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# Getting High or Getting Low?

## The External Effects of Coffeeshops on House Prices

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### **Abstract**

The legalization of cannabis is a hotly contested policy topic. While beneficial to some, cannabis dispensaries may create a negative externality for others. This paper studies the external effects of coffeeshops – Dutch cannabis sales facilities – on local house prices. Controlling for hedonic property characteristics, we use distance to coffeeshops as a measure for proximity to externalities. We employ a difference-in-difference analysis around a recent change in regulation, leading to exogenous coffeeshop closings.

Contrary to expectations, we document that coffeeshop closings have negative effects on home values. Compared to homes nearby remaining coffeeshops, homes nearby closing coffeeshops decrease 1.6 to 7.8 percent in value after closings. We further document a coffeeshop-proximity discount of around -0.8 to -2.0 percent, increasing with closeness. Both findings are robust to several sub-tests, and show that coffeeshops are generally located in lower priced areas, but closing them does not restore value.

### **Keywords:**

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

### **JEL codes:**

R11, R21, R23, R31, R52, R58

# 1 Introduction

In many countries around the world, attitudes towards cannabis usage have been changing, leading to decriminalization, toleration, and even legalization policies. Countries like Canada, Uruguay, as well as several U.S. states have recently legalized recreational cannabis.<sup>1</sup> In some other countries, including Portugal and the Netherlands, cannabis is decriminalized, meaning it remains illegal, but charges are usually not enforced.<sup>2</sup> The motives for these steps are manifold, but common arguments include the lack of a causal relationship between cannabis and crime, successful toleration tests, fighting organized crime, and negative cost-benefit relationships of prosecution (Charilaou et al., 2017).

The discussion on cannabis legalization increases the need for scientific evidence on its potential effects on society. Legalization results in newly established industries, with unknown effects, potentially providing employment opportunities and tax income. Consumers pay VAT and are no longer exposed to illegal activities. However, legalization of cannabis might lead to increase consumption (see e.g. Jacobi & Sovinsky, 2016), potentially affecting health care costs, crime rates and productivity (Marie & Zölitz, 2017). There might also be external effects from the increased exposure to cannabis related facilities, such as in a recent case in Colorado, where a couple argued that their property lost value due to the opening of a nearby cannabis growing facility, creating "pungent, foul odors"<sup>3</sup>

To shed light on the external effects of cannabis dispensaries, we examine their effect on the immediate neighborhood, using local housing markets as preference measures. The Netherlands was the first country in Europe that made cannabis usage, possession and sale effectively legal as early as 1976, with the intention to

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<sup>1</sup>In the US, Colorado and Washington were the first states to approve recreational usage in 2012: <http://www.reuters.com/article/us-usa-marijuana-legalization-idUSBRE8A602D20121107>

<sup>2</sup>Cannabis remains illegal under European Union law, which has primacy over national laws.

<sup>3</sup><https://www.denverpost.com/2018/10/28/denver-marijuana-property-tax-lawsuit/>

reduce users' exposure to hard drugs.<sup>4</sup> Cannabis sales are tolerated, however, strict regulations regarding possession and sales apply. Cannabis can only be bought by people over 18 years old – up to a certain daily amount – in so-called "coffeeshops".

Similar to liquor stores in the US, coffeeshops attract a certain clientele as their sole purpose is to sell and to smoke cannabis. Coffeeshops are therefore negatively judged in the Dutch society, as they seem to be a source of negative external effects. Some of these might be from users and tourists, crowding around coffeeshops and creating noise, traffic and odor-related nuisance.<sup>5</sup> Other concerns are due to the potential effects on teenagers' consumption behavior. Sometimes, illegal dealers loiter in the area to sell their own cannabis, either as a competition or to circumvent daily sales limits, potentially creating safety issues. As discussed further in the next section, cannabis wholesale remains illegal, making supply chains partly illegal, exposing coffeeshops to organized crime.

The empirical evidence on the effects of the recent wave of decriminalization of cannabis use is mixed, but mostly focuses on crime. While some studies show significantly positive effects of decriminalization on use of cannabis (Cerdá et al., 2012; Pacula et al., 2010; Wall et al., 2011), others find no significant effects (Anderson & Rees, 2014; Chu, 2015; Harper et al., 2012; Lynne-Landsman et al., 2013; Morris et al., 2014). In addition to a reduction in crime rates, Hunt et al. (2018) document a slight increase in "driving under the influence" (DUI) arrests after dispensaries' openings. Focusing on local crime effects and examining dispensary *closings*, Chang and Jacobson (2017) document *higher* crime rates nearby in the short run. However, the authors document similar effects for restaurant closings and therefore argue that retail activities are better than vacancy, as they "provide informal security through their customers," arguably in line with the "eyes upon the street" theory by Jacobs (1961).

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<sup>4</sup>Officially, cannabis usage is just tolerated as it remains illegal under EU law.

<sup>5</sup>Appendix Figure A presents street-scene impressions 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). Studies on similar external effects show significantly negative property price effects for noise (Theebe, 2004; Yiu & Wong, 2005; Li & Brown, 1980) and air pollution (Harrison & Rubinfeld, 1978; Hite et al., 2001). Giambona and Ribas (2018) show that the forced closing of prostitution windows in Amsterdam has a positive effect on property prices nearby due to the removal of nuisance and crime. Focusing on meth labs, Dealy et al. (2017) report a 6.5 percent decrease in property prices that are located close to a discovered meth lab, where Congdon-Hohman (2013) even reports a 10 to 19 percent discount after discovery. Thus, even more important for locational value than actual crime rates is the perception about safety in a neighborhood (Cohen, 2008; Linden & Rockoff, 2008).

So far, only Conklin et al. (2017) and Cheng et al. (2018) examine the effect of cannabis dispensaries on property prices directly. These sister studies use the same research area and period, focusing on a change of medical to recreational cannabis dispensaries in Denver, Colorado.<sup>6</sup> Both studies find an increase in housing values ranging from 6 to 8 percent for properties nearby dispensaries that switch from medical to recreational cannabis sales.

This paper adds to the ongoing debate on the societal effects of a less stringent cannabis policy, examining the implications of 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 about analyzing the effects of coffeeshop openings, such endogenous location choices or anticipation effects.

We examine the effect of exogenous coffeeshop closings on nearby property prices,

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<sup>6</sup>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.

using a difference-in-difference approach. In 2012, the Dutch government ruled that coffeeshops located in the vicinity of schools (within 250 m) should be closed. However the implementation of the rule was left to the individual municipalities. As municipalities had different perspectives on the rule and some coffeeshops started legal proceedings, closings were carried out in different waves between 2009 and 2017, providing variation over time.<sup>7</sup> This empirical setting provides an exogenous closing shock, independent of neighborhood perception and time-confounding factors, allowing for identification of the effects of cannabis dispensaries on property prices.

We employ a sample of 1.75 million housing transactions between 2000 and 2017, reflecting approximately 75 percent of all transactions in the Netherlands. The dataset contains extensive information on home characteristics, such as transaction price, asking price, time-on-the-market and location. Furthermore, we have location and status information on all coffeeshops that operated in the Netherlands since 1999. For the three major Dutch cities, reflecting more than 44 percent of all coffeeshops in the Netherlands, we have information on all school distance-related closings, including date of closing. We match all information by using individual location information, calculating distances to coffeeshops for each housing transaction between 2000 and 2017.

Compared to properties nearby remaining coffeeshops our results document a closing discount of 1.6 to 7.8 percent for homes nearby closing coffeeshops. We also document that properties nearby coffeeshops sell at a discount compared to properties further away, which in combination with our results, leads to the likely conclusion that coffeeshop locations might be endogenous to price.

Our study shows that the school distance criterion had negative effects on already discounted property areas. One potential reason was that former coffeeshop locations

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<sup>7</sup>The municipality of Rotterdam came up with the "closing rule", inspiring other municipalities and the national government. Some municipalities carried out closings before the government issued its ruling.

remained empty after closings. Even though owners got the opportunity to transform their business into a regular cafe, not all owners took this options. In general, we can not confirm that removing coffeeshops has positive effects on local house prices.

## 2 Coffeeshops in the Netherlands

### 2.1 Government Policy on Coffeeshops

In 1976, the Netherlands implemented a new policy on cannabis use. 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).<sup>8</sup> In order to officially control the sale of cannabis, the government legally tolerated selling facilities, or coffeeshops. Since 1991, coffeeshops have to fulfill five criteria: no sales to minors (over 18), 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).<sup>9</sup>

Coffeeshops emerged 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

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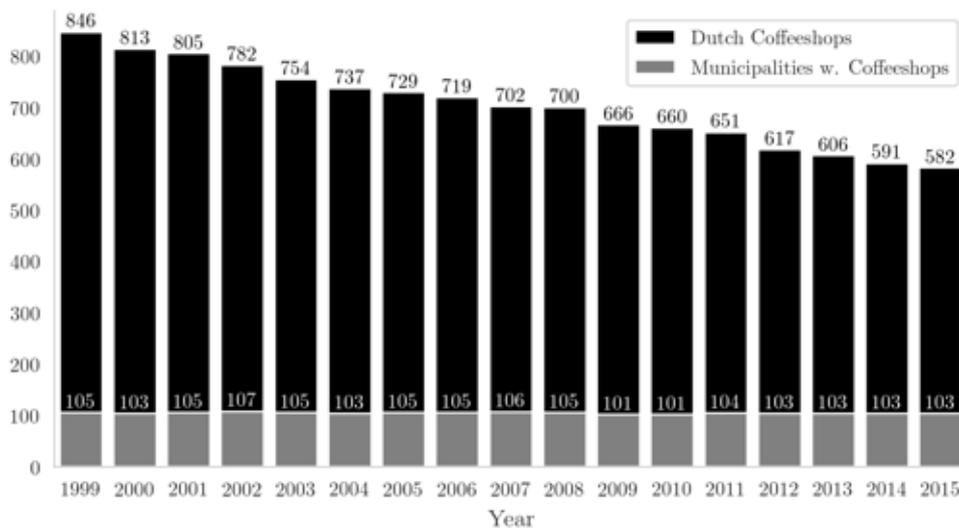
<sup>8</sup>Initially, a violation of up to 30 grams was not enforced, but the amount was lowered in 1995 (MacCoun & Reuter, 1997).

<sup>9</sup>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).

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 drastic reduction in the number of coffeeshops (Tops et al., 2001), as illustrated in Figure 1. Many cities tried to close coffeeshops, whereas others added additional operating restrictions.<sup>10</sup> 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).

**Figure 1**  
Development of Coffeeshops in the Netherlands



Notes: Development of coffeeshops over time, showing the number of coffeeshops in the Netherlands and the number of municipalities with coffeeshops (Bieleman et al., 2015a).

In recent years, the policy on coffeeshops became stricter, trying to tackle the

<sup>10</sup>Wouters (2013) find that the number of open coffeeshops is, among other, positively correlated with the number of local progressive politicians.

“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 (BIBOB) was implemented, aiming to cut coffeeshops from illegal activities.<sup>11</sup> Amongst 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 pretense to close coffeeshops in the past (e.g. in gentrification projects). Additionally, the “backdoor problem” is still prevalent, since it can only be solved by changing the liberal policy on cannabis, either by legalizing cultivation or forbidding coffeeshops completely (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. Although there is no direct empirical evidence for the effect of this policy on hard drug usage rates, there are some studies showing that coffeshop availability decreases the likelihood of illegal cannabis sourcing, 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 side, the availability of coffeeshops might increase soft drug usage, potentially causing a negative externality on society due to the potential health effects. Investigating the effect of nearby coffeeshops on soft drug usage, Wouters

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<sup>11</sup>BIBOB: *bevordering integriteitsbeoordelingen door het openbaar bestuur* (Public Administration Probity Screening Act)

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et al. (2012) find no evidence of increased cannabis or hard drug usage rate for coffeeshop proximity. However, they find that buying in coffeeshops leads to more regular usage and increased amounts consumed among users. 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, right at the time of the coffeeshop expansions. These results are in line with MacCoun and Reuter (1997), who examine countries' policies on drugs, comparing the Netherlands with other countries. Their findings indicate that the commercialization of cannabis access correlates with growth in the drug-using population.

Surveying the neighbors of coffeeshops in Rotterdam on the potential 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 neighbourhoods 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. Coffeeshops are extremely profitable businesses, with an estimated combined revenue of nearly €1 billion in the Netherlands in 2008, leading to an average revenue per shop of €1.7 million.<sup>12</sup> Based on these revenue estimates, coffeeshops pay more than €200 million in annual taxes. Coffeeshop associations also claim to do good to the local economy. According to the Maastricht association of coffeeshops, drug tourists in 2008 spent €140 million in other local businesses, creating an economic spillover effect.<sup>13</sup>

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<sup>12</sup>There are no official numbers, these were estimated by a national newspaper: <https://www.nrc.nl/nieuws/2016/01/02/omzet-coffeeshops-bedraagt-ongeveer-een-miljard-euro-a1410406>. Other estimations range between €800 million to €1.2 billion, as mentioned in the article.

<sup>13</sup>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.

## 2.3 The Distance Criterion

In the early 2000s, several municipalities decided to restrict the presence of coffeeshops around schools to protect children and teenagers from drug usage. In example, the city of The Hague decided to implement the distance criterion (*afstandscriterium*) among the first in 2007, forcing all coffeeshops within a linear distance of 500 meters from secondary schools to close.<sup>14</sup> The seven affected coffeeshops had to close within 2 years. However, after some discussions, the criterion was adapted to a new distance of 250 meters (Municipality of Amsterdam, 2007). Due to the reduction in distance, only one coffeeshop was affected and forced to close. In 2009, the shop finally closed its doors.

At the end of 2012, the national government proposed to implement the distance criterion nationally as of January 1<sup>st</sup>, 2014. However, municipalities were free to adopt the distance criterion and allowed to change its specifications. 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.<sup>15</sup> By the beginning of 2015, 41 coffeeshops were affected by the criteria, however mostly located in Amsterdam and Rotterdam (Bieleman et al., 2015a, 2010).

Amsterdam and Rotterdam, the two major affected cities, handled the situation quite differently. The city of Rotterdam can be considered as a forerunner and inventor of the criterion, implementing it among the first in 2009.<sup>16</sup> In contrast, the city of Amsterdam was rather critical towards the law, as it showed no positive

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<sup>14</sup>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 normally until age 16 to 18.

<sup>15</sup>Other municipalities adjusted the criterion by using different distance measures, different distances, and/or different school forms.

<sup>16</sup>One of the key persons during the time of the implementation decision was Ivo Opstelten, mayor of Rotterdam (1999 - 2009) and later Minister of Security and Justice (2010 - 2015), among others responsible for the policy on coffeeshops.

effects in Rotterdam. Additionally, the city faced a well-organized coffeeshop lobby. Therefore, the city hesitated to close the 27 affected coffeeshops directly and implemented the distance criterion slowly, in four stages.<sup>17</sup>

Stage one was implemented as of January 1<sup>st</sup>, 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 visible from schools. In January 2015, stage three became effective, closing three coffeeshops that were located within 150 meters walking distance of a school. Stage four was postponed by one year until January 2017 due to resistance of the coffeeshop lobby, leading to the closing of eight additionally coffeeshops within 250 meters of schools.<sup>18</sup> Due to a school relocation in 2017, six coffeeshop were reevaluated and eventually three had to close (Bieleman et al., 2015b).<sup>19</sup>

### 3 Data

#### 3.1 Data Sources

Our data set 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.<sup>20</sup> The database holds information for the last 20 years and is maintained and used mainly by cannabis users. It contains information on coffeeshop address and opening status. In addition, users share information on cannabis varieties, prices and qualities. The database does not contain information on closing reasons

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<sup>17</sup><https://www.nrc.nl/nieuws/2017/01/04/als-de-coffeeshop-sluit-gaat-de-handel-de-straat-op-6023362-a1539697>

<sup>18</sup>In the meantime, one coffeeshop was closed due to BIBOB violations.

<sup>19</sup>Due to missing observations nearby these coffeeshops, they are not considered in the analysis.

<sup>20</sup><http://www.coffeeshopdirect.com/index.htm>

and dates. Since the database is not official, we validate all coffeeshop locations in the biggest cities (as described below) and perform random spot-checks.

Figure 2 shows the distribution of coffeeshops in the Netherlands and the number of coffeeshops per municipality in 2017, where white indicates coffeeshop absence. In line with Figure 1, we document that only 103 out of 388 municipalities in 2017 actually allow coffeeshops. Since the number of municipalities with coffeeshops stays rather constant over time while the number of coffeeshops decreases, we argue the decrease in coffeshops is systematic over all municipalities. 44 percent of all Dutch coffeeshops are located in the three biggest cities: Amsterdam, Rotterdam, and The Hague. Table 1 provides an overview of the number of coffeeshops in our sample. With 28 percent, Amsterdam does not only have the most coffeeshops, but also the most school distance criterion related-closings.

**Table 1**  
**Coffeeshop Sample Overview**

City	Number of Coffeeshops		Closed due to distance criterion	Total
	Open	Closed		
Amsterdam	171	166	26	337
The Hague	55	24	1*	79
Rotterdam	38	30	17	68
Others	345**	144	-	489
Total:	609	364	44	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. \* One coffeeshop was affected by the 250 m criteria. However instead of closing, it moved to a new location which became available due to a BIBOB related closing of another shop. We still consider the closing location in our analysis. \*\* Since our total sample does not match the reported numbers of (Bieleman et al., 2015a), as shown in Figure 1, and there are no openings since 2015, there must be open coffeeshops in our sample that are either closed or never officially existed (we found illegal sales locations in the verification process). However, we only use verified locations from the biggest cities in our analysis.

Since our data source does not provide information on closing dates, we contact all municipalities individually. As described in section 2.3, different municipalities

implemented the distance criterion differently and sometimes exceptions occurred, such that coffeeshops closed due to law violations instead of school distance. To verify our data, we contacted the ten biggest cities to get information on coffeeshops and their closing times.<sup>21</sup> We confirmed that only the three major cities Amsterdam, Rotterdam and The Hague actually experienced closings due to the distance criterion, and we therefore concentrate our analysis on these cities. However, we argue that this should not affect the external validity of our analysis, given the outsized share of these cities in the sample.

Figure 3 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 a proxy, as local income statistics are not available. We combine people receiving short-term unemployment benefits and long-term social security.<sup>22</sup> We document that coffeeshops in Rotterdam and The Hague are more likely to be located in neighborhoods with a high share of social benefit recipients, whereas coffeeshops in Amsterdam are located rather in the city center, with a lower share of social benefit recipients. Anecdotally, coffeeshops in Amsterdam cater to tourists, and as such are likely to be located in tourist areas.

The underlying housing data come 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

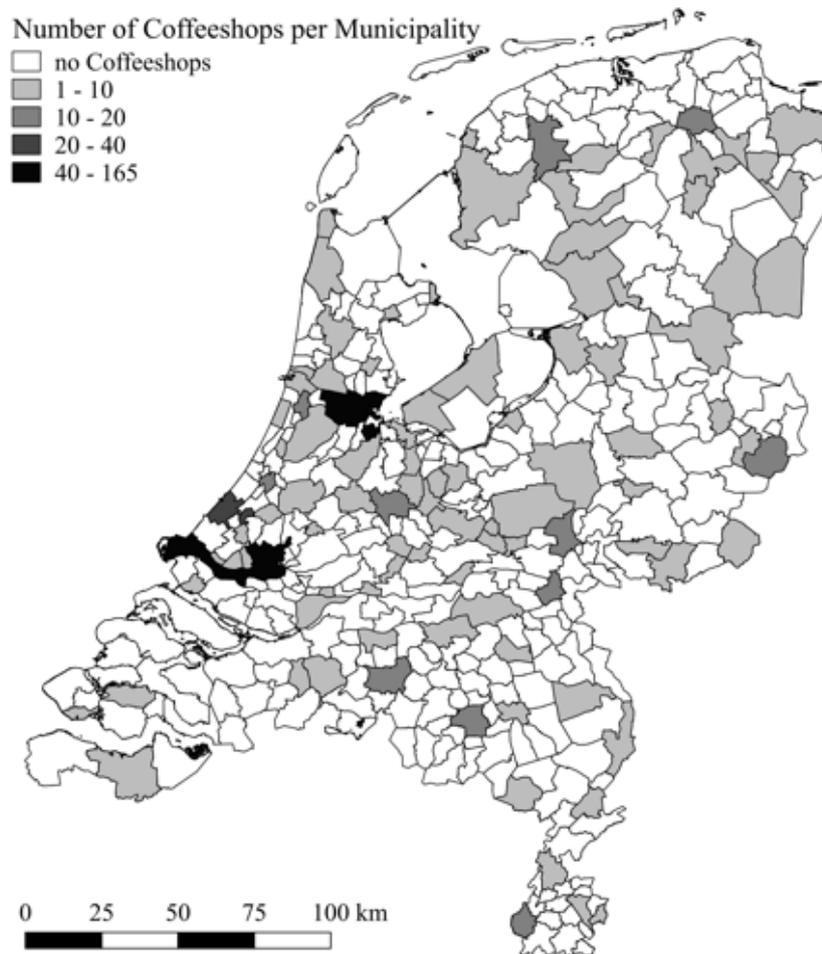
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<sup>21</sup>Even though Bieleman et al. (2010) and Bieleman et al. (2015a) already describe that all closings took place in the three biggest cities, we wanted to ensure that nothing has changed in the meantime.

<sup>22</sup>The eligibility period for unemployment benefits varies with work experience. Social security is provided after unemployment benefits have passed and until pension age is reached. Source: Dutch Statistics Office (CBS)

period in which coffeeshops were closed due to school proximity.<sup>23</sup> Our final data set consists of 1.75 million housing transactions, which is around 75 percent of all Dutch transactions that took place during this time period. For each transaction, we have detailed information on location, transaction price, time-on-the-market, structural characteristics, and quality assessments from realtors, leading to a large set of control variables.

**Figure 2**  
**Coffeeshop Distribution in the Netherlands (2017)**

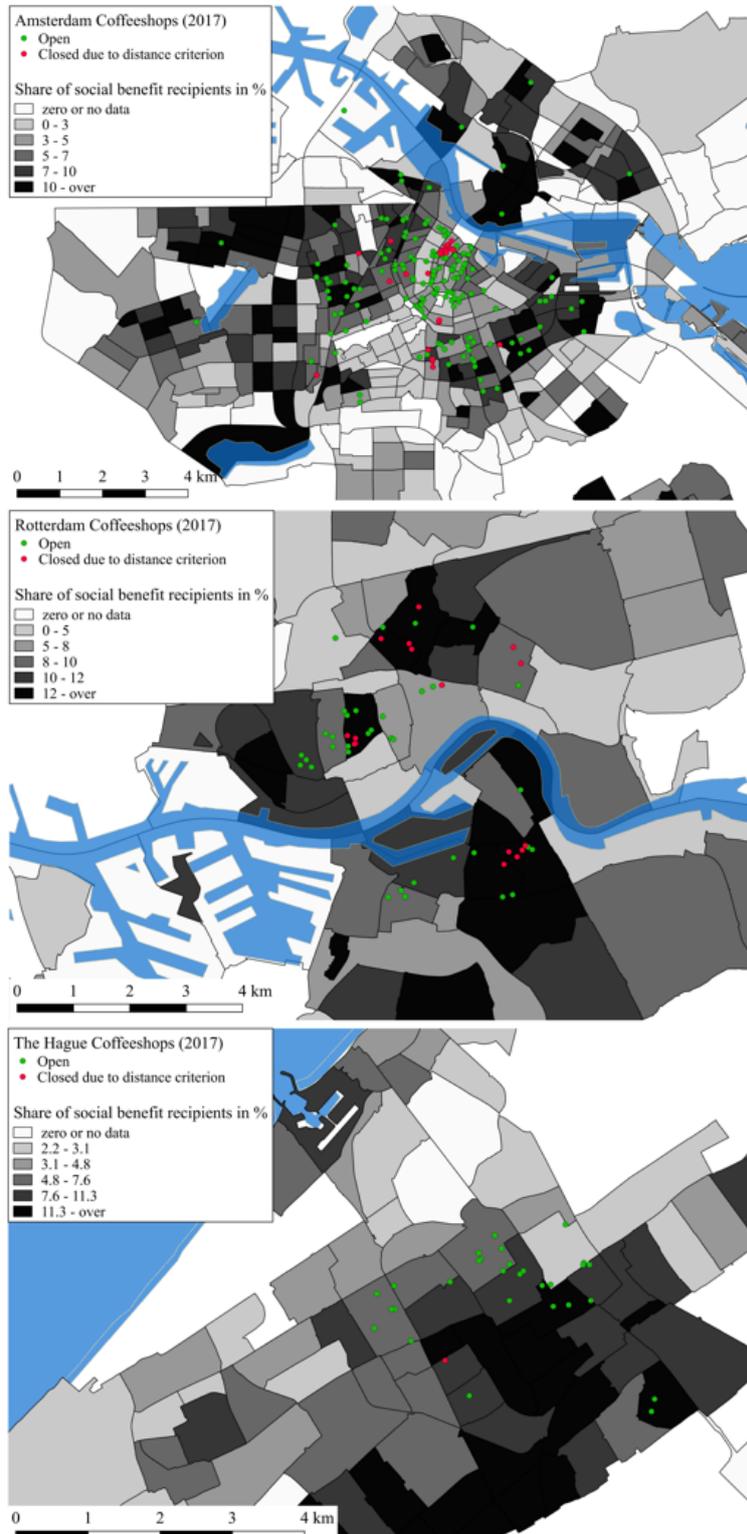


*Notes:* The map shows the location of all sample coffeeshops and gives an indication of the number of coffeeshops per municipality. Not all municipalities in the Netherlands have coffeeshops (see Figure 1).

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<sup>23</sup>To exclude potential outliers, we remove the highest and lowest 1% of observations on transaction price.

**Figure 3**  
**Coffeeshop Distribution in Amsterdam, Rotterdam, and The Hague**

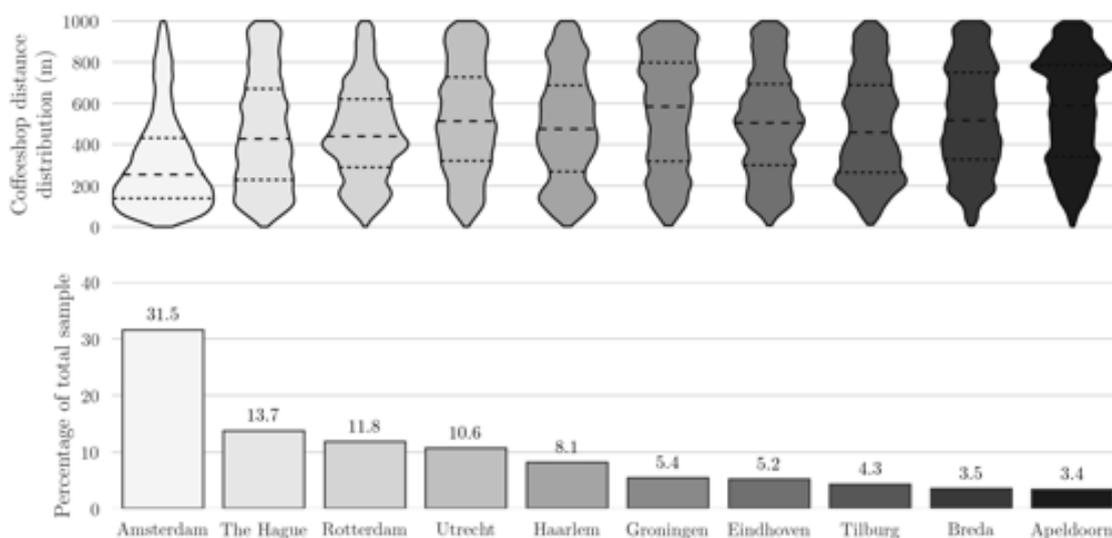


*Notes:* Illustrated are the locations of coffeeshops over different neighborhoods in the three biggest cities. We focus on coffeeshops that are open today (late July 2017) and coffeeshops that closed due to the distance criterion. We use the percentage of social benefit recipients (unemployment benefits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

### 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 the 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, and use these geocodes to calculate linear distances between every transaction and the closest coffeeshop. The top part of Figure 4 shows the distance distribution of observations and nearest coffeeshop in the ten biggest cities of the Netherlands (up to 1000 m). We document a high density of coffeeshops in the top three cities, with an average distance between 250 m to 400 m.

**Figure 4**  
**Spatial Distribution Top 10 Cities: Coffeeshop Proximity Analysis**



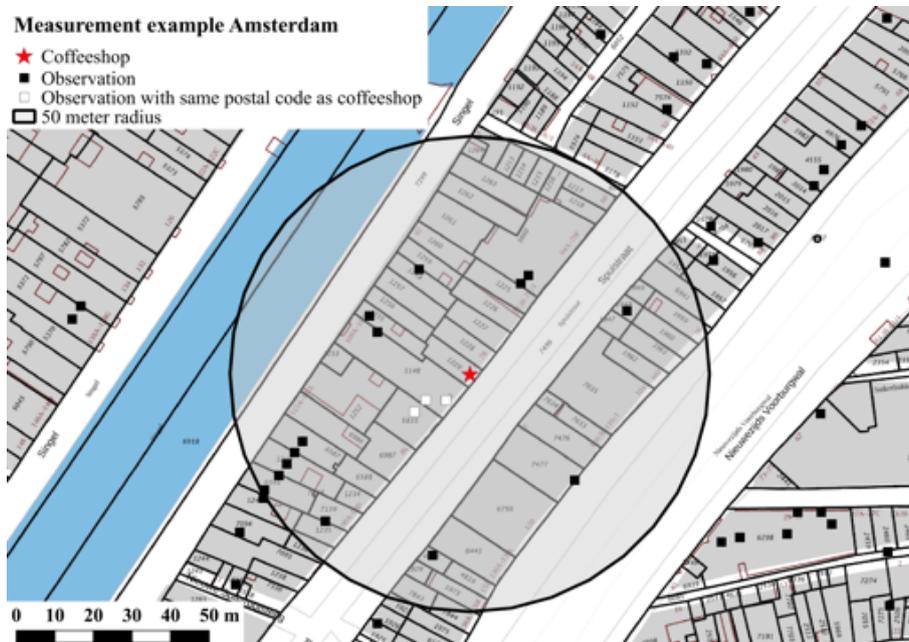
*Notes:* This graph shows the top ten cities in terms of properties in coffeeshop proximity. At the top, we show the distance distribution of the sample in quantiles, considering properties up to 1000 m coffeeshop distance. At the bottom, we show the percentage share of the biggest ten cities as part of the total Dutch sample.

Since the determination of an externality cut-off distance is rather arbitrary, we use different distances determined by three observations. First, Conklin et al. (2017) find significant external effects for cannabis dispensaries up to a cut-off distance of 0.1 miles or 161 m. 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 (some

stricter municipalities, increased it up to 350 m) (Bieleman et al., 2015a). Third, Figure 4 shows the first quartile of the distance distribution at a distance of 160 m to 250 m. Based on these three observations, we choose 150 m as an externality cut-off distance, since it seems to be a relevant cut-off, while providing a sufficiently large sample.

Due to the high density of coffeeshops, we also test smaller cut-off distances, using 100 m, 50 m as well as 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 the Netherlands, a 6-digit postcode is usually shared by half-a-street in urban areas (around 17 households), ensuring direct visibility. Figure 5 illustrates the distance definitions, using a sample of observations over a land registry map of Amsterdam and showing a 50 meter radius around a coffeeshop (red), as well as the reach of the postcode matching (white).

**Figure 5**  
**Illustration of Clustering**



*Notes:* This graph shows homes within a certain distance are affected by coffeeshops. In the illustrated case, we consider all homes within the 50 m radius as affected, indicated by a dummy. The procedure for other cut-off distances is similar. In addition, we test the effect for observations with a similar postcode as the coffeeshop, using 6-digit postal code to ensure visibility (illustrated in white).

To ensure homogeneity of test and control sample, we limit our control sample to transactions within 500 m distance of coffeeshops.<sup>24</sup> At a cut-off distance of 500 meters, our sample consists of 115,248 transactions in the three biggest cities: 72,602 (Amsterdam), 22,666 (The Hague), 19,980 (Rotterdam). A detailed breakdown per cut-off distance is shown in Table 2, documenting that the number of observations becomes smaller with proximity. The numbers are complementary to the bottom of Figure 4, illustrating the spatial distribution of our sample throughout the biggest ten cities. Around 58 percent of our sample transactions are located in the three biggest cities, which is due to the high number of coffeeshops in these cities.

**Table 2**  
**Observations per distance, area and property type**

	within 500 m	Nearby coffeeshops			
		within 150 m	within 100 m	within 50 m	same Postcode
The Netherlands					
Apartments	188,281	48,550	25,550	8,501	2,451
Houses	96,219	14,280	6,935	1,922	787
Three biggest cities					
Apartments	106,744	31,427	17,287	5,636	1,333
Houses	8,504	2,207	1,253	454	117
Amsterdam					
Apartments	70,017	23,838	13,656	4,725	1,075
Houses	2,585	880	553	220	63
The Hague					
Apartments	18,559	4,660	2,387	628	150
Houses	4,107	985	504	167	46
Rotterdam					
Apartments	18,168	2,929	1,244	283	108
Houses	1,812	342	196	67	8

*Notes:* This table documents the number of property transactions within the different cut-off distances. We limit the sample to properties within 500 m distance and test different cut-off distances for external effects. Due to additional filters, such as time-periods, the number of homes in the analysis might further decrease.

<sup>24</sup>We also try different maximum distances as a robustness check, but find that our results are not affected. Results are available upon request

Table 3 summarizes property characteristics of single-family houses and apartments in the control group and the externality group ( $d < 150m$ ), such as price, size and quality. There are slightly more apartments than single-family houses in the externality group as compared to the control group. This is likely due to more central locations of coffeeshops. Homes and apartments in both groups do not differ significantly in size and maintenance quality (inside and outside). In the externality group homes and apartments are more expensive than in the control group, both in absolute price and price per square meter, which is likely related to the more central location of treated homes, and needs to be properly controlled for.

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

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

*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 affected group is defined by the 150 m cut-off distance. The control group is limited to observations with a maximum cut-off distance of 500 m. 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. Due to the immobility of real estate, it can be argued that house prices reflect the willingness-to-pay to live at certain locations, allowing us to quantify local utility and disutility. The underlying theory assumes that people can choose location freely, allowing them to sort into specific neighborhoods and homes. Among other factors, such as structural and socio-demographic aspects, location and nearby externalities should be reflected in 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 Proximity Analysis

As the number of homes nearby closing coffeeshops is relatively small compared to the number of homes nearby open coffeeshops, we first employ a hedonic property pricing model on coffeeshop proximity, providing us with a broader understanding of coffeeshop proximity effects. For this analysis, we consider all observations within 500 m distance of open coffeeshops and distinguish among observations within externality distance, using different cut-off distances and observations in the control group (150 m to 500 m).

Equation (1) documents the model based on the hedonic framework proposed by Rosen (1974), where the natural logarithm of price,  $\ln(p_{it})$ , of property  $i$  at time  $t$ , is modeled as a function of its structural aspects  $S'_{it}$ , neighborhood characteristics  $N'_{it}$ , and environmental characteristics  $E'_{it}$ . Through the underlying data, we are able to extensively control for house quality and neighborhood factors. Since transactions are pooled over several years, we include time-fixed effect dummies  $T'_i$  to capture timely

price variation. We cluster standard errors by municipality and year.

$$\ln(p_{it}) = \alpha_{it} + S'_{it}\beta_1 + N'_{it}\beta_2 + E'_{it}\beta_3 + T'_i\beta_4 + \epsilon_{it} \quad (1)$$

To account for potential spatial dependence and omitted variables, we include neighborhood fixed-effects (Anselin & Bera, 1998; Kuminoff et al., 2010), using detailed postcode information. In the Netherlands a 6-digit postcode is shared by 17 households in urban areas on average, resulting in around 450,000 Dutch 6-digit postcode areas. However, we do not have sufficient observations for every postcode area, which would theoretically result in single-observation fixed-effects. Therefore, we use 5-digit postcode areas instead, which consist of 12 observations per postcode area, on average, in our sample.<sup>25</sup>

As shown in equation (2), we extend the hedonic model in equation (1) by  $CS_{it}$ , indicating whether observation  $i$  is within cut-off distance of a coffeeshop at time  $t$ , testing different cut-off distances. To analyze the reach of coffeeshop proximity effects, we use distance intervals instead of a dummy to examine the reach of potential proximity effects. As shown in equation (3), we form  $K = 10$  intervals of  $k = 25$  m length, considering observations up to 250 m distance as affected in different intervals. We employ observations at a distance between 250 m and 500 m as our control group.

$$\ln(p_{it}) = \alpha_{it} + S'_{it}\beta_1 + N'_{it}\beta_2 + E'_{it}\beta_3 + \gamma CS_{it} + T'_i\beta_4 + \epsilon_{it} \quad (2)$$

$$\ln(p_{it}) = \alpha + S'_{it}\beta_1 + N'_{it}\beta_2 + E'_{it}\beta_3 + \sum_{k=1}^K \gamma_k CS_{kit} + T'_i\beta_4 + \epsilon_{it} \quad (3)$$

Table 4 documents the results for coffeeshop distance effects.<sup>26</sup> We document a price discount of -0.8 percent for homes within 150 m distance to coffeeshops compared

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<sup>25</sup>We test more fixed effect specifications, but our results do not change.

<sup>26</sup>A detailed overview of control variables is presented in Appendix Section A.

to homes within 150 m to 500 m distance. Reducing the cut-off distance, we document a discount of 1.2 percent for homes within 100 meters, a discount of 1.7 percent for homes within a 50 meters' distance of coffeeshops, and a discount of 2.0 percent for observations within the same 6-digit postal code as coffeeshops, compared to homes within 150 m to 500 m distance. Examining the reach of coffeeshop proximity effects, Appendix Figure [B](#) documents a decreasing proximity discount over distance, becoming insignificant at a distance of 125 m to 150 m.

Since the external effects of coffeeshops might be similar to those of other entertainment venues, creating noise, traffic, and nuisance, we explicitly test for the proximity to similar externality sources: pubs, bars, and nightclubs.<sup>27</sup> Documented in Table [4](#) and using the similar cut-off distances, coffeeshop proximity discounts proof to be robust ranging from 0.7 percent within 150 m distance to 1.9 percent for homes sharing the same postcode level. The effect of bar proximity is comparable, showing discounts in the range 0.6 to 1.9 percent. Homes nearby nightclubs show higher price discounts, ranging from 2.5 to 4.6 percent. In contrast, homes nearby pubs show mixed effects, ranging from 0.4 to -1.5 percent.

Overall we document a significant price discount for homes nearby coffeeshops, compared to homes further away. However, due to potential endogeneity issues, we cannot make causal implications.

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<sup>27</sup>We download all locations of pubs, bars and nightclubs via Overpass API using OpenStreetMap data <https://www.openstreetmap.org/#map=12/52.3563/4.8532>

Table 4  
Proximity analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	150 m	100 m	50 m	Postcode	150 m	100 m	50 m	Postcode
Coffeeshop within 150 m (1 = yes)	-0.008*** [0.002]				-0.007*** [0.002]			
Coffeeshop within 100 m (1 = yes)		-0.012*** [0.003]				-0.011*** [0.003]		
Coffeeshop within 50 m (1 = yes)			-0.017*** [0.004]				-0.014*** [0.004]	
Coffeeshop in postcode area (1 = yes)				-0.020*** [0.005]				-0.019*** [0.005]
D: Bar nearby (1 = yes)					-0.006*** [0.003]	-0.010*** [0.003]	-0.018*** [0.003]	-0.019*** [0.003]
D: Nightclub nearby (1 = yes)					-0.025*** [0.008]	-0.042*** [0.009]	-0.042*** [0.010]	-0.045*** [0.010]
D: Pub nearby (1 = yes)					0.004* [0.002]	-0.002 [0.002]	-0.014*** [0.002]	-0.015*** [0.002]
Observations	115,248	115,248	115,248	115,248	115,248	115,248	115,248	115,248
Adj. R-squared	0.74	0.74	0.74	0.74	0.74	0.74	0.74	0.74
Quality & Location controls	YES							
Time fixed-effects	YES							

Notes: Standard errors in parentheses, \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Control group: homes in 150 m to 500 m distance from coffeeshops. Proximity to pubs, bars and nightclubs is defined the same as for coffeeshops, using the same cut-off distance. Location controls, using neighborhood level fixed-effects (5-digit level).

## 4.2 Difference-in-Difference Analysis

Since coffeeshops might not be randomly distributed, any study into 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 attitude. 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.<sup>28</sup>

Coffeeshop closings offer an alternative, but might be endogenous, too. In practice, coffeeshops close for two reasons: due to violations of the five main laws, such as having minors in the shop, creating nuisance, etc., or due to changes in regulations such as the school distance criterion.<sup>29</sup> Closings related to law violations are grouped as so called BIBOB-related closings. However, our data do not allow to examine the exact reason behind these closings. Since law violations have to be reported and gentrification-related closings are officially also BIBOB-related, we cannot rule out that BIBOB related closings are endogenous, e.g. promoted by complaining neighbors or gentrification.<sup>30</sup>

We use school distance-related closings in a quasi-experimental approach, since these are arguably exogenous. 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.

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<sup>28</sup>E.g. coffeeshops need to ensure proper supply-chain, even though it is mostly illegal <https://www.talkingdrugs.org/netherlands-paradox-cannabis-policy-front-door>

<sup>29</sup>In theory coffeeshop could also close due to customer absence, but as competition decreases over time, remaining coffeeshops are generally very profitable.

<sup>30</sup>One example of such a gentrification initiative is *project 1012*, where coffeeshops are systematically closed by linking them to BIBOB violations <https://citiesintransition.eu/cityreport/project-1012>).

Similar to Muehlenbachs et al. (2015) for shale gas, Pope and Pope (2015) for retail, McMillen and McDonald (2004) for transport or Conklin et al. (2017) for cannabis dispensaries, we use a spatial difference-in-differences (DID) framework. We group transactions based on their spatial distance to closing coffeeshops as described in section 3.2 and by sales time with respect to closings (pre vs. post), resulting in four different groups: pre-nearby, pre-far, post-nearby, post-far. Since we do not solely employ repeated sales, we cannot rely on the assumption that transactions do not systematically differ in different time periods and we therefore include hedonic control characteristics, similar to those in equation (1).

The employed model is equation (4), where  $X'_{it}$  combines structural, neighborhood, and environmental characteristics of property  $i$  at time  $t$  as well as sales time, similar as in equation (1).  $Nearby_{it}$ , 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.<sup>31</sup> Lastly,  $Nearby_{it} * Post_{it}$ , measures the interaction of two former terms, where  $d = 1$  indicates a transaction nearby a closing coffeeshop after closing.

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

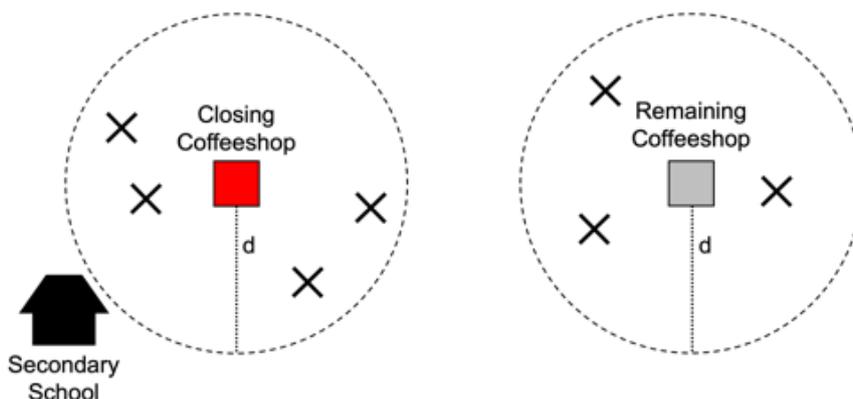
As explained in section 3.2, we use four different distance cut-off points to define  $Nearby_{it}$ : 150 m, 100 m, 50 m, 6-digit postcode. Since we document that homes nearby coffeeshops show systematic price differences compared to homes further away, we use areas around remaining coffeeshops as a control group. Homes of both groups (treatment and control), share common attributes making them arguably similar, as they are initially both nearby a coffeeshop. We compare homes within the same cut-off distance of closing and remaining coffeeshops as illustrated in Figure 6. Every

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<sup>31</sup>For  $Post_{it}$  we use the closing date of the closest closing coffeeshop, ensuring that every treatment area has a respective control area.

remaining coffeeshop is assigned as control group to one closing coffeeshop, using the closest one based on linear distance.

**Figure 6**  
**Difference-in-Difference Setup**



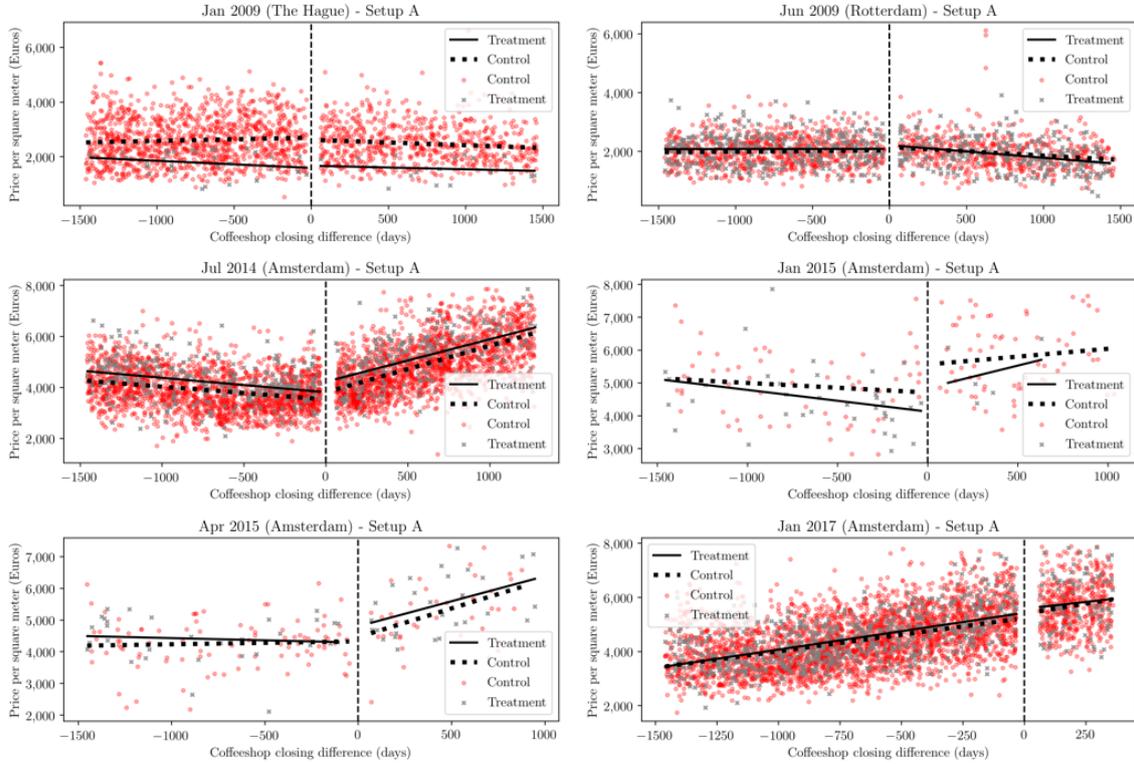
*Notes:* Illustration of analysis setup. We use homes nearby remaining coffeeshops as control group and compare the price changes before and after coffeeshop closings, testing different cut-off distances  $d$  between 150 m and 6-digit postcode level.

In the analysis, 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 observation in every group (pre-treatment, post-treatment, pre-control, post-control).

Appendix Table [II](#) provides an overview of the treatment and control groupings. Appendix Table [III](#) provides an overview by closing date and location. Examining the similar trend assumption, Figure [7](#) 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. Closing waves show different pre-post patterns, which

is related to other circumstances such as the financial crisis. However, except for January 2015, price trends in the treatment and control group are similar.

**Figure 7**  
**DID Similar Trend Graphs (150 m)**



*Notes:* We plot the adjusted price per square meter of observations over time to closing (4 years before and after), comparing treatment and control groups. Transactions within 30 days before and 60 days after closing are excluded from the analysis. We differentiate for closing years, to avoid pooling over different years. An overview of the number of observations per closing wave is documented in Appendix Table III. Due to missing post-closing observations July 2017 closings are excluded from the analysis.

Table 5 shows the result for different cut-off distances. Compared to homes nearby remaining coffeeshops, homes nearby closed coffeeshops show an price discount of 1.6 to 7.9 percent on average after closing, increasing with closeness. There is no significant price difference between homes nearby remaining coffeeshops and homes nearby closing coffeeshops ( $Nearby_{it} = 1$ ), showing that homes in both groups are similar in general. For both groups we document positive price-time trends between

2.9 to 6.7 percent ( $Post_{it} = 1$ ).<sup>32</sup>

**Table 5**  
**Difference-in-difference analysis**

	(1)	(2)	(3)	(4)
	150 m	100 m	50 m	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 6. Location controls are 5-digit postcode level fixed-effects (around 17 observations per postcode in sample).

Controlling for similar nuisance venues nearby, Table 6 reports the results, including proximity to bars, pubs and nightclubs. Coffeeshop closing effects remain negative, ranging from -1.6 to -7.8 percent for homes within 150 m to 50 m.<sup>33</sup> Systematic group differences ( $Nearby_{it} = 1$ ) remain insignificant and general post-closing time effects diminish. Similar as in Table 4, we document negative proximity effects for bars (-4.6 percent) and nightclubs (up to -30 percent), but no negative proximity effects for pubs.

<sup>32</sup>Negative closing effects are robust for different fixed-effect controls. As there are only a 61 houses in the treatment group at 150 m cut-off distance (none below 100 m), we document no significant closing effects for houses. However, closing effects are robust for apartments. Examining different locations, closing effects are robust for Amsterdam and Rotterdam. Due to the small number of observations, we cannot estimate closing effects for The Hague individually.

<sup>33</sup>Due to the small number of observations, we cannot estimate the model at postcode level.

**Table 6**  
**DID Analysis - Controlling for other nuisance venues**

	(1)	(2)	(3)
	150 m	100 m	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 5. 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 7. Closing effects remain robust for different holdout periods (base holdout period highlighted). However, we confirm that markets are sticky in the short-run, as coffeeshop closing effects become significant and higher in magnitude for longer holdout periods.

To examine variation over time further, we decompose the post period, forming time-interval dummies. To ensure there is a significant amount of observations per interval, we use yearly intervals, up to four years, as shown in equation (5). Due to

the small number of observations, we omit the postcode level analysis. As shown in Appendix Table [IV](#), coffeeshop closing effects are persistence at 50 m cut-off distance. At other cut-off distances, closing effects peak in year two after closings and become insignificant thereafter, indicating that closing effects are not long-lasting. However, as the number of observations is small for individual post-closing years, we should take these results with a grain of salt.

$$\ln(p_{it}) = \alpha_{it} + bX_{it} + \gamma_1 \text{Nearby}_{it} + \sum_y^4 \gamma_{2y} \text{Post}_{ity} + \sum_y^4 \gamma_{3y} \text{Nearby} * \text{Post}_{ity} + \epsilon_{it} \quad (5)$$

**Table 7**  
**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 [0.010]	-0.023* [0.013]	-0.063*** [0.020]	-0.073** [0.029]
post-10 days holdout	-0.013 [0.010]	-0.022* [0.013]	-0.060*** [0.021]	-0.073** [0.029]
post-30 days holdout	-0.013 [0.010]	-0.021* [0.013]	-0.062*** [0.020]	-0.073** [0.029]
<b>post-60 days holdout</b>	-0.016* [0.009]	-0.030** [0.013]	-0.074*** [0.021]	-0.076** [0.031]
post-90 days holdout	-0.018* [0.009]	-0.030** [0.013]	-0.067*** [0.021]	-0.067** [0.029]
post-180 days holdout	-0.018* [0.010]	-0.038*** [0.014]	-0.075*** [0.021]	-0.067* [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.

To verify our results we test an alternative DID setup in Appendix Section [C](#). Since all homes in our analysis are located nearby coffeeshops, we test a different control group, comparing homes nearby closing coffeeshops to homes in 150 m to 500 m distance of closing coffeeshops. As documented in Appendix Table [V](#), homes nearby coffeeshops show a discount of 1.9 percent compared to homes further away. However,

closings do not show significant effects. As both groups differ in their relative position to coffeeshops, we take these results with a grain of salt.

### 4.3 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. We control for time-varying characteristics in our model, such as improvements and decay.

We only include repeated sales in our data set and exclude sales pairs selling more than once in the same year, resulting in 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.<sup>34</sup> 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).

As shown in equation (6), we use the percentage change in 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. We divide all changes into additions & improvements, measured by  $\Delta Q_{i(t+n)}^+$  and removals & decay, measured by  $\Delta Q_{i(t+n)}^-$ . Controlling for time trends,  $Y'_{i(t+n)}$  indicates the respective

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<sup>34</sup>20 properties sell more than 4 times during the sample period and are excluded from the analysis, considered as outliers.

sales year  $t + n$  of property  $i$ . Additionally, we control for the time period  $n$  between two sales, by  $\Theta'_{in}$  and for location differences, using the 5-digit neighborhood post-code, and 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.

$$\Delta p_{i(t+n)} = \alpha + \Delta Q'_{i(t+n)^+} \gamma_1 + \Delta Q'_{i(t+n)^-} \gamma_2 + Y'_{i(t+n)} \gamma_3 + \Theta'_{in} \gamma_4 + \gamma_5 \Delta CS_{i(t+n)} + \epsilon_{i(t+n)} \quad (6)$$

Since markets might take time to incorporate closing effects, we also test for the time difference in days between sales and coffeeshop closings for treatment properties ( $\Delta CS_{i(t+n)} = 1$ ) shown in equation (7).

$$\begin{aligned} \Delta p_{i(t+n)} = & \alpha + \Delta Q'_{i(t+n)^+} \gamma_1 + \Delta Q'_{i(t+n)^-} \gamma_2 + Y'_{i(t+n)} \gamma_3 \\ & + \Theta'_{in} \gamma_4 + \gamma_5 \Delta CS_{i(t+n)} + \gamma_6 \Lambda_{i(t+n)} + \epsilon_{i(t+n)} \end{aligned} \quad (7)$$

where  $z$  equals the closing date of the closest coffeeshop and  $\Lambda_{i(t+n)}$  takes the values:

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

However, 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.

After filtering, there are 57 repeated sales pairs left within 150 m distance to closing coffeeshops. We therefore limit our analysis to a 150 m cut-off distance. Appendix Figure C plots the percentage change in price between sales pairs, over the coffeeshop closing time difference for treatment and control group. We document that there is no pattern and systematic difference between groups. The majority of homes generally increases in value over time.

Shown in Table 8, we document that homes experiencing a coffeeshop closing between their sales, drop on average 10.75 percent in value compared to homes nearby remaining coffeeshops. Controlling for linear and quadratic time difference in days between coffeeshop closings and sales at  $t+n$ , we document a general price drop of 18.72 to 34.86 percent. However, over time prices seem to recover at an diminishing rate. Appendix Table VI shows the full list of control variables.

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

**Table 8**  
**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 to closing)		[0.008]	[0.030]
$\Phi_{i(t+n)}$			-0.000**
((days to closing) <sup>2</sup> )			[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 document house prices drops for homes nearby closing coffeeshops compared to homes nearby remaining coffeeshops. In order to understand what causes our findings, we shift the focus of our analysis to potential channels. There exist two survey studies on the school distance related closings in Rotterdam and Amsterdam, but the results are inconclusive, suggesting that coffeeshop closings had limited effects on well-being.

[Bieleman et al. \(2010\)](#) conduct a survey among teenagers and neighbors for the municipality of Rotterdam on the effects of the distance criterion. Comparing neighbors of remaining and closed coffeeshops, they find a significant reduction of externalities over time for both groups, but no specific effect for closed coffeeshops. The percentage of teenagers using cannabis did not change after closings, neither did their sourcing behavior, as using teenagers still receive cannabis from older friends and consume as before.

In a similar follow-up study on distance-related closings in Amsterdam, [Bieleman et al. \(2015b\)](#) find an increase in customers for remaining coffeeshops, but no increase in negative externalities. The majority of neighbors likes to live in their neighborhood and the perceived safety did not change after closings. The number of nuisance complaint reports, before and after coffeeshops closings does not change, mainly related to noise, pollution and traffic (including wrongly parked cars). Only 7 to 5 percent of the neighbors directly related coffeeshops to nuisance-related problems.

[Chang and Jacobson \(2017\)](#) find cannabis dispensary closings in Los Angeles lead to higher crime rates nearby, in the short run. 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](#)).

In a study on the effects of retail activities on property prices, [Koster and](#)

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Rouwendaal (2012) find positive price effects for properties nearby retail activities. Assuming that the documented closing effects might not be driven by the disappearance of coffeeshops itself, but by the circumstances of the post-closing situation (operational retail vs. non-operational retail), we examine the post-closing situations of former coffeeshop locations.

Coffeeshops in Amsterdam had the opportunity to transform into regular cafes. We examine what happened to all coffeeshop locations after closing, by making use of Google Street View (GSV), allowing us to virtually "walk the streets" and analyze all 41 locations by eyesight test (for more information on GSV, see Anguelov et al., 2010). Google updates GSV on an irregular basis, but publishes the date of the image. Since GSV does not only allow to see the most current image of a certain location, but all previous instances as well, we can go virtually back in time, allowing us to track coffeeshop locations over time (on irregular intervals).

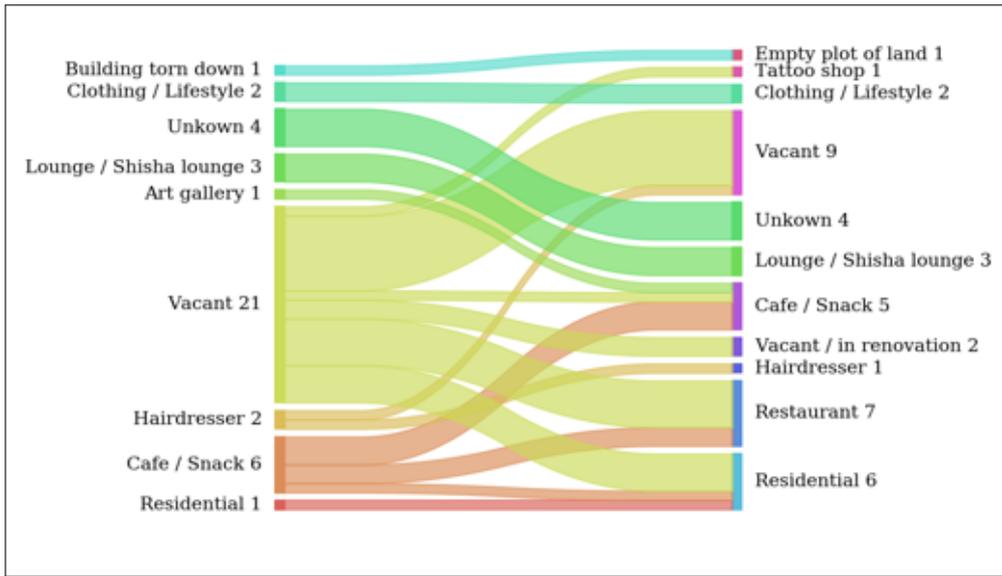
Tracking the post-closing developments, we examine the first available GSV image after closing and the most recent situation. However, for some locations there is no image post-closing available, whereas for others the first available image is the most recent image. For the majority of observations there are different images over time. We classify the post-closing state of coffeeshop locations using different categories as presented in Figure 8, tracking the development of coffeeshop locations from the first image after closing to the most current.

Most coffeeshop locations are vacant at first, often with the coffeeshop front still existence. We document that many former vacant locations turn into restaurants, cafes or bistro-type businesses and residential locations. There are four coffeeshops that we cannot classified based on GSV. Unfortunately, we cannot determine the exact timings of location transitions, as GSV images are provided on a irregular basis.<sup>35</sup>

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<sup>35</sup>We are currently collecting more detailed information, to run a separate analysis on the different post-closing states.

**Figure 8**  
**Post-closing development of coffeeshop locations**



*Notes:* On the left, we see classify based on the first available Google Street View image after closing. On the right, we classify based on the latest available image (late 2017). For some locations, the left and right side are identical as they are showing the same image.

In a first preliminary analysis, we find no significant price differences for vacant and non-vacant post-closing states. Some of the succeeding businesses, such as Shisha lounges, have a certain reputation, which is not necessarily better than those of coffeeshops, so it might be questionable if all retail activities are perceived alike. We are currently examining more detailed post-closing differences.

## 6 Implications & Conclusion

While considered a hard drug by some, many governments around the world have moved to legalize recreational use of cannabis. We document that properties close to coffeeshops show a price discount compared to coffeeshops further away. The effect is robust to different distance cut-offs, sub-markets and when controlling for similar nuisance factors. Since we cannot rule out endogeneity, due to sorting, we test a DID model, examining the change in property prices of different areas, following exogenous coffeeshop closings.

We document negative coffeeshop closing effects for homes nearby closing coffeeshops, compared to homes nearby remaining. Applying a repeated sales model on a subset of repeated sales pairs, we find similar results. Price discounts are robust for different controls, but seem to diminish over time. We preliminary examine the reasons of these results, which could be the post-closing situation of coffeeshops.

Coffeeshops in the Netherlands are always under monitoring when it comes to nuisance. However, little is know about the exact financial effects, which is illustrated by the fact that there are no official revenue figures. Given the distinctiveness of coffeeshops as a business, more transparency could be beneficial to avoid speculations. Our paper is the first to quantify social costs of coffeeshops, using property prices as a proxy. In contrary to the public opinion, we show that coffeeshops closings lead to negative housing price effects.

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# Appendix

Figure A  
Street scene impressions of coffeeshops

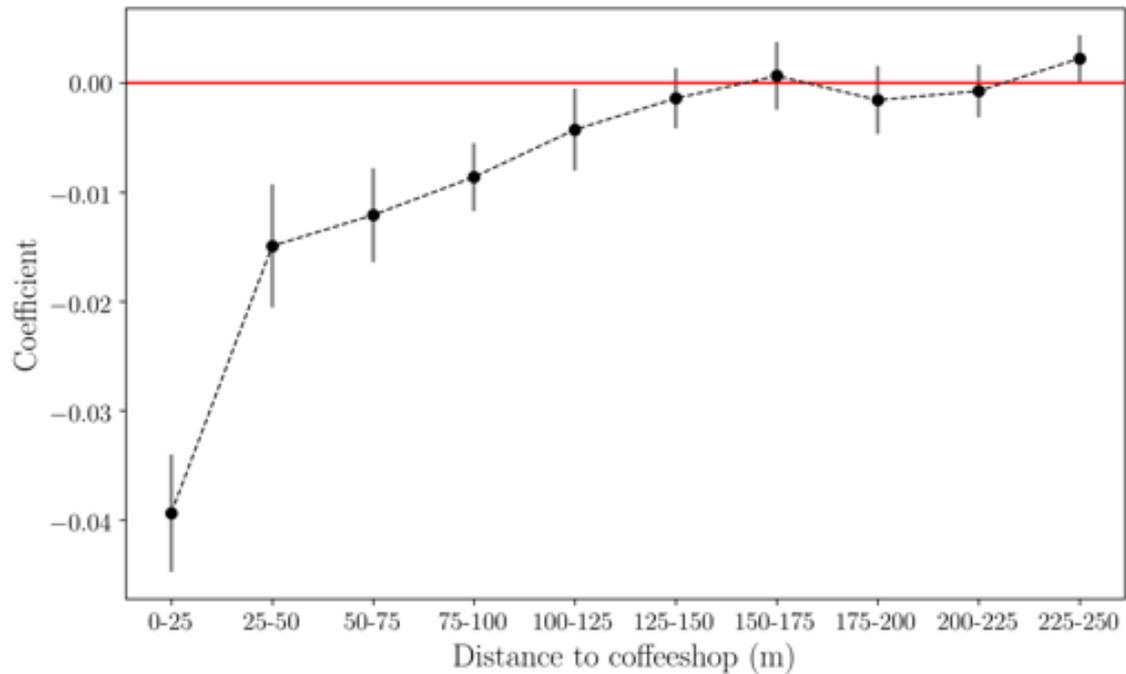


Notes: Street scene images of coffeeshops in Amsterdam. Sources: Wikimedia Commons, [www.flickr.com](http://www.flickr.com) (Terrazzo, Travelmag.com), Google Street View

## Appendix A Proximity Analysis

To examine the general fit of the hedonic regression setup proposed in equation (1), Appendix Table 1 presents the estimation results for all sample observations within 500 m of coffeeshops. Overall, the model shows a good fit, with an adjusted R-squared of 0.74. The results make intuitive sense, since positive aspects, such as more bathrooms, a garden, a parking lot, or a nearby park increase prices and negative aspects, such as low insulation, old heating technology, a bad garden, or a nearby highway decrease prices. Apartments are generally cheaper than houses. The results are in line with previous studies of Dutch house prices, of which some use the same data (see e.g. Brounen & Kok, 2011; Dröes & Koster, 2016).

**Figure B**  
Coffeeshop proximity effects



*Notes:* Measuring coffeeshop proximity effects over distance, we use ten 25 m intervals up to 250 m, against a control group of homes within 250 m to 500 m from coffeeshops. Reported are effect estimates including 95 % confidence interval.

**Table I**  
**Hedonic Regression - Control Variables**

Variables		Variables	
Size (m <sup>2</sup> )	0.005*** [0.000]	D: balcony (1 = yes)	-0.012*** [0.004]
Number of floors	0.017*** [0.002]	D: basement (1 = yes)	0.007** [0.003]
Number of rooms	0.028*** [0.001]	D: roof terrace (1 = yes)	0.048*** [0.004]
Number of bathrooms	0.012*** [0.002]	D: apartment (1 = yes)	-0.074*** [0.010]
<i>Construction period (dummy)</i>		<i>Insulation (dummy)</i>	
Construction_1500_1905 (1 = yes)	0.031*** [0.002]	One level of insulation (1 = yes)	0.011*** [0.002]
Construction_1931_1944 (1 = yes)	0.003 [0.003]	Two levels of insulation (1 = yes)	0.032*** [0.002]
Construction_1945_1959 (1 = yes)	-0.023*** [0.004]	Three levels of insulation (1 = yes)	0.036*** [0.003]
Construction_1960_1970 (1 = yes)	-0.057*** [0.004]	Four levels of insulation (1 = yes)	0.031*** [0.004]
Construction_1971_1980 (1 = yes)	-0.040*** [0.004]	Five or more levels (1 = yes)	0.024*** [0.003]
Construction_1981_1990 (1 = yes)	-0.008* [0.004]		
Construction_1991_2000 (1 = yes)	0.090*** [0.004]	Apart. quality simple (1 = yes)	-0.048*** [0.005]
Construction_2001 & later (1 = yes)	0.103*** [0.007]	Apart. quality luxurious (1 = yes)	0.083*** [0.003]
<i>House type (dummy)</i>		<i>Apartment type (dummy)</i>	
Corner house (1 = yes)	-0.053*** [0.004]	Two-floor apartment (1 = yes)	0.075*** [0.010]
Terraced house (1 = yes)	-0.055*** [0.008]	Upstairs apartment (1 = yes)	-0.061*** [0.004]
Row house (1 = yes)	-0.065*** [0.004]	Old block apartment (1 = yes)	-0.066*** [0.005]
Detached house (1 = yes)	0.069** [0.005]	Maisonnette apartment (1 = yes)	-0.057*** [0.005]
		New block apartment (1 = yes)	-0.051*** [0.006]
D: monument status (1 = yes)	0.048*** [0.004]	Service apartment (elderly) (1 = yes)	-0.026 [0.048]
D: garden (1 = yes)	0.016*** [0.004]	D: elevator (1 = yes)	0.044*** [0.002]
D: garden good location (south) (1 = yes)	0.028*** [0.002]	D: attic (1 = yes)	-0.006*** [0.002]
<i>Heating type (dummy)</i>		<i>Location I (dummy)</i>	
D: AC or Solar (1 = yes)	-0.004 [0.027]	D: city center (1 = yes)	0.008*** [0.002]
D: central or tele-heating (1 = yes)	0.018*** [0.003]	D: on a quiet road (1 = yes)	0.004*** [0.001]
D: gas or coal oven (1 = yes)	-0.069*** [0.003]	D: on a busy road (1 = yes)	-0.014*** [0.003]
<i>Parking type (dummy)</i>		<i>Location II (dummy)</i>	
D: parking lot (1 = yes)	0.073*** [0.003]	D: free view (1 = yes)	0.019*** [0.002]
D: garage (1 = yes)	0.092*** [0.003]	D: next to a park (1 = yes)	0.042*** [0.003]
D: carport (1 = yes)	0.099*** [0.003]	D: next to a forest (1 = yes)	0.060*** [0.011]
D: garage and carport (1 = yes)	0.096*** [0.010]	D: next to water (1 = yes)	0.071*** [0.003]
D: multi garage (1 = yes)	0.095*** [0.006]		
Quality & Location controls	YES	Observations	115,248
Time-fixed effects	YES	Adj. R-squared	0.74

*Notes:* Standard errors in parentheses \*\*\* p<0.01, \*\* p<0.05, \* p<0.1 Standard errors are clustered by municipality and year. D = dummy, Base values: Construction = Construction 1906 - 1930, House type = Semi-detached house, Apart. type = ground floor, Apart. quality = bad, Garden quality: normal (garden quality bad omitted), Heating = no heating. For quality, we use a scale for internal and one for external quality. Location controls by neighborhood fixed-effects. Time fixed effects by sales year.

## Appendix B Difference-in-Difference Analysis

**Table II**  
**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 III**  
**Observations by closing time and place**

Closing date	Amsterdam	Rotterdam	The Hague
<b>Treatment group (150 m)</b>			
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
<b>Control group</b>			
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 4.2 we document the number of transactions in 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 IV**  
**DID - Post-closing intervals**

	(1)	(2)	(3)
	150 m	100 m	50 m
D: Nearby* Post - Year 1 (1 = yes)	-0.009 [0.011]	-0.019* [0.011]	-0.053* [0.029]
D: Nearby* Post - Year 2 (1 = yes)	-0.024 [0.020]	-0.057*** [0.013]	-0.119*** [0.022]
D: Nearby* Post - Year 3 (1 = yes)	-0.006 [0.013]	-0.014 [0.025]	-0.056*** [0.021]
D: Nearby* Post - Year 4 (1 = yes)	-0.051* [0.028]	-0.048 [0.054]	-0.101*** [0.035]
D: Nearby (1 = yes)	-0.009 [0.008]	0.006 [0.017]	0.033 [0.027]
D: Post - Year 1 (1 = yes)	0.025 [0.020]	0.027 [0.019]	0.015 [0.035]
D: Post - Year 2 (1 = yes)	0.043* [0.023]	0.029 [0.022]	0.032* [0.019]
D: Post - Year 3 (1 = yes)	0.044*** [0.016]	0.019 [0.016]	0.017 [0.018]
D: Post - Year 4 (1 = yes)	0.075*** [0.023]	0.070*** [0.025]	0.066*** [0.022]
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. We divide post-closing transactions into years.

## Appendix C DID - Alternative setup

In this setup we use areas surrounding the treatment areas in 150 m to 500 m distance from closing coffeeshops as a control group. We do not consider remaining coffeeshops and exclude all properties within 150 m of these coffeeshops from the analysis. However, it might be argued that the control group in this case fundamentally differs from the treatment group ex-ante, given that location characteristics differ further out. Table V documents the results, comparing homes nearby closing coffeeshops with homes further away.<sup>36</sup> We do not document a significant closing effect for properties nearby coffeeshops, however, we document positive time trends, between 4.9 to 5.2 percent. Homes nearby coffeeshops sell at a discount of -1.9 percent compared to homes further away.<sup>37</sup>

**Table V**  
**Difference-in-difference analysis - alternative setup**

	(1)	(2)	(3)	(4)
	150 m	100 m	50 m	Postcode
Nearby Coffeeshop	-0.019***	-0.010	-0.016	-0.033
(1 = yes)	[0.006]	[0.006]	[0.018]	[0.025]
Nearby Coffeeshop * Post-closing	0.006	0.004	-0.002	-0.026
(1 = yes)	[0.008]	[0.014]	[0.024]	[0.042]
Post-closing	0.048**	0.049**	0.045*	0.051**
(1 = yes)	[0.023]	[0.023]	[0.024]	[0.021]
Observations	9,843	8,627	6,849	8,181
Treatment Group	1,959	871	244	113
Adj. R-squared	0.80	0.80	0.80	0.79
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. The control group are homes within 150 m to 500 m distance from closing coffeeshops and at least 150 m away from remaining coffeeshops. Location controls are coffeeshop fixed-effects.

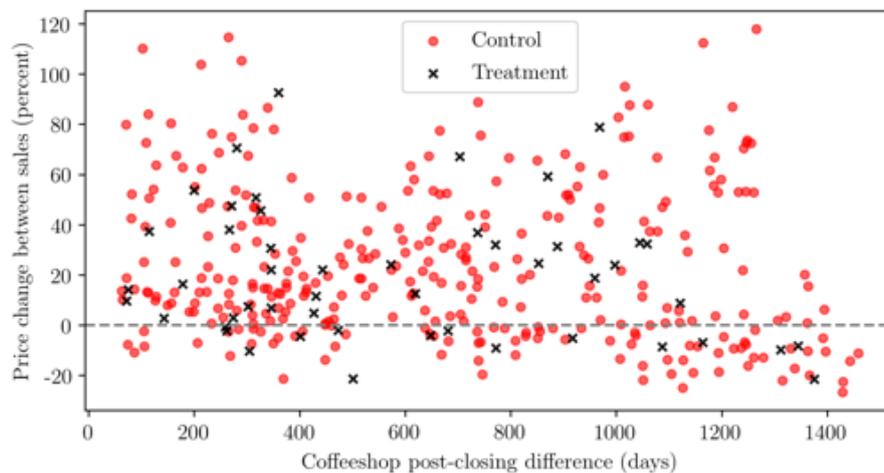
<sup>36</sup>We use coffeeshop level fixed-effects as a group identifier, but our findings remain using 5-digit postcode-level fixed-effects.

<sup>37</sup>We further examine apartment and single-family houses individually, individual cities, different fixed-effect controls, and individual closing years, but closing effects remain insignificant.

## Appendix D Repeated Sales Analysis

Figure C

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 VI**  
**Repeated Sales - Control Variables**

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