Getting High or Getting Low?

The External Effects of Coffeeshops on House Prices

Abstract

The legalization of cannabis is a hotly contested policy topic. While beneficial to some, cannabis dispensaries may create negative externalities for others. This paper studies the external effects of coffeeshops – Dutch cannabis sales facilities – on local house prices. We employ a difference-in-difference analysis and a repeat sales analysis around a change in regulation, leading to exogenous coffeeshop closings. We document that coffeeshop closings have negative effects on home values. Compared to homes nearby remaining coffeeshops, homes nearby closing coffeeshops decrease on average 1.6 to 7.8 percent in value. The findings are robust to sub-tests and unaffected by the subsequent use of coffeeshop location.

Keywords:

Externalities, Housing Markets, Residential Real Estate, Cannabis, Crime

JEL codes:

D62, H23, R21, R23, R52

1 Introduction

Policy makers around the globe are changing their attitudes towards cannabis consumption, resulting in decriminalization, toleration, and even legalization policies. The World Health Organization (WHO) recently recommended to remove cannabis from the UN list of 'particularly harmful' substances.[1] Countries like Canada, Uruguay, as well as several U.S. states have recently legalized recreational cannabis.[2] In some other countries, including Portugal and the Netherlands, cannabis is decriminalized, meaning it remains illegal, but charges are usually not enforced.[3]

The motives for these changes in policies are manifold, including lack of evidence for cannabis-related crimes, fighting organized crime, and negative cost-benefit relationships of prosecution (Charilaou et al., 2017). However, little scientific evidence exists about the potential effects of toleration and legalization on society. Legalization produces new industries, providing employment opportunities and tax income. On the other hand, cannabis consumption might increase (Jacobi & Sovinsky, 2016), potentially affecting long-run health care costs and/or productivity (Marie & Zölitz, 2017).[4] Since the number of cannabis dispensaries and related businesses increases, local residents are exposed to cannabis consumption, whether they share a liberal view on cannabis or not. In a recent court case, a Coloradan couple argued that their property lost value due to the opening of a nearby cannabis growing site, creating "pungent, foul odors".[5]

Empirical evidence on the external effects of local cannabis facilities is mixed, but mostly focused on crime. Hunt et al. (2018) document a slight increase in "driving under the influence" (DUI) arrests after dispensaries´ openings, in addition to reduced

[1] Retrieved March 2018 from: `https://bit.ly/2Ul40w8`

[2] In example: `https://reut.rs/2TlbMZF`

[3] Cannabis remains illegal under European Union law, which has primacy over national laws.

[4] Some studies show significantly positive effects on usage after cannabis decriminalization (Cerdá et al., 2012; Pacula et al., 2010; Wall et al., 2011), while others find no significant effects (Anderson & Rees, 2014; Chu, 2015; Harper et al., 2012; Lynne-Landsman et al., 2013; Morris et al., 2014).

[5] `https://dpo.st/2AxB5NG`

crime rates. Focusing on the effect of dispensary *closings* on local crime, Chang and Jacobson (2017) document *higher* crime rates nearby in the short run. As the authors document similar effects for restaurant closings, they argue that retail activities are generally better than vacancy.

So far, only Conklin et al. (2017) and Cheng et al. (2018) examine the effect of legal cannabis dispensaries on property prices. Using the same research area and period, these sister studies examine the change of medical to recreational cannabis dispensaries in Denver, Colorado.[6] Both studies find a 6 to 8 percent increase in housing values for properties nearby dispensaries that switch from medical to recreational cannabis sales. These findings are contrary to previous studies examining the external effects of illegal drug sites, which found negative local house price effects.[7]

This paper adds to the ongoing debate regarding the societal effects of a less stringent cannabis policies, examining the implications of Dutch cannabis dispensaries, so-called ”coffeeshops”, on nearby property prices. Our study is the first to examine coffeeshop *closings*, following a recent exogenous regulatory change, in combination with a large micro-level database on house transaction prices. Studying exogenous closing events alleviates methodological concerns that hamper studies of the opening or presence of coffeeshops, such as endogenous location choice or anticipation effects. We build on the study of Chang and Jacobson (2017), who also look at cannabis dispensary closings, but only consider crime rates, one potential external effect.

To examine the effect of exogenous coffeeshop closings on nearby property prices, we employ a policy change. Closings were carried out in different waves between 2009 and 2017, providing substantial variation over time.[8] This empirical setting

[6] Cheng et al. (2018) consider a bigger research area but measure municipality level data, whereas Conklin et al. (2017) focus on a more homogeneous sample using property transactions.

[7] Dealy et al. (2017) and Congdon-Hohman (2013) examine property prices nearby revealed meth labs, documenting property price discounts of 6.5 to 19 percent.

[8] The municipalities of The Hague and Rotterdam were the first to implement the school distance criterion, inspiring other municipalities and the national government.

provides an exogenous closing shock, independent of neighborhood perception and time-confounding factors, allowing for clean identification of the effects of cannabis dispensaries on local house prices.

We employ a sample of 115,248 housing transactions between 2000 and 2017 for the three biggest Dutch cities, Amsterdam, Rotterdam and The Hague, reflecting approximately 75 percent of all transactions in these cities. Besides transaction price, the dataset contains extensive information on dwelling characteristics, such as address, type, size, state of repair and time on the market. Furthermore, we have location and status information on all coffeeshops that operated in the Netherlands since 1999, 44 percent of which are located in the three major Dutch cities. We also have information on all school distance-related closings for each of these cities.

Compared to properties in the vicinity of coffeeshops that remain open, our difference-in-difference results show a closing discount of 1.6 to 7.8 percent for homes nearby closing coffeeshops, with the effect getting larger when homes are nearer. This result is robust while controlling for the presence of potential nuisance-generators like local bars and nightclubs, and the effect remains after we include different holdout periods to control for potentially sticky prices in local housing markets. The results for the repeat sales analysis, in which we compare prices of the same dwellings before and after coffeeshop closings, have the same direction and magnitude. We find a price decrease of 10.8 percent. If we control for time after closing, the initial effect becomes bigger, but it subsequently decreases over time.

The remainder of the paper will first outline Dutch government policies with respect to cannabis sales, including a detailed discussion of the coffeeshop closing rules related to school proximity that have been introduced in the last decade. Section 3 provides information regarding our dataset of housing transactions and the coffeeshop locations in Amsterdam, Rotterdam, and The Hague. The difference-in-difference and repeat sales methodologies, as well as the main results are presented in Section 4, and section 5 will explore potential causation channels. The paper ends with a short concluding section.

2 Coffeeshops in the Netherlands

2.1 Government Policy on Coffeeshops

In 1976, the Netherlands was the first country in Europe that made cannabis usage, possession and sale effectively legal.[9] The intention of the policy was to *"reduce the risk of cannabis users being exposed to hard drugs"*, such as cocaine and heroin (Wouters et al., 2012). In addition, the government wanted to reduce punishment of soft drug users. Even though cannabis possession is still officially illegal today, possession violations up to 5 grams are not enforced (MacCoun & Reuter, 1997). In order to officially control the sale of cannabis, the government legally tolerated selling facilities, so called "coffeeshops". Since 1991, coffeeshops have to fulfil five criteria to stay open: no sales to minors, no sale of hard drugs, no advertising, no public nuisance, and restricted sales per person per day (Bieleman et al., 2015a; MacCoun & Reuter, 1997; Tops et al., 2001).[10]

Coffeeshops were opened all over the Netherlands, reaching their peak between 1991 and 1995 with around 1,500 coffeeshops in the country (Bieleman et al., 1996). Neighboring countries complained about the supply opportunities just across the border and local politicians equally complained about nuisance from coffeeshops and their customers. In order to manage the situation, the Opium Act, the Dutch law regarding drugs, was changed in 1999, providing local politicians with more legislative power against coffeeshops. Municipalities could reduce tolerance of coffeeshops if they saw fit, allowing them to add operating criteria, to withdraw licenses, and to

[9] Officially, cannabis usage is just tolerated as it remains illegal under EU law.

[10] The criteria were tightened over time, increasing the minimum age from 16 to 18, lowering the maximum amount per person per day and setting the maximum amount of supply per shop to 500 grams (Bieleman et al., 2015a).

ultimately close coffeeshops (Bieleman et al., 2015a).

The law change resulted in a reduction in the number of coffeeshops (Tops et al., 2001) even as the number of municipalities with active coffeeshops hardly changed. By 2015, 582 coffeeshops remained. While many cities aimed to close coffeeshops, others added additional operating restrictions. One example of a restriction is the ban on simultaneous sales of alcohol and cannabis, leading to the closing of hasjcafés, a facility similar to a coffeeshop, but more focused on hospitality aspects. The criterion was later adapted nationally (Municipality of Amsterdam, 2007). Especially cities along the German and Belgium border attempted to reduce drug tourism, by restricting the sale of cannabis to local citizens only. However, local coffeeshops legally opposed the restrictions, arguing that they involve discrimination, and won the case (Marie & Zölitz, 2017; van Ooyen-Houben et al., 2016).

In recent years, policies on coffeeshops became stricter, trying to tackle the so called "backdoor problem". In contrast to the strictly regulated retail trade of cannabis by coffeeshops (the "front door"), the cannabis supply chain ("the backdoor") is not regulated and still mostly illegal. Private cannabis cultivation is illegal in the Netherlands and legally provided cannabis does not match the sales amounts of coffeeshops. Therefore, nearly all coffeeshops source their cannabis from illegal dealers, from within or outside the country, supporting (organized) crime (Bieleman et al., 2015a; Leydon, 2014).

In 2003, a new law was implemented, aiming to halt coffeeshops' illegal activities. Among others, it gives local politicians the power to perform random screens and raids on coffeeshops in the case of suspicion. However, the law is contentious, since it might have been used as a pretence to close coffeeshops for other reasons (e.g. in gentrification projects). Additionally, the "backdoor problem" is still prevalent (Leydon, 2014).

2.2 Effects on the Community

The main reason for the liberal policy on coffeeshops is to protect soft drugs users from hard drugs by controlling cannabis sales. Although there is no direct empirical evidence for the effect of this policy on hard drug usage rates, there are some studies showing that coffeeshop availability decreases the likelihood of illegal cannabis sourcing, thereby decreasing the risk of hard drugs exposure. Conducting a survey among 773 cannabis users, Wouters and Korf (2009) document that, in cities with fewer coffeeshops, cannabis users, especially males and minors, are more likely to buy from illegal dealers.

On the other hand, the presence of coffeeshops might increase soft drug usage, potentially causing negative externalities on society. Investigating the effect of nearby coffeeshops on soft drug usage, Wouters et al. (2012) find no evidence of more cannabis users in coffeeshop proximity. However, users buying in coffeeshops consume cannabis more frequently and in higher amounts. Studying long-term usage effects of the Dutch policy on drugs, Tops et al. (2001) notice that the lifetime prevalence of cannabis use increased by 13.1 percent between 1987 and 1997, which corresponds with the timing of the growth in the number of coffeeshops. These results are in line with MacCoun and Reuter (1997), who compare countries' policies on drugs and show that the commercialization of cannabis access correlates with growth in the drug-using population.

Coffeeshops are disputed in Dutch society, as they seem to be a source of negative external effects, such as from drug consumers and tourists, crowding and creating noise, as well as traffic and odor-related nuisance. Illegal drug dealers sometimes loiter in the area, acting as competition or circumventing daily sales limits. And as discussed, the cannabis wholesale business remains illegal, making supply chains partly illegal and therefore relating coffeeshops to organized crime. Appendix Figure A presents street-scene impressions of coffeeshops.

Surveying the neighbors of coffeeshops in Rotterdam regarding specific nuisance externalities, Bieleman et al. (2010) identify smell, noise, traffic, and groups of loitering teenagers as the main problems. They report that nuisance from soft and hard drug users are higher around coffeeshops compared to other neighborhoods of Rotterdam. Based on survey participants perception, theft and vandalism-related crimes are higher as well.

Local coffeeshop associations claim that coffeeshops operate according to national businesses standards, contributing equally to the local economy and creating positive economic spillover effects. Coffeeshops are profitable businesses with an estimated total revenue of €1 billion, or €1.7 million per shop on average, in 2008.[11] Based on these estimates, coffeeshops pay more than €200 million in annual taxes. Additionally, according to the Maastricht association of coffeeshops, local drug tourists in 2008 spent €140 million in other local businesses, such as restaurants.[12]

2.3 The Distance Criterion

In the early 2000s, several municipalities contemplated to restrict the presence of coffeeshops around schools to protect children and teenagers from drug usage. The city of The Hague proposed a distance criterion (*afstandscriterium*) already in 2007, forcing coffeeshops within a linear distance of 500 meters from secondary schools to close (Municipality of Amsterdam, 2007).[13] However, due to opposition it took time for implementation and the effective distance was reduced to 250 meters, leading to the closure of one coffeeshop in January 2009.

The national government proposed to implement the distance criterion all over the country as of January 2014. However, municipalities were free to adapt the

[11] There are no official numbers, these were estimated by a national newspaper: `https://bit.ly/2T9fGpe`. Other estimations range from €800 million to €1.2 billion, as mentioned in the article.

[12] Retrieved 2017 from: `https://bit.ly/2SDoD4I`. Nevertheless, the city of Maastricht banned tourists from coffeeshops permanently, by permitting access only to local residents.

[13] There are two types of schools in the Netherlands: Primary (basis) schools and secondary (VO) schools. Secondary education starts at the age of 12 and lasts until age 16 to 18.

distance criterion and to change its specifications, such as distance. The government proposed to close coffeeshops within 250 meters of secondary schools and coffeeshops with visible shopfronts around primary schools (Bieleman et al., 2015a). Among 103 municipalities that tolerate coffeeshops, 78 implemented the criterion formally, of which 43 used the proposed criteria. By the beginning of 2015, 44 coffeeshops were affected by the criteria, mostly located in Amsterdam and Rotterdam (Bieleman et al., 2015a, 2010).

Amsterdam and Rotterdam handled the situation quite differently. The city of Rotterdam can be considered as a forerunner regarding the policy, advocating for it since the beginning and closing coffeeshops as of June 2009, shortly after The Hague.[14] In contrast, the city of Amsterdam was rather critical towards the criterion and instead considered a new access control system to prevent minors from entering.[15] The city hesitated to close the 27 coffeeshops affected by the school distance criterion, but implemented the criterion slowly in four stages, stepping up restrictions at every stage.[16]

Stage one was implemented as of January 1st, 2014 and restricted the opening hours of all 27 coffeeshops nearby schools, allowing them to only open after schools´ closings (6 pm on weekdays). In July 2014, stage two became effective, closing eight coffeeshops that were in visibility of schools. In January 2015, stage three became effective, closing three coffeeshops that were located within 150 meters walking distance of schools. One shop had to close in April 2015 and one shop was eventually closed due to law violations, instead. After a forced break due to resistance of the local coffeeshop lobby, stage four became effective in January 2017, resulting in the closing of eight additionally coffeeshops within 250 meters of schools. Due to moving

[14] One of the key advocates during this time was Ivo Opstelten, mayor of Rotterdam (1999 - 2009) and later Minister of Security and Justice (2010 - 2015), among others responsible for the national policy on coffeeshops.

[15] This so called ”weed pass” was not implemented at the end.

[16] `https://bit.ly/2NACEiM`

plans of a local school, six shops were reassessed and given time until July 2017, when one shop had to close and five were allowed to stay open.

3 Data

3.1 Data Sources

Our initial dataset consists of all open and closed coffeeshops in the Netherlands. We retrieve information on all coffeeshops from the *Amsterdam Coffeeshop Directory* in July 2017. Despite its name, this directory provides information on all coffeeshops in the Netherlands.[17] The database goes back to 1997 and is maintained and used mainly by cannabis users. It contains information on coffeeshop address and opening status, but does not contain information on closing reasons and dates. Since closed shops are removed after a while, we only observe recently closed coffeeshops.

Table 1 provides an overview of the number of coffeeshops in our sample, showing that Amsterdam does not only have the most coffeeshops, but also the most school distance related closings. The table also shows that Dutch coffeeshops are concentrated in the three biggest cities: Amsterdam, Rotterdam, and The Hague.

Since our data source does not provide information on closing dates, we contacted municipalities individually. As described in section 2.3, different municipalities implemented the distance criterion differently. To verify our data, we contact the ten biggest cities to get information on coffeeshops and closing dates. We confirm that only the three major cities Amsterdam, Rotterdam and The Hague experienced closings due to the distance criterion, and we therefore concentrate our analysis on these three cities.

[17] `http://www.coffeeshopdirect.com/index.htm`

Table 1
Coffeeshop Sample Overview (2000 - 2017)

City	Number of Coffeeshops: Open	Number of Coffeeshops: Closed	Closed due to distance criterion	Total
Amsterdam	171	166	21*	337
The Hague	55	24	1**	79
Rotterdam	38	30	17	68
Others	345	144	-	489
Total:	609	364	39	973

Notes: Number of coffeeshops in our sample, including opening status per July 2017 (after last closing wave). Shown are the three biggest cities in terms of number of coffeeshops, focus of our analysis. * Initially, 27 shops were affected, but one closed due to law-violations and five did not have to close as the nearby school moved away. ** One coffeeshop was affected by the 250 meters criteria. However instead of closing, it was moved to a different location, which became available due to the law-related closing of another shop. We still consider the closing location in our analysis.

The underlying housing dataset consist of transactions across the Netherlands and comes from the Dutch Realtors Association (NVM), representing a market share of around 75 percent. The Dutch Realtors Association is a network of realtors, storing an extensive data set on Dutch housing transactions. In our analysis, we use transactions from 2000 up to and including 2017, covering the full period of school distance related coffeeshop closings.[18] Our final data set consists of 115,248 housing transactions in Amsterdam, Rotterdam and The Hague, representing 44 percent of the national dataset. For each transaction, we have detailed information on location, transaction price, time-on-the-market, housing type, structural characteristics, and quality assessments from realtors, leading to a large set of control variables.

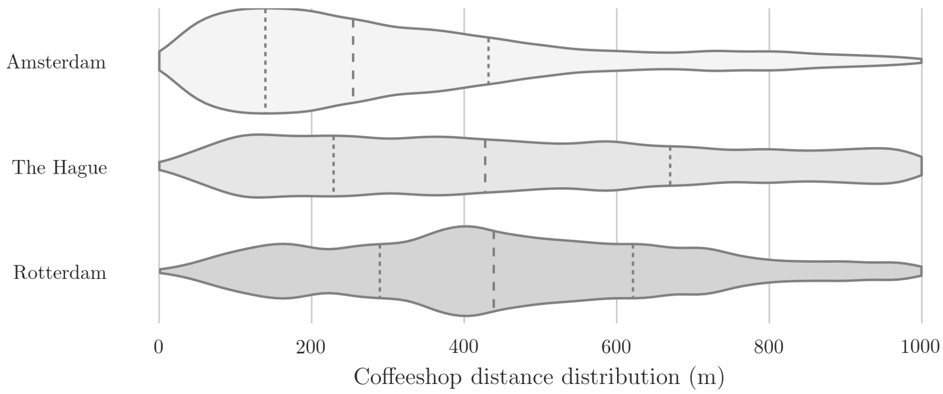
3.2 Sample Selection and Descriptive Statistics

To account for potential external effects of coffeeshops on the neighborhood, we use the linear distance between the coffeeshops and transacted homes as a proxy for

[18]To exclude potential outliers, we remove the highest and lowest 1% of observations based on transaction price.

the net effect of externalities. We geocode all housing transactions and coffeeshop locations to obtain information on latitude and longitude. Figure 1 shows the distance distribution of observations and nearest coffeeshop in our three cities (up to 1,000 meters). We document a high density of coffeeshops in these cities, with average housing-coffeeshop distances lying between 250 meters and 400 meters.

Figure 1
Property-Coffeeshop Distance Distribution



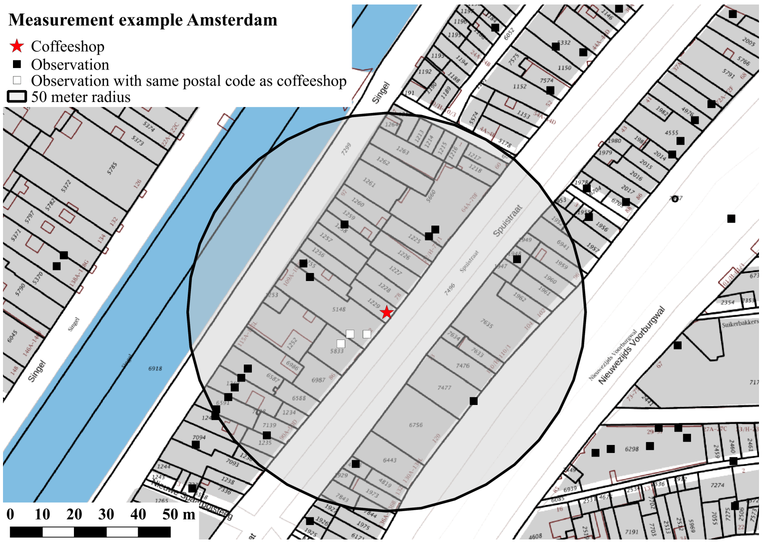
Notes: This graph shows the distance distribution of sample observations in quantiles (dashed lines), considering properties up to 1,000 meters coffeeshop distance.

Since the determination of an externality cut-off distance is rather arbitrary, we choose different distances supported by three sources. First, Conklin et al. (2017) find significant external effects for cannabis dispensaries up to a cut-off distance of 0.1 miles or 161 meters. Second, as discussed in section 2.3, all municipalities that enforced the distance criterion to coffeeshops used at least 150 meters as a cut-off distance (Bieleman et al., 2015a). Third, Figure 1 shows the first quartile of the house-coffeeshop distance distribution at a distance of 160 meters to 250 meters. Based on these three observations, we choose 150 meters as an externality cut-off distance.

Due to the high density of coffeeshops, we can also employ smaller cut-off distances: 100 meters, 50 meters, as well as the 6-digit postcode level. For the 6-digit postcode level, we consider a transaction to be within externality distance if it shares the postcode with a coffeeshop. In urban areas in the Netherlands, a 6-digit postcode is usually shared by half-a-street (around 17 households), ensuring direct visibility. Figure 2 illustrates the distance definitions, using a sample of observations over a land registry map of Amsterdam and showing a 50 meters radius around a coffeeshop (star), as well as the reach of the postcode matching (white squares). A detailed breakdown per cut-off distance per city is shown in Table 2, documenting how the number of observations becomes smaller with proximity.

Appendix Figure C illustrates the location of coffeeshops within the three biggest cities, Amsterdam, Rotterdam, and The Hague. We document that coffeeshops are generally located in the city center. In order to examine the distribution with respect to income and social status, we use the share of social benefit ("welfare") recipients as provided by the Dutch Statistics Office as a proxy, as local income statistics are not available at a granular level. We document that coffeeshops in Rotterdam and The Hague are likely to be located in neighborhoods with a high share of social benefit recipients, whereas coffeeshops in Amsterdam tend to be located in the city center, which is rather affluent, and has a lower share of social benefit recipients. Outside of Amsterdam's city center, however, coffeeshops are mostly located in poorer neighborhoods.

Figure 2
Illustration of Clustering



Notes: This graph illustrates the cut-off distance relations. In the illustrated case, we consider all observations (black) within the 50 m radius of a coffeeshop (star) as affected by external effects. Observations in white, share the same 6-digit postal code as the coffeeshop, ensuring direct visibility.

Table 2
Observations per distance, area and property type

	within 150 m	within 100 m	within 50 m	same postcode
Amsterdam				
Apartments	23,838	13,656	4,725	1,075
Houses	880	553	220	63
The Hague				
Apartments	4,660	2,387	628	150
Houses	985	504	167	46
Rotterdam				
Apartments	2,929	1,244	283	108
Houses	342	196	67	8

Notes: This table documents the number of property transactions within the different cut-off distances. Due to additional filters, such as time-periods, the number of homes in the analysis might decrease.

To get a general overview of the characteristics of properties in our sample, Table 3 summarizes property characteristics of single-family houses and apartments nearby coffeeshops ($d < 150m$), compared to observations further away (up to 500 meters). There are relatively more apartments than houses nearby coffeeshops, potentially due to the central locations. Homes and apartments do not differ significantly in size and maintenance quality (inside and outside) from observations further away. Homes and apartments nearby coffeeshops are more expensive than homes and apartments further away, both in price and price per square meter.

Table 3
Descriptive Statistics Housing Transactions
Amsterdam, Rotterdam, The Hague (2000 – 2017)

	Externality group (<150m)		Further away (150 m - 500 m)	
	Apartments	**Houses**	**Apartments**	**Houses**
No. of observations:	31,427	2,207	75,317	6,297
Size 79	159		82	151
(in m^2) [32]	[71]		[31]	[70]
Price	286,556	445,898	259,352	381,670
(in Euro)	[138,075]	[217,094]	[138,412]	[209,778]
Price per m^2	3,755	3,003	3,256	2,608
(in Euro)	[1,294]	[1,326]	[1,319]	[1,078]
Housing inside quality	7.28	6.66	7.14	6.80
(1 = worst, 9 = best)	[1.22]	[1.62]	[1.21]	[1.46]
Housing outside quality	7.31	6.90	7.19	6.98
(1 = worst, 9 = best)	[0.91]	[1.32]	[0.87]	[1.17]

Notes: Standard deviation in brackets. Inside and outside quality are ratings performed by NVM on the condition of the property. Both variables are measured on a scale from 1 = worst to 9 = best. The externality group is defined by the 150 m cut-off distance. We compare characteristics to observations within 500 m distance. We remove the top and bottom 1% observations in terms of transaction price from the data. Prices are adjusted for inflation into 2017 values, using the CPI from the Dutch Statistics Office (CBS).

4 Methodology and Results

We use local house prices to assess the external effects of coffeeshops. Following the hedonic pricing theory, coffeeshop externalities, such as from nuisance, are expected to be reflected in nearby property prices (Tiebout, 1956; Rosen, 1974). The underlying theory assumes that people can choose location freely, allowing them to sort into specific neighborhoods and homes. As people sort according to their preferences, structural and socio-demographic aspects, location and nearby externalities should be reflected in local house prices (Rosen, 1974; Tiebout, 1956), allowing us to measure the willingness-to-pay for external effects of coffeeshops through nearby house prices.

4.1 Difference-in-Difference Analysis

Since coffeeshops are unlikely to be randomly distributed, any study into their external effects faces endogeneity issues. It may well be the case that coffeeshops try to avoid vocal local opposition, and therefore chose locations where neighbors do not complain much, e.g. due to social status, education, or simply liberal attitudes. In such locations, house prices might have been lower ex-ante. In addition, many coffeeshops are suspected to have connections to organized crime, resulting in a careful decision on their location.[19]

Coffeeshop closings offer an alternative, but might be endogenous, too. In practice, coffeeshops close for two reasons: due to violation of the law, or due to regulations such as the school distance criterion. Since law violations have to be reported by someone, the resulting closings could be the result by complaining neighbors or of gentrification.[20] We therefore focus solely on exogenous school distance-related closings, so employing a quasi-experimental setup. As described in section 2.3, the school distance criterion is not only arbitrary in terms of cut-off distance, but does not consider previous coffeeshops´ popularity in the neighborhood, a prime reason why affected coffeeshops loudly complained against the legislation. We therefore argue that school distance-related closings create exogenous variation for proper identification of coffeeshops´ externality removals.

Similar to Muehlenbachs et al. (2015), Pope and Pope (2015), McMillen and McDonald (2004), and Conklin et al. (2017), we use a spatial difference-in-differences

[19] E.g. coffeeshops need to ensure proper supply-chain, even though it is mostly illegal `https://bit.ly/2EttEYH`

[20] We tried to investigate these closings further, but are not able to determine the exact reasons behind law related closings.

(DID) framework, grouping transactions based on their spatial distance and transaction date relative to closings into four different groups: pre-nearby, pre-far, post-nearby, post-far. Since we do not just employ repeated sales, we cannot rely on the assumption that transactions do not systematically vary over time and we therefore include hedonic control characteristics, controlling for structural and neighborhood attributes.

The employed model is shown in equation (1), where X'_{it} combines structural, neighborhood, and maintenance characteristics of property i at time t as well as sales time. $Nearby_{it}$, where $d = 1$ indicates that property i at time t is located nearby a closing coffeeshop and $Post_{it}$, where $d = 1$ indicates a property transaction after the closing of the closest closing coffeeshop.[21] We test four different distance cut-off points to define $Nearby_{it}$: 150 meters, 100 meters, 50 meters, and 6-digit postcode. $Nearby_{it} * Post_{it}$, measures the interaction of two former terms, where $d = 1$ indicates a transaction nearby a closing coffeeshop after closing.

$$\ln(p_{it}) = \alpha_{it} + bX'_{it} + \gamma_1 Nearby_{it} + \gamma_2 Post_{it} + \gamma_3 Nearby_{it} * Post_{it} + \epsilon_{it} \qquad (1)$$

To account for potential spatial dependence and omitted variables, we include location fixed-effects (Anselin & Bera, 1998; Kuminoff et al., 2010), using detailed postcode information. In Dutch urban areas a 6-digit postcode is shared by 17 households on average. However, we do not have sufficient observations for this level of location fixed effects, and it would result in single-observation fixed-effects. Therefore, we use 5-digit postcode areas instead, for which we have 12 observations per postcode area, on average, in our sample. We use year dummies as time fixed-effects and cluster standard errors by municipality and year.[22]

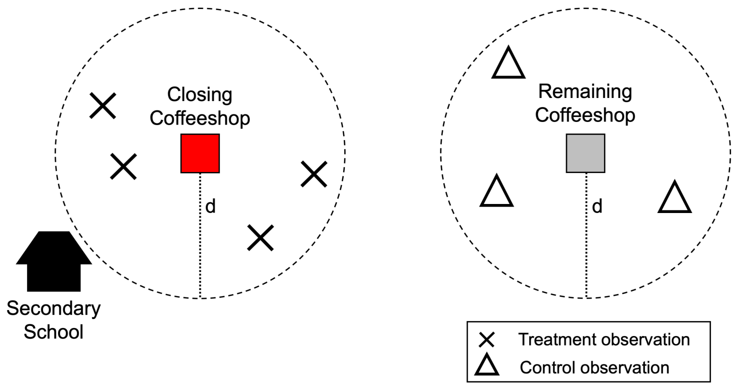
Since homes located near coffeeshops may systematically differ from those in

[21] For $Post_{it}$ we use the closing date of the closest closing coffeeshop, ensuring that every treatment area has a respective control area.

[22] We also employ Year*City interaction dummies, showing no difference.

the general market, we use homes around remaining coffeeshops as a control group instead of homes far away. Homes in both treatment and control groups are within the same cut-off distance of coffeeshops, sharing common attributes and therefore making them similar as they are initially all near a coffeeshop. Figure 3 illustrates the setup. To increase comparability further, housing transaction around every remaining coffeeshop are assigned as control group to one nearest closing coffeeshop, using linear distance. So a control group is always within the same city.

Figure 3
Difference-in-Difference Setup



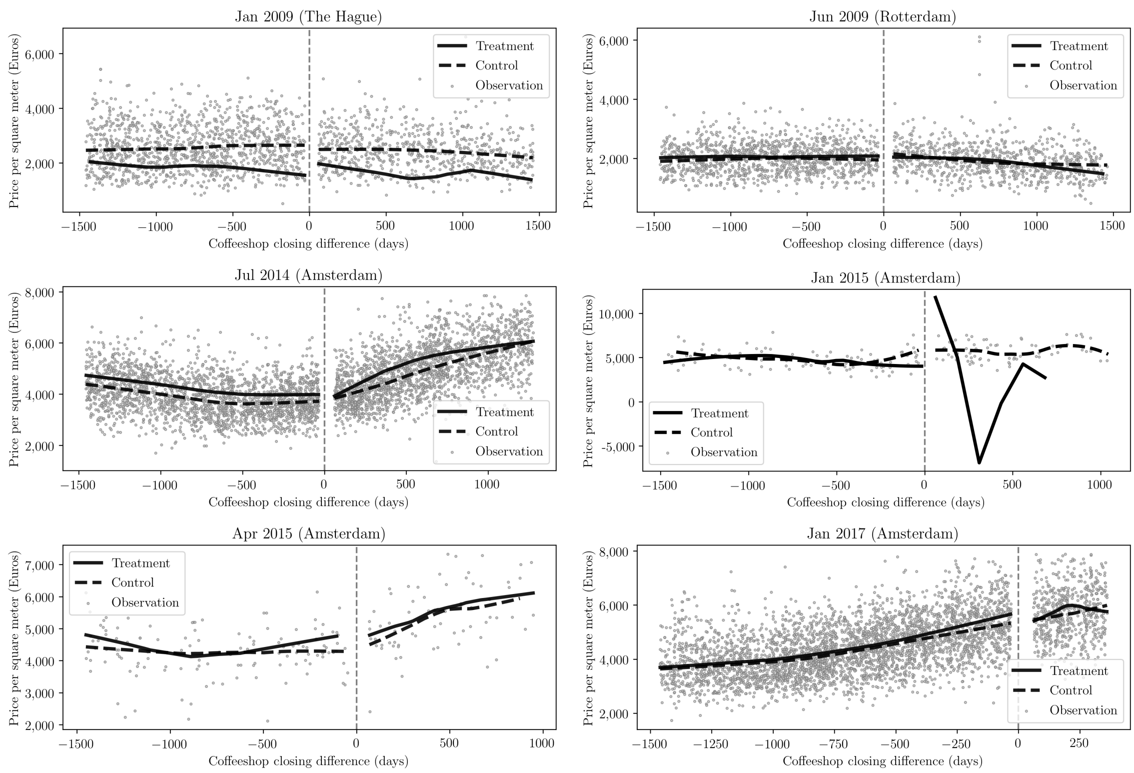
Notes: Illustration of analysis setup. We use homes nearby remaining coffeeshops as control group and compare the price changes before and after coffeeshop closings, testing different cut-off distances d between 150 meters and 6-digit postcode level.

We consider observations up to four years before and after closings. We verify our comparability assumption between treatment and control areas, by examining parallel trends. Considering expectations and adjustments of markets, we create a 90 days holdout window around coffeeshop closings (30 days before and 60 days after closings), which we later adjust to examine long-term closing effects. Furthermore, we only include closing coffeeshops for which there is at least one transaction in every group (pre-treatment, post-treatment, pre-control, post-control) [23]

[23]Appendix Table I provides more information regarding treatment and control groups, specifically regarding the observation numbers we have around coffeeshop closings and the effect of different externality cut-off distances. Appendix Table II provides this information by closing date and location.

Examining the similar trend assumption, Figure 4 plots the adjusted price per square meter of homes in treatment (gray) and control group (red) for the different closing waves, using a 150 m cut-off distance. We also plot trend lines for both groups: solid lines for the treatment groups and dotted lines for the control groups. Closing waves show different pre-post patterns, which is related to circumstances such as the financial crisis. However, except for January 2015, the overall price trends for the treatment and control groups are similar.

Figure 4
DID Similar Trend Graphs (150 m)



Notes: We compare treatment and control group price trends around coffeeshop closings (4 years before and after), estimating non-parametric trend lines, using locally weighted scatterplot smoothing (LOWESS) with a quadratic polynomial. Transactions within 30 days before and 60 days after closing are excluded from the analsis. We plot the adjusted price per square meter of observations over time to illustrate the calculation base of the trend lines. A detailed overview of the number of observations per closing wave is documented in Appendix Table II. Due to missing post-closing observations, July 2017 closings are excluded from the analysis.

Table 4 shows the first set of regression results, using different cut-off distances. Rather surprisingly, homes near closed coffeeshops show a price discount after closing of 1.6 to 7.9 percent compared to homes near remaining coffeeshops. The effects increase with proximity. When we look at the coefficient for $Nearby_{it} = 1$, we observe no significant effects, which implies that there is no significant price difference between homes near remaining coffeeshops and homes near closing coffeeshops, showing that homes in both groups are similar in general. For both groups we document positive price-time trends between 2.3 and 6.7 percent ($Post_{it} = 1$).

Table 4
Difference-in-difference analysis

	(1) 150 m	(2) 100 m	(3) 50 m	(4) Postcode
Nearby Coffeeshop	-0.009	0.006	0.032	-0.005
(1 = yes)	[0.008]	[0.017]	[0.028]	[0.068]
Nearby Coffeeshop * Post-closing	-0.016*	-0.030**	-0.074***	-0.076**
(1 = yes)	[0.009]	[0.013]	[0.021]	[0.031]
Post-closing	0.029**	0.023*	0.018	0.065***
(1 = yes)	[0.013]	[0.013]	[0.020]	[0.018]
Observations	12,412	6,765	1,979	598
Treatment Group	1,838	909	269	118
Adj. R-squared	0.79	0.79	0.81	0.80
Quality & Location controls	YES	YES	YES	YES
Time-Fixed effects	YES	YES	YES	YES

Notes: Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. *Nearby Coffeeshop* refers to closing coffeeshop. Treatment group is reported as part of total observations. Homes nearby closing coffeeshops but within 150 m of a remaining coffeeshop are excluded from the analysis. Standard errors clustered by municipality-year. Control group cut-off distance is similar to the treatment group cut-off distance (150 m, 100 m, etc.), as illustrated in Figure 3. Location controls are 5-digit postcode level fixed-effects (around 17 observations per postcode in sample).

It is likely that coffeeshops are located to other forms of entertainment that could lead to local nuisance, like bars and nightclubs, and their presence could influence our findings. We therefore repeat the analysis, controlling for potential nuisance venues nearby, distinguishing for bars, pubs, and nightclubs. We download all locations of pubs, bars and nightclubs via Overpass API using OpenStreetMap data.[24] All three entertainment venues serve alcohol. However, pubs generally also serve food and have a relaxed atmosphere, whereas bars have a more noisy and vibrant atmosphere and

[24]Retrieved 2017 from `https://www.openstreetmap.org/#map=12/52.3563/4.8532`

nighclubs (discos) focus on dancing.

Table 5 reports the results for the analysis including proximity to bars, pubs and nightclubs. Coffeeshop closing effects remain negative and are hardly affected, ranging from -1.6 to -7.5 percent for homes within 150 meters to 50 meters. Baseline treatment and control group pricing differences ($Nearby_{it} = 1$) remain insignificant and general post-closing time effects diminish. We document a negative proximity effect for bars of -4.6 percent, but only when houses are located within 50 meters. For nightclubs, we find an effect of up to -26.3 percent, but no negative proximity effect for pubs, which might be due to their more social aspect, such as serving food.

Table 5
DID Analysis - Controlling for other nuisance venues

	(1) 150 m	(2) 100 m	(3) 50 m
Nearby Coffeeshop	-0.010	0.002	0.021
(1 = yes)	[0.008]	[0.018]	[0.029]
Nearby Coffeeshop * Post-closing	-0.016*	-0.030**	-0.075***
(1 = yes)	[0.009]	[0.013]	[0.021]
Post-closing	0.028**	0.020	0.019
(1 = yes)	[0.013]	[0.013]	[0.020]
D: Bar	0.001	-0.007	-0.045***
(1 = yes)	[0.006]	[0.014]	[0.015]
D: Pub	0.008	-0.003	0.009
(1 = yes)	[0.006]	[0.008]	[0.010]
D: Nightclub	-0.013	-0.094***	-0.263***
(1 = yes)	[0.012]	[0.023]	[0.077]
Observations	12,412	6,765	1,979
Adj. R-squared	0.79	0.79	0.81
Quality & Location controls	YES	YES	YES
Time-Fixed effects	YES	YES	YES

Notes: Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Estimation is similar to the main DID analysis, with outputs presented in Table 4 However, we control for proximity to bars, pubs, and nightclubs in this specification. Proximity to these venues is defined by the same cut-off distance as for coffeeshops (150 m, 100 m, etc.).

Since housing markets are sticky in the short run, closing effects might change with different holdout periods. We therefore test different holdout periods, excluding transactions within 5 to 365 days after coffeeshop closings from the analysis. Estimations of closing effects (*Nearby Coffeeshop * Post-closing*) for different holdout periods are presented in Table 6. Closing effects remain robust for different holdout periods (base holdout period highlighted). However, our results suggest that house prices are indeed somewhat sticky in the short run, as coffeeshop closing effects become statistically significant and a bit higher in magnitude for longer holdout periods.

Table 6
DID Analysis - Heterogeneity of holdout period

Nearby Coffeeshop * Post-closing	(1) 150 m	(2) 100 m	(3) 50 m	(4) Postcode
post-5 days holdout	-0.014	-0.023*	-0.063***	-0.073**
	[0.010]	[0.013]	[0.020]	[0.029]
post-10 days holdout	-0.013	-0.022*	-0.060***	-0.073**
	[0.010]	[0.013]	[0.021]	[0.029]
post-30 days holdout	-0.013	-0.021*	-0.062***	-0.073**
	[0.010]	[0.013]	[0.020]	[0.029]
post-60 days holdout	-0.016*	-0.030**	-0.074***	-0.076**
	[0.009]	[0.013]	[0.021]	[0.031]
post-90 days holdout	-0.018*	-0.030**	-0.067***	-0.067**
	[0.009]	[0.013]	[0.021]	[0.029]
post-180 days holdout	-0.018*	-0.038***	-0.075***	-0.067*
	[0.010]	[0.014]	[0.021]	[0.035]

Notes: Standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The same estimation model as before is used, except that we adjust the holdout period, excluded from the estimation. The base holdout period of 60 days is highlighted for comparison reasons. In a different analysis, we also estimate the model for varying pre-closing holdout periods, but do not observe changes in the effects.

4.2 Repeated Sales Model

Even though the presented difference-in-difference setup allows to control for individual property characteristics, it relies on the assumption that transacted properties before and after closings are similar. However, this assumption could be violated by systematic changes in unobserved characteristics. Therefore, we verify our previous findings by applying a repeated sales approach, using repeated sales pairs at different locations relative to coffeeshops, one sale taking place before and one after closings. Since we use the same property before and after coffeeshop closings, we can be more certain that property characteristics stay constant. Furthermore, we control for time-varying characteristics in our model, such as improvements and decay.

Filtering for repeated sales only and excluding sales pairs selling more than once in the same year, our dataset consists of 15,289 properties that sold twice during the sample period, 2,545 properties that sold three times, and 249 properties that sold four times during the sample period.[25] As in the DID approach, we compare homes nearby closing coffeeshops with homes nearby remaining coffeeshops. We use the same cut-off distances, the same time window (+/- 4 years), and the same holdout period (30 days before and 60 days after coffeeshop closings). After filtering, the number of repeated sales available decreases significantly.

As shown in equation (2), we use the percentage change in transaction price $\Delta p_{i(t+n)}$ of property i between date t and $t+n$ as the dependent variable. We control for changes in property characteristics, such as refurbishments, changes in size, maintenance quality and insulation, as well as changes in the surrounding area, such as distance to (dis)amenities. We divide all changes into additions and improvements, measured by $\Delta Q'^{+}_{i(t+n)}$ and removals and decay, measured by $\Delta Q'^{-}_{i(t+n)}$.

We control for time effects using time fixed effects $Y'_{i(t+n)}$, indicating the sales year $t+n$ of property i. Additionally, we control for the time period n between two sales, for location, using the 5-digit neighborhood post-code (Θ'_{in}), and for closing coffeeshops fixed-effects. The change in coffeeshop distance of property i between t and $t+n$ is measured by $\Delta CS_{i(t+n)} \in \{0,1\}$, where $\Delta CS_{i(t+n)} = 1$ indicates a change in coffeeshop distance due to closings, our variable of interest.

$$\Delta p_{i(t+n)} = \alpha + \Delta Q'^{+}_{i(t+n)}\gamma_1 + \Delta Q'^{-}_{i(t+n)}\gamma_2 + Y'_{i(t+n)}\gamma_3 + \Theta'_{in}\gamma_4 + \gamma_5 \Delta CS_{i(t+n)} + \epsilon_{i(t+n)} \quad (2)$$

[25] 20 properties sell more than 4 times during the sample period and are excluded from the analysis, considered as outliers.

Since local housing markets may take time to incorporate closing effects, we also test for the time difference between the sales of treatment homes and coffeeshop closings, indicated by $\Lambda_{i(t+n)}$ as shown in equation (3).

$$\begin{aligned}\Delta p_{i(t+n)} = \alpha + \Delta Q'^{+}_{i(t+n)}\gamma_1 + \Delta Q'^{-}_{i(t+n)}\gamma_2 + Y'_{i(t+n)}\gamma_3 \\ + \Theta'_{in}\gamma_4 + \gamma_5 \Delta CS_{i(t+n)} + \gamma_6 \Lambda_{i(t+n)} + \epsilon_{i(t+n)}\end{aligned} \tag{3}$$

Where $\Lambda_{i(t+n)}$ is the difference in days between $t+n$ and z, the closing date of the closest coffeeshop.

$$\Lambda_{i(t+n)} = \begin{cases} (t+n) - z & \text{if } \Delta CS_{i(t+n)} = 1 \\ 0 & \text{if } \Delta CS_{i(t+n)} = 0 \end{cases}$$

Since a linear specification of the time difference might not be fully adequate, we additionally test a quadratic day difference, $\Phi_{i(t+n)}$, accounting for diminishing effects over time.

Applying the analysis setup as described above, there are 57 repeated sales pairs left within 150 m distance to closing coffeeshops, which experience a coffeeshop closing between sales. We therefore limit our analysis to the 150 meters cut-off distance. When we look at the percentage change in price between sales pairs over the coffeeshop closing time difference for treatment and control groups, we observe no systematic difference between groups. The majority of homes in both groups generally increases in value over time.

Table 7 first shows the result of the estimation of (2) in Column (1). We document that homes experiencing a coffeeshop closing between the first and second sale fall on average 10.75 percent in value compared to homes nearby remaining coffeeshops. When we subsequently estimate (3), controlling for the linear and quadratic time differences in days between coffeeshop closings and second home sales at $t+n$, we document a general price drop of 18.7 to 34.9 percent. However, prices seem to recover over time, albeit at a diminishing rate.

Even though we consider this last analysis as meaningful, there are some important limitations to consider. The number of repeated sales pairs is limited, leading to a small treatment group. Furthermore, we are not able to perform any sub-tests and robustness checks. So even though our DID findings are confirmed, we should interpret the magnitude of the estimated coefficients in the repeat sales analysis with some caution.

Table 7
Repeated sales analysis

	(1)	(2)	(3)
$\Delta CS_{i(t+n)}$	-10.754**	-18.723**	-34.857***
(1 = coffeeshop closing)	[4.957]	[9.044]	[12.170]
$\Lambda_{i(t+n)}$		0.012	0.071**
(days after closing)		[0.008]	[0.030]
$\Phi_{i(t+n)}$			-0.000**
((days after closing)^2)			[0.000]
Observations	373	373	373
Adj. R-squared	0.53	0.53	0.53
Cut-off distance	150 m	150 m	150 m
Quality & Location controls	YES	YES	YES
Time-fixed effects	YES	YES	YES

Notes: Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The dependent variable percentage is the change in price. Control group are homes nearby remaining coffeeshops at 150 m cut-off distances. We control for sales years and location fixed-effects are based on 5-digit postcode level.

5 Discussion - Channels & Mechanisms

Our results consistently document house price decreases for homes near closing coffeeshops compared to homes near remaining ones in the same city. In order to understand our findings, we shift the focus of our analysis to potential causation channels. A first channel could be related to changes in local nuisance. Two survey studies regarding exogenous coffeeshop closings in Rotterdam and Amsterdam explore local nuisance effects. Bieleman et al. (2010) conduct a survey among teenagers and local residents regarding the effects the forced coffeeshop closings in Rotterdam. They find a significant general reduction of negative externalities over time for both groups, but no specific effect due to the closing of coffeeshops. The percentage of teenagers using cannabis does not change after closings, and neither does their sourcing behavior, as they still receive cannabis from older friends.

In a follow-up study in Amsterdam, Bieleman et al. (2015b) find an increase in customers for remaining coffeeshops, but no increase in reported negative externalities. The majority of neighbors indicate that they like their neighborhood and that perceived safety does not change after closings. Nuisance complaint reports before and after exogenous coffeeshop closings remain constant. Overall, only five to seven percent of neighbors directly relate coffeeshops to nuisance-related problems in their neighborhood.

Chang and Jacobson (2017) find that cannabis dispensary closings in Los Angeles lead to higher crime rates nearby, especially in the short run.[26] Since the effect also holds for restaurant closings, they argue that, in general, *"retail establishments, when operational, provide informal security through their customers,"* which is in line with the "eyes upon the street" theory (Jacobs, 1961). This finding is in line with Koster and Rouwendal (2012), who examine the effects of retail activities on house prices in the Netherlands, finding positive price effects for homes near retail activities. This would suggest that our documented closing effects are not driven by the disappearance of coffeeshops itself, but by the circumstances of the post-closing situation (operational retail versus an empty store).

To test for the influence of the post-closing situation, we examine the developments of the former coffeeshop locations over time. We visited all closed coffeeshop locations

[26] We also tried to examine the relation between coffeeshop closings and local crime rates, but due to data limitations we do not have sufficient variation for a convincing analysis.

in Rotterdam and Amsterdam to collect information on post-closing usage as well as the time it took for the vacant store to be re-used. We did this by interviewing new shop owners and neighbors. Additionally, we examined all locations on Google Street View, allowing us to virtually "walk the streets". Google updates Street View images on an irregular base, but publishes all previous images, allowing us to go virtually back in time and track coffeeshop locations over time (at irregular intervals).

It turns out that most former coffeeshop properties are vacant for a significant amount of time before being re-used. Often, the coffeeshop storefront remains as long as the site is vacant. Appendix Figure B presents some images of vacant coffeeshops. On average, it takes around 781 days until a new business is opened in the former coffeeshop space. Figure 5 shows the first usage of closed coffeeshop locations. We document that most coffeeshops turn into shisha bars (similar to a pub but focusing on smoking the hookah, only open at night), cafes (bistro type), restaurants (proper restaurant). However, most locations change their use at least once afterwards, and quite a number remain vacant.

We test whether vacancy has an effect on our results by distinguishing between vacant and non-vacant post-closing locations, considering the status at transaction time t of property i.[27] Building on the DID model, equation (4) shows the model for our analysis, where $Vacant_{it} = 1$ indicates that the coffeeshop location is vacant at post-closing transaction time t. As we only look at post-closing locations, $Vacant_{it} = 1$ implies that $Nearby_{it} * Post_{it} = 1$.

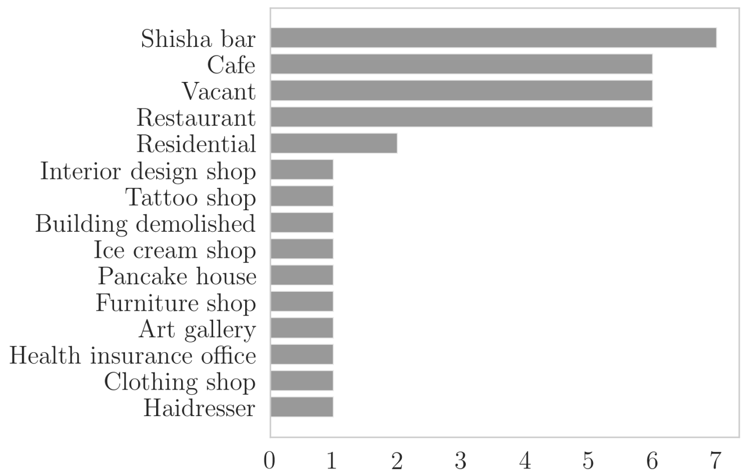
$$\ln(p_{it}) = \alpha_{it} + bX'_{it} + \gamma_1 Nearby_{it} + \gamma_2 Post_{it} + \gamma_3 Nearby_{it} * Post_{it} + \gamma_4 Vacant_{it} + \epsilon_{it} \quad (4)$$

Table 8 shows the result for the estimation of (4). Overall, we do not find significant price differences for vacant and non-vacant post-closing states. Only at

[27] We realize that there are business quality differences for non-vacant locations, but the small number of observations and examples of exceptions prevent us from distinguishing further by business type.

a 150 m cut-off distance and a 10% significance level do we document a positive effect for homes nearby vacant places compared to homes nearby non-vacant coffeeshop locations. Our results regarding the effect of the coffeeshop closings are hardly affected by including the vacancy information in the analysis. Reasons for our findings could be quality differences of succeeding businesses, such as Shisha lounges.

Figure 5
Post-closing coffeeshop location usage



Notes: Displayed are the usages of closed coffeeshop locations in Rotterdam and Amsterdam. We group different usage cases together where possible. One coffeeshop could not be inspected.

Table 8
Vacancy analysis

	(1) 150 m	(2) 100 m	(3) 50 m
Nearby Coffeeshop	-0.010	0.010	0.012
(1 = yes)	[0.008]	[0.017]	[0.024]
Nearby Coffeeshop * Post-closing	-0.030**	-0.035**	-0.064**
(1 = yes)	[0.012]	[0.017]	[0.025]
Post-closing	0.034***	0.027**	0.017
(1 = yes)	[0.011]	[0.011]	[0.015]
Vacant	0.025*	0.009	-0.011
(1 = yes)	[0.013]	[0.014]	[0.030]
Observations	9,632	5,344	1,585
Adj. R-squared	0.80	0.81	0.84
Quality & Location controls	YES	YES	YES
Time-Fixed effects	YES	YES	YES

Notes: Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Vacant controls for vacant post-closing coffeeshop locations at the time of transactions. Once a business sets up in the location, it is no longer considered as vacant. Homes nearby closing coffeeshops but within 150 m of a remaining coffeeshop are excluded from the analysis. Standard errors clustered by municipality-year.

6 Implications & Conclusion

While considered a dangerous hard drug by some, many governments around the world have moved to legalize recreational use of cannabis. The Netherlands has over 40 years of experience in decriminalized cannabis sales. We explore the exogenous closing of Dutch coffeeshops due to proximity to schools, and investigate the local external effects on house prices. Focusing on Amsterdam, Rotterdam and The Hague, we perform difference-in-difference and repeat sales analyses around these coffeeshop closings, avoiding the endogeneity concerns that would hamper a more traditional hedonic setup. By using a dataset of house prices and characteristics, we can adjust properly for changes in housing quality.

Perhaps surprisingly, we document negative local house price effects when nearby coffeeshops close. In the difference-in-difference analysis, the effect ranges from -1.6

percent for homes located up to 150 meters away to -7.4 percent for homes up to 50 meters away, and -7.6 percent for homes within the same 6-digit postal code area, all compared to homes near the closest coffeeshop that remains open. This result is robust to the inclusion of other entertainment with possible local nuisance effects, such as bars and nightclubs, and the effect remains when we consider differently holdout periods, taking care of potential price stickiness in local housing markets.

The repeat sales analysis tells a similar story in terms of direction and magnitude. Comparing the prices of the same dwelling before and after the exogenous closing of a coffeeshop, we observe a 10.8 percent decrease in price. When we control for time since closing, we observe a larger initial negative effect, but prices subsequently recover, albeit at a diminishing rate.

We subsequently explore potential causation channels for these effects. Chang and Jacobson (2017) show that closings of medical cannabis dispensaries in the US leads to more local crime, and this could also be the case in the three Dutch cities we study, since the properties in which the closed coffeeshops were housed remain vacant for more than two years after closing, reducing the informal security that customers of active retail establishments create. Also, coffeeshop owners have an incentive to reduce nuisance so as to maintain smooth operations. We therefore empirically investigate whether the vacancy resulting from coffeeshop closing is associated with the negative house price effects we find, but we don´t find convincing evidence that vacancy increases the negative effect of coffeeshop closings.

The findings in this paper have some implications for policymakers and homeowners. Contrasting expectations, and perhaps intuition, once a coffeeshop is in operation, closing it may be detrimental to local house prices. While we do not study coffeeshop openings, our findings suggest some amenity value in cannabis dispensaries, potentially benefiting some neighborhoods. Of course, further research is needed on the effect of cannabis dispensary openings.

References

Anderson, D. M., & Rees, D. I. (2014). The Legalization of Recreational Marijuana: How Likely Is the Worst-Case Scenario? *Journal of Policy Analysis and Management*, *33*(1), 221–232.

Anselin, L., & Bera, A. K. (1998). Spatial Dependence in Linear Regression Models with an Introduction to Spatial Econometrics. *Statistics Textbooks and Monographs*, *155*, 237–290.

Bieleman, B., Biesman, S., Snippe, J., & de Bie, E. (1996). *Quick Scan: Ontwikkeling in Aantal Coffeeshops in Nederland*. Groningen: Intraval.

Bieleman, B., Mennes, R., & Sijtstra, M. (2015a). *Coffeeshops in Nederland 2014*. Groningen: Intraval.

Bieleman, B., Mennes, R., & Sijtstra, M. (2015b). *Monitor Afstandscriterium Coffeeshops Amsterdam*. Groningen: Intraval.

Bieleman, B., Nijkamp, R., & Schaap, L. (2010). Monitor coffeeshopbeleid rotterdam. *Groningen-Rotterdam: Intraval*.

Cerdá, M., Wall, M., Keyes, K. M., Galea, S., & Hasin, D. (2012). Medical Marijuana Laws in 50 States: Investigating the Relationship Between State Legalization of Medical Marijuana and Marijuana Use, Abuse and Dependence. *Drug and Alcohol Dependence*, *120*(1), 22–27.

Chang, T., & Jacobson, M. (2017). Going to Pot? The Impact of Dispensary Closures on Crime. *Journal of Urban Economics*, *100*, 120–136.

Charilaou, P., Agnihotri, K., Garcia, P., Badheka, A., Frenia, D., & Yegneswaran, B. (2017). Trends of Cannabis Use Disorder in the Inpatient: 2002 to 2011. *The American Journal of Medicine*, *130*(6). Retrieved from `http://dx.doi.org/10.1016/j.amjmed.2016.12.035` doi: 10.1016/j.amjmed.2016.12.035

Cheng, C., Mayer, W. J., & Mayer, Y. (2018). The effect of legalizing retail marijuana on housing values: Evidence from colorado. *Economic Inquiry*, *56*(3), 1585–1601.

Chu, Y.-W. L. (2015). Do Medical Marijuana Laws Increase Hard-Drug Use? *The Journal of Law and Economics*, *58*(2), 481–517.

Congdon-Hohman, J. M. (2013). The Lasting Effects of Crime: The Relationship of Discovered Methamphetamine Laboratories and Home Values. *Regional Science and Urban Economics*, *43*(1), 31–41.

Conklin, J., Diop, M., & Li, H. (2017). Contact high: The external effects of retail marijuana establishments on house prices. *Real Estate Economics*.

Dealy, B. C., Horn, B. P., & Berrens, R. P. (2017). The Impact of Clandestine Methamphetamine Labs on Property Values: Discovery, Decontamination and Stigma. *Journal of Urban Economics*, *99*, 161–172.

Harper, S., Strumpf, E. C., & Kaufman, J. S. (2012). Do Medical Marijuana Laws Increase Marijuana Use? Replication Study and Extension. *Annals of*

Epidemiology, *22*(3), 207–212.

Hunt, P., Pacula, R. L., & Weinberger, G. (2018). High on crime? exploring the effects of marijuana dispensary laws on crime in california counties. *IZA Discussion Paper No. 11567*.

Jacobi, L., & Sovinsky, M. (2016). Marijuana on Main Street? Estimating Demand in Markets with Limited Access. *The American Economic Review*, *106*(8), 2009–2045.

Jacobs, J. (1961). *The death and life of great american cities.* New York, NY: Random House.

Koster, H. R., & Rouwendal, J. (2012). The impact of mixed land use on residential property values. *Journal of Regional Science*, *52*(5), 733–761.

Kuminoff, N. V., Parmeter, C. F., & Pope, J. C. (2010). Which Hedonic Models Can We Trust to Recover the Marginal Willingness to Pay for Environmental Amenities? *Journal of Environmental Economics and Management*, *60*(3), 145–160.

Leydon, M. (2014). *Front Door/Back Door: The Paradox of the Netherlands' Cannabis Policy.* Retrieved from http://www.talkingdrugs.org/netherlands-paradox-cannabis-policy-front-door

Lynne-Landsman, S. D., Livingston, M. D., & Wagenaar, A. C. (2013). Effects of State Medical Marijuana Laws on Adolescent Marijuana Use. *American Journal of Public Health*, *103*(8), 1500–1506.

MacCoun, R., & Reuter, P. (1997). Interpreting Dutch Cannabis Policy: Reasoning by Analogy in the Legalization Debate. *Science*, *278*(5335), 47–52.

Marie, O., & Zölitz, U. (2017). "high" achievers? cannabis access and academic performance. *The Review of Economic Studies*, *84*(3), 1210–1237.

McMillen, D. P., & McDonald, J. (2004). Reaction of house prices to a new rapid transit line: Chicago's midway line, 1983–1999. *Real Estate Economics*, *32*(3), 463–486.

Morris, R. G., TenEyck, M., Barnes, J. C., & Kovandzic, T. V. (2014). The Effect of Medical Marijuana Laws on Crime: Evidence from State Panel Data, 1990-2006. *PloS one*, *9*(3), e92816.

Muehlenbachs, L., Spiller, E., & Timmins, C. (2015, dec). The Housing Market Impacts of Shale Gas Development. *The American Economic Review*, *105*(12), 3633–3659.

Municipality of Amsterdam. (2007). *Fact Sheet - Minder Coffeeshops in Amsterdam* (Tech. Rep.). Amsterdam: Dienst Onderzoek en Statistiek.

Pacula, R. L., Kilmer, B., Grossman, M., & Chaloupka, F. J. (2010). Risks and Prices: The Role of User Sanctions in Marijuana Markets. *The BE Journal of Economic Analysis & Policy*, *10*(1), 11.

Pope, D. G., & Pope, J. C. (2015). When walmart comes to town: Always low housing prices? always? *Journal of Urban Economics*, *87*, 1–13.

Rosen, S. (1974). Hedonic Prices and Implicit Markets: Product Differentiation in Pure Competition. *Journal of Political Economy*, *82*(1), 34–55.

Tiebout, C. M. (1956). A Pure Theory of Local Expenditures. *The Journal of Political Economy*, *64*(5), 416–424. doi: 10.1086/257839

Tops, D., Svensson, B., & Veldhoen, G. (2001). *The Drug Policies of the Netherlands and Sweden: How Do They Compare?* Brussels: European Parliament.

van Ooyen-Houben, M. M. J., Bieleman, B., & Korf, D. J. (2016). Tightening the Dutch Coffee Shop Policy: Evaluation of the Private Club and the Residence Criterion. *International Journal of Drug Policy*, *31*, 113–120.

Wall, M. M., Poh, E., Cerdá, M., Keyes, K. M., Galea, S., & Hasin, D. S. (2011). Adolescent Marijuana Use from 2002 to 2008: Higher in States with Medical Marijuana Laws, Cause Still Unclear. *Annals of Epidemiology*, *21*(9), 714–716.

Wouters, M., Benschop, A., van Laar, M., & Korf, D. J. (2012). Cannabis Use and Proximity to Coffee Shops in the Netherlands. *European Journal of Criminology*, *9*(4), 337–353.

Wouters, M., & Korf, D. J. (2009). Access to Licensed Cannabis Supply and the Separation of Markets Policy in the Netherlands. *Journal of Drug Issues*, *39*(3), 627–651.

Appendix

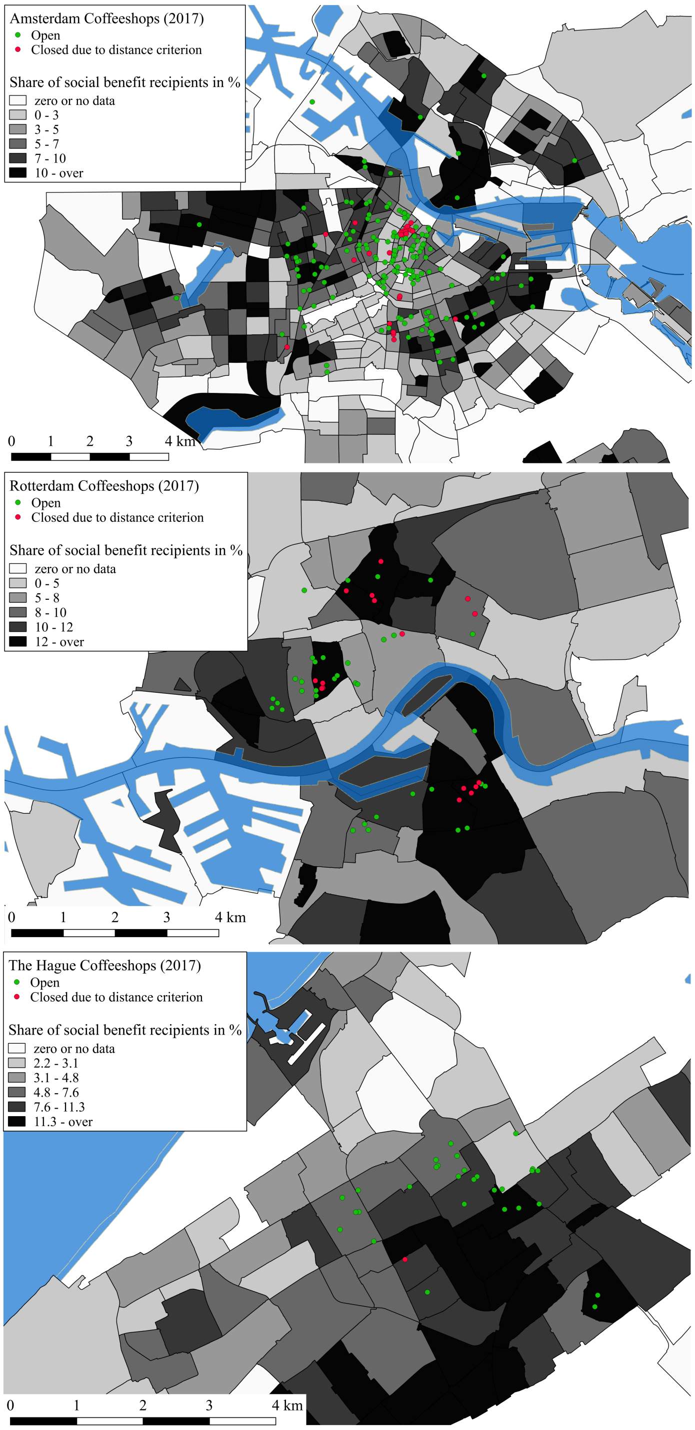
Figure A
Street scene impressions of open coffeeshops

Notes: Street scene images of coffeeshops in Amsterdam. Sources: Wikimedia Commons, `www.flickr.com` (Terrazzo, Travelmag.com), Google Street View

Figure B
Impressions of closed and vacant coffeeshops

Notes: Images of coffeeshops in Amsterdam and Rotterdam. Images taken by the authors in March 2018.

Figure C
Coffeeshop Distribution in Amsterdam, Rotterdam, and The Hague



Notes: Illustrated are the locations of coffeeshops over different neighborhoods in the three biggest cities. We focus on coffeeshops that are open today (late July 2017) and coffeeshops that closed due to the distance criterion. We use the percentage of social benefit recipients (unemployment benefits and long-term benefits), as a proxy for social status of neighborhoods.

Appendix A Difference-in-Difference Analysis

Table I
Difference-in-Difference: Groupings

Cut-off distance	Treatment		Control	
	Pre-Closing	Post-Closing	Pre-Closing	Post-Closing
150 m	1,304	534	6,991	3,583
100 m	598	311	3,902	1,954
50 m	169	100	1,170	540
Postcode	80	38	342	138

Notes: Control group: Homes nearby remaining coffeeshops at the same cut-off distance as the treatment group.

Table II
Observations by closing time and place
(150 m cut-off)

Closing date	**Amsterdam**	**Rotterdam**	**The Hague**
Treatment group			
Jan 2009	0	0	51
Jun 2009	0	610	0
Jul 2014	540	0	0
Jan 2015	41	0	0
Apr 2015	59	0	0
Jan 2017	786	0	0
Total	1,426	610	51
Control group			
Jan 2009	0	0	1,845
Jun 2009	0	1,397	0
Jul 2014	3,715	0	0
Jan 2015	126	0	0
Apr 2015	132	0	0
Jan 2017	3,510	0	0
Total	7,483	1,397	1,845

Notes: Based on the grouping definitions explained in Section 4.1, we document the number of transactions im the treatment group (150 m cutoff distance) and control group, divided by different closing dates and cities (see Section 2.3 for details). Closing date is always first day of the month. We document that closing times are location dependent.

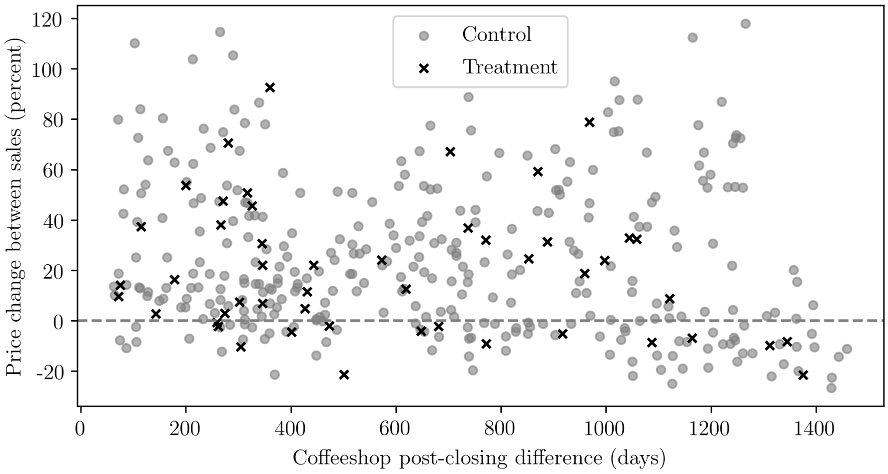
Table III
Difference-in-difference analysis - excluding The Hague

	(1) 150 m	(2) 100 m	(3) 50 m	(4) Postcode
Treatment	-0.005	0.009	0.011	0.075
	[0.008]	[0.017]	[0.023]	[0.054]
Post_Treat	-0.015*	-0.026**	-0.062***	-0.091***
	[0.008]	[0.012]	[0.018]	[0.030]
Post	0.031***	0.023**	0.007	0.059***
	[0.011]	[0.011]	[0.016]	[0.020]
Observations	10,537	5,771	1,707	532
Adj. R-squared	0.81	0.81	0.84	0.83
Quality & Location controls	YES	YES	YES	YES
Time-Fixed effects	YES	YES	YES	YES

Notes: Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. *Nearby Coffeeshop* refers to closing coffeeshop. Treatment group is reported as part of total observations. Homes nearby closing coffeeshops but within 150 m of a remaining coffeeshop are excluded from the analysis. Standard errors clustered by municipality-year. Control group cut-off distance is similar to the treatment group cut-off distance (150 m, 100 m, etc.), as illustrated in Figure 3. Location controls are 5-digit postcode level fixed-effects (around 17 observations per postcode in sample).

Appendix B Repeated Sales Analysis

Figure D
Repeated sales - price difference over post-closing time (<150 m)



Notes: Cut-off distance: 150 m. Plotted is the price difference between repeated sales in percent over the post-closing time difference. We only consider repeated sales occurring over coffeeshop closing and measure the change in price between sales. Treatment group are repeated sales nearby closing coffeeshops, control group are repeated sales nearby remaining coffeeshops (both within 150 m).

Table IV
Repeated Sales - Control Variables

Variables		Variables	
Sales date difference	0.001	Δm^2	-0.002
(days)	[0.005]		[0.120]
Δinside maintenance	3.721**	Δoutside maintenance	0.204
(level)	[1.460]	(level)	[1.080]
Δinsulation	0.364		
(layers)	[0.531]		
Additions		*Removals*	
Room added	0.690	Room removed	3.694
(1 = yes)	[2.230]	(1 = yes)	[4.104]
Roof terrace added	4.812	Roof terrace removed	9.737*
(1 = yes)	[4.787]	(1 = yes)	[5.642]
Attic added	-2.755	Attic removed	-6.781
(1 = yes)	[6.351]	(1 = yes)	[7.434]
Monument status added	10.045	Monument status removed	-22.353***
(1 = yes)	[14.625]	(1 = yes)	[2.444]
Garden added	3.301	Garden removed	3.795
(1 = yes)	[3.854]	(1 = yes)	[5.783]
Garage added	1.431	Garage removed	6.215
(1 = yes)	[6.221]	(1 = yes)	[12.563]
Carport added	-0.000	Carport removed	3.440
(1 = yes)	[0.000]	[1 = yes)	[10.717]
Garage & carport added	28.293**	Garage & carport removed	-0.000
(1 = yes)	[13.904]	(1 = yes)	[0.000]
Multigarage added	23.545***	Multigarage removed	0.000
(1 = yes)	[7.975]	(1 = yes)	[0.000]
Parkinglot added	-9.581*	Parkinglot removed	-2.667
(1 = yes)	[5.610]	(1 = yes)	[7.465]
CV or distance heating added	12.806**	CV or distance heating removed	-0.187
(1 = yes)	[5.614]	(1 = yes)	[4.879]
AC or solar added	-0.000	AC or solar removed	-0.000
(1 = yes)	[0.000]	(1 = yes)	[0.000]
Location improvements		*Location worsen*	
Forest now nearby	0.000***	Forest not nearby anymore	0.000
(1 = yes)	[0.000]	(1 = yes)	[0.000]
Park now nearby	-3.617	Park not nearby anymore	1.508
(1 = yes)	[6.090]	(1 = yes)	[12.296]
Now located next to water	8.621	Not located next to water anymore	-9.201*
(1 = yes)	[6.996]	(1 = yes)	[5.029]
Free view	0.597	No free view anymore	-3.871
(1 = yes)	[3.142]	(1 = yes)	[3.319]
No busy road nearby anymore	0.990	Busy road now nearby	7.927
(1 = yes)	[2.978]	(1 = yes)	[7.023]
Located on a quiet road	3.464	Not located on a quiet road anymore	-1.116
(1 = yes)	[3.410]	(1 = yes)	[3.968]
Time-fixed effects	YES	Observations	373
Location controls	YES	Adj. R-squared	0.53

Notes: Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1 Dependent variable: percentage change in house price. Standard errors are clustered by municipality and year. D = dummy, location controls by coffeeshop fixed-effects. Time fixed effects by sales year.