

# Effects of Job Displacement on Prescription Opioid Demand: Evidence from the Medical Expenditure Panel Survey<sup>\*</sup>

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## Abstract

This study uses micro data from the Medical Expenditure Panel Survey to investigate whether job displacement affects the likelihood that prime-age workers use or abuse opioids. I show that displacement is not associated with changes in workers' likelihood of opioid use, except for at high use thresholds, at which point displacement appears to be associated with modest reductions in the probability of prescription opioid use. This relationship holds even among non-Hispanic whites and blue-collar workers, the workers highlighted in the burgeoning "deaths of despair" literature. I do find evidence that this association is mediated by the extent to which displacement causes financial hardship among workers. While estimates of the effect of displacement on opioid use will suffer from simultaneity bias under my specification due to my inability to observe the exact relative timing of displacement and opioid use, I am able to sign the simultaneity bias as positive by placing plausible restrictions on the model. Under these restrictions, the associations between opioid use indicators and job displacement indicators I observe are upper bounds on the effects of displacement on prescription opioid use, implying that the causal effect of displacement on likelihood opioid use is at most zero and possibly negative. My results would cast doubt on the hypothesis that labor market dislocations are an important driver of the opioid epidemic or the "deaths of despair" noted by Case and Deaton (2015; 2017).

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# 1 Introduction

In recent years, scholars, pundits, and policymakers have pronounced in unison that opioid abuse in the United States has reached crisis proportions. Estimates from federal government agencies suggest that roughly 68% of the 70,200 drug overdose deaths in 2017 involved opioids, amounting to over 130 overdose deaths per day due to opioids (see for instance Centers for Disease Control and Prevention, 2018 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, 2018). This relationship between local economic distress and drug deaths has led some prominent 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 possibility of a relationship between labor market conditions and opioid abuse in the United States (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; Torbin and Nielsen, 2017). The subset of studies concerned with whether despair among workers due to worsening labor market conditions induces opioid use have thus far failed to definitively answer whether worsening labor market conditions could be a strong determinant of opioid abuse in the United States. While Charles, Hurst, and Schwartz (2018) argue that labor market dislocations associated with reduced manufacturing share of employment may increase demand for opioids among workers in affected labor markets, Ruhm (2019) and Currie, Jin, and Schnell (2018) do not observe sufficiently strong relationships between opioid use and their respective proxies for local labor market conditions to reach the same conclusion.

Notably, Case and Deaton's (2017) suggestion that worsening labor market conditions are important drivers of opioid use relies on the idea that workers or potential workers begin using or abusing opioids in response to their own poor labor market outlook. In this sense, Case and Deaton's (2017) argument is that worsening labor market conditions cause individuals to demand more opioids. Because Case and Deaton's proposal hinges on workers' and potential workers' demand for opioids, existing studies' reliance on county-level data is an insuperable weakness insofar as it renders existing studies unable to separately measure the impact of labor market dislocations on person-specific determinants of opioid use and place-specific determinants of use.<sup>1</sup>

This study leverages publicly available data from the Medical Expenditure Panel Survey (MEPS) to provide the first demand-side estimates of the effect of labor market dislocations on opioid use in the United States context. I focus on prime-age individuals who experience job displacement, which I define as losing a job due to (1) layoffs (2) place of employment dissolving or closing or (3) term of employment ending. I restrict my sample to include only individuals who were employed during the first period of their survey participation and who did not use opioids during this period. I then use a linear probability model in which my independent variable of interest is an indicator for experiencing job displacement and my dependent variables are indicators for exceeding various thresholds opioid use, as measured by individuals' number of opioid prescriptions and whether they ever had very high doses of opioids in terms of morphine milligram equivalents (MME) per day.

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<sup>1</sup>As I will discuss in the the following section, Finkelstein, Gentzkow, and Williams (2018) argue that person-specific determinants of opioid use and place-specific determinants of opioid use can be thought of as "opioid demand" and "opioid supply," respectively.

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 characteristics. While regression estimates of the effect of displacement on opioid use will suffer from simultaneity bias under my specification due to my inability to observe the exact relative timing of displacement and opioid use, I argue that I am able to sign the simultaneity bias as positive by placing plausible restrictions on the true causal effect of displacement on opioid use and vice versa. I argue further that coefficients from the regression of opioid abuse indicators on job displacement will be upper bounds on the effects of displacement on prescription opioid abuse.

I show that job displacement does not appear to have been 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 displacement appears to be associated with slightly lower likelihood of opioid use. To the extent that displacement is associated with a reduction in individuals' propensity to begin using or abusing opioids, I find that the effects are driven by individuals who experience non-layoff displacement. Importantly, I find that neither layoffs nor non-layoff displacement are associated with increases in likelihood of opioid use based on whether individuals worked in blue-collar occupations during their first reference period of survey participation, or based on whether they are non-Hispanic whites. This would appear to cast doubt on Case and Deaton's (2015; 2017) hypothesis that labor market dislocations are particularly important drivers of increasing mortality among non-Hispanic whites working in these sorts of occupations.

I present some evidence that displacement-related income reductions are the mechanism driving reductions in probability of high-threshold opioid use associated with displacement. In particular, I find that among individuals who are likely to have experienced less of an income reduction upon being displaced – individuals who spent all reference periods of their survey participation employed, individuals whose labor income did not make up the majority of their dwelling unit's income in their first year of survey participation, and individuals whose dwelling units reported nonzero business and trust income in the first year of survey participation – displacement does not appear to be associated with reductions in the 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 did spend at least one reference period without working, individuals whose labor income made up the majority of their dwelling unit's income during the first year of their survey participation, and individuals who reported zero business or trust income.

The results of this study suggest that increased demand for opioids associated with short-term labor market dislocation among prime-age workers is unlikely to be a strong driver of increasing opioid deaths in recent years. To the extent that labor market dislocations are to blame for increasing opioid abuse, these effects may be more likely to be driven by increases in opioid supply associated with labor market dislocations, or opioid demand increases associated with longer-term labor market dislocations such as multiple job displacements over a longer period of time than I am able to observe.

## 2 Background

Opioid abuse as a subject of study in economics can be traced in large part to Case and Deaton's (2015) finding that midlife mortality among non-Hispanic whites has been on the rise in the United States over the past two decades, and their attribution of this trend to so-called "poisonings," a blanket term they use to characterize deaths due to drug or alcohol overdoses. Case and Deaton's (2017) follow-up paper, which suggests that worsening economic circumstances for middle-aged non-Hispanic whites may have contributed to rising poisoning

deaths, further stoked curiosity regarding interplay between job displacement and opioid use. This curiosity coalesced into several studies which, like my own, examine whether labor market dislocations induce opioid use (Charles, Hurst, and Schwartz, 2018; Currie, Jin, and Schnell, 2018; Ruhm, 2019; Hollingsworth, Ruhm, and Simon, 2017; Roulet, 2017), as well as papers examining 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). Due to data availability constraints, most of these studies have either focused on opioid overdose deaths, which are a somewhat noisy proxy for overall opioid use, or prescription opioid abuse, which is distinct from but highly correlated with illicit opioid abuse.<sup>2</sup>

In proposing that worsening economic conditions have contributed to increasing “deaths of despair” vis-à-vis 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. In the spirit of testing the story Case and Deaton call “preliminary but plausible,” a handful of economists have worked to study whether medium- and short-term economic shocks cause greater prescription opioid use.

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.<sup>3</sup> Roulet (2017) finds no effect of job displacement on opioid use; however, there is reason to believe that the United States context would differ importantly 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 (more generous than in the U.S.) prevent large reductions in healthcare spending associated with job displacement. These two differences between the Danish context and the United States context suggest that two most obvious determinants of post-displacement prescription opioid use or abuse – namely, 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.<sup>4</sup>

The studies which most closely resemble my own using U.S. data are Ruhm (2019), Currie, Jin, and Schnell (2018), and Charles, Hurst and Schwartz (2018), all of which use county-level data from the United States to determine whether changes in economic circumstances cause greater opioid use.<sup>5</sup> Ruhm (2019) is perhaps most faithful to the letter of Case and Deaton (2017) insofar as 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. Currie, Jin, and Schnell (2018) and Charles, Hurst, and Schwartz (2018), on the other hand, focus on transitory fluctuations in labor market conditions, rendering their research designs more similar to my

<sup>2</sup>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.

<sup>3</sup>This analysis is part of a larger study on whether job displacement has a negative effect on health status in Denmark, given Denmark’s generous social safety net.

<sup>4</sup>Differences between in the health effects of job loss in Europe and the United States have been discussed in previous work, notably Schaller and Stevens (2015) who write that “The findings of relatively small health effects [of displacement] in Europe could reflect that job loss may be less of an economic shock in Europe than in the United States, given more generous social insurance and employment stability policies.”

<sup>5</sup>Charles, Hurst, and Schwartz’s (2018) interest in the interplay between opioid use and labor market dislocation is a small part of a larger study on the effects of declining manufacturing share of employment on local labor markets.

own. Currie, Jin, and Schnell (2018) use the shift-share (Bartik) instrument to measure the effect of a plausibly exogenous shift in the employment-to-population ratio on county-level opioid prescribing rates (Bartik, 1991). Charles, Hurst, and Schwartz (2018) use the same instrument to measure the effect of a plausibly exogenous shift in county-level manufacturing shares of employment on a variety of opioid use metrics, including opioid prescriptions per capita, changes in the prevalence of opioid-related deaths, and positive drug test rates at the county level.

The results of these studies paint an inconclusive picture. Ruhm (2019) argues that, conditioning on county-specific characteristics, worsening economic conditions may cause an uptick in the drug death rate. He qualifies this proposition by noting two caveats: first, he estimates that economic decline accounts for no more than one tenth of the change in the drug death rate and, second, in Ruhm's words, "a small amount of remaining omitted variables bias would be sufficient to completely eliminate the contributions of economic factors [to changes in opioid use], making it quite plausible that they play no role at all." Currie, Jin, and Schnell's (2018) results largely suggest no relationship between employment-to-population ratios and opioid prescribing rates. Though they find some evidence of an inverse relationship among young workers in highly educated counties, the authors are hesitant to interpret this relationship as causal. They explain that their findings are probably not driven by less "despair" among younger workers in high employment-to-population ratio counties, but rather young workers being able to "be more selective about their jobs and...avoid jobs that cause them pain or injury." Charles, Hurst, and Schwartz (2018), however, find the opposite, showing strong relationships between declining manufacturing share of employment and opioid use metrics in their two-stage least squares specifications.

Beyond failing to come to a consensus regarding the relationship between economic conditions and opioid use in the United States, the existing literature also importantly fails to adequately 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 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 among potential or former workers in order 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 imply a causal link, insofar as both recognize that their measures are likely very 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.

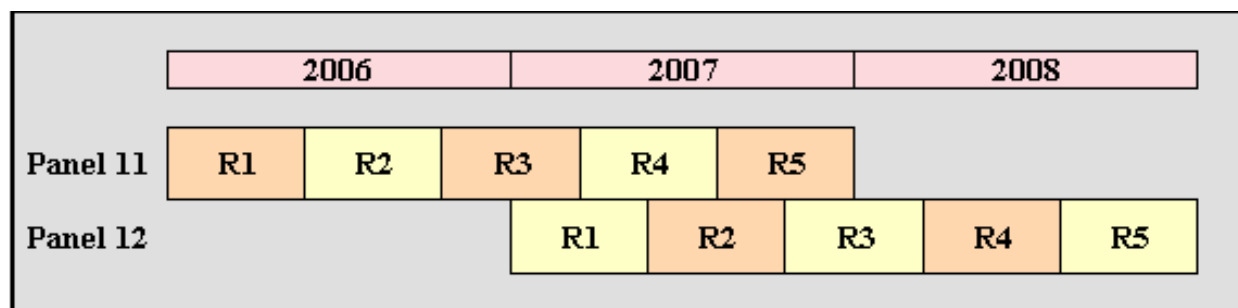
Ultimately, microdata on economic dislocation and prescription opioid are likely to provide the clearest insights on demand-side effects of the former on the latter, but, thus far, no study has used such data in the United States context. The primary project of this study is to begin to fill this gap in the literature by using MEPS data, which enables me to understand both job displacement and opioid use over time at the individual level. Overall, my analysis lends support to Ruhm's (2018) suggestion that changes in place-specific determinants of opioid use rather than person-specific determinants thereof are the primary cause of upticks in opioid use proxies caused by economic dislocations.

### 3 Data and Descriptive Results

#### 3.1 Data source

My project makes use of publicly available data collected through the MEPS, a nationally representative survey of members of the United States civilian non-institutional population. The MEPS is administered by the Agency for Healthcare Research and Quality, which operates under the auspices of the U.S. Department of Health and Human Services. MEPS administrators interview survey participants five times over the course of two years. Correspondingly, each participant's two-year participation period is partitioned into five reference periods of roughly equal length, each corresponding to a round of interviews. Furthermore, 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 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.

Figure 1: Diagram of the MEPS' overlapping panel design



Source: [https://meps.ahrq.gov/data\\_files/publications/mr25/mr25.shtml#Figure1](https://meps.ahrq.gov/data_files/publications/mr25/mr25.shtml#Figure1)

My analysis primarily relies on two types of data files. First are longitudinal files, for which the unit of observation is the MEPS survey participant. Longitudinal files contain demographic and financial information, including race, ethnicity, gender, round-by-round employment status, round-by-round health insurance coverage, round-by-round self-reported health status, etc. Second are the prescribed medicines files, for which the unit of observation is the prescription. Unlike longitudinal files, which exist for each panel (1-21) of MEPS participants, prescribed medicines files are available for each year 1996-2017, and each year's data files contains all prescriptions received and reported by each individual participating in the MEPS during that year. Prescriptions in the prescribed medicines files can be linked to individuals in the longitudinal files using panel number and

person-level identification numbers.<sup>6</sup> Information in longitudinal files is primarily obtained through in-person and self-administered interviews, though some information, such as information regarding participants' insurance coverage, is obtained by MEPS administrators on a monthly basis. The prescribed medicines files contain records for all prescriptions received by MEPS survey 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).<sup>7</sup> MEPS administrators obtain prescription information from in-person interviews with MEPS survey participants and obtain permission from survey participants to follow up with pharmacies they list as having provided medicines to them.

### 3.2 Sample selection

As I discuss in greater detail in subsection 4, one important threat to identification under my research design is the possibility of negative selection into job displacement on dimensions correlated with propensity to use opioids. In the interest of minimizing this threat, I impose a handful of sample restrictions. First, I restrict my sample to prime-age individuals, who are less likely to be marginal workers than their younger and older counterparts, and who are less likely than their older counterparts to be beset with chronic muscular-skeletal pain for which opioids are often prescribed.<sup>8</sup> 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, so as 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 for two reasons. My primary reason for doing so is that my definition of job displacement, to be discussed further 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 approach is motivated in the literature; in particular, Hilger (2016) argues that, in general, exogeneity of job displacement is not a viable assumption when individuals in the treatment group cannot be screened prior to treatment for characteristics predictive of the outcome of interest.

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 table 1. Note that 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

<sup>6</sup>Prescribed medicines files from 1996 to 2013 do not contain panel IDs for each prescription, so I merge in panel identifiers for prescriptions in these files from MEPS full year consolidated data files.

<sup>7</sup>It is not clear what proportion of all opioid prescriptions in the United States are received in an outpatient setting as opposed to a hospital, clinic, or physician's office, as no publicly available data dis-aggregate on these dimensions (see the U.S. Substance Abuse and Mental Health Service Association's 2017 catalogue of publicly available data sources on opioid use). Regardless of what proportion of overall opioid prescriptions are covered by the MEPS, prescription opioids intended for outpatient use are likely a particularly valuable subject of study from a policy standpoint, since opioids prescribed for outpatient use are more likely to be abused than opioids prescribed for use under physician supervision.

<sup>8</sup>Roulet (2017) also focuses on workers strongly attached to the labor force to avoid the threat of negative selection into displacement on health-related unobservables, though she uses workers' job tenure at their current firm as a selection criterion. Restricting on length of job tenure is not feasible using the MEPS data.

at least a college degree, likely a byproduct of the fact that these individuals are overrepresented among working individuals in the United States.

### 3.3 Identifying job displacement

While the MEPS collects detailed round-by-round information on survey participants' labor market activities, MEPS interviewers do not specifically ask participants whether they experience job displacement. As such, I follow Schaller and Stevens (2015) in constructing indicators for job displacement by classifying an individual as having been displaced in a round if they report during that round of interviews that they switched their current main job because (1) they were laid off (2) the business where they previously worked dissolved or closed or (3) their job ended. Individuals can "switch" current main jobs into unemployment; they need not work in the post-displacement period. A critically important byproduct of inferring job displacement using this variable is that I am only able to observe that individuals were displaced at some point between rounds of MEPS interviews. I am not able to pinpoint exactly when they were displaced. A more extended discussion of the implications of this data constraint for identification purposes are in order in subsection 4.

Summary statistics regarding the prevalence of job displacement thus defined, both overall and disaggregated by displacement type, among members of the analysis sub-sample are presented in table 2. Because I restrict the analysis sub-sample to individuals who report being employed in the reference period corresponding to the first round of MEPS interviews, it is natural that the proportion of members of the analysis sub-sample who experience displacement decreases as rounds progress. This is because my definition of displacement requires an individual to have a current main job in the round in which they are displaced; by restricting individuals in the analysis sample to be employed in the reference period corresponding to round one but not other rounds, all members of the analysis sub-sample can be displaced in the first round under my definition of displacement, but some proportion of the analysis sub-sample will not work in the second round and as such will be unable to meet my criteria for displacement.

As table 2 shows, 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 proportion of individuals, roughly one tenth of a percent of my analysis sample, experience both layoffs and non-layoff displacement during their participation in the MEPS.

### 3.4 Identifying prescription opioid use

My primary outcome of interest is the probability that survey participants begin to use or abuse opioids. As I discuss in sub-subsection 3.1, MEPS administrators obtain prescription information from in-person interviews with MEPS survey participants and from pharmacies to which they are referred MEPS participants. 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 in the MEPS data as being an opioid prescription if it meets two of the three following criteria:<sup>9</sup>

1. The drug matches based on its NDC to a list of opioid drugs compiled by the CDC.<sup>10</sup>

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<sup>9</sup>My prescription classification methods most closely follow that of Soni (2018), but are also informed by Moriya and Miller (2018), Moriya and Miller (2018b), Stagnitti (2017), Groenewald et al. (2016), Zhan et al. (2001), and Zhou, Florence, and Dowell (2016). An in-depth discussion of opioid classification in the MEPS Prescribed Medicines files is not in order here, so I save it for appendix A.

<sup>10</sup>This 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.



Table 1: Demographic characteristics of MEPS prime-age participants

	(1)		(2)		(3)	
	All prime-age in MEPS		Analysis sample		MME analysis sample	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
<i>U.S. Census Region</i>						
Northeast	0.18	0.39	0.18	0.39	0.17	0.38
Midwest	0.22	0.41	0.22	0.42	0.21	0.41
South	0.36	0.48	0.36	0.48	0.37	0.48
West	0.23	0.42	0.23	0.42	0.24	0.43
<i>Ten-year Age Group</i>						
25-34	0.33	0.47	0.32	0.47	0.33	0.47
35-44	0.34	0.47	0.34	0.47	0.32	0.47
45-54	0.33	0.47	0.34	0.47	0.34	0.47
<i>Sex</i>						
Female	0.51	0.50	0.46	0.50	0.47	0.50
Male	0.49	0.50	0.54	0.50	0.53	0.50
<i>Race</i>						
White	0.80	0.40	0.81	0.39	0.79	0.41
Black	0.12	0.33	0.11	0.32	0.12	0.32
American Indian/Alaska Native	0.01	0.09	0.01	0.08	0.01	0.08
Asian/Pacific Islander	0.06	0.23	0.06	0.23	0.07	0.25
Multiple races	0.01	0.11	0.01	0.11	0.02	0.14
<i>Ethnicity</i>						
Not Hispanic	0.85	0.36	0.85	0.36	0.83	0.38
Hispanic	0.15	0.36	0.15	0.36	0.17	0.38
<i>Marital Status</i>						
Married	0.59	0.49	0.60	0.49	0.59	0.49
Widowed/divorced/separated	0.15	0.36	0.15	0.36	0.14	0.34
Never married	0.25	0.43	0.25	0.43	0.28	0.45
<i>Educational Attainment</i>						
No degree	0.11	0.32	0.09	0.28	0.08	0.27
GED or HS diploma	0.49	0.50	0.48	0.50	0.47	0.50
Four-year degree	0.21	0.41	0.24	0.42	0.25	0.44
Master's, doctoral, or professional degree	0.10	0.30	0.12	0.32	0.14	0.35
Other degree	0.08	0.27	0.08	0.27	0.06	0.23
Observations	129,911		78,819		30,859	

Proportions and standard deviations are computed using survey weights from longitudinal data files. Regions are census regions, which is the most granular geographic category available in MEPS data. I assign individuals in the MEPS to ten-year age categories according to their age in their first year of survey participation. Race categories are constructed as a harmonization between two different race categorizations used over the course of MEPS panels 1-21, one applying to panels 1-5 and another applying to panels 6 onward. Marital Status variables are constructed as aggregates from more granular marital status categories in the raw MEPS data. Educational attainment variables are constructed as a harmonization between three different educational attainment schemas, one applying to panels 1 and 2 (the HIDEG1 variable), another applying to panels 3-8 (the HIDEGYR variable), another applying to panels 9-16 and 20 (the HIDEG variable), and finally another applying to panels 17-20 (the EDRECODE variable). Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. MME analysis sample is composed of all individuals in the analysis sample who entered the sample on or after 2010.

Table 2: Proportions of MEPS participants who are displaced

	(1)		(2)		(3)	
	All prime-age in MEPS		Analysis sample		MME analysis sample	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Displaced	0.08	0.27	0.09	0.28	0.08	0.27
Displaced in round 1	0.03	0.16	0.03	0.17	0.03	0.17
Displaced in round 2	0.02	0.15	0.03	0.16	0.02	0.15
Displaced in round 3	0.02	0.15	0.02	0.15	0.02	0.14
Displaced in round 4	0.02	0.13	0.02	0.13	0.01	0.12
Displaced due to layoff	0.04	0.19	0.04	0.20	0.03	0.18
Laid off in round 1	0.01	0.10	0.01	0.11	0.01	0.11
Laid off in round 2	0.01	0.10	0.01	0.11	0.01	0.10
Laid off in round 3	0.01	0.10	0.01	0.10	0.01	0.09
Laid off in round 4	0.01	0.09	0.01	0.09	0.01	0.08
Displaced b/c bus. diss. or sold/job ended	0.05	0.21	0.05	0.22	0.05	0.21
Bus. diss. or sold/job ended in round 1	0.01	0.12	0.02	0.13	0.02	0.13
Bus. diss. or sold/job ended in round 2	0.01	0.12	0.01	0.12	0.01	0.11
Bus. diss. or sold/job ended in round 3	0.01	0.11	0.01	0.11	0.01	0.10
Bus. diss. or sold/job ended in round 4	0.01	0.09	0.01	0.09	0.01	0.09
Observations	129911		78819		30859	

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. Proportions and standard deviations 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. MME analysis sample is composed of all individuals in the analysis sample who entered the sample on or after 2010.

2. The drug's nonproprietary name is that of an opioid.

3. The drug's proprietary name is that of an opioid.

Because MEPS participants must report their own prescription opioid use in order for their opioid prescriptions to be included in the prescribed medicines files, it is reasonable to be concerned that the MEPS data will underrepresent the degree to which Americans use opioids. In particular, stigma around drug use might prevent MEPS participants from reporting large amounts of opioid use. To check whether the MEPS data is a reasonable measure of Americans' actual prescription opioid use, I compare it with data on aggregate prescription opioid use reported by CDC and obtained by IQVIA.<sup>11</sup> Figure 2 plots opioid prescriptions per 100 individuals in the United States as computed in the MEPS and as reported in by the CDC. I estimate using the MEPS that the prescribing rate for all opioid medications as well as all medications not used in medication-assisted treatment (MAT) is higher than reported by the CDC. However, when I exclude opioid cough medicines (which are excluded in the CDC's prescribing rate statistics), the prescribing rates implied by the MEPS data are generally quite close to the prescribing rates reported by the CDC, though some years' prescribing rates are closer than others.<sup>12</sup> In general, figure 2 seems to justify drawing conclusions about opioid use from MEPS data.

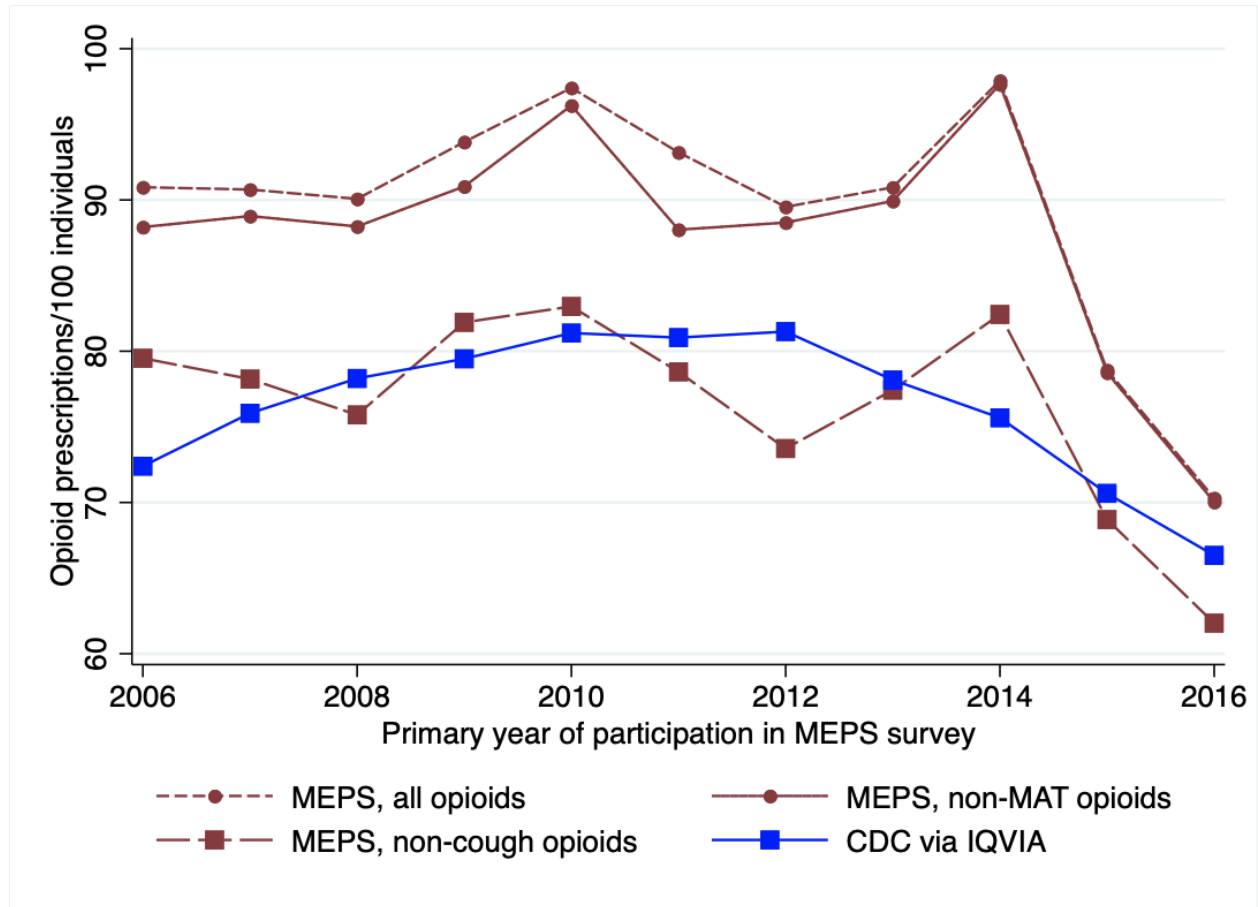
While a variety of metrics for opioid abuse exist in the literature, I focus on (1) indicators for individuals exceeding various thresholds of opioid prescriptions over the course of their survey participation and (2) indicators for individuals exceeding various thresholds of morphine milligram equivalent (MME) dosage per day.<sup>13</sup> I

<sup>11</sup> CDC IQVIA prescribing rates are taken from CDC Opioid Overdose Data: U.S. Opioid Prescribing Rate Maps, last updated October 2018.

<sup>12</sup> My methods for classifying opioid prescriptions as corresponding to cough medicines or medication-assisted treatment are discussed in detail in appendix A.

<sup>13</sup> Finkelstein, Gentzkow, and Williams (2018) provide the widest review in the economics literature of potential opioid abuse proxies

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 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 A.2) except those which contain the components "PHENYLEPHRINE", "GUAIFENESIN", "PROMETHAZINE", "CHLORPHENIRAMINE", "HOMATROPINE", "TRIPOLIDINE", "DIPHENHYDRAMINE", "BROMPHENIRAMINE", or "BROMODIPHENHYDRAMINE."

can only compute the latter 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.<sup>14</sup>

Because my outcome of interest is the probability of beginning to use or abuse opioids, I do not count opioids used in medication-assisted treatment for substance abuse disorder, namely buprenorphine and methadone, in these computations. With my analysis sample constructed to exclude survey participants who have nonzero prescription opioid use in the reference period corresponding to the first round of interviews, indicators for exceeding various thresholds of opioid prescriptions or MME amount to indicators for whether survey participants began using prescription opioids to varying extents.

It is difficult to specify how many opioid prescriptions might reasonably correspond to opioid abuse, though some estimates exist in the public health literature. Rice et al. (2012), for instance, 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 is likely to overstate the number of prescriptions which might correspond 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 table 3. By my estimates, 24.1% of Americans received a non-MAT opioid prescription between 1997 and 2015, 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, which is consistent with the theory outlined by Krueger (2017) that opioid use is concentrated among individuals with gaps in their employment status, and who are using opioids often. I discuss this idea in further detail in subsection 3.6. I also show results corresponding to indicators for accumulating between one and 15 opioid prescriptions in appendix C.

Correspondency between MME per day and opioid abuse is somewhat better established. A patient is called "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  $\geq 90$  MME per day." Finkelstein, Gentzkow, and Williams (2018) set the threshold for abuse at 120 MME per day. I report regression results on 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 present summary statistics regarding the prevalence of prescription opioid use by these metrics in table 4. 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 in table 3, these proportions are lower among individuals in my analysis sample

which can be computed using information regarding opioid prescriptions. These include (1) filling opioid prescriptions from four or more prescribers (2) filling prescriptions in any calendar quarter which result in 120 or more morphine milligrams equivalent (MME) per day, or (3) filling a new prescription before a previous one has run out (Finkelstein, Gentzkow, and Williams, 2018). Of these three abuse proxies, only the second proxy is feasible to compute using MEPS data. The MEPS Prescribed Medicines files neither identify prescribers of medicines nor record the dates when prescriptions are filled, though the MEPS does record the reference period during which survey participants received each prescription.

<sup>14</sup>For a full discussion of my imputation methods, see appendix A.

Table 3: Proportions of MEPS participants whose prescription opioid use exceeds prescription count thresholds

	(1)		(2)	
	All prime-age in MEPS Mean	Std. dev.	Analysis sample Mean	Std. dev.
Accumulated one or more opioid prescriptions	0.241	0.428	0.182	0.386
Accumulated 6 or more opioid prescriptions	0.036	0.186	0.011	0.102
Accumulated 12 or more opioid prescriptions	0.020	0.140	0.003	0.058
Observations	129911		78819	

The variables shown here are constructed by linking opioid prescriptions in prescribed medicines files to individuals represented in longitudinal files. Prescriptions are classified as opioid prescriptions according to the steps outlined in appendix subsection A.2. Analysis sample is defined as all prime-age individuals who are (1) employed during the reference period corresponding to the first round of MEPS participation. MME analysis sample is composed of all individuals in the analysis sample who entered the sample on or after 2010.

Table 4: Proportions of MEPS participants whose prescription opioid use exceeds MME thresholds

	(1)		(2)	
	All 2010+ prime-age Mean	Std. dev.	MME analysis sample Mean	Std. dev.
Ever had a prescription for greater than 60 MME per day	0.044	0.205	0.028	0.165
Ever had a prescription for greater than 90 MME per day	0.022	0.147	0.013	0.115
Ever had a prescription for greater than 120 MME per day	0.010	0.098	0.005	0.068
Observations	44881		30859	

The variables shown here are constructed by linking opioid prescriptions in prescribed medicines files to individuals represented in longitudinal files. Prescriptions are classified as opioid prescriptions according to the steps outlined in appendix subsection A.2, and MME per day for each prescription is computed as outlined in appendix subsection A.3. MME per day can only be computed from 2010 onward. 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.

at 2.8%, 1.3%, and 0.5%, respectively.

### 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 my analysis sample in table 5. For the most part, I construct the variables shown in table 5 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 my two mental health controls, 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."<sup>15</sup> It is also worth flagging here that MEPS interviewers do not ask about each health condition enumerated in table 5 during every round of interviews; for instance, MEPS interviewers only ask whether survey participants have experienced joint pain in rounds 3 and 5 of interviews, and only ask whether survey participants had health problems requiring immediate care in rounds 2 and 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 and occupation. My conditioning on pre-displacement industry and occupation is somewhat motivated by discussions in the literature. For instance, Charles, Hurst, and Schwartz (2018) argue that reductions in labor demand in manufacturing industries specifically may cause higher prescription opioid use through "substantial adverse effects on agents' wellbeing." Similarly, Currie, Jin, and Schnell (2018) speculate that the inverse relationship they observe between exogenous adverse shocks to county employment-to-population ratios and county-level prescription opioid use among younger workers in highly-educated counties may be due to workers in high employment-to-population ratio counties sorting into less pain-inducing occupations. The MEPS industry and occupation schemas, shown in table 6 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.

### 3.6 Trends in Opioid Prescribing According to the MEPS

Using the opioid classification method I describe in subsection 3.4, I find that opioid use varies considerably depending on surveyed individuals' employment. Figure 3 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 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 striking. Among individuals whose primary participation in the MEPS occurred in 1996, over 30% of individuals who never were employed during their survey participation reported receiving

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<sup>15</sup>I classify prescriptions in the prescribed medicines files as antidepressants or anti-psychotics if their therapeutic class is given as antidepressant or anti-psychotic.

Table 5: Proportions of MEPS participants facing health issues

	(1)		(2)		(3)	
	All prime-age in MEPS		Analysis sample		MME analysis sample	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Reported fair/poor mental health in R1	0.06	0.23	0.03	0.18	0.03	0.18
Rcvd. presc. for antidepressant/antipsychotic in R1	0.07	0.26	0.05	0.23	0.05	0.23
Ever reported limitations climbing stairs	0.08	0.27	0.04	0.20	0.04	0.19
Ever reported difficulty performing moderate activities	0.07	0.26	0.04	0.19	0.03	0.18
Ever reported experiencing illness/inj. requiring immed. care	0.38	0.49	0.35	0.48	0.33	0.47
Ever reported illness/inj. requiring specialist attention	0.39	0.49	0.37	0.48	0.35	0.48
Ever reported more likely to take risks than average	0.31	0.46	0.31	0.46	0.31	0.46
Ever reported health impeding social life	0.23	0.42	0.18	0.38	0.17	0.38
Ever reported taking aspirin daily	0.12	0.32	0.11	0.31	0.10	0.30
Ever reported undergoing hysterectomy	0.07	0.25	0.06	0.23	0.05	0.22
Ever reported using assistive device	0.03	0.17	0.01	0.11	0.01	0.11
Ever reported complete inability to do activity	0.06	0.23	0.01	0.11	0.01	0.11
Ever reported general phys. difficulty	0.12	0.33	0.08	0.27	0.08	0.27
Ever reported phys. difficulty impeding work	0.09	0.29	0.03	0.18	0.03	0.17
Ever reported joint pain	0.44	0.50	0.41	0.49	0.44	0.50
Ever reported difficulty bending/stooping	0.09	0.29	0.05	0.23	0.05	0.22
Ever reported difficulty grasping w/ fingers	0.04	0.20	0.02	0.14	0.02	0.13
Ever reported difficulty walking mile	0.10	0.29	0.05	0.23	0.05	0.22
Ever reported difficulty reaching overhead	0.06	0.24	0.03	0.17	0.03	0.17
Ever reported difficulty standing 20 mins	0.08	0.27	0.04	0.20	0.04	0.20
Ever reported difficulty walking 3 blks	0.09	0.28	0.05	0.21	0.05	0.21
Ever spent night inpatient in hospital	0.11	0.31	0.08	0.28	0.08	0.27
Ever missed work b/c illness/inj.	0.51	0.50	0.57	0.49	0.55	0.50
Observations	129911		78819		30859	

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.

at least one opioid prescription, compared to slightly less than 25% among individuals who reported always or sometimes working.<sup>16</sup> At its peak in 2004, a whopping 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 much 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 in estimates from the Princeton Pain Survey that 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

<sup>16</sup>I assign 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. For these latter individuals, I say the primary year of participation is their second year in the MEPS.

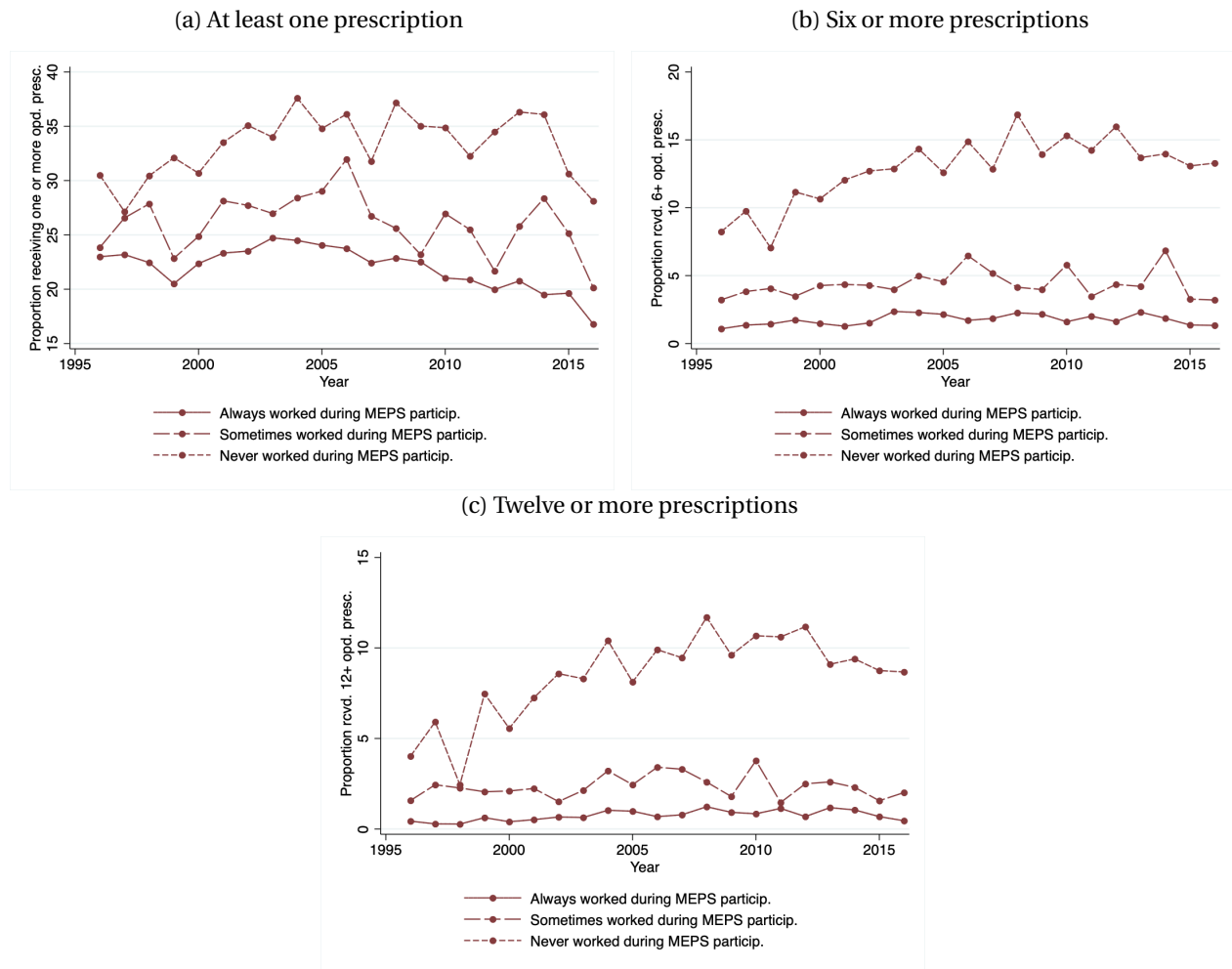
Table 6: Industry/occupation characteristics of MEPS prime-age participants employed in R1

	(1)		(2)		(3)	
	All prime-age in MEPS		Analysis sample		MME analysis sample	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
<i>Industry (~ NAICS 1-digit)</i>						
Natural Resources	0.01	0.11	0.01	0.11	0.01	0.10
Mining	0.00	0.06	0.00	0.07	0.00	0.07
Construction	0.06	0.23	0.07	0.26	0.07	0.25
Manufacturing	0.10	0.31	0.12	0.33	0.11	0.31
Wholesale And Retail Trade	0.11	0.31	0.13	0.33	0.12	0.32
Transportation And Utilities	0.05	0.21	0.05	0.22	0.05	0.22
Information	0.01	0.12	0.02	0.14	0.02	0.15
Financial Activities	0.05	0.23	0.07	0.25	0.07	0.25
Professional And Business Services	0.13	0.34	0.15	0.35	0.13	0.33
Education, Health, And Social Services	0.13	0.34	0.19	0.39	0.25	0.43
Leisure And Hospitality	0.04	0.21	0.06	0.24	0.08	0.26
Other Services	0.05	0.22	0.06	0.23	0.05	0.21
Public Administration	0.05	0.21	0.05	0.23	0.06	0.23
Military	0.00	0.04	0.00	0.04	0.00	0.04
Unclassifiable Industry	0.00	0.05	0.00	0.03	0.00	0.04
<i>Occupation (~ SOC 1-digit)</i>						
Management, Business, And Financial Oper	0.13	0.34	0.17	0.37	0.17	0.38
Professional And Related Occupations	0.19	0.39	0.24	0.43	0.26	0.44
Service Occupations	0.11	0.32	0.14	0.35	0.16	0.36
Sales And Related Occupations	0.07	0.26	0.09	0.28	0.08	0.27
Office And Administrative Support	0.10	0.30	0.12	0.33	0.11	0.32
Farming, Fishing, And Forestry	0.01	0.09	0.01	0.09	0.01	0.08
Construction, Extraction, And Maintenanc	0.08	0.27	0.10	0.30	0.08	0.28
Production, Trnsportation, Matrl Moving	0.10	0.30	0.13	0.33	0.12	0.32
Military Specific Occupations	0.00	0.04	0.00	0.04	0.00	0.04
Observations	129,911		78,819		30,859	

Industry and occupation variables are constructed as harmonizations between two sets of industry and occupation variables, one set pertaining to individuals in panels 1-6 and another pertaining to individuals in subsequent MEPS panels. The industry and occupation schemas roughly align with 1-digit NAICS and SOC schemas, respectively. 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.

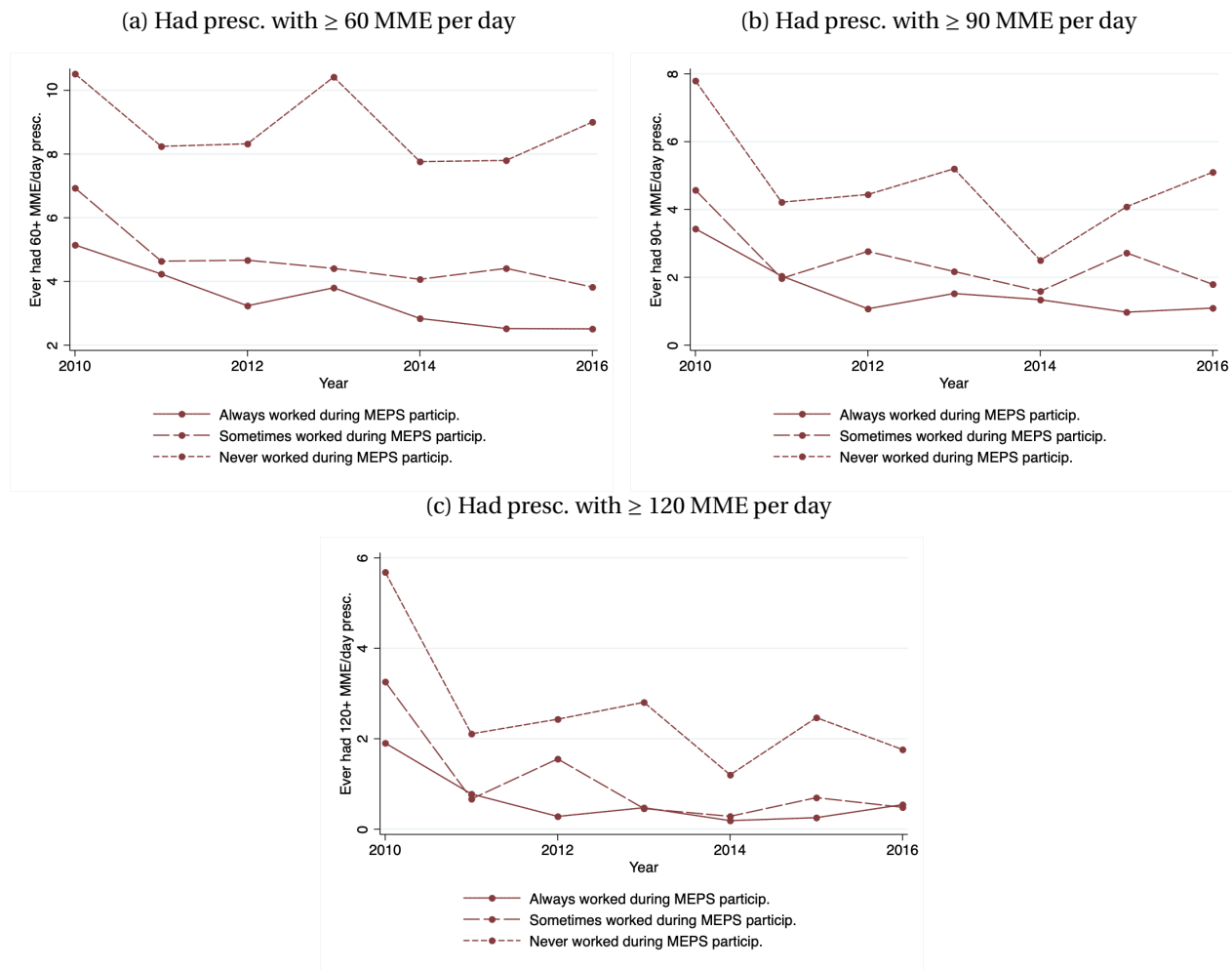


Figure 3: Proportions of prime-age individuals exceeding thresholds of non-MAT opioid prescription receipt in the United States, by employment activity



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.

Figure 4: Proportions of prime-age individuals exceeding MME per day thresholds in the United States, 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.

(2017) readily volunteers, the cross-sectional relationship is merely suggestive. In the following section, I outline a framework by which I can proceed in the direction of providing 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 job displacement, a vector of panel fixed effects, and the demographic, health status, industry, and occupation variables enumerated in tables 1, 5, and 6, respectively. Written out formally, this amounts to

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

where  $\text{EVER\_DISPLACED}_i$  is an indicator for ever having experienced job displacement during an individuals' survey participation,  $X_i$  is a vector containing the demographic, health status, industry, and occupation variables enumerated in tables 1, 5, and 6,  $\text{PANEL}_i$  is a vector consisting of indicators for being in each panel, and  $\epsilon_i$  is an error term. Because I can observe whether workers in my sample are laid off due to layoffs or other causes, I report all estimates of  $\beta$  in equation 1 as estimated using all displacement, displacement due to layoffs, and displacement due to other causes as the independent variable. I discuss the reasoning behind this in detail in subsection 4.2.

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.<sup>17</sup> Note that, for individuals in my analysis sample, we have:

$$\mathbb{1} \left( \sum_{\text{round}=2}^5 \text{OPIOID\_PRESCRIPTION\_COUNT}_{i,\text{round}} \geq k \right) = \mathbb{1} \left( \sum_{\text{round}=1}^5 \text{OPIOID\_PRESCRIPTION\_COUNT}_{i,\text{round}} \geq k \right)$$

since none of these individuals have any opioid prescriptions during the reference period corresponding to the first round of MEPS interviews.

Also recall from sub-subsection 3.4 that I am only able to 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.<sup>18</sup> Because of this data constraint, I am unable to measure the effect of displacement on opioid abuse as measured using MME per day-based metrics as I am using opioid count metrics.

<sup>17</sup>Recall from subsection 3.4 that I show (2) for  $k \in \{1, 2, 3, \dots, 15\}$  in appendix C.

<sup>18</sup>For a detailed discussion of my imputation methods, see appendix subsection A.3.

## 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,  $\beta$ , the coefficient on  $\text{EVER\_DISPLACED}_i$  in equation 1, will be at best a rough proxy for the causal effect of displacement. To see this, 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  $\beta$  to yield the causal effect of displacement, not only would  $\text{EVER\_DISPLACED}_i$ ,  $\epsilon_i$  need to satisfy  $\text{cov}(\text{EVER\_DISPLACED}_i, \epsilon_i | X_i, \text{PANEL}_i)$ , 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 these shortcomings, why study equation 1 at all? I will argue first that equation 1 is the best available specification due to data constraints, and second that estimates of  $\beta$  may still be informative if I make reasonable assumptions about the direction in which survey participants like individual A are likely to push  $\beta$  away from the true causal effect of job displacement.

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 sub-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. Namely, 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.<sup>19</sup> This data constraint – my inability to accurately observe the relative timing and displacement of displaced individuals – is an unfortunate and insuperable feature of the MEPS.

To the second point, my analysis may be redeemed somewhat if I assume that individuals such as individual A – individuals who use opioids prior to being displaced – are made more likely to be displaced by their opioid use. To see this, it is useful to consider my main specification – equation 1 – in terms of possible simultaneous causality. Job displacement may affect an individuals' opioid use, but individuals' opioid use may also affect an individuals' likelihood of being displaced. We can model this formally as follows. Let  $X'_i\gamma + \text{PANEL}'_i\rho = Z'_i\delta_1$ . Then we have the two simultaneous equations:

$$Y_i = \alpha_1 + \beta_1 \text{EVER\_DISPLACED}_i + Z'_i\delta_1 + \epsilon_i \quad (5)$$

$$\text{EVER\_DISPLACED}_i = \alpha_2 + \beta_2 Y_i + Z'_i\delta_2 + \omega_i \quad (6)$$

<sup>19</sup>It is also worth noting in passing that, as discussed in sub-subsection 3.5, many of the health status variables in  $X_i$  are not 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.

We have from equation 5 that  $\beta_1$  will be given by:

$$\beta_1 = \frac{\text{cov}(Y_i - Z_i' \delta_1, \text{EVER\_DISPLACED}_i) + \text{cov}(\epsilon_i, \text{EVER\_DISPLACED}_i)}{\text{var}(\text{EVER\_DISPLACED}_i)}$$

Using equation 6 to compute  $\text{cov}(\text{EVER\_DISPLACED}_i, \epsilon_i)$  yields:

$$\text{cov}(\text{EVER\_DISPLACED}_i, \epsilon_i) = \beta_2 \text{cov}(Y_i, \epsilon_i) + \text{cov}(Z_i' \delta_2, \epsilon_i) + \text{cov}(\omega_i, \epsilon_i)$$

Assuming  $\text{cov}(\omega_i, \epsilon_i) = 0$  yields:

$$\begin{aligned} \text{cov}(\text{EVER\_DISPLACED}_i, \epsilon_i) &= \beta_2 \text{cov}(\alpha_1 + \beta_1 \text{EVER\_DISPLACED}_i + Z_i' \delta_1 + \epsilon_i, \epsilon_i) + \text{cov}(Z_i' \delta_2, \epsilon_i) \\ \Rightarrow \text{cov}(\text{EVER\_DISPLACED}_i, \epsilon_i) &= \frac{(\beta_2 + 1) \text{cov}(Z_i' \delta_1, \epsilon_i) + \beta_2 \text{var}(\epsilon)}{1 - \beta_1 \beta_2} = \frac{\beta_2 \text{var}(\epsilon)}{1 - \beta_1 \beta_2} \text{ if } \beta_1 \beta_2 \neq 1 \end{aligned}$$

which all implies:

$$\beta_1 = \frac{\text{cov}(Y_i - Z_i' \delta_1, \text{EVER\_DISPLACED}_i) + \frac{\beta_2 \text{var}(\epsilon)}{1 - \beta_1 \beta_2}}{\text{var}(\text{EVER\_DISPLACED}_i)}$$

The simultaneity bias, then, has the same sign as  $\frac{\beta_2 \text{var}(\epsilon)}{1 - \beta_1 \beta_2}$ . To determine the direction in which  $\beta$  as specified in equation 1 will differ from the causal effect of job displacement on prescription opioid use, we need to make a judgment regarding the signs and magnitudes of  $\beta_1$  and  $\beta_2$ , in other words, the effect of job displacement on the probability of opioid use and the effect of opioid use on the probability of job displacement.

In order to understand the direction of the simultaneity bias here, we have two options. First, we can turn to the literature. As I have already discussed in detail in section 2, however, I am concerned that estimates of the effect of displacement on opioid use in the literature, virtually all of which are estimated at the county-level, are unlikely to be informative regarding the coefficient in equation 1, which is estimated at the individual level, due to the effect that changes in opioid prescribing at the county level will reflect place-specific determinants of opioid use correlated with economic changes in addition to person-specific changes. A more viable option, then, may be to compare settings in which  $\beta_2$  is likely to vary, and compare our estimates of  $\beta_1$  in these different cases.

In order to employ this strategy – to generate two contexts in which  $\beta_2$  is likely to vary – I allow my definition of displacement to vary. As discussed in section 3.3, I am able to observe whether workers are displaced due to layoffs, establishment closure, or job ending. Conventionally, labor economists have judged managers to have more discretion in choosing which workers to lay off than in determining which workers are displaced due to establishment closure or other causes (see e.g. Gibbons and Katz, 1991). If opioid use affects the likelihood of displacement through the avenue of observed worker productivity, opioid use is more likely to affect likelihood of displacement through layoffs than displacement through establishment closure.<sup>20</sup> In other words, if I rewrite equation 6 as two distinct models:

$$\text{LAID\_OFF}_i = \alpha_{2a} + \beta_{2a} Y_i + Z_i' \delta_{2a} + \nu_i \quad (7)$$

$$\text{NONLAYOFF\_DISPLACED}_i = \alpha_{2b} + \beta_{2b} Y_i + Z_i' \delta_{2b} + u_i \quad (8)$$

<sup>20</sup>Notably, productivity does not appear to be totally unrelated to establishment closure, however, as Hilger (2016) shows that workers displaced due to firm closure are negatively selected.

we expect  $|\beta_{2a}| \geq |\beta_{2b}|$ , since the productivity channel between opioid use and layoffs is stronger than the productivity channel between opioid use and non-layoff displacement, though the signs of these two are likely the same. Moreover, we expect that  $|\beta_{2b}| \leq |\beta_{2a}| \leq 1$ , since it would be surprising if opioid use corresponded to an increase or decrease in the probability of displacement – layoff or non-layoff – of greater than 100 percentage points conditional on the control vector  $Z_i$ . Finally, we expect  $|\beta_1| \leq 1$  for the same reason.

If I accept these assumptions, namely, (1) that the sign of  $\beta_{2a}$  and  $\beta_{2b}$  are likely to be the same, (2) that  $|\beta_{2a}| \geq |\beta_{2b}|$ , and (3)  $\beta_1, \beta_{2a}, \beta_{2b} \in [-1, 1]$ , then two corollaries follow. First, my estimate of the effect of displacement on opioid use is generally closer to the true causal effect when we use non-layoff displacement as our main explanatory variable than when we use layoffs, since  $\left| \frac{\beta_{2a}\text{var}(\epsilon)}{1-\beta_1\beta_{2a}} \right| - \left| \frac{\beta_{2b}\text{var}(\epsilon)}{1-\beta_1\beta_{2b}} \right| > 0$  for almost all  $\beta_1, \beta_{2a}, \beta_{2b}$  in this range, as shown in appendix figures B.1 and B.2. We see a particularly large difference between simultaneity error terms corresponding to  $\beta_{2a}$  and  $\beta_{2b}$  when we assume small  $\beta_1$  and when we assume that  $\beta_{2a}$  is much greater in absolute magnitude from  $\beta_{2b}$ , e.g., when the effect of opioid use on the likelihood of being laid off is much larger in absolute value than the effect of being displaced due to other causes.

Second, the difference between my estimates of the effect of layoffs and non-layoff displacement will shed light on the direction of the bias in measurements of the effect of displacement on opioid use. Under the assumptions that (1)  $\text{sign}(\beta_{2a}) = \text{sign}(\beta_{2b})$ , we have that (2)  $\beta_1, \beta_{2a}$ , and  $\beta_{2b}$  are all less than one in absolute value, the error terms  $\frac{\beta_{2a}\text{var}(\epsilon)}{1-\beta_1\beta_{2a}}$  and  $\frac{\beta_{2b}\text{var}(\epsilon)}{1-\beta_1\beta_{2b}}$  have the same sign. Then estimating two separate models as follows:

$$Y_i = \alpha_{1a} + \beta_{1a}\text{LAID\_OFF}_i + Z_i'\delta_{1a} + \epsilon_i \quad (9)$$

$$Y_i = \alpha_{1b} + \beta_{1b}\text{NONLAYOFF\_DISPLACED}_i + Z_i'\delta_{1b} + \varphi_i \quad (10)$$

and considering the sign of the difference between  $\beta_{1a}$   $\beta_{1b}$  should yield the direction of the simultaneity bias in regressions of opioid use indicators on displacement indicators. For example, if  $\beta_{1a} - \beta_{1b} < 0$ , we should conclude that the simultaneity bias is downward due to opioid use making individuals more likely to experience displacement through the productivity channel. If, however,  $\beta_{1a} - \beta_{1b} > 0$ , we should conclude under the enumerated assumptions that the simultaneity bias is upward. This is, in fact, what I observe.

### 4.3 Treatment effect heterogeneity

In addition to estimating my baseline specification, equation 1, I estimate a number of simple interaction models in order to understand whether the effects of displacement differ among demographic subgroups, and to investigate possible mechanisms driving my baseline results.

To explain the former, recall that part of the contribution I hope to make in this work is to investigate the image – put forth in Case and Deaton’s (2015; 2017) seminal work – of the white, working-class individual who resorts to opioid abuse in response to a lack of opportunity in the labor market. To see whether displacement makes white working-class individuals especially likely to use opioids, I estimate the two following models:

$$Y_i = \varphi_0 + \varphi_1\text{EVER\_DISPLACED}_i + \varphi_2\text{BLUE-COLLAR}_i + \varphi_3\text{EVER\_DISPLACED}_i \times \text{BLUE-COLLAR}_i + W_i'\varphi_4 + \text{PANEL}_i'\rho + \psi_i \quad (11)$$

$$Y_i = \phi_0 + \phi_1\text{EVER\_DISPLACED}_i + \phi_2\text{NON-HISP\_WH}_i + \phi_3\text{EVER\_DISPLACED}_i \times \text{NON-HISP\_WH}_i + U_i'\phi_4 + \text{PANEL}_i'\rho + \tau_i \quad (12)$$

where  $Y_i$ ,  $\text{EVER\_DISPLACED}_i$ ,  $\text{PANEL}_i$  and  $\epsilon_i$  are defined as in equation 1,  $\text{BLUE-COLLAR}_i$  is defined as an indicator for an individuals’ round 1 occupation being one of the last four occupations in table 6,  $\text{NON\_HISP\_WH}_i$

is an indicator for an individual being non-Hispanic and white, and:

$$W_i = X_i \setminus \text{BLUE-COLLAR}_i$$

$$U_i = X_i \setminus \text{NON-HISP\_WH}_i$$

To investigate possible mechanisms by which my baseline findings show negative estimates of  $\beta$ , I estimate a variety of interaction regressions to understand whether the effect of displacement on likelihood of opioid use is mediated by the loss of income experienced by displaced individuals. If displacement makes individuals less likely to use opioids only to the extent that it makes them less able to afford opioids, we would expect to see more strongly negative estimates of the effect of displacement among individuals for 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 earnings constituted the majority of earnings in their households, or individuals in families without significant non-labor (business or trust) income. In the interest of testing these hypotheses, I estimate models of the form:

$$Y_i = \kappa_0 + \kappa_1 \text{EVER\_DISPLACED}_i + \kappa_2 \text{ONENOWRKPD}_i + \kappa_3 \text{EVER\_DISPLACED}_i \times \text{ONENOWRKPD}_i + X_i' \kappa_4 + \text{PANEL}_i' \rho + \zeta_i \quad (13)$$

$$Y_i = \theta_0 + \theta_1 \text{EVER\_DISPLACED}_i + \theta_2 \text{BREADWINNER}_i + \theta_3 \text{EVER\_DISPLACED}_i \times \text{BREADWINNER}_i + X_i' \theta_4 + \text{PANEL}_i' \rho + \eta_i \quad (14)$$

$$Y_i = \delta_0 + \delta_1 \text{EVER\_DISPLACED}_i + \delta_2 \text{BIZTRUST}_i + \delta_3 \text{EVER\_DISPLACED}_i \times \text{BIZTRUST}_i + X_i' \delta_4 + \text{PANEL}_i' \rho + \mu_i \quad (15)$$

where  $Y_i$ ,  $\text{EVER\_DISPLACED}_i$ ,  $X_i$ ,  $\text{PANEL}_i$ , and  $\epsilon_i$  are defined as in equation 1,  $\text{ONENOWRKPD}_i$  is an indicator for reporting at least one period of no work,  $\text{BREADWINNER}_i$  is defined as an indicator for 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  $\text{BIZTRUST}_i$  is an indicator for 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 Results

I estimate equation 1 using ordinary least squares for both the opioid count and MME per day opioid use metrics and report coefficients on displacement indicators in table 7. Section 1 shows results from regressing indicators for exceeding opioid abuse indicators on indicators for ever experiencing displacement, controlling for the demographic, health status, and industry and occupation variables outlined in tables 1, 5, and 6. Sections 2 and 3 show results from regressing indicators for exceeding opioid abuse indicators on indicators for ever experiencing a layoff and non-layoff displacement, respectively. Panel A uses prescription count-based opioid use/abuse metrics, and Panel B uses MME-per-day-based abuse metrics.<sup>21</sup> Section 1 of both panels A and B shows no statistically significant relationship between displacement and prescription opioid use at the lower two thresholds of use for both opioid prescription count and MME per day threshold outcomes, which are one prescription and six prescriptions and 60 MME per day and 90 MME per day, respectively.<sup>22</sup> At the higher abuse thresholds, columns (3) and (6) of table 7 show statistically significant reductions in the probability of opioid

<sup>21</sup>While Panel A only shows regression results for a select few opioid prescription count indicators, regression results from the full set of indicators are shown in appendix figure C.1.

<sup>22</sup>This holds for regression results corresponding to all opioid prescription counts for counts lower than nine prescriptions. See appendix figure C.1.

use associated with any displacement. This coefficient is on the order of two tenths of a percentage point, or a reduction of 20% of the baseline probability of accumulating twelve or more opioid prescriptions conditional on being displaced.<sup>23</sup> The reduction is larger for the probability that individuals receive a 120+ MME per day prescription, on the order of three tenths of a percentage point or a 50% reduction in the baseline probability using opioids at this threshold.

Table 7: Baseline Regression Results

	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	-0.008 (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. Independent variable = individual ever laid off</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 3. Independent variable = individual ever non-layoff displaced</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
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.

Reviewing sections 2 and 3 illuminates the importance of disaggregating displacement by cause. In particular, these sections show that the reductions in the probability of high-level opioid use associated with displacement we see in the rightmost columns of both panels A and B of table 7 are driven almost entirely by reductions among non-layoff displaced workers. Relationships between being laid off and using opioids at any threshold are statistically indistinguishable from zero, whereas both tables show significant reductions in the probability of using opioids at high thresholds associated with non-layoff displacement.<sup>24</sup> These effects are small in absolute terms – two tenths of a percentage point reduction in the probability that an individual accumulates 12 or more prescriptions, and four tenths of a percentage point reduction in the probability that an individual receives a 120+ MME per day prescription – but are quite large in relative terms, on the order of 20% and 80% reductions, respectively, in the baseline probabilities of using opioids at these thresholds.

Before pivoting to conduct analysis of possible heterogeneity of displacement effects, I pause to note that, in these baseline results, regression coefficients on indicators for being laid off are reliably greater than coefficients on indicators for experiencing non-layoff displacement.<sup>25</sup> This satisfies the conditions under which, within the framework I present in subsection 4.2, I should conclude that the sign of the simultaneity bias in my estimates of the effect of displacement on opioid use is positive, and that I should interpret results in my regression analysis as upper bounds on the causal effect of displacement on propensity to abuse opioids.

Resuming my discussion of this paper's regression analysis, I investigate whether my baseline results mask heterogeneity across racial and occupational categories by estimating equations 11 and 12, which allow the relationship between displacement and opioid use to vary based on whether individuals are blue-collar

<sup>23</sup>Coefficients are of similar magnitude and at least borderline statistically distinguishable from zero for all prescription counts greater than eleven. Coefficients for prescription counts above eight are also negative and statistically distinguishable from zero; these coefficients are somewhat larger. See appendix figure C.1.

<sup>24</sup>These results hold true not only for the opioid prescription count indicator outcomes shown, but for all opioid prescription count indicator outcomes.

<sup>25</sup>This is true for all opioid prescription count outcomes, as well as those shown in table 7.



Table 8: Heterogeneity by occupation

	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>						
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 2. Independent variable = individual ever displaced</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. Independent variable = individual ever non-layoff displaced</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
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 9: Heterogeneity by self-reported race/ethnicity

	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>						
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 2. Independent variable = individual ever displaced</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 3. Independent variable = individual ever non-layoff displaced</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
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.

workers and non-Hispanic white, respectively. These are the populations Case and Deaton (2015; 2017) highlight as having been ravaged by deaths of despair. Table 8 show marginal effects of being displaced on the likelihood of exceeding opioid count and MME thresholds of opioid use, respectively, across occupation subgroups, and table 9 show marginal effects across ethnic-racial subgroups.

Beginning with panel B of table 8, point estimates in section 1 suggest that blue-collar workers may be more likely than white-collar workers to receive high MME per day opioid prescriptions if displaced, though examining sections 2 and 3 within panel B shows this effect is entirely driven by laid off workers, among whom we see statistically significant effects at the 60 and 90 MME per day thresholds. In particular, point estimates of the marginal effect of non-layoff displacement on the likelihood of receiving a high MME per day opioid prescription for blue collar workers are negative and not statistically significant. Panel A of table 8, which shows heterogeneity regression results by occupation using opioid prescription count abuse metrics, does not corroborate the idea that blue-collar workers who experience displacement or layoffs are made more likely to begin using prescription opioids. Moreover, neither panel of table 9 does not suggest positive effects of displacement among non-Hispanic whites. Indeed, point estimates at higher thresholds of use in panel A of 8 and both panels of table 9 are all negative for the groups highlighted by Case and Deaton (2015; 2017).

Table 10: Heterogeneity by whether individual did not work for at least one period

	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>						
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 2. Independent variable = individual ever displaced</i>						
At 1st 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 1st 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 3. Independent variable = individual ever non-layoff displaced</i>						
At 1st 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 1st 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
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, to investigate the mechanism by which displacement reduces, on average, workers' propensity to begin using opioids at high thresholds of use (as shown in baseline regression tables 7), I show three sets of tables which investigate possible heterogeneity in my results based on the extent to which displaced individuals' income or assets were reduced by displacement. I use three proxies for the extent to which displacement reduces income: first, whether a displaced individual experienced at least one non-working period (after the reference period corresponding to the first round of interviews); second, whether the displaced individual's labor income constituted the majority of their family's (or, more exactly, dwelling unit's) income in the first year of MEPS participation; and third, whether anyone in the individual's family reported having business or trust income in the first year of survey participation. Table 10 estimates equation 13, which allows for displacement effect hetero-

Table 11: Heterogeneity by whether family (dwelling unit) has business/trust income

	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>						
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 2. Independent variable = individual ever displaced</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 3. Independent variable = individual ever non-layoff displaced</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
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 12: Heterogeneity by share of family (dwelling unit) income coming from individuals' wage income

	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>						
Wage inc. was majority of Y1 family income	-0.004 (0.010)	-0.003 (0.003)	-0.004*** (0.001)	0.014 (0.012)	0.009 (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)	0.001 (0.005)	0.002 (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
<i>Section 2. Independent variable = individual ever displaced</i>						
Wage inc. was majority of Y1 family income	-0.009 (0.015)	-0.006* (0.003)	-0.005*** (0.001)	0.002 (0.016)	0.008 (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.	0.004 (0.009)	0.004 (0.003)	0.001 (0.002)	0.012 (0.009)	0.011 (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
<i>Section 3. Independent variable = individual ever non-layoff displaced</i>						
Wage inc. was majority of Y1 family income	-0.001 (0.013)	-0.001 (0.004)	-0.003** (0.001)	0.031* (0.019)	0.019 (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)	-0.008 (0.005)	-0.005 (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.

geneity by non-work, for prescription count and MME abuse metrics, respectively; table 11 estimates equation 15, which allows for displacement effect heterogeneity by whether a family member has business or trust income, for prescription count and MME abuse metrics, respectively; and table 12 estimates equation 14, which allows for displacement effect heterogeneity by whether the individual was the "breadwinner" for their dwelling unit, for prescription count and MME abuse metrics, respectively.

Broadly, the results in these tables support the hypothesis that the displacement-induced reductions in high-threshold opioid use shown in baseline regression tables 7 are mediated by income loss associated with displacement. For individuals who experienced at least one non-work period, individuals who had no family business or trust income, and individuals whose wage income was the majority of their family's income, displacement is associated with a statistically significant reduction in the probability of receiving twelve or more opioid prescriptions, with effect sizes on the order of the baseline effect reductions in table 7. Marginal effects in regressions of indicators for 120+ MME per day opioid use broken down along these lines are of similar magnitudes and invariably negative, though are estimated less precisely; I measure statistically significant effects among for individuals who have no family business or trust income and individuals who were "breadwinners," but not among individuals who experienced one or more non-work periods. Regardless of which opioid use metric we use, point estimates of marginal effects are more strongly negative among individuals who likely experienced a greater income shock associated with displacement, and are chiefly driven by individuals experiencing non-layoff displacement.

## 5 Discussion

This paper provides the first estimates from micro data on the effects of job demand on individuals' demand for prescription opioids. While I am able to 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 would tend 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 – zero or negative associations between displacement and likelihood of opioid use – as suggestive, but more optimistically, under the assumptions I outline, we can interpret these regression coefficients as bounds on causal estimates. To be specific, because point estimates for associations between layoffs and opioid use are greater than point estimates for associations between non-layoff displacement and opioid use, we might, under the assumptions I outline in 4.2, interpret my estimates as upper bounds on the causal effect of displacement.

In my view, the restrictions that (1) opioid use changes the likelihood of displacement in the same direction for layoffs and non-layoff displacement, either making both events more likely or less likely (2) opioid use will affect a worker's likelihood of displacement as least as much as it will affect a worker's likelihood of non-layoff displacement (3) both displacement's causal effect on likelihood of opioid use and opioid use's causal effect on likelihood of displacement are less than 100 percentage points in absolute value are very reasonable. However, the implications thereof are somewhat surprising in the context of some of the 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 mar-

ket dislocation.

How might I reconcile my findings with these seminal papers in the literature? 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.

In order to more thoroughly reject the narratives advanced by Case and Deaton (2015; 2017) and Krueger (2017), further research is in order. In particular, researchers and policymakers would benefit greatly from data sources which allow tracking of 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. 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. Clearly, the stakes are high, but, as my paper shows, the problem at hand is not well-understood.

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## A Classifying opioid prescriptions in the MEPS Prescribed Medicines files

### A.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.<sup>26</sup>

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.<sup>27</sup> 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 fairly closely in using a combination of all three measures to classify opioid prescriptions.

### A.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",

<sup>26</sup>This list can be accessed in spreadsheet form at <https://www.cdc.gov/drugoverdose/resources/data.html> within the Data Files box. I last retrieved the list on November 24th, 2019.

<sup>27</sup>As Soni (2018) notes, "the drug name 'Acetaminophen' is spelled almost 70 different ways in the MEPS files."

"PROPOXYPHENE", "TAPENTADOL", or "TRAMADOL."<sup>28</sup> 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.

### A.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 A.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

<sup>28</sup>I 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 A.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.
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/POTASSIUM IODIDE
  - CODEINE/DIPHENHYDRAMINE/PHENYLEPHRINE
  - DEXBROMPHENIRAMINE/HYDROCODONE/PHENYLEPHRINE
  - HYDROCODONE/PHENIRAMINE/PHENYLEPHRINE/PHENYLPROPANOLAMINE/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.

## B Simultaneity Bias in Main Specification Depending on Type of Displacement

Section 4.2 discusses the conditions under which our main specification, equation 1, will help to elucidate the causal effect of displacement on likelihood of prescription opioid use in the presence of data constraints which prohibit me from observing exact relative dates of displacement and opioid use. I model this scenario within a simultaneity bias framework, writing the two equations 5 and 6, and write an expression for the simultaneity bias OLS will yield when I run the regression 5, which is in effect a simplified rewriting of my main specification. I argue that the simultaneity bias will be given by:

$$\frac{\beta_1 \text{var} \epsilon_i}{1 - \beta_1 \beta_2} \text{ if } \beta_1 \beta_2 \neq 1$$

where each parameter is as defined in equations 5 and 6. I then argue that comparing settings in which  $\beta_2$  is likely to vary will be informative in helping me determine the direction of the bias. In particular, I write equations 7 and 8 and state the following assumptions:

1.  $\text{sign}(\beta_{2a}) = \text{sign}(\beta_{2b})$ , e.g. using opioids will not make a person less likely to be laid off but more likely to be displaced due to establishment closure, or vice versa
2.  $|\beta_{2a}| \geq |\beta_{2b}|$ , since managers have more discretion in deciding who to lay off than who to displace based on establishment closure (a decision over which managers likely hold little discretion) and job ending, e.g. because of a contract's term ending (a decision over which managers likely hold no discretion). To the extent that opioid use affects likelihood of displacement via worker productivity as observed by managers, the productivity channel will be stronger for layoffs than non-layoff displacement.
3.  $|\beta_1|, |\beta_{2a}|, |\beta_{2b}|$  are all less than one, since it would be surprising if displacement made individuals more than one hundred percentage points more or less likely to begin using opioids conditional on controls  $X_i$  and  $\text{PANEL}_i$  and vice versa.

Having made these assumptions, I am now prepared to examine the difference between the magnitudes of the simultaneity bias terms  $\frac{\beta_{2a} \text{var}(\epsilon)}{1 - \beta_1 \beta_{2a}}, \frac{\beta_{2b} \text{var}(\epsilon)}{1 - \beta_1 \beta_{2b}}$ . In other words, our object of interest here is:

$$\left| \frac{\beta_{2a} \text{var}(\epsilon)}{1 - \beta_1 \beta_{2a}} \right| - \left| \frac{\beta_{2b} \text{var}(\epsilon)}{1 - \beta_1 \beta_{2b}} \right|$$

where we are interested in whether the difference between these terms is positive or negative. I let:

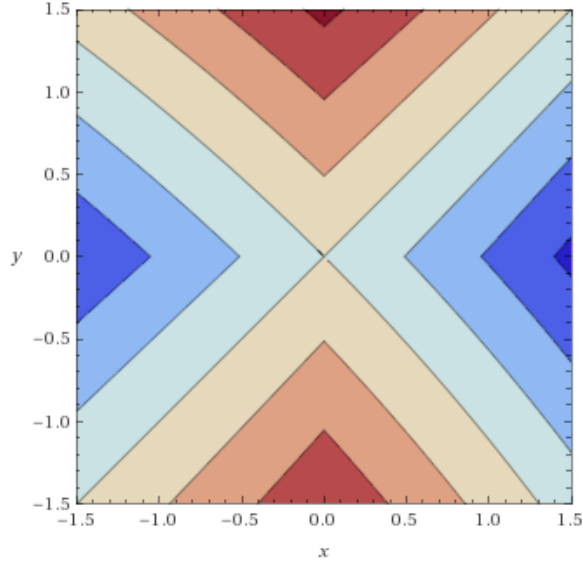
$$\begin{aligned} y &= \beta_{2a} \\ x &= \beta_{2b} \\ z &= \left| \frac{\beta_{2a} \text{var}(\epsilon)}{1 - \beta_1 \beta_{2a}} \right| - \left| \frac{\beta_{2b} \text{var}(\epsilon)}{1 - \beta_1 \beta_{2b}} \right| = \left| \frac{y \text{var}(\epsilon)}{1 - \beta_1 y} \right| - \left| \frac{x \text{var}(\epsilon)}{1 - \beta_1 x} \right| \end{aligned}$$

and plot  $z$  in the  $x, y, z$  plane using Mathematica for  $\beta_1 \in \{-0.75, -0.5, -0.25, -0.1, -0.05, 0.05, 0.1, 0.25, 0.5, 0.75\}$ . I show contour plots for  $x, y \in [-1.5, 1.5]$  in figures B.1 and B.2 where red indicates positive values of  $z$  and blue indicates negative values of  $z$ . Clearly  $z > 0$  in vast majority of the area in zones where  $|y| > |x|$  for all values of  $\beta_1$ . This is particularly true when we assume small values of  $\beta_1$ , e.g., small effects of job displacement on prescription opioid use, and when we assume larger differences between  $\beta_{2a}$  and  $\beta_{2b}$ , e.g. larger differences between the

effect of opioid use on likelihood of being laid off and the effect of of opioid use on the likelihood of non-layoff displacement.

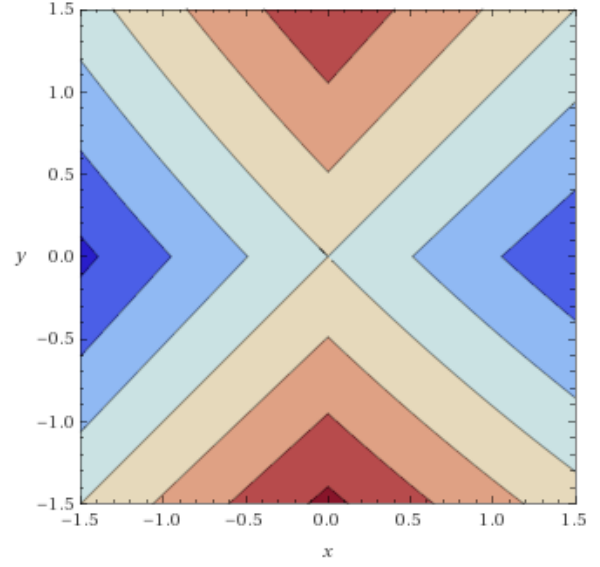
Figure B.1: Contour Plots of  $\left| \frac{\beta_{2a} \text{var}(\epsilon)}{1 - \beta_1 \beta_{2a}} \right| - \left| \frac{\beta_{2b} \text{var}(\epsilon)}{1 - \beta_1 \beta_{2b}} \right|$  for small  $\beta_1$

(a)  $\beta_1 = 0.05$



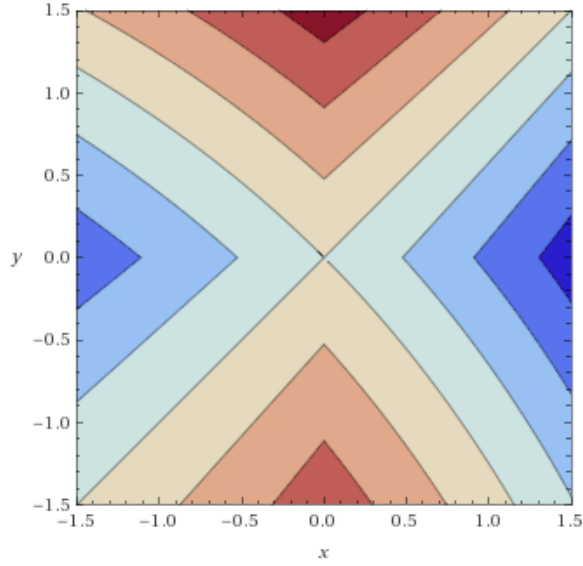
Computed by Wolfram|Alpha

(b)  $\beta_1 = -0.05$



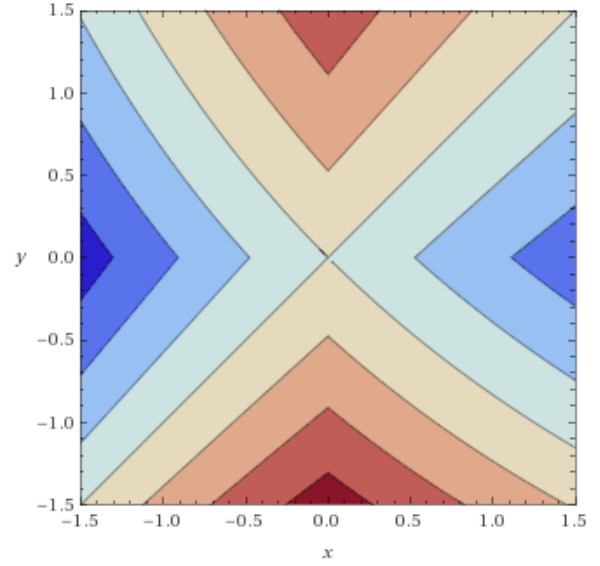
Computed by Wolfram|Alpha

(c)  $\beta_1 = 0.1$



Computed by Wolfram|Alpha

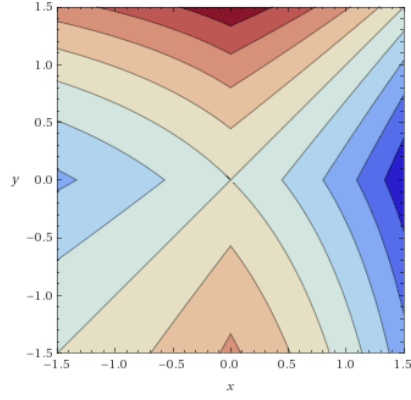
(d)  $\beta_1 = -0.1$



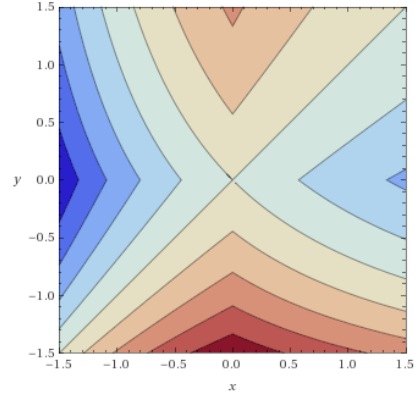
Computed by Wolfram|Alpha

Figure B.2: Contour Plots of  $\left| \frac{\beta_{2a} \text{var}(\epsilon)}{1 - \beta_1 \beta_{2a}} \right| - \left| \frac{\beta_{2b} \text{var}(\epsilon)}{1 - \beta_1 \beta_{2b}} \right|$  for large  $\beta_1$

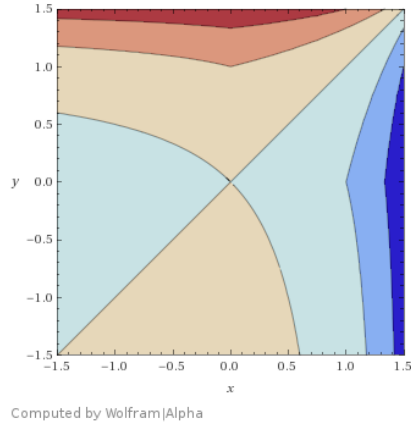
(a)  $\beta_1 = 0.25$



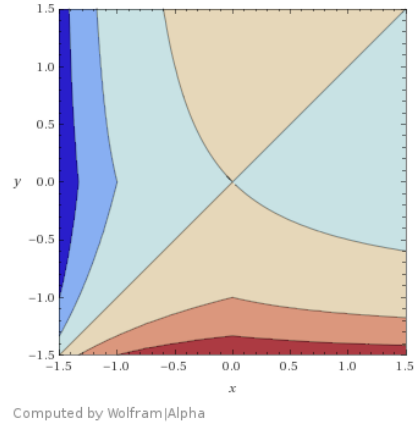
(b)  $\beta_1 = -0.25$



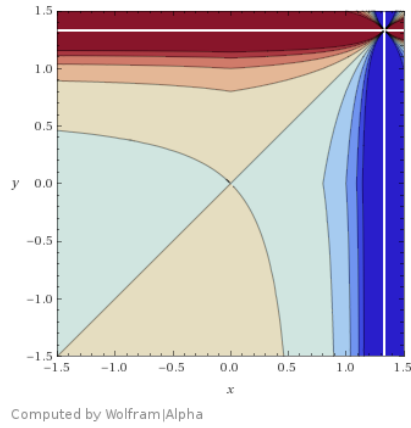
(c)  $\beta_1 = 0.5$



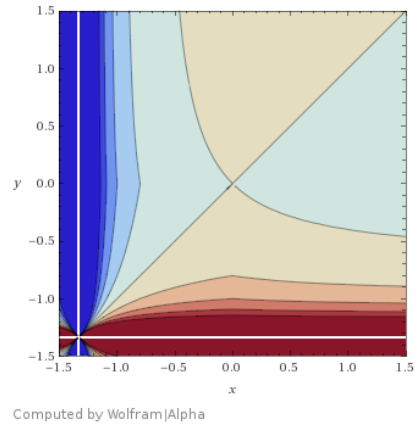
(d)  $\beta_1 = -0.5$



(e)  $\beta_1 = 0.75$



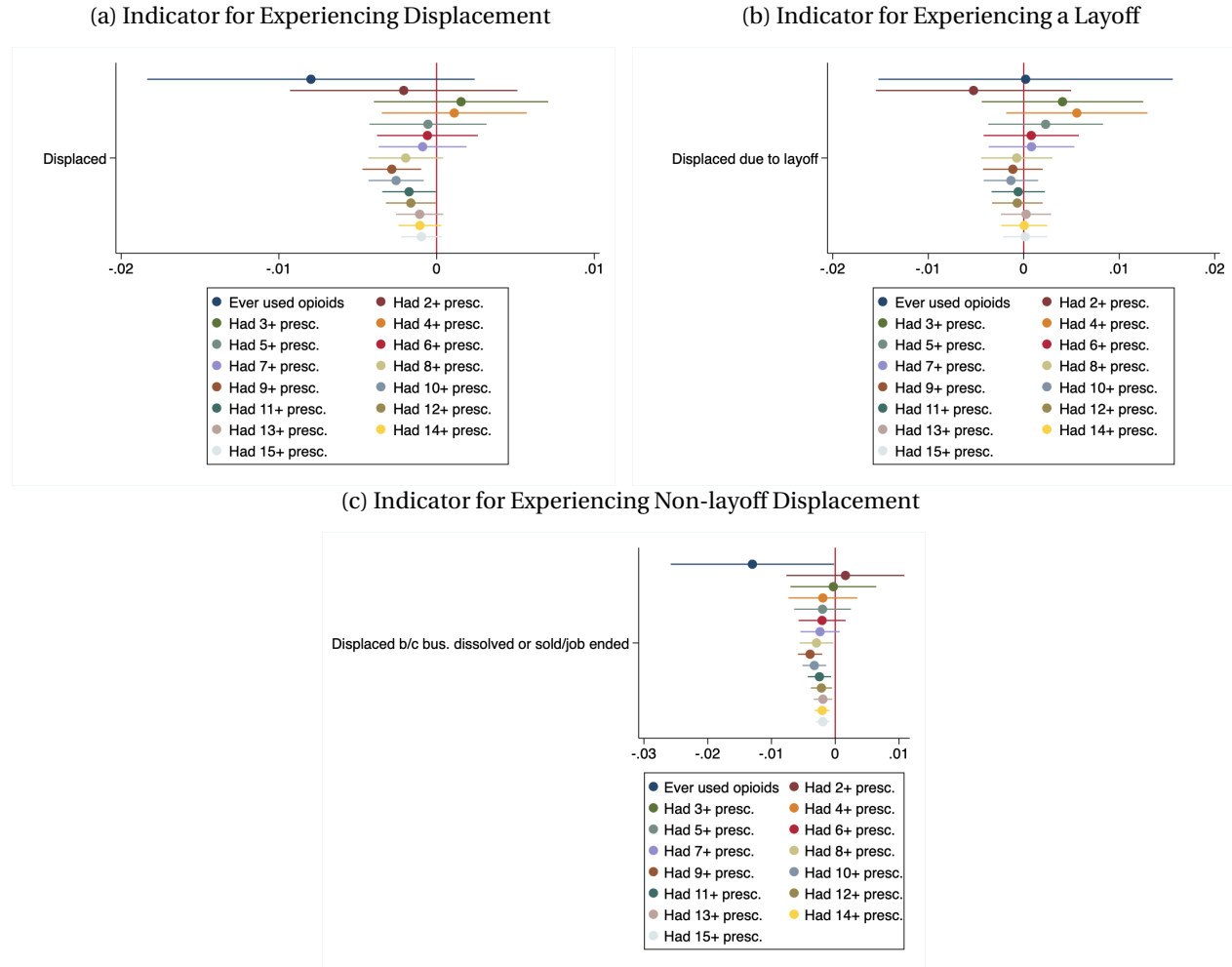
(f)  $\beta_1 = -0.75$





## C Regression results on full set of indicators for exceeding opioid count thresholds

Figure C.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.