U.S. Employment and Opioids: Is There a Connection?

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Revised June 2018

Abstract: This paper uses quarterly county-level data from 2006–2014 to examine the direction of causality in the relationship between per capita opioid prescription rates and employment-to-population ratios. We first estimate models of the effect of per capita opioid prescription rates on employment-to-population ratios, instrumenting opioid prescriptions for younger ages using opioid prescriptions to the elderly. We find that the estimated effect of opioids on employment-to-population ratios is positive but small for women, while there is no relationship for men. We then estimate models of the effect of employment-to-population ratios on opioid prescription rates using a shift-share instrument, and find ambiguous results. 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 on its own to curb the opioid epidemic.

Key words: Employment, Opioids, Prescriptions
JEL: I12, J01

*Corresponding author is Currie, jcurrie@princeton.edu. 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 Wellbeing at Princeton University. The statements, findings, conclusions, views, and opinions contained and expressed herein are not necessarily those of QuintilesIMS or any of its affiliated or subsidiary entities.

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-aged 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 per capita 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 and to information on county population from the U.S. Census.

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, per capita opioid prescriptions for younger ages are instrumented using per capita opioid prescriptions to the elderly. Places with high per capita 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.

We find 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 above-median education and 5.2% among women in counties with below-median education.

In order to evaluate the hypothesis that economic dislocation causes dependence on opioids, we also estimate models with per capita opioid prescriptions as the dependent variable and lagged employment-to-population ratios as the independent variable of interest. 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 could not easily have been forecast in 2005 given knowledge about county employment composition.

In these specifications, our results are more ambiguous. We find some evidence that higher employment-to-population ratios reduce per capita opioid prescriptions among young workers, though this effect is only statistically significant in the instrumental variable (IV) specifications and only in counties with above-median education. We conclude that the relationship between opioid prescribing and employment is actually quite weak and that factors other than economic dislocation must underlie the sharp increase in deaths due to opioids.

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

Drug overdoses involving opioids rose 200% between 2000 and 2014, becoming an acknowledged public health crisis, and calls for a public policy response (Chen et al., 2014; Dart et al., 2015; Rudd et al., 2016). Unlike in past drug epidemics, currently many overdoses and deaths involve legal opioids prescribed by a physician. Since 1999, the clinical use of opioids has quadrupled in the U.S., leading to increases in overdoses, emergency room visits, and admissions for drug treatment in addition to the surge in deaths. Although many opioids are prescribed for chronic pain, opioids are not effective for this indication over the long term because patients build up dependence (Frieden and Houry, 2016). Until very recently, prescriptions of opioids continued to rise despite the rising toll (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 US 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.[1] This literature suggests that some of 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).

In contrast to the effects of individual job loss, the broader literature linking general economic conditions to heath and health behaviors suggests that health is countercyclical. Ruhm (2000, 2005) argues that recessions are actually good for people's health because, for example, they have more leisure time for physical activity and other health seeking behaviors. 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 will also suffer deteriorations in health behaviors.

Recent studies examining the association between economic conditions and opioid abuse find mixed results. While Hollingsworth et al. (2017), Carpenter et al. (2017), Pierce and Schott (2017), and Charles et al. (2018) conclude that adverse macroeconomic conditions—including rising unemployment, detrimental trade shocks, and declining manufacturing employment—have led to increases in opioid use and mortality, Ruhm (2017) finds that variation in economic

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

conditions can explain at most one-ninth of the variation in the growth in overall drug-related mortality rates.[2] In support of Ruhm's findings, Case and Deaton (2017) paints a more nuanced portrait than their earlier paper and highlights that deaths of despair were rising even in the early 2000s—a time of great economic growth and before the Great Recession.[3] Notably, none of these papers other than Pierce and Schott (2017) use methods that address the possibility of reverse causality.[4]

Attributing the rise in opioid abuse to the deterioration of economic conditions is an appealing narrative, but any causal relationship between opioids and employment could just as easily run in the opposite direction.[5] In fact, in its most recent report on employment in the U.S., the Organization of Economic Cooperation and Development states that the opioid epidemic is responsible for recent declines in labor force participation in the U.S. (OECD, 2018). However, the evidence for this proposition is primarily descriptive and cross-sectional.

In descriptive work, Krueger (2017) demonstrates using cross-sectional data that places with the most opioid prescribing in 2015 experienced the largest declines in male labor force participation between 1999 and 2015, though he acknowledges that labor force participation was declining in many of the same places prior to the opioid epidemic. Krueger notes that if the

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[2] Ruhm controls for a variety of demographic covariates, including race, female headship, foreign-born persons. A possible critique of his approach is that variables such as female headship themselves could be affected by the use of opioids, so that their inclusion may attenuate the estimated effects.

[3] Economic dislocation could strongly affect the propensity for affected individuals to die from deaths of despair without explaining much of the overall increase in these deaths. There are other factors at work—for example, trends in the prescription of opioids may be a more important root cause of drug overdoses, and these trends may be due to provider behaviors and lax oversight of prescribing rather than to general economic conditions. Schnell and Currie (2018) show that county-level mortality rates rise with per capita prescriptions over the time period we examine. However, since a relatively small proportion of addicts in the labor force die at any point in time, trends in employment will not necessarily follow trends in mortality.

[4] Another potential concern with earlier work is that many studies use county-level unemployment rates from the Current Population Survey (CPS), although the CPS is not designed to be representative at that level of disaggregation.

[5] Others have argued that untreated pain is a major source of lost productivity in the US (Gaskin and Richard, 2012; Butikofer and Skira, 2016).

correlation is causal, then increased opioid could explain up to 20% of the observed decline in labor force participation over the period. Using data for 10 states from 2013 to 2015, Harris et al. (2017) similarly find a negative effect of opioid prescription rates on labor market outcomes, although their sample variation is too limited to allow for county fixed effects.[6]

To our knowledge then, our paper is the first 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. Our work further builds on previous studies by using IV strategies to address reverse causality, by using a relatively long panel that allows us to control for fixed differences across counties, and by incorporating detailed prescriptions and employment data that allows for county-level analyses within different age and gender groups.

2. Data

We purchased prescription data 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.[7] 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 followed by a slight moderation. To calculate opioids per capita, we divide opioid prescriptions within a county from QuintilesIMS by county-level population counts from the 2010 Census.

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

We construct quarterly, county-level employment data from 2006 to 2014 using information from the Quarterly Workforce Indicators (QWI). The QWI is a unique jobs-level data set that is publicly released by the U.S. Census Bureau. Covering 95% of employment in the US, the data set combines information from a number of sources including administrative employment records, Social Security data, and Federal tax records.[8] Because the QWI is based on administrative data, the county-level employment measures are more accurate than employment counts from surveys such as the Current Population Survey. Moreover, firm- and worker-level data are linked so that the QWI is the only data set from which it is possible to obtain employment numbers by gender, age group, county, and two-digit NAICS industry codes across the US.[9]

The concept of employment in the QWI is not exactly the same as in either worker-based surveys or employer-based counts for a number of reasons. First, since 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. Second, 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. Finally, since people may live and work in different counties, some areas may have employment-to-population ratios greater than one when computed using the QWI. Despite these measurement differences, the QWI accurately captures changes in the number of jobs over the business cycle and across areas (U.S. Census Bureau, 2017).

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[8] We drop observations if they are missing or flagged as distorted or suppressed due to failure to meet U.S. Census Bureau publication standards.

[9] We use industry codes in order to construct shift-share style instruments for employment-to-population ratios as described further below. In doing so, 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 best fits the NAICS code.

Two concerns arise when merging the opioid and employment data that are worth noting. First, as shown in Appendix Table 1, the age groups provided in the prescription and employment data do not align perfectly. When constructing the age groups that we predominately focus on—young adults and prime-aged workers—we therefore use slightly different age ranges in each data set. In particular, the young adult group consists of individuals aged 20-44 in the employment data and 19-39 in the prescription data, while the prime-age group consists of individuals aged 45-64 in the employment data and 40-64 in the prescription data.

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.

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. In order to explore this aspect of the opioid-employment relationship, we rank each county based on the proportion of individuals within the county with highest educational attainment of high school or less as of the 2000 Census. We split the data set in half based on this criterion and estimate identical regression models on each subset of the data.

Finally, because health insurance is closely linked to employment in the US, 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).

Summary Statistics

Table 1 provides means of the employment-to-population ratios and the per capita opioid prescription rates. The means are shown by gender for all workers and for the two broad age groups that we consider: young adults and prime-age adults. We also show estimates for counties with above- and below-median education, where county-level education is defined as the proportion of the county with a high school degree or less 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 per capita opioid prescription rates, we see a quite different pattern. Per capita opioid prescription rates are highest for women and for older workers. Moreover, the per capita opioid prescription rates 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. In analyses that are not shown, we also estimated separate regressions using QCEW instead of QWI employment data and obtained similar results.

Figure 4 shows the contemporaneous relationship between the log of per capita prescriptions 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.

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 in three ways.

First, we estimate models in which we regress the dependent variables of interest on lagged values of the independent variables. In so doing, 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. For demographic group *j* in county *i* in quarter *t*, these specifications are given by

$$(1)\quad ln\left[\left(\frac{employment}{population}\right)_{ijt}\right] = \alpha_0 + \alpha_1 ln\left[\frac{1}{4}\sum_{q=1}^{4}\left(\frac{prescriptions}{popultation}\right)_{ij,t-q}\right] + \gamma_t + \epsilon_{ijt}$$

$$(1')\quad ln\left[\left(\frac{employment}{population}\right)_{ijt}\right] = \alpha_0 + \alpha_1 ln\left[\frac{1}{4}\sum_{q=1}^{4}\left(\frac{prescriptions}{population}\right)_{ij,t-q}\right] + \varphi_i + \gamma_t + \epsilon_{ijt}$$

$$(2)\quad ln\left[\left(\frac{prescriptions}{population}\right)_{ijt}\right] = \alpha_0 + \alpha_1 ln\left[\frac{1}{4}\sum_{q=1}^{4}\left(\frac{employment}{popultation}\right)_{ij,t-q}\right] + \gamma_t + \epsilon_{ijt}$$

$$(2')\quad ln\left[\left(\frac{prescriptions}{population}\right)_{ijt}\right] = \alpha_0 + \alpha_1 ln\left[\frac{1}{4}\sum_{q=1}^{4}\left(\frac{employment}{population}\right)_{ij,t-q}\right] + \varphi_i + \gamma_t + \epsilon_{ijt}$$

where φ_i are fixed effects for counties, γ_t are fixed effects for time periods, and ε_{ijt} is an error term.

Third, we estimate IV models. In models where employment-to-population ratios are the dependent variable, we instrument for per capita opioid prescriptions to working-aged people using per capita opioids prescriptions to people aged 65 and older of the same gender. In the IV version of Equation (1), the first-stage equation is given by

$$(3) \quad ln\left[\frac{1}{4}\sum_{q=1}^{4}\left(\frac{prescriptions}{population}\right)_{ij,t-q}\right] = \beta_0 + \beta_1 ln\left[\frac{1}{4}\sum_{q=1}^{4}\left(\frac{prescriptions}{population}\right)_{i65+,t-q}\right] + \varphi_i + \gamma_t + \omega_{ijt}$$

where $ln\left[\frac{1}{4}\sum_{q=1}^{4}\left(\frac{prescriptions}{population}\right)_{i65+,t-q}\right]$ indicates the average number of per capita prescriptions to those aged 65+ of the same gender as group *j* in county *i* in quarters *t-1* to *t-4*.

One assumption underlying this instrument is that doctors are more likely to prescribe opioids to everyone in some places than in others (Schnell, 2017), so that places where elderly people are more likely to get prescriptions are places where working-age people are also more likely to get them.[10] At the same time, we do not expect prescriptions to the elderly to have a direct effect on the employment of working-aged people.

A second identifying assumption is that there is no third unobserved variable (other than opioid prescriptions for those of working age) that affects both the employment of the working-age population and opioid prescription rates for the elderly. While it is always possible that such a variable exists, it is hard to think of what it might be. Economic conditions should not affect prescription rates among the elderly, since they are largely out of the labor force and have prescription drug benefits under Medicare Part D. High rates of manual labor and resulting disability might lead to high rates of opioid prescribing among the elderly, but these characteristics of the local labor market would also presumably lead to high opioid prescribing

[10] The raw correlations between per capita prescriptions for those aged 18 to 44 (45 to 64) and those aged 65+ are 0.781 (0.934) and 0.721 (0.907) for women and men, respectively.

among prime-age workers. To the extent that any such omitted variables are fairly constant over time, they will be captured by county fixed effects in our models.

In regressions where employment-to-population ratios are the independent variables, we instrument for employment 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.[11] 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* for demographic group *j* in quarter *t* is given by

$$Bartik_{ijt} = \sum_{s \in \{sectors\}} \left(\frac{employment_{ijs,2005}}{employment_{ij,2005}} * \frac{\sum_{k \in \{counties \setminus i\}} employment_{kjst}}{\sum_{k \in \{counties \setminus i\}} employment_{kjs,2005}} \right)$$

In the IV version of Equation (2), the first stage is therefore given by

$$(4) \quad ln\left[\frac{1}{4}\sum_{q=1}^{4}\left(\frac{employment}{population}\right)_{ij,t-q}\right] = \beta_0 + \beta_1 ln\left[\frac{1}{4}\sum_{q=1}^{4} Bartik_{ij,t-q}\right] + \varphi_i + \gamma_t + \omega_{ijt}$$

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.

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

4. Results

While our primary focus is on models broken down by demographic group using only data from the QWI, Appendix Table 3 shows estimates of Equations (1) and (2) using all working-age people and comparing estimates obtained using the QWI and the QCEW. We show OLS estimates for all counties and for counties with above- and below-median 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 ratios are a function of per capita prescriptions. We begin with a model without county fixed effects and estimate models separately by gender and age group (young adults and prime-age adults). These estimates should arguably be most similar to past research that also does not include county fixed effects. Because of the log-log formulation, these estimates can be interpreted as elasticities.

The estimates all suggest a positive effect of lagged opioid prescriptions on employment-to-population ratios. The estimates are higher for workers aged 45 to 64 than for younger adults. For older workers, the effects are largest in high-education counties, whereas for younger workers they are largest in low-education counties. IV estimates from a specification similar to Equation (1) are shown in columns 4 and 5.[12] The IV estimates are quite similar to the OLS estimates 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.

[12] Estimates of the first stage equations are shown in Appendix Table 4. They indicate that prescriptions for the elderly are indeed strong predictors of prescriptions for working-age people in each of our four age and gender groups.

Table 3 shows estimates of Equation (1'), which do include county fixed effects in order to control for constant differences across 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 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.[13]

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 per capita opioid prescriptions 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 in columns 4 and 5 are slightly larger than the OLS, but not significantly so.[14]

Table 5 shows the same models including county fixed effects. The OLS estimates remain large and positive. The IV estimates are negative for workers aged 18 to 44 in high-education

[13] Estimates of the first stage equations with county fixed effects are shown in columns 3-4 of Appendix Table 4. These estimates are quite similar to those without county fixed effects and indicate once again that prescriptions for the elderly are strong predictors of prescriptions for working-age people in each of our four age and gender groups.
[14] First stage equations are shown in Appendix Table 5. The Bartik shift-share instrument has a coefficient slightly less than one in each of the eight education, age, and gender groups.

counties, but large and positive for older women workers in less-educated counties.[15] Taken at face value, these 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 avoid jobs that cause them pain or injury. Among older women, there appears to be a positive relationship between employment-to-population ratios and opioid prescribing, though the lack of significance of the instrument suggests that we should be careful when interpreting this estimate.

5. Robustness

There are many potential confounders in any county-level analysis. A potentially important confounder for our study is the prevalence of health insurance in the county, as health insurance is strongly related to employment in the US. While health insurance varies markedly across counties, it may not be adequately controlled for by including county fixed effects alone since the fraction of Americans with health insurance changed over our sample 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 parents' health insurance plans. In 2014, the health insurance exchanges created by the ACA opened; some states also expanded their Medicaid coverage of childless adults prior to 2014.

Tables 6 and 7 show estimates similar to the models with county fixed effects that were shown in Tables 3 and 5 but controlling for the percent insured in each county and gender group.

[15] The first stage estimates are shown in the last two columns of Appendix Table 5. They are similar to those from models without county fixed effects with one exception: the Bartik instrument is no longer a significant predictor of employment-to-population ratios among females 45-64 in high-education counties.

Our intent is not to identify a causal effect of insurance coverage, as to do so 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 Equations (1') and (2') but aggregating our data to the annual level. Because estimates of insurance coverage are available only for those aged 18 to 64, we also aggregate the two age categories.

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. As before, 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 employment-to-population ratios on per capita opioid prescriptions using annual pooled-ages data. The estimates are qualitatively similar to those shown in Table 5 in terms of the pattern of signs. However, none of the OLS estimates are statistically significant for high-education counties 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 ratios.

6. Discussion and Conclusions

Compared to most previous work, we have the advantage of a long time period that allows us to control for time-invariant differences across counties using county fixed effects. We also have detailed data on opioid prescriptions as well as quarterly employment data that are available at the county level and broken down by age and gender groups. This rich data 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 significant 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 above-median education and 5.2% among women in counties with below-median education. Our 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 per capita opioid prescriptions among young workers, though this effect is only statistically significant in the IV specifications and only in counties with education above the median.

The limited effect of employment-to-population ratios on per capita opioid prescriptions that we document should perhaps not be a surprise. 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 (Schnell and Currie, 2018; Schnell, 2017).

Overall, our analysis suggests that the relationship between opioid prescribing and employment is considerably weaker and murkier than popular narratives 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 into account 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)

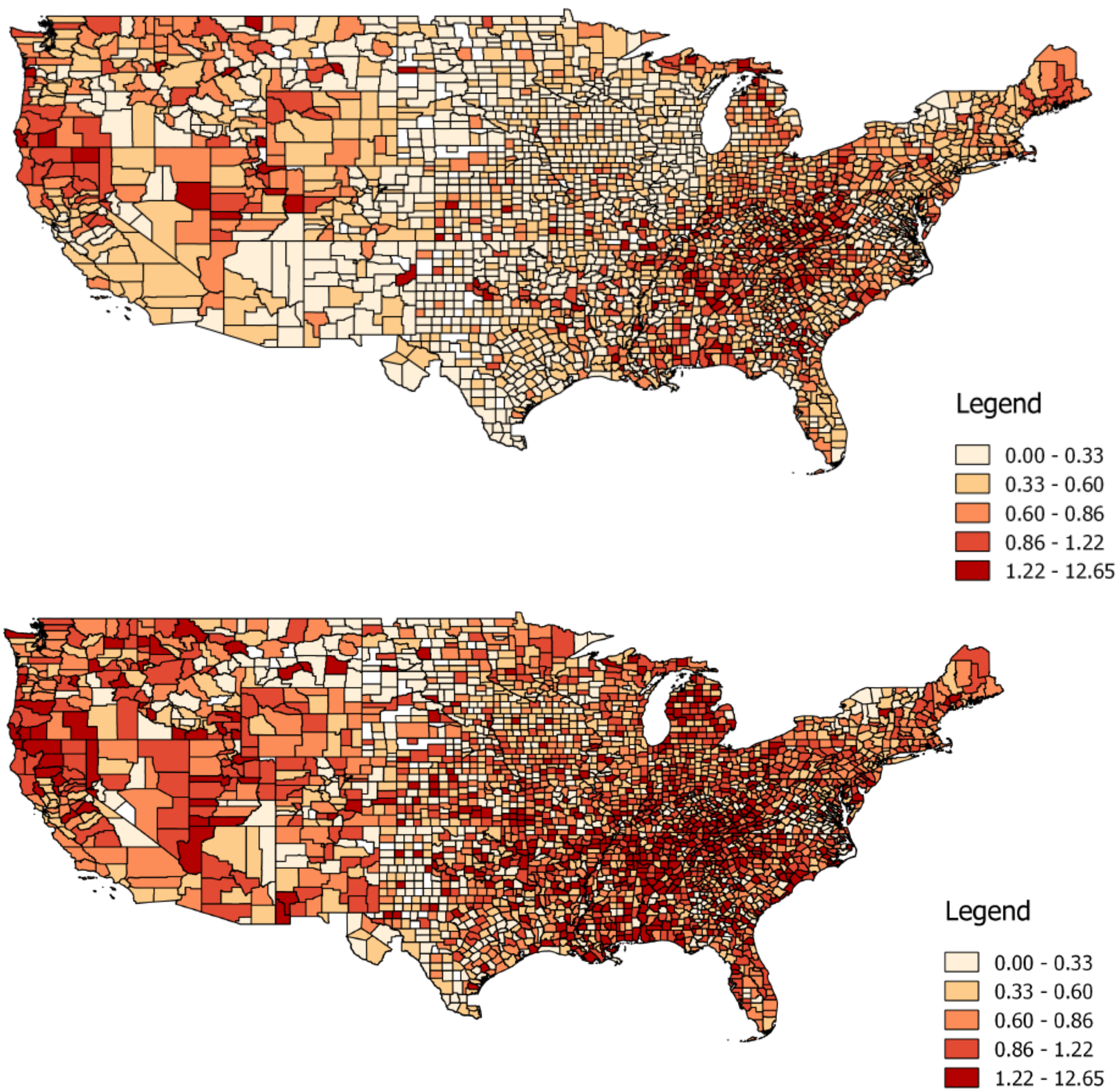
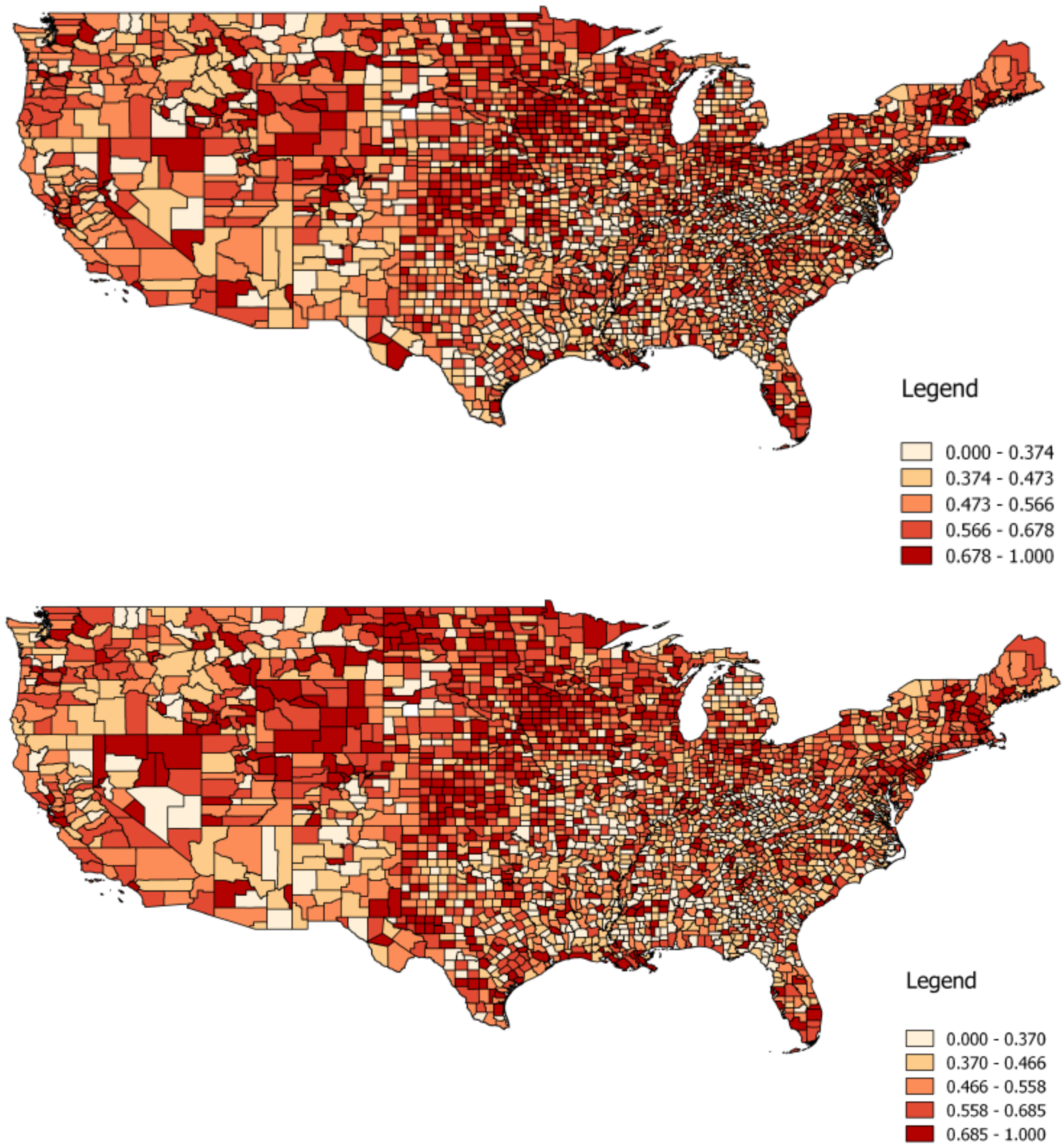


Figure 2: Employment to Population Ratios 18-64 year olds, 2006 (top), 2014 (bottom)



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

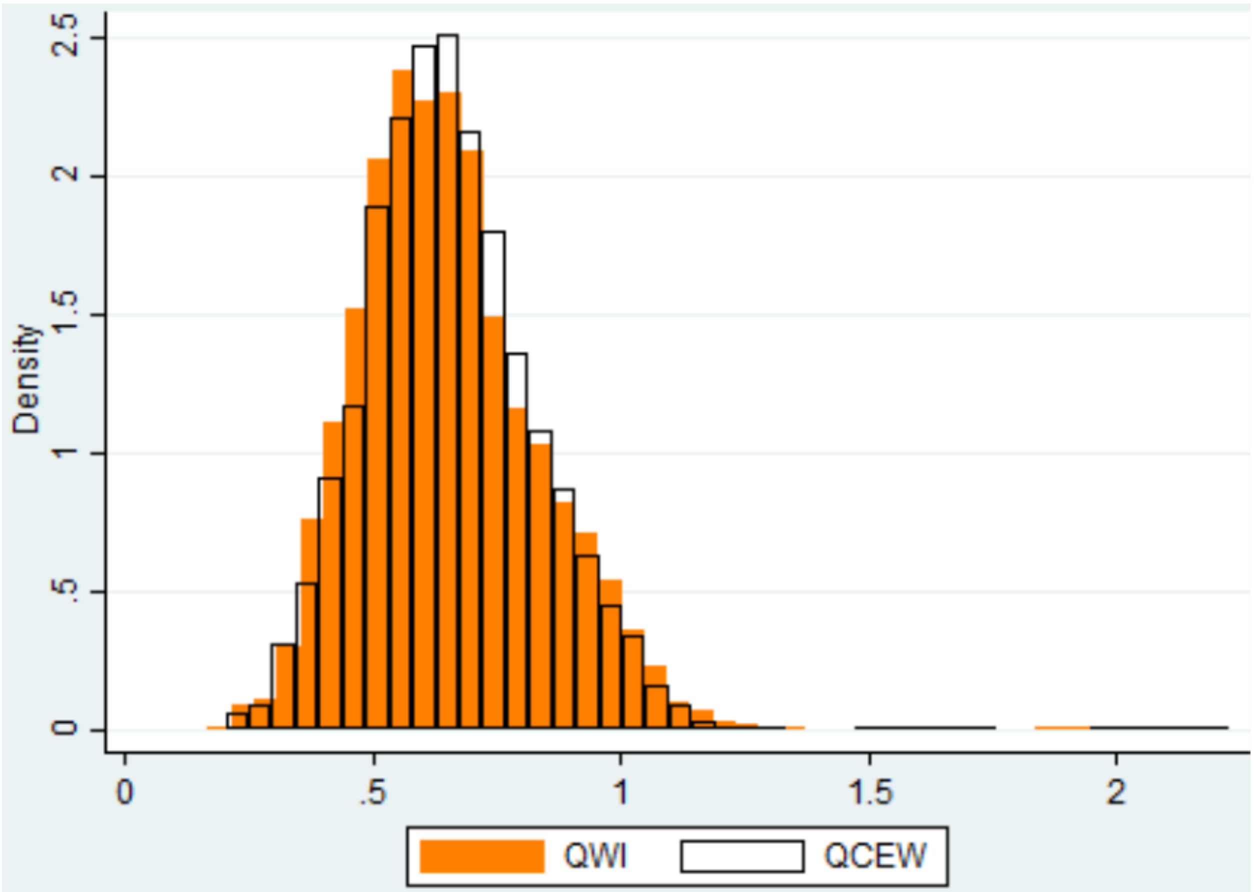
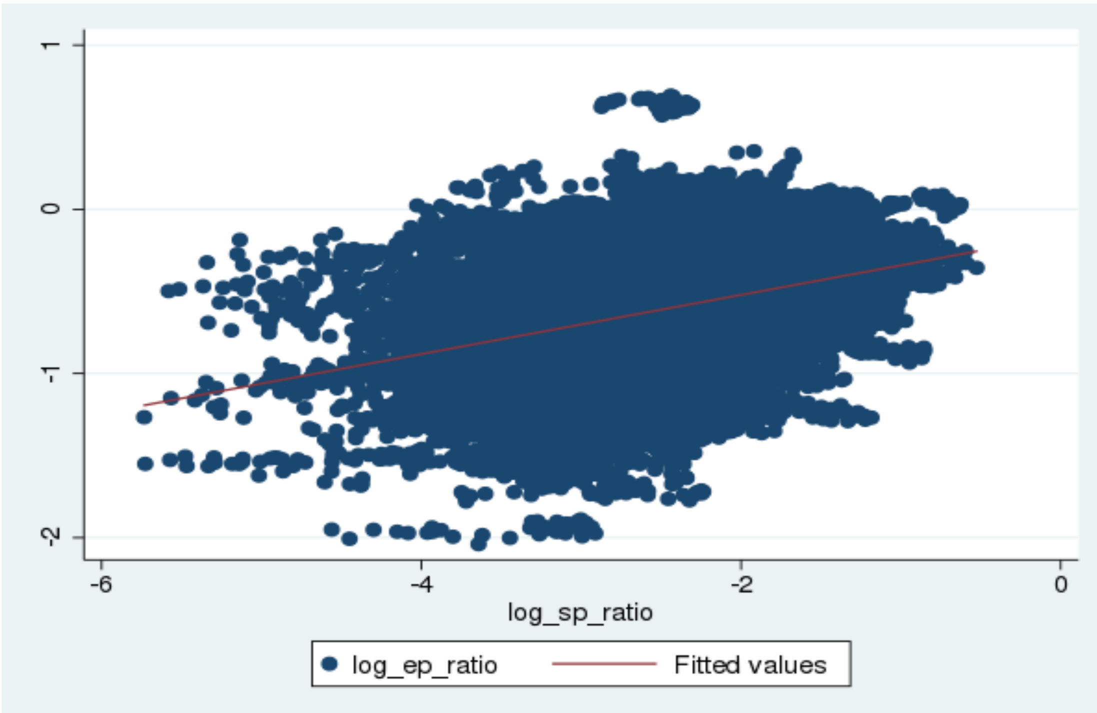
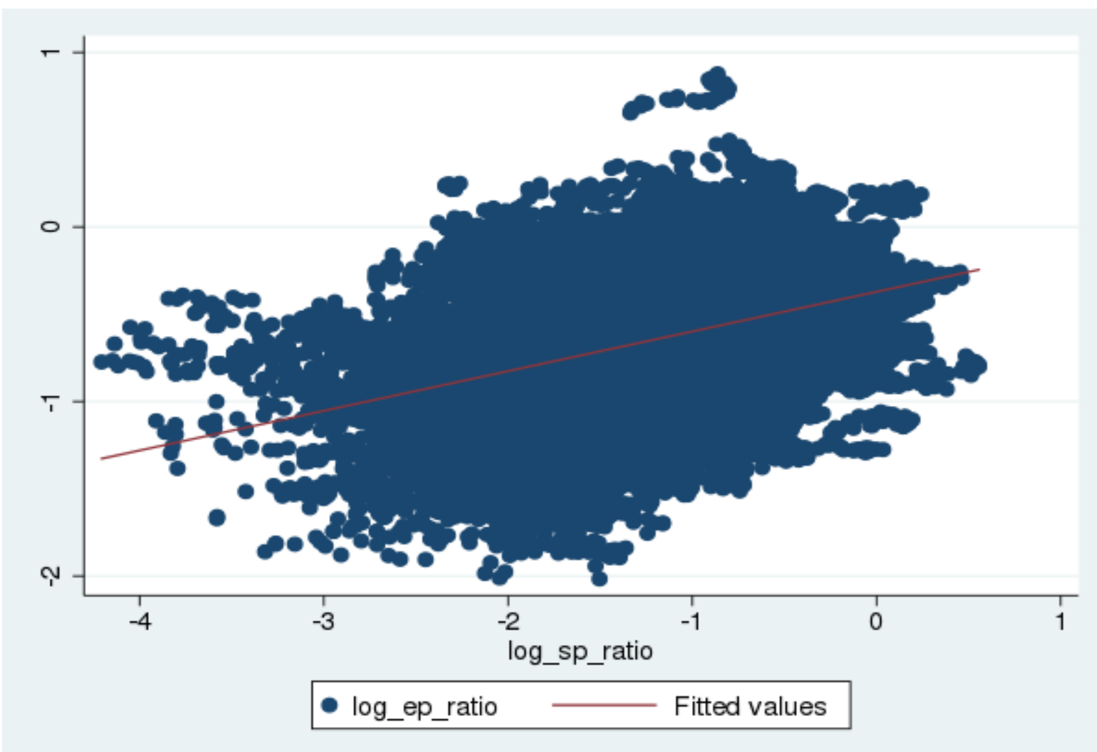


Figure 4: Log(Employment to Population) vs. Log (Scripts per Capita),

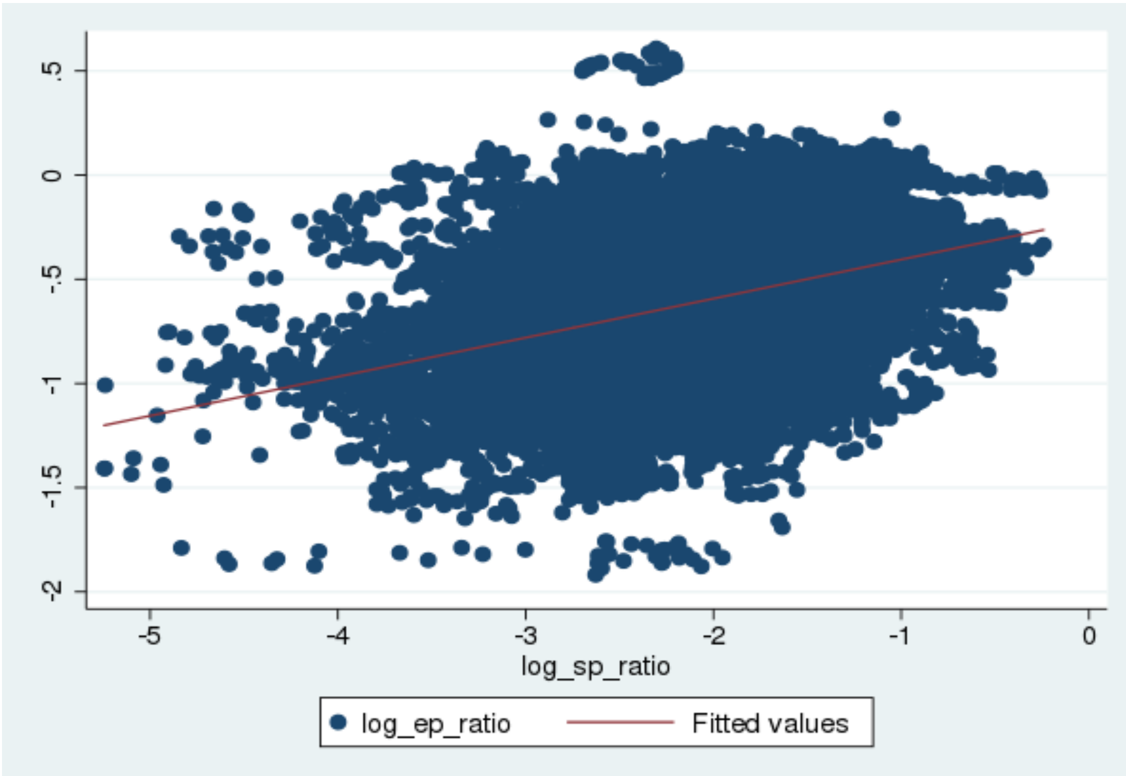
a) **Males 25-44**



b) **Males 45-64**



c) Females 25-44



d)Females 45-64

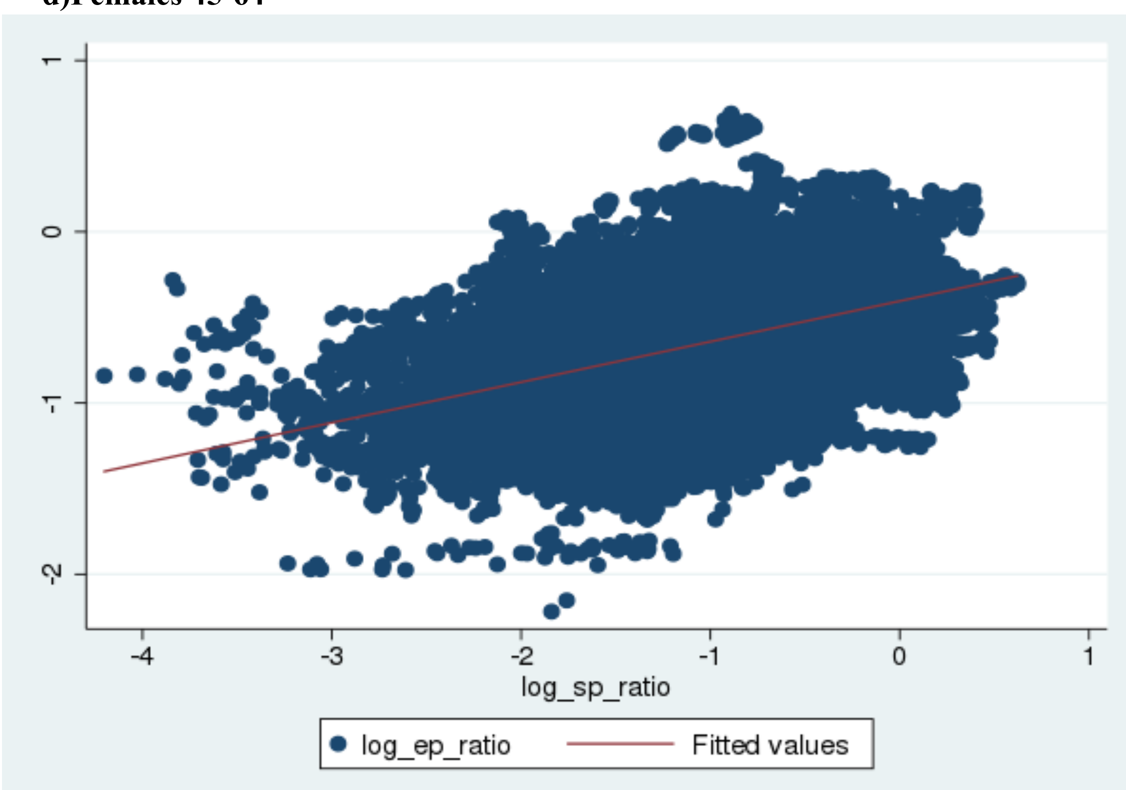


Table 1: Mean Employment-to-Population and Prescriptions Rates

Mean Employment-to-Population Ratio

	Age	All Counties	High-Education Counties	Low-Education Counties
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 Per-Capita Prescriptions

	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: High-education counties are those that are above the median in terms of the fraction of the population with a more than a high school degree. Low-education counties are those that are below the median.

Table 2: Effects of Opioid Prescriptions on Employment

(Log(Employment-to-Population) on Log(Per-Capita Opioid Prescriptions)

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(Per-Capita Opioids)	.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(Per-Capita Opioids)	.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(Per-Capita Opioids)	.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(Per-Capita Opioids)	.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:

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 3: Effects of Opioid Prescriptions on Employment

(Log(Employment-to-Population) on Log(Per-Capita Opioid Prescriptions)

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(Per-Capita Opioids)	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(Per-Capita Opioids)	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(Per-Capita Opioids)	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(Per-Capita Opioids)	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:

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: Effects of Employment on Opioids
(Log(Per-Capita Prescriptions) 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
1. 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
2. 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
3. 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
4. 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:
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 5: Effects of Employment on Opioids
(Log(Per-Capita Prescriptions) 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
1. 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
2. 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
3. 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
4. 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:
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 6: Effects of Opioids on Employment Controlling for Percent Insured
Log(Employment-to-Population) on Log(Lagged Per-Capita Opioids)

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(Per-Capita Opioids)	.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(Per-Capita Opioids)	.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(Per-Capita Opioids)	.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(Per-Capita Opioids)	.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:
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 7: Effects of Opioid Prescriptions on Employment Controlling for Percent Insured
(Log(Per-Capita Prescriptions) 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
1. 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:

All regressions include year 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.

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-to-Population); Mean = .664 (QWI), .666 (QCEW)						
Log(Per-Capita Opioids)	0.011*	0.011*	0.015*	0.014	0.010*	0.009*
	(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(Per Capita Prescriptions); Mean = .286						
Log(Emp.-to-Population)	0.158**	0.166**	0.203**	0.220*	0.137*	0.141
	(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.

Appendix Table 4: First Stage Regressions

Log(Lagged Opioids per Capita) on Log(Lagged Opioids per Capita, ages 65+)

	(1) High-Ed. Counties-IV	(2) Low-Ed. Counties-IV	(3) High-Ed. Counties-IV	(4) Low-Ed. Counties-IV
County Fixed Effects:	No	No	Yes	Yes
1. Females, 18-44				
Log(Per Capita Prescriptions, ages 65+)	.875***	1.113***	1.122***	1.065***
	(.048)	(.072)	(.055)	(.059)
R^2	0.735	0.699	0.991	0.994
2. Females, 45-64				
Log(Per Capita Prescriptions, ages 65+)	.965***	1.060***	.997***	.918***
	(.025)	(.027)	(.035)	(.031)
R^2	0.889	0.869	0.997	0.998
3. Males, 18-44				
Log(Per Capita Prescriptions, ages 65+)	.806***	.953**	1.148***	1.083***
	(.046)	(.053)	(.046)	(.056)
R^2	0.701	0.593	0.991	0.994
4. Males, 45-64				
Log(Per Capita Prescriptions, ages 65+)	.926***	.991***	1.017***	.961***
	(.025)	(.026)	(.031)	(.032)
R^2	0.879	0.828	0.997	0.998
N	10,142	10,278	10,142	10,278

Notes:

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.

Appendix Table 5: First Stage Regressions

Log(Employment to Population Ratio) on Log(Bartik Instrument)

	(1) High-Ed. Counties-IV	(2) Low-Ed. Counties-IV	(3) High-Ed. Counties-IV	(4) Low-Ed. Counties-IV
County Fixed Effects:	No	No	Yes	Yes
1. Females, 18-44				
Log(Bartik Employment-to-Population Ratio)	.922***	.897***	.807***	1.300***
	(.027)	(.014)	(.155)	(.224)
R^2	0.933	0.944	0.995	0.999
2. Females, 45-64				
Log(Bartik Employment-to-Population Ratio)	.934***	.937***	0.265	1.659***
	(.014)	(.014)	(.234)	(.249)
R^2	0.947	0.947	0.999	0.999
3. Males, 18-44				
Log(Bartik Employment-to-Population Ratio)	.924***	.904***	.941***	.721***
	(.024)	(.018)	(.117)	(.156)
R^2	0.921	0.922	0.999	0.999
4. Males 45-64				
Log(Bartik Employment-to-Population Ratio)	.945***	.938***	.631***	.787***
	(.016)	(.016)	(.158)	(.180)
R^2	0.947	0.946	0.999	0.999
N	10,159	10,278	10,142	10,278

Notes

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.

Appendix Table 6: First Stage Regressions

Log(Per Capita Prescriptions) on Log(Lagged Per-Capita Prescriptions, ages 65+)

	(4) High-Ed. Counties-IV	(5) Low-Ed. Counties-IV	(4) High-Ed. Counties-IV	(5) Low-Ed. Counties-IV
County Fixed Effects:	No	No	Yes	Yes
1. Females, 18-64				
Log(Per Capita Prescriptions, ages 65+)	1.008***	.937***	0.159	2.231***
	(.050)	(.047)	(.498)	(.465)
R^2	0.995	0.997	0.999	0.999
2. Females, 18-64, Controlling for %insured				
Log(Per Capita Prescriptions, ages 65+)	1.006***	.937***	0.109	2.169***
	(.049)	(.046)	(.498)	(.468)
%insured	-.372**	.007	.165**	.249***
	(.180)	(.191)	(.080)	(.073)
R^2	0.995	0.997	0.999	0.999
3. Males, 18-64				
Log(Per Capita Prescriptions, ages 65+)	1.029***	.965***	0.636	.608*
	(.043)	(.045)	(.394)	(.364)
R^2	0.996	0.997	0.999	0.999
4. Males, 18-64, Controlling for %insured				
Log(Per Capita Prescriptions, ages 65+)	1.029***	.969***	0.572	0.523
	(.043)	(.045)	(.385)	(.372)
%insured	-.026	.166	.266***	.281***
	(.178)	(.168)	(.096)	(.088)
R^2	0.996	0.997	0.999	0.999
N	2780	2805	2,784	2,801

Notes:
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.