Getting high or getting low? The external effects of coffeeshops on house prices

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ORIGINAL ARTICLE

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Getting high or getting low? the external effects of coffeeshops on house prices

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Abstract

Cannabis legalization 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 house prices. We employ a difference-in-difference framework around a change in regulation, leading to exogenous coffeeshop closings. We find that closings have a negative effect on house prices. Compared to homes nearby remaining coffeeshops, homes nearby closing coffeeshops decrease on average 1.6–8.5% in value. The findings are robust to a battery of tests and unaffected by the subsequent use of coffeeshop locations.

KEYWORDS

cannabis, coffeeshops, externalities, housing markets, residential real estate

JEL CLASSIFICATION

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 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 of cannabis on society more broadly. 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 focuses on crime. Hunt et al. (2018) document a slight increase in "driving under influence" arrests after dispensaries' openings, in addition to reduced crime rates. Carrieri et al. (2019) find that the liberalization of a light form of cannabis in Italy led to a reduction in the number of arrests for drug-related offences. Focusing on the effect of dispensary *closings* on local crime, Chang and Jacobson (2017) document higher crime rates, in the short run, nearby closed dispensaries. As the authors document similar effects for restaurant closings, they argue that retail activities are generally better than vacancy, whatever the type of retail activity.

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 studies examine the change of medical to recreational cannabis dispensaries in Denver, Colorado.[6] Both studies find a 6–8% 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 less stringent cannabis policies, examining the implications of Dutch cannabis dispensaries, so-called "coffeeshops," on nearby property prices. To examine the effect of coffeeshops on nearby property prices, we employ an exogenous policy change. Starting in 2007, some local municipalities restricted the presence of coffeeshops around schools to protect children and teenagers from drug usage, forcing coffeeshops nearby secondary schools to close. These local restrictions became national law in 2014, leading to many more coffeeshop closings based on their proximity to schools. Closings were carried out in different waves between 2009 and 2017, providing substantial variation over time. This

[1] Retrieved from: https://bit.ly/2Ul40w8in March 2018

[2] For example: see https://reut.rs/2TlbMZF.

[3] Cannabis remains illegal under EU 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] See https://dpo.st/2AxB5NG, retrieved March 2019.

[6] Cheng et al. (2018) consider a bigger research area but use 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 6.5–19%.

empirical setting 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.

This study examines coffeeshop *closings*, following a recent exogenous regulatory change, in combination with a large microlevel database on house prices. Although there have been a small number of studies examining the impact of opening of the cannabis dispensaries, there is no evidence whether the impact is symmetric or whether it remains in case of closure of these dispensaries. In addition, this study focuses on a non-U.S. housing market and explores the effect of closings in multiple cities simultaneously (rather than just a single city). Both are important: the valuation of the externalities created by cannabis dispensaries might differ across regions due to differences in political attitudes of societies relative to cannabis legalization. As we focus on a country with long-term experience in tolerated cannabis usage, we are able to analyze price dynamics between cannabis dispensaries and house prices at a later stage of the adoption curve. This might help policy makers in "early stage" markets, where legalized cannabis sales facilities have only recently been introduced or are yet to be introduced.

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% 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% 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-differences (DID) estimation results show a closing discount of 1.6–7.8% for homes nearby closing coffeeshops, with the effect increasing when homes are closer by. This result is robust to controlling for the presence of other 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 of the repeat sales analysis, in which we compare prices of the same dwellings before and after coffeeshop closings, verify our DID estimation results.

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 past decade. Section 3 provides information regarding our dataset of housing transactions and the coffeeshop locations in Amsterdam, Rotterdam, and The Hague. The DID and repeat sales methodologies, as well as the main results, are presented in Section 4, and Section 5 will discuss 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.[8] 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,

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

the government wanted to reduce punishment of soft drug users. Even though cannabis possession is still officially illegal today, possession violations up to 5 g are not enforced (MacCoun & Reuter, 1997). In order to officially control the sale of cannabis, the government legally tolerated selling facilities, the so-called "coffeeshops." Since 1991, coffeeshops have to fulfill 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).[9]

Coffeeshops were opened all over the Netherlands, reaching their peak around early 1990s with around 1500 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 ultimately close coffeeshops (Bieleman et al., 2015a).

The law change resulted in a constant reduction in the number of coffeeshops, with effectively no new openings (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.[10] 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). However, 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

[9] 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 g (Bieleman et al., 2015a).

[10] One example of a restriction is the ban on simultaneous sales of alcohol and cannabis, leading to the closing of hasj-cafes, a facility similar to a coffeeshop, but more focused on hospitality aspects. The criterion was later adapted nationally (Municipality of Amsterdam, 2007).

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% 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 competitors or circumventing daily sales limits. Moreover, as discussed, the cannabis wholesale business remains illegal, making supply chains partly illegal and therefore relating coffeeshops to organized crime. 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 is 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 m 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 m, leading to the closure of one coffeeshop in January 2009.

[11] There are no official numbers. These numbers were estimated by a national newspaper: https://bit.ly/2T9fGpe. Other estimations range from € 800 million to € 1.2 billion.

[12] Retrieved 2017 from https://www.newsweek.com/marijuana-and-old-amsterdam-308218. 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 16–18.

The national government proposed to implement the distance criterion all over the country as of January 2014. However, municipalities were free to adapt the distance criterion and to change its specifications, such as distance. The government proposed to close coffeeshops within 250 m 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]

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.[18] 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 above, different municipalities implemented the distance

[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] See https://bit.ly/2NACEiM. Stage one was implemented as of January 1, 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 m 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 m of schools. Due to moving 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.

[17] See https://www.coffeeshopdirect.com/index.htm.

[18] Since we do not have information on closing dates of nondistance-related closings, we cannot use these data in our analysis.

TABLE 1 Coffeeshop sample overview (2000–2017)

City	Number of coffeeshops		Closed due to-distance criterion	Total
	Open	Closed		
Amsterdam	171	166	21[a]	337
The Hague	55	24	1[b]	79
Rotterdam	38	30	17	68
Others	345	144	–	489
Total	609	364	39	973

Notes: Table 1 reports the number of coffeeshops in our sample, including opening status per July 2017 (after last closing wave) for the three biggest cities in terms of number of coffeeshops.
[a]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.
[b]One coffeeshop was affected by the 250 m criteria. However, instead of closing, it was moved to a different location, which became available due to the law-related closing of another shop.

criterion differently. To verify our data, we contact the 10 biggest cities to obtain 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.

The underlying housing dataset consist of transactions across the Netherlands and comes from the Dutch Realtors Association , representing a market share of around 75% . The Dutch Realtors Association is a network of realtors, storing an extensive dataset 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.[19] Our final dataset consists of 115,248 housing transactions in Amsterdam, Rotterdam, and The Hague, representing 44% 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 | 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 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 1000 m). We document a high density of coffeeshops in these cities, with average housing–coffeeshop distances lying between 250 and 400 m.

Since the determination of an externality cutoff distance is rather arbitrary, we choose different distances. As discussed above, all municipalities that enforced the distance criterion to coffeeshops used at least 150 m as a cutoff distance (Bieleman et al., 2015a). Besides, Figure 1 shows the first quartile of the house–coffeeshop distance distribution at a distance of 160–250 m. Based on these observations, we choose 150 m as an externality cutoff distance. Due to the high density of coffeeshops, we can also employ smaller cutoff distances: 100 m, 50 m, as well as the six-digit

[19] We eliminate outliers that are detected based on the sample distribution of the transaction price—the upper and lower boundaries for the outliers are set at the first and 99th percentile.

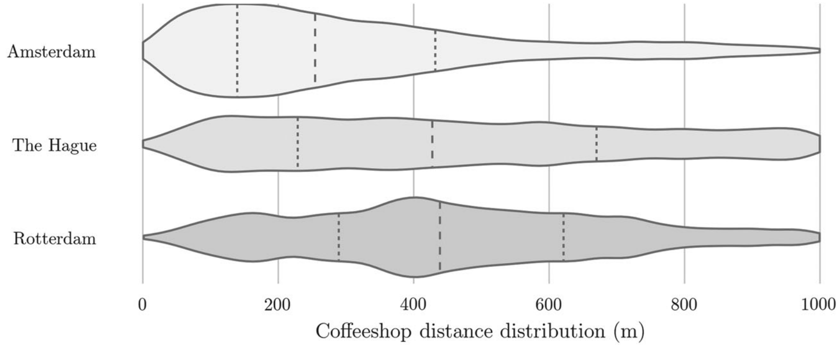

FIGURE 1 Distribution of property–coffeeshop distance. *Notes*: Figure 1 presents the distribution of distance to coffeeshops for the homes in our sample, considering properties up to 1000 m coffeeshop distance. The dashed lines indicate the quantiles.

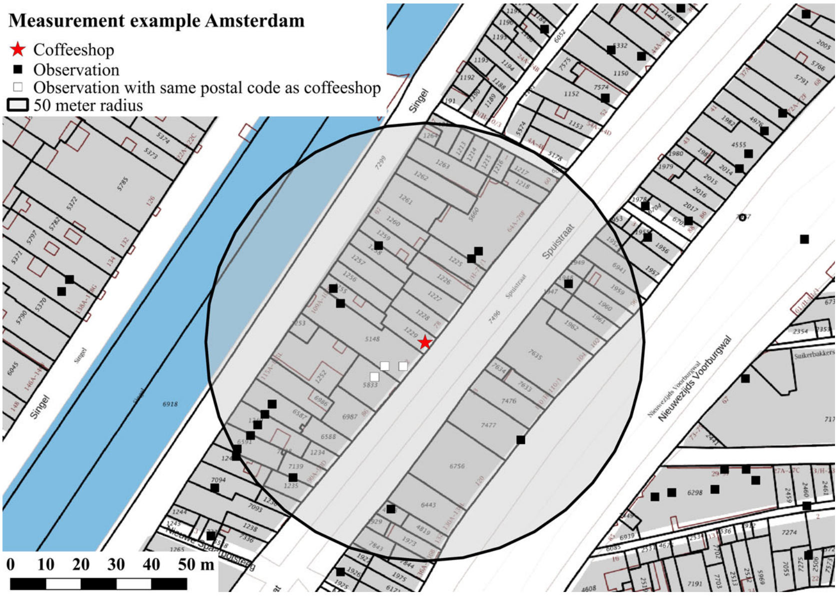

FIGURE 2 Illustration of clustering. *Notes*: Figure 2 illustrates different cutoff distance options. In the illustrated case, we consider all observations (black) within the 50-m radius of a coffeeshop (star) as affected by the externalities caused by the presence of coffeeshop. Observations in white share the same six-digit postal code as the coffeeshop, ensuring direct visibility. [Color figure can be viewed at wileyonlinelibrary.com]

postcode level. For the six-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 six-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-m radius around a coffeeshop (star), as well as the reach of the postcode matching (white squares). A detailed breakdown per cutoff distance per city is shown in Table 2.

TABLE 2 Number of transactions based on different cutoff distances

	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: Table 2 reports the number of property transactions within different cutoff distances, separately for different cities and property types.

Figure A1 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.

To get a general overview of the characteristics of properties in our sample, Table 3 summarizes characteristics for properties nearby coffeeshops closing due to the distance criterion and coffeeshops remaining open, both at a within 150 m distance. For both groups, the majority of observations are apartments, potentially due to the central locations. Properties in the control group are, on average, significantly bigger in size and have more rooms. They do not differ in the number of floors. The assessed internal maintenance quality does not differ, on average, whereas the external quality is quite different. Properties in both groups show a similar lack in structural amenities, such as a garden or a basement, not differing significantly. In terms of transaction characteristics, there is no significant difference between the median transaction year. On average, properties in the control group are significantly more expensive in absolute terms, but not on a per-square-meter basis. Table A1 presents the distribution of homes based on construction period for treatment and control groups separately. The statistics indicate that there is no significant difference between groups based on age of dwellings.

4 | METHODOLOGY AND RESULTS

We use house prices to assess the local external effects of coffeeshops. Following the hedonic pricing theory, coffeeshop externalities are expected to be reflected in nearby property prices (Rosen, 1974; Tiebout, 1956). 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 sociodemographic aspects, location, and nearby externalities should be

TABLE 3 Descriptive statistics

	Treatment group (< 150 m closing)	Control group (< 150 m remaining)	T-statistics (p value)
D: Apartment (1 = yes, 0 = no)	0.94	0.95	−1.78*
	(0.23)	(0.21)	(0.075)
Size (m^2)	79	82	−2.506**
	(40)	(40)	(0.012)
Number of rooms	3.11	3.21	−3.426***
	(1.35)	(1.38)	(0.001)
Number of floors	1.41	1.40	0.796
	(0.77)	(0.72)	(0.426)
Internal quality (1 = worst, 9 = best)	7.10	7.15	−1.778*
	(1.19)	(1.16)	(0.075)
External quality (1 = worst, 9 = best)	7.14	7.20	−3.143***
	(0.81)	(0.82)	(0.002)
D: Garden (1 = yes, 0 = no)	0.01	0.01	0.259
	(0.11)	(0.11)	(0.796)
D: Basement (1 = yes, 0 = no)	0.03	0.02	1.416
	(0.16)	(0.15)	(0.157)
Transaction year (median)	2013	2013	
Price (in Euro)	282,978	299,700	−4.938***
	(143,298)	(149,133)	(0.000)
Price per m^2 (in Euro)	3,837	3,887	−1.393
	(1,533)	(1,432)	(0.164)
Number of observations	2,160	11,329	

Notes: Table 3 reports the descriptive statistics for the homes that are transacted between 2000 and 2017 and located near coffeeshops. Standard deviations are reported in parentheses. We test statistical significance using Welch's t-test with p-values reported in parentheses. We provide the statistics separately for the homes that are located near a coffeeshop that is closed because of the school distance rule (treatment group) and for the homes that are located near open coffeeshops (control group). The homes in our sample are located within the 150-m cutoff distance of the surrounding areas of the coffeeshops. We exclude outliers based on the distribution of transaction price—the upper and lower boundaries for the outliers are set at the first and 99th percentile. Prices are adjusted for inflation into 2017 values, using the CPI from the Dutch Statistics Office (CBS). Internal and external quality are ratings performed by the Dutch Realtors Association on the condition of the property. Both variables are measured on a scale from 1 = worst to 9 = best. $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

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, for example, due to social status, education, or simply liberal attitudes. In such locations, house prices might have been lower ex ante.[20] In addition, many coffeeshops are suspected to have connections to organized crime, resulting in a careful decision on their location.[21]

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.[22] Since law violations have to be reported by someone, the resulting closings could be the result by complaining neighbors or of gentrification. We therefore focus solely on exogenous school distance-related closings, thus employing a quasi-experimental setup. As described in above, the school distance criterion is not only arbitrary in terms of cutoff distance but it also 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 coffeeshop-related local externalities.

We use a spatial DID framework, grouping transactions based on their spatial distance, and transaction date relative to closings into four different groups: prenearby, prefar, postnearby, postfar. Since homes that are transacted before and after the coffeeshop closure are not same, 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.

We propose the following empirical model:

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

where X'_{it} combines structural, neighborhood, and maintenance characteristics of property i at time t as well as time of the transaction. A detailed overview of control variables can be found in Table A2. The dummy variable $Nearby_{it}$ indicates that property i at time t is located nearby a closing coffeeshop and $Post_{it}$ indicates a property transaction after the closing of the nearest closing coffeeshop.[23] We use four different distance cutoff points to define $Nearby_{it}$: 150, 100, 50m, and six-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. In order to account for potential spatial dependence and omitted variables, we include location fixed effects, using detailed postcode information.[24] We also control for time trends using year-municipality fixed-effects and cluster standard errors by municipality and year.

[20] Using a sample of around 221,000 homes transacted between 2000 and 2017, we estimated a hedonic model using pooled ordinary least squares (OLS). Controlling for house characteristics, location, and year fixed effects, the OLS results indicate that the price of homes that are located near a coffeeshop is on average 1.5–3.6% lower depending on the proximity to the coffeeshop (150 m, 100 m, 50 m, and postcode). This result does not imply a causal effect of coffeeshops on house prices as the coffeeshop location is very likely to be endogenous. The OLS results are available upon request.

[21] For example, coffeeshops need to ensure proper supply chain, even though it is mostly illegal. (See https://bit.ly/2EttEYH.)

[22] In theory, coffeeshops could also close because of poor economic performance. However, due to the high profitability of coffeeshops in combination with decreasing competition over time (coffeeshop closings without new openings), we do not observe closings for such reasons.

[23] We measure the distance to the nearest coffeeshop and nearest closing coffeeshop for every observation i. For $Post_{it}$, we use the closing date of the nearest closing coffeeshop, ensuring that every treatment area has a respective control area. If property i is transacted after the closing date, $Post_{it} = 1$.

[24] In Dutch urban areas, a six-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 five-digit postcode areas instead, for which we have 12 observations per postcode area, on average, in our sample.

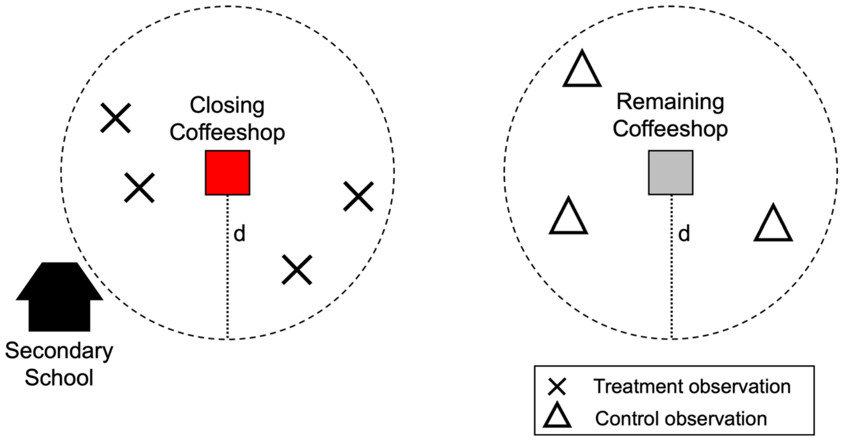

FIGURE 3 Difference-in-difference setup. *Notes*: Figure 3 illustrates the DID analysis setup. We use homes nearby remaining coffeeshops as the control group and compare the price changes before and after coffeeshop closings, based on different cutoff distances (*d*) varying between 150 m and six-digit postcode level. [Color figure can be viewed at wileyonlinelibrary.com]

Since homes located near coffeeshops may systematically differ from others, we use homes around remaining coffeeshops as a control group instead of homes far away.[25] Homes in both treatment and control groups are within the same cutoff distance of coffeeshops, sharing common attributes and therefore making them similar as they are initially all near a coffeeshop. Figure 3 illustrates the DID analysis setup. To increase comparability further, housing transactions around every remaining coffeeshop are assigned as the control group to one nearest closing coffeeshop, using linear distance. Therefore, a control region is always within the same city. Ensuring that groups are mutually exclusive, we exclude homes from the treatment group if they remain within externality distance to a remaining coffeeshop after closing.

We consider observations up to 4 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-day 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 (pretreatment, posttreatment, precontrol, postcontrol).[26]

[25] We note that our results might include a potential upward bias due to the potential opposite effects on control group properties (e.g., an increase in customers in the still-open shops), but unfortunately we are not able to control for this potential spillover effect. An alternative approach is to compare properties nearby closed coffeeshops with properties further away from closed coffeeshops (e.g., 200+ m). However, due to some methodological concerns, we believe that our current approach serves as a more reliable identification strategy as compared to this alternative approach. First, we believe that coffeeshops are not randomly located and therefore properties nearby and further away closed coffeeshops might be systematically different, potentially violating the common trend assumption. Second, coffeeshops are densely clustered within city centers. Extending the analysis distances would increase the risk of measuring confounding factors. For instance, if we draw 500 m distance circles around every coffeeshop, we cover nearly the entire city center of Amsterdam. In this case, we would essentially compare properties within the center with properties outside the city center.

[26] Table A3 provides detailed information on the size of treatment and control groups, specifically regarding the number of transactions before and after coffeeshop closings based on different cutoff distances. Table A4 provides this information

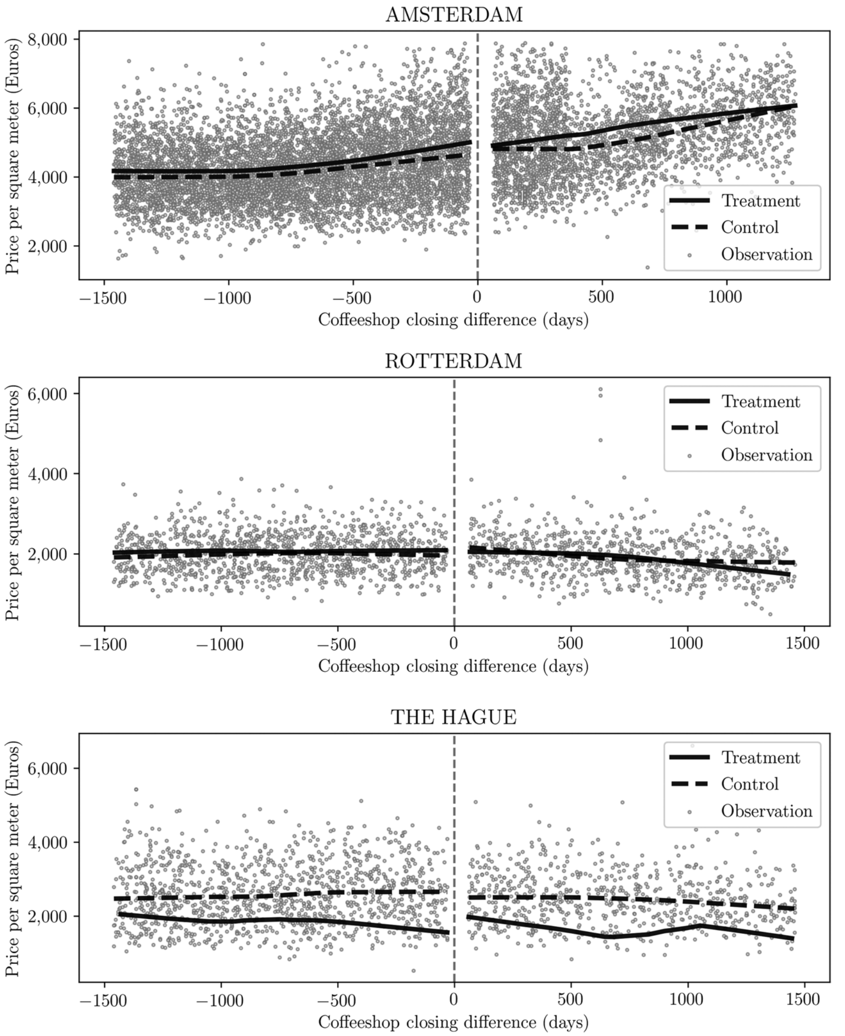

FIGURE 4 Price trends for homes in the treatment and control groups. *Notes*: Figure 4 presents the price (price per square meter) trends around coffeeshop closing dates (4 years before and after) for the homes in treatment and control groups separately. The cutoff distance is selected as 150 m. The nonparametric trend lines are estimated using locally weighted scatterplot smoothing (LOWESS) with a quadratic polynomial. Transactions within 30 days before and 60 days after closing are excluded from the analysis.

Examining the common trend assumption, Figure 4 plots the adjusted price per square meter of homes in treatment and control groups for different cities, using a 150-m cutoff distance. We plot trend lines for both groups: solid lines for the treatment groups and dotted lines for the control groups. Different cities show different pre–post patterns, which is related to circumstances such as the financial crisis. For example, prices in Amsterdam where closings are carried out after 2012

based on closing date and city. We further observe a few transactions affected by multiple closings. However, for these cases, closings do either not take place simultaneously or another coffeeshops remains nearby. These transactions are therefore not considered in the analysis, failing to satisfy the treatment group filtering criteria.

TABLE 4 Difference-in-differences estimation results

	(1) 150 m	(2) 100 m	(3) 50 m	(4) Postcode
Nearby coffeeshop (1 = yes)	−0.009	0.001	−0.022	0.026
	[0.008]	[0.023]	[0.030]	[0.060]
Nearby coffeeshop* Postclosing (1 = yes)	−0.018**	−0.025**	−0.072**	−0.071**
	[0.009]	[0.012]	[0.030]	[0.034]
Postclosing (1 = yes)	0.032**	0.021	0.005	0.064***
	[0.013]	[0.016]	[0.025]	[0.022]
House characteristics	Yes	Yes	Yes	Yes
Location fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Adj. *R*-squared	0.791	0.789	0.812	0.810
Total number of observations	12,412	6,172	1,675	598
Number of observation in the treatment group	1,838	909	269	118

Notes: Table 4 reports the DID estimations results. The dependent variable is the logarithm of transaction price. House characteristics include size, type, quality, construction period, number of floors, number of rooms, number of bathrooms, type of heating system, type of parking place, presence of garden, thermal quality, location of the home relative to city center, road, park, water, and forest. In all regressions, location and year of transaction dummies are also included. Location fixed-effects are included at a five-digit postcode level. Homes nearby closing coffeeshops but within 150 m of a remaining coffeeshop are excluded from the analysis. The control group cutoff distance is similar to the treatment group cutoff distance (150 m, 100 m, 50 m, and postcode), as illustrated in Figure 3. Heteroskedasticity-robust standard errors are in brackets. Standard errors are clustered by municipality and transaction year. *$p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

increase over time. A detailed overview of coffeeshop closing dates is shown in Table A4. We argue that, except for The Hague, preclosing price trends for both groups are similar. However, excluding The Hague from our analysis leads to similar findings, which might be due to the small number of observations in The Hague.[27]

Table 4 shows the first set of regression results, using different cutoff distances. Our results show that homes near closed coffeeshops show a price discount after closing of 1.8–7.4% compared to homes near remaining coffeeshops. The effect increases with proximity. For $Nearby_{it} = 1$ we do not observe a significant effect, which implies that there is no significant price difference between homes near remaining coffeeshops and homes near closing coffeeshops before the closing. We document positive time effects for both groups, ranging between 3.3% and 6.6% ($Post_{it} = 1$).

The global financial crisis in 2008 also influenced the housing market in the Netherlands. Both the volume of transactions and average house prices fell significantly, following the financial crisis. Considering our period of analysis, we can expect some impact of the crisis on housing prices for Rotterdam and The Hague, as coffeeshop closings took place in 2009 for these two cities (see Table A4). In a robustness analysis, we therefore restrict our sample to transactions in Amsterdam, only. As reported in Table A4, coffeeshop closings in Amsterdam started from 2014, which means that our period of analysis for Amsterdam does not cover the potential influence of financial crisis. The DID estimation results for Amsterdam are reported in Table A5 and are in line with our findings for the full sample. Only at a postcode level, we do not find significant effects. Due

[27] Results for a subanalysis excluding The Hague are available upon request.

to the large share of apartments, we further test our model for apartments only. The results are shown in Table A6 and are in line with our findings for the full sample.[28]

In order to test the validity of common trends assumption, we follow the analysis of Autor (2003) and extend Equation (1) as shown in Equation (2).

$$\ln(p_{it}) = \alpha_{it} + bX'_{it} + \gamma_1 Nearby_{it} + \gamma_z \sum_{z=1}^{Z} Nearby_{it} \times Int_{i,t,z} + \rho_z \sum_{z=1}^{Z} Int_{i,t,z} + \epsilon_{it} \qquad (2)$$

Instead of defining one *Post* period, we form Z intervals Int_z around the closing date. We test six time intervals (−2 years, −1 year, +1 year, +2 years, +3 years, +4 years) relative to the time period 3 and 4 years before closing. Following the initial setup, observations within 30 days before and up to 60 days after closing are excluded.

Figure 5 plots the coefficients of γ_z, including 90% confidence intervals. We test different cutoff distances but are not able to test individual cities due to the limited number of observations (see Table A4 for an overview of observations per city). Overall, we document no significant interaction effects preclosing, supporting the validity of parallel trends assumption. For the postclosing period, we find significant negative price effects in different years, depending on the cutoff distance. These estimates support our main findings. It seems that closing effects are concentrated within 2 years of closings, thereafter effects become insignificant for some cutoff distances. [29]

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 5–365 days after coffeeshop closings from the analysis. Estimates of closing effects for different holdout periods are presented in Table 5. Closing effects remain robust for different holdout periods. [30]

4.2 | Repeated sales analysis

Even though the presented DID setup allows us 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 differences 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 the coffeeshop closing, and one sale taking place after the closing. Since we use the same property before and after coffeeshop closings, we can be more certain that characteristics of the homes that are transacted in both periods are same. Furthermore, we control for time-varying characteristics of the homes 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, 2545

[28] We also test for time on market but do not find significant results. Instead, estimations are highly sensitive to the exclusion criteria for outliers.

[29] However, the temporary inconsistencies could also occur due to volatility in the number of transactions. Autor (2003) documents a similar inconsistency in his results.

[30] Our results suggest that house prices are indeed somewhat sticky in the short run, as coffeeshop closing effects become statistically significant and a larger in magnitude for longer holdout periods.

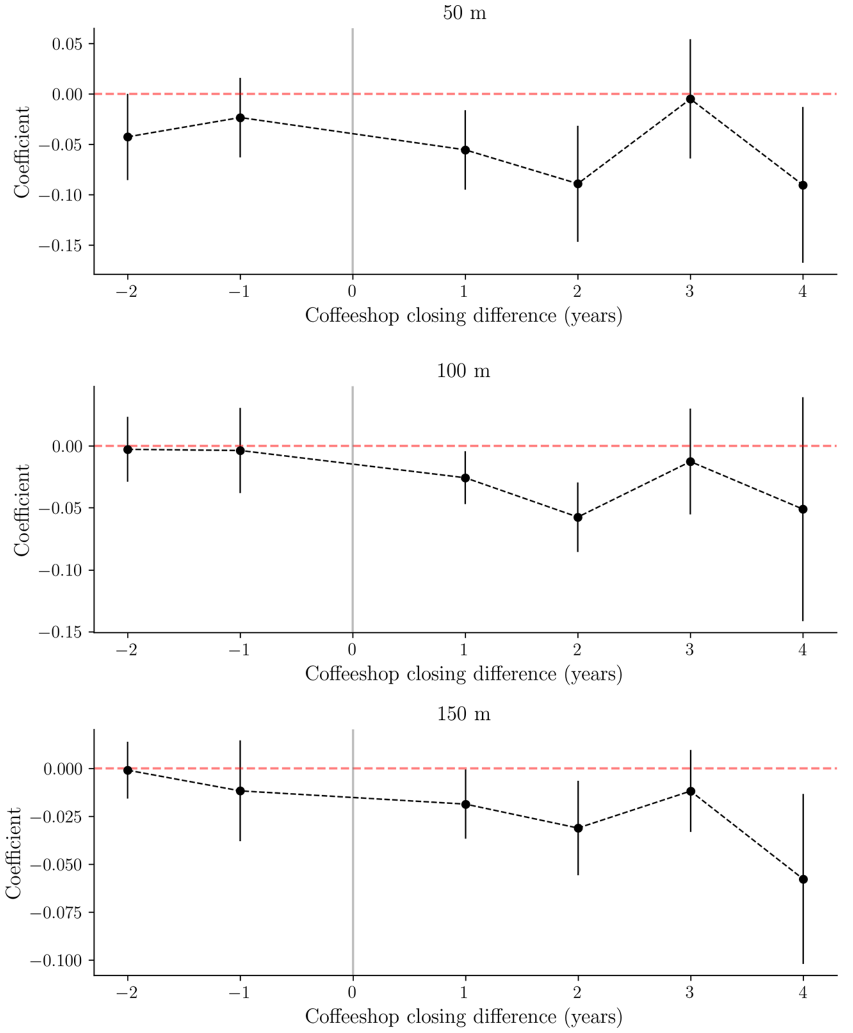

FIGURE 5 Closing effects over time (different cutoff distances). *Notes*: Similar to Autor (2003), we plot the point estimates for coefficients γ_z in Equation (2), including error bars $\pm 1.67 * SE$, representing 10% significance. Intervals are relative to coffeeshop closings and approximately 365 days long. [Color figure can be viewed at wileyonlinelibrary.com]

properties that sold three times, and 249 properties that sold four times during the sample period.[31] As in the DID approach, we compare homes nearby closing coffeeshops with homes nearby remaining coffeeshops. We use the same cutoff distances, the same time window ($\pm$ 4 years), and the same holdout period (30 days before and 60 days after coffeeshop closings).

We estimate the following empirical model:

$$\frac{\Delta p_{i(t+n)}}{p_{it}} = \alpha + \gamma_1 \Delta Q'_{i(t+n)} + \gamma_2 Y'_{i(t+n)} + \gamma_3 \Theta'_{in} + \gamma_4 \Delta CS_{i(t+n)} + \gamma_5 \Lambda_{i(t+n)} + \epsilon_{i(t+n)} \tag{3}$$

[31] Twenty properties sell more than four times during the sample period and are excluded from the analysis, considered as outliers.

TABLE 5 DID estimation results with different holdout periods

Nearby coffeeshop* Postclosing	(1) 150 m	(2) 100 m	(3) 50 m	(4) Postcode
Post-5 days holdout	−0.016*	−0.018	−0.061**	−0.070**
	[0.009]	[0.013]	[0.026]	[0.031]
Post-10 days holdout	−0.015	−0.016	−0.058**	−0.070**
	[0.009]	[0.013]	[0.026]	[0.031]
Post-30 days holdout	−0.015	−0.016	−0.061**	−0.070**
	[0.009]	[0.013]	[0.026]	[0.031]
Post-60 days holdout	−0.018**	−0.025**	−0.072**	−0.071**
	[0.009]	[0.012]	[0.030]	[0.034]
Post-90 days holdout	−0.019**	−0.023*	−0.069**	−0.061*
	[0.009]	[0.013]	[0.030]	[0.033]
Post-180 days holdout	−0.018*	−0.028**	−0.070**	−0.060
	[0.010]	[0.014]	[0.031]	[0.040]

Notes: Table 5 reports the estimations results for different holdout periods (excluded from estimation). The base holdout period of 60 days is highlighted for comparison purposes. The dependent variable is the logarithm of transaction price. House characteristics include size, type, quality, construction period, number of floors, number of rooms, number of bathrooms, type of heating system, type of parking place, presence of garden, thermal quality, location of the home relative to city center, road, park, water, and forest. In all regressions, location and year of transaction dummies are also included. Location fixed effects are included at the five-digit postcode level. Homes nearby closing coffeeshops but within 150 m of a remaining coffeeshop are excluded from the analysis. The control group cutoff distance is similar to the treatment group cutoff distance (150 m, 100 m, 50 m, and postcode), as illustrated in Figure 3. Heteroskedasticity-robust standard errors are in brackets. Standard errors are clustered by municipality and transaction year. *$p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

where the dependent variable $\frac{\Delta p_{i(t+n)}}{p_{it}}$ represents the percentage change in transaction price of property i between date t and $t+n$. We control for changes in property characteristics, such as internal–external quality, insulation level, number of rooms, presence of roof terrace, presence of attic, monumental status, presence of garden, parking opportunity, heating type, and presence of free view. These changes are denoted 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 location, using the five-digit neighborhood postcode (Θ'_{in}). The change in coffeeshop proximity 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 proximity due to closings, our variable of interest. 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 $\Lambda_{i(t+n)}$, where $\Lambda_{i(t+n)}$ is time difference (measured in 100 days) between $t+n$ and z, the closing date of the closest coffeeshop.

$$\Lambda_{i(t+n)} = \begin{cases} (t+n) - z & \text{if } \Delta CS_{i(t+n)} = 1 \\ 0 & \text{if } \Delta CS_{i(t+n)} = 0 \end{cases} \tag{4}$$

Because of the limited number of repeated sales in the dataset, we implement the repeated sales analysis focusing only on the 150-m cutoff distance. 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. When we look at the percentage change in price

TABLE 6 Repeated sales analysis

	(1)	(2)
$\Delta CS_{i(t+n)}$ (1 = coffeeshop closing)	-0.085^{**}	-0.143
	[0.041]	[0.089]
$\Lambda_{i(t+n)}$ (100 days after closing)		0.009
		[0.010]
Change in house characteristics	Yes	Yes
Location fixed effects	Yes	Yes
Time fixed effects	Yes	Yes
Number of observations	373	373
Adj. R-squared	0.50	0.50

Notes: Table 6 reports the estimations results for the sample of homes that are sold repeatedly. The dependent variable is the percentage change in transaction price. House characteristics include size, quality, type of heating system, type of parking place, thermal quality, location of the home relative to road and park. In all regressions, location and year of transaction dummies are also included. Location fixed effects are included at the five-digit postcode level. Homes nearby closing coffeeshops but within 150 m of a remaining coffeeshop are excluded from the analysis. Cutoff distance is defined as 150 m for control and treatment groups. The control group includes the homes nearby remaining coffeeshops at 150 m cutoff distances. Heteroskedasticity-robust standard errors are in brackets. Standard errors are clustered by municipality and transaction year. $^{*}p < 0.10$. $^{**}p < 0.05$. $^{***}p < 0.01$.

between sales pairs over the coffeeshop closing time difference for treatment and control groups (Figure A2), we observe no systematic difference between groups.

Table 6, Column (1) reports the result of the estimation of Equation (3), without considering the time difference between the sales of treatment homes and coffeeshop closings ($\Lambda_{i(t+n)}$). We document that homes experiencing a coffeeshop closing between the first and second sales fall on average 8.5% in value compared to homes nearby remaining coffeeshops. When we subsequently estimate Equation (2), controlling for the time differences in days between coffeeshop closings and second home sales at $t + n$, we document no significant effect for coffeeshop closings and neither is there an effect for time differences.[32]

5 | DISCUSSION

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 our focus on 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) conducted a survey among teenagers and local residents regarding the effects of 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. Besides, 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.

[32] Even though we consider the repeated sales analysis as a robustness check, there are some important limitations to consider. The number of repeated sale 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 estimated coefficients in the repeat sales analysis with some caution.

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 5–7% 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.[33] 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, 2016). 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 postclosing situation (operational retail vs. an empty store).

To test for the influence of the postclosing situation, we examine the developments of the former coffeeshop locations over time. We visited all closed coffeeshop locations in Rotterdam and Amsterdam to collect information on postclosing usage as well as the time it took for the vacant store to be reused. 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 basis, 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 reused. Often, the coffeeshop storefront remains as long as the site is vacant. On average, it takes around 781 days until a new business is opened in the former coffeeshop space. Figure 6 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), and restaurants.

We test whether vacancy has an effect on our results by distinguishing between vacant and nonvacant postclosing locations, considering the status at transaction time t of property i.[34] Building on the DID model, Equation (3) shows the model for our analysis,

$$\ln(p_{it}) = \alpha_{it} + bX'_{it} + \gamma_1 Nearby_{it} + \gamma_2 Post_{it} + \gamma_3 Nearby_{it} * Post_{it} + \gamma_4 Vacant_{it} + \epsilon_{it} \qquad (5)$$

where $Vacant_{it} = 1$ indicates that the coffeeshop location is vacant at postclosing transaction time t. As we only look at postclosing locations, $Vacant_{it} = 1$ implies that $Nearby_{it} * Post_{it} = 1$.

Table 7 shows the result for the estimation of Equation (5). Overall, we do not find significant price differences for vacant and nonvacant postclosing states. Only at a 150-m cutoff distance and a 10% significance level, we document a positive effect for homes nearby vacant places compared to homes nearby nonvacant coffeeshop locations. Our results regarding the effect of the coffeeshop closings are hardly affected by including the vacancy information in the analysis. One explanation for our findings could be quality differences of succeeding businesses, such as Shisha lounges.

[33] We also tried to examine the relation between coffeeshop closings and local crime rates. However, crime data are available only as an aggregate statistical measure, resulting in insufficient variation for a convincing analysis.

[34] Note that there are business quality differences for nonvacant locations, but the small number of observations and examples of exceptions prevent us from distinguishing further by business type.

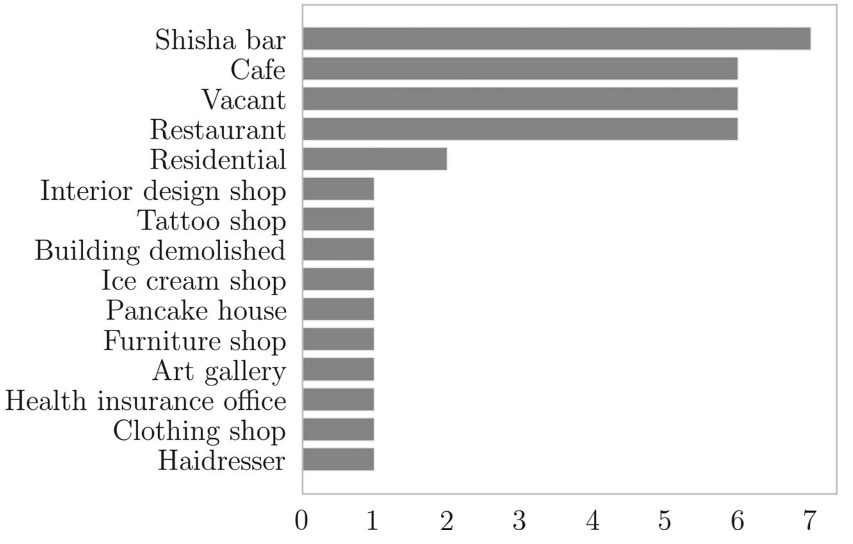

FIGURE 6 Postclosing use of closed coffeeshop locations. *Notes*: Figure 6 presents the frequency of postclosing usage cases for closed coffeeshop locations in Rotterdam and Amsterdam. We group different usage cases where possible. One coffeeshop could not be inspected.

TABLE 7 Vacancy analysis: DID estimation results

	(1) 150 m	(2) 100 m	(3) 50 m
Nearby coffeeshop (1 = yes)	−0.010	0.010	0.012
	[0.008]	[0.017]	[0.024]
Nearby coffeeshop* Postclosing (1 = yes)	−0.030**	−0.035**	−0.064**
	[0.012]	[0.017]	[0.025]
postclosing (1 = yes)	0.034***	0.027**	0.017
	[0.011]	[0.011]	[0.015]
Vacant (1 = yes)	0.025*	0.009	−0.011
	[0.013]	[0.014]	[0.030]
House characteristics	Yes	Yes	Yes
Location fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
Observations	9,632	5,344	1,585
Adj. R-squared	0.80	0.81	0.84

Notes: Table 7 reports the DID estimation results, controlling for vacancy situation of the closed coffeeshop location. *Vacant* is a dummy variable, which takes a value of one if the closed coffeeshop location is vacant at the time of transaction, and zero if the coffeeshop is still open or the closed coffeeshop location is not vacant. 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. The dependent variable is the logarithm of transaction price. House characteristics include size, type, quality, construction period, number of floors, number of rooms, number of bathrooms, type of heating system, type of parking place, presence of garden, thermal quality, location of the home relative to city center, road, park, water, and forest. In all regressions, location and year of transaction dummies are also included. Location fixed effects are included at five-digit postcode level. Homes nearby closing coffeeshops but within 150 m of a remaining coffeeshop are excluded from the analysis. The control group cutoff distance is similar to the treatment group cutoff distance (150, 100, 50 m), as illustrated in Figure 3. Heteroskedasticity-robust standard errors are in brackets. Standard errors are clustered by municipality and transaction year. *$p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

6 | CONCLUSION AND IMPLICATIONS

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 DID 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 their characteristics, we can adjust properly for changes in housing quality.

We document negative local house price effects when nearby coffeeshops close. In the DID analysis, the effect ranges from −1.8 for homes located up to 150 m away to −7.5% for homes up to 50 m away, and −7.4% for homes within the same six-digit postal code area, all compared to homes near the closest coffeeshop that remains open. This result is robust to different holdout periods, taking care of potential price stickiness in local housing markets. We further find that the effects seem to fade out after 2 years. The repeated sales analysis verifies our results 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 8.53% decrease in price.

We subsequently discuss potential causation channels for these effects. Chang and Jacobson (2017) show that closings of medical cannabis dispensaries in the United States 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 located remain vacant for more than 2 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 do not find convincing evidence that vacancy increases the negative effect of coffeeshop closings.

The findings in this paper have some implications for policy makers 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. This could further suggest that positive opening effects, such as in Conklin et al. (2017) or Cheng et al. (2018), are persistent over time and not just due to some "hype." This potential future outlook might be useful for policy makers in early adoption markets, which recently legalized or/and opened cannabis dispensaries.

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APPENDIX A

TABLE A1 Property age distribution (in percent)

Age group	Control group	Treatment group
0–10 years	2.21	1.76
11–20 years	9.29	7.36
21–30 years	6.66	7.27
31–40 years	3.02	1.99
41–50 years	0.64	0.60
51–60 years	1.65	5.42
61–70 years	2.16	1.85
71–80 years	7.03	10.28
81–90 years	4.67	3.24
91+ years	62.68	60.23

Notes: Table A1 shows the property age distribution (in percent) for the treatment and control group, as defined in Table 3. The χ^2 statistic is 0.1157 with a p-value of 0.999. We therefore, conclude that the property age distribution does not differ statistically between groups.

TABLE A2 Control variables

Structural	Apartment type
Size (m^2)	Upstairs apartment
Number of floors	Two-floor apartment
Number of rooms	Maisonette apartment
Number of bathrooms	Old block apartment
D: Roof terrace	New block apartment (suburb)
D: Balcony	**Apartment quality**
D: Garden	Apartment quality: normal
D: Garden quality good	Apartment quality: luxurious
D: Basement	D: Elevator
D: Attic	
D: Monument status	**Parking available**
	D: Parking lot
Construction period	D: Carport
Construction 1500–1905	D: Single garage
Construction 1931–1945	D: Multi garage
Construction 1945–1959	D: Garage and carport
Construction 1960–1970	
Construction 1971–1980	**Insulation (dummy)**
Construction 1981–1990	One level of insulation
Construction 1991–2000	Two levels of insulation
Construction 2001 and later	Three levels of insulation
	Four levels of insulation
House type I	Five or more levels of insulation
Caravan	
Living boat	**House type II**
Recreational home	Terraced house
Single home	Corner house
Grachtenpand (old house at canal)	Semi-detached house
Manor house (without land)	Detached house
Manor house (with land)	
Old farm house	**Realtor location assessments**
Bungalow	D: in city center
Villa	D: next to forest
Landhouse	D: next to park
	D: next to river or lake

(Continues)

TABLE A2 (Continued)

Structural	Apartment type
Heating type	D: next to busy road
D: Heating: coal or oven	D: next to quiet road
D: Heating: central or tele-heating	D: good view
D: Heating: AC or solar	
Other	
Maintenance quality ratings (inside & outside)	
Time fixed effects (year)	

Notes: Base categories are for the construction period it is "construction 1906–1930," for house type I it is "row house," for house type II it is "simple house," for apartment type it is "ground floor," for apartment quality it is "bad", for garden quality it is "normal," for heating type it is "no heating," for insulation it is "no insulation," and for parking type it is "no parking."

TABLE A3 Difference-in-differences approach: groupings

	Treatment		Control	
Cutoff	**Preclosing**	**Postclosing**	**Preclosing**	**Postclosing**
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: Table A3 reports the number of observations for treatment and control groups based on different cutoff options. The treatment group consists of homes that are sold nearby closing coffeeshops (before and after closing), and control group includes homes that are sold nearby remaining coffeeshops.

TABLE A4 Transactions by closing date and city (150 m cutoff)

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

Notes: Table A4 reports the number of transactions in the treatment group and control group (150 m cutoff distance), separated by different closing dates and cities). A closing date is always the first day of the month.

TABLE A5 Robustness DID: Amsterdam only

	(1) 150 m	(2) 100 m	(3) 50 m	(4) Postcode
Nearby coffeeshop (1 = yes)	−0.009	−0.013	−0.033	0.022
	[0.010]	[0.028]	[0.027]	[0.043]
Nearby coffeeshop * Postclosing (1 = yes)	−0.019**	−0.030***	−0.066**	−0.057
	[0.009]	[0.010]	[0.027]	[0.041]
Postclosing (1 = yes)	0.031**	0.017	−0.010	0.039
	[0.013]	[0.014]	[0.021]	[0.025]
House characteristics	Yes	Yes	Yes	Yes
Location fixed-effects	Yes	Yes	Yes	Yes
Time fixed-effects	Yes	Yes	Yes	Yes
Adj. *R*-squared	0.834	0.837	0.857	0.861
Observations	8,854	4,420	1,321	456
Treatment group	1,395	584	158	88

Notes: As the financial crisis period coincidences with the time of closings in Rotterdam and The Hague, we perform a subsample analysis for the city of Amsterdam, where closings took place only after the financial crisis. Table A5 reports the DID estimations results. The dependent variable is the logarithm of transaction price, and we use the same controls as in the analysis of Table 4. Heteroskedasticity-robust standard errors are in brackets. Standard errors are clustered by municipality and transaction year. *p <0.10. ** $p < 0.05$. *** p<0.01.

TABLE A6 Robustness DID: Apartments only

	(1) 150 m	(2) 100 m	(3) 50 m	(4) Postcode
Nearby coffeeshop	−0.010	−0.018	−0.030	0.010
(1 = yes)	[0.008]	[0.020]	[0.018]	[0.055]
Nearby coffeeshop * Postclosing	−0.018**	−0.020*	−0.075***	−0.067**
(1 = yes)	[0.008]	[0.012]	[0.026]	[0.033]
Postclosing	0.033**	0.019	0.001	0.056**
(1 = yes)	[0.014]	[0.016]	[0.022]	[0.024]
Apartment characteristics	Yes	Yes	Yes	Yes
Location fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Adj. *R*-squared	0.814	0.808	0.828	0.803
Total number of observations	11,683	5,682	1,563	567
N. Observations in treatment group	1,739	721	186	113

Notes: Table A6 reports the DID estimation results similar to Table 4, however, for apartments only. The dependent variable is the logarithm of transaction price. We use the same control variables, fixed effects, clustered standard errors, and groupings as in our price estimation, shown in Table 4. *p <0.10. ** p <0.05. *** p <0.01.

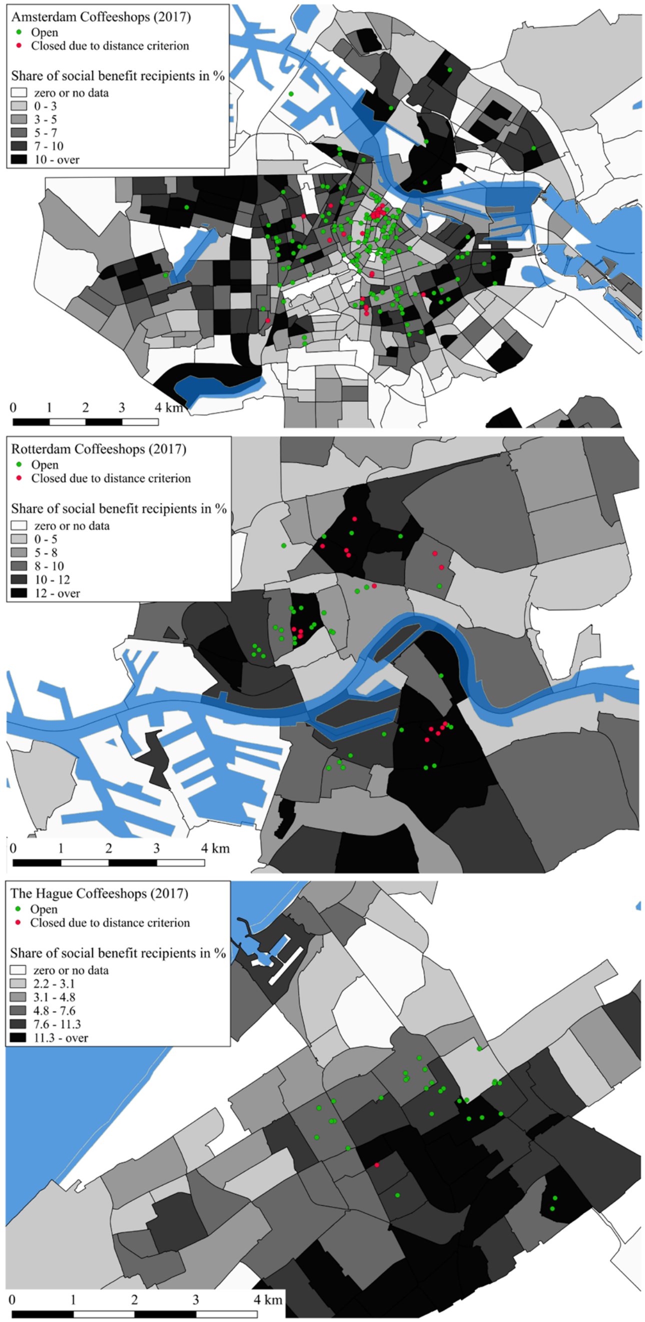

FIGURE A1 Distribution of coffeeshops in Amsterdam, Rotterdam, and The Hague. *Notes*: Figure A1 illustrates the locations of coffeeshops over different neighborhoods in the three biggest cities. We focus on coffeeshops that are open today (late July 2017) and coffeeshops that closed due to the distance criterion. We use the percentage of social benefit recipients (unemployment benefits and long-term benefits) as a proxy for social status of neighborhoods. [Color figure can be viewed at wileyonlinelibrary.com]

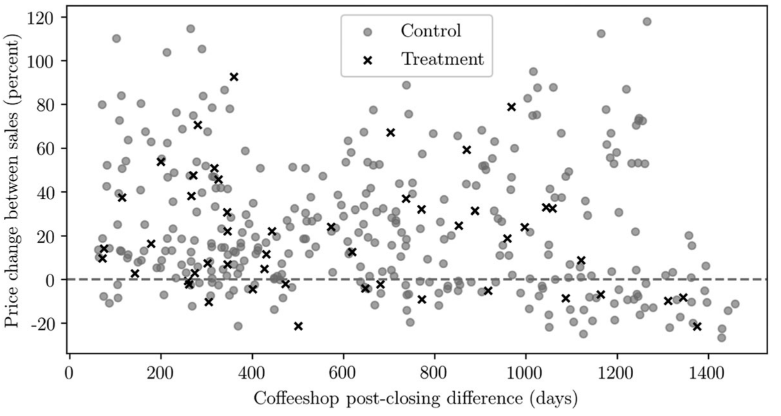

FIGURE A2 Repeated sales—price difference over postclosing time. *Notes*: Figure A2 presents the percentage price difference between repeated sales over the postclosing time. We only consider repeated sales occurring over coffeeshop closing and measure the change in price between sales. Cutoff distance is defined as 150 m for control and treatment groups. The treatment group consists of homes that are sold repeatedly nearby closing coffeeshops (before and after closing), and the control group includes homes that are sold repeatedly nearby remaining coffeeshops (both within 150 m).