NBER WORKING PAPER SERIES

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Working Paper 24440
http://www.nber.org/papers/w24440

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 2018

We thank participants at the 2017 IZA Workshop on Health and Labor Markets for their helpful feedback. We gratefully acknowledge support from the program for U.S. Health and Health Policy at the Center for Health and Well being at Princeton University. The statements, findings, conclusions, views, and opinions contained and expressed herein are not necessarily those of QuintilesIMS, nor any of its affiliated or subsidiary entities, nor of the National Bureau of Economic Research.

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NBER Working Paper No. 24440
March 2018
JEL No. I14

ABSTRACT

This paper uses quarterly county-level data to examine the relationship between opioid prescription rates and employment-to-population ratios from 2006–2014. We first estimate models of the effect of opioid prescription rates on employment-to-population ratios, instrumenting opioid prescriptions for younger ages using opioid prescriptions to the elderly. We also estimate models of the effect of employment-to-population ratios on opioid prescription rates using a shift-share instrument. We find that the estimated effect of opioids on employment-to-population ratios is positive but small for women, but there is no relationship for men. These findings suggest that although they are addictive and dangerous, opioids may allow some women to work who would otherwise leave the labor force. When we examine the effect of employment-to-population ratios on opioid prescriptions, our results are more ambiguous. Overall, our findings suggest that there is no simple causal relationship between economic conditions and the abuse of opioids. Therefore, while improving economic conditions in depressed areas is desirable for many reasons, it is unlikely to curb the opioid epidemic.

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Many observers have decried the effects of the U.S. opioid epidemic on drug overdoses and mortality. The epidemic is particularly shocking since the majority of users start taking opioids that are prescribed by their physicians, even if they later progress to illicit or illegal opioid use. Case and Deaton (2015) point to the opioid crisis as an important cause of recent increases in mortality among middle-aged, non-Hispanic white Americans. And deaths may be viewed as the tip of the iceberg in that for every person who dies, many more are suffering the debilitating effects of addiction. Krueger (2017) documents that in a recent survey of prime-age white men who were out of the labor force, 50% report chronic pain and daily use of opioid pain medications.

These observations beg the question of whether the opioid crisis is a consequence of unemployment and economic dislocation among less-skilled American workers, or whether the indiscriminate prescription of opioids has promoted economic dislocation by transforming workers with curable and chronic injuries into addicts. A fundamental barrier to answering these questions is that areas such as Appalachia, which historically have low employment-to-population rates, have also been hardest hit by the opioid epidemic. But it is not clear whether this relationship is causal or reflects omitted factors such as local variation in physician prescribing behavior.

This paper uses quarterly county-level data to examine the relationship between opioid prescription rates and employment-to-population ratios. We have data on all prescriptions of opioids from 2006 to 2014 from QuintilesIMS which can be aggregated to the county-gender-age group-quarter level. These data are linked to data on employment from the Quarterly Workforce Indicators (QWI)[1] and to information on county population from the U.S. Census.

[1] This data set combines federal administrative data from a number of sources and is available at https://lehd.ces.census.gov/doc/QWI_101.pdf.

We first estimate models with employment-to-population ratios as the dependent variable and lagged opioid prescriptions as the independent variable. In order to address potential confounding, opioid prescriptions for younger ages are instrumented using opioid prescriptions to the elderly. Places with high opioid prescription rates among the elderly also tend to have high prescription rates among people of working age. The identifying assumption in these models is that prescriptions to the elderly should not have a direct effect on employment among younger people.

In order to evaluate the hypothesis that economic dislocation causes dependence on opioids, we also estimate models with opioid prescriptions per capita as the dependent variable and lagged employment-to-population ratios. In these models we use a Bartik-style shift share instrument for employment shocks in which the composition of county employment in a base year (2005) is used to predict the impact of national industry-level employment fluctuations on employment-to-population ratios at the local level (Bartik, 1991). Because the sample period covers the Great Recession and the recovery, there is a lot of local variation in employment to exploit. Arguably, the spike in unemployment, subsequent recovery, and its differential effect on local economies are not things that could easily have been forecast in 2005 given knowledge about county employment composition.

We find that, in general, there is a positive relationship between opioid prescribing and employment. Further analysis suggests that the estimated effect of opioids on employment-to-population ratios is positive but small for women, while there is no relationship for men. Specifically, a 100% increase in opioid prescribing would lead to increases in employment of 3.8% among women in counties with education above the mean and 5.2% among women in counties with education below the mean. This finding suggests that although they are addictive and dangerous, opioids may allow some women to work who would otherwise leave the labor force.

When we examine the effect of employment-to-population ratios on opioid prescriptions, our results are more ambiguous. We find some evidence that higher employment-to-population ratios reduce opioid prescription per capita among young workers, though this effect is only statistically significant in the instrumental variables specifications and only in counties with education above the median.

The rest of the paper is laid out as follows: We discuss the background literature in Section 1 and the data in Section 2. Section 3 presents an overview of our methods, while results and robustness are discussed in Sections 4 and 5. Section 6 offers a discussion and conclusions.

1. Background

Between 2000 and 2014, drug overdoses involving opioids rose 200%, fueling widespread concern about an opioid epidemic and spurring calls for changes in public policy (Chen et al., 2014; Dart et al., 2015; Rudd et al., 2016). A distinguishing feature of the current epidemic of drug abuse is that many overdoses and deaths can be attributed to legal opioids that were prescribed by a physician. The clinical use of opioids in the United States has quadrupled since 1999, contributing to the rise in drug overdoses, emergency room visits, and admissions for drug treatment. Moreover, there is increasing evidence that opioids are not effective for chronic pain over the longer term because patients build up dependence (Frieden and Houry, 2016). Despite significant efforts to restrict the prescribing of opioids over the past decade, prescription opioid abuse and drug overdoses due to prescription opioids have continued to rise (Health and Human Services, 2014; Meara et al., 2016).

In a groundbreaking paper, Case and Deaton (2015) coined the phrase "deaths of despair," arguing that the worsening economic position of less-educated whites in the United States had fueled increases in deaths due to suicide, alcohol, and drug addiction. This idea suggests that people are dying in large part because of behaviors that are themselves a response to worsening economic status.

This hypothesis follows naturally from a large body of literature showing that economic dislocation has health consequences for affected individuals. For example, Bergemann et al. (2011), Black et al. (2012), Browning and Heinesen (2012), Eliason and Storrie (2009a,b), and Sullivan and von Wachter (2009) all find negative effects of individual job displacement on health outcomes.[2] This literature suggests that these negative effects are generated by changes in health behaviors, such as increases in smoking (c.f. Black et al., 2012; Falba et al., 2005). More recently, Pierce and Schott (2017) find that areas affected by trade shocks experienced increases in drug overdoses and suicides. Charles, Hurst, and Schwartz (2018) find that areas affected by declines in manufacturing employment in the 2000s had persistently higher unemployment as well as increases in opioid use and deaths. Hollingsworth et al. (2017) examine the effect of macroeconomic conditions on county-level deaths and emergency room visits due to opioids using a 16-year panel and county fixed effects regressions. They find that a one percentage point increase in county unemployment rates is associated with a 3.55% increase in opioid fatalities. However, beyond the inclusion of county fixed effects, they do not address the possible reverse causality.[3]

[2] Browning et al. (2006), Salm (2009), and Strully (2009) do not find negative effects. The latter two analyze relatively small data sets and have low power to detect negative outcomes. Browning et al. use Danish administrative data. However, Browning and Heinesen (2012) reanalyze the same data and find negative effects.

[3] Another potential problem is that unemployment rates are not well measured at the county level because the Current Population Survey is not designed to be representative at that level of disaggregation.

Case and Deaton (2017) paint a more nuanced portrait pointing out that, for example, "deaths of despair" (deaths due to alcohol abuse, drug overdoses, and suicides) were rising even in the early 2000s—a time of great economic growth and before the Great Recession. Moreover, deaths due to these causes have continued to rise until very recently despite the fact that unemployment has now fallen to its lowest level in the post-war period. Case and Deaton attribute the continued increase to slower-moving social trends such as increasing instability in both marriages and jobs, stagnation of real wages, and the decline of organized religion.

A recent analysis by Ruhm (2017) comes to much the same conclusion. Using nation-wide county-level mortality data, Ruhm examines the correlation between mortality due to causes associated with deaths of despair and economic conditions. He finds that variation in economic conditions explains at most one-ninth of the variation in the growth in overall drug mortality rates and very little of the variation in deaths due to prescription opioids. He concludes that improvement in economic conditions in depressed places cannot, by itself, be expected to make much impact on deaths due to overdoses.

It is possible that economic dislocation could have strong effects on the propensity of affected individuals to die from "deaths of despair," without explaining much of the overall increase in these deaths. This is because there are other factors at work—for example, trends in the prescription of opioids may be a much more important root cause of drug overdoses, and these trends in turn reflect variations in provider behaviors and lax oversight relative to other countries (Schnell and Currie, forthcoming).

It is worth noting that the broader literature linking general economic conditions (rather than individual job loss) with health and health behaviors shows quite mixed effects. Ruhm (2000, 2005) argues that recessions are actually good for people's health because, for example, they have more leisure time for physical activity. Ásgeirsdóttir et al. (2014) find that the economic crisis of 2008 was associated with many improvements in health behaviors in Iceland. Therefore, it is by no means certain that areas with declining employment, for example, will also suffer deteriorations in health behaviors.

Krueger (2017) considers the opposite causal relationship—from opioids to non-employment. Krueger uses nationally representative data from the American Time Use Survey (ATUS), which asked respondents about pain and the use of pain medications. Workers who were out of the labor force reported feeling pain about half of the time, with disabled workers reporting that they spent 71% of their time in pain. Moreover, respondents who were out of the labor force reported pain ratings 89% higher than those of workers. The ATUS also asked about pain medications. Of prime-age men who were not in the labor force, 44% took pain medication on the previous day, though this figure could also have included non-prescription over-the-counter drugs like aspirin. Krueger also reports data from the National Health Interview Surveys, which, since 2005, has asked respondents about pain lasting a whole day or more in the past three months. These rates show a limited upward trend that contrasts with the large increase in opioid prescribing.

In order to obtain more information about the role of pain in labor force dropout, Krueger conducted the Princeton Pain Survey of 571 men aged 25 to 54 who were not in the labor force. The first wave was conducted from September 30 to October 2, 2016. This survey indicated that 47% of the men had taken pain medication on the previous day and that two-thirds of these had used prescription pain medications. These observations indicate that many men who are not in the labor market are in pain and using prescription pain medications (mostly opioids). But is there a causal relationship between these phenomena? Using cross-sectional data on opioid prescription rates from 2015, Krueger shows that the places with the most opioid prescribing experienced the largest declines in male labor force participation between 1999 and 2015 and suggests that opioids might explain up to 20% of the observed decline in labor force participation over the period.

Others have argued that untreated pain is a major source of lost productivity in the United States (Gaskin and Richard, 2012). Butikofer and Skira (2016) study the withdrawal of a pain reliever that was used for rheumatoid arthritis (the drug Vioxx, which caused heart problems in some users) and estimate that the availability of Vioxx decreased disability from joint pain by 6 to 15%.

To date, the hypotheses that opioids are responsible for declining labor force attachment has received little empirical attention. Harris et al. (2017) examine the effect of the opioid prescription rate on the unemployment rate, labor force participation, and employment-to-population ratio in 10 states over the period of 2013 to 2015. Because their sample variation is limited by the short timeline, they are unable to include county fixed effects in their models.[4] They find a large negative effect of opioid prescribing on employment-to-population ratios in this basically cross-sectional approach.

To our knowledge then, ours is the first paper to examine both the hypothesis that lack of employment has a causal effect on opioid use and the hypothesis that opioid use has a causal effect on employment in a similar framework, using a relatively long panel; and our paper is one of very few using instrumental variables strategies to try to deal with reverse causality.[5]

[4] They comment that they do not have sufficient variation to include county fixed effects, but they do include MSA fixed effects in some specifications.

[5] Harris et al. (2017) instrument for the opioid prescription rate using the number of high-volume prescribers per capita. We do not use this IV strategy because of concerns that the number of high-volume prescribers per capita may also be endogenous.

2. Data

Our prescription data was purchased from QuintilesIMS, a public company specializing in pharmaceutical market intelligence. This data set contains the number of prescriptions filled for opioid analgesics at U.S. retail pharmacies in each year from 2006 to 2014.[6] In addition to the number of prescriptions, the QuintilesIMS data contain information on the patient's age group and gender, and the address of the retail pharmacy. These data show a continuous increase in the number of opioid prescriptions from 2006 to 2012, and then a slight moderation after that. We calculate opioids per capita using population counts from the 2010 Census of Population.

Quarterly county-level employment data come from the QWI, which is publicly released by the U.S. Census Bureau. The employment data describe the level of employment at the beginning of a reference quarter for each gender, age group, and two-digit NAICS industry. We drop observations if they are missing or flagged as distorted or suppressed due to failure to meet U.S. Census Bureau publication standards.

The QWI is a unique jobs-level data set that draws on many sources of federal data, including administrative records on employment collected by the states, Social Security data, Federal tax records, and other census and survey data. The QWI covers about 95% of U.S. employment. Because the indicators are based on administrative data (rather than surveys) the county-level data are highly accurate compared to, for example, counts of unemployed workers from the Current Population Surveys. Moreover, firm- and worker-level data are linked so that

[6] QuintilesIMS surveys 86% of U.S. retail pharmacies and projects prescriptions filled at the remaining 14% of retail pharmacies.

the QWI is the only U.S. data set from which it is possible to obtain employment numbers by gender, age group, county, and industry.[7]

Because the QWI is a jobs-level data set, it can yield counts that are higher than the number of workers who hold any job due to multiple job holding. Moreover, a job-worker link is considered to exist at the beginning of the quarter if the job existed in the quarter before and in the quarter following. This way of counting could lead to underestimates of the number of workers with a job when workers switch jobs between quarters. A third issue is that people may not work in the counties where they live. For example, many people commute from northern New Jersey into Manhattan. Hence, areas like Manhattan may have employment-to-population ratios greater than one when computed using the QWI. In summary, the concept of employment in the QWI is not exactly the same as in either worker-based surveys or employer-based counts, though it does accurately capture changes in the number of jobs over the business cycle and across areas (U.S. Census Bureau, 2017).

The age groups provided in the opioid and employment data do not align perfectly (see Appendix Table 1). We partition into four distinct age groupings (roughly adolescents, young adults, prime-age workers, and retirees) and focus on the two groups of working-age individuals who have been hardest hit by the opioid crisis (young adults and prime-age workers). Therefore, the young adult groups are aged 20-44 in the employment data and 19-39 in the opioids data. The older workers are aged 45 to 64 in the employment data and aged 40 to 64 in the opioids data.

We use industry codes in order to construct “Bartik” or shift-share style instruments for employment-to-population ratios as described further below. Because many counties are missing

[7] Massachusetts and Washington, D.C. do not have QWI employment data for 2005. Data for Massachusetts start at 2010 Q2, and data for Washington, D.C. start at 2005 Q3. We use those time periods as the base years for the calculation of the shift-share instrument.

data for employment in specific two-digit NAICS codes, we aggregate the two-digit NAICS codes to two-digit SIC codes in order to have fewer categories, as shown in Appendix Table 2. There are a few two-digit NAICS industries that map to multiple two-digit SIC industries; in these cases we select the SIC industry which, in our judgement, fits the NAICS codes best.

An additional source of potential discrepancy between the opioid and the employment data is that individuals may not work and fill prescriptions in the same county. For example, an individual who lives in Princeton, New Jersey and obtains a prescription from a doctor close to home but who commutes to New York City for work would cause an inconsistency in the count of individuals involved when matching employment to opioid prescription data. This could be an issue for counties that contain large urban centers of employment.

Table 1 provides means of the employment-to-population ratios and the opioid prescriptions-to-population ratios. The means are shown by gender for all workers and for the two broad age groups by gender. We also show estimates for counties that were above and below the median education levels in 2010. The employment-to-population ratios are higher among men and among workers aged 18 to 44. The most striking difference, however, is between counties with higher and lower levels of education. On average, counties with a less-educated population have a male employment-to-population ratio of 0.525 compared to 0.720 in counties with a more-educated population. Turning to the opioid prescription-to-population rates we see a quite different pattern. Opioid prescription-to-population ratios are highest for women and for older workers. Moreover, the ratios of opioid prescriptions to population are slightly higher in high-education counties, although the differences by education are dwarfed by the differences across gender and age of worker.

Figure 1 provides a "heat map" of prescriptions per capita. The figure illustrates the large geographic variation in scripts per capita across the country, as well as the worsening of the epidemic over time (which one can see by comparing 2006 and 2014). Areas that were harder hit initially tended to be places with higher-than-average unemployment such as Appalachia, Maine, and rust belt states such as Michigan and Northern California. By 2014, prescribing had further intensified in these areas, but much of the rest of the country had begun to catch up, despite the improving economic conditions in most parts of the country.

Figure 2 shows employment-to-population ratios for workers aged 18 to 64 in the same two years. These figures also show considerable variation across locations, but much less variation over time than the "heat maps" for opioid scripts shown in Figure 1.

Figure 3 compares the employment-to-population ratios computed using the QWI to those computed using the Quarterly Census of Employment and Wages (QCEW), which comes only from employer reports for counties with over 100,000 population in 2010. As the figure indicates, the distributions are substantially similar, though the QWI has a somewhat thicker right tail.

Figure 4 shows the contemporaneous relationship between the log of scripts per capita and the log of employment-to-population ratios for the four age-sex groups we consider. Perhaps surprisingly, the relationship is positive, as indicated by the fitted regression lines through the scatter plots. Figure 4 shows that the data is quite noisy at the county level. In order to minimize the impact of this noise in what follows, we keep all counties with over 100,000 in population in 2010 and then aggregate the other counties in a state into one "rest of state" area.

Much of the previous work on opioids focuses on individuals with low educational attainment. Although we have access to employment data by education level in the QWI, we do not know the education level of the patient in the prescription database and cannot match prescriptions to employment by education. Thus, to explore this aspect of the opioid-employment relationship, we rank each county based on the proportion of individuals within the county with less than or equivalent to a high school degree as of the 2000 Census. We split the data set into two halves based on this criterion and run identical regressions on each subset of data.

Finally, because health insurance is closely linked to employment in the United States, we also estimate models controlling for the percent of people aged 18 to 64 who are insured in each county. These data come from the U.S. Census Bureau's Small Area Health Insurance Estimates (SAHIE), which is the only source of data for single-year estimates of health insurance coverage status for all counties. The SAHIE estimates are based on data from the American Community Survey, tax returns, administrative data from the Supplemental Nutrition Assistance Program, Medicaid, the Children's Health Insurance Program, and the 2010 Census. Estimates are available for males and females (see https://www.census.gov/data-tools/demo/sahie/sahie.html, accessed October 30, 2017).

3. Methods

We would like to know if more opioid prescribing in a county causes people to lose their jobs, and conversely, whether a lack of employment opportunities causes people to turn to opioids. Posing the questions in this way highlights the potential simultaneity of employment and opioid prescriptions. We deal with this problem first by regressing the dependent variable on lagged values of the independent variables in each equation. By doing so we assume that any effects are not instantaneous and that it is past opioid use that affects employment and vice versa. Second, we estimate models with and without county fixed effects in order to gauge the extent to which any effects that we find are due to constant, or long-term characteristics of places rather than short-term fluctuations in either opioid prescribing or employment opportunities.

Third, we estimate instrumental variables models. In models where employment-to-population ratios are the dependent variable, opioids per capita for people of working age are instrumented using opioids per capita prescribed to people aged 65 and older of the same gender. The assumption underlying this instrument is that some places have doctors who are more likely to prescribe opioids than others (Schnell, 2017), so places where elderly people are more likely to get prescriptions are places where working-age people are also more likely to get them. The raw correlations between per capita prescriptions for working-age people and the corresponding elderly groups are 0.781, 0.721, 0.934, and 0.907 for women and men aged 18 to 44 and for women and men aged 45 to 64, respectively. At the same time, we do not expect prescriptions to the elderly to have a direct effect on the employment of working-aged people.

In regressions where employment-to-population ratios are the independent variables, we instrument using a Bartik-style shift-share instrument. Using the industry composition of the county's employment from the base year of 2005, we calculate the county's predicted employment if the level of employment in each industry had changed in the same ratio as in the rest of the nation in that industry. For example, in a place that initially had a lot of manufacturing jobs, a national shock to manufacturing would tend to have a heavy impact. We sum over industries in each county to calculate the predicted level of employment (the Bartik instrument) for each gender and age group. Thus, the expression for the Bartik instrument in county *i* and date *t* is given by:

$$Bartik_{it} = \sum_{j \in \{sectors\}} \left(\frac{employment_{ij,2005}}{employment_{i,2005}} * \frac{\sum_{k \in \{counties \setminus i\}} employment_{jkt}}{\sum_{k \in \{counties \setminus i\}} employment_{jk,2005}} \right)$$

In summary then, for each demographic group *j* we estimate the following equations:

(1) $ln(employment_{ijt}/population_{ijt}) = \alpha_0 + \alpha_1 ln(average(prescriptions/population_{ij,t-1\ldots t-4})) + \gamma_t + \varepsilon_{it}$,

(2) $ln(employment_{ijt}/population_{ijt}) = \alpha_0 + \alpha_1 ln(average(prescriptions/population_{ij,t-1\ldots t-4})) + \phi_i + \gamma_t + \varepsilon_{it}$,

where ϕ_i are fixed effects for counties and γ_t are fixed effects for time periods.

In addition to estimating models by ordinary least squares (OLS), we also estimate models similar to (2) in which we instrument $ln(average(prescriptions/population_{ij,t-1\ldots t-4}))$ with the first-stage equation given by:

(3) $ln(average(prescriptions/population_{ij,t-1\ldots t-4})) = \beta_0 + \beta_1 ln(average(prescriptions/population_{i65+,t-1\ldots t-4})) + \phi_i + \gamma_t + \varpi_{it}$,

where $ln(average(prescriptions/population_{i65+,t-1\ldots t-4}))$ indicates the average number of prescriptions per capita to elderly people of the same gender as group *j* in county *i* at times *t-1* to *t-4*.

We also estimate:

(4) $ln(prescriptions_{ijt}/population_{ijt}) = \alpha_0 + \alpha_1 ln(average(employment/population_{ij,t-1\ldots t-4})) + \gamma_t + \varepsilon_{it}$,

(5) $ln(prescriptions_{ijt}/population_{ijt}) = \alpha_0 + \alpha_1 ln(average(employment/population_{ij,t-1\ldots t-4})) + \phi_i + \gamma_t + \varepsilon_{it}$,

and in the instrumental variables version of (5), the first stage is given by:

(6) $ln(average(employment/population_{ij,t-1\ldots t-4})) = \beta_0 + \beta_1 ln(average(Bartik\ instrument_{ij,t-1\ldots t-4})) + \phi_i + \gamma_t + \varpi_{it}$.

Recently Goldsmith-Pinkham, Sorkin, and Swift (2017) have argued that the Bartik instrument amounts to using interactions between initial local employment shares and national industry employment rates. It is clear that these instruments are much more likely to meet the exogeneity assumption in models that include county fixed effects than in those without, which underscores the importance of examining these relationships using panel data.

4. Results

While our primary focus is on models broken down by demographic group, Appendix Table 3 shows estimates of Equations (1) and (4) using all working-age people and comparing estimates obtained using the QWI and the QCEW. We show OLS estimates for all counties, the top half of counties in terms of education, and the bottom half of counties in terms of education. The estimates are very similar in the two data sources, though somewhat more precisely estimated in the QWI. In what follows, we use the QWI because the QWI allows us to use employment numbers broken down by gender, age group, county, and industry.

Table 2 shows estimates for Equation (1), in which employment-to-population depends on per capita prescriptions. We begin with a model without county fixed effects, estimated for males and females in two age groups. Arguably these estimates should be most similar to past research, which also does not include county fixed effects. Because of the log-log formulation, these estimates can be treated as elasticities.

The estimates all suggest a positive effect of lagged opioid prescriptions on the employment-to-population ratios. The estimates are higher for older workers aged 45 to 64. For older workers, the effects are larger in high-education counties, whereas for younger workers they are larger in low-education counties. We also show IV estimates similar to Equation (3), although they do not include county fixed effects. The IV estimates are quite similar to OLS in this formulation. However, the instrument may not be valid without fixed effects as it is possible that the prescription rates among the elderly pick up other characteristics of counties.

Table 3 shows estimates of Equation (2), which do include county fixed effects in order to control for the many important differences between counties. The estimated elasticities are all much smaller than in Table 2, but remain positive. They are larger in the low-education counties and statistically significant in the OLS models for workers aged 18 to 44 (column 3). The IV estimates also show some evidence of significant positive effects in the models for female workers in low-education counties, though the IV estimates for males are small and not statistically significant.

Taken literally, these estimates suggest that opioid prescriptions may help some female workers to stay in the labor force, though the effects are small. In counties that have below the median level of education, a 100% increase in opioid prescriptions per capita is estimated to increase the employment-to-population ratio by 3.0% among women aged 18 to 44 and by 4.3% among women aged 45 to 64. We do not find any statistically significant effect among men.

In Table 4 we turn to the opposite question of whether higher employment-to-population ratios discourage reliance on opioids. This table, without county fixed effects, suggests that there are actually large positive effects of higher employment rates on opioid prescribing and that these effects are larger for younger workers aged 18 to 44. For these workers, the estimated effects are once again larger in less-educated than in more-educated counties. The IV point estimates are slightly larger than the OLS, but not significantly so.

Table 5 shows the same models including county fixed effects. The OLS estimates remain large and positive. However, the instrumental variables estimates are negative for workers aged 18 to 44 in high-education counties, but large and positive for older women workers in less-educated counties. Taken at face value, the estimates suggest that younger workers may be less likely to turn to opioids when employment-to-population ratios are higher. This finding might reflect less "despair," but it might also reflect workers' ability to be more selective about their jobs and who can avoid jobs that cause them pain or injury.

However, among older women, there appears to be a positive relationship between employment-to-population ratios and opioid prescribing. Such a positive relationship could reflect greater access to medical care. Most people obtain prescription opioids legally from physicians, and these prescriptions are often paid for by health insurance. People who work and have health insurance through their jobs may therefore have more access to opioids. In addition, those who obtain prescription drugs illegally from the secondary market must pay a high price, and it may be that only the employed can afford them.

5. Robustness

In a county-level analysis of this sort, there are many potential confounders. One of the most important potential confounders is the prevalence of health insurance in the county, as health insurance is strongly related to employment in the United States. While health insurance varies markedly across counties, it may not be adequately controlled for by including a county fixed effect alone since the fraction of Americans with insurance has been changing over this period. Declining health insurance coverage rates were an important motivation for the Affordable Care Act (ACA). Starting in 2011, the ACA increased coverage for those aged 18 to 26 by allowing them to be covered under their parent's health insurance plans. In 2014, the health insurance exchanges created by the ACA opened; some states also expanded their Medicaid coverage of adults prior to 2014.

Tables 6 and 7 show estimates similar to the models with county fixed effects shown in Tables 3 and 5, but controlling for the percent insured in each county and gender group. Our intent is not to identify a causal effect of insurance coverage, as this would require an additional instrument. Rather, we ask whether the estimates are sensitive to the inclusion of measures of insurance coverage. Because the county-level insurance data are annual, we first estimate models similar to Equation (2) but aggregating our data to the annual level. Because estimates on insurance coverage are available only for those aged 18 to 64, we also aggregate the two age categories. We then show estimates adding the additional “percent insured” variable.

Panels 1 and 3 of Table 6 show that our qualitative findings about the effects of opioids on employment-to-population ratios continue to hold in the annual pooled-ages data. We find a significant positive relationship between opioids per capita and employment-to-population among women and no relationship among men.

Panels 2 and 4 of Table 6 show that when we add the percent insured to the model, the estimated effect of opioid prescriptions on the employment-to-population ratio is unaffected. It remains statistically significant for females but not for males. The effects are larger in the less-educated counties, consistent with the estimates in Table 3.

Table 7 shows estimates of the effect of the employment-to-population ratio on prescriptions per capita. The estimates are qualitatively similar to those shown in Table 5 in terms of the pattern of signs. However, none of the estimates are statistically significant for high-education counties (even in OLS) and none of the IV estimates are statistically significant. Hence, the estimated effects of employment ratios on opioid prescribing are more sensitive to specification than the estimated effects of opioids on employment-to-population.

6. Discussion and Conclusions

We have the advantages of a long time period that allows us to control for county specific fixed effects, detailed data on opioid prescriptions, and quarterly employment data that are available at the county level and broken down by age and gender groups. This latter advantage allows us to explore the extent to which results differ for older and younger workers and for males and females.

The estimated effects of opioids on employment-to-population ratios are robust to changes in specification and suggest that there is a positive relationship for women and no relationship for men. More specifically, a 100% increase in opioid prescribing would lead to increases in employment of 3.8% among women in counties with education above the median and 5.2% among women in counties with education below the median. Our limited exploration of the effects of health insurance would appear to rule it out as a major explanation for these findings. An alternative explanation is that although they are addictive and dangerous, opioids nevertheless allow some women to keep working who might otherwise withdraw from the labor force.

When we examine the effect of employment-to-population ratios on opioid prescriptions, our results are more ambiguous. We find some evidence that higher employment-to-population ratios reduce opioid prescriptions per capita among young workers, though this effect is only statistically significant in the instrumental variables specifications and only in counties with education above the median.

Perhaps this very weak evidence regarding an effect of employment-to-population ratios on opioid prescribing should not surprise us. As Case and Deaton (2017) clarify, the type of despair they describe has more to do with a longer-term unraveling of the social fabric than with short-term variations in employment prospects. Moreover, there is increasing evidence that opioid prescribing patterns depend more on idiosyncratic factors such as the characteristics and preferences of local doctors than on local economies (Currie and Schnell, 2018; Schnell, 2017).

Overall, our analysis suggests that the relationship between opioid prescribing and employment is considerably murkier than simple narratives would suggest. The fact that many opioid users are still in the labor market (and are likely having their scripts paid for by employer-sponsored health insurance) is one reason that opioids are having such a large impact on American employers. This observation suggests that policy responses should be designed to take account of the fact that many addicts work, so that treatment options that help people retain their connection to the labor market are likely to be necessary to effectively combat the epidemic.

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Figure 1: Opioid scripts per person 18-64, 2006 (top) and 2014 (bottom)

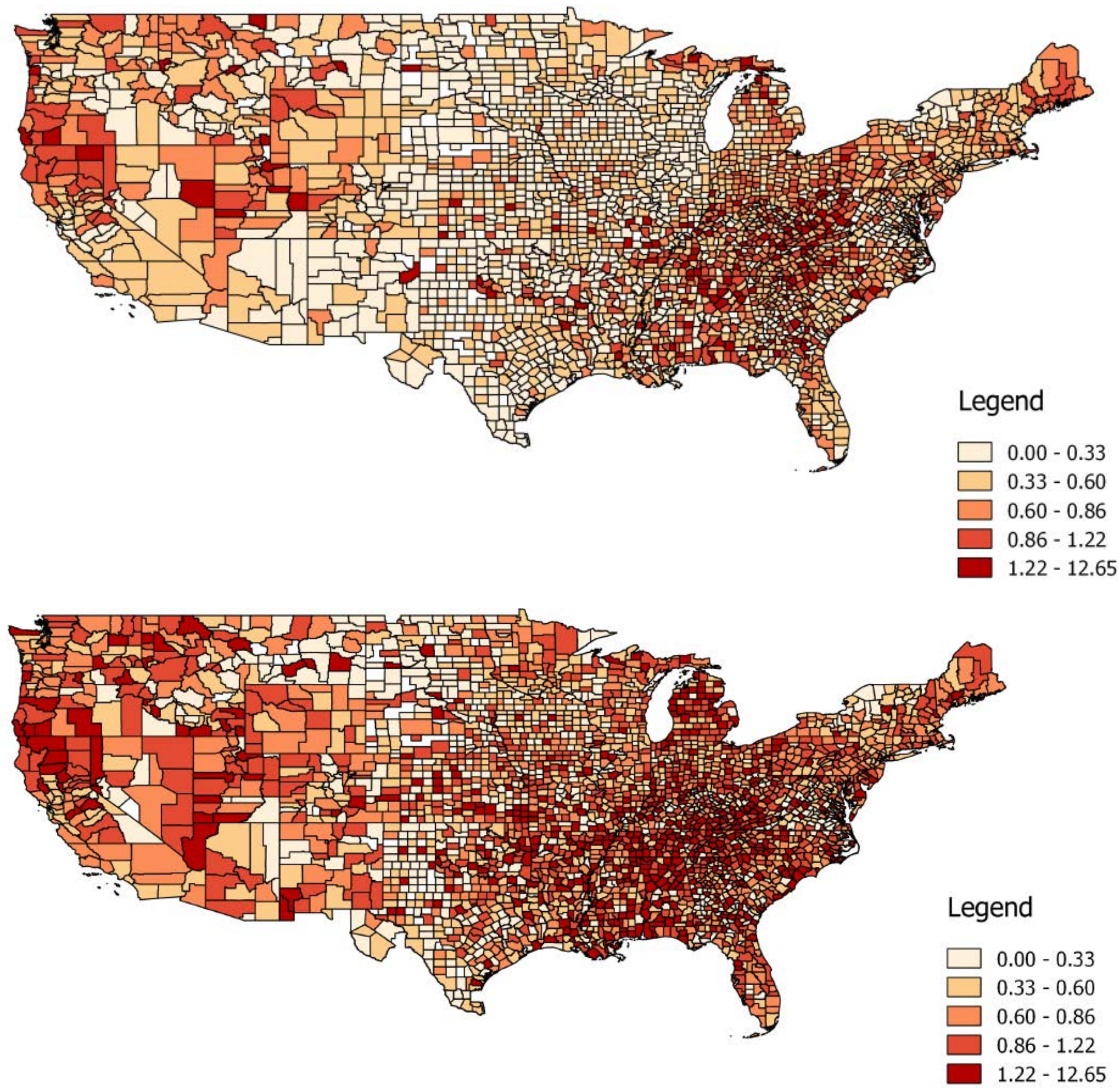
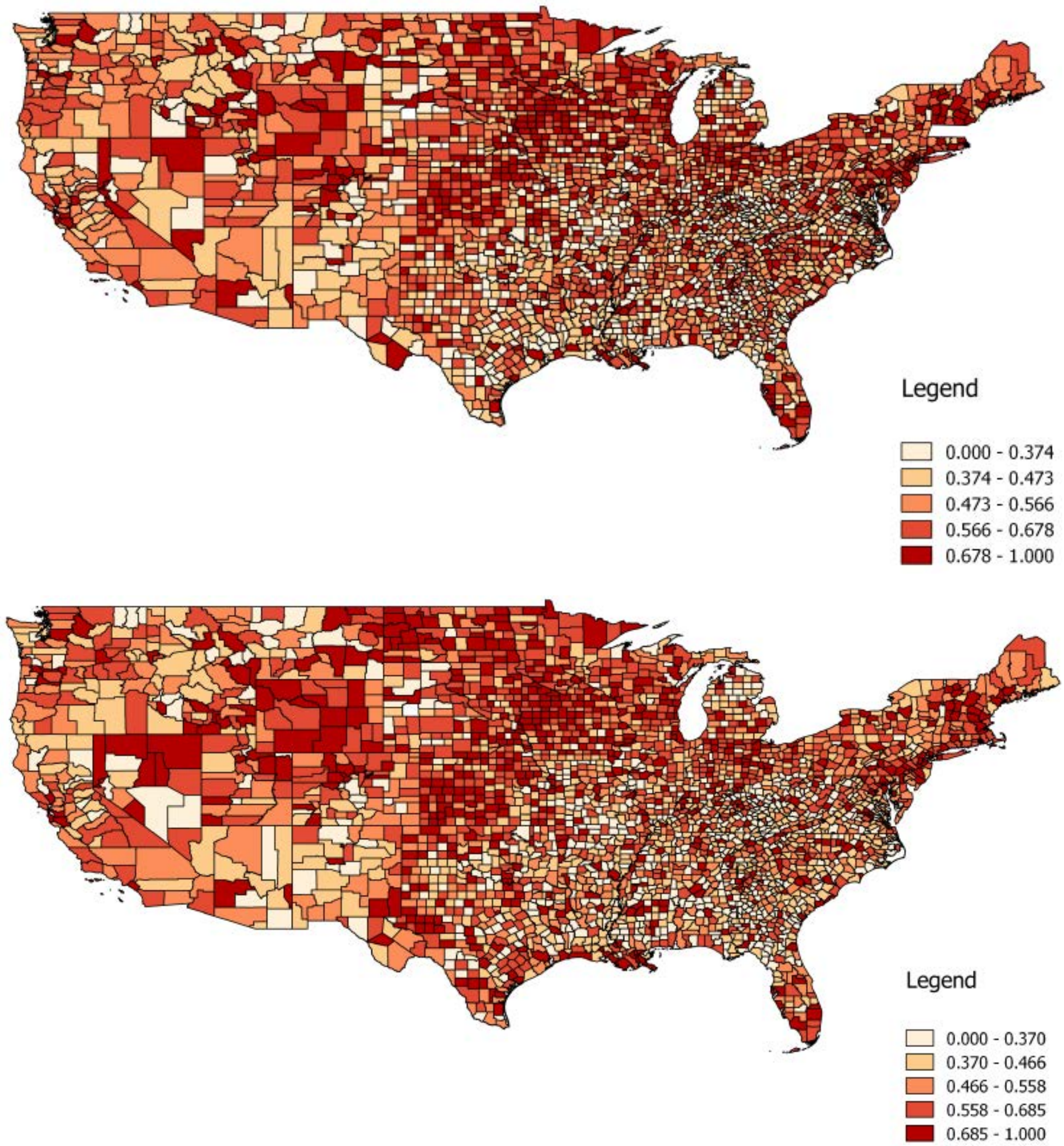


Figure 2: Employment to Population Ratios 18-64 year olds, 2006 (top), 2014 (bottom)



Notes: Employment to population ratios constrained to be between .2 and 1.

Figure 3: Comparison of Employment to Population in the QCEW and QWI, Counties over 100,000 population in 2010

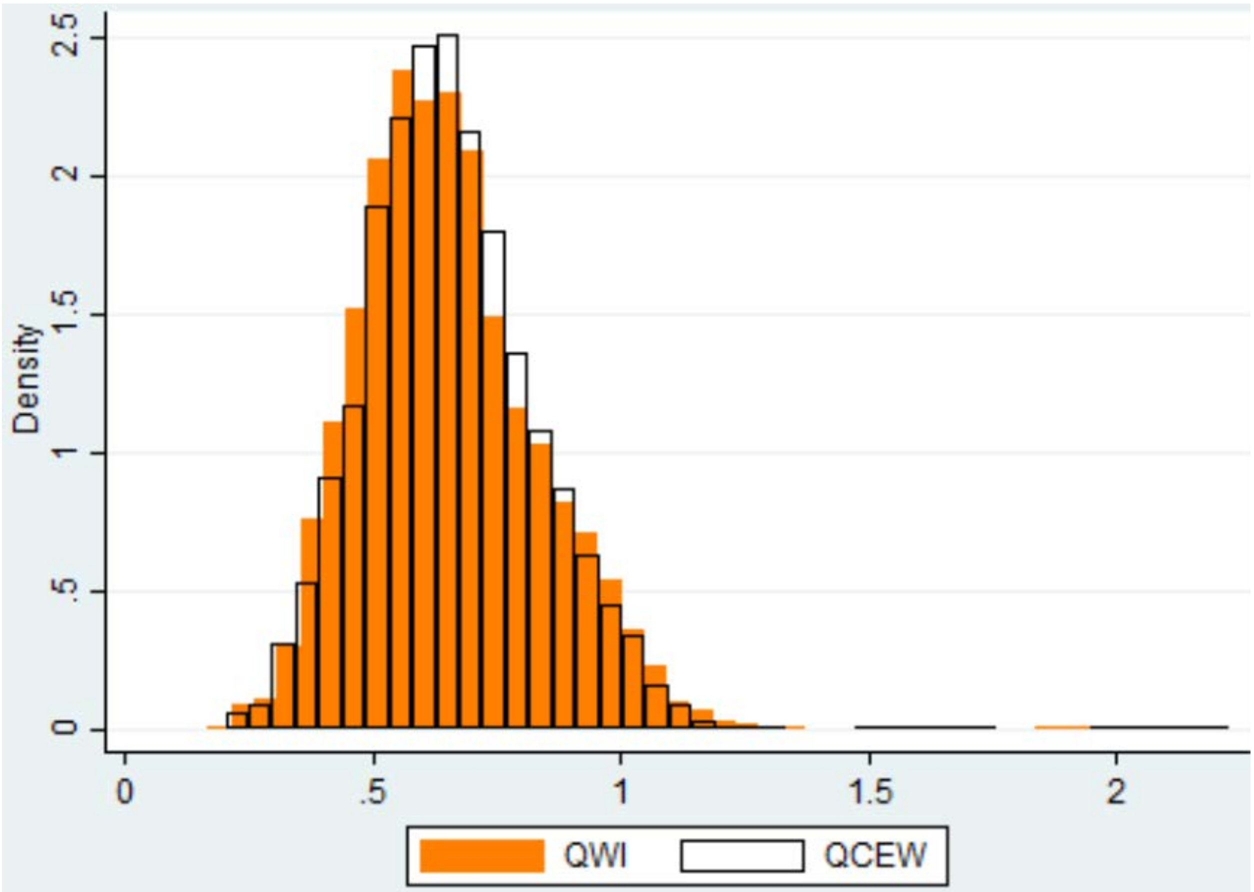
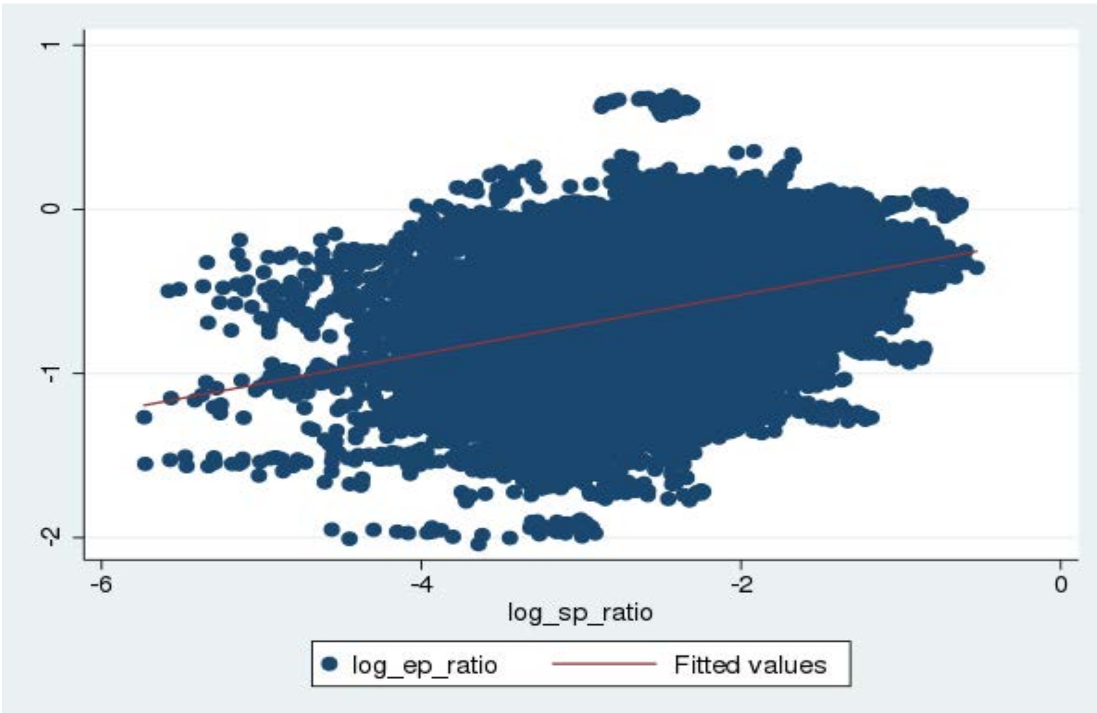
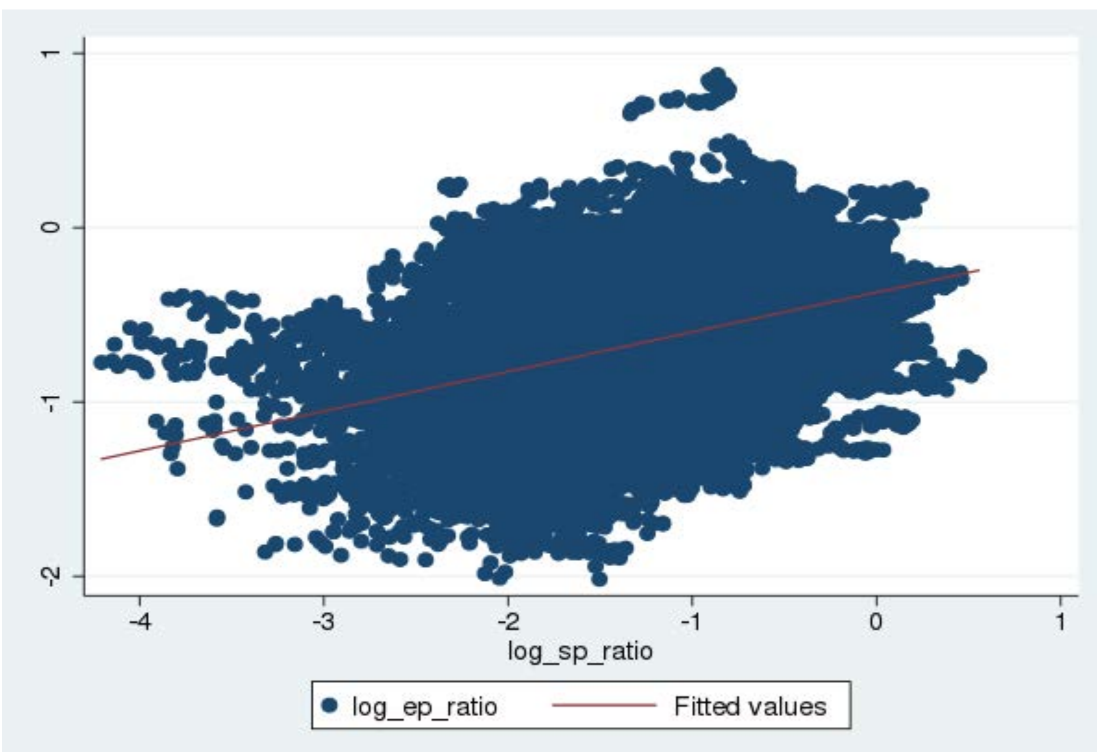


Figure 4: Log(Employment to Population) vs. Log (Scripts per Capita),

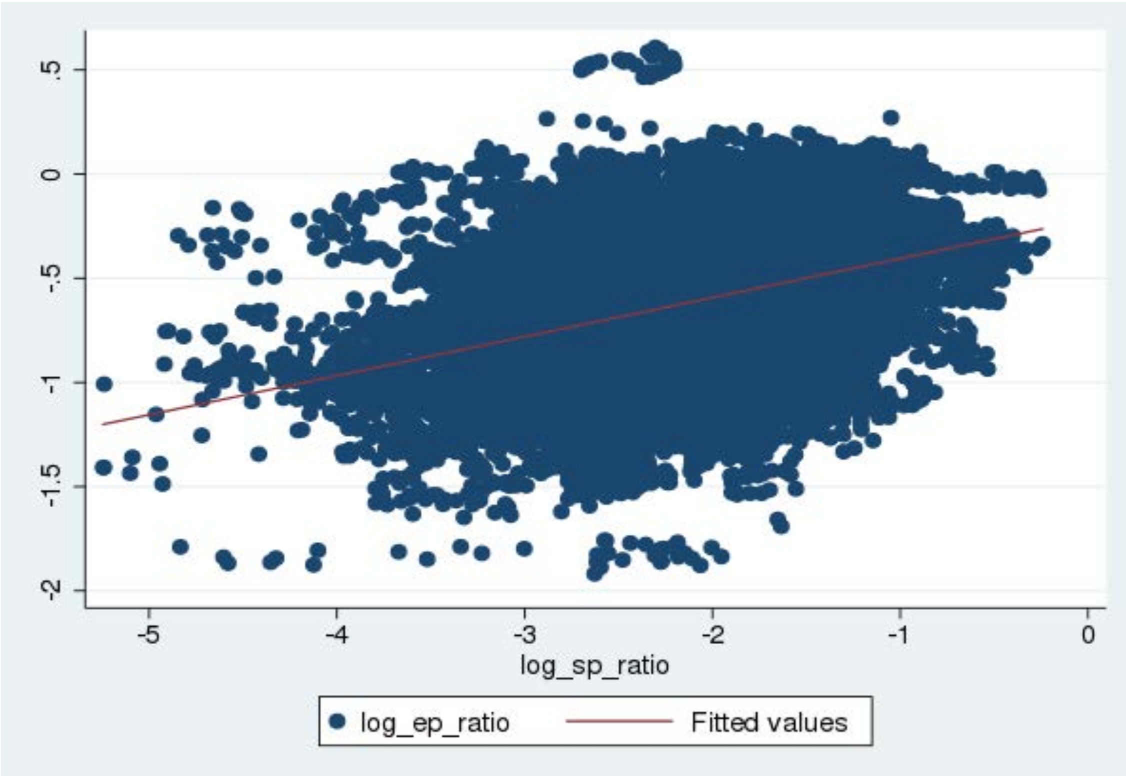
a) **Males 25-44**



b) **Males 45-64**



c) Females 25-44



d)Females 45-64

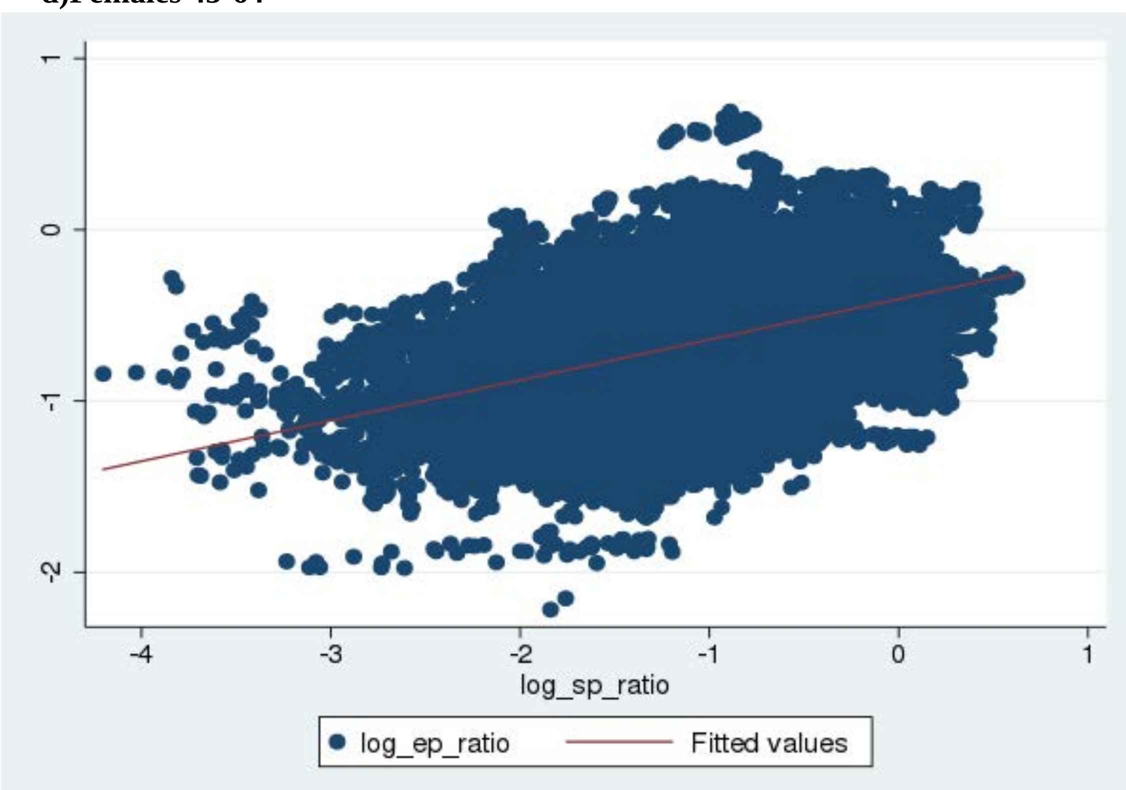


Table 1: Mean Employment to Population and Prescriptions Rates

	Age	All Counties	High Education Counties	Low Education Counties
Mean Employment/Population Ratio				
Males	All	0.624	0.720	0.525
	18-44	0.628	0.722	0.529
	45-64	0.618	0.718	0.521
Females	All	0.610	0.684	0.531
	18-44	0.621	0.697	0.541
	45-64	0.595	0.672	0.521
Mean Prescriptions/Population Ratio				
	Age	All Counties	High Education Counties	Low Education Counties
Males	All	0.180	0.207	0.192
	18-44	0.088	0.090	0.087
	45-64	0.310	0.324	0.298
Females	All	0.243	0.267	0.257
	18-44	0.138	0.138	0.138
	45-64	0.385	0.397	0.375

Notes: County level employment data by age and gender come from the Quarterly Workforce Indicators. Prescriptions were purchased from IMSQuintiles. Population counts are from the 2010 Census.

Table 2: Log(Employment to Population) on Log(Lagged Opioids per Capita)

Specifications without county fixed effects

	(1) All Counties-OLS	(2) High Ed. Counties-OLS	(3) Low Ed. Counties-OLS	(4) High Ed. Counties-IV	(5) Low Ed. Counties-IV
1. Females, 18-44; Mean = .606					
Log(Opioids per Capita)	.188***	.141***	.209***	.205***	.292***
	(.047)	(.039)	(.064)	(.039)	(.050)
R^2	0.113	0.065	0.2244		
First stage F-stat	-	-	-	251.63	310.33
2. Females, 45-64; Mean = .586					
Log(Opioids per Capita)	.222***	.253***	.156***	.251***	.205***
	(.038)	(.036)	(.060)	(.037)	(.054)
R^2	0.111	0.149	0.082		
First stage F-stat	-	-	-	633.31	721.88
3. Males, 18-44; Mean = .609					
Log(Opioids per Capita)	.193***	.164***	.169***	.223***	.297***
	(.043)	(.033)	(.065)	(.043)	(.049)
R^2	0.105	0.088	0.147		
First stage F-stat	-	-	-	381.49	240.2
4. Males, 45-64; Mean = .599					
Log(Opioids per Capita)	.215***	.264***	0.09	.313***	.192***
	(.044)	(.047)	(.061)	(.050)	(.050)
R^2	0.083	0.132	0.032		
First stage F-stat	-	-	-	844.86	486.26
N	20,453	10,142	10,278	10,142	10,278

Notes: First-stage estimates for IV: Coefficient on endogenous variable = .944, standard error = .036, R^2 = .806. All regressions include year, and quarter fixed effects. Standard errors clustered on the county and shown in parentheses. Counties with less than 100,000 people have been merged into one aggregate county for each state.

Table 3: Log(Employment to Population) on Log(Lagged Opioids per Capita)

Specifications with county fixed effects

	(1) All Counties-OLS	(2) High Ed. Counties-OLS	(3) Low Ed. Counties-OLS	(4) High Ed. Counties-IV	(5) Low Ed. Counties-IV
1. Females, 18-44; Mean = .606					
Log(Opioids per Capita)	0.024***	0.018*	0.030***	0.017	0.033**
	(.007)	(.010)	(.009)	(.016)	(.016)
R^2	0.986	0.984	0.981	0.984	0.981
First stage F-stat	-	-	-	247.8	116.21
2. Females, 45-64; Mean = .586					
Log(Opioids per Capita)	0.014*	0.015	0.019	0.035**	0.043***
	(.008)	(.011)	(.013)	(.017)	(.017)
R^2	0.986	0.985	0.982	0.985	0.981
First stage F-stat	-	-	-	211.53	68.17
3. Males, 18-44; Mean = .609					
Log(Opioids per Capita)	0.020**	0.009	0.035***	-0.003	-0.011
	(.008)	(.011)	(.011)	(.017)	(.018)
R^2	0.984	0.982	0.974	0.982	0.973
First stage F-stat	-	-	-	227.74	112.67
4. Males, 45-64; Mean = .599					
Log(Opioids per Capita)	0.005	0.0004	0.014	0.012	-0.012
	(.008)	(.011)	(.012)	(.016)	(.016)
R^2	0.989	0.988	0.984	0.988	0.984
First stage F-stat	-	-	-	283	84.82
N	20,453	10,142	10,278	10,142	10,278

Notes: First-stage estimates for IV: Coefficient on endogenous variable = .912, standard error = .033, R2 = .998. All regressions include year, and quarter fixed effects. Standard errors clustered on the county and shown in parentheses.

Counties with fewer than 100,000 people have been merged into one aggregate county for each state.

Table 4: Log(Prescriptions Per Capita) on Log(Lagged Employment to Population)

Specifications without county fixed effects

	(1) All OLS	(2) High Ed. Counties-OLS	(3) Low Ed. Counties-OLS	(4) High Ed. Counties-IV	(5) Low Ed. Counties-IV
Females, 18-44; Mean = .160					
Log(Employment to Population Ratio)	.575***	.396**	1.034***	.398***	1.137***
	(.161)	(.156)	(.244)	(.149)	(.239)
R^2	0.206	0.191	0.289		
First stage F-stat	-	-	-	98.52	335.49
Females, 45-64; Mean = .433					
Log(Employment to Population Ratio)	.479***	.554***	.497**	.568***	.570***
	(.093)	(.093)	(.196)	(.097)	(.198)
R^2	0.218	0.259	0.187		
First stage F-stat	-	-	-	292.09	246.64
Males, 18-44; Mean = .103					
Log(Employment to Population Ratio)	.479***	.420***	.751***	.416***	.808***
	(.106)	(.112)	(.232)	(.109)	(.252)
R^2	0.195	0.208	0.204		
First stage F-stat	-	-	-	151.89	274.63
Males 45-64, Mean = 0.348					
Log(Employment to Population Ratio)	.355***	.470***	0.242	.512***	.350**
	(.069)	(.071)	(.166)	(.076)	(.175)
R^2	0.196	0.25	0.142		
First stage F-stat	-	-	-	215.76	333.12
N	20,453	10,159	10,278	10,159	10,278

Notes: First-stage estimates for IV: Coefficient on endogenous variable=0.954, standard error= 0.020, R2 = .933. All regressions include year, and quarter fixed effects.
Standard errors clustered on the county and shown in parentheses.
Counties with less than 100,000 people have been merged into one aggregate county for each state.

Table 5: Log(Prescriptions Per Capita) on Log(Lagged Employment to Population)

Specifications with county fixed effects

	(1) All OLS	(2) High Ed. Counties-OLS	(3) Low Ed. Counties-OLS	(4) High Ed. Counties-IV	(5) Low Ed. Counties-IV
Females, 18-44; Mean = .160					
Log(Employment to Population Ratio)	0.546***	0.477***	0.641***	-1.143*	-0.093
	(0.088)	(0.130)	(0.121)	(0.620)	(0.448)
R^2	0.93	0.907	0.948		
First stage F-stat	-	-	-	44.7	35.68
Females, 45-64; Mean = .433					
Log(Employment to Population Ratio)	.221***	.322***	.263**	15.64	1.553***
	(.071)	(.098)	(.113)	(13.75)	(0.341)
R^2	0.948	0.94	0.958		
First stage F-stat	-	-	-	29.57	29.97
Males, 18-44; Mean = .103					
Log(Employment to Population Ratio)	.449***	.337**	.545***	-.798**	-0.881
	(.096)	(.164)	(.100)	(.355)	(.593)
R^2	0.917	0.893	0.937	0.885	
First stage F-stat	-	-	-	52.52	10.1
Males, 45-64; Mean = 0.348					
Log(Employment to Population Ratio)	.213***	.227**	.307***	0.710	-0.553
	(.065)	(.094)	(.095)	(.630)	(.551)
R^2	0.945	0.936	0.955		
First stage F-stat	-	-	-	48.75	22.2
N	20,453	10,142	10,278	10,142	10,278

Notes: First-stage estimates for IV: Coefficient on endogenous variable = .793, standard error = 0.196, R2 = 0.999. All regressions include year, and quarter fixed effects.
Standard errors clustered on the county and shown in parentheses.
Counties with less than 100,000 people have been merged into one aggregate county for each state.

Table 6: Log(Employment to Population) on Log(Lagged Opioids per Capita)

Specifications with county fixed effects and % insured

	(1) All OLS	(2) High Ed. Counties-OLS	(3) Low Ed. Counties-OLS	(4) High Ed. Counties-IV	(5) Low Ed. Counties-IV
1. Females, 18-64, Mean = 0.622					
Log(Opioids per Capita)	.025***	0.018	.034***	.038*	.052***
	(.009)	(.013)	(.012)	(.020)	(.017)
R^2	0.988	0.988	0.983	-	-
	-	-	-	342.27	114.04
2. Females, 18-64, controlling for %insured					
Log(Opioids per Capita)	.026***	0.021	.038***	.039**	.058***
	(.009)	(.013)	(.012)	(.020)	(.018)
%insured	.279***	.292***	.334***	.303***	.343***
	(.059)	(.082)	(.086)	(.081)	(.083)
R^2	0.988	0.988	0.984	-	-
First stage F-stat	-	-	-	269.99	103.19
3. Males, 18-64, Mean = 0.632					
Log(Opioids per Capita)	.020**	0.009	.038***	0.017	0.007
	(.010)	(.014)	(.012)	(.020)	(.017)
	0.989	0.988	0.983	-	-
	-	-	-	336	139.33
4. Males, 18-64, controlling for %insured					
Log(Opioids per Capita)	.022**	0.012	.042***	0.021	0.017
	(.010)	(.014)	(.012)	(.020)	(.016)
%insured	.347***	.391***	.367***	.396***	.358***
	(.061)	(.089)	(.073)	(.086)	(.070)
R^2	0.99	0.988	0.984	-	-
First stage F-stat	-	-	-	300.51	144.81
N	5585	2780	2805	2780	2805

Notes: First-stage estimates for IV: Coefficient on endogenous variable = .996, standard error = 0.031, R2 = 0.996. All regressions include year, and quarter fixed effects.
Standard errors clustered on the county and shown in parentheses.
Counties with less than 100,000 people have been merged into one aggregate county for each state.

Table 7: Log(Prescriptions Per Capita) on Log(Lagged Employment to Population)

Specifications with county fixed effects and % insured

	(1) All OLS	(2) High Ed. Counties-OLS	(3) Low Ed. Counties-OLS	(4) High Ed. Counties-IV	(5) Low Ed. Counties-IV
. Females, 18-64; Mean = 1.085					
Log(Employment to Population Ratio)	.200**	0.171	.341***	-11.59	0.211
	(.082)	(.115)	(.100)	(32.71)	(.433)
R^2	0.986	0.985	0.987	-	-
First stage F-stat	-	-	-	49.68	26.07
2. Females, 18-64; Controlling for %insured					
Log(Employment to Population Ratio)	.212**	0.186	.373***	-15.9	0.251
	(.083)	(.114)	(.101)	(64.62)	(.439)
%insured	-.229	-.411	-.395	2.287	-.359
	(.157)	(.231)	(.234)	(10.83)	(.273)
R^2	0.986	0.985	0.987	-	-
First stage F-stat	-	-	-	44.71	22.31
3. Males, 18-64; Mean = 0.802					
Log(Employment to Population Ratio)	.223**	0.141	.349***	-2.179	-2.786
	(.088)	(.141)	(.088)	(1.767)	(2.296)
R^2	0.984	0.983	0.986	-	-
First stage F-stat	-	-	-	63.76	19.67
4. Males, 18-64; Controlling for %insured					
Log(Employment to Population Ratio)	.239***	0.166	.379***	-2.276	-3.151
	(.090)	(.144)	(.091)	(2.005)	(2.892)
%insured	-.230	-.437**	-.372	.258	.733
	(.153)	(.218)	(.242)	(.619)	(.877)
R^2	0.984	0.983	0.986	-	-
First stage F-stat	-	-	-	66.04	16.23
N	5,585	2,784	2,801	2,784	2,801

Notes: First-stage estimates for IV: Coefficient on endogenous variable = .616, standard error = 0.226, R2 = 0.999. All regressions include year, and quarter fixed effects.

Standard errors clustered on the county and shown in parentheses.

Counties with less than 100,000 people have been merged into one aggregate county for each state.

Appendix Table 1

Age group classification	Opioid	Employment
1: Adolescents	0-2, 3-9, 10-19	14-18
2: Young adults	20-39	19-21, 22-24, 25-34, 35-44
3: Prime age workers	40-59, 60-64	45-54, 55-64
4: Retirees	65-74, 75-84, 85+	65-99

Appendix Table 2

NAICS industry	SIC industry
11 (Agriculture, Forestry, Fishing, and Hunting)	01-09 (Agriculture, Forestry, and Fishing)
21 (Mining, Quarrying, and Oil and Gas Extraction)	10-14 (Mining)
23 (Construction)	15-17 (Construction)
31-33 (Manufacturing) 51 (Information)	20-39 (Manufacturing)
22 (Utilities) 48-49 (Transportation and Warehousing)	40-49 (Transportation and Public Utilities: Electric, Gas, Communications, Sanitary)
42 (Wholesale Trade)	50-51 (Wholesale Trade)
44-45 (Retail Trade) 72 (Accommodation and Food Services)	52-59 (Retail Trade)
52 (Finance and Insurance) 53 (Real Estate and Rental and Leasing) 55 (Management of Companies and Enterprises)	60-67 (Finance, Insurance, and Real Estate)
54 (Professional, Scientific, and Technical Services) 56 (Administrative and Support and Waste Management and Remediation Services) 61 (Educational Services) 62 (Health Care and Social Assistance) 71 (Arts, Entertainment, and Recreation) 81 (Other Services (except Public Administration))	70-89 (Services)
92 (Public Administration)	91-99 (Public Administration)

Appendix Table 3: Comparison of QWI and QCEW OLS Estimates for All Working Age Individuals

	(1) All Counties QWI	(2) All Counties QCEW	(3) High Ed. Counties QWI	(4) High Ed. Counties QCEW	(5) Low Ed. Counties QWI	(6) Low Ed. Counties QCEW
1. Dependent Variable = ln(Employment/Population); Mean = .664 (QWI), .666 (QCEW)						
Log(Opioids	0.011*	0.011*	0.015*	0.014	0.010*	0.009*
per Capita)	(0.005)	(0.005)	(0.007)	(0.007)	(0.004)	(0.004)
R^2	0.985	0.986	0.985	0.987	0.972	0.976
N	100,744	100,738	50,156	50,154	50,588	50,584
2. Dependent Variable = ln(Prescriptions/Population); Mean = .286						
Log(Emp.	0.158**	0.166**	0.203**	0.220*	0.137*	0.141
per Capita)	(0.050)	(0.065)	(0.068)	(0.0910	(0.067)	(0.077)
R^2	0.947	0.947	0.957	0.957	0.925	0.925
N	100,517	100,517	50,048	50,48	50,469	50,469

Standard errors in parentheses. These are population weighted regressions including all available county-quarter observations. All regressions include county fixed effects.