Weeding out bad loans: externalities of the opioid crisis *

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PRELIMINARY: DO NOT CITE OR DISTRIBUTE

ABSTRACT

I examine the impact of the opioid epidemic on consumer finance in a setting of sub-prime auto lending. To identify the relation of opioid abuse and loan defaults, I use a difference-in-differences framework in two well-documented medical settings, and find that loan defaults are attributable to opioid abuse. Borrowers pay approximately 10% more for sub-prime auto loans than otherwise similar borrowers when the loan is originated in areas afflicted by the opioid crisis — areas in which the out-of-sample performance of lenders' credit models is poor. The timing of the price changes is suggestive of lenders learning about the impact of the opioid crisis on the performance of their loan portfolios.

JEL Classification: D14, D82, G29, I15

1. Introduction

Addiction to prescription opioids and heroin is a serious global crisis affecting health and economic welfare. The United States is the world's largest consumer of prescription opioids, with over 3 million people that suffer from related substance abuse disorders (Dart et al., 2015; Schuckit, 2016). The consequences of this epidemic have been devastating and are on the rise. For example, more than 400,000 people died from overdoses in the last 15 years — quadrupling since 1999 (Centers for Disease Control and Prevention, 2018). The Centers for Disease Control and Prevention (CDC) reports that mortality rate in 2016 attributed to opioid-related overdoses is higher than the deaths attributed to car accidents in the United States.

Prescription opioid abuse is not only costly in terms of mortality, but is also partly responsible for other significant economic costs. For example, costs from substance abuse treatment, health care, and criminal justice costs associated with the opioid crisis have been estimated at $78.5 billion in 2013 (Florence et al., 2016).[1] The consequences of opioid abuse have also been found to have far-reaching implications on the labor market. One-third of prime-age men who do not participate in the labor force are using prescription pain medication (Krueger, 2017). Harris et al. (2017) finds that on balance, the adverse labor market effects associated with the abuse of prescription opioids outweigh any positive labor market effects from their therapeutic use. However, the direction of causality of labor market participation and prescription opioid use remains open for debate (Currie et al., 2018).Despite the significant health and economic impact associated with the unprecedented increase in opioid supply, little is known about the economic spillover effects on financial markets arising from this channel. This paper starts to fill that void.

This study provides empirical evidence of a relation between the increasing supply of opioids and its effects on consumer finance. I explore if (1) the opioid abuse rate is a risk factor for lenders and;(2) if opioid abuse is priced in consumer credit products. If communities that are afflicted by

[1] The White House Budget Office has estimated that drug abuse costs the US government nearly $300 billion per year (Manchikanti, 2006). Meyer et al. (2014) provides an overview of the literature on the clinical and economic costs related to the opioid crisis. The earliest estimates of total economic losses ($58.3 billion) of drug abuse more generally are provided by Rice et al. (1991). Related work shows that problem drinking is also attributed to large economic costs arising from productivity losses and reduced employment (Rice et al., 1990; Mullahy and Sindelar, 1996).

the opioid crisis suffer from higher loan default rates, and lenders cannot determine ex-ante who the defaulters will be, all members in the community may face higher priced credit, credit rationing, or both. I shed light on how the lenders' inability to tell abusers from non-abusers affects areas that are afflicted by the opioid crisis. The presence of this mechanism would suggest that the consequences of the opioid crisis extend beyond its impact on the labor market, and affect the market for consumer finance products.

I estimate the externalities of the opioid crisis on consumer finance by connecting county-level opioid prescription and opioid death rates to sub-prime automotive loan origination and performance data. The sub-prime loan market is an ideal setting to study the impact of opioid abuse on consumer finance, because these borrowers are in the at risk population. Most U.S. households have a vehicle and more than one-third have an auto loan (Bricker et al., 2017). With recent auto loan balances in excess of $1.14 trillion (Zabritski, 2018), this makes for a useful setting in which to study the impact on the opioid crisis on consumer finance. I document the relation of opioid abuse and opioid prescribing rates with loan performance using a panel of 250,000 sub-prime auto loans. I then use data on the loan origination terms to explore how opioid abuse differentially affects loan terms.

I find that, after accounting for a borrowers' creditworthiness and local economic conditions, opioid death rates are related to higher default rates for borrowers.[2] I find that a one standard deviation increase in opioid death rates is associated with a 2.1% increase in loan defaults, and this represents a 7.5% increase relative to the average default rate in the sample. I also find that an increase in opioid prescribing rates is associated with a deterioration in loan performance.[3]

Identifying the relation of opioid abuse and loan defaults is empirically challenging. An omitted variable related to the changing economic conditions (e.g. unemployment or labor market participation) may plausibly result in a simultaneous increase in opioid abuse and loan defaults. As a

[2]While there is a mechanical relation of opioid deaths and loan defaults, the impact of the actual number of opioid deaths on defaults is trivially small. Instead, the opioid death rate is a proxy for opioid abuse. For example, emergency room visits related to opioid overdoses are correlated with opioid deaths. However, the data for emergency room visits is only available for a small sample of states for a limited time period.

[3]Consistent with this finding, Powell et al. (2015) provides causal estimates of the relation between the medical opioid supply and drug overdoses using differential shocks associated with medical reimbursement. The authors find that 10% increase in opioid supply leads to a 7.4% increase of opioid-related deaths.

result, the positive correlation described in the ordinary least squares specification above should be interpreted with caution. To identify the relation of opioid abuse and loan defaults, I use a difference-in-differences (DiD) framework in two well-documented settings

In October 2011, the state of Florida implemented regulations that severely reduced the national prescription opioid supply (Rutkow et al., 2015). In 2010 and 2011, Florida supplied over 50% of the total prescribed opioids despite having only 6% of the U.S. population. The implementation of Florida's Prescription Drug Monitoring Program (PMDP) and regulations related to "pill mills", put Florida on par with other states, and this resulted in a significant reduction in the national supply of illicitly used prescription opioids (Rutkow et al., 2015). The reduction in the supply of prescription opioids resulted in an increased street price for these opioids and a substitution to less costly and more powerful opioid substitutes such as heroin and fentanyl.[4] The resulting impact of the regulatory change on the increase in opioid death rates has been extensively documented (Mars et al., 2014; Cicero et al., 2014; Surratt et al., 2014; Rutkow et al., 2015; Compton et al., 2016; Kennedy-Hendricks et al., 2016).

I find that counties with high levels of opioid abuse experienced a statistically significant ($p < 0.01$) increase in loan defaults after Florida's legislative change. The DiD framework with state and year fixed effects helps to mitigate the concern related to unobserved changes in the state employment level that may be simultaneously affecting opioid abuse and loan performance. The most afflicted counties (i.e. the counties with the highest decile of opioid death rate) experienced a 13.3% increase in the loan default rate after the legislative change. Further, I find that the effect is stronger in counties within 700 miles of the Florida state line. Counties that had the highest decile of opioid death rates and were within 700 miles of Florida experienced a 16.3% increase in loan defaults. The stronger economic relation of the opioid death rate and loan defaults is consistent with Drug Enforcement Administration (DEA) reports that addicted users would make day-trips to Florida to purchase opioids for consumption.

My second setting involves the legalization of marijuana. Marijuana is known to provide relief from chronic pain similar to the analgesic benefits ascribed to prescription opioids (Hill, 2015; Jensen

[4] Also see, Evans et al. (2018), who finds a similar relation in another setting — the reformulation of Oxycontin in 2010 resulted in an increase in heroin overdoses and deaths.

et al., 2015). When states pass legislation facilitating access to marijuana, addicted opioid users face a choice of acquiring (costly) opioids illegally or marijuana. Again using a difference-in-differences (DiD) framework, I investigate if states that legalized recreational marijuana had a statistically significant ($p < 0.01$) decline in opioid deaths and loan defaults after the legislative change, relative to other states. I find that loans terminating in states that have implemented legislation allowing the use of recreational marijuana experienced a 3.2% *decrease* in the loan default rate relative to states that did not allow dispensaries to sell marijuana to recreational users. A violation of the exclusion restriction in this setting suggests that people become more responsible and are better at paying their bills when using marijuana. Instead the channel — well-documented in the medical literature (Bachhuber et al., 2014; Powell et al., 2018) — ascribes the effect of loan defaults to the substitution from opioids to marijuana. Moreover, the results show that when instrumenting with the legalization of recreational marijuana, I find that a 10% increase in opioid abuse results in a 2.5% *increase* in loan defaults.

Next I examine loan origination terms, and find that loan terms vary with opioid death rates. A one standard deviation change in opioid deaths, when instrumenting with lagged opioid prescription rates, is associated with a $24 increase in monthly loan payments. This represents a 6% increase in payments relative to the average loan in my sample after controlling for loan duration, vehicle book value, and borrower creditworthiness. The resultant increase in monthly payments compensates lenders for the higher observed loan defaults associated with the opioid death rates. However, the results show that coincident opioid deaths rates did not have a significant impact on pricing until at least 10 years after the launch of Oxycontin, the most prescribed prescription opioid. The increase in loan cost was most pronounced in last five years of the sample (after the financial crisis) when opioid death rates increased dramatically. While the correlations of increases in loan payments and increases of opioid abuse is important, the mechanism by which lenders respond to the opioid epidemic requires further analysis.

I investigate the reliability of lenders' credit models in assessing the riskiness of auto loans in relation to the opioid crisis. Lenders use information to determine the expected default rate of a prospective lender. If lenders cannot distinguish two otherwise similar borrowers because of an unobserved risk factor that affects one of the borrowers, then one may expect that lenders may ration credit or increase the the cost of credit for all prospective borrowers. Using a difference-in-differences framework, I investigate the predictive power of traditional factors, such as the borrower's FICO score and other observable credit attributes at the time of origination, on realized default rates.

I find that the power of the traditional measures such as FICO score and income declines predictably during the financial crisis. However, after the financial crisis has abated, the measures continues to deteriorate in their ability to predict loan defaults. However, once I include an indicator variable for opioid afflicted areas and an interaction term for those areas with the existing measures, I find that the lenders are able to more accurately predict loan default after the financial crisis, than in the preceding years during the financial crisis. Comparing counties that were most and least affect by the opioid crisis, I find that the traditional measures perform well out-of-sample in the counties with the lowest decile of opioid death rates after 2012. Out-of-sample, the coefficient of predicted default rates on realized default rates is statistically similar (0.973) to the coefficient (1.025) of the in-sample period from 1999 to 2006. In contrast, the predictive power of the model after 2012 was significantly lower (0.341) in counties that experienced the highest opioid death rate.

To the best of my knowledge this the first paper connecting the opioid epidemic with financial markets. The paper sheds light on how the opioid crisis adversely affects consumer credit markets. I show that opioid abuse leads to higher loan default rates. When lenders are unable to hedge this risk factor, it places a drag on loan markets. Consequently, consumers in opioid afflicted areas face higher costs of credit. The externalities described in this study may also spill over into the $100 billion market for ABS loan structures as most automotive loans are securitized.

The remainder of the paper is organized as follows. In Section 2, I provide a brief overview of the the opioids data and a novel dataset of individual auto loans. In Section 3, I examine the relation of loan performance and opioid abuse. In Section 4, I compare loan origination terms and the reliability of traditional credit models across areas that are differently affected by the opioid epidemic. I also estimate differences in borrowers' total loan costs. Section 5 concludes and discusses the broad context for the results.

2. Data

To assess the impact of opioid abuse on consumer finance I use two proxies. I append data related to opioid use in the United States from the Centers for Disease Control and Prevention (CDC), National Center for Injury Prevention and Control. I use drug poisoning rates per 100,000 for all races, both sexes, ages 20 to 79 for the years 1999 to 2016. I also use opioid prescribing rates per 100 persons from the CDC. The source for prescribing data emanates from the IQVIA Transactional Data Warehouse (TDW) for the years 2006 to 2016. This data is based on a sample of approximately 59,000 non-hospital retail pharmacies, which dispense approximately 90% of all retail prescriptions in the United States. Prescribing rates are calculated from the number of opioid prescriptions dispensed in a given year and county divided by the annual resident population from the U.S. Census Bureau. Opioid prescriptions include buprenorphine, codeine, fentanyl, hydrocodone, hydromorphone, methadone, morphine, oxycodone, oxymorphone, propoxyphene, tapentadol, and tramadol. Howevever cough and cold formulations containing opioids were not included, nor were methadone prescriptions dispensed through maintenance treatment programs.

To assess the impact of opioid abuse on loan performance, I use the opioid death rate as a proxy. While there is a mechanical relation of opioid deaths and loan defaults, the impact of the actual number of opioid deaths on defaults is trivially small. While emergency room visits related to opioid overdoses (correlated with opioid deaths) would make a better proxy, the data for emergency room visits is only available for a small sample of states for a limited number of years. To go around this issue, I calculate a new measure for opioid abuse. I divide the opioid death rate at the time of loan termination by the opioid prescription rate at the time of loan origination. Cases in which the prescription rate is high and the death rate is relatively low may be cases in which the therapeutic benefits are high relative to the costs. In contrast, when the ratio of the death rate to prescription rate is high, diversion of prescription opioids for non-medical uses may be situations in which opioid abuse is important.

The database of automotive loans used in this study comes from a lender that acquires loans in 44 U.S. states.[5] The data spans 23 years ending in 2017 and includes over 259,467 loans that were

[5]See Brown and Jansen (2018) for a more detailed description of the loan database.

originated at 3,926 dealerships, which are located in 1,903 U.S. ZIP codes. Appendix A provides definitions for the variables used in the study.

The loan sample used in the analysis is matched with county-level CDC data on opioid death rates, which are available from 1999 to 2016. Also, to account for the fact that final outcomes are uncertain for loans that are still active at the end of my full sample period, I only includes those loans whose termination can be observed in the data. This further reduces the sample for certain tests in which loan performance is included.

Table I shows the summary statistics for the data used in the paper. The average borrower in the sample of loans had a FICO score of 534, monthly gross income of $3,334. The borrowers financed an average of $16,551 on a 66 month (average) term. The average default rate is 27.93%.[6]

Table I also shows that the CDC data for the opioid death rate by county and the opioid prescription rate. The average opioid death rate (per 100,000) for the sample is 16.93 with a standard deviation of 7.23. The highest observed death rate in my sample is 139.44.[7] The average opioid prescription rate for my sample is 76.95 with a standard deviation of 19.60.[8] The highest observed prescription rate in my data is 321 per 100, while the highest observed prescription rate is in excess of 583 (for a county in which I have no loan observations).

3. Loan outcomes

In this section, I compare loan outcomes across counties and states with different opioid prescription and death rates to determine if opioid abuse affects loan outcomes. Figure 1 plots the positive association between the percent of loans that default and the opioid death rate. The scatter plot includes controls for credit score, income, prior bankruptcy (indicator) and county fixed

[6]Note that the default rate is shown as a percentage to simplify the interpretation of the coefficients in the tables.

[7]These summary statistics are similar to the full sample of opioid death rates for the United States (e.g. mean=17.76 sd=11.63), suggesting that my sample of loans may be representative of loans originated across the United States at that time. If anything, my sample is biased towards areas that have lower opioid death rates. The external validity of this study is compelling given the large sample of loans from 44 states. However, the undersampling of the most afflicted areas suggests that the results in this study may be understated.

[8]The average (non-population weighted) prescription for the United States was 72.52 during the sample period. However, the standard deviation was substantially larger (49.76), since I do not observe the highest opioid exposed counties in my sample.

effects.

3.1. opioid deaths and loan defaults

I estimate multivariate OLS models of the relation between opioid use and loan defaults. Specifically, I estimate the following:

$$Y_{i,j,\tau} = \lambda_j + \lambda_\tau + \beta_1 Z_{j,\tau} + \beta_2 X_{i,j,\tau} + \varepsilon_{i,j,\tau}, \tag{1}$$

where $Y_{i,\tau}$ is my dependent variable of interest—the loan default rate for borrower i; j the county in which the loan was originated; and the year, τ, in which the loan was originated. The variable $Z_{j,\tau}$ represents the opioid death rate for county j; and the year, τ, in which the loan was terminated. The equation also includes controls $X_{i,j,\tau}$ for individual borrower and local characteristics. In all analyses I present robust standard errors clustered at the county-year level.[9]

The use of county-level data provides three advantages. First, assessing the impact of prescription opioids is empirically challenging because of the diversion of opioid prescriptions. The literature has shown that a substantial share of prescribed opioids is diverted illegally rather than being consumed by the patient with the prescription (Surratt et al., 2014; Harris et al., 2017). Tracking the diversion of opioids to friends and family would be empirically challenging. Instead county-level analysis permits the use of per capita opioid prescriptions for analysis on loan performance. Second, cities and counties have different mitigation strategies for opioid abuse. County fixed effects allow me to control for this omitted variable. Finally, changing economic conditions can be assessed at the county level.

Table II presents OLS regression results on an indicator for loans terminated due to default. In column 1, 2, and 3, regressors include the opioid death rate at the time of the loan termination. Controls are included for the riskiness of the individual borrower (i.e. credit score, income, and prior bankruptcy). The specification also includes the unemployment rate at the time of default to control for the local economic environment. Also, county fixed effects are included in columns 1 and 2 to control for local factors. In column 2, I add year fixed effects to control for macro economic

[9] All results are robust to clustering by county.

changes; in column 3, I substitute dealership fixed effects for county fixed effects. Dealers may observe soft information about the borrower, which may provide an indication of the borrowers' likelihood to abuse drugs, and the lender does not observe this. In all specifications, borrowers who reside in counties with higher opioid death rates are more likely to default on their vehicle loans. When year fixed effects are included, a one-standard deviation increase in opioid death rate is associated with a 2.1% increase in the likelihood of default ($p < 0.01$). For the average borrower in my sub-prime loan sample, this represents a change in the likelihood of default from 27.9% to 30.0%. Standard errors are reported as heteroscedasticity-consistent standard errors, clustered by county-year to account for correlation across loans originated in the same county in the same year.[10]

Column 1 of Table II shows that the within county time series variation is strong (4.86% increase for a one standard deviation increase in the opioid death rate). Column 2 shows that much of the variation associated with the opioid death rate remains after including year fixed effects. However, the coefficient on opioid death rate is nearly half (0.650 in column 1 vs. 0.291 in column 2).[11]

As described in Section2, I also use the opioid abuse rate in my analysis. Columns 4, 5, and 6 of Table II show similar economic magnitudes across the three specifications (i.e. with county fixed effects; with county and year fixed effects; and with dealership and year fixed effects.) A one standard deviation increase in the opioid abuse rate is correlated with at 1.6% increase in the loan default rate.

3.2. Prescription drug monitoring and loan defaults

The ideal experiment would allow us to answer the question, 'what would the default rate have been if borrowers in the opioid afflicted area had not been exposed to the crisis?' An empirical challenge is distinguishing loans that are affected by the opioid crisis from loans that are unaffected. Using the opioid death rate as a regressor is insufficient, as there may be an unobserved variable that is simultaneously correlated with the opioid death rate and the default. For example, a plant closing in the affected area may simultaneously affect employment and opioid death rates if causality

[10] In results not shown, including lags for prior year opioid deaths in the specification does not significantly change the results.

[11] The results are also robust to the addition of other borrower-level controls such as homeownership, lender risk score, and non-linear factors such as credit score squared.

related to labor market participation and opioid abuse is unclear. To help address this identification challenge, I explore the impact of a regulatory change that had major ramifications for the opioid crisis.

The year 2012 saw a significant *increase* in the opioid death rate as the state of Florida passed legislation, which significantly *reduced* the opioid supply. In the two years prior to the passage of laws mandating a prescription drug monitoring program (PDMP), Florida had supplied over 50% of prescription opioids for the United States (Office of National Drug Control Policy, 2011).[12] Rutkow et al. (2015) describes the effect of the Floridas prescription drug monitoring program and pill mill laws on the of opioid prescriptions and abuse. The Drug Enforcement Administration (DEA) that numerous states were supplied by the "Florida oxy express." For example in 2010, 90 of the top U.S. physicians prescribing oxycodone, were in Florida. The change in legislation resulted in a significant increase in the street price of prescription opioids and an increase in the use of heroin and fentanyl (Mars et al., 2014; Cicero et al., 2014; Zhou et al., 2016; Beall, 2018). These opioids were significantly less costly and more powerful opioid substitutes. The DEA reports that the street price for Oxycontin increased from $20 per pill to over $80 in many parts of the United States. In contrast, black-tar heroin could be purchased for as little as $5 per dose. The resultant change from prescription opioids to heroin and fentanyl resulted in a significant increase in the opioid death rate in areas that where afflicted with the opioid crisis. (Cicero et al., 2014; Compton et al., 2016; Beall, 2018).[13]

I use the exogenous change in Florida's regulation in September 2011 to identify the impact of opioid deaths on loan defaults using a difference-in-differences (DiD) framework. I compare the impact of areas that were (were not) afflicted by the opioid crisis before and after the legislative change. The DiD regression equation is given by:

[12] The law required physicians to ascertain if a prospective patient had recently received another opioid prescription from another physician. Prior to the passage of this law, patients were able to pick up multiple prescriptions for opioids.

[13] Much in the same way that the prescription opioid reduction arising from the PDMP, Evans et al. (2018) finds that the reformulation of Oxycontin in 2010 resulted in an increase in heroin overdoses and deaths. The study finds that when addicted users lost access to Oxycodone, they sought out other powerful drugs. Heroin became the preferred alternative in many places because other alternatives were too expensive or hard to get because of legal restrictions.

$$Y_{i,j,\tau} = \lambda_j + \lambda_\tau + \beta_1 D_{i,\tau} + \beta_2 X_{i,j,\tau} + \varepsilon_{i,j,\tau}, \tag{2}$$

where $Y_{i,\tau}$ is my dependent variable of interest—the loan default rate for borrower i; j the county in which the loan was originated; and the year τ. The variable $D_{i,\tau}$ represents an indicator variable that is equal to 1 if the borrower is treated and if the loan termination was after the Florida legislative change (i.e. $\tau => 2011$), and zero otherwise. In one set of tests, a borrower is treated if the loan was originated in a county with the highest decile of opioid death rate or control otherwise. The analysis specifically excludes the state of Florida, to determine the extent to which Florida's legislative change had an impact on other states. In a second set of tests, a borrower is treated if the loan was originated in a county with the highest decile of opioid death rate *and* the county j was within 700 miles of the Florida state line. This range encompasses the Georgia, Alabama, Mississippi, Louisiana, Tennessee, South Carolina, North Carolina, Kentucky, and Virginia.[14]The equation also includes controls $X_{i,j,\tau}$ for borrower and local characteristics as well as county and year fixed effects. In all analyses I present robust standard errors clustered at the county-year level. To identify the change in the regulation, the analysis is limited to loans that were originated between 2010 and 2014 — the two years around the implementation of Florida's law.

Table III presents the results on an indicator for loans terminated due to default. In column 1, regressors include an indicator variable that is equal to 1 if borrower is treated and if the loan termination was after the Florida legislative change (i.e. $\tau => 2012$) for a sample of the highest (treated) counties by their opioid death rate. The inclusion of county fixed effects λ_j and year fixed effects λ_τ ensures that the treatment effect β is estimated only within county-year variation. The coefficient on the dummy variable indicates that the increase in death rate resulted in a 13.4% increase ($p < 0.01$) in loan defaults in the most afflicted areas. In column 2, I add controls for the macroeconomic conditions of the county at the time of loan termination. The coefficient is nearly unchanged. In column 3, I show the coefficient on a triple differences term that includes an indicator if the county was within 700 miles of the Florida state line.I find that the afflicted areas in

[14]To avoid selection on areas that were particularly hard-hit by the opioid epidemic, the states of Pennsylvania, West Virginia and Ohio were excluded from the proximity treatment. Florida is also excluded here.

states that were proximate to Florida experienced a larger increase in loan defaults after Florida's legislative change. Comparing the counties with the highest and lowest opioid death rates, states that were within 700 miles of Florida experienced a 16.3% increase in loan defaults.

Table III columns 4 and 5 repeat the analysis described in columns 2 and 3, for the opioid abuse measure. A one standard deviation increase in the opioid abuse rate corresponds to a 1.6% increase in the default rate after the legislative change. The effect (6.1%) is significantly stronger in states proximate to Florida. The stronger economic relation of the opioid death rate and loan defaults is consistent with Drug Enforcement Administration (DEA) reports that addicted users would make day-trips to Florida to purchase opioids for consumption.

3.3. Marijuana legalization and loan defaults

Recent studies in the medical literature have found that changes in laws related to the access to marijuana reduces opioid analgesics usage and opioid overdose deaths (Bachhuber et al., 2014; Bradford and Bradford, 2016, 2017). In this section, I assess the impact of laws permitting the recreation use of marijuana on opioid abuse and loan defaults.

The sale of marijuana for non-medical reasons has been legalized in several U.S. states as of 2018.[15] However, only three states have implemented laws that facilitate the sale of marijuana - Colorado in 2014, Washington in 2014, and Oregon in 2015. While there are multiple ways for individuals to access marijuana, the legalization for recreational provides wider access which may facilitate the process of substituting marijuana for opioids. I use the implementation of these laws to explore the impact on opioid abuse and loan performance.

The empirical strategy compares changes in opioid related abuse in states legalizing marijuana usage for recreational purposes to those states which have not implemented these laws. First, using a difference-in-differences strategy, I examine the non-adopting states as controls and differential timing of marijuana legalization as the treatment effect. The difference-in-differences framework compares changes in outcomes between states that adopted marijuana legalization. In this specifica-

[15] Using laws related to medical marijuana is empirically challenging. The tightening of regulations of medical marijuana dispensaries over time makes identification challenging (Pacula and Smart, 2017). Since 2010, states have been regulating medical marijuana dispensaries more tightly in response to federal marijuana policy. See for example, Powell et al. (2018)

tion, I include state fixed effects and year fixed effects to mitigate concerns to isolate the differential effect in the states that legalized marijuana.

Figure 2 plots the percentage of loans that default over time for a sample of states that legalized recreational use of marijuana and those states that did not. The scatter plot includes controls for credit score, income, and prior bankruptcy. The scatter plot shows that states, which legalized recreational use of marijuana had similar default rates prior to the legalization of marijuana. After the implementation of the laws, the default rates in states that legalized marijuana are visibly lower.

To formally identify the impact of opioid abuse, I use the legalization of marijuana as a source of exogenous variation in the opioid abuse rate in a difference-in-differences (DiD) framework. Identification originates solely from the introduction of marijuana legalization interacted with the timing of the law. This strategy allows me to non-parametrically control for the independent effects of the legalization (through year fixed effects) and state economic conditions (through state fixed effects). I do not use a time-varying measure in the interaction term because there may be migration correlated with opioid abuse. For example, opioid abuse may be related to local economic downturns[16] The DiD regression equation is given by:

$$y_{i,j,\tau} = \lambda_j + \lambda_\tau + \beta_1 D_{i,\tau} + \varepsilon_{i,j,\tau}, \tag{3}$$

where $y_{i,j,\tau}$ represents the outcome (i.e. the opioid overdose abuse in state j in year τ). $D_{i,\tau}$ represents the indicator for state marijuana legalization for states that (1) legalized recreational marijuana usage and (2) have implemented operational and legally-protected dispensaries. A borrower is treated if the loan was terminated in a state where these conditions were met. Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy) and the local environment(i.e. unemployment rate). In all analyses I present robust standard errors clustered at the state-year level.

Table IV, Column 1 to 4 report results from OLS regressions on the opioid abuse rate for

[16]See for example Hollingsworth et al. (2017). If deteriorating economic conditions cause people under age 35 to disproportionately migrate out of the state (i.e., increasing the percentage of the population more susceptible), then this unobserved variation is problematic in my specification.

indicator variables (Post-legalization) for loans that were terminated in a state that had legalized the sale of marijuana at the time of termination. The coefficient on the post-legalization indicator variables suggests that those states experienced a significant decline in the opioid abuse rate after legalization. Legalization resulted in a 3.0% decrease ($p < 0.01$) in the opioid abuse rate relative to other states that failed to implement this legislation.

Column 5 reports results from a reduced form regression on the likelihood of a loan default for an indicator variables (Post-legalization) for loans that were terminated in a state that had legalized the sale of marijuana at the time of termination. The results in column 5 indicate that legalization of marijuana resulted in a 3.2% *decrease* ($p < 0.01$) in loan defaults relative to other states that did not implement this legislation.[17]

Finally, column 6 reports slope coefficients of an IV estimator. I instrument the opioid abuse rate with an indicator for marijuana legalization (the first stage is shown in column 4), and find that a 10% change in the opioid abuse rate results in a 2.5% increase in loan defaults.

Two concerns must be addressed in the use of the instrument. First, the instrument should be sufficiently correlated with the endogenous variable. Column 4 reports the first stage of the 2SLS regression. A statistically significant variable drives the results in the first stage. Using the Staiger and Stock (1994) rule of thumb, I reject the hypothesis that the instrument is weak as the reported F-statistic exceeds the critical value (> 10). I further verify that the results are robust to weak instruments by using the tests and confidence intervals proposed by Moreira (2003) and Stock and Yogo (2005). Further, the medical literature provides strong evidence of the first stage relation.[18]

Second, the instrument cannot suffer from the same problem as the original predicting variable. The state regulations, which affect opioid abuse rates may not be exogenously determined. However, a violation of the exclusion restriction suggests that people become more responsible and are better at paying their bills when using marijuana. A more plausible explanation is that alcohol use

[17] Several studies examine the impact of medical marijuana laws on opiate addiction. These studies not only addres if a state law affects the use of opioids, but also studies the impact of retail marijuana sales through dispensaries. (Pacula et al., 2015, 2010; Freisthler and Gruenewald, 2014; Klieger et al., 2017; Powell et al., 2018) These studies are consistent with my finding that the relation of opioid abuse declines even after the year in which the laws were implemented. This finding is consistent with the hypothesis that providing improved access to marijuana dispensaries is associated with a lower incidence of opioid abuse and death.

[18] See for example, (Bachhuber et al., 2014; Bradford and Bradford, 2016, 2017).

and marijuana are substitutes. If marijuana is a substitute for alcohol, then the substitution of marijuana for alcohol may reduce alcohol related car accidents. This reduction in costs related to alcohol abuse may, in turn, be correlated with lower automotive default rates. However, the medical literature finds competing evidence on the hypothesis of marijuana and alcohol being substitutes or complements[19] The lack of compelling evidence of alcohol being a substitute suggests that this mechanism may not be explaining the results related to the relation of loan defaults and legalization of marijuana.

Another possible explanation is another change in regulations that concurrently affected opioid abuse and loan defaults. If another regulation was the cause, them the mechanism related to marijuana is not correctly identified, but the relation of opioid abuse and loan defaults would still be valid. In 2014, Colorado was one of 12 states that adopted a law that limits the initial prescriptions to a 30 day supply. However, there is no empirical support in the medical literature for the claim that the initial prescription rate has a significant impact on the opioid abuse rate. In 2017, Oregon and Washington passed legislation that adopted new rules for prescribing opioid drugs to increase public health by reducing opioid abuse.[20] The last prior law was past in Washington in 2011, three years prior to the introduction of the legalization of recreational marijuana. Oregon had not passed any regulations specifically related to opioids prior to this, but did adopt the CDC's advisory guidelines in 2016 (along with most other U.S. states).

The reliance on truly exogenous variation that does not violate the exclusion restriction is empirically challenging. To help allay some concerns that the choice of states is coincidental, I replicate the instrumental variable test shown in column 6 with a random sample of other states. If the results can be replicated by random choices of other states, the effect may be spurious. If, on the other hand, the results in column 6 are shown to be significantly different, then this would give credence to the hypothesis that opioid abuse leads to loan defaults when instrumenting for the adoption of marijuana legalization.

[19]See for example DiNardo and Lemieux (2001), Cameron and Williams (2001), Williams et al. (2004) and Mark Anderson et al. (2013).

[20]See for example, Washington ESHB 1427. For a comprehensive overview of state laws related to opioid related policy, see also the Arizona department of health services which provides an overview: https://www.azdhs.gov/documents/prevention/womens-childrens-health/injury-prevention/opioid-prevention/50-state-review-printer-friendly.pdf

Figure 4 shows that substituting a random sample of U.S. states in for the three states that implemented the legalization of recreational marijuana does not consistently produce the outcome shown in column 6 of the table. It is clear from the histogram that the sample of 5,000 regressions are centered around a mean of 0. The incidence of Colorado, Washington, and Oregon adopting the laws is at the minimum an outlier in the data.

3.4. Opioid prescription rates

Figure 3 plots the positive association between the percent of loans that default and the opioid prescription rate in reduced form. The figure shows the positive correlation of opioid prescription rates and default rates of borrowers who reside in those counties.

While the use of opioids has been associated with a reduction in chronic pain, opioids are also frequently diverted from their intended medical use. Here, I examine if opioid prescriptions are related to the observed relation of opioid deaths and loan defaults. Specifically, I use a two-stage least squares (2SLS) instrumental variables (IV) analysis to identify an important mechanism that connects the opioid death rate to automotive loan defaults.

Column 1 of Table V reports the first-stage regression—the relation the opioid prescription rate at the time of loan origination and the opioid death rate at the termination of the loan.[21] The regression shows that a one-standard deviation increase in the prescription rate at the time of origination leads to a 1.6% increase in the opioid death rate at the time of loan termination.[22]

Columns 2 and 3 report results from the second stage of a two-stage least squares (2SLS) instrumental variables (IV) analysis. The results indicate that the opioid death rate is a significant predictor of automotive loan defaults, instrumenting with opioid prescription rates when I control for county fixed effects ($p < 0.01$). When year fixed effects are added, the coefficient declines slightly. However, the results are no longer statistically significant as the first stage suffers from a

[21]This result is consistent with the medical literature, which has found a causal link between opioid supply and the opioid death rate (Centers for Disease Control and Prevention and others, 2011).

[22]The CDC data reports prescription data from 90% of non-hospital pharmacies. Pharmacies that sold significantly more opioids than could be consumed in their area, are not represented in the data. For example, Beall (2018) reports that pharmaceutical distributors sold an average of over two million hydrocodone pills per year period to two pharmacies in Williamson, West Virginia, a town with a population of 3,191. The first stage would likely be stronger if the CDC data contained data from all U.S. pharmacies, given the extreme opioid death rates in Appalachia.

weak instrument ($p > 0.10$).

4. Loan origination

Next I investigate if lenders modify origination terms in response to the increase in loan defaults associated with the opioid crisis. Here I try to address the following research questions. First, what is the effect of opioid abuse on automotive loan origination terms? Second, how do lenders' credit models perform during the opioid crisis?

4.1. Loan origination terms and loan defaults

To assess the impact of the opioid crisis on origination terms, I use payment as the primary measure of interest. The monthly payment, controlling for term and vehicle book value, provides the best measure of total consumer cost and exposure of default (when compared to other origination terms).[23]

In order for lenders to price opioid abuse as a risk-factor, they would need to observe the impact on loan performance. While lenders can observe changes in default rates, they would not be able to observe contemporaneous opioid death or prescription rates prior to 2006, as this data was not available at that time. However, lenders could modify their pricing models based on changes in default rates in areas that were affected. For example, lenders commonly include dealership fixed effects in their pricing models which could reliably pick up changes in default rates over time in afflicted areas.

Prior to the financial crisis, over 30% (fewer than 5%) of U.S. counties experience opioid death rates of less than 10 (more than 29) per 100,0000. By 2016, only 3% (over 34%) of U.S. counties would experience a comparably low death rate. Over this period fewer than 5% of U.S. counties experience opioid death rates of more than 29 per 100,0000, and over 34% would experience a comparably high death rate. Given the low impact of death rates on loan defaults early in the sample, it is unlikely that lenders would have priced in the effects of opioid exposure.

[23]For example, the interest rate is not a consistently accurate measure of the pricing of the sub-prime loans due to the impact of consumer protection laws (Melzer and Schroeder, 2017; Brown and Jansen, 2018).

At the start of the opioid crisis (i.e. 1996 to 2006), it is improbable that lenders included the opioid death rate as a priced factor in their credit model. In contrast, after the financial crisis, when opioid death rates spike, one would expect the cost of loans to reflect the higher default risk.[24]. Later in the sample when the time series trend in counties is well established, one may expect changes in origination terms.

Table VI summarizes results from regressions of monthly payment on various regressors related to opioid abuse at the time of origination. Regressions include controls for factors that affect the size of the loan payment: (1) the riskiness of the borrower (credit score, income, and prior bankruptcy); (2) loan terms (term, vehicle book value, and lender discount); (3) the environment in which the loan was originated (i.e. unemployment rate and yield spread); and (4) county fixed effects.

Column 1 shows the coefficients of an ordinary least squares regression of opioid death rates at origination on monthly payments. There is significant time series variation in the size of the payments. A one standard deviation increase in the death rate within a county is correlated with \$7 increase in monthly payments. Column 2 shows slope coefficients of an IV estimator. When instrumenting opioid death rates with lagged opioid prescription rates, I find that a one standard deviation increase in the death rate within a county is correlated with \$25 increase in monthly payments. Column 3 uses an indicator variable for treated counties related to the Florida regulatory change as described in Table III. Column 3 inform us that borrowers face higher payments in areas that are afflicted by the opioid crisis after 2012. Column 4 reports the impact on payment that the opioid death rate has when comparing the counties proximate to Florida that have the highest decile of opioid deaths with the lowest decile of opioid deaths. After the legislative change, borrowers with similar credit history in the most afflicted areas pay \$13 more per month (for a similar term loan) for vehicles that have the same book value.

In column 5, I report the coefficients on regressors for an indicator for two periods (1999 to 2006 and 2012 to 2016) interacted with the opioid death rate at origination. The results show that the relation of opioid death rates and payments is most pronounced *after* the financial crisis. Prior to the financial crisis, the relation of pricing and the opioid death rate is negative. The negative

[24]As early as 2000, reports had circulated widely of the abuse of Oxycontin (Cicero et al., 2005)

coefficient on the period prior to the financial crisis, suggests that borrowers in markets that were later afflicted by the opioid crisis, had historically paid less than comparably their comparably risky counter-parts in other areas.

4.2. credit model and opioid abuse

Next I explore the predictive power of traditional credit information to assess the riskiness of loans during the opioid epidemic. Conceptually, lenders use observable information to determine the expected default rate of a prospective borrower. If lenders cannot distinguish two otherwise similar borrowers because of an unobserved risk factor that affects one of the borrowers, then one may expect that lenders may ration credit or increase the the cost of credit for all affected borrowers. In this section, I estimate the size of these spillover effects arising from this mechanism.

Conceptually, I am interested in a borrower's expected default rate. In the ideal experiment, I would identify two loan markets that are otherwise similar. One market is shocked by the opioid crisis. Using a difference-in-differences framework, I investigate the predictive power of FICO score and other borrower credit attributes on realized default rates. As such, my analysis requires that I first estimate the likelihood that a buyer would default given the characteristics that a lender observes at the time of origination.

To construct the expected default rate, I generate a predicted default rate for each loan in the data. Specifically, I regress the default rate of loans that were originated prior to the financial crisis (i.e. prior to 2007) against the borrower- and loan-specific characteristics. I then use the coefficient estimates to predict a default rate for all loans in the data. I interpret the predicted default rate as a composite measure of the riskiness of the borrower. Importantly, the measure reflects only information about the borrower that was available at the time of the transaction. One assumption underlying the composite measure is that lenders use similar factors in assessing a buyer's riskiness—more specifically, I assume that the coefficient estimates are valid out-of-sample weights.

I assess the strength of the composite risk measure out-of-sample by comparing the predicted (ex-ante) default rates to the realized (ex-post) default rates for my sample of loans. Table VII reports coefficients from OLS regressions on the predicted default rate of the borrowers for different subsamples. As a baseline, Column 1 reports the coefficients of a DiD framework on borrower FICO score on the likelihood of default in a sample of the highest and lowest decile of opioid death rate. The results show that the FICO score is a significantly less useful predictor of loan default rates after 2012 in counties that had high opioid death rates.

Column 2 reports the coefficient from an OLS regression of the predicted default rate on the realized default rate for the *in sample* period (i.e. 1999 to 2006). The predicted default rate (i.e. the observable characteristics) predict 29.2% of the variation for this period.[25] Columns 3 and 4 report the relation for the year 2007 to 2011 and 2012 to 2016 respectively. The out-of-sample reliability of the predicted default rate declines predictably during the financial crisis. In comparison to the in-sample period, the coefficient on predicted default rate declines from 1.03 to 0.66. The predicted default rate predicts 13.1% of the variation. After the financial crisis, when macroeconomic conditions have improved, one might expect the predicted default rate to perform better than during the financial crisis. However, the coefficient on the predicted default rate declines to 0.53.

Columns 5 and 6 report the coefficient estimates from difference-in-differences regressions on the loan default rates of borrowers. The regressor represents an indicator variable that is equal to 1 if the borrower is treated and if the loan termination was after the Florida legislative change (i.e. $\tau => 2012$), and zero otherwise. A borrower is treated if the loan was originated in a county with the highest decile of opioid death rate, and zero otherwise. Columns 5 and 6 also include an interaction term of the indicator variable with the predicted default rate. When compared to the period during the financial crisis, the predicted default rate is a *better* predictor of defaults in counties in which the opioid crisis is not in the top decile. Further, the predicted default rate is significantly less reliable in counties that are most affected by the opioid crisis. Moreover, loans which were originated in the counties most affected by the opioid crisis are 4.3% more likely to default compared to other counties after the financial crisis. Column 6 compares the borrowers in

[25] In the sub-prime market for automotive loans, it is difficult to predict who will default on a loan. This is reflected in the high interest rates that lenders charge in this market. The average APR in my sample is 19.3%, reflecting the significant systematic risk associated with extending credit to individuals that are much more likely to default when economic conditions deteriorate.

treated counties with the lowest decile of opioid death rate as the counter-factual. Loans which were originated in the counties most affected by the opioid crisis are 16.9% more likely to default compared to counties least affected (lowest decile) of opioid deaths.

4.3. Borrower loan cost

Next I investigate how the total cost of automotive sub-prime loans is related to the opioid crisis.[26] Loan-level data permits the analysis of total observable costs faced by borrowers. In the loan data, I observe the down payment, monthly payments, and other fees paid over the life of the loan.

Benefits arising from the purchase of the vehicle may also be difficult to assess—depreciation over the life of the loan may provide one market-based measure of the value derived from vehicle ownership. Instead, I control for benefits derived from vehicle use by controlling for the vehicle book value. Thus using the total cost of the loan to borrowers allows me to relate the geographical exposure to the opioid crisis.

Table VIII shows the coefficients of OLS regressions for various regressors related to their geographical exposure to the opioid crisis on total total costs paid by the borrower. Column 1 shows that the contemporaneous opioid prescription rate at the time of origination, reliably predicts the total loan cost. Controlling for borrower characteristics, year and county fixed effects, a one standard deviation increase in the opioid prescription rate results in a $2,479 increase in total loan costs for sub-prime borrowers, representing an approximately 14.7% increase in costs. Using the contemporaneous opioid death rate (column 2) and abuse rate (column 3), I also find a similar increase in total loan costs attributable to the opioid crisis. A one standard deviation increase in the death (abuse) rate is correlated with the $1,691 ($1,427) increase in total loan costs. Column 4 reports the coefficient of an indicator variable for counties that were in the highest decile of opioid death rate. Borrowers in counties that were in the highest decile of opioid deaths paid $891 more for their car loans, ceteris paribus.

[26] Borrowers may incur additional unobservable costs, especially in the event of a default. For example, consumers with a history of default or vehicle repossession may face especially high interest rates on new loans or may be unable to secure additional credit at all. Moreover, borrowers without access to alternative transportation may be unable to commute to current or prospective workplaces.

4.4. Discussion

Opioid abuse has been linked with lower labor force participation (Krueger, 2017). In this paper, I explore if the use and abuse of opioids is linked with loan performance and origination terms. The mechanism by which opioid abuse relates to loan defaults is likely associated with labor force participation. Krueger (2017) finds that almost half of men that are not participating in the labor force are taking pain medication on a daily basis. While the causality of the relation of labor force participation and opioid use is unclear, there appears to be medical evidence that workers suffer from higher injury rates requiring additional medication. In contrast, it seems plausible that opioid abuse may have a negative impact on labor force participation. If opioid abuse leads to lower labor force participation, this could help to explain the results related to loan performance that are attributable to the opioid crisis.

Asymmetric information in the market for loans leads to an overall increase in loan costs for borrowers in opioid afflicted regions. The results in Table VII suggests that lender have a more difficult time assessing the creditworthiness of borrowers in areas that are acutely affected by the opioid crisis. Since lenders do not know which borrower will be adversely affected by opioid use, all borrowers in the market end up paying more for their loans as shown in Table VI and Table VIII.

I interpret my estimates as spillovers resulting from the increased medical access to prescription opioids. I find that loan defaults increase among a population that had increased access to prescription opioids. The results also show that the impact of the opioid access is partially mitigated by access to recreational marijuana.

5. Conclusion

Over 3 million Americans suffer from opioid dependence. While several studies have assessed the economic impact of this health crisis (e.g. increased mortality rate, increase medical expenses, and negative impact on labor participation and productivity) a gap in the literature exists in how financial markets are affected. This study sheds light on this important issue.

Using a matched sample individual auto loans with county-level data on opioid prescriptions and deaths, I examine the relation between opioid crisis and auto lending. Although I cannot observe the reasons for default, I find evidence suggesting that opioid abuse is a plausible and empirically relevant explanation. I find that consumers who live in areas with higher prescription rates, and consequently higher opioid death rates, default more frequently. I interpret the results as evidence of the impact of the opioid crisis on loan performance. Borrowers in areas that are most affected by the opioid crisis experience significantly higher loan defaults than comparable borrowers in other areas. These results can be identified through a plausible exogenous shock to the supply of an opioid substitute — marijuana. States that implemented laws allowing dispensaries to sell marijuana for recreational purpose experience a *decline* in opioid deaths and the correlated loan default rates.

The study shows some evidence of lenders learning about the impact of the opioid crisis on their loan portfolios by adjusting the pricing for areas that are most afflicted. The credit model of lenders perform poorly in areas that are affected by the opioid crisis. As a result, borrowers in these areas pay significantly more for access to consumer credit. This finding is consistent with a spillover effect on consumer finance attributable to the opioid crisis.

Additional work on the impact of the opioid epidemic on other financial markets is also merited. Automotive loans are securitized. As a result, loans from opioid affected areas may adversely affect asset-back structures. Moreover, the relation of opioid abuse and labor markets may also impact the performance of firms in affected areas. Thus, studies connecting the opioid epidemic and firm performance may be of some interest.

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Appendix A. Variable Definitions

This appendix reports definitions for the variables used in the analysis. Data were obtained from an indirect auto financing firm; The unemployment rate from the U.S. Bureau of Labor Statistics, the yield spread from the Federal Reserve Bank of St. Louis; and the opioids data from the Centers for Disease Control and Prevention (CDC), National Center for Injury Prevention and Control, Division of Unintentional Injury Prevention.

Amount financed: Original amount financed, measured in $.

Book value: Vehicle book value, measured in $.

Book value change: The change in book value of the vehicle over the duration of ownership of the borrower (i.e. from acquisition to loan termination).

Default: Indicator [with a value of 100] for termination of the loan due to default (i.e. failure to make payments).

Deficiency Recovery: Proceeds from vehicle liquidation minus the costs of recovery and resale, measured as a % of the vehicle book value.

Discount Total of discount and fees charged to the dealer by the lender during the loan purchase, measured in $.

FICO score: Mean of borrower's credit scores from all queried credit-reporting agencies. Assigned a value of 0 if no credit score is available.

Homeownership: Indicator for borrower's homeownership status at origination.

Income: Borrower's gross monthly income as calculated at loan underwriting, measured in $.

Lender discount: Total of discount and fees charged to the dealer by the lender during the loan purchase, measured in $.

Loan charge off: Outstanding loan balance at default calculated as the initial amount financed minus the payments made against the loan, measured in $.

Loan duration: Months between origination and loan termination due to default (i.e. the number of payments made on the loan).

Monthly payment: Monthly payment, measured in $.

Opioid death rate: Drug poisoning deaths and rates per 100,000 for all races, both sexes, ages 20 to 79 for the years 1999 to 2016. The data includes ICD-10 Codes: X40-X44,X60-X64,X85,Y10-Y14. https://www.cdc.gov/injury/wisqars/fatal.html

Opioid prescription rate: Opioid prescribing rates per 100 persons as reported by the IQVIA Transactional Data Warehouse (TDW) for 2006 to 2017 as presented on the CDC website. www.cdc.gov/drugoverdose/maps/rxrate-maps.html

Opioid abuse intensity: Opioid death rate divided by the opioid prescription rate for a given year.

Prior bankruptcy: Indicator for chapter 7 bankruptcy in the seven years prior to the loan application.

Risk Score: Credit rating assigned by the lenders proprietary algorithm.

Term: Original term of the contract, measured in months.

Uncollectible: Loans which are in default that are deemed to be uncollectible.

Unemployment rate: Metropolitan Statistical Area unemployment rate in 2010, measured in %.

Vehicle book value: Vehicle book value, measured in $.

Yield spread: Bank of America - Merrill Lynch U.S. corporate AAA–BBB option-adjusted spread at the time of origination, measured in %.

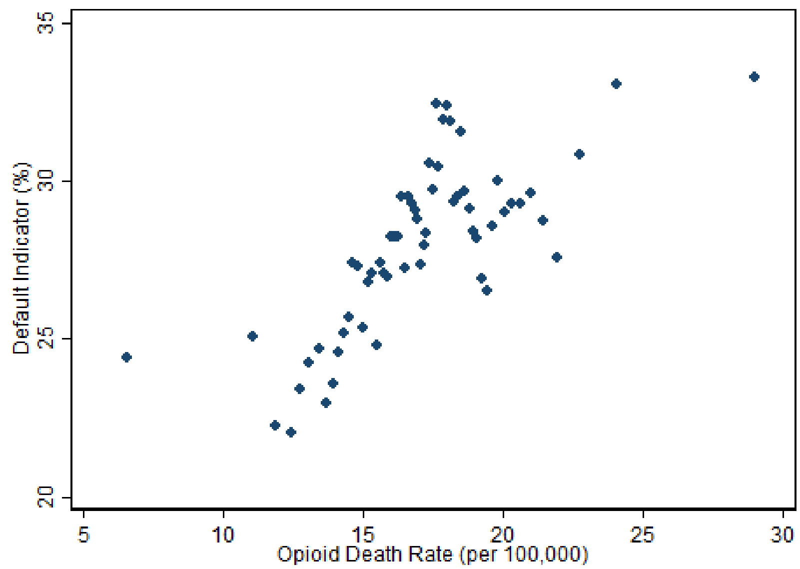

Figure 1. This figure presents a binned scatter plot of the percent of loans that default and the opioid death rate (per 100,000). Controls include credit score, income, homeownership (ind), prior chapter 7 bankruptcy, and county fixed effects.

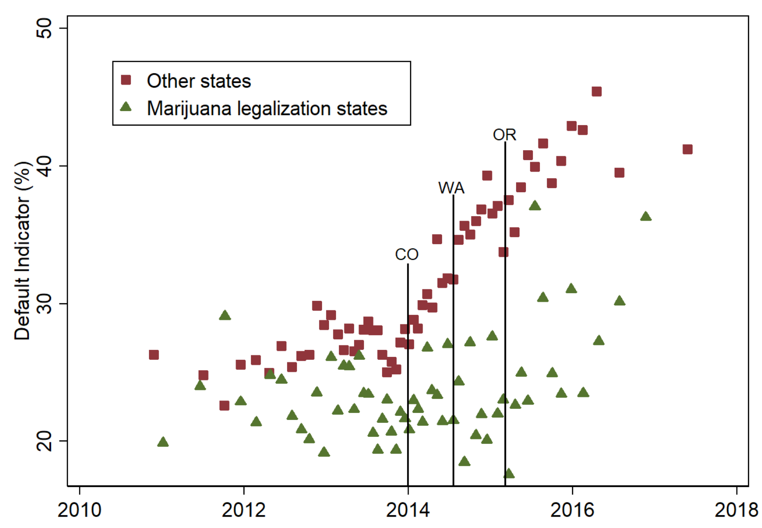

Figure 2. This figure presents a binned scatter plot of the percent of loans that default by year in which the loans terminated, showing a sample of states that legalized marijuana and those states that did not. Controls include prior income, credit score, homeownership, prior chapter 7 bankruptcy, and unemployment rate. States that legalized marijuana are plotted as green triangles; states that do not have legalized marijuana for sale in our sample period are plotted as red squares.

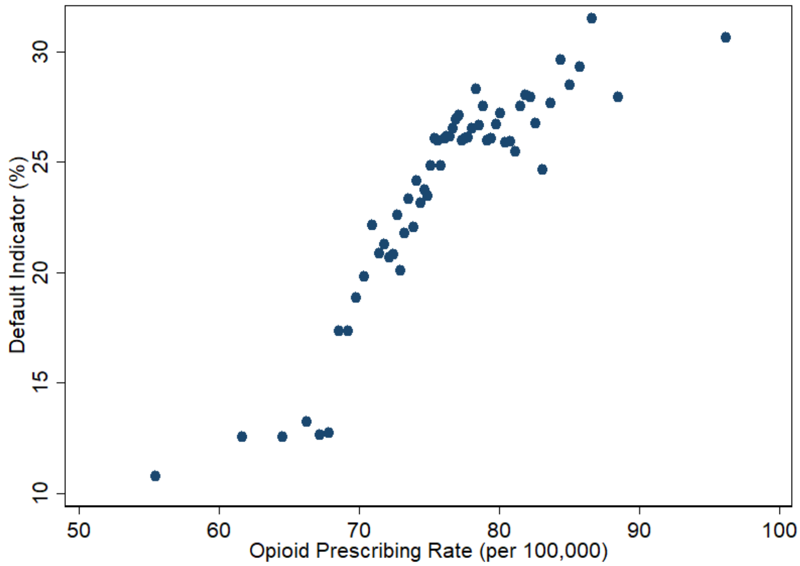

Figure 3. This figure presents a binned scatter plot of the percent of loans that default and the opioid prescription rate (per 100,000). Controls include credit score, income, homeownership (ind), prior chapter 7 bankruptcy, and county fixed effects

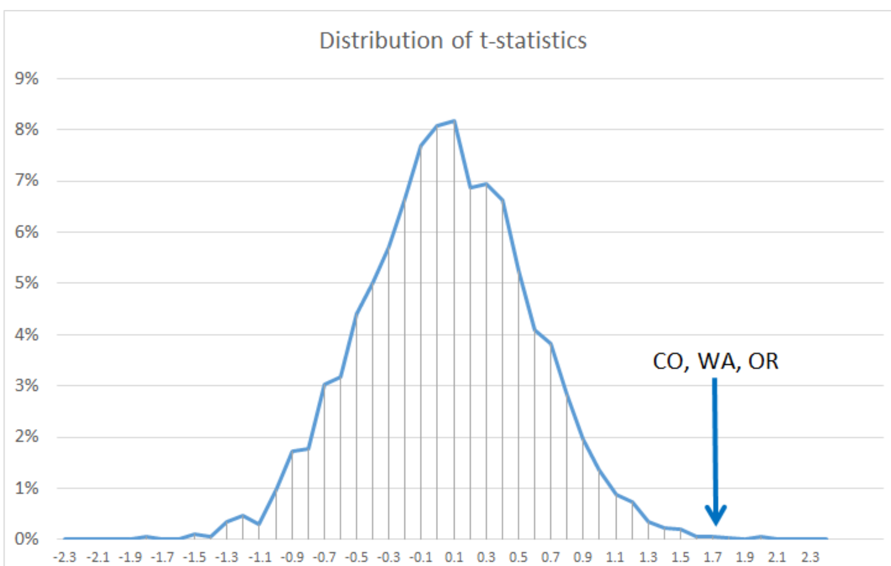

Figure 4. This figure presents a histogram of a distribution of t-statistics for the slope coefficients of an IV estimator on loan defaults. I instrument the opioid abuse rate with an indicator for marijuana legalization (the first stage is shown in Table IV column 4). For a random sample of 5,000 regressions, I randomly substitutes three states to coincide with the adoption dates of legalized marijuana.

Table I

Summary statistics. The Table shows summary statistics for variables used in the paper.

	count	mean	sd	p25	p50	p75
Borrower Characteristics:						
FICO score	136,811	534	53	498	532	568
Income ($)	136,811	3,334	2,345	2,260	3,367	4,600
Prior bankruptcy	136,811	0.26	0.44	0	0	1
Loan Origination Characteristics:						
Vehicle book value ($)	136,806	13,392	4,525	10,375	12,925	15,751
Monthly payment ($)	136,811	393	111	331	395	454
Amount financed ($)	136,811	16,551	5,068	13,200	16,370	19,515
Term (months)	136,811	66.40	8.05	60	72	72
Lender discount ($)	136,811	412	596	149	499	799
Loan Performance:						
Loan duration (months)	130,999	31.18	20.85	15	26	45
Default (%)	136,811	27.93	44.87	0	0	100
Loan charge off ($)	134,477	1,657	3,922	0	0	0
Deficiency Recovery (%)	132,200	4.51	19.41	0	0	0
Uncollectible deficiency	18,303	0.33	0.47	0	0	1
Loan Environment:						
Yield spread (%)	136,301	2.04	0.96	1.31	1.93	2.44
Unemployment rate (%)	136,787	5.48	1.88	4.1	5.1	6.6
Opioid death rate (per 100,000)	136,811	16.93	7.23	11.77	15.71	21.26
Opioid prescription rate (per 100)	87,321	76.95	19.60	64.30	75.70	87.00
Opioid abuse intensity	87,298	22.84	10.65	16.25	21.34	26.06

Table II

Loan performance and opioid abuse. This table summarizes results from regressions of an indicator for default (reported as %) on the opioid death rate (columns 1, 2, & 3) and the opioid abuse rate (columns 4, 5, & 6). Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy), and the local environment(i.e. unemployment rate). County, year, and dealership fixed effects are included as reported. Robust standard errors, clustered by county-year, are reported in parentheses. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Dep Var:			**Default indicator**			
	(1)	**(2)**	**(3)**	**(4)**	**(6)**	**(6)**
Opioid death rate	0.650***	0.291***	0.297***			
	(0.082)	(0.095)	(0.102)			
Opioid abuse rate				0.099***	0.099***	0.103***
				(0.038)	(0.029)	(0.031)
FICO score	-0.149***	-0.152***	-0.146***	-0.152***	-0.153***	-0.146***
	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
Income	-0.001***	-0.002***	-0.002***	-0.001***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior bankruptcy	-14.492***	-13.233***	-12.044***	-14.256***	-13.208***	-12.044***
	(0.789)	(0.643)	(0.584)	(0.805)	(0.644)	(0.584)
Unemployment rate	2.425***	2.120***	1.991***	2.387***	2.096***	1.959***
	(0.362)	(0.295)	(0.290)	(0.379)	(0.304)	(0.300)
County FE	Yes	Yes	No	Yes	Yes	No
Dealership FE	No	No	Yes	No	No	Yes
Year FE	No	Yes	Yes	No	Yes	Yes
Cluster	County-Yr	County-Yr	County-Yr	County-Yr	County-Yr	County-Yr
Observations	136,055	136,055	135,694	136,055	136,055	135,694
Adjusted R^2	0.091	0.106	0.117	0.089	0.106	0.117

Table III

Florida prescription drug monitoring program (PDMP) and loan performance. This table reports the coefficient estimates from difference-in-differences regressions on the loan default rate (reported as %) for a borrower. The regressor high death rate (shown in column 1 & 2; high abuse rate in column 4) represents an indicator variable that is equal to 1 if borrower is treated and if the loan termination was after the implementation of Florida prescription drug monitoring (i.e. τ => 2012), and zero otherwise. A borrower is treated if the loan was originated in a county with the highest decile of opioid death rate (opioid abuse rate), or control if the loan was originated in in any other county. The regressor in column 3 & 5 is a triple interaction as described above interacted with an indicator if the county was within 700 miles of the Florida state line. Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy), and the local environment(i.e. unemployment rate). County and year fixed effects are included as reported. Robust standard errors, clustered by county-year, are reported in parentheses. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Dep Var:			Default indicator		
	(1)	(2)	(3)	(4)	(5)
High death rate x [$\tau > 2012$]	13.367***	13.347***			
	(1.999)	(1.998)			
High death rate x [$\tau > 2012$]			16.306***		
x Florida proximate state			(3.569)		
Abuse rate x [$\tau > 2012$]				0.105***	
				(0.040)	
Abuse rate x [$\tau > 2012$]					0.377***
x Florida proximate state					(0.065)
FICO score	-0.151***	-0.151***	-0.151***	-0.197***	-0.197***
	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)
Income	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior bankruptcy	-11.001***	-10.989***	-10.955***	-17.562***	-17.524***
	(0.476)	(0.476)	(0.479)	(0.563)	(0.559)
Unemployment rate		0.472	0.566*	-0.012	-0.026
		(0.306)	(0.302)	(0.435)	(0.430)
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster	County-Yr	County-Yr	County-Yr	County-Yr	County-Yr
Sample	2010 to 2014	2010 to 2014	2010 to 2014	2010 to 2014	2010 to 2014
	excl. FL	excl. FL	excl. FL	excl. FL	excl. FL
Observations	66,683	66,683	66,683	42,464	42,464
Adjusted R^2	0.066	0.066	0.063	0.111	0.112

Table IV

Marijuana legalization and loan performance Column 1 to 4 report results from OLS regressions on the opioid abuse rate for indicator variables (post-legalization) for loans that were terminated in a state that had implemented laws allowing the recreational sale of marijuana at the time of termination. Column 5 reports results from OLS regressions on the likelihood of a loan default (reported as %) for the regressor described above. Column 6 reports slope coefficients of an IV estimator on the regressor. Column 4 shows the first stage of the 2SLS. Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy), and the local environment(i.e. unemployment rate). County, year, and dealership fixed effects are included as reported. Robust standard errors, clustered by state-year, are reported in parentheses. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Dep Var:	Opioid abuse rate				Default indicator	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	First Stage	OLS	2SLS
Post-legalization CO	-3.821***					
	(0.616)					
Post-legalization WA		-1.201*				
		(0.612)				
Post-legalization OR			-3.385***			
			(1.152)			
Post-legalization All				-2.981***	-3.211*	
				(0.544)	(1.693)	
Opioid abuse rate						1.077*
						(0.600)
FICO score	0.001	0.001	0.001	0.001	-0.169***	-0.170***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.007)	(0.007)
Income	-0.000**	-0.000***	-0.000***	-0.000**	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior bankruptcy	0.919***	0.932***	0.941***	0.894***	-15.377***	-16.340***
	(0.306)	(0.303)	(0.304)	(0.306)	(0.752)	(1.000)
Unemployment rate	-0.605**	-0.379	-0.396	-0.540*	-0.471	0.110
	(0.301)	(0.308)	(0.309)	(0.292)	(0.472)	(0.643)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	State-Yr	State-Yr	State-Yr	State-Yr	State-Yr	State-Yr
Sample	2012 to 2016	2012 to 2016	2012 to 2016	2012 to 2016	2012 to 2016	2012 to 2016
Observations	60,020	60,020	60,020	60,020	60,020	60,020
Adjusted R^2	0.448	0.446	0.446	0.448	0.086	

Table V

Opioid death rate, opioid prescription rates, and loan performance. This table reports the coefficients from an ordinary least squares regression of local capital on opioid prescription rate (column 1), representing the first stage of a two-stage least squares regression. The table also reports results from a 2-stage least squares procedure where the dependent variable is an indicator for the loan default (columns 2 and 3). The equation includes controls for borrower and local characteristics, in addition to county and year fixed effects as reported. Robust standard errors, clustered by county-year, are reported in parentheses. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Dep Var:	Opioid death rate	Default indicator	
	(1)	(2)	(3)
	First stage	2SLS	
Opioid prescription rate	0.081***		
	(0.010)		
Opioid death rate		4.764***	3.302
		(0.691)	(16.221)
FICO score	-0.004***	-0.131***	-0.146***
	(0.001)	(0.006)	(0.038)
Income	0.000***	-0.003***	-0.002***
	(0.000)	(0.000)	(0.000)
Prior bankruptcy	0.317***	-15.169***	-13.153***
	(0.085)	(0.862)	(0.665)
Unemployment rate	0.118**	-3.105***	-1.226
	(0.059)	(0.398)	(4.650)
County FE	Yes	Yes	Yes
Year FE	No	No	Yes
Sample	Full	Full	Full
Cluster	County-Yr	County-Yr	County-Yr
Observations	87,591	87,591	87,591
Adjusted R^2	0.785		

Table VI

Loan origination and opioid abuse This table summarizes results from regressions on loan payment for regressors related to the opioid death rates and opioid abuse rates at the time of origination. Column 1 reports coefficients on the opioid death rate in the year of origination. Column 2 reports the coefficients of an IV estimator in which the opioid prescription rate in the prior year is used as an instrument for the death rate at loan origination. Column 3 & 4 use an indicator variable for treated counties related to the implementation of Florida's PDMP as described in Table III. Column 5 uses regressors for an indicator for two periods (1999 to 2006; 2012 to 2016) interacted with the opioid death rate at origination. Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy), loan terms (term, vehicle book value, and lender discount) and the environment that influences the loan costs(i.e. unemployment rate and yield spread). County are as reported. Robust standard errors, clustered as reported in parentheses. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Dep Var:	Payment				
	(1)	(2)	(3)	(4)	(5)
	OLS	2SLS	OLS	OLS	OLS
Opioid death rate (origination)	0.638***	2.983***			
	(0.087)	(0.869)			
High abuse rate x [$\tau > 2012$]			0.248***		
			(0.035)		
High death rate x [$\tau > 2012$]				13.097***	
x Florida proximate state				(2.916)	
Opioid death rate (1999-2006)					-0.264**
					(0.109)
Opioid death rate (2012-2016)					0.437***
					(0.083)
FICO score	-0.159***	-0.155***	-0.158***	-0.152***	-0.164***
	(0.008)	(0.008)	(0.009)	(0.018)	(0.009)
Income	0.005***	0.004***	0.005***	0.005***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior bankruptcy	0.230	-0.978	1.999***	3.800**	0.362
	(0.646)	(0.912)	(0.762)	(1.754)	(0.844)
Yield spread	0.994**	1.067**	0.715*	1.620**	1.806*
	(0.441)	(0.505)	(0.405)	(0.633)	(1.034)
Unemployment rate	1.478***	2.729***	1.253***	1.270*	-0.345
	(0.276)	(0.600)	(0.316)	(0.675)	(0.462)
Book Value	0.017***	0.018***	0.017***	0.017***	0.017***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Discount	-0.011***	-0.010***	-0.009***	-0.009***	-0.010***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Term	-1.138***	-1.399***	-1.197***	-1.212***	-1.235***
	(0.045)	(0.108)	(0.051)	(0.096)	(0.049)
County FE	Yes	Yes	Yes	Yes	Yes
Sample	Full	Full	> 2006	Hi/Lo Decile	Excl. 2007-2011
Cluster	County-Yr	County-Yr	County-Yr	County-Yr	County-Yr
Observations	193,970	193,970	136,922	27,222	138,548
Adjusted R^2	0.521	0.514	0.536	0.504	0.540

Table VII

Credit modeling and the opioid crisis Columns 1, 5, and 6 report the coefficient estimates from difference-in-differences regressions on the loan default rates (reported as %). In column 1, the I show the FICO score interacted with an indicator that is equal to 1 if borrower is treated and if the loan termination was after the Florida legislative change (i.e. τ => 2012), and zero otherwise. For three sample periods, columns 2, 3, &4 report the coefficients of the predicted default rate and regressor for an indicator if the loan was originated in a county with the highest decile of opioid death rate Predicted default rates for all loans are generated using coefficient estimates from an unreported regression of the actual default rate of loans on all loans originated prior to 2006 against the borrower- and loan-specific characteristics in Table 1. The regressor high death rate represents an indicator variable that is equal to 1 if borrower is treated and if the loan termination was after the Florida legislative change (i.e. τ => 2012), and zero otherwise. Column 5 compares the treated borrowers with the lowest decile of opioid death rate as the counter-factual. County and year fixed effects are as reported. Robust standard errors, clustered by county-year, are reported in parentheses. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Dep Var:	Default indicator					
	(1)	(2)	(3)	(4)	(5)	(6)
FICO score	-0.093***					
	(0.009)					
High death rate x [$\tau > 2012$]	0.035***					
x FICO score	(0.006)					
Predicted default rate		1.025***	0.656***	0.526***	0.836***	0.973***
		(0.046)	(0.023)	(0.021)	(0.028)	(0.085)
High death rate		2.091	3.994	16.829***		
		(2.027)	(4.416)	(2.233)		
High death rate x [$\tau > 2012$]					23.831***	47.915***
					(4.039)	(6.987)
High death rate x [$\tau > 2012$]					-0.397***	-0.631***
x Predicted default rate					(0.064)	(0.093)
County FE	No	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes	Yes
Sample	Hi/Lo Decile	1999-2006	2007-2011	2012-2017	Full	Hi/Lo Decile
Cluster	County-Yr	County-Yr	County-Yr	County-Yr	County-Yr	County-Yr
Observations	16,633	67,019	58,862	50,797	176,742	26,612
Adjusted R^2	0.046	0.292	0.131	0.048	0.163	0.181

Table VIII

Opioid abuse and borrower loan cost. This table summarizes results from regressions of the total paid by borrowers on regressors associated with opioid abuse (1: prescription rate; 2: opioid death rate in the year of origination; and 3: opioid abuse rate in the year of origination). Column 4 reports the coefficient on an indicator for the highest decile of opioid death rate interacted with the average default rate for that county from 1995 to 2006. Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy), and the local environment(i.e. unemployment rate). County, year, and dealership fixed effects are included as reported. Robust standard errors, clustered by county-year, are reported in parentheses. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively.

Dep Var:	Total loan cost			
	(1)	(2)	(3)	(4)
Opioid prescription rate	126.554***			
	(25.259)			
Opioid death rate (origination)		234.549***		
		(37.011)		
Opioid abuse rate (origination)			134.255***	
			(23.184)	
High death rate				354.501***
x Historical default rate				(52.888)
Book Value	1.061***	1.013***	1.011***	1.115***
	(0.007)	(0.008)	(0.008)	(0.011)
FICO score	-1.464*	-1.250	-1.451*	-1.552
	(0.864)	(0.817)	(0.803)	(1.035)
Income	0.228***	0.351***	0.371***	0.317***
	(0.013)	(0.017)	(0.017)	(0.015)
Prior bankruptcy	-945.868***	-269.370***	-311.887***	93.836
	(128.400)	(75.166)	(75.571)	(74.791)
Yield spread	323.711***	-11.535	-15.581	-217.491***
	(115.279)	(34.457)	(35.027)	(46.684)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	2012-2017
Cluster	County-Yr	County-Yr	County-Yr	County-Yr
Observations	85,193	71,302	85,614	39,548
Adjusted R^2	0.487	0.379	0.374	0.627