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Opioid Crisis and Real Estate Prices *

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1 Introduction

The usage of opioids in the United States has dramatically increased over the past two decades. The Centers for Disease Control and Prevention (CDC) reports that from 1999-2018 almost 450,000 people died from an overdose involving opioids usage, including both illicit and prescription opioids[1]. The National Institute on Drug Abuse estimates that 1.7 million people in the US had a substance use disorder related to prescription opioid pain relievers in 2017[2], with visible and documented public health and economic consequences. While the existing literature has mainly focused on analyzing whether and how economic conditions played a role in the bulging opioid crisis and 'deaths of despair' (Case & Deaton, 2015), only a few papers have looked into the impact of the opioid crisis on the real economy (Ouimet *et al.*, 2020; Harris *et al.*, 2019; Jansen, 2019; Van Hasselt *et al.*, 2015, e.g). While D'Lima & Thibodeau (2019) document a negative association between house prices and drug usage, the direction of the causality and magnitudes of these effects remain unclear. In this paper we contribute to this nascent literature by estimating the impact of opioid abuse on real estate prices. We exploit a plausibly exogenous variation in the supply of opioids that is induced by a change in state-level legislation that limits the prescription of these drugs.

In response to the opioid crisis, US states passed laws and regulations limiting opioid prescriptions by physicians to address prescription drug misuse, abuse and overdose. Opioid prescription misuse is often a gateway to illicit drugs such as heroin and can result in addiction leading to long-term opioid usage. 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. However, there may be unintended consequences. Patients unable to access medical opioids may turn to heroin as last resort to reduce their pain. In addition, doctors previously unwilling to enter into open-ended opioid prescriptions may be more willing to prescribe opioids with patients referring to state limits. Understanding the impact of these policy changes on house prices is important because house prices can act as an indicator of the local economic situation and outlook. For a significant number of households, houses are the most valuable asset on their balance sheets. Rising home equity has been shown to help alleviate financing frictions and access to credit (Mian & Sufi, 2011). Housing collateral has also been documented to spur entrepreneurship, business starts and job creation, as it gives home owners pledgeable asset used for securing credit (Adelino *et al.*, 2015; Black *et al.*, 1996).

To estimate the sensitivity of house prices to the usage of prescription opioids we measure house 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 Centres for Disease Control and Prevention over the period 2006 to

[1]https://www.cdc.gov/drugoverdose/epidemic/index.html
[2]https://www.drugabuse.gov/drug-topics/opioids/opioid-overdose-crisis

2018. We start by documenting a negative correlation between home values and opioid prescription rates in the short run and over a 5-year horizon by exploiting within county variation as well as within state-year variation. This correlation is economically significant. A one standard deviation increase in dispensed opioid prescription per 100 people (41.10 prescriptions) is associated with an up to 1.36 percentage points cumulative decrease in home values over the following 5 years.

Increased and prolonged usage of opioids may result in reduction in labour productivity and consequently household income. Households may no longer invest in their houses, and be able to afford their mortgage payments. They may need to abandon their homes, leading to increased foreclosures and ultimately more vacant properties. These outcomes can affect housing values directly through the reduced quality of houses, or indirectly by reducing the attractiveness of the local area. To explore the possible underlying mechanisms between opioid prescriptions and house price changes, we test the correlation between lagged opioid prescriptions and long percentage changes in a variety of relevant variables that are related to house prices. We find that lagged opioid prescriptions are negatively correlated with median household income and the number of initiated home improvement loans, and positively correlated with vacant residential property rates and delinquent mortgages[3]. These correlations corroborate the following proposed interpretation: Delinquent mortgages have been shown in the literature to have an impact on house prices and could generate negative price spill overs to non-distressed neighbouring houses through eventual foreclosures for instance (e.g Campbell *et al.*, 2011; Anenberg & Kung, 2014). Consistent with this interpretation, we find that the relationship between lagged prescription rates and delinquent mortgages, specifically the percentage change of percentage of mortgages 90 plus days past due, is monotonically increasing for longer percentage changes. A one standard deviation increase in prescription rates translates in an up to 34.46 percentage points increase in the percentage change of percentage of mortgages 90 plus days past due over the following 5 years. The magnitude is of an order larger than the change in home prices, which highlights an economically meaningful correlation between prescription rates and delinquent mortgages and may partly be explained by large average delinquent mortgage decreases across our sample period.

We then exploit the variation in opioid prescriptions induced by the staggered passage of state laws that intend to limit the prescription of opioids. This is arguably an exogenous change in prescription rates, as most evidence suggests these are driven by supply Finkelstein *et al.* (2018) and not as much by demand for opioids (Currie *et al.*, 2019; Paulozzi *et al.*, 2014)[4]. We implement a differences-in-differences estimate where

[3]The relationship of opioids usage with home purchase loans is unclear and crime rates, both property and violent, seem uncorrelated.

[4]Ouimet *et al.* (2020) (replicated in our Online Appendix Table A.I) 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 compare the changes in house prices in years before and after the passage of the law (*the treatment*) in 'treated' counties versus 'control' counties. We show that the passage of these laws reduced opioid prescriptions as well as delinquent mortgages, and we estimate that house values in *treated* counties increased on average 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 in the second year after the passage of the law based on the interaction weighted estimate by Sun & Abraham (2020).

Although these results are consistent with a causal effect of the restricted prescription of opioids due to new laws, we cannot reject the decision the passage of these laws in each state is related to both the existing level of opioid prescriptions and consumption, as well as possible negative economic effects that may be associated with it. Similarly to Ouimet *et al.* (2020) we find no correlation between state-level economic conditions and the probability of opioid restriction laws being passed. We show that states for which the law has passed, and the ones for which the law has not changed, are on parallel trends in terms of house prices before the passage of the law, which is an identifying assumption in our methodology. Moreover, we show that the absolute prescription reduction in states that passed a law is driven by the counties within a state that are in the highest ex-ante physicians per capita quartile, where the propensity to prescribe opioids is higher as suggested by Finkelstein *et al.* (2018). House prices also rise more in these counties. Taken together, these pieces of evidence suggest that variation in county opioid prescription rates mostly drive the observed change in county house prices, and not the other way around.

Last, we document heterogeneous effects of the passage of the prescription limiting laws in house prices across different counties. The positive impact of law changes on houses prices is more pronounced for counties with higher average household income levels and lower poverty ratios. The passage of the laws has probably been most effective in regions where it had the greatest likelihood of preventing new opioid abuses, that may be locations with relatively strong prior economic conditions.

Taken together, our results provide novel evidence on the links between public health and the real economy. While unsurprisingly we find evidence of a negative relation between opioid prescription rates and house prices, more importantly, we find evidence that public health policies that were instituted with the aim of limiting opioid abuse had a far reaching effect on the housing markets.

1.1 Literature

Existing evidence on drivers of demand for opioid prescriptions has been mixed, suggesting that the observed patterns in opioid usage have been driven by variation in supply of prescription opioids. Since Case & 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 opioids occur in states with low unemployment rates (Currie *et al.*, 2019). Finkelstein *et al.* (2018) 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. 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. Ruhm (2018) finds a modest relation between economic conditions and opioid deaths.

Our paper contributes to the literature studying the impact of the opioid crisis on the U.S. economy. Harris *et al.* (2019) show the negative impact of opioid prescriptions on labor supply. Van Hasselt *et al.* (2015) and Florence *et al.* (2016) quantify the costs to the US economy due to lost productivity from opioid abuse. Cornaggia *et al.* (2020) and Li & Zhu (2019) show the impact of opioids on municipal bond rates. Jansen (2019) looks at the impact on opioids on auto loans.

We also contribute to the literature that examines the effects of public health conditions on real estate and asset markets. For instance, using 20 years of data from Massachusetts, Campbell *et al.* (2011) show that houses sold after foreclosure, or close in time to the death or bankruptcy of at least one seller, are sold at an average foreclosure discount of 28%. Tyndall (2019) 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. Cheng *et al.* (2019) find that the staggered passage of state medical marijuana laws increases state bond offerings and trading spreads by 7 to 11 basis points. 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. More recently, using data from 7th-century Amsterdam plague-, and 19th-century Paris cholera outbreaks, Francke & Korevaar (2020) show that the outbreaks resulted in large declines in house prices, and smaller declines in rent prices.

2 Opioid Crisis Background

A more aggressive approach to pain treatment started in the 1980s in the U.S. medical community. Following the 1995 FDA approval of OxyContin (oxycodone controlled-release), a new prescription opioid, 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. 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[5]. This paved a way for creation of

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

a new metric upon which doctors and hospitals would be judged. Concerns about the possible over-use of opioid prescriptions for chronic pain conditions gained attention in early 2000s. 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[6], and in 2017, the TJC issued new standards on the treatment of pain[7].

Several states have also taken 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 & Carey, 2018; Meara *et al.*, 2016; Islam & McRae, 2014). Recent measures were more drastic 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. 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 determinants such as local economic, health and political characteristics do not seem to matter. In October of 2017, the US government declared opioids a public health emergency. At the federal level, Medicare also adopted a 7-day supply limit for new opioid patients in 2018.

3 Data

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 1 reports summary statistics. Between 2006 and 2018, 2,823 counties are covered on average per annum with an average number of 82.6 opioid prescription per 100 people. These high prescription rates with large county variation are consistent with past literature and other data sets (Currie *et al.*, 2019; Harris *et al.*, 2019; Ouimet *et al.*, 2020).

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

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

To measure monthly 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. Moreover, the concern that contemporaneous drug prescriptions are correlated with local economic conditions is minimized by longer horizon percentage changes in home values. 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.

We collect data on the percentage of mortgages 90 or more days delinquent 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 capture more severe economic distress. The coverage of this measure is less extensive, 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 percent of mortgages 90 or more days delinquent between 2006 and 2018 was 2.27%. The average 1-year percentage change was -7.16% (compare Panel A in Table 1). The average reduction in percent of mortgages 90 or more days delinquent in our sample is large, as the height of percentage of delinquent mortgages was reached at the beginning of our sample in 2010. Since then, it has steadily decreased. As we are interested in cross-sectional differences, this is not a major concern.

As control variables and sample split variables we collect other county demographic and economic variables. Demographic variables include male ratio, white ratio, black ratio, Indian American ratio, Hispanic ratio, age 20-64 ratio, age over 65 ratio and migration flow from the Census Bureau, neoplasms mortality from CDC as well as number of primary care physicians, excluding hospital residents or age 75 years or over, from the Health Resources and Services Administration. Economic variables include poverty ratio and median household income from the Census Bureau, as well as unemployment rate and labour force participation rate from Bureau of Labour Statistics. All variables are normalized by contemporaneous county population. Further, to shed some light on the mechanism of opioid presrcriptions and house price changes, we collect data on home improvement loans and home purchase mortgages from the Home Mortgage Disclosure Act, vacancy rate data from the United States Postal Service, and crime data from FBI's Uniform Crime Reporting gathered by Kaplan (2021).

4 Methodology and Results

4.1 Correlation between house values and prescription rates

We first document the correlation between house values and 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 residualized 5-year percentage change in home values. The maps show that counties in the bottom quintile of residualized 5-year 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 residuals.

[Insert Figure 1]

We further examine this relationship by estimating the following specifications:

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

$$PCHomeValue_{c,t-x \ to \ t} = \alpha + \beta PrescriptionRate_{c,t-x} + \gamma Controls_{c,t-x} + \zeta_{s,t} + \epsilon_{ct} \tag{2}$$

The dependent variable $PCHomeValue_{c,t-x \ to \ t}$ in 1 and 2 is a 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.* (2020), county-level controls measured at $t - x$ include: Male population ratio, White ratio, Black ratio, American-Indian ratio, Hispanic ratio, Age 20-64 ratio, Age over 65 ratio, Migration Inflow ratio, Poverty ratio, Unemployment ratio, labor force participation ratio, neoplasm mortality, and number of physicians per county; and also county fixed effects θ_c and year fixed effects τ_t, respectively state-year fixed effects $\zeta_{s,t}$.

The results of the estimation with the percentage change in home values are shown in Figure 2. Panel A includes county fixed effects θ_c, and year fixed effects τ_t, whereas Panel B includes state-year fixed effects $\zeta_{s,t}$. All variables are winsorized at the 2 and 98 % level, and standard errors are clustered at the county level. Whereas the results presented in Panel A take into account unobserved heterogeneity across counties and over time, specification shown in Panel B explores variation across counties located within the same state at the same point in time.

We find that house values and prescription rates are negatively associated in the short run and 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 house values is estimated at -0.01, while the correlation with 5-years changes are at -0.03, when exploiting within county variation. A one standard deviation increase in prescription rates (41.10 prescriptions per 100 people for the 5-year lagged sample) translates in 1.36 percentage points reduction in house prices growth rates, which is equivalent to 25.4% of the 5-year average percentage home value increase (5.36%). The standard deviation of average county house price percentage changes is much smaller within states than across the US. Averaging within state-year standard deviations across states and years leads to a standard deviation for 5-year percentage change in house prices of 6.50% compared to 10.87% when averaging US wide annual standard deviations across years. Unsurprisingly, point estimates obtained from within state-year variation are also more modest at -0.002 for 1-year change in house value and -0.008 for 5-year change in house value. Taking a one standard deviation change of prescription rates translates into a 0.33 percentage points change in house prices.

[Insert Figure 2 about here]

To inform the discussion about potential mechanisms for the impact of opioid usage on house prices, we assess the impact of opioid usage on delinquent mortgages and other variables related to house prices, namely median household income, number of initiated home improvement loans, number of initiated home purchase loans, residential property vacancy rates, as well as property and violent crime rates. Opioid prescriptions may lead to over-consumption and addiction and therefore directly affect individual's ability to earn income and invest into, respectively afford their housing. This can affect the quality and ultimately the price of houses. We would therefore expect median household income and the number of initiated home improvement loans to be negatively correlated with lagged prescription rates. In a next stage, it may translate into more delinquent mortgages and vacant properties. Opioid prescription may also indirectly affect house prices by lowering the attractiveness of the area if residents cut the investment in their properties, or crime rates in the area increase. More delinquent mortgages and vacant properties may itself be a sign of an area being less attractive. We may further see a negative correlation with initiated home purchase loans and a positive correlation with crime rates. We apply the same framework as before and plug in each of the discussed variables as dependent variable to assess the correlation with lagged prescription rates:

$$DepVar_{c,t-x\ to\ t} = \alpha + \beta PrescriptionRate_{c,t-x} + \gamma Controls_{c,t-x} + \theta_c + \tau_t + \epsilon_{ct} \quad (3)$$

$$DepVar_{c,t-x\ to\ t} = \alpha + \beta PrescriptionRate_{c,t-x} + \gamma Controls_{c,t-x} + \zeta_{s,t} + \epsilon_{ct} \quad (4)$$

$DepVar_{c,t-x\ to\ t}$ in 3 and 4 is a log percentage change of each variable in county c, $(log(DepVar_t/DepVar_{t-x}) * 100)$ over $X = \{1,2,3,4,5\}$ years for $DepVar_{c,t-x\ to\ t}$. Prescription rates, county controls and fixed effects are as before. Table 2 presents the correlations with both set of fixed effects. We find lagged prescription rates are indeed negatively correlated with median household income and the number of initiated home improvement loans. We also find that lagged prescription rates are positively correlated with vacancy rate of residential property and delinquent mortgages. We do not find a correlation with property or violent crime rates. The correlation with the number of initiated home improvement loans is not decisive across both fixed effects. The positive correlation between the percentage change of percentage of mortgages 90 plus days past due and lagged prescription rates in the short and long run is economically meaningful and monotonically increasing over longer percentage changes. Figure 3 highlights this for both, the inclusion of county and year fixed effects in Panel and state-year fixed effects in Panel B. Considering the estimation with county and year fixed effects, a one standard deviation increase in prescription rates (41.10 prescriptions per 100 people for the 5-year lagged sample) translates in a 34.46 percentage points greater percentage change of percentage of mortgages 90 plus days past due, which is equivalent to 47.9% of the 5-year average percentage mortgages 90 plus days past due decrease (-71.94%). This highlights delinquent mortgages following opioid abuse as one possible important channel.

[Insert Figure 3 about here]

[Insert Table 2 about here]

4.2 DiD Lead & Lags

The specification above does not rule out the possibility of reverse causality, meaning that a decline in house prices can affect opioid usage, thus resulting in increase in prescription rates. For this reason we make use of a quasi-natural experiment that relies on staggered adoption of state laws limiting opioid prescription rates. Starting with Massachusetts in 2016, several states passed laws or regulations[8] to limit opioid prescriptions. The law imposed a seven-day limit of opioid prescriptions, with exemptions for cancer pain, chronic pain,

[8] We consider both 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.

and for palliative care. Several states followed suit: 8 more states followed in 2016, while 18 states passed legislation that imposes limits on opioid prescriptions in 2017 and another 5 in 2018. A short description of the state laws and regulations is included in the Internet Appendix. Panel B in Table 1 translates this into county observations. Consistent with Ouimet *et al.* (2020), Internet Appendix Table A.I shows 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 matter.

We estimate the impact of opioid prescriptions on the value of real estate by exploiting variation in opioid prescriptions induced by the staggered passage of state-level laws that aim to limit the consumption of opioids. We start by examining the link between the passage of the laws and actual opioid prescriptions, to establish the effectiveness of the law changes. We implement a differences-in-differences framework to compare changes in total county opioid prescriptions in years before and after the passage of the law (*the treatment*) in 'treated' counties 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 prescriptions, respectively of changes in house price and delinquent mortgages, before and after the law. Recent literature on staggered differences-in-differences design (e.g. Callaway & Sant'Anna, 2020; Sun & Abraham, 2020) highlight that running a staggered regression only with lead and lags are potentially problematic. First, weights across treatment cohorts can be non-intuitive. Second, already treated units act as controls for newly treated units, which is particularly problematic for trend break effects rather than unit shifts. Sun & Abraham (2020) propose an intuitive alternative approach that is based on leads and lags of the treatment. We follow their 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:

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

The dependent variable $Prescriptions_{c,t}$ is defined as total county prescriptions in year t. θ_c and τ_t 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 & Abraham (2020) 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. There are thus a three dummies that are 1 for counties that passed the law in the specific cohort year and zero for any other county. This allows to estimate cohort-specific average treatment effects. We additionally include county controls as defined before as well as lagged log county population.

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 house prices as well as the changes in mortgages 90 plus days past due in years before and after the passage of the law (*the treatment*) in 'treated' counties versus 'control' counties.

$$PCHomeValue_{c,t} = \alpha + \sum_{e \in \{16,17,18\}} \sum_{l=-5, \neq -1}^{2} \delta_{e,l} \mathbf{1}\{E_i = e\} D_{ct}^{\ell} + \gamma Controls_{c,t-1} + \theta_c + \tau_t + \epsilon_{c,t} \quad (6)$$

$$PCMtgsPastDue_{c,t} = \alpha + \sum_{e \in \{16,17,18\}} \sum_{l=-5, \neq -1}^{2} \delta_{e,l} \mathbf{1}\{E_i = e\} D_{ct}^{\ell} + \gamma Controls_{c,t-1} + \theta_c + \tau_t + \epsilon_{c,t} \quad (7)$$

Where the dependent variable $PCHomeValue_{c,t}$ is defined as in Specification 1 and the dependent variable $PCMtgsPastDue_{c,t}$ as in Specification 3 using 1-year percentage changes. County controls are the same as in previous specification except for county population. Standard errors are clustered at the state level, as the laws were introduced at the state level.

[Insert Figure 4 about here]

Figure 4 plots the coefficient of the total interaction weighted coefficient for each relative time period with the 95% confidence interval. The full set of coefficients for each $CATT(e, \ell)$ can be found in Table A.I in the Appendix.

Panel A shows that absolute county prescriptions declined more on average after the passage of the laws in *treated* counties, relative to control group. At the same time, as shown in Panel B, *treated* counties experienced a higher increase in house prices, relative to untreated counties, as well as a larger decrease in delinquent mortgages, see Panel C. Counties in states that passed a law saw their house prices rise about 0.81 percentage points more on average relative to counties in states without in the first year after the law was passed ("Plus 1") and mortgages 90 plus days past due decrease by about 6.17 percentage points more on average. This confirms the pattern we found in the long correlations.

We next explore whether the effect of the state law was strongest where the propensity to dispense opioids prior to the passage of the law was probably highest. Finkelstein *et al.* (2018) show that the number of physicians per capita is positively correlated with opioid prescriptions and is one important supply factor of opioids. We run the following standard two-way fixed effect regression with calendar year τ_t and county θ_c fixed effects. $Post_{ct}$ is a dummy that becomes one on the treatment year and stays one afterwards. To account for different propensities to supply opioids within a state and therefore different impacts of the law at the county level, we construct a dummy, $Physicians\ p.c.\ highest\ quartile_c$, that is one for counties that are in the highest quartile within each state based on a 5-year average number of physicians per capita before the first passage of any state law, i.e. between 2011 and 2015. We interact this dummy with the post coefficient. To be conservative, we continue to cluster standard errors are at the state level.

$$Prescriptions_{ct} = \alpha + \beta_1 Post_{ct} + \gamma Controls_{ct-1} + \theta_c + \tau_t + \epsilon_{ct} \tag{8}$$

$$Prescriptions_{ct} = \alpha + \beta_1 Post_{ct} + \beta_1 Post_{ct} X Physicians\ p.c.\ highest\ quartile_{c \gamma Controls_{ct-1} + \theta_c + \tau_t + \epsilon_{ct}} \tag{9}$$

(10)

We run this regression with both the absolute county prescriptions and house price changes as dependent variable. Goodman-Bacon (2021) highlights that the general estimator from a TWFE 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 first execute Goodman-Bacon (2021) decomposition. $Post_{ct}$ is the coefficient of interest. We have nine individual 2x2 DiD estimators. Earlier vs Later Treated 2x2 DiD estimators include cohort 2016 compared to cohort 2017 respectively 2018 and cohort 2017 vs cohort 2018. Later vs Earlier Treated 2x2 DiD estimators includes cohort 2017 vs cohort 2016, cohort 2018 vs cohort 2017 and cohort 2018 vs cohort 2016. Finally, for the Treated vs Untreated two-group/two period DiD estimators we have cohort 2016, 2017 respectively 2018 compared to 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 5 shows the decomposition for the three dependent variables total county prescriptions, percentage change in home values and percentage change in percentage of mortgages 90 plus days past due for the full sample. We can identify two interesting patterns. First, the individual 2x2 DiD 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 2x2 DiDs tend to have the opposite sign compared to the other 2x2 DiDs. Given that the parallel trends in Figure 4 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 in any decomposition. Hence, their impact on the total beta is marginal.

Table 3 shows that the drop in absolute prescriptions following the passage of the law was concentrated in the counties in the quartile with the highest number of physicians per capita in line with Finkelstein *et al.* (2018)'s findings. While house price changes seemed to be greater across all counties, they were greatest in counties in the top quartile of physicians per capita. This is further evidence that the change in the propensity to prescribe opioids (and therefore become addictive) caused by the law drove changes in house price changes.

[Insert Table 3 about here]

4.3 Heterogeneous Effects

We next explore how the effect of the passage of the laws that limit opioid prescriptions may have differed in counties based on observable *ex-ante* local economic variables. We again run the mentioned two-way fixed effect regression, but now consider the full sample as well as tercile splits based on the two variables: household income and poverty ratio:

$$PCHomeValue_{ct} = \alpha + \beta Post_{ct} + \gamma Controls_{ct-1} + \theta_c + \tau_t + \epsilon_{ct} \quad (11)$$

$$PCMtgsPastDue_{ct} = \alpha + \beta Post_{ct} + \gamma Controls_{ct-1} + \theta_c + \tau_t + \epsilon_{ct} \quad (12)$$

For the sample splits, we use county averages from 2011 to 2015 to capture the state of the county *prior* to the passage of laws limiting opioid prescriptions. We rank all counties within each state to form the tercile splits within each state and report summary statistics for these variables in Panel C of Table 1. The dependent variable corresponds to total county prescriptions ($Prescriptions_{c,t}$) as defined in Specification 5, respectively log percentage change in average home value ($PCHomeValue_{c,t}$), as defined in Specification 1.

County controls are the same as in previous specifications for each dependent variable and standard errors are clustered at the state level.

[Insert Figure 6 about here]

Results shown in Figure 6 document heterogeneous effect of the passage of these laws on subsequent county-level total prescriptions (in orange) and house price changes (in blue) based on the ex-ante county public health and economic conditions. The positive effect of the law passage on house prices is most pronounced in: counties with top-tercile average household income, and bottom-tercile average poverty ratio. This coincides to some extent with counties that have seen the largest drop in total prescriptions following the passage of these laws. It is difficult to develop a clear picture of which ex-ante county characteristics determined the most positive house price response to new opioid prescriptions limitations. What the results suggest is that the impact of the laws was *not* concentrated in the counties with the least favourable prior economic conditions. If anything, they point in the opposite direction: the effects on subsequent delinquent mortgages and house price changes were strongest in counties with relatively strong economic conditions. Counties with high average household income and low poverty ratio seem to have seen the strongest positive effect of limiting opioid prescription rates on subsequent house prices changes. Having in mind that house prices in general are slow moving, it is thus not surprising that the passage of these laws had a lesser effect on counties with less favorable economic conditions prior to their passage. However, the differences between the terciles generally is not large. The passage of the laws has probably been most effective in regions where it had the greatest likelihood of preventing new opioid abuses. This characteristic is difficult to pinpoint down empirically, but may help in reconciling the results. Figure 6 points to this direction. Counties with less favourable prior economic conditions might have seen more undocumented usage of opioids in response to law passage, which would result in the observed smaller effects of the passage of these laws. Yet again, the differences are marginal only.

5 Conclusion

This paper estimates the sensitivity of house prices to opioid abuse. We find a negative association between house prices and opioid prescriptions that is persistent in the long run and a positive association between delinquent mortgages and opioid prescriptions. We exploit variation in opioid prescriptions induced by staggered passage of state laws that aim to limit these prescriptions as a source of exogenous variation. House prices respond positively to the passage of the state laws and the percentage of mortgages 90 days plus past due decreases subsequent to the passage relatively. Our results have three main implications. First, they suggest that although opioid usage has been associated with low income and economically disadvantaged conditions (Case & 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 house values. Second, lost labour productivity and thus household income may be one driver of how opioids via delinquent mortgages and foreclosures impacted house prices. We present evidence on the impact of delinquent mortgages here. Third, our results indicate some heterogeneity in these effects: counties in the top quartile of physicians per capita seem to have driven the change in opioid prescriptions and report the highest change in house prices following the passage of the law. This is some evidence that the law's impact on the propensity to change opioid prescription drove house price changes. Further we find that counties with high average household income and low poverty ratio seem to have seen the strongest positive effect of limiting opioid prescription rates on subsequent house prices changes. The passage of the laws may have been more effective in regions with relatively strong prior economic conditions.

Our work offers insights into externalities of public health policies. While unsurprisingly we find evidence of a negative relation between opioid prescription rates and house prices, more importantly, 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 policy and real economic outcomes.

6 Figures & Tables

6.1 Main Figures

TABLE 1: DESCRIPTIVES MAIN VARIABLES, HETEROGENEITY VARIABLES & LAWS

	Panel A: Descriptive Stats Key Quantitative Variables								
	N total	Avg N Annual	Mean	Min	P25	Median	P75	Max	Std. Dev.
Prescription Rate (per 100)	39799	2842.79	79.28	3.50	48.80	74.80	104.30	195.80	42.96
County Prescriptions	39799	2842.79	69472.75	230.86	8122.83	25392.48	71740.47	571185.15	114847.86
Avg Home Value ($)	36329	2594.93	141981.97	47687.05	86280.58	119037.75	171709.92	429995.62	80553.01
1-year Perc Change HV (in %)	33481	2575.46	1.71	-9.90	-1.05	2.32	4.85	10.17	4.44
2-year Perc Change HV (in %)	30633	2552.75	3.17	-18.54	-2.21	4.25	9.37	19.14	8.46
3-year Perc Change HV (in %)	27799	2527.18	4.52	-26.21	-3.27	5.64	13.42	27.24	11.99
4-year Perc Change HV (in %)	24990	2499.00	6.01	-30.86	-3.79	6.94	16.97	35.00	14.86
5-year Perc Change HV (in %)	22227	2469.67	7.55	-33.26	-3.83	8.11	19.92	41.90	17.03
Percent of Mortgages 90+ days past due	5640	470.00	2.27	0.36	1.12	1.89	3.03	7.25	1.55
1- year Perc Change Mtgs 90+ days past (in %)	5170	470.00	-7.16	-54.30	-26.73	-13.19	4.43	82.66	30.05
2- year Perc Change Mtgs 90+ days past (in %)	4700	470.00	-21.10	-91.63	-49.45	-30.04	-3.14	100.62	43.89
3- year Perc Change Mtgs 90+ days past (in %)	4230	470.00	-37.52	-127.85	-72.29	-47.85	-13.15	100.25	53.06
4- year Perc Change Mtgs 90+ days past (in %)	3760	470.00	-54.33	-165.11	-94.54	-65.29	-26.30	105.46	62.05
5- year Perc Change Mtgs 90+ days past (in %)	3290	470.00	-71.94	-194.98	-115.26	-81.09	-37.48	90.28	66.01
Median Household Income (in 000$)	43955	3139.64	49.55	29.47	41.00	47.76	56.06	82.96	11.92
Poverty Ratio (in %)	43955	3139.64	0.18	0.07	0.13	0.17	0.22	0.36	0.07
UnemploymnentRate (in %)	40806	3138.92	0.06	0.02	0.04	0.06	0.08	0.14	0.03

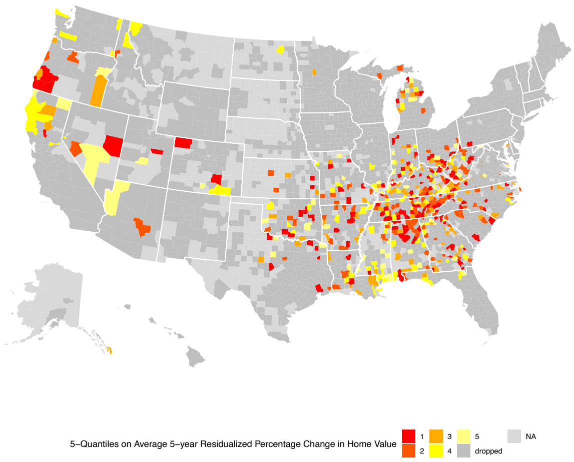
	Panel B: County/State Observations for Opioid Law Introductions					
	Opioid Prescriptions Obs		Home Value Obs		Mortgages 90+ past due Obs	
	States	Counties	States	Counties	States	Counties
State Law Passed in 2016	9	279	9	253	9	81
State Law Passed in 2017	18	1095	18	1060	18	169
State Law Passed in 2018	5	340	5	334	5	60

	Panel C: Descriptive Stats Sample Split Variables							
	N	Mean	Min	P25	Median	P75	Max	Std. Dev.
Prescription Rate (per 100) Average 2011 2015	2971	84.75	2.40	54.35	81.88	112.56	202.58	45.42
Poverty Ratio (in %) Average 2011 2015	3141	19.10	7.93	14.16	18.25	23.22	36.62	6.60
Household Income (in 000$) Average 2011 2015	3141	49.07	31.19	41.44	47.50	54.96	79.44	10.70

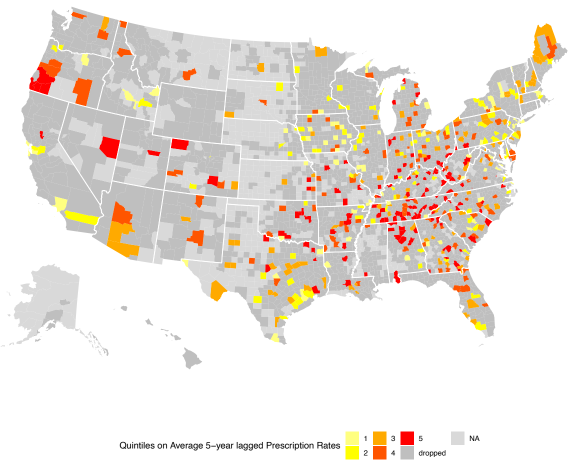
Panel A reports descriptive statistics for prescription rates and home values. Panel B reports the number of states that passed laws intended limit opioid abuse as well as the number of county observations with data. Panel C reports descriptive statistics for 2011-2015 averages that form the basis of sub-sample splits on the passage of the opioid state laws.

FIGURE 1: US COUNTY MAP ON RESIDUAL PERCENTAGE CHANGE IN HOME VALUES AND LAGGED PRESCRIPTION RATE

(A) KEEPING ONLY COUNTIES IN HIGHEST PRESCRIPTION RATE QUINTILE AND COLORING BY RESIDUALIZED PERCENTAGE CHANGE IN HOME VALUES



(B) KEEPING ONLY COUNTIES IN LOWEST RESIDUALIZED PERCENTAGE CHANGE IN HOME VALUES QUINTILE AND COLORING BY PRESCRIPTION RATE

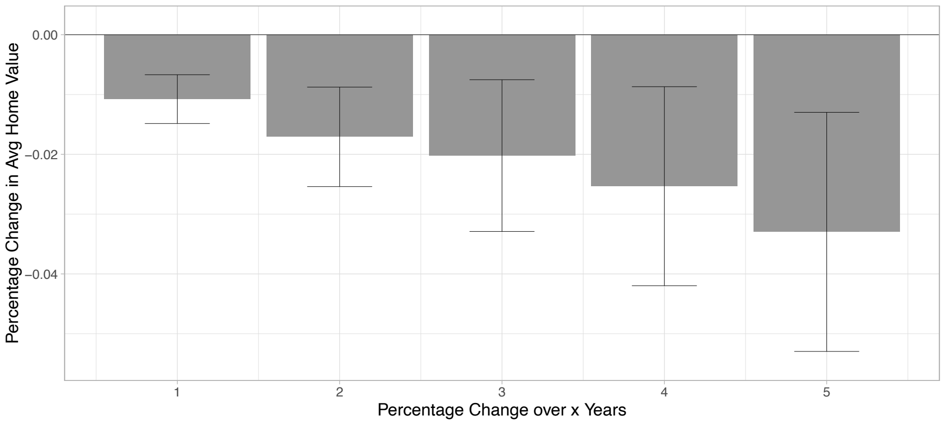


Notes: We first calculate quintiles of the county average 5-year lagged prescription rate as well as average residuals of 5-year percentage change in home values from 2011 to 2018. We take the residuals on 5-year percentage change in home values from the following regression $PCHomeValue_{c,t-5\ to\ t} = \alpha + \gamma Controls_{ct-5} + \theta_c + \tau_t + \epsilon_{ct}$, average them per county (c) across the years (t) 2011 to 2018.
For Panel A, we only keep counties in the highest prescription rate quintile. Counties dropped are dark grey, counties without data are light grey. Heat colours for the remaining counties are based on the quintiles of the residuals of 5-year percentage calculated across the whole sample. Dark red belongs to the smallest residual, i.e. lowest percentage change in home values unexplained by the other controls.
For Panel B, we reverse the approach. We only keep counties in the lowest quintile for residualized percentage change in home values and assign heat map colors based on the prescription rate quintile over the whole sample. Dark red this time corresponds to the highest prescription rate quintile.

We expect house price changes and opioid prescriptions to be negatively correlated and therefore expect dark red maps and a large overlap between the two maps.

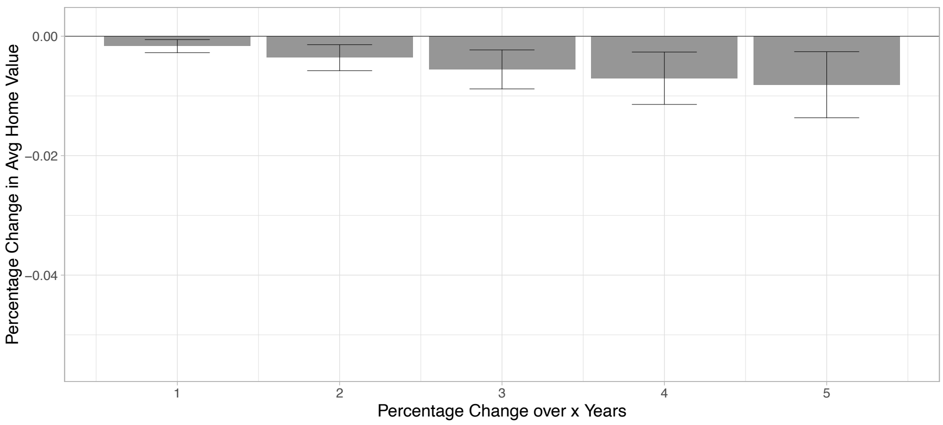
FIGURE 2: LONG CORRELATIONS PERC. CHANGE HOUSE VALUE & LAGGED OPIOID PRESCRIPTION RATES

(A) COUNTY FIXED EFFECTS & YEAR FIXED EFFECTS



	over 1 Year	over 2 Years	over 3 Years	over 4 Years	over 5 Years
Adjusted R2	0.599	0.648	0.699	0.735	0.767
No. of Observations	25026	22510	19993	17487	15075
SD Prescription Rate	42.980	42.660	42.085	41.727	41.074

(B) STATE-YEAR FIXED EFFECTS

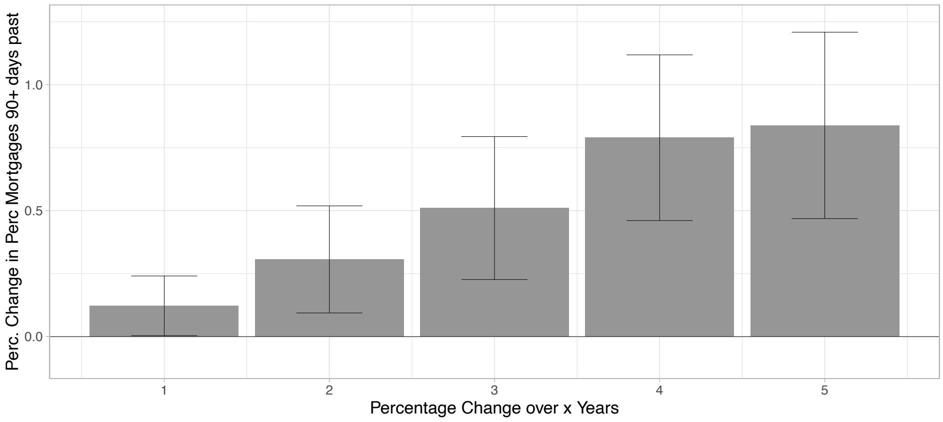


	over 1 Year	over 2 Years	over 3 Years	over 4 Years	over 5 Years
Adjusted R2	0.759	0.806	0.838	0.854	0.854
No. of Observations	25081	22572	20077	17590	15157
SD Prescription Rate within state&year	29.861	29.506	29.121	28.760	28.037

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. The plots report coefficients and 95% confidence intervals on lagged prescription rates. County controls included are 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. Prescription data is available from 2006 to 2018 and the lag determines the length of the time period. Panel A includes County 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.

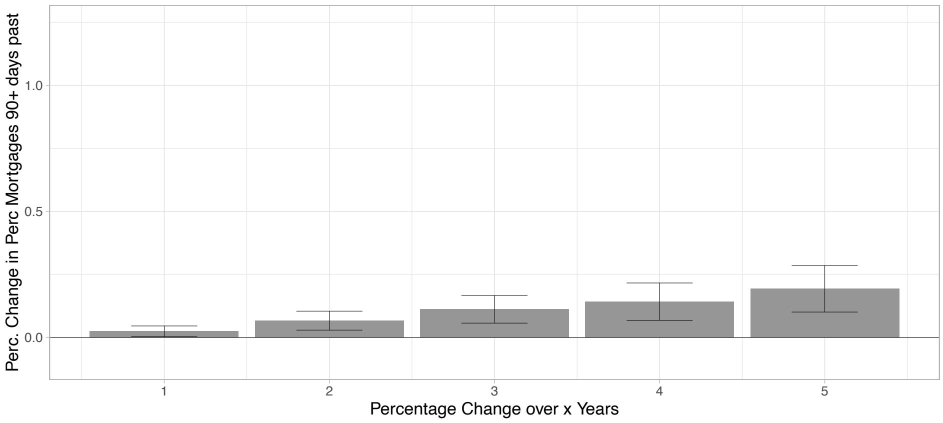
FIGURE 3: LONG CORRELATIONS PERC. CHANGE MORTGAGES 90 DAYS PLUS PAST DUE & LAGGED OPIOID PRESCRIPTION RATES

(A) COUNTY FIXED EFFECTS & YEAR FIXED EFFECTS



	over 1 Year	over 2 Years	over 3 Years	over 4 Years	over 5 Years
Adjusted R2	0.767	0.810	0.827	0.864	0.879
No. of Observations	4230	3760	3290	2820	2350
SD Prescription Rate	26.788	26.568	25.952	25.490	24.524

(B) STATE-YEAR FIXED EFFECTS



	over 1 Year	over 2 Years	over 3 Years	over 4 Years	over 5 Years
Adjusted R2	0.822	0.863	0.875	0.893	0.890
No. of Observations	4176	3712	3248	2784	2320
SD Prescription Rate within state&year	17.645	17.424	16.960	16.609	15.985

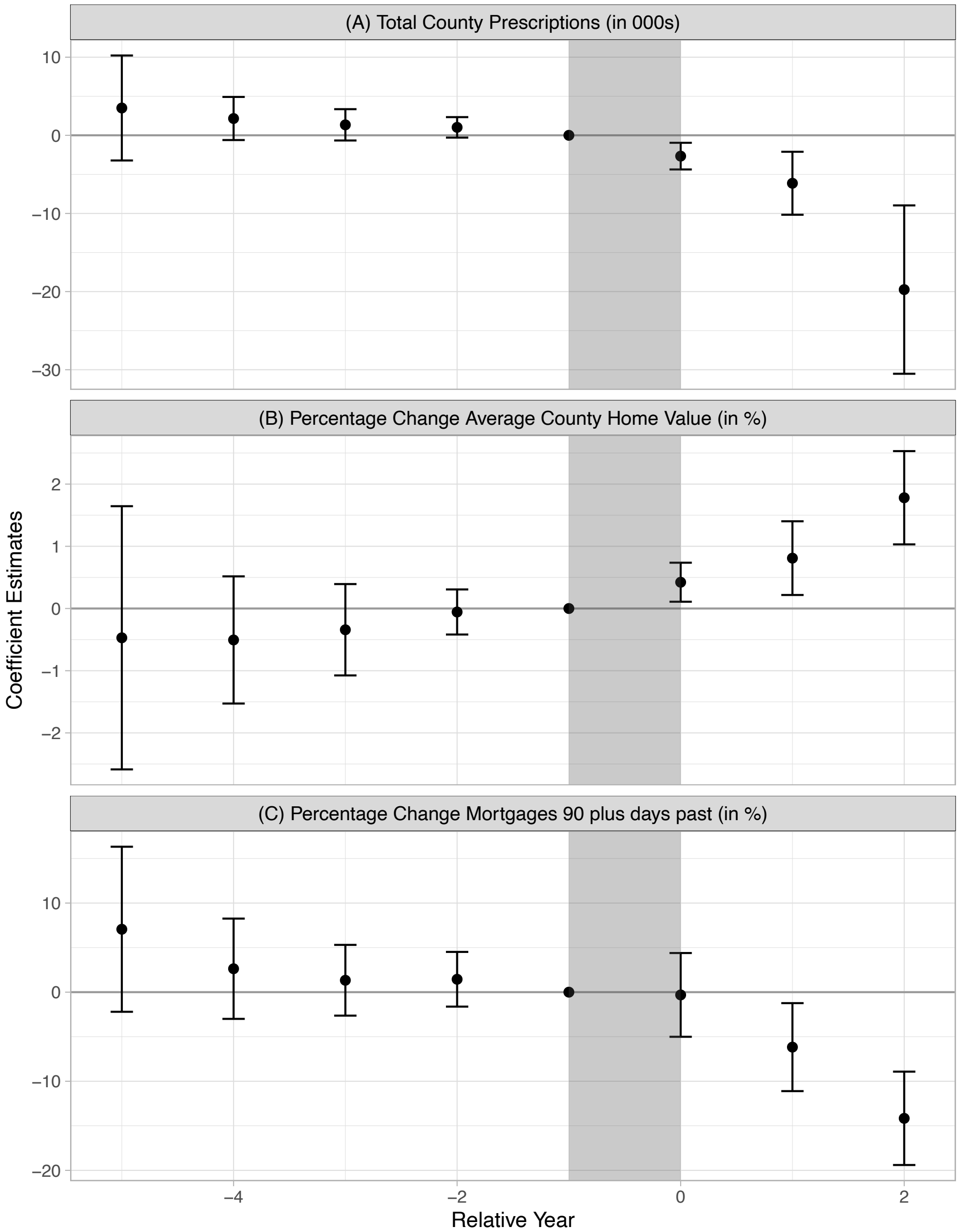
The dependent variable is a log percentage change of the percentage of mortgages 90 days plus past due ($log(MtgsPastDue_t / MtgsPastDue_{t-x}) * 100$) over 1, 2, 3, 4 and 5 years. The plots report coefficients and 95% confidence intervals on lagged prescription rates. County controls included are the 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. Prescription data is available from 2006 to 2018 and the lag determines the length of the time period. Panel A includes County 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.

TABLE 2: LONG CORRELATIONS - POSSIBLE HOUSE PRICE DRIVERS & LAGGED PRESCRIPTION RATES

	1-year Perc. Change		2-year Perc. Change		3-year Perc. Change		4-year Perc. Change		5-year Perc. Change	
Panel (A) dependent variable: Median household income										
l_prescriptionrate	-0.001	-0.002^{***}	-0.002	-0.003^{***}	-0.005	-0.006^{***}	-0.005	-0.007^{***}	-0.004	-0.009^{***}
	(0.002)	(0.000)	(0.002)	(0.001)	(0.003)	(0.001)	(0.004)	(0.001)	(0.005)	(0.001)
R2	0.207	0.187	0.366	0.339	0.438	0.388	0.519	0.449	0.569	0.469
N	27486	27540	24920	24975	22360	22421	19753	19847	17222	17288
Panel (B) dependent variable: Number of initiated home improvement loans										
l_prescriptionrate	-0.075^{***}	-0.007^{*}	-0.109^{***}	-0.011^{**}	-0.090^{***}	-0.014^{**}	-0.142^{***}	-0.021^{**}	-0.175^{***}	-0.024^{**}
	(0.016)	(0.003)	(0.024)	(0.005)	(0.033)	(0.007)	(0.041)	(0.009)	(0.045)	(0.010)
R2	0.400	0.491	0.579	0.645	0.634	0.682	0.646	0.670	0.672	0.661
N	24973	25030	22407	22470	19845	19912	17256	17353	14721	14794
Panel (C) dependent variable: Number of initiated home purchase loans										
l_prescriptionrate	-0.003	-0.004^{**}	-0.003	-0.007^{**}	0.000	-0.011^{***}	0.013	-0.012^{**}	0.027	-0.013^{*}
	(0.007)	(0.002)	(0.013)	(0.003)	(0.018)	(0.004)	(0.023)	(0.006)	(0.027)	(0.007)
R2	0.571	0.595	0.699	0.713	0.722	0.726	0.743	0.729	0.759	0.723
N	25022	25081	22454	22517	19889	19961	17285	17389	14752	14830
Panel (D) dependent variable: Vacancy rate residential properties										
l_prescriptionrate	0.023^{*}	0.019^{***}	0.090^{***}	0.036^{***}	0.182^{***}	0.047^{***}	0.234^{***}	0.052^{***}	0.267^{***}	0.062^{***}
	(0.014)	(0.004)	(0.025)	(0.009)	(0.037)	(0.013)	(0.043)	(0.017)	(0.047)	(0.022)
R2	0.234	0.227	0.348	0.266	0.491	0.302	0.641	0.326	0.758	0.338
N	19563	19633	17028	17101	14514	14599	11978	12083	9488	9589
Panel (E) dependent variable: Property crime rate										
l_prescriptionrate	0.003	0.004	0.025	0.003	0.030	0.008	0.029	0.005	0.012	0.004
	(0.011)	(0.003)	(0.018)	(0.005)	(0.024)	(0.007)	(0.028)	(0.009)	(0.032)	(0.011)
R2	0.0868	0.109	0.135	0.122	0.194	0.129	0.266	0.150	0.361	0.179
N	25469	25508	23108	23147	20738	20783	18348	18417	16002	16052
Panel (F) dependent variable: Violent crime rate										
l_prescriptionrate	-0.002	0.003	0.020	0.004	0.012	0.003	0.026	0.002	0.034	-0.002
	(0.018)	(0.005)	(0.028)	(0.008)	(0.036)	(0.010)	(0.045)	(0.013)	(0.051)	(0.015)
R2	0.0524	0.0532	0.0975	0.0678	0.152	0.0822	0.205	0.101	0.274	0.121
N	25067	25113	22741	22789	20396	20451	18039	18112	15736	15790
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County F.E.	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Year F.E.	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
State-Year F.E.	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Cluster	County	County	County	County	County	County	County	County	County	County

We calculate long percentage changes for six different dependent variables, namely median household income, the number of initiated home improvement loans, number of initiated home purchase loans, the vacancy rate of residential property, the property crime rate and violent crime rate, over 1, 2, 3, 4 and 5 years. County controls included are 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. Prescription data is available from 2006 to 2018 and the lag determines the length of the time period. Panel A includes County 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. $***$ indicates $p < 0.01$, $**$ indicates $p < 0.05$, and $*$ indicates $p < 0.1$.

Figure 4: Coefficient Estimates of Year Dummies Relative to Law Introduction

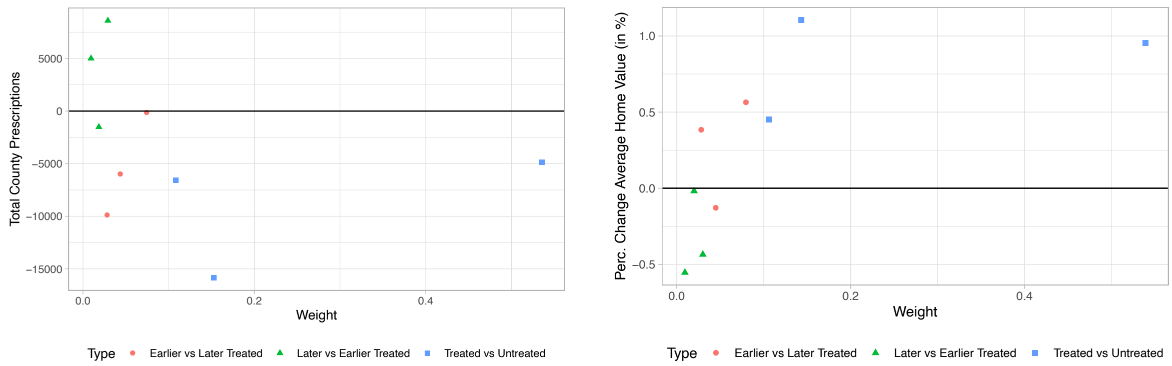


Notes: We plot the interaction weighted total coefficient for each relative time period following Sun & Abraham (2020). We first estimate $CATT(e, \ell)$, the coefficients $\delta_{e,l}$ with the following regression Dep. variable$_{c,t} = \alpha + \sum_{e \in \{16,17,18\}} \sum_{l=-5,\neq -1}^{2} \delta_{e,l} \mathbf{1}\{E_i = e\} D_{ct}^{\ell} + \gamma Controls_{c,t-1} + \theta_c + \tau_t + \epsilon_{c,t}$ and then average by sample share within each relative time period. The dependent variable in Panel A is total county prescriptions, in Panel B the log percentage change in average home values and in Panel C the log percentage change in percentage of mortgages 90 days plus past due.

Figure 5: Goodman-Bacon Decomposition without covariates

(A) Total County Prescriptions

(B) Perc. Change in Home Values



Notes: Each panel presents a Goodman-Bacon decomposition for the TWFE regression Dep. variable$_{ct}$ $= \alpha + \beta Post_{ct} + \gamma Controls_{ct-1} + \theta_c + \tau_t + \epsilon_{ct}$. The dependent variable is $Prescriptions_{ct}$, respectively $PCHomeValue_{ct}$ and the $Post_{ct}$ dummy is based on the introduction of laws / regulations at the state level. The decomposition splits the total beta (β) into the individual 2x2 DiDs coefficients. Earlier vs Later Treated includes: Cohort 2016 compared to 2017 respectively 2018; 2017 vs 2018. Later vs Earlier Treated includes 2017 vs 2016; 2018 vs 2017; 2018 vs 2016. Treated vs Untreated includes: Cohort 2016, 2017 respectively 2018 compared to Untreated.

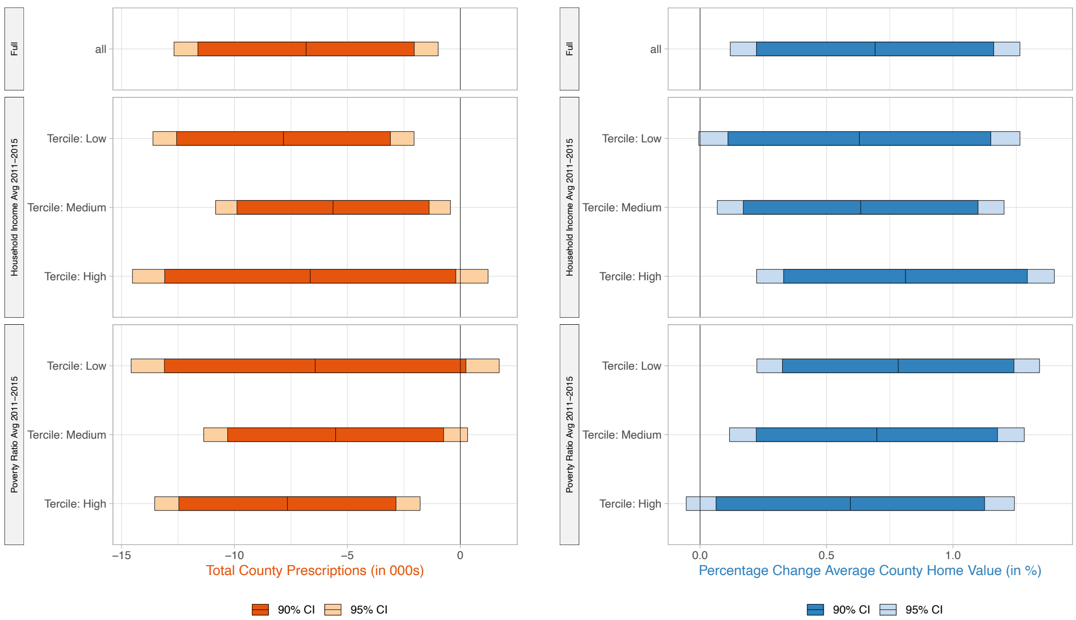
Table 3: DiD Interaction with opioid supply propensity

	Absolute Prescriptions		Percentage Change Home Prices	
Post	-4809.176^{**}	-1418.743	0.728^{**}	0.670^{**}
	(2059.140)	(2034.312)	(0.319)	(0.314)
Post X Physicians per capita		-12900.808^{***}		0.219^{*}
Highest Quartile		(1892.163)		(0.130)
Controls	Yes	Yes	Yes	Yes
County F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
Cluster	State	State	State	State
R2	0.989	0.990	0.590	0.590
N	15199	15199	14695	14695

The dependent variable is total county prescriptions, respectively a log percentage change of average county home values over 1 year. We calculate the average physicians per capita in the five years before the first state law was passed, i.e. from 2011 to 2015. County controls included are 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 and neoplasm mortality. Controls are lagged over one year. Standard errors are clustered at the state level. $***$ indicates $p < 0.01$, $**$ indicates $p < 0.05$, and $*$ indicates $p < 0.1$.

Figure 6: Staggered Difference-in-Difference of State Opioid Laws for subsets

(A) Total County Prescriptions ### (B) Perc. Change in Home Values



The dependent variable is total county prescriptions, respectively a log percentage change of average county home values over 1 year. The plots report the coefficient for the $Post_{ct}$ from the following TWFE regression: Dep. variable$_{ct} = \alpha + \beta Post_{ct} + \gamma Controls_{ct-1} + \theta_c + \tau_t + \epsilon_{ct}$. Terciles splits are based on county averages from 2011 to 2015 for prescription rates, household income and poverty ratios.

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Appendix

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.

TABLE A.I: DETERMINANTS OF OPIOIDS STATE LEGISLATION

	State Law and Regulation Indicator			
	1	2	3	4
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 table explores how local economic, health and political characteristics are related to state opioid-related legislation. All 50 US states are included. The dependent variable is an indicator variable equal to one if a state passed a opioid law or regulation between 2016 and 2018. Independent variables include: Average prescription rate, the 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: ESTIMATES FOR THE EFFECT OF OPIOID LAWS ON PRESCRIPTION, HOME VALUES AND MORTGAGES PAST DUE FOLLOWING SUN & ABRAHAM (2020)

	Panel A: Dependent Variable: Absolute Prescription				
Year Relative To	Fixed Effect	Interaction Weighted			
Legislation	Total	Total	CATT C2016	CATT C2017	CATT C2018
-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 Home Value				
Year Relative To	Fixed Effect	Interaction Weighted			
Legislation	Total	Total	CATT C2016	CATT C2017	CATT C2018
-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)		

	Panel C: Dependent Variable: Percentage Change in Mortgages 90 plus days past due				
Year Relative To	Fixed Effect	Interaction Weighted			
Legislation	Total	Total	CATT C2016	CATT C2017	CATT C2018
-5	2.884	7.060			7.060
	(4.194)	(4.726)			(4.726)
-4	0.337	2.630		3.037	1.339
	(3.278)	(2.872)		(3.531)	(4.258)
-3	0.993	1.336	2.795	0.468	2.985
	(2.236)	(2.025)	(4.208)	(2.900)	(2.205)
-2	0.954	1.447	-1.257	1.124	4.521
	(1.356)	(1.566)	(2.923)	(2.093)	(3.254)
-1	0.000	0.000	0.000	0.000	0.000
0	-1.562	-0.309	-7.839**	-0.421	5.747
	(2.423)	(2.398)	(3.410)	(2.915)	(6.899)
1	-7.588***	-6.169**	-10.084***	-5.235*	
	(2.253)	(2.519)	(3.305)	(3.019)	
2	-13.696***	-14.161***	-14.161***		
	(2.954)	(2.673)	(2.673)		

We follow Sun & Abraham (2020) to estimate both the two-way fixed effects (FE) regression as well as their suggested alternative more robust interaction weighted (IW) regression. In contrast to them, we also include controls. The specification for the FE is: $DepVar_{ct} = \alpha + \beta_{-3}D_{ct}^{-3} + \beta_{-2}D_{ct}^{-2} + \beta_0 D_{ct}^{0} + \beta_{+1}D_{ct}^{+1} + \beta_{+2}D_{ct}^{+2} + \gamma Controls_{ct-1} + \theta_c + \tau_t + \epsilon_{ct}$ and for the IW is: $PCHomeValue_{ct} = \alpha + \sum_{e\in 2016,2017,2018} \sum_{l=-5,\neq -1}^{2} \delta_{e,l} LegYear_i^e \cdot D_{ct}^l + \gamma Controls_{ct-1} + \theta_c + \tau_t + \epsilon_{ct}$ where D_{ct}^l are dummies that equal 1 for unit i being l periods away from initial treatment at calendar year t and $LegYear_i^e$ are cohort dummies for the calendar year of the legislation passage (e), i.e. for $LegYear_i^{2016}$ equal 1 for unit i at all time if it passed a opioid legislation in 2016. We always drop the dummy D_{ct}^{-1} to avoid multicollinearity. Both regressions include the same county controls as in previous specifications. The time horizon was limited to 2013 to 2018, covering a maximum relative time from -5 to +2. Standard errors are clustered at the state level. $***$ indicates p < 0.01, $**$ indicates p < 0.05, and $*$ indicates p < 0.1.