Effects of Job Displacement on Opiate Demand: Evidence From the Medical Expenditure Panel Survey

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

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Abstract

This thesis uses individual-level panel data from the Medical Expenditure Panel Survey to investigate whether job displacement affects the likelihood that prime-age workers in the United States begin using prescription opiates. My results indicate that being laid off has no effect on individuals' propensity to start using opiates, whereas displacement due to other causes decreases individuals' probability of beginning to use opiates by roughly twelve percent. I find no evidence that the effects of job displacement on opiate use differ by age, race, or pre-displacement occupation category, nor do I find any evidence that post-displacement health insurance status is an important determinant of the effect of job displacement on demand for prescription opiates. The results of this study suggest that increases in opiate use associated with county-level labor market shocks are unlikely to be driven by despair-induced increases in demand for drugs among affected workers, but rather place-specific determinants of opiate use correlated with poor labor market conditions.

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1 Introduction

In recent years, scholars, pundits, and policymakers have pronounced in unison that opiate 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 opiates, amounting to over 130 overdose deaths per day due to opiates (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 opiate 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 opiates 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 opiate abuse in the United States (Charles, Hurst, and Schwartz, 2018; Currie, Jin, and Schnell, 2018; Ruhm, 2018; Hollingsworth, Ruhm, and Simon, 2017; Krueger, 2017; Aliprantis and Schweitzer, 2018; Harris et al., 2017; Torbin and Nielsen, 2017). The subset of studies concerned with whether despair among workers due to worsening labor market conditions induces opiate use have thus far failed to definitively answer whether worsening labor market conditions could be a strong determinant of opiate 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 opiates among workers in affected labor markets, Ruhm (2018) and Currie, Jin, and Schnell (2018) do not observe sufficiently strong relationships between opiate 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 opiate use relies on the idea that workers or potential workers begin using or abusing opiates 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 opiates. Because Case and Deaton's proposal hinges on workers' and potential workers' demand for opiates, 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 opiate use and place-specific determinants of use.[1]

This thesis 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 opiate 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 opiates 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 of opiate prescriptions. The detailed health status information collected by MEPS survey administrators allows me to condition richly on health conditions correlated with opiate use as well as demographic characteristics and pre-displacement industry and occupation characteristics.

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

I show that job displacement largely makes prime-age workers in the United States less likely to begin using prescription opiates; in particular, I find little evidence of a statistically significant relationship between experiencing layoffs and beginning to use opiates, and I find consistent evidence of a strong negative effect (between two and two and a half percentage points) of non-layoff displacement on the probability of beginning to use opiates. The one subgroup among which I find any evidence that job displacement induces opiate use is individuals who experience layoffs and report poor pre-displacement health status, though I am hesitant to place too much weight on these findings since they could be driven by negative selection into layoffs on health-related productivity dimensions. Importantly, I find that neither layoffs nor non-layoff displacement differentially affects individuals based on age, race, or pre-displacement occupation, which casts doubt on Case and Deaton's (2017) hypothesis that labor market dislocations are particularly important drivers of increasing mortality among non-Hispanic whites aged 45-54, as well as Charles, Hurst, and Schwartz's (2018) hypothesis that manufacturing workers who experience labor market dislocations are made more likely to use opiates. My investigation into possible mechanisms by which non-layoff displacement might make individuals less likely to begin using prescription opiates has proven inconclusive. Though I suspect that financial hardships associated with displacement are responsible for post-displacement reductions in the probability of beginning to use opiates, I find no evidence that the effect of job displacement differs depending on individuals' post-displacement health insurance status.

Taken together, the results of this thesis suggest that increased demand for opiates associated with worsening labor market conditions is unlikely to be a strong driver of increasing opiate deaths in recent years. To the extent that labor market dislocations are to blame for increasing opiate abuse, these effects are most likely driven by increases in opiate supply associated with labor market dislocations. Policymakers would likely benefit from further investigation into the mechanisms by which labor market dislocations are associated with increases in opiate supply.[2]

2 Background

Opiate 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 opiate use. This curiosity coalesced into several papers and working papers which, like my thesis, examine whether labor market dislocations induce opiate use (Charles, Hurst, and Schwartz, 2018; Currie, Jin, and Schnell, 2018; Ruhm, 2018; Hollingsworth, Ruhm, and Simon, 2017; Roulet, 2017), as well as papers examining whether opiate use induces labor market inactivity (Krueger, 2017; Currie, Jin, and Schnell, 2018; Aliprantis and Schweitzer, 2018; Harris et al., 2017; Torbin and Nielsen, 2017), and the social determinants of opiate use in general (e.g. Finkelstein, Gentzkow, and Williams, 2018). Due to data availability constraints, most of these studies have either focused on opiate overdose deaths, which are a somewhat noisy proxy for overall opiate use, or prescription opiate abuse, which is distinct from but highly correlated with illicit opiate abuse.[3]

[2] For instance, prescribing practices of local healthcare providers could be such a place-specific (e.g. supply-side) determinant of county-level opiate use which is correlated with local labor market conditions. I discuss this possibility in more detail in section 5.

[3] 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 opiate abuse may act as a gateway to more

In proposing that worsening economic conditions have contributed to increasing "deaths of despair" vis-a-vis opiate use, Case and Deaton (2017) primarily focus on 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 small handful of economists have worked to study whether medium- and short-term economic shocks cause greater prescription opiate use.

The only existing study which directly measures the effects of job displacement on individual opiate demand is Roulet (2017), who exploits individual-level employment and healthcare utilization data from Denmark to investigate whether job displacement induces greater prescription opiate use. 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. Roulet (2017) finds no effect of job displacement on opiate 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 opiate 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.

On the other hand, the studies which most closely resemble my own using U.S. data are Ruhm (2018), 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 opiate use.[4] Ruhm (2018) 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 opiate deaths. Specifically, Ruhm (2018) 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 own. Currie, Jin, and Schnell (2018) use the shift-share (Bartik) instrument popularized by Blanchard and Katz (1992) to measure the effect of a plausibly exogenous shift in the employment-to-population ratio on county-level opiate 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 opiate use metrics, including opiate prescriptions per capita, changes in the prevalence of opiate-related deaths, and positive drug test rates at the county level.

The results of these studies paint a slightly contradictory and inconclusive picture. Ruhm (2018) 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 10% of the change in the drug death rate and, second, in Ruhm's words, "even small amounts of selection on unobservables would be sufficient to completely eliminate the contributions of economic factors" influencing the rate of deaths of despair. Currie, Jin, and Schnell's (2018) results largely sug-

dangerous substance abuse.

[4] Charles, Hurst, and Schwartz's (2018) interest in the interplay between opiate 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.

gest no relationship between employment-to-population ratios and opiate 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), on the other hand, are more confident in their formulation of the link between deaths of despair and economic conditions; their two-stage least squares specifications show strong relationships between declining manufacturing share of employment and opiate use metrics.

Beyond failing to paint a conclusive picture of the relationship between economic conditions and opiate 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 opiate 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 opiate use are attributable to person-specific or place-specific consequences of economic conditions. Ruhm (2018) 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 (2018) proposes that changes in drug-prescribing environments (e.g. the ease of obtaining opiates in a given county) may be able to explain any impact of worsening economic conditions on deaths related to prescription opiate 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 opiate 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 (2018) 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, individual-level panel data on economic dislocation and prescription opiate use is likely to provide the clearest insights on demand-side effects of the former on the latter, but, thus far, no existing study has managed to leverage such data in the United States context. The primary project of this thesis is to begin to fill this gap in the literature by using MEPS data, which enables me to understand both job displacement and opiate use over time at the individual level. Overall, my analysis lends support to Ruhm's (2018) suspicion that changes in place-specific determinants of opiate use rather than person-specific determinants thereof are the primary cause of upticks in opiate use proxies caused by economic dislocations. Furthermore, my work substantiates Currie, Jin, and Schnell's (2018) concerns that the causal relationship between high employment-to-population ratios and lower opiate use among young workers in highly educated counties is due to these workers sorting into less pain-inducing occupations.

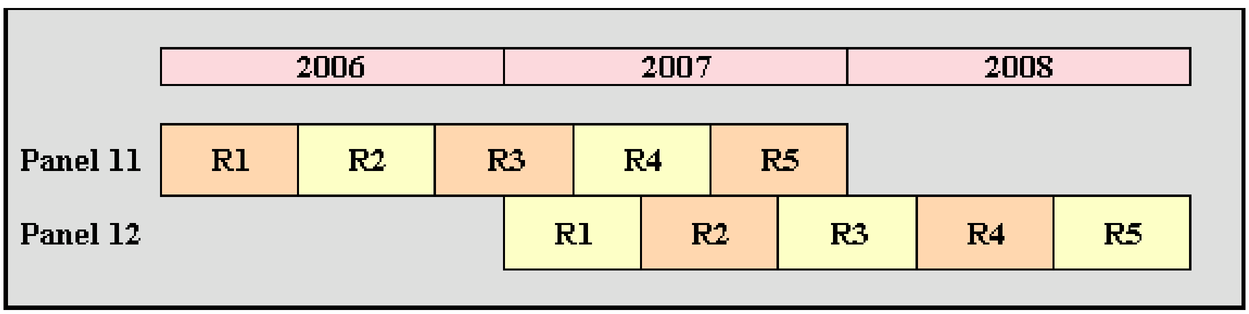
3 Data and Methodology

3.1 Data

3.1.1 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`

My analysis makes use of two types of data files compiled by the MEPS, namely "Full Year Consolidated" files and "Prescribed Medicines" files, both of which are available from 1996 to 2015 on the MEPS website.[5] The former files contain information regarding demographic characteristics, health status, insurance status, and labor market activity for all individuals participating in the MEPS in a given year; note that, for every year except 1996, the Full Year Consolidated files contain information on MEPS participants from two different panels. The information in Full Year Consolidated 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).[6] These prescriptions can be matched to MEPS survey participants using unique identifiers assigned to

[5] The 2016 Full Year Consolidated file is available as of the writing of this draft, but the 2016 Prescribed Medicines file is set to be released late spring 2019, per the MEPS website. For this reason, I am unable to use the 2016 Full Year Consolidated data file in my analysis.

[6] It is not clear what proportion of all opiate 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 opiate use). Regardless of what proportion of overall opiate prescriptions are covered by the MEPS, prescription opiates intended for outpatient use are likely a particularly valuable subject of study from a policy standpoint, since opiates prescribed for outpatient use are more likely to be abused than opiates prescribed for use under physician supervision.

each survey participant. 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. Clearly, the fact that individuals are asked to self-report their prescription drug use introduces the possibility of non-response bias, driven by stigma around using large quantities of opiates. This non-response bias is likely less severe than it would be if individuals were asked to self-report illicit drug use; all the same, the plausible presence of non-response bias makes my measures of prescription opiate use somewhat noisy proxies for actual prescription opiate use.

3.1.2 Sample selection

As I discuss in greater detail in subsection 3.2, the identifying assumption in my analysis is that job displacement is uncorrelated with future prescription opiate use conditional on controls. For this reason, I focus on prime-age individuals for whom job displacement is unlikely to be correlated with future opiate use, namely, individuals who did not receive any opiate prescriptions during the reference period corresponding to the first round of MEPS interviews. I also restrict my analysis to individuals who report being employed in the first-round of interviews for two reasons. First, I do so because of my definition of job displacement, to be discussed further in the following sub-subsection, which requires that the individual be employed in the pre-displacement period. Second, I make this restriction because restricting my analysis sample to include individuals who do not use prescription opiates 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 opiate use. I discuss this issue in greater detail in subsection 3.2. 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 opiate use, coupled with the availability of weighting variables, pares my analysis sub-sample to 24.31% of individuals in the MEPS from 1996 to 2015, or 63.3% of prime-age individuals participating in the MEPS during this time period. Demographic characteristics of this sub-sample of MEPS participants (pooling all years of data) are presented in table 1. I compute the statistics therein using the MEPS' individual-specific probability weights. As such, these estimates are nationally representative of individuals satisfying the criteria for my analysis sample for the period spanning 1996 to 2015. Unless noted otherwise, this is the case for all statistics reported in this thesis.

3.1.3 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.[7] Summary statistics regarding the prevalence of job displacement thus defined, both overall and dis-aggregated 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

[7] Individuals can "switch" current main jobs into unemployment; they need not work in the post-displacement period.

Table 1: Demographic characteristics of analysis sample

	Proportion	Standard Deviation
Age 25-34	.332	(.471)
Age 35-44	.358	(.479)
Age 45-54	.308	(.462)
Unknown education level	.006	(.079)
No degree	.093	(.290)
GED	.037	(.188)
High school diploma	.422	(.493)
Bachelor's degree	.229	(.420)
Advanced degree	.110	(.314)
Other degree	.100	(.300)
American Indian	.007	(.085)
Alaska Native (Eskimo, Aleut)	.000	(.008)
Asian or Pacific Islander	.052	(.222)
Black	.117	(.322)
White	.810	(.391)
Multiple races reported	.010	(.099)
Other	.001	(.043)
Male	.536	(.498)
Female	.463	(.498)
Observations	86,190	

Statistics are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who report working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

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 9.1% of 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.

3.1.4 Identifying prescription opiate use

My primary outcome of interest is the probability that survey participants begin to use or abuse opiates. While a variety of metrics for opiate abuse exist in the literature, I focus on indicators for individuals exceeding various thresholds of opiate prescriptions over the course of their survey participation.[8] With my analysis sam-

[8]Finkelstein, Gentzkow, and Williams (2018) provide the widest review in the economics literature of potential opiate abuse proxies which can be computed using information regarding opiate prescriptions. These include (1) filling opiate prescriptions from four or more prescribers (2) filling prescriptions in any calendar quarter which result in 120 or more morphine milligrams equivalent (MMEs) per day, or (3) filling a new prescription before a previous one has run out (Finkelstein, Gentzkow, and Williams, 2018). Of these three abuse prox-

Table 2: Job displacement summary statistics, analysis sample

	Proportion	Standard Deviation
Ever experienced job displacement	.091	(.287)
Displaced in round 1	.031	(.173)
Displaced in round 2	.025	(.159)
Displaced in round 3	.025	(.156)
Displaced in round 4	.016	(.128)
Ever laid off	.041	(.200)
Laid off in round 1	.012	(.112)
Laid off in round 2	.011	(.106)
Laid off in round 3	.011	(.104)
Laid off in round 4	.007	(.088)
Ever experienced non-layoff displacement	.052	(.223)
Non-layoff displaced in round 1	.018	(.133)
Non-layoff displaced in round 2	.014	(.119)
Non-layoff displaced in round 3	.014	(.118)
Non-layoff displaced in round 4	.008	(.093)
Observations	86,190	

Statistics are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who report working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Following Schaller and Stevens (2015), I categorize an individual as experiencing job displacement in a round if they report changing their current main job in the following round because (1) the job ended (2) the business at which they worked was dissolved or sold or (3) the individual was laid off.

ple constructed to exclude survey participants who have nonzero prescription opiate use in the reference period corresponding to the first round of interviews, indicators for exceeding various thresholds of opiate prescriptions amount to indicators for whether survey participants began using prescription opiates to varying extents. It is difficult to specify how many opiate prescriptions might reasonably correspond to "opiate abuse." Rice et al. (2012), for instance, show that diagnosed opiate abusers in a sample of 12 million employer-insured United States patients accumulated 13.3 opiate prescriptions per 12-month period on average. However, this analysis is likely to overstate the number of prescriptions which might correspond to abuse, since Rice et al. (2013) are unable to observe the number of prescriptions received by undiagnosed opiate abusers. Morden et al. (2014) designate a much lower threshold, six or more prescriptions per 12-month period, for potentially problematic "chronic" prescription opiate use. In the interest of transparency, I show indicators for accumulating between one and fifteen prescriptions, and show regression results corresponding to each of these indicators.

I construct indicators for exceeding various thresholds of opiate prescriptions by first classifying prescription records in the MEPS Prescribed Medicines files as opiate prescriptions and subsequently counting the number of opiate prescriptions received by each survey participant during each reference period. I identify opiate prescriptions in the MEPS files based on the non-proprietary drug names, National Drug Codes, and therapeutic class variables associated with each prescription.[9] Because my outcome of interest is the probability of beginning to use or abuse opiates, I do not count buprenorphine or methadone prescriptions in my analysis, as these drugs are prescribed to patients seeking to recover from addiction to illicit opiates. As I discuss further in

ies, 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.

[9] 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 opiate classification in the MEPS Prescribed Medicines files is not in order here, so I save it for appendix A.

sub-subsection 3.2.2, it is also useful for some of my analysis to count only opiate prescriptions which are not cough medicines. I identify opiate cough medicines using the prescription form variable, the nonproprietary name of the drug, and the proprietary drug name, if a proprietary name is given. To be specific, I classify an opiate prescription as a cough medicine if its form is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines (for example, if the nonproprietary name associated with the prescription is "codeine and chlorpheniramine"), or if the proprietary name associated with the prescription is listed as a proprietary name of an opiate cough medicine in the FDA Orange Book drug database.[10]

I also compute morphine milligram equivalent (MME) dosage for each prescription in the MEPS Prescribed Medicines files and use per-prescription MME dosage to compute daily MME dosage for each individual during each reference period corresponding to a round of interviews.[11] Following Finkelstein, Gentzkow, and Williams (2018), I classify an individual as having a "high MME dose reference period" if they received 120 or more MMEs per day in a reference period corresponding to a round of interviews. Regrettably, many opiate prescription records in the Prescribed Medicines files lack information regarding drug quantity per prescription and opiate component strength, both of which are necessary to compute MME dosage. In effect, missing information in opiate prescription records renders me unable to compute MME dosage for the majority of opiate prescriptions in Prescribed Medicines files earlier than the year 2000 or later than the year 2010. As such, statistics related the prevalence of abuse as measured by high MME dosage shown in this thesis should be read as lower bounds.

Summary statistics regarding the prevalence of prescription opiate use in my analysis sample are shown in table 3, and summary statistics regarding the prevalence of prescription opiate use excluding cough medicines are presented in table 4. Figure 2 shows the number of opiate prescriptions received per 100 MEPS participants in each year for which Prescribed Medicines data files are available, as well as the national opiate prescribing rate from 2006 to 2017 according to the Center for Disease Control (CDC) (see CDC Opioid Overdose Data: U.S. Opioid Prescribing Rate Maps, last updated October 2018).[12] A few features of the data are worth flagging here. First, my calculations indicate that a very small number of individuals in my analysis sample ever had a high MME dose reference period (roughly $0.0002 \cdot 86190 \approx 17$ individuals, not accounting for survey weights). To some extent, this is unsurprising since, as I discuss earlier in this subsection, many opiate prescriptions in the MEPS Prescribed Medicines files lack sufficient information to compute MME dosage. However, the small number of individuals in my analysis sample implies that regression analysis using the high MME dose reference period indicator I construct is not feasible.[13]

Second, note that excluding opiate cough medicines considerably reduces my measurement of the proportion of individuals in my analysis sample who ever received an opiate prescription, from 17.6% to 13.3%. However, for higher thresholds of opiate use, the differences between corresponding proportions in tables 3 and 4 shrink substantially. In particular, the proportions are virtually identical when we consider indicators for which the threshold number of opiate prescriptions is nine or more; it seems that individuals with large numbers

[10] For a detailed summary of my methods for to classifying cough medicines in the MEPS Prescribed Medicines files, see appendix C.

[11] For example, if an individual accumulates five opiate prescriptions over the course of the reference period corresponding to round two, each of which has total MME dosage of 120 MMEs, and the reference period corresponding to round two is 60 days long, this person would have a daily dosage of $\frac{5 \cdot 120}{60} = 10$ MMEs per day. I show the morphine milligram equivalence conversion table I use for my computation in appendix A, as well as the proportion of opiate prescriptions in each year's file for which it is possible to compute MME dosage.

[12] The source for all opioid prescribing data given by the CDC is IQVIA Xponent, which collects "a sample of approximately 50,000 retail pharmacies, which dispense nearly 90% of all retail prescriptions in the United States," according to the CDC.

[13] In calculations not shown here, I determine that it is also not feasible to conduct regression analysis for the lower abuse threshold of 90 MMEs per day, which is the CDC's threshold for potentially dangerous prescription opiate use (see CDC Guideline for Prescribing Opioids for Chronic Pain - United States, 2016). This is because only .003 percent of my analysis sample ever recorded a high MME dosage reference period under this definition.

of opiate prescriptions have large numbers of prescriptions for opiate painkillers, rather than large numbers of opiate cough syrups. This feature of the data is consistent with research in the medical literature which has argued that pain medicines are more widely abused than opiate-infused cough syrups (Butler et al., 2004; Katz et al., 2010; Sehgal, Manchikanti, and Smith, 2012; National Institute on Drug Abuse, 2018), though, as discussed previously, number of opiate prescriptions is a noisy measure for opiate abuse.

Finally, it is worth flagging the considerable difference between the trend in opiate prescriptions per 100 MEPS participants and the trend in the national opiate prescribing rate reported by the CDC.[14] It is difficult to say where exactly the trend in the opiate prescribing rate for MEPS survey participants breaks most dramatically from the prescribing rate reported by the CDC, but 2010 or 2011 are likely candidates. After these years, the MEPS trend line declines and later increases whereas the CDC trend line plateaus and later decreases. Though these differences do not necessarily mean that the data from either the MEPS or the CDC are inaccurate (for instance, the differences in trends could be due to differential trends in inpatient versus outpatient prescribing), section 4 presents empirical estimates using both the full time range of data and pre-2010 data only to err on the side of caution. Showing pre-2010 results separately is also somewhat desirable insofar as the public health and medical literatures have established that opiate prescribing (in terms of MMEs) in America was, by all accounts, rampant from the late 1990s until 2010, at which point it peaked and later declined (see for instance Atluri et al., 2014; Dart et al., 2015; Guy et al., 2017; Larochelle et al., 2015; Levy et al., 2015; Pletcher et al., 2008). As such, pre-2010 prescribing is an object of interest in and of itself from a policy standpoint. For the most part, my results do not qualitatively differ depending on whether or not I restrict my analysis to pre-2010 data, though regression coefficients are often less statistically significant when I only use pre-2010 data. This is likely due to the fact that restricting my analysis to pre-2010 data reduces the precision with which I am able to detect effects.

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

While identification of the causal effect of job displacement is thorny due to the potential for selection into displacement on opiate-correlated characteristics such as health conditions, the MEPS' rich data on survey participants' health characteristics allows me to plausibly avoid these pitfalls by conditioning on them.[15] 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 two exceptions to this are (1) the indicator for reporting "fair" or "poor" mental health in round one and (2) the variable giving how many days the survey participant missed work due to health status during the reference period corresponding to the first round of interviews. Conditioning on post-displacement mental health status is very likely "controlling for the treatment," and controlling for days missed work due to health status in reference periods after the first round of interviews is not feasible for my analysis sample, since I only require that individuals in my analysis sample be employed during the reference period corresponding to the

[14]Note that the level of the opiate prescribing rate I compute using the MEPS necessarily differs from the level of the opiate prescribing rate computed by the CDC, since the CDC data includes some prescriptions received in inpatient settings as well as buprenorphine and methadone prescriptions, both of which are categories I exclude entirely.

[15]A variety of papers have attempted to estimate the estimate the causal effect of job displacement on health status, and some (e.g. Schaller and Stevens, 2015) have argued that job displacement causes worse health in the United States. As such, conditioning on survey participants' health status throughout the duration of their survey participation may be "controlling for the treatment." As such, I show specifications in appendix B which only condition on first-round health status. The results here are qualitatively similar to my main results, though in general less strong and statistically significant. It is difficult to decide whether to attribute this difference to upward omitted variable bias due negative selection on unobservables or whether these results suggest my main specification controls for the treatment. Since negative selection into job displacement is the main threat to identification for my analysis, I err on the side of caution by conditioning on all health status variables in my main results and relegating these results to appendix B.

Table 3: Summary statistics of opiate prescription variables, analysis sample

	Mean	Standard Deviation
Ever used opiates during survey participation	.176	(.380)
Received 2 or more opiate prescriptions during survey participation	.062	(.242)
Received 3 or more opiate prescriptions during survey participation	.030	(.172)
Received 4 or more opiate prescriptions during survey participation	.018	(.136)
Received 5 or more opiate prescriptions during survey participation	.013	(.115)
Received 6 or more opiate prescriptions during survey participation	.010	(.100)
Received 7 or more opiate prescriptions during survey participation	.007	(.088)
Received 8 or more opiate prescriptions during survey participation	.006	(.077)
Received 9 or more opiate prescriptions during survey participation	.004	(.069)
Received 10 or more opiate prescriptions during survey participation	.004	(.064)
Received 11 or more opiate prescriptions during survey participation	.003	(.059)
Received 12 or more opiate prescriptions during survey participation	.003	(.055)
Received 13 or more opiate prescriptions during survey participation	.002	(.050)
Received 14 or more opiate prescriptions during survey participation	.002	(.046)
Received 15 or more opiate prescriptions during survey participation	.001	(.043)
Ever had a high MME dose reference period	.0002	(.014)
Observations	86,190	

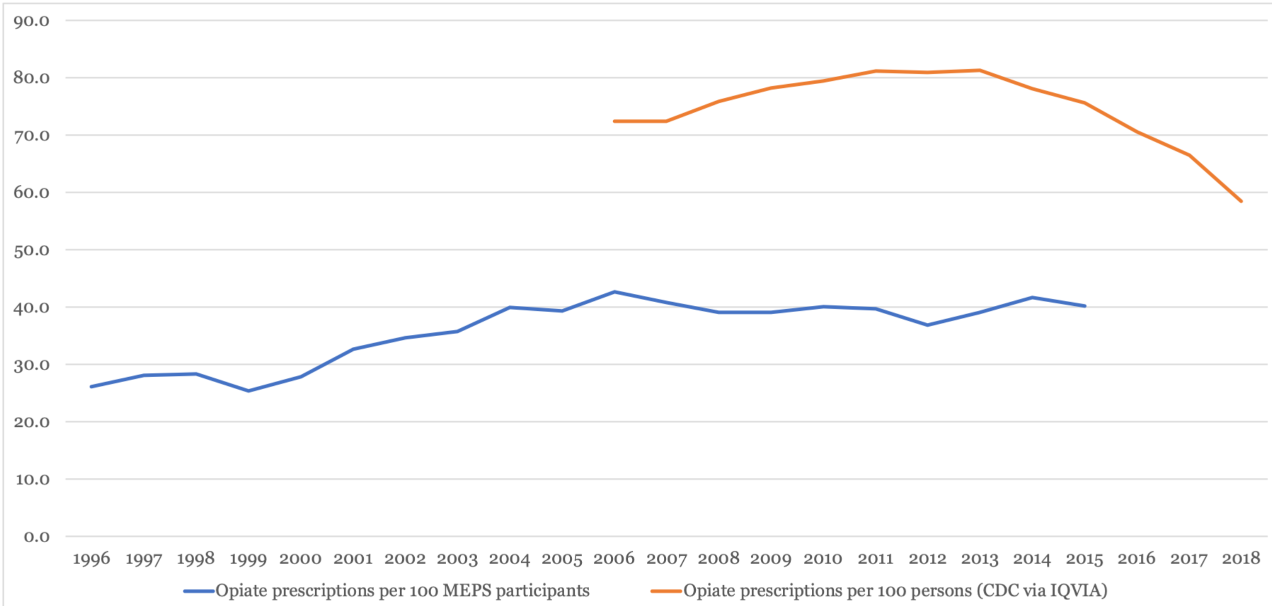
MME stands for "morphine milligrams equivalent;" following Finkelstein, Gentzkow, and Williams (2018), I designate a high MME dose reference period as a reference period in which an individual has greater than 120 MMEs of morphine per day
Statistics are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

Table 4: Summary statistics of opiate prescription variables excluding cough medications, analysis sample

	Mean	Standard Deviation
Ever used non-cough medicine opiates	.133	(.339)
Received 2 or more non-cough medicine opiate prescriptions	.046	(.210)
Received 3 or more non-cough medicine opiate prescriptions	.023	(.152)
Received 4 or more non-cough medicine opiate prescriptions	.015	(.122)
Received 5 or more non-cough medicine opiate prescriptions	.011	(.105)
Received 6 or more non-cough medicine opiate prescriptions	.008	(.091)
Received 7 or more non-cough medicine opiate prescriptions	.006	(.081)
Received 8 or more non-cough medicine opiate prescriptions	.005	(.071)
Received 9 or more non-cough medicine opiate prescriptions	.004	(.064)
Received 10 or more non-cough medicine opiate prescriptions	.003	(.060)
Received 11 or more non-cough medicine opiate prescriptions	.002	(.054)
Received 12 or more non-cough medicine opiate prescriptions	.002	(.051)
Received 13 or more non-cough medicine opiate prescriptions	.002	(.047)
Received 14 or more non-cough medicine opiate prescriptions	.001	(.043)
Received 15 or more non-cough medicine opiate prescriptions	.001	(.040)
Observations	81,587	

I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database.
Statistics are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

Figure 2: Number of opiate prescriptions per 100 individuals in MEPS survey and United States respectively, 1996-2015



Opiate prescribing rates in the MEPS are computed excluding methadone and buprenorphine. Prescription records in the MEPS do not include opiates administered in hospitals, clinics, or physicians' offices. The Center for Disease Control (CDC) prescribing rates are computed using proprietary data from the IQVIA Transactional Warehouse and retrieved from the CDC's page on Prescription Opioid Data.

first round of interviews. 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 individuals' propensity to use opiates following job displacement may be related to the degree to which their job induces pain as well as their emotional 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 opiate 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 opiate 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 tables 6 and 7 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. For the purposes of my analysis, I also partition occupational categories into "blue-collar" occupations (those requiring manual labor) and "white-collar" occupations to more clearly investigate possible links between the nature of work individuals perform and their likelihood of using opiates in response to economic shocks.

Table 5: Summary statistics of health status controls, analysis sample

Indicators	Proportion	Standard Deviation
Ever reported accomplishing less due to physical limitations†	.290	(.454)
Ever reported using an assistive device	.010	(.101)
Ever reported being diagnosed with arthritis†	.133	(.340)
Ever reported taking aspirin every (other) day†	.114	(.318)
Ever reported "fair" or "poor" health status	.176	(.381)
Ever reported difficulty bending or stooping	.052	(.222)
Ever reported difficulty climbing stairs†	.221	(.415)
Ever reported difficulty grasping with fingers	.018	(.133)
Ever reported having a hysterectomy†	.056	(.230)
Ever reported having been discharged from overnight hospital stay	.084	(.278)
Ever reported experiencing joint pain†	.369	(.482)
Ever reported limitation in activity due to health status†	.177	(.382)
Ever reported difficulty walking a mile†	.053	(.224)
Ever reported having illness or injury which required immediate care†	.360	(.480)
Ever reported having illness or injury which required seeing a specialist†	.384	(.486)
Ever reported difficulty reaching overhead	.023	(.150)
Ever reported being more inclined to take risks than other people†	.323	(.467)
Ever reported that health problems got in the way of social activities†	.393	(.488)
Ever reported difficulty standing for more than 20 minutes	.042	(.201)
Ever reported being completely unable to do activity due to health status	.011	(.108)
Ever reported difficulty walking three blocks	.045	(.208)
Ever reported physical limitations due to health status	.079	(.270)
Ever reported limitations in work activities due to health status	.034	(.183)
Reported "fair" or "poor" mental health in round one	.031	(.174)
Non-indicators	Mean	Standard Deviation
Number of days missed work due to health status, round one	.918	(4.352)
Observations	86,190	

Statistics are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
†indicates that data is only available for variables starting in 2000, at which point the MEPS substantially expected its asking health status question. Statistics above are for indicators of whether and individual ever reported being of a given health status during their time in the survey.

Table 6: Proportions of individuals in analysis sample working in condensed industry during round 1

	Proportion	Standard Deviation
Natural resources	.014	(.119)
Mining	.004	(.066)
Construction	.071	(.258)
Manufacturing	.130	(.336)
Wholesale and retail trade	.133	(.340)
Transportation and utilities	.056	(.230)
Information	.017	(.131)
Financial activities	.067	(.250)
Professional and business services	.163	(.369)
Education, health, and social services	.159	(.366)
Leisure and Hospitality	.054	(.226)
Other services	.065	(.246)
Public administration	.055	(.229)
Military	.001	(.041)
Unclassified industry	.003	(.061)
Observations	86,190	

Statistics are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who report working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

Table 7: Proportions of individuals in analysis sample working in condensed occupation during round 1

	Proportion	Standard Deviation	Occupation category
Management, business, and financial	.166	(.372)	White-collar
Professional and related occupations	.235	(.424)	White-collar
Service occupations	.140	(.347)	White-collar
Sales occupations	.087	(.283)	White-collar
Office and administrative support	.122	(.328)	White-collar
Unclassifiable occupation	.009	(.096)	White-collar
Farming, fishing, and forestry	.010	(.100)	Blue-collar
Construction, extraction, and maintenance	.099	(.299)	Blue-collar
Production, transportation, and material moving	.126	(.331)	Blue-collar
Military-specific occupations	.001	(.039)	Blue-collar
Blue-collar total	.237	(.425)	
White-collar total	.762	(.425)	
Observations	86,190		

Statistics are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who report working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

3.2 Methodology

3.2.1 Baseline specification and identification

My baseline empirical specification is a linear probability model in which I regress an indicator for accumulating k or more opiate prescriptions by the end of a MEPS participants' 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, 6, and 7, respectively. I allow k to vary between one and fifteen for the purpose of my analysis.[16] Written out formally, this amounts to

$$\mathbb{I}\left(\sum_{t=2}^{5} \text{OPIATE\_PRESC\_COUNT}_{i,t} \geq k\right) = \alpha + \beta \text{EVER\_DISPLACED}_i + X_i'\gamma + \text{PANEL}_i'\rho + \epsilon_i \tag{1}$$

for $k = 1, 2, ..., 15$ where $\text{OPIATE\_PRESC\_COUNT}_{i,t}$ is the number of opiate prescriptions received by an individual in round t, 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, 6, and 7, PANEL_i is a vector consisting of indicators for being in each panel, and ϵ_i is an error term.[17] To be exact, recall from my discussion in sub-subsections 3.1.2 and 3.1.3 that I construct EVER_DISPLACED_i as:

$$\text{EVER\_DISPLACED}_i \equiv \mathbb{I}\left(\sum_{t=1}^{4} \text{DISPLACED}_{i,t} > 0\right)$$

where $\text{DISPLACED}_{i,t}$ is an indicator for individual i having experienced job displacement in round t or, in other words, having reported switching current main jobs in round t of interviews because of (1) being laid off (2) business being dissolved or closed or (3) job ending. Therefore, in order to experience the "treatment" in my analysis, individuals must have worked during at least one reference period. To a first order, my restriction that individuals in my analysis sample be employed in the reference period corresponding to round one is out of mechanical necessity. Also, note that, for individuals in my analysis sample, we have:

$$\mathbb{I}\left(\sum_{t=2}^{5} \text{OPIATE\_PRESC\_COUNT}_{i,t} \geq k\right) = \mathbb{I}\left(\sum_{t=1}^{5} \text{OPIATE\_PRESC\_COUNT}_{i,t} \geq k\right)$$

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

I am interested in identifying β, which will represent the causal effect of job displacement on the probability of using prescription opiates under the assumption that $\text{cov}(\text{EVER\_DISPLACED}_i, \epsilon_i \big| X_i, \text{PANEL}_i) = 0$. This identification condition amounts to job displacement being as if randomly assigned conditional on the control vector X_i and the fixed effects vector PANEL_i, and could be violated if survey participants are selected into job

[16] As discussed in sub-subsection 3.1.4, choosing a suitable k at which to cut off my analysis is somewhat arbitrary. In this case, I have chosen $k = 15$ as my maximum object of interest primarily for convenience's sake in showing empirical results. Showing higher or lower maximum k does not qualitatively affect the nature of my results, as I explore further in section 4.

[17] An alternative specification might be a specification of the form $\mathbb{I}(\text{OPIATE\_PRESC\_COUNT}_{i,t} \geq k) = \alpha + \beta \text{DISPLACED}_{i,t} + \gamma_i + \cdots + \epsilon_{i,t}$ which would employ individual fixed effects. To a first order, this might appear to be an improvement over specification 1 as it would more clearly identify prescription opiate use directly following job displacement and control for individual-specific time-invariant unobservable determinants of opiate use. However, this is not the case. Since MEPS data do not give exact dates for opiate prescriptions or job displacement, it is still impossible to distinguish between opiate use preceding job displacement and job displacement preceding opiate use when the two occur during the same reference period. Thus the risk of simultaneous causality bias or omitted variable due to selection into the treatment on unobservables correlated with opiate use is not reduced under this specification.

displacement on unobservable characteristics correlated with prescription opiate use, or, similarly, if individuals were laid off in part due to their prescription opiate use rather than increasing their opiate use due to job displacement. Indeed, there is some evidence from Hilger (2016) and Roulet (2017) that displaced workers may be negatively selected into displacement on productivity and health status. If this were the case, my estimations of β in equation equation 1 would overstate the effect of job displacement on the probability of beginning to use prescription opiates. The risk of omitted variable bias and simultaneous causality bias along these lines is the reason I restrict my analysis sample to survey participants who have zero opiate prescriptions during the reference period corresponding to the first round of interviews. My rationale is that restricting individuals in my analysis sample not to have used opiates during the first reference period, during which I require that they work, reduces the likelihood that they are selected into job displacement due to low productivity associated with prescription opiate use. So my restriction that survey participants work during the first reference period serves a second purpose of pre-treatment screening against negative selection into job displacement. With all this said, however, my estimates of the effects of job displacement on opiate use are likely to suffer from some upward bias, as my pre-treatment screening and conditioning on available health status variables are not airtight guards against negative selection into displacement on unobservable characteristics associated with opiate use.

3.2.2 Treatment effect heterogeneity for subgroups of survey participants

The existing literature and economic intuition give some reason to be concerned with how the effects of job displacement on prescription opiate use vary along a variety of dimensions. For the purposes of this thesis, I am primarily interested in whether the effects of job displacement on opiate use vary along the axes of (1) basis for displacement (2) age and race (3) pre-displacement occupation (4) post-displacement health insurance coverage and (5) pre-displacement health status.

My interest in potential variation in the effects of job displacement depending on basis for displacement stems both from the literature – particularly Hilger (2016) and Roulet (2017) – and from economic intuition. It is at least hypothetically possible that low productivity and propensity to use opiates are correlated, even conditioning richly on health status, industry, occupation, and demographic characteristics. Both Hilger (2016) and Roulet (2017) make this argument, claiming that layoffs are not truly exogenous sources of job displacement, but that laid off workers are somewhat negatively selected into displacement on productivity dimensions. As such, measuring the effect of layoff-based displacement versus displacement due to business closure or employment term ending may be a prudent strategy for reducing the risk of omitted variable bias due to negative selection into treatment. On the other hand, even in the absence of selection issues, the effects of job displacement may differ according to the basis of displacement because the psychological impact of being laid off may be more strongly negative than that of displacement due to other causes. From a worker's standpoint, being laid off is plausibly more despair-inducing than displacement due to business closure because a worker may envy or feel inferior to peers from their former place of employment who were not laid off. Furthermore, being laid off plausibly induces more despair than having a job end, since, unlike a job ending, a layoff was not anticipated or pre-specified by a contract. If this were true, using equation 1 to estimate separate causal effects of layoffs and non-layoff displacement would yield coefficients $\beta_{\text{layoff}} > \beta_{\text{non-layoff displacement}}$.

My interest in testing for different effects of job displacement on prescription opiate use depending on age, race, and occupation is also motivated by the literature. In particular, my interest in effects potentially varying by age and race comes from Case and Deaton's (2015; 2017) argument that deaths due to drug and alcohol poisoning are one of the strongest drivers of recent increases in midlife mortality among non-Hispanic whites and their argument that these deaths are likely due to worsening economic outlook from generation to generation for some non-Hispanic whites. While I cannot feasibly test Case and Deaton's (2017) exact formulation of this latter hypothesis, the MEPS demographic information allows me to test whether job displacement-related increases in opiate use could contribute to recent upticks in opiate abuse and poisonings among the demographic group they highlight. To investigate this hypothesis, I interact age group and race indicator variables with displacement indicators and add these interaction terms to equation 1. Furthermore, Charles, Hurst, and Schwartz's (2018) argument that manufacturing workers increase their opiate demand after experiencing labor market dislocations associated with reductions in the manufacturing share of local employment suggests job-specific effects of displacement on opiate use. In particular, Charles, Hurst, and Schwartz (2018) argue that manufacturing workers affected by labor demand contractions suffer particularly "adverse shock[s] to wellbeing" which can lead to opiate use and abuse. To investigate this hypothesis, I add an interaction between a displacement indicator and an indicator for being a blue-collar worker, which I define as working in a blue-collar occupation during the reference period corresponding to round one to equation 1.[18]

I suspect differential effects of job displacement depending on post-displacement health insurance status, since post-displacement health insurance status might be an important mechanism by which job displacement could result in lower probability of beginning to use opiates. Although job displacement may induce despair which, in turn, may increase agents' desire for prescription opiates, it also conceivably reduces agents' ability to pay for drugs.[19] Therefore, it may be reasonable to expect differing effects for individuals who lose health insurance after displacement and those who do not. Intuitively, the "despair" effect may dominate for the latter group, leading to greater opiate use, whereas reduced ability to pay for opiates among individuals in the former group may dominate any despair-induced post-displacement desire for prescription opiates. As such, I rewrite equation 1 as:

$$\begin{aligned}\mathbb{1}\left(\sum_{t=1}^{5}\text{OPIATE\_PRESC\_COUNT}_{i,t} \geq k\right) &= \alpha + \beta_0\text{EVER\_DISPLACED}_i + \beta_1\text{HELD\_EMP\_HI}_{i,1}\\ &+ \beta_2\mathbb{1}\left(\sum_{t=1}^{4}\text{DISPLACED}_{i,t}\times\text{HELD\_EMP\_HI}_{i,t}\times(1-\text{HELD\_EMP\_HI}_{i,t+1}) > 0\right)\\ &+ X_i'\gamma + \text{PANEL}_i'\rho + \epsilon_i\end{aligned}$$

where $\text{HELD\_EMP\_HI}_{i,1}$ is an indicator for whether survey participant i held employer-offered insurance during the reference period corresponding to round one and:

$$\mathbb{1}\left(\sum_{t=1}^{4}\text{DISPLACED}_{i,t}\times\text{HELD\_EMP\_HI}_{i,t}\times(1-\text{HELD\_EMP\_HI}_{i,t+1}) > 0\right)$$

is an indicator for whether in any round of interviews the survey participant reported (1) experiencing job displacement (2) holding employer-offered insurance in the pre-displacement reference period and (3) not holding employer-offered insurance in the post-displacement reference period. I show summary statistics for this indicator in table 8. Thus β_0 in this specification is the effect of displacement for individuals who did not experience any change in health insurance coverage post-displacement. Intuitively, the "despair effect" should dominate for these individuals whereas, for individuals who lose employer-offered health insurance, either the despair effect or reduced ability to obtain opiates could dominate; this would imply $\beta_0 > \beta_0 + \beta_2$.

[18] Recall that I classify blue-collar occupations according to the taxonomy shown in table 7.

[19] This is related to the central idea of Roulet (2017), namely, that healthcare utilization after displacement is likely dependent on financial circumstances.

Table 8: Prevalence of employer-offered health insurance loss among displaced individuals in analysis sample, dis-aggregated by job displacement type

	(1) *Displaced (overall)*		(2) *Laid off*		(3) *Non-layoff displacement*	
	Proportion	Std. dev.	Proportion	Std. dev.	Proportion	Std. dev.
Lost HI when displaced	0.325	0.468	0.451	0.497	0.206	0.404
Observations	8,279		3,738		4,854	

Statistics are nationally representative, as they incorporate panel-specific survey weights.
Displaced individuals are considered to have lost employer-offered insurance due to displacement if they held employer-offered insurance in a pre-displacement reference period for a round of interviews and did not hold employer-offered insurance in a post-displacement reference period.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

Similarly, intuition regarding the mechanisms by which job displacement may affect prescription opiate use suggests that agents' pre-displacement health status may determine whether they can obtain opiate prescriptions to assuage job displacement-induced despair. For instance, if physicians are wary of prescribing opiates to patients who have not previously reported health issues for which opiates are commonly prescribed, displaced workers may find themselves unable to obtain opiate prescriptions regardless of the level of their despair or their ability to pay. In this case, only individuals who reported experiencing physical difficulty prior to displacement would be able to obtain opiates after being displaced. If this hypothesis were true, we would likely find that job displacement affects an individuals' probability of using opiate painkillers more than it affects their probability of beginning to use opiate cough medicines. Physicians may be more likely to suspect abuse and withhold opiate painkillers as opposed to, say, codeine-infused cough suppressants because pain medications such as hydrocodone and oxycodone, rather than opiate cough medicines, are known to be the most commonly abused prescription opiate medications (Butler et al., 2004; Katz et al., 2010; Sehgal, Manchikanti, and Smith, 2012). At the same time, conditional on having a history of chronic pain which would constitute a defensible rationale for seeking opiate painkillers, workers might easily parlay claims of pain into opiate prescriptions, since pain is notoriously difficult for physicians to verify, whereas respiratory conditions are relatively straightforward to diagnose (Ling, Mooney, and Hillhouse, 2011). In contrast, it is relatively straightforward for physicians to determine whether their patients suffer from respiratory illnesses for which opiate-infused cough medicines might be prescribed, due to the abundance of visible symptoms of respiratory illnesses.

In order to test the hypothesis that job displacement affects workers differently depending on their pre-displacement health status, I construct an indicator for "round one pain," for which I show summary statistics in table 9. Note that the round one pain indicator is an indicator for experiencing any of the subset of health issues enumerated in table 5 which MEPS interviewers ask about in round one of interviews.[20] I add this indicator to equation 1, interact this indicator with job displacement, and control only for the subvector of X which does not overlap with the health status indicators enumerated in table 9. These modifications amount to:

$$\mathbb{1}\left(\sum_{t=1}^{5} \text{OPIATE\_PRESC\_COUNT}_{i,t} \geq k\right)$$
$$= \alpha + \beta_0 \text{EVER\_DISPLACED}_i + \beta_1 \text{PAIN}_{i,1} + \beta_2 \text{EVER\_DISPLACED}_i \times \text{PAIN}_{i,1} + Y_i'\delta + \text{PANEL}_i'\rho + \epsilon_i$$

where Y_i is the desired subvector and I anticipate $\beta_2 > \beta_0$. Since physicians are most likely to be suspicious of

[20]Recall from sub-subsection 3.1.5 that MEPS interviewers do not ask about each health condition in table 5 in each round of interviews

opiate abuse among individuals seeking opiate painkillers, I show results for this specification both including and excluding opiate cough medicines from my left-hand-side summation.[21] I anticipate that $|\beta_2 - \beta_0|$ as measured when I exclude opiate cough medicines should be larger than the $|\beta_2 - \beta_0|$ I measure when I include all prescription opiates.

Table 9: Summary statistics of round one pain indicator and components thereof, analysis sample

	Proportion	Standard Deviation
Reported experiencing pain in round one	.306	(.460)
Reported missing work due to health status in round one	.281	(.449)
Reported "fair" or "poor" health status in round one	.072	(.259)
Reported physical limitations due to health status in round one	.039	(.194)
Reported being unable to do activity due to health status in round one	.001	(.042)
Reported limitations in work activities due to health status	.013	(.114)
Reported difficulty bending or stooping in round one	.023	(.152)
Reported difficulty grasping with fingers in round one	.007	(.088)
Reported difficulty walking a mile in round one†	.024	(.154)
Reported difficulty reaching overhead in round one	.011	(.108)
Reported difficulty standing for more than 20 mins. in round one	.019	(.136)
Reported difficulty walking three blocks in round one	.019	(.139)
Reported using an assistive device in round one	.003	(.061)
Observations	86,190	

Statistics are nationally representative, as they incorporate panel-specific survey weights.
The median individual in the analysis sample reported missing work due to health status zero days in round one; as such, the indicator for "Reported missing work due to health status in round one" can also be seen for missing work due to health status more than the median analysis sample individual.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
†indicates that data is only available for variables starting in 2000, at which point the MEPS substantially expected its asking health status question. Statistics above are for indicators of whether and individual ever reported being of a given health status during their time in the survey.

4 Results

I estimate specification 1 using ordinary least squares and show my estimates for $k = 1$ in table 10. They suggest that job displacement makes affected individuals significantly less likely to start using opiates or, in other words, to accumulate one or more opiate prescriptions. The effect size I measure, between 1.3 and 1.8 percentage points, depending on whether we restrict to pre-2010 data, is considerable, amounting to a roughly 10% reduction in the overall probability of beginning to use opiates, per summary statistics shown in table 3. Figure 3 shows the corresponding results for all thresholds of opiate use (e.g. $k = 1, 2, \ldots, 15$). Point estimates of the regression coefficients on the displacement indicator suggest that displacement makes affected individuals less likely to start using opiates at any of the thresholds. While these coefficients shrink in size to roughly one-tenth of a percentage point at the 15-prescription threshold, the reductions in probability are sizeable in comparison to the baseline probabilities of accumulating k prescriptions shown in table 3 for every level of k. In particular, the reduction in probability is roughly one quarter the overall probability for opiate use at the two- or three-prescription thresholds, one tenth for the four- to six-prescription thresholds, and closer to one half for other thresholds, though the statistical significance of these results depends on whether we include or exclude

[21] I show regression results excluding cough medicines for all specifications in appendix C.

survey participants who entered the survey after 2010 from our analysis sample.[22]

Table 10: OLS estimates, indicator for opiate use on indicator for displacement

	(1) Full time range	(2) Pre-2010
Ever experienced job displacement	-0.0130*	-0.0186**
	(0.00582)	(0.00704)
Constant	0.0251	0.0487
	(0.0229)	(0.0282)
Controls for age, race, sex, education, industry, and occupation	X	X
Controls for health status	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	62,259	40,427

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Estimates are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

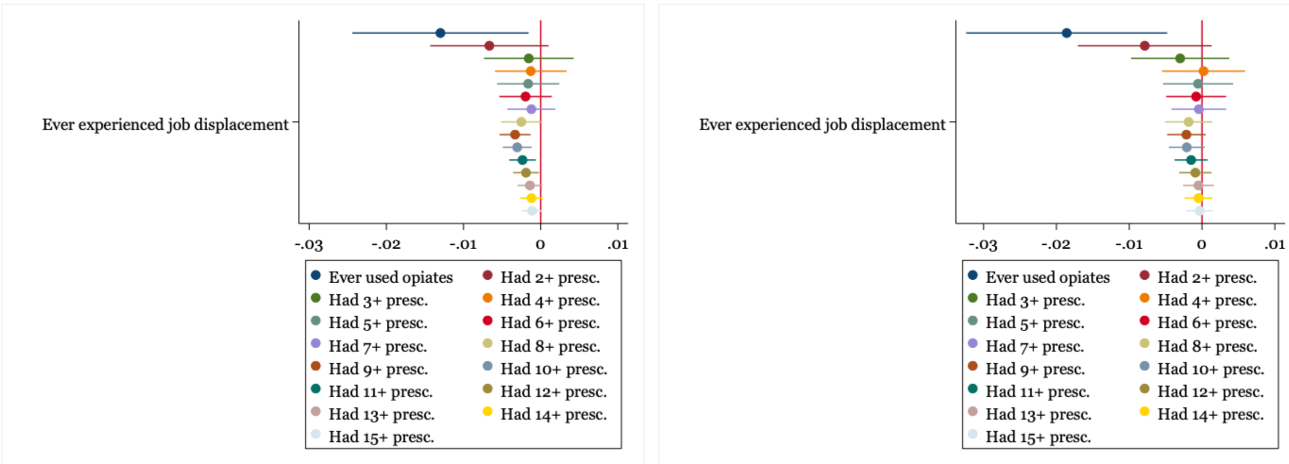
Basis for job displacement turns out to be critical for determining the effects of job displacement, as table 11 shows. Separately regressing an indicator for beginning to use opiates on an indicator for ever being laid off and ever experiencing non-layoff displacement reveals that the former has a smaller negative effect which is not statistically distinct from zero, whereas the latter has a larger negative effect, roughly two percentage points or a 12% reduction in the probability of beginning to use opiates, which is highly statistically significant. This suggests that the effects of displacement I observe in table 10 are something of a weighted average of two distinct effects, one strong and significant negative effect corresponding to non-layoff displacement and one ambiguous relationship corresponding to layoff-based displacement. Figure 4 tells much the same story for all thresholds of opiate use (e.g. $k = 1, 2, \ldots, 15$), with coefficients on layoff indicators being smaller and less statistically significant than those corresponding to non-layoff displacement, and sometimes even positive (e.g. for the three- to seven-prescription thresholds in the full time range regressions). However, coefficients on indicators for non-layoff displacement are also indistinguishable from zero for a variety of thresholds, including from $k = 2$ to $k = 5$ for either full time range or pre-2010 regressions and for a greater number of coefficients for opiate use thresholds above $k = 5$ when we only consider pre-2010 data, suggesting that even non-layoff displacement may not always change the probability that individuals begin to use opiates.[23] Just as in table 11, the effect magnitudes for non-layoff displacement shrink as k increases to roughly two tenths of a percentage point at the $k = 15$ threshold, but are quite large relative to baseline probabilities shown in table 3, with point estimates for any threshold of opiate use being roughly double the effect sizes of overall displacement as shown in figure 3. Notably, this implies that experiencing non-layoff displacement makes individuals roughly

[22] Displacement coefficients are statistically significant in the following full time range regressions: $k = 2$ ($p = 0.090$), $k = 8$ ($p = 0.061$), $k = 9$ ($p = 0.001$), $k = 10$ ($p = 0.002$), $k = 11$ ($p = 0.008$), $k = 12$ ($p = 0.026$), $k = 13$ ($p = 0.089$); and in the following pre-2010 regressions: $k = 2$ ($p = 0.093$), $k = 10$ ($p = 0.095$).

[23] Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 5$ ($p = 0.098$), $k = 6$ ($p = 0.025$), $k = 7$ ($p = 0.062$), $k = 8$ ($p = 0.020$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.002$), $k = 11$ ($p = 0.002$), $k = 12$ ($p = 0.017$), $k = 13$ ($p = 0.024$), $k = 14$ ($p = 0.002$), $k = 15$ ($p = 0.001$); and in the following pre-2010 regressions: $k = 8$ ($p = 0.019$), $k = 9$ ($p = 0.078$), $k = 14$ ($p = 0.048$).

Figure 3: Regression coefficients on indicator for job displacement in regressions of opiate use (various thresholds) on displacement

(a) Full time range (b) Pre-2010



Displacement coefficients are statistically significant in the following full time range regressions: $k = 2$ ($p = 0.090$), $k = 8$ ($p = 0.061$), $k = 9$ ($p = 0.001$), $k = 10$ ($p = 0.002$), $k = 11$ ($p = 0.008$), $k = 12$ ($p = 0.026$), $k = 13$ ($p = 0.089$); and in the following pre-2010 regressions: $k = 2$ ($p = 0.093$), $k = 10$ ($p = 0.095$). Regressions condition on age, race, sex, education, industry, occupation, health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

one hundred percent less likely to begin using opiates from the eight- to fifteen-prescription thresholds – and these estimates are highly statistically significant for all thresholds of use if we consider estimates taken from the full range of available data.

In contrast, age, race, and pre-displacement occupation do not seem to be key determinants of whether displacement makes individuals more or less likely to use prescription opiates, as tables 12, 13, and 14 show. Table 12 shows that, even though individuals from 45-54 are significantly less likely to starting using prescription opiates than young (25- to 34-year-old) individuals, the effect of job displacement – either layoffs or non-layoff displacement – do not significantly differ from the overall effect of job displacement for any age group, as coefficients on interaction terms between different displacement measures and age group indicators are not statistically distinct from zero. Furthermore, as seen in table 13, neither layoffs nor non-layoff displacement have differential effects on individuals of any racial group except Native Americans as indicated by statistically insignificant coefficients on interaction terms between racial group indicators and displacement measure indicators. The statistically significant coefficient on the Native American indicator interacted with the non-layoff displacement indicator is suggestive of a particularly strong negative effect of non-layoff displacement for Native Americans, but these results are to be taken cautiously given the small size of the Native American subpopulation in my analysis sample.[24] Though point estimates shown in table 13 suggest that white individuals are more likely than their black counterparts to begin using opiates, the relationship is not statistically significant. Similarly, table 14 gives evidence that layoffs do not differentially affect individuals working in blue-collar (e.g. physical labor-intensive) occupations, as suggested by statistically insignificant coefficients on interactions between indicators for different job displacement measures and an indicator for working in a blue-collar

[24] As shown in table 1, Native Americans make up seven tenths of a percent of my analysis sample, or roughly $86,190 \cdot 0.007 \approx 600$ individuals, not accounting for survey weights.

Table 11: OLS estimates, indicator for opiate use on indicator for displacement dis-aggregated by displacement type

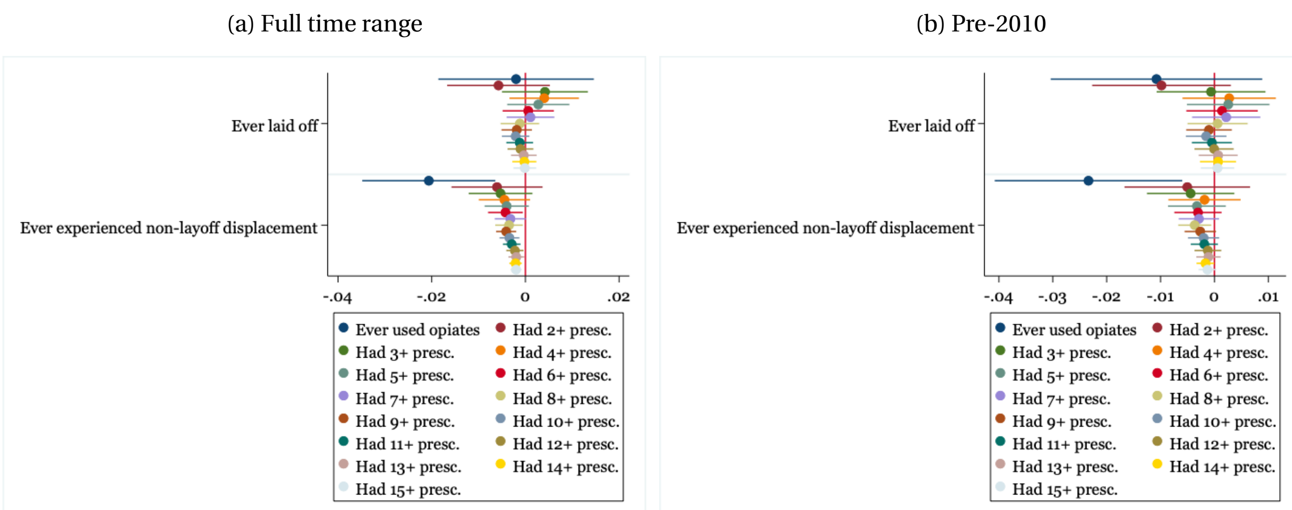
	(1) Full time range	(2) Full time range	(3) Pre-2010	(4) Pre-2010
Ever laid off	-0.00239		-0.0113	
	(0.00846)		(0.0100)	
Ever experienced non-layoff displacement		-0.0206**		-0.0236**
		(0.00724)		(0.00887)
Constant	0.0235	0.0253	0.0466	0.0485
	(0.0229)	(0.0229)	(0.0283)	(0.0282)
Controls for age, race, sex, edu., ind., and occ.	X	X	X	X
Controls for health status	X	X	X	X
Panel fixed effects	X	X	X	X
Heteroskedasticity-robust standard errors	X	X	X	X
Restricted sample	X	X	X	X
Observations	62,259	62,259	40,427	40,427

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Estimates are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Figure 4: Regression coefficients on indicator for job displacement (dis-aggregated by displacement type) in regressions of opiate use (various thresholds) on displacement, full time range and pre-2010



Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 5$ ($p = 0.098$), $k = 6$ ($p = 0.025$), $k = 7$ ($p = 0.062$), $k = 8$ ($p = 0.020$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.002$), $k = 11$ ($p = 0.002$), $k = 12$ ($p = 0.017$), $k = 13$ ($p = 0.024$), $k = 14$ ($p = 0.002$), $k = 15$ ($p = 0.001$); and in the following pre-2010 regressions: $k = 8$ ($p = 0.019$), $k = 9$ ($p = 0.078$), $k = 14$ ($p = 0.048$). No layoff coefficients are statistically significant. Non-layoff displacement includes job loss due to business closure and job ending (e.g. for term employment). Regressions condition on age, race, sex, education, industry, occupation, health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

occupation in the reference period corresponding to round one. Though point estimates from table 14 suggest that individuals working in blue-collar occupations have an overall higher probability of beginning to use opiates, the relationship is not statistically significant and the observed effect size is quite small, suggesting that working in a blue-collar occupation makes individuals only roughly five tenths of a percentage point or less than five percent more likely to begin using opiates after the reference period corresponding to the first round of interviews.

While the effect of job displacement does not appear to vary by age, race, or pre-displacement occupation, it may depend on pre-displacement health status for individuals who experience layoffs. In table 15, we see that including an interaction term between an indicator for experiencing pain in round one and both types of displacement changes the sign of the effect of layoffs on the probability of beginning to use opiates, and the coefficient on the interaction between round one pain and an indicator for being laid off is large and strongly positive (either two and a half or three and a half percentage points, depending on whether we use data from after 2010), though not statistically significant. Results from figure 5 are more suggestive of differential effects depending on round one pain: the negative effect of layoffs becomes highly statistically significant beyond the $k = 8$ threshold for both full time range and pre-2010 regressions.[25] In other words, the results suggest that layoffs make individuals who did not experience pain in the first round less likely to accumulate large numbers of opiate prescriptions. On the other hand, layoffs appeared to make individuals more likely to accumulate between seven and nine opiate prescriptions if they reported pain during the first round of interviews, and point estimates for other thresholds suggest positive effects, though none are statistically distinguishable from zero.[26] The effect sizes for regression coefficients on layoff indicators are small in absolute terms, on the order of three tenths of a percentage point or less for $k > 8$ prescriptions, but amount to roughly one hundred percent reductions in the probability of accumulating $k > 8$ opiate prescriptions for individuals who experienced layoffs but did not report experiencing round one pain. Notably, neither table 15 nor figure 5 show any such differential effect for non-layoff displacement; though adding an interaction between non-layoff displacement and round one pain causes the coefficient on non-layoff displacement to become statistically indistinguishable from zero in the $k = 1$ case, the effect becomes statistically significant once again around the $k = 9$ threshold.[27] Moreover, the effect sizes are similar to those shown in figure 4.

To further explore the possibility that the effects of job displacement differ depending on pre-displacement health status, I modify the specifications whose OLS estimates are shown in table 15 and figure 5 to exclude opiate cough medicines from survey participants' opiate prescription tally and only consider survey participants' propensity to use opiate painkillers. As I discuss in sub-subsection 3.2.2, my reasons for doing this are twofold. First, physicians may be more likely to be suspicious of requests for opiate painkillers and withhold prescription opiates from individuals without previous pain complaints. Second, conditional on having a history of chronic pain, displaced workers may more easily convince physicians to prescribe opiate painkillers than opiate cough medicines, since it is more difficult to verify whether patients actually suffer from pain than from respiratory

[25] Layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 2$ ($p = 0.073$), $k = 8$ ($p = 0.000$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.000$), $k = 11$ ($p = 0.000$), $k = 12$ ($p = 0.000$), $k = 13$ ($p = 0.000$), $k = 14$ ($p = 0.000$), $k = 15$ ($p = 0.000$). Layoff displacement coefficients are statistically significant in the following pre-2010 regressions: $k = 8$ ($p = 0.004$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.000$), $k = 11$ ($p = 0.000$), $k = 12$ ($p = 0.000$), $k = 13$ ($p = 0.002$), $k = 14$ ($p = 0.008$), $k = 15$ ($p = 0.042$).

[26] Layoff-round one pain interaction coefficients are statistically significant in the following full time range regressions: $k = 7$ ($p = 0.065$), $k = 8$ ($p = 0.064$), $k = 9$ ($p = 0.077$). Layoff-round one pain interaction coefficients are statistically significant in the $k = 8$ full time range regression ($p = 0.090$).

[27] Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 9$ ($p = 0.009$), $k = 10$ ($p = 0.019$), $k = 11$ ($p = 0.002$), $k = 12$ ($p = 0.009$), $k = 13$ ($p = 0.000$), $k = 14$ ($p = 0.000$), $k = 15$ ($p = 0.000$). Non-layoff displacement-round one pain interaction coefficients are statistically significant in the following full time range regressions: $k = 6$ ($p = 0.016$), $k = 7$ ($p = 0.058$), $k = 8$ ($p = 0.011$). Non-layoff displacement coefficients are statistically significant in the following pre-2010 range regressions: $k = 13$ ($p = 0.100$), $k = 14$ ($p = 0.000$), $k = 15$ ($p = 0.000$). Non-layoff displacement-round one pain interaction coefficients are statistically significant in the following pre-2010 regressions: $k = 6$ ($p = 0.077$), $k = 8$ ($p = 0.067$).

Table 12: OLS estimates, indicator for opiate use on indicators for job displacement and interactions between job displacement and indicators for age group (25-34 omitted)

	(1) Full time range	(2) Pre-2010
Ever laid off	0.00899	0.00216
	(0.0132)	(0.0159)
Ever experienced non-layoff displacement	-0.0312**	-0.0314*
	(0.0109)	(0.0139)
Age 35-44 (indicator)	-0.00306	-0.00512
	(0.00465)	(0.00570)
Age 45-54 (indicator)	-0.0287***	-0.0334***
	(0.00504)	(0.00615)
Ever laid off × Age 35-44 (indicator)	-0.0152	-0.0132
	(0.0197)	(0.0234)
Ever laid off × Age 45-54 (indicator)	-0.0219	-0.0303
	(0.0209)	(0.0246)
Ever non-layoff displaced × Age 35-44 (indicator)	0.0169	0.0118
	(0.0167)	(0.0209)
Ever non-layoff displaced × Age 45-54 (indicator)	0.0181	0.0143
	(0.0181)	(0.0214)
Constant	0.0251	0.0485
	(0.0230)	(0.0283)
Controls for age, race, sex, education, industry, health status, and occupation	X	X
Controls for health status	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	62,259	40,427

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Estimates are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Table 13: OLS estimates, indicator for opiate use on indicators for job displacement and interactions between job displacement and indicators for racial group (black omitted)

	(1) Full time range	(2) Pre-2010
Ever laid off	-0.00349	-0.0142
	(0.0176)	(0.0224)
Ever experienced non-layoff displacement	0.00410	-0.00891
	(0.0188)	(0.0235)
Native American	0.0163	0.0453
	(0.0221)	(0.0273)
Alaska Native	-0.238***	-0.246***
	(0.0130)	(0.0156)
Asian/Pacific Islander	-0.0603***	-0.0685***
	(0.00681)	(0.00880)
White	0.00573	0.0110
	(0.00518)	(0.00664)
Multi-race	0.00852	0.0000544
	(0.0176)	(0.0220)
Other race	-0.0362	-0.0275
	(0.0315)	(0.0389)
Native American × Ever laid off	0.0944	0.125
	(0.1000)	(0.116)
Native American × Ever non-layoff displaced	-0.146*	-0.205**
	(0.0653)	(0.0776)
Asian/Pacific Islander× Ever laid off	-0.0160	0.0186
	(0.0290)	(0.0382)
Asian/Pacific Islander × Ever non-layoff displaced	-0.00648	0.0101
	(0.0282)	(0.0378)
White × Ever laid off	0.00197	0.00201
	(0.0202)	(0.0252)
White × Ever non-layoff displaced	-0.0279	-0.0164
	(0.0205)	(0.0255)
Multi-race × Ever laid off	-0.00757	0.0158
	(0.0728)	(0.0959)
Multi-race × Ever non-layoff displaced	-0.0337	0.0610
	(0.0573)	(0.0884)
Other race × Ever laid off	-0.00267	-0.0666
	(0.0493)	(0.0583)
Other race × Ever non-layoff displaced	-0.0854	-0.113
	(0.0594)	(0.0709)
Constant	0.0134	0.0126
	(0.0118)	(0.0141)
Controls for age, race, sex, education, industry, health status, and occupation	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	62,259	40,427

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Omitted group for race dummies is black. No Alaska Natives in analysis sample were displaced; as such, coefficients on Alaska Native interactions with different displacement measures are omitted due to collinearity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews. Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Table 14: OLS estimates, indicator for opiate use on indicators for job displacement and interactions between job displacement and indicator for blue-collar occupation

	(1) Full time range	(2) Pre-2010
Ever laid off	0.00263	-0.00615
	(0.0106)	(0.0126)
Ever experienced non-layoff displacement	-0.0140	-0.0168
	(0.00889)	(0.0110)
Worked in a blue-collar occ. during round one	0.00539	0.00748
	(0.00566)	(0.00688)
Worked in a blue-collar occ. during round one × Ever laid off	-0.0134	-0.0117
	(0.0173)	(0.0206)
Worked in a blue-collar occ. during round one × Ever non-layoff-displaced	-0.0236	-0.0236
	(0.0150)	(0.0182)
Constant	0.0255	0.0520
	(0.0223)	(0.0275)
Controls for age, race, sex, education, industry, health status, and occupation	X	X
Controls for health status	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	62,259	40,427

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Omitted group for occupation classification is white-collar.
Estimates are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Table 15: OLS estimates, indicator for opiate use on indicators for displacement and round one pain-displacement interactions

	(1) Full time range	(2) Full time range	(3) Pre-2010	(4) Pre-2010
Reported experiencing pain in round one	0.00537	0.00789	0.0110*	0.0140*
	(0.00451)	(0.00454)	(0.00537)	(0.00583)
Ever laid off	-0.0122		-0.0181	
	(0.00950)		(0.0113)	
Laid off × Round one pain	0.0348		0.0245	
	(0.0190)		(0.0221)	
Ever experienced non-layoff displacement		-0.0149		-0.0127
		(0.00818)		(0.0102)
Non-layoff displaced × Round one pain		-0.0171		-0.0347
		(0.0168)		(0.0199)
Constant	0.0307	0.0316	0.0506	0.0540*
	(0.0224)	(0.0224)	(0.0276)	(0.0271)
Controls for age, race, sex, edu., ind., and occ.	X	X	X	X
Controls for health status	X	X	X	X
Panel fixed effects	X	X	X	X
Heteroskedasticity-robust standard errors	X	X	X	X
Restricted sample	X	X	X	X
Observations	62,259	62,259	40,427	40,427

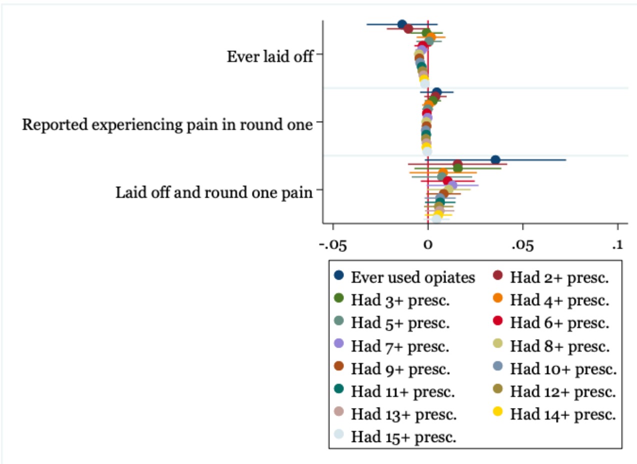
Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

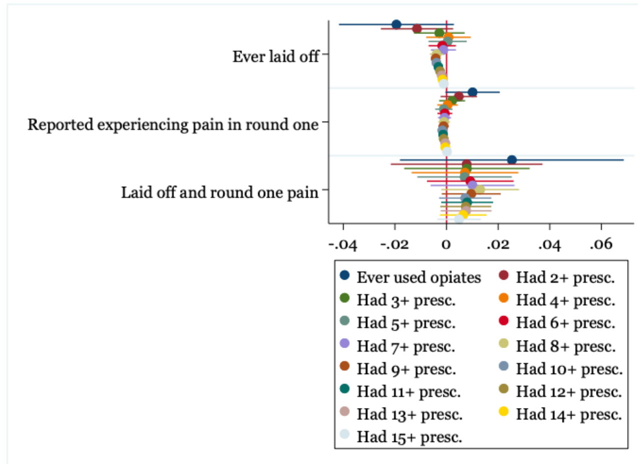
Estimates are nationally representative, as they incorporate panel-specific survey weights.
"Reported experiencing pain in round one" is an indicator for a survey participant having reported any of the following in round one: (1) having not worked due to pain (2) have "poor" or "fair" self-reported health (3) having had an inpatient stay at a hospital (4) having difficulty walking one mile or around a block (5) reporting being unable to do activity due to physical limitations (6) being unable to bend over (7) having difficulty grasping with fingers (8) having difficulty reaching overhead (9) having difficulty standing or (10) using an assistive device. These are all the physical health measures for which data is available in round one.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Figure 5: Regression coefficients on indicator for job displacement (dis-aggregated by displacement type) and round one pain-displacement interaction in regressions of opiate use (various thresholds) on displacement, full time range and pre-2010

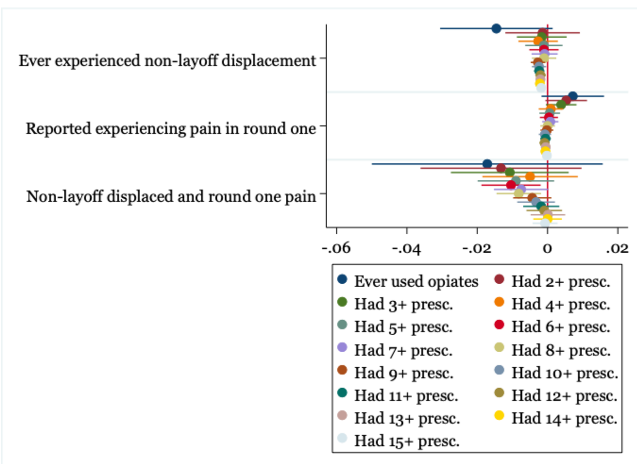
(a) Full time range, layoffs



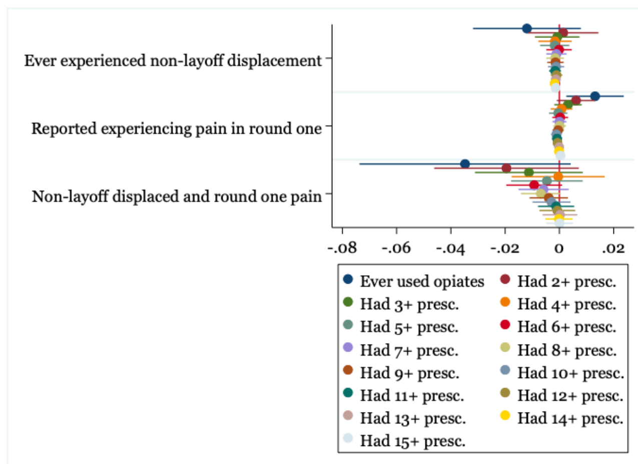
(b) Pre-2010, layoffs



(c) Full time range, non-layoff displacement



(d) Pre-2010, non-layoff displacement



Layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 2$ ($p = 0.073$), $k = 8$ ($p = 0.000$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.000$), $k = 11$ ($p = 0.000$), $k = 12$ ($p = 0.000$), $k = 13$ ($p = 0.000$), $k = 14$ ($p = 0.000$), $k = 15$ ($p = 0.000$). Layoff-round one pain interaction coefficients are statistically significant in the following full time range regressions: $k = 7$ ($p = 0.065$), $k = 8$ ($p = 0.064$), $k = 9$ ($p = 0.077$). Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 9$ ($p = 0.009$), $k = 10$ ($p = 0.019$), $k = 11$ ($p = 0.002$), $k = 12$ ($p = 0.009$), $k = 13$ ($p = 0.000$), $k = 14$ ($p = 0.000$), $k = 15$ ($p = 0.000$). Non-layoff displacement-round one pain interaction coefficients are statistically significant in the following full time range regressions: $k = 6$ ($p = 0.016$), $k = 7$ ($p = 0.058$), $k = 8$ ($p = 0.011$). Layoff displacement coefficients are statistically significant in the following pre-2010 regressions: $k = 8$ ($p = 0.004$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.000$), $k = 11$ ($p = 0.000$), $k = 12$ ($p = 0.000$), $k = 13$ ($p = 0.002$), $k = 14$ ($p = 0.008$), $k = 15$ ($p = 0.042$). Layoff-round one pain interaction coefficients are statistically significant in the $k = 8$ full time range regression ($p = 0.090$). Non-layoff displacement coefficients are statistically significant in the following pre-2010 range regressions: $k = 13$ ($p = 0.100$), $k = 14$ ($p = 0.000$), $k = 15$ ($p = 0.000$). Non-layoff displacement-round one pain interaction coefficients are statistically significant in the following pre-2010 regressions: $k = 6$ ($p = 0.077$), $k = 8$ ($p = 0.067$). Non-layoff displacement includes job loss due to business closure and job ending (e.g. for term employment). Regressions condition on age, race, sex, industry, education, occupation, health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

conditions, which cough medicines would treat.

I show results for these modified specifications in table 16 and figure 6. As expected, excluding opiate cough medicines from individuals' opiate prescription tally leads to starker differences between the effects of job displacement for workers with and without pre-displacement pain complaints. In particular, the results in table 16 show estimates of the effect of being laid off on prescription opiate use which are larger (roughly two percentage points, as opposed to roughly one and a half percentage points) and more statistically significant than those shown in table 15 for $k = 1$ prescriptions, implying that being laid off makes individuals without histories of chronic pain nearly 12% less likely to begin using opiate painkillers. The estimates I show in figure 6 corroborate this result for higher thresholds of opiate prescriptions. My results imply that being laid off leads to a statistically significant reduction in the probability of beginning to use opiate painkillers for the $k = 2$ threshold, as well as all $k > 6$ thresholds.[28] Furthermore, my estimates in figure 6 corroborate the hypothesis that being laid off makes individuals who reported pain in round one of MEPS interviews more likely to accumulate large numbers of prescriptions for opiate painkillers; coefficients on indicators for layoff-round one pain interactions are statistically significant for the two-prescription threshold, as well as for the six- to nine-prescription thresholds.[29] The effect sizes of layoffs I measure in figure 6 resemble those I measure in figure 5; shrinking to roughly two tenths of a percentage points at the 15-prescription threshold, but amounting to roughly 100% reductions in the baseline probabilities of beginning to use opiates at any threshold at which the effect is statistically significant. The coefficients on the layoff-round one pain interaction I show in figure 6 are also quite large – in fact, larger than those shown in figure 5 – relative to the baseline probabilities of using opiates at any threshold. Indeed, these effects are sufficiently large that many of the interaction coefficients outweigh the negative main effect of being laid off so that the net effect of being laid off for individuals with round one pain complaints is positive. My estimates suggest that individuals with pre-displacement health problems who are laid off are roughly 10% (or nine tenths of a percentage point) more likely to begin using non-cough medicine opiates at the two-prescription threshold and more than 100% more likely to begin using non-cough medicine opiates between the $k = 6$ and $k = 9$ prescription thresholds. Furthermore, much like table 15 and figure 5, table 16 and figure 6 suggest that the effects of non-layoff displacement depend very little on pre-displacement health status.

Table 17 and figure 7 suggest that, for the most part, post-displacement health insurance loss is not an especially important mechanism for individuals decreasing their opiate consumption after displacement. Estimating the regression discussed in sub-subsection 3.2.2, in which I modify specification 1 to condition on round one health insurance status and add an interaction between displacement and post-displacement health insurance loss, yields displacement effects which, for the most part, do not differ in sign or magnitude from the coefficients I estimate in my main specification as shown in table 11 and figure 4. The effect of layoffs on job displacement is still statistically indistinguishable from zero in all regressions with primarily small, negative point estimates, and the effect of non-layoff displacement is still large, negative, and generally statistically significant, though the degree of statistical significance varies by opiate use threshold and whether or not I include individuals who entered the survey after 2010.[30] The only importance difference I observe is that, for high thresh-

[28] Layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 2$ ($p = 0.004$), $k = 6$ ($p = 0.012$), $k = 7$ ($p = 0.000$), $k = 8$ ($p = 0.000$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.000$), $k = 11$ ($p = 0.000$), $k = 12$ ($p = 0.000$), $k = 13$ ($p = 0.000$), $k = 14$ ($p = 0.000$), $k = 15$ ($p = 0.001$). Layoff displacement coefficients are statistically significant in the following pre-2010 regressions: $k = 2$ ($p = 0.002$), $k = 3$ ($p = 0.047$), $k = 6$ ($p = 0.022$), $k = 7$ ($p = 0.046$), $k = 8$ ($p = 0.020$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.000$), $k = 11$ ($p = 0.000$), $k = 12$ ($p = 0.001$), $k = 13$ ($p = 0.005$), $k = 14$ ($p = 0.016$), $k = 15$ ($p = 0.068$).

[29] Layoff-round one pain interaction coefficients are statistically significant in the following pre-2010 regressions: $k = 4$ ($p = 0.085$), $k = 6$ ($p = 0.058$), $k = 7$ ($p = 0.062$), $k = 8$ ($p = 0.058$), $k = 9$ ($p = 0.076$). Layoff-round one pain interaction coefficients are statistically significant in the following full time range regressions: $k = 2$ ($p = 0.062$), $k = 6$ ($p = 0.049$), $k = 7$ ($p = 0.032$), $k = 8$ ($p = 0.040$), $k = 9$ ($p = 0.048$).

[30] Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 8$ ($p = 0.078$), $k = 9$

Table 16: OLS estimates, indicator for opiate use excluding cough medicines on indicators for displacement and round one pain-displacement interactions

	(1) Full time range	(2) Full time range	(3) Pre-2010	(4) Pre-2010
Reported experiencing pain in round one	-0.00126	0.00132	-0.000204	0.00130
	(0.00412)	(0.00414)	(0.00488)	(0.00534)
Ever laid off	-0.0200*		-0.0205*	
	(0.00837)		(0.0102)	
Laid off × Round one pain	0.0353*		0.0187	
	(0.0171)		(0.0195)	
Ever experienced non-layoff displacement		-0.0112		-0.00811
		(0.00739)		(0.00933)
Non-layoff displaced × Round one pain		-0.0178		-0.0325
		(0.0153)		(0.0178)
Constant	-0.0251	-0.0246	0.00876	0.0116
	(0.0212)	(0.0211)	(0.0260)	(0.0255)
Controls for age, race, sex, edu., ind., and occ.	X	X	X	X
Controls for health status	X	X	X	X
Panel fixed effects	X	X	X	X
Heteroskedasticity-robust standard errors	X	X	X	X
Restricted sample	X	X	X	X
Observations	62,259	62,259	40,427	40,427

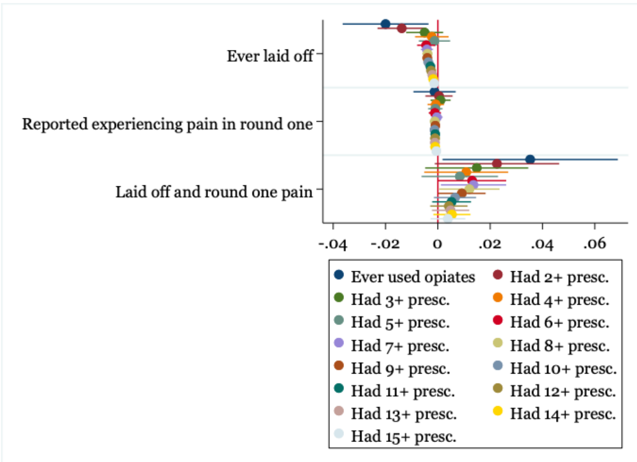
Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

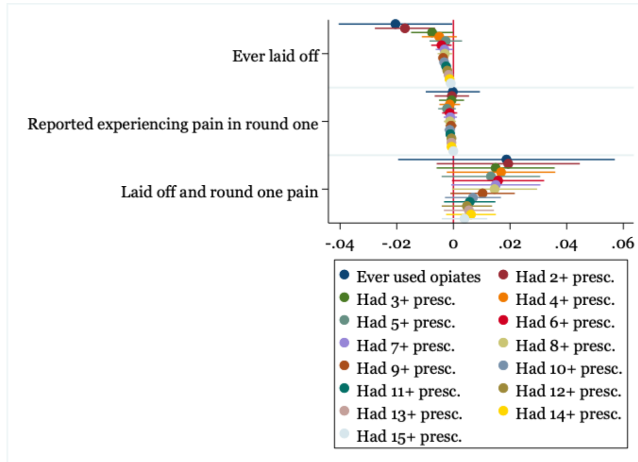
Estimates are nationally representative, as they incorporate panel-specific survey weights.
I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database.
"Reported experiencing pain in round one" is an indicator for a survey participant having reported any of the following in round one: (1) having not worked due to pain (2) have "poor" or "fair" self-reported health (3) having had an inpatient stay at a hospital (4) having difficulty walking one mile or around a block (5) reporting being unable to do activity due to physical limitations (6) being unable to bend over (7) having difficulty grasping with fingers (8) having difficulty reaching overhead (9) having difficulty standing or (10) using an assistive device. These are all the physical health measures for which data is available in round one.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Figure 6: Regression coefficients on indicator for job displacement (dis-aggregated by displacement type) and round one pain-displacement interaction in regressions of opiate use excluding cough medicines (various thresholds) on displacement, full time range and pre-2010

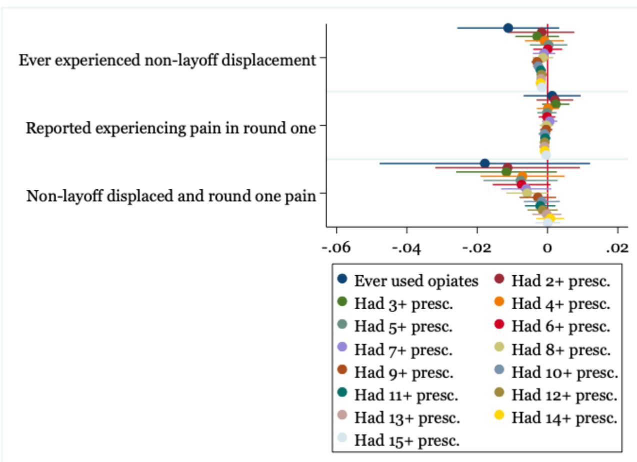
(a) Full time range, layoffs



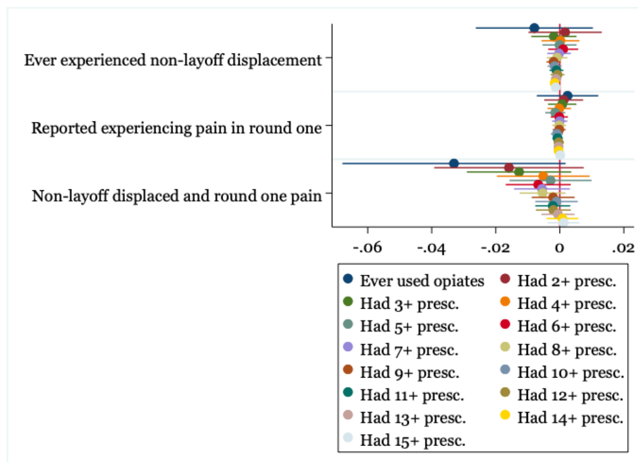
(b) Pre-2010, layoffs



(c) Full time range, non-layoff displacement



(d) Pre-2010, non-layoff displacement



Layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 2$ ($p = 0.004$), $k = 6$ ($p = 0.012$), $k = 7$ ($p = 0.000$), $k = 8$ ($p = 0.000$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.000$), $k = 11$ ($p = 0.000$), $k = 12$ ($p = 0.000$), $k = 13$ ($p = 0.000$), $k = 14$ ($p = 0.000$), $k = 15$ ($p = 0.001$). Layoff-round one pain interaction coefficients are statistically significant in the following full time range regressions: $k = 2$ ($p = 0.062$), $k = 6$ ($p = 0.049$), $k = 7$ ($p = 0.032$), $k = 8$ ($p = 0.040$), $k = 9$ ($p = 0.048$). Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 6$ ($p = 0.000$), $k = 10$ ($p = 0.001$), $k = 11$ ($p = 0.010$), $k = 12$ ($p = 0.024$), $k = 13$ ($p = 0.001$), $k = 14$ ($p = 0.000$), $k = 15$ ($p = 0.000$). Non-layoff displacement-round one pain interaction coefficients are statistically significant in the following full time range regressions: $k = 6$ ($p = 0.079$), $k = 7$ ($p = 0.096$), $k = 8$ ($p = 0.058$). Layoff displacement coefficients are statistically significant in the following pre-2010 regressions: $k = 2$ ($p = 0.002$), $k = 3$ ($p = 0.047$), $k = 6$ ($p = 0.022$), $k = 7$ ($p = 0.046$), $k = 8$ ($p = 0.020$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.000$), $k = 11$ ($p = 0.000$), $k = 12$ ($p = 0.001$), $k = 13$ ($p = 0.005$), $k = 14$ ($p = 0.016$), $k = 15$ ($p = 0.068$). Layoff-round one pain interaction coefficients are statistically significant in the following pre-2010 regressions: $k = 4$ ($p = 0.085$), $k = 6$ ($p = 0.058$), $k = 7$ ($p = 0.062$), $k = 8$ ($p = 0.058$), $k = 9$ ($p = 0.076$). Non-layoff displacement coefficients are statistically significant in the following pre-2010 range regressions: $k = 14$ ($p = 0.000$), $k = 15$ ($p = 0.000$). Non-layoff displacement-round one pain interaction coefficients are never statistically significant in pre-2010 regressions. I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database.Non-layoff displacement includes job loss due to business closure and job ending (e.g. for term employment). Regressions condition on age, race, sex, industry, education, occupation, health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

olds of opiate use (e.g. $k > 6$), the interaction between health insurance loss and non-layoff displacement are larger (roughly two percentage points), more strongly negative, and more statistically significant than coefficients on non-layoff displacement alone. In particular, for pre-2010 regressions of indicators for six or more opiate prescriptions, coefficients on indicators for non-layoff displacement are never statistically distinct from zero, whereas all coefficients on interactions between non-layoff displacement and post-displacement health insurance loss are highly statistically significant.[31]

Table 17: OLS estimates, indicator for opiate use on indicators for displacement and health insurance loss-displacement interactions

	(1) Full time range	(2) Full time range	(3) Pre-2010	(4) Pre-2010
Held employer-offered insurance in round one	0.000810	0.000946	0.000972	0.000945
	(0.00136)	(0.00136)	(0.00172)	(0.00172)
Ever laid off	0.00241		-0.0135	
	(0.0106)		(0.0125)	
Laid off and lost health insurance	-0.0108		0.00432	
	(0.0167)		(0.0195)	
Ever experienced non-layoff displacement		-0.0198*		-0.0199*
		(0.00803)		(0.00991)
Non-layoff displaced and lost health insurance		-0.00486		-0.0188
		(0.0176)		(0.0210)
Constant	0.0225	0.0241	0.0453	0.0469
	(0.0230)	(0.0230)	(0.0283)	(0.0283)
Controls for age, race, sex, edu., ind., and occ.	X	X	X	X
Controls for health status	X	X	X	X
Panel fixed effects	X	X	X	X
Heteroskedasticity-robust standard errors	X	X	X	X
Restricted sample	X	X	X	X
Observations	62,259	62,259	40,427	40,427

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

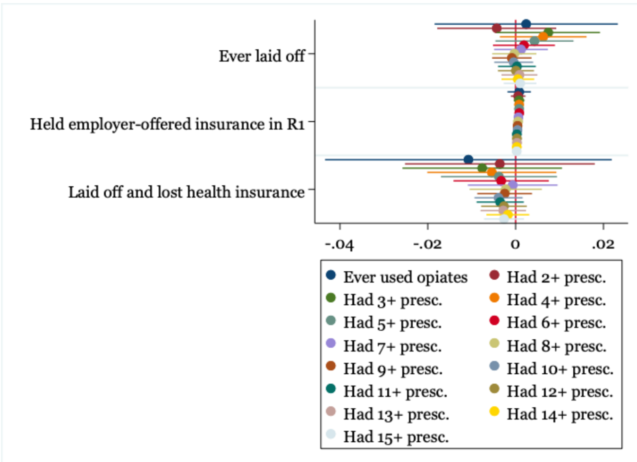
Estimates are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

($p = 0.001$), $k = 10$ ($p = 0.005$), $k = 11$ ($p = 0.005$), $k = 12$ ($p = 0.027$), $k = 13$ ($p = 0.033$), $k = 14$ ($p = 0.001$), $k = 15$ ($p = 0.005$).

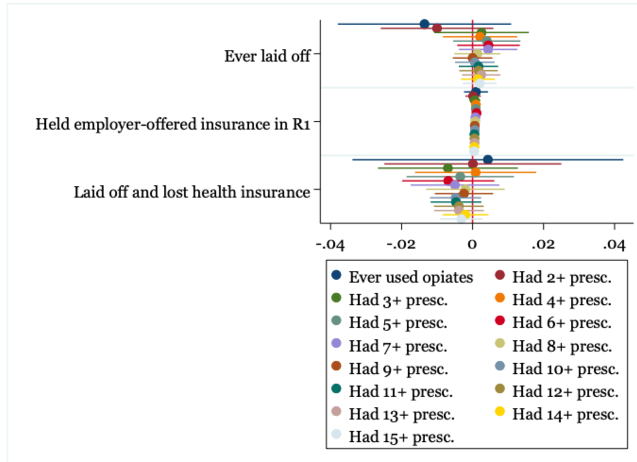
[31] $k = 6$ ($p = 0.001$), $k = 7$ ($p = 0.007$), $k = 8$ ($p = 0.047$), $k = 9$ ($p = 0.035$), $k = 10$ ($p = 0.040$), $k = 11$ ($p = 0.061$), $k = 12$ ($p = 0.058$), $k = 13$ ($p = 0.070$)

Figure 7: Regression coefficients on indicator for job displacement (dis-aggregated by displacement type) and health insurance loss-displacement interaction in regressions of opiate use (various thresholds) on displacement, full time range and pre-2010

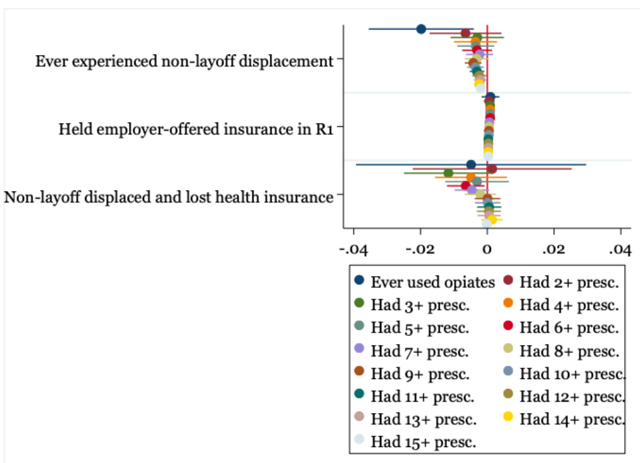
(a) Full time range, layoffs



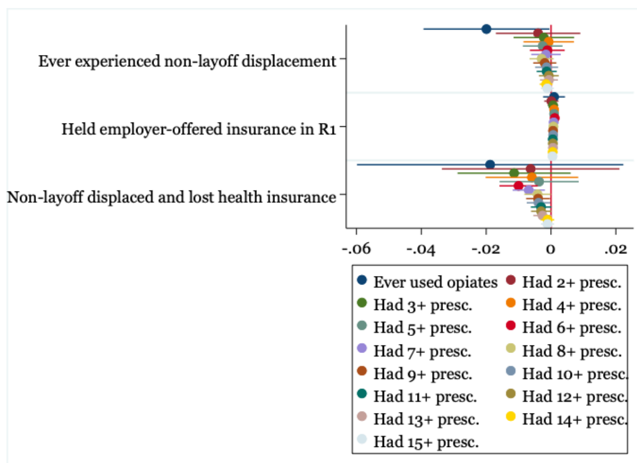
(b) Pre-2010, layoffs



(c) Full time range, non-layoff displacement



(d) Pre-2010, non-layoff displacement



All layoff coefficients and layoff-health insurance loss interaction coefficients are statistically indistinguishable from zero. Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 8$ ($p = 0.078$), $k = 9$ ($p = 0.001$), $k = 10$ ($p = 0.005$), $k = 11$ ($p = 0.005$), $k = 12$ ($p = 0.027$), $k = 13$ ($p = 0.033$), $k = 14$ ($p = 0.001$), $k = 15$ ($p = 0.005$). Non-layoff displacement-health insurance loss interaction coefficients are statistically significant in the following full time range regressions: $k = 3$ ($p = 0.086$), $k = 6$ ($p = 0.025$), $k = 7$ ($p = 0.080$). All non-layoff displacement coefficients are statistically indistinguishable from zero in pre-2010 regressions. Non-layoff displacement-health insurance loss interaction coefficients are statistically significant in the following pre-2010 regressions: $k = 6$ ($p = 0.001$), $k = 7$ ($p = 0.007$), $k = 8$ ($p = 0.047$), $k = 9$ ($p = 0.035$), $k = 10$ ($p = 0.040$), $k = 11$ ($p = 0.061$), $k = 12$ ($p = 0.058$), $k = 13$ ($p = 0.070$). Non-layoff displacement includes job loss due to business closure and job ending (e.g. for term employment). Regressions condition on age, race, sex, education, industry, occupation, health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

5 Discussion

Taken together, my results cast some doubt on the theoretical suggestions of previous papers considering the impact of labor market dislocations on job displacement. It may seem counter-intuitive that job displacement almost never causes statistically significant increases in the probability of beginning to use opiates at any use threshold. Indeed, the vast majority of statistically significant effects of job displacement I measure are decreases in the probability of opiate use.[32] Even point estimates of the the effect of layoffs on opiate use, which, as I discuss in subsection 3.2, are the most likely of my estimates to be biased upward by negative selection into the treatment on unobservables, are largely negative. While my findings are at odds with Charles, Hurst, and Schwartz's (2018) idea that reductions in well-being associated with job displacement lead workers to seek opiates, and somewhat incongruent with Case and Deaton's (2017) deaths of despair narrative which proposes that opiate demand should increase in response to labor market dislocations, they are not altogether inexplicable. Post-displacement decisions regarding prescription opiate use are likely not only driven by "despair" but also by the difficult financial circumstances brought on by job displacement, as I discuss in sub-subsection 3.2.2. Taking both despair and financial hardship into consideration, it is altogether probable that the latter dominates so that the net effect of displacement is a lower probability of beginning to use opiates.

My results in tables 12, 13, and 14 also cast doubt on Charles, Hurst, and Schwartz's (2018) and Case and Deaton's (2017) arguments. First, my results show that blue-collar workers, who Charles, Hurst, and Schwartz (2018) claim are differentially affected by labor market dislocations, are not in fact differentially impacted by displacement. If anything, point estimates for the effects of job displacement conditional on working in a blue-collar occupation are more strongly negative than those of experiencing job displacement overall, though these are to be interpreted with caution as they are not statistically significant. Furthermore, my results show that neither 45- to 54-year-old individuals nor whites are differentially affected by job displacement, which casts some doubt on the idea that rising mortality among individuals in these groups is driven by especially harsh effects of labor market dislocations on the probability of their beginning to use and abuse opiates.

For all of the doubt that my results cast on Case and Deaton's (2017) theory linking deaths of despair to labor market dislocation, it is worth recalling a critical feature of Case and Deaton's argument which allows it to coexist with my own, namely their focus on the effects of a worsening long-term (e.g. generation to generation) labor market outlook on the probability of opiate use as opposed to transitory labor market dislocations such as job displacement. It is possible in theory for a worsening long-term labor market outlook to increase the probability of opiate use while short-term labor market dislocations such as job displacement decrease the probability of opiate use. Furthermore, it is altogether possible that Case and Deaton's (2017) population of interest, namely individuals discouraged by their labor market prospects due to long-term worsening outlook, overlaps relatively little with my population of interest, since individuals who abuse opiates in response to having fewer labor market opportunities may be quite different than individuals who held jobs during the first reference period of MEPS participation and lost their jobs for reasons unrelated to drug use. My findings are more irrevocably at odds, however, with Charles, Hurst, and Schwartz's (2018) argument that transitory labor market dislocations cause upticks in demand for prescription opiates. Rather, my estimates of the effects of job displacement on opiate demand suggest that any increases in opiate use associated with local (e.g. county-level) labor market shocks, such as those observed by Charles, Hurst, and Schwartz (2018), Ruhm (2018), and, for some types of counties, Currie, Jin, and Schnell (2018), are likely due to increases in opiate supply associated with negative labor market shocks, increases in prescription opiate demand among individuals other than affected workers,

[32] The exceptions here are positive coefficients on interactions between job displacement and poor health status in round one. See tables 15 and 16, as well as figures 5 and 6.

or some combination of the two.[33] In this way, my thesis lends some credence to Ruhm's (2018) argument that changes in the "drug environment" – that is, changes in place-specific characteristics pertaining to the availability of risky drugs – are the primary reason that worsening economic circumstances over time are associated with higher county-level opiate drug deaths.

Another potentially important implication of my study is that Roulet's (2017) framework for understanding the effect of job displacement on opiate use holds up well in the United States. In particular, Roulet's (2017) framework is that post-displacement healthcare utilization is likely determined by post-displacement financial hardship or lack thereof, and Denmark has reduced or eliminated post-displacement reductions in healthcare utilization because they have increased individuals' post-displacement ability to pay for healthcare. An unsurprising extrapolation from Roulet's (2017) study, then, is that post-displacement healthcare utilization (in this case, prescription opiate use) would decrease in the United States, where job displacement is associated strongly with reduced ability to pay for healthcare services.

Aside from this thesis's contributions to discussions within the existing literature, my thesis also sheds light on the circumstances in which individuals experiencing job displacement may or may not parlay any post-displacement despair they feel into opiate prescriptions. My analysis of post-displacement health insurance loss as a mechanism for the effects of job displacement I observe suggests that non-insurance forms of financial hardship associated with job displacement are likely to be the primary mechanisms by which job displacement makes individuals less likely to begin using prescription opiates. The exception to this is that non-layoff displaced individuals are much less likely to accumulate many (e.g. more than eight) opiate prescriptions if they lose insurance. More importantly, perhaps, my findings in table 15 and figure 5 indicate that being laid off makes individuals with poor health records much more likely to accumulate large numbers of opiate prescriptions, whereas being laid off makes individuals without pre-displacement health problems much less likely to start using prescription opiates. These results may suggest that individuals who experience layoffs do in fact experience post-displacement despair, and that these individuals are able to convince physicians upon displacement that they need prescription opiates. Furthermore, table 16 and figure 6 show that layoffs make individuals who report pre-displacement pain more likely to begin using opiate painkillers at both high and low thresholds, whereas the effects of layoffs for individuals without histories of chronic pain closely resemble the effects of non-layoff displacement. The fact that my results are stronger when I exclude opiate cough medicines from individuals' opiate prescription tally and focus on opiate painkillers could support the idea that laid off workers rely on deception to obtain prescription opiates, since pain may be more difficult for physicians to verify than respiratory conditions for which opiate cough medicines would be prescribed. Correspondingly, laid off individuals without pre-displacement health issues may experience the same sort of despair, but are prevented from obtaining prescription opiates because their physicians are suspicious of new claims of chronic pain.

On the other hand, there is some reason to believe that these results are explained by mechanisms other than physician skepticism, given the consensus in the medical and public health literatures that physicians in the United States exercised little caution in prescribing opiates prior to 2010 (Atluri et al., 2014; Dart et al., 2015; Guy et al., 2017; Larochelle et al., 2015; Levy et al., 2015; Pletcher et al., 2008). First, the results could suggest differences in the degree to which layoffs induce despair depending on pre-displacement health status. It may be that workers with poor pre-displacement health experience more despair upon being laid off than workers in good health. This might be the case if workers in good health are more confident about their ability to secure new jobs when they are laid off but workers in poor health are more pessimistic about their employment prospects after being laid off. Another explanation may be that both workers with pre-displacement health is-

[33] Here I use "demand" and "supply" in the same sense as Finkelstein, Gentzkow, and Williams (2018), which I discuss in section 2, where "demand" refers to person-specific determinants of opiate use and "supply" refers to place-specific determinants of opiate use.

sues and those without experience the same degree of despair, but that laid off workers who were in good health prior to displacement do not somatize their despair with opiates. Finally, and perhaps most importantly, my results in tables 15 and 16, as well as those shown in figures 5 and 6, could be fully explained by negative selection into layoffs on unobservable characteristics associated with prescription opiate use. It may be that individuals who experience pain in round one are laid off because they start taking prescription opiates after round one due to pain and are correspondingly less productive.[34] As such, I am hesitant to make too much of the results I observe in tables 15 and 16 and figures 5 and 6.

My thesis suggests several further directions for research. Perhaps the most urgent research project stemming from my thesis would be to investigate the same research question – does job displacement induce prescription opiate abuse? – using an instrumental variables research design. Such a study could find stronger negative effects of job displacement (especially non-layoff displacement) on prescription opiate use. This is because my empirical strategy may still suffer from some omitted variable bias due to negative selection into job displacement on opiate-correlated unobservable characteristics, despite my efforts to mitigate such bias using pre-screening and health status conditioning. Charles, Hurst, and Schwartz (2018) and Currie, Jin, and Schnell (2018) both use the shift-share (Bartik) instrument to generate plausibly exogenous labor market shocks (Bartik, 1991). If geographic data associated with MEPS survey participants were available, it would be possible to use the rough NAICS industry codes associated with participants' round one employment to instrument layoffs using the shift-share instrument.[35] This analysis is in fact feasible using existing data from the MEPS. Researchers are able to access confidential MEPS files containing geographic data for survey participants by successfully completing an application process and by agreeing to conduct any analysis at the MEPS data center.

Another fruitful direction for research stemming from my thesis would be to further investigate the mechanisms by which job displacement makes individuals less likely to begin using opiates, which I have thus far assumed to be financial hardship but for which I have no firm evidence. My only foray into this research area (using displacement-related health insurance loss) has proven inconclusive, but investigating the effect of income loss associated with job displacement on prescription opiate use could yield more conclusive results. Investigating income loss as a mechanism for decreased prescription opiate use using MEPS data is difficult, however, since the MEPS only collects self-reported income data at the beginning of each of a survey participants' two years of survey participation, making MEPS income variables sub-optimal for identifying income shocks associated with job displacement.[36]

Finally, my thesis motivates further investigation into the question of how place-specific determinants of opiate use change in response to local labor market shocks. As discussed earlier in this section, this thesis gives reason to attribute increases in opiate use associated with local labor market shocks observed by Charles, Hurst, and Schwartz (2018), Currie, Jin, and Schnell (2018), and Ruhm (2018) to place-specific factors, since the demand-side impact of labor market dislocations on opiate use appears to be negative. However, the economics literature has yet to pinpoint the mechanisms by which labor market dislocations might increase local supply of prescription opiates. One possibility is that highly-trained physicians strategically relocate in the wake of negative labor market shocks so as to avoid reductions in compensation associated with having a greater proportion of their clientele covered by low-reimbursement insurers such as Medicaid. If this were the case, the remaining physicians in an afflicted area would be worse-trained physicians who, according to Currie and Schnell (2018), prescribe opiates at a much higher rate than physicians trained at higher-ranked medical schools.

[34] See subsection 3.2 discussion of Hilger (2016) and Roulet (2017), who argue that laid off workers are negatively selected on productivity dimensions.

[35] See Pinkham, Sorkin, and Swift (2019) for a detailed, up-to-date discussion of how to construct and use the shift-share instrument.

[36] See Banthin and Selden (2006) for a review of income variables in the MEPS.

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A Classifying opiate prescriptions

A variety of papers have attempted to classify prescriptions in the MEPS Prescribed Medicines files. Prescriptions might be classified as opiate 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 opiates 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 opiates). 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 opiates currently available in the United States and counting prescriptions as opiates if the National Drug Codes given for them in the MEPS Prescribed Medicines files match to National Drug Codes in the CDC database.

For a variety of reasons, none of the above methods are foolproof. Counting opiate 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.[37] Classifying opiates based on therapeutic class variables is unreliable because some prescription records whose non-proprietary names would suggest them being opiates 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 opiate prescriptions. Finally, counting opiate prescriptions using National Drug Codes is unreliable because many prescriptions in the MEPS files whose names would indicate that they are opiate 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 opiate prescriptions. In particular, my process is as follows:

1. Form a list of all proprietary names of prescription opiates available in the United States by searching for the names butorphanol, codeine, dihydrocodeine, fentanyl, hydrocodone, hydromorphone, levorphanol, meperidine, morphine, nalbuphine, opium, oxycodone, oxymorphone, pentazocine, propoxyphene, tapentadol, or tramadol in the FDA Orange Book, a database of approved drug products with therapeutic equivalence evaluations. For each entry in the list, give the non-proprietary name and the opiate component of the drug (e.g. "hydrocodone" for "hydrocodone and acetaminophen"). Then search for misspellings of the aforementioned non-proprietary names among prescriptions whose therapeutic class is given as "narcotic analgesic" or "narcotic analgesic combination." Add each of these misspellings to the list, also entering the properly spelled non-proprietary names and opiate components. Finally, search manually through MEPS Prescribed Medicines files for prescription record names which match sub-strings of either proprietary or non-proprietary names of opiate drugs, adding these to the list alongside their properly

[37] As Soni (2018) notes, “the drug name ‘Acetaminophen’ is spelled almost 70 different ways in the MEPS files.”

spelled non-proprietary names and opiate components.[38]

2. Merge the completed list to the Prescribed Medicines records, replacing misspelled non-proprietary names and proprietary names in the original Prescribed Medicines files with the properly spelled non-proprietary names in the list.

3. Merge CDC database of prescription opiates to Prescribed Medicines files using the National Drug Code associated with each prescription record therein. If an observation matches to a CDC catalogue entry based on its National Drug Code but prescription name is missing in the MEPS Prescribed Medicines file, replace prescription name with non-proprietary name associated with the National Drug Code in CDC database.

4. Classify a prescription record as an opiate prescription if its non-proprietary name contains one of the following strings: butorphanol, codeine, dihydrocodeine, fentanyl, hydrocodone, hydromorphone, levorphanol, meperidine, morphine, nalbuphine, opium, oxycodone, oxymorphone, pentazocine, propoxyphene, tapentadol, or tramadol.[39]

Figure 8 shows the prevalence of the four most commonly prescribed opiates in the MEPS data – namely codeine, hydrocodone, oxycodone, and tramadol – in the MEPS Prescribed Medicines files over time. Table 18 shows MME conversion factors.

Table 18: MME Conversion Factors

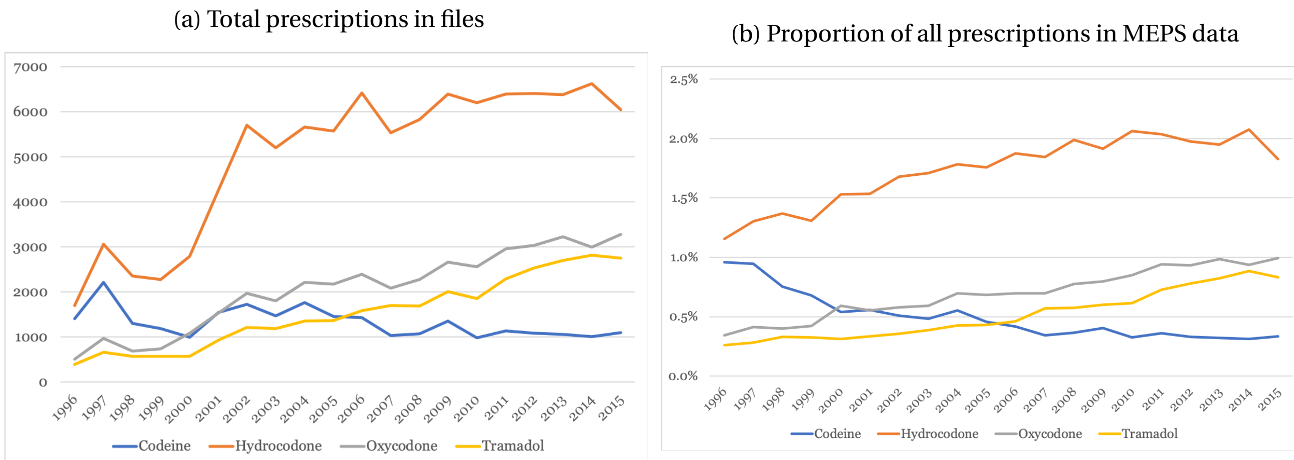
Opiate component	**Drug form**	**Conversion factor**	**Converting from**
Butorphanol	–	7	Milligrams
Codeine	–	0.15	Milligrams
Dihydrocodeine	–	0.25	Milligrams
Fentanyl	Tablets	0.13	Micrograms
Fentanyl	Lozenge	0.13	Micrograms
Fentanyl	Oral Spray	0.18	Micrograms
Fentanyl	Film	0.18	Micrograms
Fentanyl	Nasal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

Source for conversion factors: CDC Oral MME Equivalents Database, September 2017.

[38] My complete list, containing over one thousand misspellings and proprietary names found in the MEPS, can accessed `https://github.com/dustinswonder/thesis`.

[39] For the string "opium," I take care not to classify prescription records whose associated name is "ipratropium bromide" or some misspelling thereof as opiate prescriptions.

Figure 8: Prevalence of four most commonly prescribed opiates in MEPS Prescribed Medicines files, 1996-2015



I classify an opiate prescription as a codeine, hydrocodone, oxycodone, or tramadol prescription if the drugs' opiate component is codeine, hydrocodone, oxycodone, or tramadol, respectively. Therefore results shown in the figures above reflect both pure narcotic drug prescriptions as well as narcotic-analgesic combination prescriptions.

B Regression estimates conditioning only on round one health status

There exists a literature in health economics which attempts to estimate the health effects of job displacement, with some studies arguing that job displacement worsens health for displaced individuals in the United States (see, for instance, Schaller and Stevens, 2015). As such, it is altogether possible that my main specifications, in which I control for individuals' health status for the duration of their survey participation, "control for the treatment" so to speak. This would be true if a mechanism by which job displacement increases prescription opiate use is worse post-displacement health. To address these concerns, the following tables show OLS estimates of the effects of job displacement in a modified formulation of my main specification in which I only condition on round one (pre-displacement) health status. The risk with these estimates is that they will give upwardly biased estimates of the effects of job displacement due to negative selection into the treatment on health-related dimensions that I do not control for here, both because I am not able to control for health status for the duration of individuals' survey participation and because, as discussed in subsections 3.1.5 and 3.2, only a subset of health status variables are available during round one. In particular, rather than being able to control for the full vector of health status variables enumerated in table 5, I can only control for the vectors enumerated in table 9.

By and large, my results do not differ qualitatively from those shown in the main portion of my thesis. As table 20 shows, non-layoff displacement is no longer statistically significant when I exclude health status controls from beyond round one; point estimates of the effects of non-layoff displacement are also somewhat smaller. As figure 10 shows, positive coefficients on indicators for experiencing a layoff are statistically significant at some thresholds of prescription opiate use, though I am hesitant to place too much weight on these results due to the high risk that my estimates here suffer from omitted variable bias associated with negative selection into layoffs on health-related dimensions associated with opiate use. Displacement still does not appear to differentially affect individuals depending on their age, race, pre-displacement occupation, or post-displacement health insurance status. I see fewer statistically significant coefficients in my regressions including round one pain indicator interactions with different measures of job displacement, though point estimates are largely in line with what I observe in my main analysis.

Table 19: OLS estimates, indicator for opiate use on indicator for displacement conditional on round one health status only

	(1) Full time range	(2) Pre-2010
Ever experienced job displacement	-0.00448	-0.0108
	(0.00587)	(0.00691)
Constant	0.114***	0.135***
	(0.0238)	(0.0280)
Controls for age, race, sex, education, industry, and occupation	X	X
Controls for round one health status	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	69,318	47,486

Standard errors in parentheses

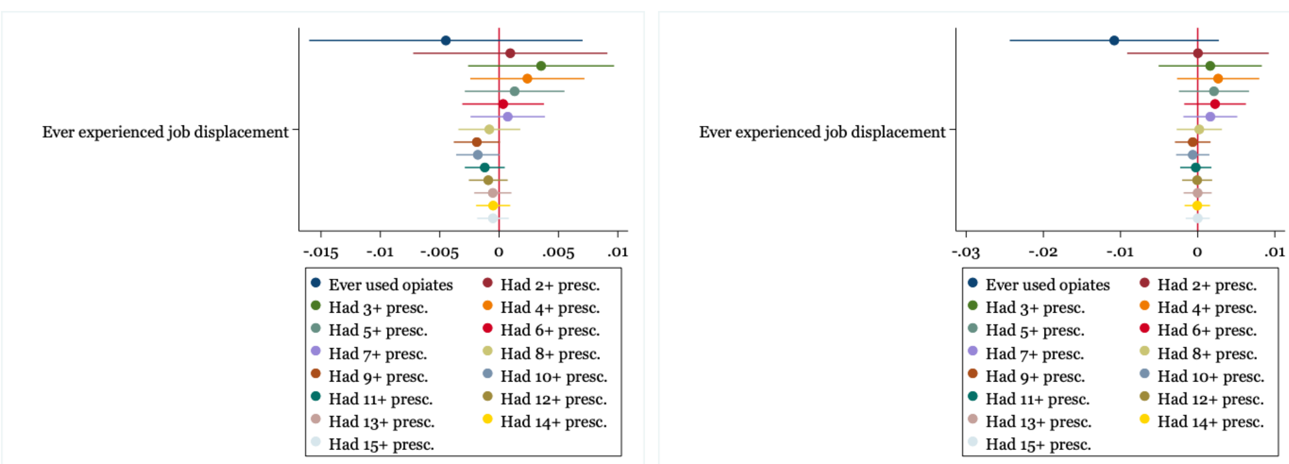
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Estimates are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Figure 9: Regression coefficients on indicator for job displacement in regressions of opiate use (various thresholds) on displacement conditional on round one health status only

(a) Full time range

(b) Pre-2010



Displacement coefficients are statistically significant in the following full time range regressions: $k = 9$, ($p = 0.058$), $k = 10$, ($p = 0.054$); no displacement coefficients were statistically significant in the pre-2010 regressions. Regressions condition on age, race, sex, education, industry, occupation, round one health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

Table 20: OLS estimates, indicator for opiate use on indicator for displacement dis-aggregated by displacement type conditional on round one health status only

	(1) Full time range	(2) Full time range	(3) Pre-2010	(4) Pre-2010
Ever laid off	0.00846		-0.00288	
	(0.00871)		(0.0101)	
Ever experienced non-layoff displacement		-0.0141		-0.0162
		(0.00727)		(0.00865)
Constant	0.113***	0.115***	0.134***	0.136***
	(0.0238)	(0.0238)	(0.0280)	(0.0280)
Controls for age, race, sex, edu., ind., and occ.	X	X	X	X
Controls for round one health status	X	X	X	X
Panel fixed effects	X	X	X	X
Heteroskedasticity-robust standard errors	X	X	X	X
Restricted sample	X	X	X	X
Observations	69,318	69,318	47,486	47,486

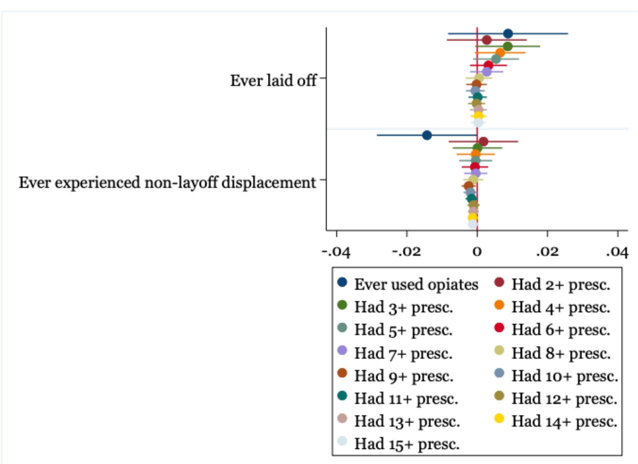
Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

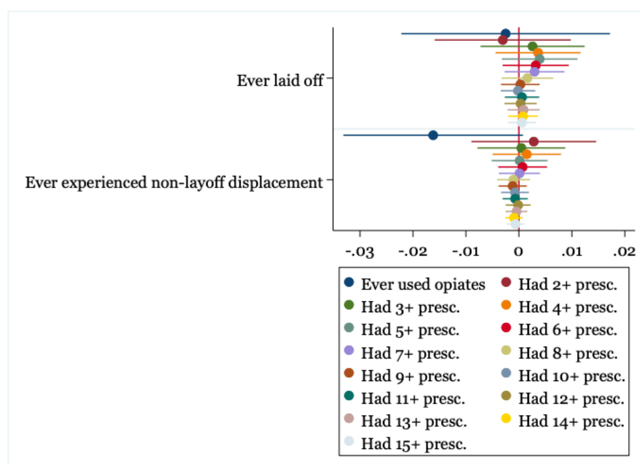
Estimates are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Figure 10: Regression coefficients on indicator for job displacement (dis-aggregated by displacement type) in regressions of opiate use (various thresholds) on displacement conditional on round one health status only, full time range and pre-2010

(a) Full time range



(b) Pre-2010



Layoff coefficients are statistically significant in the following full time range regressions: $k = 3$ ($p = 0.065$), $k = 4$ ($p = 0.076$). Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 9$ ($p = 0.016$), $k = 10$ ($p = 0.054$), $k = 11$ ($p = 0.083$), $k = 14$ ($p = 0.050$), $k = 15$ ($p = 0.034$); and in the following pre-2010 regressions: $k = 8$ ($p = 0.019$), $k = 9$ ($p = 0.078$), $k = 14$ ($p = 0.048$). No layoff or non-layoff displacement coefficients are statistically significant in pre-2010 regressions. Non-layoff displacement includes job loss due to business closure and job ending (e.g. for term employment). Regressions condition on age, race, sex, education, industry, occupation, health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

Table 21: OLS estimates, indicator for opiate use on indicators for job displacement and interactions between job displacement and indicators for age group (25-34 omitted) conditional on round one health status only

	(1) Full time range	(2) Pre-2010
Ever laid off	0.000517	-0.0146
	(0.0179)	(0.0220)
Ever experienced non-layoff displacement	0.0131	-0.00157
	(0.0183)	(0.0213)
Age 35-44 (indicator)	0.00453	0.00360
	(0.00460)	(0.00549)
Age 45-54 (indicator)	-0.000292	-0.00235
	(0.00477)	(0.00566)
Ever laid off × Age 35-44 (indicator)	-0.0108	-0.000121
	(0.0202)	(0.0233)
Ever laid off × Age 45-54 (indicator)	-0.00300	-0.00519
	(0.0217)	(0.0250)
Ever non-layoff displaced × Age 35-44 (indicator)	0.0179	0.0141
	(0.0170)	(0.0208)
Ever non-layoff displaced × Age 45-54 (indicator)	0.00664	-0.0108
	(0.0181)	(0.0209)
Constant	0.114^{***}	0.135^{***}
	(0.0238)	(0.0280)
Controls for age, race, sex, education, industry, health status, and occupation	X	X
Controls for round one health status	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	69,318	47,486

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Estimates are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Table 22: OLS estimates, indicator for opiate use on indicators for job displacement and interactions between job displacement and indicators for racial group (black omitted) conditional on round one health status only

	(1) Full time range	(2) Pre-2010
Ever laid off	-0.00349	-0.0142
	(0.0176)	(0.0224)
Ever experienced non-layoff displacement	0.00410	-0.00891
	(0.0188)	(0.0235)
Native American	0.0543*	0.0767**
	(0.0230)	(0.0274)
Alaska Native	-0.0515	-0.0429
	(0.105)	(0.104)
Asian/Pacific Islander	-0.0857***	-0.0892***
	(0.00684)	(0.00870)
White	0.0146**	0.0209**
	(0.00521)	(0.00643)
Multi-race	0.0307	0.0314
	(0.0192)	(0.0251)
Other race	-0.0393	-0.0274
	(0.0345)	(0.0421)
Native American × Ever laid off	0.0940	0.104
	(0.0935)	(0.106)
Native American × Ever non-layoff displaced	-0.185**	-0.232***
	(0.0611)	(0.0614)
Asian/Pacific Islander× Ever laid off	-0.0190	0.00727
	(0.0265)	(0.0336)
Asian/Pacific Islander × Ever non-layoff displaced	-0.00589	0.0205
	(0.0280)	(0.0357)
White × Ever laid off	0.0109	0.0139
	(0.0206)	(0.0248)
White × Ever non-layoff displaced	-0.0308	-0.0172
	(0.0201)	(0.0235)
Multi-race × Ever laid off	0.00756	0.0336
	(0.0825)	(0.110)
Multi-race × Ever non-layoff displaced	-0.0443	0.0657
	(0.0606)	(0.0923)
Other race × Ever laid off	-0.0572	-0.0886
	(0.0622)	(0.0600)
Other race × Ever non-layoff displaced	-0.138***	-0.136**
	(0.0399)	(0.0483)
Constant	0.0660***	0.0691***
	(0.0122)	(0.0139)
Controls for age, race, sex, education, industry, R1 health status, and occupation	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	69,318	47,486

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Omitted group for race dummies is black. No Alaska Natives in analysis sample were displaced; as such, coefficients on Alaska Native interactions with different displacement measures are omitted due to collinearity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews. Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Table 23: OLS estimates, indicator for opiate use on indicators for job displacement and interactions between job displacement and indicator for blue-collar occupation conditional on round one health status only

	(1) Full time range	(2) Pre-2010
Ever laid off	0.0179	0.00597
	(0.0113)	(0.0132)
Ever experienced non-layoff displacement	-0.00921	-0.0120
	(0.00891)	(0.0106)
Worked in a blue-collar occ. during round one	0.0129*	0.0145*
	(0.00566)	(0.00666)
Worked in a blue-collar occ. during round one × Ever laid off	-0.0263	-0.0223
	(0.0174)	(0.0201)
Worked in a blue-collar occ. during round one × Ever non-layoff-displaced	-0.0185	-0.0155
	(0.0152)	(0.0181)
Constant	0.111***	0.132***
	(0.0232)	(0.0273)
Controls for age, race, sex, education, industry, health status, and occupation	X	X
Controls for round one health status	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	69,318	47,486

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Omitted group for occupation classification is white-collar.
Estimates are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Table 24: OLS estimates, indicator for opiate use on indicators for displacement and round one pain-displacement interactions conditional on round one health status only

	(1) Full time range	(2) Full time range	(3) Pre-2010	(4) Pre-2010
Reported experiencing pain in round one	0.0730***	0.0745***	0.0757***	0.0771***
	(0.00414)	(0.00416)	(0.00477)	(0.00480)
Ever laid off	-0.00577		-0.0135	
	(0.00911)		(0.0104)	
Laid off × Round one pain	0.0347		0.0292	
	(0.0178)		(0.0198)	
Ever experienced non-layoff displacement		-0.0168*		-0.0184*
		(0.00762)		(0.00890)
Non-layoff displaced × Round one pain		0.00491		0.000620
		(0.0150)		(0.0169)
Constant	0.103***	0.104***	0.115***	0.116***
	(0.0213)	(0.0213)	(0.0242)	(0.0242)
Controls for age, race, sex, edu., ind., and occ.	X	X	X	X
Controls for round one health status	X	X	X	X
Panel fixed effects	X	X	X	X
Heteroskedasticity-robust standard errors	X	X	X	X
Restricted sample	X	X	X	X
Observations	69,318	69,318	47,486	47,486

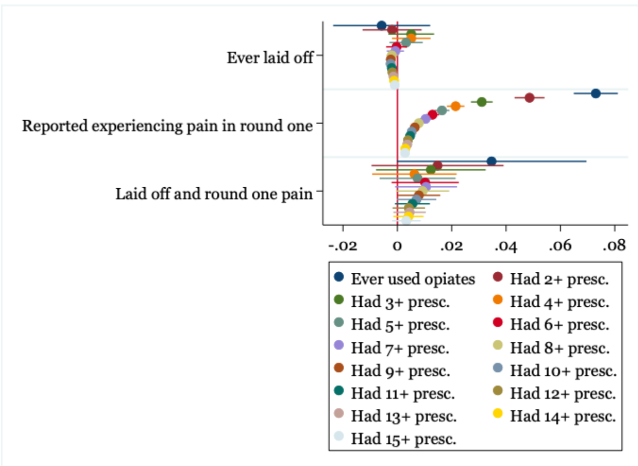
Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

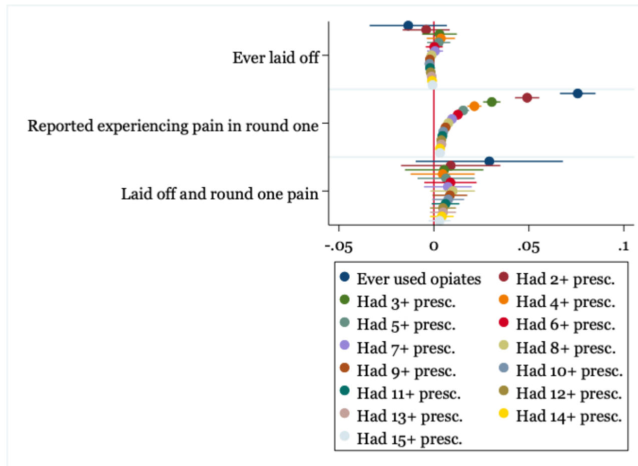
Estimates are nationally representative, as they incorporate panel-specific survey weights.
"Reported experiencing pain in round one" is an indicator for a survey participant having reported any of the following in round one: (1) having not worked due to pain (2) have "poor" or "fair" self-reported health (3) having had an inpatient stay at a hospital (4) having difficulty walking one mile or around a block (5) reporting being unable to do activity due to physical limitations (6) being unable to bend over (7) having difficulty grasping with fingers (8) having difficulty reaching overhead (9) having difficulty standing or (10) using an assistive device. These are all the physical health measures for which data is available in round one.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Figure 11: Regression coefficients on indicator for job displacement (dis-aggregated by displacement type) and round one pain-displacement interaction in regressions of opiate use (various thresholds) on displacement conditional on round one health status only, full time range and pre-2010

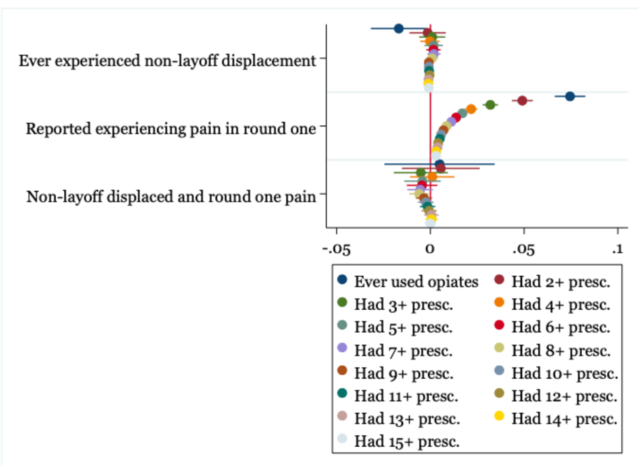
(a) Full time range, layoffs



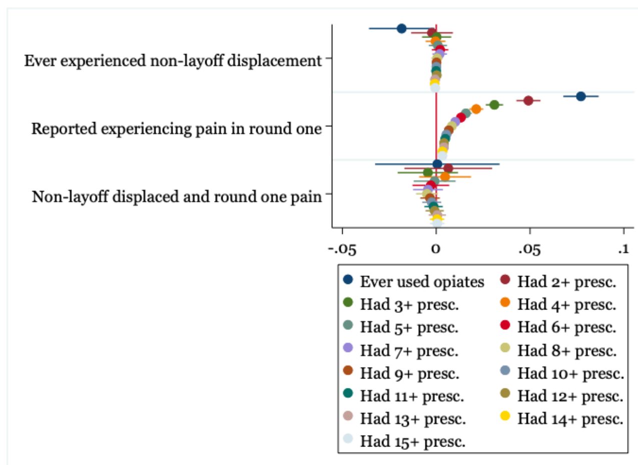
(b) Pre-2010, layoffs



(c) Full time range, non-layoff displacement



(d) Pre-2010, non-layoff displacement



Layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 8$ ($p = 0.049$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.000$), $k = 11$ ($p = 0.000$), $k = 12$ ($p = 0.000$), $k = 12$ ($p = 0.000$), $k = 13$ ($p = 0.000$), $k = 14$ ($p = 0.000$), $k = 15$ ($p = 0.000$). Layoff-round one pain interaction coefficients are statistically significant in the following full time range regressions: $k = 7$ ($p = 0.069$), $k = 8$ ($p = 0.062$), $k = 9$ ($p = 0.049$), $k = 10$ ($p = 0.058$), $k = 11$ ($p = 0.092$). Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 13$ ($p = 0.063$), $k = 14$ ($p = 0.006$), $k = 15$ ($p = 0.027$). Non-layoff displacement-round one pain interaction coefficients are statistically significant in the following full time range regressions: $k = 8$ ($p = 0.024$).
Layoff displacement coefficients are statistically significant in the following pre-2010 regressions: $k = 9$ ($p = 0.003$), $k = 10$ ($p = 0.000$), $k = 11$ ($p = 0.000$), $k = 12$ ($p = 0.000$), $k = 13$ ($p = 0.002$), $k = 14$ ($p = 0.010$), $k = 15$ ($p = 0.043$). Layoff-round one pain interaction coefficients are statistically significant in the following pre-2010 regressions: $k = 8$ ($p = 0.098$), $k = 9$ ($p = 0.071$), $k = 10$ ($p = 0.077$), $k = 11$ ($p = 0.010$). Non-layoff displacement coefficients are never statistically significant in pre-2010 range regressions. Non-layoff displacement-round one pain interaction coefficients are statistically significant in the $k = 8$ pre-2010 regression ($p = 0.095$). Non-layoff displacement includes job loss due to business closure and job ending (e.g. for term employment). Regressions condition on age, race, sex, industry, education, occupation, round one health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

Table 25: OLS estimates, indicator for opiate use on indicators for displacement and health insurance loss-displacement interactions conditional on round one health status only

	(1) Full time range	(2) Full time range	(3) Pre-2010	(4) Pre-2010
Held employer-offered insurance in round one	0.00186	0.00196	0.00247	0.00238
	(0.00134)	(0.00134)	(0.00166)	(0.00166)
Ever laid off	0.00530		-0.0160	
	(0.0110)		(0.0125)	
Laid off and lost health insurance	0.00620		0.0265	
	(0.0171)		(0.0196)	
Ever experienced non-layoff displacement		-0.0152		-0.0164
		(0.00809)		(0.00965)
Non-layoff displaced and lost health insurance		0.00429		-0.000330
		(0.0175)		(0.0206)
Constant	0.111^{***}	0.112^{***}	0.131^{***}	0.133^{***}
	(0.0238)	(0.0238)	(0.0281)	(0.0280)
Controls for age, race, sex, edu., ind., and occ.	X	X	X	X
Controls for round one health status	X	X	X	X
Panel fixed effects	X	X	X	X
Heteroskedasticity-robust standard errors	X	X	X	X
Restricted sample	X	X	X	X
Observations	69,318	69,318	47,486	47,486

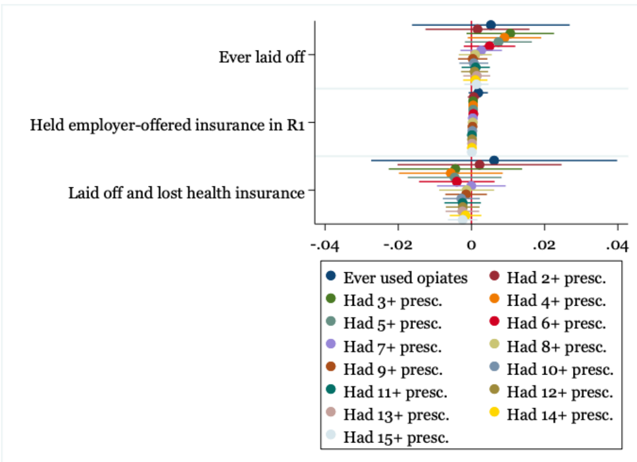
Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

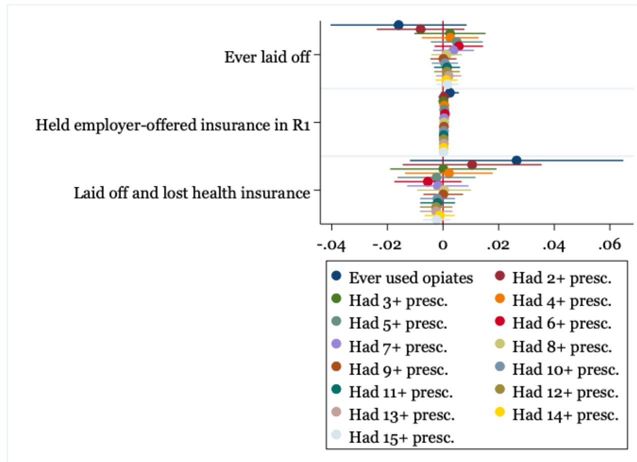
Estimates are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Figure 12: Regression coefficients on indicator for job displacement (dis-aggregated by displacement type) and health insurance loss-displacement interaction in regressions of opiate use (various thresholds) on displacement conditional on round one health status only, full time range and pre-2010

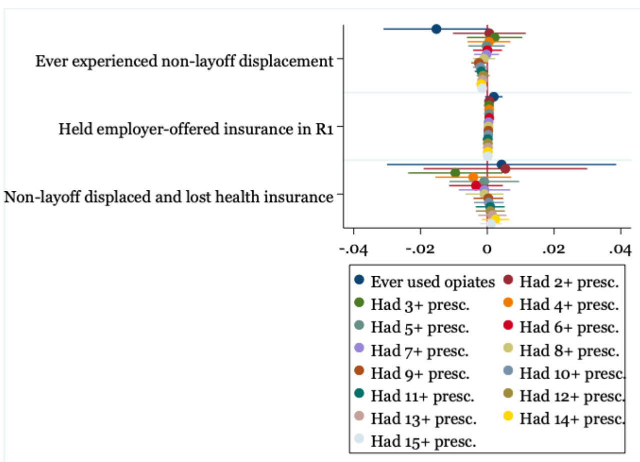
(a) Full time range, layoffs



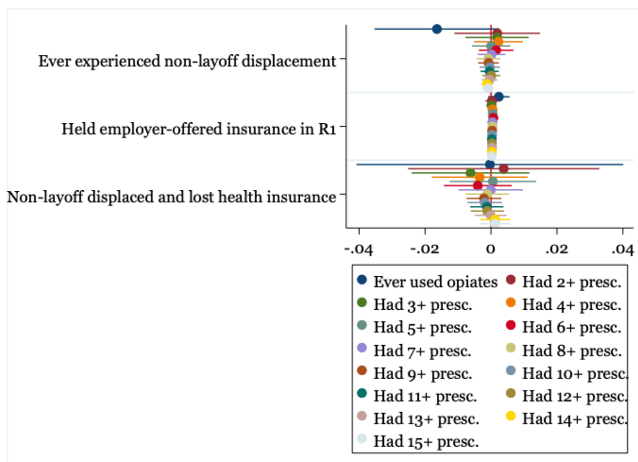
(b) Pre-2010, layoffs



(c) Full time range, non-layoff displacement



(d) Pre-2010, non-layoff displacement



Layoff coefficients are statistically significant in the following full time range regressions: $k = 2$ ($p = 0.082$), $k = 3$ ($p = 0.078$). Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 9$ ($p = 0.026$), $k = 10$ ($p = 0.067$), $k = 11$ ($p = 0.077$), $k = 13$ ($p = 0.097$), $k = 14$ ($p = 0.004$), $k = 15$ ($p = 0.014$). Interaction coefficients are never significant in the regressions above, nor or layoff or non-layoff displacement coefficients in pre-2010 regressions. Non-layoff displacement includes job loss due to business closure and job ending (e.g. for term employment). Regressions condition on age, race, sex, education, industry, occupation, round one health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

C Results excluding cough medicines

As discussed in detail in subsection 3.2, it is useful for some of my analysis to consider opiate prescriptions excluding cough medicines. I classify an opiate prescription as a cough medicine using non-proprietary names associated with prescriptions, proprietary names associated with some opiate prescriptions, and the drug form variable associated with opiate prescriptions. To be exact, I classify an opiate prescription as a cough medicine if it meets one of the three criteria:

1. The non-proprietary drug name associated with the prescription indicates that the drug is a cough medicine because it has the drug components of a cough medicine as catalogued in the FDA's database of approved drug products with therapeutic equivalence evaluations (the Orange Book).

2. The prescription record has a proprietary drug name associated with it, and the proprietary name is catalogued as a cough medicine in the FDA's database of approved drug products with therapeutic equivalence evaluations (the Orange Book).

3. The prescription is classified as an opiate prescription using the methodology described in appendix A, and the form of the prescription is "syrup" or "expectorant."

This method is clearly imperfect and is likely to undercount the number of opiate cough medicines in the MEPS Prescribed Medicines files due to data entry errors for the non-proprietary name variable, which I discuss at length in appendix A, and missing data for the drug form variable for some prescription records.

Figure 13 shows the proportion of all opiate prescriptions which are not classified as cough medicines. Opiate prescriptions not classified as prescriptions for cough medicines make up an increasing proportion of all opiate prescriptions in the MEPS Prescribed Medicines files over the period for which data is available, though non-cough medicine opiate prescriptions make up the vast majority of all opiate prescriptions for every year. The fact that non-cough medicine opiate prescriptions make up a greater proportion of all opiate prescriptions for more recent years is consistent with the statistics I show in appendix A figure 8, which shows that prescriptions for hydrocodone, oxycodone, and tramadol, all of which are primarily used as painkillers, increase as a proportion of all opiate prescriptions in the MEPS Prescribed Medicines files whereas codeine, which is the primary opiate drug used in cough medicines, becomes less prevalent in MEPS prescription records over time.

Tables 26 through 31 and figures 14 through 16 show results for most of the main specifications shown in section 4 modified to exclude cough medicines from survey participants' opiate prescription tally. I do not show results for the specifications in which I interact displacement indicators with pre-displacement health status indicators here, however, as these results are shown in the main text in table 16 and figure 6. The results shown below are virtually identical to their counterparts within section 4; none of the results differ qualitatively from the corresponding results in the main text, though non-layoff displacement decreases individuals' propensity to begin using opiate painkillers somewhat less than it reduces individuals' propensity to begin using any opiates whatsoever (see table 27). I still find that the effect of job displacement on the probability that individuals begin to use opiates does not vary along the axes of age, race, occupation, and post-displacement health insurance status.

Table 26: OLS estimates, indicator for opiate use excluding cough medicines on indicator for displacement

	(1) Full time range	(2) Pre-2010
Ever experienced job displacement	-0.0149**	-0.0185**
	(0.00523)	(0.00632)
Constant	-0.0219	0.0129
	(0.0208)	(0.0255)
Controls for age, race, sex, education, industry, and occupation	X	X
Controls for health status	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	62,259	40,427

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Estimates are nationally representative, as they incorporate panel-specific survey weights.
I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Table 27: OLS estimates, indicator for opiate use excluding cough medicines on indicator for displacement disaggregated by displacement type

	(1) Full time range	(2) Full time range	(3) Pre-2010	(4) Pre-2010
Ever laid off	-0.00873		-0.0146	
	(0.00753)		(0.00889)	
Ever experienced non-layoff displacement		-0.0170**		-0.0187*
		(0.00660)		(0.00806)
Constant	-0.0234	-0.0222	0.0110	0.0122
	(0.0208)	(0.0208)	(0.0256)	(0.0255)
Controls for age, race, sex, edu., ind., and occ.	X	X	X	X
Controls for health status	X	X	X	X
Panel fixed effects	X	X	X	X
Heteroskedasticity-robust standard errors	X	X	X	X
Restricted sample	X	X	X	X
Observations	62,259	62,259	40,427	40,427

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Estimates are nationally representative, as they incorporate panel-specific survey weights.
I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Table 28: OLS estimates, indicator for opiate use excluding cough medicines on indicators for job displacement and interactions between job displacement and indicators for age group (25-34 omitted)

	(1) Full time range	(2) Pre-2010
Ever laid off	-0.00670	-0.0127
	(0.0115)	(0.0138)
Ever experienced non-layoff displacement	-0.0200*	-0.0134
	(0.0101)	(0.0131)
Age 35-44 (indicator)	-0.00734	-0.0101
	(0.00425)	(0.00520)
Age 45-54 (indicator)	-0.0287***	-0.0304***
	(0.00462)	(0.00561)
Ever laid off × Age 35-44 (indicator)	-0.00115	0.00771
	(0.0176)	(0.0210)
Ever laid off × Age 45-54 (indicator)	-0.00473	-0.0150
	(0.0185)	(0.0213)
Ever non-layoff displaced × Age 35-44 (indicator)	0.00940	-0.00314
	(0.0153)	(0.0192)
Ever non-layoff displaced × Age 45-54 (indicator)	-0.000557	-0.0139
	(0.0164)	(0.0194)
Constant	-0.0219	0.0123
	(0.0208)	(0.0256)
Controls for age, race, sex, education, industry, health status, and occupation	X	X
Controls for round one health status	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	69,318	47,486

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Estimates are nationally representative, as they incorporate panel-specific survey weights.
I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Table 29: OLS estimates, indicator for opiate use excluding cough medicines on indicators for job displacement and interactions between job displacement and indicators for racial group (black omitted)

	(1) Full time range	(2) Pre-2010
Ever laid off	-0.00933	-0.0199
	(0.0163)	(0.0204)
Ever experienced non-layoff displacement	0.0187	0.00628
	(0.0183)	(0.0226)
Native American	-0.00750	0.0274
	(0.0200)	(0.0248)
Alaska Native	-0.172***	-0.175***
	(0.0119)	(0.0144)
Asian/Pacific Islander	-0.0538***	-0.0606***
	(0.00592)	(0.00750)
White	0.00857	0.0149*
	(0.00478)	(0.00609)
Multi-race	0.0116	0.0144
	(0.0167)	(0.0204)
Other race	-0.0108	0.000111
	(0.0313)	(0.0387)
Native American × Ever laid off	0.117	0.123
	(0.0949)	(0.111)
Native American × Ever non-layoff displaced	-0.137*	-0.154*
	(0.0580)	(0.0731)
Asian/Pacific Islander× Ever laid off	-0.000964	0.0318
	(0.0249)	(0.0325)
Asian/Pacific Islander × Ever non-layoff displaced	-0.0116	0.00656
	(0.0262)	(0.0344)
White × Ever laid off	0.000436	0.00401
	(0.0185)	(0.0228)
White × Ever non-layoff displaced	-0.0409*	-0.0294
	(0.0197)	(0.0243)
Multi-race × Ever laid off	-0.0545	0.0344
	(0.0551)	(0.0885)
Multi-race × Ever non-layoff displaced	-0.0337	0.0610
	(0.0573)	(0.0884)
Other race × Ever laid off	-0.000459	-0.0467
	(0.0492)	(0.0574)
Other race × Ever non-layoff displaced	-0.0918	-0.111
	(0.0513)	(0.0617)
Constant	-0.0129	-0.0107
	(0.0107)	(0.0127)
Controls for age, race, sex, education, industry, health status, and occupation	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	62,259	40,427

Standard errors in parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database. Omitted group for race dummies is black. No Alaska Natives in analysis sample were displaced; as such, coefficients on Alaska Native interactions with different displacement measures are omitted due to collinearity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews. Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Table 30: OLS estimates, indicator for opiate use excluding cough medicines on indicators for job displacement and interactions between job displacement and indicator for blue-collar occupation

	(1) Full time range	(2) Pre-2010
Ever laid off	-0.00913	-0.0142
	(0.00952)	(0.0114)
Ever experienced non-layoff displacement	-0.0139	-0.0147
	(0.00802)	(0.00992)
Worked in a blue-collar occ. during round one	0.00240	0.00478
	(0.00512)	(0.00623)
Worked in a blue-collar occ. during round one × Ever laid off	0.00211	0.000381
	(0.0153)	(0.0180)
Worked in a blue-collar occ. during round one × Ever non-layoff-displaced	-0.0113	-0.0132
	(0.0139)	(0.0166)
Constant	-0.0227	0.0113
	(0.0202)	(0.0249)
Controls for age, race, sex, education, industry, health status, and occupation	X	X
Controls for health status	X	X
Panel fixed effects	X	X
Heteroskedasticity-robust standard errors	X	X
Restricted sample	X	X
Observations	62,259	40,427

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Omitted group for occupation classification is white-collar.
I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database.
Estimates are nationally representative, as they incorporate panel-specific survey weights.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Table 31: OLS estimates, indicator for opiate use excluding cough medicines on indicators for displacement and health insurance loss-displacement interactions

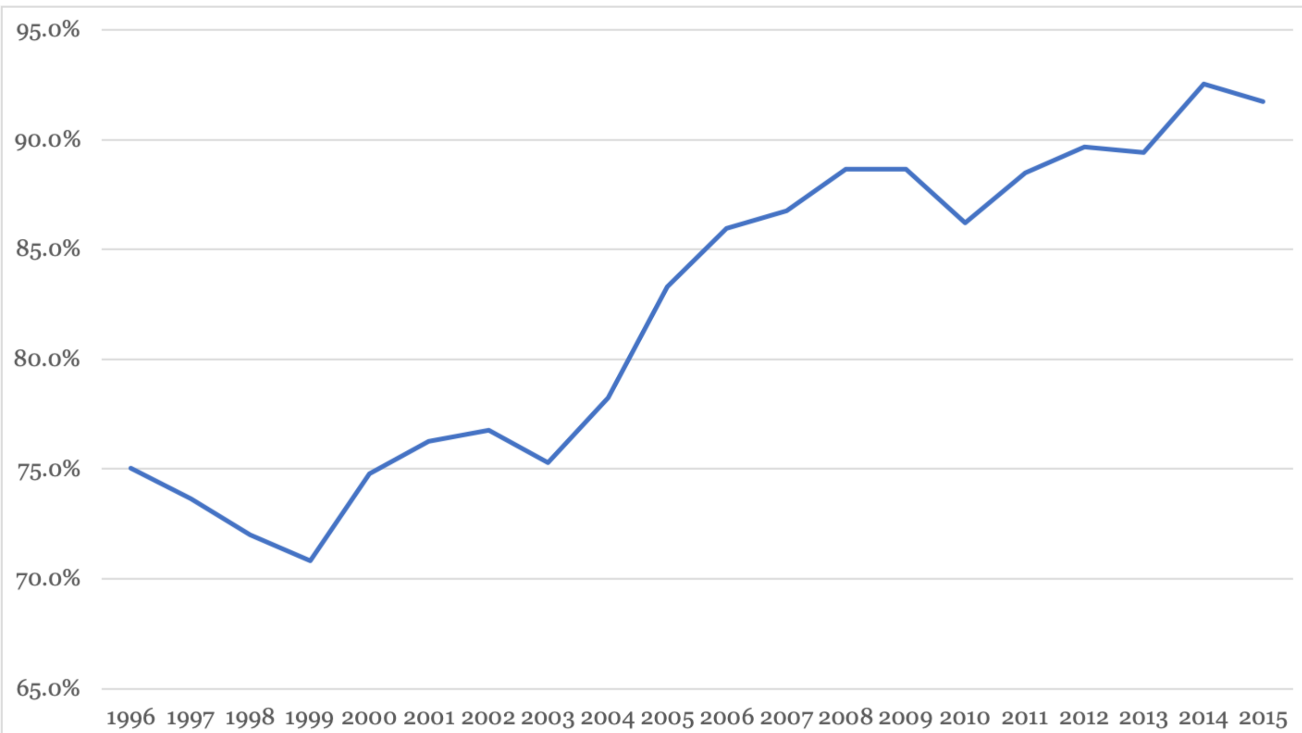
	(1) Full time range	(2) Full time range	(3) Pre-2010	(4) Pre-2010
Held employer-offered insurance in round one	0.00126	0.00128	0.00147	0.00139
	(0.00120)	(0.00120)	(0.00151)	(0.00152)
Ever laid off	-0.00869		-0.0165	
	(0.00947)		(0.0113)	
Laid off and lost health insurance	-0.000575		0.00339	
	(0.0148)		(0.0172)	
Ever experienced non-layoff displacement		-0.0157*		-0.0139
		(0.00736)		(0.00907)
Non-layoff displaced and lost health insurance		-0.00733		-0.0244
		(0.0156)		(0.0186)
Constant	-0.0250	-0.0239	0.00903	0.00991
	(0.0209)	(0.0208)	(0.0256)	(0.0256)
Controls for age, race, sex, edu., ind., and occ.	X	X	X	X
Controls for health status	X	X	X	X
Panel fixed effects	X	X	X	X
Heteroskedasticity-robust standard errors	X	X	X	X
Restricted sample	X	X	X	X
Observations	62,259	62,259	40,427	40,427

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Estimates are nationally representative, as they incorporate panel-specific survey weights.
I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database.
Analysis sample is restricted to include only (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.
Many health status variables are not available before 2000; as such, analysis for this specification excludes individuals surveyed before 2000.

Figure 13: Opiates not classified as cough medicines as a proportion of all opiate prescriptions in MEPS Prescribed Medicines Files

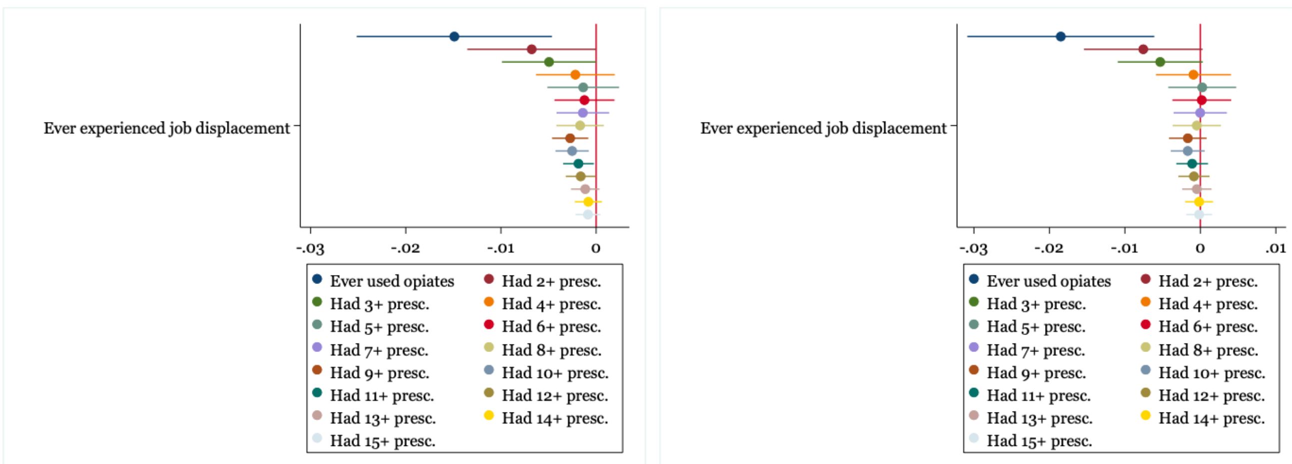


I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database.

Figure 14: Regression coefficients on indicator for job displacement in regressions of opiate use (various thresholds) excluding cough medicines on displacement

(a) Full time range

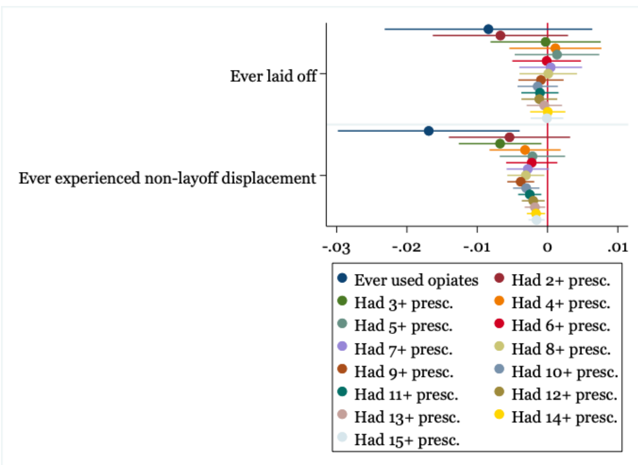
(b) Pre-2010



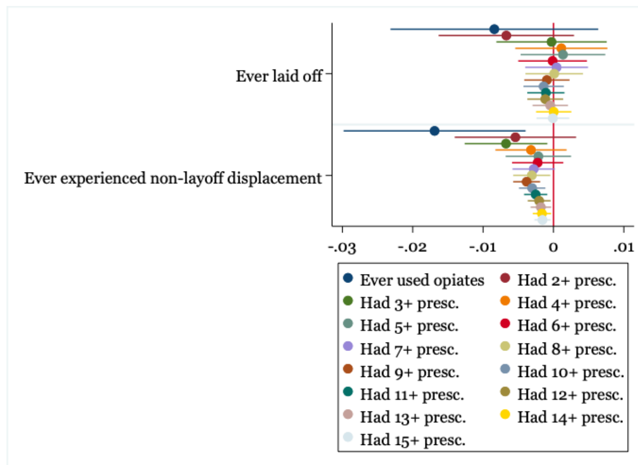
Displacement coefficients are statistically significant in the following full time range regressions: $k = 2$ ($p = 0.050$), $k = 3$ ($p = 0.051$), $k = 9$ ($p = 0.005$), $k = 10$ ($p = 0.005$), $k = 11$ ($p = 0.025$), $k = 12$ ($p = 0.044$) and in the following pre-2010 regressions: $k = 2$ ($p = 0.059$), $k = 3$ ($p = 0.064$). I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database. Regressions condition on age, race, sex, education, industry, occupation, health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

Figure 15: Regression coefficients on indicator for job displacement (dis-aggregated by displacement type) in regressions of opiate use (various thresholds) excluding cough medicines on displacement, full time range and pre-2010

(a) Full time range



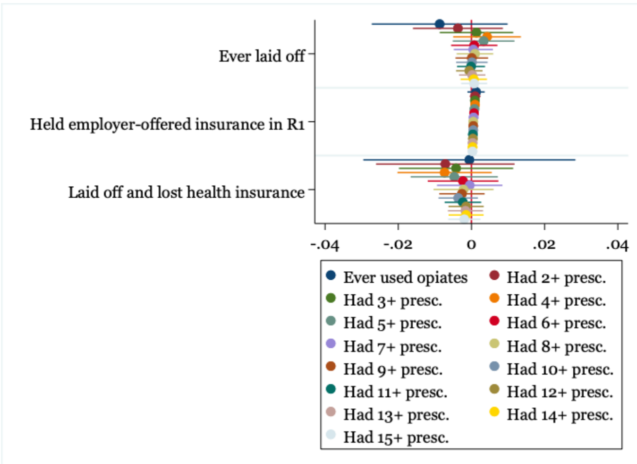
(b) Pre-2010



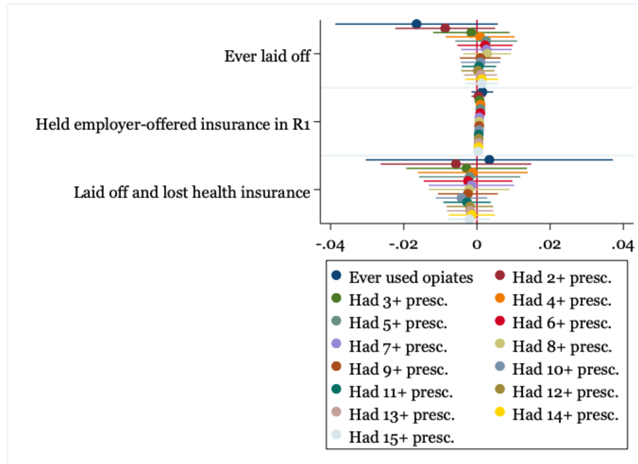
Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 3$ ($p = 0.024$), $k = 7$ ($p = 0.067$), $k = 8$ ($p = 0.022$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.002$), $k = 11$ ($p = 0.003$), $k = 12$ ($p = 0.014$), $k = 13$ ($p = 0.017$), $k = 14$ ($p = 0.014$), $k = 15$ ($p = 0.007$); and in the following pre-2010 regressions: $k = 3$ ($p = 0.090$), $k = 9$ ($p = 0.053$). No layoff coefficients are statistically significant for full time range regressions, though the layoff coefficient is significant in the $k = 2$ pre-2010 regression ($p = 0.037$). I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database. Non-layoff displacement includes job loss due to business closure and job ending (e.g. for term employment). Regressions condition on age, race, sex, education, industry, occupation, health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.

Figure 16: Regression coefficients on indicator for job displacement (dis-aggregated by displacement type) and health insurance loss-displacement interaction in regressions of opiate use excluding cough medicines (various thresholds) on displacement, full time range and pre-2010

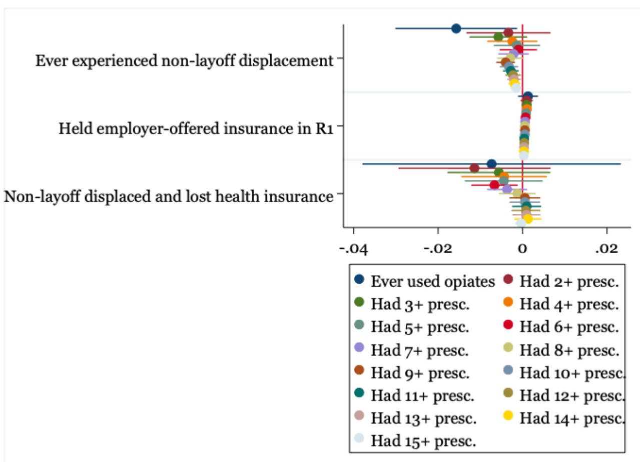
(a) Full time range, layoffs



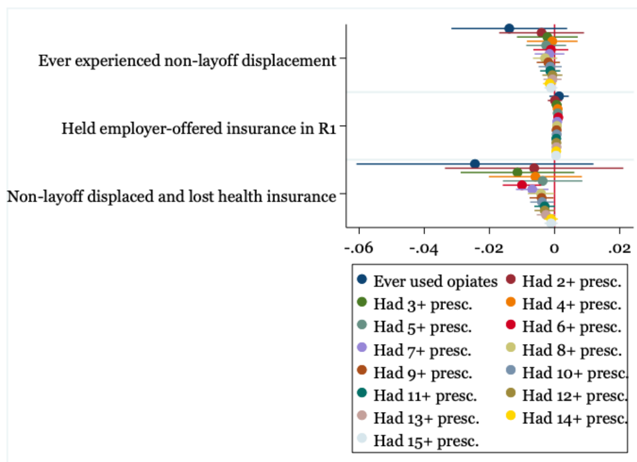
(b) Pre-2010, layoffs



(c) Full time range, non-layoff displacement



(d) Pre-2010, non-layoff displacement



All layoff coefficients and layoff-health insurance loss interaction coefficients are statistically indistinguishable from zero. Non-layoff displacement coefficients are statistically significant in the following full time range regressions: $k = 3$ ($p = 0.099$), $k = 8$ ($p = 0.69$), $k = 9$ ($p = 0.000$), $k = 10$ ($p = 0.004$), $k = 11$ ($p = 0.003$), $k = 12$ ($p = 0.016$), $k = 13$ ($p = 0.014$), $k = 14$ ($p = 0.007$), $k = 15$ ($p = 0.029$). Non-layoff displacement-health insurance loss interaction coefficients are statistically significant in the $k = 6$ regression ($p = 0.018$). All non-layoff displacement coefficients are statistically indistinguishable from zero in pre-2010 regressions. Non-layoff displacement-health insurance loss interaction coefficients are statistically significant in the following pre-2010 regressions: $k = 6$ ($p = 0.001$), $k = 7$ ($p = 0.007$), $k = 8$ ($p = 0.047$), $k = 9$ ($p = 0.035$), $k = 10$ ($p = 0.040$), $k = 11$ ($p = 0.061$), $k = 12$ ($p = 0.058$), $k = 13$ ($p = 0.070$). I classify an opiate prescription as a cough medication if the form of the prescription is "syrup" or "expectorant," if the drug name suggests that the components of the drug are only used in combination with opiates for cough medicines, or if the proprietary name associated with it matches to an opiate-infused cough syrup in the FDA Orange Book Database. Non-layoff displacement includes job loss due to business closure and job ending (e.g. for term employment). Regressions condition on age, race, sex, education, industry, occupation, health status, and panel (e.g. panel fixed effects). Furthermore, standard errors are robust to heteroskedasticity. Estimates are nationally representative, as they incorporate panel-specific survey weights. Analysis sample is restricted to include (1) prime-age individuals (2) individuals who reported working in the reference period corresponding to to the first round of MEPS interviews and (3) individuals with zero prescription opiate use in the reference period corresponding to the first round of MEPS interviews.