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Spillover Effects of the Opioid Epidemic on Consumer Finance

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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 weak predictive performance of traditional credit measures and the resulting higher default rates generate a negative externality for borrowers in opioid-afflicted areas, as evidenced by 5.7% higher loan costs for subprime borrowers.

I. Introduction

Prescription opioid and heroin addiction is a global epidemic that affects both health and economic welfare. In the United States, over 2 million people suffer from opioid-related use disorders and over 700,000 people have died from overdoses in the last 20 years. The epidemic shows no signs of abating during Covid (Bauman and Lopez (2021)), as deaths from opioid overdoses are now more common than fatalities from automobile accidents (Centers for Disease Control and Prevention (2018a)). 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)).

Although many of the health impacts and some of the economic impacts of opioid abuse have been examined, little is known about the opioid epidemic's spillover effects on financial markets. This study provides the first empirical evidence of a relation between opioid abuse and consumer credit. This article investigates i) whether local exposure to opioid abuse is a significant risk factor for lenders, and ii) whether this risk factor creates costly externalities for borrowers. If communities

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with high rates of opioid abuse experience higher loan default rates, and if traditional credit scoring attributes (e.g., FICO) 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. The consequences of the opioid epidemic could thus extend beyond the labor market and affect the pricing of consumer finance products, possibly leading to other repercussions (e.g., deteriorating credit-market conditions) for consumers.

The subprime loan market is an ideal setting in which to study the impact of opioid abuse on consumer finance because its borrowers fall within the at-risk population for opioid abuse (Zedler, Saunders, Joyce, Vick, and Murrelle (2017)) and its size is large. Most U.S. households have a vehicle, and more than one-third have an auto loan (Bricker, Dettling, Henriques, Hsu, Jacobs, Moore, Pack, Sabelhaus, Thompson, and Windle (2017)). Recent auto loan balances exceed $1.1 trillion, and 40% of these loans are nonprime or lower credit (Zabritski (2018)).

Using a difference-in-differences framework in a natural experiment, I document the relation of opioid abuse to loan performance using a panel of 118,709 subprime auto loans. To assess the impact of opioid abuse on consumer finance, the study uses county-level data on drug-poisoning rates (i.e., deaths). Drug-poisoning rates are a useful proxy for opioid abuse, as 70.6% of overdose deaths in the United States involve opioids (Centers for Disease Control and Prevention (2021)). 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 borrowers.

I find that counties with higher rates of drug-related deaths have higher loan default rates. After accounting for borrowers' creditworthiness and local economic conditions, I find that a 1-standard-deviation increase in the county-level drug-related death rate is associated with a 12.6% increase ($p < 0.01$) in loan defaults, relative to the mean. This *increase* in default rates is comparable to about a 4.7% *reduction* in the average borrower's FICO score. 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, Petry, Badger, and Bickel (1997), Kirby, Petry, and Bickel (1999)).[1] 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 the aforementioned ordinary least squares (OLS) regression. First, one must rule out the possibility that an omitted variable related to changing local 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 loan defaults cause an increase in opioid abuse. To address these concerns, I identify a relation between opioid abuse and loan defaults using a difference-in-differences framework in a natural experiment.

[1]The psychological effects associated with opioid abuse alter brain chemistry, sometimes leading to repeated use of the drug. As substance dependence develops, initial enjoyment gives way to anxiety about the next use. Thus, individuals who are facing strong withdrawal symptoms may be more interested in satisfying their drug cravings than in making consistent car payments.

In this 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)). (Prescription opioids are the drugs to which many opioid abusers initially become addicted.) 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, Saloner, Cunningham, and Barry (2014), Reiman, Welty, and Solomon (2017), and Powell, Pacula, and Jacobson (2018)). Drug-abuse treatment efforts that increase the availability of substitutes for opioid drugs have been shown to reduce the pathological behavior associated with substance abuse (Bickel, Madden, and Petry (1998)).

Using a difference-in-differences specification, I find that states that legalize recreational marijuana experience significant declines in the drug-related death rate and loan defaults relative to other states. Put simply, legal marijuana appears to crowd out illicit opioid use and its negative effects on household finance. I use two strategies to alleviate concerns that differences in the economic and political conditions across states are driving the relation between opioid abuse and loan defaults. First, in the difference-in-difference analysis, I construct the control group from borrowers living in states where recreational marijuana use was illegal during the sample period but has since been legalized. Second, I instrument for changes in opioid abuse using the timing of marijuana legalization in each state. I find that states that legalize recreational marijuana during the sample period (2012 to 2016) experience a significant decline ($p < 0.01$) in the drug-related death rate (after legalization), relative to states that do not legalize marijuana sales during the sample period but do so later. When I then instrument the drug-related death rate with the legalization of recreational marijuana, I find that a decrease in drug-related deaths results in a decrease in loan defaults.

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 lenders cannot distinguish between 2 otherwise similar borrowers who are differentially shocked by an unobserved risk factor (i.e., the opioid epidemic), then they may ration credit or increase the cost of credit similarly for both borrowers. I investigate the predictive power of traditional risk-assessment factors (e.g., the borrower's FICO score, income, and other observable credit attributes) on realized default rates and find that data on the drug-related death rate significantly improves lenders' 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 still be profitable for them). In further tests, I examine loan repayments and find that they vary with the local drug-related death rate during the height of the opioid epidemic. The out-of-sample performance of the lenders' traditional credit model declines by over 24% in areas with high levels of drug-related deaths; less affected areas see no such declines. Adding data on the drug-related death rate to the model increases the adjusted R^2 of the (out-of-sample) payment prediction model by 19%

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in areas in the highest tercile of the drug-related death rate, but does not lead to meaningful improvements in less affected areas.

In the final tests, I compare how the opioid epidemic differentially affected the total loan costs in the period of peak opioid abuse, relative to an earlier period (before the great financial crisis) with lower abuse rates. 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. I find that during the peak of the opioid epidemic, borrowers in counties at the 75th percentile of the drug-related death rate pay $1,394 more over the life of an average subprime auto loan, relative to borrowers in counties at the 25th percentile. This represents a 5.7% increase over the total average loan cost for the average subprime borrower, ceteris paribus. By comparison, differences in the drug-related death rate had no significant impact on total loan costs before the epidemic intensified. The higher overall default rate, combined with the poor out-of-sample predictive performance of traditional borrower credit attributes (e.g., FICO score), may explain why borrowers in opioid-afflicted areas pay so much more for subprime auto loans.

This article makes three contributions. First, by connecting the opioid epidemic with financial markets, it adds to the literatures on opioid addiction and economic outcomes (Florence, Luo, Xu, and Zhou (2016), Krueger (2017), Currie, Jin, and Schnell (2018), Harris, Kessler, Murray, and Glenn (2020), Ouimet, Simintzi, and Ye (2020), and Park and Powell (2021)), on the economic spillovers from substance abuse more generally (Levitt and Porter (2001), Aliprantis and Schweitzer (2018), and Case and Deaton (2020)), and on the relation between health conditions and finance (Himmelstein, Thorne, Warren, and Woolhandler (2009), Dobbie and Song (2015), Mahoney (2015), Cohn and Wardlaw (2016), and Xue, Zhang, and Zhao (2021)) (including recent analyses of COVID-19 and financial outcomes (Goodell (2020)). Using new data on auto loan outcomes and origination terms, I find that opioid abuse, as proxied by drug-related deaths, 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.[2] The resulting defaults can also spill over into the $100 billion auto loan securitization market.

Second, by showing that drug abuse has predictive power in credit modeling and that borrowers in opioid-afflicted areas pay more for access to credit, this article lends support to the theoretical literature's predictions on how supply-side responses to asymmetric information affect credit availability (Akerlof (1970), Stiglitz and Weiss (1981)).

Third, this article extends the literature on the externalities associated with deteriorating credit-market conditions (Campbell, Giglio, and Pathak (2011), Anenberg and Kung (2014), and Mian, Sufi, and Trebbi (2015)). Specifically, it supports the argument that the opioid epidemic's impact on local credit markets could be a factor in the economic decay in opioid-afflicted areas.

[2]These results may be conservative due to the limited availability of loan data in the areas that are most exposed to the opioid epidemic. Forty percent of the approximately 12 million auto loans (average loan balance of $16,000) per year are nonprime or lower credit (Jefferies (2018)).

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II. Data

To assess the impact of opioid abuse on consumer finance, the study uses county-level data on drug poisoning death rates as a proxy for county-level opioid abuse. Data on drug poisoning deaths per 100,000 persons, for all races, both sexes, and ages 20–79, from 1999 to 2016 comes from the National Center for Injury Prevention and Control (NCIPC) of the Centers for Disease Control (CDC). The data shows that the mortality rate attributable to opioids increased fivefold from 1999 to 2016, with most of the increase occurring after 2011. Further details on the variables used in this study are in Appendix A of the Supplementary Material.

While a direct measure of opioid abuse would be ideal, the county-level measure of drug-related deaths is a reasonable substitute, for two reasons. First, data on drug-related deaths are readily available, and recent data from the CDC shows that 70.6% of overdose deaths involve the use of prescription or nonprescription opioids. Opioid abuse, in contrast, is difficult to measure, and panel data is not available.[3] Second, the assumption underlying my use of the county-level drug-related death rate – that the opioid death rate per abuser is relatively constant across counties – seems reasonable.

My use of a county-level (rather than individual-level) measure also allows me to shed light on how opioid abuse within communities causes negative spillover effects both for individual borrowers and for the community as a whole. For example, one indirect effect of opioid abuse on the local community is a crime (Hammersley, Forsyth, Morrison, and Davies (1989)).

Although a mechanical relation exists between drug-related deaths and loan defaults, the impact of this relation on the total number of loan defaults is small, as the vast majority of loan defaults are not directly caused by overdose deaths. At the end of 2016, for example, approximately 3.2 million subprime auto loans were 90 days delinquent. In that year, 63,632 drug overdose deaths occurred (Centers for Disease Control and Prevention (2018b)).

I match the county-level data on drug-related death rates 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 259,467 loans, which were originated at 3,926 dealerships in 1,903 U.S. zip codes. To avoid censorship, I remove loans from the sample if their full term is not observed or if the CDC does not report the drug-related death rate for the county of origination. I also remove loans if the credit score, income, prior bankruptcy flag, loan terms, or vehicle book value is not available. The resultant sample is 118,705 loans.

Table 1 shows the summary statistics for the loans used in the article. The average borrower in the sample has a FICO score of 533 and a monthly gross income of $3,330. Borrowers purchase vehicles with an average book value of $13,380 on a 66-month (average) term. The average default rate is 28.6%. In this setting, a default is defined as a delinquency that leads to lender efforts to repossess

[3]Emergency room (ER) visits related to opioid overdoses (which are correlated with drug-related deaths) would also make a good proxy, but data on these visits are only available for a small sample of states and a limited number of years.

TABLE 1

Summary Statistics

Table 1 reports summary statistics for the sample of 118,705 auto loans extended by 3,926 dealerships in 1,903 ZIP codes. Means, standard deviations, and the 25th, 50th, and 75th percentiles are reported. The data summarizes borrower, vehicle, loan, and loan environment characteristics. Appendix A of the Supplementary Material reports definitions for the variables used in the analysis.

	Mean	Std. Dev.	P25	P50	P75	Count
Borrower, vehicle, and loan characteristics						
FICO_SCORE	533	53	497	532	567	118,705
MONTHLY_INCOME ($ '000s)	3.33	2.37	2.25	3.37	4.60	118,705
PRIOR_BANKRUPTCY (=1)	0.26	0.44	0.00	0.00	1.00	118,705
DISCOUNT	421.64	605.33	150	499	799	118,705
VEHICLE_BOOK_VALUE ($ '000s)	13.38	4.54	10.35	12.93	15.78	118,705
TERM (months)	66	8	60	72	72	118,705
DOWN_PAYMENT ($)	1.19	1.46	0.30	1.00	1.50	118,705
DEFAULT (%)	28.57	45.17				118,705
TOTAL_PAYMENTS_TO_LENDER ($ '000s)	4.06	4.67	0.00	2.69	7.18	110,922
TOTAL_LOAN_COST ($ '000s)	20.94	8.04	15.04	19.86	25.71	106,779
Loan environment						
DRUG_DEATH_RATE (per 100,000)	16.79	7.51	11.20	15.20	21.71	118,705
ALCOHOL_DEATH_RATE (per 100,000)	9.11	4.96	5.92	7.30	11.42	114,871
TAXABLE_MARIJUANA_SALES ($ millions)	5.48	28.95	0.00	0.00	0.00	118,705
LABOR_FORCE_PARTICIPATION (%)	66.80	4.32	63.28	67.01	69.80	118,703
UNEMPLOYMENT_RATE (%)	5.49	1.84	4.10	5.20	6.50	118,683
YIELD_SPREAD (%)	2.03	0.95	1.31	1.93	2.43	118,176
COUNTY_INCOME	4.40	0.79	3.85	4.14	4.81	117,428

the vehicle. The summary statistics of the loans in this sample are similar to those reported by Jefferies (2018) and Zabritski (2018) for the total U.S. subprime auto loan market.

Table 1 also summarizes the CDC data for the drug-related death rate by county. The average drug-related death rate (per 100,000) for the sample is 16.79, with a standard deviation of 7.51. The highest observed death rate in the CDC data is 139.44.[4]

III. The Opioid Epidemic and Loan Performance

In this section, I compare loan outcomes across counties and states with different drug-related death rates to determine if opioid abuse affects loan outcomes.

I estimate an ordinary least squares model of the relation between opioid abuse (as proxied by drug-related deaths) and loan defaults. Specifically, I estimate the following:

$$Y_{i,j,\sigma,\tau} = \beta_1 Z_{j,\tau} + \beta_2 X_i + \lambda_j + \lambda_{\sigma,\tau} + \varepsilon_{i,j,\sigma,\tau}, \quad (1)$$

where $Y_{i,j,\sigma,\tau}$ is the dependent variable of interest – an indicator of loan default for borrower i, in county j, in state σ, in year τ. The variable $Z_{j,\tau}$ represents the drug-related death rate for county j in year τ. The equation includes controls X_i for

[4]The summary statistics are similar to the full sample of drug-related death rates for the United States (e.g., Mean = 17.76 and SD = 11.63), suggesting that my sample of loans is representative of the exposure of subprime loans to the opioid epidemic at that time. The slightly lower average death rate in my sample indicates that, if anything, my sample is biased toward areas with lower drug-related death rates. Given the large sample of loans from 44 states, the external validity of this study is compelling. And the study's undersampling of the most afflicted areas suggests that the results may be conservative.

TABLE 2

Loan Performance OLS

Table 2 contains coefficient estimates from ordinary least squares regressions on an indicator for loans terminated due to default (reported as %) on the drug-related death rate. Controls are included for the riskiness of the individual borrower and the local environment. County, year, state × 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.

	Dependent Variable: DEFAULT				
	1	2	3	4	5
DRUG_DEATH_RATE	0.235***	0.356***	0.455***	0.420***	0.479***
	(0.078)	(0.085)	(0.094)	(0.088)	(0.099)
FICO_SCORE		−0.149***	−0.145***	−0.149***	−0.145***
		(0.006)	(0.006)	(0.006)	(0.006)
MONTHLY_INCOME		−1.666***	−1.609***	−1.647***	−1.590***
		(0.187)	(0.175)	(0.180)	(0.168)
PRIOR_BANKRUPTCY		−13.175***	−12.541***	−13.002***	−12.414***
		(1.148)	(1.094)	(1.163)	(1.119)
VEHICLE_BOOK_VALUE		−0.374***	−0.307***	−0.384***	−0.321***
		(0.066)	(0.069)	(0.066)	(0.069)
TERM		0.186***	0.185***	0.192***	0.192***
		(0.030)	(0.028)	(0.031)	(0.028)
DOWN_PAYMENT		−1.437***	−1.496***	−1.438***	−1.487***
		(0.304)	(0.320)	(0.296)	(0.310)
YIELD_SPREAD		−2.006***	−1.765***	−1.927***	−1.716***
		(0.211)	(0.227)	(0.211)	(0.232)
ALCOHOL_DEATH_RATE		−0.103	−0.032	−0.023	0.022
		(0.135)	(0.125)	(0.180)	(0.145)
UNEMPLOYMENT_RATE		1.842***	1.811***	1.881***	1.887***
		(0.369)	(0.401)	(0.455)	(0.457)
LABOR_FORCE_PARTICIPATION		1.972***	2.251***	3.032***	3.099***
		(0.367)	(0.427)	(0.493)	(0.481)
County FE	Yes	Yes	No	Yes	No
Dealership FE	No	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	No	No
State × year FE	No	No	No	Yes	Yes
No. of obs.	117,452	116,035	115,685	116,020	115,670
Adj. R^2	0.068	0.111	0.120	0.114	0.123

an individual borrower, loan, and vehicle characteristics. The specification also includes county (λ_j) fixed effects, which help address persistent local variation. To help rule out time-varying heterogeneity at the state level (e.g., state-level initiatives to address the opioid epidemic), I also include state by year-fixed effects (λ_{σ}, τ). In the analyses, I present heteroscedasticity-robust standard errors clustered at the county level to account for intertemporal correlation across loans originated in the same county.[5]

Table 2 presents OLS regression results on the percentage of loans terminated due to default. The regressor is the county-year drug-related death rate as described in Section II. Column 1 shows that the county-level drug-related death rate is positively correlated ($p < 0.01$) with loan defaults. To control for the local economic environment, the specification includes county and year-fixed effects.

[5]Figure C.1 of the Supplementary Material graphs coefficient plots that show the variation in confidence intervals (90% and 99%) for different clustering strategies. The relation between the drug-related death rate and loan defaults remains statistically significant at the 1% level.

In column 2, controls for the riskiness of the individual borrower (credit score, income, and prior bankruptcy), the vehicle (book value), and the loan terms (term and down payment) are introduced.

The specification in column 2 also includes 3 time-varying controls: the labor participation rate, the unemployment rate, and the alcohol abuse rate. In column 3 (and column 5), I substitute dealership fixed effects for county fixed effects. Dealership fixed effects capture any differences in the behavior of dealership personnel that affect loan outcomes (e.g., dealership sales incentives that disregard the likelihood of loan default). Their inclusion helps address the concern that dealers differentially treat prospective borrowers who show a propensity to abuse drugs. Finally, in columns 4 and 5, I replace the year-fixed effects with state-by-year fixed effects and find that the results are largely unchanged.

In all specifications, I find that borrowers who reside in counties with higher drug-related death rates are more likely to default on their auto loans. When I include the full set of controls in the specification (column 5), a 1-standard-deviation increase in the drug-related death rate is associated with a 3.6-percentage-point increase in the likelihood of default, a 12.6% increase relative to the mean.[6] For the average borrower in the loan sample, this represents an increase, in the likelihood of default, from 28.5% to 32.1%, which is commensurate with a 25-point *decrease* in a borrower's FICO score.[7]

The medical literature has identified a mechanism that could help explain the link between 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. 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. Addicted users who are in withdrawal are likely to be more interested in satisfying their cravings than in maintaining consistent employment or making 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. 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 more likely that the pharmacological properties of opioids are negatively impacting labor force participation. The lack of labor market participation, combined with the significant acquisition costs associated with satisfying an opioid addiction, could largely explain the changes in loan performance that I attribute to the opioid epidemic.

[6]In untabulated results, I find that the inclusion of lags for prior year drug-related deaths in the specification does not significantly change the results. The results are also robust to adding nonlinear factors such as credit score squared.

[7]I divide the change in the default rate (3.6%) by the coefficient on the FICO score (−0.145) to determine how the marginal income affects the likelihood of default.

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Marijuana Legalization and Loan Defaults

In an ideal experiment, borrowers would be randomly assigned to the opioid epidemic to determine how the epidemic affects their loan default rate. To instead use the drug-related death rate as a regressor in an OLS specification is empirically insufficient, since an unobserved variable could be correlated with both the drug-related death rate and loan defaults. For example, a manufacturing plant's closing could simultaneously affect employment (and thus loan defaults) and opioid abuse (and drug-related deaths).

To help address this identification challenge, I take advantage of laws legalizing the recreational use of marijuana. Because these regulatory changes alter the supply of a nonopioid analgesic and potential opioid substitute – marijuana – but not the supply (or prices) of prescription opioids, they could be a source of exogenous variation in the drug-related death rate, which would be useful empirically. Using a difference-in-differences approach, I, therefore, assess how these laws differentially impact both the drug-related death rate and loan defaults.

Recent studies in the medical literature found that laws allowing legal access to marijuana reduce the use of opioid analgesics and deaths from an opioid overdose (Bachhuber et al. (2014), Powell et al. (2018)). In 1 survey, 97% of opioid-using medical marijuana patients reported decreasing their opioid consumption when they used marijuana (Reiman et al. (2017)).

As of Jan. 2018, 3 U.S. states had implemented laws permitting the legalized the sale of marijuana for nonmedical reasons: Colorado in 2014, Washington in 2014, and Oregon in 2015. While there are multiple ways to access marijuana, laws that permit its recreational sales and use should provide wider and less costly access to it, which in turn may facilitate its substitution for opioids.[8] 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 nonadopting 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.

To formally identify the impact of opioid abuse, I use the legalization of marijuana as a source of exogenous variation in the drug-related death rate in a difference-in-differences (DiD) framework. Identification originates solely from the introduction of marijuana legalization interacted with the timing of the law. This strategy allows me to 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

[8]Laws related to medical marijuana pose an empirical challenge, as states with medical marijuana dispensaries have regularly changed their regulations in response to changes in federal marijuana policy since 2010 (Powell et al. (2018)).

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(2)
$$y_{i,\sigma,\tau} = \lambda_\sigma + \lambda_\tau + \beta_1 D_{i,\tau} + \varepsilon_{i,\sigma,\tau},$$

where $y_{i,\sigma,\tau}$ represents the outcome (i.e., the drug-related death rate in state σ in year τ), and $D_{i,\tau}$ represents the indicator for states that have i) legalized recreational marijuana usage and ii) implemented operational and legally protected dispensaries. A borrower is treated if the loan is originated in a state where those 2 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 county unemployment rate, which might influence access to health 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.

Column 1 of Table 3 reports results from OLS regressions on the drug-related death rate for indicator variables (POST_LEGALIZATION) for loans that were originated in states with legalized marijuana sales. The coefficient on the postlegalization indicator variables suggests that the legal-marijuana states experience a significant 1.1% decline ($p < 0.01$) in drug-related deaths after legalization, relative to other states.[9]

TABLE 3
Loan Performance: Difference in Difference

Table 3 reports results from OLS regressions on the drug-related death rate (in columns 1 and 2) and the loan default rate (reported as % in columns 3 and 4). The dependent variable is an indicator variable (POST_LEGALIZATION) equal to 1 for loans that were terminated in states that had implemented laws allowing the recreational sale of marijuana, or 0 for loans terminated in all other states (columns 1 and 3) or in states that legalized the recreational sale of marijuana after the end of the sample period and prior to 2021 (AZ, CA, IL, MA, ME, MI, NJ, NM, NY, NV, SD, VA, and VT) (columns 2 and 4). Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy), the loan (term, down payment), the vehicle (vehicle book value), and the macroeconomic environment (unemployment rate, alcohol death rate, labor market participation, and yield spread). State- and year-fixed effects are included as reported. Robust standard errors, clustered by state year, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	DRUG_DEATH_RATE		DEFAULT	
	1	2	3	4
POST_LEGALIZATION	−1.104***	−0.796**	−4.875***	−6.002***
	(0.359)	(0.385)	(1.292)	(1.429)
Borrower controls	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes
Vehicle controls	Yes	Yes	Yes	Yes
Environment controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sample	All	MJ legal	All	MJ legal
No. of obs.	50,326	20,615	50,326	20,615
Adj. R^2	0.626	0.632	0.087	0.088

[9]To satisfy a concern that the use of loan-level data underestimates the standard errors, I examine the relation between marijuana legalization and drug-related death rates using observations that are collapsed at the county level. To retain the differences in the borrower characteristics of the loan data, I have also collapsed the borrower characteristics. Table C.1 of the Supplementary Material reports these

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Column 3 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 originated in states that had legalized marijuana sales. The results indicate that marijuana legalization results in a 4.9% *decrease* ($p < 0.01$) in loan defaults relative to other states.

The analysis in columns 1 and 3 of Table 3 compares the 3 states (Oregon, Colorado, and Washington) that legalized marijuana with all other states. To help allay concerns that the choice of states is coincidental, I repeat the test shown in column 3 of Table 3 with placebos constructed using random samples of 3 states.[10] If the results can be replicated with these random samples, then the effect that I attribute to opioid abuse may be spurious. If the results consistently differ from those of the original test, then my hypothesis that opioid abuse leads to loan defaults will become more credible. In Figure C.2 of the Supplementary Material, the histogram shows a sample of 10,000 regressions that are centered around a mean of 0. This iterative procedure provides a distribution of t-statistics for the states that did not legalize recreational marijuana during my sample period. It shows that when random U.S. states are substituted for the 3 states that legalized recreational marijuana, the outcome in column 2 of Table 3 is very rarely reproduced. The incidence of Colorado, Washington, and Oregon adopting the laws is, at minimum, a significant outlier in the data.

Nevertheless, one must consider that during the sample period of 2012 to 2016, the opioid crisis was most acute in the Appalachian states of Ohio, West Virginia, Tennessee, and Kentucky. In these states, the economic and political conditions were different from those in other regions. To help alleviate concerns that the control group is not representative of the treatment group of states, I create a second control group consisting of states that legalized marijuana after the end of the sample period and prior to 2021 (AZ, CA, IL, MA, ME, MI, NJ, NM, NY, NV, SD, VA, and VT). I then conduct a balance test between the treatment and the control groups. Table C.2 of the Supplementary Material reports the summary statistics and differences in observable characteristics of the treatment and control groups.

The balance test shows no statistically significant differences in borrower FICO score, prior bankruptcy, vehicle book value, down payment, loan term, yield spread, or labor participation rate. However, the table does show that incomes were \$213 per month higher in the treatment group, and the unemployment rate was slightly lower (0.86 percentage points).

Income is unlikely to drive the change in default rates, for 2 reasons. First, the specifications directly control income. Second, the income differences are persistent, as shown in Figure C.3 of the Supplementary Material. The figure presents borrower monthly income for each month across the states that first legalized the sale of marijuana and across the states that subsequently legalized marijuana, with

results. The coefficients on the legalization variable are significantly larger (2.7%) than in the comparable specification in Table 3. That is, marijuana legalization leads to a statistically significant reduction in opioid abuse, as measured by the drug-related death rate.

[10]In this analysis, I draw from 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 can easily access marijuana dispensaries across the state line.

dotted lines representing the adoption dates of legalized marijuana in Colorado, Washington, and Oregon.

Returning to Table 3, the results in column 2 show that marijuana legalization led to a 0.8% decline ($p < 0.01$) in drug-related death rates after legalization, relative to the control group of states that later legalized marijuana. Further, column 4 shows that the default rate in the treated states declined by 6.0%. These results support the findings from my initial test, in which the control group comprised all states that did not legalize recreational marijuana during the sample period.

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. A violation of the parallel trends assumption represents a threat to the identification. To investigate this potential threat, I compare differences in outcome pretends by plotting a difference in difference around the time that recreational marijuana was legalized.

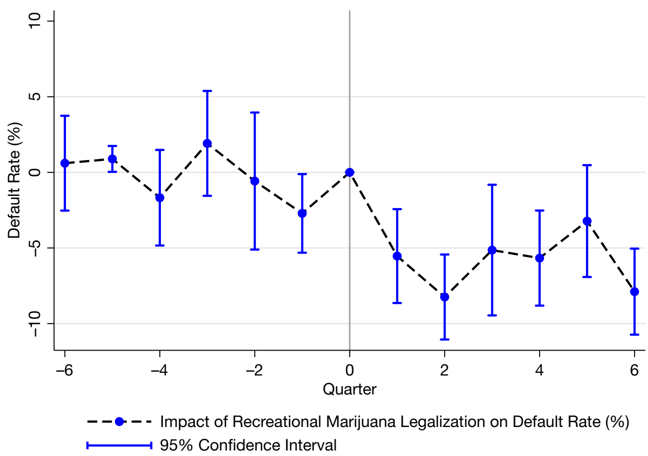
Figure 1 presents differences in the percentage of loans that default across states that legalized the sale of marijuana during the sample period and states that had not legalized the sale of marijuana during the sample period but did so subsequently. The figure shows a regression discontinuity for the date when marijuana legalization was implemented and is centered on that date. The regression includes the same set of controls described in Table 3 and shows confidence intervals at 95%.

In the 6 quarters preceding the legalization of marijuana, the loan default rates in the treatment group were not different from those in the control group at the 95% level. The figure shows that the default rate, though noisy, appears to be centered

FIGURE 1

Impact of Recreational Marijuana Legalization on Auto Loan Defaults

Figure 1 presents differences in the percentage of loans that default across i) states that legalized the sale of *marijuana* during the sample period and ii) states that did not legalize the sale of marijuana during the sample period but did so subsequently. The figure shows a regression discontinuity for the date on which marijuana legalization was implemented and is centered on the legalization date. Controls include monthly income, credit score, prior bankruptcy, down payment, loan term, book value of the vehicle, unemployment rate, and labor force participation. Standard errors are clustered by calendar quarter and state. The figure includes confidence intervals at 95%.



around 0%. That is, the default rates in the prelegalization period are similar in the treatment group and the control group. In contrast, the default rate is consistently and significantly lower in the treatment group after the legalization of marijuana.

Another explanation for the observed relation between the drug-related death rate and marijuana legalization is that other regulations (e.g., laws restricting access to opioids) that were passed concurrently with marijuana legalization are driving the results. If this is happening, then the mechanism I attribute to marijuana is incorrectly identified, but the relation between opioid abuse and loan defaults remains valid. Appendix B of the Supplementary Material describes how the absence of important new opioid regulations in Washington, Oregon, and Colorado during my sample period is consistent with the inference that marijuana legalization explains the subsequent reduction in opioid abuse.

To further allay the concern that another regulatory change that occurred simultaneously with the implementation of marijuana legalization drives the relation between the drug-related death rate and loan defaults, I examine the relation between taxable marijuana sales and loan defaults.[11] Table C.3 of the Supplementary Material reports that, in the first 3 states with legal recreational marijuana, an increase in taxable marijuana sales leads to a decline in drug-related death rates relative to all other states (column 1) and to states that legalized marijuana after the sample period (column 2). Further, columns 3 and 4 show that increases in marijuana sales are correlated with reductions in the loan default rate. These results reaffirm the main results from Table 3.

As a final test of the identification strategy, I instrument the drug-related death rate with an indicator for marijuana legalization. Table 4 reports these results. Two concerns must be addressed in the use of the instrument. First, the instrument should be sufficiently correlated with the endogenous variable. In this case, the relevance condition is easily verifiable. Columns 1 and 2 show that a statistically significant variable drives the first-stage results. OLS regressions on the drug-related death rate on an indicator variable (POST_LEGALIZATION) for loans terminated in a state that implemented laws allowing recreational marijuana sales are statistically significant, whether the control group consists of all other states (column 1) or only of states that subsequently legalized marijuana (column 2). The table also reports the Cragg–Donald F-statistic (77.809 for the full sample, and 73.310 for the restricted sample) to test the significance of the instruments. In addition, the medical literature (e.g., Bachhuber et al. (2014)) provides strong evidence of the first-stage relation.

Columns 3 and 4 of Table 4 report a 2-stage least squares (2SLS) regression, in which an indicator for marijuana legalization is used as an instrument for opioid abuse. The coefficient estimate of the 2SLS analysis is similar to the one for the full sample, but is 1.5 percentage points (7.536 vs. 6.038) larger than the one in the difference-in-difference analysis (i.e., Table 3).

One concern about the instrument and its reliance on exogenous variation is that the exclusion restriction could be violated. If, for example, people who use

[11]Studies of the impact of medical marijuana laws on opiate addiction (Pacula, Kilmer, Grossman, and Chaloupka (2010), Pacula, Powell, Heaton, and Sevigny (2015), and Powell et al. (2018)) are consistent with the relation I find between opioid abuse (as proxied by the drug-related death rate) and marijuana legalization, and with my hypothesis that improved access to marijuana dispensaries is associated with a lower incidence of drug-related deaths.

TABLE 4

Loan Performance: 2-Stage Least Squares

Table 4 reports results from a 2-stage least squares regression. Columns 1 and 2 report the first stage: OLS regressions on the drug-related death rate on an indicator variable (postlegalization) of loans that were terminated in a state that implemented laws allowing the recreational sale of marijuana. Columns 3 and 4 report a 2-stage least squares regression, in which an indicator for marijuana legalization is used as an instrument for drug-related death rates. The sample includes all states (columns 1 and 3) or states that implemented laws allowing the recreational sale of marijuana after the end of the sample period and prior to 2021 (columns 2 and 4). Regressions include controls for the riskiness of the borrower (credit score, income, and prior bankruptcy), the loan (term, down payment), the vehicle (vehicle book value), and the environment (unemployment rate, alcohol death rate, labor market participation, and yield spread). State- and year-fixed effects are included as reported. To test the significance of the instruments, the Cragg–Donald F-statistic is reported. Robust standard errors, clustered by state year, are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	DRUG_DEATH_RATE		DEFAULT	
	1st Stage		2nd Stage	
	1	2	3	4
POST_LEGALIZATION	−1.104***	−0.796**		
	(0.359)	(0.385)		
DRUG_DEATH_RATE			4.415***	7.536**
			(1.675)	(3.625)
Borrower controls	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes
Vehicle controls	Yes	Yes	Yes	Yes
Environment controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sample	All	MJ legal	All	MJ legal
Adj. R^2	0.626	0.632		
No. of obs.	50,326	20,615	50,326	20,615
F-stat.	77.809	73.310		

marijuana become more responsible and better at bill paying, or if the economic environment improves as a result of marijuana legalization, then marijuana alone could account for the changes I observe in default rates. Marijuana policies could also impact the labor supply and other economic outcomes.

However, if the exclusion restriction is violated because legal marijuana positively impacts economic outcomes, then we would expect to see the effects of this not only among opioid users but in other populations, such as heavy drinkers. In unreported results, a placebo test shows that the alcohol death rate, unlike the drug-related death rate, does *not* decrease with the implementation of marijuana legalization. This finding helps to mitigate concerns that the effect I observe on loan defaults results from an unobserved local economic effect.

IV. 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 between otherwise similar borrowers who 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 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 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 model that predicts the likelihood of default for each loan in the data. Specifically, I regress the default rate of loans that originated prior to the height of the opioid epidemic (i.e., before 2012) against the borrower- and loan-specific characteristics, then use the coefficient estimates to predict a default rate for the loans originated after 2011. I interpret the predicted default rate as a composite measure of the riskiness of each borrower. Importantly, the measure reflects only information about the prospective loan risk 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 5 reports the coefficient estimates from ordinary least squares regressions on the loan default rates for loans originated after 2011 on the drug-related death 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 12.4% of the variation in default rates. Notably, when I add the lagged drug-related death rate (in column 2), the lender is able to predict 14.3% of defaults, a 15.3% improvement in the out-of-sample performance of the credit model. Next, I examine whether this result is robust to the inclusion of county-level income changes. Column 3 shows that the coefficient estimate is not

TABLE 5
Credit Modeling

Table 5 reports the coefficient estimates from ordinary least squares regressions on the loan default rates (reported as %) for loans originating after 2011 on the lagged drug-related death 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. All columns include controls for the unemployment rate and labor force participation rate. Column 3 includes a control for county per capita income ($ '0,000s). 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.

	Dependent Variable: DEFAULT		
	1	2	3
PREDICTED_DEFAULT_RATE	1.559***	1.514***	1.514***
	(0.041)	(0.042)	(0.042)
DRUG_DEATH_RATE		0.370***	0.370***
		(0.086)	(0.086)
UNEMPLOYMENT_RATE	−4.817***	−4.737***	−4.744***
	(0.518)	(0.542)	(0.543)
LABOR_FORCE_PARTICIPATION	−1.866***	−1.837***	−1.833***
	(0.626)	(0.659)	(0.660)
COUNTY_INCOME			0.359
			(1.332)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
No. of obs.	19,617	19,286	19,286
Adj. R^2	0.124	0.143	0.143

statistically different. Moreover, the predictive power of the lender model is unchanged.

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 i) have high rates of interest and ii) allow lenders to easily repossess the 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 5, 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 6 reports the coefficient from an OLS regression of the *predicted* payments on the *realized* payments. Columns 1–3 report the coefficients for loans terminating before 2012 for counties sorted by drug-related death rate tercile. Based on observable characteristics, the lender is able to predict 21.5% (column 1) of the variation in payment for low opioid exposure areas and 24.5% (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 not statistically different from 1.0 for the middle ($p < 0.10$) and high terciles ($p < 0.10$).

Columns 4–9 of Table 6 report the out-of-sample performance of the predicted payment rate that was trained on the loan data terminating before 2012. The results show that after 2011, the power of the traditional loan characteristics in predicting payment rates declines precipitously in counties in the highest tercile of drug-related death rate: The out-of-sample performance of the credit model declines by 24% in these areas but does not drop in less affected areas. In fact, for the areas in the lowest (middle) tercile of drug-related death rate, the predictability of out-of-sample loan performance generally improves after the great financial crisis, as evidenced by a 33% (6%) gain. The improvement in model predictability is consistent with reports, from lenders, that the predictive power of risk models improved as the financial crisis revealed more risk data on prospective borrowers. This contrasts sharply with the poor model performance in counties that had the highest tercile of opioid abuse as proxied by the drug-related death rate.

In the next set of tests, I investigate how the addition of data on drug-related deaths improves lenders' ability to predict loan repayment. Columns 7–9 in Table 6 report these results. Adding data on drug-related deaths to the payment model increases the R^2 of the out-of-sample payment-prediction model by 19% in the highest tercile of drug-related death rates (column 9). In contrast, the addition of drug-related death rate data provides only minor improvements in R^2 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 not in more strongly affected areas. In the more strongly affected areas, new models that capture an "opioid risk factor" may be needed.

V. 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 i) all payments

TABLE 6

Loan Payment and the Opioid Crisis

Table 6 reports the coefficient estimates from ordinary least squares regressions on the total loan payments made by borrowers for i) loans terminating before 2012 (sample columns 1–3), and ii) loans originating after 2011 (sample columns 4–9). Columns represent terciles (high, medium, and low) of the county-level drug-related death 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 for the unemployment rate and labor force participation rate, 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.

	Dependent Variable: TOTAL_PAYMENTS_TO_LENDER								
	Precrisis			During Opioid Crisis: Using Model Trained on Pre-2012 Data					
	1	2	3	4	5	6	7	8	9
PREDICTED_PAYMENT_RATE	0.908***	1.016***	1.009***	0.131***	0.153***	0.107***	0.129***	0.153***	0.112***
	(0.036)	(0.067)	(0.057)	(0.022)	(0.025)	(0.019)	(0.022)	(0.025)	(0.021)
DRUG_DEATH_RATE							156.492*	19.601	64.900***
							(88.465)	(46.000)	(16.151)
UNEMPLOYMENT_RATE	531.733***	381.719***	440.177***	−869.708***	−694.248***	−545.225**	−860.636***	−696.775***	−541.582***
	(71.177)	(47.689)	(55.967)	(88.396)	(129.940)	(217.267)	(87.497)	(129.278)	(176.951)
LABOR_FORCE_PARTICIPATION	−8.901	−12.359	−84.945**	−26.428	−14.812	−21.913	−9.458	−13.408	−55.051*
	(23.543)	(26.376)	(41.434)	(29.709)	(38.963)	(24.215)	(31.909)	(38.687)	(28.477)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	No	No	No	No	No	No	No	No	No
Drug death rate tercile	Low	Middle	High	Low	Middle	High	Low	Middle	High
No. of obs.	8,384	8,007	7,469	9,798	9,693	9,014	9,798	9,693	9,014
Adj. R^2	0.215	0.192	0.245	0.285	0.202	0.186	0.295	0.202	0.221

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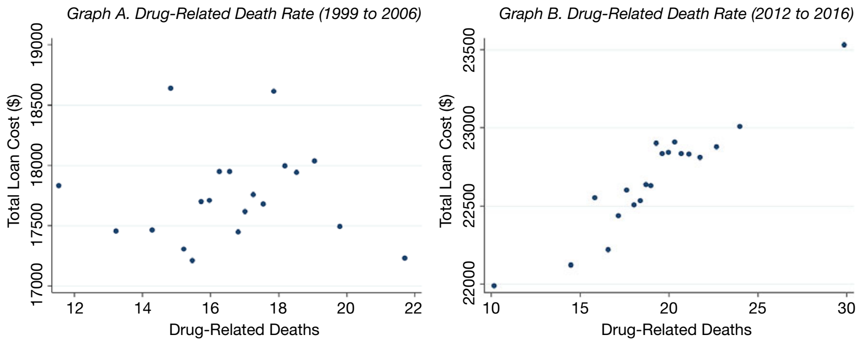
FIGURE 2

Auto Loan Costs and Drug-Related Death Rate

Figure 2 presents a binned scatter plot of the total loan costs versus the drug-related death rate for loans originated in (Graph A) the years 1999–2006 (the early stages of the opioid epidemic), and (Graph B) the years 2012–2016 (when opioid abuse was widespread). The total loan costs represent i) all payments made by the borrower, ii) all costs arising from the repossession and sale of the vehicle, and iii) postdefault 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.



of principal, interest, and fees made by the borrower to the lender, ii) the loss of value and costs borne by the borrower for the repossession and sale of the vehicle, and iii) all payments made by the borrower arising from postdefault collections efforts.

Graphs A and B of Figure 2 are binned scatter plots of the total loan costs versus the drug-related death rate for the years 1999–2006 and 2012–2016, respectively.[12] The figures show a sharp change in the effect of opioid abuse on total loan costs over time. While Graph A of Figure 2 shows no significant relation between drug-related death rate and total loan costs during the early stages of the epidemic, Graph B of Figure 2 shows a strong positive correlation during the later stages.

The ordinary least squares regressions in Table 7 confirm the results described in the figures. I find that the higher opioid abuse rates (as proxied by the drug-related death rate) 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 a drug-related death. Between 2012 and 2016, however, borrowers residing in counties at or above the 75th percentile of drug-related death rates pay $1,394 more over the life of an average subprime auto loan, compared with buyers in counties at or below the 25th percentile. This represents a 5.7% 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.

In addition to paying significantly higher direct financial costs for loans, buyers in opioid-afflicted areas may also incur indirect costs that are not observable in the data. For example, consumers with a history of default or vehicle repossession

[12]To avoid any confounding effects of the Great Recession, the years 2007–2011 have been excluded from this analysis.

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TABLE 7

Opioid Abuse and Borrower Loan Cost

Table 7 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 drug-related death rate. Columns 1 and 3 report results before the financial crisis (1999–2007); columns 2 and 4 report results after the financial crisis (2012–2016). 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.

	Dependent Variable: TOTAL_LOAN_COST			
	1	2	3	4
DRUG_DEATH_RATE	−0.069	166.154***	−36.633	149.914***
	(45.728)	(23.378)	(53.545)	(20.370)
FICO_SCORE	−4.408***	−5.232***	−4.384***	−5.066***
	(0.476)	(1.153)	(0.444)	(1.119)
MONTHLY_INCOME	378.396***	291.179***	375.618***	290.211***
	(53.254)	(14.008)	(53.361)	(14.192)
PRIOR_BANKRUPTCY	−273.151***	−91.140	−259.940***	−57.931
	(75.295)	(58.129)	(79.320)	(59.360)
DISCOUNT	−0.529***	−1.009***	−0.512***	−1.019***
	(0.065)	(0.043)	(0.062)	(0.045)
VEHICLE_BOOK_VALUE	904.570***	1,061.949***	907.864***	1,064.154***
	(21.811)	(10.959)	(22.025)	(11.119)
TERM	78.117***	93.130***	76.118***	93.284***
	(10.617)	(5.142)	(10.622)	(5.085)
YIELD_SPREAD	−68.011	−130.232***	−49.282	−131.857***
	(58.078)	(41.782)	(56.243)	(41.069)
ALCOHOL_DEATH_RATE			340.149***	302.886***
			(90.976)	(47.075)
LABOR_FORCE_PARTICIPATION			172.201*	−60.394
			(103.395)	(85.794)
UNEMPLOYMENT_RATE			−173.253	47.173
			(107.453)	(72.496)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sample	1999–2007	2012–2016	1999–2007	2012–2016
No. of obs.	45,737	48,499	45,737	48,499
Adj. R^2	0.508	0.594	0.514	0.598

may face especially high-interest rates on future loans or be unable to secure credit. Moreover, borrowers without access to alternative transportation may be unable to commute to their workplaces. If this is the case, then the total realized cost described in this study only captures a fraction of the costs incurred.

VI. Conclusion

While several studies have examined the economic impacts of the opioid epidemic (e.g., increases in mortality and medical expenses, and decreases in labor participation and productivity), this is the first article 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 drug-related deaths, I examine the relation between the opioid epidemic and auto lending and find evidence that opioid abuse is an empirically relevant explanation for higher loan default rates. I identify these results through a natural experiment involving 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 drug-related death rates and loan default rates. While the mechanism underlying loan defaults is unobserved, 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), and Gul and Pesendorfer (2007)). Such choices are likely to be unconducive to servicing consumer debt.

The results of 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 Tables 5 and 6 suggest that lenders find it more difficult to assess the creditworthiness of borrowers in areas strongly affected by the opioid crisis. If lenders cannot predict which borrowers are at risk of using opioids, then the 20 million borrowers in markets with high opioid use will pay more for their loans, as shown in Table 7. 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.

This article 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 i) loan securitization and ii) the impact of the opioid epidemic on the supply of consumer finance in afflicted areas.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109022001399.

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