Spillover Effects of the Opioid Epidemic on Consumer Finance *

Mark Jansen†

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ABSTRACT

I examine the impact of the opioid epidemic on subprime auto lending. Using a difference-in-differences framework, I find that county-level increases in opioid abuse cause an increase in loan defaults. Moreover, I find that traditional credit scoring attributes (e.g., FICO score) fail to predict loan performance deterioration associated with opioid addiction. The resulting higher default rates and weaker predictive performance of traditional credit measures generate a negative externality for borrowers in opioid-afflicted areas, as evidenced by 4.4% higher loan costs for subprime borrowers.

JEL Classification: D14, D82, G29, I15

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†University of Utah, Eccles School of Business, 1655 Campus Center Dr, Salt Lake City, UT 84112. (O) (801) 213-6910. (f) (775) 201-1325. E-mail:mark.jansen@eccles.utah.edu

1. Introduction

Prescription opioid and heroin addiction is a global epidemic that affects both health and economic welfare. In the United States, over two million people suffer from opioid-related use disorders and over 400,000 people have died from overdoses in the last 15 years—amounting to a four-fold increase in the mortality rate since 1999. Deaths from opioid overdoses are now more common than fatalities from automobile accidents (Centers for Disease Control and Prevention, 2018). In addition to its effects on health and mortality rates, opioid abuse has significant economic costs. In 2015, the total annual cost of the opioid epidemic was estimated at $504 billion (Council of Economic Advisers, 2017). Opioid abuse also affects the labor market: one-third of prime-age men who do not participate in the labor force are using prescription pain medication (Krueger, 2017; Aliprantis and Schweitzer, 2018).

Although the health impacts and some economic impacts of opioid abuse have been examined, little is known about the spillover effects on financial markets. This study provides the first empirical evidence of a relation between opioid abuse and consumer credit. I investigate (1) whether local exposure to opioid abuse is a significant risk factor for lenders and (2) whether this risk factor creates costly externalities for lenders and consumers. If communities with high rates of opioid abuse experience higher loan default rates, and if traditional credit scoring (e.g., FICO) attributes fail to identify borrowers who are prone to abusing opioids, then lenders in those markets will face higher credit risks and borrowers may face credit rationing and higher prices as a result. The consequences of the opioid epidemic could thus extend beyond the labor market to affect the pricing for consumer finance products, possibly leading to other repercussions (e.g., deteriorating credit-market conditions) for consumers in those areas.

A growing literature on the externalities imposed by mortgage loan defaults suggests that deteriorating credit-market conditions can adversely affect local economic activity (Campbell et al., 2011; Anenberg and Kung, 2014; Mian et al., 2015; Mian and Sufi, 2018). The opioid epidemic's impact on local credit markets could help to explain the economic decay that is observed in opioid-afflicted areas. The externalities described in this study could also spill over into the $100 billion auto loan securitization market. The subprime loan market is an ideal setting in which to study the impact of opioid abuse on consumer finance, not only because the borrowers in this market fall within the at-risk population for opioid abuse (Zedler et al., 2015, 2017) but also because the market is large: most U.S. households have a vehicle, and more than one-third have an auto loan (Bricker et al., 2017). Recent auto loan balances exceed $1.14 trillion, and 40% of these loans are non-prime or lower credit (Zabritski, 2018).

Exploiting variation in county-level opioid mortality and opioid prescription rates, I calculate a new measure for opioid abuse: I divide the opioid death rate by the lagged opioid prescription rate.[1] Using a difference-in-differences framework in two natural experiments, I document the relation of opioid abuse to loan performance using a panel of 149,300 non-prime, subprime, and deep subprime auto loans.[2] I then explore whether traditional credit attributes allow lenders to identify risk factors associated with opioid addiction. Finally, I use loan repayment and collections data to examine how increases in county-level opioid abuse manifest real costs for lenders and subprime borrowers.

I find that counties with higher rates of opioid abuse have higher loan default rates. After accounting for borrowers' creditworthiness and local economic conditions, I find that a one-standard deviation increase in county-level opioid abuse is associated with a 3.4% increase ($p < 0.01$) in loan defaults, relative to the mean. This *increase* in default rates is comparable to a $464 *decrease* in a borrower's monthly income. Studies on the intertemporal choices of opioid-dependent patients show that these individuals tend to choose more immediate rewards even if the rewards are smaller (Madden et al., 1997; Kirby et al., 1999).[3] Such choices are likely to be unconducive to servicing consumer debt.

The identification of the relation between opioid abuse and loan defaults could be problematic in

[1] Frieden and Houry (2016) find that 1.82 of every 1,000 patients who receive prescription opioids die from an opioid-related cause. The median death occurs within 2.6 years of the initial prescription, which matches the average time-to-default (2.6 years) of the loan sample. When the prescription rate is high and the death rate is comparatively low, this suggests that the opioid prescriptions have high therapeutic benefits relative to their costs. In contrast, when the death rate is high and the prescription rate is low, this suggests a greater diversion of prescribed opioids for non-medical use.

[2] Experian defines subprime auto loan consumers as having a FICO score from 501 to 600. Although this market is distinct from non-prime (FICO 601 to 660) and deep subprime (less than 500), I refer to all three of these segments hereafter as "subprime".

[3] Prescription opioids moderate pain signals. The ensuing psychological effects associated with the abuse of opioids alter brain chemistry, sometimes leading to repeated use of the drug—initial enjoyment leads to anxiety about the next use as substance dependence develops. Thus, individuals facing strong withdrawal symptoms may be more interested in satisfying their next drug craving than in making consistent car payments.

the aforementioned ordinary least squares (OLS) regression. First, one must rule out the possibility that an omitted variable related to changing economic conditions is driving the increases in both opioid abuse and loan defaults. Such a variable could bias the estimates from an OLS specification. Second, reverse causality is a concern, as one could plausibly argue that defaults cause an increase in opioid abuse. To address these concerns, I identify the relation between opioid abuse and loan defaults using a difference-in-differences framework in two natural experiments that have different identifying assumptions.

In the first natural experiment, I examine the effects of an exogenous increase in opioid potency on loan defaults as users shifted from prescribed pharmaceuticals to illicit street drugs. In September 2011, the state of Florida implemented a prescription drug monitoring program and new rules limiting the supply of opioid drugs from high-prescribing pain clinics known as "pill mills"—regulations that severely reduced the state's prescription opioid supply (Rutkow et al., 2015; State of Florida, 2011). In the two years prior to the passage of these regulations, Florida accounted for over 43% of the total prescribed opioids in the U.S., despite having only 6% of the population. Much of this supply traveled to other states via what law enforcement officials termed the "Florida oxy express" (Alvarez, 2011). In high-abuse areas throughout the nation, a significant portion of the illicitly used prescription opioids originated in Florida (Beall, 2018).[4] The 2011 Florida regulations significantly reduced the supply of prescribed opioids, which led to higher street prices for these drugs. In turn, buyers shifted towards less costly and more powerful opioids such as heroin and illicit fentanyl. Opioid death rates across the U.S. increased following these regulatory changes (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 that were more reliant on prescription opioids from Florida (i.e., those with high levels of opioid abuse three years prior to the new regulations and within 600 road miles of a Florida pharmacy) experience higher rates of opioid abuse and have more loan defaults after Florida's legislative changes.[5] The difference-in-differences specification with county and year fixed

[4] Counties with higher opioid abuse rates in the pre-regulation change period saw opioid consumption rates in excess of what could have been plausibly supplied through local prescriptions.

[5] The stronger economic relation between opioid abuse and counties close to Florida is consistent with Drug Enforcement Agency (DEA) reports that, in the past, addicted users would make day trips to Florida to purchase

effects helps to mitigate concerns that cross-sectional differences in counties or national trends in employment levels or heroin availability are simultaneously affecting opioid abuse and loan performance. After the legislative change, counties that were more reliant on prescription opioids from Florida experience a 4.1% increase in loan default rates relative to counties with below-median historical opioid abuse levels.

In the second natural experiment, I exploit the legalization of marijuana, a drug whose analgesic benefits for chronic pain have been compared to those of prescription opioids (Hill, 2015). When states legalize marijuana, addicted opioid users can choose to acquire opioids, often illegally and at high costs, or less-expensive legal marijuana from a dispensary. The medical literature has documented that many choose to substitute marijuana (Bachhuber et al., 2014; Reiman et al., 2017; Powell et al., 2018). Drug-abuse treatment efforts that increase the availability of substitutes for opioid drug use have been shown to reduce the pathological behavior associated with substance abusers (Bickel et al., 1998).

Using a difference-in-differences specification, I find that states that legalize recreational marijuana experience significant declines in opioid abuse and loan defaults relative to other states. Legal marijuana appears to crowd out illicit opioid use and its negative effects on household finance. To alleviate the concern that another regulatory change, occurring simultaneously with the implementation of marijuana legalization, is driving the relation of opioid abuse and loan defaults, I further examine the relation of taxable marijuana sales and loan defaults (via decreases in opioid abuse).[6] I find that a 10% increase in taxable marijuana sales results in a 2.6% decline in loan defaults.

Using two different settings, occurring at different times, with a different set of assumptions, I find the same result: increases in opioid abuse lead to increases in loan defaults. The differences between the two natural experiments are noteworthy. In states with legal recreational marijuana, the marginal abuser of prescription opioids can shift to a less costly analgesic that is widely available and of predictable quality. In contrast, when the 2011 Florida laws reduced the prescription supply, the marginal abuser was forced to choose between scarcer, more expensive prescription opioids and cheaper heroin, a drug of inconsistent quality and erratic supply. The results suggest that the

prescription opioids.

[6] Alternative hypotheses, such as the substitution of marijuana for alcohol, are discussed in Section IV.

substitution of heroin leads to an *increase* in opioid abuse (as measured by the ratio of opioid overdose deaths to opioid prescription rates), while the switch to marijuana leads to a *decrease* in opioid abuse. Each experiment reveals a substitution for prescription opioids, but only when marijuana is substituted do we observe positive spillover effects on credit markets. These behaviors are consistent with borrowers spending considerable time and money obtaining these drugs.[7]

To better understand the relation between opioid abuse and loan defaults, I investigate the reliability of lenders credit models in assessing the riskiness of auto loans during the opioid epidemic. Lenders use a broad range of information to determine the likelihood that a prospective borrower will default. But if the lenders cannot distinguish two otherwise similar borrowers that are differentially shocked by an unobserved risk factor (i.e., the opioid epidemic), then they may ration credit or increase the cost of credit for both borrowers. I investigate the predictive power of traditional factors—namely, the borrower's FICO score, income, and other observable credit attributes—on realized default rates. I find that opioid abuse data significantly improves a lender's ability to predict out-of-sample loan defaults.

While loan default rates are an important predictor of loan profitability, subprime lenders are principally concerned with actual repayments (as loans in default can be profitable for subprime lenders). In further tests, I examine loan repayments and find that they vary with local opioid abuse during the height of the opioid epidemic. I find that the out-of-sample performance of the lenders' traditional credit model declines by over 30% in areas with high levels of opioid abuse, while areas less affected by the opioid epidemic do not see such declines. Adding opioid abuse data to the model increases the adjusted R^2 of the (out of sample) payment prediction model by 19% in the highest tercile of opioid abuse. In contrast, the opioid data provides no significant improvements in areas less affected by opioid abuse.

I further examine the impact of opioid abuse on loan performance by examining loan collection efforts in another difference-in-differences setting. State regulatory restrictions on wage garnishment limit a lender's ability to collect on a loan deficiency after default (Brown and Jansen, 2018). I

[7]The medical literature provides some guidance on the mechanism that links opioid abuse and loan defaults. Bickel and Marsch (2001) describe a "reinforcement pathology" among drug abusers that is characterized by delay discounting, impulsivity, and loss of control.

investigate the impact of opioid abuse on collections using the differential ability of lenders to collect, based on whether their states allow wage garnishment. Predictably, early in the epidemic, opioid abuse had no significant impact on collections in either regulatory regime. In contrast, during the opioid epidemic, a one-standard deviation increase in opioid abuse leads to a 3.1% reduction in collections in states that permit wage garnishment. In states where laws against wage garnishment limit collections efforts, the epidemic has no impact on collections.

In the final tests, I compare the total loan costs from the period of peak opioid abuse with those of the early sample period (pre-financial crisis), when the opioid epidemic had not yet taken hold. The total loan costs reflect not only the increase in contracted payments but also the added costs of financial penalties attributable to delinquency and default. Over the life of an average subprime auto loan, borrowers in counties at the 75^{th} percentile of opioid abuse pay \$1,118 more than borrowers in counties at the 25^{th} percentile. This represents a 4.4% increase over the total average loan cost for the average subprime borrower, *ceteris paribus*. The higher overall default rate, combined with a poor out-of-sample predictive performance of traditional borrower credit attributes (e.g., FICO score), may explain an important negative externality for households: borrowers in opioid-afflicted areas pay so much more for subprime auto loans.

This paper makes two contributions. First, by connecting the opioid epidemic with financial markets, it adds to the literature on the economic spillovers from substance abuse.[8] Using new data on individual auto loan outcomes and origination terms, I find that opioid abuse leads to higher loan default rates. The economic implications of this finding are significant. If the relation I identify persists in subprime markets, then the opioid epidemic may be responsible for an additional 80,000 auto loan defaults per year, representing \$1.2 billion of outstanding debt.[9] Second, this paper describes how lenders' inability to identify addicted borrowers adversely affects consumer credit markets. By showing show that borrowers in opioid-afflicted areas pay significantly more for access to consumer credit, I lend support to the theoretical literature's predictions on how supply-side

[8]Other papers in this literature include Levitt and Porter (2001), which finds that alcohol abuse imposes externalities in the form of increased auto fatalities. More recently, Ouimet et al. (2019) show that opioid prescriptions have detrimental effects on firm growth and value.

[9]40% of the approximately 12 million auto loans per year are non-prime or lower credit. The average opening balance of these loans is approximately \$16,000 (Jefferies, 2018).

responses to asymmetric information affect credit availability (Akerlof, 1978; Stiglitz and Weiss, 1981).

2. Data

To assess the impact of opioid abuse on consumer finance, I start with two variables: drug poisoning rates (i.e., deaths) per 100,000 persons, for all races, both sexes, and ages 20 to 79, from 1999 to 2016, based on data from the National Center for Injury Prevention and Control (NCIPC) of the Centers for Disease Control (CDC); and opioid prescribing rates per 100 persons, based on CDC data. The CDC's prescribing data originates in the IQVIA Transactional Data Warehouse (TDW) and 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. Figure 1 shows the variation in opioid prescription rates per U.S. county in 2006. Prescribing rates are calculated from the number of opioid prescriptions dispensed in a given year and county divided by the annual resident population as reported by the U.S. Census Bureau.[10] Health issues that cause pain vary little geographically and cannot explain the geographic disparities in opioid prescribing rates. Instead, Schnell and Currie (2017) find that prescribing rates vary significantly with doctors' educational experience.

To assess the impact of opioid abuse on loan performance, I calculate the ratio of the opioid death rate at the time of loan termination and the opioid prescription rate at the time of loan origination.[11] Figure 2 shows a binned scatter plot of the opioid abuse rate for the sample period from 2006 to 2016.[12] Although a mechanical relation exists between opioid deaths and loan defaults,

[10]Two shortcomings in the data may weaken the results. First, some of the pharmacies that sold disproportionately large numbers of opioids are not represented in the IQVIA 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. Those pharmacies do not appear in the data, presumably because they did not report their opioid sales. Second, the deaths attributable to opioids may be undercounted due to coroners commonly misidentifying the cause of death or simply choosing "other external cause of mortality."

[11]Frieden and Houry (2016) find that 1.82 of every 1,000 patients who receive prescription opioids die from an opioid-related cause. The median death occurs within 2.6 years of the initial prescription, which matches the average time-to-default (2.6 years) of the loan sample. Also, Powell et al. (2015) provide causal estimates of the relation between the medical opioid supply and drug overdoses using differential shocks to medical reimbursement. The authors find that a 10% increase in opioid supply leads to a 7.4% increase in opioid-related deaths.

[12]Emergency room (ER) visits related to opioid overdoses (which are correlated with opioid deaths) would also

the the impact of this relation on the number of loan defaults is small.[13]

I match the county-level CDC data on opioid use with new data on the origination terms and outcomes of subprime automotive loans. The database of automotive loans comes from a lender that acquires loans in 44 U.S. states. The data spans 23 years ending in 2017 and includes over 259,467 loans, which were originated at 3,926 dealerships in 1,903 U.S. zip codes. I only include loans that terminate before the end of the sample period; this reduces the sample to 149,300 loans. Appendix A provides definitions for the variables used in the study.

Table I shows the summary statistics for the loans used in the paper. The average borrower in the sample has a FICO score of 534 and monthly gross income of $3,278. Borrowers finance an average of $16,424 on a 66-month (average) term. The contracted loan term is much longer than the actual loan duration—the average default or loan payoff occurs after 31 months. The average default rate is 27.5%.[14] The summary statistics of the loans in my sample are similar to those reported by Jefferies (2018) and Zabritski (2018) for the total U.S. subprime auto loan market.

Table I also summarizes the CDC data for the opioid death rate by county and the opioid prescription rate by county. The average opioid death rate (per 100,000) for the sample is 16.85, with a standard deviation of 7.22. The highest observed death rate in the CDC data is 139.44.[15] The average opioid prescription rate for my sample is 72.83 per 100 persons (i.e., 0.72 prescriptions per year for each U.S. person), with a standard deviation of 21.96.[16] The highest observed prescription rate in my data is 321 per 100 persons, which is much lower than the highest rate in the CDC data (583 per 100 persons, for a county in which my sample includes no loan observations).

make a good proxy, but data on these visits is only available for a small sample of states and for a limited number of years.

[13]At the end of 2016, approximately 3.2 million subprime auto loans were 90 days delinquent (Brown and Colton, 2018). In that year, 63,632 drug overdose deaths occurred (Centers for Disease Control and Prevention and others, 2018).

[14]The default rate is shown as a percentage to simplify the interpretation of the coefficients in the tables.

[15]These summary statistics are similar to the full sample of opioid death rates for the United States (e.g., $mean =$ 17.76 and $sd = 11.63$), suggesting that my sample of loans may be representative of the exposure of subprime loans to the opioid epidemic 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 conservative.

[16]For the United States, the average (non-population weighted) prescription rate was 72.52 during the sample period. However, the standard deviation is substantially larger (49.76), since my sample does not include loans from the highest opioid exposed counties in the U.S.

3. The opioid epidemic and loan performance

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.

3.1. Opioid abuse and loan defaults

I estimate multivariate ordinary least squares 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 the dependent variable of interest—an indicator of loan default for borrower i; j is the county where the loan originated; and τ is the year the loan originated. The variable $Z_{j,\tau}$ represents the opioid abuse rate for county j and the year τ. The equation includes controls $X_{i,j,\tau}$ for individual borrower and local labor market characteristics. The specification also includes county(λ_j) and year (λ_τ) fixed effects. In all analyses, I present heteroscedasticity-robust standard errors clustered at the county level to account for intertemporal correlation across loans originated in the same county.

The use of county-level data has three advantages. First, a county-level analysis permits the use of per capita opioid prescription data, which allows for a relatively straightforward analysis of loan performance. This is important because a substantial share of prescribed opioids is diverted locally 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. Second, county fixed effects allow me to absorb persistent factors, like the different mitigation strategies for opioid abuse employed by cities and counties. And third, a county-level analysis allows me to assess local time-varying economic conditions, such as the unemployment rate. The year fixed effects should absorb time-varying macroeconomic shocks. A remaining omitted variable is thus of a local time-varying nature which is more difficult to measure empirically.

Table II presents OLS regression results on the percentage of loans terminated due to default.

The regressor is the county opioid abuse as described in section 2. Controls are included for the riskiness of the individual borrower (e.g., credit score, income, and prior bankruptcy). To control for the local economic environment, the specification includes county and year fixed effects. In addition, columns 3 and 4 include time-varying controls—the labor participation rate, the unemployment rate, and the alcohol abuse rate. In columns 2 and 4, I substitute dealership fixed effects for county fixed effects. This lets me account for soft information that dealers but not lenders observe about the borrower—for example, information that could hint at the borrowers' likelihood to abuse drugs. This measure also captures any differences in the behavior of dealership personnel that affect loan outcomes (e.g., dealership sales incentives that disregard the likelihood of loan default). In all specifications, I find that borrowers who reside in counties with higher opioid abuse rates are more likely to default on their auto loans. When including the full set of controls in the specification (column 3), a one-standard deviation increase in the opioid abuse rate is associated with a 0.9% increase in the likelihood of default.[17] For the average borrower in the loan sample, this represents an increase in the likelihood of default from 27.5% to 28.4%. This *increase* in default rates is comparable to a $464 *decrease* in a borrower's monthly income.[18] The stability of the *positive* association between opioid abuse and loan defaults suggests that time-varying factors do not explain this relation

The medical literature provides some guidance on the mechanism that links opioid abuse and loan defaults. Bickel and Marsch (2001) describe a "reinforcement pathology" among drug abusers that is characterized by delay discounting, impulsivity, and loss of control.[19] Studies on the intertemporal choices of opioid-dependent patients (Madden et al., 1997; Kirby et al., 1999) show that these individuals are likely to choose more immediate rewards even if they are smaller. The illicit acquisition of a consistent supply of high quality opioids is both costly and time-consuming.

[17]In untabulated results, I find that using the opioid death rate as a proxy produces a stronger economic relation between opioid abuse and loan defaults. The smaller magnitude of the coefficient on the opioid abuse rate (when compared to the opioid death rate as a proxy for opioid abuse) may be attributable to the smaller standard deviation of the opioid prescription rate of the loan sample, relative to the *higher* actual loan standard deviation of the opioid prescription rate across the U.S.

[18]Untabulated results suggest that including lags for prior year opioid deaths in the specification does not significantly change the results. 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.

[19]Bickel et al. (2014) provide a review of the medical literature related to reinforcement pathology.

Addicted users who are in withdrawal are likely to be more interested in satisfying their craving than in maintaining consistent employment or making their monthly car payments.

Another explanation for the relation between opioid abuse and loan defaults centers on labor force participation. Krueger (2017) finds that almost half of working-age men who are not participating in the labor force are taking pain medication daily. The causality of the relation between labor force participation and opioid use is unclear. In theory, workplace injuries could be pushing workers out of the workforce and into opioid dependence, but there is no medical evidence of higher rates of workplace injuries in recent years. It seems far more likely that the pharmacological properties of opioids are negatively impacting labor force participation.[20] The lack of labor market participation combined with the significant acquisition costs associated with satisfying an opioid addiction would also help explain the changes in loan performance that I attribute to the opioid epidemic.

3.2. Prescription opioid supply shock and loan defaults

The ideal experiment would allow one to determine what the default rate would have been if borrowers in the opioid-afflicted area had not been exposed to the opioid epidemic. Thus, an empirical challenge is to distinguish loans that are affected by the opioid epidemic from loans that are not. Using opioid abuse as a regressor in an OLS specification is insufficient, as an unobserved variable may be correlated with both the opioid abuse and the default rate. For example, a plant closing could simultaneously affect employment (and thus loan defaults) and opioid abuse (The unclear causality between labor market participation and opioid abuse does not preclude this possibility (Krueger, 2017)). To help address this identification challenge, I explore the impact of a regulatory change that had major ramifications for the supply of prescription opioids.

In the two years before 2012, Florida accounted for over 43% of prescription opioids in the United States, despite having only 6% of the population (Office of National Drug Control Policy, 2011). In 2010, 90 of the top 100 oxycodone-prescribing U.S. physicians were in Florida. The Drug Enforcement Administration (DEA) noted that numerous states were being supplied with opioids

[20] Harris et al. (2017) find that the adverse effects of prescription opioid abuse on the labor market outweigh any positive effects from the drugs' therapeutic use.

via the "Florida oxy express" (Alvarez, 2011). Patients came from around the country to procure opioids, sometimes obtaining prescriptions from several physicians in a day. In October 2011, the state of Florida took action, implementing a prescription drug monitoring program and new rules targeting the high-prescribing pain clinics known as "pill mills." The PDMP law mandated that physicians ascertain whether patients had recently received an opioid prescription from another physician. These regulations severely reduced not only the state's, but the nation's, prescription opioid supply (Rutkow et al., 2015).

The regulations had a number of effects. Following the legislative change, the street price of prescription opioids across the U.S. soared, leading to an increase in the use of less costly and more dangerous opioid substitutes such as heroin and illicit fentanyl (Mars et al., 2014; Cicero et al., 2014; Rutkow et al., 2015; Zhou et al., 2016).[21] The DEA reports that, after Florida's regulatory change, the street price for an 80-mg OxyContin pill increased from $20 to over $80 in many parts of the United States. In contrast, black-tar heroin could be purchased for about $10 per dose.[22] The resultant substitution to heroin and illicit fentanyl resulted in significant increases in the rates of opioid abuse and opioid-related deaths (Cicero et al., 2014; Compton et al., 2016; Beall, 2018).

I use the exogenous change in Florida's regulation to identify the causal impact of opioid abuse on loan defaults using a difference-in-differences (DiD) framework. I estimate the impact of the regulation change by exploiting the variation in counties' exposure to the regulatory change due to differences in their prior opioid abuse rates. Before the 2011 regulation change, counties with higher opioid abuse rates typically had opioid consumption rates that exceeded what was supplied through their local pharmaceutical distribution; much of the excess supply emanated from Florida (Beall, 2018). Consequently, the regulatory shock should have a greater effect on those areas. I examine whether counties with these higher rates of opioid abuse experienced larger changes in loan defaults by estimating the event-study specification:

[21] Prescription opioids and heroin are pharmacologically similar, but heroin's significantly lower street cost suggests that consumers have concerns about other unobserved costs (e.g., overdose-related morbidity). The unreliable purity of street heroin (estimated to range from 11% to 72%) leads to significant increases in accidental overdoses, relative to illicitly used prescription opioids (Darke and Zador, 1996).

[22] Withdrawal symptoms for heroin begin 6 to 12 hours after the last dose, so heroin users may take multiple doses to substitute for each 80-mg pill of OxyContin, which provides a 12-hour high. Thus, street prices for a week's supply of heroin ($280) were similar to those for Oxycontin, prior to the Florida regulatory change.

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

where $Y_{i,\tau}$ is the dependent variable of interest—an indicator of loan default for borrower i; j is the county in which the loan originated; and τ is the year the loan originated. The variable $D_{i,\tau}$ represents an indicator variable that is equal to 1 if the borrower is treated and the loan termination comes after the Florida legislative change (τ => 2012), and zero otherwise. In one set of tests, a borrower is treated if the loan originated in a county with an above-median rate of opioid abuse prior to 2008, and control otherwise. The analysis specifically *excludes* Florida, to determine the extent to which Florida's legislative change affected other states. In a second set of tests, a borrower is treated if the loan originated in a county that had above-median opioid abuse *and* the county j was within a day's drive (i.e., 600 miles) of a Florida pharmacy. A stronger economic relation between the opioid abuse and loan defaults would be expected for counties closer to Florida if driving distance imposes costs on abusers of prescription opioids. This range encompasses counties in Georgia, Alabama, Mississippi, Louisiana, Tennessee, South Carolina, North Carolina, Kentucky, and Virginia.[23]

I include county fixed effects to account for fixed cross-sectional differences across counties, and year fixed effects to account for national shocks and trends in heroin availability, enforcement, prices, and other factors common across counties. The equation also includes controls $X_{i,j,\tau}$ for borrower and local characteristics. In all analyses I present robust standard errors clustered at the county level to account for serial correlation. To identify the change in the regulation, the analysis is limited to loans that were terminated between 2009 and 2014—the years around the implementation of Florida's law.

The specification above identifies the differences in opioid abuse across counties with higher (and lower) initial rates of opioid abuse, and tests for a trend break after Florida's regulatory change. The dependent variable in columns 1 and 2 of Table III is the opioid abuse rate in county j and year τ. The regressor is the opioid abuse rate in county j *prior* to 2008 interacted with the Florida

[23]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.

legislative change timing. In column 1, regressors include an indicator variable that is equal to 1 if the borrower is treated and the loan termination came after the Florida legislative change. The inclusion of county fixed effects and year fixed effects ensures that the treatment effect is estimated only within county variation. I find that treated counties experience a 3.3% increase ($p < 0.01$) in opioid abuse. In column 2, I show the coefficient on a triple differences term that includes an indicator if the county is within 600 miles of the Florida state line. I find that opioid-afflicted counties that are proximate to Florida experience a larger (4.1%) increase in opioid abuse after Florida's legislative change.

In the next set of tests I examine the impact of the legislative change on loan default rates, Table III, columns 3 and 4 report the coefficient estimates. The results show that, following the legislative change, the loan default rate increased by 2.0% in the most opioid-afflicted areas. Column 4 shows that this effect is more pronounced in counties closer to Florida: counties with historically high opioid abuse rates that are proximate to Florida experience a 4.1% increase in loan defaults relative to the control group. This result, when combined with the results from column 2, suggests that the local average treatment effect of opioid abuse on loan outcomes arises from the people most likely to be abusers

3.3. Marijuana legalization and loan defaults

The Florida experiment partially addresses the identification challenge but falls short of the ideal experiment, which would determine what the default rate would have been if borrowers in the opioid-afflicted area had not been exposed to the opioid epidemic. If the treatment and control group experience differential shocks that simultaneously affect loan performance, then the change in the control group would not serve as a valid counterfactual for the change in the treatment group (e.g., if a region has bad economic shocks, the abuse rate will be higher.) In the next setting, I address this shortcoming by examining the impact of a different regulatory change, one that affected the supply of a non-opioid analgesic (and potential opioid substitute) but not the supply (or prices) of prescription opioids. Specifically, I assess the impact of laws legalizing the recreational use of marijuana on opioid abuse and loan defaults. Using a difference-in-differences approach, I assess the differential impact of the exogenous change in opioid abuse on areas that were differentially affected by changes in access to marijuana.

Recent studies in the medical literature find that laws allowing legal access to marijuana reduce the use of opioid analgesics and deaths from opioid overdose (Bachhuber et al., 2014; Bradford and Bradford, 2016; Powell et al., 2018). In a recent survey, 97% of medical marijuana patients report decreasing their opioid consumption when they use marijuana (Reiman et al., 2017).

As of January 2018, three U.S. states implemented laws permitting the legalized the sale of marijuana for nonmedical reasons: Colorado in 2014, Washington in 2014, and Oregon in 2015.[24] While there are multiple ways to access marijuana, laws that permit recreational sales and use should provide wider and less costly access to the drug. This, in turn, may facilitate the substitution of marijuana for opioids. I therefore use the implementation of these laws to identify the impact of opioid abuse on loan performance.

The empirical strategy compares changes in opioid abuse in states that do and states that do not implement laws permitting the sale of recreational marijuana. Using a difference-in-differences strategy, I use the non-adopting states as controls and the differential timing of marijuana legalization as the treatment, then compare the changes in outcomes between states. To mitigate concerns about a differential effect in the states that legalized marijuana, I include state fixed effects and year fixed effects in the specification.

Identification in the difference-in-differences model requires that changes in the control group serve as an appropriate counterfactual for the treatment group absent the policy change. This is commonly referred to as a "parallel trends" assumption. To investigate this potential threat to identification, I compare differences in outcome pretends in Figure 3. The figure plots a regression discontinuity around the time recreational marijuana was legalized. Figure 3b shows the abnormal default rates in the pre- and post-adoption periods. The trend lines show a decline in the default rate, matching the decline in the opioid abuse rate observed in Figure 3a. After legalization, opioid abuse and defaults continue to decline consistent with the expansion of dispensaries in those states.

[24] I focus on recreational use because laws related to medical marijuana pose an empirical challenge: since 2010, states with medical marijuana dispensaries have regularly changed their regulations on dispensaries in response to federal marijuana policy (Pacula and Smart, 2017; Powell et al., 2018).

To formally identify the impact of opioid abuse, I use the legalization of marijuana as a source of exogenous variation in opioid abuse 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). 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 abuse rate in state j in year τ); and $D_{i,\tau}$ represents the indicator for states that (1) legalized recreational marijuana usage and (2) have implemented operational and legally protected dispensaries. A borrower is treated if the loan is terminated in a state where those two conditions are met. Regressions include controls for the riskiness of the borrower (e.g., credit score, income, and prior bankruptcy). In addition, I control for the unemployment rate, which might influence access to insurance or the ability to pay for prescription drugs. The specification includes state fixed effects, to account for fixed cross-sectional differences across states, as well as year fixed effects, to account for national shocks and trends in heroin availability, enforcement, prices, and other factors common across states. In all analyses I present robust standard errors clustered at the state level.

Table IV, Column 1 reports results from OLS regressions on the opioid abuse rate for indicator variables (post-legalization) for loans that were terminated in states that had legalized marijuana sales at the time of termination. The coefficient on the post-legalization indicator variables suggests that the states that legalized marijuana experience a significant 1.3% decline ($p < 0.01$) in opioid abuse after legalization, relative to other states.[25] Column 2 reports the results from a reduced form regression on the likelihood of a loan default for an indicator variable (post-legalization) for loans that were terminated in states that had legalized marijuana sales at the time of termination. The

[25] Studies of the impact of medical marijuana laws on opiate addiction (Pacula et al., 2010, 2015; Klieger et al., 2017; Powell et al., 2018) are consistent with the finding of the relation between opioid abuse and marijuana legalization, and with the hypothesis that improved access to marijuana dispensaries is associated with a lower incidence of opioid abuse and death.

results indicate that marijuana legalization results in a 4.7% *decrease* ($p < 0.01$) in loan defaults relative to other states.

To alleviate the concern that another regulatory change, occurring simultaneously with the implementation of marijuana legalization, is driving the relation of opioid abuse and loan defaults, I further examine the relation of taxable marijuana sales and loan defaults. The quarterly taxable marijuana sales in each state (or 0 otherwise), allows me to formally examine the relation of marijuana usage and opioid abuse. Table IV, column 3 reports that a 10% increase in taxable marijuana sales leads to a 0.7% decline in opioid abuse. Column 4 shows that increases in marijuana sales also reduce the loan default rate.

The differential impact associated with the switch from prescription opioids to marijuana (as opposed to heroin or fentanyl) could be explained by marijuana's less addictive nature and lower acquisition costs. In the case of an opioid prescription supply reduction, prescription opioid abusers who want to avoid withdrawal must choose between costly prescription opioids and heroin (or fentanyl) of varying degrees of purity. In contrast, marijuana offers legality, consistency in supply, and predictable quality. The results show that the substitution of heroin leads to an *increase* in opioid abuse, while the switch to marijuana leads to a *decrease* in opioid abuse. While each of these experiments assesses a substitution away from prescription opioids, only in the case of marijuana do we observe positive spillover effects on credit markets.

3.4. Robustness

There are a number of alternative explanations for the relation between marijuana legalization and lower loan default rates. One plausible explanation is that an additional separate change in regulations concurrently affected opioid abuse and loan defaults. If this happened, then the mechanism attributed to marijuana is incorrectly identified, but the relation between opioid abuse and loan defaults remains valid.[26]

Another possible explanation is that alcohol and marijuana are substitutes; substituting marijuana for alcohol could reduce alcohol-related car accidents, which in turn could lead to lower

[26] This is further discussed in Appendix C.

default rates. However, there is ongoing debate in the medical literature over whether marijuana substitutes for—or merely complements—alcohol.[27] To help address this concern, I examine the impact of alcohol abuse on loan performance. In untabulated results, I include the alcohol death rate as a proxy to control for alcohol abuse, and find that the relation of opioid abuse and marijuana legalization is generally unchanged. I further explore the the hypothesis that alcohol abuse is driving the results in results later in this section.

To help allay concerns that the choice of states is coincidental, I repeat the test shown in Table IV, column 2 with a placebo constructed from a random sample of states.[28] If the results can be replicated with this random sample, then the effect that I attribute to opioid abuse may be spurious. If the results are different from the results of the original test, this gives credence to the hypothesis that opioid abuse leads to loan defaults. Figure B.1 shows that when a random sample of U.S. states is substituted for the three states that legalized recreational marijuana, the outcome in Table IV, column 2 is not consistently reproduced. The histogram shows a sample of 10,000 regressions that are centered around a mean of 0. This iterative procedure provides a distribution for the states where no marijuana legalization took place. The incidence of Colorado, Washington, and Oregon adopting the laws is at minimum an outlier in the data.[29]

I next consider the time series changes related to the opening of recreational marijuana dispensaries in states that legalized them. Table B.I examines the impact of marijuana legalization in the years immediately before and after its implementation. The table reports coefficients for four years surrounding legalization. The regression informs us that opioid abuse (column 1) decreases significantly in the two years after marijuana legalization. Notably, loan defaults are significantly lower, as shown in column 2. The larger declines in both opioid abuse and loan defaults in the second post-implementation year are unsurprising, since access to marijuana increases as more dispensaries open. In contrast, a placebo test in column 3 shows that the alcohol death rate does

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

[28] In this analysis, I use the full sample of U.S. states rather than a selected sample of states near Washington, Oregon, and Colorado. Using bordering states as the counterfactual would confound the analysis since the residents of those states have easy access to marijuana dispensaries right across the state line.

[29] In Table B.II, I replicate the results of Table IV for each individual state. I find that the relations described earlier generally hold in the three states individually.

not decrease with the implementation of marijuana legalization. This finding helps to alleviate concerns that a decline in alcohol abuse is causing the decrease in loan defaults that I attribute to a decline in opioid abuse.[30]

One concern about the earlier tests is the differences in characteristics between the treatment and control groups. To help address this, I follow Abadie et al. (2010), and construct a synthetic control as a weighted average of states, such that the chosen weights reproduce the first and second moment of the values associated with the treatment group. Whereas OLS assigns all states in the control group the same weight in the estimation, this procedure allows a comparison between states by assigning different weights to each state. I construct a set of states that have similar observable demographics and borrower characteristics prior to the passage of marijuana legalization. Table B.V, Panel A compares the pre-treatment borrower characteristics and demographics (e.g., unemployment, income inequality, poverty, and veteran and disability status) in states that legalized marijuana and states that did not. The table shows that the borrower characteristics and demographics are significantly different in some dimensions. However, we can see in Table B.V, Panel B that the synthetic counterfactual has similar borrower characteristics and demographics to the marijuana legalization states.

I then estimate the effect of marijuana legalization on opioid abuse and loan defaults as the difference between states that legalized marijuana and a synthetic version of those same states; the synthetic version allows me to estimate a counterfactual of how those states would have evolved if they had not legalized marijuana.

Using the difference-in-differences (DiD) framework described earlier, I identify the impact of opioid abuse on loan performance. Table B.III replicates the results of Table IV and Table B.I using synthetic controls. I find that the results are stronger in this set of specifications.

In the final robustness test, I instrument the opioid abuse rate with an indicator for marijuana legalization, using the synthetic control weightings described above. Table B.IV reports these results. I find that a 10% increase in opioid abuse results in a 3.4% increase in loan defaults. Column 1 uses marijuana legalization as an instrument, while column 2 uses taxable marijuana

[30] Column 4 shows that using the opioid death rate as a measure provides similar results as using the opioid abuse rate.

sales. Two concerns must be addressed in the use of the instrument. First, the instrument should be sufficiently correlated with the endogenous variable. The relevance condition is easily verifiable. Table B.III, column 1 reports the first stage of the 2SLS regression. A statistically significant variable drives the results in the first stage. I reject the hypothesis that the instrument is weak using the Kleibergen-Paap F-statistic. 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). In addition, the medical literature provides strong evidence of the first stage relation.[31]

The second concern about the instrument is reliance on exogenous variation that does not violate the exclusion restriction. If, for example, people become more responsible and better at bill paying when using marijuana, then marijuana alone could account for the changes I observe in default rates, and the exclusion restriction in this specification would be violated. The earlier discussion on alternative explanations (e.g., alcohol abuse) is intended to partially address these concerns.

4. The opioid epidemic and loan origination

Lenders typically use observable information to determine the expected default rate of a prospective borrower. If lenders cannot distinguish otherwise similar borrowers, differentially shocked by an unobserved risk factor (i.e., the opioid epidemic), then they may ration credit or increase the cost of credit for all borrowers. In this section, I explore the predictive power of traditional credit information in assessing the riskiness of loans during the opioid epidemic. Specifically, I use a difference-in-differences (DiD) framework to investigate how traditional credit attributes, such as the borrower's FICO score, predict default rates in areas exposed to the the opioid epidemic.

First, I estimate the likelihood that a buyer would default, given the full set of 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 terminated prior to the height of the opioid epidemic (i.e., pre-2012) against the borrower- and loan-specific characteristics, then use the coefficient estimates to predict a default rate for all

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

loans in the data. I interpret the predicted default rate as a composite measure of the riskiness of each 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 over time. I thus assume that the coefficient estimates are valid out-of-sample weights.

Table V reports the coefficient estimates from ordinary least squares regressions on the loan default rates for loans originated after 2011 on the opioid abuse rate and the predicted default rate, which represents a composite measure of the borrower riskiness. Column 1 shows that the predicted default rate, county and year fixed effects, and in-sample contemporaneous unemployment rate and labor force participation account for 13.3% of the variation in default rates. Notably, when I add the opioid abuse rate, the lender is able to predict 14.7% of defaults, a 10.5% improvement in the out-of-sample performance of the credit model.[32] To further explore whether this change is attributable to opioid abuse, I divide the sample into states where marijuana was legalized (column 3) and states where marijuana was not legalized (column 4). I find that the opioid abuse rate improves the credit model in states that do not legalize marijuana. This finding is consistent with the hypothesis that traditional borrower credit attributes omit an important default risk factor: opioid abuse.

While loan default rates are an important predictor of loan profitability, subprime lenders are principally concerned with the actual customer payments. Subprime lenders can still profit from auto loans that default because these loans (1) have high rates of interest and (2) allow lenders to easily repossess collateral and sue borrowers for deficiencies. In the next set of tests, I examine the lenders' ability to predict loan repayment. Using the methodology described for the tests in Table V, I construct a counterfactual payment rate from loans terminating prior to 2011. I assess the strength of this composite risk measure out-of-sample by comparing the predicted (ex-ante) default rates to the realized (ex-post) default rates for the sample of loans.

Table VI reports the coefficient from an OLS regression of the *predicted* payments on the *realized* payments. Columns 1, 2, and 3 report the coefficients for the *in-sample* period (i.e., loans

[32]In untabulated results, I find that out-of-sample predictions of loan defaults are comparable when using lagged opioid abuse data rather than contemporaneous data.

terminating before 2012) for counties sorted by opioid tercile. Based on observable characteristics, the lender is able to predict 22.9% (column 1) of the variation in payment for low opioid exposure areas and 24.2% (column 3) of the variation in payment for high opioid exposure areas. The mechanical relation of the in-sample predicted payment rate and the realized payment rate is close to 1.0.

Table VI, columns 4 to 9 report the out-of-sample performance of the predicted payment rate. After 2011, the power of the traditional loan characteristics in predicting payment rates declines precipitously in counties in the highest tercile of opioid abuse: the out-of-sample performance of the credit model declines by over 30% in these areas, while areas less affected by the opioid epidemic see no declines.[33]

In the next set of tests, I investigate how the addition of opioid abuse data improves lenders' ability to predict loan repayment. Table VI, columns 7, 8, and 9 report these results. Adding opioid abuse data to the payment model increases the R^2 of the out-of-sample payment prediction model by 19% in the highest tercile of opioid abuse (column 9). In contrast, the addition of opioid-abuse data provides no significant improvements in areas less affected by opioid abuse (columns 7 and 8). These findings suggest that traditional credit models still work well in areas that are unaffected or lightly affected by the opioid epidemic, but new models that capture the "opioid risk factor" are needed in opioid-afflicted areas.

I further investigate the impact of opioid abuse on subprime loan performance by examining loan collections efforts. Specifically, I examine the differential impact of opioid abuse and loan collections efforts across states that do and states that do not prohibit wage garnishment—the process by which creditors recover outstanding debt directly from borrowers' income. Eight U.S. states explicitly prohibit or severely restrict the use of post-judgment wage garnishment. In doing so, they effectively eliminate post-judgment collections efforts against borrowers that defaulted on their loans (Brown and Jansen, 2018).

Table VII describes results of a difference-in-differences setting. Prior to the opioid epidemic one would not expect opioid abuse rates to have an impact on collections in either of the two regulatory

[33] The out-of-sample period is *after* the financial crisis, a time when traditional credit measures perform more reliably.

regimes. During the opioid epidemic, collections efforts should be impaired in states that permit wage garnishment but unimpaired in other states. Table VII examines this hypothesis. Columns 1 and 2 confirm that loans terminating before 2011 were not significantly affected by opioid abuse. Post 2011, however, a one standard deviation increase in opioid abuse leads to a 3.2% reduction in collections in states that permit wage garnishment, but has no impact in other states. In contrast, in states that restrict collections efforts, the opioid epidemic had no impact. This difference-in-differences specification supports the hypothesis that collections efforts are impaired by the opioid epidemic, providing further evidence of the epidemic's financial repercussions.

5. Borrower loan cost

In the final tests, I investigate how the opioid epidemic affects the total realized cost of automotive subprime loans. The total realized cost includes (1) all payments of principal, interest, and fees made by the borrower to the lender, (2) the loss of value and costs associated with the repossession and sale of the vehicle, and (3) all payments made by the borrower arising from post-default collections efforts. The novel data set allows me to observe all transfers from the borrower to the lender. However, the buyer may also incur indirect costs that are not observable in the data. For example, consumers with a history of default or vehicle repossession may face especially high interest rates on future loans or be unable to secure credit. Borrowers without access to alternative transportation may be unable to commute to their workplace. This suggests that, in opioid-afflicted regions, the total realized cost described in this study only captures a fraction of the costs incurred by borrowers.

Figure 4a and 4b are binned scatter plots of the total loan costs versus the opioid abuse rate for the years 1999 to 2006 and 2012 to 2016, respectively. The figures show a sharp change in the effect of opioid abuse on total loan costs over time. Figure 4a shows a weak positive relation between opioid abuse and total loan costs during the early stages of the epidemic; Figure 4b shows a strong relation.

The ordinary least squares regressions in Table VIII confirm the results described in the figures. I find that the higher opioid abuse rates at the height of the epidemic are associated with increases in total realized loan costs for subprime borrowers. Between 1999 and 2006, total realized loan costs are not significantly higher in areas with higher rates of opioid abuse. Between 2012 and 2016, however, borrowers residing in counties at or above the 75$^{\text{th}}$ percentile of opioid abuse pay \$1,118 more over the life of an average subprime auto loan, compared with buyers in counties at or below the 25$^{\text{th}}$ percentile. This represents a 4.4% increase over the total average loan cost, *ceteris paribus*. The higher overall default rate, combined with a poor out-of-sample predictive performance of traditional borrower credit attributes (e.g., FICO score), may explain why borrowers in opioid-afflicted areas pay significantly more for subprime auto loans.

6. Conclusion

While several studies have assessed economic impacts of the opioid epidemic (e.g., increases in mortality and medical expenses, and labor participation and productivity), this is the first paper to examine the epidemic's effects on household finance. Specifically, I use new data to explore links between opioid abuse and loan performance and origination terms.

Using a sample of individual auto loans matched with county-level data on opioid prescriptions and deaths, I examine the relation between the opioid epidemic and auto lending. I find evidence that opioid abuse is an empirically relevant explanation for higher loan default rates. These results are identified through exogenous shocks in two experiments.

The first shock is to the supply of prescription opioids. I find that, after new regulations tighten the prescription opioid supply, counties with significant opioid problems experience even higher rates of abuse as users switch to heroin and fentanyl. This, in turn, results in increases in subprime auto loan default rates. The second shock involves the supply of an opioid substitute—recreational marijuana. I find that states that implement laws allowing dispensaries to sell marijuana for recreational use experience *declines* in both opioid abuse and loan default rates. While I do not observe the mechanism for default, studies on the intertemporal choices of opioid-dependent patients show that these individuals tend to choose more immediate rewards even if the rewards are smaller (Madden et al., 1997; Kirby et al., 1999; Bernheim and Rangel, 2004; Cutler and Glaeser, 2005; Gul and Pesendorfer, 2007). Such choices are likely to be unconducive to servicing consumer debt.

The results in this study suggest that asymmetric information in the subprime loan market leads to an overall increase in loan costs in opioid-afflicted regions. The results in Table V and Table VI suggest that lenders find it more difficult to assess the creditworthiness of borrowers in the areas most affected by the opioid crisis. If lenders cannot predict which borrowers are at risk of using opioids, the 20 million borrowers in markets with high opioid use will pay more for their loans, as shown in Table VIII. This, together with the significantly higher default rates in these areas, results in borrowers paying significantly more for access to consumer credit. This is consistent with a spillover effect on consumer finance attributable to the opioid epidemic.

If the relation I identify is representative of all subprime borrowers, then the opioid epidemic may be responsible for an additional 80,000 auto loan defaults per year, representing $1.2 billion of outstanding debt.[34] The literature on the externalities associated with deteriorating credit-market conditions (Campbell et al., 2011; Anenberg and Kung, 2014; Mian et al., 2015) suggests that the opioid epidemic's impact on local credit markets could be a factor in the economic decay that is observed in opioid-afflicted areas.

This paper presents initial evidence that the opioid epidemic is significantly affecting a financial market. Given the magnitude of this effect, more work on the opioid epidemic's market effects is warranted. Two promising avenues for future research are provided by (1) automotive loan securitization and (2) the impact of opioid epidemic on the supply of consumer finance in afflicted areas.

[34] These results may be conservative, due to the limited availability of loan data in areas that are most exposed to the opioid epidemic.

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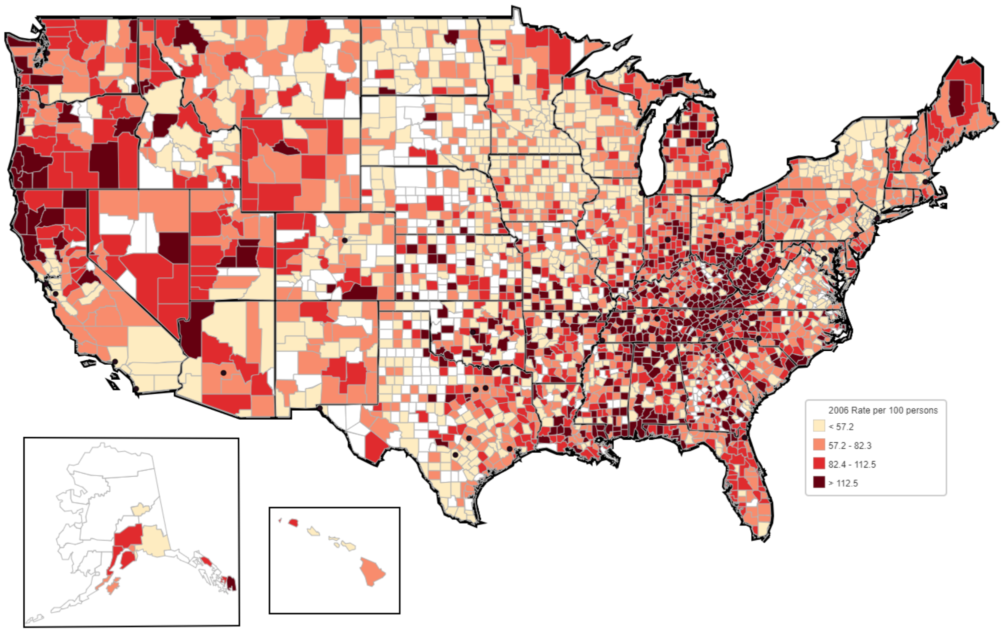


Figure 1. This figure presents a heatmap of the opioid prescription rate (per 100 persons) for each U.S. county for the year 2006. Source: CDC

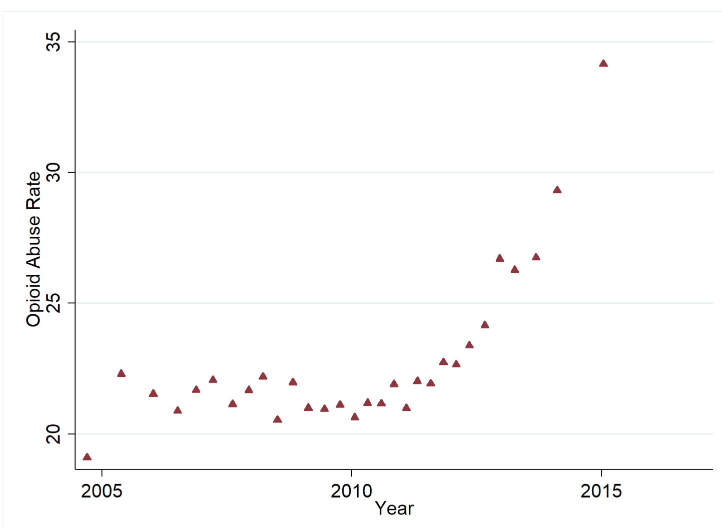
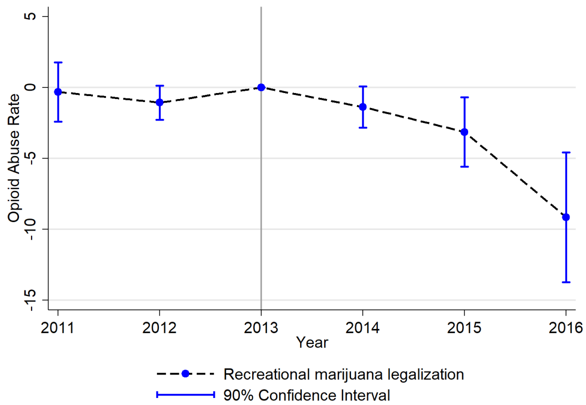
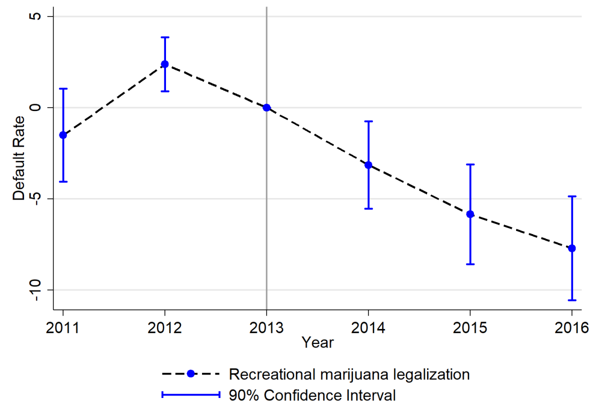


Figure 2. This figure presents a binned scatter plot of the opioid abuse rate for the years 2006-2016. The opioid abuse rate represents the ratio of opioid deaths at the time of loan termination to the lagged opioid prescription rate. When the prescription rate is high and the death rate is comparatively low, this suggests that the opioid prescriptions have high therapeutic benefits relative to their costs. In contrast, when the death rate is high and the prescription rate is low, this suggests a greater diversion of prescribed opioids for non-medical use.

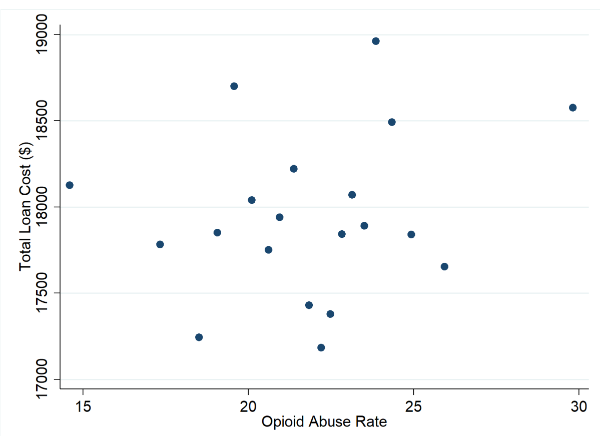


(a) Opioid Abuse Rate

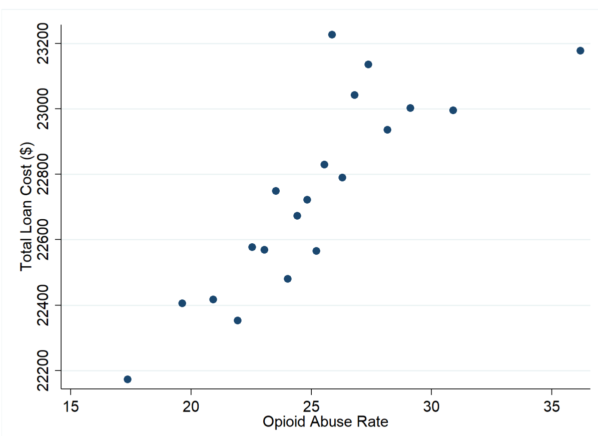


(b) Default Rate (%)

Figure 3. This figure presents trend lines of (a) the opioid abuse rate; and (b) the percent of loans that default. The figure shows a regression discontinuity for the date on which marijuana legalization was implemented and is centered on the first legalization date (2014) otherwise. Controls include prior income, credit score, prior chapter 7 bankruptcy, and unemployment rate.



(a) Opioid Abuse Rate (1999 to 2006)



(b) Opioid Abuse Rate (2012 to 2016)

Figure 4. This figure presents a binned scatter plot of the total loan costs versus the opioid abuse rate for loans originated in (a) the years 1999 to 2006 (the early stages of the opioid epidemic); and (b) the years 2012 to 2016 (when opioid abuse was widespread). The total loan costs represent (1) all payments made by the borrower, (2) all costs arising from the repossession and sale of vehicle, and (3) post-default collections efforts. Controls include borrower credit score, income, and prior chapter 7 bankruptcy; vehicle book value and loan term; county unemployment rate and labor market participation rate; and county and year fixed effects.

Table I

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

	mean	sd	p25	p50	p75	count
Borrower Characteristics:						
FICO score	534	53	498	532	568	144,091
Monthly Income ($)	3,278	2,357	2,167	3,329	4,567	149,300
Prior bankruptcy	0.26	0.44	0	0	1	149,300
Loan Origination Characteristics:						
Vehicle book value ($)	13,313	4,552	10,275	12,850	15,700	149,295
Contracted monthly payment ($)	391	111	329	393	451	149,298
Amount financed ($	16,424	5,134	13,055	16,285	19,430	149,300
Term (months)	66	8.44	60	72	72	149,300
Interest rate (%)	19.48	3.2	17.95	19.5	21	149,300
Loan-to-Value	1.26	0.2	1.14	1.25	1.38	149,295
Down payment ($)	1,235	15,019	400	1,000	1,500	149,078
Loan Performance:						
Loan duration (months)	30.73	20.9	14	26	44	141,754
Actual payments ($)	4,053	4,688	0	2,625	7,143	137,282
Default (%)	27.53	44.67	0	0	100	149,300
Collections ($)	341	1,586	0	0	0	144,333
Total loan cost ($)	21,000	8,119	15,069	19,918	25,791	132,108
Loan Environment:						
Yield spread (%)	2.04	0.94	1.32	1.93	2.44	148,566
Unemployment rate (%)	5.44	1.86	4.1	5.1	6.5	146,842
Labor force participation (%)	66.74	4.3	63.26	67.01	69.72	127,295
Opioid prescription rate (per 100)	72.83	21.96	59.3	73.8	86.8	147,618
Opioid death rate (per 100,000)	16.85	7.22	11.59	15.74	20.79	149,300
Alcohol death rate (per 100,000)	9.42	4.82	6.11	8.11	11.89	143,984
Opioid abuse rate	24.82	15.71	16.28	21.34	26.44	147,616
Taxable marijuana sales (ln)	0.68	12.15	0	0	0	149,300

Table II

Loan performance and opioid abuse. This table contains coefficient estimates from ordinary least squares regressions on an indicator for loans terminated due to default (reported as %) on the opioid abuse rate. Controls are included for the riskiness of the individual borrower, and the local environment. County, year, and dealership fixed effects are included as reported. Robust standard errors, clustered by county, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep Var:	Default (%)			
	(1)	(2)	(3)	(4)
Opioid abuse rate	0.048**	0.064***	0.060***	0.079***
	(0.019)	(0.022)	(0.022)	(0.028)
FICO score	-0.153***	-0.148***	-0.152***	-0.148***
	(0.006)	(0.006)	(0.006)	(0.006)
Income	-0.002***	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Prior bankruptcy	-12.458***	-11.838***	-12.632***	-12.044***
	(1.053)	(1.017)	(1.091)	(1.052)
Unemployment rate			1.722***	1.661***
			(0.348)	(0.373)
Labor force participation			1.776***	1.981***
			(0.411)	(0.456)
Alcohol abuse			-0.004	0.054
			(0.137)	(0.125)
County FE	Yes	No	Yes	No
Dealership FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	117,658	117,310	116,241	115,890
Adjusted R^2	0.102	0.112	0.106	0.116

Table III

Opioid abuse, loan performance and regulatory changes. This table reports the coefficient estimates from difference-in-differences regressions on (1) the opioid abuse rate (col 1 & 2); and (2) the loan default rate (reported as %) for a borrower (col 3 & 4). The regressors for high prior opioid abuse (col 1 & 3) represent an indicator variable that is equal to 1 if borrower is treated and if the loan termination occurred after the implementation of Florida regulatory changes related to prescription drug monitoring and pill mills (τ => 2012), and 0 otherwise. A borrower is considered treated if the loan was originated in a county with above the median of opioid abuse rate *prior* to 2008. The regressor in columns 2 & 4 is a triple interaction—the D-in-D described above interacted with an indicator on the county's distance from a Florida pharmacy (1 if within 600 miles, 0.5 if within 1,200 miles, 0.25 otherwise). The sample period is 2009 to 2014. Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy), and the local environment (unemployment rate & labor market participation rate). County and year fixed effects are included as reported. Robust standard errors, clustered by county, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep Var:	Opioid abuse rate		Default rate (%)	
	(1)	(2)	(3)	(4)
High prior opioid abuse x [τ => 2012]	3.331***		1.983	
	(0.648)		(1.233)	
High prior opioid abuse x [τ => 2012]		4.093***		4.126**
x Florida proximate county		(0.991)		(1.895)
FICO score	0.001*	0.001*	-0.189***	-0.189***
	(0.001)	(0.001)	(0.006)	(0.006)
Income	-0.000	-0.000	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Prior bankruptcy	0.211	0.219	-14.398***	-14.399***
	(0.253)	(0.255)	(0.526)	(0.526)
Unemployment (%)	0.036	0.021	-0.799	-0.811
	(0.412)	(0.415)	(0.494)	(0.495)
Labor force participation (%)	-0.272	-0.276	-0.886**	-0.865**
	(0.451)	(0.449)	(0.386)	(0.389)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	39,348	39,348	46,665	46,665
Adjusted R^2	0.772	0.771	0.108	0.108

Table IV

Loan performance and marijuana legalization. This table reports results from OLS regressions on the opioid abuse rate (col 1 & 4) and the loan default rate (reported as %)(columns 2, & 5). Regressors include an indicator variable (post-legalization) of loans that were terminated in a state that implemented laws allowing the recreational sale of marijuana (col 1 & 2); the quarterly taxable marijuana sales that occurred in each state or 0 otherwise (col 4 & 5). Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy) and the local environment (unemployment rate & labor market participation). The sample period is 2012 to 2016. State and year fixed effects are included as reported. Robust standard errors, clustered by state, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep Var:	Opioid abuse	Default (%)	Opioid abuse	Default (%)
	(1)	(2)	(3)	(4)
Post-legalization	-1.323***	-4.714***		
	(0.401)	(1.487)		
Legal marijuana sales (ln)			-0.074***	-0.261***
			(0.022)	(0.084)
FICO score	0.000	-0.188***	0.000	-0.188***
	(0.000)	(0.009)	(0.000)	(0.009)
Income	0.000	-0.002***	0.000	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Prior bankruptcy	0.100	-15.063***	0.099	-15.063***
	(0.091)	(0.785)	(0.091)	(0.785)
Labor force participation	-0.040	0.421	-0.033	0.441
	(0.233)	(0.607)	(0.232)	(0.610)
Unemployment rate	-0.060	2.473***	-0.066	2.455***
	(0.067)	(0.494)	(0.066)	(0.500)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	58,674	58,674	58,674	58,674
Adjusted R^2	0.932	0.079	0.932	0.079

Table V

Credit modeling and the opioid epidemic. The table reports the coefficient estimates from ordinary least squares regressions on the loan default rates (reported as %) for loans originating after 2011 on the opioid abuse rate and the predicted default rate, a composite measure of the borrower riskiness. I construct the counterfactual default rate by regressing the default rate of loans terminating before 2012 against borrower- and loan-specific characteristics. I then use the coefficient estimates to predict a default rate for all loans originated after 2011. The table reports results from the full sample (col 1 & 2); states in which marijuana was legalized (col 3); and states in which marijuana is not legalized (col 4). Controls as well as county and year fixed effects are as reported. Robust standard errors, clustered by county, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep Var:	Default (%)			
	(1)	(2)	(3)	(4)
Predicted default rate	1.378***	1.408***	1.206***	1.447***
	(0.033)	(0.032)	(0.068)	(0.035)
Opioid abuse rate		0.221***	0.008	0.234***
		(0.048)	(0.270)	(0.050)
Unemployment rate	-8.333***	-8.536***	-6.764***	-8.557***
	(0.490)	(0.522)	(1.074)	(0.600)
Labor force participation	-4.775***	-7.930***	1.813	-7.215***
	(0.594)	(0.627)	(1.829)	(0.686)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sample	Full	Full	MJ legal	No legal MJ
Observations	19,668	19,617	3,157	16,460
Adjusted R^2	0.133	0.147	0.118	0.138

Table VI

Loan payment and the opioid crisis. The table reports the coefficient estimates from ordinary least squares regressions on the total loan payments made by borrowers for (1) loans terminating before 2012 (sample columns 13), and (2) loans originating after 2011 (sample columns 49). Columns represent different levels (high, medium, and low terciles) of opioid abuse rate. The regressor is the predicted payment, which represents a composite measure of the repayment propensity of the borrower. To construct the counterfactual repayment rate, I regress the default rate of loans terminating before 2012 against the borrower- and loan-specific characteristics. I then use the coefficient estimates to predict a payment rate for all loans in the data. Controls as well as county and year fixed effects are as reported. Robust standard errors, clustered by county, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep Var:	Total payments to lender								
	In Sample			Out of Sample					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Predicted payment rate	0.920***	1.037***	1.004***	0.110***	0.165***	0.148***	0.110***	0.162***	0.155***
	(0.035)	(0.082)	(0.097)	(0.023)	(0.026)	(0.023)	(0.023)	(0.024)	(0.023)
Opioid abuse rate							19.964	-44.290	42.839***
							(23.354)	(41.492)	(15.965)
Unemployment rate	477.605***	413.310***	486.199***	-881.981***	-733.762***	-461.375**	-871.911***	-736.797***	-500.840***
	(58.062)	(43.011)	(52.835)	(146.441)	(97.918)	(193.155)	(149.217)	(95.949)	(165.271)
Labor force participation	3.310	-5.951	-11.773	-42.239*	43.784	-21.055	-42.802*	40.175	-35.854
	(29.349)	(27.818)	(24.577)	(21.918)	(56.974)	(43.774)	(22.325)	(53.589)	(49.537)
County FE	No	No	No	No	No	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Opioid Tercile	Low	Medium	High	Low	Medium	High	Low	Medium	High
Observations	8,726	7,956	7,178	9,263	10,331	8911	9,263	10,331	8,911
Adjusted R^2	0.229	0.184	0.242	0.261	0.247	0.167	0.262	0.249	0.199

Table VII

Loan collections and opioid abuse. This table contains coefficient estimates from ordinary least squares regressions on the collections income ($) from defaulted loans on the opioid abuse rate. The sample is split into (1) two time series groups (2006 to 2008 and 2012 to 2016); and (2) states in which wage garnishment is prohibited or permitted. Controls are included for the riskiness of the individual borrower, and the local environment. County and year fixed effects are included as reported. Robust standard errors, clustered by county, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep Var:	Collections Income			
	(1)	(2)	(3)	(4)
Opioid abuse rate	10.491	-1.015	3.432	-6.332***
	(7.185)	(0.809)	(3.743)	(1.912)
FICO score	-0.922***	-2.858***	-0.705***	-2.352***
	(0.096)	(0.174)	(0.163)	(0.158)
Income	-0.001	-0.003	-0.013***	-0.003
	(0.004)	(0.007)	(0.004)	(0.004)
Prior bankruptcy	-123.810***	-52.196*	-84.166***	-135.162***
	(20.715)	(27.732)	(16.044)	(14.250)
Book value	0.005*	0.005***	0.009***	0.002
	(0.003)	(0.002)	(0.002)	(0.002)
Unemployment (%)	-17.809*	48.169***	1.433	48.631***
	(9.119)	(11.364)	(16.871)	(11.504)
Labor force participation (%)	73.719***	76.453***	18.242	-29.356*
	(18.335)	(23.359)	(25.376)	(15.930)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Years	2006 - 2008	2006 - 2008	2012 - 2016	2012 - 2016
Collection	Prohibited	Permitted	Prohibited	Permitted
Sample	Full	Full	Full	Full
Observations	14,578	58,072	10,482	39,178
Adjusted R^2	0.009	0.024	0.012	0.013

Table VIII

Opioid abuse and borrower loan cost. This table summarizes results from regressions on the total loan costs related to a subprime auto loan. Total loan costs include payments of principal and interest, fees, and collections payments after loan default. Coefficient estimates are reported on the opioid abuse rate. Columns 1 and 3 report results before the financial crisis (1999 to 2007); columns 2 and 4 report results after the financial crisis (2012 to 2016). Regressions include controls for the riskiness of the borrower and the contract origination terms. Local economic effects as well as county and year fixed effects are included as reported. Robust standard errors, clustered by county, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep Var:	Total loan costs			
	(1)	(2)	(3)	(4)
Opioid abuse rate	-0.442	91.980***	-0.843	83.043***
	(4.065)	(15.607)	(4.633)	(13.122)
FICO score	-4.405***	-5.285***	-4.371***	-5.110***
	(0.482)	(1.141)	(0.444)	(1.107)
Income	0.378***	0.292***	0.375***	0.291***
	(0.053)	(0.014)	(0.053)	(0.014)
Prior bankruptcy	-273.376***	-87.530	-260.204***	-54.089
	(75.301)	(61.154)	(79.247)	(61.797)
Discount	-0.528***	-1.014***	-0.511***	-1.026***
	(0.068)	(0.042)	(0.064)	(0.044)
Book value	0.905***	1.061***	0.908***	1.064***
	(0.022)	(0.011)	(0.022)	(0.011)
Term	78.137***	93.606***	75.488***	93.687***
	(10.203)	(5.160)	(10.196)	(5.093)
Yield spread	-67.793	-132.220***	-61.599	-136.027***
	(57.907)	(42.022)	(57.923)	(41.062)
Alcohol death rate			330.994***	315.160***
			(101.487)	(47.206)
Labor force participation			181.568*	-68.028
			(107.653)	(91.567)
Unemployment (%)			-188.968	76.192
			(113.644)	(74.928)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sample	1999 to 2007	2012 to 2016	1999 to 2007	2012 to 2016
Observations	45,735	48,499	45,735	48,499
Adjusted R^2	0.508	0.591	0.514	0.596

Internet Appendix to:
"Spillover Effects of the Opioid Epidemic on Consumer Finance"*

For Online Publication

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*Mark Jansen, University of Utah, Eccles School of Business, 1655 Campus Center Dr, Salt Lake City, UT 84112. (O) (801) 213-6910. (f) (775) 201-1325. E-mail:mark.jansen@eccles.utah.edu

Appendix A. Variable Definitions

This appendix reports definitions for the variables used in the analysis. Data were obtained as follows: loan data 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.

Actual payments: Total loan payments received by the lender from the borrower, measured in $.

Alcohol death rate: Alcohol-induced cause of death per 100,000 persons for all races, both sexes, and all ages for the years 1999 to 2016. Accessed at at http://wonder.cdc.gov/mcd-icd10.html.

Amount financed: Original amount financed, measured in $.

Book value: Vehicle book value, measured in $.

Collections: Funds recovered through collections (e.g., wage garnishment), measured in $.

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

FICO score: Mean of borrower's credit scores from all queried credit-reporting agencies.

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

Contracted payment: Contracted monthly payment, measured in $.

Opioid death rate: Drug poisoning death rates per 100,000 persons for all races, both sexes, and ages 20 to 79 for the years 1999 to 2016. The data includes ICD-10 Codes: X40-X44, X60-X64, X85, and Y10-Y14. Accessed at at http://wonder.cdc.gov/mcd-icd10.html.

Opioid prescription rate: Opioid prescribing rates per 100 persons as reported by the IQVIA Transactional Data Warehouse (TDW) for 2006 to 2017 and presented on the CDC website (www.cdc.gov/drugoverdose/maps/rxrate-maps.html). Opioid prescriptions include buprenorphine, codeine, fentanyl, hydrocodone, hydromorphone, methadone, morphine, oxycodone, oxymorphone, propoxyphene, tapentadol, and tramadol. However, cough and cold formulations containing opioids were not included; nor were methadone prescriptions dispensed through maintenance treatment programs.

Opioid abuse rate: Opioid death rate divided by the lagged opioid prescription rate.

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

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

Total Loan Costs: The total loan cost represents (1) all payments of principal, interest, and fees made by the borrower to the lender, (2) the loss of value and costs associated with the repossession and sale of the vehicle, and (3) all payments made by the borrower arising from post-default collections efforts.

Unemployment rate: Metropolitan Statistical Area unemployment rate, 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 %.

Appendix B. Supporting Figures and Tables

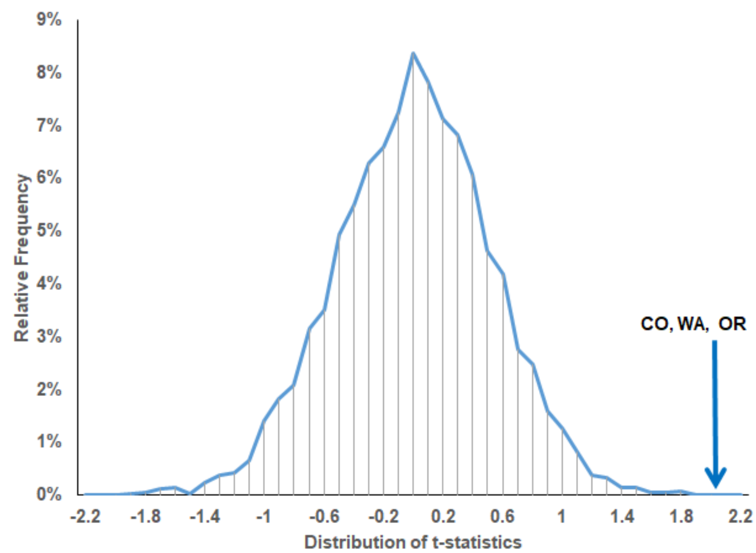

Figure B.1. This figure presents a histogram of a distribution of t-statistics for the slope coefficients of a difference-in-difference specification on loan defaults. I use the specification in Table IV column 2: $Y_{i,j,\tau} = \lambda_j + \lambda_\tau + \beta_1 D_{j,\tau} + \beta_2 X_{i,j,\tau} + \varepsilon_{i,j,\tau}$, where $Y_{i,\tau}$ is my dependent variable of interest—an indicator of loan default for borrower i; j is the county where the loan originated; and τ is the year the loan originated. The equation includes controls $X_{i,j,\tau}$ for individual borrower and local labor market characteristics. The specification also includes county(λ_j) and year (λ_τ) fixed effects. $D_{j,\tau}$ represents the indicator for states that (1) legalized recreational marijuana usage and (2) have implemented operational and legally protected dispensaries. For a sample of 10,000 regressions, I randomly substitute three states to coincide with the adoption dates of legalized marijuana.

Table B.I

Loan performance trends around marijuana legalization. This table contains results from ordinary least squares regressions on (1) the opioid abuse rate; (2) an indicator for loans terminated due to default (reported as %); (3) the alcohol death rate; and (4) the opioid death rate. Coefficient estimates are reported for indicator variables for years around the time of the legalization of the recreational sale of marijuana. Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy), and the local environment (unemployment rate). The sample period is 2012 to 2016. State and year fixed effects are included as reported. Robust standard errors, clustered by state, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep Var:	Opioid abuse rate	Default (%)	Alcohol death rate	Opioid death rate
	(1)	(2)	(3)	(4)
Legalization ($\tau - 2$)	0.697	1.865	-1.086**	-0.305
	(0.633)	(2.727)	(0.522)	(0.400)
Legalization ($\tau - 1$)	0.331	-0.872	-0.608*	-0.274
	(0.899)	(1.182)	(0.356)	(0.340)
Legalization ($\tau + 1$)	-2.625**	-2.971***	-0.449***	-1.173*
	(1.290)	(1.049)	(0.158)	(0.665)
Legalization ($\tau + 2$)	-3.673***	-3.389***	0.023	-1.542**
	(1.154)	(0.542)	(0.109)	(0.594)
FICO score	0.000	-0.184***	-0.000	0.001
	(0.001)	(0.009)	(0.001)	(0.001)
Income	-0.000*	-0.002***	-0.000***	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Prior bankruptcy	0.331	-17.066***	-0.176	0.235
	(0.473)	(0.797)	(0.113)	(0.288)
Unemployment rate	-1.318***	-0.575	0.072	-0.609**
	(0.390)	(0.456)	(0.159)	(0.242)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	62,094	79,337	76,807	62,094
Adjusted R^2	0.484	0.100	0.713	0.563

Table B.II

Impact of marijuana legalization on opioid abuse and loan performance. This table contains results from ordinary least squares regressions on the opioid abuse rate. The regressor represents an indicator variable (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. Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy), and the local environment (unemployment rate and labor force participation). The sample period is 2012 to 2016. State and year fixed effects are included as reported. Robust standard errors, clustered by state, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep Var:	Opioid abuse rate			Default (%)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-legalization CO	-1.643***			-5.928***		
	(0.350)			(0.776)		
Post-legalization WA		-0.930**			0.140	
		(0.393)			(0.860)	
Post-legalization OR			-2.003***			-5.799***
			(0.331)			(0.666)
FICO score	0.000	0.000	0.000	-0.184***	-0.191***	-0.191***
	(0.000)	(0.000)	(0.000)	(0.010)	(0.009)	(0.009)
Income	0.000	0.000	0.000	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prior bankruptcy	0.113	0.129	0.117	-15.296***	-15.735***	-15.589***
	(0.095)	(0.104)	(0.106)	(0.878)	(0.872)	(0.908)
Unemployment (%)	-0.062	-0.065	-0.060	-0.424	-0.434	-0.226
	(0.077)	(0.081)	(0.092)	(0.634)	(0.660)	(0.666)
Labor force participation (%)	-0.033	0.008	0.041	-1.555**	-1.264	-1.794***
	(0.260)	(0.256)	(0.268)	(0.624)	(0.748)	(0.656)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	54,089	51,814	49,277	56,847	54,347	51,748
Adjusted R^2	0.932	0.933	0.933	0.096	0.092	0.093

Table B.III

Loan performance trends around marijuana legalization with synthetic controls. The table reports coefficient estimates on opioid abuse rates (col 1 to 4) and default rates (col 5 to 8) for indicator variables (post-legalization). The regressor in Columns 1 & 4 is for loans that were terminated in a state that had implemented laws allowing the recreational sale of marijuana. Columns 2 & 5 use the quarterly taxable marijuana sales that occurred in each state or 0 otherwise. Columns 3, 4, 7, & 8 use indicator variables for years around the time of the legalization of the recreational sale of marijuana. Following Abadie et al. (2010), I use entropy weights such that the means in the reweighted control group data match the mean and variance from the treatment group as shown in Table B.V. Sample period is 2012 to 2016. State and year fixed effects are included as reported. Robust standard errors, clustered by state, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep Var:	Opioid abuse rate				Default (%)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-legalization All	-2.686**				-6.733***			
	(1.219)				(1.312)			
Legal marijuana sales (ln)		-0.154**				-0.377***		
		(0.069)				(0.070)		
Legalization ($\tau-3$)			1.196	1.196			6.028***	6.028***
			(1.129)	(1.129)			(1.544)	(1.544)
Legalization ($\tau-2$)			0.580	0.580			3.590**	3.590**
			(0.557)	(0.557)			(1.415)	(1.415)
Legalization ($\tau-1$)			0.295	0.295			4.037***	4.037***
			(0.740)	(0.740)			(1.259)	(1.259)
Legalization ($\tau+1$)			-2.550***	-2.550***			-1.631	-1.631
			(0.829)	(0.829)			(1.917)	(1.917)
Legalization ($\tau+2$)			-5.401**	-5.401**			-5.177***	-5.177***
			(2.225)	(2.225)			(1.278)	(1.278)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	52,640	52,640	52,640	52,640	63,010	63,010	63,010	63,010
Adjusted R^2	0.494	0.494	0.504	0.504	0.043	0.043	0.043	0.043

Table B.IV

Loan performance with synthetic controls. The table reports slope coefficients of an IV estimator on the abnormal opioid abuse rate. Following Abadie et al. (2010), I use entropy weights such that the means in the reweighted control group data match the mean and variance from the treatment group as shown in Table B.V. Sample period is 2012 to 2016. State and year fixed effects are included as reported. Robust standard errors, clustered by state, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep Var:	Default rate (%)	
	(1)	(2)
Opioid abuse rate	1.664***	1.619***
	(0.303)	(0.285)
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	52,640	52,640
Kleibergen-Paap Wald F-stat	51.14	57.62

Table B.V

Synthetic controls summary statistics. This table compares the pre-treatment borrower characteristics and demographics in states that legalized marijuana and states that did not, and reports the mean, variance, and skewness of variables used in Table B.III. I calculate the mean, variance, and skewness in the treatment group (marijuana legalization=1) and search for a set of entropy weights such that the means in the reweighted control group data match the mean and variance from the treatment group using the methodology described in Abadie et al. (2010).

(Panel A:) Before synthetic controls

	Treatment:			**Control:**		
	mean	variance	skewness	mean	variance	skewness
Prior bankruptcy	0.403	0.241	0.394	0.214	0.168	1.398
Labor force participation	63.210	17.000	0.583	66.660	19.560	-0.266
Unemployment rate	5.807	3.699	0.536	5.424	3.369	1.042
Medicare prescription drug eligible (%)	26	35	0.609	33	74	0.803
Female divorced (%)	14	1	-0.573	13	4	-0.144
Agriculture & mining employment (%)	1.416	1.172	4.657	1.230	2.271	6.953
Construction employment (%)	6.623	2.748	0.918	6.548	2.196	0.533
Homeowner (%)	63.750	32.260	-0.679	64.560	50.640	0.014
Primary care MD per capita	0.001	0.000	0.159	0.001	0.000	0.799
Veteran hospital indicator	0.221	0.172	1.343	0.629	0.233	-0.535
Rent per capita	0.024	0.000	0.554	0.023	0.000	1.103
Income per capita	10.700	0.029	-0.004	10.600	0.027	0.067

(Panel B:) After synthetic controls

	Treatment:			**Control:**		
	mean	variance	skewness	mean	variance	skewness
Prior bankruptcy	0.403	0.241	0.394	0.374	0.234	0.522
Labor force participation	63.210	17.000	0.583	63.610	20.520	0.337
Unemployment rate	5.807	3.699	0.536	5.713	2.653	1.263
Medicare prescription drug eligible (%)	26	35	0.609	28	32	1.535
Female divorced (%)	14	1	-0.573	13	3	-0.739
Agriculture & mining employment (%)	1.416	1.172	4.657	1.374	8.519	5.306
Construction employment (%)	6.623	2.748	0.918	6.591	2.207	0.804
Homeowner (%)	63.750	32.260	-0.679	64.160	79.360	-0.270
Primary care MD per capita	0.001	0.000	0.159	0.001	0.000	-0.302
Veteran hospital indicator	0.221	0.172	1.343	0.294	0.208	0.903
Rent per capita	0.024	0.000	0.554	0.024	0.000	0.425
Income per capita	10.700	0.029	-0.004	10.670	0.052	1.004

Appendix C. Other Regulatory Changes

Another possible explanation for the relation of opioid abuse and loan defaults is that an additional, separate change in regulations concurrently affected these measures. If this occurred, then the mechanism attributed to marijuana is incorrectly identified, but the relation between opioid abuse and loan defaults remains valid. One such change could be the 2014 adoption, by 12 states, of laws that limited initial opioid prescriptions to a 30-day supply. However, the medical literature provides no empirical evidence that these laws significantly affected the opioid abuse rate. Another such change could be the passage of new prescribing rules for opioid drugs by Oregon and Washington.[35] However, prior to 2017, Washington's last update to its opioid regulations was in 2011, three years before the state legalized recreational marijuana; Oregon did not pass any new regulations related to opioids prior to or in the year immediately following its legalization of marijuana, but did adopt the CDC's advisory guidelines in 2016 (along with most other U.S. states). To summarize, while there were a number of other legislative changes in the states that legalized recreational marijuana, it seems implausible that these changes caused the impact on opioid abuse and the resultant change in loan default rates that I observe. The absence of any other meaningful regulation of opioids in these states suggests that another factor (e.g., marijuana legalization itself) explains the reduction in opioid abuse after the legalization of recreational marijuana.

[35] See for example, Washington ESHB 1427. For a comprehensive overview of state laws related to opioid-related policy, see the Arizona Department of Health Services' site: https://www.azdhs.gov/documents/prevention/womens-childrens-health/injury-prevention/opioid-prevention/50-state-review-printer-friendly.pdf.