interviews; as such, restricting myself to controlling only for round one health status prevents me from controlling for the entire gamut of health status conditions enumerated in section 4 of 1. In particular, the tables and figures shown below will give estimates from regressions controlling for the following round one health characteristics:

1. Reported using assistive device
2. Reported complete inability to do activity
3. Reported general physical difficulty
4. Reported physical difficulty impeding work
5. Reported joint pain
6. Reported difficulty bending/stooping
7. Reported difficulty grasping with fingers
8. Reported difficulty walking one mile
9. Reported difficulty reaching overhead
10. Reported difficulty standing 20 minutes
11. Reported difficulty walking 3 blocks
12. Reported fair of poor round one mental health status
13. Received a prescription for an antidepressant or antipsychotic in round one, according to the prescription's therapeutic category.

While many of the point estimates of associations between displacement and opioid use are greater under these specifications than under my main specifications, only three of the positive coefficients are statistically distinguishable from zero (laid off workers are significantly more likely to receive three or four opioid prescriptions at the $p = 0.01$ threshold, and are marginally significantly more likely to receive a 90 MME per day prescription).

These results are difficult to interpret: on one hand, these estimates may suggest that, while displacement by and large appears to have no statistically significant effect on workers' likelihood of opioid abuse, it may slightly increase the likelihood of opioid use among laid off individuals if we do not control for negative health effects of being laid off. On the other hand, these results could be explained by increased omitted variables bias and selection bias relative to my main specification. In particular, as discussed above I am able to control for fewer health status variables in these specifications, introducing the possibility of greater omitted variables bias. Failing to control adequately for health conditions correlated with individuals' likelihood of both

experiencing displacement and beginning to use opioids will likely bias my estimates of the effect of displacement on likelihood of opioid use upward. Furthermore, failing to condition on the full set of available health status variables increases the likelihood that individuals are selected into displacement on un heobservedalth dimensions correlated with greater opioid use. In light of the additional identification challenges associated with these specifications, I find the results here worth noting but ultimately less reliable than my main results.

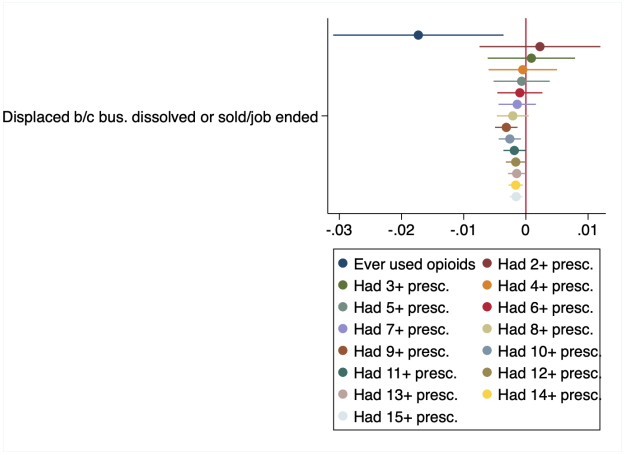
Table C.1: Baseline regression results, controlling only for round one health status, all displacement types

	Panel A: Opioid Count Outcomes			Panel B: MME per Day Outcomes		
	Ever used opds (1)	Rcvd. ≥ 6 opd. prsc. (2)	Rcvd. ≥ 12 opd. prsc. (3)	Ever 60+ MME/day (4)	Ever 90+ MME/day (5)	Ever 120+ MME/day (6)
Section 1. Independent variable = individual ever displaced						
Ever displaced	-0.004	0.002	-0.001	0.006	0.005	-0.002
	(0.006)	(0.002)	(0.001)	(0.005)	(0.004)	(0.001)
Mean of outcome	0.207	0.022	0.010	0.031	0.016	0.006
Section 2. Independent variable = individual ever displaced						
Ever laid off	0.014	0.004	0.001	0.013	0.013*	-0.000
	(0.008)	(0.003)	(0.001)	(0.008)	(0.007)	(0.003)
Mean of outcome	0.221	0.024	0.010	0.040	0.021	0.008
Section 3. Independent variable = individual ever non-layoff displaced						
Ever non-layoff displaced	-0.017**	-0.001	-0.002**	0.002	0.002	-0.004***
	(0.007)	(0.002)	(0.001)	(0.007)	(0.005)	(0.001)
Mean of outcome	0.197	0.020	0.009	0.026	0.013	0.005
Observations	78,819	78,819	78,819	30,859	30,859	30,859

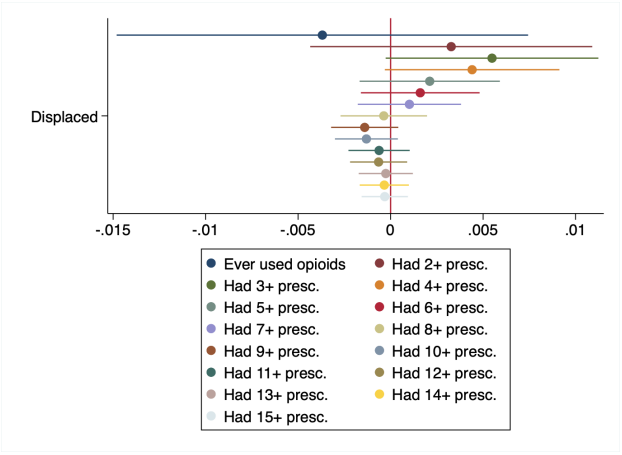
Notes: Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, dates of participation in the survey, and round one health status for the subset of issues enumerated above. Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. Regression is estimated using pooled data from 1996-2017.

Figure C.1: Baseline regression results of regression of all prescription count indicators on displacement, controlling only for round one health status

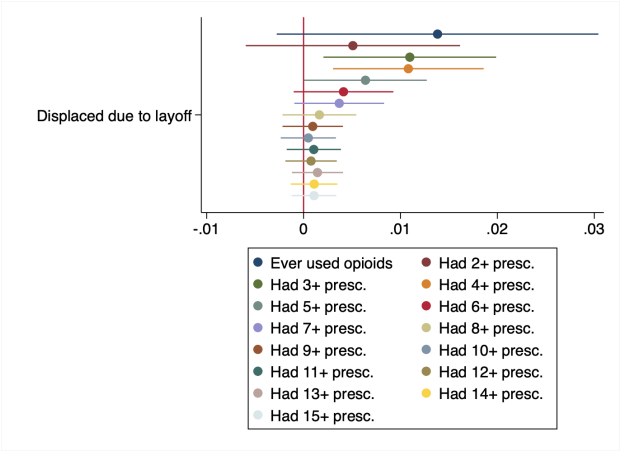
(a) Indicator for experiencing non-layoff displacement



(b) Indicator for experiencing displacement



(c) Indicator for experiencing a layoff



Notes: These figures plot regression estimates and 95% confidence intervals from the displacement coefficient in equation 1. Standard errors are robust to heteroskedasticity. Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, dates of participation in the survey, and round one health status for the subset of issues enumerated above. Estimates are computed using survey weights. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. Regression is estimated using pooled data from 1996-2017.