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Published in:
Journal of Economic Behavior & Organization

DOI:
10.1016/j.jebo.2022.05.005

IMPORTANT NOTE: You are advised to consult the publisher's version (publisher's PDF) if you wish to cite from it. Please check the document version below.

Document Version
Publisher's PDF, also known as Version of record

Publication date:
2022

Link to publication in University of Groningen/UMCG research database

Citation for published version (APA):

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“No drugs in my back yard:” The ambivalent reception of cannabis retailers

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\textbf{A R T I C L E   I N F O}

\textbf{Article history:}  
Received 9 March 2021  
Revised 11 April 2022  
Accepted 11 May 2022

\textbf{JEL classification:}  
H43  
I12  
K23  
K42  
L83  
R21

\textbf{Keywords:}  
Drug policy  
Cannabis sales  
Disamenity  
Housing demand  
Job creation  
Crime

\textbf{A B S T R A C T}

Can individuals’ aversion to drug markets curb the benefits of decriminalization? We investigate the effect of two policies on housing demand in the Netherlands: the distance-to-school criterion, which closed some cannabis shops in a few cities; and the zero-tolerance policy, which banned shops within municipal jurisdictions. While a small increase in the distance to retailers raised house prices by 1–5%, a substantial increase reduced them by 1–6%. Both policies reduced property crime, but the zero-tolerance was also related to fewer jobs. Our findings reveal that cities benefit from having cannabis shops, but households’ aversion to related nuisances deprecates surrounding areas.

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\textbf{1. Introduction}

Decriminalizing and legalizing drug markets are often presented as part of the solution for the war on drugs (Miron and Zwiebel, 1995; Becker et al., 2006). Indeed, recent studies confirm its positive impacts on several dimensions, such as health care (Bradford and Bradford, 2018), violence (Gavriloa et al., 2017), and traffic deaths (Anderson et al., 2013). However, decriminalization may also have side effects on the distribution of nuisances (e.g., Adda et al., 2014) and social outcomes (e.g., Marie and Zölitz, 2017). Moreover, certain households may present an unjustifiable aversion to this sort of market.

\textsuperscript{∗} The authors thank the editor, Daniel Houser and an anonymous referee, as well as Jason Barr, Stephen Billings, Lucas Davis, John Friedman, Jonathan Hall, Marco Manacorda, Olivier Marie, Jan van Ours, João dos Santos, Danna Thomas, Christian Traxler, Srimay Vellupuram, and participants in the Xi Transatlantic Workshop on the Economics of Crime (TWEC), 14th Meeting of the Urban Economics Association (UEA), and in the Urban Economics Session at the 45th EEA Conference for their helpful comments and suggestions. They also thank NVM for providing its housing transaction data, Intraval for providing data on closures and drug policies, and Martijn Droes, Marc Francke, and Dorinth van Dijk for helping with housing data. The Online Appendix is available here.

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https://doi.org/10.1016/j.jebo.2022.05.005

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As pointed out by Roth (2007), this aversion creates market constraints, which are hard to predict. If local authorities tie their policies to housing demand, residents may have the power to push legal drug retailers away from their neighborhood and eventually from the city. In this scenario, a gentrification policy becomes a prisoner’s dilemma: the existence of legal retailers minimizes the overall damage and makes everybody better off; but if the individual costs are not equally shared, no one wants to live near the drug outlets, which makes a decriminalization policy unsustainable.

This paper estimates both the aggregate impact that cannabis retailers have on the city and the households’ aversion to this type of business. To identify these effects, we exploit two types of bylaws that restricted the location of cannabis retailers (or “coffeeshops”) in the Netherlands. One is the distance-to-school criterion, which establishes a minimum distance between coffeeshops and schools. This policy closed some, but not all, coffeeshops in a few cities, slightly increasing the distance from households to the nearest shop. The other is the zero-tolerance policy, which bans all coffeeshops from a municipality and hence substantially increases the cost of finding cannabis.

These two policies are just some of the broader developments in the Dutch drug policy. While a few countries, such as Canada and Uruguay, and some states in the U.S. recently legalized the sale of recreational marijuana, the Netherlands has experienced its decriminalization for more than four decades. By applying an unprecedented approach to cannabis, its drug laws have evolved as critical issues emerged. In the first 20 years, legal retailers spread throughout the country, gradually moving from backstreets to more accessible locations (Jansen, 1991). At the same time, some groups, including citizens and politicians, grew dissatisfied with the approach, complaining about the externalities created by an excessive number of coffeeshops (Bieleman et al., 2010). As a result, regulation became stricter. In the last 20 years, local authorities have gained the legal power to revoke licenses and close cannabis stores whenever it deemed it necessary.

With the law in cities’ favor, we confirm that they have used their legal tools to close cannabis retailers in less valuable areas. Hence, since municipalities can pick their targets, we focus our analysis on two policies that do not give them room to close specific establishments. That is, the same rule is enforced to everyone within their jurisdiction. Then, we compare areas that were affected by a closure criterion with those that were not. For the sake of causal inference, the exogenous variation comes from the clearance provided by the new legislation. For example, more conservative towns may have been more willing to ban cannabis sales before, but they did not have the legal means until recently. To control for unobserved differences between zero-tolerance areas and the others, we estimate a model with fixed effects. Given that time-variant events might also influence these policies, we verify whether outcomes had changed before their implementation.

For each policy, we look at changes in house prices to assess welfare gains (Rosen, 1974; Thaler, 1978; Banzhaf, 2018).\(^1\) Our hypothesis is that a marginal increase in the distance to cannabis outlets reduces local nuisances. Hence, the value of houses situated near closed shops should increase. On the other hand, a significant increase in distance is expected to reduce users’ willingness to come to the city, diminishing its economic attraction. Moreover, the ban on legal suppliers may increase the market for illegal dealers. Accordingly, households, including non-users, should be less attracted to the city after a ban. Depending on the level of externality and cost of law enforcement, a ban on suppliers may be inefficient (Gruber, 2011).\(^2\) As a result, property values should decline.

Consistent with our main hypothesis, we find that the closures caused by the distance-to-school criterion raised house prices by 4–5% in surrounding areas, between 0.2 and 0.4 miles from a closed establishment. However, houses within 0.2 miles were not as much affected because there were still near other coffeeshops after the closures. By contrast, the zero-tolerance policy reduced prices by 2–6% within 0.3 miles of closed shops. Given that these policies caused distinct effects on the shortest distance to shops, our results imply that the relationship between housing values and this distance is non-monotonic. Namely, households are found to pay a 2–7% premium to be more than 0.3 miles away from a cannabis retailer, but they have a 5–7% discount if they are farther than two miles. Nevertheless, the non-monotonic relationship is not necessarily explained solely by the demand for legal cannabis. According to our results, it is also related to the different types of externality that appear near and far away from cannabis shops.

As regards observed nuisances, we find that both policies reduced by 12–24% the number of property crimes. We also show that they reduced crimes related to soft drugs, but just temporarily, and had no effect on violence. These results help us to understand why households pay a premium to live far from cannabis shops.\(^3\) In line with the decrease in house prices, the municipalities that banned cannabis shops altogether have also experienced other adverse effects. First, the ban raised the number of crimes related to hard drugs by almost 24%, which corroborates the idea that hard drugs are a substitute for soft drugs. Second, the zero-tolerance policy reduced by 9% the number of jobs in those municipalities. Therefore, overall economic activity suffers from strict constraints on the drug market, and the costs of banning retailers outweigh the benefits, deprecating local housing.

As a robustness check, we further apply a regression discontinuity design to estimate the effect of the distance-to-school criterion, which confirms the increase in house prices. This increase also persists after controlling for the time-varying relationship between prices and distance to schools. Furthermore, the effect of both policies remains the same after we control

\(^1\) The relationship between amenities and house prices has been broadly used to measure the monetary benefit of public policies. A non-exhaustive list includes studies on the value of school quality (Black, 1999; Cellini et al., 2010), industrial pollution and health risk (Greensite and Gallagher, 2008; Currie et al., 2015), air quality (Chay and Greensite, 2005), and quality of neighboring properties (Rossi-Hansberg et al., 2010).

\(^2\) See also Buchanan and Tullock (1975) and Glaser and Shleifer (2001).

\(^3\) Unfortunately, our data are not granular enough to estimate crime displacement. However, the spatial distribution of effects on house prices suggests that this mechanism does not explain our main findings.
for the distance to bars and nightclubs. Finally, neither policy is found to affect housing supply, and the non-monotonic pattern is not explained by the sorting of demographic groups.

The main contribution of this paper is to unveil the ambivalent effect of drug decriminalization in the same context. Previous studies tend to point to only one side of the story because the policy is investigated at a single level. For instance, Conklin et al. (2017) and Cheng et al. (2018) find that the legalization of cannabis dispensaries increased housing capitalization in Colorado. These studies compare a situation in which legal suppliers exist with another in which they do not. In another study, Adda et al. (2014) show that housing demand declined after the decriminalization of drug possession in a London borough. In their case, areas criminalizing possession co-exist with decriminalization areas in the same city. In our study, we use the zero-tolerance policy to replicate the former framework and the distance-to-school criterion to replicate the latter.

As a standard practice, those studies also compare homes near dispensers or hot spots with others farther away. The main concern with this approach is that comparison areas not only follow distinct trends but are also subject to different interventions and spillovers. To alleviate this concern, we exploit a unique setting that switches off the “treatment,” so we can compare homes that were equally distant to the shops. This sort of comparison allows us to control for other interventions in trade areas, such as changes in law enforcement, that do not necessarily lead to closures. By comparing homes farther away from shops, we can also verify the magnitude of general equilibrium effects.

In addition to the analysis of housing demand, the present work is also related to studies on legalization and unemployment (van Ours, 2006; Sabia and Nguyen, 2018), demand for cannabis (Williams, 2004; Jacobi and Sovinsky, 2016), substitute drugs (Chu, 2015; Bradford and Bradford, 2018), and criminal activity (Gavrilova et al., 2017). In terms of policy implications, the aversion of households to drug businesses and their associated nuisances can potentially constrain the legal market (Roth, 2007). This idea is also present in studies that examine payments for organ transplant (Elias et al., 2019), the marketplace of sex work (Cunningham et al., 2017; Giambona and Ribas, 2018), the history of methamphetamine production (Congdon-Hohman, 2013; Dealy et al., 2017), and the aversion to diverse neighborhoods (Funderburg and MacDonald, 2010; Diamond and McQuade, 2019). Our paper is also broadly related to studies on the capitalization effect of property crimes and risk perception (Thaler, 1978; Gibbons, 2004; Pope, 2008; Linden and Rockoff, 2008; Gautier et al., 2009).

The remainder of the paper is organized as follows. Section 2 gives an overview of the history of drug policy in the Netherlands and explains the process of closing cannabis shops. Section 3 describes the sample and data sources. Section 4 discusses how the relationship between distance to cannabis shops and house prices has changed after stricter regulations were adopted, and which areas were targeted by closures. Section 5 explains the identification strategy and the difference-in-difference model that we apply. Section 6 presents the estimated effects of closure policies on housing values, and Section 7 presents the results for potential mechanisms. Finally, Section 8 concludes the paper. Some of the results mentioned in this introduction are in the Online Appendix.

2. Drug policy in the Netherlands

2.1. The first two decades

For decades, the Netherlands has been one of the main travel destinations for recreational marijuana users. Although cannabis sale and possession are still criminal offenses under Dutch law (Article 3 of the Dutch Opium Act), these activities have been tolerated by the Public Prosecutor since 1976 (van Laar and van Ooijen-Houben, 2009). The Dutch policy towards cannabis falls somewhat between the decriminalization of possession, as seen in Italy, Spain, and Portugal, and the complete legalization of markets, as seen in Canada and Uruguay (MacCoun and Reuter, 1997). This approach is sustained by a history of pragmatic tolerance towards moral issues and leniency towards petty crimes, giving priority to the reduction of harm over moral outrage (Buruma, 2007).

The Criminal Proceedings Guidelines of 1980 was the first official document that created the possibility to establish cannabis retailers, known as coffeeshops (Nederlandse Rijksoverheid, 1980). Under these guidelines, tolerated outlets would face criminal prosecution only if they were to sell hard drugs, publicly advertise their products, or act provocatively. From 1987 to 1991, more rules were added, forming the AHOJ-G criteria — see Section 9 of the Online Appendix. Shop owners who violate one or more of these criteria are subject to criminal prosecution (Nederlandse Rijksoverheid, 2015).

The national guidelines of 1980 also gave more discretion to local authorities. The municipalities could, for instance, enforce constraints on opening hours, number and location of establishments, and sales of other soft drugs (e.g., magic mushrooms). The direct result was that regulation became more lenient and cannabis shops spread from backstreets to more accessible locations (Jansen, 1991). In the whole country, the number of cannabis outlets peaked around 1996 with approximately 1500 establishments (Bieleman et al., 1996).

As the number of suppliers soared, the system of pragmatic tolerance and local discretion caused some critical problems. First, residents and local politicians started to complain about the social nuisance created by an excessive number of coffeeshops (Bieleman et al., 2010; 2015). Second, the demand from residents in countries nearby, such as Germany and France, led to disputes in the European Union (de Kort, 1995; Blom and van Mastrigt, 1996). Finally, selling small quanti-
ties of cannabis at the front door is tolerated, but transactions at the back door (i.e., the supply chain) remain unregulated. Importing cannabis and commercial growing are both criminal offenses.\(^5\) As a result, almost all cannabis outlets work with criminal suppliers. For more than two decades, this “back door” problem was associated with a rise in amateur production, hidden in ordinary houses, and organized crime (Bovenkerk and Hogewind, 2003).

### 2.2. The last two decades

Given all these issues, the late 1990s marks the beginning of stricter policies. Since 1996, municipalities have been allowed to ban cannabis retailers within their jurisdiction (Breunese et al., 1996; Bruijn and Post, 2017). Nowadays, about 70% of the municipalities have a zero-tolerance policy towards cannabis sales, and almost every city provides a limited number of licenses to this type of business (Bieleman et al., 2017). Even so, in areas with coffeeshops, a ban cannot be immediately enforced. First, the municipality has to go through a legal battle against existent owners, which may take years.\(^6\)

Since 1999, a new provision in the Opium Act (Article 13b) has helped local authorities in this battle, giving them the power to close shops that do not comply with the AHOJ-G criteria. Although the new provision reduced the disturbance around coffeeshops, not many of them were closed because they often complied with the AHOJ-G criteria.

A more meaningful change was the Public Administration Probity in Decision-Making Act of 2003 (Bevordering Integriteitsbeoordelingen door het Openbaar Bestuur, BIBOB), which gave local governments the right to refuse permits to any company owner under criminal investigation. Unlike the violation of the AHOJ-G, the BIBOB allows the municipality to shut down a business even before the investigation is over. Some argue that the BIBOB has been used as an instrument for gentrification, mostly targeting cannabis retailers and brothels (e.g., Neuts et al., 2014; Barber, 2014).

The new stringent policies reduced the number of cannabis shops in the country from 846 in 1999 to 573 in 2016 (Bieleman et al., 2017).\(^7\) Fig. 1 presents in the left-hand map the density of coffeeshops in 2016 and the location of retailers that have closed since 1999. The presence of cannabis shops is still higher in the three largest cities: Amsterdam, with 173 shops, Rotterdam, with 40, and The Hague, with 36. In the map we also observe that other cities — namely, Utrecht, Eindhoven, Tilburg, Groningen, Nijmegen, Enschede, Haarlem, Arnhem, Leiden, Leeuwarden, and Hilversum — have between six and 20 shops each. The population in these cities varies between 83,000 in Hilversum and 290,000 in Utrecht. In addition to these cities, 24 other municipalities have three to five shops, and 62 have one or two.

The right-hand map in Fig. 1 shows in red the eight municipalities that had marijuana outlets in 1999 but applied a zero-tolerance policy up to 2016. In (light and golden) yellow are the municipalities that closed some of their coffeeshops, which include all those that once had six shops or more, except for Haarlem. In green are the municipalities where the number of shops remained the same from 1999 to 2016. For our estimates, the red areas are treated differently from the yellow ones because they had no nearby substitute for the closed retailers. Whereas the coffeeshop closures in the yellow

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\(^5\) A household is not allowed to have more than five marijuana plants.

\(^6\) In some cases, municipalities just stopped providing new permits after a shop owner died or retired.

\(^7\) Based on data from LexisNexis, it has been rare for coffeeshops to go out of business.
areas affected access to a retailer at the intensive margin — i.e., marginally increasing the distance to the nearest one —, the closures in the red areas affected access at the extensive margin — i.e., for all residents there was a significant increase in the distance to the nearest shop.

2.3. The distance-to-school criterion

In Fig. 1, we also highlight in golden yellow the municipalities that closed coffeeshops for being near schools. In 2002, 64% of the municipalities with a cannabis retailer had already implemented a ‘distance-to-school’ criterion (Bieleman et al., 2003). Although it was not one of the AHOJ-G criteria, the national government proposed a nationwide distance-to-school criterion in May 2011. The plan was to close all retailers located within 350 m of high schools (Bieleman et al., 2017). Yet, this idea was abandoned in November 2012 (van Ooijen-Houben et al., 2014). Ever since, municipalities keep adopting their own rules on the distance to schools. The minimum distance to a school varies from 50 m to 500 m, based on direct or overall proximity and type of school (van Laar and van Ooijen-Houben, 2009).

Although the share of municipalities that officially applies the distance-to-school criterion rose to 75% in 2014 (Bieleman et al., 2015), only four have closed retailers because of it: The Hague, Amsterdam, Rotterdam, and Amersfoort. The Hague implemented this criterion in 2007, but two years later only one out of 40 retailers was closed. Amsterdam has applied it since 2014, yet through several steps, from restricting shops’ operating hours to gradually increasing the minimum distance to schools. In 2017, the city authorities were still battling to move cannabis shops away from schools (Aydin et al., 2018).

The enforcement of the criterion had a more meaningful impact in Rotterdam and Amersfoort. The former closed 17 retailers in 2009, whereas the latter closed two of the seven shops it had in 2010. In Rotterdam, the establishments should not be within 200 m (or the walking distance of 250 m) of a high school, in the direct vicinity of a primary school, or within 400 m of both a secondary and a primary school. In Amersfoort, they should not be within 400 m of a high school or 100 m of a primary school.

Still, the criterion applied by Rotterdam was so fuzzy that a few coffeeshops managed to stay open, even within the minimum distance. Fig. 2 shows the distribution of cannabis outlets by distance to schools in those cities. The blue bars represent the number of retailers before the policy, and the red bars represent the number of closures afterwards. Note that not every shop within 200 m of a secondary school was closed, but some between 200 and 400 m were. Moreover, the closures took place in several neighborhoods. Fig. A1 of the Online Appendix displays their location, as well as the location of current retailers.

Even though the closure of coffeeshops can be triggered by one of many unlawful events and motivated by gentrification policies, the enforcement of a distance-to-school criterion should provide an exogenous time variation in the location of retailers. In Section 5, we explain how this policy yields comparable areas within municipalities.

3. Sample and data sources

Since our goal is to combine all the information available on cannabis retailers, drug policy, house prices, crime rates, and employment levels, we retrieve our data from a variety of sources. For all these sources, we restrict the sample to cities and towns that have had at least one retailer within one mile of their borders since 1999. It is worth noting that the number of municipalities in the Netherlands declined from 538 in 1999 to 388 in 2017. Hence, we adjust all the data listed below to the municipal areas in 2017. Even so, the municipal aggregation does not affect the distribution of coffeeshops.

Another sample restriction is applied to Amsterdam and The Hague. The former contains more than 30% of retailers in the Netherlands. While other cities where the sale of cannabis is allowed had, on average, five establishments in 1999, or 25,000 inhabitants per shop; Amsterdam had 288 establishments, or 2500 inhabitants per shop. Our concern is that this city is not an appropriate benchmark for other municipalities, so its inclusion would strongly drive the estimates in the comparison group. Moreover, Amsterdam has always been flooded with tourists searching for recreational drugs. Indeed, de Castro (2018) shows that the closure of coffeeshops in Amsterdam led to a reduction in the rents listed on Airbnb.com. This finding is consistent with the reduction in house prices found by Aydin et al. (2018), but it is mostly driven by the value of cannabis supply to tourists. It does not tell us much about housing demand, or the value of coffeeshops to residents.\footnote{Given our identification strategy, explained in Section 5, almost all houses and neighborhoods of Amsterdam would be excluded from the sample anyways. The reason is many discretionary closures took place in different parts of the city over the last 20 years.}

The Hague had the second-largest number of cannabis retailers in 1999, with 70 establishments, and the second-highest density, with about 6300 inhabitants per shop.\footnote{The third- and fourth-highest densities were Leeuwarden and Maastricht, with 6800 and 7600 inhabitants per shop, respectively.} However, by 2003, 41% of its cannabis shops had been closed. Unfortunately, we are unable to accurately locate these shops or identify the reasons for their closure. As a result, we consider that neither The Hague nor Amsterdam is a reliable control group for the policies that we evaluate below, so we exclude them from the sample.
3.1. Location and closures of cannabis shops

Following Sanderink (2016), the list of cannabis shops comes initially from two online directories: www.coffeeshopdirect.com and www.coffeeshopsinfo.nl. Both sites are collaborative and collect information on each retailer that has ever operated since 1998. For each shop, they include name, full address, status (open or closed), reviews, and street view.

To validate the information provided on these websites, we use several other sources, described in Section 10 of the Online Appendix. After many steps, we confirmed the closure of 123 shops (not including those in Amsterdam and The Hague) and discovered the date and reason for 77 of them. The 46 remaining closures happened outside the areas included in our empirical analysis.

3.2. House prices

Our source for house prices is the Dutch Association of Real Estate Brokers and Experts (Nederlandse Vereniging van Makelaars en Taxateurs, NVM), which has more than 4000 members. Its members handle approximately 70% of all transactions.

Fig. 2. Distribution of Cannabis Retailers per Distance to Schools. This figure shows on the left the number of retailers per distance to secondary school. On the right, the graphs show the number per distance to primary school. The blue bars represent retailers operating just before the enforcement of the distance-to-school criterion. The red bars represent those closed by this policy. (a) Rotterdam. (b) Amersfoort. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)
of owner-occupied homes in the Netherlands. According to de Wit et al. (2013), its sample is unbiased and gives a reliable picture of the Dutch housing market.

NVM members are required to report all dwellings offered for sale. The information given includes geocoded location, transaction price, and the date of sale, as well as initial asking price and first day on the market. Location refers to the centroid of six-digit postal codes, which is a narrow range of about six house numbers on the same street. For this paper, we consider housing transactions from 1999 to 2016, excluding dwellings that were either withdrawn from the market or remained unsold for more than six years, and had either more than 500 square meters or less than 20 square meters. We also exclude houses that were never closer than three miles from the nearest coffeeshop.

The NVM records also include an exhaustive list of characteristics and subjective assessments of the dwellings. Table A1 of the Online Appendix reports their descriptive statistics. On average, the dwellings in our sample are 115 square meters, have four rooms and two floors, and are one mile from a cannabis retailer. A third of them are townhouses, and another third are apartments. As regards the surroundings, properties are, on average, 0.2 miles from the nearest primary school, 0.6 miles from a secondary school, and 1.3 miles from a train station.

3.3. Other data sources

Registered crime data come from two sources. From the CBS StatLine, available at statline.cbs.nl, we obtained the number of crimes by type and year at the municipal level. This information is available from 2005 to 2015, and we aggregate it by municipal area in 2017. This aggregation does not allow us to verify whether variations in crime happened near coffeeshops. However, since the zero tolerance was applied in multiple municipalities and not at the same time, changes in municipal level can be used to estimate their net impact.10

From Rotterdam’s buurtmonitor (rotterdam.buurtmonitor.nl), we obtain the annual number of crimes, from 2005 to 2016, at the neighborhood (buurt) level in this city.11 Most of the municipalities in the Netherlands report neighborhood-level crimes since 2010, which is not helpful for our study. The buurtmonitor, however, can be used to assess the distance-to-school criterion in Rotterdam, allowing us to compare neighborhoods within the city.

From the CBS StatLine, we also collect variables such as demographic composition, number of employees, unemployment, and regional gross domestic product (GDP). Demographic characteristics are available up to 2013 at the four-digit postcode level. The number of workers employed per municipality is available from 2004 to 2014. The unemployment rate of municipal residents is available from 2003 to 2016. Finally, GDP is available from 1995 to 2016, but at the COROP level. COROP (Coördinatiecommissie Regionaal Onderzoeksprogramma) is a socio-economic area that comprises municipalities within a province. Accordingly, we use the COROP GDP to control for macroeconomic shocks.

From CBS, we also obtain the housing stock in a 500m-by-500m raster grid from 2000 to 2012.12 From the original grid, we exclude cells that never had a house or were never closer than three miles from a coffeeshop. These data are used to estimate changes in housing supply after closures. Table A2 of the Online Appendix presents the descriptive statistics of the additional variables.

The last part of our data set includes the location of bars (and nightclubs), schools, and train stations. The locations of primary and secondary schools in 2016 and train stations in 2010 are from VU Geoplaza (geoplaza.vu.nl). These data are used in our cross-sectional estimates and to verify whether the distance-to-school criterion coincides with other events happening near schools. The location of establishments serving alcoholic beverages comes from Orbis from Bureau van Dijk. These data are used to test whether the closing of coffeeshops affected the density of bars, which could also influence housing demand and crime rates. Table A1 shows the average distance from homes in our sample to these amenities.

4. Descriptive analysis of house prices and cannabis retailers

To describe the location of cannabis retailers, we estimate the relationship between the log price per square meter and distance to them, controlling for town fixed effects, year effects, and housing characteristics. This relationship gives the relative premium of living near coffeeshops. In Table 1, we present it for two different periods: 1999–2001, when the number of coffeeshops was the highest; and 2012–2016, after most of the closures took place. For both periods, we consider two regressors: the shortest distance to a retailer in 1999, and the one in 2016. While the former indicates what happened to the coffeeshops’ original areas, the latter indicates what happened to the areas where they remained open.

There are three facts that we observe in Table 1: 1) cannabis shops tend to appear in the most expensive areas; 2) in two decades, these areas have become even more expensive, and; 3) shops were closed in less valuable areas. The third remark helps to explain why the relationship between distance and prices became steeper, from column (1) to (5). Even so, an appreciation also happened in areas that still have retailers, which may be due to the enforcement of a stricter regulation. The policy of nuisance reduction and ultimately closing some outlets may have unleashed the potential value of the areas with a remaining coffeeshop. Still, the effect of closures per se remains to be seen.

10 Bisschop et al. (2017) apply a similar strategy to estimate the impact of street prostitution zones on crime in large Dutch cities.
11 Rotterdam has 92 neighborhoods (buurten); a buurt has about 7000 inhabitants on average.
Table 1
Cross-Sectional Relationship between House Prices and Distance to Retailer.

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<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
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<tr>
<td>Distance in 1999</td>
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<td></td>
<td></td>
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<tr>
<td>&lt; 0.2</td>
<td>0.044***</td>
<td></td>
<td>0.056***</td>
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<tr>
<td></td>
<td>(0.014)</td>
<td></td>
<td>(0.016)</td>
</tr>
<tr>
<td>0.2–0.4</td>
<td>0.024**</td>
<td></td>
<td>0.048***</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td></td>
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<tr>
<td>0.4–1.0</td>
<td>0.015*</td>
<td></td>
<td>0.032***</td>
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<td>(0.008)</td>
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Distance in 2016 (miles)

| < 0.2                   | 0.057***        | 0.099**   | 0.067***  | 0.094**   |
|                         | (0.017)        | (0.041)   | (0.019)   | (0.042)   |
| 0.2–0.4                 | 0.035**         | 0.071**   | 0.060***  | 0.088**   |
|                         | (0.013)        | (0.035)   | (0.015)   | (0.038)   |
| 0.4–1.0                 | 0.027***        | 0.063***  | 0.046***  | 0.076***  |
|                         | (0.009)        | (0.021)   | (0.010)   | (0.023)   |

House characteristics

| Year dummies | Yes | Yes | Yes | Yes | Yes | Yes |
|             | Yes | Yes | Yes | Yes | Yes | Yes |
| Town fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |

Number of observations

| Number of observations | 171,804 | 171,804 | 171,804 | 300,967 | 300,967 | 300,967 |
|                       | 201    | 201     | 201     | 201     | 201     | 201     |

This table presents the linear regression of log price per square meter of traded houses on their shortest distance to a cannabis retailer. This distance is represented by three dummies: less than 0.2 miles, between 0.2 and 0.4 miles, and between 0.4 and one mile; the reference is the group of houses farther than one mile. Columns (1) to (3) show estimates for transaction prices from 1999 to 2001 as a function of distance in 1999, distance in 2016, and distances in both 1999 and 2016, respectively. Columns (4) to (6) show the same regressions but from 2012 to 2016. All regressions include house characteristics, as in Table A1, year dummies, and town fixed effects. The sample includes properties located up to 3.1 miles from the nearest retailer in towns that have ever had a retailer within one mile. Clustered robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

We also estimate the relationship between demographic characteristics and the distance to a retailer. Table A3 of the Online Appendix displays the estimates. In summary, areas near coffeeshops concentrate more men, people between 15 and 39 years old, single-person households, and Western immigrants. In contrast, these areas present relatively fewer children and adults over 40. The closures were more often in areas with a lower share of adults under 40, higher shares of adults over 40 and non-Western immigrants, and more elderly people in the vicinity. Over time, these areas have experienced an outflow of adults under 40, compensated by an inflow of adults over 40 and Western immigrants.

These initial findings stress the importance of controlling for local characteristics, as well as using a comparison group that is equally distant to a retailer, when estimating the effect of closures. Presumably, the postcode fixed effects should control for the selection of areas with closed retailers, and the comparison group should control for the overall appreciation of areas with a coffeeshop. If specific variations in property values triggered the closure policies, we should observe this variation before treatment (see Fig. 3 below).

5. Empirical strategy

This section describes our empirical models. First, it presents the reduced-form difference-in-differences (DiD), which compares houses whose nearest retailer was closed with houses that were equally distant from a retailer. For this comparison, we consider only closures forced by the distance-to-school criterion (DTSC) and the zero-tolerance policy (ZTP). Homes whose nearest shop was closed for another reason are excluded from the analysis.

The closures made by those policies result in two instrumental variables (IVs): nearest shop closed by the DTSC and nearest shop closed by the ZTP. While the former instrument is used to identify the effect of a small but significant change in the distance to retailers, the latter is used to identify the effect of a much larger increase in distance. Accordingly, we apply an IV strategy, described in the second part of this section, to estimate the nonlinear effect of distance to cannabis outlets on house prices. Other empirical models used in this study are described in the Online Appendix.

5.1. Reduced form

Closing a coffeeshop should, if anything, increase the shortest distance from homes to retailers. Thus, we first estimate the effect that closures have on house prices. Let $y_{it}$ be the log price per square meter of house $i$ in location $l$ and year $t$, $c_i$ be the indicator whether the nearest retailer is closed, and $q_i$ be the year of closure. The average effect is identified by the following DiD estimator:

$$
\tau = E(y_{it}|c_i = 1, t \geq q_i) - E(y_{it}|c_i = 0, t \geq q_i) − [E(y_{it}|c_i = 1, t < q_i) - E(y_{it}|c_i = 0, t < q_i)] \quad (1)
$$
where \( c_i \) is one if the nearest retailer is closed and zero otherwise. Location refers to the centroid of a six-digit postcode.

The DiD parameter, \( \tau \), is estimated using a fixed-effect model and varies with the initial distance to a cannabis shop:

\[
y_{ltk} = \begin{cases} 
\tau^0 c_i \cdot I(t \geq q_l) + \beta^0 x_i + \theta^0 gd p_{ltk} + \mu^0_l + \mu_l + \epsilon_{ilt} & \text{if } d_{lt} \in [0, D^0) \\
\tau^1 c_i \cdot I(t \geq q_l) + \beta^1 x_i + \theta^1 gd p_{ltk} + \mu^1_l + \mu_l + \epsilon_{ilt} & \text{if } d_{lt} \in [D^0, D^1) \\
\vdots & \vdots \\
\tau^n c_i \cdot I(t \geq q_l) + \beta^n x_i + \theta^n gd p_{ltk} + \mu^n_l + \mu_l + \epsilon_{ilt} & \text{if } d_{lt} \in [D^{n-1}, D^n). 
\end{cases}
\]

where \( x_i \) is a vector of house characteristics, \( gd p_{ltk} \) is the regional log GDP, \( \{\mu^j_l\}_{j=0}^n \) are year-specific effects, \( \mu_l \) is the postcode fixed effect, \( \epsilon_{ilt} \) is the deal-specific error term, \( d_{lt} \) is the initial distance from location \( l \) to the nearest shop, and \( \{D^j\}_{j=0}^n \) is a set of distance thresholds — say 0.2, 0.3, 0.4, 0.6, and 0.8 miles. Standard errors are clustered by postcode to account for serial error correlation (Bertrand et al., 2004).

Since the regulation of sales and the demand for cannabis have evolved, the (dis)amenity created by retailers has also varied. Even if the distance of a home to retailers remains the same, its price may respond to changes in nuisances. To control for time-varying unobservables, we let the year-specific effects, \( \{\mu^j_l\}_{j=0}^n \), vary across location ranges so that they capture distinct improvements happening near and far away from cannabis outlets. This type of DiD is different from those applied by, for instance, Linden and Rockoff (2008) and Conklin et al. (2017), who instead compare near and distant areas over time. By breaking the effect into several location ranges, we can also verify the magnitude of general equilibrium effects on further areas.
In practice, we investigate two types of closures: those caused by the DTSC, and those followed by a ZTP. The two treated groups comprise homes whose nearest retailer was closed by one of these policies. The comparison group is of homes whose distance to the nearest and second-nearest shop does not change during the sample period. For the DTSC, we restrict the comparison group to homes that are in the same municipality and still close to a retailer. To test whether treated and comparison groups follow similar trends before the closures, we extend model (2) by adding lead and lagged variables — i.e., we replace $I(t ≥ q_l)$ with $\{I(t − k ≥ q_l)\}_{k=−5}^3$.

5.2. Instrumental variable

As presented later, the ZTP has a much larger impact on the distance to a coffeeshop than the DTSC does. This difference allows us to identify the nonlinearity in the distance function and to test whether having other shops nearby after a closure makes any difference. Accordingly, we use $c_l · I(t ≥ q_l) · I(\text{policy})$ as instruments for changes in distance, where ‘policy’ is either DTSC or ZTP. Then we estimate the following model:

$$y_{ilt} = γ_1(I(d_{il} < D^1) + γ_2(I(d_{il} > D^2) + βx_l + θgdpt_l + μ_l + μ_i + ε_{ilt}),$$

where $I(d_{il} < D^1)$ and $I(d_{il} > D^2)$ are endogenous and indicate whether a house is near and far away from retailers, respectively. Coefficients $γ_1$ and $γ_2$ represent the distance gradients.

If the distance, $d_{il}$, did not increase in some locations, its variance would not change, and it would be canceled out by the postcode fixed effects, $μ_l$. Plus, with the IVs, the variation in $d_{il}$ must come from either the DTSC or the ZTP. In our main results, we set $D^1$ to be 0.4 miles and $D^2$ to be 2 miles. These choices are based on the specification that maximizes Kleibergen and Paap’s (2006) LM statistic for under-identification and Wald statistic for weak identification. Still, we present alternative specifications, varying $D^1$ and $D^2$, in the Online Appendix. We also present results using restricted cubic splines with three knots, which generates two endogenous variables.

6. Main results

Our main results are separated into two parts. First, we show the estimates for the reduced-form effects of closing coffeeshops on the shortest distance to the remaining ones and on house prices. In this part, we also present placebo tests for changes in prices before the actual closures. Then, we present our IV estimates for the distance function of house prices and demographic composition.

6.1. The effect of closing retailers on house prices

The effect of closing cannabis retailers is estimated as in Eq. (2). However, we consider only two types of closures that we assume are exogenous. Namely, those caused by the DTSC and those caused by a ZTP. Other types of closures are less likely to be exogenous, because they depend on local authorities choosing which businesses to penalize.

The top panel of Table 2 shows how the shortest distance to a retailer is affected by those policies. If the DTSC closes a house’s nearest shop, its distance to the next one increases by 0.12–0.24 miles. If the closure followed a ZTP, the increase in distance is much higher. After such closures, the nearest retailer is found, on average, more than 3 miles away.

The second panel of Table 2 presents the reduced-form effects of those policies on housing demand, which are found to be contradictory. On the one hand, a closure due to the DTSC increased by 4–5% the value of properties between 0.2 and 0.4 miles from a retailer. While the ZTP, increased the effect on prices was smaller (1.5%) and insignificant; possibly because the effect on the shortest distance to a retailer was not large enough to make a home really far from it. That is, moving from 0.1 to 0.25 miles from shops is not as critical as moving from 0.2 to 0.35 miles. As we show below, house prices are significantly higher farther than 0.3 miles. The effect of the DTSC is also confirmed by a regression discontinuity design (RDD). See Table A4 of the Online Appendix.

On the other hand, the ZTP reduced by 6% the price of homes between 0.2 and 0.3 miles and by 2% within 0.2 miles. As we explain in the next part, the effect below 0.2 miles is smaller because it results from two opposing forces: the positive effect of having a shop within a convenient distance, and the negative effect of being too close to it. Properties that were more than 0.3 miles away from the closed retailer were not significantly affected by the ZTP. That is, further areas are not much affected by the new general equilibrium.

This result indicates that coffeeshops may add value to the local economy. In their complete absence, housing demand declines and properties depreciate, particularly in the area where they used to be. This finding is consistent with

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13 Municipalities that adopted the DTSC have also changed their drug policy. While a between-municipality comparison would also capture the changes in regulation, the within-municipality group controls for them.

14 One may argue that the DTSC was not strictly applied (see Fig. 2). Hence, the municipality, say Rotterdam, might have relaxed the rules in strategic areas. Given this concern, the RDD takes into account the fact that coffeeshops over 400 m from secondary schools were safe, while those under 400 m were at risk of closure. Then, for each house in Rotterdam and Amersfoort, we calculate the distance of its nearest retailer from a secondary school. To control for location, we estimate the discontinuities at 400 m on log prices and probability of closures, before and after the DTSC. That is, our RDD is actually a difference-in-discontinuity estimator (Grembi et al., 2016; Giambona and Ribas, 2018). Details are in Section 12 of the Online Appendix.
Table 2
Estimated Effect of Closures on Distance to Retailer and House Price.

<table>
<thead>
<tr>
<th>Distance to retailer (miles)</th>
<th>Initial distance to retailer (miles)</th>
<th>0–0.2</th>
<th>0.2–0.3</th>
<th>0.3–0.4</th>
<th>0.4–0.6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distance-to-school</td>
<td></td>
<td>0.147***</td>
<td>0.118***</td>
<td>0.127***</td>
<td>0.162***</td>
</tr>
<tr>
<td>(0.010)</td>
<td></td>
<td>(0.011)</td>
<td>(0.020)</td>
<td>(0.026)</td>
<td></td>
</tr>
<tr>
<td>Zero-tolerance</td>
<td></td>
<td>3.562***</td>
<td>3.070***</td>
<td>3.172***</td>
<td>3.416***</td>
</tr>
<tr>
<td>(0.161)</td>
<td></td>
<td>(0.221)</td>
<td>(0.173)</td>
<td>(0.117)</td>
<td></td>
</tr>
<tr>
<td>Log price per m²</td>
<td></td>
<td>0.015</td>
<td>0.041**</td>
<td>0.053**</td>
<td>−0.001</td>
</tr>
<tr>
<td>Distance-to-school</td>
<td></td>
<td>(0.017)</td>
<td>(0.021)</td>
<td>(0.020)</td>
<td></td>
</tr>
<tr>
<td>(0.015)</td>
<td></td>
<td>(0.021)</td>
<td>(0.020)</td>
<td>(0.012)</td>
<td></td>
</tr>
<tr>
<td>House characteristics</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Regional GDP</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year dummies × DTSC</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Postcode fixed effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Number of observations</td>
<td>73,937</td>
<td>50,368</td>
<td>45,659</td>
<td>94,497</td>
<td></td>
</tr>
<tr>
<td>Number of postcodes</td>
<td>8561</td>
<td>5445</td>
<td>5044</td>
<td>10,504</td>
<td></td>
</tr>
<tr>
<td>with distance-to-school</td>
<td>479</td>
<td>210</td>
<td>98</td>
<td>83</td>
<td></td>
</tr>
<tr>
<td>with zero-tolerance</td>
<td>284</td>
<td>210</td>
<td>228</td>
<td>484</td>
<td></td>
</tr>
<tr>
<td>comparison</td>
<td>7798</td>
<td>5025</td>
<td>4718</td>
<td>9937</td>
<td></td>
</tr>
</tbody>
</table>

This table presents the difference-in-differences (DiD) estimates, as in Eq. (2), for two outcomes: shortest distance to a cannabis retailer (top panel) and log price per square meter of traded houses (bottom panel). Each regression contains two DiDs: one for closures forced by the distance-to-school criterion (DTSC), and another for closures forced by the zero-tolerance policy. Each column shows a regression using a different sample, defined by the initial distance to a retailer. All regressions include house characteristics, as in Table A1, year dummies, year dummies times a dummy for municipalities that implemented the DTSC, and six-digit postcode fixed effects. The sample excludes houses in which the nearest retailer was closed for another reason. Clustered robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Conklin et al. (2017), who show that prices increase by 8% after medical dispensaries are converted into retailers, and with Cheng et al. (2018), who show a rise of 6% in municipalities in Colorado that legalized marijuana retail.

Nevertheless, if there are many retailers in the city, closing some of them reshuffles the housing demand, raising prices in adjacent neighborhoods and reducing prices further away. Although the city benefits from having cannabis retailers, households seem to pay to be just far enough from them. The latter is in line with Adda et al.’s (2014) evidence that the decriminalization of cannabis possession decreases house prices in hot spots and increases them in areas where it is still a crime.

As regards the consistency of our findings, one may argue that they result from differences in the previous trends between places where shops were closed and where they were not — i.e., a violation of the parallel trend assumption (Abadie, 2005). Fig. 3 shows the difference in log prices between treated and comparison groups for each year before and after the closure of the nearest shop. Before, the difference fluctuates around zero and does not exhibit a distinct trend for houses affected by the DTSC and by the ZTP.

After the nearest coffee shop closes due to the DTSC, prices immediately increase between 0.3 and 0.4 miles away and persist at least until the third year. The positive effect on homes between 0.2 and 0.3 miles appears as significant only four years later. Therefore, it takes a few years for the housing demand to adjust to the new policy fully.

Similarly, the effect of a ZTP seems gradual. For homes within 0.3 miles, the effect appears at least a year after the last coffee shop was closed in the municipality. For homes between 0.4 and 1 miles away, we observe a short-term effect that does not persist after three years. Overall, the ZTP shifts housing demand in different areas and with different timings, but the net impact on the local economy ends up negative after three years.

6.2. The estimated distance function

6.2.1. House prices

The findings in the previous part suggest that the relationship between house prices and distance to cannabis shops is non-monotonic. That is, a small increase in distance is followed by an increase in prices, while a large increase in distance depreciates prices. Table 3 presents the estimated distance function, as in Eq. (3), using two estimators. The first one takes into account changes in distance provoked by any closure. Then we restrict the sample to homes whose distance to the nearest and second-nearest shop has not changed, and those whose distance was affected by either policy. The closures

---

15 The higher significance of the effect for this range in Table 2 is due to the aggregation of values in the post-treatment period, which produces narrower confidence intervals.
Table 3
Estimated Effect of Distance to Retailer on House Price.

<table>
<thead>
<tr>
<th>Final distance (miles)</th>
<th>Initial distance to retailer (miles)</th>
<th>Least square</th>
<th>Instrumental variable</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>&lt; 0.3</td>
<td>&lt; 0.4</td>
<td>&lt; 0.5</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>&lt; 0.3</td>
<td>−0.016***</td>
<td>−0.013***</td>
<td>−0.015***</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.008)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>≥ 2.0</td>
<td>−0.056***</td>
<td>−0.034***</td>
<td>−0.028***</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.021)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>House characteristics</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Regional GDP</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year dummies</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year dummies × DTSC</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Postcode fixed effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Number of observations</td>
<td>199,172</td>
<td>274,769</td>
<td>350,269</td>
</tr>
<tr>
<td>Number of postcodes</td>
<td>22,087</td>
<td>30,268</td>
<td>38,518</td>
</tr>
</tbody>
</table>

This table presents the relationship between changes in the distance to the nearest cannabis retailer and in the log of house price per square meter, as in Eq. (3). The distance is represented by two dummies: less than 0.3 miles, and more than two miles; the reference is the group of houses between 0.3 and two miles. All regressions include house characteristics, as in Table A1, year dummies, year dummies times a dummy for municipalities that implemented the distance-to-school criterion (DTSC), and postcode fixed effects. The columns show the results from different samples, based on their initial distance to a retailer. In the top panel, regressions exploit any variation in the distance. In the bottom panel, variations in the distance are instrumented by the closures forced by the DTSC and the zero-tolerance policy. The sample in the bottom panel excludes houses in which the nearest retailer was closed for another reason. Clustered robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

made by the two policies are used as IVs for the distance function. In both models, we control for house characteristics, postcode fixed effects, regional GDP, and year dummies.

Since both house prices and the operation of coffeeshops can jointly respond to other local events, the variation in distance can be endogenous in the first estimated model (in the top panel of Table 3). With the IVs, though, changes in distance are triggered by exogenous events, which are not preceded by variations in house prices (see Fig. 3). Indeed, the least-square estimates point to the same conclusion as the IV estimates, but the magnitudes are at most half as large. Fig. A2 of the Online Appendix confirms that closures brought about for procedural reasons also increase the price of properties farther than 0.2 miles. However, it also indicates that prices farther than 0.4 miles were increasing before these closures.

The IV estimates in Table 3 reveal that houses are 2–7% less valuable if within 0.3 miles of retailers, but they are also 5–7% cheaper if located farther than two miles. In other words, house prices peak at a distance at which cannabis shops are far enough to avoid any disturbance, but they decrease moving farther. This result is robust to different distance categorizations; see Table A6 of the Appendix. If we expand the definition of “near” to within 0.4 miles, the magnitudes of IV estimates become much larger. However, the IVs get weaker. Thus, we choose to present more conservative estimates as our main results. Section 7 examines some reasons for the non-monotonicity in housing prices.

We also estimate the distance function with a restricted cubic spline. Fig. A3 of the Online Appendix suggests that prices are 15–25% higher about two miles from the retailers, and decline moving farther away. This pattern is similar to the one Giambona and Ribas (2018) report for prostitution in the Netherlands. Their evidence is that households pay an increasing premium to live as far as two kilometers from areas with prostitution.

6.2.2. Demographic composition

A critical concern is that the non-monotonic pattern resulted from the sorting of distinct demographic groups after the closures. That is, the households driving values down around retailers were different from those affecting values far away. To verify this issue, we estimate the share of demographic groups using the same IV model as in Table 3. For most groups, the estimates are not significant. In Table 4, we present those that are. Table A7 of the Online Appendix presents the remaining results.
Table 4
Estimated Effect of Distance to Retailer on Demographic Composition.

<table>
<thead>
<tr>
<th>Final distance (miles)</th>
<th>Initial distance to retailer (miles)</th>
<th>$&lt; 0.3$</th>
<th>$&lt; 0.4$</th>
<th>$&lt; 0.5$</th>
<th>$&lt; 0.6$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log share of 25–39 years old</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$&lt; 0.4$</td>
<td>-0.515$^*$</td>
<td>-0.466$^{**}$</td>
<td>-0.527$^*$</td>
<td>-0.545$^{**}$</td>
<td></td>
</tr>
<tr>
<td>(0.273)</td>
<td>(0.236)</td>
<td>(0.275)</td>
<td>(0.277)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$&gt; 2.0$</td>
<td>-0.462$^{**}$</td>
<td>-0.347$^{**}$</td>
<td>-0.394$^*$</td>
<td>-0.361$^{*}$</td>
<td></td>
</tr>
<tr>
<td>(0.222)</td>
<td>(0.169)</td>
<td>(0.211)</td>
<td>(0.201)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log share of 60+ years old</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$&lt; 0.4$</td>
<td>0.514$^{*}$</td>
<td>0.465$^*$</td>
<td>0.567$^*$</td>
<td>0.590$^{**}$</td>
<td></td>
</tr>
<tr>
<td>(0.306)</td>
<td>(0.251)</td>
<td>(0.290)</td>
<td>(0.293)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$&gt; 2.0$</td>
<td>0.502$^{*}$</td>
<td>0.395$^{**}$</td>
<td>0.454$^{*}$</td>
<td>0.411$^*$</td>
<td></td>
</tr>
<tr>
<td>(0.260)</td>
<td>(0.181)</td>
<td>(0.223)</td>
<td>(0.214)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log share of single-person households</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$&lt; 0.4$</td>
<td>0.204$^{**}$</td>
<td>0.209$^{**}$</td>
<td>0.288$^{*}$</td>
<td>0.327$^{**}$</td>
<td></td>
</tr>
<tr>
<td>(0.195)</td>
<td>(0.101)</td>
<td>(0.140)</td>
<td>(0.155)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$&gt; 2.0$</td>
<td>0.165$^{**}$</td>
<td>0.159$^{**}$</td>
<td>0.230$^{**}$</td>
<td>0.236$^{**}$</td>
<td></td>
</tr>
<tr>
<td>(0.077)</td>
<td>(0.070)</td>
<td>(0.105)</td>
<td>(0.109)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log share of couples with children</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$&lt; 0.4$</td>
<td>-0.323$^{*}$</td>
<td>-0.286$^{*}$</td>
<td>-0.331$^{**}$</td>
<td>-0.341$^{*}$</td>
<td></td>
</tr>
<tr>
<td>(0.176)</td>
<td>(0.150)</td>
<td>(0.167)</td>
<td>(0.166)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$&gt; 2.0$</td>
<td>-0.288$^{**}$</td>
<td>-0.248$^{**}$</td>
<td>-0.270$^{**}$</td>
<td>-0.250$^{**}$</td>
<td></td>
</tr>
<tr>
<td>(0.143)</td>
<td>(0.105)</td>
<td>(0.124)</td>
<td>(0.118)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Year dummies</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Neighborhood fixed effects</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of observations</td>
<td>2182</td>
<td>3226</td>
<td>4285</td>
<td>5264</td>
<td></td>
</tr>
<tr>
<td>Number of neighborhood</td>
<td>151</td>
<td>211</td>
<td>280</td>
<td>338</td>
<td></td>
</tr>
</tbody>
</table>

This table presents the relationship between changes in the distance to the nearest cannabis retailer and in the log share of certain demographic groups in the neighborhood. Results for other groups are presented in Table A7 of the Online Appendix. A neighborhood is defined by its four-digit postcode. The distance is represented by two dummies: less than 0.4 miles, and more than 2 miles; the reference is the group of houses between 0.4 and 2 miles. All regressions include year dummies and neighborhood fixed effects, and the dummies for distance are instrumented by the closures caused by the distance-to-school criterion and the zero-tolerance policy. The columns show the results from different samples, based on their initial distance to a retailer. The sample excludes neighborhoods in which the nearest retailer was closed for another reason. Clustered robust standard errors are in parentheses. $^*,$ $^{**},$ $^{***}$ represent statistical significance at the 1%, 5%, and 10% levels, respectively.

As regards the demographics, adults under 40 and couples with children are the least willing to live either near or far away from shops. In contrast, elderly and single people are the most willing to live in these areas. Despite these differences, these results indicate that the same demographic groups explain the results in Table 3.

7. Additional results

In this section, we present additional results to help us understand the ambivalent effect of closure policies on house prices. First, we show that these policies affect the whole distribution of prices, yet the effect is higher in the lower tail. Then we verify whether our main results are explained by changes in housing supply or other amenities, such as bars and schools. Finally, we investigate what also happened in terms of employment and crime rates after closures.

7.1. Estimated effects on the distribution of prices

From the previous results, one can make inferences on the average capitalization effect of closing retailers (Kuminoff and Pope, 2014). This average should lie between former residents’ and future residents’ willingness to pay for coffeeshops (after closures), as long as the comparison group is not affected by those policies (Banzhaf, 2018). To shed some light on the distribution of tastes, we also estimate the effect on conditional quantiles of house prices, which reveals the house’s risk of under- and overvaluation. Details are in Section 11 of the Online Appendix.

Fig. 4 presents the estimated quantile effects of closures. The left-hand graph shows the effect caused by the DTSC, for homes between 0.2 and 0.4 miles. The right-hand graph shows the effect of a ZTP, for homes within 0.3 miles. These distance ranges are defined based on the results from Table 2. In both cases, the effect is higher in the lower tail.

The DTSC is found to shift the entire distribution of prices, raising all quantiles. Overall, those households agree that being 0.13 miles more distant is beneficial. Besides, the higher effect at the lower tail means that a house is less likely to be underpriced. That is, the DTSC made it easier to find a buyer. The effect of the ZTP has the opposite pattern. Households living within 0.3 miles agree that the loss of coffeeshops is detrimental. Moreover, those houses are more likely to be underpriced — i.e., it became harder to find a buyer.

To complement this analysis, we also estimate the effects on the number of days a house stays on the market. Table A5 of the Online Appendix displays these estimates. Indeed, they confirm that after the DTSC, houses are sold more quickly. The ZTP presents similar effects, but they are not found to be significant.
Fig. 4. Quantile Effects of Closings on House Prices. This figure presents the quantile regression coefficients on the residual of a hedonic model, which includes house characteristics, as in Table A1, regional GDP, year dummies, year dummies times a dummy for municipalities that implemented the distance-to-school criterion (DTSC), and six-digit postcode fixed effects. See equation (K.1) in the Online Appendix. In the left-hand graph, the sample includes properties located between 0.2 and 0.4 miles from the nearest retailer. In the right-hand graph, the sample includes properties located within 0.3 miles of the nearest retailer. Both samples exclude houses in which the nearest retailer was closed for another reason. The graphs come from separate quantile regressions, which have a simple difference-in-differences specification. The grey area represents the 95% confidence interval of the coefficients.

Table 5
Estimated Effect of Closing Policies on Housing Stock.

<table>
<thead>
<tr>
<th>Initial distance to retailer (miles)</th>
<th>&lt; 0.3</th>
<th>&lt; 0.4</th>
<th>&lt; 0.6</th>
<th>&lt; 0.8</th>
<th>&lt; 1.0</th>
<th>&lt; 3.0</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distance-to-school</td>
<td>−0.011</td>
<td>−0.016</td>
<td>−0.012</td>
<td>−0.012</td>
<td>−0.011</td>
<td>−0.001</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.014)</td>
<td>(0.012)</td>
<td>(0.012)</td>
<td>(0.011)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Number of observations</td>
<td>1547</td>
<td>1781</td>
<td>2332</td>
<td>2813</td>
<td>3313</td>
<td>5438</td>
</tr>
<tr>
<td>Number of cells</td>
<td>119</td>
<td>137</td>
<td>181</td>
<td>218</td>
<td>258</td>
<td>424</td>
</tr>
<tr>
<td>Zero-tolerance</td>
<td>−0.007</td>
<td>0.001</td>
<td>0.001</td>
<td>−0.001</td>
<td>0.000</td>
<td>−0.006</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.011)</td>
<td>(0.010)</td>
<td>(0.008)</td>
<td>(0.008)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Number of cells</td>
<td>15,525</td>
<td>20,065</td>
<td>31,459</td>
<td>43,711</td>
<td>56,720</td>
<td>211,362</td>
</tr>
<tr>
<td>Number of cells</td>
<td>1196</td>
<td>1546</td>
<td>2426</td>
<td>3373</td>
<td>4377</td>
<td>16,383</td>
</tr>
<tr>
<td>Year dummies</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Cell fixed effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

This table presents the difference-in-differences (DiD) estimates, as in Eq. (2), for the log number of homes in a 500m-by-500m cell. Two separate DiDs are run using Poisson regressions: one for closures forced by the distance-to-school criterion, and another for closures forced by the zero-tolerance policy. Each column uses a different sample, defined by the initial distance of the cell to a retailer. All regressions include year dummies and cell fixed effects. The sample excludes cells in which the nearest retailer was closed for another reason. The top panel includes cells only in Rotterdam and Amersfoort. Clustered robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

7.2. Housing supply and other amenities

Other than the effect of the closure policies on housing demand, our main findings could also be explained by changes in housing supply and other amenities. Regarding the housing supply, Table 5 shows that the number of houses near cannabis shops is not significantly affected. Given the inelastic supply, our results on house prices should be interpreted as shifts in housing demand.

As regards the DTSC, the concern is that the relationship between house prices and distance to schools has changed over time. Table A8 of the Online Appendix shows the estimated effect controlling for this time-varying relationship. Indeed, the demand for houses closer to primary schools has increased in Rotterdam and Amersfoort. Nevertheless, the estimated effect of the closure policy remains nearly the same after controlling for this process.

Another concern is whether our results were explained by other businesses that tend to cluster around coffee shops, such as bars and nightclubs. Table A9 of the Appendix confirms that the ZTP slightly increased the distance of homes to these establishments. This evidence suggests that bars seem to cluster around cannabis retailers. Notwithstanding, the estimated effects on house prices do not change after we control for the time-varying distance to bars.
Table 6

<table>
<thead>
<tr>
<th></th>
<th>jobs/inhabitant</th>
<th>unemployment</th>
<th>soft drug offenses</th>
<th>hard drug offenses</th>
<th>violence</th>
<th>property crimes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Distance-to-school</td>
<td></td>
<td></td>
<td>−0.659</td>
<td>0.017</td>
<td>0.048</td>
<td>−0.238***</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.459)</td>
<td>(0.208)</td>
<td>(0.073)</td>
<td>(0.059)</td>
</tr>
<tr>
<td>Zero-tolerance</td>
<td>−0.039***</td>
<td>0.001</td>
<td>−0.054</td>
<td>0.238***</td>
<td>−0.097</td>
<td>−0.125**</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.001)</td>
<td>(0.080)</td>
<td>(0.069)</td>
<td>(0.140)</td>
<td>(0.063)</td>
</tr>
<tr>
<td>Regional GDP</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Municipal jobs and unemployment</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year dummies × province</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year dummies × Rotterdam</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Municipality fixed effects</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Neighborhood fixed effects</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>No. of observations</td>
<td>730</td>
<td>876</td>
<td>1090</td>
<td>1150</td>
<td>1150</td>
<td>1150</td>
</tr>
<tr>
<td>No. of municipalities/neighborhoods</td>
<td>73</td>
<td>73</td>
<td>109</td>
<td>115</td>
<td>115</td>
<td>115</td>
</tr>
<tr>
<td>with distance-to-school</td>
<td>0</td>
<td>0</td>
<td>32</td>
<td>32</td>
<td>32</td>
<td>32</td>
</tr>
<tr>
<td>with zero-tolerance</td>
<td>3</td>
<td>3</td>
<td>4</td>
<td>4</td>
<td>4</td>
<td>4</td>
</tr>
<tr>
<td>comparison</td>
<td>69</td>
<td>69</td>
<td>80</td>
<td>80</td>
<td>80</td>
<td>80</td>
</tr>
</tbody>
</table>

This table presents the difference-in-differences (DiD) estimates for labor outcomes and crime. Each regression contains two DiDs: one for closures forced by the distance-to-school criterion, and another for closures forced by the zero-tolerance policy. The first two columns show linear regressions using a sample of municipalities. The last four columns show Poisson regressions using a sample that combines municipalities and neighborhoods of Rotterdam. The samples exclude municipalities that have never had cannabis retailers or in which retailers were closed for another reason. Clustered robust standard errors are in parentheses. ***, **, * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

7.3. Change in employment and crimes

To understand the changes in housing demand, we finally look at other objective outcomes related to employment and nuisances. Table 6 presents the average DiD estimates for these outcomes. Fig. 5 shows the estimates per year before and after the closures. We use this figure to verify the timing of the effects and whether previous trends explain our results.

The effects on employment outcomes are at the municipal level, so we only report the effects of the ZTP. The effect on crime rates, however, is estimated at the neighborhood level in Rotterdam. Thus, at least for Rotterdam, we compare areas where the DTSC closed shops with areas that still have coffeeshops. For other municipalities, crime rates are at the municipal level.

7.3.1. Estimated effects on employment

In terms of labor market outcomes, the ZTP had no effect on the unemployment rate. This null evidence is consistent with other studies that investigate the relationship between access to cannabis and labor outcomes (e.g., van Ours, 2006; Sabia and Nguyen, 2018).16

Although the employment of municipal residents does not change, the number of workers employed in the city gradually declines after the ban — see Fig. 5. Banning cannabis retailers from the jurisdiction reduces by 4 percentage points (or 9%) the number of jobs per inhabitant. One explanation for this economic impact is that tight quantity constraints on any business should make the economy less efficient (Buchanan and Tullock, 1975; Gruber, 2011). However, we cannot verify whether the economic loss is either the cause or the consequence (or both) of a depreciated housing market.

7.3.2. Estimated effects on drug-related crimes

Regarding drug consumption, we find that both policies reduced the number of crimes related to soft drugs. This effect, though, is not persistent and becomes insignificant four years later. On the other hand, the ZTP has another side effect. After closing the last cannabis outlet, crimes related to hard drugs soared by 24%. This effect is also increasing over time, reaching over 50% four years after introduction of the policy. The result suggests that legal access to soft drugs is not a gateway, but a substitute for hard drugs (Model, 1993; van Ours, 2003; Chu, 2015).17 Once again, we cannot tell whether the depreciated housing market and the declining number of jobs either cause or result from the rise in illicit drug offenses.

7.3.3. Estimated effects on violence and property crimes

Neither policy significantly affects violence, but both are found to reduce the number of thefts by at least 12%. Unlike the evidence from Chang and Jacobson (2017), Hunt et al. (2018), and Brinkman and Mok-Lamme (2019), this result contradicts the "eyes on the street" role of cannabis retailers. Both studies find that marijuana dispensers reduced property crimes in

16 DiStefano (2002) is one of a few who shows that more access leads to a lower employment rate. See also van Ours and Williams (2015) for a comprehensive review.

17 DiNardo and Lemieux (2001) and Conlin et al. (2005) present a similar pattern regarding alcohol and illicit drugs, and Bradford and Bradford (2018) present evidence for the substitution between cannabis and prescription drugs. In contrast, Melberg et al. (2010) and Kelly and Rasul (2014) present evidence for the gateway effect.
Fig. 5. Estimated Differences in Employment and Crimes around the Year of Closure. This figure presents the lead and lagged difference-in-differences (DiD) on labor outcomes and crime rates. Panel (a) shows the changes before and after cannabis retailers are closed by the distance-to-school criterion (DTSC). Panel (b) shows the changes before and after the zero-tolerance policy. The labor outcomes (the first two graphs in each panel) are estimated by a linear regression using a sample of municipalities. Crime rates are estimated by a Poisson regression using a sample that combines municipalities and neighborhoods of Rotterdam. The samples exclude municipalities that have never had cannabis retailers or in which retailers were closed for another reason. Robust standard errors are clustered by municipality. Vertical lines around the point estimates represent 90% and 95% confidence intervals, respectively.
California and Colorado by increasing foot traffic in low-density areas. In our case, retailers were already located in high-density areas, so their presence had the opposite effect. Table A10 of the Online Appendix shows the types of property crimes affected by those policies. In both cases, the effect is driven by major property crimes, such as burglary and motor vehicle theft. Thus, rather than being the eyes on the street, cannabis outlets seem to create a hot spot for thieves, which justifies, among other reasons, households’ aversion to areas with a coffeeshop.

Another explanation is that with fewer shops, the rate of petty crimes might decline. Then, law enforcement could allocate more resources to prevent thefts. However, we do not find that coffeeshops are significantly related to minor thefts or nuisances. See Table A10 of the Online Appendix.

8. Conclusion

After 40 years of a pragmatic tolerance towards cannabis sales and consumption, households in the Netherlands may feel that this progressive policy has a cost. Accordingly, in the last two decades, local authorities have gained more power to curb this industry, and the number of cannabis retailers has halved. In this study, we assess the capitalization effect of two types of policy introduced in this period: the distance-to-school criterion, which restricts to location of cannabis shops in the city, and the ban on cannabis shops. These policies allow us to identify two kinds of effect. The first is the individual aversion of local households to those businesses. The second is the aggregate effect of allowing them in the city.

Our results suggest that households prefer to stay away from the trade areas. This aversion is justified by, among other issues, the concentration of property crimes and cannabis consumption around the outlets. On the other hand, the decriminalization of cannabis retailers has arguably had positive effects on law enforcement, public health, and the economy. In particular, we evidence that the presence of these retailers in the city reduces the incidence of crimes related to hard drugs, and raises both the number of jobs and housing demand.

This conflict between individual aversion and social welfare has implications for gentrification policies. If the goal is to regenerate depreciated areas, cannabis outlets may become an easy target, given their legal vulnerability and households’ aversion. At the limit, local authorities that aim to appreciate property values may decide to ban retailers altogether. The consequence, however, would be an overall depreciation.

Declaration of Competing Interest

The authors have no financial and personal relationships with other people or organizations that could inappropriately influence (bias) their work.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2022.05.005.

References


