

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

Dustin Swonder

Princeton University

PRELIMINARY AND IN PROGRESS

Abstract

I investigate 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 use thresholds, at which point displacement is associated with modest reductions in workers' likelihood of prescription opioid use. My results suggest that any increases in opioid use caused by poor local labor market conditions are mediated by increased opioid supply associated with economic hardship rather than increased opioid demand among affected workers. Absent such effects, the strong cross-sectional relationship between labor market hardship and opioid use is likely explained by opioid use causing labor market hardship, rather than vice versa.

First version: December 7, 2019. This version: January 18, 2020. [Click for latest version.](#)

Keywords: Opioids; opioid abuse; deaths of despair

JEL: H51, I12, I18

^{*}I am very grateful to David Cutler and Alex Albright, who read numerous drafts of this article, and to Anne Hall, who nudged me towards this research question and who provided invaluable feedback at various stages of the project. Thanks also to Coly Elhai and Ian Sapollnik for helpful comments. and to seminar participants at the U.S. Department of the Treasury Office of Economic Policy. Views expressed herein are the views of the author only; they do not represent the views of the U.S. Department of Health and Human Services, Agency for Healthcare Research and Quality, or the Medical Expenditure Panel Survey. I declare that I have no relevant or material financial interests related to the research described in this paper. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

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 roughly 68% of the 70,200 drug overdose deaths in 2017 involved opioids, amounting to over 130 overdose deaths per day due to opioids (Centers for Disease Control and Prevention, 2018a and National Institute on Drug Abuse, 2019). Importantly, data from these agencies also show that poorer, whiter regions of the country are some of the hardest-hit by upticks in opioid deaths; for instance, according to the Centers for Disease Control and Prevention (CDC), opioid overdoses increased 70% from July 2016 to September 2017 in the Midwest (see Centers for Disease Control and Prevention, 2018a). This relationship between local economic distress and drug deaths has led some scholars to propose that poor local labor market conditions may be an important determinant of demand for opioids in these distressed regions. Case and Deaton (2017) have ventured as far as postulating that poor local labor market conditions in some regions of the United States are an important driver of increases in the midlife mortality rate for non-Hispanic whites in the United States since the late 1990s.

Case and Deaton's (2017) suggestion has spurred a flurry of research investigating the relationship between labor market conditions and opioid abuse in the United States (Venkataramani et al., 2019; Metcalf and Wang, 2019; Charles, Hurst, and Schwartz, 2018; Currie, Jin, and Schnell, 2018; Ruhm, 2019; Hollingsworth, Ruhm, and Simon, 2017; Krueger, 2017; Aliprantis and Schweitzer, 2018; Harris et al., 2018; Laird and Nielsen, 2017). The subset of studies concerned with whether despair among workers due to worsening labor market conditions induces opioid use have been unable to definitively answer this question. While Venkataramani et al. (2019) and Charles, Hurst, and Schwartz (2018) argue that manufacturing-related labor market dislocations may increase demand for opioids in affected counties, Metcalf and Wang (2019), Ruhm (2019) and Currie, Jin, and Schnell (2018) are all dubious.

Notably, Case and Deaton (2017) argue that worsening labor market conditions cause individuals to demand more opioids. Because Case and Deaton's proposal 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 prescription opioid 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 MEPS data, which enables me to understand both job displacement and opioid use over time at the individual level.

This study leverages Medical Expenditure Panel Survey (MEPS) data to provide the first demand-side estimates of the effect of labor market dislocations on opioid use in the United States. I focus on prime-age individuals who experience non-layoff job displacement, which I define as losing a job due to place of employment dissolving or being sold or term of employment ending. My sample includes only individuals who were employed during the first period of their survey participation and who did not use opioids during this period. I then estimate a linear probability model in which my independent variable of interest is an indicator for experiencing non-layoff job displacement and my dependent variables are indicators for exceeding various thresholds opioid use in terms of number of opioid prescriptions and morphine milligram equivalent (MME) dosage per day. The detailed health status information collected by MEPS survey administrators allows me to condition richly on health conditions correlated with opioid use as well as demographic characteristics and pre-displacement industry and occupation. I argue that simultaneity and omitted variables bias under my main specification, if present, are likely positive, which would imply that the associations I measure are upper bounds on the effects of displacement on prescription opioid abuse.

I show that non-layoff job displacement is not associated with U.S. prime-age workers' propensity to use opioids conditional on industry, occupation, health status, and demographic characteristics, except for at high thresholds of use, at which point non-layoff displacement appears to be associated with slightly lower likelihood of opioid use. Importantly, I do not find evidence that non-layoff displacement is associated with particularly strong increases in likelihood of opioid among non-Hispanic whites or individuals working in blue-collar occupations during their first reference period of survey participation. This casts some doubt on Case and Deaton's (2015; 2017) hypothesis that labor market dislocations are particularly important drivers of increasing mortality among blue-collar non-Hispanic whites.

Displacement-related income reductions appear to be the mechanism driving reductions in probability of high-threshold opioid use associated with displacement. In particular, I find that among individuals whose displacement is less likely to have caused financial hardship – individuals who were employed for the duration of their survey participation, individuals whose labor income did not make up the majority of their dwelling unit's income in their first year of MEPS participation, and individuals whose dwelling units accrued business or trust income in the first year of MEPS 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.

In sum, my results 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 are likely driven by increases in opioid supply associated with labor market dislocations, or increased opioid demand increases associated with longer-term labor market dislocations than I am able to observe, such as multiple job displacements over a longer period of time.

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 deaths due to drug or alcohol overdoses. Case and Deaton's (2017) follow-up paper suggests that worsening economic circumstances for middle-aged non-Hispanic whites have contributed to rising poisoning deaths. In proposing that worsening economic conditions have contributed to increasing "deaths of despair" by increasing workers' opioid use, Case and Deaton (2017) highlight long-term changes in economic conditions, for instance, fewer opportunities in the labor market for blue-collar workers from generation to generation. This proposition is difficult to investigate causally, as Case and Deaton (2017) readily concede. Nevertheless, several studies have now examined whether labor market dislocations induce opioid use (Venkataramani et al., 2019; Metcalf and Wang, 2019; Charles, Hurst, and Schwartz, 2018; Currie, Jin, and Schnell, 2018; Ruhm, 2019; Hollingsworth, Ruhm, and Simon, 2017; Roulet, 2017), whether opioid use induces labor market inactivity (Krueger, 2017; Currie, Jin, and Schnell, 2018; Aliprantis and Schweitzer, 2018; Harris et al., 2018; Torbin and Nielsen, 2017), and the social determinants of opioid use in general (e.g. Finkelstein, Gentzkow, and Williams, 2018).

The only existing study which directly measures the effects of job displacement on individual opioid demand is Roulet (2017), who exploits individual-level employment and healthcare utilization data from Denmark

to investigate whether job displacement induces greater prescription opioid use.¹ 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 and foremost, 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, in general, generous unemployment insurance policies prevent large reductions in healthcare spending associated with job displacement. These two differences suggest that two most obvious determinants of post-displacement prescription opioid use or abuse – despair and financial hardship – do not apply in Denmark to the extent that they do 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 closely resemble my own using U.S. data are Venkataramani et al. (2019), Metcalf and Wang (2019), Ruhm (2019), Currie, Jin, and Schnell (2018), and Charles, Hurst and Schwartz (2018), all of which use county-level data to study whether changes in economic circumstances cause greater opioid use. Ruhm (2019) is most faithful to the letter of Case and Deaton (2017); he seeks to identify a causal relationship between medium-run changes in local economies and opioid deaths. Specifically, Ruhm (2019) measures the effects of changes in county-level poverty rates, median home values, and a variety of other proxies for economic performance on county-level drug death rates using a variety of specifications, including fixed effects and two-stage least squares. Venkataramani et al. (2019), Metcalf and Wang (2019), Currie, Jin, and Schnell (2018), Charles, Hurst, and Schwartz (2018), on the other hand, all focus on transitory fluctuations in labor market conditions, rendering their research designs more similar to my own. Venkataramani et al. (2019) use event-study specifications to study whether auto manufacturing firm closures in manufacturing-intensive counties induce more opioid deaths. Metcalf and Wang (2019), Currie, Jin, and Schnell (2018), and Charles, Hurst, and Schwartz (2018) all use the shift-share (Bartik) instrument to measure the effect of plausibly exogenous shifts in coal-mining share of county employment, the employment-to-population ratio, and the manufacturing share of county employment, respectively, on proxies for opioid abuse (Bartik, 1991).

These studies paint conflicting pictures. Ruhm (2019) finds that worsening economic conditions may increase in the drug death rate, though he writes that economic decline accounts for no more than one tenth of the change in the drug death rate and that “a small amount of remaining omitted variables bias would be sufficient to completely eliminate the contributions of economic factors.” Venkataramani et al. (2019) find strong effects of auto plant closures on the rate of opioid overdose 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 relationships between declining manufacturing share of employment and opioid use metrics in their two-stage least squares specifications.

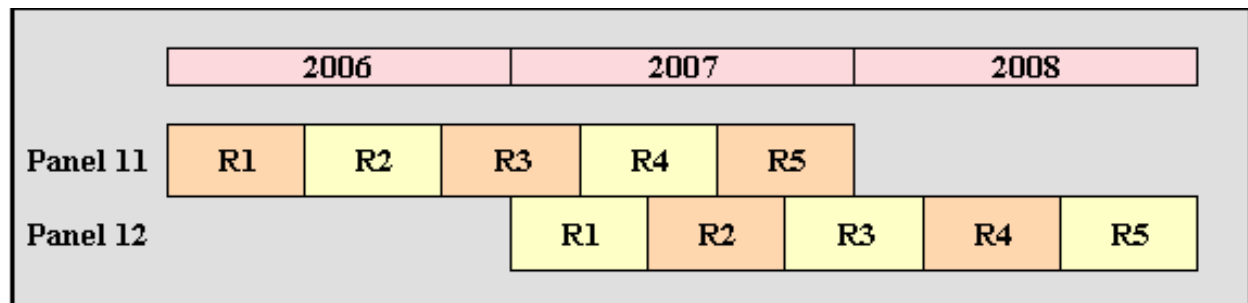
Reconciling the results of these papers is beyond the scope of my study; more importantly for the purpose of this project, I wish to highlight the inability of these papers to identify and measure separate supply and demand effects of economic shocks on opioid use. This distinction is most clearly drawn 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 the analysis in the existing literature is conducted at the county level, the existing literature is unable to directly determine whether any effects of economic conditions on prescription opioid use are attributable to person-specific or place-specific

¹The 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.

consequences of economic conditions. Ruhm (2019) and Charles, Hurst, and Schwartz (2018) are, to varying degrees, attuned to the supply-demand distinction and its importance for the link between economic conditions and deaths of despair proposed by Case and Deaton (2017). In particular, Ruhm (2019) proposes that changes in drug-prescribing environments (e.g. the ease of obtaining opioids in a given county) may be able to explain any impact of worsening economic conditions on deaths related to prescription opioid use, and Charles, Hurst, and Schwartz (2018) concede that their results “leave open the question of which specific persons in the community increase drug use when jobs disappear” due to industry shifts away from manufacturing. Both of these papers attempt to give some evidence on this question. The former uses differential trends for opioid analgesic and illicit opioid availability around 2010 to show that changes in the drug environment are a likely mechanism for changes in opioid deaths associated with changes in economic conditions. The latter uses failed drug tests as a proxy for drug demand to argue that declines in the manufacturing share of employment likely increases drug demand among affected workers. However, neither paper argues that the associations they observe necessarily imply a causal link, as both recognize that their measures are noisy proxies for their variables of interest. Specifically, Ruhm (2019) acknowledges that differential trends in drug availability only tell part of the story of actual drug availability in a given community, and Charles, Hurst, and Schwartz (2018) acknowledge that estimates from their drug test analysis may suffer from upward omitted variable bias, since drug tests are not assigned randomly to workers, but are targeted towards individuals suspected of drug abuse. These shortcomings underscore the importance of my study in providing causal evidence on the effect of labor market dislocation on affected workers’ demand for opioids.

3 Data and Descriptive Results

Figure 1: Diagram of the MEPS’ overlapping panel design



Source: Chowdhury (2011)

3.1 Data source

My project uses data collected through the MEPS, a nationally representative survey of members of the United States civilian non-institutional population. MEPS administrators interview survey participants five times over the course of two years. Each participant’s participation period 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 are participating 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 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. First are longitudinal files, for which the unit of observation is the MEPS participant. Second are prescribed medicines files, for which the unit of observation is the prescription. Prescriptions in the prescribed medicines files can be linked to individuals in the longitudinal files using panel number and person-level identification numbers. Information in longitudinal files is primarily obtained through interviews. 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 (see, for instance, Stagnitti, 2015).² MEPS administrators obtain prescription information from in-person interviews with MEPS participants and obtain permission from survey participants to follow up with pharmacies they list as having provided medicines to them.

3.2 Sample selection

To minimize threats to identification under my research design, I impose a handful of sample restrictions. One important threat to identification is the possibility of negative selection into job displacement on unobserved dimensions correlated with propensity to use opioids. To combat this threat, I first restrict my sample to prime-age individuals, who are less likely to be marginal workers than their younger and older counterparts. Second, I restrict my analysis sample to individuals who did not receive any opioid prescriptions during the reference period corresponding to the first round of MEPS interviews, to reduce the likelihood of individuals being selected into job displacement due to their opioid use. Finally, I restrict my analysis to individuals who report being employed in the first round of interviews. My primary reason for imposing this last restriction is that my definition of job displacement, to be discussed in the following subsection, 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 during which they report working is a way of screening individuals prior to treatment in order to avoid negative selection into the treatment (in this case job displacement) on unobservable characteristics associated with future opioid use.

My exclusion of individuals not working in the first round of interviews and individuals with non-zero first round opioid use, coupled with the availability of health status control variables (the full set of which are only available from panel 4 onwards), pares my analysis sub-sample to 23.8% of individuals in the MEPS from 1996 to 2017 and 60.67% of prime-age individuals participating in the MEPS during this time period, or 27.29% of individuals in the MEPS from panel 4 onwards and 69.87% of prime-age individuals participating the MEPS during this period. Demographic characteristics of this sub-sample of MEPS participants (pooling all years of data) are presented in section 1 of table 1. Individuals in my analysis sample appear to resemble the set of all prime-age individuals in the MEPS on demographic dimensions, though they are slightly more likely to be male and hold at least a college degree, likely a byproduct of the fact that these individuals are overrepresented among working individuals in the United States.

²It 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.

Table 1: Characteristics of MEPS participants

	Proportions of MEPS participants (%)		
	All prime-age in MEPS	Analysis sample	MME analysis sample (2010+)
<i>Section 1. Demographic characteristics</i>			
<i>U.S. Census Region</i>			
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
<i>Ten-Year Age Group</i>			
25-34	32.7	32.3	33.4
35-44	34.2	34.2	32.2
45-54	33.2	33.6	34.4
<i>Sex</i>			
Female	50.8	46.4	46.9
Male	49.2	53.6	53.1
<i>Race</i>			
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
<i>Ethnicity</i>			
Not Hispanic	84.8	85.0	82.6
Hispanic	15.2	15.0	17.4
<i>Marital status</i>			
Married	59.4	60.4	58.8
Widowed/divorced/separated	15.4	14.8	13.6
Never married	24.9	24.8	27.5
<i>Educational attainment</i>			
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
<i>Section 2. Industry of round one employment</i>			
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
<i>Section 3. Occupation of round one employment</i>			
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, Transportation, Matrl Moving	10.2	12.5	11.9
Military Specific Occupations	0.1	0.2	0.2
<i>Section 4. Health status</i>			
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

I assign individuals in the MEPS to age groups according to their age in their first year of participation. Race, educational attainment, industry, and occupation variables are all a harmonization of different variable codings used from 1996 to 2017. Marital status variables are recoded from more granular marital status categories. The industry and occupation schemas roughly 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.

3.3 Measuring job displacement

While the MEPS collects round-by-round information on participants' labor market activities, MEPS interviewers do not ask participants whether they experience job displacement. As such, I follow Schaller and Stevens (2015) in classifying an individual as having been 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. I focus on displacement due to these latter two causes, which I will refer to henceforth as "non-layoff displacement," for reasons I will explain in subsection 4.2. Individuals can "switch" current main jobs into unemployment; they need not work in the post-displacement period. In inferring job displacement from job switching, I can only observe that individuals were displaced at some point between rounds of MEPS interviews. I am not able to pinpoint exactly when they were displaced.

I show summary statistics regarding the prevalence of job displacement thus defined, both overall and disaggregated by displacement type, among members of the analysis sub-sample in section 1 of table 2. Approximately 9% of individuals (or 7,100 individuals) in my analysis sample experience any displacement, with layoffs accounting for slightly less than half of all job displacements and non-layoff displacements (e.g. business closure or employment ending) accounting for slightly more than half. A small number of unlucky individuals, roughly one tenth of a percent of my analysis sample, experience both layoffs and non-layoff displacement during their participation in the MEPS.

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+)
<i>Section 1. Displacement</i>			
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
<i>Section 2. Opioid use</i>			
<i>High MME per day prescriptions</i>			
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
<i>Prescription counts</i>			
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

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.

3.4 Measuring prescription opioid use

My primary outcome of interest is the probability that survey participants begin to use opioids. As I discuss in subsection 3.1, MEPS administrators obtain prescription information from in-person interviews with MEPS participants and from pharmacies to which MEPS participants refer them. The information MEPS administrators receive regarding these prescriptions includes a National Drug Code (NDC), a drug name, and a generic drug name. 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), Moriya and Miller (2018b), Stagnitti (2017), Groenewald et al. (2016), Zhan et al. (2001), and Zhou, Florence, and Dowell (2016). My methods essentially

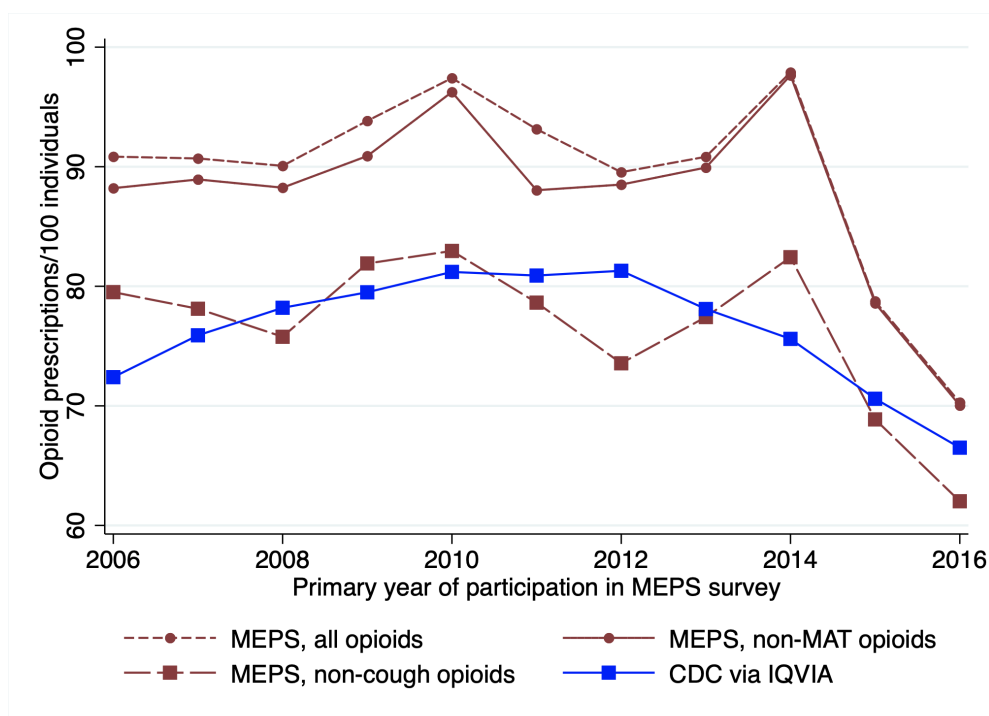
amount to a prescription meeting two of the three following criteria:

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

I do not count opioids used in medication-assisted treatment for substance abuse disorder, namely buprenorphine and methadone because my outcome of interest is the probability of beginning to use or abuse opioids.

Because MEPS participants self-report prescription opioid use, it is reasonable to be concerned that the data will suffer from underreporting due to stigma around drug use. To check whether the MEPS data is a reasonable measure of Americans' opioid use, I compare it with opioid prescribing data reported by CDC and obtained by IQVIA. Figure 2 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 medication-assisted treatment (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 2 seems to justify drawing conclusions about opioid use from MEPS data.

Figure 2: Opioid prescribing rate over time, by data source



CDC IQVIA prescribing rates are taken from CDC Opioid Overdose Data: U.S. Opioid Prescribing Rate Maps, last updated October 2018 (CDC 2018b). 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 guaiaacolsulfonate", "brompheniramine", or "bromododiphenhydramine."

³This list can be accessed in spreadsheet for at <https://www.cdc.gov/drugoverdose/resources/data.html> within the Data Files box. I last retrieved the list on November 24th, 2019.

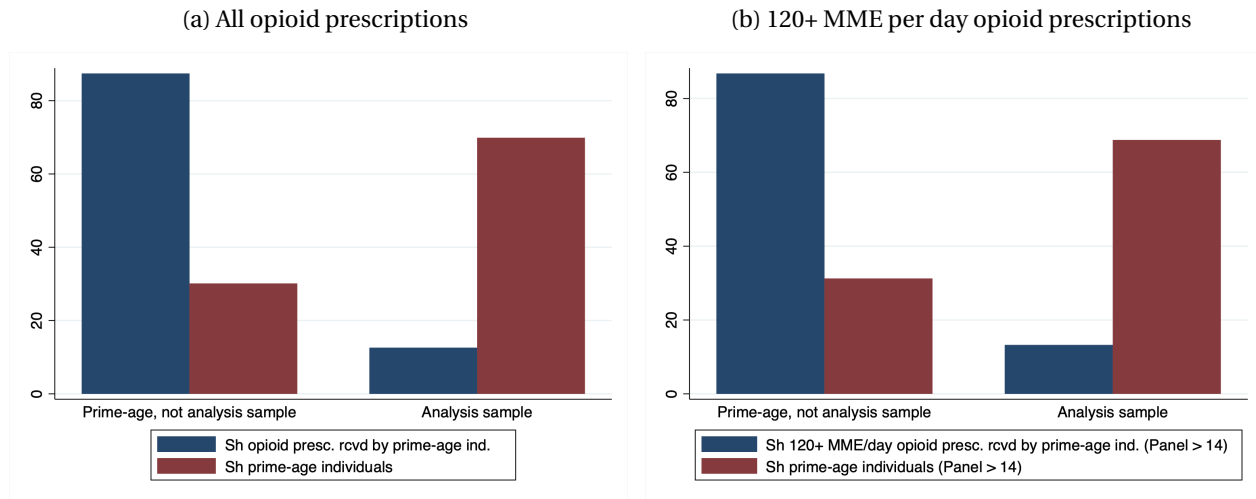
While a variety of metrics for opioid abuse exist in the literature, I focus on (1) indicators for exceeding various thresholds of opioid prescriptions over the course of their survey participation and (2) indicators for exceeding various thresholds of morphine milligram equivalent (MME) dosage per day. I can only compute prescriptions' MME per day for individuals who entered the MEPS on or after 2010, 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 given prescription requires that I know the strength of the opioid component of the drug prescribed, which is not always available. To overcome this obstacle, I use a list (compiled from the IBM Micromedex Red Book Drug Database) of possible drug strengths for opioid drugs based on their drug components to conservatively impute the opioid component strength (IBM Red Book, 2019). For a detailed discussion of my imputation methods, see appendix subsection B.3.

It is difficult to specify how many opioid prescriptions might reasonably 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 12-month period on average. However, this analysis 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 12-month period, for potentially problematic "chronic" prescription opioid use. In the interest of transparency, 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 considerably to 18.2%, 1.1%, and 0.3%, respectively.

Correspondency between MME per day and opioid abuse is better-established. A patient is "opioid tolerant" if they are using more than 60 MME per day. According to the CDC, physicians should "avoid or carefully justify increasing dosage to ≥ 90 MME per day" (CDC, N.D.). Finkelstein, Gentzkow, and Williams (2018) set the threshold for abuse at 120 MME per day. I report regression results for all three of these MME per day outcomes, though, as discussed, the fact that it is only feasible to compute MME for a restricted analysis sample reduces my power to rule out small displacement effect sizes. 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. As with the prescription count indicators, these proportions are lower among individuals in my analysis sample at 2.8%, 1.3%, and 0.5%, respectively.

My analysis sample accounts for a small proportion of the overall opioid use I measure among prime-age individuals in the MEPS. As figure 3 shows, individuals in the analysis sample and MME analysis sample make up the vast majority – about 70% and 69%, respectively – of prime-age MEPS participants from 1999-2017 but only account for roughly 13% 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 – about 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 should serve as a reminder that analyses of individuals transitioning to opioid abuse can only explain a relatively small proportion of overall opioid abuse

Figure 3: Share prescriptions linked to prime-age individuals by inclusion in analysis sample, 1999-2017



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.

among prime-age individuals in the United States at any given time. However, studying the transition from “opioid naïveté” to opioid abuse is a worthwhile research goal because every opioid user was once a non-user.

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

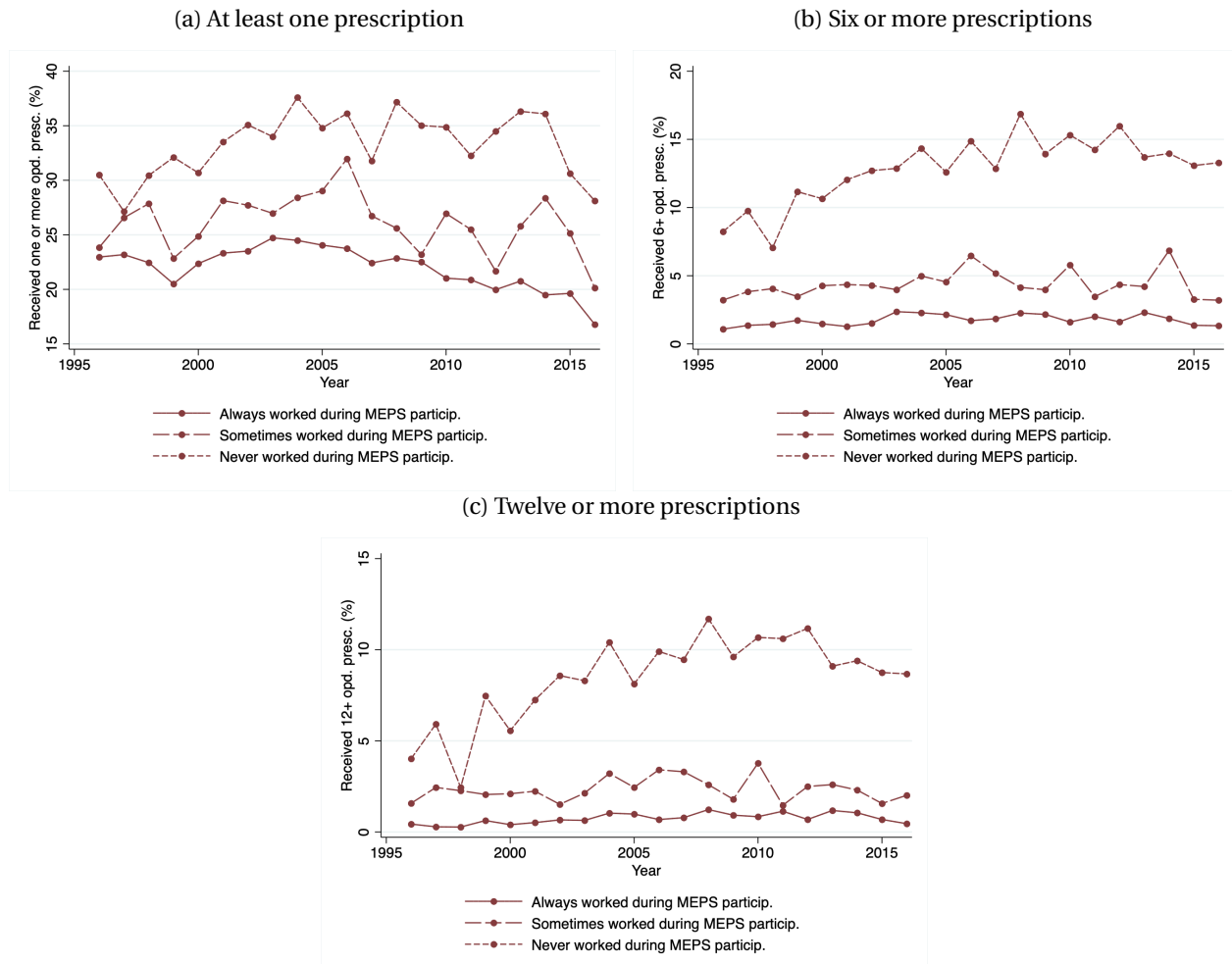
The MEPS’ rich data on survey participants’ health characteristics allows me to condition on participants health status when consider the impact of displacement on likelihood of opioid abuse. I show summary statistics for relevant health characteristics of individuals in the analysis sample in section 4 of table 1. For the most part, I construct the variables shown therein using round-specific health status variables, setting each indicator to one if, in any given round of interviews, a survey participant reports experiencing the health issue in question. The exceptions to this are two mental health variables, 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 choose in favor of measures of participants’ mental health over longer periods of time because conditioning on post-displacement mental health status is very likely “controlling for the treatment.” I classify prescriptions in the prescribed medicines files as antidepressants or anti-psychotics if their therapeutic class is given as antidepressant or anti-psychotic. Of course, some would argue that controlling for any health condition throughout the course of the survey, rather than prior to displacement, is “controlling for the treatment” because I control for negative health effects of displacement (see e.g. Schaller and Stevens, 2015). For this reason, I reproduce my main results controlling only for round-one health status in appendix C.⁴

In addition to conditioning on 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

⁴The results therein by and large resemble the results I show in the main text of the paper, though I suspect they suffer from omitted variables bias. Appendix C contains a discussion regarding the tradeoffs between specifications.

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 (NAICS) and Standard Occupation Classification (SOC) schemas, respectively.

Figure 4: Proportions of prime-age individuals exceeding thresholds of non-MAT opioid prescription receipt, by employment activity

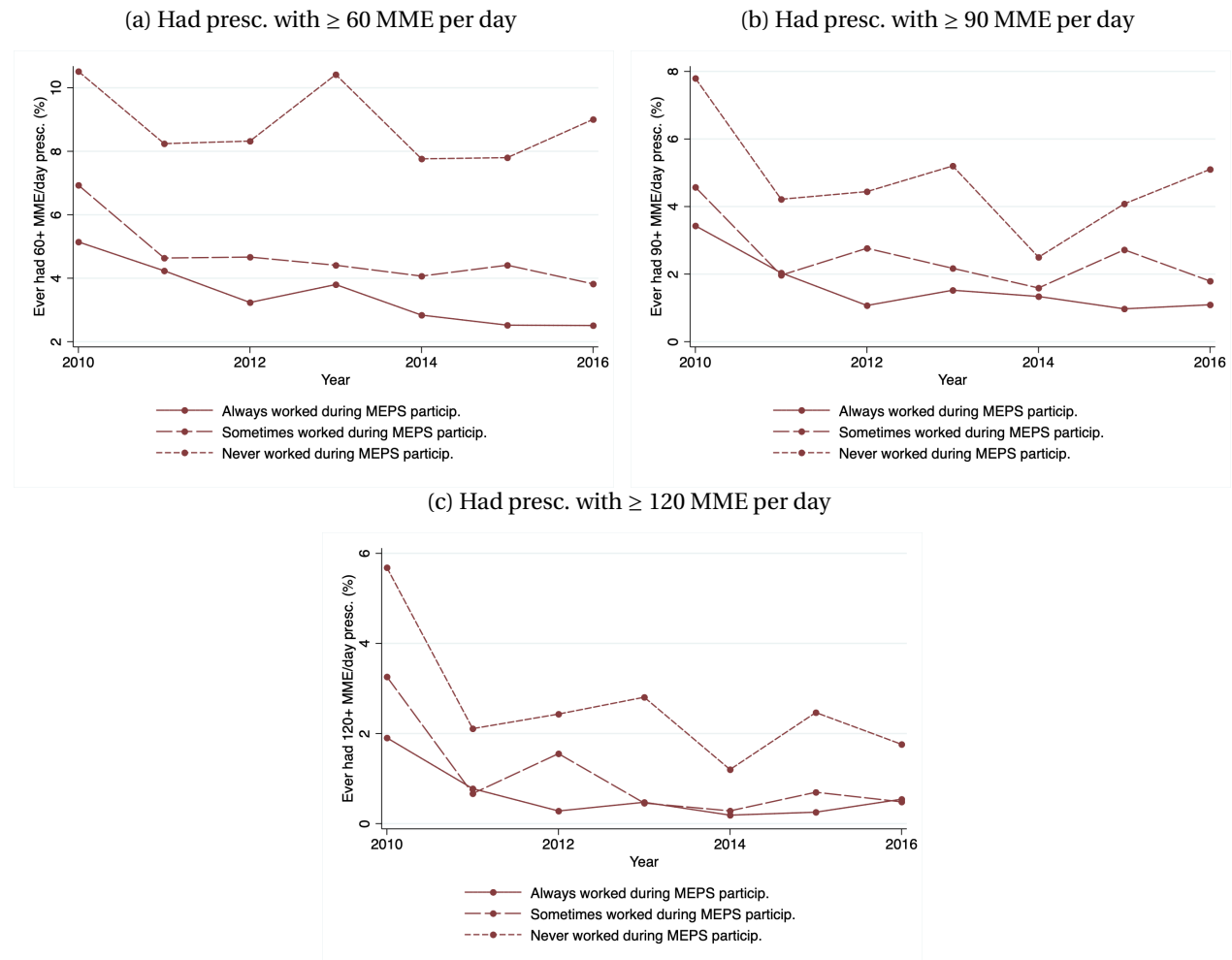


Statistics presented here are computed among prime-age individuals using survey weights. I compute these statistics by assigning each individual in the MEPS a primary year of participation in the survey, which is always their first year of participation except for individuals for which I have no data 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.

3.6 Trends in opioid prescribing

Opioid use varies considerably depending on surveyed individuals' employment. Figure 4 shows the proportion of individuals who exceed opioid prescription count thresholds over the period 1996-2016 by three employment categories: (1) reported having been employed for the duration of every reference period preceding a

Figure 5: Proportions of prime-age individuals exceeding MME per day thresholds, by employment activity



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.

round of MEPS interviews (2) reported having been employed for the duration of at least one reference period preceding at least one round of MEPS interviews and (3) reported never having been employed for the duration of a reference period preceding a MEPS interview.

The differences are considerable. Among individuals whose primary MEPS participation year was 1996, over 30% of individuals who never were employed during their survey participation reported receiving at least one opioid prescription, compared to slightly less than 25% among individuals who reported always or sometimes working. At its peak in 2004, 38% of individuals who were never employed during their survey participation reported receiving at least one prescription, as opposed to just 24% of individuals who always worked.

The gap between always-working and never-working individuals is larger in relative terms at high thresholds of opioid use. Among survey participants whose primary year of participation in the MEPS was 1996, individuals who never worked during their survey participation were nearly eight times as likely as individuals who always worked during their survey participation 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%). These ratios remained fairly constant as prescribing rates among all three groups peaked in 2008, at which point 16.9% of never-working individuals received six or more opioid prescriptions and 11.7% received twelve or more prescriptions. Similarly, roughly double the share of never-working individuals receive high-MME per day opioid prescriptions relative to always-working prescriptions. However, the proportion of individuals receiving high-MME per day prescriptions has fallen considerably since 2010, the first year for which data are available.

The strong cross-sectional relationship between opioid use and employment is consistent with Krueger (2017), who reports that 30% of prime-age non-labor force men he surveyed took prescription pain medication the day before they were surveyed, a number similar to though slightly higher than my 2016 measurement of the proportion of never-working individuals who reported receiving at least one opioid prescription during their survey participation in the MEPS. However, as Krueger (2017) readily volunteers, the cross-sectional relationship is merely suggestive. In the following section, I outline a framework by which I can provide causal evidence on the question of whether labor market dislocation leads to opioid abuse.

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

$$Y_i = \alpha + \beta \text{NON_LAYOFF_DISPLACED}_i + X_i' \gamma + \text{PANEL}_i' \rho + \epsilon_i \quad (1)$$

where $\text{NON_LAYOFF_DISPLACED}_i$ 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 enumerated in table 1; PANEL_i is a vector consisting of indicators for being in each panel; and ϵ_i is an error term. I use $\text{NON_LAYOFF_DISPLACED}_i$ as the explanatory variable in the regression results I report in the main text because I see it as a more plausibly exogenous job displacement than job displacement due to being laid off or job displacement overall. I support this choice in greater detail in the following subsection. However, I report estimates of β in equation 1 as estimated

using all displacement and displacement due to layoffs in appendix A.

My outcomes of interest Y_i can be written formally as:

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

$$\mathbb{1}\left(\max(\text{MME per day}_{\text{round}} | \text{round} \in \{1, 2, 3, 4, 5\}) \geq t\right) \text{ for } t \in \{60, 90, 120\} \quad (3)$$

which are indicators for individual i accumulating more than k 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 (2) for $k \in \{1, 2, 3, \dots, 15\}$ in appendix A. Recall from subsection 3.4 that I can only compute MME per day for individuals whose survey participation began in or after 2010, and that I must impute drug strengths for a non-trivial proportion of prescriptions for which this information is missing.

4.2 Identification in main specification

I am interested in using equation 1 to understand the causal effect of job displacement on the probability that an individual begins using opioids. With that said, β , the coefficient on $\text{NON_LAYOFF_DISPLACED}_i$ in equation 1, will be at best a rough proxy for the causal effect of displacement.

First, I have to contend with negative selection into displacement. I am particularly concerned with selection on unobserved health characteristics correlated with future opioid use, as it is unlikely that the health conditions enumerated in section 4 of table 1 represent the full set of health conditions which are both correlated with opioid use and likelihood of job displacement. I combat this bias by focusing on displacement due to business dissolution or sale or job ending. Displacement due to these causes is more likely than layoff-based displacement to be orthogonal to workers' health status and, by and large, their productivity (see for instance Gibbons and Katz, 1991).⁵ To the extent selection bias remains a threat, it is almost certainly positive. If unobserved health conditions affected workers' likelihood of experiencing displacement, they probably make workers more likely to experience displacement. Furthermore, such conditions almost certainly make workers more likely to receive opioid prescriptions.

A thornier shortcoming of my main specification is the question of relative timing of displacement and opioid prescription receipt. To understand this shortcoming, 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" according to my empirical specification, but it is not possible that individual A began to use opioids due to job displacement since her opioid use preceded her displacement. Indeed, in order for β to yield the causal effect of displacement, not only would $\text{NON_LAYOFF_DISPLACED}_i$, ϵ_i need to satisfy $\text{cov}(\text{NON_LAYOFF_DISPLACED}_i, \epsilon_i | X_i, \text{PANEL}_i)$, but the "treated" analysis sample would need to be free of any individuals such as individual A. Clearly, this is not a reasonable assumption.

Given this shortcoming, why study equation 1? First, equation 1 is the best available specification due to data constraints, and second, estimates of β may still be informative if I make reasonable assumptions about the direction in which survey participants like individual A are likely to push β away from the true causal effect of job displacement.

⁵Of course, as Hilger (2016) shows, non-layoff displacement is not entirely orthogonal to productivity, especially when workers cannot be pre-screened prior to displacement. My sample selection, which serves as pre-screening, assuages some of my concerns regarding correlation between non-layoff displacement and unobserved health factors here.

To see the first point, consider a survey participant in my analysis sample who received 12 opioid prescriptions during the reference period corresponding to round three. Say that this survey participant told her MEPS interviewer in her third round interview that she had switched her current main job due to a layoff, business closure, or her job ending, meaning that she was displaced at some point in round three. As I discuss in subsection 3.3, I am unable to observe the date at which the survey participant in question was displaced. As a consequence, I am unable to determine whether the survey participant was displaced first and began using opioids thereafter or vice versa. Indeed, if I write the fixed effects regression

$$Y_{i,\text{round}} = \alpha_i + \beta \text{DISPLACED}_{i,\text{round}} + X'_{i,\text{round}}\gamma + \text{PANEL}'_i\rho + \epsilon_{i,\text{round}} \quad (4)$$

in favor of equation 1, I may be in the same position with the survey participant in question as I was with survey participant A. 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 absurd to attribute her opioid use to her displacement. Hence specification 4 is not an improvement over 1. My inability to accurately observe the relative timing and displacement of displaced individuals is an unfortunate and insuperable feature of the MEPS. Just as importantly, almost none of the health status variables in X_i are available for every round of a survey participants' participation. Using a specification for which I consider outcomes at the individual level rather than at the individual-round level allows me to condition on a greater number of health conditions in considering the possible impact of displacement on propensity to use opioids.

To the second point, the argument that individuals such as individual A are likely to push β upward follows directly from my argument regarding the sign of selection bias earlier in this subsection. To the extent that individuals using opioids prior to displacement affect my results, their opioid use plausibly makes them more likely to experience displacement. In this cause, individuals such as individual A will bias upward my estimates of β .

4.3 Treatment 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:

$$Y_i = \varphi_0 + \varphi_1 \text{NON_LAYOFF_DISPLACED}_i + \varphi_2 W_i + \varphi_3 \text{NON_LAYOFF_DISPLACED}_i \times W_i + (X_i \setminus W_i)'\varphi_4 + \text{PANEL}'_i\rho + \psi_i \quad (5)$$

where W_i is the interaction category. In the main text of the paper, I estimate two interaction models displacement differ among 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 the contribution I hope to make in this work is to investigate 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 were true, I would expect to observe positive ψ_2 in both of these interaction models. However, my results do not support this hypothesis. In appendix A, I also show results from simple interaction models which investigate the possibility of differential impact of displacement on workers based on whether they experienced pain or depression in the reference period corresponding to their first round of survey participation; the evidence from these interaction models does not support the idea that displacement makes individuals from either of these groups disproportionately made more likely to use opioids.

I also estimate three regressions which test whether the effect of displacement on opioid use is mediated

by the extent to which displacement poses a financial difficulty 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. In the interest of testing this, I estimate equation 5 setting W_i equal to an indicator for (1) reporting at least one period of no work (2) the survey participant having reported an income for year 1 of their survey participation which exceeds half of the total income reported by individuals in their dwelling unit, and (3) anyone in the survey participant's dwelling unit having reported having business or trust income in the first year of their survey participation.

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. Tables A.1 and A.2 report the same set of coefficients where I use displacement and layoff indicators, respectively, as independent variables. In each table, 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 through all reference periods (4) workers whose dwelling units reported having business or trust income in year one of their survey participation 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 in some cases 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 MMEs 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 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 on the same orders of magnitude as their corresponding baseline results in section 1. 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)
<i>Section 1. Baseline</i>						
Ever non-layoff displaced	-0.013** (0.007)	-0.002 (0.002)	-0.002** (0.001)	0.002 (0.006)	0.001 (0.004)	-0.004*** (0.001)
Mean of outcome	0.197	0.020	0.009	0.026	0.013	0.005
<i>Section 2. Heterogeneity by occupation</i>						
Blue-collar	-0.027** (0.011)	-0.005 (0.003)	-0.002 (0.002)	-0.009 (0.007)	-0.001 (0.005)	-0.001 (0.003)
Mean of outcome (blue-collar)	0.167	0.017	0.011	0.025	0.016	0.005
White-collar	-0.007 (0.008)	-0.001 (0.002)	-0.002*** (0.001)	0.006 (0.008)	0.003 (0.006)	-0.005*** (0.001)
Mean of outcome (white-collar)	0.197	0.020	0.009	0.026	0.013	0.005
<i>Section 3. Heterogeneity by race/ethnicity</i>						
Non-Hisp. white	-0.022** (0.009)	-0.001 (0.003)	-0.002* (0.001)	0.003 (0.011)	0.002 (0.007)	-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	0.002 (0.008)	-0.004*** (0.002)	-0.002** (0.001)	-0.001 (0.004)	0.000 (0.003)	-0.002 (0.001)
Mean of outcome (not non-hisp. white)	0.197	0.020	0.009	0.026	0.013	0.005
<i>Section 4. Heterogeneity by whether individual did not work for at least one period</i>						
At lst 1 pd did not work	-0.025* (0.015)	-0.012** (0.006)	-0.006** (0.003)	-0.009 (0.012)	-0.007 (0.009)	-0.007 (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 (0.007)	-0.001 (0.002)	-0.001 (0.001)	0.005 (0.008)	0.003 (0.005)	-0.004*** (0.001)
Mean of outcome (worked all ref pds)	0.197	0.020	0.009	0.026	0.013	0.005
<i>Section 5. Heterogeneity by whether dwelling unit has business/trust income</i>						
Family has biz/trust inc.	-0.025* (0.013)	-0.004* (0.002)	-0.001 (0.001)	-0.010 (0.008)	-0.002 (0.007)	-0.004*** (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 (0.008)	-0.001 (0.002)	-0.003** (0.001)	0.005 (0.008)	0.002 (0.005)	-0.004*** (0.001)
Mean of outcome (no fam biz/trust inc.)	0.197	0.020	0.009	0.026	0.013	0.005
<i>Section 6. Heterogeneity by share of dwelling unit income from individuals' wage income</i>						
Wage inc. was majority of Y1 family income	-0.002 (0.013)	-0.001 (0.004)	-0.003** (0.001)	0.028 (0.019)	0.017 (0.014)	-0.006*** (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** (0.007)	-0.003 (0.002)	-0.002* (0.001)	-0.007 (0.006)	-0.004 (0.003)	-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

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.

Coefficients on indicators for ever experiencing a layoff in these regressions, reported in table A.2, are generally larger than the coefficients reported in table 3. Coefficients on indicators for ever experiencing displacement, reported in appendix table A.1 lie between the two. Generally, coefficients in these two appendix tables are less statistically significant than their counterparts in table 3, though the same patterns between sections are distinguishable. I do not observe patterns in either of these tables which would suggest that displacement induces blue-collar workers or non-Hispanic whites to abuse opioids, and I observe patterns in both 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.

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) 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 are somewhat surprising in the context of recent discussion of the interplay between labor market behavior and opioid use. The implication is 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, particularly for individuals who experience non-layoff displacement. This would appear, at least at first glance, to be at odds with some important articles in the economics literature around opioid use, particularly Case and Deaton (2015; 2017) and Krueger (2017), both of which argue that a strong aggravating factor in the opioid epidemic has been labor market dislocation.

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, a relatively exogenous source of displacement. Case and Deaton (2015; 2017) and Krueger (2017) arguably are more interested in individuals who experience adversity in the labor market over longer periods of time, and for reasons that might be more strongly related to their productivity in the workplace than displacement. Data limitations prevent me from investigating this question: for one, I only observe individuals in the MEPS for two years, scarcely a sufficient time frame to investigate individuals' long-term labor market difficulties.

More thoroughly addressing the narratives advanced by Case and Deaton (2015; 2017) will require further research. In particular, researchers and policymakers would benefit greatly from data sources which track individuals over longer periods of time, which could shed light on the effect of longer-term labor market dislocations on individuals' likelihood of opioid abuse. Furthermore, as referenced briefly in section 2, it would be helpful to have physician-level data on opioid prescribing for specific geographies, which could shed light on the supply-side response of physicians to county-level labor market dislocations and help illuminate whether the findings of recent papers such as Currie, Jin, and Schnell (2018) and Charles, Hurst, and Schwartz (2018) are dictated in part, as I suspect they are, 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 overprescribed 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

Table A.1: 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)
<i>Section 1. Baseline</i>						
Ever displaced	-0.008 (0.005)	-0.001 (0.002)	-0.002** (0.001)	0.004 (0.005)	0.004 (0.004)	-0.003* (0.001)
Mean of outcome	0.207	0.022	0.010	0.031	0.016	0.006
<i>Section 2. Heterogeneity by occupation</i>						
Blue-collar	-0.020** (0.009)	-0.000 (0.003)	-0.001 (0.002)	0.010 (0.009)	0.017* (0.009)	0.001 (0.003)
Mean of outcome (blue-collar)	0.184	0.022	0.011	0.030	0.020	0.007
White-collar	-0.003 (0.007)	-0.001 (0.002)	-0.002** (0.001)	0.002 (0.006)	-0.000 (0.004)	-0.004** (0.002)
Mean of outcome (white-collar)	0.207	0.022	0.010	0.031	0.016	0.006
<i>Section 3. Heterogeneity by race/ethnicity</i>						
Non-Hisp. white	-0.018** (0.008)	-0.001 (0.002)	-0.001 (0.001)	0.006 (0.008)	0.005 (0.006)	-0.003 (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 (0.006)	-0.000 (0.002)	-0.002** (0.001)	0.001 (0.004)	0.002 (0.003)	-0.002 (0.001)
Mean of outcome (not non-hisp. white)	0.207	0.022	0.010	0.031	0.016	0.006
<i>Section 4. Heterogeneity by whether individual did not work for at least one period</i>						
At 1st 1 pd did not work	-0.011 (0.013)	-0.008 (0.005)	-0.006** (0.003)	0.007 (0.011)	-0.003 (0.008)	-0.008 (0.005)
Mean of outcome (at 1st 1 pd did not work)	0.211	0.026	0.012	0.036	0.016	0.007
Worked all ref pds	-0.009 (0.006)	-0.001 (0.002)	-0.001 (0.001)	0.004 (0.006)	0.006 (0.005)	-0.003* (0.002)
Mean of outcome (worked all ref pds)	0.207	0.022	0.010	0.031	0.016	0.006
<i>Section 5. Heterogeneity by whether dwelling unit has business/trust income</i>						
Family has biz/trust inc.	-0.011 (0.011)	-0.001 (0.003)	0.002 (0.002)	0.005 (0.009)	0.008 (0.007)	0.003 (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 (0.006)	-0.000 (0.002)	-0.003*** (0.001)	0.004 (0.006)	0.003 (0.004)	-0.004*** (0.001)
Mean of outcome (no fam biz/trust inc.)	0.207	0.022	0.010	0.031	0.016	0.006
<i>Section 6. Heterogeneity by share of dwelling unit income from individuals' wage income</i>						
Wage inc. was majority of Y1 family income	-0.005 (0.010)	-0.003 (0.003)	-0.004*** (0.001)	0.011 (0.012)	0.008 (0.009)	-0.004 (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 (0.006)	0.000 (0.002)	-0.001 (0.001)	0.002 (0.005)	0.003 (0.004)	-0.002 (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

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.2: 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)
<i>Section 1. Baseline</i>						
Ever laid off	0.000 (0.008)	0.001 (0.003)	-0.001 (0.001)	0.009 (0.008)	0.010 (0.007)	-0.001 (0.003)
Mean of outcome	0.221	0.024	0.010	0.040	0.021	0.008
<i>Section 2. Heterogeneity by occupation</i>						
Blue-collar	-0.012 (0.012)	0.004 (0.004)	0.001 (0.002)	0.030* (0.018)	0.036** (0.018)	0.005 (0.005)
Mean of outcome (blue-collar)	0.203	0.026	0.011	0.037	0.026	0.010
White-collar	0.007 (0.010)	-0.001 (0.003)	-0.002 (0.002)	0.001 (0.009)	0.001 (0.006)	-0.003 (0.003)
Mean of outcome (white-collar)	0.221	0.024	0.010	0.040	0.021	0.008
<i>Section 3. Heterogeneity by race/ethnicity</i>						
Non-Hisp. white	-0.007 (0.011)	-0.001 (0.003)	-0.000 (0.002)	0.013 (0.013)	0.014 (0.011)	-0.000 (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 (0.010)	0.004 (0.004)	-0.001 (0.001)	0.004 (0.006)	0.005 (0.005)	-0.002 (0.002)
Mean of outcome (not non-hisp. white)	0.221	0.024	0.010	0.040	0.021	0.008
<i>Section 4. Heterogeneity by whether individual did not work for at least one period</i>						
At lst 1 pd did not work	0.004 (0.015)	-0.003 (0.006)	-0.004 (0.003)	0.019 (0.015)	0.004 (0.010)	-0.005 (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 (0.010)	-0.001 (0.003)	0.000 (0.002)	0.006 (0.010)	0.014 (0.009)	-0.001 (0.004)
Mean of outcome (worked all ref pds)	0.221	0.024	0.010	0.040	0.021	0.008
<i>Section 5. Heterogeneity by whether dwelling unit has business/trust income</i>						
Family has biz/trust inc.	0.013 (0.017)	0.003 (0.005)	0.005 (0.004)	0.027 (0.018)	0.022 (0.014)	0.013 (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 (0.009)	0.000 (0.003)	-0.002* (0.001)	0.005 (0.009)	0.007 (0.008)	-0.005** (0.002)
Mean of outcome (no fam biz/trust inc.)	0.221	0.024	0.010	0.040	0.021	0.008
<i>Section 6. Heterogeneity by share of dwelling unit income from individuals' wage income</i>						
Wage inc. was majority of Y1 family income	-0.011 (0.015)	-0.007* (0.004)	-0.005*** (0.002)	-0.002 (0.016)	0.006 (0.014)	-0.002 (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 (0.009)	0.004 (0.003)	0.001 (0.002)	0.014 (0.009)	0.012 (0.008)	-0.001 (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

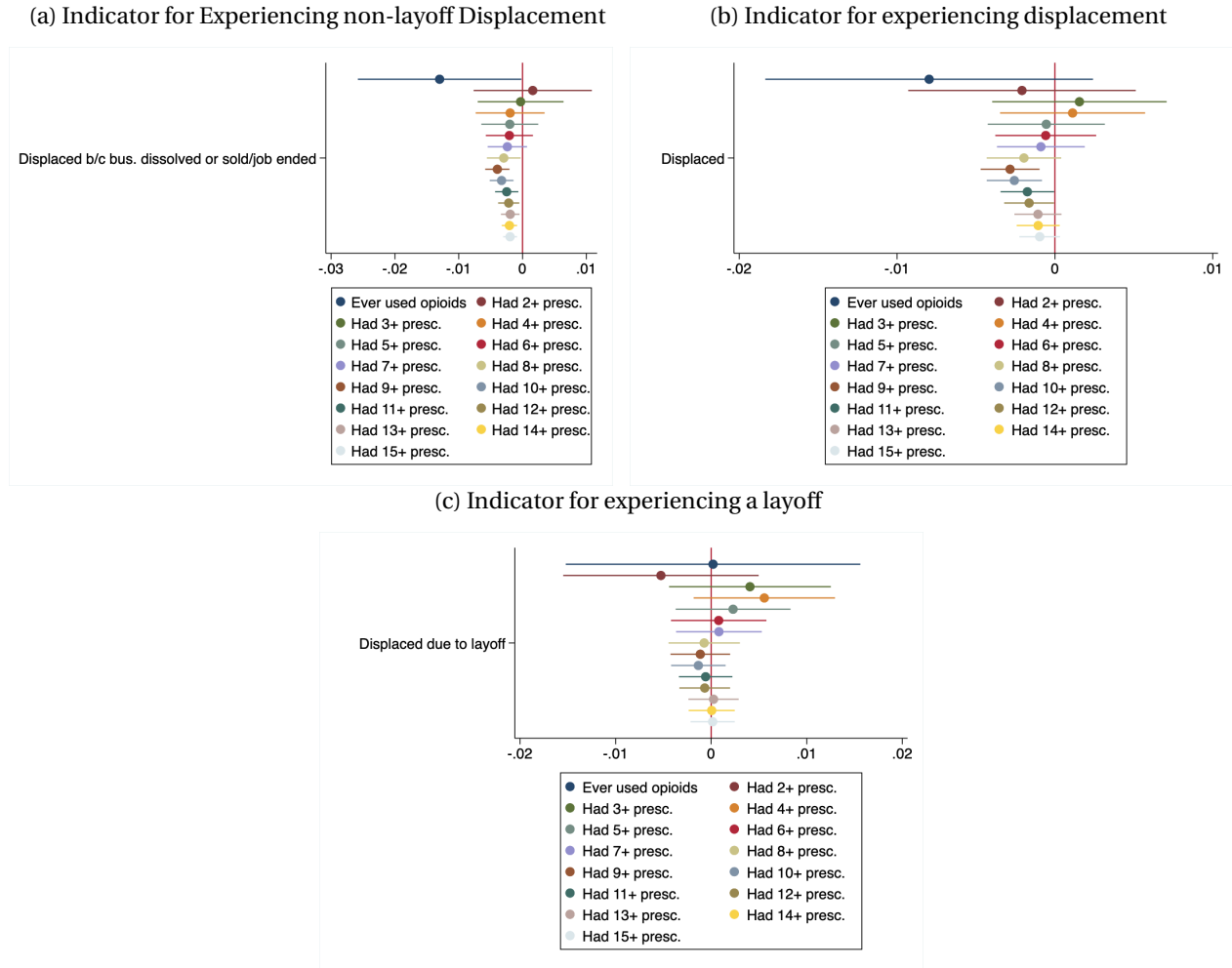
Regression estimates control for region, age group, an indicator for Hispanic ethnicity, marital status, industry, occupation, higher education, health status, and dates of participation in the survey. Standard errors are robust to heteroskedasticity. Estimates are computed using survey weights. Analysis 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.3: 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)
<i>Section 1. Independent variable = individual ever displaced</i>						
Had R1 pain	-0.023 (0.014)	-0.017*** (0.004)	-0.007*** (0.002)	0.011 (0.013)	0.006 (0.009)	-0.006** (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 (0.006)	0.003* (0.002)	-0.000 (0.001)	0.001 (0.005)	0.003 (0.004)	-0.001 (0.002)
Mean of outcome (no r1 pain)	0.207	0.022	0.010	0.031	0.016	0.006
<i>Section 2. Independent variable = individual ever displaced</i>						
Had R1 pain	0.010 (0.021)	-0.010 (0.007)	-0.001 (0.005)	0.019 (0.021)	0.028 (0.020)	-0.005 (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 (0.008)	0.003 (0.003)	-0.000 (0.001)	0.005 (0.007)	0.003 (0.005)	0.001 (0.003)
Mean of outcome (no r1 pain)	0.221	0.024	0.010	0.040	0.021	0.008
<i>Section 3. Independent variable = individual ever non-layoff displaced</i>						
Had R1 pain	-0.047*** (0.018)	-0.023*** (0.005)	-0.012*** (0.002)	0.010 (0.016)	-0.003 (0.010)	-0.006** (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 (0.007)	0.003 (0.002)	0.000 (0.001)	-0.002 (0.006)	0.003 (0.005)	-0.003*** (0.001)
Mean of outcome (no r1 pain)	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

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.1: Baseline Regression Results of Regression of All Prescription Count Indicators on Displacement



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.4: 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)
<i>Section 1. Independent variable = individual ever displaced</i>						
Fair/poor R1 m. hlth	-0.043* (0.026)	-0.016* (0.010)	-0.011*** (0.004)	-0.039** (0.018)	-0.012 (0.016)	-0.011*** (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 (0.005)	0.000 (0.002)	-0.001 (0.001)	0.006 (0.005)	0.005 (0.004)	-0.002 (0.002)
Mean of outcome (good or better r1 m. hlth)	0.207	0.022	0.010	0.031	0.016	0.006
<i>Section 2. Independent variable = individual ever displaced</i>						
Fair/poor R1 m. hlth	-0.051 (0.036)	-0.018 (0.011)	-0.007 (0.006)	-0.038 (0.024)	-0.008 (0.021)	-0.011** (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 (0.008)	0.002 (0.003)	-0.000 (0.001)	0.012 (0.008)	0.011 (0.007)	-0.001 (0.003)
Mean of outcome (good or better r1 m. hlth)	0.221	0.024	0.010	0.040	0.021	0.008
<i>Section 3. Independent variable = individual ever non-layoff displaced</i>						
Fair/poor R1 m. hlth	-0.032 (0.032)	-0.015 (0.013)	-0.013*** (0.003)	-0.035 (0.023)	-0.014 (0.021)	-0.010** (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* (0.007)	-0.001 (0.002)	-0.002* (0.001)	0.004 (0.007)	0.002 (0.005)	-0.004*** (0.001)
Mean of outcome (good or better r1 m. hlth)	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

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: Heterogeneity by whether participant received an antidepressant or antipsychotic prescription 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)
<i>Section 1. Independent variable = individual ever displaced</i>						
Rcvd. presc. for antidepressant/antipsychotic in R1	0.007 (0.026)	0.002 (0.011)	-0.008* (0.004)	0.013 (0.028)	-0.021 (0.014)	-0.006 (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* (0.005)	-0.001 (0.002)	-0.001 (0.001)	0.003 (0.005)	0.006 (0.004)	-0.002* (0.001)
Mean of outcome (no r1 antidep./antipsy. presc.)	0.207	0.022	0.010	0.031	0.016	0.006
<i>Section 2. Independent variable = individual ever displaced</i>						
Rcvd. presc. for antidepressant/antipsychotic in R1	0.020 (0.039)	0.010 (0.018)	-0.006 (0.007)	0.007 (0.044)	-0.020 (0.023)	0.005 (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 (0.008)	0.000 (0.002)	-0.000 (0.001)	0.009 (0.008)	0.013* (0.007)	-0.002 (0.002)
Mean of outcome (no r1 antidep./antipsy. presc.)	0.221	0.024	0.010	0.040	0.021	0.008
<i>Section 3. Independent variable = individual ever non-layoff displaced</i>						
Rcvd. presc. for antidepressant/antipsychotic in R1	-0.006 (0.032)	-0.004 (0.013)	-0.009** (0.004)	0.013 (0.034)	-0.021 (0.015)	-0.013*** (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** (0.007)	-0.002 (0.002)	-0.002* (0.001)	0.001 (0.006)	0.003 (0.005)	-0.003*** (0.001)
Mean of outcome (no r1 antidep./antipsy. presc.)	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

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, 2018a; Moriya and Miller, 2018b; 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.⁶

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.⁷ 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",

⁶This list can be accessed in spreadsheet at <https://www.cdc.gov/drugoverdose/resources/data.html> within the Data Files box. I last retrieved the list on November 24th, 2019.

⁷As Soni (2018) notes, "the drug name 'Acetaminophen' is spelled almost 70 different ways in the MEPS files."

"PROPOXYPHENE", "TAPENTADOL", or "TRAMADOL."⁸ 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

⁸I take care, however, not to count prescriptions whose drug names contain the string "TROPIMUM" 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. I last retrieved the list on November 24th, 2019.

"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 or 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

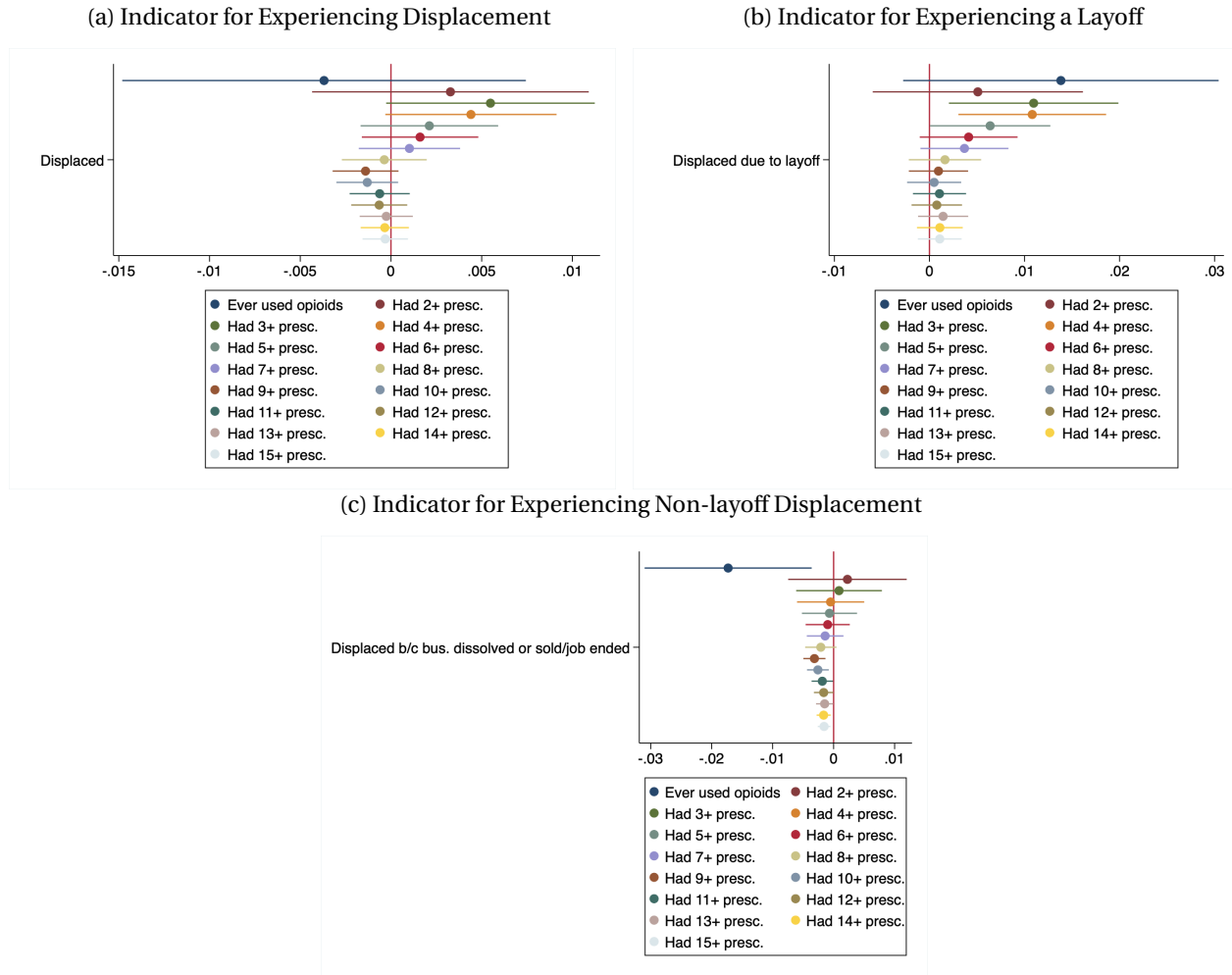
experiencing displacement and beginning to use opioids will likely bias my estimates of the effect of displacement on likelihood of opioid use upward. Furthermore, failing to condition on the full set of available health status variables increases the likelihood that individuals are selected into displacement on unheobservedalth 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. \geq 6 opd. prsc. (2)	Rcvd. \geq 12 opd. prsc. (3)	Ever 60+ MME/day (4)	Ever 90+ MME/day (5)	Ever 120+ MME/day (6)
<i>Section 1. Independent variable = individual ever displaced</i>						
Ever displaced	-0.004 (0.006)	0.002 (0.002)	-0.001 (0.001)	0.006 (0.005)	0.005 (0.004)	-0.002 (0.001)
Mean of outcome	0.207	0.022	0.010	0.031	0.016	0.006
<i>Section 2. Independent variable = individual ever displaced</i>						
Ever laid off	0.014 (0.008)	0.004 (0.003)	0.001 (0.001)	0.013 (0.008)	0.013* (0.007)	-0.000 (0.003)
Mean of outcome	0.221	0.024	0.010	0.040	0.021	0.008
<i>Section 3. Independent variable = individual ever non-layoff displaced</i>						
Ever non-layoff displaced	-0.017** (0.007)	-0.001 (0.002)	-0.002** (0.001)	0.002 (0.007)	0.002 (0.005)	-0.004*** (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

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



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.