Opioids and the Labor Market

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Abstract: This paper finds evidence that opioid availability decreases labor force participation while a large labor market shock does not influence the share of opioid abusers. We first identify the effect of availability on participation using the geographic variation in opioid prescription rates. We use a combination of the American Community Survey (ACS) and Centers for Disease Control and Prevention (CDC) county-level prescription data to examine labor market patterns across both rural and metropolitan areas of the United States from 2007 to 2016. Individuals in areas with higher prescription rates are less likely to participate after accounting for standard demographic factors and regional controls. This relationship remains significant for important demographic groups when increasingly strong panel data controls, including a full-set of geographic fixed effects and measures of local labor market conditions in 2000, are introduced to the regressions. We also investigate the possibility of reverse causality, using the Great Recession as an instrument to identify the effect of weak labor demand on opioid abuse. The share abusing opioids did not increase after the onset of the Great Recession. The evidence on the frequency of abuse is more ambiguous since the identified increases could be the continuation of a pre-trend.

Keywords: Opioid Prescription Rate, Labor Force Participation, Great Recession, Opioid Abuse
JEL Classification Codes: I10, J22, J28, R12

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1 Introduction

In her July 2017 Senate testimony, Federal Reserve Chair Yellen stated that she thought the opioid crisis is "related to the decline in labor force participation among prime age workers" (Yellen (2017), p 22).[1] But as Yellen acknowledged in her testimony, it is challenging to determine whether the opioid crisis has causal impacts on labor markets or whether it is more a symptom of weak labor markets.

Although the amount of pain Americans reported did not increase from 1999 to 2010, the amount of legally sold opioids nearly quadrupled during this period (Ossiander (2014)), so that by 2013 enough opioid prescriptions were written for every American adult to have their own bottle of pills (CDC (2017)). In addition to the general rise in opioid prescriptions, prescription rates also vary widely across geography and physician training (Currie and Schnell (2018)).

Krueger (2017) exploits the geographic variation in opioid prescriptions to show that areas with high prescription rates have lower labor force participation rates for prime-age adults. While Krueger acknowledges that his results are preliminary and that the direction of causality is difficult to determine, these results reveal substantial patterns that warrant further examination. Harris et al. (2017) examine labor market effects of opioid prescriptions but are limited to a panel of 10 large states over the period 2010 to 2015. They find large negative effects of opioid prescriptions on participation and employment rates. Currie et al. (2018) examine the connections between prescriptions and the employment rate and find that higher numbers of opioid prescriptions are associated with a higher employment rate for women and a statistically significant increase in men's employment rate.

Alternatively, researchers have studied whether changes in the labor market could be driving the opioid crisis, motivated by the long-term decline in participation (Abraham and Kearney (2018)). Short-term fluctuations in local economic conditions have been tied to increased opioid deaths (Hollingsworth et al. (2017)), and one prominent hypothesis holds that over the long term, declining labor market prospects lead to "deaths of despair" (Case and Deaton (2015)). Deaths from suicides and drug overdoses could lead to a steepening education-mortality gradient.[2] Currie et al. (2018) also examine the effects of economic conditions on opioid prescriptions although their results are more ambiguous on this question. Ruhm (2018) examines the risk of drug deaths over time and population subgroups and finds that overdoses respond to the drug environment as characterized in terms of the availability and cost of drugs.

This paper focuses on two aspects of the relationship between opioids and the labor market. We first use the panel variation in opioid prescription rates in narrowly defined geographies across

[1] Drug overdose has become the leading cause of death for Americans under 50 years old (Katz (2017)), with the increase since 2010 due to opioids like heroin, OxyContin, and fentanyl. According to the National Institute on Drug Abuse (NIDA), "Opioids are a class of drugs that include the illegal drug heroin, synthetic opioids such as fentanyl, and pain relievers available legally by prescription, such as oxycodone (OxyContin), hydrocodone (Vicodin), codeine, morphine, and many others." Quinones (2016) provides a timeline of the crisis.

[2] Measuring the relationship between mortality and age (Auerback and Gelman (2016)) or education (Goldring et al. (2016), Bound et al. (2015)) is surprisingly difficult.

the United States to identify the effect of the legal opioid supply on labor force participation. A contribution of our analysis is improved measurement of both prescriptions and labor market status for both rural and metropolitan areas through the use of Public Use Microdata Areas (PUMAs). Over the time period we analyze, about one-third of the US population lived in an area where the specific county is not identified the American Community Surveys (ACS), generally due to not meeting a minimum population threshold. Using consistently defined geographic areas of adjoining counties, we are able to examine within-state variation in outcomes and treatments for the US population.

We find that individuals in geographic areas with higher opioid prescription rates are less likely to participate in the labor force and have lower employment rates when standard demographic factors are accounted for. To be specific, our baseline estimates associate a 4.9 percentage point reduction in labor force participation for prime-age men in a high prescription area (90th percentile) relative to those living in a low prescription area (10th percentile). Women's participation rates are also also lower in high prescription areas: There is a 1.4 percentage point difference between 90th and 10th percentile areas. This general relationship of lower participation in areas with higher historical prescription rates remains after panel data controls, including a full set of geographic fixed effects, as well as a variable controlling for local labor market conditions in 2000, a year largely predating the growth of opioid prescriptions. The measured impacts are largest for men with a high school diploma or less, where the effects are 7.4 percentage points for whites and 9.7 percentage points among non-whites. The estimated effects are large and robust across a number of alternative specifications.

Another contribution of this paper is an investigation into the role of reverse causality in generating these results, using the Great Recession (GR) as an instrument to identify the effect of weak labor demand on opioid use. The massive increase in nonemployed individuals caused by the GR, along with the relative stability of the opioid market between 2004 and 2010, provides a scenario in which the direction of causality can be clearly determined.[3] We find that the share of individuals abusing opioids did not increase due to the GR, and we show that these results are not driven by heterogeneous effects across different observed characteristics. The evidence on the frequency of abuse is more ambiguous since observed increases could be the continuation of a pre-trend.

We interpret our results as evidence that the supply of opioid prescriptions is a more important driver of the opioid crisis than economic misfortune (Ruhm (2018)). Our results on the relationship between the legal opioid supply and individual-level labor force participation outcomes contribute to the stock and nuance of evidence on the effects of opioids on the labor market (Krueger (2017), Harris et al. (2017)). And while the stability of opioid abuse rates in response to the GR is not a direct test of the "deaths of despair" hypothesis, which pertains to long-term conditions, the lack of response to such a large labor market shock suggests that the main contributors to "deaths of

[3] We provide evidence on the stability of the opioid market in terms of the price of heroin, the self-reported availability of heroin, and legal prescription rates. There were important changes in the legal opioid market (Evans et al. (2018), Alpert et al. (2017)) at the very end of this time period and in the illegal opioid market just after this time period (Ciccarone (2017)).

despair" would need to be found outside the labor market.[4]

The remainder of the paper is organized as follows: Section 2 investigates how the supply of legal opioids affects labor force participation. Section 2.1 describes the individual-level data used in the analysis, and discusses our empirical specification and identification strategy, with 2.2 presenting our results. Section 3 investigates the possibility of reverse causality by studying whether weak labor demand has an effect on opioid abuse. Section 3.3 describes the individual-level data used in the analysis, with 3.4 discussing our empirical specification and presenting our results. Section 4 concludes.

2 Does Opioid Availability Affect Labor Supply?

2.1 Data on Labor Force Participation and Prescription Rates

We measure the labor market status of individuals using the Integrated Public Use Microdata Series (IPUMS-USA) 1% sample of the American Community Survey (ACS) from 2006 to 2016. In this data source, the county of the individual observations are not identified in all cases, with counties typically being non-identified due to having a population level below a threshold. In those cases the identified geographic unit is a Public Use Microdata Area (PUMA), which have population over 100,000. About one third of the US population lived in a non-identi?ed county in our sample period, shown in the purple areas in Figure 1.

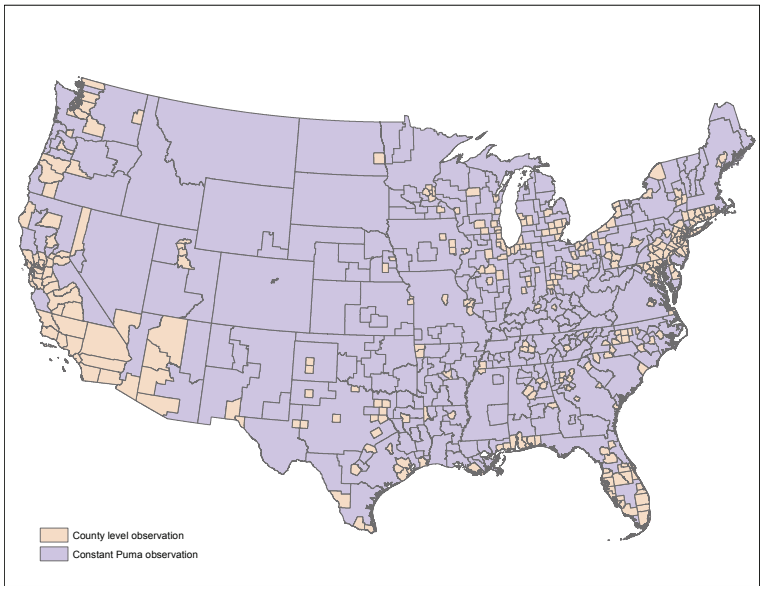
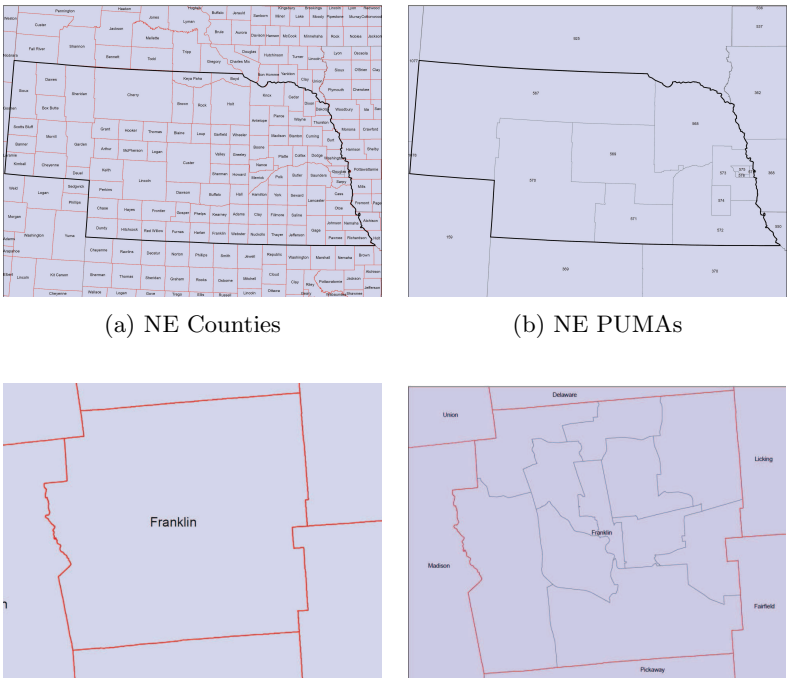


Figure 1: Geographic Areas
Note: Identified counties between 2006 and 2016 are shown in tan, and non-identified counties (aggregated into CPUMAs) are shown in purple.

[4]The distinction between short-term and long-term is important because the "deaths of despair" hypothesis has been formulated in terms of a failure of spiritual and social life in the US (Bellini (2018), 2:20), and not in terms of short-term unemployment shocks (Bellini (2018), 3:40). One potential measure of long-term trends would be wage growth (Betz and Jones (2017), Schweitzer (2017)).

PUMAs have the desirable characteristics that they generally aggregate adjoining counties that can matched to county-level data sources. Unfortunately, PUMA boundaries change during our sample period, so we actually use the IPUMS-provided consistent-PUMA (CPUMA) geography, which "is an aggregation of one or more 2010 US Census PUMAs that, in combination, align closely with a corresponding set of 2000 PUMAs" (Schroeder and Riper (2016)). In a small number of cases where CPUMA boundaries cross county lines we have to be further aggregate to identified consistent areas in both individual-level data and prescription rates.

To clarify the relationship between counties and CPUMAs Figure 2 show maps for two example using the state of Nebraska and Franklin County Ohio (home of Columbus and the most populous county in Ohio). In Nebraska, sets of adjoining counties are aggregated into 7 CPUMAs while two counties are directly identified. Whereas Franklin County includes several CPUMAs, we use the county as our unit of observation because prescription data is only available down to the county level.



(a) NE Counties (b) NE PUMAs

(c) Franklin County (d) Franklin County PUMAs

Figure 2: Nebraska and Franklin County, Ohio

We use the Centers for Disease Control and Prevention's (CDC's) annual county-level data on prescription rates from 2006 to 2016 to assign prescription rates to our geographic areas. The population-weighted average of counties is used when the geographic area is a CPUMA formed by adding multiple counties together. The CDC notes that prescription opioid dataset is "based on a sample of approximately 59,000 retail (non-hospital) pharmacies, which dispense nearly 88% of all retail prescriptions in the US" and "covers 87% of all counties." According to the CDC, a prescription is considered "an initial or retail prescription dispensed at a retail pharmacy in the

sample, and paid for by commercial insurance, Medicaid, Medicare, or cash or its equivalent." In cases where prescription data in a county is not available, which are all smaller counties, the CPUMA is assigned the average prescription rate of the observed counties within the CPUMA. Figure 3 shows these data for 2010.

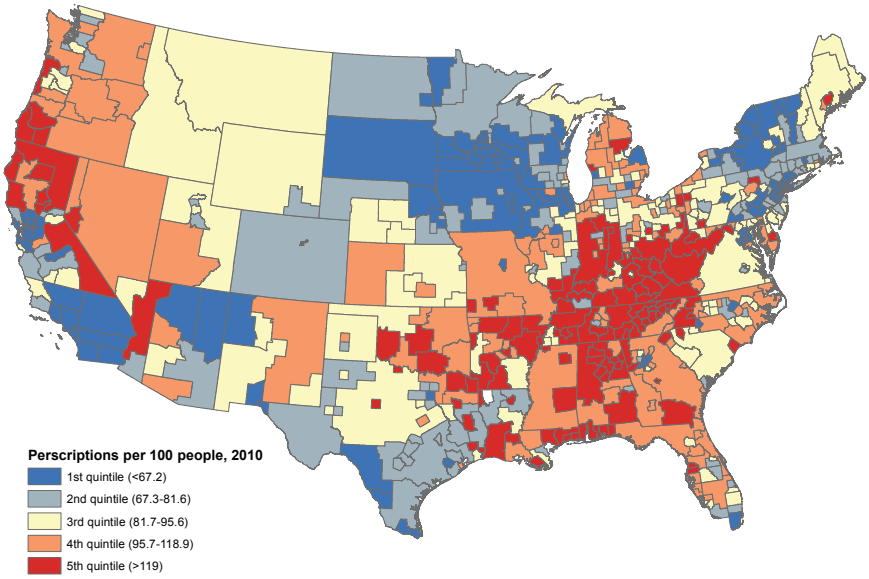


Figure 3: Prescription Rates by Geographic Areas

Ideally, one would also want information on the strength and duration of each prescription, however, county-level data on the total milligrams of morphine equivalent (MME) prescribed, rather than the number of prescriptions, is publicly available only for the year 2015. The correlation coefficient between a county's number of prescriptions per person and their MME prescribed is 0.91 in 2015. Further reassuring us about the appropriateness of county-level prescription counts, the time pattern of national MME quantities are very similar to the time pattern of our average prescription counts between 2006 and 2016 (FDA (2018)).

2.2 Model and Empirical Specification

Our approach to measuring the labor market effects of prescription opioids follows Krueger (2017), but our data will allow annual frequency regressions on a broader set of geographic units. Given the complexity of possible causal relationships between labor market status and opioids, we begin with a Directed Acyclic Graph (DAG) to highlight the specific identification strategy that we will use. This approach will also highlight possible robustness tests. Figure 4 shows a DAG of our assumed model for this problem.

$R \equiv$ Region
$I \equiv$ Illegal Opioid Supply
$P \equiv$ Prescription Opioid Supply
$X \equiv$ Individual Characteristics
$L \equiv$ Labor Market Conditions
$O \equiv$ Opioid Abuse
$Y \equiv$ Labor Force Participation

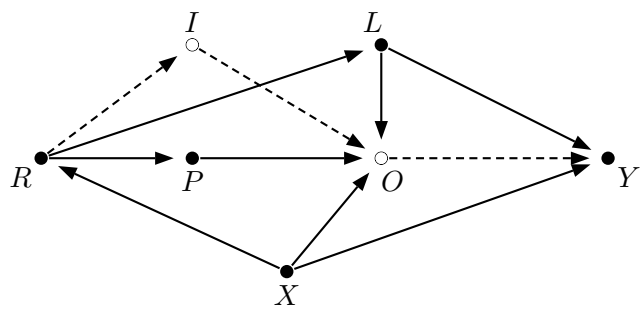


Figure 4: Directed Acyclic Graph of Opioids Affecting Labor Force Participation
Note: This Figure follows the convention from Pearl (2009) of communicating that a variable is observed by drawing a solid line to its descendants, and communicating that a variable is unobserved by drawing a dashed line to its descendants.

We model labor market outcomes such as labor force participation, Y, as a function of overall labor market conditions, L, opioid abuse, O, and individual characteristics, X, such as age, education, race, marital status, and gender, so that $Y_t = f(L_t, O_t, X)$.

We do not observe the specific relationship between opioid abuse, O, and labor force participation Y. However, opioid abuse, O, is expected to respond to the supply from both prescription, P, and illegal sources, I.[5] Observing P and Y allow this path to be identified, but I would have the potential to affect opioid supplies and therefore individuals' labor force status.[6] In addition, any factors which might alter use given the supply would make the identification imperfect.

Labor supply may also depend on economic conditions nationally and particular to the individual's region R. These factors will require controls in order for the relationship between opioid supply and labor force outcomes to be revealed. However, the fact that opioid prescriptions P are going to vary only by location means that there could be a tradeoff between the specificity of the controls for national and local economic conditions, L, and the strength of the observed relationship between P and Y. At the limit, geographic controls that flexibly vary by time period would make any relationship impossible to identify.

Following the model in our DAG and the approach of Krueger (2017), the primary equation is a linear probability model on an individual i's labor force status based on a combination of their individual characteristics, time effects indexed by t, and spatial effects indexed by j, along with the average number of opioid prescriptions in their geographic area in the prior year:

$$Y_{ijt} = \alpha P_{jt-1} + X_i'\beta + L_j'\gamma + \delta_t + \epsilon_{it}. \tag{1}$$

The linear probability model summarizes the individual responses into area labor market averages with demographic controls. The results are reliably away from zero and one, making a linear probability model a reasonable approach. The lagged prescription rate serves to keep the timing focused on impacts of opioids on labor market outcomes, although the strong correlation between prescriptions from one year to the next makes treating the relationship as causal problematic.

[5] See Appendix A for a simple model motivating this expectation.
[6] Some of the influence of the diversion of legal prescriptions to the illegal market will be captured through P.

While we focus on prime-age labor force participation, we also consider employment and unemployment probabilities. We include age and age squared, education level dummy variables, race dummy variables, and marital status for individual characteristics and run all regressions separately for men and women, given prior evidence of differential impacts. Our baseline approach to the geographic patterns is a set of census division dummies and the manufacturing employment share in the geographic area. These controls should pick up some of the underlying variation in labor market status that is not explained by variation in individual characteristics. Finally, we include a time dummy for each year, so that important national events like the Great Recession are absorbed.

Krueger (2017) runs a regression based on two periods of 3-year pooled Current Population Survey (CPS) data (1999-2001 and 2014-2016) and county-level data from 2015 on opioid prescription rates converted to MME. The regression in Krueger (2017) is run on a set of largely metropolitan counties that are identified in the CPS and state-level averages for the non-identified counties.

While our specification conceptually parallels Krueger (2017), instead of a largely cross-sectional regression, we have annual, county-level data on prescriptions from the CDC spanning the period from 2006 to 2016. In addition, we are able to break many rural areas into sets of adjoining counties within states in the ACS. These features enable us to run panel regressions on individuals' labor force status from 2007 to 2016 with CDC data on average prescriptions per person in 648 geographic units, composed of identified counties and CPUMAs.

Given the importance of some rural areas in the opioid crisis and substantial variation over time and location in prescription rates, we believe that our approach should yield better estimates of the effects of opioid prescription rates on labor force outcomes across the nation. However, drawing from the ACS weakens the link to published labor force statistics that are drawn from the CPS, and our prescription data are less specific about the effective quantities of opioids prescribed, as noted in Section 2.1.

2.3 Estimation Results

Prime-age individuals (ages 24 to 54) can be sorted into three distinct labor market statuses: out of the labor force, employed, or unemployed. Running population-weighted linear probability models on states produces estimates of the labor force participation rate, the employment-to-population ratio, and the unemployment rate for areas and the marginal impacts of the regressors on these rates. Table 1 shows the results of these regressions for prime-age men and women. In the case of both prime-age men and women, the number of opioid prescriptions in their geographic area in the prior year is associated with a lower probability of labor force participation and a lower employment rate with a high level of statistical significance. Because the underlying prescriptions data are available only for the identified geography, we use robust standard errors, which are clustered by geographic areas. Consistent with the anecdotal evidence and Krueger (2017)'s estimates, the effects are substantially larger among men than among women (-0.046 versus -0.014 for labor force participation). For the fraction of the population that is unemployed, the coefficients on the lagged

opioid prescription rate are an order of magnitude smaller and not statistically significantly affected for prime-age women. The results for the employment-to-population ratio and labor force participation are quite similar to each other for both men and women, which implies that the primary effect that opioid prescription levels have appears to be on the individual's decision to participate in the labor market. Recognizing this pattern, we focus our attention on the participation rate going forward.

Table 1: Labor Market States of Prime Age Men and Women

	Men Participate	Men Emp/Pop	Men Unem/Pop	Women Participate	Women Emp/Pop	Women Unem/Pop
Lagged Prescrip.	-0.046***	-0.049***	0.004*	-0.014**	-0.015**	0.001
	(0.006)	(0.007)	(0.002)	(0.005)	(0.005)	(0.001)
Age	0.071***	0.092***	-0.021*	0.082***	0.085***	-0.003
	(0.013)	(0.015)	(0.008)	(0.020)	(0.020)	(0.007)
Age^2	-0.293***	-0.357***	0.064	-0.402***	-0.406***	0.004
	(0.048)	(0.055)	(0.033)	(0.076)	(0.077)	(0.028)
Age^3	0.053***	0.062***	-0.009	0.084***	0.084***	-0.000
	(0.008)	(0.009)	(0.006)	(0.013)	(0.013)	(0.005)
Age^4	-0.004***	-0.004***	0.000	-0.006***	-0.006***	-0.000
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)
Less than HS	-0.200***	-0.250***	0.051***	-0.325***	-0.369***	0.044***
	(0.006)	(0.008)	(0.002)	(0.004)	(0.005)	(0.001)
High School	-0.087***	-0.126***	0.038***	-0.134***	-0.166***	0.031***
	(0.001)	(0.002)	(0.001)	(0.003)	(0.003)	(0.001)
Some College	-0.040***	-0.059***	0.020***	-0.056***	-0.075***	0.019***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
White	0.019***	0.022***	-0.003**	0.025***	0.031***	-0.007***
	(0.002)	(0.003)	(0.001)	(0.003)	(0.004)	(0.001)
Black	-0.068***	-0.098***	0.029***	0.048***	0.022***	0.026***
	(0.003)	(0.004)	(0.002)	(0.005)	(0.006)	(0.002)
Hispanic	0.053***	0.061***	-0.008***	0.012***	0.008*	0.004***
	(0.004)	(0.005)	(0.001)	(0.003)	(0.003)	(0.001)
Married	0.116***	0.153***	-0.037***	-0.080***	-0.056***	-0.023***
	(0.002)	(0.002)	(0.000)	(0.003)	(0.003)	(0.000)
Manufact Share	0.225***	0.268***	-0.044*	0.154***	0.149**	0.005
	(0.050)	(0.055)	(0.018)	(0.043)	(0.049)	(0.014)
constant	0.436**	0.111	0.325***	0.336	0.229	0.106
	(0.133)	(0.152)	(0.079)	(0.195)	(0.197)	(0.068)
R^2	0.09	0.11	0.02	0.06	0.06	0.02
N	5,835,200	5,835,200	5,835,200	6,021,178	6,021,178	6,021,178

All regressions include year and Census division fixed effects.
Robust standard errors with clustering on geographic units.

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

The individual controls are generally statistically significant and important individual determinants of labor force status that could vary across geographic areas in important ways. Manufacturing share is a reliable predictor of higher likelihood of participation, but declines in the manufacturing share would reduce this effect similar to the results in Charles et al. (2018). While not shown, the time dummies and the census division fixed effects are also generally statistically significant. In the case of the participation rate, the time fixed effects reflect both the effects of the recession and longer-term trend in participation rates. This treatment, while appropriate, absorbs most of the aggregate decline in working-age participation.

Evaluating the scale of these coefficients on lagged prescription rates depends on the level of variation that we see in prescription rates. As the first panel of Figure 5 shows, the difference between the 10th and 90th percentile prescription rates is roughly 1 log point for most of the period from 2006 to 2016. In contrast, the time variation in the median is from 4.28 athe beginning of the period to a peak of 4.41 and back down to 4.19 in 2016. Given the widely varying prescriptions rate by geography, these estimates suggest that opioids reduced participation rates by roughly 4.6 percentage points for prime-age men in high prescription rate geographies relative to geographies with very low prescription rates. These are clearly large estimates even when compared to the overall variation in labor force participation rates across geographic areas. The implications for women are about a third the size of those for male populations, but a reduction in participation rates of 1.4 percentage points for those in high-prescription rate areas relative to low-prescription rate areas is still economically important to communities.

These results are similar to Krueger (2017) in the sign and in the pattern of generally stronger effects for men than for women. Given Krueger (2017)'s strategy of estimating over two 3-year periods, the most relevant comparison of his results to ours would be the sum of the "Log Opioids per Capita" and "Log Opioids x Period 2" coefficients. Using Krueger (2017)'s column 6 regressions, which are most similar to our regressions, his results indicate a somewhat smaller of log-point increase MME of about -0.02 for prime-age men and -0.004 for prime-age women. This latter result (for women) combines a positive impact on labor force participation in the early period with a -0.014 effect of log opioids in the second period. Overall, our results look similar to Krueger (2017)'s but with larger estimated effects.

Our results are also generally consistent with Harris et al. (2017) in that the effects are negative and substantial for participation and employment rates, while positive but small for unemployment rates. The sizes of Harris et al. (2017)'s effects are larger than our estimates at -0.057 for labor force participation and -0.064 for employment rates, with the inclusion of county-level fixed effects. There are a number of possible sources for the different results: Harris et al. (2017) use a county-level panel, so the regressions are less flexible in accounting for demographic characteristics, the regressions use contemporaneous opioid prescriptions, and the sample covers 10 states from 2010 to 2015.

Our results are not consistent with Currie et al. (2018). In their county-level panel they find *positive* effects of county-level opioid prescription rates on the employment-to-population ratio, for

both men and women and for both the 18-44 and 45-64 age groups. They interpret those results as indicating that opioids facilitate returning to or continuing to work. This is the opposite sign of our results for the employment-to-population ratio for both prime-age men and women as shown in Table 1. Currie et al. (2018) obtain this positive results in regressions both with and without county fixed effects, so it does not seem to be a product of the level of regional controls, which we find to be important to our results. In addition, while not shown, our results for more narrowly defined groups are very similar to our main results, although the effects tend to smaller in younger groups. We do not have access to Currie et al. (2018)'s instrument (opioid prescription rates for older individuals), but the sign of the coefficients in Currie et al. (2018) are not impacted by the use of the IV approach. At this point we are unclear what leads to this difference. There are still other differences in Currie et al. (2018)'s analysis including: different data sources, very limited demographic controls, the use of quarterly data, the use of a one period lag on opioid prescription rates, and the aggregation of all counties below 100,000 population into state aggregates. We intend to further investigate differences in the approaches in subsequent work.

Given the number of observations available in the ACS in each of the geographic units, it is possible to explore the effects of opioid prescription rates on more narrowly defined subsamples of the population. Given the influential results in Case and Deaton (2015) and Case and Deaton (2017), we explore effects by education level and race. For our purposes we split the sample into non-Hispanic whites (white) and minorities including hispanics (nonwhite). Table 2 shows results for prime age men by race (white and nonwhite) and education level (high school graduation and lower versus some college or higher). The coefficients on lagged log prescriptions rates continue to be significantly negative for men, although there is substantial variation between education levels. The coefficient for white prime-age males with an education level of high school or less is nearly four times higher than the equivalent coefficient for white prime-age men with some college or higher education. This result shows there are quite large effects for relatively disadvantaged white men along the lines suggested in Case and Deaton (2015) and Case and Deaton (2017). It is worth emphasizing that this effect is on top of the generally lower participation rate expected for this group, which is accounted for in the other controls.

While Case and Deaton (2015)'s results focused attention on white households, our results are just as troubling for nonwhite prime-age men. The coefficient for nonwhite men with a high school degree or less is -0.097. Nonwhite men with come college or more also experience a larger likelihood of being out of the labor market in higher opioid prescription areas than their white counterparts (-0.041 versus -0.019). By our measures it is hard to argue that white prime-age men have been more affected than minorities.

Table 2: Labor Force: Prime Age Men by Race and Education

	White HS or less	White More than HS	Nonwhite HS or less	Nonwhite More than HS
Lagged Prescrip.	-0.074***	-0.019***	-0.097***	-0.041***
	(0.007)	(0.003)	(0.013)	(0.005)
R^2	0.07	0.03	0.11	0.04
N	2,053,403	2,418,539	735,239	628,019

All regressions include full set of controls with year and Census division fixed effects.
Robust standard errors with clustering on geographic units.

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

Table 3 repeats this analysis for groups of prime-age women. For white women with some college or more, there is no statistically significant coefficient on being in a higher or lower opioid prescription county. Again, nonwhite women with and without post-high school educations have statistically significantly lower participation rates in high opioid prescription areas. While most of the coefficients on lagged log prescription rates continue to be statistically significant for key demographic splits of prime-age women, the coefficients reported here are generally less than half the magnitude of the coefficients for equivalent male populations. These patterns help to fill in some of the nuances on the impacted populations that were not separately identifiable in Krueger (2017).

Table 3: Labor Force: Prime Age Women by Race and Education

	White HS or less	White More than HS	Nonwhite HS or less	Nonwhite More than HS
Lagged Prescrip.	-0.038***	0.004	-0.028**	-0.011**
	(0.009)	(0.004)	(0.008)	(0.004)
R^2	0.04	0.03	0.03	0.03
N	1,735,326	2,817,007	651,820	817,025

All regressions include full set of controls with year and Census division fixed effects.
Robust standard errors with clustering on geographic units.

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

Overall, these results suggest an important regional pattern in labor force participation that is reliably correlated with the frequencies of opioid prescriptions. The effects are also generally larger for prime-age men with lower education levels regardless of race.

2.4 Weakening Our Identifying Assumptions

The key challenge illustrated in the DAG in Figure 4 is finding appropriate geographic controls without entirely absorbing the geographic variation in the prescription data. In our preferred

specification, we chose to supplement individual controls with the manufacturing employment share in the individual's location, Census division fixed effects, and year fixed effects. The variation across locations seen in prescriptions is a critical source of variation, but there may also be other important reasons why local markets always have lower labor force participation, beyond the aggregated individual characteristics and the manufacturing share of employment. We can use more detailed geographic fixed effects to absorb these other factors, but these effects will also absorb the average differences in prescription rates. Figure 5 shows the 10th and 90th percentiles of prescription rates in the data (Panel 1) and after subtracting the mean levels of prescriptions in each geographic area (Panel 2). This reduced amount of variation is likely to cause an understatement of the implied effects of opioids, if opioid prescriptions do substantially lower participation for areas where prescriptions have been steadily higher. To explore the robustness of our results to regional controls, we examined increasing the level of controls up to the inclusion of fixed effects for all our geographic units.

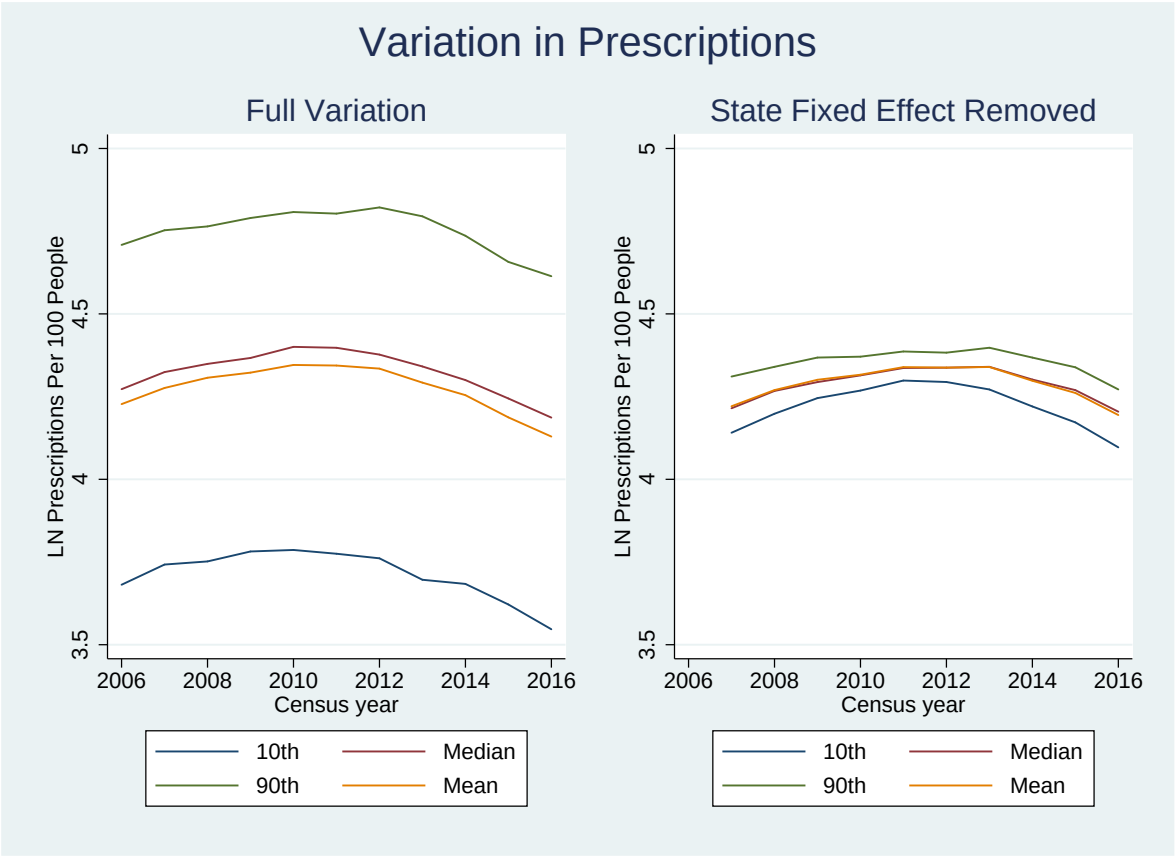


Figure 5: Identifying Variation
Note: This figure shows the variation in the natural logarithm of prescription rates across our geographic areas (defined in Section 2.1).

Our first effort to tighten the regional controls uses state fixed effects in the place of the Census divisions. Table 4 shows these results. First, it is worth briefly noting that the coefficients on the demographic factors are changed only negligibly. Being geographically determined, the coefficient

on manufacturing share in the geographic unit does fall for both men and women. The coefficients on prescription rates also are altered by the inclusion of state fixed effects. For men, the magnitude of the coefficients is essentially unchanged, going from -0.046 in the baseline regressions to -0.051, which is still statistically significant. For women, the inclusion of state fixed effects reduces the magnitude of the opioid prescription coefficient, so that the coefficient is no longer statistically significant at the 95% level. When we examine the more impacted, lower-education demographic groups, the effects continue to be statistically significant. The results for whites show slightly weaker coefficients, while the estimated coefficients for low-education nonwhites rise. Overall, we conclude that most of the results are generally robust to the inclusion of state-level regional controls.

Table 4: Prime Age Labor Force Regressions with State Fixed Effects

	Men	Women	White Men <=HS	Nonwhite Men <=HS	White Women <=HS	Nonwhite Women <=HS
Lagged Prescrip.	-0.051***	-0.008	-0.073***	-0.125***	-0.027***	-0.036***
	(0.007)	(0.004)	(0.008)	(0.016)	(0.008)	(0.009)
Age	0.071***	0.081***	-0.018	-0.091*	-0.151***	-0.154**
	(0.013)	(0.020)	(0.020)	(0.037)	(0.029)	(0.050)
Age^2	-0.293***	-0.397***	0.024	0.325*	0.508***	0.548**
	(0.048)	(0.076)	(0.077)	(0.145)	(0.114)	(0.198)
Age^3	0.053***	0.083***	0.003	-0.048	-0.068***	-0.079*
	(0.008)	(0.013)	(0.013)	(0.025)	(0.019)	(0.034)
Age^4	-0.004***	-0.006***	-0.001	0.002	0.003*	0.004
	(0.000)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Less than HS	-0.199***	-0.324***	-0.100***	-0.121***	-0.205***	-0.159***
	(0.006)	(0.004)	(0.006)	(0.007)	(0.004)	(0.003)
High School	-0.087***	-0.135***				
	(0.001)	(0.003)				
Some College	-0.039***	-0.056***				
	(0.001)	(0.002)				
White	0.020***	0.025***				
	(0.002)	(0.003)				
Black	-0.069***	0.050***		-0.100***		0.021***
	(0.003)	(0.005)		(0.004)		(0.006)
Hispanic	0.055***	0.015***		0.099***		0.013**
	(0.003)	(0.003)		(0.006)		(0.005)
Married	0.116***	-0.080***	0.155***	0.170***	-0.062***	-0.042***
	(0.002)	(0.003)	(0.002)	(0.005)	(0.004)	(0.007)
Manufact Share	0.207***	0.205***	0.308***	-0.020	0.290***	0.082
	(0.055)	(0.041)	(0.062)	(0.167)	(0.057)	(0.086)
constant	0.442**	0.290	1.376***	2.199***	2.362***	2.371***
	(0.134)	(0.191)	(0.193)	(0.359)	(0.277)	(0.459)
R^2	0.09	0.06	0.07	0.11	0.04	0.03
N	5,835,200	6,021,178	2,053,403	735,239	1,735,326	651,820

All regressions include year and Census division fixed effects.
Robust standard errors with clustering on geographic units.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

For our next step, we add participation rates based on the 2000 Census (2000 LFPR) to the regression in addition to Census divisions and the manufacturing share. The idea is to use each location's relative labor market position prior to much of the growth in opioid prescriptions as a control for the longer-term issues in regional labor markets. Of course, while far lower than today, opioid prescriptions were nonzero in 2000, but we have no source for county-level prescriptions prior to 2006. Table 5 shows these results. Not surprisingly the 2000 LFPR is a strongly significant predictor of individual participation rates. Again, adding this regressor leaves the coefficients on the demographic controls largely unchanged, but the coefficients on the manufacturing share

decline substantially and are often statistically insignificant at conventional significance levels. The coefficients on lagged prescription rates are generally reduced in their absolute values, but generally continue to be statistically significant with the notable exception of the coefficient in the regression for all prime-age women. Focusing on the regressions for less-educated men, the results continue to indicate substantial effects with moving from a low (10th percentile) to a high (90th) prescription area, resulting in implied reductions of 5.7 percentage points and 7.9 percentage points of participation among white and minority prime-age men, respectively. The results for low-education white and nonwhite women, while smaller, are in no sense trivial, with predicted effects of 2.3 percentage points and 1.8 percentage points, respectively. These results indicate that post-2006 opioid prescription patterns are an important factor in the participation rates even for areas of the United States with persistently weaker labor markets. While we would really like to have a pre-opioid prescription labor force participation rate for all communities, this result limits the potential scale of reverse causality being the primary source of the labor force participation patterns that we see in the data for 2007 to 2016.

To complete the range of regional controls, we introduce fixed effects for each county or aggregation of counties. Krueger (2017) includes this level of controls for one regression and identifies a smaller but still significant relationship between prescription rates and participation rates. Table 6 shows the results with a full set of fixed effects. Not surprisingly, the coefficients of interest are smaller, roughly a quarter to a third the size of the baseline coefficients, and less statistically significant for several groups. Notably, all prime-age men in Table 2 had a coefficient of -0.046 but are at -0.011 and statistically significant at only the 95% level with a full set of geographic fixed effects. Looking at the effects for workers with high school or lower educations, the effects were quite large (-0.074 for white and -0.097 for nonwhite prime age men) in Table 2, but these coefficients are at best a third the size (-0.022 and -0.018, respectively) and for nonwhite men with a high school degree or less and the estimate is not statistically significant at the 95% level. This indicates that an important amount of the identification of coefficients in Table 2 and 3's estimates relied on between geographic mean differences in prescription rates.

For women, the estimated results in this exercise are quite comparable to the estimates without the full set of geographic fixed effects, although the estimates are less precise and, in the case of nonwhite prime age women, no longer statistically significant at the 5% level. At least for women, the estimates do not rely on between-geographic differences. That said, the importance of persistent regional patterns in prescriptions still argues for a preferred specification that does not control for regional differences quite as flexibly.

Table 5: Prime Age Labor Force Regressions with 2000 Control

	Men	Women	White Men <=HS	Nonwhite Men <=HS	White Women <=HS	Nonwhite Women <=HS
Lagged Prescrip.	-0.031***	-0.006	-0.057***	-0.079***	-0.023***	-0.018**
	(0.005)	(0.004)	(0.006)	(0.013)	(0.007)	(0.006)
2000 LFPR	0.003***	0.002***	0.004***	0.006***	0.003***	0.003***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
Age	0.070***	0.081***	-0.021	-0.087*	-0.152***	-0.150**
	(0.013)	(0.020)	(0.020)	(0.036)	(0.029)	(0.049)
Age^2	-0.290***	-0.400***	0.036	0.309*	0.510***	0.532**
	(0.048)	(0.076)	(0.077)	(0.143)	(0.113)	(0.197)
Age^3	0.053***	0.084***	0.001	-0.046	-0.068***	-0.076*
	(0.008)	(0.013)	(0.013)	(0.025)	(0.019)	(0.034)
Age^4	-0.004***	-0.006***	-0.001	0.002	0.003*	0.004
	(0.000)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Less than HS	-0.194***	-0.322***	-0.099***	-0.120***	-0.206***	-0.159***
	(0.006)	(0.004)	(0.006)	(0.007)	(0.004)	(0.003)
High School	-0.082***	-0.132***				
	(0.001)	(0.003)				
Some College	-0.037***	-0.054***				
	(0.001)	(0.002)				
White	0.020***	0.025***				
	(0.002)	(0.003)				
Black	-0.067***	0.049***		-0.094***		0.023***
	(0.003)	(0.005)		(0.005)		(0.006)
Hispanic	0.056***	0.015***		0.098***		0.014**
	(0.003)	(0.003)		(0.006)		(0.005)
Married	0.116***	-0.080***	0.155***	0.168***	-0.063***	-0.043***
	(0.002)	(0.003)	(0.002)	(0.005)	(0.004)	(0.007)
Manufact Share	0.085	0.081*	0.103*	-0.083	0.099	-0.152
	(0.051)	(0.041)	(0.052)	(0.161)	(0.051)	(0.088)
constant	0.172	0.193	1.102***	1.603***	2.188***	2.105***
	(0.131)	(0.192)	(0.191)	(0.366)	(0.273)	(0.457)
R^2	0.09	0.06	0.07	0.12	0.04	0.03
N	5,835,200	6,021,178	2,053,403	735,239	1,735,326	651,820

All regressions include year and Census division fixed effects.
Robust standard errors with clustering on geographic units.

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

Table 6: Prime Age Labor Force Regressions with a Full Set of Geographic Fixed Effects

	Men	Women	White Men <=HS	Nonwhite Men <=HS	White Women <=HS	Nonwhite Women <=HS
Lagged Prescrip.	-0.011*	-0.015**	-0.022**	-0.018	-0.029***	-0.023
	(0.004)	(0.005)	(0.007)	(0.013)	(0.009)	(0.014)
Age	0.068***	0.079***	-0.020	-0.083*	-0.156***	-0.146**
	(0.013)	(0.020)	(0.020)	(0.036)	(0.029)	(0.049)
Age^2	-0.284***	-0.391***	0.035	0.296*	0.528***	0.513**
	(0.048)	(0.076)	(0.077)	(0.144)	(0.113)	(0.196)
Age^3	0.052***	0.082***	0.001	-0.044	-0.071***	-0.073*
	(0.008)	(0.013)	(0.013)	(0.025)	(0.019)	(0.034)
Age^4	-0.004***	-0.006***	-0.001	0.002	0.003**	0.003
	(0.000)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Less than HS	-0.191***	-0.322***	-0.100***	-0.116***	-0.202***	-0.158***
	(0.006)	(0.004)	(0.005)	(0.007)	(0.004)	(0.003)
High School	-0.079***	-0.133***				
	(0.001)	(0.002)				
Some College	-0.035***	-0.055***				
	(0.001)	(0.002)				
White	0.024***	0.024***				
	(0.002)	(0.003)				
Black	-0.069***	0.051***		-0.098***		0.024***
	(0.003)	(0.004)		(0.004)		(0.006)
Hispanic	0.056***	0.017***		0.092***		0.013**
	(0.003)	(0.003)		(0.005)		(0.005)
Married	0.116***	-0.080***	0.155***	0.162***	-0.062***	-0.045***
	(0.002)	(0.003)	(0.002)	(0.005)	(0.004)	(0.007)
Manufact Share	0.190***	0.233***	0.216***	0.465***	0.304***	0.466***
	(0.033)	(0.039)	(0.050)	(0.120)	(0.064)	(0.141)
constant	0.288*	0.327	1.192***	1.644***	2.408***	2.225***
	(0.126)	(0.189)	(0.184)	(0.338)	(0.276)	(0.453)
R^2	0.10	0.06	0.08	0.15	0.05	0.04
N	5,835,200	6,021,178	2,053,403	735,239	1735326	651,820

All regressions include year and Census division fixed effects.
Robust standard errors with clustering on geographic units.

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

2.5 Investigating Our Identifying Variation

Our analysis incorporates two features not included in prior work: (1) the broader regional patterns possible with the use of CPUMAs for non-identified counties, and (2) prescription data from 2006 to 2016. These enhancements are one of the reasons our results differ from some prior

work, but they also open up questions. Notably, are our results the product of including more rural counties in the analysis? As well, Evans et al. (2018) and Alpert et al. (2017) indicate that opioid prescription abuse patterns shift after the 2010 reformulation of OxyContin, suggesting that our results might over- or understate effects after 2010. To investigate the role of our enhancements, we ran regressions with interactions on the lagged prescription coefficient to account for differences in the data which are measured at the CPUMA-level or after 2010. The regressions maintain the coefficients on other controls tobe equivalent as in the prior tables in order to highlight the particular response of the prescription coefficients to these two innovations.

The results for the interaction of prescriptions with an indicator for whether the data are only identified at the county or CPUMA level are shown in Table 7. The primary coefficients are all still statistically significant although in several cases a bit smaller than was seen in Tables 1 to 3. In all cases the interaction is negative and most cases statistically significant (except in the all prime-age women regression). This indicates that patterns in these low-population regions are on average worse than in the high-population identified counties, but not in a way that causes the results to be dependent on including these counties.

Table 7: Prime Age Labor Force Regressions with CPUMA Interaction

	Men	Women	White Men <=HS	Nonwhite Men <=HS	White Women <=HS	Nonwhite Women <=HS
Lagged Prescrip.	-0.035***	-0.013**	-0.057***	-0.066***	-0.031***	-0.022*
CPUMA*Prescrip	-0.006***	-0.000	-0.007***	-0.017***	-0.003**	-0.004**
R^2	0.09	0.06	0.07	0.12	0.04	0.03
N	5,835,200	6,021,178	2,053,403	735,239	1735326	651,820

All regressions include full set of controls with year and Census division fixed effects.
Robust standard errors with clustering on geographic units.

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

The interaction with prescription rates after 2010 is show in Table 8. Based on the results in Evans et al. (2018) and Alpert et al. (2017) we might expect that prescriptions became a less important indicator after the 2010 reformulation of OxyContin because users appear in their results to substitute to the illegal market. That is not what we find when we include the post-2010 interaction. The results are stronger after 2010. The base response to prescriptions is negative and generally statistically significant, but the interaction uniformly increases the scale of the effects after 2010. We have no means to explore the patterns of illegal usage after 2010, but these results could be consistent with the spatial distribution of the illegal supplies of opioids roughly approximating the distribution of post-2010 prescription rates.

Table 8: Prime Age Labor Force Regressions with Post-2010 Interaction

	Men	Women	White Men <=HS	Nonwhite Men <=HS	White Women <=HS	Nonwhite Women <=HS
Lagged Prescrip.	-0.036***	-0.006	-0.061***	-0.082***	-0.024*	-0.021*
Post*Prescrip	-0.015***	-0.013***	-0.021***	-0.026***	-0.023***	-0.012**
R^2	0.09	0.06	0.07	0.11	0.04	0.03
N	5,835,200	6,021,178	2,053,403	735,239	1,735,326	651,820

All regressions include year and Census division fixed effects.
Robust standard errors with clustering on geographic units.

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

3 Reverse Causation: Does the Labor Market Drive Opioid Abuse?

3.1 Model

A reasonable hypothesis is that opioid abuse increases due to poor labor market conditions, and this reverse causation is what drives the correlation between opioid prescription rates and labor market outcomes (Case and Deaton (2015)). This would recast the DAG in Figure 4 as the DAG below in Figure 6: A labor market shock like the Great Recession (GR) could change overall labor market conditions (L) and an individual's labor market outcomes (Y). Note that we assume prescription rates do not respond to labor market conditions.

$R \equiv$ Region
$I \equiv$ Illegal Opioid Supply
$P \equiv$ Prescription Opioid Supply
$X \equiv$ Individual Characteristics
$L \equiv$ Labor Market Conditions
$O \equiv$ Opioid Use
$Y \equiv$ Labor Force Participation
$GR \equiv$ Great Recession

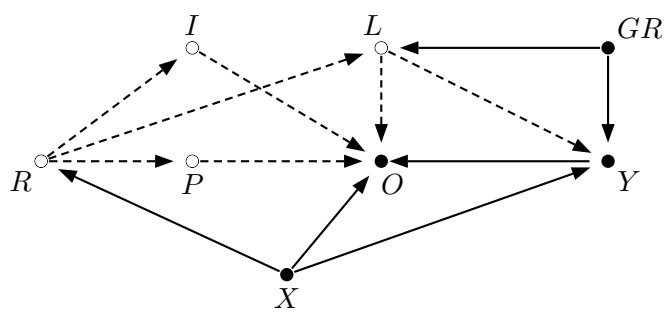
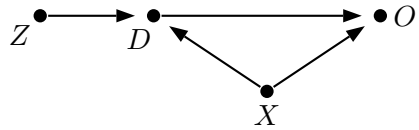


Figure 6: Directed Acyclic Graph of Opioids Affecting Labor Force Participation
Note: This figure follows the convention from Pearl (2009) of communicating that a variable is observed by drawing a solid line to its descendants, and communicating that a variable is unobserved by drawing a dashed line to its descendants.

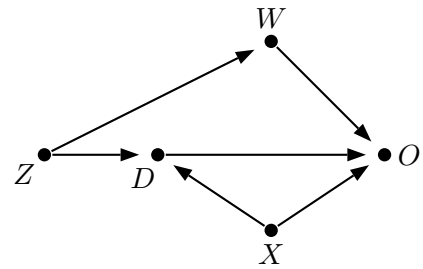
Assuming that the drug environment stayed relatively stable over the time period in question, the DAG in Figure 6 can be recast in terms of a standard model from the program evaluation literature (Heckman and Vytlacil (2005)) that can be studied with potential outcomes (Aliprantis (2015)), as shown in Figure 7 where the GR is identified as one of the time periods Z. Measuring treatment with nonemployment should help to capture the effects of overall labor market conditions rather than individual-level participation decisions.

$Z \equiv$ Instrument (Time Period)
Pre-GR v. Post-GR
(2006+2007 v. 2009+2010)
$D \equiv$ Treatment (U or NLF)
$X \equiv$ Individual Characteristics
$O \equiv$ Opioid Use
$W \equiv$ Variable Violating
Exclusion Restriction

(a) Variables



(b) The Assumed Model



(c) Another Possible Model

Figure 7: Directed Acyclic Graph of Unemployment Affecting Opioid Abuse
Note: This figure follows the convention from Pearl (2009) of communicating that a variable is observed by drawing a solid line to its descendants, and communicating that a variable is unobserved by drawing a dashed line to its descendants.

We assume that an individual's selection into treatment follows a latent index model

$$D_i = \mathbf{1}\{D_i^* > 0\} \tag{2}$$

where $D_i^* = \mu(X_i, Z_i) - V_i$ and V_i follows a standard normal distribution. The potential outcome of individual i in each treatment state $O_i(D)$ is

$$O_i(0) = \mu_0(X_i) + U_{0i}; \tag{3}$$

$$O_i(1) = \mu_1(X_i) + U_{1i}. \tag{4}$$

We divide the NSDUH into years acting as a normal labor market ($Z_i = 0$ in 2006 and 2007), a period of weak labor demand ($Z_i = 1$ in 2009 and 2010), and a "placebo" period that provides evidence of time-trends ($P_i = 0$ in 2004 and 2005 and $P_i = 1$ in 2006 and 2007). We are interested in estimating Local Average Treatment Effect (LATE) parameters

$$\triangle^{LATE}(Z) \equiv \mathbb{E}[O_i(1) - O_i(0)|D_i(1) - D_i(0) = 1] = \frac{\mathbb{E}[O_i|Z_i = 1] - \mathbb{E}[O_i|Z_i = 0]}{\mathbb{E}[D_i|Z_i = 1] - \mathbb{E}[D_i|Z_i = 0]}.$$

3.2 Identifying Assumptions

Since we are using the Great Recession as an instrument for nonemployment, the biggest threat to identification is from a variable, W, violating the exclusion restriction that the instrument only affects the outcome variable through treatment. Anything that might have changed contemporaneously with the GR that affects opioid use is a candidate W.

We present evidence that the most obvious potential W's do not violate the exclusion restriction, indicating that the basic features of the drug environment did not change between 2004 and 2010. Specifically, there were no major changes in prices, trends in prescribing rates, the self-reported ease of access to heroin, the self-reported rate of selling drugs, or impacts from new state-level laws and enforcement.

Looking first at drug prices, we see that the price of heroin, the closest illegal substitute for prescription opioids, was relatively stable between 2004 and 2010 (Figure 8).[7] Similarly in the legal market, there were no major changes in the price of oxycodone over this time period (see the Marketscan data in Figure 4 of Evans et al. (2018)).

Looking at other measures of supply, we see that there were no changes in trends in opioid prescribing rates between 2006 and 2010 (Guy et al. (2017)). In terms of illegal drugs, Table 9 shows self-reported measures from the National Survey on Drug Use and Health (to be described in the next section). The ease of access to heroin and the share of respondents who reported selling drugs did not change over the time period under investigation.

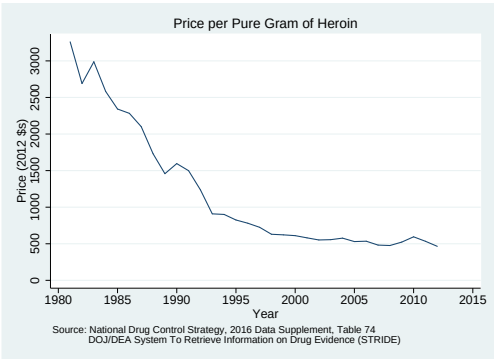


Figure 8: Average Price per Pure Gram of Heroin in the United States

Table 9: Potential Ws

Men 24-64 w HS or Less	Easy to Get Heroin (%)	Sold Drugs (%)
Mean in 2004+2005	20.2	2.2
Change in 2006+2007	0.2	0.0
	(0.6)	(0.2)
Change in 2009+2010	-0.4	0.3
	(0.6)	(0.2)

Note: The values in this table show the mean for the period 2004+2005, and then the change relative to the previous period for 2006+2007 and 2009+2010.

Finally, we might expect that there were changes in the opioid market through laws and enforcement, especially since many states adopted laws between 2004 and 2010 to reduce the abuse of opioids. Meara et al. (2016) study a national sample of disabled Medicare beneficiaries aged 21-64 years, half of whom used opioids in a given year. Examining the 81 controlled-substance laws added by states between 2006 through 2012, Meara et al. (2016) find no impact of such laws on potentially hazardous use of opioids or overdose.[8]

3.3 Data on Labor Market Outcomes and Opioid Abuse

Because there is no measure of opioid abuse in the CDC or Census data we have used thus far, we now turn to a new data set. We did not use these survey data earlier in the analysis because the survey uses nonstandard definitions when measuring labor market outcomes.

The best measurement of drug use among civilians in the United States is the National Survey on Drug Use and Health (NSDUH). The NSDUH gathers annual, individual-level data on drug use by means of in-person interviews with a large national probability sample. Every year, about 70,000

[7] These data on the average price of heroin in the US come from the Drug Enforcement Administration's System To Retrieve Information on Drug Evidence (STRIDE) program, and are reported in Table 74 of ONDCP (2016).

[8] Prescription-drug monitoring programs (PDMPs) are an example of such laws.

people from the US civilian, noninstitutionalized population age 12 and older are interviewed. The surveys are conducted by the US Department of Health and Human Services (HHS) and use computer-assisted methods to provide respondents with a private and confidential means of responding to questions. Respondents are given $30 for participating in the NSDUH.

In addition to variables covering drug use in great detail, the NSDUH has the additional strength for our purposes of having information on demographic characteristics and labor market outcomes such as labor force participation, employment, and hours worked. The observed characteristics (X_i) we include in the analysis are indicators for GED status, high school graduation, military participation, current school enrollment, and having children in the household, as well as four discrete levels of health status, several age levels, race, and marital status. The NSDUH measures unemployment for both the week and the year before the respondent took the survey, but only measures labor force participation for the week before the respondent took the survey. Thus we measure treatment for the week before the survey. We measure treatment as nonemployment, either being unemployed or not in the labor force, since there was a large response to the GR in terms of labor force participation.

We measure outcomes as the non-medical use of prescription pain relievers, which we refer to interchangeably as opioid abuse.[9] We refer to the entire class of pain killers (analgesics) in the NSDUH survey as opioids, even though a very small share of such pain killers are non-opioids.[10] We investigate use over the past year, both in terms of any use and the number of days used.[11]

3.4 Estimation Results

Table 10 shows that the LATE of nonemployment on ever abusing opioids is zero for prime-age men. We consider these null effects to be economically significant (Abadie (2018)): If labor market outcomes drive opioid abuse, it is surprising that a shock as massive as the GR did not increase opioid abuse. Even if this mechanism operates over a longer time horizon than the one studied here, one would still expect to find some short-term effects. Table 10 does, however, also give some evidence that nonemployment increased the intensity of opioid use among those prime-age men who were using before the labor market shock.

[9] "Non-medical" means the use of a drug that was not prescribed for the respondent or that was taken only for the experience or feeling it caused. Respondents are explicitly told that this use does not refer to "over-the-counter" drugs.

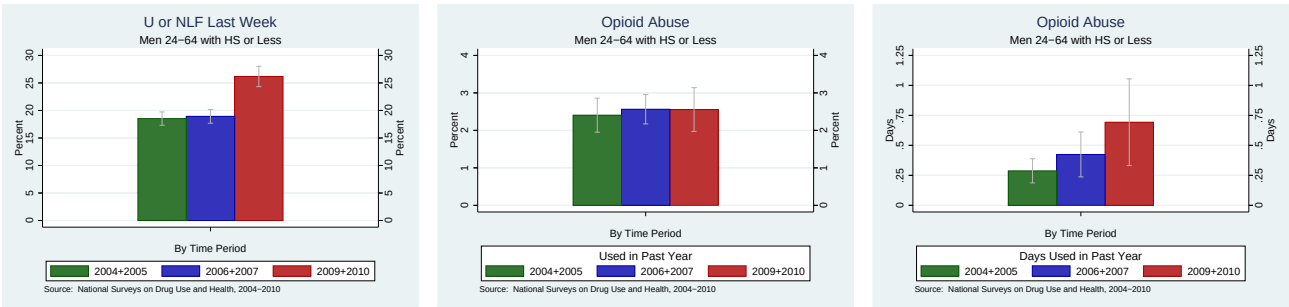
[10] See footnote 11 in Carpenter et al. (2017).

[11] The NSDUH asks respondents about drug use in the past year and the past month, and the previous literature has shown that estimates can be sensitive to the reference period used (Carpenter et al. (2017)). Our use of annual variables is motivated by results in the literature on the intergenerational elasticity (IGE) in earnings showing how transitory fluctuations can attenuate results when outcomes are measured over a short time horizon (Mazumder (2005)).

Table 10: LATEs of Nonemployment on Opioid Abuse

	Ever Abused in Last Year (%)			Days Abused in Last Year		
			1st Stage			1st Stage
	$\triangle^{LATE}(Z)$	P-Value	F-Stat	$\triangle^{LATE}(Z)$	P-Value	F-Stat
All Adults 18+	–4.6	0.04	226	1.4	0.09	226
	(2.2)			(0.8)		
Men 24-64	0.4	0.90	181	2.2	0.15	181
	(3.3)			(1.5)		
Men 24-64	–0.1	0.98	142	3.7	0.07	142
w HS or Less	(3.2)			(2.1)		

Figures 9a-9c decomposes these results graphically. We see the very strong first stage in Figure 9a, where nonemployment increased by 7 percentage points for the sample of prime-age men with low educational attainment. Given the magnitude of this first stage, the results in Figure 9b may be surprising, as they show there was no change in the share of people abusing opioids over this same time period. Figure 9c indicates that the intensity of opioid use increased over time, but that this increase could be the result of a trend pre-dating the GR.



(a) $E(D|Z)$ (b) $E(O|Z)$ (c) $E(O|Z)$

Figure 9: Nonemployment and Opioid Abuse
Note: $D_i \in \{0,1\}$ is unemployed or not in the labor force in the last week. In (b) $O_i \in \{0,1\}$ is ever used a pain reliever non-medically in the last year, and in (c) $O_i \in \{0,1,\ldots,365\}$ is days used a pain reliever non-medically in the last year. Men 24-64 with high school or less.

3.4.1 Heterogeneous Effects

Since the previous literature has focused on opioid use among prime-age men with low educational attainment, we focus on the effects of nonemployment for males aged 24-64 with a high school degree or less who are not currently enrolled in college. We examine these effects in greater depth with Local Average Treatment Effect (LATE) parameters

$$\triangle^{LATE}(X_i, Z) \equiv \mathbb{E}[O_i(1) - O_i(0)|X_i, D_i(1) - D_i(0) = 1] = \frac{\mathbb{E}[O_i|X_i, Z_i = 1] - \mathbb{E}[O_i|X_i, Z_i = 0]}{\mathbb{E}[D_i|X_i, Z_i = 1] - \mathbb{E}[D_i|X_i, Z_i = 0]}$$

that account for covariates X_i by applying the Wald estimator to principal strata (terciles) of the predicted probability of treatment estimated on those with $Z_i = 0$, or terciles of $\widehat{\mu}(X_i, 0)$.

We estimate $\mu(X_i, 0)$ using a linear probit model with $\mu(X_i, 0) = X_i'\beta$ on data from the 2006 and 2007 waves of the NSDUH, and use the estimated coefficients to predict, for each wave of the NSDUH, an individual's probability of treatment during a normal labor market. To increase power and allow for the estimation of LATEs, we discretize estimated values $\widehat{\mu}(X_i, 0)$ into terciles of $\widehat{\mu}(X_i, 0)$, with the lowest tercile being least likely to be nonemployed, and the highest tercile being most likely to be nonemployed.

Table 11 presents conditional LATEs. While there is some evidence that the extensive and intensive margins increased for the group most likely to be nonemployed (the third tercile), it is difficult to distinguish these results as being distinct from sampling variation. The extensive margin is estimated to have actually decreased for the first and second terciles. And while the evidence for the intensive margin increasing for the third tercile might be more compelling, this result starts to look like noise when we look at the placebo pre-period (Figure 10c).

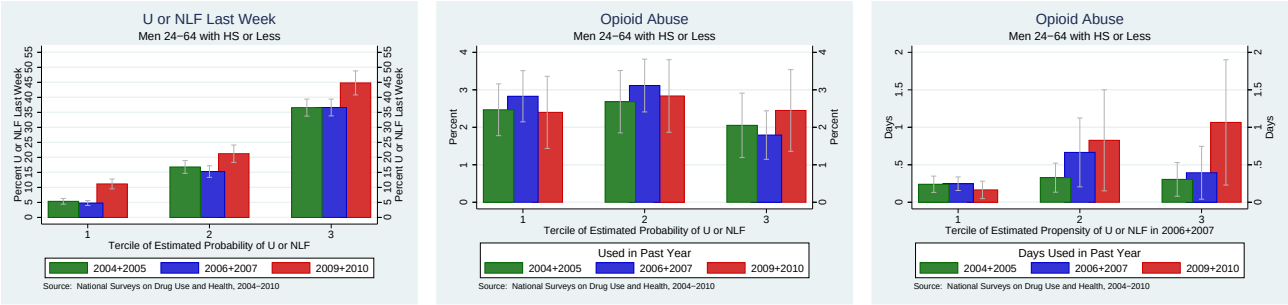
Table 11: LATEs of Nonemployment on Opioid Abuse

	Ever Abused in Last Year (%)			Days Abused in Last Year		
			1st Stage			1st Stage
	$\triangle^{LATE}(Z)$	P-Value	F-Stat	$\triangle^{LATE}(Z)$	P-Value	F-Stat
First Tercile	–6.8	0.26	98	–1.3	0.24	98
	(6.1)			(1.1)		
Second Tercile	–4.7	0.52	38	2.7	0.59	38
	(7.2)			(5.0)		
Third Tercile	7.8	0.12	37	8.0	0.07	37
	(4.9)			(4.5)		

Note: The first tercile is the tercile least likely to be nonemployed ($D = 1$), while the third tercile is the group most likely to be nonemployed.).

Looking at a graphical decomposition of these results, Figure 10a shows that there is a steep slope for the relationship between the actual probability of nonemployment with the estimated probability of nonemployment. The figure also shows that each of these groups experienced a large labor market shock due to the GR, even if they each started from much different bases.

The changes in the share of those ever abusing after the GR could be interpreted as sampling variation over the longer period shown in Figure 10b. And while the changes in days abused is most compelling for the third tercile, we see evidence of a pre-trend for the second tercile.



(a) $E(D|Z)$ (b) $E(O|X, Z)$ (c) $E(O|X, Z)$

Figure 10: Treatment

Note: $D_i \in \{0, 1\}$ is unemployed or not in the labor force in the last week. In (b) $O_i \in \{0, 1\}$ is ever used a pain reliever non-medically in the last year, and in (c) $O_i \in \{0, 1, \ldots, 365\}$ is days used a pain reliever non-medically in the last year. Men 24-64 with high school or less.

We interpret the analysis of heterogeneous effects as evidence that, aside from sampling error, the aggregate time series patterns along the extensive margin are stable across observed characteristics that predict nonemployment. We also note that although observed characteristics predict much different labor market outcomes (Figure 10a), we conclude that these characteristics are not predictive of the share of people abusing opioids (Figures 10b). We find this evidence difficult to reconcile with labor market outcomes being a primary driver of opioid abuse.

3.5 Robustness: Alternative Measures of Opioid Abuse

In the main analysis we define opioid abuse as the non-medical use of a prescription pain reliever. To determine how consequential our preferred measure is in driving our results, we repeat our analysis with alternative measures of opioid abuse.

We replicate our analysis with a measure of abuse that includes any use of prescription pain relievers, regardless of use being medical or non-medical. This measure might yield different results because it is possible that some respondents misreport non-medical use as medical use. As well, different types of opioid use could have positive or negative relationships with labor market outcomes (Savych et al. (2018)).

We also replicate the analysis measuring opioid abuse as the use of either pain relievers or heroin. This variable might be a more accurate measure of abuse if some prescription abusers substituted to heroin toward the end of our sample period. There is a relationship between non-medical use of prescription opioids and heroin use (Compton et al. (2016)), and there is evidence this relationship changed over the time period we consider. Cicero et al. (2015) find that concurrent abuse of heroin and prescription opioids increased between 2008 and 2014 in a national sample of respondents in treatment, and the reformulation of OxyContin created an inflection point in heroin deaths in the last third of 2010 (Evans et al. (2018), Alpert et al. (2017)).

Table 12 shows that the two additional measures of opioid abuse described above yield results almost identical to those using the preferred measure in the main analysis. The preferred measure

used in the main analysis, displayed in the first column, is "Non-Medical Use of Pain Relievers, Last Year (NM)." Compared to the preferred measure, any (medical or non-medical) use of pain relievers exhibits similar patterns in changes, but starts at a higher level. The measure displayed in the final column accounts for heroin use, and the inference is similar to that from the preferred measure.

Table 12: Use of Pain Relievers within the Last Year

	Non-Medical Ever Used (%)	Medical or Non-Medical Ever Used (%)	Medical or Non-Medical or Heroin Ever Used (%)
All Adults 18+			
Mean in 2004+2005	2.5	4.3	4.4
Change in 2006+2007	0.2	0.3	0.3
	(0.1)	(0.1)	(0.1)
Change in 2009+2010	-0.2	-0.0	-0.0
	(0.1)	(0.1)	(0.1)
Men 24-64 w HS or Less			
Mean in 2004+2005	2.4	5.0	5.2
Change in 2006+2007	0.2	0.7	0.6
	(0.2)	(0.3)	(0.3)
Change in 2009+2010	-0.0	0.3	0.5
	(0.2)	(0.3)	(0.3)

Note: The values in this table show the mean for the period 2004+2005 and the change relative to the previous period for 2006+2007 and 2009+2010. The preferred measure used in the main analysis, displayed in the left column, is "Non-Medical Use of Pain Relievers, Last Year (NM)."

4 Conclusion

The scale of the opioid crisis makes it likely that there would be substantial labor market impacts, but the specific nature of those relationships is important to clarify. Our analysis highlights the strong negative relationship between opioid prescription rates and labor force statuses. Taken at face value, our results suggest that solving the opioid crisis would substantially improve economic conditions in counties that have had high levels of opioid prescriptions by boosting the prime-age male participating rate by more than 4 percentage points. And these results are typically larger and more statistically reliable for demographic groups that have seen weak and declining participation, namely white and nonwhite prime-age men with a high school education or less. Of course, individuals may not smoothly return to the labor market, but it is hard to argue that we should not be trying to bring this group back into the labor market as a response to the opioid crisis.

With data currently available, it is difficult to identify any strategy that would fully remove the possibility that individuals are reacting to their circumstances with drugs rather than their circumstances developing following drug use. Nonetheless, our work using the Great Recession as a shock on the labor market to identify a response in drug usage cautions against the view that improving economic conditions will solve the drug abuse problem. In addition, controlling for labor markets in 2000, before most of the rise in opioid prescriptions, still shows substantial effects of opioid prescriptions.

Our analysis is just one part of the developing literature on the opioid crisis that should help to inform policy-makers as they attempt to rein in the crisis. Important issues that we were not able to address include the rise of illegal, synthetic opioid supplies and deaths associated with opioids. The challenges inherent in investigating the impacts of illegal opioid use on the labor market primarily rest with the paucity of data available on the illegal opioid supply. While immediate answers are wanted for the crisis, improving the data around drugs and the outcomes for individuals could help to refine policy strategies that are developed.

While many relevant policy issues are outside the scope of this paper, our work serves to show the scale of the impact of the opioid crisis on the labor market. In our view, the impact of the opioid crisis on regional labor markets looks to be large and statistically robust.

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A A Model of Opioid Use

Our theory is that individuals use opioids because they are in pain or because they are addicted. Individuals feel mental pain $\pi_m \in \{0,1\}$ or physical pain $\pi_p \in \{0,1\}$, and their overall state of pain at time t is $\pi_t = \max\{\pi_{mt}, \pi_{pt}\}$. An individual's type $\tau \in \{0,1\}$ is an indicator for whether they are susceptible to opioid addiction, and their utility from using opioids $O_t \in \{0,1\}$ is $u(\pi_t, O_t, O_{t-1}, \tau)$.[12]

Being susceptible to addiction means that $u(\pi_t, 1, 1, 1) >> u(\pi_t, 0, 1, 1)$, while not being susceptible to addiction means that $u(\pi_t, 1, 1, 0) \approx u(\pi_t, 0, 1, 0)$.[13] With this utility framework, we might model the decision to use opioids with a latent index model $O_t = \mathbb{1}\{O_t^* > 0\}$ with $O_t^* = \mu(X, \pi_t, O_{t-1}, \tau, P, I) - V$, where X represents demographic characteristics of the individual, and P represents local prescription rates, I represents illegal opioid supply, and V represents idiosyncratic factors.

Our theory for the way opioid use affects labor market outcomes is as follows: The local prescription rate an individual faces, P_{t-1}, influences the probability of previous opioid use, $Pr[O_{t-1}|P_{t-1}]$, and therefore also influences the probability of current usage $Pr[O_t|O_{t-1}]$. Labor market outcomes like labor force participation, $Y \in \{0,1\}$, are a function of overall labor market conditions L, opioid use O, and individual characteristics like age, educational attainment, and gender X, so that potential outcomes are $Y_t(L_t, O_t, X)$.

Our theory for the way labor demand affects opioid use is also based on the theory that individuals use opioids because they are in pain or because they are addicted. Individuals feel mental pain $\pi_m \in \{0,1\}$ or physical pain $\pi_p \in \{0,1\}$, and their overall state of pain is $\pi = \max\{\pi_m, \pi_p\}$. Our empirical specification would define treatment as mental pain, $D \equiv \pi_m$, and measure treatment as an individual being either unemployed or not in the labor force. We model opioid usage as potential outcomes that are a function of being jobless D, and demographic characteristics, $O_t(D_t, X)$. Note that by using time as an instrumental variable and assuming that the drug environment did not change over the time period in question, we do not need to model how opioid supply impacts potential outcomes.

[12] Within the group given chronic opioid prescriptions, Banta-Green et al. (2009) find three groups that could be cast in terms of the 8 possible groupings in the set $\{\pi_m, \pi_p, \tau\}$: (i) those with moderate mental and physical pain; (ii) those with elevated mental pain; and (iii) those with elevated mental and physical pain. Group (i) was the vast majority, and (ii) and (iii) were at elevated risk of addiction, so that the observed types in the set $\{\pi_m, \pi_p, \tau\}$ are $(1,1,0), (1,0,1), (1,1,1)$.

[13] See Volkow and McLellan (2016) for a discussion of the importance of the distinction between addiction and related aberrant behaviors. We also abstract from the time horizon of (prescribed) use, which is a critical factor for addiction (Brat et al. (2018), Fishbain et al. (2008)).