Pleasure and Pain: The Connection Between Opioid Prescriptions and Social Security Disability Insurance in the U.S.

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I examine the connection between prescription opioid use and Social Security Disability Insurance in the United States. Previous research has found that a correlation exists between opioids and disability rates, but little work has been done to understand whether a causal relationship exists between the two. I perform a county-level analysis using two exogenous shocks to prescription opioid rates, a 2010 reformulation of OxyContin and the implementation of Prescription Drug Monitoring Programs, to examine whether prescription opioid rates affect disability rates. The reformulation strategy yields no significant results. The strategy exploiting cross-state differences in mandatory Prescription Drug Monitoring Program implementation timing, however, yields results that are negative and significant. This finding supports previous research and suggests that increasing prescription rates cause more people to become addicted to opioids and try to leave the labor market through channels including disability insurance.

I. Introduction

Rising disability insurance and opioid abuse rates have dominated news headlines over the past decade. The percent of working-age adults receiving Social Security Disability Insurance (SSDI) doubled between 1989 and 2009, from 2.3% to 4.6% (Autor 2011). This growth led to an almost 200 billion additional dollars spent on either SSDI payments or Medicare expenses from SSDI recipients (Autor 2011). The rise in opioid use and abuse has been even more dramatic. Between 1990 and 2010, the number of opioid prescriptions tripled and by 2014, over four million Americans used pain medication recreationally (Buchmueller and Carey 2018; Evans, Leiber, and Power 2019). In 2017, enough opioid medication was dispensed that every person in America could take pain medication every day for a month (Krueger 2017). The opioid epidemic and rising SSDI rates are both, at their root, health crises. Furthermore, both phenomena have been geographically concentrated in similar areas, leading researchers to believe that the two are related in some way (Krueger 2017; GAO 2020). To curb the health-related and pecuniary harm incurred from rising SSDI and opioid prescription rates, it is important to understand all the causes and consequences of these two epidemics and whether the two epidemics are causally related to each other.

SSDI rolls began rapidly increasing after a period of stagnation because of the 1984 liberalization of the program. This reform made the application process more favorable to individuals suffering from musculoskeletal injuries (i.e. back pain) and mental illness. In 1983, only 13% of SSDI recipients were on disability rolls because of musculoskeletal disorders. By 2003, that figure had doubled to 26% (Autor and Duggan 2006). Overall, SSDI recipients shifted from having life-threatening conditions like cancer or heart disease to individuals with non-life-threatening chronic pains. This shift has caused people to stay on SSDI for longer, driving up enrollment even further (Autor and Duggan 2003).

The rise in opioid use cannot be tied to one event like the 1984 liberalization of SSDI. Instead, the opioid epidemic must be attributed to changing culture in medicine, accelerated by successful lobbying by large pharmaceutical companies. Opioids have been used as pain relievers since the invention of opioids in the early 19th century (Rieder 2019). Rieder (2019) describes medical opinion about opioid use as a pendulum, swinging away from opioid prescriptions in the mid-1900s, then back towards heavy use in the 1990s and into the 2000s. The pendulum started swinging back away from opioids around 2014, but not before the average physician was writing over 200 opioid prescriptions per year (Schnell and Currie 2018). While opioids are a powerful, important tool to relieve pain, they also pose risks, as 26% of patients receiving opioids may become dependent or addicted (Frieden and Houry 2016).

While SSDI and opioid use are interesting and important individually, the connection between the two is equally necessary to understand. Does increased opioid use cause someone to want to get out of the labor force, turning to disability insurance? Are doctors more likely to prescribe opioids to individuals on SSDI? Does the Medicare that comes with SSDI receipt allow SSDI recipients to afford more prescription pain killers? Unfortunately, these questions, along with many related ones, remain unanswered. Many researchers have explored how opioids and SSDI affect and are affected by various outcomes, but few have analyzed how they interact with each other.

Figure 1 shows the overlap in which counties are in the top third of the distribution for disability insurance and opioid prescription rates. The overlap shown in this map makes it clear that there is some sort of relationship between disability insurance and prescription opioid use.

The question remains, however, whether that relationship is causal. This paper will address that gap in the literature.

The two main issues plaguing research about the connection between opioids and SSDI are reverse causality and omitted variable bias. Many recent studies have shown that opioid use is highly correlated with adverse labor market outcomes, including disability applications and receipt. It is unclear, however, which direction causality flows. Furthermore, many variables that likely affect both disability applications and opioid prescriptions can be difficult or impossible to measure, such as doctor culture, safety standards, or poor labor market opportunities. While some of these difficult-to-measure confounding factors can be captured using a fixed-effects model, I am not confident that all these unmeasurable variables are constant over time within a specific place. I implement two empirical strategies to overcome endogeneity barriers and examine whether prescription opioid use in the United States has a causal effect on SSDI rates.

My first strategy is to examine a 2010 reformulation of OxyContin, which was an exogenous shock that decreased prescription opioid rates. This reformulation was a demand shift, decreasing OxyContin prescription rates across the country. Being an exogenous demand shift, the 2010 OxyContin reformulation should have no effect on disability rates outside of through the change in prescription rate. I exploit pre-reformulation prescription rate differences across counties and find that counties with higher pre-reformulation prescription rates did not see a change in SSDI rates that was significantly different than the change that lower pre-reformulation prescription rate counties saw. If I had found a significant result, that would suggest that opioid prescription rates have a causal effect on SSDI rates. This result instead suggests that either prescription opioid rates do not affect SSDI rates or the nationwide shock did not yield enough county-level variation to make the county-level strategy viable.

My second strategy is to exploit the implementation of Prescription Drug Monitoring Programs (PDMPs) in states across the country to examine whether a different exogenous demand shock to opioid prescription rates yields the same results. I find that the exogenous shock to prescription opioid rates using PDMPs does have a statistically significant effect on SSDI rates. This suggests that the lack of significance in the reformulation result was due to a lack of county-level variation caused by the nationwide shock rather than evidence that a relationship does not exist. The results using PDMPs suggests that increases in prescription opioid rates increase disability rates. These PDMP results are consistent with previous findings that exogenous increases in prescription opioid rates cause people to leave the labor market through channels like disability insurance.

The paper proceeds as follows: Section II is a review of the current literature regarding opioids, SSDI, and the connection between the two. Section III explains the theory of why prescription opioids and SSDI may be connected. Section IV describes what data I use during my analysis, including a discussion of summary statistics. Section V lays out my identification strategies. Section VI is the results of my analysis and section VII is concluding remarks and discussion of future research.

II. Literature

The opioid epidemic

Since the mid-1990s, opioid addiction and overdose death rates have risen dramatically in the United States. Alpert et al. (2019) find that the introduction and large-scale marketing of OxyContin starting in 1996 was a driving force in the increase in opioid addiction and death. Metcalf and Wang (2019) explain that synthetic opioids like Fentanyl are also partially to blame for the spike in opioid addiction and disproportionately to blame for the spike in opioid overdose deaths. Between 1999 and 2017, opioid overdose deaths increased four-fold, from 17,000 to 70,000 per year. The path to addiction for opioids often starts with prescription opioids like OxyContin then sometimes moves to the black market. Metcalf and Wang explain that many overdose deaths occur when people addicted to opioids mix synthetic opioids with heroin.

While societal and prevailing medical views about prescription pain killers have certainly played a role in the opioid epidemic, pharmaceutical companies are also very much to blame. Approximately 50% of all prescription opioids during the peak of the epidemic, between 2006 and 2012, came from 15% of pharmacies (Abelson et al. 2018). When CEOs of the highest-producing pharmacies were asked about their role in the epidemic, they all denied having any role in the epidemic (O'Harrow and Highman 2018). However, internal documents from those same companies show that executives had knowledge of how detrimental and addictive opioids could be and continued to push their use, prioritizing profit over well-being (Higham, Horwitz, and Rich 2019). During this time period, drug companies pumped 76 billion pain pills into U.S. pharmacies despite knowing their addictive nature (Heller and Bernstein 2019). While we can certainly condemn and make moral statements about the opioid epidemic and what drug companies could and should have done, that is not the purpose of this paper. This paper looks at one specific outcome, SSDI, that may be connected in some way to the opioid epidemic.

The opioid epidemic and labor market outcomes

Many studies conducted over the last few years have investigated the connection between the opioid epidemic and a variety of economic and social outcomes. Aliprantis, Fee, and Schweitzer (2019) examine whether opioid usage rates affect labor market outcomes. The researchers regress county-level opioid prescription rates on labor market participation and find a negative and significant coefficient. They also find that reverse causality is unlikely because opioid rates neither spiked nor fell during or after the Great Recession, instead continuing on their same trend, leading the researchers to make causal interpretations of the negative coefficient. While this research is an important start to discovering the causes and effects of the opioid epidemic, it does not give us a full picture of how opioid use affects labor market outcomes.

Currie, Jin, and Schnell (2018) also use an identification strategy that allows them to make a causal interpretation of the effect of opioids on labor markets. They instrument overall prescription rate with elderly prescription rate and, differing from Aliprantis, Fee, and Schweitzer (2019), find that increasing prescription rates lead to an increase in employment. Currie, Jin, and Schnell (2018) posit that this result shows that opioids can help injured people get back to work quicker.

Multiple other papers find results contrary to Currie, Jin, and Schnell (2018), and instead corroborate Aliprantis, Fee, and Schweitzer's finding that opioid use either worsens or, at the very least, does not improve labor market outcomes. Harris et al. (2019) use Prescription Drug Monitoring Programs as an exogenous shock in opioid use and find a negative relationship between prescriptions and labor force participation. Savych, Neumark, and Lea (2019) use local opioid prescription patterns as an instrument for individual opioid use and find that long-term opioid prescriptions do not speed up the process of people returning to the labor market. Krueger (2017) finds that areas with higher opioid prescription rates had lower labor force participation rates but does not attempt to find a causal relationship. These differing results have led to be little consensus about how the opioid epidemic has affected labor market outcomes and further research in the topic is necessary.

Opioid epidemic and social outcomes

Researchers have also examined how opioid use affects social outcomes. Alpert, Powell, and Pacula (2018) look at the substitutability of OxyContin and heroin by exploiting the recent OxyContin reformulation. In 2010, Purdue Pharma released an abuse-deterrent formulation of OxyContin that could no longer be crushed, making OxyContin more difficult to abuse because it could no longer snorted or injected. Alpert et al. (2018) find that, while the reformulation lowered OxyContin overdoses, it also caused heroin overdoses and deaths to increase, offsetting the decrease in OxyContin deaths. Evans, Lieber, and Power (2019) find similar results but add that total overdose deaths may decrease in the long run. Park and Powell (2019) use this 2010 OxyContin reformulation to try to uncover the effects of opioids on various labor market outcomes. Park and Powell do a state-level time trend analysis exploiting pre-reformulation variation in illicit opioid rates and find a marginally significant relationship only between opioid usage and disability rates. Park and Powell find no significant results for other measures of aggregate employment.

Buchmueller and Carey (2018) and Harris et al. (2019), use Prescription Drug Monitoring Programs (PDMPs), to examine opioid usage rates. Prescription Drug Monitoring Programs allow, and sometimes even require, doctors to access a patient’s prescription history before prescribing them opioids. These programs became increasingly popular in the late 2000s as opioid abuse increased. Buchmueller and Carey (2018) find that only the PDMPs that required doctors to use them were successful in curbing opioid use, but those mandatory PDMPs did significantly reduce prescription rates in the states in which they were implemented.

Researchers have also been studying various other topics related to opioid use and abuse. For example, Rice et al. (2014) examine the pecuniary costs of opioid use on employers and find that opioid abusers cost employers money both through increased healthcare costs and increased work-loss costs. Schnell and Currie (2018) find that physicians who attended better ranked medical schools write significantly fewer opioid prescriptions, suggesting that physician education matters in this epidemic. Because the epidemic is a recent and ongoing phenomenon, much more research is and must be conducted about its causes and effects.

Disability Insurance

More studies have focused on SSDI than opioid use because SSDI has been on the forefront of political and social conversations for decades more than has opioid abuse. One series of papers has examined why people apply for disability insurance in the first place. There is a consensus in the literature that decreased health in the United States has not driven rising SSDI rolls (Autor 2011; Autor and Duggan 2003; Autor and Duggan 2006; Kessler et al. 2005; Duggan and Imberman 2008). Instead, overall health in the United States has improved while SSDI enrollment has gone up. If health does not significantly affect applications and enrollment, what does?

Black, Daniel, and Sanders (2002) use exogenous shocks in coal prices to examine how labor market conditions affected SSDI application rates in West Virginia. They find that long-term job creation and destruction had major impacts on SSDI application rates. During economic booms, when jobs are being created and wages are rising, disability application rates and receipt rates fall. When wages start to fall and jobs disappear, more people apply for SSDI. This story is consistent with the idea that, during hard economic times, individuals turn to SSDI as a source of income. Autor and Duggan (2006) also find that disability rolls and applications rise and fall with unemployment.

Hu et al. (2001) and Lahiri, Song, and Wixon (2008) deepen our understanding of what causes an individual to apply for SSDI. Hu et al. find that individuals who work in more hazardous conditions are more likely to apply for disability, *ceteris paribus*. Lahiri, Song, and Wixon identify a series of variables that are all correlated with propensity for one to apply for SSDI, including whether someone has health insurance, severity of an injury, availability of Medicare, and number of visits to the doctor. These analyses show that, while societal and economic factors affect one's decision to apply for SSDI, personal factors play into that decision as well.

Bound (1989) was one of the first papers to examine how the SSDI program affects labor market participation. Opponents of the program argue that SSDI disincentivizes work. Bound, however, finds this argument to be largely untrue. Bound examines labor market participation among rejected SSDI applicants and finds that, because rejected applicants are systematically different from SSDI recipients in that their disabilities are less severe, his results represent the maximum workforce involvement of SSDI recipients in a counterfactual in which disability insurance was not available. In other words, because rejected applicants are healthier than SSDI recipients, their workforce participation is the upper bound of what recipients' workforce participation would be if SSDI did not exist. Bound finds that in this counterfactual, at most 33% of current recipients would work under normal conditions, and at most 50% of current SSDI recipients would work at all.

Chen and Van der Klauuw (2008) build on Bound (1989) by looking at how the unemployment and labor force participation rates would have changed overall in the absence of the SSDI program. They find that, while there are work disincentives from SSDI, labor force participation among SSDI applicants would have been only 12-23 percentage points higher if SSDI did not exist, a bit smaller than Bound's findings. Chen and Van der Klauuw also conclude that the unemployment rate in 1998 would have been about half a percentage point higher in the absence of SSDI. Lahiri, Song, and Wixon (2008) corroborate Bound (1989) and find that, at most, 37% of disability beneficiaries would have returned to work if not for SSDI. They also find that most denied applicants did not return to work within seven years of their initial application. Von Wachter, Song and Manchester (2011) find results similar to Bound for older cohorts, but find that among younger cohorts, more individuals would return to the labor force if not for SSDI.

One reason that rejected applicants may have weak labor market attachment is because of the eligibility definition of SSDI. Someone is eligible if they cannot "engage in any substantial gainful activity by reason of any medically determinable physical or mental impairment which can be expected to result in death or to be of long-continued and indefinite duration" (Autor and Duggan 2006). As such, if someone's application is denied, but they wish to appeal that decision, as most individuals do, they must stay out of the labor force. If they get a job during the appeals process, they will automatically get denied. French and Song (2014) find that 60% of individuals denied benefits at the first stage of the application process end up receiving SSDI benefits within 10 years based on the appeals process. Furthermore, 10 years after their initial application, 6% of applicants are still in the appellate phase of the application process.

Connection between Opioid Use and Disability Insurance

Only a few papers have looked explicitly at the relationship between opioid use and SSDI. Morden et al. (2014) look at opioid usage rates among SSDI recipients, and find that, between 2007 and 2011, the number of opioid prescriptions for SSDI recipients increased while dosage of those prescriptions decreased. This suggests that doctors were writing more, but smaller, opioid prescription for SSDI recipients. Furthermore, Morden et al. find that about half of SSDI recipients had at least one opioid prescription written for them, while about 25% of SSDI recipients were chronic users, meaning they had at least 6 opioid prescriptions written for them over the course of a year. The researchers also find that an increasing number of SSDI recipients are chronic users, with many recipients filling at least 13 prescriptions in a calendar year, twice the amount that would deem them a chronic user. While this information helps us begin to understand the relationship in question, much more work is needed to find correlations and causal links between the opioids and SSDI. A GAO report from 2020 also finds that there exists a strong correlation between prescription opioid use and disability rates but do use an empirical strategy that yields a causal estimate.

Park and Powell (2019) analyze whether there exists a causal relationship between opioid use and disability rates. Park and Powell (2019) estimate an event study, exploiting the 2010 OxyContin reformulation as an exogenous shock to opioid use that may affect various labor market outcomes, including disability rates, over time. The authors are interested in illicit opioid use, however, rather than prescription rates, and use survey data asking about individual illicit opioid use. This strategy is not ideal for a few reasons. First, survey data is likely unreliable when asking about illicit substance use. Second, Park and Powell's data only identifies the state of residence, rather than the county. Disability rates vary significantly within states and between counties, making a county-level analysis more appropriate. Third, Park and Powell only use some states in their analysis, limiting external validity. This paper improves on Park and Powell's important contribution.

III. Theory

There are a few theories as to how opioid usage could affect disability rates. On the one hand, if someone is unable to work because of pain and can manage that pain only with opioids, prescription rates should decrease disability rates. Opioids can be a very effective tool to manage pain (Rieder 2019). If used effectively and correctly, they could help someone who is unable to work otherwise due to something like chronic back pain. In this case, an exogenous shock decreasing opioid prescriptions would increase disability rates.

On the other hand, if someone becomes addicted to opioids, they may be less likely to work. Prescription opioids can be used to help with pain management but can also be used for illicit, non-medical reasons. And, as previously mentioned, someone can start by using opioids for medical reasons then become addicted. While opioid addiction does not make someone eligible for SSDI, people with other conditions may be looking to exit the labor force after becoming addicted and may do so for reasons other than their addiction (Krueger 2017). If this is the case, an exogenous shock that decreases opioid prescriptions would decrease disability rates.

A third theory is that opioids generally affect disability rates, but *prescription* opioids do not affect disability rates. In other words, decreases in prescription opioid rates may be offset by increases in illicit opioid use. If people who are using prescription opioids for non-medical reasons substitute other opioids when faced with things like PDMPs and the OxyContin reformulation, there may be no change to their other behavior. If this substitution effect is in play from prescription opioids to illicit opioids, we would expect that an exogenous shock to prescription opioids without a corresponding exogenous shock to illicit use, which is what these two shocks are, would have no effect on disability rates.

IV. Data

I construct an annual, county-level dataset from 2006-2017 to look at how prescription opioid usage affects disability rates. My SSDI enrollment rate data comes from the Social Security Administration and county-level prescription rate data is from the Center for Disease Control (CDC). This CDC data includes the number of retail opioid prescriptions written per 100 U.S. residents for about 90% of counties from 2006-2017. The remaining county-level controls come from the 5-year American Community Survey averages, downloaded from Social Explorer.[1] All monetary variables are adjusted for inflation to 2017 dollars.

For my analysis using Prescription Drug Monitoring Programs as an exogenous shock to prescription opioid rates, I use information from Buchmueller and Carey (2018) on when various states implemented any PDMP and a mandatory, or "must access," PDMP.[2] Buchmueller and Carey find that only mandatory PDMPs have a significant effect on prescription rates. As such, I use Buchmueller and Carey's compilation of dates when states implemented mandatory PDMPs. If a state implemented a mandatory PDMP in June or before of a year, I code that year and all years after it with a 1. If a state implemented a mandatory PDMP in July or after of a year, I code all years after that year with a 1 and the year it was implemented and all years prior with a 0.

[1] I match the 5-year averages with the middle year of the average. For example, I use the 2012-2016 ACS with my 2014 data. Social Explorer's most recent ACS was 2013-2017, so I use that data for 2015, 2016, and 2017. The ACS on Social Explorer only goes back to 2006-2010, so I use that data for 2006, 2007, and 2008.

[2] A mandatory PDMP differs from a non-mandatory PDMP in that doctors are required to access it before prescribing opioids. A non-mandatory PDMP does not require a doctor to access it and, Buchmueller and Carey find that doctors ignore the database unless the law requires that they access it.

Summary Statistics

Table 1 shows the means for the variables of interest and controls at the county level. Counties that had an above-median prescription rate in 2009 are "high prescribing counties" and counties that had a below-median prescription rate in 2009 are "low prescribing counties." These initial summary statistics are encouraging. It is important to note the differences between high and low prescribing counties. These counties must be as good as randomly assigned, conditional on the controls. The summary statistics for the two categories show that that is likely the case. High prescribing counties are generally smaller than low prescribing counties with less education, lower income, more poverty, and less employment. This is all to be expected. The encouraging part of the summary statistics is that high and low prescribing counties are similar in terms of race, industrial breakdown, age, and gender. This points to the conclusion that high prescribing rate and low prescribing rate counties differ only in terms of variables for which I am controlling. Another important note from table 1 is that high prescribing counties have a significantly higher disability rate than low prescribing counties. This is consistent with the findings in Park and Powell (2019), GAO (2020), and Krueger (2017). The discrepancy in observations between high and low prescribing counties can be explained by the data missing from the CDC prescription rate data. As mentioned before, the CDC prescription rate data has data for about 90% of counties between 2006 and 2017. The number of observations is very similar, though, which is to be expected.

The summary statistics from counties with and without PDMPs also look similar in important ways, which is also promising. Counties with PDMPs have higher prescription rates, which is unsurprising. PDMPs were not instituted randomly. It is likely that states with higher prescription rates implemented mandatory PDMPs, which would explain why counties with

PDMPs have higher prescription rates. It is important to include county fixed effects and state trends to control for the pre-PDMP differences in those that did and did not institute mandatory PDMPs. It is also important to note that only 10 states have implemented mandatory PDMPs, and many of those implemented PDMPs a few years into the sample. As such, it makes sense that there are many more county-years without a PDMP than county-years with a PDMP. Most of the demographic characteristics are similar between county-years with and without PDMPs. Age, race, gender, education, and industry all look similar. The poverty rates and median household incomes are different, but not so much to cause any concern. Overall, the similarities between the county-years with and without a mandatory PDMP means that it is unlikely that there are uncontrollable, fundamental differences between PDMP and non-PDMP counties other than their prescription rates.

V. Empirical Strategies

2010 Reformulation

The first empirical strategy I employ to examine the effects of prescription opioid use on SSDI rates exploits an exogenous shock to demand of opioid prescriptions, the 2010 OxyContin reformulation. A large volume of literature has shown that this reformulation significantly decreased opioid prescriptions, deaths from opioid prescriptions, and prescription opioid misuse nationwide (Aliprantis, Fee, and Schweitzer 2019; Aliprantis, Powell, and Pacula 2018; Evans, Leiber, and Power 2019; Severtson et al. 2016; Butler et al. 2013; Sessler et al. 2014; Havens et al. 2014; Larochelle et al. 2015; Coplan et al. 2016). Because this shock happened independent of any other major policy shift, it may allow us to get a causal interpretation of the effect of opioid use on SSDI rates. The fact that this was a nationwide shock, though, means that there is less variation in the explanatory variable of interest at the county-level and thus may not yield significant results even if a relationship exists.

My specification for examining the effect of prescription opioid use on SSDI using the 2010 OxyContin reformulation is as follows:

$$pctdis_{it} = \alpha + \beta_1\, yrafter2010_t + \beta_2\, post + \beta_3\, [yrafter2010_t\ x\ post] + \beta_4\, [yrafter2010_t \text{ x } high_i] + \beta_5\, [post \text{ x } high_i] + \beta_6\, [yrafter2010_t \text{ x } post \text{ x } high_i] + \delta_i + \Gamma' X_{it} + \varepsilon$$

Pctdis$_{it}$ is a measure of the percent of people on disability in county-year *it*. *Yrafter2010*$_t$ is a year variable centered around 2010 for ease of interpretation. *Post* is an indicator variable that equals 1 for all years after 2009 and equals 0 for the year 2009 and before. *High*$_i$ is in indicator variable that equals 1 if county *i*'s prescription rate was above the median in 2009 and equals 0 if county *i*'s prescription rate was below the median in 2009. δ_i estimates county fixed effects and X_{it} is a vector of controls for county-year *it*, including employment-population ratio, percent of people who have attained various levels of education, log population, percent of people in each age group, and percent of people who identify as various races. I also allow for a more flexible specification by including a quadratic of *yrafter2010*, which is interacted with *high*$_i$ and *post* just like the linear *yrafter2010*. All regressions are weighted by population.

This specification is, in effect, a triple difference. The constant, α, estimates the percentage of disability recipients in 2010, and β_1 estimates the change in the disability rate from 2006-2009 for counties in the bottom 50%. $\beta_1 + \beta_2$ estimates the jump in percent disability from 2009-2010 for counties in the bottom 50%, and $\beta_1 + \beta_3$ predicts the annual change in the disability rate from 2010-2017 for counties in the bottom 50%. β_4 tells us the difference in slope of percent disability from 2006-2009 between counties in the bottom 50% and counties in the top 50%. $\beta_4 + \beta_5$ will tell us the difference in the jump in percent disability from 2009-2010 for counties in the bottom 50% and counties in the top 50%. Finally, β_6 estimates the difference in slope of percent disability from 2010-2017 between counties in the bottom 50% and counties in the top 50%.

The coefficients in which I am most interested are the coefficients on *post*, β_2, and *post* x $high_i$, β_5. β_2 tells us whether there was a jump in disability rates in lower-prescribing counties. If prescription opioid rates do affect SSDI rates, I would expect this coefficient to be statistically significant, but, as there are two competing theories, I have no expectation for the sign. If prescription opioids have a causal impact on SSDI rates, I would expect β_5 to have the same sign as the coefficient on β_2. In other words, if prescription rate matters to SSDI, low prescribing counties should have some change in SSDI post-reformulation and high prescribing counties should have a larger change.

Prescription Drug Monitoring Programs

I also examine whether PDMPs affect opioid prescription rates. I base my empirical strategy on Buchmueller and Carey (2018), which finds that mandatory PDMPs decreased prescription opioid rates in the states in which they were enacted. As such, these PDMPs represent an exogenous shock to opioid prescription rates in the states in which they were enacted. While PDMPs were being passed around the same time as other regulations regarding opioid prescription abuse, these PDMPs were passed as independent state laws. I can thus use these laws as natural experiments to look at the causal effect of prescription opioid rates on SSDI rates by exploiting the differences in implementation date across states. This strategy should yield more precise results because it exploits cross-state differences in implementation timing rather than a national shock, leading to greater variation in the explanatory variable of interest at the county level. Furthermore, the laws were implemented at the state level, making the changes in behavior at the county level plausibly exogenous.

My specification for this second strategy of analyzing the effect of prescription opioids on SSDI is as follows:

$$pctdis_{it} = \alpha + \beta\, PDMP_{it} + \delta_i + \zeta_t + [\Psi s \text{ x } year] + \Gamma' X_{it} + \varepsilon$$

$Pctdis_{it}$ is again the percent of people receiving SSDI benefits in county-year *it*. $PDMP_{it}$ is an indicator that equals 1 if county *i* had a mandatory PDMP law in place in year *t*. δ_i is county fixed effects and ζ_t is year fixed effects. The interaction term controls for state-specific trends. X_{it} is the vector of controls as the first strategy for county-year *it*. All regressions using this identification strategy are weighted by population as well.

VI. Results

Empirical Results using 2010 OxyContin Reformulation

Figure 2 shows the weighted averages of percent disability over time. The blue dots are the average percent disability in year *t* for counties that were below the median prescription rate in 2009. The red dots are the averages of percent disability in year *t* for counties that were above the median prescription rate in 2009. As you can see, disability rates seem to change in a non-linear way between 2006 and 2017. Figure 3 adds a line of best fit based on my linear specification and figure 4 adds a line of best fit based on my quadratic specification. These graphs make clear that the quadratic specification is a better fit than the linear specification and the jump picked up in the linear coefficient may be picking up not a jump but instead the results of trying to fit a round peg into a square hole and use a linear line of best fit for a quadratic relationship. Figure 4 makes it look like the jump in disability post-reformulation is as big, if not bigger, for low prescribing counties, which is not consistent with prescription opioid rates having an effect on disability rates.

Table 2 shows regression results for the specification exploiting the 2010 OxyContin reformulation. Column 1 is the linear specification of the triple-difference with no controls except for county fixed effects. Column 2 adds controls but is still a linear specification. The negative sign on employment ratio in columns 2 and 4 makes sense because a higher employment-to-population ratio should be associated with lower disability insurance rates. Both linear specifications show that the 2010 reformulation caused an increase in disability insurance. Both linear specifications also show that the jump in disability after the reformulation was significantly larger for higher prescribing counties than lower prescribing counties. These two specifications, though, are not preferred because, as the graphs show, the true fit is likely quadratic. The jump picked up by the *post* coefficient and the interaction between the *post* and $high_i$ coefficient may be biased because it is assuming a linear relationship when a linear relationship does not exist.

Column 3 is the quadratic triple difference with no controls and column 4 adds controls. The coefficient on *post* is still positive and significant for both quadratic specifications but the coefficient on the interaction term is negative in column 3 and indistinguishable from 0 in column 4. Column 4 is my preferred specification because it allows for a quadratic fit and includes controls like education and employment that likely affect both opioid prescription rates and disability rates.

The coefficient on *post* being positive in columns 3 and 4 means that, for low-prescribing counties, there was a significant increase in disability rates after the reformulation. However, it is possible that something else in 2010 caused a significant jump in disability rates. The only way to tell whether it was the OxyContin reformulation that caused the jump is to look at the interaction term. If the jump is significantly larger in the same direction for high-prescribing counties compared to low-prescribing counties, that would suggest that the reformulation is what was driving at least part of the increase in disability rates post-reformulation. The regression results from both columns 3 and 4 suggest that that is not the case. In column 3, the coefficient on the interaction term of interest is significant but negative, showing that high-prescribing counties had a smaller increase in 2010 than low-prescribing counties. Column 4 is further evidence that the reformulation itself is not driving the increase in disability insurance. The coefficient on the interaction term between *post* and $high_{it}$ is indistinguishable from 0. This means that there was no significant difference in the change in disability insurance between high and low prescribing counties after the reformulation.

Empirical Results using PDMPs

Tables 3 and 4 show the results from the empirical strategy exploiting the exogenous shock in opioid prescriptions from mandatory PDMPs. Table 3 reports results from a quasi-first-stage regression. While Buchmueller and Carey (2018) did find that mandatory PDMPs decreased opioid prescription rates in the states in which they were enacted, less research has been done to corroborate that fact than the fact that the 2010 OxyContin reformulation decreased opioid prescriptions. As such, table 3 is five regressions testing whether mandatory PDMPs are in fact associated with a decrease in prescription rates. Column 1 is a linear regression controlling for state-specific trends, county fixed effects and year fixed effects. Column 2 adds a vector of county-level controls. The coefficient of interest in both specifications is *PDMP*. In both specifications, the coefficient on *PDMP* is negative and significant at the 1% level. This finding corroborates Buchmueller and Carey's finding that mandatory PDMPs decreased prescription rates, making them a legitimate exogenous shock to use in this study.

Columns 3, 4, and 5 of table 3 show results of a lagged first-stage regression of prescription rate on whether a mandatory PDMP was implemented in county i. Columns 3, 4, and 5 use a one, two, and three year lags, respectively. If a mandatory PDMP was implemented in county i in 2010, for example, county i would be coded with a 1 starting in 2010 in column 1 and column 2, a 1 starting in 2011 in column 3, a 1 starting in 2012 in column 4, and a 1 starting in 2013 starting in column 5. All prior county-years and all years in counties that never implemented a mandatory PDMP are coded with a 0. Columns 3, 4, and 5 are all negative, but only column 3 is significant at the 5% level. Column 5 is significant as well, but only at the 10 percent level. These results suggest that the impact of mandatory PDMPs on prescription rates was fairly immediate.

Table 4 shows the results of the reduced-form regression of disability rates on PDMPs. Column 1 is an unlagged regression of disability rate on PDMP controlling for state-specific trends, county fixed effects, year fixed effects, and a continuous year variable. Column 2 adds a vector of county-level controls. Columns 3, 4, and 5 are lagged results with state-specific trends, county fixed effects, and year fixed effects. The coefficient of interest in all five specifications is the coefficient on *PDMP*. Columns 1 and 2 have similar results, suggesting that the controls are not helping the regression much. Due to concerns of over-controlling, I leave the vector of county controls out of the rest of the regressions. The coefficients on *PDMP* in columns 1 and 2 are negative and significant at the 5 percent level, suggesting that the decrease in prescription opioid rates caused by mandatory PDMPs also caused a decrease in disability rates. The results in columns 3, 4, and 5 are even larger in magnitude and significant at the 1% level.

VII. Discussion and Conclusions

I exploit two exogenous shocks to prescription opioid rates: the 2010 OxyContin reformulation and "must-access" Prescription Drug Monitoring Programs. An abundance of previous research has found that the 2010 reformulation decreased opioid prescription rates and my own analysis complemented by Buchmueller and Carey (2018) found that mandatory PDMPs did so as well. If there exists a causal relationship between opioid prescriptions and SSDI rates, regressing disability rates on these shocks should pick up that effect. Exploiting the reformulation yielded no significant results but exploiting cross-state differences in mandatory PDMP implementation yielded negative and statistically significant results.

The lack of significant results using the reformulation is unsurprising, as the strategy is imprecise compared to the strategy using PDMPs. This is the case because the reformulation is a national rather than a state-level shock, as the PDMPs are. Pre-reformulation differences in prescription rates may have affected behavior and yielded significant results, but those individual decisions may have been endogenous to the national shock. A drop-off in disability rates following the reformulation would have been marginal evidence of a relationship between prescription rate and SSDI, the lack of that drop-off does not mean that a relationship does not exist. In other words, while the strategy did not yield significant results, that may have been more the result of an imprecise strategy that lacked county-level variation than evidence of no causal relationship. The strategy exploiting PDMPs should yield more precise results because it exploits cross-state variation rather than a national shock, leading to more variation in the explanatory variable of interest at the county-level. As such, the significant results in the PDMP regressions and not the reformulation results suggest that a relationship exists, but it was not picked up in the reformulation results because of a limited empirical strategy.

The results from the PDMP regressions suggest that prescription rates do have a causal effect on disability rates, and that relationship is slightly lagged. The coefficient on *PDMP* of the un-lagged regressions is about -0.05 and significant at the 5% level. This means that implementing a mandatory PDMP decreases disability rates in counties of the state that implemented the PDMP by about one-twentieth of a percentage point. Lagging the results by a year, however, increases the magnitude to -0.1 and makes the results significant at the 1% level. The magnitude of the coefficient doubles when the results are lagged by one year. Lagging the results by two and three years keeps the results significant at the 1% level but lowers the magnitude of the coefficient by about 30% and 50% respectively. This suggests that, following an exogenous drop in opioid prescription rates, disability rates fall slowly in the first year, then more quickly in the second year, then continue falling slowly for the next few years.

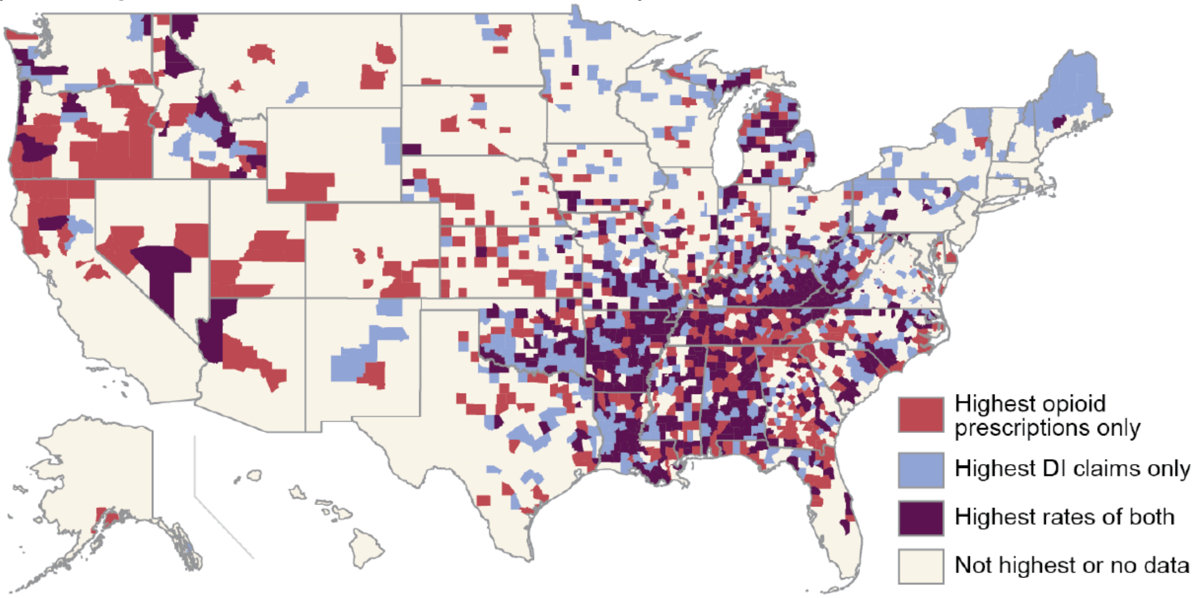
These results make sense given the structure of SSDI applications. The process of applying for disability can take many months and, sometimes, depending on appeals, up to ten years (French and Song 2014). For many people, though, the wait is much shorter, and people get access to disability within a year or two of applying (French and Song 2014). If a causal relationship exists between prescription opioids and SSDI, we expect that to show up within a few years of the exogenous shock, but potentially not immediately. This timeline is consistent with the results based on mandatory PDMPs. In the un-lagged regressions, the coefficients on *PDMP* were small, negative, and significant at the 5% level. In the year after they were implemented, the results become more significant and larger in magnitude, which is consistent with the process of gaining SSDI receipt taking some time after the exogenous shock to prescription opioid rates.

The results from the PDMP specifications suggest that a positive relationship exists between opioid prescription rates and disability insurance. Decreasing prescription rates *caused* a decrease in disability rates. This finding supports the findings in Aliprantis, Fee, and Schweitzer (2019), Harris et al. (2019), and Savych, Neumark, and Lea (2019) that prescription opioids cause people to become addicted and try to leave the labor market. One way that people leave the labor market, according to this paper, is through SSDI.

Further research is necessary examining the relationship between opioids and disability insurance. To fully understand this relationship, a dataset with prescription and non-prescription opioid use would be necessary. Unfortunately, this data does not yet exist except in survey form and even that data is limited. Further research is also necessary to look at reverse causality. Disability rates may affect opioid usage rates, not just the other way around. Endogeneity concerns make this research difficult because so many factors affect both SSDI and opioid usage rates. Much more work must be done but this paper's finding that prescription opioid rates do affect SSDI rates is an important step in understanding the relationship between opioids and disability insurance in the United States.

Figure 1: Map of Opioid and SSDI Rates by County in 2017

Counties with the Highest Rates of Opioid Prescriptions and Disability Insurance (DI) Claims (In the Top Third of the Distribution for Each Rate), 2017



Source: GAO analysis in MapInfo of Centers for Disease Control and Prevention and Social Security Administration data. | GAO-20-120

Figure 2: Average Disability Rates

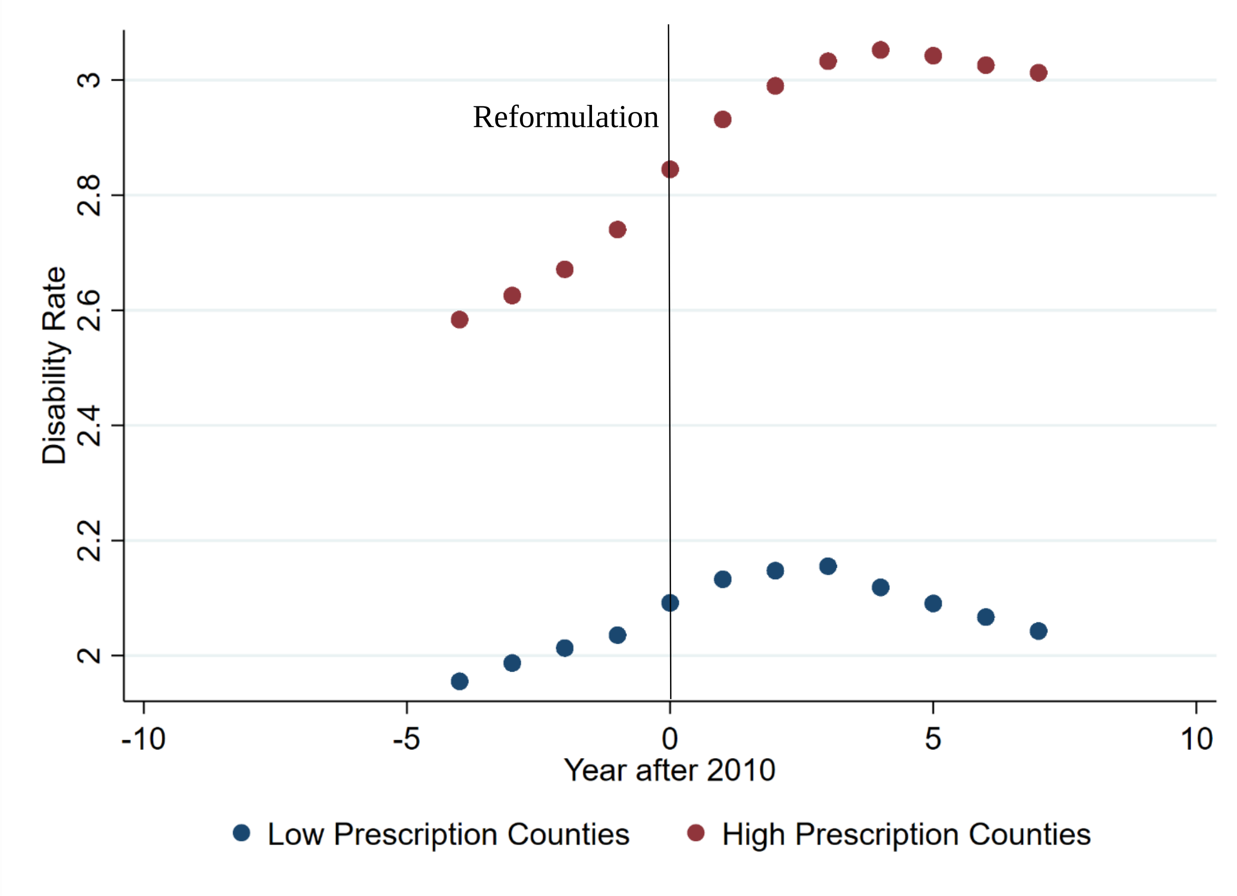


Figure 3: Linear Best Fit Line

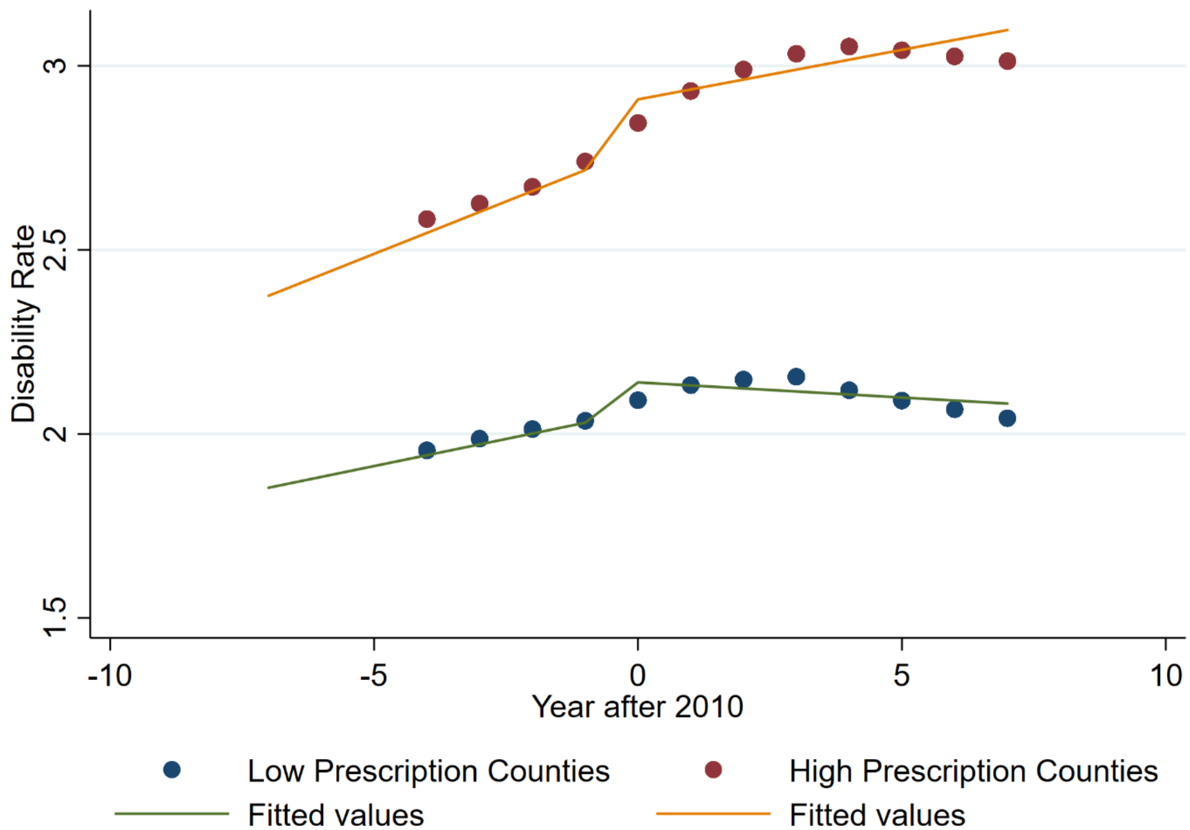


Figure 4: Quadratic Best Fit Line

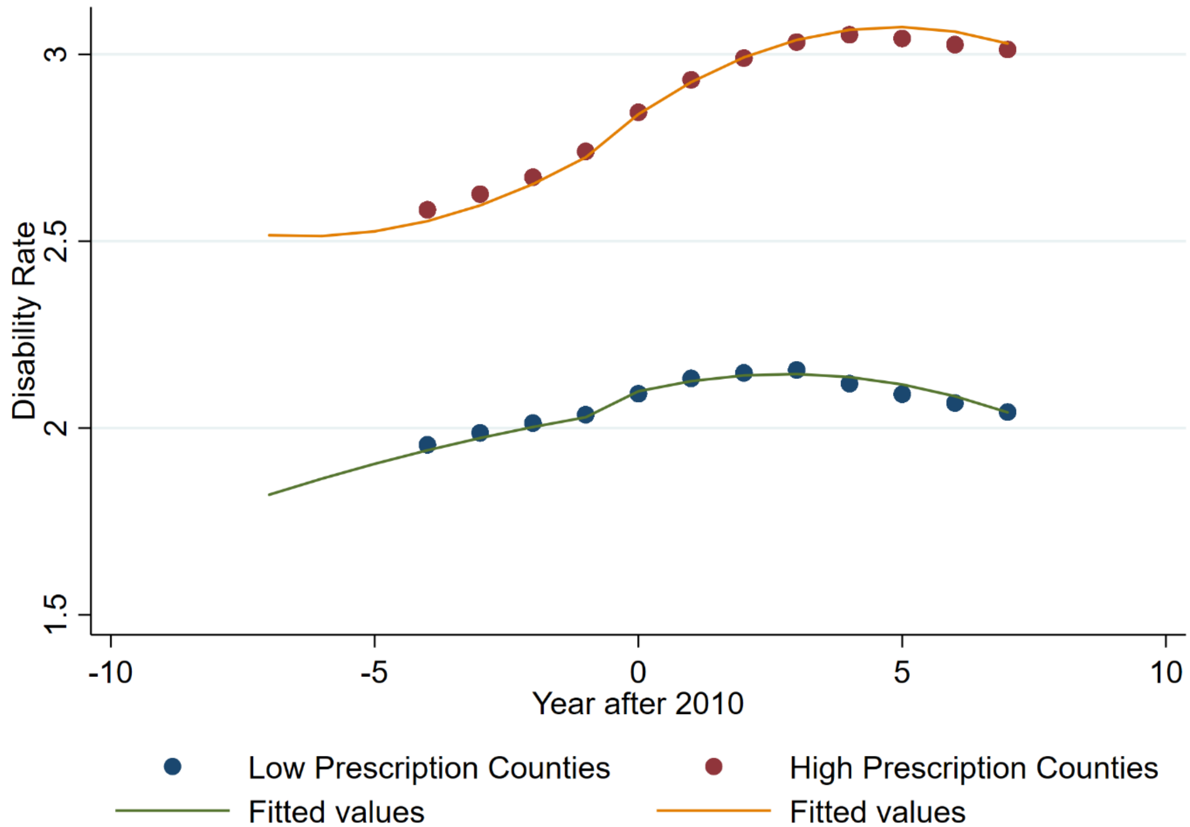


Table 1: Summary Statistics

VARIABLES	Mean for All Counties	Mean for High Prescribing Counties	Mean for Low Prescribing Counties	Mean for County-years w/PDMP	Mean for County-years w/out PDMP
Prescription Rate per 100 People	85.40 (47.11)	118.12 (42.92)	56.19 (22.74)	95.68 (47.14)	84.11 (46.96)
Percent Disability	2.85 (2.04)	3.51 (2.18)	2.38 (1.67)	4.14 (2.64)	2.70 (1.89)
Population	99,346 (318,495)	86,240.99 (154,307)	139,076.3 (451,279.3)	91,264.52 (231,070.4)	100,290.9 (327,178.8)
Percent Male	49.99 (2.38)	49.44 (1.75)	50.23 (2.32)	49.96 (2.27)	49.99 (2.40)
Median Age	40.48 (5.22)	40.11 (5.17)	40.21 (4.70)	40.43 (4.08)	40.49 (5.34)
Percent White	83.61 (16.69)	83.60 (15.05)	83.93 (16.41)	83.77 (14.79)	83.59 (16.90)
Percent Black	8.97 (14.51)	10.42 (14.60)	8.03 (13.99)	8.71 (13.00)	9.00 (14.67)
Percent less than HS	15.40 (7.06)	16.15 (6.36)	14.46 (7.22)	16.50 (5.97)	15.27 (7.16)
Percent HS Diploma	35.00 (7.04)	35.35 (6.29)	34.38 (7.78)	38.02 (6.46)	34.65 (7.02)
Percent Agriculture	6.89 (7.46)	4.70 (4.90)	6.69 (6.64)	5.98 (6.53)	6.99 (7.55)
Percent Manufacturing	12.39 (7.26)	13.32 (7.06)	12.55 (6.99)	12.62 (7.18)	12.36 (7.27)
Median HH Income	49,300.91 (12,954.54)	46,454.65 (10,634.64)	52,851 (14,545.7)	45,863.28 (11,805.97)	49,702.55 (13,023.25)
Percent in Poverty	16.08 (6.53)	17.38 (5.82)	14.72 (6.40)	18.44 (6.19)	15.81 (6.51)
Employment Ratio	0.55 (0.82)	0.53 (0.72)	0.57 (0.83)	0.51 (0.76)	0.56 (0.08)
Observations	33,823	16,460	16,279	3,945	33,765

Table 2: 2010 OxyContin Reformulation Results

VARIABLES	(1) Percent Disability	(2) Percent Disability	(3) Percent Disability	(4) Percent Disability
Year	0.0294***	-0.00756	0.0209**	-0.0286
	(0.00383)	(0.00686)	(0.00960)	(0.0219)
Post	0.0801***	0.0577***	0.0469***	0.0720***
	(0.00428)	(0.00728)	(0.00607)	(0.0110)
Post x Year	-0.0376***	-0.0142***	0.0120	0.0528***
	(0.00378)	(0.00480)	(0.00922)	(0.0132)
High x Year	0.0275***	0.0158***	0.0729***	0.0483***
	(0.00520)	(0.00461)	(0.0129)	(0.0122)
Post x High	0.0550***	0.0427***	-0.0180**	-0.00666
	(0.00599)	(0.00768)	(0.00826)	(0.00796)
Post x High x Year	0.00764	0.00954	-0.0101	-0.00547
	(0.00522)	(0.00582)	(0.0115)	(0.0122)
$Year^2$			-0.00170	-0.00669**
			(0.00139)	(0.00275)
Post x $Year^2$			-0.00416***	0.00168
			(0.00141)	(0.00344)
High x $Year^2$			0.00909***	0.00622***
			(0.00183)	(0.00187)
Post x High x $Year^2$			-0.0130***	-0.00880***
			(0.00200)	(0.00211)
Employment Ratio		-1.035***		-0.848**
		(0.378)		(0.405)
Population Controls		X		X
Age Controls		X		X
Race Controls		X		X
Education Controls		X		X
County Fixed Effects	X	X	X	X
Constant	2.333***	12.75***	2.342***	16.48***
	(0.00536)	(3.580)	(0.00642)	(4.058)
Observations	32,967	32,967	32,967	32,967
R-squared	0.985	0.987	0.986	0.988

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; Standard errors clustered by county

Table 3: First-stage Regressions using PDMPs

VARIABLES	(1) Prescription Rate	(2) Prescription Rate	(3) Prescription Rate	(4) Prescription Rate	(5) Prescription Rate
PDMP	-2.954***	-2.949***			
	(0.865)	(0.865)			
PDMP1YearLag			-2.287**		
			(1.032)		
PDMP2YearLag				-0.681	
				(1.061)	
PDMP3YearLag					-1.730*
					(1.032)
State Trends	X	X	X	X	X
Age Controls		X			
Population Controls		X			
Employment Controls		X			
Race Controls		X			
Education Controls		X			
County Fixed Effects	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X
Constant	2,411***	3,088***	2,434***	2,488***	2,453***
	(89.37)	(265.0)	(91.50)	(91.33)	(90.80)
Observations	33,823	33,823	33,823	33,823	33,823
R-squared	0.952	0.954	0.952	0.952	0.952

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; Standard errors clustered by county

Table 4: Second Stage Results using PDMPs

VARIABLES	(1) Percent Disability	(2) Percent Disability	(3) Percent Disability	(4) Percent Disability	(5) Percent Disability
PDMP	-0.0463**	-0.0462**			
	(0.0205)	(0.0190)			
PDMP1YearLag			-0.100***		
			(0.0215)		
PDMP2YearLag				-0.079***	
				(0.0169)	
PDMP3YearLag					-0.0656***
					(0.014)
Year	0.0385***	0.0291***	0.0382***	0.0385***	0.0387***
	(0.00487)	(0.00519)	(0.00487)	(0.00488)	(0.00488)
State Trends	X	X	X	X	X
Age Controls		X			
Population Controls		X			
Employment Controls		X			
Race Controls		X			
County Fixed Effects	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X
Constant	-45.81***	-17.21**	-47.63***	-46.90***	-46.45***
	(2.400)	(8.355)	(2.435)	(2.457)	(2.490)
Observations	37,664	37,664	37,664	37,664	37,664
R-squared	0.988	0.990	0.988	0.988	0.988

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; Standard errors clustered by county

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