Opioid Crisis and Real Estate Prices

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Abstract

We study the impact of opioid abuse on real estate prices. We document that opioid death rates and excess prescription rates are negatively associated with house prices. Exploiting the staggered passage of opioid-limiting legislation, we find that a decrease in opioid abuse results in higher county-level house prices. This effect is due to fewer mortgage delinquencies, lower vacancy rates, more home improvement loans, and increased population inflow. Our findings are consistent with improved real estate conditions and a rise in local demand. These results highlight the importance of public health policy in mitigating the economic costs of the opioid epidemic.

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I. Introduction

Opioid usage in the United States has surged over the past two decades, resulting in nearly 500,000 deaths from overdoses between 1999 and 2019, according to the Centers for Disease Control and Prevention.[1] The National Institute on Drug Abuse estimates that, in 2017 alone, 1.7 million Americans suffered from substance abuse related to prescription opioid pain relievers, with documented public health and economic consequences.[2] While the literature has focused on analyzing the role of economic conditions in the opioid crisis and ”deaths of despair” (Case and Deaton, 2015; Finkelstein, Gentzkow, Li, and Williams, 2022), fewer studies have examined the impact of the opioid crisis on the real economy (Ouimet, Simintzi, and Ye, 2025; Harris, Kessler, Murray, and Glenn, 2020; Cornaggia, Hund, Nguyen, and Ye, 2022). We contribute to the understanding of this problem by estimating the impact of opioid abuse on real estate values.

Opioid abuse degrades human capital and hurts families, among the noncollege-educated population in particular (Harris et al., 2020; Alpert, Evans, Lieber, and Powell, 2021). Prolonged drug use can lead to reduced labor productivity, lower household income, and increased likelihood of job loss, resulting in mortgage delinquencies. This, in turn, can degrade home quality and area attractiveness, as residents may opt to invest less in their properties. Understanding the impact of opioid usage on home values is important, as homes are often the most valuable asset held by many households (Favilukis, Ludvigson, and Van Nieuwerburgh, 2017). When home prices stagnate or fall, people’s financial situation can suffer, as rising home equity can alleviate financing issues and facilitate access to credit, promoting entrepreneurship

[1] www.cdc.gov/drugoverdose/epidemic/index.html

[2] www.drugabuse.gov/drug-topics/opioids/opioid-overdose-crisis

and job creation (Mian and Sufi, 2011; DeFusco, 2018; Jensen, Leth-Petersen, and Nanda, 2022; Adelino, Schoar, and Severino, 2015).

We start by documenting a negative association between opioid abuse and home values over a five-year period. To estimate the sensitivity of home values to opioid abuse, we measure values at the county level using the Zillow Home Value Index (ZHVI) and opioid abuse in two ways. First, we follow Cornaggia et al. (2022) and use the opioid death rate, which corresponds to the number of opioid-related deaths per 100,000 people at the county level. Second, we construct a new measure of excess opioid prescriptions that corresponds to the residual of a regression of prescription rates on variables that plausibly capture legitimate usage of these medicines (cancer rate, prevalence of hospice admissions, and ambulatory surgery).

Exploiting within-county (over time) variation, we find that the change in house price growth over five years is between 0.23 percentage points to 1.37 percentage points lower for a one standard deviation increase in opioid death rates and excess prescription rates, respectively. The upper bound of the estimated effect across the three opioid abuse measures (opioid death rates, excess prescription rates and prescription rates) corresponds to 23.5% relative to the mean percentage change in home values over five years. To understand how the most affected areas compare to the least affected ones, we also examine a P5 to P95 interpercentile range change in opioid abuse. A P5 to P95 change in excess prescription rates is associated with 4.26 percentage points less house price growth over five years, which corresponds to 55.5% relative to the mean percentage change in home values. For an average home in our sample, these estimated differences translate into a dollar value differential of up to $6,100. As a benchmark, the monthly income for the median household in the United States in 2018 was $5,265.

These associations could be driven by observed and unobserved confounding effects, such as local economic conditions. Hence, we continue our analysis by exploiting variation in opioid abuse induced by the staggered adoption of laws aimed at reducing opioid abuse. Since 2016 and in response to the opioid crisis, several U.S. states have passed laws or implemented regulations limiting opioid prescriptions to address prescription misuse, drug abuse, and overdoses. These measures generally aim to restrict the duration of prescriptions or total dosage, in particular for first-time prescriptions, to prevent overly generous prescriptions and subsequent addiction. Their staggered adoption by different states arguably induces exogenous variation in opioid abuse, as most evidence suggests that supply drives abuse (Finkelstein et al., 2022) and demand does not have the same effect (Currie, Jin, and Schnell, 2019; Paulozzi, Mack, and Hockenberry, 2014).[3]

We provide a difference-in-differences (DID) estimate, in which we compare the changes in home values in years before and after the passage of one of these opioid-limiting laws (*the treatment*) in *treated* versus *control* counties. We first show that the passage of these laws reduced excess opioid prescriptions. We also document that the growth in overdose death rates declines, despite overdose death rates continue to increase after the passage of the laws, possibly due to an increase in usage of illicit drugs in the short run, as access to prescription drugs shrinks. This pattern suggests that the effects of these laws are likely to operate mostly at the extensive margin of consumption of prescription drugs, preventing new addictions. We then show that house values in *treated* counties increased on average upon the adoption of these laws. We document that house values increased by 0.42 percentage points more in the year of passage, 0.78 percentage points more in the first year after passage, and 1.76 percentage points more in the second year after

[3]Ouimet et al. (2025) show that the only variable that significantly predicts passage of these laws in the cross-section of states is the (age-adjusted) opioid overdose death rate, while economic conditions or political economy do not seem to play a role. We find similar evidence when replicating their analysis.

passage. This effect corresponds to an increase in home value of $4,274 over three years for the average house. Considering that the average home value in our sample is $143,150, this is economically meaningful. Importantly, we also evaluate our main identifying assumption: we show that states in which an opioid-limiting law passed and those in which one did not pass were on parallel trends in terms of home value changes before the laws' passage.

Since our *treatment* is at the state level, we interact the passage of the state laws with measures of opioid supply at the county level to measure treatment intensity at the same level as the observed outcome. First, we establish that the decline in excess prescriptions and the growth rate of opioid deaths in treated states is primarily driven by counties with the highest opioid supply, as proxied by the number of physicians per capita and by opioid-related payments to physicians by pharmaceutical companies.[4] Second, we find that home values rise significantly more in these counties upon the passage of an opioid-limiting law. We similarly show that the impact of the reduction in opioid abuse and the growth in house prices were more pronounced in counties with higher opioid abuse. Taken together, these results suggest that variation in opioid abuse mostly drives the observed change in home values at the county level and not the other way around.

To explore the possible drivers of the link between opioid abuse and home values, we study the effect of opioid usage on delinquent mortgages, vacancy rates, and home improvement loans. Delinquent mortgages have been shown to affect home values and could generate negative price spillovers to nondistressed neighboring houses (e.g. Gupta, 2019; Campbell, Giglio, and

[4]Finkelstein et al. (2022) show that number of physicians per capita significantly predicts the propensity to prescribe opioids at the county level, while Engelberg, Parsons, and Tefft (2014) find similar evidence in case of opioid-related payments to physicians by pharmaceutical companies.

Pathak, 2011; Anenberg and Kung, 2014). We show that the rate of change in mortgage delinquency rates was about 6.66 percentage points lower on average one year after the passage of the opioid-limiting laws in *treated* counties, relative to the control group. We also find that the relative percentage of home improvement loans increased, while residential vacancy rates decreased significantly more one year after the passage of the laws in *treated* counties, relative to the control group.

Next we show that the passage of the opioid-limiting laws led to an increase in (high-income) migration inflow, with the effect becoming most pronounced two years after the laws' enactment. Poverty driven by opioid abuse can degrade local real estate quality and make local areas less attractive, reducing in-migration. By curbing opioid abuse, these laws may have helped stabilize or reverse economic decline, making communities more appealing to new residents, particularly relative to areas where opioid abuse continued unabated. Consistent with individuals needing time to update their expectations to respond to changing conditions, the effect grows over time. Taken together, our findings suggest that the opioid epidemic affects home values through two channels: changes in local real estate quality and shifts in demand for space.

We use two additional approaches to study the impact of opioid abuse on house values. First, we use a spatial regression discontinuity design, where we compare border counties in states that passed opioid-limiting laws with neighboring counties in states that did not see a change (i.e., counties on the other side of a state line). Our results are consistent with the difference-in-differences estimates. In the second approach, we follow Cornaggia et al. (2022) and use instrumental variables to estimate the impact of opioid abuse on home values. We instrument opioid abuse in two ways. The first is based on the aggressiveness of Purdue Pharma's marketing of Oxycontin, which induced overprescription.[5] The second is based on ”leaky” supply chains and the desirability of the product for patients when compared to less available and less attractive pain killers. Overall, the findings using the instrumental variable approach support our baseline results.

Estimating aggregate economic effects is challenging in the absence of a general equilibrium model. However, our simple back-of-the-envelope calculation suggests that total home value increased by $68.9 billion in the 32 states that enacted opioid-limiting legislation relative to control states in the years of the laws' passage alone. This estimate is based on a total home value base of $16.4 trillion in these states in the year before the laws took effect and a 0.42% increase in house prices. For comparison, Purdue Pharma's 2022 settlement agreement required the company to pay $6 billion to several U.S. states as compensation for damages related to the opioid crisis.

Our paper contributes to the rapidly growing literature on the impact of the opioid crisis on the U.S. economy. Harris et al. (2020), Van Hasselt, Keyes, Bray, and Miller (2015) and Florence, Luo, Xu, and Zhou (2016) study the impact of the opioid epidemic on human capital. They show a negative impact of opioid prescriptions on labor supply and quantify the costs associated with lost labor productivity. Agarwal, Li, Roman, and Sorokina (2023), Cornaggia et al. (2022), Ho and Jiang (2021), Li and Zhu (2019), Li and Ye (2024), and Jansen (2023) document financial effects of the crisis, including the impact of opioid abuse on municipal bond rates, firms' stock prices, banks' deposit growth, and consumer credit. Our paper focuses on the

[5]Purdue Pharma L.P., was an American privately held pharmaceutical company founded by John Purdue Gray. It was sold to Arthur, Mortimer, and Raymond Sackler in 1952, and then owned principally by the Sackler family and their descendants. The company manufactured pain medicines such as hydromorphone, fentanyl, codeine, hydrocodone and oxycodone, also known by its brand name, OxyContin.

impact of the opioid crisis on the price of real estate, which is a key indicator of the local economy and many household's most valuable asset.

A few related papers also investigate the relationship between the opioid crisis and the local real estate market. Karimli (2022) and Luo and Tidwell (2025) study the implications for the mortgage market and find that lenders are less likely to approve loans from areas with higher rates of opioid abuse. Karimli (2022) further suggests that the opioid epidemic depressed local house prices, resulting in more defaults using a sample between 2004 and 2017. Ho and Jiang (2021) document a positive impact on real estate prices in California between 2010 and 2018 following a regulation that aimed to limit opioid overuse, but they do not analyze the channel. D' Lima and Thibodeau (2023) use prescription data from Ohio between 2006 and 2012 to document a negative association between home values and opioid usage. We summarize the main results, area, and period of study as well as the identification strategy of these papers in Internet Appendix Table A1. While this body of literature focus on local real estate markets, our paper looks at real estate prices as main outcome variable in a nationwide setting and sheds light on the causal impact of the opioid crisis on real estate prices at this scale. Importantly, we cover all three stages of the opioid crisis with our sample period from 2006 to 2018, where different types of opioids played different roles.

More broadly, we contribute to the literature that examines the effects of public health conditions and resulting regulations on real estate and housing markets. Davis (2004) studies the elasticity of housing values with respect to cancer health risks, while (Tyndall, 2021) and Conklin, Diop, and Li (2020) assess the impact of marijuana dispensaries on nearby house prices. A nascent line of research also investigates the effects of public health crises, such as the COVID-19 pandemic, the 2003 Hong Kong SARS epidemic, or the 7th century Amsterdam plague, on housing markets (e.g. Gupta, Mittal, Peeters, and Van Nieuwerburgh, 2022; Wong, 2008; Francke and Korevaar, 2021). We contribute to the literature by studying whether the economic consequences of opioid abuse can be reversed by opioid-limiting regulation and, if so, how quickly. We find that, following the passage of an opioid-limiting law, excess prescriptions decrease, the growth of overdose death rates slow, and house prices increase in treated states.

II. Data

A. Measuring opioid abuse at the county level

A key step of our empirical analyses is measuring opioid abuse, i.e. the excessive or improper use of opioid drugs, either prescription or illicit, in a way that leads to harm, dependence, or addiction.[6] First, we follow Cornaggia et al. (2022) and measure opioid abuse through mortality. Because opioid mortality captures the worst consequences of addiction, we also use opioid prescriptions and a measure of excess prescriptions. While opioid prescriptions may be initially intended for legitimate purposes, opioids have been excessively prescribed, and Alpert, Powell, and Pacula (2018) show that nonmedical use is highly correlated with oxycodone supply and OxyContin prescriptions. Following this line of reasoning, we introduce a new measure ”excess prescriptions” to better capture opioid prescriptions that are not likely medically necessary.

For opioid mortality, we license the restricted All-County Mortality Micro data from the Centers for Disease Control and Prevention (CDC). These data record the precise cause of every

[6]We discuss the background of the opioid crisis in Internet Appendix Section A

death in the United States by county, allowing us to identify all opioid-related deaths in each county.[7] Following Cornaggia et al. (2022), we construct the variable *OpioidDeathRate*, which is the number of opioid-related deaths scaled by the county's population (in 100,000s). We describe the exact steps and death cause codes used for identification of opioid-related deaths in the Internet Appendix 2.

Opioid mortality data have advantages over opioid prescription data. First, mortality statistics represent the entire population, while prescription data may not. Second, opioid mortality data are less influenced by such factors as travel for treatment and prescriptions. Additionally, mortality data are unaffected by legitimate use of opioids. However, it's important to note that opioid mortality, as an extreme outcome, is a low-frequency measure and only captures the worst cases of abuse. It provides a lower bound for opioid abuse since people can misuse opioids without fatally overdosing and yet can still endure significant health and social consequences. Therefore, we also collect historical data on opioid prescriptions for a more comprehensive understanding.

For total prescriptions and excess prescriptions, we use data from the CDC, which reports county-level opioid prescriptions sourced from IQVIA Xponent starting in 2006. IQVIA Xponent collects opioid prescriptions as identified by the National Drug Codes from approximately 49,900 retail (nonhospital) pharmacies, which covers nearly 92% of all retail prescriptions in the United States. We construct the variable *PrescriptionRate*, which is the count of annual opioid prescriptions at the county level per 100 people.

[7] In contrast to the public-use CDC data, the restricted data provides opioid-related rather than drug poisoning deaths and is not left-censured for counties with fewer than 10 drug poising deaths (see Cornaggia et al. (2022) for a detailed discussion).

Last, we construct a new measure of excess prescriptions. This measure corresponds to the residual of a regression where the dependent variable is prescription rates and the explanatory variables aim to capture the prevalence of conditions that lead to legitimate opioid prescriptions. Dowell, Ragan, M, T, and Roger (2022) in the 2022 CDC clinical practice guidelines provide a list of conditions for which opioids can be called for. Opioids may be appropriate for managing short-term acute postoperative pain, for treating pain related to active cancer treatment, and for palliative or end-of-life care. We measure these dimensions through the cancer rate, hospice percentage, and ambulatory surgery percentage. *CancerRate* is the cancer incidence rate from the restricted access file from the National Cancer Institutes Survey of Epidemiology and End Results (SEER). As county information is only available for 21 states, we use the state cancer incidence rate from National Program of Cancer Registries and SEER for counties without information. *HospicePercentage* is the percentage of Medicare beneficiaries using hospice services, and *AmbulatorySurgeryPercentage* is the percentage of Medicare beneficiaries using ambulatory surgery centers. Both measures are collected from the Medicare Geographic Variation file and are available at the county level from 2007 onward. More formally, excessive prescriptions correspond to the residual of the following regression:

$$
\begin{aligned}
(1) \qquad PrescriptionRate_{c,t} = \quad & \alpha + \beta_1 CancerRate_{c,t} + \beta_2 HospicePercentage_{c,t} \\
& + \beta_3 AmbulatorySurgeryPercentage_{c,t} + \tau_t + \epsilon_{ct}
\end{aligned}
$$

We also include year fixed effects to account for time-varying trends in prescription use or illnesses across all counties. Ideally, we would like to estimate this model on a set of counties without opioid prescription abuse and then apply those coefficients to calculate excessive opioid prescriptions. Following this idea, we estimate this model for all counties, except counties with more than eight medical offices or pharmacies prescribing medically unnecessary opioids so called ”pill mills”; i.e., we drop the counties with the highest likelihood of excessive prescriptions and apply the estimated coefficients to the full population.[8] We report the coefficients of this regression in Internet Appendix Table A2. In line with our expectations, all three variables are generally positively related with opioid prescriptions.

Panel A in Table 1 reports summary statistics. The number of observations varies with the opioid abuse proxy because the data coverage differs across measures. The average opioid death rate is 7.4 per 100,000 residents, and the average prescription rate corresponds to 83.5 opioid prescriptions per 100 residents. The average excess prescription rate is 1.1 and thus larger than 0, which suggests that the excluded pill mill counties do have excess prescriptions.

[Insert Table 1 approximately here]

B. County house prices, demographics, and economic conditions

To measure average annual value of a typical house within a county, we use the 2019 revision of the Zillow Home Value Index (ZHVI). This smoothed, seasonally adjusted measure incorporates property hedonic characteristics, location, and market conditions for more than 100 million US homes, including new construction and nontraded homes, to compute the typical value for homes in the 35th to 65th percentile within a county. We calculate one- to five-year percentage changes in home values. The average home value across counties was $143,150 between 2006

[8] See Section 3 for detailed definition of pill mill counties and how we identify them.

and 2018 and grew by 1.4% over one year and 5.4% over five years with considerable cross-sectional variation (compare Panel B in Table 1).

We collect additional county demographic and economic variables for our analysis. Demographic variables include male population ratio, White population ratio, Black population ratio, Indian American population ratio, Centers for Disease Control and Prevention population ratio, age 20-64 ratio, over age 65 ratio, and migration flow; these are obtained from the Census Bureau. Cancer (i.e., neoplasms) mortality is obtained from the CDC. The number of primary care physicians, excluding hospital residents or those age 75 years or over, is obtained from the Area Health Resources Files of the Health Resources & Service Administration. Economic variables include poverty ratio and median household income, obtained from the Census Bureau, as well as the unemployment rate and the labor force participation rate, obtained from Bureau of Labor Statistics. These variables are normalized by contemporaneous county population and winsorized at the 2% and 98% levels. All variables are defined in Internet Appendix Section 1.

C. Opioid-limiting state laws

Starting with Massachusetts in 2016, several states passed laws or regulations to limit opioid prescriptions.[9] We collect information on the laws and their year of the passage.[10] Including Massachusetts, nine states passed legislation that limited opioid prescriptions in 2016,

[9]We consider both laws and regulations, as they are similar in their restrictions and both legally binding. We refer to them jointly as laws. If multiple laws were passed by both the house and the senate in a state, we consider the year the first law passed, as that initiated the restrictions. Laws differ in their level of restrictions. However, all laws, even if a second law was passed, limit opioid prescriptions.

[10]For an overview of the laws see Internet Appendix D

while 18 states followed in 2017, and another five in 2018. Figure A1 depicts the treated states on a map, and Table A3 translates this into county observations.

To measure opioid supply side drivers at the county level, we use data on the number of primary physicians per capita and collect data on direct or indirect payments or other transfers of value made from pharmaceutical and medical device manufacturers and their distributors to physicians, nonphysician practitioners, and teaching hospitals. Data on physician opioid-related payments come from the Centers for Medicare & Medicaid Services Open Payments database and cover August 2013 to December 2019.[11] To compute opioid-related physician payments by the manufacturers, we follow Fernandez and Zejcirovic (2018) and Hadland, Rivera-Aguirre, Marshall, and Cerdá (2019): we identify opioid related payments through the National Drug Code directory published by the US Food and Drug Administration, which includes information on the substance names included in drugs.[12] We then use the substance names to identify opioids following the Anatomical Therapeutic Chemical (ATC) Classification System of the WHO (ATC code N02A).[13] If a payment occurred for multiple drugs, we split the amount paid by the number of drugs promoted. We consider all payments made to physicians and teaching hospitals related to the identified opioid drugs. We identify the county of the physician or teaching hospital based on unique city and state combinations. If this is impossible, we use the zip code and assign the county based on the zip code centroid. Finally, we aggregate by county and year. Counties without payments related to opioid payments are set to 0, as the coverage is countrywide and no information therefore equals no payments.

[11] www.cms.gov/priorities/key-initiatives/open-payments/data

[12] www.fda.gov/drugs/drug-approvals-and-databases/national-drug-code-directory

[13] www.whocc.no/atc$_{d}dd_{i}ndex/?code = n02a$

III. Opioid abuse and home values

A. County-level correlation

We begin by documenting the correlation between home values and opioid abuse. We exploit within-county variation as well as within state-year variation. Figure 1 presents county-level heat maps of average five-year lagged opioid death rates and average five-year percentage changes in home values over the sample period. The maps show that counties in the bottom quintile of percentage change in home values overall correspond to counties with the highest opioid death rates, suggesting a negative correlation in the cross-section between opioid abuse and home values.

[Insert Figure 1 approximately here]

We further examine this relationship by estimating the following specification:

$$PCHomeValue_{c,t-x\ to\ t} = \alpha + \beta OpioidAbuse_{c,t-x} + \gamma \boldsymbol{X}_{c,t-x} + \theta_c + \tau_t + \epsilon_{c,t} \tag{2}$$

The dependent variable $PCHomeValue_{c,t-x\ to\ t}$ in equation 2 is the log percentage change of average county c home values, ($log(HV_t/HV_{t-x}) * 100$) over $X = \{3, 4, 5\}$ years. $OpioidAbuse_{c,t-x}$ captures one of the three opioid abuse measures, i.e., opioid death rates, excess prescriptions, and opioid prescription rates, for county c at $t - x$. We also include a vector of time-varying county-level controls $\boldsymbol{X}_{c,t-x}$, measured with a lag at time $t - x$. Following Ouimet et al. (2025), county-level controls measured at $t - x$ include male population ratio, White population ratio, Black population ratio, American-Indian population ratio, Hispanic population ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and number of physicians per county. We include county fixed effects θ_c, and control for general macroeconomic conditions by including year fixed effects τ_t. In addition, in a separate specification, we use state-year fixed effects $\zeta_{s,t}$ instead of θ_c and τ_t to control for time-varying local market conditions.

Table 2 reports the results of estimating Equation 2 and shows robust statistically significant negative correlations between home values and each opioid abuse measure.[14] Columns 1 to 3 include county fixed effects θ_c and year fixed effects τ_t, whereas we use state-year fixed effects $\zeta_{s,t}$ in columns 4 to 6. Starting with opioid mortality in Panel A, we observe that the negative association persists across the different time horizons and strengthens in the long run.[15] The correlation between opioid death rates and five-year percentage change in home values is estimated at -0.029 (-0.026), when exploiting within county (state-year) variation. A one standard deviation increase in opioid death rates (7.912) rates translates into a 0.23 (0.21) percentage point reduction in home value growth rates, which is equivalent to 4.08% (3.77%) of the five-year average percentage home value increase (5.54%).

Panels B and C show the results for opioid prescriptions.[16] The estimated economic magnitude is larger for the excess prescription and prescription rates. Point estimates obtained from within-county variation for the correlation between excess prescriptions (opioid prescriptions) and the five-year percentage change in home values are -0.029 (-0.028). A one

[14]Internet Appendix Table A5 shows that our results are robust to dropping ”pill mill” counties

[15]The number of observations varies across regressions, as longer lags reduce the number of available periods.

[16]The number of observations differs across each panel, because data coverage varies by opioid abuse measure. We use the maximum sample possible in each regression.

standard deviation increase in excess prescription rates (opioid prescriptions), 46.59 (46.42) translates into a 1.37 (1.29) percentage point reduction in the home value growth rate, which is equivalent to 17.78% (23.49%) of the five-year average percentage of home value increase (7.68% and 5.50%, respectively).

We also consider the P5 to P95 interpercentile range in excess prescription rates to capture the variation between counties that are more and less affected by opioid abuse. Focusing on within-county variation, we find that a P5 to P95 interpercentile range change is associated with a 4.26 percentage point difference in house price growth over five years. This corresponds to 55.5% relative to the mean percentage change in home values. For an average home in our sample, these estimated differences translate into a dollar value differential of up to $6,100. As a benchmark, the monthly income for the median household in the US in 2018 was $5,265.

The spatial patterns from the map as well as the correlations are consistent with opioid abuse affecting home prices. We next exploit some plausibly exogenous variation from the passage of opioid-limiting state laws. In the subsequent analyses, we focus only on opioid mortality and excess prescriptions as the more precise measures of opioid abuse.

[Insert Table 2 approximately here]

B. Adoption of opioid-limiting state laws: difference-in-differences estimates

1. Impact on opioid abuse

In this section, we exploit variation in opioid usage induced by the staggered adoption of state laws limiting prescriptions to estimate the impact of opioid abuse on home values.

The aim of the laws was to limit unnecessary first-time prescriptions and therefore avoid those prescriptions leading to long-term opioid addiction. Yet the impact of the passage of these laws is unclear. The laws may have impacted both the intensive margin, i.e., already users of prescription opioids, and the extensive margin, i.e., new users. On the one hand, reducing first-time prescriptions should reduce the likelihood of addictions and therefore reduce long-term opioid abuse in these states. On the other hand, the impact on the intensive margin is less clear. Reducing access to prescription opioids may worsen opioid abuse if it diverts demand to heroin and illegally manufactured opioids. Although we do not directly observe the intensive and extensive margins, we consider as dependent variables the opioid death rate, the prescription death rate, the one-year difference in opioid death rate, and excess absolute prescriptions. The opioid death rate is defined as in Section A and includes both opioid deaths related to illicit and prescription opioids. The prescription death rate is the number of prescription opioid-related deaths per 100,000, similar to the opioid death rate. The one-year difference in opioid death rates is simply the first difference in opioid death rates. Excess absolute prescriptions is the residual of absolute opioid prescriptions, similar to the excess prescription rate. We start by examining the link between the passage of the laws and opioid abuse to establish the effectiveness of the laws. We implement a difference-in-differences framework to compare changes in county-level opioid abuse in years before and after the passage of the laws (*the treatment*) in *treated* versus *control* counties. We run a regression with lead and lag dummies relative to the year of the passage to establish the path of county-level opioid abuse before and after a law.

We follow the Sun and Abraham (2021) approach to estimate cohort-specific average treatment effect on the treated $(CATT(e, \ell))$, ℓ periods from initial treatment for cohort first treated at time e. The control group therefore consists of never-treated counties, i.e., counties in states that did not pass opioid-limiting legislation. Standard errors are clustered at the state level, as the laws were introduced at the state level. Our baseline specification to estimate the impact of the passage of the laws on opioid abuse across time and states therefore is:

$$(3)\ OpioidAbuse_{c,t} = \alpha + \sum_{e \in \{16,17,18\}} \sum_{l=-5, \neq -1}^{2} \delta_{e,l} \mathbf{1}\{E_i = e\} D_{ct}^{\ell} + \gamma \boldsymbol{X}_{c,t-1} + \theta_c + \tau_t + \epsilon_{c,t}$$

All opioid abuse variables are measured at the county c and year t level. τ_t and θ_c are time and unit fixed-effects, representing calendar year and county fixed effects. $D_{i,t}^{\ell}$ are relative period indicators, which are equal to one for a county calendar year observation, where the time relative to the passage of the law statement matches the dummy statement and zero otherwise. For instance, the relative period dummy minus 2, $D_{i,t}^{-2}$, is equal to one for any county in calendar year 2014 that passed a law in 2016. As standard, we drop the relative period dummy ”minus 1” to avoid multicollinearity and focus on the change around the passage of the law. Sun and Abraham (2021) interact these standard lead lag dummies with cohort specific indicators; i.e. $\mathbf{1}\{E_i = e\}$. In our specification, there are three cohorts, with states (and thus counties) implementing opioid laws in 2016, 2017, and 2018. Thus, there are three dummies that are equal to 1 for counties that passed the law in the specific cohort year and zero for any other county. This approach allows us to estimate cohort-specific average treatment effects. We additionally include county controls as defined before.

We restrict t to 2013–2018 to focus on the years around the passage of the laws, with the first law being passed in 2016 and the last in 2018. Hence, for counties where a law passed in 2016, the relative period goes from ”minus 3” to ”plus 2.” For counties where a law passed in 2018, the relative period goes from ”minus 5” to ”plus 0.” Finally, we calculate the proposed interaction-weighted estimator by aggregating the cohort-specific coefficients across each relevant time by their sample share in the relevant period. Figure 2 plots the estimates of the total interaction weighted coefficient for each relative period with the 95% confidence interval.

Panel A shows that the opioid death rates increased in treated counties relative to control counties before the passage of the law and continued to increase afterward. In terms of opioid death rates, treated and control counties seem to be on different trajectories. This result is consistent with the Internet Appendix Table A4 and Ouimet et al. (2025) documenting that the only variable that significantly predicts the passage of these laws in the cross-section of states is the (age-adjusted) opioid overdose death rate, while economic conditions or political economy are insignificant.

Because the legislation targeted opioid prescriptions, we next zoom in on opioid deaths related to prescriptions. Panel B reports parallel trends for the prescription opioid-related death rate. In contrast to opioid death rates in general, we observe parallel trends up to four years before the passage of a law and a drop in prescription-related opioid deaths after passage. Panel C also shows a drop in the one-year difference in the opioid death rate following the passage of one of these laws. The parallel trends of the growth rate in opioid death rates suggest that treated and control counties were on similar paths when considering the growth rate in opioid deaths.[17] Lastly, Panel D suggests that there was also a pronounced drop in excess absolute prescriptions following the passage of an opioid-limiting law.

While the laws did not lead to an immediate drop in total opioid death rates, we interpret the evidence as consistent with a trend break in opioid death rates. First, opioid prescriptions and

[17] In Figure A2 in the Internet Appendix, we report results for illicit overdose death rates and the one-year difference in illicit overdose death rates. The patterns are similar to the patterns for total opioid death rates.

prescription deaths drop following the passage of a law. We then observe that the growth in opioid death rates slows, which suggests that the laws did break the trend of new addictions.

[Insert Figure 2 approximately here]

2. Impact on house prices

We next apply the same framework to compare the changes in county-level home values in years before and after the passage of the law in *treated* versus *control* counties.

$$(4)\, PCHomeValue_{c,t} = \alpha + \sum_{e \in \{16,17,18\}} \sum_{l=-5, \neq -1}^{2} \delta_{e,l} \mathbf{1}\{E_i = e\} D^{\ell}_{ct} + \gamma \boldsymbol{X}_{c,t-1} + \theta_c + \tau_t + \epsilon_{c,t}$$

where the dependent variable $PCHomeValue_{c,t}$ is a one-year percentage change in home values defined as in Equation 2. County controls are the same as in the previous specification. Figure 3 plots the estimates of the total interaction weighted coefficient for each relative time period with the 95% confidence interval. We report average treatment effect on the treated in Internet Appendix Table A6 and the full set of coefficients for each $CATT(e, \ell)$ in the Internet Appendix Table A7 .

Treated counties experienced a higher increase in home values, relative to untreated counties. Counties in states that passed a law saw their home values rise 0.42 percentage points more in the year of passage, 0.78 percentage points more in the first year afterward, and 1.76 percentage points more in the second year afterward relative to control counties.[18]

[18] Given that house prices represent the sum of the discounted cash flows these assets produce, we also estimate the effect of opioid abuse on rents. Internet Appendix Figure A3 shows that median county rents significantly increase two years after the passage of the opioid-limiting laws.

[Insert Figure 3 approximately here]

An identifying assumption in our analysis is that states in which a law has passed (*treatment*) as well as those in which one has not (*control*) are on parallel trends of home value changes before the passage of the law. As mentioned in Section 1 and documented in Internet Appendix Table A4, the only variable that significantly predicts the passage of these laws in the cross-section of states is the (age-adjusted) opioid overdose death rate. In contrast, economic differences, societal conditions or political economy are not associated with the passage of a law. This finding gives us confidence that it is likely that home value changes were on a similar growth pattern prior to the passage of opioid-limiting laws. Further, Figure 3 suggests that the parallel trends assumption is not violated.

These results suggest that the adoption of state laws limiting opioid abuse had a significant effect on the housing markets, resulting in an increase in home values.

3. County-level evidence

In our baseline results, the treatment variable is defined at the *state* level, while the outcome variable (home values) varies at the *county* level. In this section, we exploit county-level variation in opioid abuse and the propensity to dispense opioids *prior* to the passage of an opioid-limiting law to define the treatment variable at the same level as the outcome.

To begin, we exploit county-level variation in the propensity to dispense opioids *prior* to the passage of a law to define the treatment variable at the county level. As the laws limit opioid prescriptions, they should be particularly effective in limiting the onset of abuse and thus affecting house prices in counties that had a higher opioid-prescription propensity. We use two proxies for opioid supply at the county level. First, we follow Finkelstein et al. (2022), who show that the *number of physicians per capita* is positively correlated with opioid prescriptions and is an important supply factor for opioids. Second, we follow Engelberg et al. (2014) and use opioid-related pharmaceutical company payments to physicians as a proxy for physicians' propensity to prescribe opioids. We estimate the following standard two-way fixed effect regression with calendar year τ_t and county θ_c fixed effects.[19]

$$
\begin{aligned}
y_{c,t} = \;& \alpha + \beta_1 Post_{ct} + \beta_2 Post_{c,t} \times OpioidSupplyTop_c \\
& + \gamma \boldsymbol{X}_{c,t-1} + \theta_c + \tau_t + \epsilon_{c,t}
\end{aligned}
\tag{5}
$$

As dependent variables $y_{c,t}$, we consider the excess prescription rate, the one-year difference in the opioid death rate as well as home value changes. To account for different propensities to supply opioids within a state and therefore different impacts of the law at the county level, we construct an indicator variable, $OpioidSupplyTop_c$, that is equal to one for counties in the top half of the distribution within a given state based on the average number of physicians per capita between 2009 and 2013 or is equal to one for counties in the top half of the distribution within a given state based on total opioid-related payments to physicians between August 2013, the first month of data, and December 2015, and thus before the first passage of any state law. $Post_{c,t}$ is an indicator variable that is equal to one for the county-years following a law's

[19] In Internet Appendix Section 2 we execute a Goodman-Bacon (2021) decomposition to assess the extent to which the two-way fixed effect specification is plagued by "bad" comparisons, i.e., the weight and sign of *Later* versus *Earlier Treated* comparisons. We find that, while the bad comparisons may take on the opposite sign, their weight is small, typically less than 10%.

introduction. The coefficient of interest is β_2, which captures the intensity of the opioid-limiting laws on counties that were more exposed to opioid abuse, as proxied by likely opioid supply.

Table 3, Columns 1 and 3 show that the drop in opioid abuse, as measured by excess prescriptions and the one-year difference in the opioid death rate, following the passage of a law, was concentrated in the counties with the most physicians per capita, in line with Finkelstein et al. (2022)'s findings. This finding is echoed in Columns 2 and 4, where we proxy for opioid supply using opioid-related pharmaceutical company payments to physicians. While home values seem to increase following the passage of the laws across all counties, they were more pronounced in counties in the top half of the distribution of physician payments (Column 6). These results provide county-level evidence that opioid-limiting laws had the strongest home value effect in counties that were more exposed to the opioid crisis.

[Insert Table 3 approximately here]

As an alternative measure of county-level variation in opioid abuse, we consider the three-year average opioid death rates *prior* to the passage of a law, i.e., between 2013 and 2015. Opioid-limiting laws should have the strongest impact on home values in counties most exposed to the opioid crisis. The interaction term in regression 5 is now $OpioidDeathRateTop_c$, which is equal to one for counties in the top half of the distribution across the United States (within a give state) based on average opioid death rates. Table 4 reports the results. These results mirror the opioid supply interaction results in Table 3. The drop in opioid abuse, as measured by excess prescription and the one-year difference in the opioid death rate, was more pronounced in counties with higher opioid death rates following the passage of a law. We also observe that the growth in home values was more pronounced in counties with higher opioid death rates following the passage of a law.

[Insert Table 4 approximately here]

Following this line of reasoning, opioid-limiting laws should have little to no impact on opioid abuse and therefore house prices in counties where opioid abuse was low. We exploit this idea and use counties with low abuse as a placebo test. Specifically, we identify as placebo counties the ones that were in the lowest opioid abuse quintile based on average opioid death rates between 2013 and 2015. We restrict the sample to placebo counties and control counties and rerun specifications 3 and 4. We report the parallel trend plots in Internet Appendix Figure A4. Panels A and B highlight that there was no significant change in opioid abuse, the one-year difference in the opioid death rate, or excess absolute prescriptions, after the passage of a law. The introduction of the opioid-limiting laws did not seem to have any effect in these counties. Panel C highlights that there was also no significant change in home price growth following the passage of opioid-limiting laws in these counties. These results further corroborate our conclusion that the reduction in opioid abuse, following the passage of the laws, drove changes in house prices rather than unobservable differences across states.

C. Economic mechanisms

1. House market dynamics: delinquency rates, home improvement loans, and vacancy rates

The evidence presented in the previous section shows that opioid abuse affects home values. The decrease in home values can be driven by a reduction in household income and less ability to service a mortgage, which may lead to defaults and higher vacancy rates in the most affected areas. In less extreme cases, drops in home value might be due to lack of maintenance, reflected in fewer home improvement loans. In this section, we explore these channels.

We collect data on the percentage of mortgages delinquent by 90 or more days by county and month from the Consumer Financial Protection Bureau. The data comes from the National Mortgage Database and is aggregated at the county level. Ninety-day delinquency rates generally capture borrowers that have missed three or more payments and hence arguably capture more severe and persistent economic distress. The coverage of this measure is less extensive than our main data, covering only 470 counties. Delinquency rates are only reported for counties with a sufficient number of sample records to avoid unreliable estimates.

We also collect data on the number of home improvement loans from the Home Mortgage Disclosure Act and residential property vacancy rates from the United States Postal Service. We report summary statistics for these variables in Internet Appendix Table A8. We apply the same framework as in Equation 4 to compare the changes in delinquent mortgages, residential vacancy rates, and home improvement loans in years before and after the passage of a law (*the treatment*) in treated versus control counties.

[Insert Figure 4 approximately here]

Figure 4 plots the estimated coefficients for these channels. We find that the rate of change in mortgage delinquency rate is about 6.7 percentage points lower on average one year after the passage of the laws in *treated* counties, relative to the control group. Similarly, the rate of change in home improvement loans is up to 17.0 percentage points higher one year after the passage of a law, and the rate of change in the vacancy rate is as much as 8.6 percentage points lower one year after treatment.

These results suggest that the adoption of state laws limiting opioid abuse had a significant effect on the housing markets: It reduced the relative percentage of delinquent mortgages and vacancy rates while significantly increasing the number of home improvement loans, ultimately resulting in an increase in home values.

2. Migration

Motivated by the established association between opioid abuse and housing market metrics, in this section, we study the impact of opioid abuse on migration. Impoverishment from opioid abuse and the change in the quality of life in the area would make local areas less attractive and consequently reduce in-migration. Relatedly, the passage of the laws leading to a reduction in opioid abuse may facilitate in-migration, as the overall desirability of these areas improves.

We collect county-level inflow migration data from the Internal Revenue Service (IRS). The Statistics of Income Tax Stats estimate migration inflows based on year-to-year address changes reported on individual income tax returns filed. Three measures of migration inflow are calculated from number of returns filed, number of personal exemptions claimed, and total adjusted gross income. We define them as ”number of households,” ”number of individuals,” and ”total income,” in line with the IRS.

Figure 5 shows the results of estimating Equation 4, with county migration inflow as the dependent variable. Panel A shows the results with the natural logarithm of the total household income inflow, Panel B the natural logarithm of the number of households moving into the county, and Panel C the natural logarithm of the number of individuals moving into the county as the dependent variables. We can see that the treated counties experienced an inflow of (high-income) households following treatment, suggesting that positive income shocks bolstered housing demand. Across all three measures of migration inflow, the effect is more pronounced after two years, consistent with individuals having more time to update their beliefs and change their behavior.

[Insert Figure 5 approximately here]

D. Interpretation and discussion

The results from the previous section show that the passage of opioid-limiting laws is followed by a decrease in mortgage delinquencies and property vacancy rates as well as an increase in home improvement loans and population inflows. These effects are consistent with a decrease in defaults, an improvement in the quality of local real estate, and an increase in the local demand for space. This can be due to improving labor markets, as argued by Ouimet et al. (2025), or because of improvements in the area's quality of life and economic conditions (Dougal, Parsons, and Titman, 2015).

The results of our empirical analysis are also consistent with a "spatial externalities" argument à la Ambrus, Field, and Gonzalez (2020), according to which, if a negative shock to a county is severe enough, there is an outflow of (high-income) households and the county tips into an equilibrium with relatively low-income households. In the context of the cholera-outbreak in one neighborhood of nineteenth century London, Ambrus et al. (2020) model a rental market with frictions in which low-income households exert a negative externality on their neighbors. Similar to their setup, the opioid crisis affected people directly and not the local infrastructure (as would be the case with hurricanes or earthquakes).[20] In contrast to Ambrus et al. (2020), the opioid crisis affected the whole country, with varying treatment intensity across counties.

While the goal of the introduction of the laws was to limit first-time prescriptions and reduce long-term opioid addiction and abuse, an unintended consequence might have been to divert demand to heroin and illegally manufactured opioids. To shed light on this important unintended consequence, we test whether the passage of a law had stronger effects in counties where drug possession was more heavily prosecuted and the switch to illegal drugs was therefore likely harder. We collect county-level data on the number of cocaine and opium possession arrests from the Uniform Crime Reports of the Federal Bureau of Investigation. Internet Appendix Table A9 documents that counties with stricter drug enforcement regulation experienced a larger reduction in excess prescriptions and one-year difference in opioid death rate and a stronger increase in house prices.[21] These findings suggest that the extent of substitution to illegal drugs may have indeed played a role in shaping the laws' overall effectiveness and the impact on house prices.

Our preferred interpretation relies on the internal validity of our quasi-natural experiment. We have discussed in Section III the formal identifying assumption of parallel trends. We assume that states that adopted a law (*treatment*) and the ones that did not (*control*) are on parallel trends in terms of home values before treatment. Roth (2022) highlights that such a pretest may fail to detect preexisting trends that produce meaningful bias in the treatment effect. We follow Roth (2022) to identify whether our pre-test is likely to be effective. To assess that, we plot a linear

[20]Opioid abuse can still affect local infrastructure indirectly.

[21]We define counties with stricter drug enforcement regulation as those that are in the top half of the distribution of drug possession arrests per capita, both in raw terms and after controlling for drug possession covariates.

violation in Figure A5 with a hypothesized slope based on having 50% power, i.e., the probability of passing the pre-test is 50%. The estimated slope is 0.268, meaning that treated states' home values rise every year by 0.268 percentage points more relative to control states. Given a one-year average percentage change in home values of 1.40% and a standard deviation of 5.02%, we consider this an economically meaningful deviation. The likelihood ratio for this hypothesized trend is 0.461; i.e., the chance of seeing the observed pre-test coefficients under the hypothesized trend relative to under parallel trends is only about half. Further, the 95% confidence interval on the point estimate on percentage change in home value in $t = +2$, is outside of the expected coefficient (in blue) we would find based on the hypothesized trend. This result gives us confidence that our pre-test is reasonably effective.

We assume that no other regulatory changes have occurred simultaneously in treated states that could have influenced both opioid abuse and home values. Additionally, we assume there is no contamination between treated and control states, meaning that opioid users do not migrate from treated to control states. Furthermore, we assume that, at the state level, no other laws that could affect housing prices were enacted around the same time as the opioid prescription-limiting laws. These laws could include changes in local zoning, permitting, housing affordability initiatives, and other housing market regulations. We provide a list of relevant real estate market regulation changes in Internet Appendix I for the period from 2016 to 2018. Note that these regulations would need to have been passed simultaneously in the same states that enacted opioid prescription-limiting laws and must have impacted the housing market in the same direction to be a significant concern affecting our estimates. Since most of these laws aimed to increase housing supply–which could damp home price growth, assuming constant demand–this could result in our estimates being understated.

IV. Alternative Identification Strategies

A. State-border regression discontinuity design

In this section, we employ a spatial regression discontinuity (RD) design exploiting state borders. In our estimation, we compare counties located within a narrow distance from the state border under the assumption that border counties share otherwise similar general economic conditions. We define as *treated* counties located in the state that passed an opioid-limiting law. The border distance of treated counties is measured to the nearest county where no opioid-limiting state law was passed. Formally, we estimate the following model:

$$y_c = \beta Treat_c + \sum_{p=1}^{P}[\gamma_{p0} + \gamma_{p1} Treat_c] Distance^p] + \gamma \boldsymbol{X}_c + \epsilon_c \tag{6}$$

where y_c is a county-level outcome of opioid abuse or house prices. We consider a one- or two-year difference in excess prescription rates or opioid death rates as measures of opioid abuse and a one- or two-year percentage change in home values. We calculate the difference (percentage change), from the *treatment year - 1* to the *treatment year* for one year changes, and to the *treatment year + 1* for two year changes. For control counties, we calculate the difference (or percentage change) from 2015 to 2016 or 2017, as the first law passed in 2016. We therefore assume that there were no state-specific shocks in 2016 or 2017 and that control observations from 2015 are valid comparisons for later treated counties. $Treat_c$ is an indicator variable equal to one for counties in a state that passed an opioid-limiting law, and $\sum_{p=1}^{P}[\gamma_{p0} + \gamma_{p1} Treat_c] Distance^p]$ is a polynomial of order *P* (one or two) of the border distance (distance to the threshold). As controls, we include the following variables as of 2015: male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and the number of physicians. We follow Calonico, Cattaneo, and Titiunik (2014) to choose the optimal bandwidth, which in this case corresponds to the distance to the border, and use robust standard errors.

[Insert Table 5 approximately here]

Table 5 shows the results. We show that treated counties show significantly lower excess prescription rates when compared to control ones. Estimated coefficients for the difference in excess prescription rates over one year and two years are between 4.1 and 4.9 prescriptions per 100 people. Similarly, estimated coefficients for the difference in opioid death rates over one year and two years are between 4.1 and 5.5 opioid deaths per 100,000 people. We then estimate the difference in terms of the percentage change in average home values over one and two years between treated and control counties around the border. The estimated coefficient is between 1.2 for the one-year period and 2.3 for the two-year period. Figure 6 shows the regression discontinuity plots for the abuse variables, and Figure 7 shows the regression discontinuity plots for the percentage change in house prices. The results are overall consistent with previous difference-in-differences approach.[22]

[Insert Figure 6 approximately here]

[Insert Figure 7 approximately here]

[22] The results are robust to estimating Equation 6 without control variables (compare Internet Appendix Table A10) and based on a fixed bandwidth of a 150 km border distance (compare Internet Appendix Table A11).

An identifying assumption in our RD design is that there are no spillover effects across the state borders. This would occur, for instance, if users can cross the border to fill their prescriptions or due to ”doctor shopping,” when patients search for (out-of-state) for doctors who will prescribe opioids. Although recent evidence suggests that only 0.7 percent of all patients with an opioid prescriptions are ”doctor shoppers” (McDonald and Carlson, 2013,0), it might still be the case that patients can cross borders to have their prescriptions filled, which can bias our estimates upward.[23] To address this concern we exclude counties with five or more ”pill mill” pharmacies and eight or more ”pill mill” pharmacies from our analysis, respectively. Internet Appendix Table A12 documents the results, which are overall consistent with our main specification.

The internal validity of our quasi-experimental design relies also on the assumption that the treatment and control groups are similar, on average, in all other relevant aspects, except for the treatment assignment, allowing us to isolate the causal effect of the treatment on the outcome. Figure A8 in the Internet Appendix shows no significant differences in main economic variables across the state border, including our outcome variables, home values, opioid death rates, and excess prescription rates, before treatment.

B. Instrumental variable strategies

Following Cornaggia et al. (2022), we employ instrumental variables to study the impact of opioid abuse on house prices. We apply two alternative instruments. The first is based on the aggressiveness of Purdue Pharma’s marketing of reformulated oxycodone (branded as OxyContin) between 1997 and 2004. The second relies on the supply of opioid types that offer

[23]States vary in how restrictive they are in filling out-of-state controlled substance prescriptions.

the same level of pain relief but carry a higher risk of addiction and are distributed through channels with weak oversight.[24] While both instruments are highly correlated with opioid death in a county, Cornaggia et al. (2022) show that both instruments are uncorrelated with socioeconomic conditions. Purdue Pharma's marketing aggressiveness and supply chain conditions are thus unlikely to be related to local home values through any other economic mechanism than opioid abuse (*identifying assumption*). We describe the IV construction and results in detail in Internet Appendix Section H. Overall, the IV results are consistent with our main difference-in-differences approach.

V. Conclusion

This paper estimates the sensitivity of home values to opioid abuse. We find a negative association between home values and opioid abuse that increases monotonically and persists over a 5-year period. We exploit variation in opioid abuse induced by the staggered passage of state opioid-limiting laws. Home values respond positively to the passage of state laws intended to reduce opioid abuse.

Our analysis identifies several economic mechanisms behind this relationship. Anti-opioid legislation reduces mortgage delinquencies and vacancy rates and leads to an increase in home improvement loans and migration inflows. These results are consistent with a decrease in defaults, an improvement in the quality of local real estate, and an increase in the local demand for space driving the observed effect. Additionally, residential sorting may contribute to the impact on

[24]We thank Cornaggia et al. (2022) for sharing their data.

house values. Overall, our results point to a broad set of area externalities driving the observed patterns in home values.

These results have two main implications. First, while opioid abuse is linked to economic disadvantages (Case and Deaton, 2015), restricting prescription supply not only curbs usage but also strengthens housing markets. Second, opioid-related labor productivity losses and spatial externalities contribute to home value declines, reinforcing the broader economic consequences of the crisis.

Overall, our work offers insights into externalities of public health policies. We find evidence that public health policies that were instituted with the aim of limiting opioid abuse had a far reaching effect on the real economy. We believe that this study will foster further interest in examination of transmission and feedback effects of public health policies and real economic outcomes.

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TABLE 1

Summary statistics

The unit of observation is county-year. The sample period is 2006 to 2018. We report descriptive statistics for opioid abuse proxies in Panel A. Opioid abuse proxies include the opioid death rate, which is the annual drug overdose death rate per 100,000 residents, the one-year difference in the annual drug opioid death rate, the illicit (opioid) death rate, which is the annual illicit drug overdose death rate per 100,000 residents, the prescription (opioid) death rate, which is the annual prescription drug overdose death rate per 100,000 residents, excess prescription rate, which is the residualised prescription rate accounting for medical opioid use cases, and the prescription rate, which is the number of retail opioid prescriptions dispensed per 100 persons. Panel B reports county level home value statistics: the raw estimated home value of a typical house within a county based on the 2019 revision of the Zillow Home Value Index (ZHVI), as well as 1, 3, 4 and 5-year log percentage changes in county level home value. The percentage change in home values are winsorized at the 2 and 98% level.

Panel A: Opioid abuse proxies	Observations	Min	P25	Median	P75	Max	Mean	Std. Dev.
Opioid Death Rate	40513	0.000	0.000	4.541	10.687	139.978	7.378	10.019
1-year Diff. Opioid Death Rate	37205	−130.719	−1.995	0.000	3.154	130.719	0.443	9.381
Illicit Death Rate	40513	0.000	0.000	0.000	3.966	132.720	3.260	6.547
Prescription Death Rate	40513	0.000	0.000	1.807	5.840	130.719	4.118	6.758
Excess Prescription Rate	32526	−105.295	−29.072	−4.798	24.316	506.042	1.061	46.326
Prescription Rate	36704	0.000	53.200	77.800	106.900	583.800	83.548	46.577

Panel B: Home values	Observations	Min	P25	Median	P75	Max	Mean	Std. Dev.
Avg Home Value ($)	33481	26,415	85,275	117,307	169,095	1,529,600	143,150	96,894
1-year Perc. Change HV (in %)	30633	−38.532	−1.410	1.930	4.680	24.290	1.403	5.020
3-year Perc. Change HV (in %)	24990	−27.719	−4.347	4.131	12.108	27.265	3.406	12.109
4-year Perc. Change HV (in %)	22227	−33.415	−5.146	4.868	14.936	34.859	4.407	14.948
5-year Perc. Change HV (in %)	19524	−35.525	−5.564	5.422	16.839	41.259	5.414	16.915

TABLE 2

Opioid abuse & home values

The unit of observation is county-year. The sample period is 2006 to 2018 in Panel A and C and 2007 to 2018 in Panel B due to missing observations in estimating excess prescription. The dependent variable is a log percentage change of average county home values ($log(HV_t/HV_{t-x}) * 100$) over 3, 4 and 5 years. The key independent variable is the opioid death rate in Panel A, the excess prescription rate in Panel B and the prescription rate in Panel C. County controls include the male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. All independent variables are measured at the county level (c) at time $t-x$. Columns 1 to 3 include county and year fixed effects and columns 4 to 6 include state-year fixed effects. All variables are winsorized at the 2 and 98% level. Standard errors are clustered at the county level. *** indicates 1% significance, ** 5% significance, and * 10% significance.

	1	2	3	4	5	6
	Percentage change in average home value over					
	3-years	4-years	5-years	3-years	4-years	5-years
Panel A: Opioid mortality rates						
Opioid Death Rate$_{c,t-x}$	$-0.016*$	$-0.030**$	$-0.029*$	$-0.016**$	$-0.023***$	$-0.026**$
	(0.009)	(0.012)	(0.015)	(0.007)	(0.009)	(0.011)
R2	0.738	0.774	0.805	0.844	0.859	0.859
N	20494	17991	15544	20494	17991	15544
Std. dev. opioid abuse measure	8.375	8.066	7.912	8.375	8.066	7.912
Panel B: Excess prescription rates						
Excess Prescription Rate$_{c,t-x}$	$-0.013**$	$-0.020***$	$-0.029***$	$-0.005***$	$-0.006**$	$-0.006**$
	(0.006)	(0.008)	(0.009)	(0.002)	(0.002)	(0.003)
R2	0.770	0.798	0.826	0.846	0.856	0.850
N	17943	15444	13056	17943	15444	13056
Std. dev. opioid abuse measure	45.67	46.19	46.59	45.67	46.19	46.59
Panel C: Prescription rates						
Prescription Rate$_{c,t-x}$	$-0.017***$	$-0.021***$	$-0.028***$	$-0.005***$	$-0.006***$	$-0.006**$
	(0.006)	(0.008)	(0.009)	(0.002)	(0.002)	(0.003)
R2	0.739	0.773	0.805	0.844	0.859	0.859
N	19984	17479	15068	19984	17479	15068
Std. dev. opioid abuse measure	45.60	46.09	46.42	45.60	46.09	46.42
County F.E.	Yes	Yes	Yes	No	No	No
Year F.E.	Yes	Yes	Yes	No	No	No
State-Year F.E.	No	No	No	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

TABLE 3

Impact of opioid limiting laws on opioid abuse and home values:

By supply propensity

The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is excess prescription rate in columns 1 and 2, the 1-year difference in opioid death rates in columns 3 and 4, and the log percentage change of average county home values over 1 year in columns 5 and 6. $\text{Post}_{c,t}$ is a dummy equal to one in the year of the passage of the law in the respective county and thereafter; Physicians Rate StateTop_c is a dummy equal to one for counties whose average physicians per capita between 2009 to 2013 is in the top half within its respective state and Opioid Payment StateTop_c is a dummy equal to one for counties in the top half within its respective state based on opioid related payments to physicians from August 2013 (data start) until the end of 2015. One-year lagged controls include: Male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We include county and year fixed effects. Standard errors are clustered at the state level. *** indicates 1% significance, ** 5% significance, and * 10% significance.

	1	2	3	4	5	6
	Excess Prescription Rate		1-Yr Diff. Opioid Death Rate		Percentage Change Home Prices	
$\text{Post}_{c,t}$	-0.455	1.098	0.550	0.168	$0.590**$	$0.509*$
	(1.480)	(1.720)	(0.334)	(0.413)	(0.292)	(0.296)
$\text{Post}_{c,t}$ X Physicians Rate StateTop_c	$-4.544***$		$-1.228**$		0.131	
	(0.985)		(0.558)		(0.100)	
$\text{Post}_{c,t}$ X Opioid Payments StateTop_c		$-7.296***$		-0.459		$0.280**$
		(1.093)		(0.488)		(0.117)
R2	0.950	0.950	0.0733	0.0728	0.611	0.612
N	14723	14723	14988	14988	14695	14695
County F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

TABLE 4

Impact of opioid limiting laws on opioid abuse and home values:

By prior opioid abuse

The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is excess prescription rate in columns 1 and 2, the 1-year difference in opioid death rates in columns 3 and 4, and the log percentage change of average county home values over 1 year in columns 5 and 6. $\text{Post}_{c,t}$ is a dummy equal to one in the year of the passage of the law in the respective county and thereafter; $\text{Opioid Death Rate Top}_c$ is a dummy equal to one for counties whose average opioid death rate between 2013 and 2015 is in the top half nationwide and $\text{Opioid Death Rate StateTop}_c$ is a dummy equal to one for counties whose average opioid death rate between 2013 and 2015 is in the top half within its respective state. One-year lagged controls include: Male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We include county and year fixed effects. Standard errors are clustered at the state level. *** indicates 1% significance, ** 5% significance, and * 10% significance.

	1	2	3	4	5	6
	Excess Prescription Rate		1-Yr Diff. Opioid Death Rate		Percentage Change Home Prices	
$\text{Post}_{c,t}$	0.992	−0.411	0.801*	0.790**	0.384	0.611**
	(1.602)	(1.398)	(0.405)	(0.362)	(0.338)	(0.294)
$\text{Post}_{c,t}$ X Opioid Death Rate Top_c	−6.119***		−1.429***		0.444*	
	(1.070)		(0.514)		(0.240)	
$\text{Post}_{c,t}$ X Opioid Death Rate StateTop_c		−4.689***		−1.718***		0.091
		(0.684)		(0.499)		(0.091)
R2	0.950	0.950	0.0735	0.0739	0.612	0.611
N	14723	14723	14988	14988	14695	14695
County F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

TABLE 5

Impact of opioid limiting laws on opioid abuse and home values: Around state borders

The unit of observations is county. In Panel A, the dependent variables is a one or two-year difference in excess prescription rates in column 1 to 4 and a one or two-year difference in opioid death rates in columns 5 to 8. For treated counties, we calculate the difference from the treatment year - 1 to the treatment year and treatment year + 1 respectively. For control counties, we calculate the difference from 2015 to 2016 or 2017, as the first law was passed in 2016. In Panel B, the dependent variables is a one or two-year percentage change in home values. For treated counties, we calculate the percentage change from the treatment year - 1 to the treatment year and treatment year + 1 respectively. For control counties, we calculate the percentage change from 2015 to 2016 or 2017, as the first law was passed in 2016. We include the following control variables as of 2015: male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We follow Calonico et al. (2014) to choose the optimal bandwidth and use robust standard errors. *** indicates 1% significance, ** 5% significance, and * 10% significance.

Panel A: Opioid abuse

	1	2	3	4	5	6	7	8
	Difference in Excess Prescription Rates over				Difference in Opioid Death Rates over			
	1 year		2 years		1 year		2 years	
RD Estimate	-4.853***	-4.866***	-4.327**	-4.089*	-4.438**	-4.141**	-5.471***	-5.198***
	(1.183)	(1.436)	(1.864)	(2.392)	(1.944)	(1.769)	(1.834)	(1.777)
Observations	2361	2361	2043	2043	2398	2398	2082	2082
MSEBandwidth	115	180	153	197	76	171	89	195
Effective LHS Obs	575	773	707	804	423	766	488	813
Effective RHS Obs	640	913	594	718	449	903	389	737
Polynominal Order	1	2	1	2	1	2	1	2

Panel B: Home Values

	1	2	3	4
	Percentage Change in Home Values over			
	1 year		2 years	
RD Estimate	1.226***	1.293***	2.296***	2.236***
	(0.462)	(0.441)	(0.789)	(0.789)
Observations	2326	2326	2012	2012
MSEBandwidth	92	200	99	205
Effective LHS Obs	474	775	503	776
Effective RHS Obs	530	982	423	750
Polynominal Order	1	2	1	2

FIGURE 1

Opioid death rate and home value

The unit of observation is the county. We calculate within-state quintiles based on the average 5-year percentage change in home values and the 5-year lagged opioid death rate over our sample period (2006 to 2018). We restrict the sample to counties with averages based on more than five observations and those in the highest opioid death rate quintile within each state. These counties are colored based on their within-state quintile of average home value change: Dark red indicates counties in the lowest quintile (lowest home value growth), and light yellow indicates those in the highest quintile (highest home value growth). Excluded counties are shown in dark grey, and counties without data are shown in light grey. Dark red thus reflects a negative correlation between opioid death rates and home value changes.

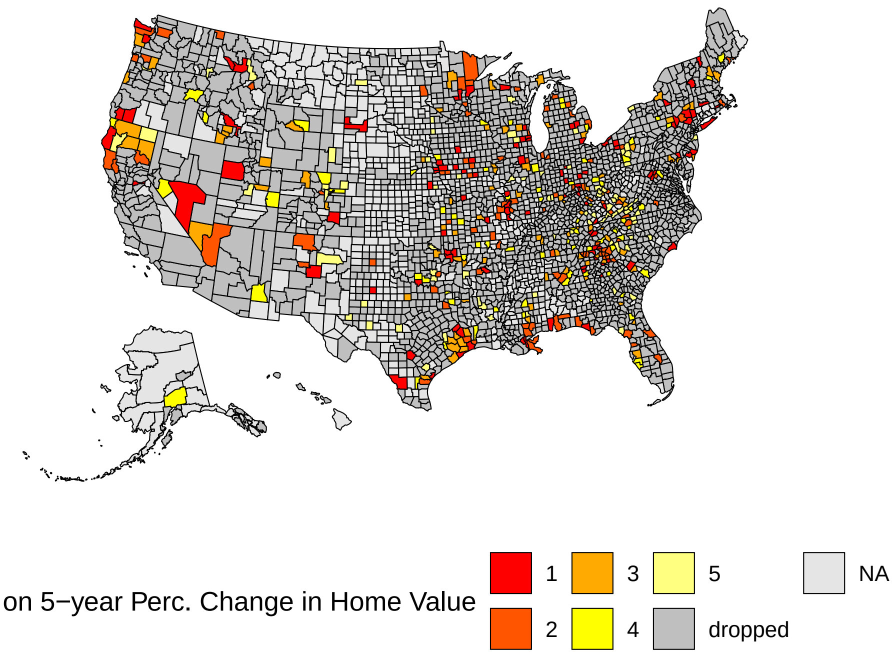
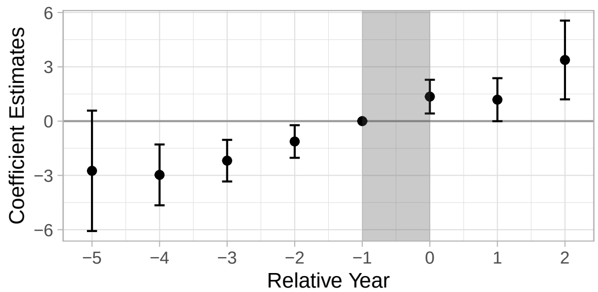


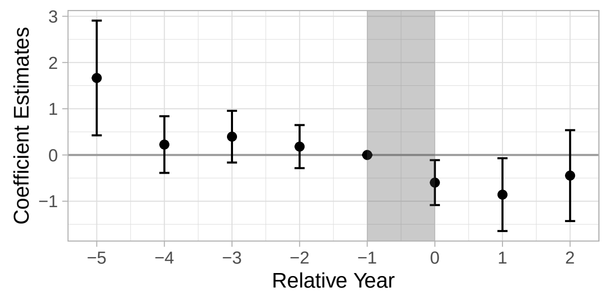
FIGURE 2

Impact of opioid limiting laws on opioid abuse

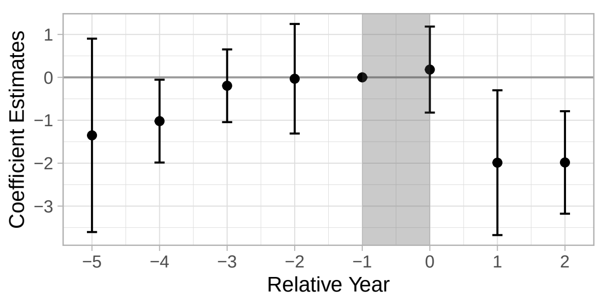
The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is the annual opioid death rate in Panel A, the annual prescription death rate in Panel B, the one-year difference in opioid death rate (in %) in Panel C and the excess absolute total county prescriptions in Panel D. One year-lagged controls include male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. In Panel D, we additionally control for log total county population. We plot the interaction weighted total coefficient with a 95% confidence interval for each relative time period following Sun and Abraham (2021). Standard errors are clustered at the state level.



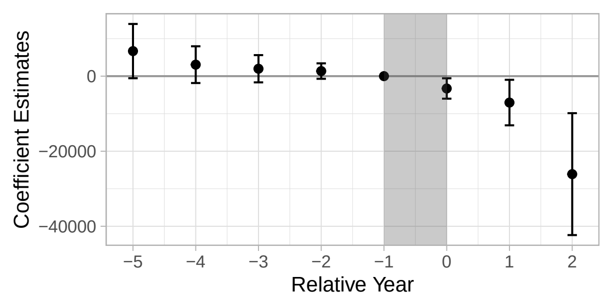
(a) **Opioid Death Rate**



(b) **Prescription Death Rate**



(c) **1-Yr Diff. Opioid Death Rate**



(d) **Excess Absolute Prescriptions**

FIGURE 3

Impact of opioid limiting laws on home values

The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is log one-year percentage change in average county home values (in %). One year-lagged controls include male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We plot the interaction weighted total coefficient with a 95% confidence interval for each relative time period following Sun and Abraham (2021). Standard errors are clustered at the state level.

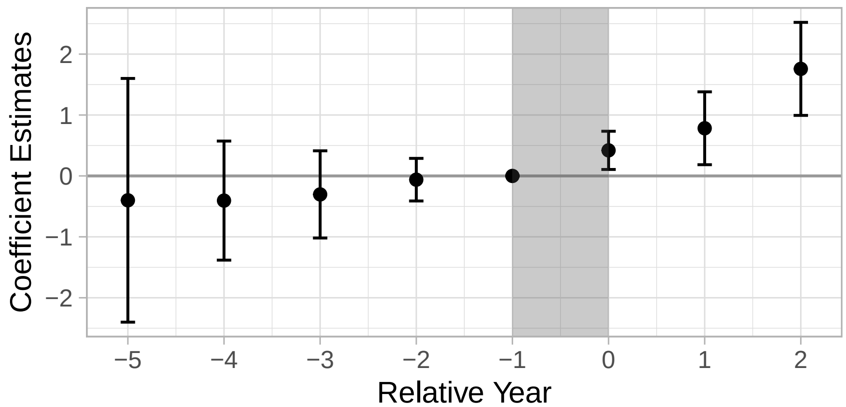
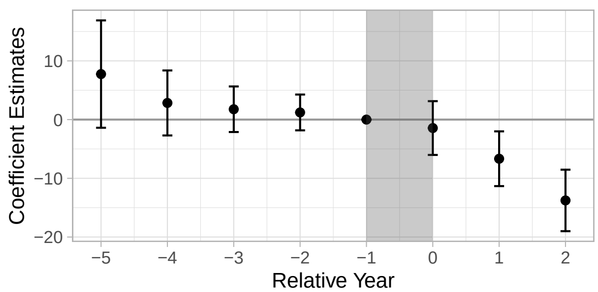


FIGURE 4

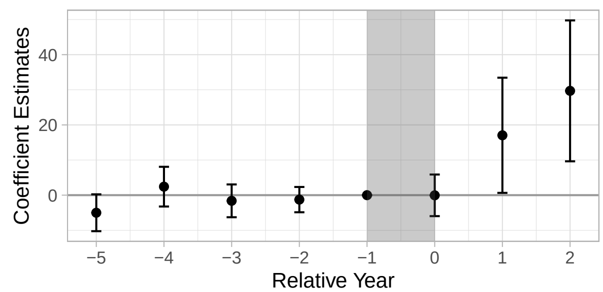
Impact of opioid limiting laws on delinquent mortgages,

home improvement loans and vacancy rates

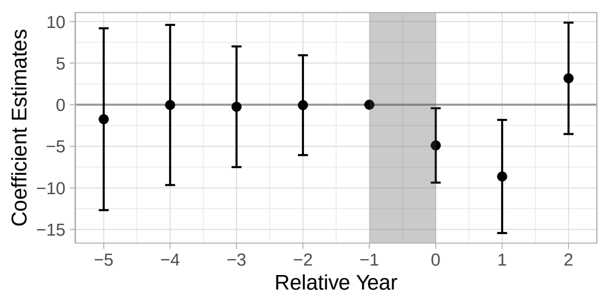
The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is the log one-year percentage change in mortgages 90 plus days past due (in %) in Panel A, the log one-year percentage change in the number of home improvement loans (in %) in Panel B and the log one-year percentage change in the residential vacancy rate (in %) in Panel C. One year-lagged controls include male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We plot the interaction weighted total coefficient with a 95% confidence interval for each relative time following Sun and Abraham (2021). Standard errors are clustered at the state level.



(a) **Mortgages 90 plus days past due**



(b) **Home improvement loan**

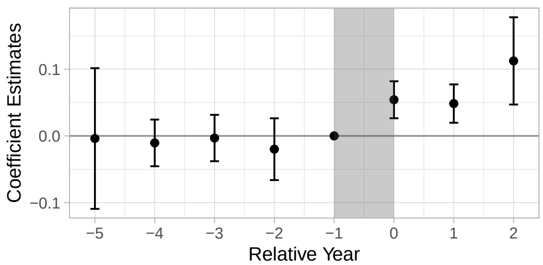


(c) **Residential vacancy rate**

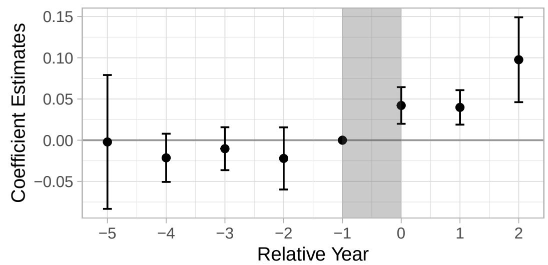
FIGURE 5

Impact of opioid limiting laws on migration inflow

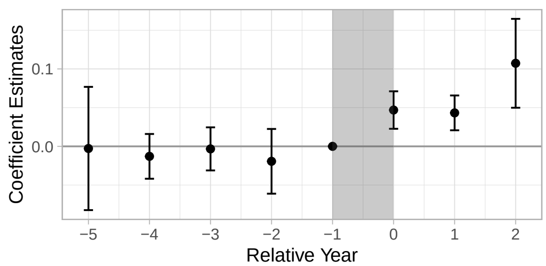
The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is the log total migration inflow income in Panel A, the log total migration inflow number of households in Panel B and the log total migration inflow number of individuals in Panel C. One year-lagged controls include male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We plot the interaction weighted total coefficient with a 95% confidence interval for each relative time period following Sun and Abraham (2021). Standard errors are clustered at the state level.



(a) **Total income**



(b) **Number of households**



(c) **Number of individuals**

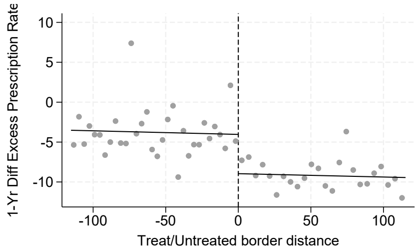
FIGURE 6

Impact of opioid limiting laws on opioid abuse:

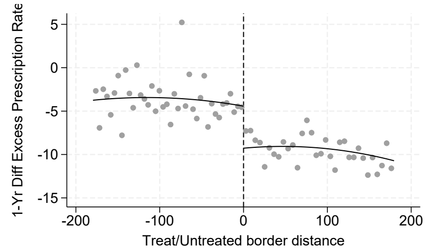
Regression discontinuity plots around state borders

The unit of observation is county. In Panel A, the dependent variable is a one or two-year difference in excess prescription rates. In Panel B, the dependent variables is a one or two-year difference in opioid death rate. For treated counties, we calculate the difference from the treatment year - 1 to the treatment year and treatment year + 1 respectively. For control counties, we calculate the difference from 2015 to 2016 or 2017, as the first law was passed in 2016. We include the following control variables as of 2015: male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We follow Calonico et al. (2014) to choose the optimal bandwidth. Standard errors are clustered at the state level. The regression continuity plots correspond to Panel A in Table 5.

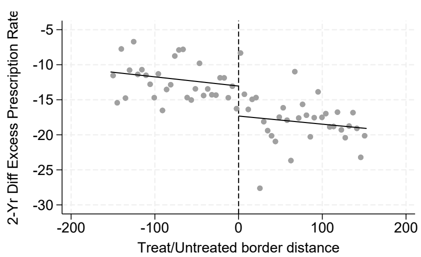
(a) Excess Prescription Rates



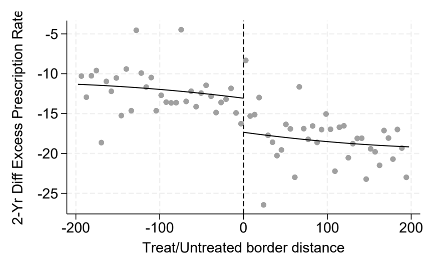
(a.1) 1-yr difference & Linear polynomial



(a.2) 1-yr difference & Quadratic polynomial

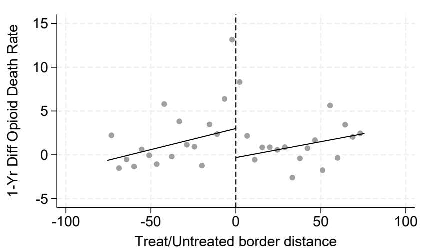


(a.3) 2-yr difference & Linear polynomial

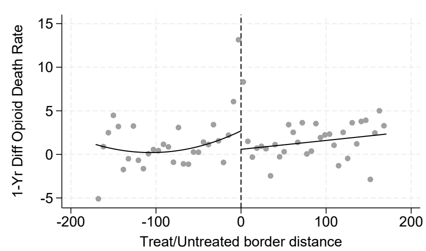


(a.4) 2-yr difference & Quadratic polynomial

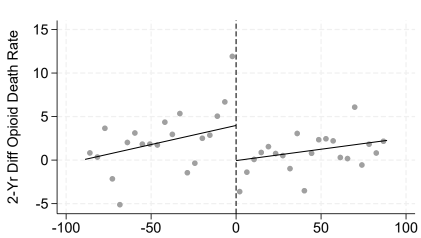
(b) Opioid death rate

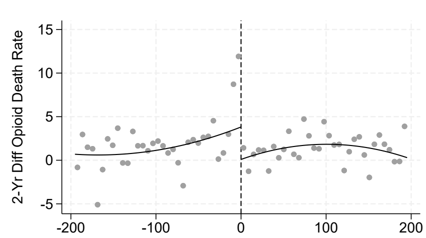


(b.1) 1-yr difference & Linear polynomial



(b.2) 1-yr difference & Quadratic polynomial





(b.3) 2-yr difference & Linear polynomial

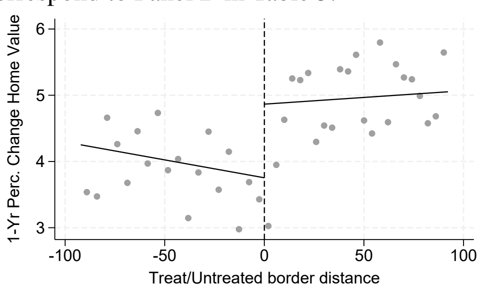
(b.4) 2-yr difference & Quadratic polynomial

FIGURE 7

Impact of opioid limiting laws on home values:

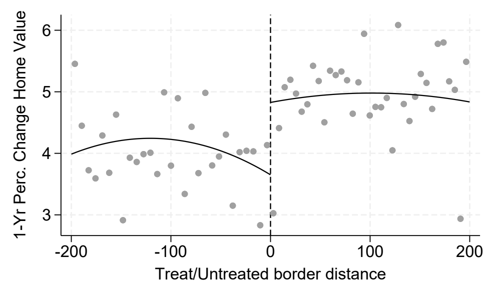
Regression discontinuity plots around state borders

The unit of observation is county. The dependent variables is a one or two-year percentage change in home values. For treated counties, we calculate the difference from the treatment year - 1 to the treatment year and treatment year + 1 respectively. For control counties, we calculate the percentage change from 2015 to 2016 or 2017, as the first law was passed in 2016. We include the following control variables as of 2015: male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We follow Calonico et al. (2014) to choose the optimal bandwidth. Standard errors are clustered at the state level. The regression continuity plots correspond to Panel B in Table 5.



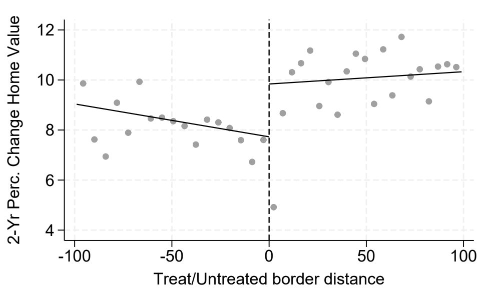
(a) **1-yr percentage change &**

Linear polynomial



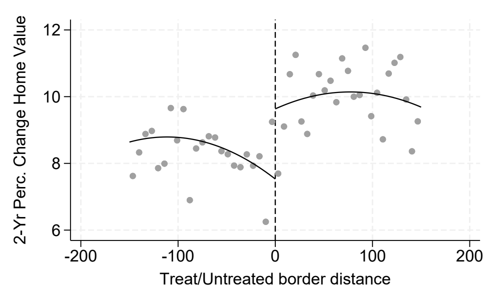
(b) **1-yr percentage change &**

Quadratic polynomial



(c) **2-yr percentage change &**

Linear polynomial



(d) **2-yr percentage. change &**

Quadratic polynomial

Internet Appendix for

Opioid Crisis and Real Estate Prices

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A. Internet Appendix

A. Opioid crisis background

The opioid crisis in the US evolved in three waves (**?**). The first wave started in the mid-1990s and continued through 2010 and marked itself with an unprecedented increase in prescription opioids. In the 1980s the US medical community adopted a more aggressive approach to pain treatment. Further, the American Academy of Pain Medicine and the American Pain Society advocated for greater use of opioids, arguing that there were minimal long-term risk of addiction from these drugs following the FDA approval of OxyContin (oxycodone controlled-release), a new prescription opioid, in 1995. The Joint Commission on Accreditation of Healthcare Organizations (TJC) further institutionalized this stance in 2001, determining that the treatment and monitoring of pain should be the fifth vital sign.[1] This paved a way for the creation of a new metric upon which doctors and hospitals would be judged.

The second wave from 2010 to 2013 was characterised by a widespread increase in heroin use and deaths. Concerns about the possible over-use of opioid prescriptions for chronic pain conditions gained attention in early 2000s and efforts to reduce opioid prescription may have partly contributed to the diversion of opioid prescriptions and the increase in heroin use.

The current third wave that started in 2013 manifests itself with a movement towards extremely addictive synthetic opioids, in particular fentanyl. Opioid prescription regulations have been tightening further. In 2014, the Agency for Healthcare Research and Quality (AHRQ) concluded that evidence-based medicine to support opioids' use in chronic non-terminal pain is limited at best (**?**). In 2016, the CDC issued a new policy recommendation for prescribing opioids

[1] www.medpagetoday.com/publichealthpolicy/publichealth/57336

advising amongst others to maximize non-opioid treatment.[2] To address the opioid epidemic the TJC revised and issued new standards on the treatment of pain in 2017.[3] In October of 2017, the US government declared opioid crisis a public health emergency.

Evidence on drivers of demand for opioid prescriptions has been mixed.[4] Most of the literature suggests that the observed patterns in opioid usage have been driven by variation in the supply of prescriptions. Since **?**, a number of studies have shown that economic conditions are not a significant driver of regional patterns of opioid use. In fact, most deaths attributed to opioid abuse occur in states with low unemployment rates (**?**). **?** show that differences in prescription opioid supply from physicians are a key contributor to opioid abuse, compared to patient-specific factors, such as mental health or poor economic prospects. The idea that supply-side factors determine opioid abuse is corroborated by **?**, who show that the introduction and marketing of OxyContin were important determinants of the opioid crisis. **?** conclude that opioid prescription rates cannot be explained by variation in the health of the population and instead suggest that the patterns reflect the lack of a consensus among doctors on best practices when prescribing opioids.

Several states have taken specific action to address the opioid epidemic. First measures involved the development of prescription drug monitoring programs (PDMPs) with the goal of enabling doctors to better identify drug-seeking patients. However, many of these programs relied on voluntary participation of providers and they were not welcomed by physicians with, at best, mixed evidence on their effectiveness (**???**). Recent measures were more drastic adopting

[2] www.cdc.gov/mmwr/volumes/65/rr/rr6501e1.htm

[3] www.jointcommission.org/standards/r3-report/r3-report-issue-11-pain-assessment-and-management-standards-for-hospitals/

[4] See **?** for a review.

legislation that explicitly sets limits on opioid prescriptions (with some exceptions such as cancer treatment or palliative care). In 2016, Massachusetts became the first state to limit opioid prescriptions to a 7-day supply for first time users. As of 2018, 32 states have legislation limiting the quantity of opioids which can be prescribed. A description of the state laws and regulations in a map is included in Appendix D. These laws seem to be more likely to pass in states that suffer from high rates of deaths related to opioids, as shown in Appendix Table A4, while other potential determinants such as local economic, health and political characteristics do not seem to be correlated. At the federal level, Medicare also adopted a 7-day supply limit for new opioid patients in 2018.

B. Related literature

TABLE A1

Summary of Literature

This table summarizes key studies on the impact of opioids on economic outcomes. Data sources, identification strategies, and outcomes are listed.

Paper	Time Period	Opioid measure	Dependent Variable	Area	Identification Strategy	Main Empirical Results	Data sources
D' Lima and Thibodeau (2023)	2006 to 2012	Number of pills dispensed per pharmacy / per property	Real estate prices	Ohio	Use distributor-pharmacies where distributors' reporting responsibilities may weaken monitoring incentives. Results based on dispense measures by vertically integrated pharmacies may potentially imply a causal narrative on supply side factors having a negative effect on house prices.	A one standard deviation increase in the standardized number of pills by distributor pharmacies is associated with a 5.8% decrease in house price appreciation for nearby properties (within 2 miles)	Opioid pharmacy dispense data (of Oxycodone and Hydrocodone) is from the Automation of Reports and Consolidated Orders System (ARCOS) released by the Washington Post and the repeat sales observations are from Zillow's property-level data.
Ho and Jiang (2021)	2010 to 2017	Opioid prescription rates	Firm returns - then real estate prices	California vs Illinois	The authors focus on two regulations in California, a revision of the guidelines for Prescribing Controlled Substances for Pain to add additional and more stringent directions for physicians to prescribe opioids in 2014 and the startup of the technical assistance program in November 2015.	The main results is a negative causal effect between county-level opioid prescription rates and equity returns of firms. The authors also find that the opioid prescription reduction assistance program provided by California Health Care Foundation to certain counties in California raised the median prices of existing single-family homes by $28,678 on average.	County-level housing data is from Zillow and opioid prescription rates data is from the Centers for Disease Control and Prevention
Karimli (2022)	2004 to 2017	County-level opioid mortality (public use data)	Mortgage defaults	USA	The author instruments for changes in opioid overdose death rates using the 2010 introduction of the abuse-deterrent version of OxyContin.	Depressed local house prices, resulting from the epidemic, have caused more defaults by squeezing households' current home equity. Defaults have originated from borrowers' strategic motives or constrained liquidity positions. A one standard deviation exogenous increase in opioid drug overdose mortality rises 90+ consecutive days delinquency rate by 3-4 percentage points on average. The author also confirms a negative relationship between opioid abuse and house prices.	The author combines Fannie Mae and Freddie Mac's Single-Family Loan Level Data geo-coded via the Home Mortgage Disclosure Act (HMDA) to identify loan performance data. Opioid mortality data is from the Multiple Cause of Death database by the Center for Disease and Control (CDC).
Luo and Tidwell (2023)	2010 to 2017	County-level opioid mortality rate (public use data); opioid prescription rate; and opioid abuse rate (ratio of drug overdose mortality rate to opioid prescription rate)	Mortgage loan approval	USA (50 states)	The U.S. Drug Enforcement Administration's tightening controls of hydrocodone products serves as a supply-side shock to opioid accessibility and a quasi-natural experiment. Using a difference-in-differences framework, they find that mortgage loan application acceptance rates significantly increase following the rescheduling in states with relatively greater hydrocodone intensity prior to the policy shift.	Lenders are less likely to approve loans from areas with relatively higher levels of opioid abuse. Average loan approval rate declines by 38.43-107.6 basis points when there is an interquartile increase in the proxies for local opioid epidemic intensity. Consistent with risk channeling, originated mortgage loans are more likely to have lower loan-to-income ratios in areas with higher rates of opioid abuse.	Mortgage application records are collected from the Home Mortgage Disclosure Act (HMDA) Loan Application Registry. The severity of the opioid epidemic is measured using county-level data on opioid prescriptions, and drug overdose deaths from the Centers for Disease Control and Prevention (CDC).

C. Data

1. Variable definitions

Opioid abuse variables:

Opioid Death Rate is measured at the county level as the annual drug overdose death rate per 100,000 residents. Source: Restricted All-County Mortality Micro data from the Centers for Disease Control and Prevention

1-year Difference Opioid Death Rate is measured at the county level as one-year difference in the annual drug opioid death rate. Source: Restricted All-County Mortality Micro data from the Centers for Disease Control and Prevention

Illicit Death Rate is measured at the county level as the annual illicit drug overdose death rate per 100,000 residents, where illicit deaths are all deaths that have any opium (T40.0), heroin (T40.1) and synthetic opioids (T40.4) cause. Source: Restricted All-County Mortality Micro data from the Centers for Disease Control and Prevention

Prescription Death Rate is measured at the county level as the annual prescription drug overdose death rate per 100,000 residents, where prescription deaths are the rest (T40.2-T40.3) following CDC classification. Source: Restricted All-County Mortality Micro data from the Centers for Disease Control and Prevention

Excess Prescription Rate is measured at the county level as the residualised prescription rate accounting for medical opioid use cases, specifically cancer rate, hospice percentage and ambulatory surgery percentage. Source: Own calculation.

Prescription Rate is measured at the county level as the number of retail opioid prescriptions dispensed per 100 persons. Source: Centers for Disease Control and Prevention

Home value and channel outcome variables:

Percentage Change Home Value is measured at the county level as log percentage change of average county home values ($log(HV_t/HV_{t-x}) * 100$) over 1, 3, 4 or 5 years. Source: Zillow Home Value Index

Percentage Change Mortgages 90 Plus Days Past Due is measured at the county level as the one-year log percentage change in mortgages 90 plus days past due (in %). Source: Consumer Financial Protection Bureau

Percentage Change Home Improvement Loans is measured at the county level as the one-year log percentage change in mortgages 90 plus days past due (in %). Source: Home Mortgage Disclosure Act

Percentage Change Residential Vacancy Rate is measured at the county level as the one-year log percentage change in the residential vacancy rate (in %). Source: United States Postal Service

Log Total Migration Inflow Income is measured at the county level as the change in total adjusted gross income based on year-to-year address changes reported on individual income tax returns filed. Source: Internal Revenue Service from the Statistics of Income Tax Stats

Log Total Migration Inflow Number of Households is measured at the county level as the change in total number of returns filed based on year-to-year address changes reported on individual income tax returns filed. Source: Internal Revenue Service from the Statistics of Income Tax Stats

Log Total Migration Inflow Number of Individuals is measured at the county level as the change in total number of personal exemptions claimed based on year-to-year address changes reported on individual income tax returns filed. Source: Internal Revenue Service from the Statistics of Income Tax Stats

Difference-in-Differences interaction variables:

Physicians Rate StateTop is a dummy equal to one for counties whose average physicians per capita between 2009 to 2013 is in the top half within its respective state. Source: Area Health Resources Files from the Health Resources and Services Administration

Opioid Payment StateTop is a dummy equal to one for counties in the top half within its respective state based on opioid related payments to physicians from August 2013 (data start) until the end of 2015. Source: Centers for Medicare & Medicaid Services Open Payments databas

Opioid Death Rate Top is a dummy equal to one for counties whose average opioid death rate between 2013 and 2015 is in the top half nationwide. Source: Restricted All-County Mortality Micro data from the Centers for Disease Control and Prevention

Opioid Death Rate StateTop is a dummy equal to one for counties whose average opioid death rate between 2013 and 2015 is in the top half within its respective state. Source: Restricted All-County Mortality Micro data from the Centers for Disease Control and Prevention

Control variables:

Male population ratio is measured at the county level by male population divided by total county population. Source: Census Bureau

White ratio is measured at the county level by white population divided by total county population. Source: Census Bureau

Black ratio is measured at the county level by black population divided by total county population. Source: Census Bureau

American-Indian ratio is measured at the county level by American-Indian population divided by total county population. Source: Census Bureau

Hispanic ratio ratio is measured at the county level by Hispanic population divided by total county population. Source: Census Bureau

Age 20-64 ratio is measured at the county level by the population between ages 20 and 64 divided by total county population. Source: Census Bureau

Age over 65 ratio is measured at the county level by the population over the age of 65 divided by total county population. Source: Census Bureau

Migration inflow ratio is measured at the county level by the population with residence in a county this year but not in the prior year normalized by total county population. Source: Census Bureau

Poverty ratio is measured at the county level by the population in poverty divided by total county population. Source: Census Bureau

Unemployment ratio is measured at the county level by the number of unemployed divided by the sum of employed and unemployed at a county. Sources: Bureau of Labor Statistics

Labor force participation ratio is measured at the county level by labor force divided by total county population. Sources: Bureau of Labor Statistics

Neoplasm mortality is measured at the county level by the number of deaths due to neoplasm (ICD-10 C00-D48), normalized by total county population. Source: Centers for Disease Control and Prevention

Physicians is measured at the county level by the number of primary care physicians, excluding hospital residents or age 75 years or over. Source: Area Health Resources Files from the Health Resources and Services Administration

2. Opioid mortality variable

We follow **?** to construct the main variable Opioid Deaths as well as the split into Presciprtion and Illicit deaths. Opioid Deaths is the number of opioid-related deaths per 100,000 people in a given county. We lay out their data construction steps below.

The first step is to identify drug-related deaths, i.e. deaths with the underlying ICD-10 cause codes X40-X44 (accidental poisoning), X60-X64 (intentional poisoning), X85 (homicide), and Y10-Y14 (undetermined intent). Next, deaths are further restricted to causes that are related to opioids, i.e. deaths with a contributing cause code of T40.0 (opium), T40.1 (heroin), T40.2-T40.3 (prescription) and T40.4 (synthetic opioids, primarily fentanyl). The second step is to calculate county population. In line with **?** and the public health literature, county population data is from the National Cancer Institutes Survey of Epidemiology and End Results (SEER). This enables us to calculate opioid mortality rates. Opioid Deaths are split into Prescription and Illicit categories based on the multiple cause portion of the death certificate data. Illicit deaths are all deaths that have any opium (T40.0), heroin (T40.1) and synthetic opioids (T40.4) cause. Prescription deaths are the rest (T40.2-T40.3) following CDC classification. Even though not all fentanyl (synthetic opioid) are illicit, fentanyl overdoses are generally attributed to illicitly manufactured fentanyl, such as Acetyl fentanyl, which has not been approved for legal prescription use; see **?** and **?**.

3. ”Pill mill” counties

A limitation of prescription data is the potential misalignment between the prescription of the drug and intake. Drug consumers may have traveled miles to reach a doctor and pharmacy where they can receive a prescription and subsequently the drugs. A typical ”pill mill” has a store front pain clinic with doctors prescribing opioids after a brief consultation, and usually limited proof of medical purpose. The prescriptions are often filled at the clinic to avoid other pharmacies challenging the legitimacy of the prescriptions. These pill mills are considered to have worsened the opioid crisis, as they were responsible for dispensing a large fraction of opioids.[5] Drug intake in pill mill counties is unlikely to be equivalent to prescription rates, leading to noise. Furthermore, pill mill counties may be correlated to weaker economic areas with implications for home value growths. These counties may therefore bias our analysis.

To address this concern we follow **?** to identify counties that are most likely to have a pill mill. The Automation of Reports and Consolidated Orders System (ARCOS) data provides information on the milligrams of active ingredient (MME) dispensed by pharmacy.[6] We classify a pharmacy as a pill mill if it dispenses opioid MME in the top 5% of the sample. We consider counties with more than 8 pill mills (equivalent to 7.5% of counties) as pill mill counties. In robustness analysis, we define pill mill counties as those with more than 5 pill mills (equivalent to 14.1% of counties).

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[5] Between 2006 and 2012 15% of pharmacies received for instance 48% of pain pills, see https://www.washingtonpost.com/investigations/the-opioid-crisis-15-percent-of-the-pharmacies-handled-nearly-half-of-the-pills/2019/08/12/b24bd4ee-b3c7-11e9-8f6c-7828e68cb15f_story.html.

[6] The Drug Enforcement Agency (DEA) collected this data and made it available to the public following a FOIA lawsuit by the Washington Post. Only the two most common forms of opioid prescriptions, OxyContin and Hydrocontin, are covered.

4. Excess opioid prescriptions

TABLE A2

Legitimate opioid prescription drivers

The unit of observation is county-year. The sample period is 2007 to 2018. The dependent variable is prescription rate or absolute prescription. Columns 3 and 4 are restricted to counties with at most 7 pill-mills. Independent variables are cancer rate, the percentage of FFS Medicare beneficiaries using Hospice services with at least one covered stay and the percentage of FFS Medicare beneficiaries using Ambulatory Surgery Center (ASC) in columns 1 and 3. In columns 2 and 4 we switch to total prescriptions and therefore also measure independent variables in levels, specifically the total cancer count in 000s, the number of FFS Medicare beneficiaries using Hospice services with at least one covered stay and the number of FFS Medicare beneficiaries using ASC. We additionally control for the natural logarithm of total population in columns 2 and 4. All independent variables measured at time t. Standard errors are clustered at the county level. *** indicates 1% significance, ** 5% significance, and * 10% significance.

	1 Prescription Rate	2 Absolute Prescriptions	3 Prescription Rate	4 Absolute Prescriptions
Cancer Rate$_{c,t}$	$0.037***$ (0.003)		$0.031***$ (0.003)	
Medicare Hospice PCT$_{c,t}$	-8.441 (31.603)		-47.577 (32.710)	
Medicare Surgery PCT$_{c,t}$	$57.512***$ (5.918)		$56.351***$ (6.277)	
Cancer Count$_{c,t}$		$0.524***$ (0.107)		$0.337***$ (0.053)
Medicare Hospice Count$_{c,t}$		$45.198**$ (19.968)		$51.849***$ (6.207)
Medicare Surgery Count$_{c,t}$		$5.578**$ (2.665)		$3.789***$ (1.377)
Log(Population)	no	yes	no	yes
Year F.E.	yes	yes	yes	yes
R2	0.0565	0.884	0.0523	0.872
N	32526	32407	29746	29628
Sample	full	full	Pill Mills ¡ 8	Pill Mills ¡ 8

D. Opioid laws and regulations

Opioid Laws and Regulations Passed between 2016 and 2018

We gather information on states with opioid limiting laws or regulations from Ballotpedia[7]. Between 2016 and 2018, the following states have passed first-time opioid prescription limiting law or regulations:

Alaska (*2017 / Law*) limits first-time opioid prescriptions to seven days except for chronic pain or patients with travel/ logistical barriers.

Arizona (*2016 / Regulation*) limits first-time opioid prescriptions to seven days for insured people under state's Medicaid or state's employee insurance plan. In 2018, a new law limits first-time opioid prescription to five days.

Colorado (*2017 / Regulation*) limits first-time opioid prescriptions to seven days with 2 more seven-day prescriptions and a fourth seven-day prescriptions upon department approval possible. In 2018, a new law limits first-time opioid prescription to seven days with one possible seven day extensions. Exceptions include chronic pain patients, cancer patients, patients under hospice care, and patients experiencing post-surgical pain.

Connecticut (*2016 / Law*) limits first-time opioid prescriptions to seven days except for chronic pain patients. in 2018, a second law reduce opioid prescription limits for minors from seven days to five days.

Delaware (*2017 / Regulation*) limits first-time opioid prescriptions to seven days unless the doctor determines a patient requires more. Patients receiving longer supply must undergo a physical exam and are educated about the danger of opioid abuse.

[7]Source: https://ballotpedia.org/Opioid_prescription_limits_and_policies_by_state

Florida (*2018 / Law*) limits opioid prescriptions for acute pain to three days, with some exceptions allowing seven days.

Hawaii (*2017 / Law*) limits first-time opioid prescriptions to seven days except for cancer patients, post-operative care patients and patients in palliative care.

Indiana (*2017 / Law*) limits first-time opioid prescriptions to seven days unless the doctor determines a patient requires more or the patient is in palliative care.

Kentucky (*2017 / Law*) limits first-time opioid prescriptions to three days unless the doctor determines a patient requires more or the patient is treated for chronic pain, cancer-related pain or post-surgery pain.

Louisiana (*2017 / Law*) limits first-time opioid prescriptions to seven days except for chronic pain patients, cancer patients, or patients receiving hospice care.

Maine (*2016 / Law*) limits first-time opioid prescriptions to seven days for acute pain and thirty days for chronic pain. Morphine milligram equivalents (MME) are limited to 100 per day except for cancer patients, hospice and palliative care patients and substance abuse disorder treatment patients.

Massachusetts (*2016 / Law*) limits first-time opioid prescriptions to seven days except for cancer pain patients, chronic pain patients, and palliative care patients.

Michigan (*2017 / Law*) limits opioid prescriptions to seven days for acute pain.

Minnesota (*2017 / Law*) limits opioid prescriptions to four days for acute dental or ophthalmic pain.

Missouri (*2017 / Regulation*) limits first-time opioid prescriptions to seven days for Medicaid recipients.

Nebraska (*2016 / Regulation*) limits opioid prescriptions to 150 doses of short-acting opioids in 30 days. In 2018, a law was passed to limit opioid prescriptions to seven days for patients under 19.

Nevada (*2017 / Law*) limits first-time opioid prescriptions to fourteen days for acute pain and 90 morphine milligram equivalents per day. Exceptions are possible, but require additional scrutiny by doctors, respectively blood and radiology tests to determine the cause of pain.

New Hampshire (*2016 / Law*) limits opioid prescriptions to seven days in an emergency room, urgent care setting or walk-in clinic.

New Jersey (*2017 / Law*) limits first-time opioid prescriptions to five days for acute pain except for cancer pain patients, hospice care patients, patients in a long-term care facility or substance abuse treatment patients.

New York (*2016 / Law*) limits first-time opioid prescriptions to seven days for acute pain except for chronic pain patients, cancer pain patients and patients in hospice or palliative care.

North Carolina (*2016 / Law*) limits first-time opioid prescriptions to five days for acute pain and seven days for post-surgery patients. Exemptions are for cancer patients, chronic pain patients, hospice or palliative care patients as well as patients being treated for substance use disorders.

Ohio (*2017 / Regulation*) limits opioid prescriptions to seven days for acute pain and an average 30 morphine equivalent does per day except for cancer patients, chronic pain patients, hospice or palliative care patients and patients treated for substance use disorders.

Oklahoma (*2018 / Law*) limits opioid prescriptions to seven days for acute pain.

Pennsylvania (*2016 / Law*) limits opioid prescriptions to seven days in emergency rooms and urgent care centers except for cancer patients, chronic pain patients and hospice and palliative care patients.

Rhode Island (*2016 / Law*): limits opioid prescription to 30 morphine milligram equivalents per day for a maximum of 20 doses except for cancer pain patients, chronic pain patients and hospice and palliative care patients.

South Carolina (*2018 / Regulation*) limits first-time opioid prescriptions to five days or 90 morphine milligram equivalents per day except for cancer pain patients, chronic pain patients, sickle cell disease-related patients, palliative care patients and substance abuse disorder treated patients.

Tennessee (*2018 / Law*) limits first-time opioid prescriptions to three days, but allows for ten and thirty day prescriptions if certain requirements are met.

Utah (*2017 / Law*) limits first-time opioid prescriptions to seven days for acute pain except for complex or chronic conditions patients

Vermont (*2017 / Regulation*) sets opioid limits for minor, moderate, severe and extreme pain. Adults suffering from moderate pain are limited to 24 morphine milligram equivalents per day and with severe pain to 32 morphine milligram equivalents per day.

Virginia (*2017 / Regulation*) limits opioid prescriptions to seven days for acute pain and 14 days for post-surgical pain except under extenuating circumstances.

Washington (*2017 / Law*) limits opioid prescriptions for Medicaid patients under the age of 20 to 18 tablets and for patients 21 years and older to 42 tablets, equivalent to about a seven day supply. Limits can be exceeded if deemed necessary by the prescriber and do not apply to cancer patients as well as hospice and palliative care patients.

West Virginia (*2018 / Law*) limits opioid prescriptions to seven days for short-term pain, four days fro emergency room prescriptions and three days for prescriptions by a dentist or optometrist except for cancer patients, hospice patients and nursing home/ long/term care patients.

FIGURE A1

Passage of opioids legislation by state

We colour states by the year in which they passed an opioid distribution law or regulation.

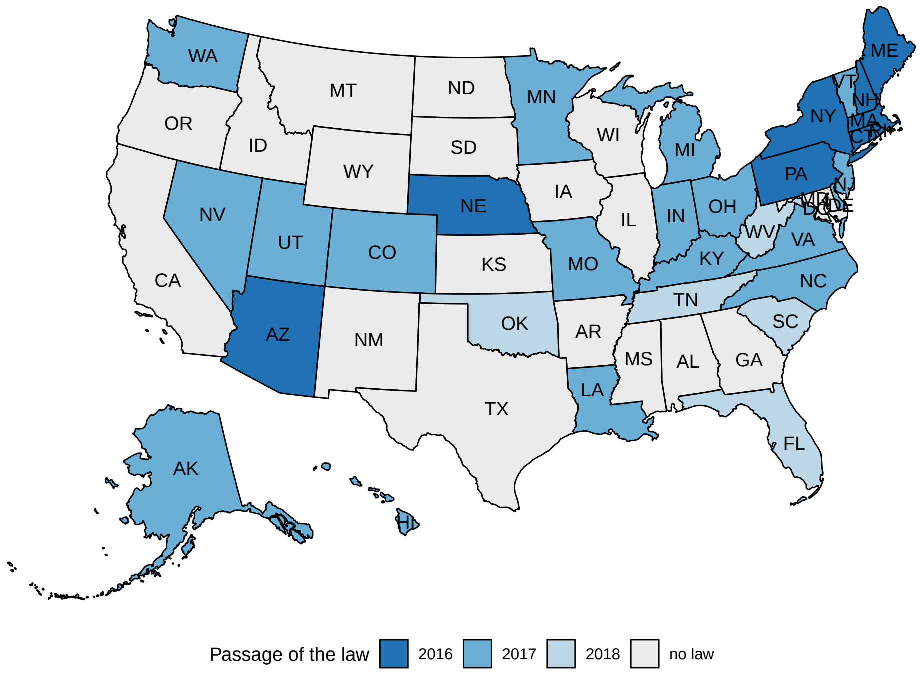


TABLE A3

County/State observations for opioid law introductions

The table reports the number of states that passed laws intended limit opioid abuse as well as the number of observations with data for home value, opioid deaths, and excess prescription, at the county level.

Treatment year	Treatment observations					
	Home Value		Opioid Death		Excess Prescriptions	
	States	Counties	States	Counties	States	Counties
2016	9	228	9	234	9	231
2017	18	973	18	996	18	974
2018	5	320	5	323	5	322

TABLE A4

Determinants of opioids state legislation

This is a cross-sectional regression with all 50 US states (s). The unit of observation is state. The dependent variable is an indicator variable equal to one if a state passed a opioid law or regulation between 2016 and 2018. Following **?**, independent variables include: Average state prescription rate between 2006 and 2015 per 100,000 people; Age adjusted overdose death rate, unemployment rate, ln(median household income in current dollars), poverty ratio, ln(GDP per capita in current dollars) at the state level as of 2015; Democratic and Republican are indicators that equal one if the state governor, state senate and state house are all Democratic, respectively all Republican, in 2015. In addition to **?**, we also include the following independent variables as of 2015 violent crime rate, property crime rate, ln(police expenditure per capita) and the ln(state level child welfare spending per capita) as of 2014. Standard errors are robust. *** indicates 1% significance, ** 5% significance, and * 10% significance.

	1	2	3	4
	State Law and Regulation Indicator			
Avg Prescription Rate$_s$	-0.003	-0.001	-0.002	-0.002
	(0.003)	(0.008)	(0.004)	(0.008)
Age Adjusted Overdose Death Rate$_s$	0.032***	0.032**	0.030***	0.034**
	(0.009)	(0.013)	(0.010)	(0.014)
Unemployment Rate$_s$		0.012		0.020
		(0.101)		(0.106)
Ln(Median Household Income)$_s$		0.020		-0.101
		(1.413)		(1.596)
Poverty Ratio$_s$		-0.020		-0.025
		(0.053)		(0.059)
Ln(GDP per capita)$_s$		0.416		0.435
		(0.655)		(0.703)
Violent Crime Rate$_s$		-8.619		-22.150
		(64.431)		(78.898)
Property Crime Rate$_s$		12.049		14.520
		(16.643)		(18.241)
Ln(Police per capita)$_s$		-0.188		-0.143
		(0.498)		(0.557)
Ln(Childwelfare State per capita)$_s$		0.070		0.091
		(0.102)		(0.122)
Democratic$_s$			0.001	-0.061
			(0.179)	(0.247)
Republican$_s$			-0.061	0.033
			(0.159)	(0.198)
R2	0.178	0.231	0.181	0.233
N	50	50	50	50

E. Correlations

TABLE A5

Opioid abuse & home values: Excluding pill mill counties

The unit of observation is county-year. The sample period is 2006 to 2018 in Panel A.1, B.1, A.3, and B.3 and 2007 to 2018 in Panel A.2 and B.2 due to missing observations in estimating excessive prescription. *We exclude counties with 5 or more pill mills in Panel A and counties with 8 or more pill mills in Panel B.* The dependent variable is a log percentage change of average county home values ($log(HV_t/HV_{t-x}) * 100$) over 3, 4 and 5 years. The key independent variable is the opioid death rate in Panel A.1 and B.1, the excess prescription rate in Panel A.2 and B.2 and the prescription rate in Panel A.3 and B.3. County controls include the male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. All independent variables are measured at the county level (c) at time $t-x$. Columns 1 to 3 include county and year fixed effects and columns 4 to 6 include state-year fixed effects. All variables are winsorized at the 2 and 98 % level. Standard errors are clustered at the county level. *** indicates 1% significance, ** 5% significance, and * 10% significance.

	1	2	3	4	5	6
	Percentage change in average home value over					
	3-years	4-years	5-years	3-years	4-years	5-years
Panel A: Excluding counties with 5 or more pill mills						
Panel A.1: Opioid mortality rates						
Opioid Death Rate$_{c,t-x}$	-0.013	$-0.030**$	$-0.033**$	$-0.013*$	$-0.019**$	$-0.020*$
	(0.009)	(0.012)	(0.015)	(0.007)	(0.009)	(0.011)
R2	0.747	0.784	0.814	0.836	0.853	0.853
N	16710	14636	12615	16710	14636	12615
Panel A.2: Excess prescription rates						
Excess Prescription Rate$_{c,t-x}$	-0.005	-0.009	$-0.016*$	$-0.003*$	-0.004	-0.004
	(0.006)	(0.008)	(0.009)	(0.002)	(0.002)	(0.003)
R2	0.778	0.807	0.834	0.839	0.850	0.844
N	14585	12514	10549	14585	12514	10549
Panel A.3: Prescription rates						
Prescription Rate$_{c,t-x}$	-0.007	-0.009	-0.014	$-0.003*$	-0.004	-0.003
	(0.006)	(0.008)	(0.009)	(0.002)	(0.002)	(0.003)
R2	0.747	0.783	0.812	0.837	0.853	0.853
N	16222	14145	12157	16222	14145	12157
County F.E.	Yes	Yes	Yes	No	No	No
Year F.E.	Yes	Yes	Yes	No	No	No
State-Year F.E.	No	No	No	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

	1	2	3	4	5	6
	Percentage change in average home value over					
	3-years	4-years	5-years	3-years	4-years	5-years
Panel B: Excluding counties with 8 or more pill mills						
Panel B.1: Opioid mortality rates						
Opioid Death Rate$_{c,t-x}$	-0.011	$-0.025{*}{*}$	$-0.026{*}$	$-0.015{*}{*}$	$-0.022{*}{*}$	$-0.024{*}{*}$
	(0.009)	(0.012)	(0.015)	(0.007)	(0.008)	(0.011)
R2	0.742	0.779	0.810	0.836	0.852	0.852
N	18439	16167	13952	18439	16167	13952
Panel B.2: Excess prescription rates						
Excess Prescription Rate$_{c,t-x}$	-0.008	$-0.013{*}$	$-0.022{*}{*}$	$-0.004{*}{*}$	$-0.005{*}{*}$	$-0.005{*}$
	(0.006)	(0.007)	(0.009)	(0.002)	(0.002)	(0.003)
R2	0.775	0.804	0.831	0.838	0.849	0.842
N	16130	13861	11702	16130	13861	11702
Panel B.3: Prescription rates						
Prescription Rate$_{c,t-x}$	$-0.010{*}$	$-0.014{*}$	$-0.021{*}{*}$	$-0.004{*}{*}$	$-0.005{*}{*}$	$-0.005{*}$
	(0.006)	(0.008)	(0.009)	(0.002)	(0.002)	(0.003)
R2	0.742	0.778	0.809	0.836	0.852	0.851
N	17951	15676	13494	17951	15676	13494
County F.E.	Yes	Yes	Yes	No	No	No
Year F.E.	Yes	Yes	Yes	No	No	No
State-Year F.E.	No	No	No	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

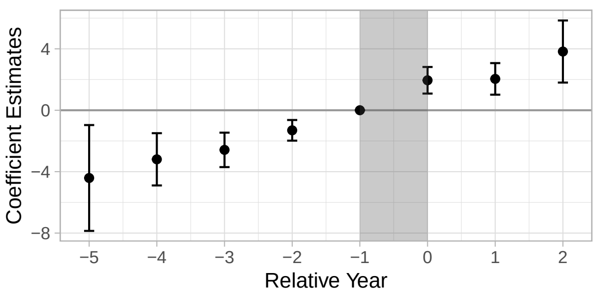
F. Difference-in-Differences

1. Additional DiD estimates

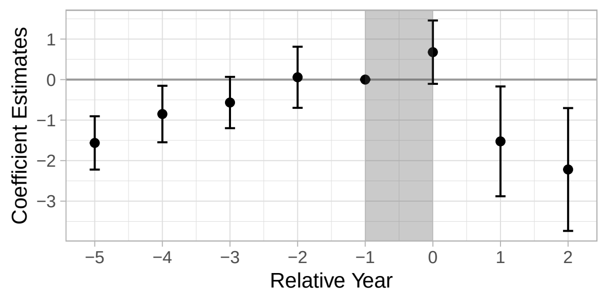
FIGURE A2

Impact of opioid limiting laws on illicit overdose deaths

The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is the annual illicit overdose death rate in Panel A and the one-year difference in illicit overdose death rate (in %) in Panel B. One year-lagged controls include male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We plot the interaction weighted total coefficient with a 95% confidence interval for each relative time period following **?**. Standard errors are clustered at the state level.



(a) **Illicit Death Rates**



(b) **1-Yr Difference Illicit Death Rate**

TABLE A6

Impact of opioid limiting laws:

Average effect of treatment on treated

The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is opioid death rate, prescription death rate, 1-year difference in opioid death rate (in %), absolute excess prescription, excess prescription rate and the log percentage change in average county home values. We estimate **?**'s interaction weighted (IW) regression and calculate average effect of treatment on treated (ATT). County controls include the male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. For the specification with absolute excess prescription we additionally control for county population. We include county and year fixed effects. Standard errors are clustered at the state level. *** indicates 1% significance, ** 5% significance, and * 10% significance.

	1 Opioid Death Rate	2 Prescription Death Rate	3 1-Yr Difference Opioid Death Rate	4 Absolute Excess Prescription	5 Excess Prescription Rate	6 Percentage Change Home Prices
ATT	1.441***	-0.692***	-0.870**	-6589.061***	-2.863***	0.672***
	(0.358)	(0.211)	(0.441)	(1582.890)	(0.717)	(0.152)
R2	0.671	0.537	0.076	0.983	0.950	0.616
N	15396	15396	15396	15091	15091	14695
County F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

TABLE A7

?: Estimates for the effect of opioid laws on home values

The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is the log percentage change in average county home values. We estimate a two-way fixed effects regression with relative time treatment dummies based on the passage of the law in column 1 as well as **?**'s interaction weighted regression in columns 2 to 5. Column 2 reports the sample share weighted average of the $CATT$ in columns 3 to 5. County controls include the male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. Standard errors are clustered at the state level. *** indicates 1% significance, ** 5% significance, and * 10% significance.

	Dependent Variable: Percentage Change in Average County Home Value				
Year Relative To	Fixed Effect	Interaction Weighted			
Legislation	Total	Total	CATT Treat-year 2016	CATT Treat-year 2017	CATT Treat-year 2018
-5	-0.523	-0.400			-0.400
	(0.964)	(1.021)			(1.021)
-4	-0.319	-0.405		-0.384	-0.467
	(0.534)	(0.499)		(0.612)	(0.774)
-3	-0.135	-0.303	0.636	-0.645	0.053
	(0.373)	(0.365)	(0.621)	(0.520)	(0.571)
-2	0.015	-0.061	-0.111	-0.140	0.210
	(0.174)	(0.178)	(0.455)	(0.243)	(0.265)
-1	0.000	0.000	0.000	0.000	0.000
0	0.427***	0.420***	0.488**	0.476**	0.203
	(0.154)	(0.160)	(0.241)	(0.198)	(0.434)
1	0.913***	0.783**	1.383***	0.640*	
	(0.302)	(0.305)	(0.311)	(0.370)	
2	1.620***	1.758***	1.758***		
	(0.361)	(0.390)	(0.390)		

FIGURE A3

Impact of opioid limiting laws on rent

The unit of observation is county-year. The sample period is 2013 to 2018. We collect median gross county rent data from the American Community Survey Five-year Estimates data. Gross rent is the sum of the contract rent plus estimated average monthly cost of utilities and fuels. We estimate Equation **??** where the dependent variable is log percentage change in median rent. Controls include one year-lagged male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We plot the interaction weighted total coefficient with a 95% confidence interval for each relative time period following **?**. Standard errors are clustered at the state level.

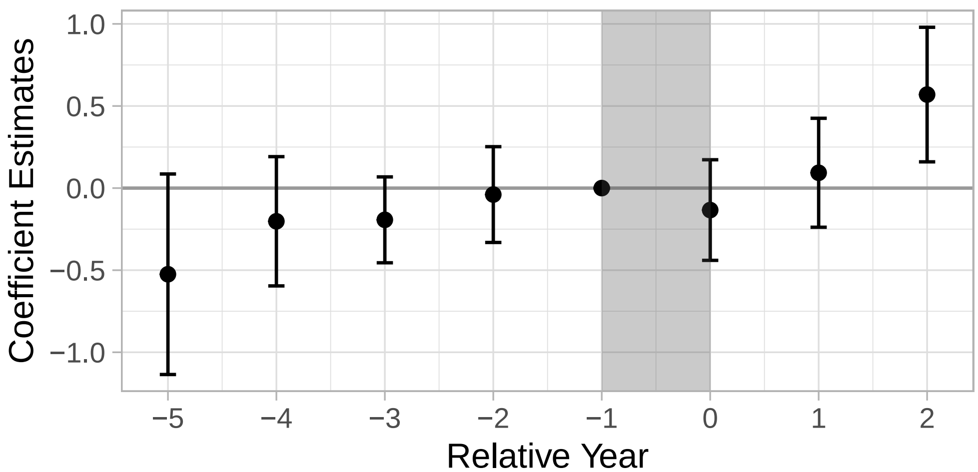
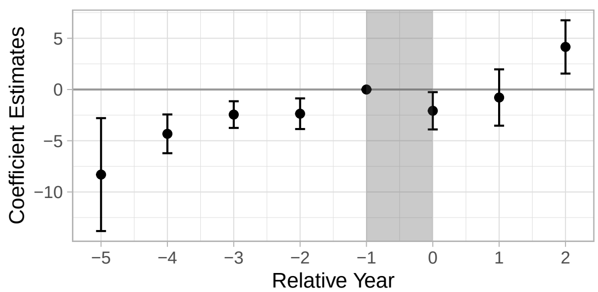


FIGURE A4

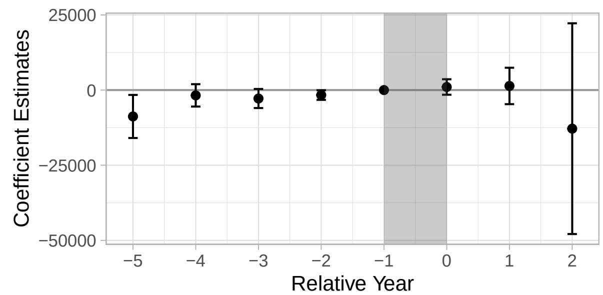
Impact of opioid limiting laws:

Placebo test with counties in the lowest abuse quantile

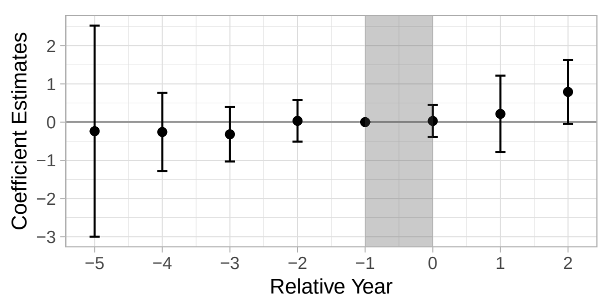
The unit of observation is county-year. The sample period is 2013 to 2018. We restrict the sample to control counties and treated counties that are in the lowest opioid abuse quintile based on average opioid death rates between 2013 and 2015. The dependent variable is the one-year difference in opioid death rate (in %) in Panel A, the excess absolute total county prescriptions in Panel B and the log percentage change in average county home values in Panel C. One year-lagged controls include male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. In Panel B, we additionally control for log total county population. We plot the interaction weighted total coefficient with a 95% confidence interval for each relative time period following **?**. Standard errors are clustered at the state level.



(a) **1-Yr Difference Opioid Death Rate**



(b) **Excess Absolute Prescriptions**



(c) **Percentage change in house price**

TABLE A8

Summary statistics: Economic mechanism variables

The unit of observation is county-year. The sample period is 2013 to 2018. We report descriptive statistics for house market dynamics variables in Panel A, namely 1-year percentage change in delinquent mortgages 90 or more days past due, 1-year percentage change in the number of home improvement loans, and 1-year percentage change in residential vacancy rates. We report descriptive statistics for migration variables in Panel B, namely the natural logarithm of migration inflow in number of households, number of individuals, and total income. All variables are winsorized at the 2 and 98% level.

Panel A: House market dynamics	Observations	Min	P25	Median	P75	Max	Mean	Std. Dev.
1-year Perc. Change Mtgs 90+ days past (in %)	2820	−54.418	−32.975	−22.002	−11.778	83.173	−22.101	16.111
1-year Perc. Change residential vacancy rate (in %)	15354	−103.301	−15.415	1.575	16.086	66.783	−2.530	32.483
1-year Perc. Change # home improvement loans (in %)	15004	−83.729	−8.554	−1.053	4.955	56.373	−3.285	19.151

Panel B: Migration	Observations	Min	P25	Median	P75	Max	Mean	Std. Dev.
Log(Migration inflow total income)	15388	7.048	9.603	10.458	11.629	16.132	10.729	1.508
Log(Migration inflow # households)	15396	3.689	5.948	6.690	7.748	11.867	6.938	1.335
Log(Migration inflow # individuals)	15396	4.431	6.669	7.384	8.427	12.348	7.625	1.305

TABLE A9

Impact of opioid limiting laws on opioid abuse and home values: By drug possession arrests

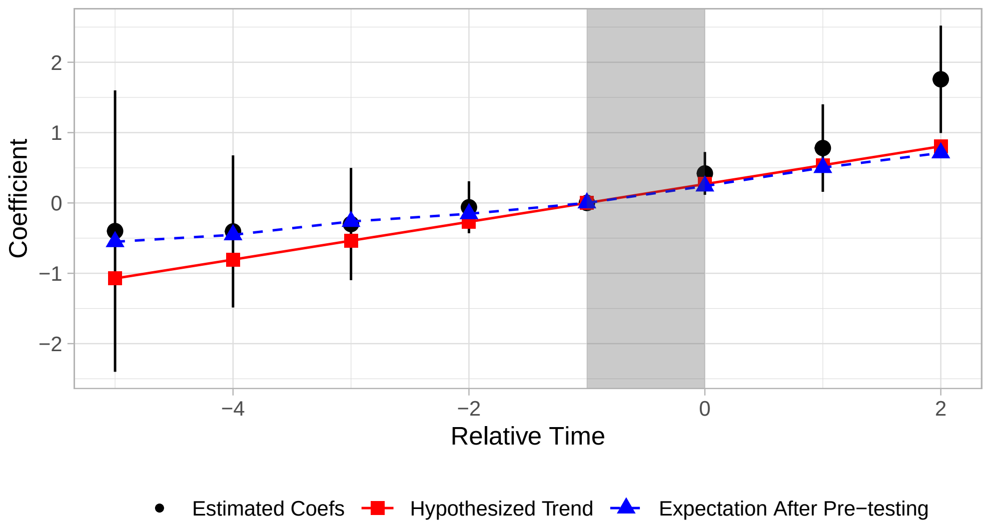
The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is excess prescription rate in columns 1 and 2, the one-year difference in opioid death rates (in %) in columns 3 and 4, and the log one-year percentage change of average county home values (in %) in columns 5 and 6. $\text{Post}_{t,c-1}$ is a dummy equal to one in the year of the passage of the law in the respective county and thereafter; Opium Arrest Rate StateTop_c is a dummy equal to one for counties whose average opium possession arrests per capita between 2009 to 2013 is in the top half within its respective state. Resid Opium Arrest Rate StateTop_c is a dummy equal to one for counties whose residualised average opium possession arrests per capita between 2009 to 2013 is in the top half within its respective state. We regress average opium possession arrests per capita between 2009 to 2013 demographic, urbanity and political variables, specifically the 2009 to 2013 average Male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, unemployment ratio, logarithm of average household income, the proportion of time between 2009 and 2013 the state had a democratic governor, and the 2010 population density. One-year lagged controls include: Male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We include county and year fixed effects. Standard errors are clustered at the state level. *** indicates 1% significance, ** 5% significance, and * 10% significance.

	1	2	3	4	5	6
	Excess Prescription rate		1-Yr Difference Death Rate		Percentage Change Home Prices	
$\text{Post}_{c,t}$	-0.849	-1.118	0.167	0.095	0.491	$0.588*$
	(1.712)	(1.442)	(0.441)	(0.455)	(0.295)	(0.297)
$\text{Post}_{c,t}$ X Opium	$-3.516***$		-0.497		$0.295***$	
Arrest Rate StateTop_c	(1.196)		(0.487)		(0.106)	
$\text{Post}_{c,t}$ X Resid Opium		$-3.174**$		-0.337		0.135
Arrest Rate StateTop_c		(1.288)		(0.433)		(0.093)
R2	0.949	0.949	0.0739	0.0734	0.608	0.608
N	13854	13806	14110	14062	13840	13792

FIGURE A5

Impact of opioid limiting laws on home values with hypothesized trend based on 50% power

The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is log percentage change in average county home values. We follow **?** and plot a linear violation of the pre trend based on a 50% power in red. Black are coefficients we find in our regression and blue are the expected coefficient we would find based on the hypothesized trend in red.



2. Goodman-Bacon Decomposition

We assess the extent to which our results are plagued by ”bad” comparisons, i.e. the weight and sign of *Later* vs *Earlier Treated* comparisons by executing the **?** decomposition. **?** highlights that the general estimator from a two-way fixed effects approach is a ”weighted average of all possible two-group/two-period (2x2) DiD estimators”. The main coefficient is therefore a combination of many different treatment effects with possible non-intuitive and, at worst, negative weights. To understand which 2x2 DiD estimators drives the aggregate results, we implement a **?** decomposition. We run the following regression with home value changes and opioid abuse measures as dependent variable:

$$y_{ct} = \alpha + \beta_1 Post_{ct} + \theta_c + \tau_t + \epsilon_{ct} \tag{1}$$

β_1 is the coefficient of interest. We have nine individual 2x2 DiD estimators. *Earlier* vs *Later Treated* 2x2 DiD estimators include *cohort 2016* vs *cohort 2017*, *cohort 2016* vs *cohort 2018*, and *cohort 2017* vs *cohort 2018*. *Later* vs *Earlier Treated* 2x2 DiD estimators include *cohort 2017* vs *cohort 2016*, *cohort 2018* vs *cohort 2017*, and *cohort 2018* vs *cohort 2016*. Finally, for the *Treated* vs *Untreated* 2x2 DiD estimators we have *cohort 2016* vs *Untreated*, *cohort 2017* vs *Untreated*, and *cohort 2018* vs *Untreated*. We calculate and then plot the weight each 2x2 DiD estimators takes in the total beta (β), as well as the individual coefficient of each 2x2 DiD estimator.

Figure A6 shows the decomposition for the percentage change in home values for the full sample. We can identify two patterns. First, the individual estimate from *Treated* vs *Untreated* units receive the greatest weight within the total beta. This is reassuring, as these are probably the cleanest comparisons. Second, coefficients from *Later* vs *Earlier Treated* tend to have the opposite sign compared to the other estimates in the home value decomposition. Given that the parallel trends in Figure **??** point towards a trend break rather than a unit shift, it is unsurprising that these ”bad” comparisons take on the opposite sign. However, the weight attached towards these coefficients is small with less than 9% for the whole group. Hence, their impact on the total beta is marginal.

We report the decomposition for the first stage variables opioid death rate, prescription death rate, 1-yr difference in opioid death rate, and excessive absolute prescriptions in Appendix Figure A7. The take-away is very similar to the percentage change in home values decomposition. Estimates from *Treated* vs *Untreated* units receive the greatest weight within the total beta. These results reassure us that the two-way fixed effect specification is not severely biased and thus allows us to draw conclusions from the heterogeneity analysis.

FIGURE A6

Goodman-Bacon decomposition of home values

The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is the log percentage change in average county home values. We show the **?** decompositions for the TWFE regression $y_{ct} = \alpha + \beta Post_{ct} + \theta_c + \tau_t + \epsilon_{ct}$. We do not include any controls in the regression.

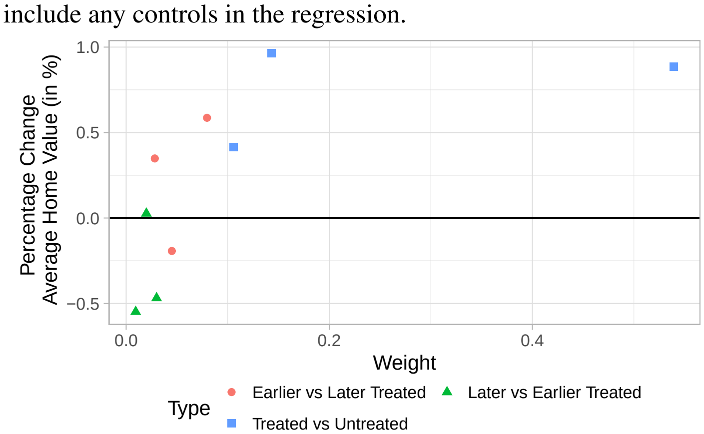
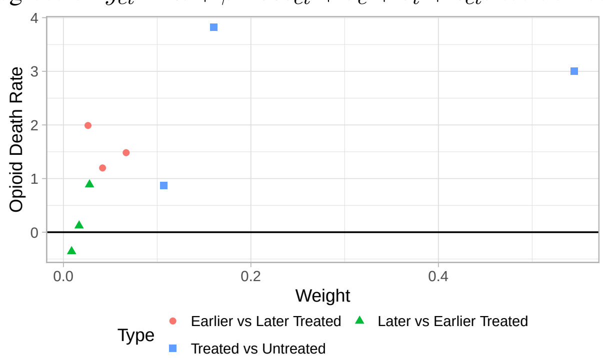


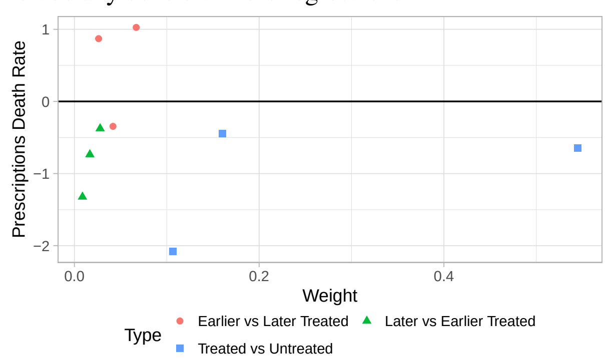
FIGURE A7

Goodman-Bacon decomposition of opioid abuse

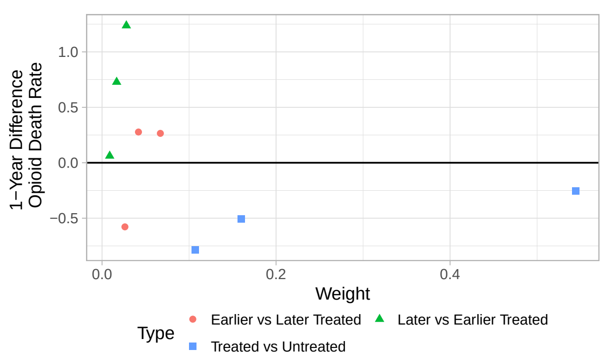
The unit of observation is county-year. The sample period is 2013 to 2018. The dependent variable is the annual opioid death rate in Panel A, the annual prescription death rate in Panel B, the one-year difference in opioid death rate in Panel C and the excess absolute total county prescriptions in Panel D. We show the **?** decompositions for the TWFE regression $y_{ct} = \alpha + \beta Post_{ct} + \theta_c + \tau_t + \epsilon_{ct}$. We do not include any controls in the regression.



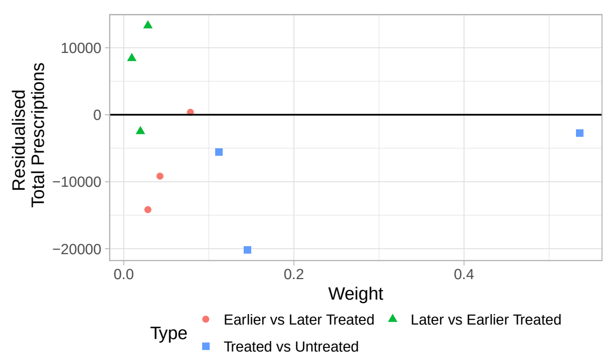
(A) Opioid Death Rate



(B) Prescription Death Rate



(C) 1-Yr Diff Opioid Death Rate (in %)



(D) Excess Absolute Prescriptions

G. Additional RD estimates: opioid laws and regulations

TABLE A10

Impact of opioid limiting laws on opioid abuse & home values:

Regression discontinuity around state borders without controls

The unit of observations is county. In Panel A, the dependent variables is a one or two-year difference in excess prescription rates in column 1 to 4 and a one or two-year difference in opioid death rates in columns 5 to 8. For treated counties, we calculate the difference from the treatment year - 1 to the treatment year and treatment year + 1 respectively. For control counties, we calculate the difference from 2015 to 2016 or 2017, as the first law was passed in 2016. In Panel B, the dependent variables is a one or two-year percentage change in home values. For treated counties, we calculate the percentage change from the treatment year - 1 to the treatment year and treatment year + 1 respectively. For control counties, we calculate the percentage change from 2015 to 2016 or 2017, as the first law was passed in 2016. *We do not include any controls.* We follow **?** to choose the optimal bandwidth and use robust standard errors. *** indicates 1% significance, ** 5% significance, and * 10% significance.

Panel A: Opioid abuse	1	2	3	4	5	6	7	8
	Difference in Excess Prescription Rates over				Difference in Opioid Death Rates over			
	1 year		2 years		1 year		2 years	
RD Estimate	-4.807***	-4.996***	-2.613	-2.351	-4.307**	-4.359***	-5.124***	-5.098***
	(1.453)	(1.554)	(2.044)	(2.465)	(1.961)	(1.683)	(1.799)	(1.658)
Observations	2573	2573	2240	2240	2783	2783	2445	2445
MSEBandwidth	98	174	136	191	76	181	86	202
Effective LHS Obs	576	847	731	886	509	932	570	975
Effective RHS Obs	623	983	610	777	530	1070	448	875
Polynominal Order	1	2	1	2	1	2	1	2

Panel B: Home Values	1	2	3	4
	Percentage Change in Home Values over			
	1 year		2 years	
RD Estimate	1.403***	1.322***	2.696***	2.665***
	(0.406)	(0.416)	(0.696)	(0.760)
Observations	2543	2543	2209	2209
MSEBandwidth	115	235	130	232
Effective LHS Obs	603	873	668	871
Effective RHS Obs	715	1211	602	910
Polynominal Order	1	2	1	2

TABLE A11

Impact of opioid limiting laws on opioid abuse & home values: RD around state borders with fixed 150km bandwidth

The unit of observations is county. In Panel A, the dependent variables is a one or two-year difference in excess prescription rates in column 1 to 4 and a one or two-year difference in opioid death rates in columns 5 to 8. For treated counties, we calculate the difference from the treatment year - 1 to the treatment year and treatment year + 1 respectively. For control counties, we calculate the difference from 2015 to 2016 or 2017, as the first law was passed in 2016. In Panel B, the dependent variables is a one or two-year percentage change in home values. For treated counties, we calculate the percentage change from the treatment year - 1 to the treatment year and treatment year + 1 respectively. For control counties, we calculate the percentage change from 2015 to 2016 or 2017, as the first law was passed in 2016. We include the following control variables as of 2015: male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. *We set a fixed bandwidth of 150km*, which is in between the minimum and maximum optimal bandwidth estimates in the main specifications in Table **??**. Standard errors are clustered at the state level. *** indicates 1% significance, ** 5% significance, and * 10% significance.

Panel A: Opioid abuse								
	1	2	3	4	5	6	7	8
	Difference in Excess Prescription Rates over				Difference in Opioid Death Rates over			
	1 year		2 years		1 year		2 years	
RD Estimate	-4.551***	-3.226	-3.263	1.350	-3.907**	-5.266*	-5.758***	-8.632***
	(1.437)	(2.210)	(2.511)	(3.373)	(1.799)	(3.101)	(1.935)	(3.227)
Observations	2361	2361	2043	2043	2398	2398	2082	2082
MSEBandwidth	150	150	150	150	150	150	150	150
Effective LHS Obs	700	700	699	699	708	708	708	708
Effective RHS Obs	794	794	583	583	811	811	602	602
Polynominal Order	1	2	1	2	1	2	1	2

Panel B: Home Values				
	1	2	3	4
	Percentage Change in Home Values over			
	1 year		2 years	
RD Estimate	1.207**	1.086	2.308***	2.377*
	(0.480)	(0.831)	(0.865)	(1.433)
Observations	2326	2326	2012	2012
MSEBandwidth	150	150	150	150
Effective LHS Obs	672	672	672	672
Effective RHS Obs	797	797	589	589
Polynominal Order	1	2	1	2

TABLE A12

Impact of opioid limiting laws on opioid abuse & home values: RD around state borders excluding pill mill counties

The unit of observations is county. *We exclude counties with 5 or more pill mills in Panel A and counties with 8 or more in Panel B.* In Panel A.1 And B.1, the dependent variables is a one or two-year difference in excess prescription rates in column 1 to 4 and a one or two-year difference in opioid death rates in columns 5 to 8. For treated counties, we calculate the difference from the treatment year - 1 to the treatment year and treatment year + 1 respectively. For control counties, we calculate the difference from 2015 to 2016 or 2017, as the first law was passed in 2016. In Panel A.2 and B.2, the dependent variables is a one or two-year percentage change in home values. For treated counties, we calculate the percentage change from the treatment year - 1 to the treatment year and treatment year + 1 respectively. For control counties, we calculate the percentage change from 2015 to 2016 or 2017, as the first law was passed in 2016. We include the following control variables as of 2015: male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We follow **?** to choose the optimal bandwidth and use robust standard errors. *** indicates 1% significance, ** 5% significance, and * 10% significance.

Panel A: Excluding counties with 5 or more pill mills

Panel A.1: Opioid abuse

	1	2	3	4	5	6	7	8
	Difference in Excess Prescription Rates over				Difference in Opioid Death Rates over			
	1 year		2 years		1 year		2 years	
RD Estimate	-4.587***	-4.474***	-3.986*	-3.557	-4.565**	-3.741*	-6.161***	-5.621***
	(1.348)	(1.592)	(2.324)	(2.636)	(2.126)	(1.944)	(2.035)	(1.938)
Observations	2051	2051	1794	1794	2088	2088	1833	1833
MSEBandwidth	108	175	125	197	75	170	84	188
Effective LHS Obs	520	706	581	740	391	710	432	732
Effective RHS Obs	533	780	447	628	390	786	317	629
Polynominal Order	1	2	1	2	1	2	1	2

Panel A.2: Home Values

	1	2	3	4
	Percentage Change in Home Values over			
	1 year		2 years	
RD Estimate	1.309***	1.358***	2.621***	2.505***
	(0.461)	(0.449)	(0.789)	(0.805)
Observations	2018	2018	1765	1765
MSEBandwidth	91	191	98	198
Effective LHS Obs	442	698	471	710
Effective RHS Obs	456	828	365	636
Polynominal Order	1	2	1	2

Panel B: Excluding counties with 8 or more pill mills

Panel B.1: Opioid abuse

	1	2	3	4	5	6	7	8
	Difference in Excess Prescription Rates over				Difference in Opioid Death Rates over			
	1 year		2 years		1 year		2 years	
RD Estimate	-4.670***	-4.626***	-4.103**	-3.909	-5.068**	-4.480**	-6.330***	-5.674***
	(1.248)	(1.515)	(1.972)	(2.523)	(2.163)	(1.898)	(2.049)	(1.887)
Observations	2185	2185	1903	1903	2222	2222	1942	1942
MSEBandwidth	114	177	151	196	70	165	79	186
Effective LHS Obs	547	733	673	763	380	724	418	751
Effective RHS Obs	592	842	547	669	395	823	325	660
Polynominal Order	1	2	1	2	1	2	1	2

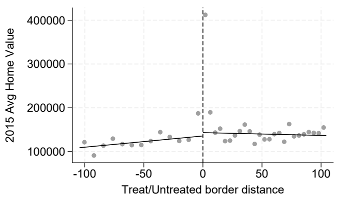
Panel B.2: Home Values

	1	2	3	4
	Percentage Change in Home Values over			
	1 year		2 years	
RD Estimate	1.304***	1.341***	2.566***	2.382***
	(0.491)	(0.448)	(0.829)	(0.795)
Observations	2152	2152	1874	1874
MSEBandwidth	81	178	90	191
Effective LHS Obs	406	705	449	722
Effective RHS Obs	436	849	354	659
Polynominal Order	1	2	1	2

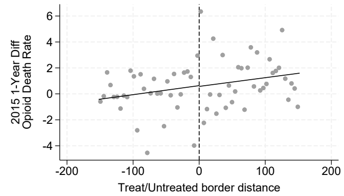
FIGURE A8

Regression discontinuity plots for covariates in 2015

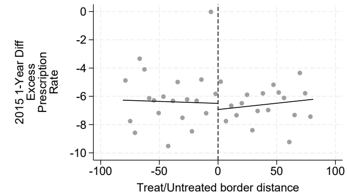
The unit of observation is county. The dependent variables are levels in home value (Panel A), 1-year difference in opioid death rate, 1-year difference in excess prescription rate (Panel C), labour participation rate (Panel D), unemployment rate (Panel E) and poverty ratio (Panel F) as of 2015. We follow **?** to choose the optimal bandwidth and do not include any other controls. We use robust standard errors. No estimate is significant at the 10% level.



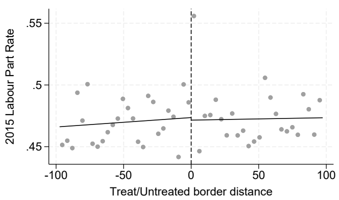
(a) **Home Value**



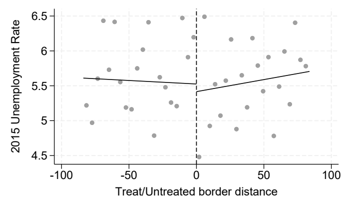
(b) **1- Year Diff. Opioid Death Rate**



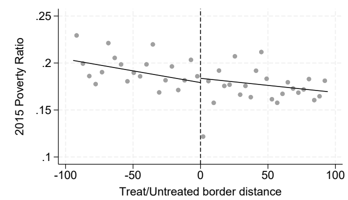
(c) **1- Year Diff. Excess Prescription Rate**



(d) **Labour participation rate**



(e) **Unemployment Rate**



(f) **Poverty Ratio**

H. Instrumental variable strategies

? employ instrumental variables to establish causal effects of opioid abuse on municipal finance conditions. In this section, we follow their approach and apply two alternative instrumental variables. The first one is based on the aggressiveness of Purdue's marketing of the reformulated oxycodone (branded as OxyContin). The second is based on the ”leaky” supply chains and the desirability of the product by addicts.

To assess the aggressiveness of Purdue's marketing, we obtain data on the quantity of OxyContin distributed to three-digit zip codes.[8] We calculate the percentage change in the quantity of OxyContin distributed by Purdue Pharma between 1997 and 2004 and use this as instrument for prescription rates after linking three-digit zip codes to counties. Internet Appendix Table A13 Panel A shows the results. The first stage regression shows a strong positive association between the aggressiveness of Purdue marketing and excess prescription rates, respectively opioid death rates, using a 5-year lag. In the second stage regression we find a negative effect of instrumented opioid abuse on home values, which is consistent with our previous estimates. These estimates are significant when we instrument the opioid death rate.

The second IV builds on two components. The first component is the type of opioid: we focus on those opioids with the highest addictive potential and the highest desirability to addicts. The second component is the distribution channel for these pills: we focus on pills sold through pharmacies with the least oversight and most potential for abuse – ”leakiest” supply chains. Opioid abuse in contrast to more legitimate opioid use for treatment should be highest under such conditions. The Washington Post published detail pain pill transaction data between 2006 and

[8] We thank **?** for sharing their data.

2014 based on the Drug Enforcement Administration's Automation of Reports and Consolidated Orders System.[9] Within this database, we focus on strong types of opioids, namely fentanyl, hydromorphone, levorphanol, oxycodone, and oxymorphone, that have the highest addictive potential and the highest desirability to addicts. Further, we consider only ”retail” pharmacies as distribution channel, as retail pharmacies have the least oversight and therefore most potential for abuse. Within this opioid and distribution subset, we calculate the annual distribution of morphine milligram equivalent (MME) per county. Standardizing opioid strength using the MME value for each pill (e.g., oxycodone is 50% stronger than hydrocodone, so it has an MME multiplier of 1.5) allows us to account for for different dosages. Finally we scale the total annual distribution by 1000 county inhabitants.

Internet Appendix Table A13 Panel B documents a strong positive association between availability and desirability of opioids and excess prescription rates, respectively opioid death rates. The second stage regressions show a mostly negative relation between instrumented opioid abuse and home values. These estimates are significant in the state-year fixed effect specification.

Using instrumental variables has the advantage of using a source of exogenous variation in a variable that is endogenous. An identifying assumption of this methodology is that the instrument is not correlated with the outcome variable through any other channel but the one considered in the analysis. In this case we rely on the assumption that Purdue marketing aggressiveness and supply chain conditions are not related to local home values through any other economic mechanism than opioid abuse. The findings presented in this section using the IV approach provide support for our baseline results.

[9] https://wpinvestigative.github.io/arcos/

TABLE A13

Instrumental variable regressions

The unit of observation is county-year. The sample period is 2006 to 2018 for specifications where we instrument for opioid death rate and 2007 to 2018 for specifications where we instrument for excess prescription rate. The dependent variable is the 5-year percentage changes in home values from $t-5$ to t. We run a two-stage least squares regression with *Purdue Marketing* as instrument in Panel A and leaky supply chains (*Supply Chain*) as instrument in Panel B. *Purdue Marketing* is defined as growth in pill distribution between 1997 and 2004. Given that this variable is constant at the county level, we can only run the specification with state-year fixed effects. *Supply Chain* is defined as annual MME per 1000 county inhabitants distribution of strong types of opioid to retail pharmacies. As this variable is time-varying, we include county and year fixed effects in columns 1, 2, 5, and 6 and state-year fixed effects in columns 3, 4, 7, and 8. Five-year lagged controls include male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. We cluster standard errors at the county level. *** indicates 1% significance, ** 5% significance, and * 10% significance.

Panel A: Purdue marketing instrument

	1	2	3	4
	IV: Stage 1	IV: Stage 2	IV: Stage 1	IV: Stage 2
	Excess Prescription rate	5-Yr PC Home value	Opioid death rate	5-Yr PC Home Value
PurdueMarketing_c	$1.290***$		$0.120***$	
	(0.303)		(0.041)	
Est. Excess Prescription $\text{Rate}_{c,t-5}$		-0.048		
		(0.033)		
Est. Opioid Death $\text{Rate}_{c,t-5}$				$-0.618*$
				(0.374)
State-Year F.E.	yes	yes	yes	yes
Controls	Yes	Yes	Yes	Yes
R2	0.359	-0.0235	0.372	-0.320
N	13068	13068	15529	15529
F-statistic	103.3		31.7	

Panel B: Supply Chain instrument

	1	2	3	4	5	6	7	8
	IV: Stage 1	IV: Stage 2	IV: Stage 1	IV: Stage 2	IV: Stage 1	IV: Stage 2	IV: Stage 1	IV: Stage 2
	Excess Prescription rate	5-Yr PC Home value	Excess Prescription rate	5-Yr PC Home Value	Opioid death rate	5-Yr PC Home value	Opioid death rate	5-Yr PC Home Value
$\text{SupplyChain}_{c,t-5}$	$0.334***$		$1.085***$		$0.102***$		$0.134***$	
	(0.056)		(0.094)		(0.027)		(0.019)	
Est. Excess Prescription $\text{Rate}_{c,t-5}$		-0.041		-0.012				
		(0.065)		(0.008)				
Est. Opioid Death $\text{Rate}_{c,t-5}$						-0.223		-0.086
						(0.214)		(0.059)
County F.E.	yes	yes	no	no	yes	yes	no	no
Year F.E.	yes	yes	no	no	yes	yes	no	no
State-Year F.E.	no	no	yes	yes	no	no	yes	yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.405	0.177	0.405	0.0251	0.635	0.159	0.360	0.0215
N	10494	10494	10573	10573	12837	12837	12907	12907
F-statistic	353.9		1281.4		172.7		694.4	

I. Other laws and regulations

Below is a list of relevant housing laws that were passed during the 2016-2018 period. In brackets, we list if the state is in our treatment or control group, as well as the year in which the opioid limiting laws were passed in case of treatment. In curly brackets, we denote the direction of the predicted change in housing values.

- California (2016 / Law) - Affordable Housing Zoning: California passed a series of laws to streamline affordable housing development, including Senate Bill 35, which limits local governments' ability to deny affordable housing projects in areas not meeting their housing production goals. [Control.] {+/-}

- Colorado (2017 / Zoning Reform) - Land Use and Affordable Housing: Colorado enacted legislation that encouraged municipalities to adopt zoning reforms to promote affordable housing, including density bonuses and streamlined permitting for affordable housing developments. [Treatment, 2017.] {+/-}

- Illinois (2018 / Law) - Accessory Dwelling Unit (ADU) Legislation: Illinois passed legislation allowing for the development of accessory dwelling units (ADUs), such as granny flats and garage apartments, in an effort to increase affordable housing options. [Control.] {-}

- Massachusetts (2016 / Law) - Zoning Reform for Housing Production: The Massachusetts Legislature passed Chapter 40R, a law that incentivizes municipalities to adopt zoning that allows for more housing production, particularly near transit stations, in response to a growing housing crisis. [Treatment, 2016.] {-}

- Minneapolis, Minnesota (2018 / City Level Zoning Ordinance) - Ending Single-Family Zoning: The Minneapolis 2040 Plan was adopted, which included a landmark zoning reform that ended single-family zoning citywide, allowing for duplexes and triplexes on all residential lots. [Treatment, 2017.] {-}
- New York City (2016 / City Level Zoning Regulation) - Mandatory Inclusionary Housing: New York City adopted Mandatory Inclusionary Housing (MIH) requirements as part of its Zoning for Quality and Affordability (ZQA) plan. This regulation requires certain new residential developments to include affordable housing units. [Treatment, 2016.] {+/-}
- Seattle, Washington (2019 / City Level Zoning Regulation) - Mandatory Housing Affordability (MHA): Seattle implemented the Mandatory Housing Affordability program, which upzoned many parts of the city while requiring developers to either include affordable housing in their projects or contribute to a city fund for affordable housing. [Treatment, 2017.] {-}
- Texas (2017 / Law) - Municipal Annexation Reform: Senate Bill 6 was passed in Texas, significantly limiting the power of municipalities to annex land without the consent of the property owners. This law affected zoning practices by reducing the ability of cities to control land use through annexation. [Control.] {+/-}
- Virginia (2019 / Law) - Proffers Reform: Virginia passed a law reforming the way local governments can use proffers conditions imposed on developers as part of the zoning approval process to ensure they are reasonable and directly related to the impact of the proposed development. [Control.] {+/-}

- Washington, D.C. (2016 / Zoning Regulation) - Zoning Code Update: Washington, D.C., updated its zoning code for the first time in over 50 years, which included new regulations to promote higher density development in certain areas and reduce parking requirements, thereby encouraging more sustainable urban growth. [Control.] {-}