CONTEMPORARY ECONOMIC POLICY

THE EFFECT OF MARIJUANA DISPENSARY OPENINGS ON HOUSING PRICES

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We evaluate the effect of medical and recreational dispensary openings on housing prices in Denver, Colorado. Using an event study approach, we find that the introduction of a new dispensary within a half-mile radius of a new home increases home prices by approximately 7.7% on average. The effect diminishes for homes further from new dispensaries but is consistent over time. Our results provide important and timely empirical evidence on the socioeconomic impacts of marijuana legalization. (*JEL* R32, R38, R5)

I. INTRODUCTION

Over the last several years, an increasing number of states have voted to legalize marijuana use and dispensing. The trend toward legalizing marijuana has been driven largely by a shift in public opinion in favor of legalization. In an April 2018 poll by Quinnipiac University, 63% of respondents supported legalizing marijuana in the United States, the highest level ever measured by Quinnipiac (Quinnipiac University 2018). Likewise, according to a 2018 poll by the Pew Research Center, public support of marijuana legalization has nearly doubled since 2000 (Geiger 2018).

Supporters of legalization argue that the legal marijuana industry has become a major and quickly growing source of revenue for states that have passed legalization. In Colorado, for example, revenue from marijuana taxes, licenses, and fees totaled over $725 million between 2014 and April 2018, and the state has experienced steady revenue increases every year since legalization (Colorado Department of Revenue 2018). Supporters also argue young black men face a disproportionate number of marijuana-related arrests (*New York Times* Editorial Board 2014).

On the other hand, critics argue that legalization has led to increases in traffic deaths related to marijuana, corruption among marijuana regulators, and increasing black market and cartel activity (Droege 2018; Hunt 2017). Others have concerns over the potential health consequences of youth drug use and homelessness (Droege 2018; National Institute on Drug Abuse 2018). Perhaps most prominently, Jeff Sessions, the current United States Attorney General, has expressed concerns over violence and "big money" around marijuana (Santos 2017).

Despite this lively debate, very little empirical evidence exists on the impacts of legalization. This paper adds to this sparse literature in several important ways. First, ours is the first study to identify the effect of new dispensary openings on housing prices in Denver, Colorado. Previous papers have identified price effects from dispensaries converting from medical to recreational (Conklin, Diop, and Li 2017) or from municipality-level marijuana legalization (Cheng, Mayer, and Mayer 2018). However, it is possible that new dispensary openings have a different impact on housing prices than dispensary conversions or legalization. Second, our identification strategy, unique in the marijuana hedonics literature, allows us to identify the effect of dispensary openings by comparing sales prices before and after the introduction of a new dispensary in treated areas relative to control areas, controlling for the presence of

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ABBREVIATIONS

Colorado: CO
DD: Difference in Differences
QCEW: Quarterly Census of Employment and Wages
RMLs: Retail Marijuana Laws

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existing dispensaries. Specifically, our treatment group includes houses in the immediate vicinity of a dispensary (radii of 0.25, 0.25–0.5, and 0.5–0.75 miles around a dispensary), while our control group includes houses in an area in which a dispensary will open in the subsequent 6–12 months. Though dispensaries may open in different regions of the city with varying demographics, new dispensary locations are highly limited by zoning restrictions. Thus, our control group construction, combined with neighborhood-by-year fixed effects, plausibly addresses dispensary location selection bias. Our analysis, combined with existing estimates in the literature, provides a comprehensive analysis of the impact of marijuana dispensaries on housing prices.

We find that the introduction of a dispensary increases home transaction prices by 7.7% for houses within 0.25 miles of a new dispensary relative to control houses on average, but this impact diminishes to around 4.7% for houses within 0.25–0.5 miles of a new dispensary. The effect disappears entirely for houses further out between 0.5 and 0.75 miles. Importantly, we also find that the first dispensary to an area has a slightly larger effect on housing prices than the subsequent dispensaries, meaning the primary treatment effect should be viewed as conditional on the presence of preexisting dispensaries.

Our study has very timely policy implications. At least 12 states are considering marijuana legalization in 2018 (Sanders 2018). As these states move through the legislative process, it is vital that they have an understanding of the benefits and costs associated with legalization and the opening of dispensaries. Our results suggest that despite potential costs, legalization is capitalized as a net benefit in housing prices.

The remainder of this paper is organized as follows. Section II outlines the related literature and our contribution. Section III provides background on marijuana legalization in our study area, Denver, CO. Section IV outlines our data sources and cleaning process. Section V develops our identification strategy and model. Section VI summarizes key findings and provides several robustness checks for our results. Finally, Section VII offers concluding remarks.

II. LITERATURE REVIEW

Our paper contributes to a growing literature on the socioeconomic effects of legalized marijuana broadly, and a relatively small literature on the effects of dispensaries on housing prices. First, several studies have evaluated the local impacts of dispensary openings (and closures) on local crime rates. Chang and Jacobson (2017) for example, find that localized property crime rates are likely to decrease in response to medical marijuana dispensary openings. See Freisthler et al. (2016, 2017) and Kepple and Freisthler (2012) for similar applications.

The impact of dispensary openings on residential housing values has received far less attention in the literature. Rosen (1974) introduced the hedonic pricing method to recover preferences for particular characteristics of goods. Since then, countless empirical studies have used this methodology to value a house's structural, locational, and environmental amenities. Most relevant for our purposes, however, are studies that examine the price impact of a unique event, such as a store or stadium opening, a construction announcement, or some form of arguably exogenous change. In such situations, a difference-in-difference estimator is often used, meaning the change in housing prices between a control group and a treated group is compared before and after the unique event (Lang, Opaluch, and Sfinarolakis 2014). Tu (2005), for example, uses a hedonic model with a difference-in-difference estimator to identify the house price impact of FedEx Field, a sports stadium outside of Washington, DC. Tu (2005) finds that houses nearby the stadium sell for less than those further away; however, the construction of the stadium actually narrowed the housing price gap, especially in the homes closest to the stadiun. Similarly, Ahlfeldt and Kavetsos (2014) and Dehring, Depken, and Ward (2007) find localized positive treatment effects from new stadiums (the Dallas Cowboys football stadium in Dallas, and the New Wembley and Emirates Stadiums in London). In a more recent application, Humphreys and Nowak (2017) utilize a difference-in-difference hedonic model to identify the change in nearby property values after two former National Basketball Association teams—the Seattle SuperSonics and the Charlotte Hornets—left their respective cities. By exploiting team departures from stadiums that continued to operate for other purposes, the authors isolate a positive net effect on property prices from the departure of a team, as opposed to the aforementioned studies which do not differentiate between a stadium and team effect.

In another recent application, Brooks, Humphreys, and Nowak (2018) find no impact from strip clubs on nearby house prices in Seattle, WA using a difference-in-difference hedonic model. However, they do find weak evidence of negative price effects around two specific clubs and evidence of a positive price effect for properties within 1,000–2,000 ft of a club.

Similar to the present study, Cerrato Caceres and Geoghegan (2017) and Pope and Pope (2015) examine the impact of a unique store opening on nearby housing prices. Pope and Pope (2015) utilize a dataset of over 1 million housing sales to estimate the price impact of 159 Walmart openings between 2000 and 2006. They utilize a difference-in-difference hedonic approach, comparing the price of homes sold very close to a Walmart, before and after the store opens, to those further away from the store. They find that homes within 0.5 miles of a new Walmart increase in value by around 2.3% relative to homes 2–4 miles away. Cerrato Caceres and Geoghegan (2017) use a similar hedonic model to find that houses within 0–400 m of a new grocery store opening experience a 7% increase in value relative to those 800 or more meters away, and houses within 400–800 m experience a 4% increase in value relative to those 800 or more meters away.

Although the impact of other types of stores on housing prices has been well explored in the literature, to the best of our knowledge there is currently only one empirical study that applies the hedonic method to evaluate the impact of individual marijuana dispensaries on housing prices. Conklin, Diop, and Li (2017) identify the price impact of retail conversions (from a medical-only dispensary to a recreational dispensary), rather than new dispensary openings, in Denver, CO. The authors utilize a difference-in-difference approach to find that homes within 0.1 miles of a conversion experience an 8.4% increase in value relative to houses further away. The effect appears to be highly localized, as there was no significant price effect for homes within 0.1–0.25 miles from a conversion.

One limitation of the Conklin, Diop, and Li (2017) analysis however, is that the exact date of conversion from medical to recreational dispensing is unknown. Instead, the authors rely on the date recreational dispensing became legal. This assumption introduces measurement error and could attenuate their results. We provide an important robustness check and corroborate their findings, absent this particular source of measurement error.

Cheng, Mayer, and Mayer (2018) offer another application of the hedonic methodology to the legal marijuana industry. However, while the analysis of Conklin, Diop, and Li (2017) estimated a localized treatment effect, Cheng, Mayer, and Mayer (2018) examine the effect of adoption of municipality-level retail marijuana laws (RMLs) on housing prices. They use a difference-in-difference approach to compare housing prices before and after the adoption of RMLs in municipalities that adopted RMLs relative to those that did not. They find that housing prices increased by around 6% in response to RML adoption.

Our study provides several additional contributions to this relatively small literature. First, while Conklin, Diop, and Li (2017) showed that conversions affect housing prices, their analysis did not identify the mechanisms behind their result. Recreational marijuana legalization expanded the potential market for marijuana by more than 3,400%.[1] Given the use of January 2014 as the treatment date, Conklin, Diop, and Li (2017) may simply be picking up the effect of the expanded market. For instance, the relevant dispensaries in their study likely experienced dramatic increases in sales, which has the potential to change the dynamics of a neighborhood. In our study, we can identify the effect of a unique dispensary opening during the prerecreational period and we find that the effect remains robust, indicating that the findings of Conklin, Diop, and Li (2017) are not simply due to the passing of the recreational law.

Second, one might also wonder if the effect of a conversion is limited to locations where a dispensary already existed, or if the introduction of a medical dispensary and a subsequent conversion have similar effects. For example, it might be the case that a new medical dispensary decreases housing prices by some amount but a subsequent conversion to recreational dispensing increases nearby housing prices by roughly 8% (Conklin, Diop, and Li 2017). If this is true, then the estimates of the conversion effect should be considered net of any opening effects. Instead, our results imply the effect is present for new dispensary openings.

1. There were 111,030 patients with medical marijuana cards in January 2012, the date recreational marijuana was legalized. The Colorado population at the time was 5.186 million with 22.5% below the age of 18. If 25% was below the age of 21, then recreational legalization gave access to approximately 3.88 million people, an increase of approximately 3,400%.

Third, the effects of marijuana dispensaries on housing prices previously estimated in the literature are rather large in comparison to similar hedonic papers. Our estimates serve as an additional point estimate and together, our work provides compelling evidence that marijuana dispensaries have relatively large effects on neighboring housing prices.

Lastly, we use an alternative identification strategy to those used in the previous two papers. While our identification strategy is not necessarily better than previous approaches, the estimates provide a robustness check for the previous results and together, our paper combined with the conversion results from Conklin, Diop, and Li (2017) provide an extensive analysis of the effects of marijuana dispensing on housing prices. Interestingly, despite the differences between our approach and the approaches of Conklin, Diop, and Li (2017) and Cheng, Mayer, and Mayer (2018), all of our results are very close in magnitude. This suggests that dispensary conversions, dispensary openings, and legalization status have similar effects on housing prices.

III. BACKGROUND ON MARIJUANA LEGALIZATION IN COLORADO

The federal Controlled Substances Act classifies marijuana as a Schedule I controlled substance, and the use, sale, or distribution of marijuana has been federally illegal since the 1930s. The punishment for possession without intent to distribute is a minimum fine of $1,000 and up to 1 year in prison, and the punishment for taking part in a marijuana-related business can bring fines of up to $250,000 and up to 5 years in prison (FindLaw n.d.). Despite this, in recent years there has been a wave of state-level medical and recreational marijuana legalization. As of May 2018, 9 states have legalized recreational marijuana and 29 states have legalized medical marijuana (Robinson, Berke, and Gould 2018).

Amendment 20 legalized medical marijuana in Colorado in November 2000. This amendment allowed patients who had a documented "debilitating medical condition" to possess no more than 2 ounces of marijuana and no more than six marijuana plants but it did not establish a distribution system. Until the limit was removed in June 2009, caregivers were only permitted to provide five or fewer patients with medical marijuana. In June 2010, HB 10-1284 legalized medical marijuana centers, optional premises cultivation centers, and medical marijuana-infused product producers. Licensing for these centers began August 1 of the same year.

On November 6, 2012, Colorado citizens voted to legalize recreational marijuana and recreational dispensary sales began January 1, 2014. Figure 1 shows the number of retail and medical dispensaries over time in Denver. In instances where the same location was granted both a medical and recreational license, we assign the dispensary its first license date.

Figure 2 provides a map of the neighborhoods in Denver and the distribution of dispensaries in our dataset. Interstate 25 and Interstate 70 are shown for reference. The neighborhoods were developed by the Denver Assessor and were designed to group relatively homogeneous households.[2] As can be seen from the map, dispensaries tend to cluster in certain areas due to zoning restrictions. Thus, it is important to control for the presence of nearby existing dispensaries and neighborhood changes that might affect housing prices. For reference, there are 177 neighborhoods in our sample.[3]

Finally, Figure 3 shows a representative dispensary opening (pentagon) and the location of housing transactions surrounding the dispensary within a year of the dispensary opening date. A quarter-mile buffer and half-mile buffer are also shown for reference. The map shows that there are quite a few housing transactions surrounding new dispensaries during the sample period.

IV. DATA

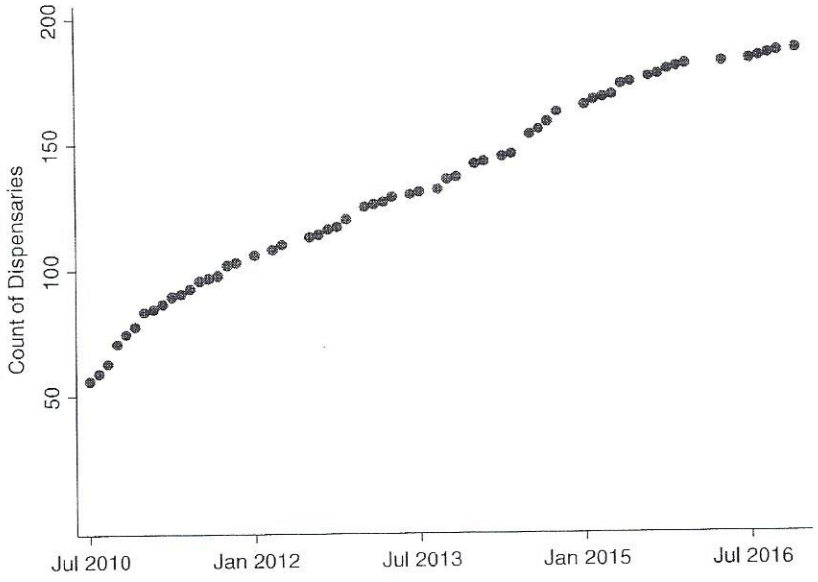
The data for our analysis of Denver dispensaries and house prices come from two primary sources. First, we gather housing information from three separate datasets obtained from Denver's Open Data Catalog. The first dataset is a shapefile, which contains a unique identifier (pin) and coordinates for every parcel in Denver. We use this file primarily to obtain the coordinates of each property.

The second is a property characteristics file. This file has data on land and building square footage, stories, number of bedrooms and bathrooms, year of construction, neighborhood, and the unique pin identifier for every residential

2. These are the same neighborhoods used by Conklin, Diop, and Li (2017).

3. The average neighborhood covers 0.235 mile^2 with the smallest being 0.035 mile^2 and the largest being 1.03 mile^2. While there is clearly a distribution, most of the neighborhoods are relatively similar sized.

FIGURE 1
Number of Marijuana Dispensaries over Time



building in Denver. In addition, we calculated the distance from each house to the nearest school. To limit the analysis to single family houses, we keep only the properties from this dataset classified as condominium, rowhouse, single family residential, or vacant land.

The third is a property sales dataset, which includes sale date, sale price, and the unique pin identifier for all property sales between January 1, 2008 and May 24, 2017 (the date of download). After merging these three datasets together by the unique pin, we dropped duplicate observations and observations with no information on sale price.

Our second primary data source includes data on marijuana facilities in Denver provided by the city and county of Denver. The dataset includes renewal application dates and coordinates for medical and recreational cultivation, sales, infused product manufacturing, and testing facilities.[4] We removed cultivation, manufacturing, and testing facilities, limiting the data to 209 medical and recreational dispensaries. We only consider unique dispensary location openings—and not conversions from medical to recreational—in our analysis. Unfortunately, the data do not include opening date information. To obtain opening dates, we called each dispensary, and 75 provided an opening month and year. For the remainder, we utilized data from the Quarterly Census of Employment and Wages (QCEW) that provides the initial liability date of every business in Colorado. Through both of these methods, we obtained opening dates for 188 dispensaries, and the remainder were excluded from analysis. We then calculated the distance between every home and every dispensary (in feet). We drop housing transactions that occur within a specified radius of any of the dispensaries for which we do not have opening dates. For instance, in our primary specification, we drop transactions that occurred within 0.25 miles of any of the dispensaries with unknown opening dates.

For our primary analysis, we created three separate datasets of homes that sold within a half year of a dispensary opening date: one including only homes sold within 0.25 miles of a dispensary, one including only homes sold within 0.25–0.5 miles of a dispensary, and one including only homes sold within 0.5–0.75 miles of a dispensary. Summary statistics for select housing variables in the 0.25-mile radius dataset are presented in Table 1. Summary statistics on these

4. To be clear, we observe the latest (not first) application date for each dispensary. Dispensaries have to reapply each year.

FIGURE 2
Map of Marijuana Dispensaries and Neighborhoods

variables for the 0.25–0.5 and 0.5–0.75-mile radii are presented in Tables S1 and S2 (Supporting information). Saleprice is the sale price of the home in U.S. dollars; num^{25}, num^{5}, and num^{75} are the number of existing dispensaries within 0.25, 0.25–0.5, and 0.5–0.75 miles of the home not including the opening dispensary; DistanceSchool is the distance to the nearest K-12 school in feet; stories is the number of stories; age is the age in years; sqft bldg and land sqft are the square footage of the building and the lot size; bedrooms and bathrooms are the number of bedrooms and bathrooms; area above grade is the square footage of the portion of the building that is above-grade; and renovated is a dummy variable indicating whether the property has been renovated. A small number of homes in the raw sales data have sales prices of $1, $10, or $100. We drop these values as they clearly do not represent true sales prices. We also dropped sales prices below the first percentile and above the 99th percentile of sales prices ($30,000 and $3.45 million respectively).

V. MODEL AND IDENTIFICATION

We utilize an event study approach to estimate the effect of medical and recreational dispensary openings on nearby housing prices. The approach is similar to a difference-in-difference approach, but an event study assigns each treated location a distinct pre- and posttreatment period. Although we observe individual housing transactions, we only observe dispensary opening months and years. As such, the temporal level of analysis is the month-year. Our primary model is as follows:

$$(1)\quad \ln p_{it}^{r} = \alpha + \beta_1 \text{after}_{it}^{r} + \beta_2 T_{it}^{r} + \beta_3 DD_{it}^{r} + \beta_4 \text{num}_{it}^{r} + \mathbf{X}_i \boldsymbol{\beta}_5 + \gamma_{ny} + \delta_m + \varepsilon_{it},$$

FIGURE 3
Map of Representative Dispensary Opening and Housing Transactions

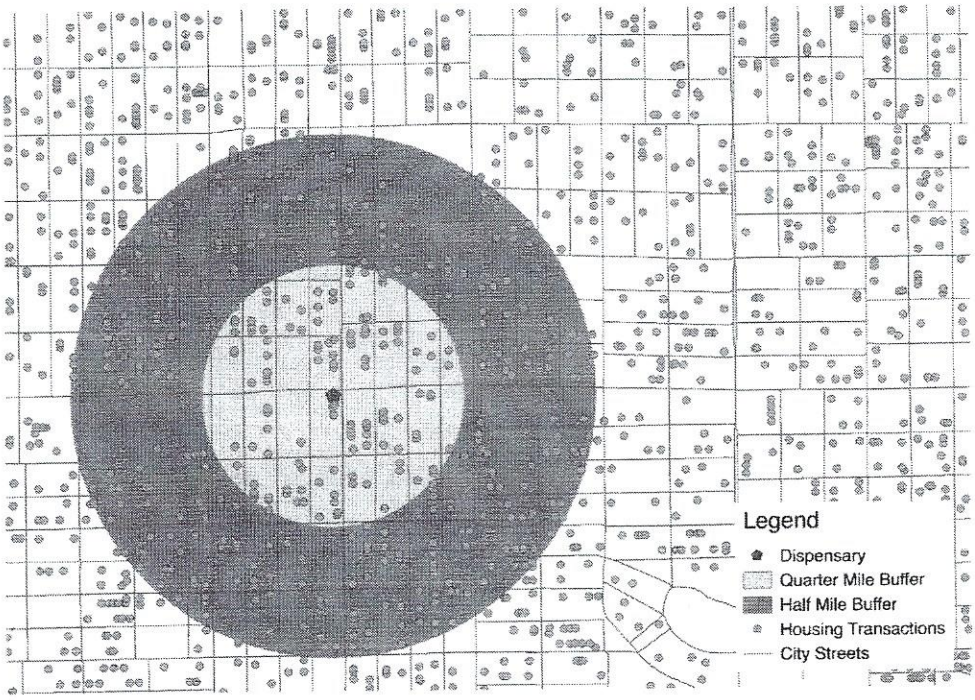


TABLE 1
Summary Statistics for Denver Houses, 0.25-Mile Radius

Variable	Obs	Mean	Standard Deviation	Min	Max
saleprice	5,037	314,835.2	230,047.4	30,500	2,460,000
num^{25}	5,037	0.589	1.254	0	11
DistanceSchool (ft)	5,037	435.004	213.518	12.996	1,421.889
stories	5,037	1.356	0.599	1	4
age	5,037	61.833	40.684	0	138
sqft bldg	5,037	1,655.24	918.202	351	8,538
bedrooms	5,037	2.264	0.887	1	11
area above grade	5,037	1,292.837	632.869	226	7,143
bathrooms	5,037	1.901	0.912	0.5	10
land sqft	5,037	3,818.058	2,982.085	50	39,900
renovated	5,037	0.621	0.485	0	1

Notes: Data include homes sold within 1 year of a dispensary opening date. Though 11 nearby dispensaries may seem high, we double checked this on Google maps and the number is valid. To be sure, our primary results are robust to dropping observations with abnormally small values for certain housing characteristics including sqft bldg, area above grade, land sqft, and bathrooms.

where $\ln p_{it}^{r}$ is the log of the sales price of house i in month and year t within radius r of a given new dispensary, after_{it}^{r} is a dummy for whether house i was sold 6 months before or 6 months after the opening date of the new dispensary (the month of opening is assigned after = 1), T_{it}^{r} is a dummy for treatment versus control locations, num_{it}^{r} is a count of the existing dispensaries within a radius r of housing transaction i in time period t, $\boldsymbol{X}_i$ is a vector of physical housing characteristics, γ_{ny} are neighborhood-by-year fixed effects where neighborhoods are defined above, and δ_m are month fixed effects. All standard errors are clustered at the dispensary level.

We define treatment status as follows. Consider a representative dispensary, dispensary $d1$,

which opened in July 2014. Housing sale i is considered treated if house i is within a half mile of dispensary $d1$ and house i was sold between January 2014 and up to January 1, 2015.[5] The variable $after^r_{it}$ is equal to one if house i was sold after dispensary $d1$ opened (including July) and zero otherwise. Control locations are locations in which a different dispensary, call it dispensary $d2$, opened 6 months to 1 year after the opening of dispensary $d1$. For example, considering the opening date of dispensary $d1$, dispensary $d2$ would be considered a control location if $d2$ opened between January 2015 and July 2015. Thus, housing transactions surrounding dispensary $d2$ between January 2014 and January 2015 are defined as control housing transactions. However, the variable $after^r_{it}$ for control location $d2$ is defined based on the opening date of dispensary $d1$.[6]

Defining control locations in this manner has two primary advantages over previous studies that define treated and control locations based on doughnuts around treated locations (Pope and Pope 2015). First, our method controls for geographic selection bias in the sense that dispensaries might choose to locate in areas with higher or lower housing prices. Second, our method does not rely on an arbitrarily defined boundary between treated and control locations. Third, since dispensaries tend to cluster in particular locations, typically because of zoning restrictions, we can explicitly control for the presence of existing dispensaries surrounding housing sales. Our identification strategy relies on the assumption that small differences in dispensary opening dates, on the order of 6 months, are due to random permitting procedures and not systematic selection based on preferred locations.[7]

The variable of interest is therefore $DD^r_{it} = after^r_{it} * T^r_{it}$. The coefficient β_3 is interpreted as the effect of a new marijuana dispensary in a treated location relative to a control location (a location in which a dispensary will soon open), after the new dispensary opened relative to before the new dispensary opened. The results should be interpreted as conditional on the number of existing dispensaries within the specified radius. We also include neighborhood-by-year fixed effects (γ_{ny}) to control for any time-varying neighborhood specific unobservables, such as changes in neighborhood composition. The neighborhood-by-year fixed effects are important in cities like Denver with rapidly increasing populations and housing prices. Finally, we also include month fixed effects (δ_m) to control for seasonal variation in housing prices.

We should note, however, that our identification strategy is not without limitations. The traditional doughnut difference-in-difference strategy relies on the assumption that properties located close to the treatment area should be very similar to properties slightly further away. A potential concern in our setting is that control locations might be in completely different areas of the city. However, we assume that the neighborhood-by-year fixed effects (assigned at the unit of observation, i.e., the housing transaction) sufficiently control for any location specific differences that might bias the results.

VI. RESULTS AND DISCUSSION

A. *Main Specification Results*

In this section, we estimate three specifications of Equation (1) to find the impact of dispensary openings on nearby housing prices in Denver.

Table 2 displays the results of estimating Equation (1) with radii of 0.25, 0.25–0.5, and 0.5–0.75 miles around a dispensary.[8] Our primary variable of interest, difference in differences (DD), is positive and statistically significant for the first two radii. We observe a treatment effect of a 7.7% increase in home prices within a 0.25-mile radius relative to control houses,

5. To be clear, houses sold in January 2015 are not considered treated in this example.

6. There are an average of 1.05 control locations for every treated location.

7. The publicly available Marijuana Licenses data that provide the application dates, addresses, and coordinates of all marijuana dispensaries provide an application date for each dispensary, but the application date is the date of the latest (not first) application. The dispensaries reapply for licenses each year (not necessarily every 12 months). Unfortunately, we do not know how many applications a particular dispensary filed or when the first application was filed. This makes it difficult to know how long the application process typically takes because some, in fact many of the application dates reported occurred after the opening dates. However, we can have a sense of the distribution of application processing times by comparing dispensary opening dates to initial application dates for dispensary openings that occurred within 12 months of the initial application date at the beginning of legalization. For instance, three dispensaries opened August 2010, three opened September 2010, six opened October 2010, four opened November 2010, two opened December 2010, and four opened January 2011, all with initial application dates of February 2010. This suggests that the processing time for applications is between 6 and 11 months.

8. The unit of observation is a housing transaction so the number of observations reported is the number of housing transactions in the sample.

TABLE 2
Main Specification Results

	(1) 0.25 miles	(2) 0.25–0.5 miles	(3) 0.5–0.75 miles
T	−0.041	−0.034	0.029
	(0.034)	(0.025)	(0.021)
after	−0.068**	−0.043	0.013
	(0.034)	(0.027)	(0.022)
DD	0.077**	0.047*	−0.012
	(0.037)	(0.028)	(0.023)
num^{25}	−0.011		
	(0.009)		
num^{5}		0.001	
		(0.003)	
num^{75}			0.002
			(0.003)
controls	Y	Y	Y
nbhd-year FE	Y	Y	Y
month FE	Y	Y	Y
R^2	0.733	0.753	0.733
N	5,037	12,440	18,770

Notes: Clustered robust standard errors in parentheses. Standard errors clustered at the dispensary level. All additional controls are included in the regressions but coefficients are not shown for brevity. nbhd-year is a neighborhood-by-year fixed effect where neighborhoods are defined in Section IV. FE, fixed effects.

*Significant at 10%.

**Significant at 5%.

an effect of 4.7% at a radius of 0.25–0.5 miles, and a null effect at a radius of 0.5–0.75 miles. Overall, our results suggest that the effect of a new dispensary on home prices diminishes with the distance from the new dispensary. The magnitude of our findings falls between the two previous studies in the literature (Conklin, Diop, and Li 2017; Cheng, Mayer, and Mayer 2018).

Importantly, the coefficient on T_{it}^{r}, our treatment dummy, shows there is no significant difference in housing prices between treated and control locations throughout the sample period on average. We formally test for parallel trends in the following section. In our main specification, we also find that the number of existing nearby dispensaries (num^{25}) does not have a statistically significant effect on housing prices. However, when using census tract-by-year fixed effects in place of neighborhood-by-year fixed effects, or if neighborhood fixed effects are omitted, the coefficients on num^{25} and num^{75} are negative and statistically significant. This indicates that the neighborhood-by-year fixed effects are absorbing the variation in num^{25} in our primary specification and that dispensaries tend to cluster in locations with lower housing prices.

There are many reasons that could explain a positive treatment effect of new dispensaries on housing prices. The effects likely depend on where the dispensary is located and what the dispensary displaced. For instance, if a new dispensary opens in a location that was previously a vacant lot or a failed business, the dispensary will bring new foot traffic and business to the area. The increase in foot traffic is an increase in "eyes on the street," which previous work suggests reduces crime in the immediate area (Chang and Jacobson 2017; Freisthler et al. 2016, 2017. Moreover, dispensaries are known for having high levels of security such as security guards and cameras. Reduced crime is certainly attractive to a homebuyer, and this could explain some of the observed premium of living near a dispensary.

Another possible explanation is that zoning restrictions force dispensaries to cluster, which leads to high levels of local competition. In these circumstances, dispensaries likely prioritize welcoming and safe-looking storefronts, which might attract tourists and wealthy clientele to the neighborhood.

What is most surprising is that dispensaries appear to affect housing prices similarly to other less controversial businesses, such as grocery stores. For example, Pope and Pope (2015) find that the introduction of a Walmart increases nearby housing prices by 2.3% and Cerrato Caceres and Geoghegan (2017) find that new grocery stores increase nearby housing prices by 7%. The mechanisms through which grocery stores affect housing prices are more obvious than dispensaries. For instance, all households need groceries and nearby grocery stores provide convenience. If public sentiment surrounding marijuana is positive, homebuyers may also prefer to select into neighborhoods with more dispensaries for convenience. Ultimately however, our data do not allow us to directly determine the underlying mechanisms driving this result, so these potential explanations should be considered speculative.

Our estimates, along with previous estimates in the literature, indicate that living near a new dispensary is associated with net benefits that are capitalized in housing prices. Although we observe a net benefit to housing prices from living near a new dispensary opening, we are likely capturing certain costs as well. For example, there is some evidence that higher dispensary density is associated with youth trying alternative forms of marijuana consumption (e.g., edibles or vaping) at a slightly younger age (Borodovsky et al. 2017). There is also evidence that increased dispensary density is associated with higher levels of marijuana abuse– or dependence–related

hospitalizations (Mair et al. 2015). However, our result suggests the benefits of living near a dispensary outweigh these potential costs.

B. Test of Parallel Trends

A primary concern with event studies, or difference-in-difference studies, is that there are similar trends in the dependent variable prior to treatment, that is, the parallel trends assumption. To test the parallel trends assumption, we decompose the $after_{it}^{r}$ variable in our primary model into a series of dummy variables, ϕ_{ik}, where k indicates month pairs pre- and posttreatment. We then interact the treatment dummy with ϕ_{ik} to gain a sense of the treatment effect prior to treatment and over time. We use a 12-month window on either side of the opening date of a new dispensary. The omitted category is 1–2 months prior to treatment. The model is the following:

$$\ln p_{it}^{r} = \alpha + \sum_{k} \beta_1^k \phi_{ik} + \beta_2 T_{it}^{r} + \sum_{k} \beta_3^k \left(\phi_{ik} * T_{it}^{r}\right) + \beta_4 \text{num}_{it}^{r} + \boldsymbol{X_i}\boldsymbol{\beta_5} + \gamma_{my} + \delta_m + \varepsilon_{it}.$$

Table 3 displays the results. None of the pretreatment interaction terms are statistically significant while all of the posttreatment interaction terms are statistically significant. We also implemented the test with indicators for each month pre- and posttreatment (rather than 2-month pairs) and results were similar. We report the 2-month pairs to save space. These results suggest that the parallel trends assumption is valid in our context.

TABLE 3
Test of Parallel Trends

	(1) 0.25 miles
T	−0.109**
	(0.054)
T*1(11–12 months before opening)	0.059
	(0.042)
T*1(9–10 months before opening)	0.057
	(0.037)
T*1(7–8 months before opening)	0.001
	(0.031)
T*1(5–6 months before opening)	0.046
	(0.037)
T*1(3–4 months before opening)	0.014
	(0.029)
T*1(0–1 months after opening)	0.078**
	(0.037)
T*1(2–3 months after opening)	0.121**
	(0.055)
T*1(4–5 months after opening)	0.130***
	(0.049)
T*1(6–7 months after opening)	0.095**
	(0.045)
T*1(8–9 months after opening)	0.100*
	(0.051)
T*1(10–11 months after opening)	0.123**
	(0.052)
controls	Y
nbhd-year FE	Y
month FE	Y
month of event study FE	Y
R^2	0.719
N	8,137

Notes: Clustered robust standard errors in parentheses. Standard errors clustered at the dispensary level. All additional controls are included in the regressions but coefficients are not shown for brevity. nbhd-year is a neighborhood-by-year fixed effect where neighborhoods are defined in Section IV. Month fixed effects are calendar month fixed effects to control for seasonal variation in housing demand and supply. Month of event study FE are dummies for month of event study, such as 1–2 month prior to a dispensary opening or 2–3 months after a dispensary opening. FE, fixed effects.

*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

C. Additional Specification Results

Dispensaries tend to cluster due to zoning restrictions. Thus, it is possible that the effect of a new dispensary to an area is attenuated by the presence of preexisting dispensaries. In this section we test whether the treatment effect changes with the number of existing dispensaries within 0.25 miles of the home sale by including an interaction between the number of existing dispensaries and our primary variable of interest for the 0.25-mile specification (and all other appropriate interactions). This new interaction variable is defined as num^{25}DD. Results from this specification are shown in column 1 of Table 4. The coefficient on num^{25}DD is statistically insignificant, which could indicate that the treatment effect does not change with the number of existing dispensaries or that our data lack sufficient power to identify the effect. To test which of the explanations is more likely true, we estimate our model but limit the sample to transactions that occur in areas with no preexisting dispensaries.

Column 2 of Table 4 displays the results of estimating our primary model limited to housing transactions in areas with no preexisting dispensaries. The coefficient on DD is positive and significant, suggesting that home sales within 0.25 miles of a dispensary opening with no preexisting nearby dispensaries sell for 10.4% higher than control houses. This is a slightly larger treatment effect than the specification with all homes included (7.7%); however, the estimates are not statistically different from one another.

TABLE 4
Additional Specification Results

	(1) # Dispensaries	(2) No Dispensaries
T	−0.041	−0.036
	(0.034)	(0.034)
after	−0.068*	−0.070**
	(0.034)	(0.036)
DD	0.071*	0.104***
	(0.037)	(0.04)
num^{25}	−0.015	
	(0.013)	
num^{25}DD	0.007	
	(0.011)	
controls	Y	Y
nbhd-year FE	Y	Y
month FE	Y	Y
R^2	0.733	0.765
N	5,037	3,687

Notes: Clustered robust standard errors in parentheses. Standard errors clustered at the dispensary level. All additional controls are included in the regressions but coefficients are not shown for brevity. Column 1 interacts the primary variable with the count of existing dispensaries. Column 2 limits the sample to housing transactions occurring in areas with zero existing dispensaries. nbhd-year is a neighborhood-by-year fixed effect. FE, fixed effects.

*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

D. Robustness Checks and Relation to Previous Literature

In this section, we perform several robustness checks. First, we allow the time period bandwidth around dispensary openings (6 months before and after a dispensary opening) to vary by re-estimating Equation (1) with a 12-month time window before and after a dispensary opening for all three radii.[9] Results are displayed in columns 1–3 of Table 5. The coefficient on DD is statistically significant at a radius of 0.25 miles and slightly larger than the 6-month window coefficient (8.6% and 7.7%, respectively). The results are consistent with the parallel trends test in Table 3. However, the coefficients on DD are not statistically significant for the 0.25–0.5- or 0.5–0.75-mile radii using the larger time window.

Second, we more directly compare our estimates to existing estimates in the literature by estimating Equation (1) using a new radius of 0.1 miles and the same time window of 6 months before and after a dispensary opening. Results from this estimation are provided in column 4 of Table 5. Although the point estimate is 6.5% (close to that of Conklin, Diop, and Li 2017), the coefficient on DD is not statistically significant using this alternative radii. Our results are slightly different from previous estimates in the literature. For instance, Conklin, Diop, and Li (2017) find an 8% treatment effect within a 0.1-mile radius but find no effect for a 0.1–0.25-mile radius whereas we find an approximate 8% treatment effect using a 0.25-mile radius and no treatment effect within a 0.1-mile radius. The difference in results is likely due to two factors. First, we use a different control group than Conklin, Diop, and Li (2017) leading to significantly fewer observations at the 0.1-mile radius (715 compared with 19,555). Second, Conklin, Diop, and Li (2017) estimate the effect of dispensary conversions where we estimate the effect of dispensary openings.

A potential concern in our setting is that the introduction of a dispensary affects the composition of home sales, which would lead to biased estimates of the effects on housing prices. For instance, if dispensaries are in fact a disamenity, we might expect wealthier households to move in response to dispensary openings, all else equal. We can test for such an effect by estimating our primary model but replacing the dependent variable with housing characteristics in place of sales prices. This analysis is equivalent to the analysis performed by Conklin, Diop, and Li (2017) and Pope and Pope (2015). The results are presented in Table 6. We find that dispensary openings do not have a statistically significant effect on the characteristics of the homes being sold including building square footage, land square footage, number of bedrooms, number of bathrooms, number of stories, or house age.

VII. CONCLUSION

In this study, we evaluate the impact of marijuana dispensary openings on nearby home values in Denver, CO. We provide several important contributions to the literature examining this relationship. First, we are the first study to identify the impact of new dispensary openings, rather than dispensary conversions or municipality-level legalization, on housing values. Second, we develop an identification strategy, unique in the marijuana hedonics literature that plausibly addresses dispensary location selection bias.

We find that a new dispensary opening increases house prices by 7.7% for houses within a 0.25-mile radius of a dispensary

9. Note we also change the control location opening dates to correspond to the longer time period.

TABLE 5
Robustness Specification Results

	(1) 0.25 miles	(2) 0.25–0.5 miles	(3) 0.5–0.75 miles	(4) 0.1 miles, 6 months
T	−0.092**	−0.018	−0.025	−0.001
	(0.037)	(0.02)	(0.022)	(0.089)
after	−0.118***	−0.043*	−0.053**	−0.108
	(0.043)	(0.023)	(0.027)	(0.091)
DD	0.086**	0.03	0.036	0.065
	(0.043)	(0.024)	(0.027)	(0.102)
num^1				−0.102**
				(0.051)
num^{25}	−0.005			
	(0.007)			
num^5		0.001		
		(0.004)		
num^{75}			−0.003	
			(0.003)	
controls	Y	Y	Y	Y
nbhd-year FE	Y	Y	Y	Y
month FE	Y	Y	Y	Y
R^2	0.721	0.746	0.735	0.753
N	8,137	18,372	26,497	715

Notes: Clustered robust standard errors in parentheses. Standard errors clustered at the dispensary level. All additional controls are included in the regressions but coefficients are not shown for brevity. nbhd-year is a neighborhood-by-year fixed effect. num^1 is the number of existing dispensaries within 0.1 miles of the home not including the opening dispensary. FE, fixed effects.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

TABLE 6
Housing Composition Regression Results

	(1) sqft bldg	(2) land sqft	(3) bedroom	(4) bathroom	(5) stories	(6) age
T	−12.942	−69.028	0.027	0.066	−0.073*	0.807
	(25.803)	(116.57)	(0.054)	(0.046)	(0.041)	(0.714)
after	−20.867	20.79	0.021	0.082*	−0.019	0.527
	(26.95)	(123.636)	(−0.054)	(0.049)	(0.041)	(0.722)
DD	33.751	−41.953	−0.031	−0.072	0.044	−0.156
	(29.394)	(130.884)	(0.06)	(0.051)	(0.043)	(0.773)
controls	Y	Y	Y	Y	Y	Y
nbhd-year FE	Y	Y	Y	Y	Y	Y
month FE	Y	Y	Y	Y	Y	Y
R^2	0.902	0.777	0.634	0.75	0.675	0.976
N	5,037	5,037	5,037	5,037	5,037	5,037

Notes: Clustered robust standard errors in parentheses. Standard errors clustered at the dispensary level. All additional controls are included in the regressions but coefficients are not shown for brevity. nbhd-year is a neighborhood-by-year fixed effect. num^1 is the number of existing dispensaries within 0.1 miles of the home not including the opening dispensary. FE, fixed effects.
*Significant at 10%.

relative to a control group. For houses within a 0.25–0.5-mile radius, the effect is slightly lower at around 5%. Further out between 0.5 and 0.75 miles, the effect dissipates entirely suggesting the treatment effect of a new dispensary is diminishing in the distance from the dispensary. The treatment effect is also larger, around 10%, for homes with no preexisting dispensaries nearby.

Our results, combined with previous estimates in the literature provide a comprehensive analysis of the effect of marijuana dispensing on housing prices. These results will inject empirical evidence into a lively debate about marijuana legalization. This is vitally important, as in 2018 at least 12 states plan to consider legalization. Our findings suggest that legalization and dispensary openings create localized net benefits.

One limitation of this paper is that legalization occurred relatively recently, limiting the number of dispensaries and nearby home sales. In future work, it would be worthwhile to re-examine the research questions asked in this paper after several more years of legal marijuana sales, particularly in other states. Second, our data do not allow us to differentiate between medical and recreational dispensary openings. Recreational legalization significantly expanded the market, and therefore, future research should evaluate whether medical and recreational laws have different effects on socioeconomic outcomes. Furthermore, there is more work needed to examine the variety of social impacts of legalization. Although changes in home prices are certainly important to understand, there are a number of other socioeconomic effects that have received little to no attention in the empirical literature. If policymakers wish to make truly informed decisions regarding marijuana legalization, it is vital that more of these relationships are explored and quantified.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Table S1. Summary Statistics for Denver Houses, 0.25–0.5-mile radius

Table S2. Summary Statistics for Denver Houses, 0.5–0.75-mile radius

Table S3. Summary Statistics for Denver Houses, 0.1-mile radius

Table S4. Main Specification Robustness with Census Tract Fixed Effects