Contact High: The External Effects of Retail Marijuana Establishments on House Prices

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Abstract

Using publicly available data from the city of Denver and the state of Colorado, this study examines the effects of retail conversions (conversions from medical marijuana to retail marijuana stores) on neighboring house values in Denver, Colorado. The study period reflects a time before and after retail marijuana sales became legal in Colorado in 2014. Using a difference-in-differences approach, we compare houses that were in close proximity to a conversion (within 0.1 miles) to those that are farther away from a conversion. We find that single family residences close to a retail conversion increased in value by approximately 8% relative to houses that are located slightly farther away. We perform a battery of robustness checks and falsification tests to provide additional support for this finding. To our knowledge this is the first study to examine at a micro-level the highly localized effect of retail marijuana establishments on house prices and hope that it can contribute to the debate on retail marijuana laws.

Key Words: *Retail marijuana, house prices, difference-in-differences, recreational marijuana legalization*

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Introduction

Attitudes towards marijuana use in the United States have changed considerably in the last few decades. According to Gallup polls, the percentage of adults that support the legalization of marijuana has increased from only 12% in 1969 to 58% in 2015 (Jones (2015)). State regulations have also shifted in response to these changing attitudes. Possession of small amounts of marijuana has been decriminalized in twenty-one states, and over the past two decades twenty states have legalized medical marijuana (MML). In the past four years alone, voters in four states (and Washington D.C.) legalized recreational marijuana (RML),[1] *while voters in an additional four states (out of a possible five) voted to legalize recreational marijuana in the November 2016 elections.*

Voters, policy-makers, and economists are interested in the ways recreational marijuana legalization affects communities. Despite the interest in the subject, the impacts of RML are not well understood at this point because only a handful of states have legalized recreational marijuana, and all of them within the last four years. Thus, RML remains a controversial issue at least in part due to a lack of data and empirical evidence about its effects. A primary concern of opponents is that the legalization of retail marijuana will increase crime in local communities. For example, legalizing recreational marijuana may lead to higher rates of driving under the influence (DUI), increased theft, and elevated crime rates. Another concern is that legalization will increase use and abuse of marijuana and other drugs, particularly among children. On the other hand, legalizing recreational marijuana may have positive impacts on a community. RML may be accompanied by a decrease in alcohol use if marijuana and alcohol are substitute goods.[2] If the negative externalities of alcohol exceed those of marijuana, legalization may have a net positive impact. Additionally, by bringing black-market economic activity into legal markets RML may even reduce drug related crime. RML may also increase local governments' tax receipts, enabling the government to provide greater services. Thus, the net impact of RML on local communities remains an empirical question.

In this paper we add to the debate on RML's impact on local communities by examining the

[1] We adopt these acronyms (MML and RML) from Hunt and Miles. Cheng et al. (2016) also uses RML to refer to retail (recreational) marijuana laws. We will also use the terms recreational and retail interchangeably throughout the paper.

[2] Evidence is mixed as to whether alcohol and marijuana are complements or substitutes. For example, Anderson et al. (2013), Crost and Guerrero (2012), and DiNardo and Lemieux (2001) provide evidence that the two consumption goods are substitutes. However, other studies argue that marijuana and alcohol are complements (Williams et al. (2004) and Pacula (1998)).

effects of retail marijuana stores on nearby house prices in Denver, Colorado. Analyzing house prices is a useful way to examine the issue since the net effects of RML are likely to be capitalized into house prices. Colorado presents an ideal environment to investigate the relationship between RML and house prices because the state of Colorado legalized recreational sales beginning on January 1, 2014.[3] Importantly, only existing medical marijuana facilities were allowed to sell recreational marijuana. This allows us to examine the impact of what we refer to as *"retail conversion"* – a store's conversion[4] from medical to retail marijuana sales – on neighboring house values in Denver.

Using a difference-in-differences approach with publicly available data from Denver, we compare houses that are in close proximity to a retail conversion to those that are slightly farther away from a retail conversion before and after the legalization of recreational sales. There are several features of RML in Denver that help us to identify the causal impact of a store's conversion from medical to retail sales on house prices. First, since only existing medical marijuana stores were allowed to conduct recreational sales, we avoid the potential endogeneity of store location. Given the opportunity, retail marijuana stores would likely choose to locate in certain areas based on neighborhood characteristics that would also affect house prices. However, since only existing medical stores were allowed to sell retail marijuana, the siting decision was made *before* implementation of RML. Second, the fact that retail sales began at the beginning of 2014 gives us a clear time for our pre-treatment and post-treatment periods – 2013 and 2014, respectively. Third, since the list of stores approved for retail sales was not publicly released until the end of 2013, we can consider conversion to retail as an unexpected event. Finally, the data used in our analysis identifies each property's neighborhood. This allows us to control for both time-invariant and time-varying neighborhood characteristics that affect property values.

Our results indicate that retail conversion has a large positive impact on neighboring property values after controlling for property attributes and neighborhood characteristics. We find that after the law went into effect, single family residences close to a retail conversion (within 0.1 miles) increased in value by approximately 8.4% relative to houses that are located slightly farther from a conversion (between 0.1 miles and 0.25 miles). While at first glance this estimate may seem

[3]In addition to Colorado, three other states (and Washington D.C.) have legalized recreational marijuana. We investigate the impact of RML in Denver, Colorado because of the wealth of publicly available Denver property data.

[4]Technically, medical marijuana stores did not convert to recreational stores. Rather, they added recreational sales to their existing businesses. For ease of exposition, we will refer to this as conversion throughout the paper.

large, our results are very much in line with existing research concerning the external effects of RML. In a related paper, Cheng et al. (2016) find an approximately 6% increase in house prices for municipalities in Colorado that adopted RML. Whereas Cheng et al. (2016) are looking at a municipality level effect, our analysis is more micro in nature. We are measuring the effect of an existing store's conversion to retail on local property values. We perform a battery of robustness checks that confirm the positive relationship between retail conversion and nearby house values. A key finding in these tests is that the effect of retail conversion is highly localized. Properties located within 0.1 miles of a retail conversion experience a large increase in value, however, properties located farther than 0.1 miles appear not to be impacted by retail conversion. We also emphasize that the focus of this study is to identify and quantify the external effects of retail conversions, not the underlying drivers of these effects. Potential explanations include, but are not limited to: a surge in housing demand spurred by marijuana-related employment growth, lower crime rates, and additional amenities locating in close proximity to retail conversions. Identifying and determining the underlying mechanism driving the relationship between retail conversions and house prices remains a puzzle that we leave to future research.

We are also able to examine and estimate the welfare effects of such retail conversions. The obvious direct effect of a retail conversion is the increase in asset value that accrues to an owner of a property, which we approximate to be almost $27,000 for an average house within 0.1 miles of a conversion. These increases in house values, however can also have a secondary effect that leads to an increase in property tax revenue, which can add significantly to the local government's revenue from marijuana sales. This implies that the net effect of RML can be felt beyond the direct tax revenue from recreational sales.

A few caveats to our analysis are worth noting. First, we are measuring the effect of retail conversion on house prices, not the effect of the store per se. The stores already existed as medical marijuana facilities. Therefore, any effect of the store establishment on property values normally occurred prior to our study period. Second, our analysis focuses only on Denver, so one should be careful in generalizing our results to other urban areas. Finally, we are investigating relatively short-term effects of retail conversion on nearby house prices since our data extends to only two years after conversion. Our analysis is silent on longer-term impacts of retail conversion.

To our knowledge, our paper is the first to analyze the effects of retail marijuana *conversions* on property values at the local level. The results indicate that retail conversion has a large positive, but highly localized effect on property values. Understanding the relationship between house prices and retail conversion is particularly important since voters in four states voted to legalize recreational marijuana in the November 2016 election, and others are likely to follow. Voters should understand the full impact of RML prior to voting on the issue, including its effect on property values. Similarly, policymakers in states that adopt RML will want to know how to proceed with implementation of the laws. Most likely they will follow models used in precedent retail marijuana states, particularly if negative impacts of RML implementation in those states are limited. Our analysis shows that in Denver, Colorado the retail conversion model had a short-run net positive impact on nearby property values.

Background

In November of 2000 voters in Colorado passed a state constitutional amendment (Amendment 20) that effectively legalized medical marijuana.[5] Starting in June 2001, patients with debilitating medical conditions, as well as their primary caregivers were allowed to possess up to two ounces of marijuana and no more than six marijuana plants for medical use by the patient. Patients needed to be a resident of Colorado and have written documentation from a physician that the use of medical marijuana might be a beneficial treatment for the patient's debilitating medical condition. Also, caregivers were limited in the number of patients they could work with. In the first several years after Amendment 20 the size of the medical marijuana industry remained relatively small. However, after liberalization of the definition of caregivers and their ability to dispense medical marijuana from 2007 to 2009, the industry began a period of rapid growth. In response, Senate Bill 10-109 and House Bill 10-1284 were passed in 2010 with the intention of regulating and controlling the distribution of medical marijuana and imposing restrictions on patients, caregivers, and doctors.[6]

Twelve years after medical marijuana was legalized in Colorado, the issue of recreational marijuana was put on the ballot. On November 6, 2012 Colorado voters passed Amendment 64 that

[5] A copy of the Amendment is available on Sensible Colorado's website: http://sensiblecolorado.org/amendment-20.

[6] Details of House Bill 10-1284 and Senate Bill 10-109 are available at http://www.leg.state.co.us/clics/clics2010a/csl.nsf/fsbillcont/0C6B6577EC6DB1E8872576A80029D7E2?Open&file=1284_01.pdf and http://medicalmarijuana.procon.org/sourcefiles/SB109CO.pdf, respectively.

legalized the private possession and use of recreational marijuana by individuals in Colorado that are at least 21 years old. The amendment also governs the operation of marijuana-related-facilities (Murphy (2015)). Although Amendment 64 passed in 2012, there was still considerable uncertainty surrounding how the law would be implemented. In May of 2013, Governor John Hickenlooper signed into law the recommendations from a task force regarding the implementation of Amendment 64 in Colorado, including the requirement that retail licenses initially only be issued to locations that were already licensed to distribute medical marijuana or that had applied for a medical license prior to Amendment 64 passing into law.[7] Another important piece of the new law was that the Colorado Department of Revenue (CDR) needed to convert the existing Medical Marijuana Enforcement Division into the new Marijuana Enforcement Division (MED) to provide regulatory oversight. Licensing was the responsibility of MED, which adopted its final regulations and licensing procedures on September 9, 2013. MED began accepting applications for retail licenses from existing licensed medical facilities in early October 2013. On December 23, 2013, CDR issued licenses for retail marijuana businesses to begin conducting retail sales on January 1, 2014. The fact that licenses were not issued, nor publicly announced, until the end of December, 2013 is important for our study. Up until that date, there was still uncertainty regarding who would be licensed to sell recreational marijuana on January 1, 2014.

In addition to state level licensing, retail facilities are also required to obtain licenses from local governments. In fact, local governments have the ability to ban retail marijuana sales altogether. The City of Denver opted to allow retail marijuana facilities within its boundaries. On September 16, 2013, the Denver City Council passed a bill that outlined the City's regulations for the retail marijuana industry (Meyer (2013)).[8] The bill allowed four types of licenses: cultivation, product manufacturing, testing, and retail sale. Importantly, only existing medical marijuana facilities in good standing would be able to apply for a retail sale license through the end of 2015. This limitation is crucial to our identification strategy since it allows us to abstract away from the potentially endogenous relationship between marijuana business siting and residential property prices.

[7]The task force recommendations are available at http://www.colorado.gov/cms/forms/dor-tax/A64TaskForce FinalReport.pdf.

[8]A copy of the bill is available at https://www.denvergov.org/Portals/723/documents/Am%2064% %20CB13-0570_Bill_MarijuanaLicensing_AMENDMENTS2_09-16-13.pdf.

Literature Review

Economists have long been interested in estimating the impacts of amenities and disamenities, such as air pollution, school quality, and crime in urban areas. Since households' marginal willingness to pay (MWTP) for amenities or to avoid exposure to disamenities is not directly observed in the market, economists have turned to comparing housing price differences across locations to estimate households' MWTP for these goods (bads).[9] Rosen (1974) developed an empirical methodology for estimating demand and supply parameters for the characteristics of a commodity if no explicit solutions for the hedonic price function is available. The difficulties in identifying the hedonic price function for local amenities are well documented and addressed in the literature, further contributing to the hedonic methodology becoming an important tool in applied research.[10] For example, Edel and Sclar (1974) note that the pricing of local amenities is better identified in short-run changes in housing prices due to exogenous shocks to those amenities, as is the case in our study. We explain our identification strategy in detail in a subsequent section of the paper.

Several studies have applied the hedonic pricing methodology to examining the relation between property values and location-specific amenities in various areas of environmental and urban economics. Following numerous inconclusive studies of the effect of school quality on student achievement, Black (1999) adopted the alternative strategy of estimating the value that parents place on school quality by estimating how much more houses located in areas with better schools fetch. To address endogeneity from omitted variables at the school district level and within and across school districts, Black (1999) focuses on houses located in school attendance district boundary areas and finds that households are willing to pay about 2.1% ($3,948 at the mean) more for houses associated with schools with higher test scores in the suburbs of Boston, MA. Adopting a modeling technique similar to ours to examine the effect on house prices of changes in admission policies to Chicago's magnet schools in 1997, Bonilla et al. (2016) find that improved admission probabilities due to the reform increased house prices for homes within the 1.5-mile radius by about 5.4% with the premium being higher for homes in areas near multiple magnet schools. The hedonic

[9]If the level of amenity varies across locations and if households are mobile, which is generally the case, then demand for an amenity will be capitalized in housing prices. Standard assumptions about consumer preferences imply that properties in high-amenity (low-disamenity) locations must have higher values than equivalent properties in low-amenity (high-disamenity) areas (Davis (2004)).

[10]See Epple (1987), Bartik (1987), Ekeland et al. (2002), Davis (2004), Chay and Greenstone (1998), Kuminoff et al. (2010), and Locke and Blomquist (2016).

price approach has also been used to uncover the elasticity of housing values with respect to crime (Gibbons (2004), Linden and Rockoff (2008), Pope (2008)); air pollution (Chay and Greenstone (1998)); the location of group homes (Colwell et al. (2000)); cancer health risks (Davis (2004)); and proximity to power plants (Davis (2011)), strip clubs (Brooks et al. (2015)), communication antennas (Locke and Blomquist (2016)), underground petroleum contamination (Zabel and Guignet (2012)), and wind turbines (Hoen et al. (2015)).

Other recent studies that employ hedonic difference-in-differences estimators include McMillen and McDonald (2004), Voicu and Been (2008), Billings (2011) and Billings (2015). Examining the impact of transport amenities on neighboring house values, McMillen and McDonald (2004) and Billings (2011) document a positive effect of new rail transit systems on house prices in Chicago, Illinois and Charlotte, North Carolina, respectively. Billings (2011) further shows that adjacent houses experience smaller price increases possibly due to the adverse effect of increased noise, traffic, and parking. Billings (2015) cautions against positive bias from housing renovations when examining the pricing of amenities and shows that his previous findings about the impact of Charlotte's new transit system on neighboring house values still obtain after accounting for renovations. The quality of neighboring houses also affects house values. Urban gardening has gained popularity, particularly in cities such as Detroit and Milwaukee that are saddled with vast amounts of vacant land. Using New York City data, Voicu and Been (2008) find that community gardens have significant positive effects on neighboring property values, especially in the poorest neighborhoods.

Until very recently, studies concerning the effects of marijuana legalization largely focused on land use and related social issues (Freisthler et al. (2013) and Boggess et al. (2014). But in a parallel study, Cheng et al. (2016) explore the effect of retail marijuana legalization in Colorado on house prices at the municipality level. Using a difference-in-differences approach, Cheng et al. (2016) document a 6% increase in housing values on average in municipalities that allow retail marijuana sales. To identify the effect, they use variation in time of adoption of Amendment 64 by municipalities along with municipality and time fixed effects. Even though the amplitude of the effect on house prices they document is still positive, but slightly smaller than our findings, Cheng et al. (2016) cover larger and more heterogeneous areas. In contrast, our study focuses on one city in Colorado (Denver). Our identification is within smaller and more homogenous units – neighborhoods rather than municipalities – and we control for unobservables affecting housing prices using neighborhood fixed effects. Since identification is within neighborhood, the treated and control groups in our study are more likely to share similar observable and unobservable characteristics. However, the two studies should be viewed as complementary given their difference in geographic focus. In contrast to the documented positive effect of retail marijuana on residential property values, Billings et al. (2016) documents a negative spillover effect of legal marijuana business establishment on commercial rents in the near vicinity in Denver. This very localized effect on nearby commercial space is not necessarily incompatible with the positive effect on housing prices documented in Cheng et al. (2016) and this paper for various reasons. First, Billings et al. (2016) deal with the spillover effect on commercial properties, whereas we are examining residential properties. Second, their analysis focuses on the first wave of medical marijuana openings in 2009, whereas we are looking at the switch from medicinal to retail of already established medical marijuana dispensaries. Their study is actually compatible with the negative effect we find close to medical marijuana dispensaries before their conversion to retail. Third, the authors construct their control (comparison group) differently than we do. They select commercial sites that potentially could have had a dispensary close by, but did not, whereas our comparison group is simply housing units that are located further away from retail dispensaries. Therefore, Billings et al. (2016) answers a very different question with a very different approach.

Data

Our analysis is based on two main sources of publicly available data: a list of retail licenses granted by the Colorado Department of Revenue and property information from the City of Denver. As of December 23, 2013, the Colorado Department of Revenue Marijuana Enforcement Division (MED) issued licenses to 103 retail marijuana businesses in Denver to begin conducting recreational sales on January 1, 2014. Only locations that were already licensed as medical marijuana businesses were allowed to apply for a retail license (Colorado Department of Revenue Marijuana Enforcement Divisions (2013)).[11] The list of stores approved for retail sales includes the name of the business and the physical address. From the physical address we manually obtained the latitude and longitude

[11] The list includes locations both inside and outside Denver and is available at http://s3.amazonaws.com/assets.gazette/pdf/MED%20Licensed%20Retail%20Stores%20as%20of%2012-23-2013.pdf. As of January 10, 2014, the list of retail licensed locations in Denver had not changed and is available at https://www.colorado.gov/ pacific/sites/default/files/Retail%20Stores%20011014.pdf.

of each of the 103 stores. This information is critical to calculating the distance between the stores and residential property transactions.

The residential property information is publicly available from the City of Denver's Open Data Catalog. To perform our analysis, we need information from three separate databases.[12] The first database, hereafter referred to as the residential file, includes information on 199,255 residential properties in Denver. This database includes a unique property identifier, the current owner, the property address, property type, property characteristics (e.g., number of bedrooms and bathrooms, square footage, year built) and neighborhood.[13] The neighborhood boundaries are created by Denver's Assessment Division for use in their property valuation process. According to the City and County Assessor, the 283 neighborhoods are meant to define areas of relatively homogenous single family residences based on location and other factors such as structure age, design, and distance to amenities.[14] The average neighborhood size is approximately 0.54 square miles. Initially we keep 195,446 properties that are classified as either condominium, rowhouse, single family residential, or vacant land.[15]

The second data set from the City of Denver used in our analysis, which we refer to as the address file, includes 282,193 residential and commercial property addresses in Denver. The file also includes the latitude and longitude of each of the addresses which we use to calculate the distance between the retail marijuana stores and residential properties. There are a few issues working with this database. First, the address combined with the latitude and longitude does not create a unique identifier. There are multiple observations of the same address, and those observations can have different latitudes and longitudes.[16] This can occur with apartment complexes, mixed use buildings, condominium complexes, multi-unit residential, and commercial properties. Since our analysis focuses on single family residences, this is not a major issue in our final data set. In fact, for 99% of the residential transactions we use in our analysis there is only one address observation in the Denver addresses database. Second, there is no unique property identifier to match to the

[12] All Denver property data was downloaded on November, 11, 2015 from https://www.denvergov.org/ opendata/search?q=property.

[13] The address in the residential file is less precise than the one reported in the address file discussed below. Thus, we do not use the address in the residential file to obtain the latitude and longitude of the property. Rather, we obtain that information from the address file.

[14] After cleaning the data as described below, our sample includes 257 of the 283 neighborhoods.

[15] Our final analysis only includes single family residences.

[16] There are 193,493 unique addresses based on street number, street direction, street name, and street type. Of those, 188,172 are associated only with one observation, and thus, one latitude and longitude.

other two Denver databases. Therefore, we match the databases by address. However, since the street direction field (e.g., N., E., S., W.) is not filled out as frequently in the residential file as in the addresses file, we match the database in two steps. First, we match the 195,446 observations from the residential file to the addresses file based on the street number, street direction, street name, and street type and keep only those observations that match exactly (115,412). In the second round of matching, we take all observations from the residential file that were not matched in the first round (80,034) and match based on street number, street name and street type. If the street directions are not blank and do not match, we drop the observations. If the street direction is blank in only one of the databases, then we consider it a match. This creates a match for 69,766 more residential properties. The second round of matching opens the possibility of incorrect matches.[17] We manually checked 50 of the addresses from the second round of matching and found that the difference in street direction mattered for only one observation. Therefore, we do not believe this is a major concern for our analysis. We drop the remaining 10,068 residential properties have no match after the first and second rounds of matching.[18]

The third data source from Denver includes 211,071 property sales and transfers from 2008 to 2015. The file includes the sale date, the sale price, and the same unique property identifier as in the residential properties data. We merge the property sales data with the residential property information based on the property's unique identifier. After merging the three public datasets from the City of Denver, we match this data to the retail marijuana store location data. We calculate the distance between each property sale and each store location as well as the number of stores within different distance rings from the property (e.g., within .10 miles; within .10 - .25 miles). Since our identification strategy requires sales before and after medical stores converted to retail, we include only sales in 2013 and 2014. To reduce the effects of data entry errors, we exclude observations for which: (1) the sales price is missing (2) the sale price is less than $30,000; (3) the sale price is greater than $2.7 million (the 99th percentile); (4) the square footage is missing or zero; (5) the lot area is missing or zero; (6) the property type is not a single family residence; (7) the number

[17] For example, 700 Washington Street may be a different property from 700 N. Washington Street, even though our second round of matching classifies both addresses as the same property. This is only a potential problem for the 69,766 observations matched in the second round.

[18] In robustness checks (unreported), we perform the entire analysis discussed below on the subsample of observations that had an exact match after the first round of matching and results are qualitatively unchanged.

of bedrooms is zero; (8) the number of bathrooms is less than one.[19,20] Our final data set includes 19,555 observations. Descriptive statistics for the properties included in our sample are presented in Table I. The average sale price is approximately $315,000. The average house is roughly 1400 square feet with 2.8 bedrooms and 2.27 bathrooms. The average age of the houses sold in our sample is 56 years, but ranges from zero years (new construction) to 140 years old.[21] Although the maximum number of bedrooms (8) and bathrooms (10.5) appear high, the 99th percentile of the distribution for both of these variables is five, and our main results are materially unaffected when we exclude properties with more than five bedrooms or bathrooms.

Figure 1 presents a Denver neighborhood map showing the location of the 103 retail conversions. Also included on this map are the property sales transactions that occurred in 2014 (post-conversion) within one mile of a retail conversion. The transactions are differentiated by color depending on their distance from a retail conversion. Several facts emerge from Figure 1. First, there are many retail conversions that have a significant number of sales at varying distances from the store. This is crucial to our identification strategy. Second, retail conversions tend to cluster in certain areas. For example, in south Denver, retail conversions cluster on South Broadway and South Downing Street, in the Overland, Rosedale, Platt Park and University neighborhoods. Third, there are several retail conversions that are not located near any single family residential sales transactions in 2014. Visual inspection of these locations using Google Maps shows that these stores tend to either be in high density locations with few (or no) single family residences or industrial locations. Figures 2 and 3 show sales transactions occurring near one retail conversion in 2013 (pre-conversion) and 2014 (post-conversion), respectively.[22] Our identification strategy – discussed

[19]Conditions (1) through (8) drop 9,712 observations, 6,568 observations, 320 observations, 1,336 observations, 2 observations, 10,918 observations, 32 observations, and 12 observations respectively. 98% of the observations with a sale price less than $30,000 also have a sale price less than or equal to $100. This suggests that these sales are non-arms-length transactions and should be excluded from the analysis. In fact, the majority of "sales" where the sales price is missing or less than $30,000 would not normally be considered a true sales transaction. Most of these transfers are either quitclaim deeds or transfers by the Public Trustee. Many of the quitclaims deed observations simply add an individual to the deed or change the current owner's name (e.g., marriage), and 95% of the quitclaim observations have a sales price of $10 or less. Transfers by the Denver Public Trustee are foreclosures– Colorado is a "deed of trust" state – and do not have a sales price listed.

[20]In unreported robustness checks, we code missing values (or zero values) as zero and create indicator values for the missing variable. For example, we create a missing value indicator for observations where square footage is zero or missing and we re-code square footage as zero for those observations. The results presented below are materially unchanged when we re-code these variables and include missing value indicators.

[21]The exclusion of new constructions does not affect our findings. For the sake of space, these results are not reported, but they are available upon request.

[22]The retail conversion, doing business as Higher Ground MMC, is located at 2215 East Mississippi Avenue, Denver, CO 80210.

in detail below – essentially looks at how differences in sales prices by location (e.g., within 0.10 mile versus 0.10 - 0.25 miles in 2013) change after retail conversion in 2014.

A couple of qualifications need to be made regarding our data. First, it is important to note that for stores to sell retail marijuana, they must be licensed by both the state and local governments. Although the stores on the list we obtained were approved for retail marijuana sales by the state, only a fraction of these stores were licensed by the city of Denver as of January 1, 2014, and thus, began retail sales on that date. This is not a major concern for our analysis for several reasons. First, since it is costly for a business to apply for a license from the state, the application for, and receipt of, a license from the state sends a strong signal that the licensee plans to actually meet city level requirements to open the business. Buyers and sellers in the residential property market are likely to respond to this signal. Second, we are fairly confident that the majority of the stores that applied for a retail license from the state also received a license from Denver at some point. Although there is no publicly available data that indicates how many of the initial 103 state-licensed retail businesses also received licenses from the city of Denver, according to the city of Denver's website, 93 retail locations were licensed in Denver in September 2014.[23] Also, if a store's application was rejected by the city of Denver, there would be little incentive to renew the retail license at the state level. Over 80% of the businesses that were licensed for retail sales in Denver by the state of Colorado at the beginning of 2014 still have retail licenses from the state as of September 2015. Taken together, these facts suggest that the majority of the stores that were licensed by the state at the beginning of 2014 also received licenses from the city of Denver at some point in 2014.

Proposed Model

While we initially remain agnostic as to any effect (positive, negative, or neutral) a retail conversion has on nearby property values, we use a difference-in-differences approach combined with a hedonic price model to determine that impact. The methodology adopted in this paper is similar in design to Linden and Rockoff (2008), who examine the effect of crime on property values. Using a difference-in-differences approach, Linden and Rockoff (2008) estimate households'

[23]Information is available at https://www.denvergov.org/content/denvergov/en/denver-marijuana-information /statistics/licensing-and-locations.html.

valuation of living in close proximity to a convicted sex offender by exploiting variation in the threat of crime within small, relatively homogeneous groupings of homes in North Carolina. They document a 4% decline in property prices within 0.1 mile of a sex offender and a 12% decline for adjacent properties following the arrival of an offender in the neighborhood. As in our study, that effect is extremely localized and dissipates quickly with distance. Pope (2008) further shows that housing prices tend to immediately rebound once the sex offender moves out of the neighborhood. Pope and Pope (2015) also uses a difference-in-differences approach to determine the effect of living in close proximity to a Wal-Mart, and Muehlenbachs et al. (2015) examine the effect of shale gas development on property values.

To proceed with our difference-in-differences estimation, we designate the treatment as a retail conversion, which by law, occurred on or after January 1, 2014. In line with much of this literature, we are also aware that the effect of the treatment can potentially vary with distance. We initially define a treatment area in our study to be single family residential properties sold within a 0.1 mile radius of at least one retail conversion in 2014. We then compare this treatment area to a control group of the single family residences sold within 0.1 and 0.25 miles of at least one retail conversion in 2014. The idea being that the area within 0.1 and 0.25 miles is very close in proximity to the treatment area and therefore the main difference between the two areas is simply the proximity to the dispensary; any economic effects not involving recreational marijuana would likely affect both areas in the same way. Later we will relax this assumption and vary our treatment and control area radius.

As mentioned previously, we restrict our analysis to property transactions in the years 2013 and 2014, where we treat January 1, 2014 as the first day of the post-treatment period. We also would like to re-emphasize that if any statistical significance is found, this would only measure the effect of a *change* from a medical marijuana dispensary to a retail marijuana dispensary and not the actual presence of a dispensary itself. Determining the original effect of the physical medical marijuana facility is out of the scope of our data,[24] but we believe that measuring this switching effect can be quite important. Several states in the US have legalized medical marijuana and that process is typically seen as a first step into legalizing marijuana for recreational use/sale. In fact,

[24]Information on the location of medical marijuana facilities is only available beginning in 2013, thus we cannot study the impact of medical siting on property values.

the model used by Colorado that allowed only existing medical dispensaries to convert to retail establishments was also used in Oregon and will also be implemented in Nevada. If other states are to follow the same procedure, then our analysis will be instrumental in the debate over the external effects of such a policy.

Preliminary Evidence

If we simply look at the average prices for single family residences, we see that the largest gains between 2013 and 2014 occurred within 0.1 of at least one retail conversion. Table II shows the mean transaction price for single family residences that are within 0.1 miles of at least one retail conversion (Ring 1), within 0.1 and 0.25 miles of at least one retail conversion (Ring 2), 0.25 and 0.5 miles of at least one retail conversion (Ring 3), 0.5 and 0.75 miles of at least one retail conversion (Ring 4), and 0.75 and 1 mile of at least one retail conversion (Ring 5). The means are also separated by year (2013 and 2014). For Ring 1 we see that house prices increased by about $60,000 between 2013 and 2014. In contrast, the increase is only $26,000 for Ring 2. The increase in average price between 2013 and 2014 for Rings 3 and 4 are $31,000 and $27,000, respectively. For Ring 5, there is actually a $4,600 decrease in average house prices between 2013 and 2014. Obviously, there may be other factors that could be driving some of these price changes, but univariate evidence clearly shows the largest gains between 2013 and 2014 occurred for houses located within Ring 1. While there are about 5 times as many transactions in Ring 2 as Ring 1, we also emphasize that the geometric area of Ring 2 is about 5 times the size of Ring 1. Table II also shows that dispensaries are generally located in low house price areas.

Estimation

Following Linden and Rockoff (2008), we take a difference-in-differences approach to determine the effect of a change on house prices. Our empirical model is as follows:

$$\begin{aligned}\ln P_i =& \alpha + \beta X_i + \gamma R_{i,0.1} + \delta R_{i,0.25} + (\psi R_{i,0.1} + \phi R_{i,0.25}) * Post\_dummy \\ &+ \varphi Post\_dummy + \sum_j \left(\lambda_j nbhd_j\right) + \sum_j \left(\rho_j Post\_dummy * nbhd_j\right) + \varepsilon_i \end{aligned} \quad (1)$$

where P_i is the transaction price of the property. X_i are the physical characteristics of the property including unit size, lot size, bedrooms, bathrooms, age, age^2, and stories.[25] $R_{i,0.1}$ is an indicator for the treatment area that equals one if the property is located within 0.1 miles of at least one medical dispensary that converted to a retail dispensary. $R_{i,0.25}$ is an indicator that equals one if the property is located between 0.1 and 0.25 miles of at least one medical dispensary that converted to a retail dispensary. $Post\_dummy$ is a binary variable that indicates the transaction occurred on or after January 1, 2014.[26] $nbhd\_j$ is a series of 257 binary variables that indicate whether the house is located in a neighborhood. The neighborhood dummies control for the time invariant characteristics of the neighborhood and $Post\_dummy * nbhd\_j$ controls for time-varying characteristics of the neighborhood. The key parameters of interest in this analysis are ψ and ϕ, which will determine the effect (if any exists) of a change to a retail dispensary for houses located within 0.1 miles of a retail conversion and for houses located within 0.1–0.25 miles of a retail conversion, respectively.

Primary Results

The coefficient estimates from Equation (1) are presented in Table III. Our primary estimate of interest ($\widehat{\psi}$), labeled "Within 0.1 miles of at least one store x Post-Retail" in Table III, is both positive and statistically significant, confirming the unconditional differences in mean transaction prices in Table II. The result implies that being located near at least one medical facility that converts to retail in 2014 is associated with an 8.4% increase in sales price after the retail conversion occurs. The magnitude of this effect is only slightly larger than the much broader (municipality level) finding from Cheng et al. (2016). Also of note is the estimation of γ which measures the effect of "Within 0.1 miles of at least one store" which we find to be statistically significant and negative.

[25] Coulson et al. demonstrate that a "new housing premium" can potentially exist under certain conditions, so we also carried out a specification which included a binary variable for new construction when age=0. Our results do not change substantially.

[26] There is typically a lag time between when a purchase price is agreed upon between the buyer and the seller and the closing date of the transaction. This raises the possibility that we mis-classify some observations as occurring in the post-treatment period (2014) even though the sale price was agreed upon prior to treatment (2013). For example, the observed prices of transactions occurring in early January 2014 may have been agreed upon prior to the public release of approved retail conversions disclosed on December 23, 2013. This does not appear to be a major concern for our study for two reasons. First, in our sample, transaction volume in 2014 is lowest during the month of January. Thus, the potential for mis-classification is relatively low. Second, our main results reported below are not sensitive to shifting the date one month prior to the closing date to account for the lag between agreement and closing.

This implies that houses located in the immediate vicinity of stores that convert to retail have lower values than houses in surrounding areas, *ceteris paribus*, consistent with the univariate evidence in Table II. As mentioned previously, the original placement of medical marijuana dispensaries was the subject of much controversy and involved zoning them for certain areas (especially away from schools). We can interpret this negative coefficient as either the depressing effect the medical dispensary had on that area, the idea that medical dispensaries were placed in already poor areas, or a combination of both factors. If we look at our control group, the estimation of ϕ – labelled "Within 0.25 miles of at least one store x Post-retail" – shows no price difference (in terms of proximity to at least one retail dispensary) of those single family residences between 0.1 and 0.25 miles and those that are located greater than 0.25 miles from at least one retail dispensary.[27]

To summarize, several important facts emerge from Table III. First, retail conversions tend occur in areas with lower house prices, on average. Second, after the conversion, houses located near the conversion (within 0.1 miles) experience an 8.4% increase in price relative to properties located more than 0.25 miles away from the conversion. Third, houses located slightly farther from the conversion (0.1 - 0.25 miles) do not experience the same increase in prices. This suggests that retail conversions have a positive impact on house prices, but the effect is highly localized.

One may also be interested in what could potentially be driving this result. First, one area that has benefited greatly from the legalization of marijuana is the labor market. The recreational marijuana industry created an incredible number of marijuana related jobs that contributed to an increase in demand in the housing market. It is estimated that 18,000 new marijuana related jobs in Colorado were created in 2015.[28] Even in 2014, observers of the nascent recreational marijuana market could already see signs of extreme interest in marijuana related jobs.[29] This period also coincided with a large overall boom in the Denver economy, resulting in a tightening of the real estate market, and creating an affordability issue. In June 2015, the residential real estate market was reported to be incredibly tight with 1/6 the residential listings of a normal market.[30] Taken together, an increase in marijuana-related jobs, a tightened housing market, as well as the evidence

[27]While some similar studies cluster standard errors, we do not feel it is appropriate given the relatively small size of neighborhoods and the fact that some dispensaries straddle the border of multiple neighborhoods and thus unobserved characteristics across neighborhoods will likely be correlated.

[28]http://www.chicagotribune.com/business/ct-colorado-marijuana-industry-20161031-story.html.

[29]http://articles.latimes.com/2014/mar/14/business/la-fi-mo-colorado-marijuana-job-fair-20140314.

[30]http://money.cnn.com/2015/06/04/real_estate/marijuana-denver-housing-market/index.html.

we present that the housing stock near (within 0.1 miles) of a medical dispensary is relatively lower valued than other housing stock all combine to provide a story that there were many competitive buyers for the more affordable units in the city and the fact that they are located near retail dispensaries is not considered a disamenity. In fact, if many of these buyers have marijuana-related jobs, living near a retail dispensary could be considered an amenity.

Also, there is no evidence of increased marijuana usage or crimes due to recreational marijuana legalization. Since Colorado was one of the first states to legalize recreational marijuana, journalists as well as government officials paid very close attention to many of the concerns brought up during the legalization process. In fact, the State of Colorado Department of Public Health and Environment established the Retail Marijuana Public Health Advisory Committee to analyze marijuana user behavior in Colorado as well as examine any links between health and marijuana usage. The committee produced an initial report in 2014 as well as a follow-up report in 2016 (Colorado Department of Public Health and Environment (2016)) that focuses on the time period after retail sales began. They find no statistically significant change in usage for high schoolers in the period between 2005 and 2013 suggesting that even after the passage of Amendment 64 (but before dispensaries opened up), there was not much of an effect. The 2016 report finds that the 2015 usage (after retail sales are allowed) for high school students and adults was not statistically different from previous numbers. The 2016 report also finds that for high schoolers, past-month marijuana use (21%) is actually lower than past-month alcohol use (30%). In fact, this lack of changes may have been aided by the fact that the bulk of marijuana-related tax revenue flows to public schools, public health efforts, and youth prevention programs.[31]

In terms of crime that coincided with our period of study, the first four months (January 2014 April 2014) of legal recreational marijuana sales coincided with a reduction in crime from the same period in 2013 with 4.8% less robberies, 3.7% less assaults, and a 10% reduction in crime overall.[32] While increasing DUIs related to marijuana were expected to be a major issue, according to the Colorado State Patrol which only began tracking it in 2014, they only accounted in 2015 for 15% of DUIs, which was 1.3 percentage points lower than previous the year's figure.[33]

[31] http://www.9news.com/news/where-does-all-that-colorado-pot-tax-go/298554132.

[32] https://www.nytimes.com/2014/06/01/us/after-5-months-of-sales-colorado-sees-the-downside-of-a-legal-high.html.

[33] http://www.denverpost.com/2016/02/11/fewer-people-charged-in-colorado-with-driving-while-high-in-2015.

In summary, the evidence from 2014-2015 paints a picture of economic growth, a tighter housing market, and lower crime rates, all while tax revenue is being generated for public works and marijuana usage is staying relatively flat.

Composition of Sales

Although we control for property characteristics in Equation (1), a potential concern is that the types of houses selling before retail conversions are significantly different from houses selling in the post-retail period. If this is the case, then relative increases in prices may be due to the changing composition of houses being sold, rather than capitalization of net benefits of retail conversions into housing prices. Another related concern is that prior to treatment, properties located near a (future) retail conversion may be significantly different from properties located slightly further away. To alleviate these concerns, we follow an approach similar to Pope and Pope (2015) by estimating the following model:

$$\begin{aligned} Prop\_Char_i =& \alpha + \gamma R_{i,0.1} + \delta R_{i,0.25} + (\psi R_{i,0.1} + \phi R_{i,0.25}) * Post\_dummy \\ &+ \varphi Post\_dummy + \sum_j \left(\lambda_j nbhd_j\right) + \sum_j \left(\rho_j Post\_dummy * nbhd_j\right) + \varepsilon_i \end{aligned} \tag{2}$$

where $Prop\_Char_i$ is one of the housing attributes included in Equation (1). All other variables in Equation (2) are as defined in equation (1). The results are reported in Table IV. We can see that for the most part, housing characteristics in the pre-treatment period for transactions within 0.1 miles and 0.1 to 0.25 miles are statistically indistinguishable from properties located farther away. Importantly, the first row of Table IV shows that prior to treatment, properties located within 0.1 miles of a (future) retail conversion are similar to the control group in terms of square footage, lot size, number of beds, number of baths, and the age of the property, *ceteris paribus*. This suggests that in the pre-treatment period, the treatment and control groups are relatively similar in terms of observable characteristics.

We are primarily concerned, however, with the interaction of the spatial indicators with $Post\_dummy$. Table IV shows that most of the interactions are not significantly different from zero, with the number of stories as a notable exception. In the post-treatment period, buyers are purchasing houses with fewer stories within 0.1 miles of at least one conversion. But, since we find an increase in house prices in this area – despite the fact that buyers are purchasing less housing capital – we can safely say that the price increase is not due to changes in housing composition for nearby sales.

Parallel Trends

A key identifying assumption of the difference-in-differences estimator is the parallel trends assumption, which assumes that the outcome of interest would have followed the same trend in both the treated and control group in the absence of treatment. In the context of our paper, parallel trends assumes that house prices near retail conversions would have evolved in a similar manner to prices for houses located slightly further away if the retail conversion did not occur. To test the parallel trends assumption, we follow the method outlined in Pischke (2005) and employed in Autor (2003) which allows for leads and lags of the treatment. Using property sales from 2008 through 2015, we estimate the following model:[34]

$$\begin{aligned} \ln P_i = &\alpha + \beta X_i + \gamma R_{i,0.1} + \delta R_{i,0.25} + \sum_k \Big((\psi_k R_{i,0.1} + \phi_k R_{i,0.25}) * Year_k \Big) \\ &+ \sum_k \Big(\varphi_k Year_k \Big)) + \sum_j \Big(\lambda_j nbhd_j \Big) + \sum_j \sum_k \Big(\rho_{jk} Year_k * nbhd_j \Big) + \varepsilon_i \end{aligned} \tag{3}$$

where $Year_k$ is a matrix of indicator variables for transaction year with $k \neq 2013$. ψ_k is interpreted relative to the omitted year (2013). All other variables are as defined in equation (1). The parallel trends assumption implies that $\psi_k = 0 \quad \forall \quad k < 2013$. $\psi_{2014} > 0$ suggests that properties located near a retail conversion experienced a larger increase in prices in 2014 relative to properties located slightly further away. The coefficient on the lag term (ψ_{2015}) will tell us if the effect of the retail conversion on nearby property prices accelerates, stabilizes, or mean reverts in the second year after RML was implemented (Autor (2003)). Finally, the estimates of ϕ_k will provide important information regarding both price trends across geographic locations as well as the breadth of the retail conversion effect.

[34] In this section we use transactions from 2008 through 2015, however, for most of the estimations in the paper we only include transactions in 2013 and 2014. We focus on just one pre- (2013) and one post-treatment (2014) period in most of our estimations because there are several estimations below where it is necessary to know the locations of medical stores that did not convert to retail stores. Information on medicinal marijuana locations is only available beginning in 2013. Focusing on just 2013 and 2014 makes it easier to compare results across different specifications.

Table V presents the specification including the lead and lag terms. Several important facts emerge from Table V. First, the coefficients on all of the lead terms (2008 through 2012) are not significantly different from zero. The difference in prices between properties located near (future) retail conversions relative to properties located further away did not change significantly in the years preceding the implementation of RML. This suggests that the parallel trends assumption is met and that difference-in-differences is a valid identification strategy.[35] Second, properties located near retail conversions experienced an 8.3% increase in prices after conversion (2014) relative to properties located further away. This is consistent with our primary results in it Table III. Third, the estimate $\hat{\psi}_{2015}$ (0.086) is statistically significant and nearly identical to $\hat{\psi}_{2014}$ (0.083). This provides some evidence that the retail conversion effect is stable and not short-lived. Finally, all of the lead and lag terms for properties that are located 0.10 to 0.25 miles (ϕ_k) are not significantly different from zero. This provides additional support that prices were following similar trends in different areas of Denver prior to implementation of retail conversions. Also, the parallel trends continued in 2014 and 2015 for properties located *more* than 0.10 miles from a retail conversion.

Taken together, the results in Table V suggest that our difference-in-differences identification strategy allows us to interpret the effect of retail conversions on nearby property values as causal in nature. Also, it appears that the effect is highly localized and relatively stable over a two year period after conversion.

Only Nearby Properties

As a first robustness check, we restrict our sample to include only single family residences that were close (within 0.1 miles) to at least one dispensary that switched from medical to retail (within 0.25 miles). We estimate the following model on the subsample of properties located within 0.25 miles of a retail conversion:

$$\begin{aligned}\ln P_i =& \alpha + \beta X_i + \gamma R_{i,0.1} + \psi R_{i,0.1} * Post\_dummy + \varphi Post\_dummy \\ &+ \sum_j \Big(\lambda_j nbhd_j\Big) + \sum_j \Big(\rho_j Post\_dummy * nbhd_j\Big) + \varepsilon_i. \end{aligned} \tag{4}$$

[35] Mora and Reggio (2012) note that evidence of parallel trends before treatment eliminates the need to adopt the more flexible assumptions (e.g., parallel growths assumption) proposed in their paper. We thank an anonymous referee for this suggestion.

Thus, we are comparing nearby properties (within 0.10 miles) with properties located slightly farther away (within 0.25 miles). This robustness check is implemented in order to use the houses within 0.1-0.25 miles as a control group instead of the main result which uses houses that are further away. This is to head off any differences that could arise between housing stock that is close to dispensaries versus those that are further away. The results of this estimation are in first column of Table VI. $\widehat{\psi}$ – again labeled "Within 0.1 miles of at least one store x Post-Retail" – remains positive and statistically significant. In fact, this shows that relative to the houses within 0.1 to 0.25 miles of at least one retail conversion, the closer houses experienced an 7.9% increase in property values, which is close to the coefficient (8.4%) reported in Table III. Also consistent with the results from Table III, prior to the retail conversion houses located within 0.10 miles of the store are worth roughly 10% less than houses located slightly farther away (within 0.25 miles).

Next we change our definition of treatment to include only properties located within 0.10 miles of *exactly* one retail conversion. The control group includes only properties located within 0.1 to 0.25 miles of *exactly* one retail conversion. Notice that because of store agglomeration, both the treatment and the control groups will change, but it is primarily the number of observations in the control group that is reduced by this restriction. Figure 4 shows retail conversions along with the treatment ("Within 0.10 Miles") and control group ("Within 0.10 - 0.25 Miles") for this specification. Our results are materially unchanged. The second column of Table VI shows that relative to the houses within 0.1 to 0.25 miles of *exactly* one retail conversion, the closer (within 0.1 mile) houses saw an approximate 11% (11.4%) increase in value. Note that these areas are also poorer areas since the houses in that area sold previously for on average 13% (12.7%) less than houses farther away.[36]

We can also look at a comparison between these two treatment areas (within 0.1 miles of at least one retail conversion and within 0.1 miles of exactly one retail conversion) in columns (1) and (2) of Table VI.[37] One main difference is that prior to the conversion, the houses within 0.1 miles of *exactly* one retail conversion have a larger difference in value (-12.7%) relative to their control

[36]Initially, we considered examining the effect of the switch of exactly one dispensary as our primary specification. However, given that many of these dispensaries are clustered in certain areas, it is hard to find a large sample of houses, particularly in the control group (e.g., farther away), within proximity to exactly one dispensary. Thus, our primary specification in Table III includes properties located near at least one retail conversion.

[37]As suggested by an anonymous referee, we could have included dummies for different numbers of nearby conversions instead. However, an overwhelming number of properties in our sample have only one conversion within their treatment area, hence our decision to compare one to at least one conversions.

group than houses within 0.1 miles of *at least* one retail conversion (-10%). While this may seem odd, a potential explanation is that the houses that are within 0.1 and 0.25 miles of *exactly* one retail conversion are likely different (and higher quality) than those that were within 0.1 and 0.25 miles of *more than* one retail conversion. Post treatment, houses within 0.1 miles of *exactly* one retail conversion increase in value more than those within 0.1 miles of *at least* one retail conversion as well (11.4% versus 7.9%). Again, a potential explanation for this slight discrepancy is that the treatment effect is likely dampened when dealing with an area proximate to *more than* one retail conversion.

Expanding Treatment and Control Groups

Admittedly, the choice of distance to define the treatment and control groups is somewhat arbitrary. To this point, we have followed Linden and Rockoff (2008) and Pope (2008) by using a 0.1 mile radius to define treatment. We will now estimate the model with varying control and treatment groups, respectively. First we hold the treatment group constant (at least one retail conversion within 0.1 miles) and re-estimate Equation (4), but we expand the control group to be properties within 0.1 and 0.5 miles, 0.1 to 0.75 miles, and 0.1 to 1 mile of at least one retail conversion. The results are presented in Table VII. Expanding the control group does not materially affect our main results. Within 0.1 miles of a retail conversion (pre-conversion), houses sold for about 10-11% less than the control area. However, the retail conversion brings house prices back up by about 8-9% relative to the control area, regardless of how the control area is defined.

In order to further demonstrate the highly localized nature of the treatment effect, we next expand the treatment group to be single family residences that had at least one retail conversion within 0.25 miles. Given the fact that conditional house prices both before and after a retail conversion (within 0.1 to 0.25 miles) were statistically indistinguishable from houses located over 0.25 miles away from a retail conversion, we expect that expanding the treatment group in such a fashion will dilute the results found in the previous section. The control group will be the respective adjacent "ring" (Equation (4)), properties within 0.25 to 0.5 miles. The results in Table VIII show that the coefficient on the post-treatment dummy variable is now insignificant when our treatment group is within 0.25 miles of at least one switch. As expected this particular result bolsters the argument that the effect is highly localized (within 0.1 miles), so expanding the treatment group will distort the results in that the newly defined treatment group will likely dilute the effect that we have shown to exist within 0.1 miles of a retail conversion.[38]

Non-Converting Stores

We also analyze price changes for houses close to a medical dispensary that did switch to retail versus price changes for houses close to a medical dispensary that *did not* switch to retail. We estimate the following model:

$$\begin{aligned}\ln P_i =& \alpha + \beta X_i + \gamma R_{i,0.1} + \delta R_{i,0.25} + (\psi R_{i,0.1} + \phi R_{i,0.25}) * Post\_dummy \\ &+ \tau N_{i,0.1} + \eta N_{i,0.25} + (\kappa N_{i,0.1} + \pi N_{i,0.25}) * Post\_dummy \\ &+ \varphi Post\_dummy + \sum_j \left(\lambda_j nbhd_j\right) + \sum_j \left(\rho_j Post\_dummy * nbhd_j\right) + \varepsilon_i \end{aligned} \quad (5)$$

where $N_{i,0.1}$ is an indicator that equals one if the property is located within 0.1 miles of at least one unconverted medical store and *no* conversions. $N_{i,0.25}$ is an indicator that equals one if the property is located between 0.1 and 0.25 miles of at least one unconverted medical store and *no* conversions. All other variables in Equation (5) are as defined in equation (1). As before, ψ is the price impact of being located near a retail conversion. However, we are also interested in whether we see a similar effect for non-conversions in 2014. Although these dispensaries are not converting, we will still consider a 2014 sale to be in the "post-treatment" period. Testing the null hypothesis that $\kappa = 0$ is analogous to a falsification test. If retail conversion has a causal impact on prices, then we should not see a price impact for stores that did not convert to retail.

The coefficient estimates from Equation (5) are reported in Table IX. Consistent with our previous results, houses located within 0.1 miles of a retail conversion experience an 8.4% increase in prices in 2014 relative to houses located further away. However, houses located near unconverted stores do not experience the same increase in prices in 2014. In fact, the estimate of $\hat{\kappa}$ – labelled "Within 0.1 miles of at least one unconverted store and no conversions x Post-retail" – is negative (-0.012) and not statistically different from zero. Moreover, a Wald test fails to reject the null

[38] We also estimate Equation 1 for each of the 34 neighborhoods where there existed sales in 2013 and 2014 within 0.1 miles and 0.1-0.25 miles for a retail conversion and find a positive treatment effect in 22 of those neighborhoods.

hypothesis that $\gamma = \tau$, but rejects the null hypothesis that $\psi = \kappa$ at the 10% level of significance. In other words, we find no evidence of a price differential between properties located near a future conversion and those near a non-converter in the pre-treatment period. However, those properties located near a retail conversion do experience a larger price increase – relative to those near a non-converter – after conversion. This provides evidence that it is the actual conversion to retail that is increasing neighboring property values.

As we stated in the introduction, the fact that Colorado only allowed existing medical stores to convert to retail helps us avoid the the siting decision since store siting occurred *before* implementation of RML. However, this does not necessarily solve for the siting problem when comparing retail conversions to unconverted stores since some of the existing stores in Denver did not convert to retail at the beginning of 2014.[39] A potential concern in comparing converting and non-converting stores is that the stores that converted to retail chose to do so because of underlying economic forces in that area that also positively impacted house prices. In other words, it is possible that retail conversions are located in areas that are significantly different from the areas where unconverted stores are located. If this is the case, then we would incorrectly attribute price changes to retail conversions when in fact they are due to other economic forces. We do not believe this a major concern for our study for the following reasons. First, conversions and unconverted stores are often located in close proximity. Figure 5 shows the locations of the retail conversions as well as non-converting medical stores at the beginning of 2014. Although some retail conversions do not have unconverted stores nearby (and vice versa), for our purposes the most important takeaway from Figure 5 is that many of the retail conversions do in fact have unconverted stores located in very close proximity. This suggests that the decision to convert to retail is not driven purely by the underlying economic fundamentals of the location. Second, the unconditional average house price for properties located near (future) retail conversions ($320,000) is not statistically significantly different from the average for properties located near an unconverted store and no conversions ($314,000) in the pre-treatment period. Moreover, we fail to reject the null hypothesis that $\gamma = \tau$ in Equation (5). Finally, even if retail conversions are located in high-growth areas relative to the non-converters, this would

[39] 103 out of 193 existing medical stores converted to retail at the beginning of 2014. Only one store that applied for a retail license in 2014 was denied the license (Colorado Department of Revenue Enforcement Division - Marijuana (2015)). Thus,the post-application approval process does not appear to play a large role in determining the location of retail conversions. Rather, the decision to convert seems to be under the purview of the business itself.

only be problematic for comparisons between retail conversions and unconverted stores. The siting decisions of medical stores (that eventually converted to retail) were made *before* implementation of RML, and the majority of our comparisons in this paper compare prices near retail conversions relative to properties located further away. Our test of the parallel trends assumption in Table V shows that prices near retail conversions trended similarly to properties located further away prior to 2014.

Economic Impact

One of the direct effects of the legalization of recreational marijuana sales is a new source of income from the taxation of these sales. In Denver, there are state taxes (a portion of which gets redistributed back to Denver) as well as city taxes and business fees on recreational marijuana sales. Marijuana revenue for the city of Denver was estimated to be around $22 million for 2014 and $29.55 million for 2015.[40] The local marijuana taxes levied in the Denver go toward the general fund and some of the money goes toward regulation, enforcement, public health, and education. Money has also gone toward building a recreation center.[41]

In addition to taxes levied directly on the recreational marijuana industry, property taxes represent a potential indirect channel through which RML impacts tax revenues for Denver. Given that after January 1, 2014, houses within 0.1 miles of at least one retail conversion increased in value by approximately 8.4%, we can provide a rough estimate of the increase in value for those houses as well as an estimate of increases in property tax revenue generated by the increases in value. In 2013 the average sales price for houses within at least 0.1 miles of a switching facility was approximately $320,000, which implies that a 8.4% increase would result in a $26,880 increase that can be attributed to the switch to recreational sales.

In the city of Denver, properties are appraised biennially (in every odd numbered year) and the appraisal uses recent sales as comparables. Thus, sales in 2014 will affect 2015 appraisals. The property tax rate for the city of Denver in 2015 was 78.127 mills and the assessed value is defined to be 7.96% of the appraised value.[42] Thus, for an average house within 0.1 miles of

[40] https://www.denvergov.org/content/denvergov/en/denver-marijuana-information/statistics/revenue-and-sales.html.

[41] http://www.denverpost.com/2016/05/26/marijuana-sales-tax-revenue-huge-boon-for-colorado-cities.

[42] https://www.denvergov.org/content/denvergov/en/treasury-division/property-taxes.html.

a retail conversion, the approximate increase in property tax revenue is $167. Although the full implications of RML on tax revenues in Denver is well beyond the scope of this study, our results suggest that retail marijuana can impact city tax revenue above and beyond the direct effect of taxing the recreational marijuana industry. Specifically, retail conversions can potentially increase property tax revenues for local governments as well.

Conclusion

In this paper we contribute to the debate on the impacts of recreational marijuana legalization on local communities by examining the effects of retail marijuana stores on nearby house prices in Denver, Colorado. This is an important question for voters, policy makers, and economists. Colorado presents an ideal environment to investigate this question because only existing medical marijuana facilities were allowed to sell recreational marijuana, which facilitates identification of the incremental effect of recreational marijuana. Using a difference-in-differences model, we compare houses that are in close proximity to a retail conversion to those that are slightly farther away from a retail conversion before and after the legalization of recreational sales. We find that after the law went into effect at the end of 2013, single family residences close to a retail conversion (within 0.1 miles) increased in value by approximately 8.4% relative to houses that are located slightly farther from a conversion (between 0.1 miles and 0.25 miles) in 2014 compared to the previous year. We perform a battery of robustness checks that confirm the positive relationship between retail conversion and nearby house values. In addition to sales and business taxes generated by the retail marijuana industry, the associated increase in property tax revenues represents another potentially appealing selling point for legalization.

In addition, we find the effect of retail conversion on house prices to be highly localized. Furthermore, the effect of retail conversion on nearby house prices in Denver is measured over a relatively short time period. Our analysis is silent on the long-term impacts of retail conversion. Importantly, we are agnostic as to the underlying cause of our results. While we hypothesize that some contributing factors may have included an increase in housing demand as a result of an increase in marijuana-related employment, lower crime rates, and additional amenities locating in close proximity to retail conversions, finding the specific channels that explain our results is an open puzzle that we leave for future research. To our knowledge, our paper is the first to address the relationship between retail conversions and house prices, which is important because several states are currently considering legalizing recreational marijuana and others are likely to follow.

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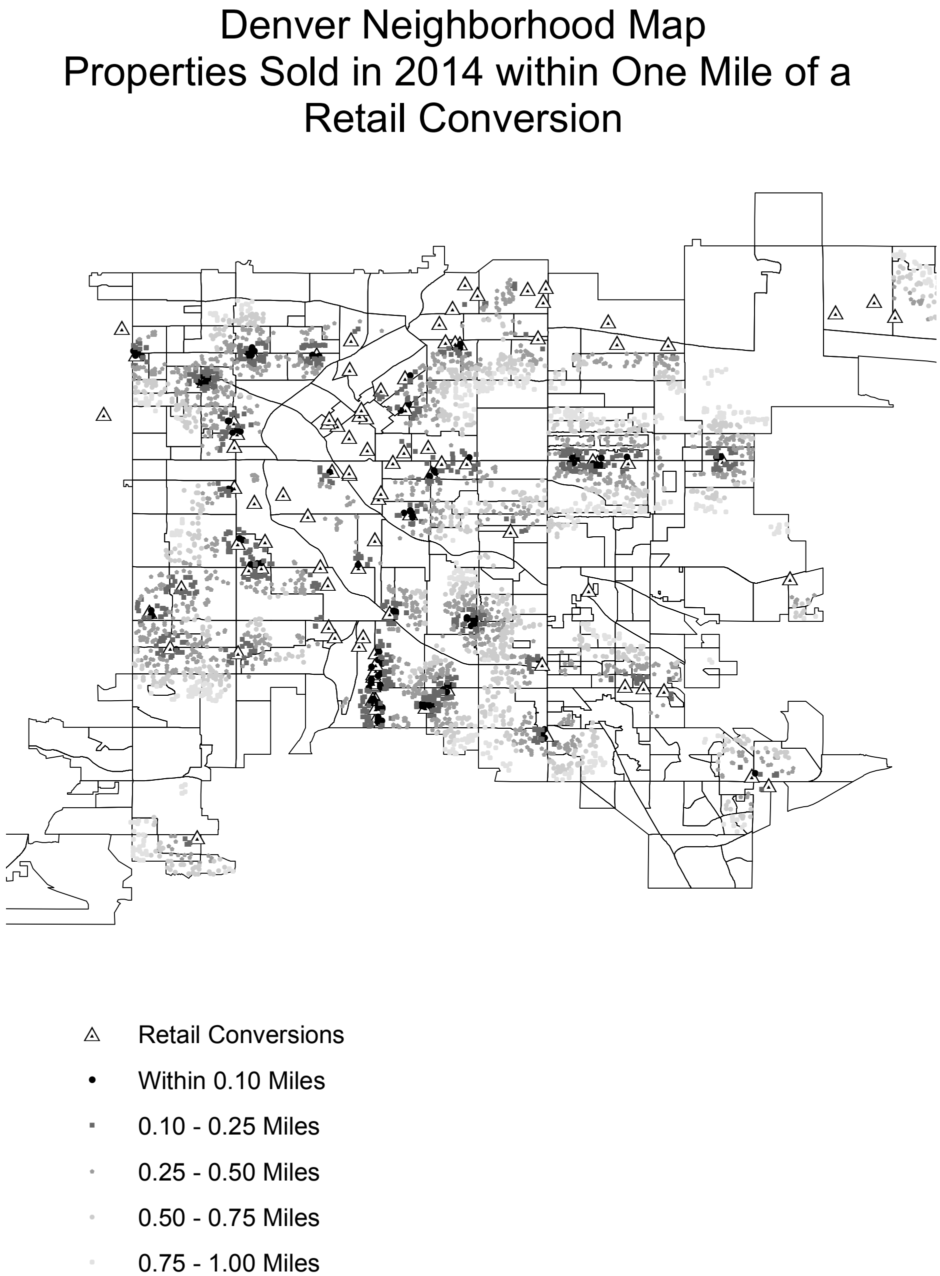

Figure 1: Sales within one mile of a retail conversion in 2014.

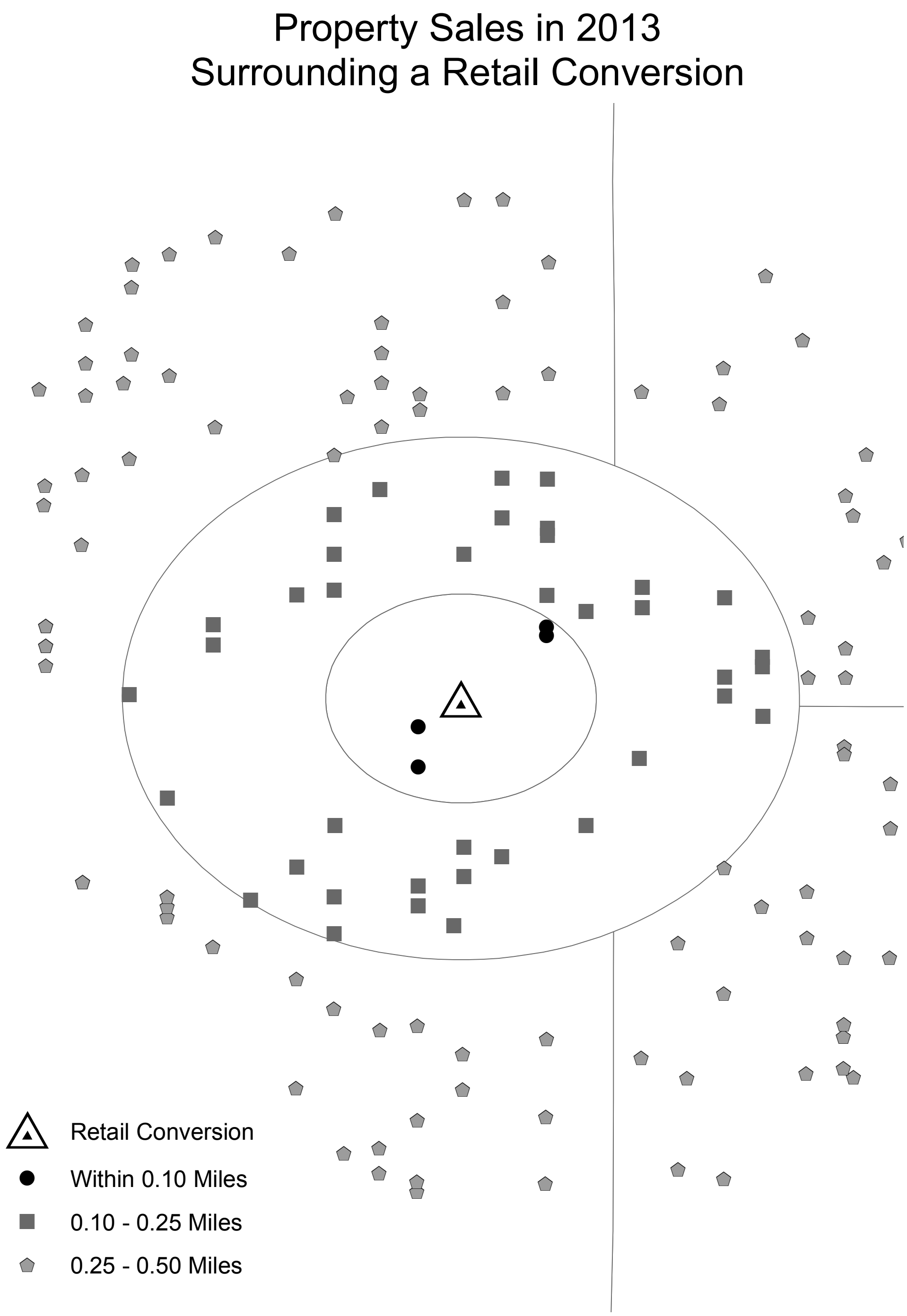

Figure 2: Sales located near Higher Ground MMC in 2013 (pre-conversion).

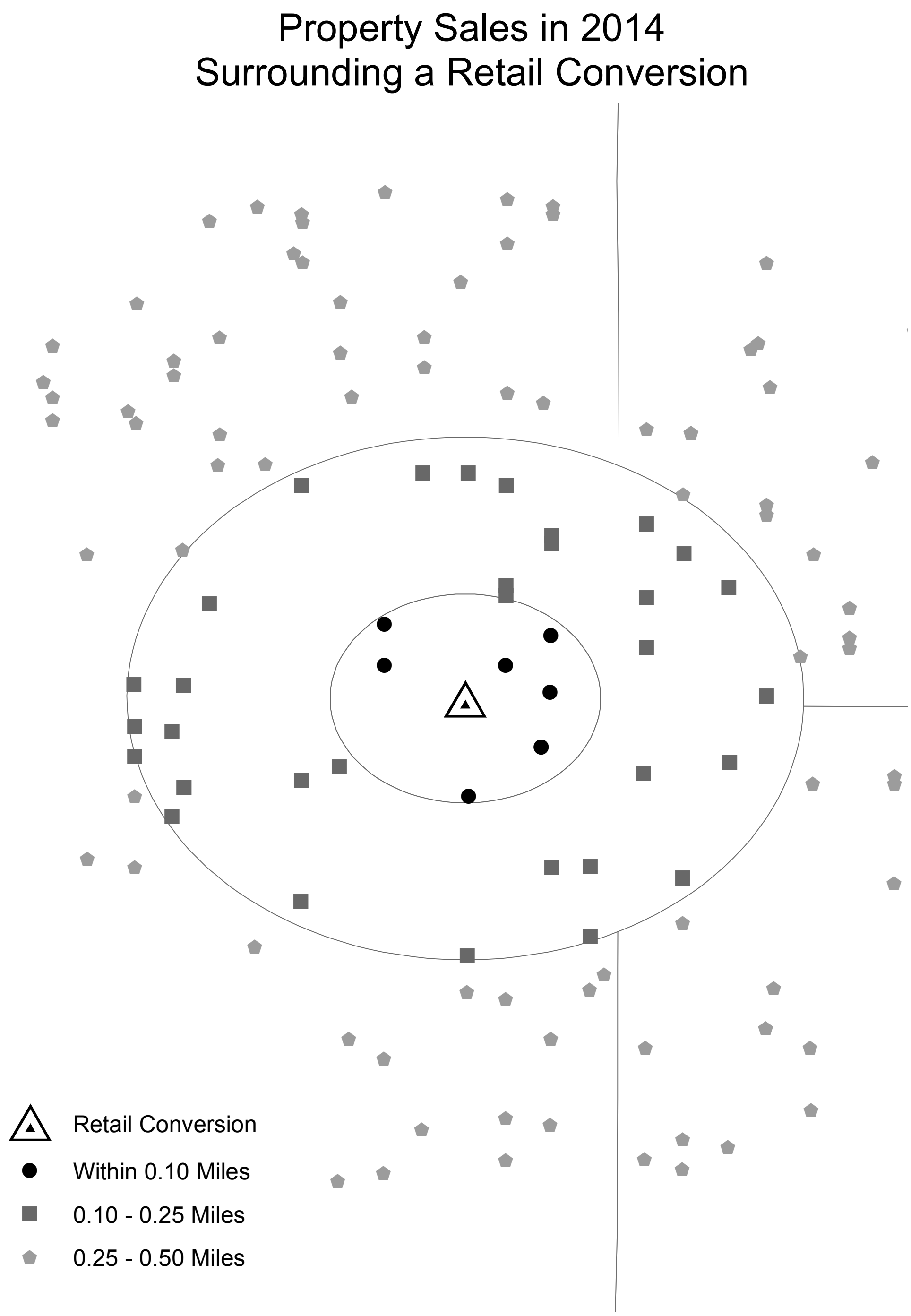

Figure 3: Sales located near Higher Ground MMC in 2014 (post-conversion).

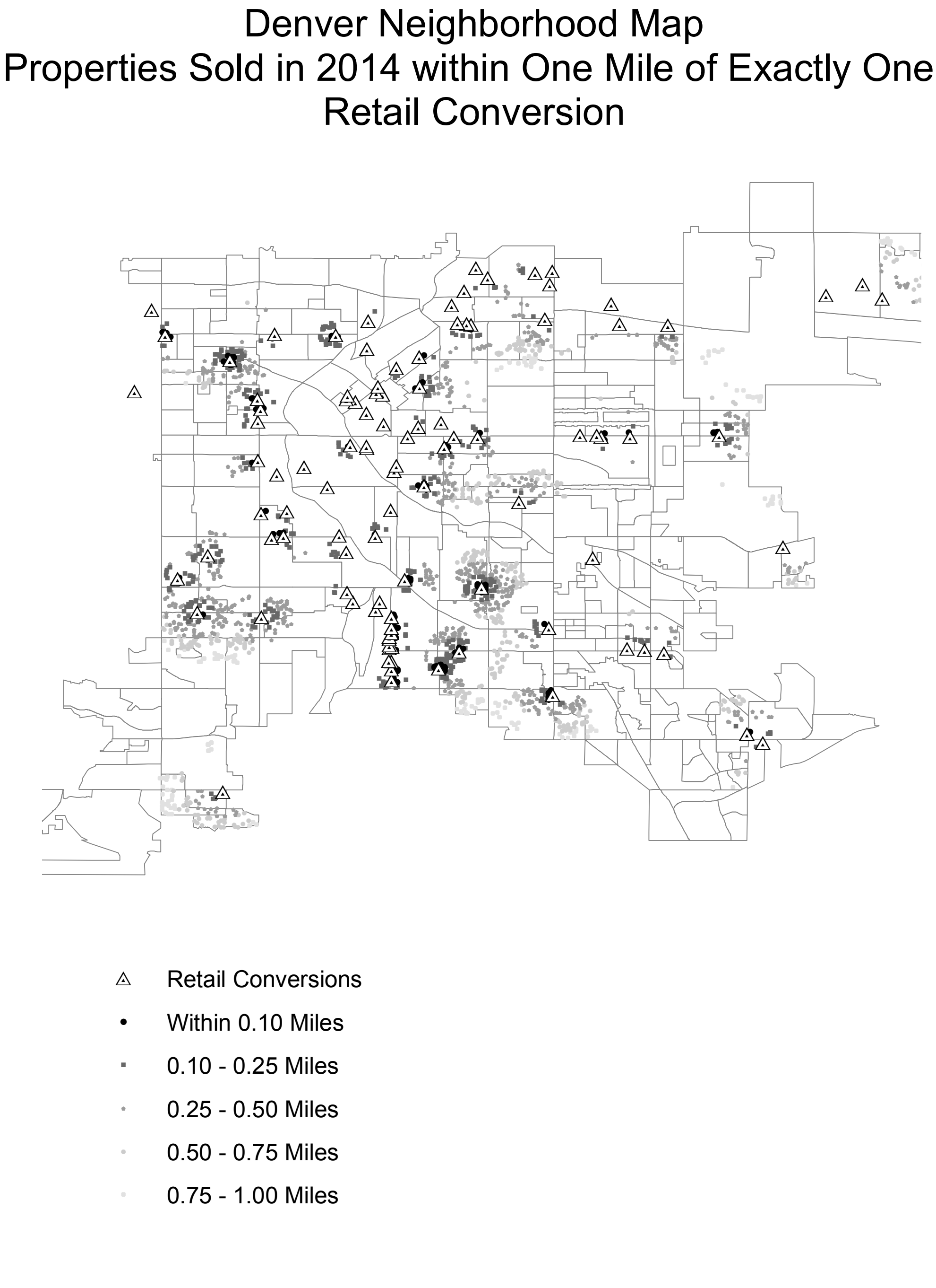

Figure 4: Sales within one mile of exactly one retail conversion in 2014.

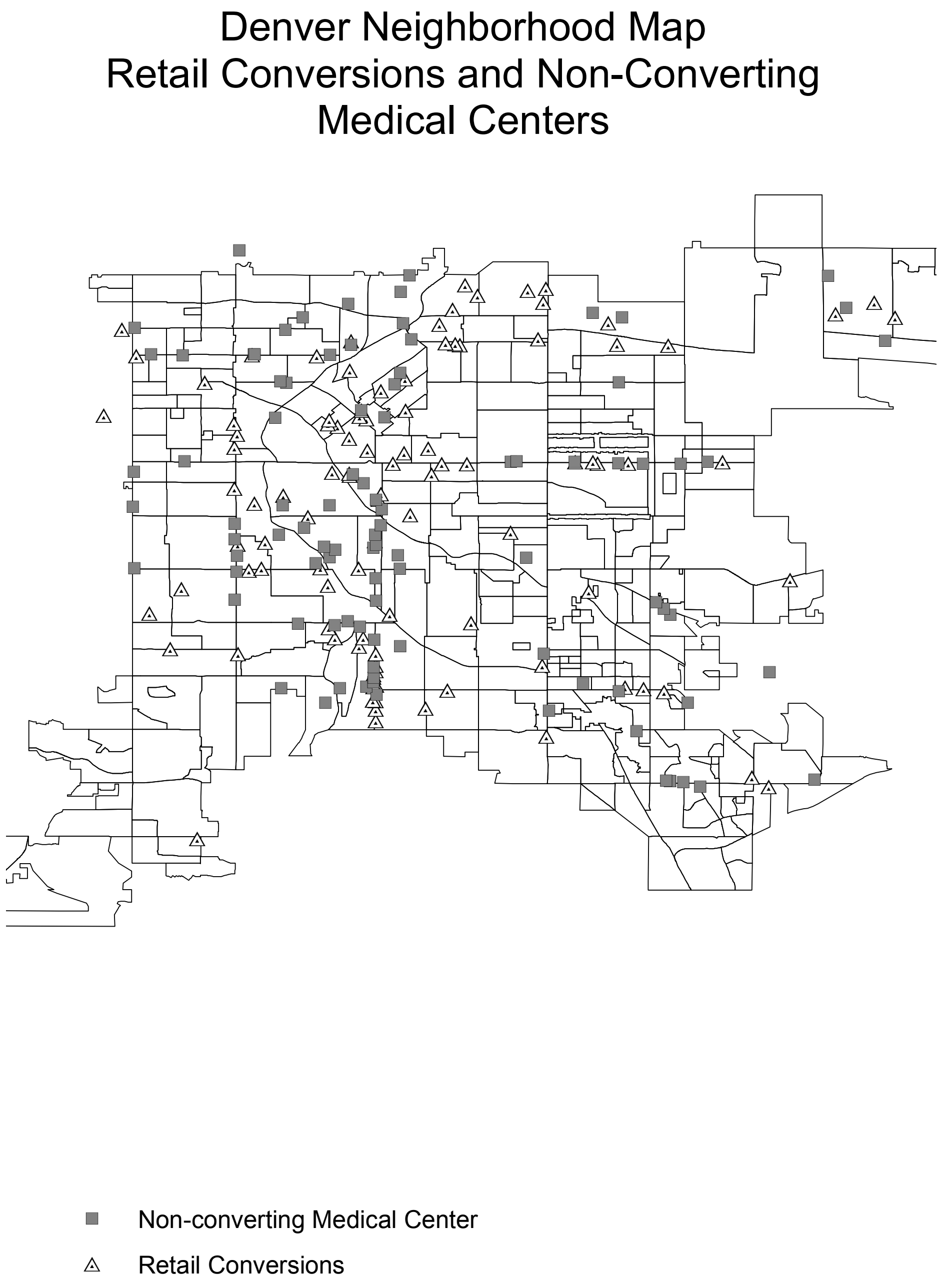

Figure 5: Retail Conversions and Non-Converting Medical Centers

Table I: Descriptive Statistics

VARIABLES	(1) Mean	(2) S.D.	(3) Min	(4) Max
ln Price	12.66	0.59	10.31	14.81
ln(Square Feet)	7.26	0.44	5.62	9.43
ln(Lot Area)	8.69	0.37	6.87	11.25
# Beds	2.81	0.81	1	8
# Baths	2.27	0.98	1	10.5
# Stories	1.38	0.50	1	3
Age	56.22	37.34	0	140
N	19,555			

Note: This table reports descriptive statistics for the sample of property transactions used in our analysis.

Table II: Transaction Prices at Different Distances from Retail Stores

	Obs.	Mean	S.D.	Min	Max
Ring 1					
Price (2013)	139	$319,963	$185,033	$60,000	$1,145,000
Price (2014)	160	$382,539	$190,591	$75,000	$1,268,000
Ring 2					
Price (2013)	751	$350,008	$233,924	$30,000	$2,700,000
Price (2014)	774	$376,016	$242,194	$34,190	$2,680,000
Ring 3					
Price (2013)	1794	$347,940	$231,703	$43,200	$2,042,700
Price (2014)	1704	$379,016	$263,501	$30,000	$2,595,000
Ring 4					
Price (2013)	1321	$417,779	$308,550	$33,000	$2,515,000
Price (2014)	1267	$445,019	$326,646	$50,000	$2,670,000
Ring 5					
Price (2013)	744	$432,835	$278,783	$35,000	$2,350,000
Price (2014)	626	$428,244	$299,903	$38,250	$2,500,000

Note: This table reports descriptive statistics for transaction price by year at different distances from medical marijuana locations that switched to retail locations: Ring 1 includes properties within 0.1 miles; Ring 2 includes properties within 0.1 - 0.25 miles; Ring 3 includes properties within 0.25 - 0.5 miles; Ring 4 includes properties within 0.5 - 0.75 miles; and Ring 5 includes properties within 0.75 - 1 mile.

Table III: Impact of Retail Marijuana on Property Values

VARIABLES	(1) ln Price
Within 0.1 miles of at least one store	-0.109***
	(0.034)
Within 0.1 miles of at least one store x Post-retail	0.084**
	(0.040)
Within 0.25 miles of at least one store	0.001
	(0.014)
Within 0.25 miles of at least one store x Post-retail	-0.001
	(0.018)
Post-retail	0.039
	(0.030)
Constant	8.444***
	(0.107)
Observations	19,555
R-squared	0.749
Property Characteristics	Yes
Neighborhood dummies	Yes
Neighborhood x year dummies	Yes

Note: This table reports coefficient estimates from equation (1). The sample includes properties sold in 2013 and 2014. Heteroskedasticity robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table IV: Relationship between property characteristics and treatment

VARIABLES	(1) ln(Square Feet)	(2) ln(Lot Area)	(3) # Beds	(4) # Baths	(5) # Stories	(6) Age
Within 0.1 miles of at least one store	-0.019	-0.035	-0.083	-0.002	0.079*	3.536
	(0.035)	(0.025)	(0.071)	(0.073)	(0.041)	(2.643)
Within 0.1 miles of at least one store x Post-retail	-0.047	0.010	0.046	-0.164	-0.121**	4.597
	(0.046)	(0.033)	(0.096)	(0.102)	(0.053)	(3.314)
Within 0.25 miles of at least one store	0.007	-0.002	0.006	-0.006	0.003	3.192***
	(0.016)	(0.012)	(0.036)	(0.037)	(0.018)	(1.177)
Within 0.25 miles of at least one store x Post-retail	-0.017	-0.015	-0.041	-0.032	-0.004	-2.731*
	(0.023)	(0.017)	(0.051)	(0.052)	(0.025)	(1.640)
Post-retail	0.066	-0.073	0.182	0.185	0.006	-0.036
	(0.059)	(0.055)	(0.169)	(0.177)	(0.078)	(1.754)
Constant	7.012***	8.794***	2.900***	2.089***	1.250***	29.564***
	(0.017)	(0.014)	(0.041)	(0.041)	(0.029)	(0.693)
Observations	19,555	19,555	19,555	19,555	19,555	19,555
R-squared	0.486	0.545	0.214	0.423	0.479	0.747
Neighborhood fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood x year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table reports coefficient estimates from equation (2). Heteroskedasticity robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table V: Impact of Retail Marijuana on Property Values

VARIABLES	(1) ln Price
Within 0.1 miles of at least one store	-0.104***
	(0.034)
Within 0.1 miles of at least one store x 2008	0.029
	(0.050)
Within 0.1 miles of at least one store x 2009	0.052
	(0.049)
Within 0.1 miles of at least one store x 2010	0.040
	(0.051)
Within 0.1 miles of at least one store x 2011	0.025
	(0.048)
Within 0.1 miles of at least one store x 2012	-0.026
	(0.050)
Within 0.1 miles of at least one store x 2014	0.083**
	(0.040)
Within 0.1 miles of at least one store x 2015	0.086*
	(0.045)
Within 0.25 miles of at least one store	0.000
	(0.014)
Within 0.25 miles of at least one store x 2008	-0.031
	(0.022)
Within 0.25 miles of at least one store x 2009	-0.023
	(0.022)
Within 0.25 miles of at least one store x 2010	-0.027
	(0.023)
Within 0.25 miles of at least one store x 2011	0.006
	(0.024)
Within 0.25 miles of at least one store x 2012	-0.000
	(0.022)
Within 0.25 miles of at least one store x 2014	-0.000
	(0.018)
Within 0.25 miles of at least one store x 2015	-0.015
	(0.020)
Constant	7.647***
	(0.062)
Observations	68,625
R-squared	0.787
Property Characteristics	Yes
Year dummies	Yes
Neighborhood dummies	Yes
Neighborhood x year dummies	Yes

Note: This table reports coefficient estimates from equation (1). The sample includes properties sold in 2013 and 2014. Heteroskedasticity robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table VI: Impact of Retail Marijuana on Property Values Excluding Distant Locations

VARIABLES	(1) ln Price	(2) ln Price
Within 0.1 miles of at least one store	-0.100***	
	(0.034)	
Within 0.1 miles of at least one store x Post-retail	0.079*	
	(0.041)	
Within 0.1 miles of exactly one store		-0.127**
		(0.050)
Within 0.1 miles of exactly one store x Post-retail		0.114*
		(0.060)
Post-retail	0.001	0.012
	(0.080)	(0.082)
Constant	8.346***	8.317***
	(0.317)	(0.378)
Observations	1,888	1,100
R-squared	0.766	0.819
Property Characteristics	Yes	Yes
Neighborhood dummies	Yes	Yes
Neighborhood x year dummies	Yes	Yes

Note: This table reports coefficient estimates from equation (4). The sample includes only properties located within 0.25 miles of a store. Heteroskedasticity robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table VII: Impact of Retail Marijuana on Property Values with Different Control Groups

VARIABLES	(1) ln Price	(2) ln Price	(3) ln Price
Within 0.1 miles of at least one store	-0.107***	-0.103***	-0.101***
	(0.033)	(0.033)	(0.033)
Within 0.1 miles of at least one store x Post-retail	0.090**	0.081**	0.078**
	(0.039)	(0.039)	(0.039)
Post-retail	0.096*	0.088	0.088
	(0.056)	(0.054)	(0.054)
Constant	8.328***	8.671***	7.716***
	(0.201)	(0.140)	(0.159)
Observations	6,070	10,129	12,683
R-squared	0.758	0.770	0.777
Property Characteristics	Yes	Yes	Yes
Neighborhood dummies	Yes	Yes	Yes
Neighborhood x year dummies	Yes	Yes	Yes

Note: This table reports coefficient estimates from equation (4) with different distances used for the control group. The control group in column (1) includes properties that are further than 0.1 but within 0.5 miles of at least one retail conversion. The control group in column (2) includes properties that are further than 0.1 but within 0.75 miles of at least one retail conversion. The control group in column (3) includes properties that are further than 0.1 but within 1 mile of at least one retail conversion. Heteroskedasticity robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table VIII: Impact of Retail Marijuana on Property Values with an Expanded Definition of Treatment

VARIABLES	(1) ln Price
Within 0.25 miles of at least one store	-0.021
	(0.014)
Within 0.25 miles of at least one store x Post-retail	0.023
	(0.019)
Post-retail	0.085
	(0.058)
Constant	8.314***
	(0.201)
Observations	6,070
R-squared	0.758
Property Characteristics	Yes
Neighborhood dummies	Yes
Neighborhood x year dummies	Yes

Note: This table reports coefficient estimates from equation (4) with an expanded definition of treatment. The treatment group includes properties located within 0.25 miles of at least one retail conversion and the control group is the next adjacent ring (properties located between 0.25 and 0.5 miles from at least one retail conversion). Heteroskedasticity robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Table IX: Impact of Retail Marijuana on Property Values for Retail Conversions and Medicinal Stores that did not Switch to Retail

VARIABLES	(1) ln Price
Within 0.1 miles of at least one store	-0.117***
	(0.034)
Within 0.1 miles of at least one store x Post-retail	0.084**
	(0.040)
Within 0.25 miles of at least one store	-0.007
	(0.014)
Within 0.25 miles of at least one store x Post-retail	-0.001
	(0.019)
Within 0.1 miles of at least one unconverted store and no conversions	-0.054*
	(0.031)
Within 0.1 miles of at least one unconverted store and no conversions x Post-ret	-0.012
	(0.042)
Within 0.25 miles of at least one unconverted store and no conversions	-0.038**
	(0.016)
Within 0.25 miles of at least one unconverted store and no conversions x Post-re	0.004
	(0.022)
Post-retail	0.038
	(0.030)
Constant	8.455***
	(0.107)
Observations	19,555
R-squared	0.749
Property Characteristics	Yes
Neighborhood dummies	Yes
Neighborhood x year dummies	Yes

Note: This table reports coefficient estimates from equation (1) with additional controls for properties near non-converting medical medical marijuana stores. Heteroskedasticity robust standard errors are reported in parentheses. ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.