The Opioid-Labor Dynamic: Examining the Effect of Prescription Opioids on Labor Force Participation Among Prime-Age Workers

David Kim

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Abstract

Linking state-level opioid prescription rates to individual-level weighted observations from the CPS, I estimate the effects of prescription opioids on labor force participation. From 2006 to 2012, when opioid prescription rates increase by 8.9 percentage points, a negative, significant effect on labor force participation among the prime-age population is estimated. However, in contrast to the early literature surrounding prescription opioids and labor market outcomes, I find that opioids have a positive, significant effect during periods of declining prescription rates. From 2006 to 2017, I estimate that the 13.7 percentage point decrease in opioid prescribing rates is responsible for 22% of the observed decline in the labor force participation rate within the prime-age population. Accounting for potential endogeneity by exploiting plausible exogenous variation in the differences between cross-state prescribing behavior, I similarly find that the model estimates a significant, positive effect for the general prime-age working population. These findings suggest that when prescription rates are at a healthy, normalized level, prescription opioids can have a positive effect on labor force participation among the prime-age population.

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1 Introduction

Little is known about the impact of the opioid crisis on the labor market. Evidence from early literature that specifically examines the relationship between opioid prescriptions and labor market outcomes strongly points to a significant, negative relationship. Krueger (2017) estimates that a 3.5 factor growth in prescription opioids from 1999 to 2015 is responsible for about 43% of the decline in labor force participation for prime-age males, and about 70% of the decline for prime-age females. Harris et al. (2019) examine county-level labor market outcomes for the general population and find that a ten percent increase in prescription opioids causes a 0.56 percentage point reduction in labor force participation.

Linking a panel of state-level prescription rate data to weighted individual-level observation data, I estimate the effect of prescription opioids on labor force participation by age and gender. In addition, to account for potential endogeneity with prescription rates and labor market forces, I employ a two-stage IV regression model, using the concentration of heavy prescribers as exclusion restrictions. From 2006 to 2017, prescribing rates experience a net decrease of 13.7 percentage points. During this time period, I find that prescription opioids have a positive, significant effect on labor force participation. I estimate that the decline in opioid prescribing rates is responsible for 7.9% of the decline in the labor force participation rate for prime-age males, 51% for prime-age females, and 22% for the general prime-age population.

These results are unexpected and contradict findings from the early literature, which consistently identifies adverse effects on labor market outcomes from prescription opioids, even in periods of declining prescribing rates. My results suggest that opioid prescriptions, when prescribed at appropriate levels, can have a positive effect on labor force participation. Conversely, these results suggest that when prescribing rates are at an appropriate level, decreasing opioid prescriptions can have a depressing effect on labor force participation. Interestingly, the models, estimate a positive effect of opioids on participation rates only when incorporating state and year fixed effects. When fixed effects are excluded, the model consistently generates negative, significant effects of opioids on labor. I theorize that including fixed effects dilutes the adverse effects of high-opioid states, and correspondingly estimates a clearer effect on the aggregate level. To test my theory, I drop the top ten states with the highest prescription rates in 2012 from the model and rerun the regressions. Every model variant subsequently estimates both a positive and significant effect of opioids on labor. These results align with the rationale that the concentration of high opioid states bias the estimates, unless state and year fixed effects are included.

The rest of the paper proceeds as follows. Section 2 provides background on current labor and opioid trends. Section 3 reviews relevant literature and delineates the original contribution of my study to the existing body of literature. Section 4 describes the construction of the dataset, and illustrates preliminary patterns and trends with summary statistics tables, graphs, and figures. Section 5 outlines the methodology, including model regression equations, calculations, and explanations for specific methodological choices. Section 6 displays the results and corresponding analysis and discussion. Section 7 concludes the paper with the study's limitations, implications, and future extensions for the study of prescription opioids on labor market outcomes.

2 Background

2.1 Labor Force Trends

Metrics that assess the robustness of the economy in regards to the labor market include the unemployment rate, employment-to-population ratio, and the labor force participation rate. These various indicators provide unique views on the strength of the labor market, and when pieced together, can offer meaningful insight. For instance, a number of provocative trends recently emerging within the U.S. labor market and across certain demographics has caught the attention of government agencies, economists, and policymakers.

In September 2018, according to the U.S. Department of Labor, the unemployment rate dropped to 3.7%, hitting its lowest level since 1969. Employers added 134,000 jobs to payrolls, a record 96th straight month of gains (Morath & Torry, 2018). Historically, unemployment rates below 4% are very rare.[1] Today, with current unemployment so low, some businesses are having to adjust their hiring strategies in order to source labor for high-skilled jobs, and even for entry-level jobs. The Wall Street Journal reports that "unfilled jobs in the U.S. exceeded the number of unemployed Americans by more than one million as the summer came to a close" (Morath, 2018). In other words, there were more available jobs than unemployed Americans looking for work last September.

Taking a closer look at the current labor shortage, one troubling trend is the declining labor force participation among the prime-age working population. Key findings from a report from the Economic Innovation Group regarding this

[1] Morath and Torry (2018) note that very low unemployment rates are observed only during periods of strong economic growth, coinciding with the enlistment of young men from the civilian labor force during periods of war.

particular age group note that from 2007 to 2017, half of U.S. states lost prime working age adults, and that 80% of U.S. counties lost prime working age adults. Throughout the next decade, 65% of counties will continue losing prime working age adults (Fikri & Ozimek, 2019). The report illustrates more troubling forecasts: by 2037, two-thirds of U.S. counties will contain fewer prime working age adults than they did in 1997, despite adding 24.1 million prime working age adults and 98.8 million people in total over that same period (Fikri & Ozimek, 2019).[2]

Even within the prime-age working population, there have been perplexing movements when cutting the labor force data by gender and age groups. Employment data are typically divided in three strata, grouped by age: 15-24, 25-54, and 55-64 years old. The middle range, 25-54, is conventionally described as the prime working age. Men within this prime working age, more specifically millennial men aged 25-34, in perhaps the most physically-robust time of their lives, are seeing historically low rates of participation in the labor force. This troubling phenomenon presents an intriguing element when contrasting against comparable demographic groups. For instance, women in the same age group have caught up to their older counterparts age 35-44, whereas these young men consistently lag behind their respective counterparts. Consequently, the causes behind the declining labor force participation rate within the prime-age population have become a salient point of focus for labor economists and policymakers.

2.2 The Opioid Crisis

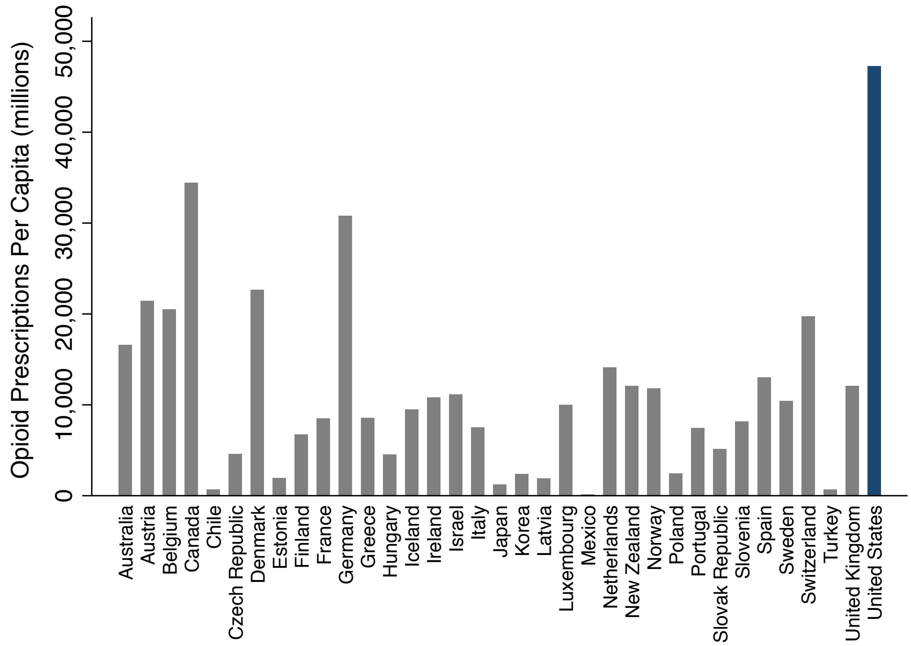
One of the biggest health challenges that the United States currently faces is the opioid epidemic, commonly referred to as the opioid crisis. Although opioid pain

[2] For more key findings regarding population loss and shifting demographics, see page 3 in https://eig.org/wp-content/uploads/2019/04/Heartland-Visas-Report.pdf.

relievers are generally safe when taken for a short time, regular use, even as prescribed by a doctor, can lead to dependence: when misused, opioids can ultimately cause addiction, overdose incidents, and deaths (National Institute on Drug Abuse). The increase in prescription and illicit opioids has rapidly escalated the use of healthcare services, driving substantial increases in emergency consultations and hospitalizations, and propelling the number of overdose deaths to alarming numbers (OECD). The opioid crisis is not unique to the United States; Figure 2 reveals that compared to other countries in the Organization for Economic Cooperation and Development (OECD), the U.S. outranks its members in the total number of opioid prescriptions.

Drug overdose deaths are now more responsible for deaths each year than car crashes, gun violence, or HIV/AIDS (Lopez, 2019). The Centers for Disease Control and Prevention (CDC) reports that overdose deaths are in part to blame for the drops in annual life expectancy in the past three years (CDC). On October 26, 2017, President Trump declared the opioid crisis a national Public Health Emergency under federal law, effective immediately. Since President Trump took office, more than $1 billion in funding has been allocated or spent directly addressing the drug addiction and opioid crisis (White House).

Figure 2.1: Total Opioid Prescriptions in the OECD



The National Center for Health Statistics outlines alarming trends in the data regarding drug overdose deaths.[3] For all drug overdose deaths, the age-adjusted rate increased from 6.1 in 1999 to 21.7 in 2017 per 100,000 population. Specifically regarding opioids from 1999 to 2017, "almost 218,000 people died in the United States from overdoses related to prescription opioids; overdose deaths involving prescription opioids were five times higher in 2017 than in 1999" (CDC). Consequently, as awareness regarding drug overdose deaths and the opioid crisis has heightened, states have taken more aggressive steps to strengthen regulation surrounding prescription pain medication.

State-by-state initiatives that implement policies to improve regulation around controlled substances include legalizing marijuana, instituting day supply limits to written prescriptions for opioids, requiring substance abuse disorder assessments prior to opioid prescription, mandating ID checks for pharmacists before dispensing prescriptions, and continuing medical education for clinicians who prescribe

[3] For more relevant statistics, see https://www.CDC.gov/opioids

controlled substances.[4] States are continually sourcing funding for drug prevention programs like PDMPs (Prescription Drug Monitoring Programs) that help reform prescription prescribing behavior. These policies may have an edifying impact on prescribing practices, as we observe substantial declines in annual U.S. prescription rates starting from 2012, with a 22.6 percentage point decrease in the total prescribing rate from 2012 to 2016. Overall prescribing data suggest that some prescribing practices improved nationwide in 2017. However, opioids are still the main driver behind drug overdose deaths—as the data suggest, despite the improvements in prescribing practices, there are still significant increases in drug overdose deaths, including deaths from opioid abuse.

2.3 Socioeconomic Impact: Costs of Opioid Abuse

Shifting demographics, the effects of the opioid crisis, and the interaction between these moving forces are not without its socioeconomic costs. As Ozrimek and Fikri (2019) find, population loss perpetuates economic decline, with "deleterious effects on housing markets, local government finances, productivity, and dynamism." Ozrimek and Fikri (2019) also estimate that a one percentage point decline in a county's population growth rate is associated with a two to three percentage point decline in its startup rate over the past decade.

As we just observed with significantly increasing drug overdose mortality rates, opioid abuse bears consequences not only to the individual, but also collectively to the broader economy. Figure 2.2 illustrates a clear negative correlation between opioid prescription rates and labor force participation rates for 2012. As the opioid prescribing rate per 100 persons increases, the labor force participation rate

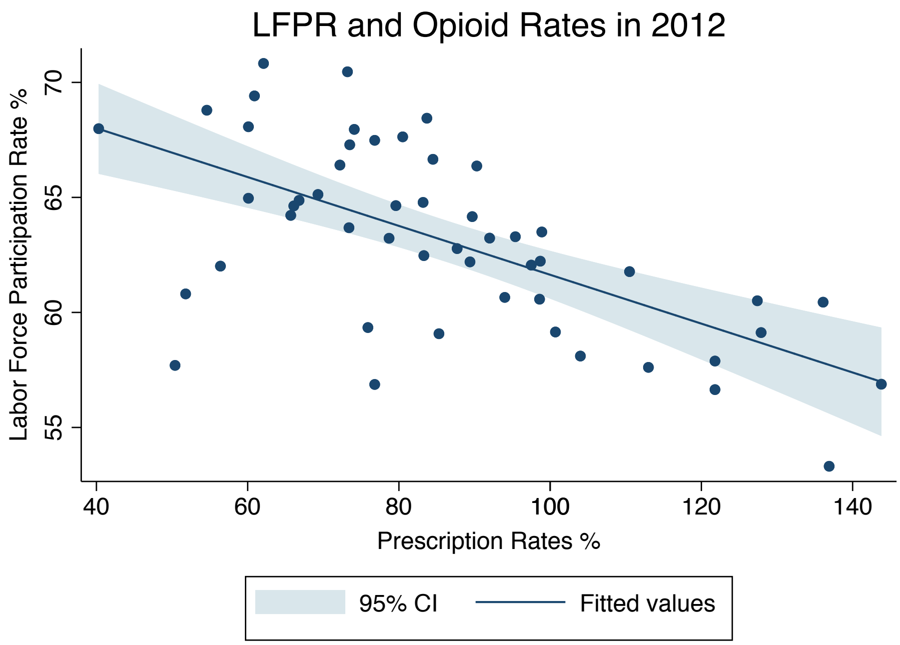
[4] To see which states opted in these policies, see https://www.athenahealth.com/insight/infographic-opioid-regulations-state-by-state.

decreases. Unsurprisingly, this troubling relationship incurs economic costs that have severe, potential ramifications.

Birnbaum et al. (2011) measure the costs of opioid abuse and dependence from an employer perspective by analyzing administrative claims data. Employers who pay a substantial portion of healthcare costs for their employees through company-sponsored insurance are inevitably affected by employee disability and absence in the workplace. Birnbaum et al. (2011) estimate an annual economic burden of $55.7 billion from opioid abuse, with nearly half of that burden stemming from workplace costs. White et al. (2005) find that the average per-patient annual healthcare costs of commercially insured patients diagnosed with abuse exceeded those of a comparison population by over $20,000 from 2003 to 2007. Cummings et al. (2014) specify their estimates among commercially insured patients from 2006 to 2012, examining the excess healthcare and work-loss costs from prescription opioid abuse. The study finds that relative to the comparison patients used as controls, abusers had significantly higher annual healthcare resource utilization, leading to $10,627 in per-patient incremental annual healthcare costs. Additionally, abusers had $1,244 in excess annual work-loss costs (Cummings et al., 2014).[5]

[5] Under federal law, programs like the Emergency Medical Treatment and Labor Act (EMTALA) can add additional strain for hospitals in terms of high emergency room visits costs. Through EMTALA, anyone seeking treatment at an emergency department must be treated, regardless of insurance status or ability to pay; since its enactment in 1986, this program remains an unfunded mandate (American College of Emergency Physicians).

Figure 2.2: Labor Force Participation Rate and Opioid Prescription Rates by State, 2012



From the payer perspective, Oderda et al. (2015) find that costs for abusers range from $23,000-$25,000 per year. For societal costs including criminal justice expenses, Oderda et al. (2015) estimate that expenditures and lost productivity costs are in excess of $50 billion per year due to prescription opioid abuse and misuse in the United States. Florence et al. (2013) examine the economic burden of prescription opioid overdose, abuse, and dependence from a societal perspective, and estimate the total economic burden to be about $78.5 billion in just 2013 alone. A study by the Council of Economic Advisers projects the total, overall cost of the epidemic in 2015 at $504 billion, a remarkable 2.8 percent of GDP.

Evidently, the prevalence of abuse, dependence, and misuse of opioids presents serious societal and economic costs. And while many studies have focused on the effects of the opioid epidemic on the economy and mortality rates, there is little literature available regarding the epidemic's effect on labor market outcomes—a relationship my paper seeks to quantify.

3 Literature Review

3.1 Prime-Age Labor Force Participation

The persistent decline in labor force participation among the prime-age population has led economists to test various hypotheses for the driving forces behind the decline. Likewise, the prevalence of prescription opioid abuse nationwide has galvanized government agencies and researchers to examine prescription opioids' effects on a variety of elements, ranging from pregnancies to economic costs. Yet, only a limited amount of research has been invested in studying the opioid-labor dynamic. Early literature that seeks to determine a causal relationship between opioids and labor trends finds that prescription opioids have a strong, negative effect on labor market outcomes.

One of the earliest studies that addresses the opioid epidemic and its effect on labor force participation first takes into account health barriers and the use of pain medication. Krueger (2017) finds in his Princeton Pain Survey that nearly half of prime age men not currently in the labor force take pain medication on a daily basis, and that in nearly two-thirds of these cases, these men take *prescription* pain medication. In the same vein, Krueger (2017) discovers that in areas where relatively more opioid pain medication is prescribed, labor force participation declines even further, demonstrating that the opioids and labor force participation are geographically linked.

Diving into prescription opioid data from the CDC, the difference between the lowest and highest prescription rate among states is close to, or over 100 percentage points. For example, in 2009, the District of Columbia has a prescribing rate of 34.5% per 100 persons, whereas West Virginia has a prescribing rate of 146.9% per

100 persons, for a difference of 112.4 percentage points.[6] The differences in states' prescription rates establish a clear regional pattern to opioid prescription rates and drug overdoses: evidence from his study demonstrates that labor force participation rate is lower and declines more heavily in areas that have higher levels of opioid prescriptions. These results remain consistent with the CDC's assertion that variation among prescribing practices influence pain medication use, rather than the differences in prevailing health issues across areas.

Finally, to estimate the effect of an increase in opioid prescriptions on labor force participation, Krueger assumes exogeneity within cross-county differences in opioid prescription rates as a result of differences in medical practices, controlling for demographic characteristics and regional variation. In other words, in his labor force regressions, the estimated coefficient of the opioid prescription rate reflects inherent differences across regions, and the interaction between prescriptions and time captures the effect of changes in prescriptions on labor force participation over time (Krueger, 2017). Ultimately, Krueger finds that the increase in opioid prescriptions from 1999 to 2015 accounts for a 1.4 percentage decline in prime-age male labor force participation, suggesting that opioids are responsible for about 43 percent of the estimated decline during that period. Applying the same methodology for women, the growth in opioid prescriptions accounts for about 70% of the estimated decline in female labor force participation during that time period.

[6] For West Virginia, an annual prescribing rate of 146.9% per 100 persons in 2012 indicates that there were about 147 opioid prescriptions for every 100 persons for that year.

3.2 Duality in the Arrows of Causality

There are some caveats that Krueger (2017) acknowledges about his labor force regressions: namely, omitted variable bias such as demand for opioids driven by worker's health conditions and associated pains, which would be correlated with opioid prescription rates. Abraham and Kearney (2017) recognize that while the problems of depressed labor force participation and opioid use are interrelated, the arrows of causality run in both directions. In other words, the methodology potentially suffers from reverse causality. For example, Abraham and Kearney (2017) suggest that weak labor market prospects and a corresponding sense of economic despair might drive people to opioid use. Case and Deaton (2017) touch upon the idea that economic despair first leads people to pain relievers, and that subsequent addiction acts more swiftly than heavy drinking and smoking. As such, Abraham and Kearney (2017) assert that it remains an open question as to how much opioids and labor market forces have driven each other.

Indeed, instead of examining the effects of opioids on the labor market, Gascon & Spiller (2009) focus on finding a relationship between the unemployment rate and the rate of opiate exposure. Now, the arrows of causality run in the opposite direction of Krueger's study. Citing Kentucky as one of the states with the highest abuse rate for these substances, the authors use data from the Census Bureau, Department of Labor, and the Kentucky Regional Center to analyze unemployment and opioid abuse from 2000 to 2005 on the county level. In three of the six years, Gascon & Spiller (2009) find a significant, positive relationship between unemployment rates and opiate exposure rates. In counties with higher unemployment, the mean opiate exposure rate was higher than that of counties with lower unemployment, demonstrating that labor market forces can affect opioid exposure rates. Interestingly, the largest negative correlation was observed in 2004, the only year when unemployment decreased statewide. Nonetheless, the nature of the study focuses on how unemployment affects opioid exposure, the opposite direction in which Krueger (2017) examines the opioid-labor relationship.

Hollingsworth et al. (2017) analyze how deaths and emergency department visits related to opioid analgesics use varies with short-term fluctuations in macroeconomic conditions, looking at the unemployment rate as the lone neutral variable. According to Ruhm (2000), when the economy strengthens, smoking, obesity, and unhealthy diets increase, whereas physical health improves when economic conditions temporarily deteriorate. Given the severity of the opioid epidemic, Hollingsworth et al. focus primarily on opioids, as opioids comprise the majority of drug overdose deaths and are quite possibly the most sensitive to macroeconomic conditions, Additionally, economic conditions and severe adverse drug outcomes are separately examined for whites, blacks, and Hispanics, based on evidence from Case and Deaton (2015) that changes in mortality rates vary by race.

Hollingsworth et al. (2017) find that a one percentage point increase in the county unemployment rate leads to a 3.6 percentage point increase in the opioid death rate per 100,000 capita, and a 7.0 percentage point increase in the opioid overdose ED visit rate per 100,000 capita. In most estimates, these effects are largely driven by changes in the death rates of whites with much smaller, positive increases predicted for Hispanics. These findings remain relatively consistent regardless of the time period observed, indicating an anti-cyclical relationship where economic conditions and severe adverse consequences of substance abuse are not restricted to periods of recession. Ultimately, Hollingsworth et al. (2017) suggest that with the increased availability of prescription opioids, the consumption of these drugs rises when economic conditions worsen, which leads to an increase in adverse outcomes such as high emergency department visits, or even death.

The direction of this relationship, which focuses on the effect of unemployment on opioid related mortality deaths and ED visits, is in the complete reverse direction to that of Krueger's (2017) study. Gascon & Spiller (2009) similarly explored the relationship between the unemployment rate and the rate of opiate exposure in Kentucky. Juxtaposing these studies side by side, I observe that the arrows of causality indeed run in both directions, as Abraham and Kearney (2017) assert. I acknowledge the issue of endogeneity, which in applied econometrics, usually arises in one of three ways, including omitted variable bias, measurement error, and simultaneity. In my case, I would observe simultaneous causality bias, which arises when at least one of the explanatory variables is determined simultaneously along with the outcome or dependent variable (Woolridge, 2002). Based on these studies, it seems that while opioid rates affect labor force participation, similar labor force outcomes such as the unemployment rate also affect opioid related results. This complicates the analysis as it introduces potential endogeneity, requiring exogenous variation within the explanatory variable (Woolridge, 2002).

Likewise, Harris et al. (2019) are concerned with issues of endogeneity between prescription opioids and labor market outcomes. Opioid prescriptions may be correlated with unobserved factors that also impact labor market outcomes, where counties experiencing declining labor market conditions might also experience worsening population health (Harris et al., 2019). In addition, the authors recognize the prospect of simultaneous causality bias, noting Hollingsworth et al. (2017). To establish a causal effect of prescription opioids on county labor market outcomes, Harris et al. (2019) exploit plausibly exogenous variation in the concentration of high-volume prescribers as exclusion restrictions, implementing the instrumental variables approach to combat simultaneity bias as Woolridge (2002) proposes. Using variation in provider-prescribing behavior as an instrument to achieve causal identification and combat simultaneity concerns aligns with the literature regarding variability in prescription claims.

Doctor and Menchine (2017) allude to policies attempting to avoid over-prescription while preserving autonomy of medical practices; Satel (2017) denotes the history of establishing the standards for pain management: first, how physicians interpreted those standards to encourage the use of opioids, and how aggressive marketing from pharmaceutical companies compounded the issue. Laird and Nelson (2017) employ plausible exogenous variation in physician practices, and estimate a slight decrease in labor force participation from opioids in Denmark. On the issue of physician-contributive abuse, Currie and Schnell (2017) examine the role of physician education on the opioid epidemic, finding that physicians trained at the lowest ranked US medical schools prescribe nearly three times as many opioids per year as physicians trained at the top medical school.[7] The results are striking—Currie and Schnell (2017) estimate that if all general practitioners mirrored the prescribing behavior of physicians from the top ranked schools, there would have been 56.5% fewer opioid prescriptions and 8.5% fewer deaths from 2006 to 2014. Thus, the large deviations in prescriptions that stem from heterogenous physician prescribing behavior justify the approach of exploiting exogenous variation in provider practices to instrument for endogeneity.

The instrument framed in Harris et al. (2019) is the concentration of high-volume opioid prescribers per capita in a given county. Taking data from prescription drug monitoring program (PDMP) databases of ten U.S. states from 2010 to 2015, county labor market data from the Bureau of Labor Statistics (BLS), and merging data on opioid prescriptions by physician from the Medicare Part D summary claims database, Harris et al. (2019) identify the number of heavy

[7] 18 states, including Kentucky and West Virginia, require continuing medical education for clinicians who prescribe controlled substances.

prescribers in each county in the nation's top-five percent of opioid prescriptions written and the nation's top-one percent of doses prescribed. Research from Betses & Brennan (2013) and Daubresse et al. (2017) uses the top-five percent as the benchmark to define high-volume prescribers. The top one-percent prescribers by dosage is a secondary measure of even heavier prescribers (Harris et al., 2019). The intuition behind the authors' identification strategy revolves around the idea that individuals who live in close proximity to high-volume prescribers face lower time and transportation costs, and encounter less difficulty in accessing prescription opioids within family and social circles (Harris et al., 2019). Consequently, labor markets in counties with a higher number of heavy prescribers would naturally be more affected.

The results from the study align with early findings from Gascon & Spiller (2009), albeit with the opioid rates and labor market outcomes reversed: prescription opioids have the strongest measurable adverse effects in counties with higher labor force participation rates and employment-to-population ratios (Harris et al., 2019). In particular, Harris et al. (2019) find that a ten percent increase in prescriptions caused a 0.56 percentage point reduction in labor force participation. Second, although prescription opioids have a strong negative effect on employment-to-population ratios and the labor force participation rate, the study found only a marginal adverse effect on the unemployment rate, suggesting that prescription opioids are primarily leading people to exit the labor force entirely (Harris et al., 2019).

Lastly, the study tested whether the results were driven by the counties with unusually high opioid prescription rates by excluding all counties from the three states with the highest average per capita opioid prescriptions. Interestingly, the results strengthened when high-opioid states were excluded. In other words, in areas with lower levels of per capita opioid prescriptions, the increase in per capita opioid prescriptions impacted labor markets more adversely. This finding is provocative, as it implies that perhaps in high-opioid states, most of the proverbial damage to the labor market from opioid abuse has already been inflicted (Harris et al., 2019).

3.3 Contribution to the Literature

Most of the literature surrounding the consequences of the opioid crisis revolve around mortality rates, health outcomes, and socioeconomic costs. Not much research has focused on the opioid epidemic's impact on labor market outcomes. Krueger (2017) takes a bottom-up approach to pain medication and examines the repercussion of prescription opioids on the labor force participation rate among prime age workers, separating the effects by gender. Harris et al. (2019) look at all three major indicators of the labor market, employing an instrumental variables approach to account for potential endogeneity. My paper contributes to the existing body of literature by taking the most effective elements from the current literature, and addressing their limitations through a more comprehensive dataset and adjusted empirical methods.

The initial empirical approach of my study will replicate the methodology Krueger (2017) employed in his labor force regressions, the linear probability model. At the time of his study, due to the lack of availability on prescription opioid data, Krueger (2017) incorporated interaction terms and manipulated the regression estimates to deduce the impact of prescription opioid abuse on labor force participation rate. For instance, Krueger only had 2015 county-level opioid prescription rates (MME per capita). However, much of prescription rate data is now public and available from a multitude of platforms and services, allowing more flexibility with the observed time periods and possibilities for stratified analysis among various demographics. In addition, endogeneity concerns such as simultaneous causality bias were mentioned, but not directly addressed in the methodology. To combat those concerns, I then employ a two-stage instrumental variable approach, exploiting plausible exogenous variation in prescribing practices similar to that of Harris et al. (2019), thereby establishing a clearer causal link between prescription opioids and labor force participation.

While Krueger (2017), Harris et al. (2019), and Hollingsworth et al. (2017) use opioid data on the county level, my study examines prescription opioid data on the state level. There are a few advantages with using state-level prescription data instead of county-level data. Admittedly, as counties within the same state could face different economic climates as well as differences in public health funding, county-level data allow for a more granular estimate of opioids' effect on labor. At the same time, measurement error in prescription rates and labor market outcomes is more likely to be higher for smaller geographic units. CDC prescription rate data reflect this issue, where a complete dataset is available for all fifty states and the District of Columbia from 2006 to 2017, but the percent of available data fluctuates every year on the county level. In addition, Krueger (2017) notes in his regressions that in only "41 percent of observations, opioid prescriptions per capita could be matched directly at the county level; in 34 percent of observations we had to aggregate over counties to match at the metropolitan or central city level; and in the remainder of cases we used the average of counties in the balance of the state." While most of the observations from the CPS include state-level identification, about 60% of observations do not include county-level identifying information.

Harris et al. (2019) do not run into this issue as the bulk of their employment and opioid data come directly from states' Prescription Drug Monitoring Programs (PDMP) and Part D Medicare files. However, their study is a broad overview of labor market indicators, without stratified analysis by gender, race, or age due to the nature of the data. As I link my state-level prescription data to weighedt individual-level observation data, the empirical analysis can be extended to estimate opioids on labor for specific demographics. Given current labor market forces and the ongoing phenomenon within the prime-age working population, measuring the impact of the opioid epidemic for these demographics could aid further discussion in the policy realm. As a result, my paper seeks to extend the early literature and address some of the limitations, in order to provide additional meaningful insight into the impact of the opioid crisis on labor force participation.

4 Data

4.1 Data Sources

I begin constructing my dataset by linking state-level prescription rate data to weighted individual-level observation data. Individual-level data comes from the Current Population Survey (CPS). Due to the nature of sampling, although CPS gathers data mirroring the demographic composition of the U.S., the sample is still not fully representative of current demographic levels nationwide. As a result, the dataset includes a variable, WTFINL, that assigns a weight to each individual observation that must be used in analyses of the data for an accurate representation of the U.S. demographic environment. The weights make adjustments to each observation to closely reflect the known distribution of the entire population according to age, sex, race, and ethnicity; otherwise, a weight of zero is allotted to populations not sampled in other monthly surveys. As a result, the regressions are correspondingly weighted.[8]

Opioid prescription rates data was sourced from the Centers for Disease Control and Prevention, or CDC.[9] The CDC provides opioid prescription rates by state and county from 2006 to 2017. The percentage of available data for county-level prescriptions varies from 87.1% to 94.3%, while state-level prescription data is fully

[8] WTFINL is the second stage weight in CPS, based on the inverse probability of selection into the sample. For more information on the final weight variable, see https://cps.ipums.org/cps-action/variables/WTFINL#codes_section.

[9] CDC sources its opioid prescribing data from IQVIA Xponent, a sample of approximately 50,000 retail (non-hospital) pharmacies which dispense nearly 90% of all retail prescriptions in the United States.

available.[10] To match the time period of available prescription rate data from the CDC, I similarly extract CPS data from 2006 to 2017.

Combining these two datasets provides a great degree of flexibility in how the model estimates the effects of prescription opioids on labor market outcomes. Since core survey variables identify an individual's age, race, sex, and location, I can separate the model to estimate for specific demographic groups. For instance, the newly constructed dataset would theoretically allow me to single out the effects of opioids on young, married women ages 25-34 in California, or single, Hispanic men ages 80-90 in the South. However, scientific literature and simplicity of analysis point to stratifying the estimates by either gender or age.

4.2 Labor and Opioid Trends

Table 4.1 displays summary statistics for the prime-age population labor force participation rate from 2006 to 2017. I extract data for the participation rates from the BLS. BLS offers seasonally adjusted rates by monthly or quarterly time intervals. To capture less fluctuation and arbitrary bias in later estimates where I calculate the change in labor force participation between two time periods, I take the quarterly adjusted rates. Figure 4.1 illustrates the change in the labor force participation rate over time. I choose Q1 rates to represent the rate for each year. Participation rate drops to its lowest point in 2015, and begins increasing. This trend applies to both males and females, with a much lesser observed increase in participation rate for males than for females.[11]

[10] See Table A1 and A2 in the Appendix for the total number of prescriptions and the prescribing rate per 100 persons by year, and the number of counties with available data by year.
[11] Figure A1 depicts the labor force participation rate for prime-age males and females.

Table 4.1: Summary Statistics: Prime-Age Population Labor Force Participation Rates, 2006-2017

Year	Mean	Sd	Variance	Range	Min	Max
2006	82.9	0.0816	0.0066	0.2	82.8	83
2007	83	0.2	0.04	0.4	82.9	83.3
2008	83.075	0.1258	0.0158	0.3	82.9	83.2
2009	82.675	0.2629	0.0691	0.6	82.3	82.9
2010	82.175	0.2629	0.0691	0.5	81.9	82.4
2011	81.6	0.1154	0.0133	0.2	81.5	81.7
2012	81.425	0.05	0.0025	0.1	81.4	81.5
2013	81	0.1414	0.02	0.3	80.8	81.1
2014	80.925	0.05	0.0025	0.1	80.9	81
2015	80.875	0.1258	0.0158	0.3	80.7	81
2016	81.325	0.15	0.0225	0.3	81.2	81.5
2017	81.675	0.0957	0.0091	0.2	81.6	81.8

Figure 4.1: Prime-Age Population Labor Force Participation Rates, 2006-2017

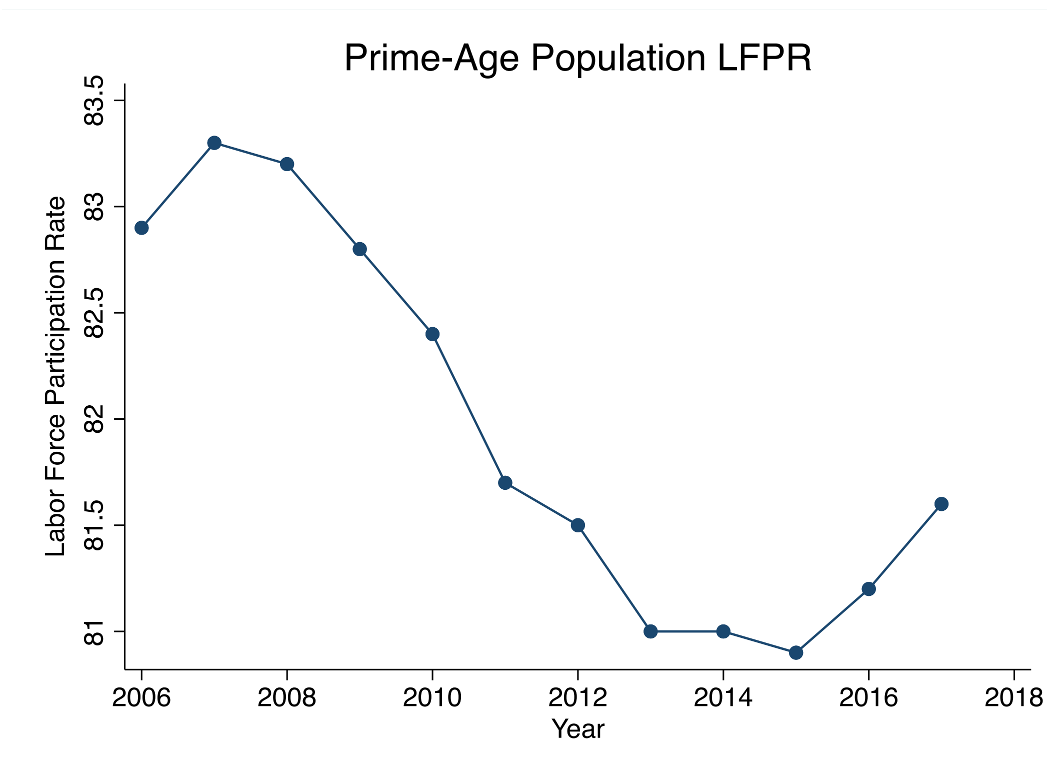


Table 4.2 displays summary statistics for state-level opioid prescription rates from 2006 to 2017. Figure 4.2 plots the national prescribing rate per one hundred persons by year.[12] Starting in 2006, the national prescription rate steadily increases until reaching its peak in 2012. From 2012 to 2017, prescription rates experience a substantial decline, a drastic 22.6 percentage point decrease in national prescribing rates. The difference in prescribing rates between these two time periods later serves as a pivot for estimating the effect of opioids in periods of growth vs. periods of decline.

Figure 4.3 shows a histogram of prescription rates for all states. Upon inspection, the distribution of the rates across the nation and throughout the time period look approximately normal, or slightly left skewed. Krueger (2017) takes the log of opioid rates in his labor force regressions, but taking the log of the rates transforms the distribution to be strongly right-skewed. Consequently, I assume a one-to-one relationship between opioids and labor force participation.

Table 4.2 Summary Statistics: Opioid Prescription Rates, 2006-2017

Year	Mean	Sd	Variance	Range	Min	Max
2006	76.64902	21.6405	468.3113	98.19	31.7	129.9
2007	80.17255	22.43967	503.5389	93.9	41.2	135.1
2008	82.67255	23.79845	566.3664	111	34.5	145.5
2009	83.66078	24.44883	597.7452	112.5	34.4	146.9
2010	85.55882	24.42708	596.6821	106	37.1	143.1
2011	85.4098	24.05258	578.5265	99.8	39.8	139.6
2012	86.18431	24.00489	576.235	103.5	40.3	143.8
2013	83.09608	23.00858	529.3948	101.3	41.1	142.4
2014	80.68824	22.03779	485.6643	95.1	40.1	135.2
2015	75.63725	20.1122	404.5004	89.3	35.7	125
2016	70.81765	19.37365	375.3383	88.5	32.5	121
2017	62.31765	17.64824	311.4603	78.7	28.5	107.2

[12] See Figure A2 in the Appendix for graphs on the maximum rate, minimum rate, and the range of rates by year.

Figure 4.2: National Opioid Prescribing Rate Per 100 Persons, 2006-2017

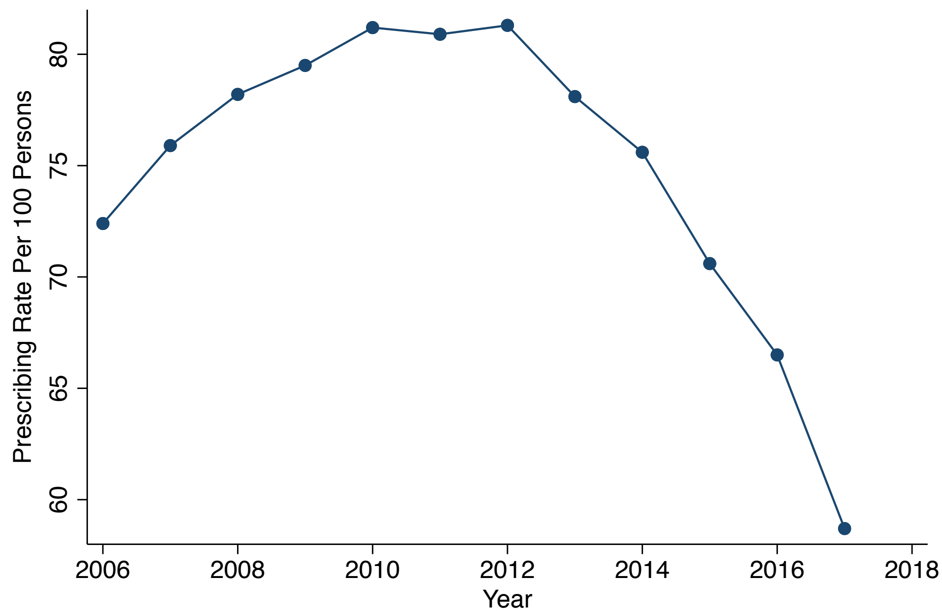
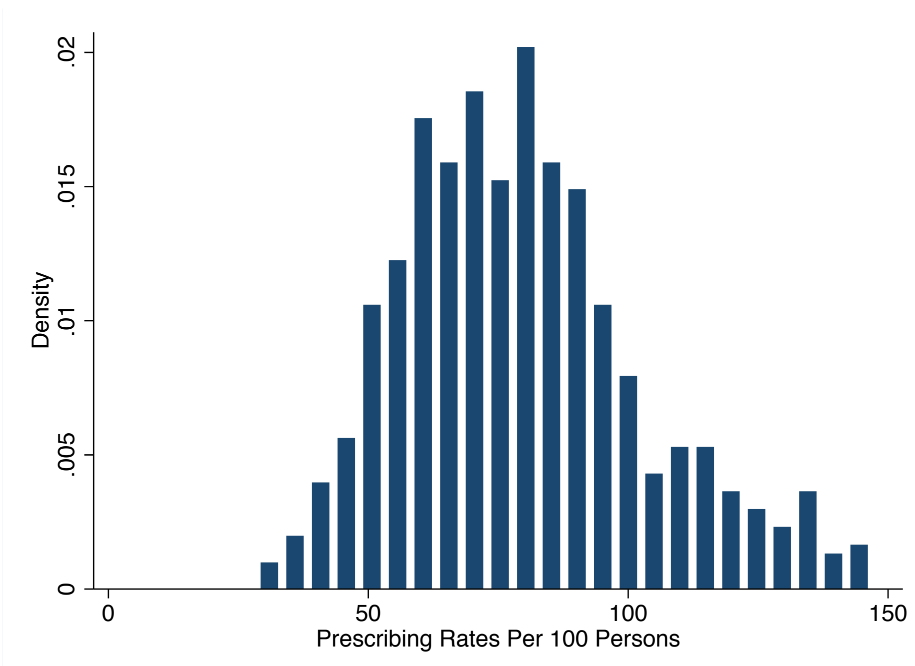


Figure 4.3: Opioid Prescription Rates, 2006-2017



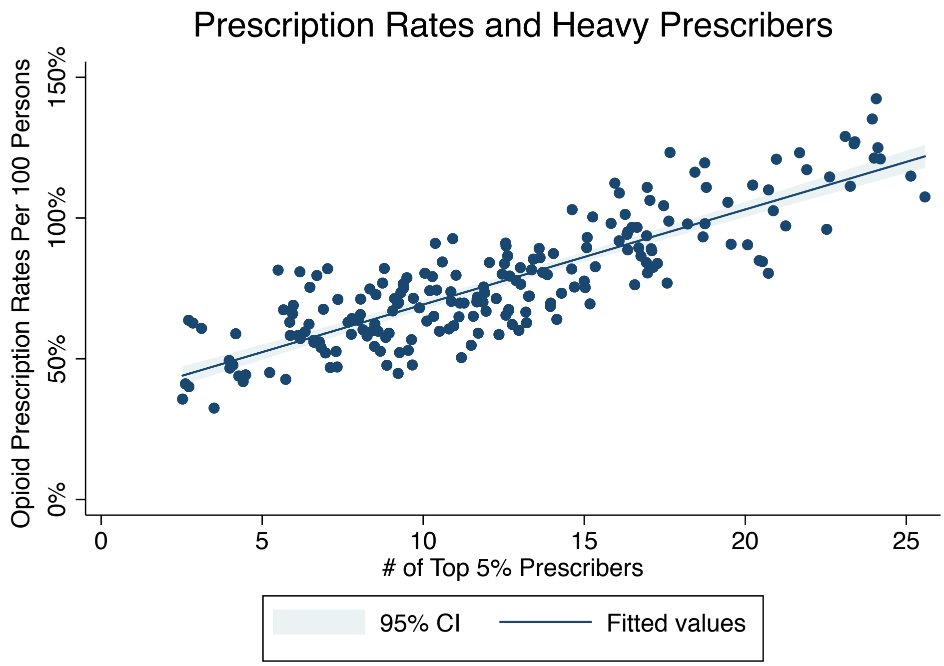
4.3 Instruments

Lastly, I extract data for the instrumental variables from Medicare Part D Summary files, which summarize the wealth of data from the Centers for Medicare and Medicaid Services (CMS). This dataset is publicly available for anyone to access, and identifies providers by their National Provider Identifier (NPI) and the specific prescriptions that were dispensed at their discretion. For each prescriber, the dataset includes the total number of claims for each drug; the Part D Summary files specially consolidates information on the individual opioid prescribing rates of health providers that participate in Medicare Part D program.

Harris et al. (2019) identify the concentration of heavy prescribers in each county, assuming exogeneity in the differences among cross-county prescribing behavior. Heavy prescribers are defined as physicians who make the top 5% of claims, and physicians who make the top 1% of dosages prescribed. Following Harris et al. (2019), I use a national distribution so that the measure of per-capita heavy prescribers is comparable across states on an annual basis. Then, I normalize the number of the top 5% and top 1% prescribers in each state, weighting every state by its respective population in each year. Otherwise, more populous states would skew the distribution of heavy prescribers given that those states would naturally possess a higher number of heavy prescribers.

A valid instrument must be strong and relevant. In other words, an instrument must be correlated with the endogenous regressor, but uncorrelated with the outcome variable. Figure 4.4 displays a positive correlation between prescription rates and the number of top 5% prescribers. As the number of heavy prescribers increase, prescription rates increase as well. The measure of fit between prescription rates and heavy prescribers, defined by R^2, is about 0.73, indicating a strong positive correlation.

Figure 4.4: Opioid Prescription Rates and the Number of Top 5% Prescribers



The Medicare Part D files are not without its limitations; despite its wealth of information on prescribers and drugs alike, the dataset captures information only on beneficiaries enrolled in the Medicare Part D prescription drug program, which accounts for approximately two-thirds of all Medicare beneficiaries (CMS). Consequently, the data might not be completely representative of a physician's practice or behavior, as the physician might also treat patients outside of the Medicare Part D prescription drug program. To test the strength and relevance of the instruments, a series of tests will measure the necessity and exogeneity of the instruments, as described later in Section 5, Methodology.

5 Methodology

5.1 Initial Methodological Approach

To begin the analysis, I replicate the same methodology for the labor force regressions as Krueger (2017). As the individual-level weighted observation data is extracted from the same source, replicating Krueger's (2017) methodology to estimate the effect of prescription opioids on labor force participation rate can serve as a baseline reference.

5.1.1 Linear Probability Models

The initial empirical model to assess the magnitude of the effect of prescription opioids on labor force participation draws from Krueger's (2017) labor force regressions, as follows:

$$labor_{\mathrm{i,s,t}} = \beta_0 + \beta_1 timeperiod_{\mathrm{t_1\text{-}t_0}} + \beta_2 ln(rates)_{\mathrm{s,t}} + \beta_3 (timeperiod \times ln(rates))_{\mathrm{s,t}} + \beta_4 X_{\mathrm{i,s,t}} + \beta_5 regions_{\mathrm{s}} + \varepsilon_{\mathrm{i,s,t}} \tag{1}$$

The linear probability model follows the linear multiple regression model, applied to a binary outcome variable. The binary outcome variable is *labor,* which indicates an individual's participation in the labor force. If *labor* equals zero, the individual is not currently in the labor force; likewise, if *labor* equals one, the individual is a participant in the labor force. The linear probability model then takes on this form:

$$\Pr(labor = 1 | timeperiod,\ rates,\ X,\ regions) = \beta_0 + \beta_1 timeperiod_{\mathrm{t_1\text{-}t_0}} + \beta_2 ln(rates)_{\mathrm{s,t}} + \beta_3 (timeperiod \times ln(rates))_{\mathrm{s,t}} + \beta_4 X_{\mathrm{i,s,t}} + \beta_5 regions_{\mathrm{s}} \tag{2}$$

The regression coefficients of the linear probability model employed here are still estimated by OLS, so the usual heteroskedasticity-robust standard errors can be used for confidence intervals and hypothesis tests (Stock & Watson, 2015). Now, in typical interpretations of linear probability models, the regression model estimates the probability that the dependent variable will equal one, given the constraints and controls. In the context of labor force participation, the resulting outcome for *labor* will be the probability that a certain individual is a participant in the labor force. Ordinarily, with linear probability models, the estimated coefficient of an explanatory variable would be interpreted as the predicted change in the probability of labor force participation. For instance, β_2 is the coefficient that represents the change in labor force participation rate as the cause of a percentage change in opioid prescription rates. If β_2 were -0.05, that would suggest that if prescription rates increased by 0.1, we would expect the probability of an individual participating in the labor force to decrease by 0.15 percentage points. However, due to data limitations at the time of his study, Krueger (2017) manipulates a specific set of variables in the linear probability model to identify a causal effect of prescription opioids on labor.

Taking individual-level labor force data from CPS in 1999-2001 and 2014-2016, Krueger (2017) links the log of 2015 county-level opioid prescription rates to the CPS dataset.[13] The first variable, $timeperiod_{t_1\text{-}t_0}$, is a dummy variable that essentially estimates the change in labor force participation between the two specified time periods. For my replicated model, $rates_s$ is the 2011 opioid prescription rates on the state-level. The variable ($timeperiod$ x $ln(rates)$) interacts the change in labor force participation with the effect of prescription opioids on labor. Adding this interaction term allows me to estimate the combined effect of the change in time and prescription rates on labor. $X_{i,s,t}$ is a vector of explanatory variables that includes the demographic composition of each individual observation such as marital status, race, age, and ethnicity. Lastly, $regions_s$ classifies each state

[13] My dataset uses prescription rates at the state level, as described in Section 4.

into its Census-Bureau designated divisions, which are commonly used to control for regional differences.[14] Krueger (2017) adds in Autor et al. (2013) China import exposure variables as controls, and observes largely no change on the estimated coefficients of the opioid prescription variables.

Instead of interpreting a unit change in the coefficient of prescription rates as a direct change in the probability of labor force participation, Krueger (2017) looks at three variables of interest: the factor increase in opioid prescriptions per capita from 1999 to 2015, $\Delta(\textit{opioidprescriptions})_{t_1\text{-}t_0}$, the decrease in the labor force participation rate from 1999 to 2015 (β_1), and the interacted effect between that time period and county-level 2015 prescription rates (β_3). By assuming cross-county differences in opioid prescription rates as an exogenous result of differences in medical practices and prescribing behavior, the magnitude of the effect in the growth of opioid prescriptions on labor force participation can be estimated, as follows:

$$(\textit{effect of opioid prescriptions on labor})_{t_1\text{-}t_0} = \frac{\Delta\ln(\textit{opioidprescriptions})_{t_1\text{-}t_0} \times \beta_3(\textit{timeperiod} \text{ x } \textit{rates})_{s,t}}{\beta_1 \textit{timeperiod}_{t_1\text{-}t_0}} \tag{3}$$

The natural log of the increase in opioid prescriptions per capita is taken to keep the units consistent, since the log of prescription rates is used in the model.[15] As discussed in the literature review, Krueger (2017) finds that a 3.5 factor increase in opioid prescriptions per capita results in a 1.4 percentage point decline in male

[14] For the breakdown of each state's divisional classification, see: https://www2.census.gov/geo/pdfs/maps-data/maps/reference/us_regdiv.pdf

[15] Krueger (2017) takes the log of prescription rates, citing that a one-to-one direct relationship between opioids and labor does not hold in that labor force participation might be affected by drugs other than opioids.

labor force participation, accounting for about 43% of the observed decline from 1999 to 2016.

Another limitation of this methodology stems from the simplicity of the linear probability model. Since the model mimics the properties of a linear OLS regression, the predicted probabilities could drop below zero or exceed one depending on the estimated coefficients. Predicting a probability less than zero or greater than one would make no statistical sense, as probability lies within the bounds [0, 1]. However, this is an inevitable consequence of linear probability models: when there is a wide range of probabilities, particularly in the extremes from 0.01 to 0.99, a linear approximation no longer becomes a valid estimation approach. As a result, logistic models such as probit and logit regressions, specifically designed for binary dependent variables, are more commonly used.

5.1.2 Logit Regression Model

Logistic regressions are nonlinear formulations that force the predicted values to be between zero and one by using the cumulative probability distributive function. (Stock & Watson, 2015). Logit regressions follow the cumulative standard logistic distribution function, denoted by F:

$$\Pr(labor{=}1|timeperiod,\ rates,\ X,\ regions) = \tag{4}$$

$$F(\beta_0+\beta_1 timeperiod_{t_1\text{-}t_0}+\beta_2 ln(rates)_{s,t}+\beta_3(timeperiod \text{ x } ln(rates))_{s,t}+\beta_4 X_{i,s,t}+\beta_5 regions_s)$$

$$= \frac{1}{1+\left(\frac{1}{e^{\left(\beta_0+\beta_1 timeperiod_{t_1\text{-}t_0}+\beta_2 ln(rates)_{s,t}+\beta_3\left(timeperiod_{t_1\text{-}t_0} \text{ x } ln(rates)\right)_{s,t}+\beta_4 X_{i,s,t}+\beta_5 regions_s\right)}}\right)}$$

Since the logit regression imposes the estimated probabilities to never exceed one or drop below zero, the model offers a closer fit to the data especially in cases where the predicted values from a linear probability model are on the extreme ends. With logit regressions, the coefficients and significance tests from the regression outputs indicate the relationship between labor and rates, but beyond that, are difficult to interpret.

Instead, logit coefficients are often interpreted by computing predicted probabilities, and the corresponding differences in predicted probabilities through a method called the odds ratio. The odds ratio eases interpretation by representing the odds that *labor* equals one when prescription rates change by one unit. Still, such a direct interpretation provides no meaningful significance in our context, as discussed. Thus, the logit regression and odds ratio models will be employed as a complimentary extension, namely to confirm the sign and significance of the estimated coefficients from the linear probability model. Furthermore, if the linear probability model estimates predicted values that are in a reasonable range, both this model as well as the logit regression model would generate similar results.

5.1.3 Fixed Effects Regression Using Panel Data

In addtion, I run a simple linear fixed effects regression using state-level panel data for labor force outcomes and prescription rates, as follows:

$$labor_{s,t} = \beta_0 + \beta_1 rates_{s,t} + \gamma_s + \delta_t + \varepsilon_{i,s,t} \tag{5}$$

Compared to Harris et al. (2019)'s data and methodology, this model looks at all states, not just ten. However, while my model estimates the prescription opioid effect on the state level, Harris et al. (2019) run their model at the county-level, estimating more granular effects in county labor market outcomes. Furthermore, as the prescription rates are no longer linked to individual-level labor data, I can no longer cut the data to run regressions specific to demographic groups. Thus, without additional controls, this model serves mostly as a complementary illustration to the results from the more robust models employed in my methodology.

5.2 Extended Linear Probability Model with State and Year Fixed Effects, and Lagged Specifications

Due to data limitations at the time, Krueger's method to measure the effect of prescription opioids on labor force participation required a manipulation of specific variables, and only estimated the effect of a net increase in opioid prescriptions.[16] The CDC now provides prescription rate data for all fifty states including the District of Columbia from 2006 to 2017. Accordingly, linking state-level data to

[16] Krueger (2017) uses only one year's worth of county-level prescribing data (2015 prescribing rates) to estimate the effects of opioids on labor from 1995 to 2016.

individual-level observations from the CPS then creates a powerful and incredibly useful dataset. The CPS randomly samples individuals on a monthly basis, assigning a weight to each individual. By weighting each observation correspondingly, the sample data offers a more accurate representation of the United States' demographic composition. Thus, every regression model for the entirety of the analysis is weighted in order estimate effects more precisely.[17] Linking a strongly balanced panel of state-level labor market outcomes and prescription rate data linked to individual-level CPS allows for more flexibility in the model. Accordingly, an extended linear probability model with state and year fixed effects allows additional hypotheses to be tested, greatly extending the scope of the analysis beyond the state-level by allowing the data to be stratified by specific demographic groups. The extended model is as follows:

$$labor_{i,s,t}=\beta_0+\beta_1 rates_{s,t}+\beta_2 X_{i,s,t}+\beta_3 regions_s+\gamma_s+\delta_t+\varepsilon_{i,s,t} \qquad (6)$$

The outcome variable $labor_{i,s,t}$ is still a binary variable at the individual level. $X_{i,s,t}$ and $regions_s$ are also the same as before; $X_{i,s,t}$ is a vector of controls specifying the demographic composition of each individual. Likewise, $regions_s$ classify each state by the Census-Bureau designated regional division. The biggest differences are $rates_{s,t}$, which include the prescription rate for all fifty states and the District of Columbia from 2006 to 2017. Furthermore, as explained in Section 3, I do not take the log of prescription rates, as the distribution of rates is close to a normal approximation. γ_s and δ_t are state dummies and year dummies that effectively function as state and year fixed effects, respectively. With this extended model, I can now take the estimated coefficient of $rates_{s,t}$ to calculate the net effect of prescription opioids on labor, as follows:

[17] Replicated models from Krueger's methodology earlier were also weighted.

$$\left(\textit{effect of opioid prescriptions on labor}\right)_{\mathrm{t_1\text{-}t_0}} = \frac{\beta_1 \, \textit{rates}_{\mathrm{s,t}} \left[\sum_{\mathrm{i=1}}^{51} \left(\textit{rates}_{\mathrm{t_1}}^{\mathrm{i}} - \textit{rates}_{\mathrm{t_0}}^{\mathrm{i}}\right) \times \mathrm{N^i}\right]}{\Delta\left(\textit{labor force participation}\right)_{\mathrm{t_1\text{-}t_0}}} \tag{7}$$

Equation 7 takes the summation of the difference in rates of every state during a given time period, and multiplies the estimated effect of prescription rates on labor force participation. Using U.S. annual prescribing rates, this method would then estimate the aggregated effect of prescription opioids on labor force participation for the entire country, for the specified time period. Moreover, the method is very flexible and can easily be adjusted to look at specific states or particular time periods. In the case where the effect of opioids on labor force participation is estimated in more than one state, the N^i variable appropriately weights each state by its population to normalize its contribution to the overall estimated effect.

An additional methodological approach involves employing lagged specifications with prescription rates. The intuition behind lagging rates is that the process for abuse to develop and directly affect labor force participation is not immediate. Furthermore, if the number of heavy prescribers are constant over time, a contemporaneous model without lagged rates would generate similar estimates. Figure A3 in the Appendix illustrates the number of heavy prescribers between 2013 and 2016. Considering the top five high-opioid states by prescribing rate in 2012, the number of heavy prescribers for the other states either increase or decrease over time. Thus, employing lagged rates in the regressions help disentangle the effect of opioids on labor force participation over time.

5.3 IV Regression Model Using GMM2S Estimation

There are a variety of concerns surrounding a potential endogenous relationship between prescription opioids and labor force participation, as Abraham and

Kearney (2017) and Krueger (2017) describe, and Harris et al. (2019) address. To control for simultaneous causality bias, an instrumental variables approach is implemented using the concentration of high-volume prescribers as exclusion restrictions. The first-stage equation takes on this form:

$$rates^{1}_{i,s,t} = \alpha_0 + \alpha_1 X_{i,s,t} + \alpha_2 \boldsymbol{Z}_{i,s,t} + \alpha_3 regions_{s} + \gamma^{1}_{s} + \delta^{1}_{t} + \varepsilon^{1}_{i,s,t} \tag{8}$$

The first-stage estimation uses $\boldsymbol{Z}_{i,s,t}$ as the instruments. The instruments are the number of prescribers in the top 5% of claims prescribed, and top 1% of doses prescribed, respectively. The second-stage estimation essentially follows Equation 6, but with the newly instrumented regressor, $rates^{1}_{i,s,t}$.

In order for the instrumental variable approach to perform well, the instruments must be valid, e.g. relevant and orthogonal to the errors in the second-stage, with the condition $E[Z'\varepsilon] = 0$ holding. Stock and Watson (2015) note that for instruments to be valid, the instrument must be relevant, or correlated with the endogenous regressor, and that the instruments must be exogenous, where $corr(Z, \varepsilon) = 0$. Violations of instrument validity are definitely a concern—if the concentration of heavy prescribers is also correlated with unobserved health or cultural factors affecting labor market outcomes, that could result in weak or invalid instruments (Harris et al., 2019).

Next, Baum (2014) recommends that for any overidentified model estimating with instrumental variables, the Sargan–Hansen test of overidentifying restrictions be routinely performed. The Sargan-Hansen test assesses whether the instruments in question fulfill the exogeneity condition, wherein the correlation with the error term is zero. Baum (2014) acknowledges that while instrumental variables techniques are powerful, a strong rejection of the null hypothesis of the Sargan–Hansen test should cast strong doubt in the validity of the estimates. As there is only one endogenous regressor and two instruments, the model is overidentified and the Sargan-Hansen test can be employed. The Stata command ivreg2 can directly test the validity of the exogeneity assumption using the Sargan-Hansen J test. Based on these tests, the estimates from the second-stage IV regression could be considered more illustrative than hardline evidence.

Lastly, a IV-GMM2S regression model is employed. Harris et al. (2019) implement a two-stage least-squares (2SLS) model, which would have been the preferred IV model of choice if not for the presence of heteroskedasticity. When errors are independently and identically distributed, or i.i.d., then the IV Generalized Method of Moments (GMM) estimator mirrors the standard 2SLS estimator (Baum, 2014). Moreover, if the errors are homoskedastic, there would be no substantial difference in the results between two-stage IV-GMM estimation and IV-2SLS estimation. However, if heteroskedasticity is present, computing robust standard errors using IV-GMM estimates is more efficient than 2SLS estimates, especially in cases where there is only one endogenous regressor (Baum, 2014). Based on this econometric theory, and given that prescription rates is the only endogenous regressor in question, I choose to run the two-stage IV-GMM regression model for the instrumental variables approach.

6 Results and Analysis

6.1 Initial Baseline Results

The results in this section display estimates from the models and calculations that follow the methodology in Krueger (2017). In constructing his labor force regression models, Krueger acknowledges the limitations in his model due to data constraints. Nevertheless, I replicate Krueger's methodology to obtain baseline references for later comparison.

6.1.1 Effects of Prescription Opioids from 2006 to 2012

Tables 6.1 and 6.2 display the results from the initial linear probability model. The total number of opioid prescriptions dispensed in the United States increased by a factor of 1.14 from 2006 to 2012.[18,19] As seen in Column 1 of Table 6.1, the estimated decline in labor force participation among prime-age males is 1.75 percentage points. Next, I need the coefficient of the interaction term between time and opioids. Even after adding demographic and regional controls, the sign, significance, and magnitude of the coefficients change only slightly. I take the coefficient of the interaction from Column 5, as the model variant in this column includes all the desired controls.

[18] The time period Krueger (2017) observes is a much larger range: 1995-2016, whereas my time period is 2006-2012. As a result, I capture a smaller growth in opioid prescriptions of about 1.14x, whereas Krueger (2017) captures a larger increase of 3.5x. Likewise, I observe a smaller estimated decline in labor force participation in my time period.

[19] As Krueger (2017) specifies a time period where the net amount of prescription opioids increases substantially, I identify the largest span of time of largest growth within my dataset.

Table 6.1: Linear Probability Models for Labor Force Participation of Prime-Age Males, 2006-2008 and 2010-2012

VARIABLES	(1) labor	(2) labor	(3) labor	(4) labor	(5) labor
Time Period	-0.0175***		-0.0197***	-0.0157***	-0.0158***
	(0.0005)		(0.0007)	(0.0007)	(0.0007)
Log Opioid Rate		-0.0310***	-0.0246***	-0.0232***	-0.0297***
		(0.0007)	(0.0014)	(0.0013)	(0.0016)
Log Rate x Time			-0.0086***	-0.0092***	-0.0091***
			(0.0020)	(0.0020)	(0.0020)
Married				0.0948***	0.0945***
				(0.0006)	(0.0006)
White				0.0541***	0.0533***
				(0.0018)	(0.0018)
Black				-0.0153***	-0.0168***
				(0.0020)	(0.0020)
Hispanic				0.0014**	0.0019***
				(0.0007)	(0.0007)
Asian				0.0336***	0.0350***
				(0.0021)	(0.0021)
Age				0.0127***	0.0128***
				(0.0003)	(0.0003)
Age Squared/1000				-0.1973***	-0.1979***
				(0.0040)	(0.0040)
Mid Atlantic					-0.0089***
					(0.0012)
East North Central					0.0017
					(0.0011)
West North Central					0.0115***
					(0.0012)
South Atlantic					0.0091***
					(0.0011)
East South Central					-0.0098***
					(0.0017)
West South Central					0.0032**
					(0.0012)
Mountain					0.0058***
					(0.0012)
Pacific					-0.0080***
					(0.0011)
Constant	0.9063***	0.8840***	0.9000***	0.6172***	0.6148***
	(0.0003)	(0.0003)	(0.0005)	(0.0063)	(0.0063)
Observations	1,903,810	3,684,927	1,903,810	1,903,810	1,903,810
R-squared	0.0008	0.0007	0.0015	0.0379	0.0384

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6.2: Linear Probability Models for Labor Force Participation of Prime-Age Females, 2006-2008 and 2010-2012

VARIABLES	(1) labor	(2) labor	(3) labor	(4) labor	(5) labor
Time Period	-0.0075***		-0.0099***	-0.0095***	-0.0094***
	(0.0007)		(0.0009)	(0.0009)	(0.0009)
Log Opioid Rate		-0.0144***	-0.0067***	-0.0388***	-0.0400***
		(0.0009)	(0.0018)	(0.0019)	(0.0022)
Log Rate x Time			-0.0096***	-0.0112***	-0.0108***
			(0.0026)	(0.0026)	(0.0026)
Married				-0.0648***	-0.0652***
				(0.0007)	(0.0007)
White				0.0369***	0.0331***
				(0.0022)	(0.0022)
Black				0.0172***	0.0160***
				(0.0024)	(0.0024)
Hispanic				-0.1043***	-0.0948***
				(0.0011)	(0.0011)
Asian				-0.0240***	-0.0202***
				(0.0027)	(0.0027)
Age				0.0105***	0.0106***
				(0.0004)	(0.0004)
Age Squared/1000				-0.1259***	-0.1264***
				(0.0052)	(0.0052)
Mid Atlantic					-0.0392***
					(0.0016)
East North Central					-0.0131***
					(0.0015)
West North Central					0.0289***
					(0.0015)
South Atlantic					-0.0157***
					(0.0015)
East South Central					-0.0424***
					(0.0022)
West South Central					-0.0445***
					(0.0017)
Mountain					-0.0270***
					(0.0017)
Pacific					-0.0397***
					(0.0015)
Constant	0.7562***	0.7452***	0.7545***	0.5627***	0.5875***
	(0.0005)	(0.0003)	(0.0007)	(0.0082)	(0.0083)
Observations	2,049,623	3,967,562	2,049,623	2,049,623	2,049,623
R-squared	0.0001	0.0001	0.0001	0.0130	0.0149

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6.3: Linear Probability Models for Labor Force Participation of Prime-Age Population, 2006-2008 and 2010-2012

VARIABLES	(1) labor	(2) labor	(3) labor	(4) labor	(5) labor
Time Period	-0.0125***		-0.0148***	-0.0131***	-0.0131***
	(0.0004)		(0.0006)	(0.0006)	(0.0006)
Log Opioid Rate		-0.0238***	-0.0171***	-0.0301***	-0.0340***
		(0.0006)	(0.0012)	(0.0012)	(0.0014)
Log Rate x Time			-0.0088***	-0.0101***	-0.0099***
			(0.0017)	(0.0017)	(0.0017)
Female				-0.1449***	-0.1449***
				(0.0004)	(0.0004)
Married				0.0122***	0.0119***
				(0.0005)	(0.0005)
White				0.0438***	0.0414***
				(0.0014)	(0.0014)
Black				0.0077***	0.0063***
				(0.0016)	(0.0016)
Hispanic				-0.0466***	-0.0417***
				(0.0006)	(0.0007)
Asian				0.0023	0.0048***
				(0.0017)	(0.0017)
Age				0.0126***	0.0126***
				(0.0003)	(0.0003)
Age Squared/1000				-0.1709***	-0.1715***
				(0.0033)	(0.0033)
Mid Atlantic					-0.0249***
					(0.0010)
East North Central					-0.0062***
					(0.0010)
West North Central					0.0192***
					(0.0010)
South Atlantic					-0.0040***
					(0.0009)
East South Central					-0.0269***
					(0.0014)
West South Central					-0.0214***
					(0.0011)
Mountain					-0.0118***
					(0.0011)
Pacific					-0.0246***
					(0.0010)
Constant	0.8303***	0.8133***	0.8260***	0.6420***	0.6538***
	(0.0003)	(0.0002)	(0.0004)	(0.0052)	(0.0053)
Observations	3,953,433	7,652,489	3,953,433	3,953,433	3,953,433
R-squared	0.0003	0.0003	0.0005	0.0412	0.0423

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

By multiplying the log of the factor increase in prescription opioids, ln(1.14) by the interaction, -0.0091, I estimate that the increase in opioid prescriptions from 2006 to 2012 is responsible for about 0.12 percentage points of the decline in labor force participation for prime-age males.[20] 0.12 of the 1.75 percentage point decline in labor force participation is about 6.7%. This empirical approach suggests that prescription opioids are responsible for about 6.7% of the estimated decline for that time period.

Table 6.2 follows the exact structure of Table 6.1, but for prime-age females. Column 1 estimates the change in labor force participation to be about a 0.75 percentage point decline. Replicating the same procedure, ln(1.14) x -0.0108, the model estimates that the increase in opioid prescriptions accounts for about 0.14 percentage points of the decline in labor force participation for prime-age females. 0.14 of the 0.75 percentage point decline in labor force participation is 0.2059. Between 2006 to 2012, these results suggest that prescription opioids were responsible for about 21% of the estimated decline in female participation.

Lastly, Table 6.3 displays the estimates for the entire prime-age population. Rather than running regressions separated by gender, gender is instead included as a control. The estimated decline in labor force participation for the prime-age population is about 1.25 percentage points. Multiplying ln(1.14) by -0.0099, the model suggests that the increase in opioid prescriptions accounts for about 0.13 percentage points of the decline in labor force participation. 0.13 of the 1.25 percentage point decline in labor force participation is 0.104. For the time period 2006 to 2012, this suggests that prescription opioids were responsible for about 10% of the estimated decline for the prime-age working population.

[20] Interestingly, despite the large difference between our observed time ranges, my estimated coefficients of the interaction terms for both prime-age males and females are remarkably similar to those of Krueger (2017).

Table 6.4 illustrates the complete calculations of Equation 3 for 2006 to 2012, and summarizes the effect of prescription opioids on labor force participation by gender. For comparison, I also include the estimates from Krueger (2017), for the time period 1999 to 2016.[21] In that time, opioid prescriptions grew by a factor of 3.5, which Krueger (2017) suggests were responsible for 43% of the decline in labor force participation rate for prime-age men. From 2006 to 2012, I estimate a much smaller effect, of about 6.7%.

Table 6.4: Estimated Effects of Prescription Opioids on Labor from 2006-2012

	Prime-Age Males	Prime-Age Females	Prime-Age Population
1999-2016	$\frac{\ln(3.5) \times -0.011}{-0.032} \approx 43\%$	$\frac{\ln(3.5) \times -0.014}{-0.025} \approx 70\%$	N/A
2006-2012	$\frac{\ln(1.14) \times -0.0091}{-0.0175} \approx 6.7\%$	$\frac{\ln(1.14) \times -0.0108}{-0.0075} \approx 21\%$	$\frac{\ln(1.14) \times -0.0099}{-0.0125} \approx 10\%$

The growth in opioid prescriptions from 2006 to 2012 compared to 1999 to 2015 is much smaller, so a lesser effect is expected. On the other hand, the decline in labor force participation is almost halved (3.2 percentage point decline for 1999-2016 vs. 1.75 percentage point decline for 2006-2012), so the estimated effect of 6.7% during my time period is lower than expected. The difference between the estimated effect for prime-age females is substantial as well. The difference in the estimated effect between these two time periods could suggest that most of the negative effects prescription opioids had on labor force participation occurred in the earlier stages of the epidemic.

[21] Krueger (2017) did not include estimates for the general prime-age working population.

6.1.2 Logit Regression Results and Odds Ratio Estimates

Table A3 displays estimates from the logit regression model.[22] Compared to the linear probability model, logit regressions fits the model so that predicted probabilities are only be between zero and one. The weakness of linear probability models is that when expected probabilities occupy a wide range of values, from 1% to 99% for example, it is possible for the predicted probabilities to exceed zero or one. In addition, if the regressors have extremely high or low values, results can be further aggravated. Logistic regression coefficients are more difficult to interpret, but still illustrate two important indicators: the sign and significance of the regressors. In Table A3, similarly to the linear probability model outputs, opioid prescription rates have a negative, significant effect on labor force participation among prime age males, females, and the general prime-age population.

Table A4 displays the odds ratios for the logit regression. The odds ratio allows for a different, and sometimes simpler, interpretation of the logit regression estimates. The odds ratio represents the odds of $labor$ = one when $rates$ changes by one percentage point. If the odds ratio is greater than one, then the odds that $labor$ = one increases. Conversely, if the odds ratio is less than one, then the odds that $labor$ = one decreases. While it is difficult to directly compare the results of the estimated logit coefficients and odds ratio coefficients to those of the linear probability model, comparing the estimates can reveal whether the linear probability still holds. First, the sign and significance of the coefficients from the linear model and the logit regression are very close. As for the magnitude of the effects of opioid prescription rates and various controls on labor force participation, the estimated degree of impact is similar as well. Thus, I establish that the linear probability model holds in that aspect.

[22] The logit regression and odds ratio tables are placed in the Appendix as these models are executed only for illustrative purposes.

6.1.3 Complimentary State-Level Panel Data Analysis

As a further complimentary extension of the initial analysis, I run a simple fixed effects regression with a strongly balanced panel of state-level labor force participation rates and state-level prescription rates, and no additional controls.[23] First, for the full time period from 2006 to 2017, the model estimates a significant, positive effect on labor force participation with only state-fixed effects. This effect is reversed with including both state and year fixed effects, as the regression estimates a positive, but insignificant effect on labor force participation.

Overall, this model is just an preliminary exercise to provide an early idea of how prescription rates might affect labor market outcomes. As this data is very limited in terms of demographic composition, and especially without controls, the results serve as a baseline reference. Additionally, the extended models in the next section focus on the prime-age working population, whereas the state-level employment data captures participation for all ages, not just the prime-age working population.

6.2 Extended Linear Probability Model with Fixed Effects and Lagged Rates, 2006 to 2017

Table 6.5 displays the estimates from the extended regression with state and time fixed effects. With state-level prescription data from 2006 to 2017 linked to robust monthly CPS weighted data at the individual-level, a more comprehensive and flexible analysis is possible. Additionally, in replicating Kruger's methodology, the effect of only an increase in opioid prescriptions was estimated. With this model, I

[23] While the dataset includes both labor force participation rate and unemployment rate, I only analyze the participation rate as that particular labor market outcome is the focus of the study.

estimate the same time period as well as the full time period. Column 1 includes demographic and regional controls, but without state and year fixed effects. Column 2, 3, and 4 exclude regional controls but include state and year fixed effects. Additionally, Columns 3 and 4 further stratify the model by men and women, respectively. In Column 1, the effect of opioid prescription rates on labor force participation is both negative and significant. However, in Columns 2 to 4, where state and year fixed effects are incorporated over simple regional controls, the magnitude of the effect of opioids on labor is smaller, positive, and insignificant.

There are a few potential explanations behind how state and year fixed effects reverse the initially estimated negative effect of opioid prescriptions to become positive. Namely, opioid prescriptions tend to cluster and concentrate in certain states, and this trend still applies to the regional level—certain regional divisions prescribe higher concentrations of opioid prescriptions than other divisions. Not every state has a high prescription rate level; some states have significantly higher rates of opioid prescriptions, and some of those states are clustered in the same regional division. As a result, when implementing state fixed effects over simple regional controls, the effect of these clustered areas becomes diluted across the nation, since the model now estimates the effect of opioid prescription by accounting for unobserved variation within each state, every year.

Table 6.5: Extended Linear Probability Model with Fixed Effects, 2006 to 2017

VARIABLES	(1) All labor	(2) All labor	(3) Men labor	(4) Women labor
Prescription Rates	-0.0454***	0.0056	0.0052	0.0050
	(0.0010)	(0.0040)	(0.0048)	(0.0063)
Female	-0.1428***	-0.1427***		
	(0.0003)	(0.0003)		
Married	0.0133***	0.0124***	0.0965***	-0.0658***
	(0.0003)	(0.0003)	(0.0004)	(0.0005)
White	0.0415***	0.0416***	0.0472***	0.0392***
	(0.0010)	(0.0010)	(0.0013)	(0.0016)
Black	0.0097***	0.0110***	-0.0206***	0.0294***
	(0.0011)	(0.0012)	(0.0015)	(0.0018)
Hispanic	-0.0402***	-0.0373***	0.0082***	-0.0907***
	(0.0005)	(0.0005)	(0.0005)	(0.0008)
Asian	0.0038***	0.0056***	0.0341***	-0.0173***
	(0.0012)	(0.0012)	(0.0015)	(0.0019)
Age	0.0131***	0.0130***	0.0126***	0.0113***
	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Age Squared/1000	-0.1774***	-0.1762***	-0.1933***	-0.1393***
	(0.0024)	(0.0024)	(0.0029)	(0.0037)
Mid Atlantic	-0.0259***			
	(0.0007)			
East North Central	-0.0083***			
	(0.0007)			
West North Central	0.0196***			
	(0.0007)			
South Atlantic	-0.0109***			
	(0.0007)			
East South Central	-0.0334***			
	(0.0010)			
West South Central	-0.0243***			
	(0.0008)			
Mountain	-0.0180***			
	(0.0008)			
Pacific	-0.0272***			
	(0.0007)			
Constant	0.6808***	0.5866***	0.5845***	0.4898***
	(0.0039)	(0.0064)	(0.0078)	(0.0100)
Observations	7,652,489	7,652,489	3,684,927	3,967,562
R-squared	0.0406	0.0423	0.0399	0.0176
State FE	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Furthermore, the model looks at all years from 2006 to 2017. Recall that opioid prescription rates peaked in 2012; from 2012 to 2017, there is a substantial fall in rates, a decline of 22.6 percentage points.[24] Now that the fixed effects model includes another five years wherein rates decline, the results suggest that no significant relationship exists between prescription opioids and labor force participation. Moreover, the model estimates a positive opioid-labor effect, which conflicts with the initial results and findings from early literature.

Before further analysis, as an additional methodological approach, I implement an extended regression that takes a one or two year lag of prescription rates. Tables 6.6 and 6.7 run a one year lag and a two year lag, respectively. The premise behind employing lagged specifications centers around the idea that it takes time for opioid prescription abuse to develop; once an individual has consistent access to the drugs, it takes additional time for addiction and abuse to drive an individual's exit from the labor force. For instance, with a one year lag, the model would now estimate the effect of 2006 opioid prescriptions rates on the observations from 2007. In this way, the lagged regression now accounts for the time it takes opioids to affect participation. Thus, by taking the lag of opioid prescription rates by one or two years, the estimated effect of the opioids might change from the earlier estimates in the model with contemporaneous rates.

Once I incorporate lagged rates, the estimated effect of prescription opioids becomes significant and increases in magnitude. Interestingly, the one year lag estimates a slightly lesser, but more significant effect for prime-age males, while the two year lag estimates a slightly stronger, and more significant effect for prime-age females. For the general prime-age working population, both models estimate a positive, significant effect.

[24] See Figure 4.2 to see the change in the national mean prescription rate over time. To see the aggregate nationwide prescription rate by year, see Table A1 in the Appendix.

Table 6.6: Extended Linear Probability Model with Fixed Effects, 1 Year Lag, 2006 to 2017

VARIABLES	(1) All labor	(2) All labor	(3) Men labor	(4) Women labor
1 Year Lag Rate	-0.0549***	0.0148***	0.0125**	0.0157**
	(0.0011)	(0.0046)	(0.0055)	(0.0071)
Female	-0.1421***	-0.1420***		
	(0.0003)	(0.0003)		
Married	0.0137***	0.0129***	0.0971***	-0.0656***
	(0.0004)	(0.0004)	(0.0004)	(0.0005)
White	0.0414***	0.0418***	0.0472***	0.0396***
	(0.0011)	(0.0011)	(0.0013)	(0.0017)
Black	0.0095***	0.0114***	-0.0204***	0.0300***
	(0.0012)	(0.0012)	(0.0015)	(0.0018)
Hispanic	-0.0401***	-0.0367***	0.0090***	-0.0900***
	(0.0005)	(0.0005)	(0.0005)	(0.0008)
Asian	0.0036***	0.0061***	0.0350***	-0.0171***
	(0.0013)	(0.0013)	(0.0016)	(0.0020)
Age	0.0131***	0.0131***	0.0126***	0.0115***
	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Age Squared/1000	-0.1782***	-0.1774***	-0.1934***	-0.1412***
	(0.0025)	(0.0025)	(0.0030)	(0.0039)
Mid Atlantic	-0.0281***			
	(0.0008)			
East North Central	-0.0084***			
	(0.0007)			
West North Central	0.0181***			
	(0.0008)			
South Atlantic	-0.0113***			
	(0.0007)			
East South Central	-0.0291***			
	(0.0011)			
West South Central	-0.0241***			
	(0.0008)			
Mountain	-0.0190***			
	(0.0008)			
Pacific	-0.0292***			
	(0.0007)			
Constant	0.6872***	0.5734***	0.5754***	0.4747***
	(0.0041)	(0.0071)	(0.0086)	(0.0111)
Observations	6,970,203	6,970,203	3,356,647	3,613,556
R-squared	0.0402	0.0418	0.0398	0.0175
State FE	NO	Yes	Yes	Yes
Year FE	NO	Yes	Yes	Yes

Robust standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6.7: Extended Linear Probability Model with Fixed Effects, 2 Year Lag, 2006 to 2017

VARIABLES	(1) All labor	(2) All labor	(3) Men labor	(4) Women labor
2 Year Lag Rate	-0.0608***	0.0206***	0.0130**	0.0265***
	(0.0011)	(0.0051)	(0.0062)	(0.0080)
Female	-0.1410***	-0.1409***		
	(0.0003)	(0.0003)		
Married	0.0140***	0.0134***	0.0979***	-0.0653***
	(0.0004)	(0.0004)	(0.0005)	(0.0006)
White	0.0416***	0.0423***	0.0473***	0.0405***
	(0.0011)	(0.0012)	(0.0014)	(0.0018)
Black	0.0099***	0.0121***	-0.0201***	0.0313***
	(0.0013)	(0.0013)	(0.0016)	(0.0019)
Hispanic	-0.0400***	-0.0365***	0.0096***	-0.0898***
	(0.0005)	(0.0005)	(0.0006)	(0.0009)
Asian	0.0038***	0.0066***	0.0357***	-0.0166***
	(0.0013)	(0.0014)	(0.0016)	(0.0021)
Age	0.0133***	0.0133***	0.0129***	0.0116***
	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Age Squared/1000	-0.1805***	-0.1800***	-0.1965***	-0.1429***
	(0.0026)	(0.0026)	(0.0032)	(0.0041)
Mid Atlantic	-0.0297***			
	(0.0008)			
East North Central	-0.0089***			
	(0.0008)			
West North Central	0.0170***			
	(0.0008)			
South Atlantic	-0.0122***			
	(0.0008)			
East South Central	-0.0273***			
	(0.0011)			
West South Central	-0.0241***			
	(0.0008)			
Mountain	-0.0206***			
	(0.0009)			
Pacific	-0.0311***			
	(0.0008)			
Constant	0.6877***	0.5607***	0.5655***	0.4622***
	(0.0043)	(0.0079)	(0.0096)	(0.0123)
Observations	6,301,473	6,301,473	3,034,814	3,266,659
R-squared	0.0396	0.0412	0.0397	0.0176
State FE	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

To help determine which rate I should use to measure prescription opioids' effect on labor, I test each rate by specific time periods. In addition to the full time period 2006 to 2017, I run additional lagged regressions from 2006 to 2012, and 2012 to 2017. Table 6.8 summarizes the sign and significance of the estimated coefficients for each rate by time period.

Table 6.8: Estimated Coefficients Based on Change in Opioid Prescriptions

	Sign	Significance
Panel 1: 2006-2012 Δ opioid prescription rate = +0.089		
$Rates_{t}$	–	YES
$Rates_{t-1}$	–	YES
$Rates_{t-2}$	–	YES
Panel 2: 2012-2017 Δ opioid prescription rate = –0.226		
$Rates_{t}$	–	YES
$Rates_{t-1}$	+	NO
$Rates_{t-2}$	+	NO
Panel 3: 2006-2017 Δ opioid prescription rate = –0.137		
$Rates_{t}$	+	NO
$Rates_{t-1}$	+	YES
$Rates_{t-2}$	+	YES

Panel 1 shows an 8.9 percentage point increase in rates for 2006 to 2012, the same time period observed for the initial regression in Section 6.1. For this time period, each rate is estimated to have a negative, significant effect as expected, since we know that in this time period, prescription rates experience their largest growth.

Panel 2 captures the maximum decline in prescription rates of 22.6 percentage point, from 2012 to 2017, wherein prescribing rates reach their peak in 2012, but fall greatly by 2017. Panel 3 summarizes the effect of a 13.7 percentage point decline in prescription rates on labor force participation for 2006 to 2017, also displayed in

Tables 6.5-6.7. The critical detail to note in both Panel 2 and 3, which specify intervals that include declining opioid rates, is that both lagged rates are estimated to have a positive, significant effect on labor force participation. In Panel 3, for the full time period, all rates, including the contemporaneous rate, have a positive effect. The magnitude of this positive effect is stronger in the two year lag than the one year lag. In addition, for $Rates_{t-2}$, the coefficients of the year fixed effects strongly corroborate with the change in prescription rates. From 2009 to 2014, with each incremental year, the estimated decline in labor force participation increases. From 2014 to 2017 however, the estimated decline in labor force participation lessens with each incremental year. Considering the two year lag, this trend matches directly with the rise and fall of prescription rates. Panel 2 does show a positive but insignificant effect for the two year lag between 2013 and 2017, which could be the result of smaller sample of years. Otherwise, these findings support the two year lag, as it aligns most closely with the movement in prescription rates. As such, I take the estimated coefficients from the two year lag regression to interpret the resultant changes in labor force participation from prescription opioids.

Table 6.9 and 6.10 illustrate the calculations of Equation 7 that measure the effect of prescription opioids on labor force participation, using the coefficients from the two year lagged rate. However, the interpretation of the effects are different, as the change in opioid rates differ accordingly by time period. To calculate the expected effect of opioids on labor force participation rate, I multiply the estimated coefficient of prescription rates in the model by the observed difference in prescription rates, $\beta_1 rates_{s,t-2}\left[\sum_{i=1}^{51}\left(rates_{t_1}^{i} - rates_{t_0}^{i}\right) \times N^{i}\right]$. I use my preferred model with the two year lagged rate. As the model considers all fifty states including the District of Columbia, and prescription rate data on the U.S. level is available, I simply take the difference in U.S. rates between 2006 and 2012, simplifying the calculation to $\beta_1 \, rates_{s,t\text{-}2}(rates^{U.S.}_{t_1}\text{-}rates^{U.S.}_{t_0})$. Naturally, weighting is not necessary when estimating the effect using country-level rates.

Table 6.9: Estimated Effect of Prescription Opioids on Labor from 2006-2012[25]

	Prime-Age Males	Prime-Age Females	Prime-Age Population
+Δopioids	0.089 × -0.0536 = -0.0047	0.089 × -0.0375 = -0.0033	0.089 × -0.0408 = -0.0036
−Δlaborforce	$\frac{-0.47\%}{-1.8\%} \approx 26\%$	$\frac{-0.33\%}{-0.9\%} \approx 37\%$	$\frac{-0.36\%}{-1.4\%} \approx 28\%$

Table 6.9 includes results from 2006 to 2012, where the change in rates was positive 8.9 percentage points. The estimated effects for males, females, and the general population is negative. This indicates that an increase in prescription opioids resulted in a decrease in labor force participation during this time period. Following the methodology, I multiply the coefficient by the difference in rates to get the estimated effect of the increase in prescription opioids on labor force participation. For males, I estimate a 0.47 percentage point decline; for females, a 0.33 decline, and for the general prime-age population, a 0.36 decline in participation rates due to opioids. As mortality rates for men are higher due to drug overdose deaths than women, opioids could similarly have a stronger effect on males than females.

Next, I estimate the degree to which the increase in prescription opioids are responsible for the observed decline in labor force participation during this time period. Using labor force participation rates from the BLS to calculate $\Delta(\textit{labor force participation})_{t_1\text{-}t_0}$, I estimate that between 2006 and 2012, the increase in prescription opioids was responsible for 26% of the observed labor force participation decline for prime-age males, 36% for prime-age females, and 28% for

[25] To see the estimated regression coefficients for this time period, see Table A6 in the Appendix.

the general prime-age population. These estimated effects are higher than the effects estimated using Krueger's methodology. The changes in labor force participation rate calculated from the BLS are similar to the estimated changes in participation rate in Krueger's labor force regressions. Therefore, as the specified time period is the same, the main driver behind the disparity between the results can be mostly attributed to the differences in the estimated effect of opioids on labor force participation.

Moreover, the results from this secondary part of the methodology are best considered illustrative, as the observed change in labor force participation rate is arbitrarily calculated. For instance, to get $\Delta(\textit{labor force participation})_{\mathrm{t_1\text{-}t_0}}$, I use rates from the BLS. The BLS determines the participation rates from CPS, the Current Population Survey. Data for the rates can be extracted on a monthly rate, or a quarterly rate. I use the first quarter Q1 rate to calculate the change in labor force participation: 2016 Q1 rate – 2006 Q1 rate. However, as the rates fluctuate by quarter (and by month), the estimated effect calculated in the second row of Table 3.9 could change drastically depending on which quarter I observe. For example, had I used 2016 Q2 rate – 2006 Q2 rate as the change in participation rate, or the difference between the 2016 May rate and 2006 May rate, a very different estimated effect could have calculated. As such, the estimates in the second row would best be considered as an illustrative application.

At the same time, this extended model and methodology present a very powerful analytical tool. The BLS can stratify the rates by age, ethnicity, race, gender, marital status, and educational level. As my dataset also uses individual-level weighted observations from the CPS, the model can separate out the effects of opioid prescriptions by specific demographic characteristics. Furthermore, with yearly state-level employment data from Global Insight, I can further extend the analysis (running illustrative calculations seen in the second rows of Table 6.9 and

6.10), to estimate the effect of prescription opioids on labor market outcomes in one state, or a cluster of states such as a regional division. However, following Krueger (2017), the models separate the labor force regressions by gender.

Next, the results displayed in Table 6.10 are estimates for the entire time range of prescription rate data, 2006 to 2017. The net change in prescription rates was negative 13.7 percentage points. The estimated coefficients for males, females, and the general population is positive. Contrary to the previous time period where prescription rates decreased, a positive effect for the full time period indicates that prescription opioids had a positive effect on labor force participation. In other words, a decrease in prescription opioids resulted in a decrease in labor force participation during this time period. Replicating the same methodology from before, I multiply the coefficient by the difference in rates to get the estimated effect of the decrease in prescription opioids on labor force participation. For males, I estimate a 0.15 percentage point decline; for females, a 0.36 decline, and for the general prime-age population, a 0.28 decline in participation rates due to a decrease in prescription opioids, calculated in the first row.

Table 6.10: Estimated Effect of Prescription Opioids on Labor from 2006-2017

	Prime-Age Males	Prime-Age Females	Prime-Age Population
$-\Delta$opioids	-0.130 × 0.0130 = -0.0015	-0.137 × 0.0265 = -0.0036	-0.137 × -0.0206 = -0.0028
$-\Delta$laborforce	$\frac{-0.15\%}{-1.9\%} \approx 7.9\%$	$\frac{-0.36\%}{-0.7\%} \approx 51\%$	$\frac{-0.28\%}{-1.3\%} \approx 22\%$

As before, I estimate the degree to which the increase in prescription opioids are responsible for the observed decline in labor force participation during this time period. Using labor force participation rates from the BLS, I estimate that between 2006 and 2017, the decline in prescription opioids are responsible for 7.9% of the observed labor force participation decline for prime-age males, 51% for prime-age females, and 22% for the general prime-age population. Now that results for both time periods are computed, I consistently observe a larger impact from opioids on female labor participation for both time periods, and for both Krueger's and my methodology. According to the American Society of Addictive Medicine, women are more likely to have chronic pain, be prescribed prescription pain relievers, be given higher doses, and take the medication for longer periods of time than men; in addition, women tend to become dependent on prescription pain relievers more quickly than men. Mathematically, to measure the degree to which opioids are responsible for the change in labor force participation, I divide the effect of opioids on labor by the change in participation rate. The consistently smaller declines in female labor force participation than male labor force participation further contributes to a larger estimated impact of opioids for females.

These results are striking, and contradict the early literature surrounding prescription opioid effects on labor. Prescription opioids' positive estimated effect on labor in 2006 to 2017 suggests that perhaps since the considerable 22.6 percentage point decline in prescription rates from 2012 to 2017, opioids were being used for their intended purpose—helping people and workers with their pain. When prescription levels were abnormally high, abuse and addiction overpowered the more positive and helpful influence of opioids and, as a result, labor participation suffered. However, once high-opioid states became more aware of the epidemic and started implementing state-wide policies to tackle the issue, rates began dropping on the state-level and nationwide.

For instance, in 2012, New York required prescribers to check their state's Prescription Drug Monitoring Program (PDPM) before prescribing opioids. As a result, New York experienced a 75% drop in patients who saw multiple prescribers to get new prescriptions for the same drug. Enhanced PDMPs, increased funding, and government-driven initiatives to combat the opioid crisis have already helped normalize the inflated levels of prescription opioids. At normal prescribing levels, the results suggest that opioids have a positive effect on labor force participation. Conversely, decreasing opioid prescriptions then has a depressing effect on labor force participation, when rates are at a healthy, appropriate level.

Another possibility behind prescription opioids' positive effect on labor is that the data includes only official, legally dispensed opioid prescribing data. It fails to completely capture the illegal or illegitimate side of the opioid crisis, where an increasing number of users, who later become abusers and addicts, eventually turn to illegal forms of opioids. Synthetic opioids, which mimic the effects of naturally occurring opioids, were responsible for over 28,000 deaths in the United States, more deaths than from any other type of opioid (CDC). While synthetic opioids are manufactured by pharmaceutical companies, they are also manufactured illicitly in clandestine labs and distributed illegally through the drug market (CDC).

In addition, the CDC reports that deaths involving synthetic opioids are driven by increases in fentanyl-involved overdose deaths, and that the fentanyl is more likely to be illicitly manufactured. The vast body of scientific study and research regarding the health outcomes from opioids further indicate that while opioid overdose deaths were initially driven by prescription opioid misuse, heroin and other illicit opioid drugs and misuse have been the main driver of deaths in recent years (O'Donnell, 2017). Thus, the positive effects we observe in the model estimates probably capture mostly prescription opioid use. As discussed, at an appropriate level of prescribing rates, the results suggest that opioids have a beneficial impact on labor force participation.

While these results are unexpected, the model estimates a positive effect of opioids on labor only when incorporating state and year fixed effects. Fixed effects allow the model to control for unobserved intra-state variation over time. When fixed effects are excluded, the model consistently generates negative, significant effects on labor. On the contrary, when state and year fixed effects are included in, the estimated effect of opioids on labor turns positive. I theorize that including fixed effects dilutes the adverse effects of high-opioid states, and correspondingly estimates a clearer effect on the aggregate level. To test my theory, I drop the top ten states with the highest prescription rates in 2012 from the model and rerun the regressions. Every model variant subsequently estimates both a positive and significant effect of opioids on labor.[26] These results align with the rationale that the concentration of high opioid states biases the estimates, unless state and year fixed effects are included.

Finally, there still exists potential endogeneity for the extended model with fixed effects, namely simultaneity bias. To mitigate this concern, the concentration of high volume prescribers is exploited as a plausible source of exogenous variation in an instrumental variables approach.

6.3 Instrumental Variables Approach

Table 6.11 displays the first-stage estimates of the instruments on prescription rates. The concern that prescription opioids might be correlated with the outcome variable can be accounted for by taking an instrumental variables approach. The instruments are the number of top 5% prescribers by claims, and top 1% prescribers by dosage. Cross-state differences in prescriber behavior is exploited as exogenous variation, so that a clearer causal effect can be established between opioid

[26] See Tables A7 and A8 to see the regression results when the top ten high-opioid states are excluded from the model, with a contemporaneous lag and a two year lag, respectively.

prescriptions and labor force participation. Surprisingly, for all but the two year lag rate, the top 5% prescribers by claims have a positive effect.[27]

As Figure 4.4 illustrated a clear positive correlation between prescription rates and the number of prescribers by top 5% of opioid claims, a significant, negative effect on prescription rates is unexpected. The top 1% of prescribers by dosage on the other hand, has a significant positive effect for all rates. A key difference between the extended model with fixed effects and the two-stage IV-GMM regression model is the availability of data. The IV model is limited to 2013 to 2016.[28] Due to this narrow time frame, the positive correlation observed between prescription rates and top 5% prescribers could be weaker in 2013 to 2016. This finding perhaps alludes to the newfound emphasis placed on prescribers and pharmaceutical companies over patients' demand for opioids. Using county-specific federal data, Knight (2019) linked higher opioid-related marketing dollars spent in a county to higher rates of doctors who prescribed those drugs, and ultimately, a higher number of overdose deaths in that county. More specifically, Knight (2019) estimated that "for each three additional payments made to physicians per 100,000 people in a county, opioid overdose deaths [increased by] 18 percent." Hadland et al. (2019) emphasize that "amid a national opioid overdose crisis, reexamining the influence of the pharmaceutical industry [is] warranted."

[27] Even though the two year lagged rate would only leave two years of observations for estimation, I run the IV models with lagged specifications to maintain consistency with my preferred model from Section 6.2.

[28] Medicare Part D files, the dataset that yields instrument data, are only available from 2013 to 2016. As a result, the IV regression is limited to time span for when the instrument data is available.

Table 6.11: First-Stage Estimates

	Providers in Top 5% Opioid Prescriptions/100,000 Population	Providers in Top 1 Percent Opioid Doses/100,000 Population
Prime-Age Males		
$Rates_{t}$	.01824***	.01669***
	(.00006)	(.00018)
$Rates_{t-1}$	.00965***	.03017***
	(.00006)	(.00027)
$Rates_{t-2}$	-.00382***	.02511***
	(.00008)	(.00041)
Prime-Age Females		
$Rates_{t}$	.01838***	.01625***
	(.00006)	(.00017)
$Rates_{t-1}$	.00955***	.03025***
	(.00006)	(.00027)
$Rates_{t-2}$	-.00400***	02504***
	(.00008)	(.00040)
Prime-Age Population		
$Rates_{t}$	.01831***	.01647***
	(.00004)	(.00012)
$Rates_{t-1}$	.00960***	.03021***
	(.00004)	(.00019)
$Rates_{t-2}$	-.00391***	.02507***
	(.00006)	(.00029)

Robust standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Indeed, the connection between big pharma and the heavy marketing towards prescribers has caught the attention of states, policymakers, and the media. Purdue Pharma has been fighting a mounting pile of lawsuits from twenty-six states, with New York recently adding a lawsuit to the mix and alleging that Purdue "played down the health risks and overpromoted its signature opioid painkiller to bolster sales" (Porter Jr, 2018). Unsurprisingly, this issue has reached the Food and Drug Administration, with FDA Commissioner Scott Gottlieb proposing that the agency would approve new opioid painkillers only if comparative advantages over existing drugs in reducing pain could be proven (Burton, 2019). Alongside newly invigorated lawsuits and proposed regulations hitting pharmaceutical companies, a slew of state-wide initiatives have been targeting prescribers. One regulation requires clinicians who prescribe controlled substances, which include opioid medication, to continue medical education. As some of these initiatives are recent, their impact on improving prescribing behavior is only beginning to take effect in the healthcare space.

I also test for the exogeneity of the instruments using the Hansen J test. The intention behind employing a two-stage instrumental variables approach is to account for endogeneity in the explanatory variables, and thus establish a clearer, causal link between the endogenous regressor and the outcome variable. If the instrument violates orthogonality conditions, or is correlated with the outcome variable itself, it defeats the purpose behind utilizing the IV model. In addition, the Hansen J test can be used only if the model is overidentified; as prescription rates is the only endogenous regressor in question, and there are two instruments, the exogeneity of the instruments can be tested. Ideally, we would not want to reject the null hypothesis of the J test, which assumes exogeneity in the instruments. The corresponding p-values among the IV various model variants of the Hansen J test range from 0.29 to 0.84, indicating that the instruments satisfy orthogonality conditions. In addition, given a R^2 value of 0.7252, the instruments are strongly correlated with the endogenous regressor, suggesting that the instruments are relevant. The results from the combination of these tests indicate that the instruments are valid.

Table 6.12 displays results from the second stage IV regression, complete with state and year fixed effects and lagged specifications.[29] Table 6.13 summarizes the sign and significance of the coefficient estimates for the all three rates. Due to data limitations, the IV regression estimates the effects of opioids on labor from 2013 to

[29] Table 6.12 contains results only for the two year lag IV model. To see results for the contemporaneous rate and one year lag rate, see Tables A9 and A10 in the Appendix, respectively.

Table 6.12: IV GMM2S Regression Estimates with 2 Year Lag, 2013-2016

VARIABLES	(1) All labor	(2) All labor	(3) Men labor	(4) Women labor
2 Year Lag Rate	-0.0787***	0.1913**	0.1918*	0.2025
	(0.0020)	(0.0878)	(0.1058)	(0.1375)
Female	-0.1420***	-0.1420***		
	(0.0006)	(0.0006)		
Married	0.0144***	0.0138***	0.1001***	-0.0659***
	(0.0006)	(0.0006)	(0.0007)	(0.0009)
White	0.0396***	0.0407***	0.0400***	0.0437***
	(0.0018)	(0.0018)	(0.0022)	(0.0028)
Black	0.0108***	0.0132***	-0.0260***	0.0386***
	(0.0020)	(0.0020)	(0.0025)	(0.0031)
Hispanic	-0.0386***	-0.0340***	0.0149***	-0.0883***
	(0.0008)	(0.0008)	(0.0009)	(0.0014)
Asian	-0.0011	0.0024	0.0301***	-0.0188***
	(0.0021)	(0.0021)	(0.0025)	(0.0033)
Age	0.0138***	0.0139***	0.0130***	0.0126***
	(0.0003)	(0.0003)	(0.0004)	(0.0005)
Age Squared/1000	-0.1872***	-0.1879***	-0.1979***	-0.1568***
	(0.0043)	(0.0043)	(0.0053)	(0.0066)
Mid Atlantic	-0.0312***			
	(0.0013)			
East North Central	-0.0008			
	(0.0013)			
West North Central	0.0264***			
	(0.0013)			
South Atlantic	-0.0110***			
	(0.0013)			
East South Central	-0.0214***			
	(0.0019)			
West South Central	-0.0219***			
	(0.0014)			
Mountain	-0.0174***			
	(0.0014)			
Pacific	-0.0347***			
	(0.0013)			
Constant	0.6891***	0.3052**	0.3041**	0.1971
	(0.0070)	(0.1196)	(0.1441)	(0.1872)
Observations	2,450,205	2,450,205	1,180,064	1,270,141
R-squared	0.0395	0.0409	0.0396	0.0181
State FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

2016, the period when prescribing rates declined heavily by 11.6 percentage points. The results continues the trend in the earlier estimates, where opioid prescriptions are estimated to have a positive effect on labor. While the contemporaneous rate shows insignificance, Table 6.13 shows that both of the lagged rates are estimated to have a positive, significant effect.[30] However, these results are only for the regressions where state and year fixed effects are incorporated. As I observed in the results from the earlier models, when only regional controls are included and state and year fixed effects are excluded, all three rates have a negative, significant effect on labor force participation.

Table 6.13: Estimated Coefficients Based on Change in Opioid Prescriptions, 2013-2016

	Sign	Significance
IV GMM2S Regression Δ opioid prescription rate = −11.6%		
$Rates_{t}$	+	NO
$Rates_{t-1}$	+	YES
$Rates_{t-2}$	+	YES

These findings are striking. Harris et al. (2019) found significant, negative effects of prescription opioids on labor force participation within this time period, using the same instruments generated from Medicare Part D files. However, the data sample used in their study included ten states, which constituted only 41.4% of the U.S. population in 2012. My dataset and methodological approach examines all fifty states, including the District of Columbia, with state-level prescription rate panel data. Again, only when state and year fixed effects are included, the estimated effect of opioids on labor become positive, for both the extended model

[30] Significant, positive estimates apply only to the general prime-age population and for prime-age males. For prime-age females, while the estimated effects are positive for each rate, they are not significant.

and the IV model. Once state and year fixed effects are excluded, every model variant estimates a negative, significant effect of prescription opioids on labor force participation.

Lastly, the estimated coefficients for the two year lagged rate are considerably greater in magnitude than the contemporaneous and one year lagged rates. The estimated change in the labor force participation rate with a one year lag is 0.0770 for the general prime-age population. In contrast, the estimated change in labor force participation with the two year lag model is 0.1913. I expect the inflated estimate stems from the limited sample period, where the two year lagged model has only two years of observations to estimate the effect of prescribing rates on participation.

Due to the widely fluctuating estimates from the one year and two year lag, the results from the two-stage IV regression are best taken as complimentary evidence to the earlier results from the extended linear probability model with fixed effects: opioids at a healthy, appropriate level can have a positive effect on labor force participation. As only a few years have passed since the substantial decline in prescribing rates, I expect that once more data becomes available, a clearer, positive effect of opioids on labor might be observed.

7 Conclusion

The opioid crisis has undoubtedly had a deleterious impact on labor force participation and the broader economy. As prime-age labor force participation rates continue to decline, the opioid-labor epidemic is only beginning to be understood by economists and policymakers. As Mericle (2017) notes, "the opioid epidemic is intertwined with the story of declining prime-age participation." Evidence from early literature strongly supports this statement, pointing to significant, negative effects of prescription opioids on labor market outcomes. For the period 1999 to 2015, Krueger (2017) estimates that a 3.5 factor growth in prescription opioids was responsible for about 43% of the decline in labor force participation for prime-age males, and about 70% of the decline for prime-age females. Harris et al. (2019) examine general county-level labor market outcomes, and find that a ten percent increase in prescription causes a 0.56 percentage point reduction in labor force participation.

My paper combines the stratification between gender and age group that Krueger (2017) implements, and the instrumental variable approach Harris et al. (2019) executes. Fusing together the best elements of the data and methodology from these two studies, I extend the analysis of the opioid-labor dynamic. Linking state-level prescription rate data from the CDC to weighted, individual-level observation data from the CPS, I find that when prescribing rates increased by 8.9 percentage points in 2006 to 2012, prescription opioids had a significant, negative effect on the labor force participation rate of the general prime-age population. Prescription opioids were responsible for about 26% of labor participation decline in prime-age males, 37% of the decline in prime-age females, and 28% of the decline among the prime-age population. From 2006 to 2017, when prescribing rates experienced a net decline of 13.7 percentage points, I find that prescription opioids had a positive, significant effect on labor force participation. The decrease in opioid prescribing rates during this time period was responsible for 7.9% of the decline in the participation rate for prime-age males, 51% for prime-age females, and 22% for the prime-age population.

These results contradict the findings from the early literature, which consistently identified adverse effects on labor market outcomes from prescription opioids, even during periods of declining prescription rates. My results suggest that when prescribing rates are at a more healthy, normal level, opioids have a positive effect on labor force participation. Conversely, a positive effect of opioids during a time period when labor force participation declines indicates that decreasing opioid prescriptions have a depressing effect on labor force participation when prescribing rates are at an appropriate level.

Interestingly, when fixed effects are excluded, every model variant consistently generates negative, significant effects of opioids on labor. In contrast, when state and year fixed effects are included in the models, the estimated effect of opioids on labor turns positive. Fixed effects allow the model to control for unobserved intra-state variation over time. I theorize that including fixed effects dilute the adverse effects of high-opioid states, and correspondingly estimates a clearer effect on the aggregate level. To test my theory, I drop the top ten states with the highest prescription rates in 2012 from the model and rerun the regressions. Every model variant subsequently estimates both a positive and significant effect of opioids on labor.[31] These results align with the rationale that the concentration of high opioid states bias the estimates, unless state and year fixed effects are included. Future

[31] See Table A7 and A8 to see the regression results when the top ten high-opioid states are excluded from the model, with a contemporaneous lag and a two year lag, respectively.

studies that include state and year fixed effects can support or contradict these findings. In addition, I only focus on the prime-age working population, but the models and dataset could further develop the analysis for different age groups, and for other particular demographic groups.

This study carries some structural limitations in the data and model. First, these findings do not necessarily suggest that easing access to higher supplies of prescription opioids will improve labor market outcomes or welfare. The estimated positive effect on labor from opioids includes current state initiatives that brought the prescribing rate down from abnormally high levels to current levels. Second, the data on opioids includes only legally obtained opioids. While prescription opioids may serve as a beneficial influence on labor force participation, this study only captures this effect for legal opioids. According to the American Society of Addiction Medicine, four in five new heroin users start out misusing prescription painkillers. 94% of respondents in a 2014 survey of people undergoing treatment for opioid addiction said they chose to use heroin because prescription opioids were "far more expensive and harder to obtain" (ASAM). Thus, the results of the study most likely do not reflect all legitimate uses of prescription opioids. With current available data, it is very difficult to separately identify the effects of legally prescribed opioids and illicit opioids. There is a large difference between use and abuse of opioids; while controlled, healthy use might positively impact labor force participation, abuse could certainly do the opposite. Therefore, future extensions of this study could focus on establishing the extent to which prescription opioid use is legitimate.

Another important caveat of these results is omitted variable bias. Despite addressing potential endogeneity with an instrumental variables approach, accounting for unobserved variation within states over time, and controlling for demographic characteristics, there are still potential omitted variables that could heavily bias the results. For example, Krueger (2017) acknowledges that other variables, such as workers' health conditions and pain that drive demand for pain medication, could also be correlated with prescription opioid rates. Krueger (2017) further controls for labor participation with Chinese import variables from Autor (2013). These controls were not included in my model. Harris et al. (2019) also remark on the role of disability rates on prescription opioids, citing the disentangling of disability from prescription opioids as an area for future work.

This is the first paper to examine the effect of opioid prescription rates on labor force participation rates using state and year fixed effects for all fifty states and the District of Columbia. Most likely a result of the fixed effects, the findings from my study contradict the early literature that points to negative effects on labor outcomes from opioids. These results do not understate the severity and harmful impact of the opioid crisis, but rather provide evidence suggesting that prescription opioids are not completely harmful to labor force participation. Hopefully, these results can develop our elementary understanding of the complex dynamic between opioids and labor market outcomes, and further the discussion surrounding the best approach for tackling the ongoing crisis.

Appendix

Table A1: Total Number and Rate of Opioid Prescriptions Dispensed, United States, 2006–2017

Year	Total Number of Prescriptions	Prescribing Rate Per 100 Persons
2006	215,917,663	72.4
2007	228,543,773	75.9
2008	237,860,213	78.2
2009	243,738,090	79.5
2010	251,088,904	81.2
2011	252,167,963	80.9
2012	255,207,954	81.3
2013	247,090,443	78.1
2014	240,993,021	75.6
2015	226,819,924	70.6
2016	214,881,622	66.5
2017	191,218,272	58.7

Table A2: Total Number and Percentage of Counties with Available Opioid Prescribing Data, United States, 2006–2017

Year	Number of Counties (Total)	Number of Counties (with Available Data)	Percentage of Counties (with Available Data)
2006	3143	2754	87.6
2007	3143	2746	87.4
2008	3143	2758	87.8
2009	3143	2750	87.5
2010	3143	2741	87.2
2011	3142	2745	87.4
2012	3142	2736	87.1
2013	3142	2753	87.6
2014	3142	2960	94.2
2015	3142	2963	94.3
2016	3142	2962	94.3
2017	3142	2955	94

Table A3: Logit Regression Model with Log Opioid Rates for Labor Force Participation of Prime-Age Population, 2006-2008 and 2010-2012

VARIABLES	(1) labor	(2) labor	(3) labor	(4) labor	(5) labor
Time Period	-0.0865***		-0.0994***	-0.0918***	-0.0915***
	(0.0030)		(0.0041)	(0.0042)	(0.0041)
Log Opioid Rate		-0.1600***	-0.1210***	-0.2212***	-0.2538***
		(0.0041)	(0.0083)	(0.0085)	(0.0101)
Log Rate x Time			-0.0524***	-0.0638***	-0.0612***
			(0.0116)	(0.0118)	(0.0115)
Married				0.0837***	0.0815***
				(0.0033)	(0.0033)
Female				-1.0628***	-1.0633***
				(0.0033)	(0.0033)
White				0.2951***	0.2788***
				(0.0090)	(0.0091)
Black				0.0491***	0.0398***
				(0.0099)	(0.0101)
Hispanic				-0.3296***	-0.2942***
				(0.0043)	(0.0044)
Asian				0.0009	0.0197*
				(0.0111)	(0.0111)
Age				0.0898***	0.0903***
				(0.0018)	(0.0018)
Age Squared/1000				-1.2160***	-1.2209***
				(0.0231)	(0.0231)
Mid Atlantic					-0.1872***
					(0.0077)
East North Central					-0.0510***
					(0.0075)
West North Central					0.1646***
					(0.0081)
South Atlantic					-0.0364***
					(0.0073)
East South Central					-0.1849***
					(0.0100)
West South Central					-0.1577***
					(0.0079)
Mountain					-0.0920***
					(0.0080)
Pacific					-0.1857***
					(0.0074)
Constant	1.5878***	1.4719***	1.5577***	0.3697***	0.4592***
	(0.0021)	(0.0014)	(0.0029)	(0.0364)	(0.0371)
Observations	3,953,433	7,652,489	3,953,433	3,953,433	3,953,433

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A4: Odds Ratio Estimates with Log Opioid Rates for Labor Force Participation of Prime-Age Population, 2006-2008 and 2010-2012

VARIABLES	(1) labor	(2) labor	(3) labor	(4) labor	(5) labor
Time Period	0.9171***		0.9005***	0.9083***	0.9115***
	(0.0028)		(0.0037)	(0.0038)	(0.0037)
Log Opioid Rate		0.8639***	0.9044***	0.8131***	0.7775***
		(0.0035)	(0.0075)	(0.0069)	(0.0079)
Log Rate x Time			0.9281***	0.9218***	0.9363***
			(0.0106)	(0.0108)	(0.0107)
Married				1.0876***	1.0852***
				(0.0036)	(0.0036)
Female				0.3455***	0.3453***
				(0.0011)	(0.0011)
White				1.3428***	1.3211***
				(0.0121)	(0.0120)
Black				1.0505***	1.0418***
				(0.0105)	(0.0105)
Hispanic				0.7188***	0.7441***
				(0.0031)	(0.0033)
Asian				1.0015	1.0198*
				(0.0111)	(0.0113)
Age				1.0939***	1.0945***
				(0.0020)	(0.0020)
Age Squared/1000				0.2965***	0.2950***
				(0.0069)	(0.0068)
Mid Atlantic					0.8307***
					(0.0064)
East North Central					0.9522***
					(0.0072)
West North Central					1.1937***
					(0.0096)
South Atlantic					0.9613***
					(0.0070)
East South Central					0.8303***
					(0.0083)
West South Central					0.8592***
					(0.0068)
Mountain					0.9086***
					(0.0073)
Pacific					0.8307***
					(0.0062)
Constant	4.8931***	4.3753***	4.7745***	1.4557***	1.5852***
	(0.0105)	(0.0062)	(0.0137)	(0.0530)	(0.0587)
Observations	3,953,433	7,652,489	3,953,433	3,953,433	3,953,433

Robust seeform in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A6: Extended Linear Probability Model with Fixed Effects, 2 Year Lag, 2006 to 2012

VARIABLES	(1) Men labor	(2) Women labor	(3) All labor
2 Year Lag Rate	-0.0536***	-0.0375*	-0.0408***
	(0.0166)	(0.0221)	(0.0140)
Female			-0.1412***
			(0.0005)
Married	0.0961***	-0.0640***	0.0135***
	(0.0006)	(0.0008)	(0.0005)
White	0.0586***	0.0409***	0.0477***
	(0.0021)	(0.0025)	(0.0016)
Black	-0.0115***	0.0256***	0.0132***
	(0.0023)	(0.0027)	(0.0018)
Hispanic	0.0036***	-0.0905***	-0.0386***
	(0.0008)	(0.0012)	(0.0007)
Asian	0.0440***	-0.0096***	0.0145***
	(0.0023)	(0.0030)	(0.0019)
Age	0.0131***	0.0113***	0.0132***
	(0.0004)	(0.0005)	(0.0003)
Age Squared/1000	-0.2009***	-0.1366***	-0.1783***
	(0.0044)	(0.0057)	(0.0036)
Constant	0.6351***	0.5411***	0.6325***
	(0.0214)	(0.0283)	(0.0180)
Observations	1,574,392	1,693,188	3,267,580
R-squared	0.0403	0.0170	0.0420
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A7: Extended Linear Probability Model with Fixed Effects Excluding Top Ten High-Opioid States, No Lag, 2006-2017

VARIABLES	(1) Men labor	(2) Women labor	(3) All labor
Prescription Rate	0.0218**	0.0083	0.0167***
	(0.0085)	(0.0064)	(0.0054)
Female			-0.1432***
			(0.0003)
Married	-0.0738***	0.0924***	0.0067***
	(0.0006)	(0.0004)	(0.0004)
White	0.0361***	0.0467***	0.0396***
	(0.0017)	(0.0014)	(0.0011)
Black	0.0204***	-0.0202***	0.0064***
	(0.0019)	(0.0016)	(0.0013)
Hispanic	-0.0892***	0.0064***	-0.0377***
	(0.0008)	(0.0005)	(0.0005)
Asian	-0.0187***	0.0334***	0.0044***
	(0.0020)	(0.0016)	(0.0013)
Age	0.0105***	0.0132***	0.0129***
	(0.0003)	(0.0002)	(0.0002)
Age Squared/1000	-0.1262***	-0.1977***	-0.1720***
	(0.0040)	(0.0031)	(0.0026)
Constant	0.5724***	0.6043***	0.6338***
	(0.0084)	(0.0065)	(0.0054)
Observations	3,434,734	3,203,430	6,638,164
R-squared	0.0192	0.0366	0.0423
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A8: Extended Linear Probability Model with Fixed Effects Excluding Top Ten High-Opioid States, 2 Year Lag, 2006-2017

	(1)	(2)	(3)
	Men	Women	All
VARIABLES	labor	labor	labor
Prescription Rate	0.0218**	0.0083	0.0167***
	(0.0085)	(0.0064)	(0.0054)
Female			-0.1432***
			(0.0003)
Married	-0.0738***	0.0924***	0.0067***
	(0.0006)	(0.0004)	(0.0004)
White	0.0361***	0.0467***	0.0396***
	(0.0017)	(0.0014)	(0.0011)
Black	0.0204***	-0.0202***	0.0064***
	(0.0019)	(0.0016)	(0.0013)
Hispanic	-0.0892***	0.0064***	-0.0377***
	(0.0008)	(0.0005)	(0.0005)
Asian	-0.0187***	0.0334***	0.0044***
	(0.0020)	(0.0016)	(0.0013)
Age	0.0105***	0.0132***	0.0129***
	(0.0003)	(0.0002)	(0.0002)
Age Squared/1000	-0.1262***	-0.1977***	-0.1720***
	(0.0040)	(0.0031)	(0.0026)
Constant	0.5724***	0.6043***	0.6338***
	(0.0084)	(0.0065)	(0.0054)
Observations	3,434,734	3,203,430	6,638,164
R-squared	0.0192	0.0366	0.0423
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A9: IV GMM2S Regression Estimates, No Lag, 2013-2016

	(1)	(2)	(3)	(4)
	All	All	Men	Women
VARIABLES	labor	labor	labor	labor
Prescription Rate	-0.0839***	0.0417	0.0240	0.0630
	(0.0022)	(0.0341)	(0.0405)	(0.0536)
Female	-0.1420***	-0.1420***		
	(0.0006)	(0.0006)		
Marital Status	0.0145***	0.0139***	0.1001***	-0.0659***
	(0.0006)	(0.0006)	(0.0007)	(0.0009)
White	0.0396***	0.0408***	0.0400***	0.0438***
	(0.0018)	(0.0018)	(0.0022)	(0.0028)
Black	0.0110***	0.0132***	-0.0260***	0.0387***
	(0.0020)	(0.0020)	(0.0025)	(0.0031)
Hispanic	-0.0388***	-0.0340***	0.0149***	-0.0883***
	(0.0008)	(0.0008)	(0.0009)	(0.0014)
Asian	-0.0012	0.0025	0.0302***	-0.0187***
	(0.0021)	(0.0021)	(0.0025)	(0.0033)
Age	0.0138***	0.0139***	0.0130***	0.0125***
	(0.0003)	(0.0003)	(0.0004)	(0.0005)
Age Squared/1000	-0.1872***	-0.1878***	-0.1979***	-0.1567***
	(0.0043)	(0.0043)	(0.0053)	(0.0066)
Mid Atlantic	-0.0292***			
	(0.0013)			
East North Central	-0.0000			
	(0.0013)			
West North Central	0.0288***			
	(0.0013)			
South Atlantic	-0.0096***			
	(0.0013)			
East South Central	-0.0223***			
	(0.0019)			
West South Central	-0.0213***			
	(0.0014)			
Mountain	-0.0164***			
	(0.0014)			
Pacific	-0.0340***			
	(0.0013)			
Constant	0.6867***	0.5134***	0.5350***	0.3941***
	(0.0070)	(0.0431)	(0.0511)	(0.0677)
Observations	2,450,205	2,450,205	1,180,064	1,270,141
R-squared	0.0395	0.0410	0.0397	0.0181
State FE	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A10: IV GMM2S Regression Estimates, 1 Year Lag, 2013-2016

VARIABLES	(1) All labor	(2) All labor	(3) All labor	(4) All labor
1 Year Lag Rate	-0.0800***	0.0770*	0.0580	0.1015
	(0.0021)	(0.0422)	(0.0504)	(0.0662)
Female	-0.1420***	-0.1420***		
	(0.0006)	(0.0006)		
Marital Status	0.0144***	0.0139***	0.1001***	-0.0659***
	(0.0006)	(0.0006)	(0.0007)	(0.0009)
White	0.0396***	0.0408***	0.0400***	0.0438***
	(0.0018)	(0.0018)	(0.0022)	(0.0028)
Black	0.0109***	0.0132***	-0.0260***	0.0387***
	(0.0020)	(0.0020)	(0.0025)	(0.0031)
Hispanic	-0.0388***	-0.0340***	0.0149***	-0.0883***
	(0.0008)	(0.0008)	(0.0009)	(0.0014)
Asian	-0.0012	0.0025	0.0302***	-0.0187***
	(0.0021)	(0.0021)	(0.0025)	(0.0033)
Age	0.0138***	0.0139***	0.0130***	0.0125***
	(0.0003)	(0.0003)	(0.0004)	(0.0005)
Age Squared/1000	-0.1872***	-0.1878***	-0.1979***	-0.1567***
	(0.0043)	(0.0043)	(0.0053)	(0.0066)
Mid Atlantic	-0.0304***			
	(0.0013)			
East North Central	-0.0008			
	(0.0013)			
West North Central	0.0276***			
	(0.0013)			
South Atlantic	-0.0109***			
	(0.0013)			
East South Central	-0.0227***			
	(0.0019)			
West South Central	-0.0220***			
	(0.0014)			
Mountain	-0.0174***			
	(0.0014)			
Pacific	-0.0346***			
	(0.0013)			
Constant	0.6878***	0.4648***	0.4891***	0.3401***
	(0.0070)	(0.0555)	(0.0664)	(0.0870)
Observations	2,450,205	2,450,205	1,180,064	1,270,141
R-squared	0.0395	0.0410	0.0397	0.0181
State FE	No	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure A1: Labor Force Participation Rate for Prime-Age Males and Females

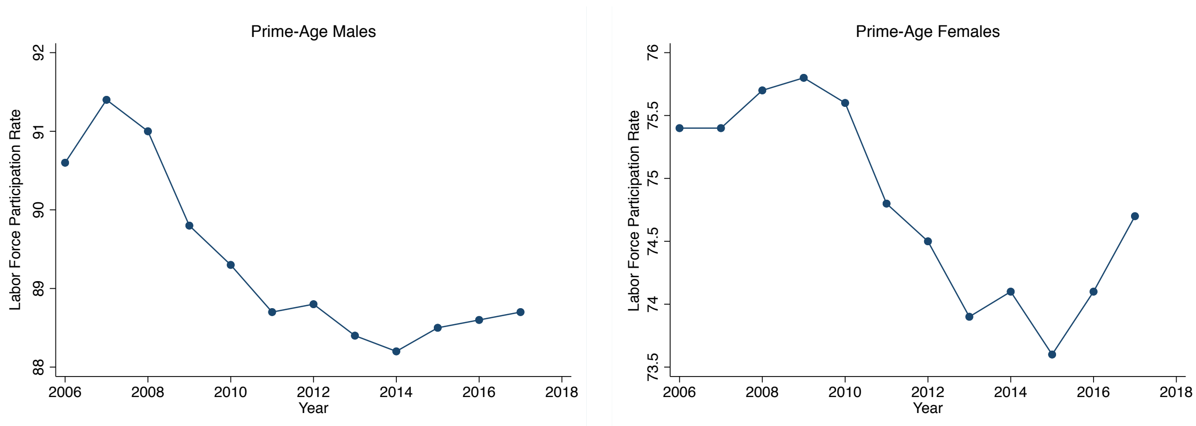


Figure A2: National Opioid Prescribing Rates, 2006-2017

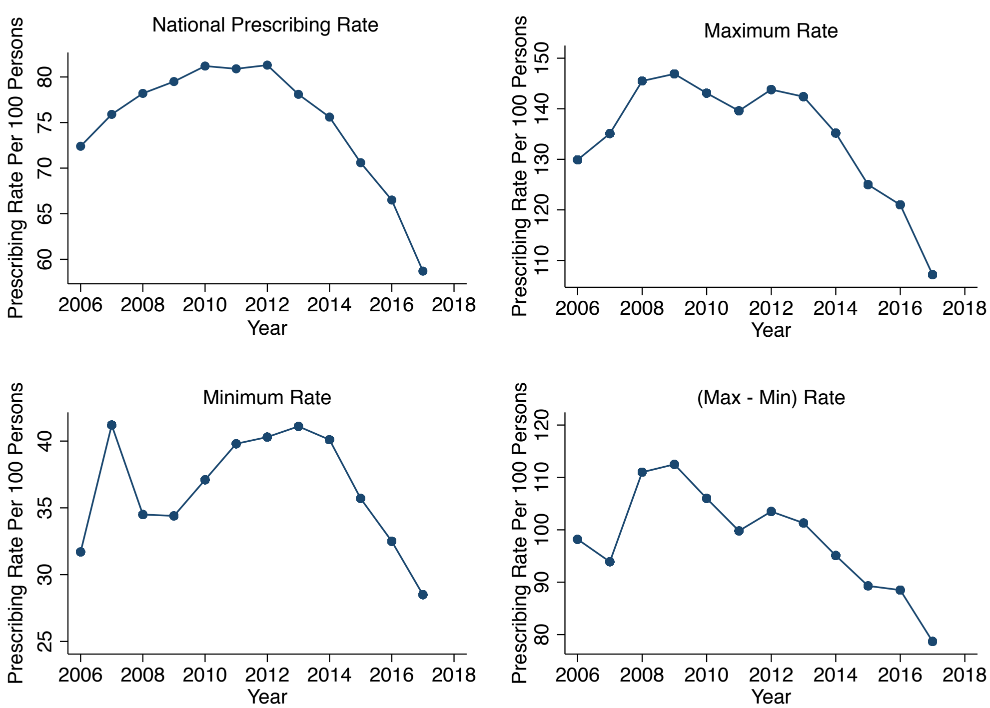
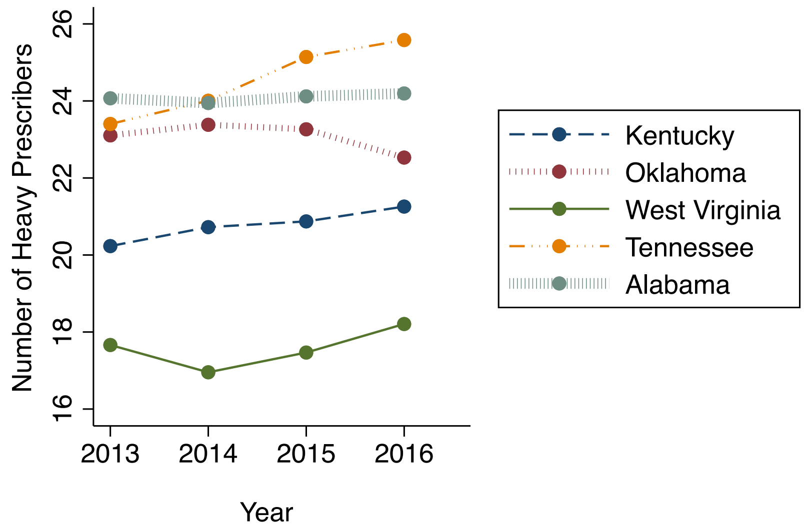


Figure A3: Number of Heavy Prescribers in High-Opioid States, 2013-2016



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David Kim