Opioid Crisis and Real Estate Prices*

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Abstract

This study investigates the impact of opioid abuse on real estate prices using variation in opioid prescription rates induced by the staggered passage of opioid-limiting legislation. Employing difference-in-differences, regression discontinuity design, and instrumental variables, we find that effective anti-opioid legislation results in an increase in county-level house prices. This is due to a decrease in mortgage delinquencies and vacancy rates, and an increase in home improvement loans and population inflow. These results are consistent with an improvement in the quality of local real estate, and an increase in the local demand for space being the main drivers of the increase in real estate prices. Our results highlight the need for policy interventions to address the opioid epidemic's economic costs.

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1 Introduction

Opioid usage in the United States has surged over the past two decades, resulting in nearly 500,000 deaths from opioid-related 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 use disorders related to prescription opioid pain relievers, with documented public health and economic consequences.[2] While the existing literature has mainly focused on analyzing the role of economic conditions in the bulging opioid crisis and "deaths of despair" (Case and Deaton, 2015; Finkelstein et al., 2022), fewer studies have examined the impact of the opioid crisis on the real economy (Ouimet et al., 2021; Harris et al., 2019; Cornaggia et al., 2021). We contribute to the understanding of this problem by estimating the impact of opioid abuse on real estate values.

Opioid abuse has been linked to human capital loss and direct costs to families, especially among non-college educated population (Harris et al., 2019; Alpert et al., 2021). Prolonged opioid use can result in reduced labor productivity, leading to lower household income and job loss. As a result, families may be unable to invest in their homes or meet mortgage payments, resulting in an increase in mortgage delinquencies and the number of vacant properties. This can ultimately affect the quality and value of homes in the area. Additionally, opioid abuse can lower the attractiveness of the neighborhood by reducing residents' investments in their properties, or through residential sorting (Ahern and Giacolleti, 2022; Hacamo, 2021).

Understanding the impact of opioid abuse on home values is important because these can act as an indicator of the local economic situation and outlook. Moreover, for a significant number of households, houses are the most valuable asset on their balance sheet (Favilukis et al., 2017). Rising home equity has been shown to help alleviate financing frictions and access to credit (Mian and Sufi, 2011; DeFusco, 2018), and housing collateral has been documented to spur entrepreneurship, new businesses and job creation, as it gives home owners a pledgeable asset that can be used for securing credit (Jensen et al., 2022; Adelino et al., 2015).

To estimate the sensitivity of home values to the usage of prescription opioids we measure home values at the county level using the Zillow Home Value Index (ZHVI), and we use historic opioid prescriptions at the county level reported by the CDC between 2006 and 2018. We start by documenting a negative correlation between home values and opi-

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

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

oid prescription rates in the short run and over a 5-year horizon. To do so, we exploit within county (over time) variation, as well as within state-year variation. Using within county variation, we estimate that a one standard deviation increase in dispensed opioid prescriptions per 100 people is associated with an up to 1.35 percentage points cumulative decrease in home values over the following 5 years. Considering that the average home value in our sample is $140,034, this translates into a $1,890 decrease in housing wealth using the within county estimates.[3]

Since 2016, and in response to the opioid crisis, several US states passed laws and regulations limiting opioid prescriptions by physicians to address prescription drug misuse, abuse and overdoses. These laws generally aim to restrict duration or total dosage, in particular for first-time prescriptions, to prevent overly generous prescription and thus reduce addiction and long-term opioid usage.[4] The staggered adoption of regulation by different states arguably induces exogenous variation in prescriptions, as most evidence suggests these are driven by supply (Finkelstein et al., 2022) and not as much by demand for opioids (Currie et al., 2019; Paulozzi et al., 2014).[5] We implement a difference-in-differences empirical test, in which we compare the changes in home values in years before and after the passage of the law (*the treatment*) in *treated* versus *control* counties. We first establish that the passage of these laws indeed reduced opioid prescriptions. Then, we show that house values in *treated* counties increased on average upon the adoption of these regulations. We document that house values increased by 0.42 percentage points more in the year of the passage of the law, 0.81 percentage points more in the first year, and 1.78 percentage points more in the second year after the passage of the law.

To address the concern in staggered difference-in-differences estimates that already treated units act as control for units that are treated at a later stage (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021), we use interaction weighted estimates by Sun and Abraham (2021). Importantly, as our identifying assumption, we show that states for which the law has passed, and the ones for which it has not, are on parallel trends in terms of home value changes before the passage of the law.[6] Last, we implement a Goodman-Bacon (2021) decomposition and show that our estimates receive a greater weight from

[3]As a reference point, in 2018, 40% of Americans were not able to cover a $400 emergency with cash, savings or a credit-card charge that they could quickly pay off. https://www.federalreserve.gov/publications/files/2018-report-economic-well-being-us-households-201905.pdf

[4]Nevertheless, there may be unintended consequences of these laws. Patients that are unable to access medical opioids may turn to heroin or other illicit drugs as last resort to reduce their pain.

[5]Ouimet et al. (2021) 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.

[6]We also show that the confidence interval of the t=+2 coefficient falls outside of a linear trend based on 50% power following Roth (2022).

the difference between *treated* units and *untreated* ones. This provides further assurance to our estimates, as these differences provide arguably the cleanest comparison.

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 have a measure of treatment intensity at the same level as the observed outcome. First, we show that the prescription reduction in treated states is driven by the counties with the highest *ex ante* opioid supply within a state, as proxied by the number of ex-ante physicians per capita, and by opioid related payments to physicians by pharmaceutical companies, respectively.[7] Second, we find that home values rise significantly more in these counties upon the passage of the law. Taken together, these results suggest that variation in opioid prescription rates mostly drive the observed change in home values at the county level, and not the other way around.

To explore the possible underlying mechanisms driving the link between opioid prescriptions and home value changes, we study the effect of opioid abuse on delinquent mortgages, vacancy rates and home improvement loans. Delinquent mortgages have been shown in the literature to have an impact on home values and could generate negative price spill overs to non-distressed neighbouring houses (e.g Gupta, 2019; Campbell et al., 2011; Anenberg and Kung, 2014). We show that following the staggered adoption of opioid limiting laws the rate of change in mortgage delinquency rates was by about 6.17 percentage points lower on average one year after the passage of the laws in *treated* counties, relative to the control group. We also find that the relative percentage of home improvement loans increased, while the residential vacancy rates decreased significantly more one year after the passage of the laws in *treated* counties, relative to the control group.

In additional analysis, we show that counties that had a higher exposure to opioid abuse, subsequently experience a larger outflow of (high-income) households, relative to those with lower exposure to the opioid crisis. This set of results suggests that the degree of neighborhood impoverishment in response to opioid abuse encourages the unaffected to leave and the affected to stay, consistent with Ambrus et al. (2020) predictions. These findings also illustrate the potential economic costs of spatially correlated shocks induced by a pandemic (Gupta et al., 2022), when there are significant externalities from neighbours' socio-economic status. Passage of opioid-limiting laws reverses this trend: following treatment, we observe a significantly larger population and income inflow into treated counties. Taken together, our findings suggest a broader set of channels in which lost labor productivity and household income are one of the drivers of how opioid abuse impacted home values via delinquent mortgages and lower home improvement investments, but

[7] Finkelstein et al. (2022) show that number of physicians per capita is a significant predictor of the propensity to prescribe opioids at the county level, while Engelberg et al. (2014) find similar evidence in case of opioid related payments to physicians by pharmaceutical companies.

also through negative neighborhood externalities which resulted in spatial redistribution of households.

In additional tests, we address potential measurement concerns and use additional empirical strategies. We use different proxies for opioid abuse, including opioid-related overdose deaths, and we find similar results to our baseline estimates. We also show similar results when we exclude counties that are most likely to contain "pill-mills".

As an alternative approach to the difference-in-difference estimate, we utilize spatial regression discontinuity design to estimate the impact of opioid-limiting laws. We compare border counties in states that passed the laws with neighboring counties on the other side of the state border that were not subject to this change. Our results are consistent with the difference-in-differences estimates.

Because our main approach exploits negative variation in opioid usage driven by the passage of the laws, we use an additional alternative methodology. We follow Cornaggia et al. (2021) and use instrumental variables to estimate the impact of opioid abuse on home values. We instrument opioid abuse using two alternative sources of variation. The first instrument is based on the aggressiveness of Purdue marketing of the product Oxycontin, which induced excessive prescription rates. The second one is based on "leaky" supply chains and the desirability of the product by patients when compared to less available and less attractive pain killers. Overall, the findings using this instrumental variable approach provide support for our baseline results.

Estimating the aggregate economic effects using our empirical approach is a challenging task, given the absence of a general equilibrium model. Although beyond the scope of our study, we present back-of-the-envelope calculations of the aggregate economic impact based on our estimates, assuming no other effects of health on wealth and general equilibrium considerations that may have occurred in the economy due to regulatory changes. Our findings indicate that the loss in housing wealth induced by the opioid epidemic between 2006 and 2018 was at least \$146 billion, using county and year variation, or \$36 billion using state-year variation. By contrast, in 2022, Purdue Pharma agreed to pay \$6 billion as part of the settlement to compensate several US states for the damages associated with the opioid crisis.

1.1 Literature

Existing evidence on drivers of demand for opioid prescriptions has been mixed. Most of the literature suggests that the observed patterns in opioid usage have been driven by variation in supply of prescription opioids. Since Case and Deaton (2015) 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 (Currie et al., 2019). Finkelstein et al. (2022) show that the differences in the supply of prescription opioids from doctors is a key contributor to opioid abuse, as opposed to patient-specific factors such as mental health or poor economic prospects. The idea that supply-side factors are important determinants of opioid abuse is corroborated by Alpert et al. (2021), who show that the introduction and marketing of OxyContin were important determinants of the opioid crisis. Paulozzi et al. (2014) conclude that opioid prescription rates cannot be explained by variation in the underlying health of the population and instead suggest that the patterns reflect the lack of a consensus among doctors on best practices when prescribing opioids.

Our paper also contributes to the literature on the impact of the opioid crisis on the U.S. economy. Harris et al. (2019), Van Hasselt et al. (2015) and Florence et al. (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 to lost labor productivity. Cornaggia et al. (2021), Li and Zhu (2019) and Jansen (2022) document relevant financial effects of the crisis, including the impact of opioid abuse on municipal bond rates and auto loans conditions. Our paper is related to D' Lima and Thibodeau (2022). Using prescription data from Ohio between 2006 and 2012 when prescribed opioids were most problematic, they document a negative association between home values and opioid usage. Our paper analyzes the magnitudes of these effects, as well as the economic mechanisms behind it, using a national data sample and an identification strategy based on the staggered introduction of opioid supply regulation in a later stage of the opioid crisis.

More broadly, we contribute to the literature that examines the effects of public health conditions on real estate and asset markets, i.e., the impact of health on wealth. Tyndall (2021) studies house price effects of legalized recreational marijuana in Vancouver, Canada, and finds that introduction of marijuana dispensaries imposes a negative price effect on nearby properties. Wong (2008) investigates the effect of the 2003 Hong Kong Severe Acute Respiratory Syndrome (SARS) epidemic on housing markets to find that prices declined by 1%-3% for affected housing complexes. Using data from 7th-century Amsterdam plague-, and 19th-century Paris cholera outbreaks, Francke and Korevaar (2021) show that the outbreaks resulted in large declines in home values, and smaller declines in rent prices. Our paper belongs to the nascent line of research that studies the effects of (global) pandemics on real estate and housing markets (Gupta et al., 2022).

2 Opioid crisis background

The opioid crisis in the US evolved in three waves (Maclean et al., 2021). 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.[8] 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 (Chou et al., 2014). In 2016, the CDC issued a new policy recommendation for prescribing opioids advising amongst others to maximize non-opioid treatment.[9] To address the opioid epidemic the TJC revised and issued new standards on the treatment of pain in 2017.[10] In October of 2017, the US government declared opioid crisis a public health emergency.

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 (Buchmueller and Carey, 2018; Meara et al., 2016; Islam and McRae, 2014). Recent measures were more drastic

[8] https://www.medpagetoday.com/publichealthpolicy/publichealth/57336

[9] https://www.cdc.gov/mmwr/volumes/65/rr/rr6501e1.htm

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

adopting 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 IA.1. 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 A.I, 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.

3 Data

We use several different data sources in our main analysis. We proxy for local opioid abuse with historic opioid prescriptions. The Centres for Disease Control and Prevention 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 (non-hospital) pharmacies, which covers nearly 92% of all retail prescription in the United States. Our key independent variable, prescription rate, is the count of annual opioid prescriptions at the county level per 100 people. Panel A in Table I reports summary statistics. Our data covers an average of 2,823 counties per annum over the 2006-2018 period. The average prescription rate corresponds to 82.6 opioid prescriptions per 100 people. Average prescription rates and county variation are consistent with the literature and other data sets (Currie et al., 2019; Harris et al., 2019; Ouimet et al., 2021).

To measure average annual home values 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 from more than 100 million US homes, including new constructions, as well as non-traded homes, to compute the typical value for homes in the 35th to 65th percentile within a county. We calculate 1 to 5-year percentage changes in home values to allow initial prescriptions rates to turn into the onset of drug abuse. From 2006 to 2018, the ZHVI covers on average 2,575 counties per year. The average home value across counties was $140,000 and grew by 1.5% over one year, respectively 5.4% over 5 years with considerable cross-sectional variation (compare Panel B in Table I).

[Insert Table I about here]

We collect additional county-level demographic and economic variables for our analysis. Demographic variables include male population ratio, white population ratio, black population ratio, Indian American population ratio, Hispanic population ratio, age 20-64 ratio, age over 65 ratio and migration flow and are obtained from the Census Bureau. Neoplasms mortality is obtained from CDC. The number of primary care physicians excluding hospital residents or 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 unemployment rate and labour force participation rate obtained from Bureau of Labour Statistics. These variables are normalized by contemporaneous county population and winsorized at the 2 and 98 % level.

Starting with Massachusetts in 2016, several states passed laws or regulations to limit opioid prescriptions.[11] We collect information on the limits and the year of the passage of these opioid prescription limiting state laws.[12] Including Massachusetts, 9 states passed legislation that imposed limits on opioid prescriptions in 2016, while 18 states followed in 2017 and another 5 in 2018. Figure A.I pictures the treated states on a map and Table A.II translates this into county observations.

To measure opioid supply side drivers at the county level, we use the 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 manufactures and their distributors to physicians, non-physician practitioners, and teaching hospitals. Data on physician opioid related payments come from the Centers for Medicare & Medicaid Services Open Payments database, and it covers August 2013 to December 2019.[13] To compute opioid-related physician payments by the manufacturers, we follow Fernandez and Zejcirovic (2018) and Hadland et al. (2019): we identify opioid related payments through the National Drug Code (NDC) directory published by the U.S. Food and Drug Administration (FDA), which includes information on the substance names included in drugs.[14] We then use the substance names to identify opioid drugs following the Anatomical Therapeutic Chemical (ATC) Classification System of the WHO (ATC code N02A).[15] If

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

[12]For an overview of the laws https://ballotpedia.org/Opioid_prescription_limits_and_policies_by_state

[13]Source: https://www.cms.gov/OpenPayments/Data/Dataset-Downloads

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

[15]https://www.whocc.no/atc_ddd_index/?code=n02a

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 not possible, we use the Zipcode and assign the county based on the zipcode centeroid. Last, we aggregate by county and year. Counties without payments related to opioid payments are set to 0, as the coverage is US wide and no information is therefore equivalent to no payments.

4 Results

4.1 Opiod abuse and home values

4.1.1 Correlation between home values and prescription rates

We first document the correlation between home values and opioid abuse, as proxied by prescription rates. We exploit within county variation as well as within state-year variation. Figure 1 presents county-level heat maps of 5-year lagged county prescription rates and 5-year percentage change in home values for the year 2018, the last year in our sample with most observations. The maps show that counties in the bottom quintile of percentage change in home values overall correspond to the counties with the highest prescription rates, suggesting a negative correlation in the cross-section between prescription rates and 5-year percentage change in home value.

[Insert Figure 1]

We further examine this relationship by estimating the following specification:

$$PCHomeValue_{c,t-x\ to\ t} = \alpha + \beta PrescriptionRate_{c,t-x} + \gamma Controls_{c,t-x} + \theta_c + \tau_t + \epsilon_{ct} \quad (1)$$

The dependent variable $PCHomeValue_{c,t-x\ to\ t}$ in equation 1 is the log percentage change of average county c home values, ($log(HV_t/HV_{t-x}) * 100$) over $X = \{1, 2, 3, 4, 5\}$ years. $PrescriptionRate_{c,t-x}$ captures county c prescription rate at $t - x$. We also include a vector of time-varying county-level controls $Controls_{c,t-x}$, measured with a lag at time $t - x$. Following Ouimet et al. (2021), 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 macro-economic 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.

Figure 2 shows the results of estimating Equation 1. Panel A includes county fixed effects θ_c, and year fixed effects τ_t, whereas we use state-year fixed effects $\zeta_{s,t}$ in Panel B.

We find that home values and prescription rates are negatively correlated in the short run. This negative association is persistent and stronger in the long run. The estimated coefficients for the correlation between prescription rates and changes in average home value are monotonically decreasing over 1 to 5 years. The correlation between prescription rates and 1 year percentage change in home values is estimated at -0.011, while the correlation with 5-years changes is -0.033, when exploiting within county variation. A one standard deviation increase in prescription rates translates in 1.35 percentage points reduction in home value growth rates, which is equivalent to 25.2% of the 5-year average percentage home value increase (5.36%). Point estimates obtained from within state-year variation are at -0.002 for 1-year change in home value and -0.008 for 5-year change in home value. Taking a one standard deviation change of prescription rates over 5 years translates into a 0.33 percentage points decrease in home values over the same period.

[Insert Figure 2 about here]

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

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.

We start by examining the link between the passage of the laws and actual opioid prescription rates to establish the effectiveness of the law changes. We implement a difference-in-differences framework to compare changes in county opioid prescription rates in years before and after the passage of the law (*the treatment*) in *treated* versus *control* counties. We run a regression with lead and lag dummies relative to the year of the passage of the law to establish the path of total county prescription rates, and changes in home values and delinquent mortgages, before and after the law.

The literature on staggered differences-in-differences design (e.g. Callaway and Sant'Anna, 2021; Sun and Abraham, 2021) highlights that running a staggered regression only with lead and lags are potentially problematic. First, weights across treatment cohorts can be non-intuitive and at worst negative, as they are proportional to group sizes as well as the variance of the treatment dummy in each pair. Second, already treated units act as controls for newly treated units, which is particularly problematic for trend break effects rather than unit shifts. 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. Our baseline specification to estimate the impact of the passage of the laws on opioid prescriptions across time and states therefore is:

$$PrescriptionRate_{c,t} = \alpha + \sum_{e\in\{16,17,18\}} \sum_{l=-5,\neq -1}^{2} \delta_{e,l} \mathbf{1}\{E_i = e\} D_{ct}^{\ell} + \gamma Controls_{c,t-1} + \theta_c + \tau_t + \epsilon_{c,t} \tag{2}$$

The dependent variable $PrescriptionRate_{c,t}$ is defined as county prescription rates in year t. 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, that 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 time 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 time 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, respectively counties, implementing the opioid law in 2016, 2017 respectively 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 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 law with the first law being passed in 2016 and the last in 2018. Hence, for counties with the law passed in 2016, the relative time period goes from "minus 3" to "plus 2". For counties with the law passed in 2018, the relative time 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 time period.

We then 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.

$$PCHomeValue_{c,t} = \alpha + \sum_{e\in\{16,17,18\}} \sum_{l=-5,\neq -1}^{2} \delta_{e,l} \mathbf{1}\{E_i = e\} D_{ct}^{\ell} + \gamma Controls_{c,t-1} + \theta_c + \tau_t + \epsilon_{c,t} \tag{3}$$

Where the dependent variable $PCHomeValue_{c,t}$ is a one-year percentage change in home values defined as in Equation 1. County controls are the same as in the previous specification. Standard errors are clustered at the state level, as the laws were introduced at the state level.

[Insert Figure 3 about here]

Figure 3 plots the estimates of the total interaction weighted coefficient for each relative time period with the 95% confidence interval. The full set of coefficients for each $CATT(e, \ell)$ as well as the coefficients for lead and lag indicators of the two-way fixed effects regression without cohort-specific indicators can be found in Table IA.I in the Appendix IA.2[16].

Panel A shows that prescription rates declined more on average after the passage of the laws in *treated* counties, relative to the control group. As shown in Panel B, *treated* counties also 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 the passage of the law, 0.81 percentage points more in the first year, and 1.78 percentage points more in the second year after the passage of the law relative to control counties. These results suggest that the adoption of state laws limiting opioid abuse had the intended result in reducing opioid prescription rates. Importantly, they also had a significant effect on the housing markets, resulting in an increase in home values.

4.1.3 Pre-trends

An identifying assumption in our analysis is that states for which the law has passed (*treatment*), and the ones for which it has not (*control*), are on parallel trends in terms of home value changes before the passage of the law. Table A.I in Appendix A.1, consistent with Ouimet et al. (2021), shows 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 not significant. The fact that economic and political conditions do not seem to differ between treated and control states gives us confidence that it is likely that home value changes were on a similar growth pattern prior to the passage of the law. Further, Figure 3 suggests that the parallel trend assumption is not violated.[17]

Still, 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

[16] We repeat this exercise with prescription rates as dependent variable and report the results in Table **??**. As before, the parallel trends assumption is not violated.

[17] A related additional assumption is that no other laws that eventually passed at the same time had an impact on home values.

whether our pre-test is likely to be effective. To assess whether our pre-test is likely to be well powered against violations of parallel trends, we plot a linear violation in Figure A.II in Appendix A.1 with a hypothesized slope based on having 50% power, i.e. the probability of passing the pre-test is 50%. The estimated slope is 0.267, meaning that treated states' home values rise every year by 0.267 percentage points more relative to control states. Given a 1-year average percentage change in home values of 1.45% and a standard deviation of 4.53%, we consider this an economically meaningful deviation. The likelihood ratio for this hypothesized trend is 0.568, i.e. the chance of seeing the observed pre-test coefficients under the hypothesised 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 expected coefficient (in blue) we would find based on the hypothesized trend. This result gives us confidence that our pre-test is reasonably effective.

4.1.4 Rents

Given that house prices represent the sum of the discounted cash flows these assets produce, in this subsection we ask a related question: what effect did opioid abuse have on rents? We estimate Equation 3, with change in the median county rent as the main dependent variable[18]. As we can see from Figure A.III, following the introduction of opioid limiting laws, median county rents significantly increase 2 years after the passage of the laws.

4.1.5 Goodman-Bacon decomposition

Goodman-Bacon (2021) 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 Goodman-Bacon (2021) decomposition. We run the following regression with both prescription rates and home value changes as dependent variable:

$$DepVar_{ct} = \alpha + \beta_1 Post_{ct} + \gamma Controls_{ct-1} + \theta_c + \tau_t + \epsilon_{ct} \tag{4}$$

β_1 is the coefficient of interest. We have nine individual 2x2 DiD estimators. *Earlier*

[18] We collect median gross county rent data from the American Community Survey 5-year Estimates data. Gross rent is the sum of the contract rent plus estimated average monthly cost of utilities and fuels.

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 4 shows the decomposition for the two dependent variables prescription rates and 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 3 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.

[Insert Figure 4 about here]

4.2 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 the propensity to dispense opioids *prior* to the passage of the law to define the treatment variable at the same level as the outcome. 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 one important supply factor of opioids. Second, we follow Engelberg et al. (2014) and use opioid-related pharmaceutical companies' payments to physicians as a proxy for physicians' propensity to prescribe opioid drugs. We estimate the following standard two-way fixed effect regression with calendar year τ_t and county θ_c fixed effects.

$$\begin{aligned} DepVar_{ct} =& \alpha + \beta_1 Post_{ct} + \beta_2 Post_{ct} \times OpioidSupply\ top\ tercile_c \\ &+ \gamma Controls_{ct-1} + \theta_c + \tau_t + \epsilon_{ct} \end{aligned} \tag{5}$$

We use both county prescription rates and home value changes as dependent variable

$DepVar_{ct}$. 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, $OpioidSupply\ top\ tercile_{c}$, that is equal to one for counties in the highest tercile based on a 5-year average number of physicians per capita (total opioid related payments to physicians, respectively) before the first passage of any state law, i.e. between 2011 and 2015. $Post_{ct}$ is an indicator variable that is equal to one for the county-years following the law introduction. Figure IA.I in Appendix IA.2 visualise the top tercile on a United States map. The coefficient of interest is β_2, which captures the intensity of the opioid limiting laws on counties that were *ex ante* more exposed to opioid abuse, as proxied by the relative opioid supply. Table II, Column 2 shows that the drop in prescription rates following the passage of the law was concentrated in the counties with the highest number of physicians per capita, in line with Finkelstein et al. (2022)'s findings. This finding is echoed in Column 3, where we proxy for opioid supply using county-level opioid related pharmaceutical companies' payment to physicians. While home value changes seem to increase following the passage of the laws across all counties, they were greatest in counties in the top tercile of physicians payments (Column 6). These results provide further county-level evidence that opioid limiting laws had the strongest home value effect in counties that were ex ante more exposed to the opioid crisis.

[Insert Table II about here]

4.3 Opioid abuse and home values: economic mechanisms

The evidence presented in the previous section shows that opioid abuse results in lower home values. The decrease in home value can be driven by a reduction in household income, and lower ability to service a mortgage, which may lead to default and, ultimately, 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 delinquent mortgages by 90 or more days by county and month from the Consumer Financial Protection Bureau. The underlying data comes from the National Mortgage Database and is aggregated at the county level. 90-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 across the US. Delinquency rates are only reported for counties with a sufficient number of sample records to avoid unreliable estimates. The average mortgage delinquency rate between

2006 and 2018 was 2.41%. The average 5-year percentage change was -66.98% (see Table A.III in Appendix A.2). The average reduction in mortgage delinquency rates in our sample is large, as the peek of delinquency rates was reached at the beginning of our sample in 2010. Since then, it has steadily declined. As we explore cross-sectional variation in delinquency rates in our analysis, this is not a first-order concern. In addition to these data, we also collect data on the number of home improvement loans from the Home Mortgage Disclosure Act (HMDA), and residential property vacancy rates from the United States Postal Service (USPS). We report summary statistics for these variables in Table A.III in Appendix A.2.

[Insert Table III about here]

As depicted in Table III there is a significant positive long correlation between lagged prescription rates and the percentage change in mortgage delinquency rates, as proxied by the percentage of mortgages that are 90 days plus past due. The correlation is economically meaningful. To interpret the economic magnitude, consider a mortgage delinquency rate of 2.41% (the average in our sample): over 5-years this would have decreased to 0.80% based on the average 5-year percentage change (-66.98%), as reported in Table A.III. Using the county and year fixed effects estimate (column 1), a one standard deviation increase in prescription rate (27.1 prescriptions per 100 people for the 5-year lagged sample) is associated with 22.69 percentage points higher rate of change of delinquent mortgages. Starting out at a 2.41% mortgage delinquency rate, delinquent mortgages would have decreased only to 1.34% (by -66.98% + 22.69% = -44.29%) instead of 0.80%. This result suggests that an increase in mortgage delinquencies following opioid abuse is a possible important channel of how opioid abuse translates into lower housing values.

We also document a negative correlation between home improvement loans and prescription rates. The estimated coefficient is -0.024 when state-year fixed effects are included and -0.175 when county and year fixed effects are included. This means that for a one standard deviation increase in prescription rates (43.3), the rate of change in home improvement loans at the county level is between 1.0 and 7.6 percentage points lower.

Last, we show a positive correlation between residential vacancy rates and prescription rates. The estimated coefficient ranges between 0.062 and 0.267, which represents an increase between 2.7 and 11.6 percentage points in the percentage change of vacancy rates for a one standard deviation increase in prescription rates (43.6).

To further explore these associations, we apply the same framework as in Equation 3 to compare the changes in delinquent mortgages, residential vacancy rates and home improvement loans in years before and after the passage of the law (*the treatment*) in treated versus control counties.

[Insert Figure 5 about here]

Figure 5 plots the estimated coefficients for these channels. We find that the rate of change in mortgage delinquency rate is about 6.17 percentage points lower on average one year after the passage of the laws in *treated* counties, relative to control group. Similarly, the rate of change in home improvement loans is up to 30 percentage points higher two years after the passage of the law and the rate of change in 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, by reducing 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 as already documented.

4.3.1 Migration

Motivated by the results that areas that are more affected by the opioid crisis become less attractive to live, in this section we study the impact of opioid abuse on migration out of the county. We expect that both impoverishment from opioid abuse and also the change in the quality of the neighborhood would have driven residents out. We collect county level outflow and inflow migration data from the Internal Revenue Service (IRS). The Statistics of Income Tax Stats estimate migration outflows and inflows based on year-to-year address changes reported on individual income tax returns filed. Three measures of migration are reported, namely total adjusted gross income, number of returns filed and number of personal exemptions claimed. We define them as "total income", "number of households" and "number of individuals" in line with the IRS.

Table IV looks at the link between county level migration outflow and opioid abuse. In Panels A and B, we proxy for opioid abuse using 5-year lagged county prescription rates, whereas in Panel C we use opioid overdose death rates (as defined in Section 6.2). In Panel A, the dependent variable is 5-year change in migration outflow. Columns 1, 3 and 5 include county and year fixed effects, while columns 2, 4 and 6 include state-year fixed effects. In columns 1 and 2 we use the 5-year percentage change in total household income outflow from the county, in columns 3 and 4 we use the 5-year percentage change in the number of households who have left the county, while in columns 5 and 6 we use the 5-year change in the number of individuals who have left the county. Across the columns, we can see that an increase in the 5-year lagged county prescription rates is associated with an increase in the subsequent 5-year change in the number of households (individuals) who leave the county, as well as with the total household income outflow. While in columns 1, 3 and 5 the estimated coefficients are imprecisely estimated, in columns 2, 4 and 6 they are significant at the 99% level. In Panel B, we use the natural logarithm of the total household income (columns 1 and 2), of the number of households (columns 3 and 4), and of the number of individuals (columns 5 and 6), as the dependent variables. Similarly to results in Panel A, we see a positive relation between 5-year lagged prescription rates and the subsequent number and total income of households (individuals) who leave the county. While in columns 1, 3 and 5 the estimated coefficients are imprecisely estimated, in columns 2, 4 and 6 they are significant at the 99% level. In Panel C, we link whether the county is in the top tercile opioid overdose deaths in each year with the natural logarithm of the total household income (columns 1 and 2), of the number of households (columns 3 and 4), and of the number of individuals (columns 5 and 6), as the dependent variables. We obtain statistically significant coefficients at the 99% level in all specifications.

[Insert Table IV about here]

What kind of impact did opioid limiting laws have on migration in and out of the treated counties? Figure 6 shows the results of estimating Equation 2, with county migration inflow as the dependent variable. Panel A shows the results with natural logarithm of the total household income inflow, Panel B the natural logarithm of the number of households , and Panel C the natural logarithm of the number of individuals (columns 5 and 6), 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 had a desired effect on bolstering housing demand.

[Insert Figure 6 about here]

5 Discussion

5.1 Possible interpretations

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, 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 labour markets, as argued by Ouimet et al. (2021), or because of improvements in neighbourhood quality).

Results of our empirical analysis are also consistent with a "neighborhood externalities" story à la Ambrus et al. (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 neighbourhood 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 neighbours. Similar to their setup, the opioid crisis affected people directly, not the local infrastructure (as would be the case in cases of hurricanes, or earthquakes).[19] In contrast to Ambrus et al. (2020), opioid crisis affected the whole country, with varying treatment intensities across counties.

Our findings are consistent with the Ambrus et al. (2020) model assumption that one of the channels of the effect of opioid abuse is an increase in the share of low-income households in affected counties through death or income-affecting disability of wage-earners. Our findings are also consistent with other direct channels, such as the opioid epidemic temporarily reducing local amenities in affected areas, or inducing rich tenants having higher willingness to pay for such amenities to leave. We interpret the results shown in previous Section (Table IV) as evidence of neighborhood externalities having two distinct effects on neighborhood composition. On the one hand, externalities from low-income neighbors, who have lost a family member due to opioid overdose deaths drive (high-income) households away. But why would we not observe an outflow of low-income households as well? A potential explanation is that price responses to neighborhood impoverishment increased the affordability of housing in affected areas.

While the focus of Ambrus et al. (2020) model is to study the micro-location effects (within a neighborhood) of a temporary health driven (cholera outbreak) income shock, our paper examines the cross-county impact of opioid abuse.[20] The main difference between Ambrus et al. (2020) set up and ours, is that their unit of analysis, blocks, are much smaller than our unit of analysis, counties. As such, there is more scope for different amenities (local institutions, local infrastructure, etc.) evolving in affected versus non-affected counties during the long period of opioid abuse in our case. As our results on mortgage delinquencies and home improvement loans indicate, this introduces other channels through which housing price differences can be explained.

[19] Note that opioid abuse can still affect local infrastructure indirectly.

[20] As such, our work also ties into a literature that show evidence of (persistent) income differences across cities or towns even long after specific sources of economic advantage have become obsolete (Bleakley and Lin, 2012; Hanlon, 2017).

5.2 Aggregate versus local economic effects of the opioid crisis

Estimating aggregate economic effects using our empirical exercise is admittedly challenging in the absence of a general equilibrium model. Although this is out of the scope of our paper, we provide back of the envelope calculations for aggregate economic impact based on our estimates, abstracting from other effects of health on wealth, as well as general equilibrium considerations that took place in the economy with, for instance, the changes in regulation. For this exercise, we take the agreement of the Sackler family to pay $6bn on a final settlement with several US states as a benchmark. The Sacklers are the billionaire owners of Purdue Pharma, who have been widely blamed for helping to spark the US opioid epidemic with the marketing of OxyContin. In 2022 they agreed to pay $6bn to compensate US states for the damages associated with the opioid crisis.

We provide a calculation based on our long-term correlations analysis. Between 2006 and 2011, US aggregate housing wealth decreased from $29.2 trillion to $22.7 trillion, which is equivalent to a -22.26% 5-year percentage change. Our estimates show that a one unit increase in prescription rates per 100 people for the 5-year lagged sample is associated to 0.033 percentage points reduction in house prices growth rates when using county and year fixed effects, and to a 0.008 percentage points reduction with state-year fixed effects.[21] Vowles et al. (2015) find that rates of opioid misuse estimates from 38 studies between 2000 and 2013 averaged between 21% and 29% across most calculations. Assuming that 21% of prescription are misused, we calculate the aggregate housing wealth impact of a 21% opioid prescription rate reduction shock in 2006, i.e. a decrease from 72.4 prescription per 100 to to 57.2 per 100. The reduction of 15.2 prescriptions per 100 people for the 5-year lagged sample translates into a 0.50 percentage points increase in home value growth rates given the county and year fixed effects estimate, respectively an increase of 0.12 percentage points given the state-year fixed effect estimate. Thus, the the US aggregate housing wealth would have decreased only by 21.76% (22.14%) to $22.85 trillion ($22.74 trillion). This equates to $146 billion ($36 billion) housing wealth lost. Figure 7 shows the aggregate housing wealth path from 2006 to 2018 as estimated by Zillow and for the 21% prescription rate shock in 2006 with county and year fixed effects, respectively state and year fixed effects. Figure 8 translates this into the actual gap in aggregate housing wealth lost. While the deviation from the path seems small, the actual estimated wealth lost is a two or even three digit billion dollar figure given the large aggregate housing wealth base.

[Insert Figure 7 about here]

[21] Zillow aggregate home value estimates: https://public.tableau.com/app/profile/zillow.real.estate.research/viz/TotalMarketValue/States

[Insert Figure 8 about here]

We next make use of our natural experiment estimates. We first calculate one year percentage changes in house prices in 2016, 2017, and 2018 and adjust them for states by 0.423%, 0.810% and 1.781% respectively based on the year of the passage of the law. We next recalculate what state total home value would have been without the passage of the law. In the year of the passage of the law ($t = 0$), the aggregate value that all states gained is $69.57 billion (note that for some states this is in 2016, for some in 2017 and some in 2018). Taking just the states that passed the law in 2016, we can accumulate the home value difference over three years: aggregate home value would have been $184.54 billion lower without passing the law for a home value base of $5.91 trillion in 2015. For states that passed the law in 2017 and accumulating over two years this corresponds to $102.55 billion for a home value base of 7.78 trillion in 2016. Last, for states that passed the law in 2018 this corresponds to $11.70 billion for a home value base of $2.77 trillion in 2017.

5.3 Limits to internal and external validity

Our estimates, including aggregate effects rely on the internal validity of our quasi-natural experiment. We have discussed in Section 4 the formal identifying assumption of parallel trends. We assume that states that adopted the law (*treatment*), and the ones that did not (*control*), are on parallel trends in terms of home values before the treatment. In addition, we assume that no other changes in regulation have occurred simultaneously in treated states that affected both prescription rates as well as home values. Similarly, we assume no contamination between treated and control states. For instance, no migration of opioid users from treated to control states.

Our previous estimates for aggregate effects also rely on the external validity of our natural experiment and, as mentioned before, on potential general equilibrium effects as a result of the passage of the law such as the increased consumption of illicit drugs and potential migration of opioid consumers. Nevertheless, if taken into account, these effects would plausibly increase the magnitude of the estimated economic impact.

Importantly, comparing our back of the envelope housing wealth loss estimates with the value of the Sackler settlement reveals that the aggregate housing wealth effect of the opioid crisis is several orders of magnitude larger than the agreed settlement value.

6 Robustness

6.1 Alternative empirical strategies

6.1.1 State-border regression discontinuity design

In this section we employ a spatial regression discontinuity (RD) design exploiting state-border boundaries. 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 opioid-limiting laws. 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:

$$y_c = \beta Treat_c + \sum_{p=1}^{P} [\gamma_{p0} + \gamma_{p1} Treat_c] Distance^p + \epsilon_{ct} \tag{6}$$

where y_c is a county level outcome, e.g. a 1 or 2 year difference in prescription rates or a 1 or 2 year percentage change in home values, $Treat_c$ is an indicator variable equal to one for counties in a state that passed opioid-limiting laws 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). We calculate the percentage change from the *treatment year - 1* to the *treatment year*, and to the *treatment year + 1*, respectively for two year changes. For control counties, we calculate the difference (percentage change, respectively) from 2015 to 2016 or 2017, as the first law was passed in 2016. 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 physicians. We follow Calonico et al. (2014) to choose the optimal bandwidth, which in this case corresponds to the distance to the border. Standard errors are clustered at the state level.

[Insert Table V about here]

Table V shows the results.[22] We show that treated counties show significant lower prescription rates when compared to control ones. Estimated coefficients for the difference in prescription rates over 1 year and 2 years are between 3.6 and 4.3 prescriptions per 100 people. This is a difference of about 5% evaluated at the mean. We then estimate the difference in terms of the percentage change in average home values over 1 and 2

[22]Results of estimating Equation 6 without control variables are shown in Internet Appendix Table IA.II.

years between treated and control counties around the border. The estimated coefficient is between 1.2 for one-year period and 2.2 for the two year period. Figure 9 shows the regression discontinuity plots. Results are overall consistent with previous differences-in-differences approach.

[Insert Figure 9 about here]

An important identifying assumption in our RD design is that there are no spill-over 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 (out of state) for doctors who will prescribe powerful medications. Although recent evidence suggests that only 0.7 percent of all patients with an opioid prescriptions are "doctor shoppers" (McDonald and Carlson, 2014, 2013), it might still be the case that patients can cross the border to have their prescriptions filled, which can bias our estimates upwards.[23] To address this concern we exclude counties with more than 8 "pill mill" pharmacies from our analysis. [24] Panel B of Figure 10 shows the results, which are overall consistent with the our main specification.

[Insert Figure 10 about here]

The internal validity of our quasi-experimental design relies on an important 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 A.IV in the Appendix shows no significant differences in main economic variables across the state border, including our outcome variables, home values and prescription rates, before treatment.

6.1.2 Purdue marketing and supply chain instrumental variables

Cornaggia et al. (2021) 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. We obtain data on the quantity of OxyContin distributed to 3-digit zip codes.[25] We calculate

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

[24]See Section 6.2.1 for detailed definition of pill mill counties.

[25]We thank Cornaggia et al. (2021) for sharing their data.

the percentage change in the quantity of OxyContin distributed by Purdue Pharma between 1997 and 2003 and use this as instrument for prescription rates after linking 3-digit zip codes to counties.

[Insert Table VI about here]

Table VI shows the results. The first stage regression shows a strong positive association between the aggressiveness of Purdue marketing and prescription rates using a 4 and 5-year lag. In the second stage regression we find a negative effect of instrumented prescriptions on home values, which is consistent with our previous estimates. These estimates are significant when we cluster the standard errors by state and year, but not when clustering by county.

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 2014 based on the Drug Enforcement Administrationâs Automation of Reports and Consolidated Orders System.[26] 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.

[Insert Table VII about here]

Table VII shows the results. We find a strong positive association between prescription rates and availability and desirability of opioids. The second stage regressions show a negative relation between prescriptions and home values, however these are not statistically significant at conventional levels.

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

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.

6.2 Measurement

Measuring opioid abuse accurately is challenging. We proxy opioid abuse via opioid prescriptions assuming that high levels of opioid prescription lead to opioid abuse due to its highly addictive nature. However, prescription rates may not lead to abuse *one for one*. Illnesses and the need for justified opioid prescription may differ by region, as well as the likelihood of opioid prescription turning into opioid abuse. Finally, opioid prescriptions may be distributed in one county, but consumed in another county. We also use an alternative measure of opioid abuse by Cornaggia et al. (2021) and Li and Zhu (2019) is opioid mortality. Opioid mortality in a county implies a high addiction rate, albeit it only captures the most severe cases, because this outcome is at the very end of the addiction stage. We construct three measures of overdose death, *OpioidDeath*, namely annual drug overdose mortality rate per 100,000 residents, 3-year drug overdose mortality rate per 100,000 residents, and a dummy for counties in the top tercile for the 3-year drug overdose mortality rate per 100,000 residents.

Our key measure, *OpioidDeath*, is the drug overdose mortality rate per 100,000 residents. Following Cornaggia et al. (2021) and Li and Zhu (2019), we use data on county-level opioid mortality rates available in the Multiple Cause of Death data from the National Center for Injury Prevention and Control (NCIPC) of the Centers for Disease Control (CDC). The database complies county-level mortality data from 1999 based on the death certificates for all U.S. residents. Deaths are classified by the International Classification of Diseases, 10th Revision (ICD-10).We define drug overdose (or poisoning) deaths as those with ICD-10 underlying cause-of-death codes X40-X44 (unintentional overdose), X60-X64 (suicide by drug self-poisoning), X85 (homicide by drug poisoning), or Y10-Y14 (undetermined intent).

There are two potential limitations of this data set. First, the aggregate drug poisoning death counts include not only overdose deaths caused by opioid abuse but also deaths caused by other types of drugs with abuse potential (e.g., cocaine, methamphetamine,

amphetamine, prescription stimulants). However, this is unlikely to confound our results because deaths involving opioids account for the vast majority of overall drug mortality in the U.S. While opioid deaths significantly increased during our sample period, deaths due to non-opioids drugs remain relatively stable. Second, if a county has fewer than 10 deaths in a given year, CDC data suppress the report of death counts to protect personal privacy. This implies that our overdose death rate is left-censored. To avoid potential biases arising from this censorship, we try to extend the coverage by focusing in addition to the annual overdose death rate on three-year overdose death rates as well as on on a dummy capturing counties in the top tercile of overdose death rates, which allows us to extend the coverage by imputing overdose death rates for suppressed data, as described in Appendix IA.3.

Since overdose deaths are at the end of the abuse timeline, we run to some extent contemporaneous regressions between the one-year percentage change in home values and overdose death rates:

$$PCHomeValue_{c,t-1\ to\ t} = \alpha + \beta OpioidDeath_{c,t} + \gamma Controls_{c,t-1} + \theta_c + \tau_t + \epsilon_{ct} \tag{7}$$

For instance, for the percentage change of home values between 2017 and 2018, we use the annual overdose death rate from 2018 and the three-year overdose death rate from 2016, 2017 and 2018 for both the quantitative variable and the top tercile dummy. Thus t corresponds to the last year of the three years for the three-year death rate. County controls are the same as in our main specification and are lagged by one year. As in our main specifications, we consider both county and year fixed effects as well as state-year fixed effects. We report results for counties with observations during the full sample period, i.e. 13 observations. Results are robust to considering all available county data. For the annual overdose death rate this leads to about 7,300 county-year observations, and about 11,800 respectively 17,500 county-year observations for the 3-year rate and 3-year rate dummy. Results are shown in Table VIII.

Consistent with our previous estimates, changes in home values are negatively correlated to opioid abuse across all measures of overdose death and considering either fixed-effect specification. The results are more pronounced and significant for the 3-year overdose death rate top tercile dummy, which benefits from the largest sample. The percentage change in one year home values is 0.218 percentage points lower for counties in the top tercile of the 3-year overdose death rate, when using county and year fixed effects. The point estimate is 0.173 percentage points when using state-year fixed effects.

[Insert Table VIII about here]

6.2.1 Excluding "pill mill" counties

Another limitation of using prescription rates as an opioid abuse measure is the potential misalignment between the prescription of the drug and intake. Drug consumers may have travelled 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.[27] 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 Ouimet et al. (2021) and drop 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.[28] We classify a pharmacy as a pill mill if it dispenses opioid MME in the top 5% of the sample. We then drop counties with more than 8 pill mills (equivalent to 6.3% of counties). Table IX shows that our main results are robust to dropping "pill mill" counties.

[Insert Table IX about here]

7 Conclusion

This paper estimates the sensitivity of home values to opioid abuse. We find a negative association between home values and opioid abuse that is monotonically increasing and persistent over a 5-year period. We exploit variation in opioid abuse induced by the staggered passage of state laws that aim to limit these prescriptions as a source of variation in opioid prescriptions. Home values respond positively to the passage of the state laws intended to reduce opioid abuse.

We study possible underlying economic mechanisms for this relation. We find that opioid abuse is negatively correlated with the number of initiated home improvement

[27] 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.

[28] 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.

loans, and positively correlated with vacant residential property rates and delinquent mortgages. Passage of effective anti-opioid legislation results in a decrease (increase) in mortgage delinquencies and property vacancy rates (the number of home improvement loans and migration inflows, respectively), consistent with a decrease in defaults, an improvement in the quality of local real estate, and an increase in the local demand for space being the main drivers of the observed effect. Our findings are also consistent residential sorting where further impact on house values can be rationalized through the associated outflow migration of people from areas that are most affected by the opioid crisis. Overall, our results point to a broad set of neighbourhood externalities channel(s) driving the observed patterns in home values.

Our results have two main implications. First, they suggest that although opioid usage has been associated with low income and economically disadvantaged conditions (Case and Deaton, 2015), limiting the supply of prescription drugs has both a significant impact on reducing opioid usage, as well as a relevant economic impact, namely in positively affecting home values and reducing the percentage of delinquent mortgages. Second, lost labor productivity and thus household income may be one driver of how opioid abuse impacted home values via delinquent mortgages, but also through negative neighborhood externalities of opiod abuse which resulted in spatial redistribution of households.

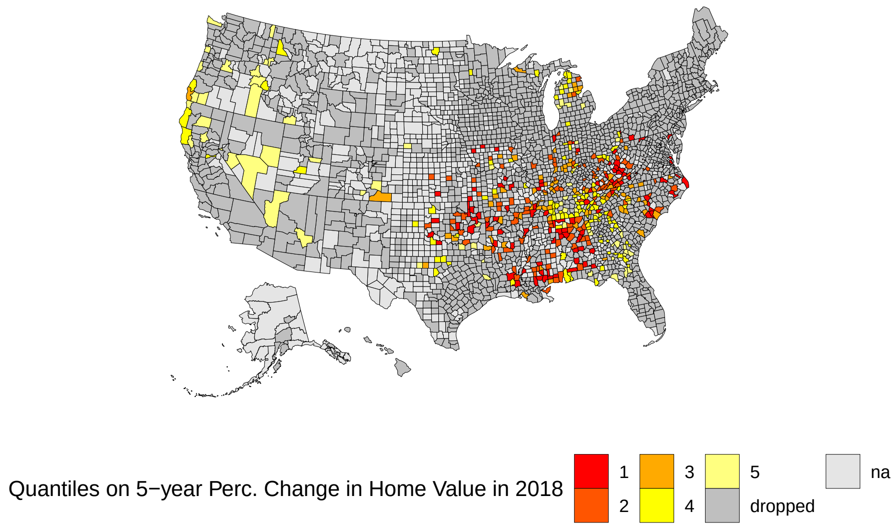
Our work offers insights into externalities of public health policies. We find evidence that public health policies that were instituted with the aim of limiting opioid abuse had a far reaching effect on the real economy. We believe that this study will foster further interest in examination of transmission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

8 Figures & Tables

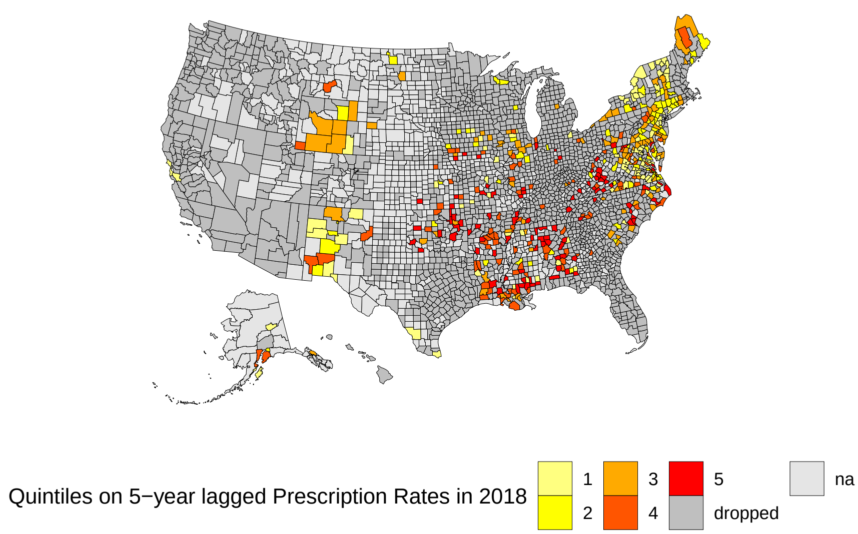
8.1 Main Figures

FIGURE 1: HOME VALUE AND OPIOID PRESCRIPTION RATE

(A) COUNTIES IN HIGHEST PRESCRIPTION RATE QUINTILE COLORED BY PERCENTAGE CHANGE IN HOME VALUES



(B) COUNTIES IN LOWEST PERCENTAGE CHANGE IN HOME VALUES QUINTILE COLORED BY PRESCRIPTION RATE

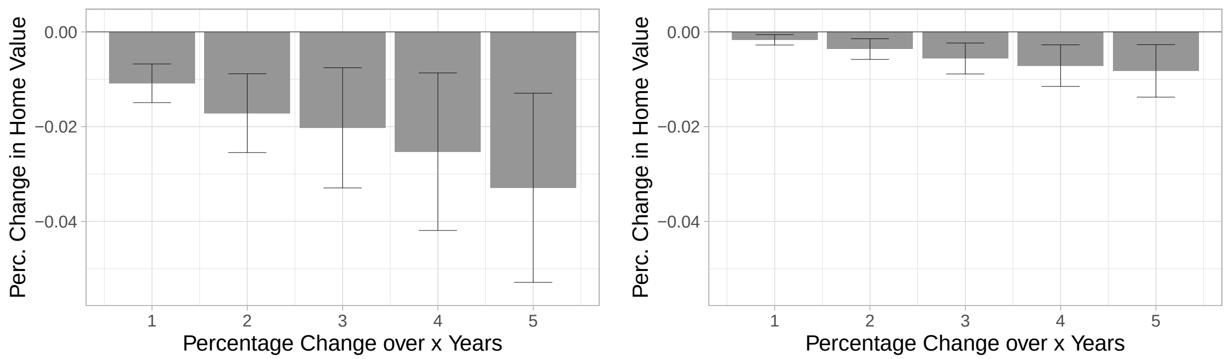


Notes: We plot percentage changes in home value between 2014 and 2018 and prescription rates in 2014. This is the last year of our sample with the most observations. Panel A shows counties in the highest prescription rate quintile in 2014. Excluded counties are dark grey, counties without data are light grey. Heat colours for the remaining counties are based on the quintiles of the 5-year percentage change in home values from 2014 to 2018. Dark red represents the lowest percentage change in home values. Panel B shows counties in the lowest quintile of percentage change in home values and assigns heat map colors based on the prescription rate quintile in 2014. Dark red in Panel B corresponds to the highest prescription rate quintile.

FIGURE 2: HOME VALUE AND OPIOID PRESCRIPTION RATE: CORRELATIONS

(A) COUNTY & YEAR FIXED EFFECTS

(B) STATE-YEAR FIXED EFFECTS



(A)

	over 1 year	over 2 years	over 3 years	over 4 years	over 5 years
x-year lagged Prescription Rate	-0.011*** (0.002)	-0.017*** (0.004)	-0.020*** (0.006)	-0.025*** (0.008)	-0.033*** (0.010)
Adj. R2	0.599	0.648	0.699	0.735	0.767
No. of Obs	25,026	22,510	19,993	17,487	15,075
SD Pres. Rate	41.203	41.670	42.136	42.459	42.620

(B)

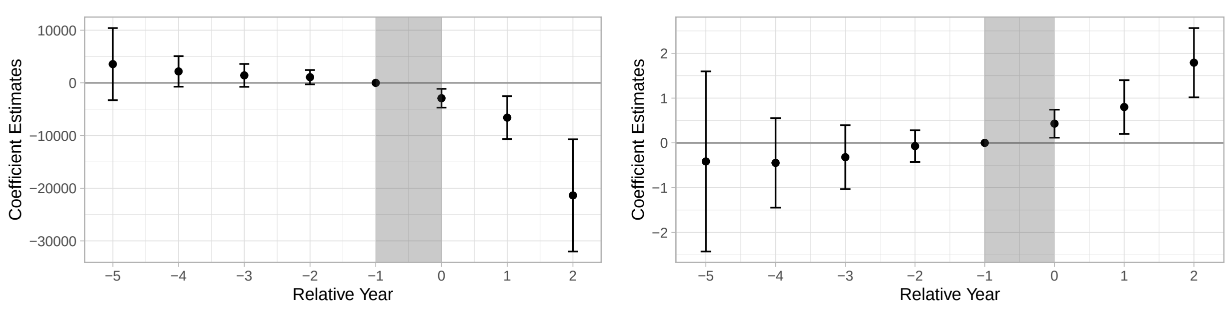
	over 1 year	over 2 years	over 3 years	over 4 years	over 5 years
x-year lagged Prescription Rate	-0.002*** (0.001)	-0.004*** (0.001)	-0.006*** (0.002)	-0.007*** (0.002)	-0.008*** (0.003)
Adj. R2	0.759	0.806	0.838	0.854	0.854
No. of Obs	25081	22572	20077	17590	15157
SD Pres. Rate	41.247	41.714	42.216	42.563	42.640

Notes: The sample period is 2006 to 2018. The dependent variable is a log percentage change of average county home values ($log(HV_t/HV_{t-x}) * 100$) over 1, 2, 3, 4 and 5 years. We report and plot coefficients and 95% confidence intervals on lagged prescription rates. 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. Controls are lagged over the same period as the prescription rate. Panel A includes county and year fixed effects and Panel B state-year fixed effects. All variables are winsorized at the 2 and 98 % level. Standard errors are clustered at the county level.

FIGURE 3: THE EFFECT OF OPIOID LIMITING LAWS ON PRESCRIPTION RATE AND HOME VALUES

(A) COUNTY PRESCRIPTIONS

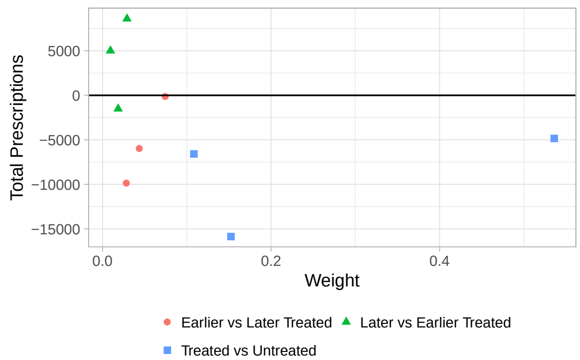
(B) PERCENTAGE CHANGE IN AVERAGE COUNTY HOME VALUE (IN %)



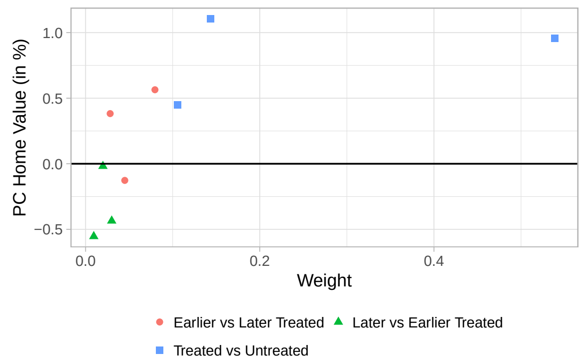
Notes: The sample period is 2013 to 2018. The dependent variable is total county prescriptions in Panel A and the log percentage change in average county home values in Panel B. Controls include one year-lagged male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, unemployment ratio, labor force participation ratio, neoplasm mortality, and physicians. In Panel A we additionally control for log 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.

Figure 4: Goodman-Bacon decomposition

(A) County Prescriptions

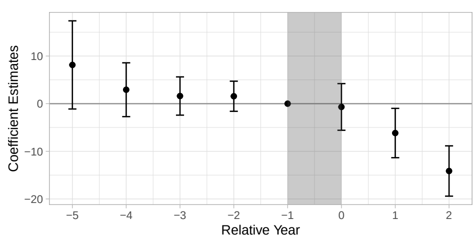


(B) Perc. Change in Home Values

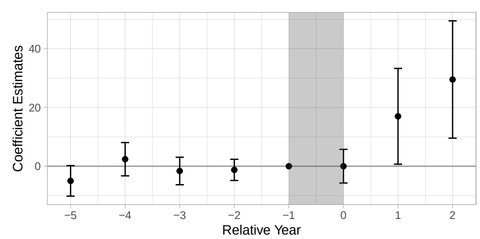


Notes: The sample period is 2013 to 2018. The dependent variable is *Prescriptions* in Panel A and *PCHomeValue* in Panel B. We show the Goodman-Bacon (2021) decompositions for the TWFE regression $Dep.\ variable_{ct} = \alpha + \beta Post_{ct} + \theta_c + \tau_t + \epsilon_{ct}$. We do not include any controls in the regression.

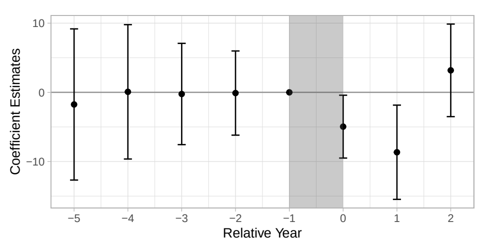
Figure 5: The effect of opioid limiting laws on year-on-year changes in delinquent mortgages, home improvement loans and vacancy rates



(A) Mortgages 90 plus days past due



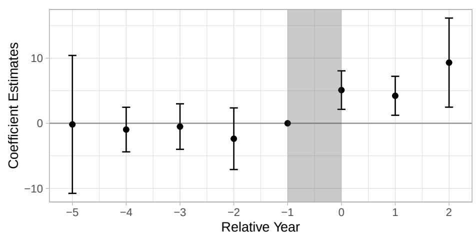
(B) Home improvement loans



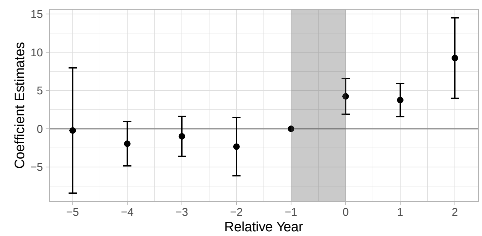
(C) Residential vacancy rate

Notes: The sample period is 2013 to 2018. The dependent variable is the log percentage change in mortgages 90 plus days past due (in %) in Panel A, the log percentage change in the number of home improvement loans (in %) in Panel B and the log percentage change in the residential vacancy rate(in %) in Panel C. 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 following Sun and Abraham (2021). Standard errors are clustered at the state level.

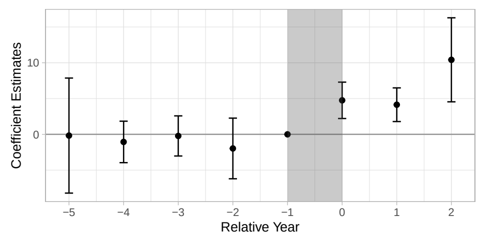
FIGURE 6: THE EFFECT OF OPIOID LIMITING LAWS ON MIGRATION INFLOW



(A) TOTAL INCOME



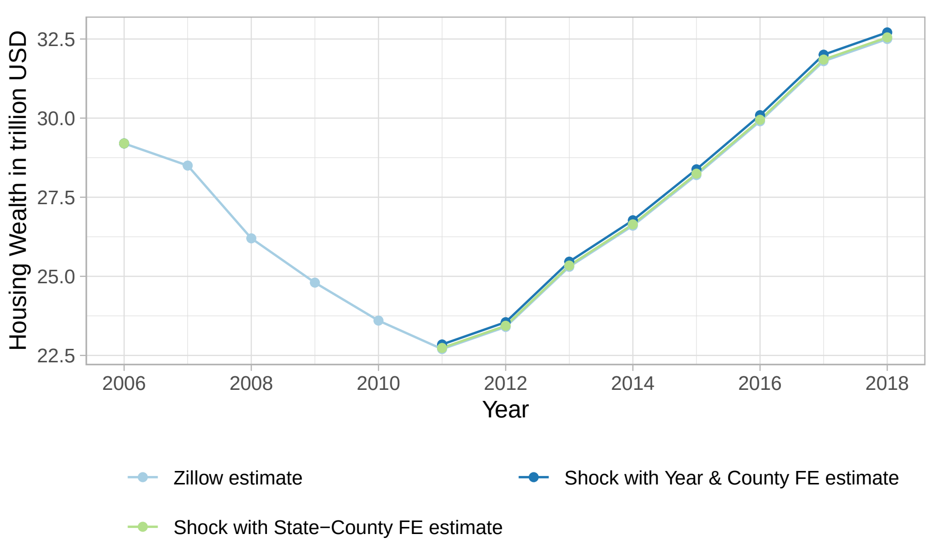
(B) NUMBER OF HOUSEHOLDS



(C) NUMBER OF INDIVIDUALS

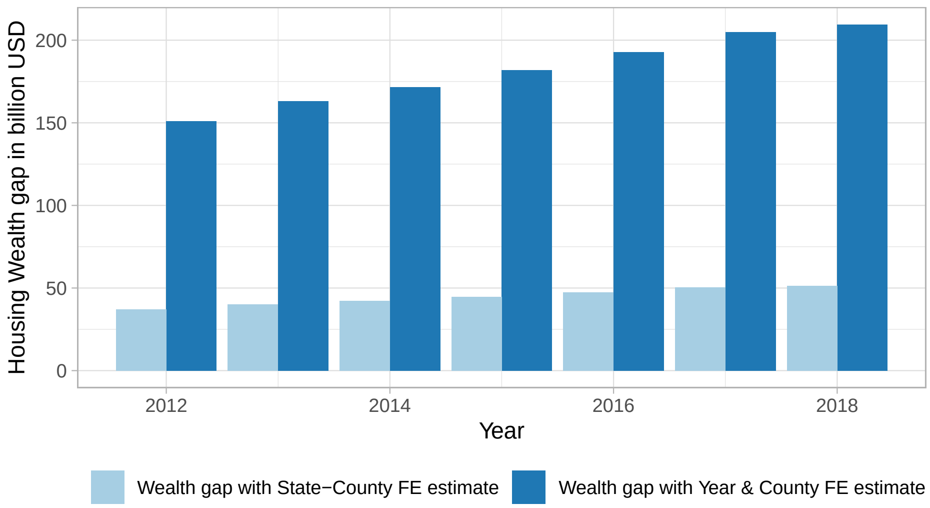
Notes: The sample period is 2013 to 2018. The dependent variable is the log total migration inflow income in Panel A, the log total migration inflow number of households in Panel B and the log total migration inflow number of individuals in Panel C. Controls include one year-lagged male population ratio, 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 Sun and Abraham (2021). Standard errors are clustered at the state level.

FIGURE 7: HOUSING WEALTH OVER TIME



Notes: We report US aggregate housing wealth by year as well the path estimated with a 21% prescription rate reduction shock in 2006 and the same housing wealth growth rate thereafter.

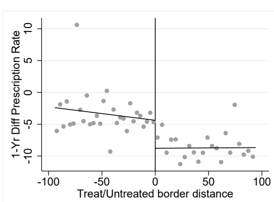
FIGURE 8: HOUSING WEALTH GAP OVER TIME



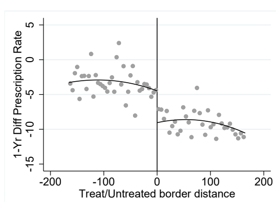
Notes: We report the gap in US aggregate housing wealth by year estimated with a 21% prescription rate reduction shock in 2006 and the same housing wealth growth rate thereafter.

FIGURE 9: OPIOID LAW IMPACT ON PRESCRIPTION RATES AROUND STATE BORDERS: RD PLOTS

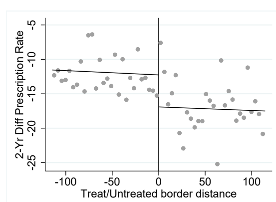
(A) ALL COUNTIES



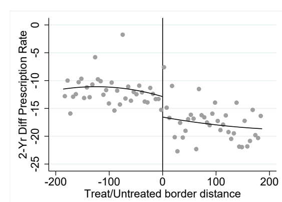
(A.1) 1-YR DIFFERENCE & LINEAR POLYNOMIAL



(A.2) 1-YR DIFFERENCE & QUADRATIC POLYNOMIAL

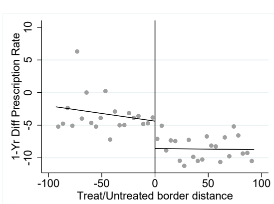


(A.3) 2-YR DIFFERENCE & LINEAR POLYNOMIAL

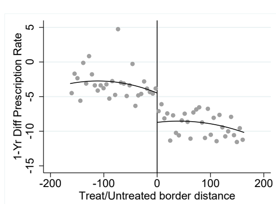


(A.4) 2-YR DIFFERENCE & QUADRATIC POLYNOMIAL

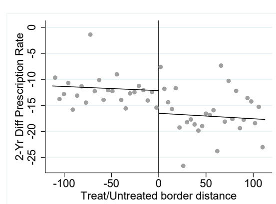
(B) EXCLUDING COUNTIES WITH MORE THAN 8 PILL MILLS



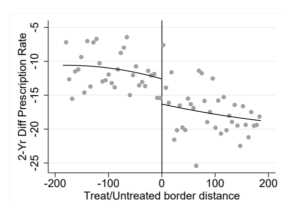
(B.1) 1-YR DIFFERENCE & LINEAR POLYNOMIAL



(B.2) 1-YR DIFFERENCE & QUADRATIC POLYNOMIAL



(B.3) 2-YR DIFFERENCE & LINEAR POLYNOMIAL

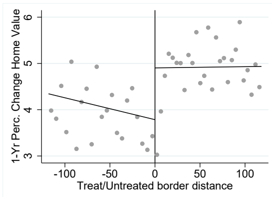


(B.4) 2-YR DIFFERENCE & QUADRATIC POLYNOMIAL

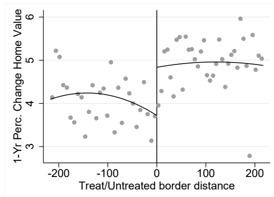
Notes: The unit of observations are counties. We consider all counties in Panel A and all counties but counties with more than 8 pill mills in Panel B. We calculate one or two-year difference in prescription rates from the treatment year - 1 to the treatment year, respectively treatment year + 1. For control counties, we calculate the difference from 2015 to 2016 or 2017, as the first law was passed in 2016. 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 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 V.

FIGURE 10: OPIOID LAW IMPACT ON HOME VALUES AROUND STATE BORDERS: RD PLOTS

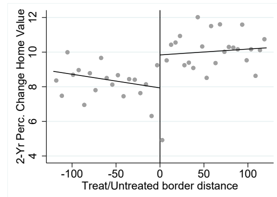
(A) ALL COUNTIES



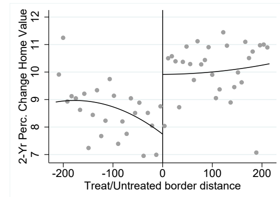
(A.1) 1-YR PERC. CHANGE & LINEAR POLYNOMIAL



(A.2) 1-YR PERC. CHANGE & QUADRATIC POLYNOMIAL

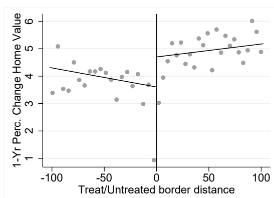


(A.3) 2-YR PERC. CHANGE & LINEAR POLYNOMIAL

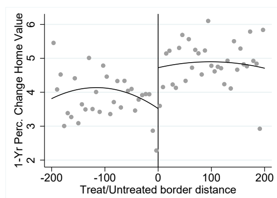


(A.4) 2-YR PERC. CHANGE & QUADRATIC POLYNOMIAL

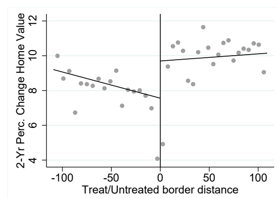
(B) EXCLUDING COUNTIES WITH MORE THAN 8 PILL MILLS



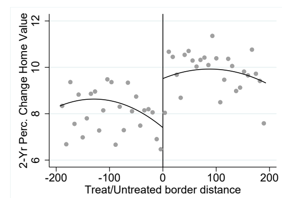
(B.1) 1-YR PERC. CHANGE & LINEAR POLYNOMIAL



(B.2) 1-YR PERC. CHANGE & QUADRATIC POLYNOMIAL



(B.3) 2-YR PERC. CHANGE & LINEAR POLYNOMIAL



(B.4) 2-YR PERC. CHANGE & QUADRATIC POLYNOMIAL

Notes: The unit of observations are counties. We consider all counties in Panel A and all counties but counties with more than 8 pill mills in Panel B. We calculate one or two-year percentage change in home values from the treatment year - 1 to the treatment year, respectively treatment year + 1. For control counties, we calculate the percentage change from 2015 to 2016 or 2017, as the first law was passed in 2016. 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 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 V.

8.2 Main Tables

TABLE I: SUMMARY STATISTICS

	Panel A: Opioid abuse proxies								
	N total	Avg N Annual	Mean	Min	P25	Median	P75	Max	Std. Dev.
Prescription Rate (per 100)	36,704	2,823	82.60	4.20	53.20	77.80	106.90	198.99	42.48
County Prescriptions	36,704	2,823	71,802.60	301.05	8,895.14	26,982.68	75,065.89	582,964.53	117,074.74
Annual Drug Overdose Death Rate	12,990	999	18.82	4.70	11.05	15.83	23.49	56.51	11.14
3-year Drug Overdose Death Rate (in %)	20,073	1,544	17.02	5.24	10.61	14.96	21.01	46.59	8.94
	Panel B: Home values								
	N total	Avg N Annual	Mean	Min	P25	Median	P75	Max	Std. Dev.
Avg Home Value ($)	33,481	2,575	140,033.56	47,116.70	85,275.08	117,306.50	169,095.08	425,161.58	79,347.56
1-year Perc Change HV (in %)	30,633	2,553	1.45	-10.27	-1.40	1.91	4.66	10.23	4.53
2-year Perc Change HV (in %)	27,799	2,527	2.50	-19.42	-2.94	3.25	8.67	19.10	8.56
3-year Perc Change HV (in %)	24,990	2,499	3.38	-27.70	-4.33	4.09	12.04	27.19	12.07
4-year Perc Change HV (in %)	22,227	2,470	4.36	-33.35	-5.12	4.77	14.87	34.60	14.89
5-year Perc Change HV (in %)	19,524	2,440	5.36	-35.52	-5.54	5.33	16.75	41.10	16.85

Our sample period covers 2006 to 2018. We report descriptive statistics for opioid abuse proxies in Panel A. This includes retail opioid prescriptions dispensed per 100 persons per year, total county level retail opioid prescriptions, annual drug overdose death rate per 100,000 residents considering ICD-10 underlying cause-of-death codes X40-X44 (unintentional overdose), X60-X64 (suicide by drug self-poisoning), X85 (homicide by drug poisoning), or Y10-Y14 (undetermined intent), as well as 3-year drug overdose death rate per 100,000 residents with the same cause-of-death codes that aggregates the deaths across three years. 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, 2, 3, 4 and 5-year log percentage changes in county level home value.

TABLE II: OPIOID SUPPLY PROPENSITY INTERACTION

	(1)	(2)	(3)	(4)	(5)	(6)
	Prescription Rate			Percentage Change Home Prices		
Post	-2.533^{*}	-1.658	0.261	0.731^{**}	0.673^{**}	0.569^{*}
	(1.310)	(1.423)	(1.376)	(0.319)	(0.317)	(0.319)
Post X Physicians per capita Tercile 3		-2.266^{*}			0.148	
		(1.204)			(0.185)	
Post X Phys. Opioid Payment Rate Tercile 3			-5.998^{***}			0.346^{**}
			(1.415)			(0.160)
R2	0.950	0.950	0.950	0.589	0.590	0.590
N	15199	15199	15199	14695	14695	14695

The sample period is 2013 to 2018. The dependent variable is prescription rate in columns 1 to 3, respectively a log percentage change of average county home values over 1 year in columns 4 to 6. *Post* is a dummy equal to one in the year of the passage of the law in the respective county and thereafter; *Physicians per capita Tercile 3* is a dummy equal to one for counties whose average physicians per capita between 2011 to 2015 is in the top tercile and *Phys. Opioid Payment Rate Tercile 3* is a dummy equal to one for counties in the top tercile based on opioid related payments to physicians from August 2013 (data start) until the end of 2015. 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 and neoplasm mortality. We include county and year fixed effects. Standard errors are clustered at the state level. $***$ indicates $p < 0.01$, $**$ indicates $p < 0.05$, and $*$ indicates $p < 0.1$.

TABLE III: ECONOMIC MECHANISMS: CORRELATION WITH PRESCRIPTION RATES

	(1)	(2)	(3)	(4)	(5)	(6)
	5-year percentage change in					
	Mortgage delinquency rate		No. of home improvement loans		Residential vacancy rate	
Lag Prescription Rate	0.837***	0.192***	−0.175***	−0.024**	0.267***	0.062***
	(0.188)	(0.047)	(0.045)	(0.010)	(0.047)	(0.022)
R2	0.904	0.901	0.672	0.661	0.758	0.337
N	2350	2320	14721	14794	9488	9589
Std. dev. prescription rate	27.11	27.08	43.29	43.34	43.55	43.63
County F.E.	Yes	No	Yes	No	Yes	No
Year F.E.	Yes	No	Yes	No	Yes	No
State-Year F.E.	No	Yes	No	Yes	No	Yes

The sample period is 2006 to 2018.The regression specification and controls is the same as in Equation 1 but for the dependent variables. The dependent variable is a 5-year percentage changes in the mortgage delinquency rate (percent of mortgages 90 days plus past due) in columns 1 and 2, over 5 years, in the number of home improvement loans columns 3 and 4, and in the residential vacancy rates in columns 5 and 6. The key independent variable of interest is the lagged prescription rate. 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 lagged over five years. All variables are winsorized at the 2 and 98 % level. Standard errors are clustered at the county level. $***$ indicates $p < 0.01$, $**$ indicates $p < 0.05$, and $*$ indicates $p < 0.1$.

TABLE IV: OPIOID ABUSE AND MIGRATION OUTFLOW

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 5-year lagged prescription rates and 5-year percentage change in migration outflow measures						
	Perc Change Total income		Perc Change no. households		Perc Change no. individuals	
Lag Prescription Rate	-0.005	0.008^{**}	0.002	0.015^{***}	0.001	0.015^{***}
	(0.016)	(0.004)	(0.010)	(0.003)	(0.011)	(0.003)
R2	0.506	0.468	0.628	0.640	0.638	0.634
N	17208	17275	17222	17288	17222	17288
Panel B: 5-year lagged prescription rates and log migration outflow measures						
	Log(Total income)		Log(Households)		Log(Individuals)	
Lag Prescription Rate	-0.003	0.394^{***}	-0.001	0.380^{***}	-0.003	0.380^{***}
	(0.013)	(0.023)	(0.008)	(0.027)	(0.009)	(0.027)
R2	0.989	0.825	0.995	0.809	0.994	0.806
N	17215	17281	17222	17288	17222	17288
Panel C: Opioid overdose death and log migration outflow measures						
	Log(Total income)		Log(Households)		Log(Individuals)	
3-year overdose death rate top tercile	0.613^{*}	13.370^{***}	0.648^{***}	14.440^{***}	0.836^{***}	15.181^{***}
	(0.355)	(2.020)	(0.226)	(2.025)	(0.240)	(2.009)
R2	0.991	0.863	0.996	0.817	0.995	0.812
N	18260	18250	18264	18254	18264	18254
County F.E.	Yes	No	Yes	No	Yes	No
Year F.E.	Yes	No	Yes	No	Yes	No
State-Year F.E.	No	Yes	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

The sample period is 2006 to 2018. The dependent variable is a measure of total migration outflow based on on individual income tax returns filed with the IRS. In column 1 and 2, it is based on the total adjusted gross income, in column 3 and 4 the number of households approximated by the number of returns filed and in column 5 and 6 the number of individuals approximated by personal exemptions claimed. In Panel A, we calculate 5-year percentage changes for the dependent variables and in Panel B and C we calculate logs of the dependent variable. In Panel A and B the key independent variable of interest is the 5-year lagged prescription rate. The specification therefore follows Equation 1 and takes the same 5-year lagged controls. The specification in Panel C follows Equation 7. We consider only the most populated opioid overdose measure as independent variable, namely "3-year overdose death rate top tercile". Controls are lagged by one year. Standard errors are clustered at the county level. $***$ indicates $p < 0.01$, $**$ indicates $p < 0.05$, and $*$ indicates $p < 0.1$.

TABLE V: OPIOID LAW IMPACT ON PRESCRIPTION AND HOME VALUES AROUND STATE BORDERS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All counties				Excluding pill mill counties			
	Difference in County Prescription Rates over ...							
	1 year		2 years		1 year		2 years	
RD Estimate	-4.219***	-4.176***	-4.004**	-3.544*	-3.972***	-3.837***	-3.794**	-3.269
	(1.004)	(1.099)	(1.771)	(2.047)	(1.062)	(1.174)	(1.914)	(2.188)
Observations	2389	2389	2066	2066	2210	2210	1923	1923
MSEBandwidth	94	165	115	186	93	163	113	186
Effective LHS Obs	504	754	574	783	479	714	545	744
Effective RHS Obs	546	872	474	695	507	804	439	645
Polynominal Order	1	2	1	2	1	2	1	2
	All counties				Excluding pill mill counties			
	Percentage Change in County Average Home Values over...							
	1 year		2 years		1 year		2 years	
RD Estimate	1.157*	1.195*	2.158*	2.165*	1.180*	1.257*	2.232**	2.279**
	(0.699)	(0.708)	(1.148)	(1.176)	(0.703)	(0.685)	(1.065)	(1.116)
Observations	2334	2334	2020	2020	2157	2157	1879	1879
MSEBandwidth	118	217	121	215	102	200	109	193
Effective LHS Obs	570	788	583	784	496	736	519	729
Effective RHS Obs	660	1041	498	769	544	918	427	666
Polynominal Order	1	2	1	2	1	2	1	2

The unit of observations are counties. In columns 1 to 4 we consider all counties. In columns 5 to 8 we exclude counties with more than 8 pill mills. We calculate one or two-year difference in prescription rate, respectively percentage changes in home values from the treatment year - 1 to the treatment year, respectively treatment year + 1. For control counties, we calculate the difference, respectively percentage change from 2015 to 2016 or 2017, as the first law was passed in 2016. 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 physicians. We follow Calonico et al. (2014) to choose the optimal bandwidth. Standard errors are clustered at the state level. $***$ indicates $p < 0.01$, $**$ indicates $p < 0.05$, and $*$ indicates $p < 0.1$.

TABLE VI: INSTRUMENTAL VARIABLE: PURDUE MARKETING

	(1) IV: Stage 1	(2) IV: Stage 2	(3) IV: Stage 1	(4) IV: Stage 2
Panel A: 4-year percentage change in home values				
Purdue Marketing	1.322***		1.322***	
	(0.298)		(0.116)	
Estimated Prescription Rate		−0.030		−0.030*
		(0.025)		(0.017)
R2	0.357	0.857	0.357	0.857
N	19726	17965	19726	17965
F-statistic	41.1	7.5	228.6	15.4
Panel B: 5-year percentage change in home values				
Purdue Marketing	1.329***		1.329***	
	(0.303)		(0.122)	
Estimated Prescription Rate		−0.039		−0.039*
		(0.031)		(0.021)
R2	0.362	0.857	0.362	0.857
F-statistic	17183	15532	17183	15532
N	39.6	7.4	215.0	16.1
State-Year F.E.	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Std. Error	County	County	State-Year	State-Year

The sample period is 2006 to 2018. The depend variable is 4-year percentage changes in home values in Panel A and 5-year percentage changes in Panel B. We run a two-stage least squares regression with *Purdue Marketing* as instrument. *Purdue Marketing* is defined as growth in pill distribution between 1997 and 2003. 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 and neoplasm mortality. We also include state-year fixed effects. In columns 1 and 2 we cluster standard errors at the county level and in columns 3 and 4 at the state and year level. ∗ ∗ ∗ indicates p < 0.01, ∗∗ indicates p < 0.05, and ∗ indicates p < 0.1.

TABLE VII: INSTRUMENTAL VARIABLE: SUPPLY CHAIN

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	8)
	State-Year F.E.				County & Year F.E.			
	Cluster County		Cluster state x year		Cluster County		Cluster state x year	
	IV: Stage 1	IV: Stage 2	IV: Stage 1	IV: Stage 2	IV: Stage 1	IV: Stage 2	IV: Stage 1	IV: Stage 2
Panel A: 4-year percentage change in home values								
Supply Chain	1.020^{***}		1.020^{***}		1.020^{***}		1.020^{***}	
	(0.065)		(0.071)		(0.065)		(0.071)	
Estimated Prescription Rate		-0.006		-0.006		-0.006		-0.006
		(0.007)		(0.006)		(0.007)		(0.006)
R2	0.413	0.839	0.413	0.839	0.413	0.839	0.413	0.839
N	14479	12910	14479	12910	14479	12910	14479	12910
F-statistic	59.3	8.5	175.6	10.7	59.3	8.5	175.6	10.7
Panel B: 5-year percentage change in home values								
Supply Chain	1.025^{***}		1.025^{***}		0.279^{***}		0.279^{***}	
	(0.065)		(0.071)		(0.042)		(0.031)	
Estimate Prescription Rate		-0.011		-0.011		-0.144		-0.144
		(0.009)		(0.007)		(0.092)		(0.158)
R2	0.415	0.845	0.415	0.845	0.948	0.796	0.948	0.796
N	14479	12910	14479	12910	14411	12840	14411	12840
F-statistic	59.4	8.2	175.6	14.1	7.4	37.2	13.0	17.9
County F.E.	No	No	No	No	Yes	Yes	Yes	Yes
Year F.E.	No	No	No	No	Yes	Yes	Yes	Yes
State-Year F.E.	Yes	Yes	Yes	Yes	No	No	No	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Std. Error	County	County	State-Year	State-Year	County	County	State-Year	State-Year

The sample period is 2006 to 2018. The depend variable is 4-year percentage changes in home values in Panel A and 5-year percentage changes in Panel B. We run a two-stage least squares regression with leaky supply chains (*Supply Chain*) as instrument. *Supply Chain* is defined as annual MME per 1000 county inhabitants distribution of strong types of opioid to retail pharmacies. 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 and neoplasm mortality. We include state-year fixed effects in columns 1 to 4 and county and year fixed effects in columns 5 to 8. In columns 1, 2, 5 and 6 we cluster standard errors at the county level and in columns 3, 4, 7 and 8 at the state and year level. $***$ indicates $p < 0.01$, $**$ indicates $p < 0.05$, and $*$ indicates $p < 0.1$.

TABLE VIII: HOME VALUE AND OVERDOSE DEATH RATES

	One-year percentage change in home values					
	(1)	(2)	(3)	(4)	(5)	(6)
Overdose death rate	-0.014^{*}	-0.005				
	(0.008)	(0.005)				
3-year overdose death rate			-0.001	-0.008^{*}		
			(0.009)	(0.004)		
3-year overdose death rate top tercile					-0.218^{**}	-0.173^{***}
					(0.093)	(0.052)
County F.E.	Yes	No	Yes	No	Yes	No
Year F.E.	Yes	No	Yes	No	Yes	No
State-Year F.E.	No	Yes	No	Yes	No	Yes
R2	0.685	0.801	0.657	0.783	0.647	0.778
N	7288	7249	11773	11756	17467	17462
Std. dev. overdose variable	10.542	6.585	8.992	5.600	.469	.366

The sample period is 2006 to 2018. The dependent variable is a 1-year log percentage change of average county home values ($log(HV_t/HV_{t-1}) * 100$). *Overdose death rate* is the annual overdose deaths per 100,000 county inhabitants at t, *3-year overdose death rate* is the 3-year overdose death rates per 100,000 county inhabitants for the years t, $t-1$ and $t-2$, and *3-year overdose death rate top tercile* is a dummy equal to one for counties in the tercile with the highest 3-year overdose death rates. We restrict the sample to counties with data in every period. 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 and neoplasm mortality. Columns 1, 3 and 5 control for county and year fixed effects and columns 2, 4 and 6 for state-year fixed effects. We require counties to have data for the whole time series, i.e. the full 13 years, to avoid counties dropping in and out depending on suppressed data. Standard errors are clustered at the county level. $***$ indicates p < 0.01, $**$ indicates p < 0.05, and $*$ indicates p < 0.1.

TABLE IX: HOME VALUE AND OPIOID PRESCRIPTION RATES: CORRELATIONS EXCLUDING COUNTIES WITH MORE THAN 8 PILL MILLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Log Perc. Change Home Value									
	Lag over					Lag over				
	1 Year	2 Years	3 Years	4 Years	5 Years	1 Year	2 Years	3 Years	4 Years	5 Years
Lag Prescription Rate	-0.007^{***}	-0.011^{***}	-0.012^{*}	-0.016^{*}	-0.022^{**}	-0.001^{*}	-0.002^{**}	-0.004^{**}	-0.005^{**}	-0.005^{*}
	(0.002)	(0.004)	(0.006)	(0.008)	(0.010)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
County F.E.	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Year F.E.	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
State-Year F.E.	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
R2	0.636	0.689	0.740	0.776	0.807	0.753	0.802	0.835	0.850	0.851
N	22930	20605	18279	15964	13743	22971	20656	18354	16059	13818

The sample period is 2006 to 2018.The regression specification and controls is the same as in Equation 1, but we drop counties with more than 8 pill mills, equivalent to dropping the top 6.3% counties based on the number of pill mills. The dependent variable is a log percentage change of average county home values ($log(HV_t/HV_{t-x}) * 100$) over 1, 2, 3, 4 and 5 years. 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 variables are winsorized at the 2 and 98 % level. Columns 1 to 5 include county and year fixed effects and columns 6 to 10 state-year fixed effects. Standard errors are clustered at the county level. $***$ indicates p < 0.01, $**$ indicates p < 0.05, and $*$ indicates p < 0.1.

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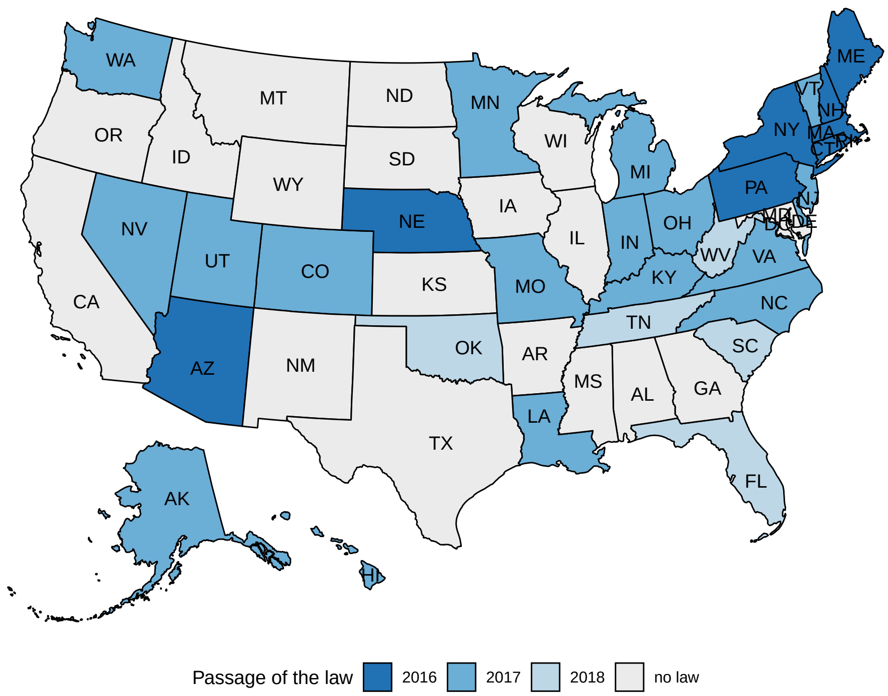
A Appendix

A.1 Opioid laws and regulations

FIGURE A.I: PASSAGE OF OPIOIDS LEGISLATION BY STATE



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

TABLE A.I: DETERMINANTS OF OPIOIDS STATE LEGISLATION

	(1)	(2)	(3)	(4)
	State Law and Regulation Indicator			
Avg Prescription Rate	−0.003	0.004	−0.002	0.004
	(0.003)	(0.006)	(0.004)	(0.006)
Age Adjusted Overdose Death Rate	0.031***	0.027**	0.029**	0.026**
	(0.011)	(0.012)	(0.012)	(0.013)
Unemployment Rate		−0.008		−0.010
		(0.085)		(0.089)
Ln(Median Household Income)		1.505		1.527
		(1.241)		(1.290)
Poverty Ratio		0.041		0.042
		(0.051)		(0.052)
Ln(GDP per capita)		0.132		0.112
		(0.610)		(0.634)
Democratic			0.003	0.030
			(0.203)	(0.211)
Republican			−0.071	−0.015
			(0.165)	(0.176)
R2	0.159	0.208	0.163	0.209
N	50	50	50	50

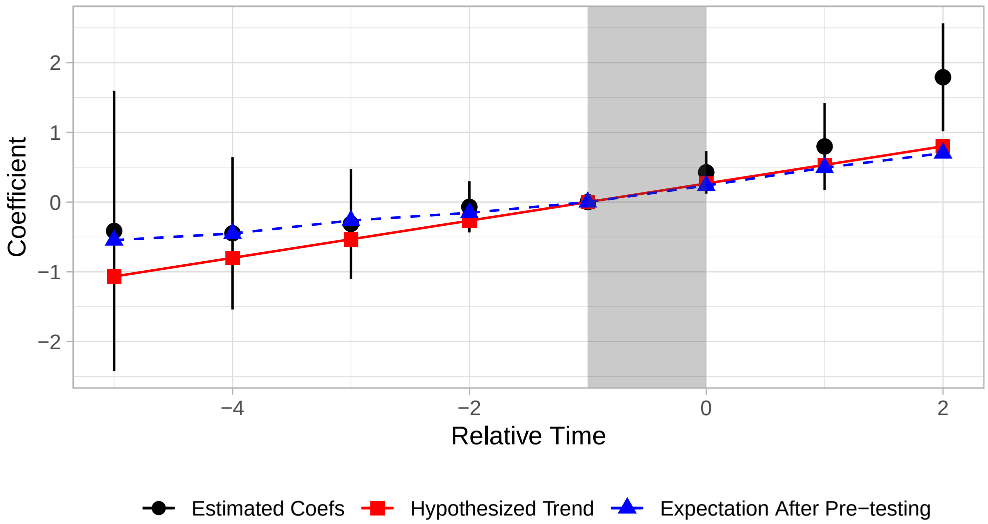
This is a cross-sectional regression with all 50 US states. The dependent variable is an indicator variable equal to one if a state passed a opioid law or regulation between 2016 and 2018. Following Ouimet et al. (2021), independent variables include: Average state prescription rate between 2006 and 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. Standard errors are robust. $***$ indicates $p < 0.01$, $**$ indicates $p < 0.05$, and $*$ indicates $p < 0.1$.

TABLE A.II: COUNTY/STATE OBSERVATIONS FOR OPIOID LAW INTRODUCTIONS

	Opioid Prescriptions Observations		Home Value Observations	
	States	Counties	States	Counties
State Law Passed in 2016	9	279	9	253
State Law Passed in 2017	18	1095	18	1060
State Law Passed in 2018	5	340	5	334

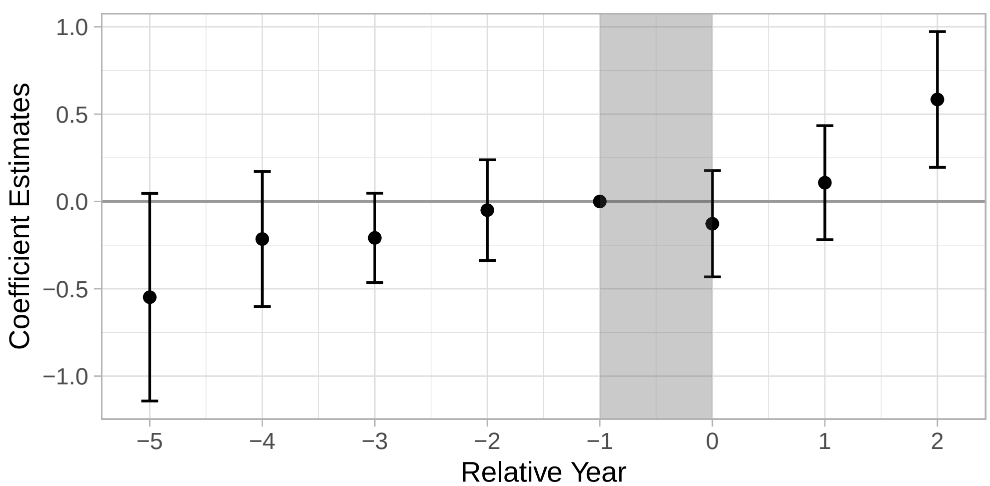
The table reports the number of states that passed laws intended limit opioid abuse as well as the number of observations with data for opioid prescriptions, respectivelyu home value, at the county level.

FIGURE A.II: EVENT PLOT FOR HOME VALUE WITH HYPOTHESIZED TREND BASED ON 50% POWER



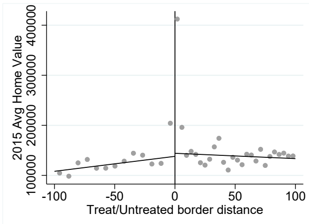
Notes: The sample period is 2013 to 2018. The dependent variable is log percentage change in average county home values. We follow Roth (2022) and plot a linear violation of the pre trend based on a 50% power in red. Black are coefficients we find in our regression and blue are the expected coefficient we would find based on the hypothesized trend in red.

FIGURE A.III: THE EFFECT OF OPIOID LIMITING LAWS ON RENT

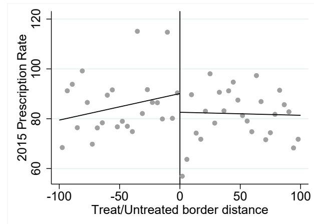


Notes: The sample period is 2013 to 2018. The dependent variable is log percentage change in median rent. Controls include one year-lagged male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio, migration inflow ratio, poverty ratio, 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 Sun and Abraham (2021). Standard errors are clustered at the state level.

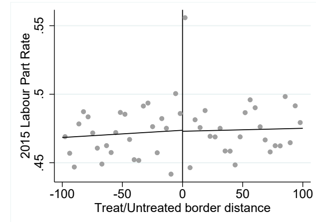
FIGURE A.IV: REGRESSION DISCONTINUITY PLOTS FOR COVARIATES IN 2015



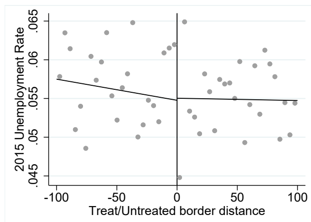
(A) HOME VALUE



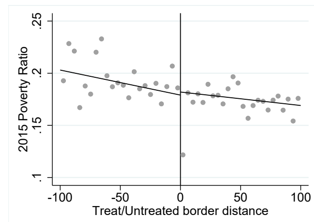
(B) PRESCRIPTION RATE



(C) LABOUR PARTICIPATION RATE



(D) UNEMPLOYMENT RATE



(E) POVERTY RATIO

Notes: The unit of observations are counties. The dependent variables are levels in home value (Panel A), prescription rate (Panel B), labour participation rate (Panel C), unemployment rate (Panel D) and poverty ratio (Panel E) as of 2015. We follow Calonico et al. (2014) to choose the optimal bandwidth and do not include any other controls. Standard errors are clustered at the state level.

A.2 Economic mechanisms

TABLE A.III: SUMMARY STATISTICS ECONOMIC MECHANISMS

Panel A: Summary statistics: Channel variables									
	N total	Avg N Annual	Mean	Min	P25	Median	P75	Max	Std. Dev.
Percent of Mortgages 90+ days past due	5,170	470	2.41	0.42	1.27	2.06	3.16	7.36	1.55
5- year Perc Change Mtgs 90+ days past (in %)	2,820	470	-66.98	-195.90	-113.08	-75.51	-29.35	94.28	68.83
No. of home purchase loans	38,290	3,191	894.51	5.00	62.00	190.00	644.00	9,480.96	1,865.67
5-year Perc Change home pruchase loans (in %)	22,208	3,173	16.82	-88.31	-9.65	23.19	48.10	98.90	43.96
Residential vacancy rate (in %)	28,109	3,123	4.25	0.00	1.56	3.41	5.98	15.27	3.64
5-year Perc Change residential vacancy rate (in %)	11,335	2,834	-14.01	-833.29	-33.51	-7.03	14.31	639.88	79.47
Panel B: Summary statistics: Migration									
	N total	Avg N Annual	Mean	Min	P25	Median	P75	Max	Std. Dev.
Log(Mig-out total income)	40,586	3,122	10.18	7.29	9.08	9.97	11.13	14.08	1.57
5-year Perc Change Mig-out total income (in %)	24,923	3,115	12.97	-49.82	-2.57	13.05	28.88	69.77	25.24
Log(Mig-out no households)	40,607	3,124	6.54	3.69	5.56	6.40	7.42	9.99	1.44
5-year Perc Change Mig-out no households (in %)	24,949	3,119	-1.91	-49.79	-12.54	-1.07	9.60	36.78	18.22
Log(Mig-out no individuals)	40,607	3,124	7.20	4.33	6.24	7.07	8.06	10.59	1.42
5-year Perc Change Mig-outno individuals (in %)	24,949	3,119	-0.93	-51.03	-12.73	0.07	11.90	40.07	19.52
Log(Mig-in total income)	40,535	3,118	10.18	7.18	9.03	10.00	11.19	14.04	1.61
Log(Mig-in no households)	40,556	3,120	6.51	3.61	5.51	6.37	7.43	9.99	1.47
Log(Mig-in no individuals)	40,556	3,120	7.21	4.32	6.23	7.07	8.09	10.58	1.44

We report summary statistic for the economic mechanism variables delinquent mortgages, home improvement loans and residential vacancy rates in Panel A and for migration outflow and inflow in Panel B.

IA Internet Appendix

IA.1 Opioid laws and regulations

Opioid Laws and Regulations Passed between 2016 and 2018

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.

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.

IA.2 Difference-in-differences estimates: opioid laws and regulations

TABLE IA.I: SUN AND ABRAHAM (2021): ESTIMATES FOR THE EFFECT OF OPIOID LAWS ON PRESCRIPTIONS AND HOME VALUES

Panel A: Dependent Variable: Total County Prescriptions

Year Relative To	Fixed Effect	Interaction Weighted			
Legislation	Total	Total	CATT Treat-year 2016	CATT Treat-year 2017	CATT Treat-year 2018
-5	2689.698	3497.861			3497.861
	(3056.589)	(3427.335)			(3427.335)
-4	1157.515	2151.615		2176.786	2071.731
	(1947.820)	(1407.503)		(1559.227)	(3165.726)
-3	860.781	1341.518	595.802	1485.804	1448.472
	(1297.242)	(1022.672)	(2522.228)	(1244.869)	(2484.415)
-2	581.373	1017.781	-585.046	1507.798**	676.753
	(841.488)	(668.658)	(2617.587)	(748.834)	(1136.858)
-1	0.000	0.000	0.000	0.000	0.000
0	-2702.161***	-2662.167***	-5339.808***	-2405.188**	-1449.456
	(1017.822)	(870.132)	(1900.458)	(1210.829)	(1253.547)
1	-7006.290***	-6136.949***	-13886.934***	-4287.188*	
	(2512.969)	(2056.392)	(4490.847)	(2310.707)	
2	-18439.869***	-19745.131***	-19745.131***		
	(5731.248)	(5496.416)	(5496.416)		

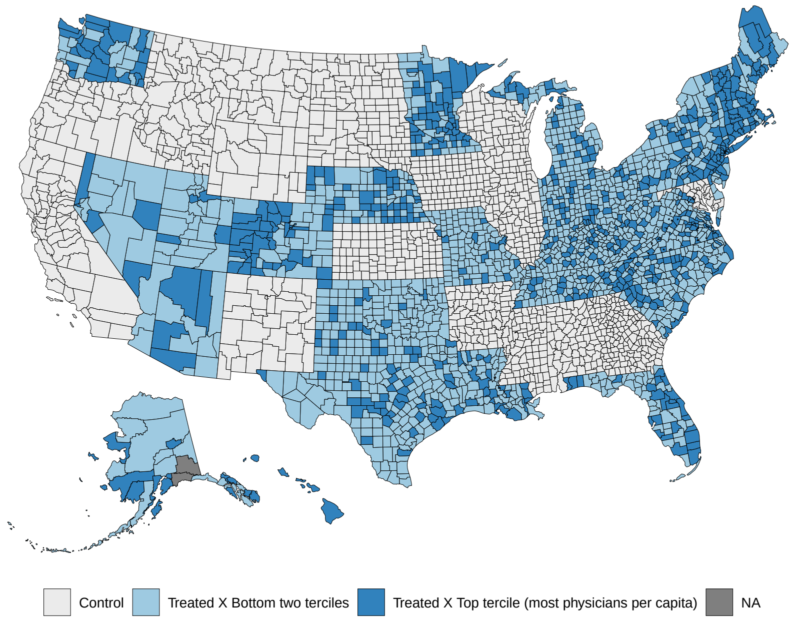
Panel B: Dependent Variable: Percentage Change in Average County Home Value

Year Relative To	Fixed Effect	Interaction Weighted			
Legislation	Total	Total	CATT Treat-year 2016	CATT Treat-year 2017	CATT Treat-year 2018
-5	-0.584	-0.471			-0.471
	(1.014)	(1.080)			(1.080)
-4	-0.405	-0.506		-0.495	-0.539
	(0.555)	(0.522)		(0.638)	(0.801)
-3	-0.166	-0.342	0.530	-0.680	0.070
	(0.380)	(0.375)	(0.692)	(0.528)	(0.574)
-2	0.016	-0.056	-0.151	-0.114	0.198
	(0.176)	(0.185)	(0.473)	(0.248)	(0.285)
-1	0.000	0.000	0.000	0.000	0.000
0	0.437***	0.423***	0.549**	0.465**	0.193
	(0.153)	(0.160)	(0.222)	(0.200)	(0.436)
1	0.954***	0.810***	1.418***	0.665*	
	(0.302)	(0.302)	(0.304)	(0.367)	
2	1.664***	1.781***	1.781***		
	(0.360)	(0.382)	(0.382)		

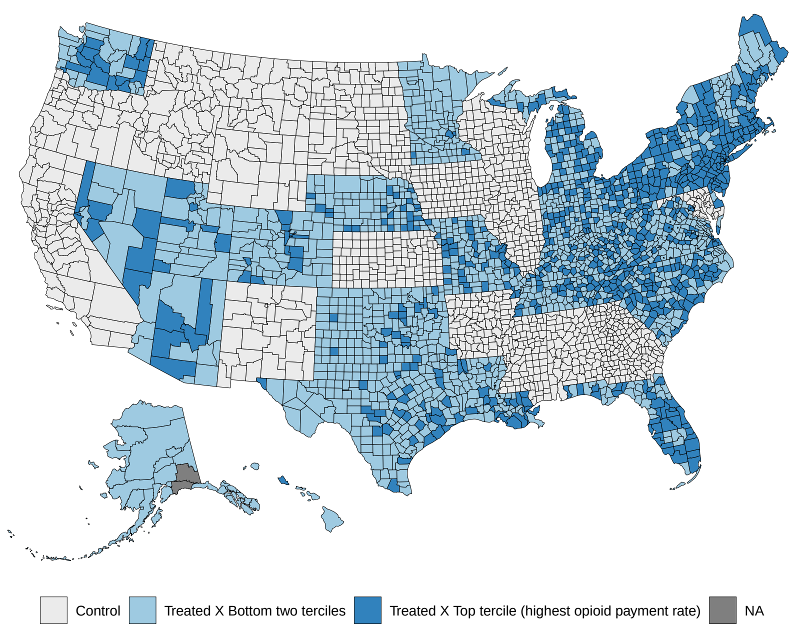
The sample period is 2013 to 2018. The dependent variable is total county prescription in Panel A and the log percentage change in average county home values in Panel B. We estimate a two-way fixed effects (FE) regression with relative time treatment dummies based on the passage of the law in column 1 as well as Sun and Abraham (2021)'s interaction weighted (IW) 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. In Panel A, we additionally include log total population as control. Standard errors are clustered at the state level. $***$ indicates $p < 0.01$, $**$ indicates $p < 0.05$, and $*$ indicates $p < 0.1$.

Figure IA.I: Interaction of opioids laws with supply propensity dummies

(a) Panel (A): Top physicians per capita tercile



(b) Panel (B): Top physician opioid payment rate tercile



Notes: We visualise the realtive treatment intensity within a state based on total physicians per capita in Panel A and physician opioid payment in Panel B. Control states/counties are coloured in grey. Treated states in the bottom two terciles based on either measure are coloured in light blue. Treated sates in the top terciel based on either measure, i.e. those where the treatement was strongest, are colored in dark blue.

TABLE IA.II: OPIOID LAW IMPACT ON PRESCRIPTION AND HOME VALUES AROUND STATE BORDERS: NO CONTROLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All counties				Excluding pill mill counties			
	Difference in County Prescription Rates over ...							
	1 year		2 years		1 year		2 years	
RD Estimate	-4.101***	-3.963***	-2.524	-2.286	-3.938***	-3.819**	-2.361	-2.105
	(1.363)	(1.410)	(2.531)	(2.599)	(1.430)	(1.488)	(2.674)	(2.750)
Observations	2725	2725	2375	2375	2546	2546	2232	2232
MSEBandwidth	104	191	117	203	104	189	118	202
Effective LHS Obs	644	944	691	955	624	895	673	915
Effective RHS Obs	684	1075	567	848	646	1004	541	793
Polynominal Order	1	2	1	2	1	2	1	2
	Percentage Change in County Average Home Values over...							
	1 year		2 years		1 year		2 years	
RD Estimate	1.338*	1.307*	2.622**	2.638*	1.358*	1.384*	2.671**	2.762**
	(0.760)	(0.785)	(1.299)	(1.355)	(0.755)	(0.766)	(1.284)	(1.313)
Observations	2551	2551	2217	2217	2374	2374	2076	2076
MSEBandwidth	134	233	151	232	125	227	143	230
Effective LHS Obs	676	872	733	871	629	826	678	827
Effective RHS Obs	821	1203	672	910	729	1109	612	854
Polynominal Order	1	2	1	2	1	2	1	2

The unit of observations are counties. In columns 1 to 4 we consider all counties. In columns 5 to 8 we exclude counties with more than 8 pill mills. We calculate one or two-year difference in prescription rate, respectively percentage changes in home values from the treatment year - 1 to the treatment year, respectively treatment year + 1. For control counties, we calculate the difference, respectively percentage change from 2015 to 2016 or 2017, as the first law was passed in 2016. We do not include additional control variables. We follow Calonico et al. (2014) to choose the optimal bandwidth. Standard errors are clustered at the state level. $***$ indicates $p < 0.01$, $**$ indicates $p < 0.05$, and $*$ indicates $p < 0.1$.

IA.3 Overdose death rate variables

As Li and Zhu (2019), we rely on the public-use mortality data that captures drug overdose mortality. These data is censored, suppressing counties with less than 10 deaths. To address the censorship issue, we first impute missing annual death rate from 2-year death rates and annual death rates. This allows us to impute below 10 death rates for annual death rates that follow or are followed by annual death rates' above 10. We also consider 3-year death counts, as this substantially increases the number of counties covered. The 10 death cut-off is now aggregated across three years. For both the annual as well as the 3-year death counts, we calculate overdose death rates per 100,000 residents. The 3-year death rate accounts for 3 years of county population.

Finally, we construct a dummy to capture counties in the top tercile of the 3-year death rate. This allows us to impute the maximum death rate of previously suppressed counties. By definition, the maximum number of deaths of suppressed observations is 9 which allows us to calculate a maximum death rate. Counties that fall into the bottom two terciles with the "maximum" death rate will be part of the bottom two terciles with their true death rate as well, which is lower or equal to the maximum death rate. However, we cannot stop after this first iteration, as counties whose imputed maximum death rate may actually be lower and therefore push counties within the bottom two terciles above the cutoff. We therefore first drop counties that are in the top tercile with their imputed death rate, as we cannot assign these counties with certainty. Next, we repeat the tercile construction in an iterative way, dropping of top-tercile imputed counties until we are only left with imputed counties at the bottom two terciles. This allows us to substantially increase observations and address the censorship issue. We end up with three alternative opioid overdose death measures.