UNIVERSITY OF AMSTERDAM

UvA-DARE (Digital Academic Repository)

A World Divided: Refugee Centers, House Prices and Household Preferences

Dröes, M.I.; Koster, H.R.A.

DOI
10.1093/jeg/lbac005

Publication date
2023

Document Version
Final published version

Published in
Journal of Economic Geography

License
CC BY-NC

Link to publication

Citation for published version (APA):
Dröes, M. I., & Koster, H. R. A. (2023). A World Divided: Refugee Centers, House Prices and Household Preferences. *Journal of Economic Geography*, *23*(1), 51-90. https://doi.org/10.1093/jeg/lbac005

General rights
It is not permitted to download or to forward/distribute the text or part of it without the consent of the author(s) and/or copyright holder(s), other than for strictly personal, individual use, unless the work is under an open content license (like Creative Commons).

Disclaimer/Complaints regulations
If you believe that digital publication of certain material infringes any of your rights or (privacy) interests, please let the Library know, stating your reasons. In case of a legitimate complaint, the Library will make the material inaccessible and/or remove it from the website. Please Ask the Library: https://uba.uva.nl/en/contact, or a letter to: Library of the University of Amsterdam, Secretariat, Singel 425, 1012 WP Amsterdam, The Netherlands. You will be contacted as soon as possible.

UvA-DARE is a service provided by the library of the University of Amsterdam (https://dare.uva.nl)

Download date:31 Aug 2023

Journal of Economic Geography (2023) **23**, 51–90
Advance Access Publication 26 April 2022
https://doi.org/10.1093/jeg/lbac005

A world divided: refugee centers, house prices and household preferences

Martijn I. Dröes *,**,‡ and *Hans R.A. Koster* ***,****,†

*Faculty of Economics and Business, Amsterdam Business School, University of Amsterdam, Plantage Muidergracht 12, 1018 TV Amsterdam, The Netherlands
**Amsterdam School of Real Estate (ASRE), Jollemanhof 5, 1019 GW Amsterdam, The Netherlands
***Department of Spatial Economics, Vrije Universiteit Amsterdam, De Boelelaan 1105, 1081 HV Amsterdam, The Netherlands
****Tinbergen Institute, Gustav Mahlerplein 117, 1082 MS Amsterdam, The Netherlands
†Centre for Economic Policy Research, 33 Great Sutton St, London EC1V 0DX, UK
‡Correspondence to: *m.i.droes@uva.nl*

Abstract

Using detailed housing transactions data from the Netherlands covering more than two decades, we examine the disamenity effect associated with the opening of refugee centers (RCs). We show that the opening of an RC decreases local house prices by 5.8%. The effect has become stronger in the past decade, in line with an increasing share of nationalist votes. Using micro-data on home buyers' characteristics, we further identify households' preferences. The results show that the willingness to pay is more negative for larger RCs and more positive for foreign-born individuals. The latter is indicative of a more positive attitude of foreign-born individuals toward refugees.

Keywords: House prices, refugee centers, attitudes, externalities
JEL classifications: R31, O18, D12
Date submitted: 15 December 2020 **Editorial decision:** 23 February 2022

1. Introduction

According to the United Nations Refugee Agency *UNHCR* there are currently a record number of 70.8 million forcibly displaced persons around the globe, of which 25.9 million are refugees (UNHCR, 2019).[1] The European Union (EU), for example, faced an increase from about 300 thousand applications in 2012 to 1.3 million in 2015 due to the war in Syria. The influx of refugees to Europe has decreased since, due to the refugee deal between Turkey and the EU. However, almost three million refugees have fled Ukraine since the escalation of the conflict with Russia in February 2022, and many more are expected to flee to EU countries.

When refugees come to the EU they have to be accommodated to await the outcome of their asylum procedure. Some refugees stay in large camps at the point where they entered

1 In this paper, we use the term 'refugees' as referring to persons applying for asylum. Those can be (involuntary) refugees as well as other types of (economic) migrants. We will not make a distinction between refugees and asylum seekers because for many countries it is a priori unclear what type of asylum seeker they are dealing with. Both refugees and migrants are therefore typically accommodated in refugee centers for some time.

© The Author (2022). Published by Oxford University Press.
This is an Open Access article distributed under the terms of the Creative Commons Attribution-NonCommercial License (https://creativecommons.org/licenses/by-nc/4.0/), which permits non-commercial re-use, distribution, and reproduction in any medium, provided the original work is properly cited. For commercial re-use, please contact journals.permissions@oup.com

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

the EU (i.e. Poland, Greece, Italy and Spain), but at the same time, many refugees are placed in dedicated refugee centers (RCs) within EU member states.

The increasing refugee flows in the last decade go hand in hand with an increasing popularity of populist, anti-immigration, political parties that aim to limit the number of newcomers (Ivarsflaten, 2008). Hence, it is not too surprising to see that the opening of an RC, or even the plans to open one, can lead to substantial local opposition.[2] This has led to a sharp division in opinions whether and where new RCs are supposed to be opened.

The aim of this paper is to estimate the willingness to pay (WTP) of households to live close to an RC. We do so by using hedonic price techniques and detailed housing transactions data covering the whole of the Netherlands between 1990 and 2015.[3] We geographically match these data with the locations of opened, planned and closed RCs. Given the considerable amount of anecdotal evidence on opposition against the opening of RCs, we would expect the average WTP to be negative. That is, an RC is most likely perceived as a disamenity.

The main contribution of this paper is that we are among the first to examine the variation in preferences of the local population living near RCs. Hedonic studies typically focus on average treatment effects. Yet, the disamenity effect captures both *negative external effects* (e.g. due to more traffic, noise pollution and increased crime levels), as well as *attitudes* of incumbent households toward refugees. We would expect that the attitudes toward refugees are, on average, negative as households generally prefer to live near households of their own type (Schelling, 1969). We expect to find considerable heterogeneity in the WTP for RCs among households, related to for instance the country of origin of the incumbent households as well as their political preferences. Consequently, we match the housing transaction data to micro-data on income, household composition and, importantly, whether the person is foreign-born. We further gather information on local election outcomes of the Dutch national elections and examine survey data that includes several measures of subjective well-being (e.g. capturing neighborhood nuisance, willingness to move and fear of crime).

There are two main identification challenges when aiming to measure the causal effect of RCs on house prices. First, RCs are not randomly allocated across space and time. To address this issue, we use a difference-in-differences (DID) methodology in which house price changes within a short distance of a realized RC are compared with areas where RCs were planned to be opened after 2015 but were canceled. The idea is that these areas were selected based on the same unobservable traits. Yet, RCs may not be randomly canceled as this may for example be the result of protest or prohibitively high land values. As a consequence, we also estimate the treatment effect by only using the variation in the opening date of RCs. That is, we show results that are *conditional* on the treatment areas. In a standard DID model, this would not be possible as there is no variation in the timing of treatment.[4] Our approach avoids that we have to make a strong parallel trends

2 There have been many large-scale demonstrations in places where RCs were planned (see e.g. Toonen, 2015; Volkskrant, 2016; DeStem, 2017; De Stentor, 2017). In several cases these demonstrations led to conflicts between the police and protesters (see e.g. Algemeen, 2016; Bakker, 2016).

3 The Netherlands has a population density of 407.4 per km^2. This is almost as dense as the San Francisco Bay area. The overall surface area is also comparable.

4 Our approach does require that the timing of the openings of RCs is unrelated to unobserved differences in price trends between the treatment areas. Reassuringly, using an event-study approach, we will show there are no such trends in prices before opening of RCs.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

assumption, prevalent in a standard DID approach. Moreover, we use the existing road network to create (100 m wide) travel corridors between RCs and the nearest shopping area. The idea is supported by anecdotal evidence that exposure to refugees is concentrated inside these corridors as refugees often visit a local shopping center to buy products and for recreational purposes (Kuppens et al., 2017; Keukenkamp and Goudsmit, 2021). We utilize this additional information to estimate the treatment effect using a triple-differences approach.

The second identification challenge is that the average effect of RCs on house prices identifies the overall disamenity effect of RCs, but gives little insight into the distribution of attitudes toward refugees and does not necessarily correspond to the underlying household-specific WTP. Rather than just presenting reduced-form estimates, our paper contributes to the existing literature by applying a structural two-step non-parametric hedonic pricing method in the spirit of Ekeland et al. (2004) and Bajari and Benkard (2005). We address the issue that the WTP for RCs is not point-identified (see Bajari and Kahn, 2005) and use instrumental variables to mitigate simultaneity problems in the second stage (see Ekeland et al., 2004).

Our preferred baseline estimate shows that the opening of an RC decreases house prices by 5.8% on average, which is economically sizable. The statistical evidence suggests that the effect is confined to 2 km. The effect is still there 10 years after the opening of an RC so the effect seems to be permanent. The triple-differences estimates further suggest that the effect is more pronounced in walking routes of refugees between RCs and shopping districts. These results are in line with the additional survey data. In particular, we show that the probability to move and experience dissatisfaction and nuisance increase when an RC is opened. By contrast, we do not find effects on fear of crime near RCs.

Further results indicate that there is considerable heterogeneity in the WTP of households. In particular, the effect is lower during the Yugoslavian civil war and higher toward the end of the sample period (i.e. during the Syrian war). In addition, we find that a standard deviation increase in the local share of nationalist votes in the previous Dutch national election is associated with an increased effect of RCs by 1 percentage point, which is sizable (about 17% of the baseline estimate).

Based on the non-parametric hedonic price approach, we find that the WTP distribution is left skewed. The mean WTP after the opening of a RC is –€16 thousand (about 7% of average house prices). Interestingly, for 16.5% of the people, we cannot reject the null hypothesis that the WTP is different from zero. Hence, this implies that not everyone has strong negative attitudes toward RC openings. Based on the idea that the size of an RC proxies for the negative external effect of an RC, we find that households are willing to pay about 9%, relative to the mean WTP for a standard deviation increase in the capacity of an RC. We further find that newly built RCs have a more pronounced negative external effect of about 14% of the mean WTP. Households' attitudes toward RCs—measured by household characteristics—also play a very important role. Among other things, we show that foreign-born persons and families have a more positive WTP of about 7% of the mean WTP.

Based on the heterogeneous WTP estimates, we perform a back-of-the-envelope calculation to infer the type and location where RCs should be placed. We find that the best strategy—despite households' preferences for small RCs—is to build large RCs in sparsely populated areas to minimize the total loss in housing values. If RCs have to be placed in denser areas, the effects can be mitigated by keeping them small, putting them in existing buildings and placing RCs in areas with higher shares of foreign-born people and families.

Our paper contributes to several strands of literature. In particular, this is not the first paper that examines the effect of RCs on house prices in the Netherlands. Using data from several Dutch provinces from 1997 to 1999, Theebe (2002) and Theebe and Eichholtz

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

(2003) find no effect of RCs on house prices. In a more recent contribution and using data from 2009 to 2017 and a spatial matching procedure, Daams et al. (2019) find that only prices of single-family homes in rural areas decrease by about 5% within 0.5–1.0 km of an RC. For RCs exceeding a capacity of 500 refugees, the effect doubles. Hence, there seems to be some discrepancy between the results by Theebe (2002), Theebe and Eichholtz (2003) and Daams et al. (2019).

We improve on these papers in several ways. First, our sample period encompasses both Theebe (2002), Theebe and Eichholtz (2003) and Daams et al. (2019). This allows us to show that the effect has changed over time. As mentioned, we find that votes for nationalist parties have increased over time and are associated with a more negative WTP for RCs. Second, our methodological approach improves upon Theebe (2002), Theebe and Eichholtz (2003), Daams et al. (2019) and other similar studies. Instead of using matching on observables and standard DID techniques, we identify the effect based on the variation in the opening dates of RCs and show robustness using a triple-differences approach. Third, many hedonic studies that examine externalities only focus on the average local treatment effect using reduced-form estimates. As mentioned, we also show results based on a *structural* hedonic approach to measure preferences. This allows us to show that the WTP varies considerably not only across areas, but also across households. We show that the potential effect of RCs is certainly not confined to households living in single-family homes in rural areas, even though many RCs are placed in such areas.

This paper further relates to a small but growing literature studying the impact of refugees on the housing market (Tumen, 2016; Akgündüz et al., 2018). A paper by Dustmann et al. (2018), for example, finds that the inflow of refugee immigrants in rural areas is associated with an increase in vote shares for nationalist parties, while in urban areas the effect is the opposite. This very much relates to our findings, where foreign-born households appear to be less opposed to RCs and predominately live in urban areas. Moreover, we find that RCs in urban areas have a smaller impact on house prices and we show that the effect of the opening of RCs depends on the local share of nationalist votes.

As a refugee is an involuntary migrant, our paper also belongs to the broader migration literature. As this literature is vast, we only mention a couple of examples related to housing markets here. In particular, Saiz (2007) and Gonzalez and Ortega (2013) show that an increased demand by immigrants for housing led to increases in rents and house prices throughout (local) housing markets in respectively the USA and Spain. Using data from Spain, Moraga et al. (2019) find that immigrant inflows to existing, dense, neighborhoods cause natives to move but also increase real estate development. Ottaviano and Peri (2006) argue that housing prices are higher in places with high inflows of immigrants. By contrast, Bakens et al. (2013) find the opposite for the Netherlands.

The remainder of this paper is structured as follows. In Section 2, we discuss the institutional context regarding RCs. Section 3 introduces the data, followed by reduced-form hedonic price regressions in Section 4. Section 5 reports the non-parametric regression results. Section 6 provides a conclusion and discussion.

2. Institutional context

2.1. The inflow of refugees

The inflow of refugees in Europe has varied considerably over time and there are a variety of underlying causes. To provide some background, Figure 1 depicts the number of

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

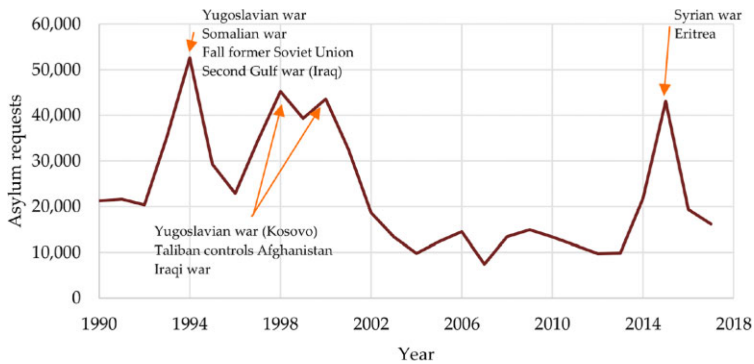

Figure 1. Inflow of refugees in the Netherlands.
Notes: This figure shows the number of asylum seekers (first) requests in the Netherlands and some of the underlying causes.
Source: Statistics Netherlands.

asylum seeker requests in the Netherlands from 1990 until 2017. The peak in requests in 1994 is mainly due to the Yugoslavian civil war, which started in 1992. About 25% of the refugees coming to the Netherlands in 1994 were from Yugoslavia. Other important categories were Somalians (10%), but also refugees from the former Soviet Union (9%) and Iraqi refugees due to the Second Gulf war (5%). The sudden influx of refugees in 1998 is due to another outburst in the Yugoslavian war (i.e. in Kosovo) and the Taliban taking over control in Afghanistan. Finally, in 2015, the Syrian war led to an unprecedented amount of refugees from one country (43% of the total) but there were also many Eritrean refugees (17%) fleeing because of political repression. According to these results, the inflow of refugees and, consequently, the need to increase capacity to house those refugees are a recurring event and will most likely remain a political and societal challenge in the future.[5]

2.2. Refugees and RCs

With the increase of the number of asylum seekers over time, the Dutch government decided to implement a new regulation in 1987 which led to the creation of dedicated RCs. Although the exact reasons why an RC is opened at a particular location is rather opaque and subject to negotiations with local municipalities, the government agency responsible for opening RCs, called *COA*, explicitly aims to distribute RCs evenly over the country and between different provinces.[6] Figure 2 depicts the spatial allocation of

5 The asylum application is evaluated by the *Immigration and Naturalization Service*. By law, a decision should be made within 6 months but it is known that the procedure can take years, for example due to the possibility to appeal the decision (NRC, 2018). In addition, as refugee centers accommodate anyone who applies for asylum, including economic migrants, it can take a long time to process asylum requests. If the application is successful the asylum seeker obtains a residence permit and municipalities (depending on their population) have to provide housing to the asylum seekers (for a detailed overview see, CBS, 2018).

6 Although the local population is informed about the potential opening of an RC, whether an RC will actually open is typically fairly uncertain, even when they are formally announced. Moreover, a municipality may receive funds from *COA* when opening an RC. The size of the funds can be substantial (up to a couple of hundred thousand euros). If such funds are invested in the local neighborhood, the (negative) estimates we will show later may be considered as being conservative.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

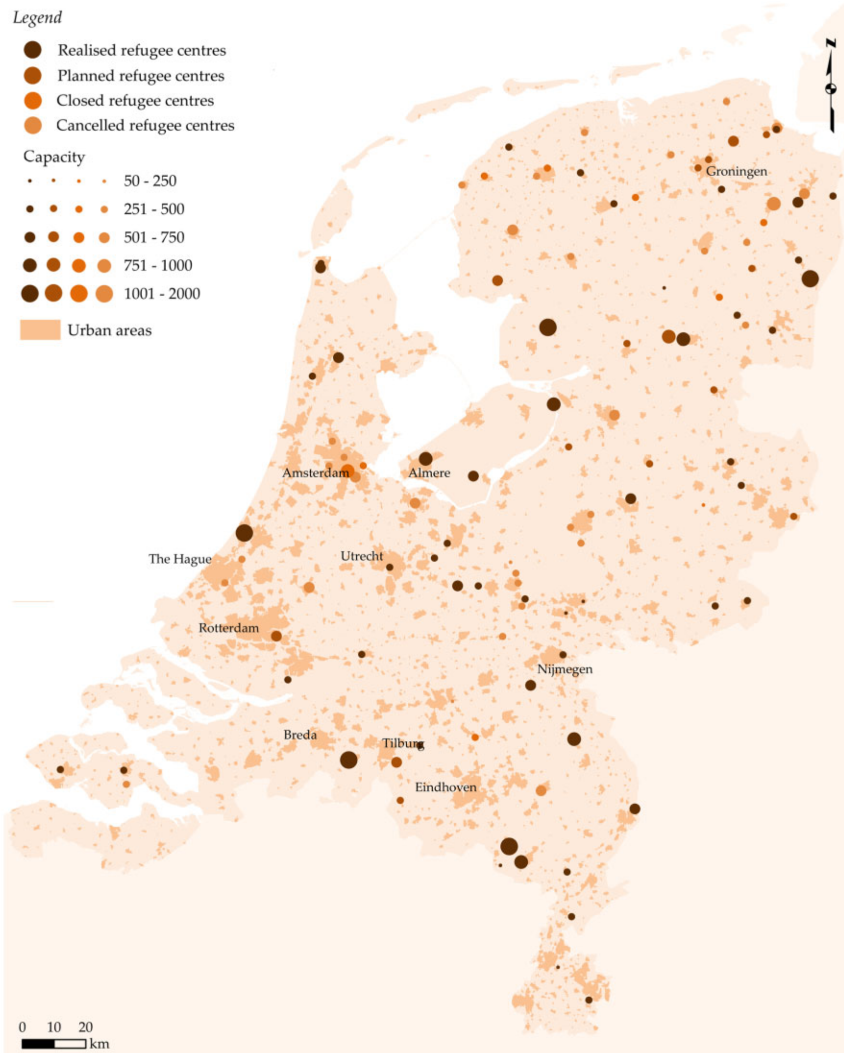

Figure 2. Spatial distribution of RCs.
Notes: This figure shows the location and size (in persons) of RCs in the Netherlands. The RCs are separated into four groups: Those that were realized before 2015 and still present in 2015, those that were planned to be opened after 2015 (in 2016–2018), those that were realized before 2015 and closed somewhere before this date and centers that we planned to be built after 2015 but were canceled. For the first three categories, we have the opening date which we use to measure the effect of RCs on house prices.

RCs. They seem to be randomly distributed across space. In Appendix A, we calculate Duranton and Overman's (2005) measure for spatial concentration, which confirms that RCs are indeed randomly distributed within the Netherlands. Yet, house prices and the types of houses sold near RCs are quite different, something that is particularly important for our research design. Appendix B.1 elaborates on these differences in more detail.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

3. Data and descriptive statistics

3.1. RC data

The RC data are obtained from the website of *COA*, www.coa.nl, and contains all realized permanent RCs that were still open in 2015. There are 51 such centers. Note that *COA* indicates for RCs that are opened whether they will be permanent or temporary, and we thus follow this classification. Based on online sources (e.g. news articles), we hand-collected 10 centers that were opened before 2015 but were closed before this date. We further added 48 RCs from www.nrc.nl that were planned to be opened between 2016 and 2018.[7] Most of them (33 RCs) did not open eventually, we refer to those as 'canceled' RCs. We will use housing transactions near those RCs as control/placebo group in one of the analyses later on. Transactions near the other RCs will belong to the treatment group. For all RCs we have the opening date (unless canceled), its capacity, its type (process accommodation, central accommodation and family accommodation), and we know whether the RC is realized in an existing or a new building.[8]

At this point, it is important to note that we will use the opening date of RCs in our sample to construct the treatment variable and measure the house price effects of RCs (see Section 4 for more details). Keeping this in mind, there are several potential issues with the RC data. First, our main dataset is largely based on the stock of RCs in 2015. However, some RCs have been opened and subsequently closed before this date, such that the RCs in 2015 are a subsample of the full population of RCs. If an RC closes because of large effects on house prices, not taking into account the treatment effect of eventually closed RCs would lead to an *underestimation* of the overall treatment effect. We will show that without closed RCs we indeed find a slight underestimate, although it is not statistically significantly lower.[9]

The second potential issue is that we do not include temporary RCs, which are typically only open for a year or two. As housing markets take time to adjust; and changes in amenities are more likely to capitalize into house prices when they are permanent, it makes sense to focus on permanent RCs. Of course, we also show some results related to semi-permanent (i.e. opened and closed) RCs, although these RCs were open for a couple of years, rather than just one or 2 years as is the case for temporary RCs. Also, temporary RCs are most likely not built at exactly the same place as permanent RCs, making it less likely that the local treatment effect we estimate is polluted.[10]

There is anecdotal evidence that exposure to refugees is concentrated in corridors between the RC and the nearest shopping center, as the refugees walk through these corridors to the shopping street to obtain clothes, food and other items (Kuppens et al., 2017;

7 Specifically, we obtain data on planned locations from https://www.nrc.nl/nieuws/2016/10/14/groot-deel-van-de-geplande-azcs-komt-er-niet-4828253-a1526722. We double checked these data and complemented it using various internet sources.

8 Three relatively small RCs specialize in the reception of refugee *families*, while three other locations are focusing on providing shelter for refugees that just entered the country. Due to privacy considerations, the data on country of origin of individual refugees are not publicly accessible. In addition, refugees cannot choose their own refugee center location.

9 A secondary issue is that the closed RCs may be a selective subsample of all closed RCs. If the ones we were able to find on the internet are those with a relatively large price effect, we would overestimate the treatment effect by including the closed RCs in our sample. We will thus examine robustness by estimating the treatment effect with and without closed RCs.

10 In Appendix B.2, we discuss the representativeness of the RC data related to this issue in more detail. Particularly after 2005, the permanent RC capacity in our sample seems to cover most of the stock of refugees.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

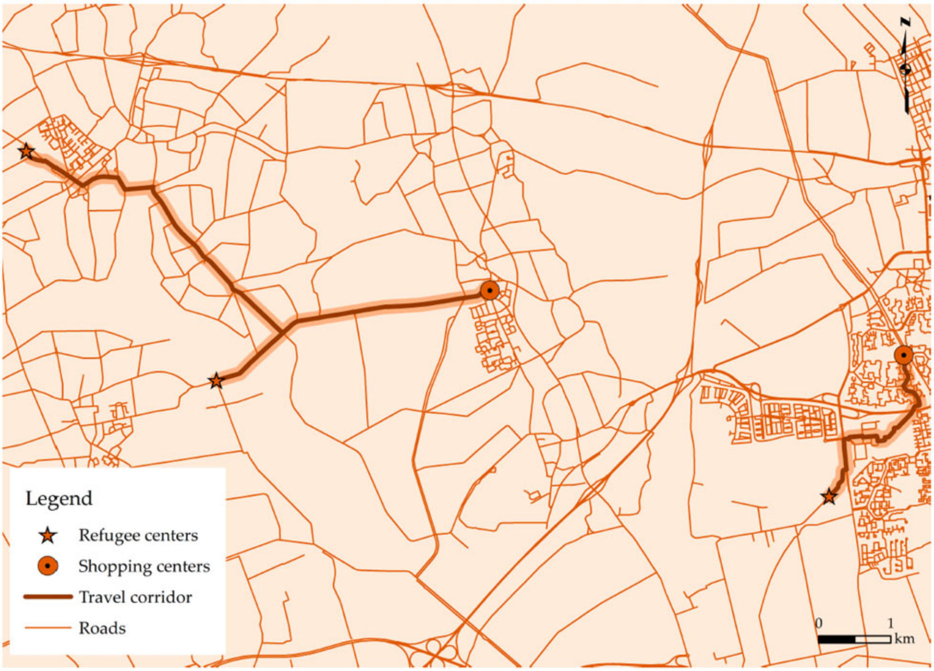

Figure 3. Corridors between RCs and nearest shopping districts.
Notes: This figure shows the 100-m wide corridors between RCs and the nearest shopping area with at least 25 shops. The existing road network in 2015 is used to create these corridors.

Keukenkamp and Goudsmit, 2021). We can use this as an additional source of variation to identify the effect of RCs on house prices. To implement this empirically, we create corridors (100 m wide) from the RC to the nearest shopping center (consisting of more than 25 shops), based on the existing road network in 2015.[11] Figure 3 shows an example of such corridors. The average length of a corridor is 1.9 km so most shopping centers are within reasonable walking distance of RCs. We will compare price developments within these corridors versus price developments outside those corridors but within near distance of an RC.

Finally, Table 1 shows descriptive statistics for the RCs. On average, the capacity is 532 persons. There is quite a bit of variation: the capacity varies from 100 to 2000. About one-third of the RCs are in newly constructed buildings. This share is somewhat higher for planned RCs (about 50%). For closed RCs, the average opening spell is 9 years. The number of RCs has increased gradually over time although toward the end of the sample period (Syrian war) there has been a marginally higher growth. We will split the sample into parts to examine whether the marginal effect of RCs has changed over time.

3.2. Housing transactions data and the Dutch housing market

There are about 8 million homes in the Netherlands. About 58% is owner-occupied and about 170 thousand houses are sold each year. The housing transactions data are taken

11 We obtain data on shopping locations from *Locatus*, see Koster et al. (2019) for more information.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table 1. Descriptive statistics: RC dataset

	Realized		Planned		Closed		Canceled	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
RC capacity	532.4	322.0	496.7	136.9	413.5	221.5	434.8	173.4
Year of opening RC	2005	10.35	2017	0.799	1996	6.620		
Year of closure RC					2005	3.843		
Construction year of the building	1973	31.90	1989	36.75	1952	41.90		
Newly built	0.314	0.469	0.533	0.516	0.100	0.316		

Notes: The number of observations is 51, 15, 10 and 33, respectively. This table shows the descriptive statistics across four different categories of RCs.

from the *Dutch Association of Realtors* (in Dutch: *NVM*) and are provided by *Brainbay*. It covers the period 1990–2015, and captures about 60–70% of the market. Toward the end of the sample period, the coverage is even 90% or more. In Appendix B.3, we discuss the representativeness of the *NVM* data in more detail. The main benefit of this dataset in comparison to other datasets is that besides sales prices the data contains information about list prices, time on market and an extensive set of housing characteristics. We also know the exact location of each property. The full dataset contains about 2.6 million transactions. We exclude a very small number of observations (<0.01%) for which we do not know the exact location or which were in the near vicinity (<0.35%) of two RCs so that the treatment effect cannot be determined properly.

Table 2 contains the descriptive statistics for the house price dataset. The average house price is €203,635 in the overall sample. Because we have the exact locations of the properties and RCs, we can calculate for each property the distance to the nearest RC. Because the spatial extent of the effect of RCs on house prices is a priori unclear, we will start by using a 2-km threshold to estimate the treatment effect, but will also test for other thresholds. About 7.4% of housing transactions (154,424 houses, 194,436 observations) are within 2 km of an eventually opened RC (i.e. the treatment group) and 2.8% of housing transactions are within 2 km *after* an RC has opened. The descriptive statistics for the different RC categories (i.e. opened, planned and closed) within the treatment group are reported in Appendix B.1. The descriptive statistics for the canceled RCs are also reported there. Finally, note that only about 0.01% of the observations are inside corridors after the opening of an RC within 2 km of a property.

3.3. Household level data

We use data from the *Sociaal Statistisch Bestand*, provided by *Statistics Netherlands*, which contain basic information on demographic characteristics, such as age, country of birth and gender. We only keep individuals that are a potential homeowner, so we keep people that are 25 years or older. We aggregate the data to the household level. Furthermore, we use information on household characteristics, such as household size, whether there are children in the household, as well as the marital status of the adults. We link these data to the *Integraal Huishoudens Inkomen* panel dataset to obtain information on households' disposable income. We matched this data with the housing transactions

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table 2. Descriptive statistics: house price dataset

	Mean	SD	Min	Max
Sales price (€)	203,635	114,670	25,000	1,000,000
List price (€)	216,377	124,549	22,916	1,400,000
Time on market (days)	135.1	185.7	0	1825
RC, <2 km	0.0736	0.261	0	1
RC opened, <2 km	0.0283	0.166	0	1
RC opened, <2 km and inside corridor	0.000956	0.0309	0	1
Within corridor to shopping area	0.0012	0.034	0	1
Size in m^2	117.0	37.58	26	250
Number of rooms	4.336	1.330	0	25
Terraced property	0.320	0.466	0	1
Semi-detached property	0.277	0.447	0	1
Detached property	0.121	0.326	0	1
Property has garage	0.324	0.468	0	1
Property has garden	0.973	0.161	0	1
Maintenance state is good	0.865	0.342	0	1
Property has central heating	0.894	0.308	0	1
Property is (part of) listed building	0.00607	0.0777	0	1

Notes: The number of observations is 2,640,378. The dataset also includes nine construction decade indicators which we will use in the regression analysis. Apartments are the reference category for the type of house dummies. The variable 'Refugee center opened' is one *after* an RC gets opened and captures the mean treatment variable. The variable 'Refugee center' is the treatment versus control group indicator. The corridors between RCs and shopping areas (>25 shops) are 100 m wide and based on the road network in 2015. The corridor indicator is one after an RC gets opened within 2 km of a property and that property is within the corridor. The sample period is 1990–2015.

data to have information on characteristics of the buyer. The household level data are only available as of the year 2000.

The descriptive statistics for the matched dataset, containing housing transactions and household characteristics, are reported in Table 3. We focus on observations within 2 km of an RC. The average house price is somewhat higher than in the full sample (about 12.5%) because the data are available as of the year 2000. The average yearly household disposable income is €35,847 (with a standard deviation of €23,642). About one-third of the households in our sample are single households and about 5% are foreign-born. We refer to foreign-born individuals as those that are born in a non-western country, implying that those are born outside of the EU.

3.4. Survey data: satisfaction, fear of crime and local employment

Finally, as a supplementary data source, we use individual information about neighborhood satisfaction, willingness to move, fear of crime and local employment based on several cross-sectional waves of the Dutch housing demand survey. Unfortunately, these data can only be matched to the RC data at the neighborhood level. That is, we neither know the exact location of the survey respondents, nor can we follow them over time. Although the survey results provide useful support for our hedonic findings, because of the data limitations, we report the survey data and results based on that data in Appendix D.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table 3. Descriptive statistics: combined housing and household data

	Mean	SD	Min	Max
Sales price (in €)	228,837	118,179	32,000	1,000,000
RC, <2 km	1	0	1	1
RC opened, <2 km	0.433	0.496	0	1
Capacity of nearest RC	493.6	248.4	75	1700
Age of head of the household	38.58	12.12	25	94
Share of household that is (non-western) foreign-born	0.0470	0.212	0	1
Disposable income	35,847	23,642	6019	1,000,000
Household size	2.174	1.154	1	11
Single household	0.335	0.472	0	1
Single parent with kids	0.0395	0.195	0	1
Couple	0.381	0.486	0	1
Couple with kids	0.244	0.430	0	1
Person is male	0.692	0.462	0	1
Size of the house (in m^2)	112.7	36.46	26	250
Number of rooms	4.243	1.363	0	14
Terraced property	0.312	0.463	0	1
Semi-detached property	0.250	0.433	0	1
Detached property	0.0839	0.277	0	1
Property has garage	0.282	0.450	0	1
Property has garden	0.984	0.127	0	1
Maintenance state is good	0.887	0.317	0	1
Property has central heating	0.931	0.254	0	1
Property is (part of) listed building	0.00938	0.0964	0	1

Notes: The number of observations is 57,728. Because of confidentiality restrictions, the minimum and maximum values refer to the 0.01% and 99.99% percentile. This implies that we exclude the bottom and top 62 observations. We only include observations within 2 km of a realized or planned RC. The dataset also includes six construction decade indicators which we will use in the regression analysis. The sample period is 2000–2015.

4. Reduced-form hedonic price analysis

4.1. Econometric framework

We first aim to estimate the treatment effect of RCs on house prices. Let P_{it} be the transaction price of property i sold in year t and $\mathcal{RC}_{it}$ be an indicator variable that equals one once an RC has opened within $\overline{d}$ km of the property. We aim to estimate:

$$\log P_{it} = \beta \mathcal{RC}_{it} + \delta X_{it} + \lambda_j + \lambda_t + \epsilon_{it}, \tag{1}$$

where X_{it} are a set of housing attributes (e.g. house size, construction year), λ_j are (six-digit) postcode fixed effects, λ_t are a set of year-fixed effects as well as a separate set of month-fixed effects, and ϵ_{it} is the error term. Because postcodes are small (about 15–20 addresses), this implies that we basically identify the effects of RCs using variation in house prices *over time*. Yet, we will also show robustness using a repeat sales methodology, which ‘differences out’ property-fixed effects. This comes at the cost of losing many observations and induces potential selection effects (i.e. cheaper houses sell more frequently). Finally, as treatment is at the property level, we use standard errors clustered at the neighborhood level, but our results are robust to using different levels of clustering (e.g. municipality and postcode area).

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

We initially start with $\overline{d} = 2$km. Using a threshold distance, rather than a continuous distance measure, to capture effects of spatial variables is a common strategy if the aim is to measure the average treatment effect (see, e.g. Theebe, 2002; Theebe and Eichholtz, 2003; Gibbons and Machin, 2005; Davis, 2011; Dröes and Koster, 2016; Daams et al., 2019). Because the choice of 2 km is arbitrary, (i) we will test whether the effect reaches beyond 2 km by adding additional dummy variables for greater distances, (ii) we will show robustness using shorter distances and (iii) show results using an alternative strategy based on travel corridors between RCs and shopping areas.

The parameter β captures the treatment effect on house prices. We estimate three different versions of Equation (1). First, we Estimate (1) using all available data, which then boils down to a standard DID specification.[12] The DID framework takes into account that RCs may potentially be opened in locations with lower house prices. The treatment effect β captures a relative price decrease in comparison to properties in the rest of the Netherlands as the result of opening an RC.

The main issue with a standard DID approach is that there may be unobserved reasons why an RC is opened in a particular area, for example in areas where prices are declining (i.e. there are potentially unobserved trends correlated to the treatment indicator). Therefore, in a second specification, as control group, we use observations within 2 km of RCs that are planned to be opened after 2015 (i.e. in 2016–2018) but were canceled. These areas should be comparable in terms of unobserved traits that are potentially correlated with the decision to open an RC.

Using canceled RCs as control group may be problematic when RCs are canceled non-randomly, for example, because of public opposition, less demand for RCs or lack of space.[13] Hence, we employ a third approach where we only use the variation in the opening dates of the eventually realized RCs to identify the treatment effect. In this way, the parallel trend assumption common in a standard DID framework (see Bertrand et al., 2004; Abadie, 2005; Donald and Lang, 2007) is much less restrictive—as the price trends, conditional on the opening of an RC, between properties near existing RCs and future RCs should be the same.[14]

This assumption is violated if the *timing* of the construction of RCs within eventually treated areas is non-random. In order to explore this potential issue further, we undertake an event study where we decompose the effect based on the years before and after the opening of an RC. More specifically, we expand Equation (1):

$$\log P_{it} = \sum_{\tau=-T}^{T} \beta_\tau RC_{it,\tau} + \delta X_{it} + \lambda_j + \lambda_t + \epsilon_{it}, \tag{2}$$

where $\tau = -T, \ldots, T$ are the years before and after treatment and $\tau = 0$ is the year of treatment. This results in a response function capturing the estimated coefficient for each year before and after opening of an RC and allows us to investigate whether there is a

12 Note that the treatment group dummy is absorbed by the postcode fixed effects λ_j and the before/after dummy by the year fixed effects λ_t.

13 Based on news sources we find that there was protest in relation to 24% of the eventually canceled RCs compared with 21% of all other RCs (including those that are planned).

14 This approach is equivalent to allow for differential effects between the treatment and control group of the control variables as well as the year-fixed effects. For example, when the control group is the rest of the Netherlands, or the canceled RCs. Only the number of observations would be higher (and the standard errors artificially lower).

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

transitory or permanent effect on house prices *and* whether pre-existing price trends are important.

To the extent one is still worried that unobservable trends may bias our results, we further consider a triple-differences identification strategy based on (100 m-wide) corridors from RCs to the nearest shopping area as refugees may walk through these corridors:

$$\log P_{it} = \beta_1 \mathcal{RC}_{it \in \mathcal{C}} + \beta_2 \mathcal{RC}_{it} + \delta X_{it} + \lambda_j + \lambda_t + \epsilon_{it}, \tag{3}$$

where $\mathcal{RC}_{it \in \mathcal{C}}$ now equals 1 if a transaction is within the corridor *and* within 2 km of a realized RC.[15] In the above equation, we measure the change in prices in corridors within 2 km of an opened RC *conditional* on being within 2 km of an RC. In the unlikely case that the effect within 2 km of an RC is partly capturing some local price trends, this is even less likely to be the case for the difference between the corridor and the other observations within 2 km of an RC.[16,17]

In the empirical analysis, we also consider additional robustness checks and extensions. For example, we test the impact on the mark-up (i.e. the difference between the sales prices and list price) and the time on the market, and test whether the effect of RCs on house prices is constant over time. We also add an interaction effect with the local share of right-wing votes to examine the impact of political preferences.

4.2. Baseline results

Table 4 reports the baseline results for the log-linear hedonic price function (see Equation (1)). In Column (1), we include all transactions. The opening of an RC has, on average, an effect on house prices of $e^{-0.0303} - 1 = -3.0\%$. This effect is statistically significant at the 1% level. In Column (2), we limit our sample to observations that are within 2 km of an RC that has been opened or will be opened, as well as observations that are within 2 km of a planned but canceled RC. The latter observations are used as control group. Using this specification, the effect is –5.1%.

There is some evidence that RCs may be canceled because of local protests (see e.g. RTL Nieuws, 2015). As a result, using a control group that includes canceled RCs might not be appropriate. In Column (3), we estimate what we consider to be the preferred specification by only including observations that are within 2 km of an RC that has been opened (i.e. the treatment group) or will be opened (i.e. the control group). The coefficient indicates that prices decrease by –5.8% once an RC is opened.

In Columns (4)–(9), we present the results based on the 100 m-wide corridors to the nearest shopping area, see Equation (3). First, we estimate the same specification as in Columns (1)–(3). Based on the full sample, we find a negative effect on house prices

15 The triple difference model should in principle also include the interaction effect between the corridor and treatment indicator. This is, however, collinear with the postcode fixed effects.

16 Note that because the treatment effect may extend beyond the corridor, we may underestimate the treatment effect using this approach. To mitigate this issue we will exclude observations between 100 m and 1 km from the road toward the shopping center.

17 An alternative approach would be to use synthetic control methods (SCMs). SCM is typically somewhat sensitive to assumptions on what variables to use to determine the pre-trends (for a discussion and application, see Acemoglu et al., 2016). Moreover, the timing of the treatment is typically implemented in the same year and treatment status is permanent (so RCs could not be closed) (Arkhangelsky et al., 2021). We therefore choose to not apply SCM in this context, but rather explain why we use different control groups and show robustness of our results for the choice of control groups.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table 4. Baseline regression results

Dependent variable: the log of house price

	Treatment <2 km			Corridor analysis			Triple-differences		
	(1) Full sample	(2) Planned and canceled	(3) Only RC locations	(4) Full sample	(5) Planned and canceled	(6) Only RC locations	(7) Full sample	(8) Planned and canceled	(9) Only RC locations
RC opened, <2 km	-0.0303^{***}	-0.0524^{***}	-0.0599^{***}				-0.0228^{**}	-0.0559^{***}	-0.0643^{***}
	(0.0077)	(0.0086)	(0.0089)				(0.0100)	(0.0101)	(0.0104)
RC opened (<2 km)				-0.0575^{***}	-0.0823^{***}	-0.0868^{***}	-0.0348^{**}	-0.0297^{*}	-0.0287^{*}
×within corridor				(0.0132)	(0.0132)	(0.0154)	(0.0161)	(0.0154)	(0.0175)
Housing characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Postcode-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year- and month-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,640,378	318,193	194,436	2,440,027	166,004	98,703	2,440,027	166,004	98,703
R^2	0.92	0.93	0.93	0.92	0.93	0.93	0.92	0.93	0.94

Notes: The estimates are based on the Dutch association of realtors data between 1990 and 2015. Our preferred specification in Column 3 only focuses on observations near RCs. For Columns (4)–(9), the corridors are between RCs and the nearest shopping center of at least 25 shops. For all corridor specifications, the treated observations are conditioned to be within 2 km of an RC (so houses in corridors beyond that distance are not considered to be treated). In addition, between 100 m and 1 km from the corridor is excluded from the sample. Standard errors are clustered at the neighborhood level and in parentheses.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

inside the corridor and within 2 km of an RC of –5.6% (see Column (4)). The specification in Column (5) is in line with the preferred baseline estimate, albeit somewhat stronger. We think this points toward the fact that the effects within the travel corridors may be stronger. In Column (6), where we just use the variation in the timing of RCs, the effect becomes –8.3% which is again somewhat stronger than our previously reported preferred estimate in Column (3).

Next, we estimate a triple-differences specification where we compare the price change in the corridors to price changes outside the corridor but still within 2 km of a recently opened RC. We report the results of this analysis in Columns (7)–(9). If opening of RCs is correlated with declining prices due to unobserved reasons, assuming that this decline also occurs in both the corridor and the rest of the treatment area, we can consistently estimate the treatment effect by taking the difference between both. If there actually is an effect within 2 km of an RC, we would underestimate the treatment effect. The results show that the effect is very much in line with what we found before. The effect inside the corridor within 2 km of an RC ranges from –2.8% to –3.4% and is statistically significant, albeit only at the 10% significance level in the last two specifications. It is very unlikely that just in the direction of shopping centers we pick up a spurious negative price trend, which strengthens our claim that the effect we find is causal. In addition, it points toward the importance of the externality effect: people do not seem to like when refugees walk through their neighborhood and it is in line with survey evidence in Appendix D.3 showing that people experience noise nuisance from RCs. If we look at the final estimate in Column (9), it suggests that the treatment effect within the travel corridors is about 50% stronger.

Overall, these results are in line with our findings on subjective well-being reported in Appendix D and based on satisfaction, fear of crime and employment data from several waves of the Dutch housing needs survey. We find that the opening of an RC increases the probability that households are dissatisfied with the neighborhood they live in; that those households are more likely to be willing to move within the next 2 years; and that they also experience more nuisance. The effects are economically sizable and support our main findings. Interestingly, households do not seem to experience an increased fear of crime and unemployment within the local neighborhood does not seem to be affected by the opening of an RC.

4.3. Further robustness checks and extensions

Table 5 reports several robustness checks and extensions based on our preferred regression model (see Table 4, Column (3)). In Column (1), we examine whether the effect on house prices is transitory or permanent by undertaking an event study (see Equation (2)). We report the effects up to 5 years before the opening of an RC and 10 years after (with 1 year before opening as reference category). The results are reported in Figure 4 and show that at the moment of placement there is a very clear discrete negative jump in prices of –3.8%, while the effect is statistically insignificant in the years preceding treatment, suggesting that price developments (i.e. pre-trends) before treatment were similar.[18]

18 If anything, the effect is slightly positive 2–4 years before opening of an RC. Excluding the (small) sample of refugee centers that were opened and subsequently closed during the sample period, this effect becomes smaller and even more statistically insignificant.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table 5. Robustness and extensions

Dependent variable: the log of house price

	(1) Event study	(2) Distance profile	(3) Within 1 km	(4) Within 750 m	(5) Opened RCs only	(6) Over time	(7) Political vote
RC opened, <2 km	See Figure 4	−0.0814***			−0.0520***		−0.0573***
		(0.0146)			(0.0142)		(0.0087)
RC opened, 2–5 km		−0.0487					
		(0.0350)					
RC opened, 5–10 km		0.0152					
		(0.0147)					
RC opened, <1 km			−0.0620***				
			(0.0138)				
RC opened, <750 m				−0.0552***			
				(0.0257)			
RC $\times D_{1990-1994}$						−0.0647**	
						(0.0265)	
RC $\times D_{1995-1999}$						−0.0218	
						(0.0171)	
RC $\times D_{2000-2004}$						−0.0287***	
						(0.0106)	
RC $\times D_{2005-2009}$						−0.0687***	
						(0.0135)	
RC $\times D_{2010-2015}$						−0.1096***	
						(0.0180)	
RC × (sharenationalist− Mediansharenationalist)							−0.0045***
							(0.0008)
Housing characteristics	Yes	No	Yes	Yes	Yes	Yes	Yes
Postcode-fixed effects	Yes	No	Yes	Yes	Yes	Yes	Yes
Year- and month-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	194,436	194,436	52,838	28,101	147,839	194,436	194,436
R^2	0.93	0.93	0.94	0.93	0.93	0.93	0.93

Notes: This table uses the data within 2 km of RCs (also see specification (3), Table 4). Standard errors are clustered at the neighborhood level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Hence, although we do not have detailed data on announcement dates, our results seem to indicate that there are no anticipation effects before the opening of an RC. Further results indicate that after a few years the effect seems to become larger (up to –9.6%, 10 years after opening of an RC). This might be indicative of second-order neighborhood composition effects, although the confidence bands are overlapping in many cases and become larger in the years after opening of an RC. Overall, the evidence points toward a permanent effect of RCs, as the effect is always statistically significantly negative after treatment (even after 10 years). In Appendix C.1, we show a very similar event study analysis using list prices.

Next, we investigate whether the treatment effect extends beyond 2 km. Because distance to the nearest RC varies over time, we can also identify those effects. The results in Column (2) seem to confirm our baseline estimate and show that the effect decays over distance. Within 2 km, the effect is –7.8% which is highly statistically significant. At 2–5 km, the effect is still negative but no longer statistically significant. The point estimate

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

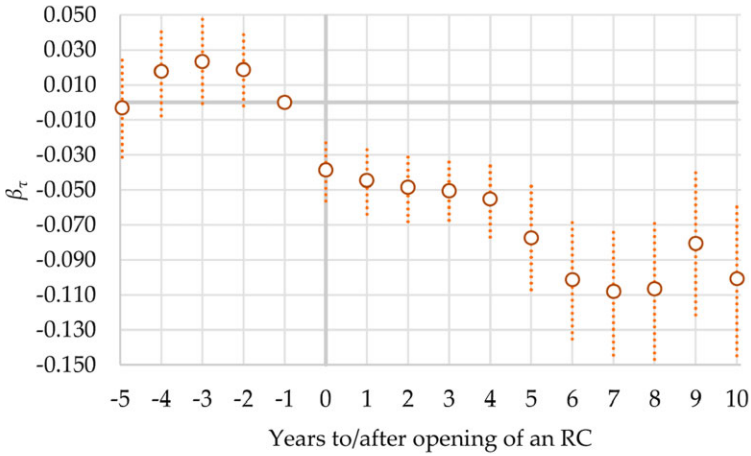

Figure 4. Event study.

Notes: We allow the effect of RCs to be dependent on the years to/after opening, see Equation (2). The event window ($\tau = -T$ to T) in our sample runs from –28 to 27 years. We report 5 years before, and until 10 years after, the opening of an RC. One year before treatment is the reference category. Table 5 reports the number of observations and R^2. Recall that only observations within 2 km of an (eventually) opened RC are included. In Figure C2, Appendix C.1, we extend the event study to 10 years before treatment.

is also considerably smaller than within 2 km. Between 5 and 10 km, we find a relatively small positive coefficient that is, however, also not statistically significantly different from zero.

In Columns (3) and (4), we show the average treatment effect at 1 km and 750 m, respectively. The effect within 1 km is –6.0%, which is statistically significant at the 1% level. Using a 750-m threshold, we still find a statistically significant effect of –5.4%, even though there are only 28 thousand observations left.[19] These results imply that even with this quality data it is difficult to measure the potential distance decay of the effect at a granular level. Overall, though, the sign and size of the average treatment effect seem to be relatively robust.

The specification in Column (5) is based on the sub-sample of RCs that are still open at the end of the sample period. That is, RCs that have been closed are excluded. The effect (–5.1%) is somewhat smaller compared with the baseline estimate (although not statistically significantly so), suggesting that RCs that are closed probably had a somewhat larger price effect. Although it would also be interesting to study the price effect of closings in more detail, the number of RC closings is too small to obtain precise and robust estimates.

More generally, to study whether there has been a shift in attitudes regarding refugees, we examine whether the treatment effect systematically varies over time. We interact the treatment dummy with 5-year period dummies. The results, reported in Column (6), show that particularly at the end of the 1990s (–2.2%) and the beginning of the 2000s (–2.8%),

19 Choosing even lower thresholds (e.g. 500 m) leads to imprecise results due to a lack of observations. Nevertheless, the estimated effect is still negative (–0.035 with a standard error of 0.0256, a p-value of 0.175, and based on 11 thousand observations) and in line with the results we present here.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

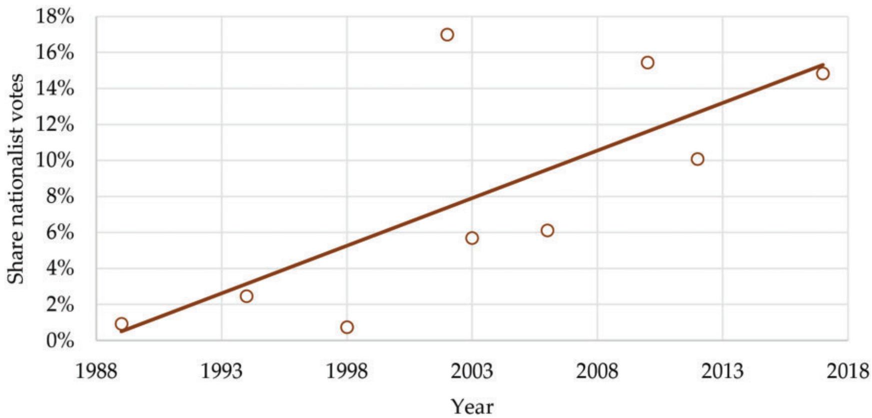

Figure 5. Share of nationalist votes.
Notes: This figure shows the average share across municipalities of nationalist votes per year. Nationalist votes include votes to several political parties: CD, LPF and PVV. We use the national Dutch elections of 1989, 1994, 1998, 2002, 2003, 2006, 2010 and 2012.

the effect was smaller than the baseline estimate. This was a period when the Yugoslavian war occurred on the European mainland, with the Netherlands being directly involved in the peacekeeping force. Apparently, the opening of RCs was less of an issue at that time. After that, the effect seems to become larger with the most recent period (2010–2015) showing effects of up to –10.4%. This seems to go hand-in-hand with the increased popularity of the more nationalist movements in the Netherlands (also at a European level) reflected in the rise of the nationalist political parties like Lijst Pim Fortuyn (LPF) (founded in 2002) and Partij Voor de Vrijheid (PVV; founded in 2005). It may also relate to the increasing media coverage of refugee issues and (criminal) incidents where refugees were involved.[20]

To examine in more detail the role of political preferences we include the interaction effect between openings of RCs and the municipal share of votes to nationalist, anti-migration, political parties in the Dutch national elections between 1989 and 2012. These data are publicly available per municipality from the electoral council. Figure 5 shows how the average share has increased over time.[21] Merging the political vote data to our transaction database, the local votes in national elections for nationalist parties in the merged dataset vary between 2.2% and 16%. The main effect is evaluated at the median share of nationalist votes in the elections data, which is 5.8%, and we use the share in the election *before* a specific RC is opened.

The results in Column (7), Table 5 show that the marginal effect of the opening of an RC for the median share of nationalist votes is 5.6%. For a standard deviation increase in

20 An example is the widely covered Keulen incident. During New Year's Eve in 2015 a considerable amount of women were sexually harassed by foreigners, some of which turned out to be refugees (WDR, 2016).

21 The share of nationalist votes comprises the votes of several parties (i.e. Centrum Democraten (CD), LPF, PVV) that are considered to be anti-migrant. Until 1998 the votes mainly went to the CD. In 2002, we observe many votes cast on the newly founded LPF, but, with the murder of their political leader Pim Fortuyn, the votes declined until PVV was founded in 2006. Overall though there seems to be an increasing trend in nationalist votes in national elections over the past decades.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

the share (which is 2.3 percentage points), the marginal effect is 1.0 percentage point higher. This result should be interpreted with caution, as a high share of nationalist votes might affect the probability that an RC is opened. In addition, the opening of an RC might affect subsequent voting behavior (Dustmann et al., 2018).[22]

In Appendix C.2, we examine several extensions based on interaction effects between RC characteristics and the RC dummy indicating the opening of an RC. The results suggest that the treatment effect is larger in rural areas and for larger RCs (>500 persons), which is in line with Daams et al. (2019). Also, the effect is higher when an RC opens in a new building, possibly because such a building is more visible or noticed in the urban landscape. The effect seems to be smaller for family RCs.[23]

Moreover, in Appendix C.3, we explore some further models and show that our estimates are robust to using a repeat sales model and a time-varying hedonic coefficient model. We also show results based on list prices, interaction with the number of refugees, and we show that there is a 16% increase in the time on market after the opening of an RC.

5. Non-parametric hedonic price analysis

5.1. Identification of preference parameters

A considerable literature on hedonic pricing focuses on recovering estimates for marginal changes in characteristics and estimates average effects for the population. We go beyond estimating the average marginal effect and aim to recover household-specific WTP estimates for RCs. Bajari and Benkard (2005) and Bajari and Kahn (2005) also consider identification of preferences in a hedonic price model in the presence of heterogeneity in households' preferences. They show that, given a *linear* utility function, housing preference parameters are identified. Instead, we follow Ekeland et al. (2004) and use a more general utility function that allows for interactions between housing attributes and household characteristics. We combine this approach with an insight of Bajari and Kahn (2005), who shows that housing preferences can be identified, even though the variable of interest is dichotomous.[24]

To investigate how the WTP for RCs relate to households' preferences, let us assume that the underlying utility function of household j occupying property i in year t is

$$U(\mathcal{RC}_{it}, X_{it}, Z_{jt}) = \alpha_{0j} + \alpha_{1j}\mathcal{RC}_{it}W_{it} + \alpha_{2j}\mathcal{RC}_{it}Z_{jt} + \alpha_{3j}\mathcal{RC}_{it}X_{it} + f(Z_{jt}) + g(X_{it}) + C_{jt}, \quad (4)$$

where α_{0j} is a constant; α_{1j}, α_{2j}, and α_{3j} are the preference parameters of interest; W_{it} are attributes of RCs (such as its size); Z_{jt} are household characteristics (such as age and

22 Yet, in terms of house prices we do not observe strong anticipation effects and we use the lagged share of political votes.

23 Using education data for 2015 from *EDM*, a marketing service provider, we also examined the effect in areas with a relatively high share (> 25%) of highly educated people (dummy variable and interaction effect). The treatment effect is –4.2% in areas with a high share and –8.3% in areas with a low share of highly educated people. This should be interpreted with caution though as education can be a proxy for other household characteristics such as income (which is typically highly correlated with education). In Section 5, we look at whether the WTP for RCs varies for households with different incomes.

24 Recent papers by Bishop and Timmins (2018) and Bishop and Timmins (2019) show alternative ways to estimate preferences. Bishop and Timmins (2018) use panel data on houses and individuals to estimate the demand for air quality. They observe households multiple times and use the variation over time, assuming that preferences do not change over time. Given that we only include households and transactions near RCs, the approach relying on repeated observations is not feasible. Alternatively, Bishop and Timmins (2019) use a maximum-likelihood approach to estimate preferences for violent crimes. This approach, however, is only applicable for continuously distributed housing attributes, while the placement of an RC is dichotomous.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

household composition); and C_{jt} measures other consumption. The functions $f(Z_{jt})$ and $g(X_{it})$ determine the level of utility based on household characteristics and housing attributes, respectively. As utility is assumed to be additively separable, these two functions do not play any role in defining the utility-maximizing outcome with regard to $\mathcal{RC}_{it}$. Let us further assume a budget constraint given by $I_{jt} = C_{jt} + P(\mathcal{RC}_{it}, X_{it})$, where I_{jt} is household income. To obtain the indirect utility function we then can replace C_{jt} in Equation (4) by $I_{jt} - P(\mathcal{RC}_{it}, X_{it})$, where

$$P_{ijt} = \gamma_{1j}(W_{it}, X_{it})\mathcal{RC}_{it} + \gamma_{2j}(W_{it}, X_{it})X_{it} + \lambda_j + \mu_t + \epsilon_{it}, \quad (5)$$

which is the hedonic price function.

Because $\mathcal{RC}_{it}$ is a dichotomous housing attribute, there is no first-order condition for utility maximization (see Bajari and Kahn, 2005). Recall that the implicit price to live near an RC is defined as γ_{1j}. Utility maximization then implies:

$$\begin{array}{l} [\mathcal{RC}_{it} = 1] \Rightarrow [\alpha_{1j}W_{it} + \alpha_{2j}Z_{jt} + \alpha_{3j}X_{it} \geq \gamma_{1j}], \\ [\mathcal{RC}_{it} = 0] \Rightarrow [\alpha_{1j}W_{it} + \alpha_{2j}Z_{jt} + \alpha_{3j}X_{it} \leq \gamma_{1j}]. \end{array} \quad (6)$$

Hence, if a household lives near an RC they are willing to pay at least γ_{1j}, while if a household does not live near an RC they are willing to pay maximally γ_{1j}. We will address this issue explicitly in the next subsection.

A further concern is the potential simultaneity issue when we allow the WTP for RCs to vary with house size, because the consumption of house size and whether a household lives nearby an RC are jointly determined. We investigate whether this is important, by using the approach outlined in Ekeland et al. (2004). They show that in additive non-parametric models, preferences and consumption can be identified. That is, Ekeland et al. (2004) propose to use $\mathrm{E}[X_{it}|Z_{jt}]$ and $\mathrm{E}[X_{it}^2|Z_{jt}]$ as instruments for X_{it}. This is a valid approach because the hedonic price model is generically non-linear, which provides us with the identifying variation to measure α_{3j}. In any case, we show that our results are robust, regardless of whether we address simultaneity of house size.

5.2. Non-parametric estimation

We follow a similar approach as Bishop and Timmins (2018). We start with conditioning out the postcode and time fixed effects:

$$\tilde{P}_{ijt} = \gamma_{1j}(W_{it}, X_{it})\tilde{\mathcal{RC}}_{it} + \gamma_{2j}(W_{it}, X_{it})\tilde{X}_{it} + \tilde{\epsilon}_{it}, \quad (7)$$

where $\sim$ denotes that these are variables for which the fixed effects have been partialled out. This implies that everyone is assumed to have the same preferences regarding the house and time fixed effects (as is the case in Bajari and Kahn, 2005, for the unobserved housing attribute).

We then use local linear regression techniques to estimate γ_{1j}:

$$(\hat{\gamma}_{1j}, \hat{\gamma}_{2j}) = \underset{\gamma_{1j}, \gamma_{2j}}{\arg\min} \sum_{\ell=1}^{J} \prod_{k=1}^{\mathcal{K}} K\left(\frac{V_{i\ell t}^k - V_{ijt}^k}{h}\right) \times (\tilde{P}_{i\ell t} - \gamma_{1j}\tilde{\mathcal{RC}}_{it|\ell} - \gamma_{2j}\tilde{X}_{it|\ell})^2, \quad (8)$$

where ℓ are (other) individuals, γ_{1j} are the parameters of interest, and $V_{i\ell t} = \left\{\frac{W_{it|\ell} - \overline{W}}{\sigma_W}, \frac{X_{it|\ell} - \overline{X}}{\sigma_X}\right\}$, where $k = 1, \ldots, \mathcal{K}$ are the number of variables to be included in the kernel function.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

We specify $K(\cdot)$ to be a Gaussian kernel function:

$$K\left(\frac{V_{i\ell t}^{k}-V_{ijt}^{k}}{h}\right)=\frac{1}{\sqrt{2h\pi}}\mathrm{e}^{-\left(\frac{V_{i\ell t}^{k}-V_{ijt}^{k}}{2h}\right)^{2}}. \tag{9}$$

Hence, $K(\cdot)$ determines the vector of weights for an individual j. The weight is maximized when an individual ℓ with identical observable characteristics as j lives in exactly the same house. The bandwidth h determines how 'smooth' the function to be estimated is. When $h \to \infty$, Equation (7) collapses to a standard linear hedonic price function. By contrast, if $h \to 0$ we estimate for each individual a separate (unweighted) regression, which would be impossible given that we typically would have only one observation per individual.

The question remains what is the 'right' bandwidth. The previously applied literature usually just picks a somewhat arbitrary value of around 3 (see Bajari and Kahn, 2005; Bishop and Timmins, 2018, 2019). Instead, we will use a 'leave-one-out' cross-validation procedure to determine h:

$$(\hat{h})=\arg\min_{h}\sum_{j=1}^{J}(\tilde{P}_{ijt}-\hat{\tilde{P}}_{ijt\neq j}(h))^{2}, \tag{10}$$

where $\hat{\tilde{P}}_{ijt\neq j}$ is the predicted price for j in a regression where j is excluded. We exclude predicted prices below the 1st percentile and above the 99th percentile to mitigate the issue that the outcome is affected by outliers.

However, as shown in Equation (6) we only identify lower bounds (when people do reside near an RC) and upper bounds (when individuals do not reside near an RC) because $\mathcal{RC}$ is a dummy variable. This implies

$$\underline{\gamma}_{1j}=\mathcal{RC}_{it}\hat{\gamma}_{1j}+(1-\mathcal{RC}_{it})\min_{j}(\hat{\gamma}_{1j}),\qquad \overline{\gamma}_{1j}=\mathcal{RC}_{it}\max_{j}(\hat{\gamma}_{1j})+(1-\mathcal{RC}_{it})\hat{\gamma}_{1j}. \tag{11}$$

Hence, we set the lower and upper bounds, respectively, to the minimum and maximum implicit price in the sample.

Taking these boundaries into account, to recover the utility parameters in Equation (4), we use the following maximum-likelihood function:

$$\begin{aligned}(\hat{\alpha}_0,\hat{\alpha}_1,\hat{\alpha}_2,\hat{\alpha}_3)=\arg\max_{\alpha_0,\alpha_1,\alpha_2,\alpha_3}\sum_{j=1}^{J}\log\Bigg(&\Phi\left(\frac{\overline{\gamma}_{1j}-\alpha_0-\alpha_1W_{it}-\alpha_2Z_{jt}-\alpha_3X_{it}}{\sigma}\right)\\&-\Phi\left(\frac{\underline{\gamma}_{1j}-\alpha_0-\alpha_1W_{it}-\alpha_2Z_{jt}-\alpha_3X_{it}}{\sigma}\right)\Bigg).\end{aligned} \tag{12}$$

where $\Phi(\cdot)$ is the standard cumulative normal distribution and we assume $\mu_{jt}\sim N(0,\sigma^2J)$.

Bajari and Kahn (2005) assume that the second stage error term is normally distributed, so that they can use a Probit model where the coefficient related to the implicit prices is normalized to minus one. Given that $\mu_{jt}\sim N(0,\sigma^2J)$, the Probit model will lead to consistent estimates of $\{\alpha_{1j},\alpha_{2j},\alpha_{3j}\}$, however, typically with rather large standard errors. By assuming explicitly defined upper and lower bounds, our estimates are more precise and can be estimated using interval regressions.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table 6. Linear and non-parametric models

Dependent variable: the log of house price		
	(1) Linear model	(2) Local linear model
RC opened, <2 km	−16,020***	−15,562***
	(5403)	(6998)
Housing characteristics	Yes	Yes
Postcode-fixed effects	Yes	Yes
Year- and month-fixed effects	Yes	Yes
Observations	62,475	62,475
R^2	0.9116	
Bandwidth	∞	2.728

Notes: We only include observations within 2 km of an RC. Standard errors (which are bootstrapped for the local linear model) are clustered at the neighborhood level and in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Finally, to address potential simultaneity of house size, we regress house size on individual characteristics. We then include a control function of the first stage residual in Equation (12) (Blundell and Powell, 2003; Yatchew, 2003). An important assumption of the control function approach is that endogenous house size must be continuously distributed, which is fulfilled in our application.

5.3. Results of non-parametric hedonic price models

In this section, we examine whether the WTP varies by RC attributes and household characteristics as to identify households' demand for RCs. We report the results of linear models for our sample (that includes households characteristics) in Table 6. When we rely on OLS, we find in Column (1) that the average WTP for RCs is €16,020. Given the average house price in the sample of €228,837, this means that the reduction in house prices is on average 7%. We note that this is similar, albeit slightly higher, than the results using logs (see Section 4.2).

In Column (2), we report the mean estimate of the non-parametric regression. To determine the 'smoothness' of the non-parametric hedonic price function, we use the cross-validation approach as outlined in Equation (10). The root mean-squared error is minimized when the bandwidth equals 2.728, which is close to values chosen in the literature (see Bajari and Kahn, 2005; Bishop and Timmins, 2019). We find an average WTP for RCs of –€15,562. A histogram of the estimated WTP parameters is reported in Figure 6. There is substantial heterogeneity in preferences to live nearby an RC. Almost all values are negative (99.6%). Yet, for 8.1% (16.5%) of the households we cannot reject the null hypothesis that the WTP is statistically significantly lower than zero at the 5% (10%) level. Hence, it seems that a reasonable share of the population is not too much concerned about the opening of RCs.

In Table 7, we report the second-stage results. Recall that we do not point-identify the WTP of households because our variable of interest is dichotomous. Hence, we estimate Equation (12) to be able to recover the utility parameters $\{\alpha_{1j}, \alpha_{2j}, \alpha_{3j}\}$. Although we

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

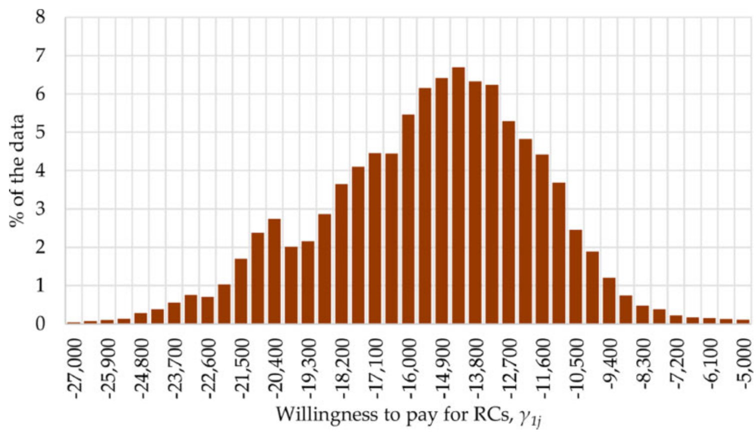

Figure 6. WTP for RCs.
Notes: We report here the WTP for RCs based on the estimates of Equation (7). We exclude the estimates below the 1st and above the 99th percentile to ensure that our results are not driven by outliers.

report several versions of the model, the results are largely robust so we focus the discussion on the results reported in Column (4). There we include RC, household and housing characteristics, and aim to address potential endogeneity issues.

We can distinguish between a negative externality effect—captured by RC characteristics (e.g. RC size, newly built)—and the differences in attitudes—captured by household characteristics. The negative externality effect is pronounced: a standard deviation increase in RC capacity (256 persons) decreases the WTP by €1382 (8.9% of the mean WTP). We further find that newly built RCs have a more pronounced negative external effect (13.7% of the mean WTP); although this result is only statistically significant at the 10% level.

Besides externalities, the overall preferences and attitudes as captured by household characteristics also play a very important role in determining the WTP. We find that foreign-born have a €1030 higher WTP, which is 6.6% of the mean WTP. The effect of a standard deviation change in income is smaller (only 1.9% of the mean WTP) and statistically insignificant. We find that families with kids are more favorable toward RCs. Their WTP is around €1086 higher, which is 7.0% of the mean WTP.

To summarize, the location and size of RCs matter; external effects are lower for relatively small RCs in existing buildings. As the household composition may vary over space, the overall effect also depends on local demographics. Particularly, we find consistent evidence that support of the local population will be greater in neighborhoods with more families and a high share of foreign-born people.

5.4. Where to build RCs?

Given the preferred non-parametric WTP estimates (see Table 7, Column (4)) and the average demographics in an area we can determine what is the best location to open RCs. Let us consider an inflow of 12,500 additional refugees (which is about the standard deviation of asylum applications throughout the years). We consider two cases: one where only small RCs are opened with a capacity of 250 refugees. In the second case, only relatively large RCs are opened with a capacity of 1250 refugees. We assume that RCs will

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table 7. Explaining heterogeneity in the WTP for RCs

Dependent variable: the WTP for RCs, $\hat{\gamma}^*_{1j}$	Maximum likelihood			+ Control function
	(1) ML	(2) ML	(3) ML	(4) ML-CF
RC capacity (in 100 s)	-700^{***}	-677^{***}	-506^{**}	-540^{**}
	(246)	(245)	(241)	(245)
RC is newly built	−1959	-2288^{*}	-2167^{**}	-2129^{*}
	(1377)	(1378)	(1099)	(1097)
RC relative capacity (in sd)	−111	−153	−317	−185
	(325)	(313)	(260)	(273)
Income (in sd)		-684^{**}	-505^{**}	−288
		(306)	(242)	(237)
Age 30–49		−405	−311	−36
		(267)	(215)	(200)
Age 50–69		−1036	−513	−80
		(0.668)	(416)	(511)
Age ≥ 70		-1868^{*}	−31	273
		(1.031)	(917)	(956)
Non-western foreigner		1280^{**}	1188^{**}	1030^{**}
		(508)	(469)	(459)
Household size		426^{*}	210	375^{*}
		(230)	(160)	(201)
Household—couple		116	−276	−21
		(0.544)	(484)	(472)
Household—kids		1224^{***}	906^{***}	1086^{***}
		(317)	(306)	(313)
Household—share male		148	58	91
		(198)	(187)	(189)
Housing attributes	No	No	Yes	Yes
Number of observations	57,728	57,728	57,728	57,728
McFadden pseudo-R^2	0.038	0.058	0.143	0.144

Notes: 'ML' stands for maximum likelihood and 'CF' for the control function approach. We only include observations within 2 km of an RC. Bootstrapped standard errors are clustered at the neighborhood level and in parentheses.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

be opened in the centroid of neighborhoods. There are roughly 4000 neighborhoods in the Netherlands. We draw circles of 2 km around each centroid to determine average *household* characteristics for that particular neighborhood. We evaluate the WTP estimates at the average *housing* attributes in the sample. By applying our estimates across all neighborhoods, we can choose those neighborhoods with the least negative *total* WTP. This is based on the average WTP *and* total number of housing units in that particular area. Furthermore, we allow for the construction of only one RC per municipality, which is in line with the current practice.

At least three caveats are important to mention before considering the results. First, we do not take into account preferences of refugees themselves, which would be necessary if one is willing to undertake full cost–benefit analyses of the placement of RCs. Second, we do not take into account other costs, such as construction costs and wages for the RC staff,

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

which may vary between locations and may be higher when RCs are small. Third, once RCs are opened, preference-based sorting may occur, which then leads to a different demographic composition of the neighborhood and possibly in turn affects house prices. Although the treatment effect we estimate includes this effect, we abstract from discussing the price effects of RC-induced sorting, which are typically considered second-order effects.

We report maps of the *average* households' WTP for RCs in Figure 7. In Figure 7(A), we focus on the construction of small RCs. One can observe that the WTP vastly differs between areas. For example, the *average* WTP is considerably smaller in cities such as Amsterdam, Rotterdam and The Hague. These are areas which, for example, have higher shares of foreign-born people. Especially in rural areas the average estimated WTP is strongly negative. The WTP ranges from –€13.7 thousand to –€18.9 thousand. Maybe surprisingly, we show that a couple of areas with a very high negative *average* WTP are selected. This is because of a low population in these areas so that few people are affected.

We also consider the alternative scenario where 10 new RCs will be opened with a capacity of 1250, see Figure 7(B). The average WTP per location is now much larger and ranges from –€19.2 thousand to –€40 thousand. However, the areas that are selected overlap with the previous case. Hence, although the targeted areas are typically rural areas with high negative average WTP values, the *total WTP* is still smaller in those areas.

In Figure 8, we vary the capacity of RCs and show the total WTP across all targeted RC locations for different capacity levels. Figure 8 suggests that the total WTP is considerably *larger* if RCs are *smaller*. For example, for an average capacity of 250 the total costs as capitalized in housing values are about €1.8 million, while this is 37% lower when an average capacity of 1250 is chosen. Hence, despite the fact that households

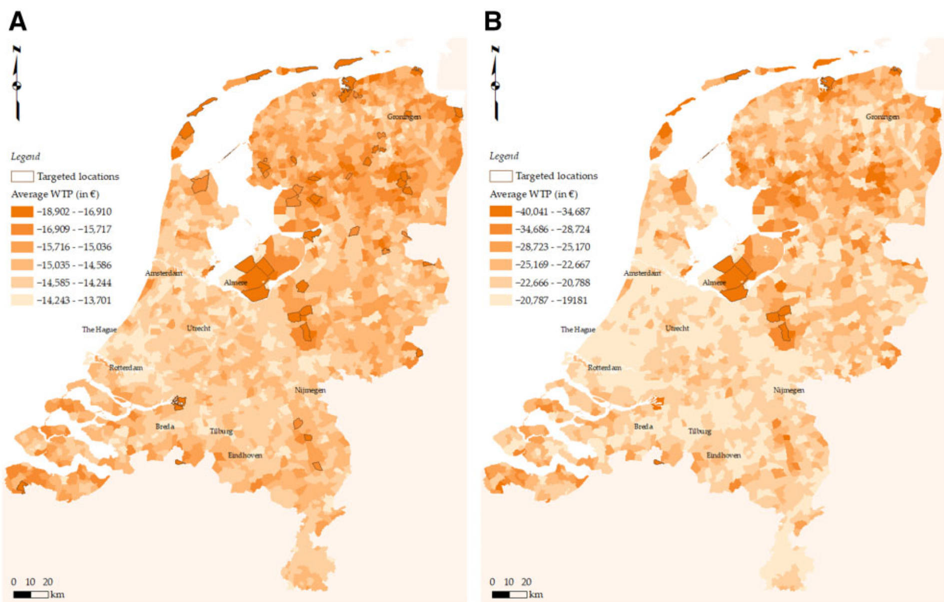

Figure 7. The WTP for RCs. (A) RC capacity of 250 and (B) RC capacity of 1250.
Notes: We report the average WTP per neighborhood. We rank the areas with the highest total WTP and assign one RC per municipality to determine the set of selected locations. We consider a sudden inflow of 12,500 refugees.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

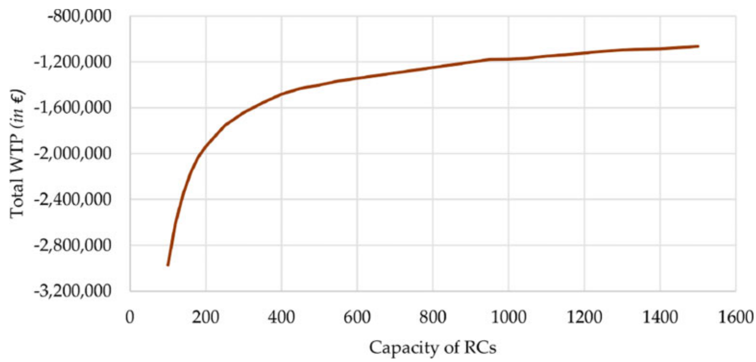

Figure 8. Total WTP for an inflow of 12,500 refugees.
Notes: This figure reports the total WTP given an inflow of 12,500 refugees. We assume that RCs will be opened in the centroid of neighborhoods and further assume a maximum of one RC per municipality.

dislike large RCs, it seems preferable to build a few large RCs in sparsely populated areas. Although it is contentious to let only some households carry a relatively heavy burden, it is in line with current practices (e.g. the correlation between RC capacity and log population density <2 km is –0.344).

Yet, *COA* currently opens RCs across the whole of the Netherlands, also in urban areas. Our results imply that if such areas are chosen, the effect can be mitigated by placing RCs in existing buildings, so that the RC fits in the current urban environment and possibly attracts less attention. The effect can be further mitigated by opening RCs in locations with high shares of foreign-born people and families.

6. Conclusion

The number of refugees around the world has increased steadily in the last decade. This has had a profound impact on many countries, regions and cities. Many of these refugees have to await their asylum procedure in dedicated RCs. In this paper, we use data on RCs opened in the Netherlands between 1987 and 2015 and house prices to measure how much households are willing to pay to avoid living near RCs. This disamenity effect captures both negative externalities caused by RCs as well as attitudes of locals toward refugees.

The results show that house prices within 2 km of an RC decline by 5.8% after opening of an RC. The effect seems to be higher in corridors from RCs to local shopping areas, implying that households may dislike refugees walking through their neighborhood. Furthermore, for a standard deviation increase in the local share of nationalist votes the marginal effect is 1.0 percentage point higher suggesting that the effect does not only capture a negative externality but possibly also incumbent households' attitudes toward refugees.

To examine this further, we use household information to identify individual preferences. The mean WTP is about –€16 thousand but we show that there is considerable heterogeneity in the WTP. For example, the WTP is about €1400 lower for a standard deviation (i.e. 250 refugees) increase in the capacity of a RC. It is higher when RCs are in existing buildings (about €2000). Both foreign-born people and families have a higher

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

WTP of about €1000 so they seem to be more tolerant toward the opening of RCs near their properties. Importantly, for about 15% of the sample, we cannot reject the null hypothesis that the WTP is different from zero, implying that a reasonable share of the population does not seem to have strong negative attitudes toward RC openings.

Using spatial data on demographics, we show that despite more pronounced disamenity effects of larger RCs, it makes sense to concentrate RCs in a few, preferably sparsely populated, areas. Still, if RCs are opened in urban areas, the effects can be mitigated by using existing buildings and placing them in neighborhoods with higher shares of foreign-born people and families. Of course, the decision to open RCs relates to other factors than just households' preferences, such as general humanitarian concerns, economies of scale and possibilities for future integration. Although such considerations are important, we show that the disamenity effect of RCs can be reduced considerably by carefully choosing locations and that WTP is an informative additional measure to guide policy decisions regarding placement of RCs. An alternative policy could be to mitigate the disamenity effect by aiming to change the attitudes of incumbent households toward refugees. For example, in the Netherlands there is a special day each year in which local households can visit RCs. However, whether this type of policy is effective in changing attitudes toward refugees remains to be seen.

Supplementary material

Supplementary data for this paper are available at *Journal of Economic Geography* online.

Acknowledgments

We thank Piet Eichholtz, Wolter Hassink, Giovanni Peri, Dorinth van Dijk, Jos van Ommeren, Wouter Vermeulen and seminar participants at the ASSA-AREUEA 2020 conference in San Diego, research seminar in Reading, UEA 2019 conference in Amsterdam, BZK/UU 2019 research seminar and the UvA 2018 Research Day for useful comments. We also thank Brainbay for providing the Dutch Association of Realtors (NVM) data on housing transactions. Finally, we thank Boris Velthuijs for valuable research assistance.

References

Abadie, A. (2005) Semiparametric difference-in-differences estimators. *Review of Economic Studies*, 72: 1–19.

Acemoglu, D., Johnson, S., Kermani, A., Kwak, J. (2016) The value of connections in turbulent times: evidence from the United States. *Journal of Financial Economics*, 121: 368–391.

Achbari, W., Leerkes, A. (2017) Van perceptie naar feit, *Cahier* (2017–16). Policy report, Available at: https://repository.wodc.nl.

Akgündüz, Y., Van den Berg, M., Hassink, W. (2018) The impact of the Syrian refugee crisis on firm entry and performance in Turkey. *The World Bank Economic Review*, 32: 19–40.

Algemeen, D. (2016) Protest tegen azc eindigt met ingreep door politie en ME. Available at: www.ad.nl.

Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., Wager, S. (2021) Synthetic difference-in-differences. *American Economic Review*, 111: 4088–4118.

Bajari, P., Benkard, C. (2005) Demand estimation with heterogeneous consumers and unobserved product characteristics: a hedonic approach. *Journal of Political Economy*, 113: 1239–1276.

Bajari, P., Kahn, M. (2005) Estimating housing demand with an application to explaining racial segregation in cities. *Journal of Business and Economic Statistics*, 23: 20–33.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Bakens, J., Mulder, P., Nijkamp, P. (2013) Economic impacts of cultural diversity in the Netherlands: productivity, utility, and sorting. *Journal of Regional Science*, 53: 8–36.
Bakker, K. (2016) Steenbergen: Het protest dat de toon zette voor AZC-debat. Available at: www.nos.nl/nieuwsuur.
Bertrand, M., Duflo, E., Mullainathan, S. (2004) How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics*, 119: 249–275.
Bishop, K., Timmins, C. (2018) Using panel data to easily estimate hedonic demand functions. *Journal of the Association of Environmental and Resource Economists*, 5: 517–543.
Bishop, K., Timmins, C. (2019) Estimating the marginal willingness to pay function without instrumental variables. *Journal of Urban Economics*, 109: 66–83.
Blundell, R., Powell, J. (2003) Endogeneity in nonparametric and semiparametric regression models. Chapter 8, In M. Dewatripont, L. Hansen and S. Turnovsky (eds) *Advances in Economics and Econometrics: Theory and Applications*, pp. 312–357. Cambridge: Cambridge University Press.
Boumeester, H., Dol, K., Mariën, G. (2015) *Verhuiswensen en Feitelijk Gedrag op de Nederlandse Woningmarkt 2006–2011*, Technical report. Den Haag (The Netherlands): OTB.
CBS. (2018) *Uit de Startblokken—Cohortstudie naar Recente Asielmigratie.* Technical report. Den Haag (The Netherlands): CBS.
COA. (2003) *Centraal Orgaan opvang Asielzoekers Jaarverslag 2002*, Technical report. Den Haag (The Netherlands): COA.
COA. (2004) *Centraal Orgaan Opvang Aasielzoekers Jaarverslag 2003*, Technical report. Den Haag (The Netherlands): COA.
Daams, M. N., Proietti, P., Veneri, P. (2019) The effect of asylum seeker reception centers on nearby house prices: evidence from the Netherlands. *Journal of Housing Economics*, 46: 101658.
Davis, L. (2011) The effect of power plants on local housing values and rents. *Review of Economics and Statistics*, 93: 1391–1402.
De Stentor. (2017) Protest tegen azc in Apeldoorn. Available at: www.destentor.nl.
DeStem, B. (2017) AZC-protest in gemeente bernheze zonder incidenten. Available at: www.bndestem.nl.
Donald, S., Lang, K. (2007) Inference with difference-in-differences and other panel data. *Review of Economics and Statistics*, 89: 221–233.
Dröes, M. I., Koster, H. R. A. (2016) Renewable energy and negative externalities: the effects of wind turbines on house prices. *Journal of Urban Economics*, 96: 121–141.
Duranton, G., Overman, H. (2005) Testing for localization using micro-geographic data. *Review of Economic Studies*, 72: 1077–1106.
Duranton, G., Overman, H. (2008) Exploring the detailed location patterns of UK manufacturing industries using microgeographic data. *Journal of Regional Science*, 48: 213–243.
Dustmann, C., Vasiljeva, K., Piil Damm, A. (2018) Refugee migration and electoral outcomes. *Review of Economic Studies*, 86: 2035–2091.
Ekeland, I., Heckman, J., Nesheim, L. (2004) Identification and estimation of hedonic models. *Journal of Political Economy*, 112: S60–S109.
Gibbons, S., Machin, S. (2005) Valuing rail access using transport innovations. *Journal of Urban Economics*, 57: 148–169.
Gonzalez, L., Ortega, F. (2013) Immigration and housing booms: Evidence from Spain. *Journal of Regional Science*, 53: 37–59.
Ivarsflaten, E. (2008) What unites right-wing populists in Western Europe? Re-examining grievance mobilization models in seven successful cases. *Comparative Political Studies*, 41: 3–23.
Keukenkamp, S., Goudsmit, R. (2021) Plan om statushouders met een boog om Oegstgeestse villawijk te laten lopen valt verkeerd. Available at: www.trouw.nl.
Klier, T., McMillen, D. (2008) Evolving agglomeration in the U.S. auto supplier industry. *Journal of Regional Science*, 48: 245–267.
Koster, H. R. A., Pasidis, I. N., Van Ommeren, J. N. (2019) Shopping externalities and retail concentration: evidence from the Netherlands. *Journal of Urban Economics*, 114: 103194.
Koster, H. R. A., Van Ommeren, J. N. (2019) Place-based policies and the housing market. *Review of Economics and Statistics*, 101: 1–15.
Kuppens, J., Scholten, L., Ferwerda, H. (2017) *Risicoanalyse en Monitoring Leefbaarheid en Veiligheid Rondom AZC Apeldoorn*, Technical report. Arnhem (Netherlands): Bureau Beke.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Moraga, J., Ferrer-i Carbonell, A., Saiz, A. (2019) Immigrant locations and native residential preferences: emerging ghettos or new communities. *Journal of Urban Economics*, 112: 133–151.

NRC. (2018) Op papier hebben we al de snelste asielprocedure. Available at: www.nrc.nl.

Ottaviano, G. I. P., Peri, G. (2006) The economic value of cultural diversity: evidence from US cities. *Journal of Economic Geography*, 6: 9–44.

RTL Nieuws. (2015) Protest tegen Azc leidt vaak tot minder vluchtelingen. Available at: www.rtlnieuws.nl.

Saiz, A. (2007) Immigration and housing rents in American cities. *Journal of Urban Economics*, 61: 345–371.

Schelling, T. (1969) Models of segregation. *American Economic Review*, 59: 488–493.

Silverman, B. (1986) *Density Estimation for Statistics and Data Analysis*. New York (NY): Chapman and Hall.

Theebe, M. (2002) Asielzoekers en de waarde van uw woning. ESB, 87(4342), p. 24, 11 January 2002.

Theebe, M., Eichholtz, P. (2003) Local housing market effects of asylum seekers' accommodation centers. Meteor Working Paper. Maastricht University School of Business and Economics, Maastricht University.

Toonen, A. (2015) Protest tegen AZC enschede 'duur feestje van de democratie'. Available at: www.nrc.nl.

Tumen, S. (2016) The economic impact of Syrian refugees on host countries: quasi-experimental evidence from Turkey. *American Economic Review: Papers and Proceedings*, 106: 456–460.

UNHCR. (2019) *UNHCR Global Trends 2018*. Technical report. Available at: www.unhcr.org

Volkskrant. (2016) Drie arrestanten azc-protest heesch komen uit andere gemeente. Available at: www.volkskrant.nl.

WDR. (2016) Fakten zur kölner silvesternacht: Wer sind die täter. Available at: www.wdr.de

Yatchew, A. (2003) *Semiparametric Regression for the Applied Econometrician*. Cambridge: Cambridge University Press.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Appendix

A. Concentration of refugee centers

A.1 *Concentration of refugee centers*

In this Appendix, we test whether refugee centers are spatially concentrated. If RCs are randomly distributed over space, it is less likely that there will be a strong correlation between unobservable locational endowments and RCs.

Hence, we employ a point-pattern methodology as introduced by Duranton and Overman (2005, 2008) to test for the concentration of RCs. This methodology exploits the fact that our data are continuous over space.[25] We basically estimate Kernel densities for different distances and investigate whether they deviate significantly from a randomly generated spatial distribution. Below, we briefly discuss the procedure.

Let $K(d)$ denote the estimated kernel density at a given distance d, d_{ik} denotes the distance between location i and k, where $i = 1, \ldots, n$. Then,

$$K(d) = \frac{1}{n(n-1)h} \sum_{i=1}^{n-1} \sum_{k=i+1}^{n} \Omega\left(\frac{d - d_{ik}}{h}\right), \tag{A.1}$$

where n is the total number of realized and canceled RCs in 2015, h is the bandwidth and

$$\Omega(\cdot) = \frac{1}{\sqrt{2\pi}} \mathrm{e}^{-\frac{1}{2}\left(\frac{d-d_{ik}}{h}\right)^2}. \tag{A.2}$$

Equation (A.2) implies that we use a normal density function. Following Duranton and Overman (2005, 2008) and Klier and McMillen (2008), we use a bandwidth h equal to Silverman's plug-in bandwidth (see Silverman 1986). More specifically, $h = 1.06\sigma_{d_{ik}} n^{-1/5}$, where $\sigma_{d_{ik}}$ is the standard deviation of the estimated bilateral distances between RCs. Distances d cannot be negative, so we use the reflection method, proposed by Silverman (1986), to deal with this issue.

We aim to test whether the estimated concentration is statistically different from a random geographical pattern so we have to define counterfactual location patterns. For each of the 1000 bootstrap runs, we draw n locations and put them randomly across the Netherlands.

To investigate whether there is a statistically significant concentration of RCs, we calculate the difference between $\hat{K}(d)$ and the upper confidence band of the randomly generated patterns, denoted by $\overline{K}(d)$. RCs may also be significantly dispersed, then $\hat{K}(d) < \underline{K}(d)$. To define $\underline{K}(d)$ and $\overline{K}(d)$, we treat each of the estimated density functions for each simulation as a single observation. Following Duranton and Overman (2005), we choose identical local confidence levels in such a way that the global confidence level is 5%.

We report the results when we estimate global concentration indices as per Equation (A.1) for each distance below the median bilateral distances between RC location. In Figure A1, we report the results when including all RCs. We can clearly see that the actual distribution of RCs in the Netherlands falls well within the confidence bands at each distance d. The spatial distribution of refugee centers is thus close to random. This seems to be in line with the

25 It has been argued that many measures of concentration use arbitrary spatial units (such as counties, cities or zip codes), which may be problematic as they may lead to biases in the measure of concentration. The Duranton concentration index controls for overall agglomeration, is invariant to scale and aggregation and, importantly, provides an indication of statistical significance.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

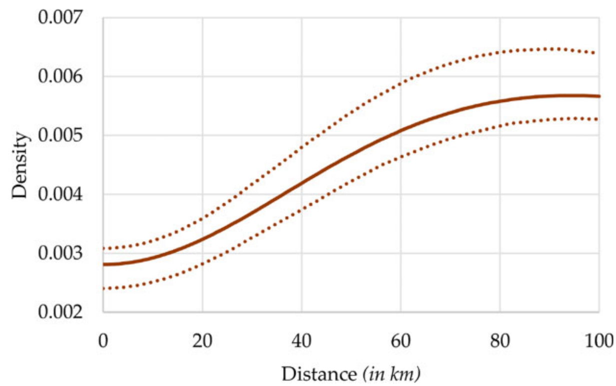

Figure A1. *K*-density for all RCs.
Notes: This figure uses the Duranton and Overman (2005) methodology by examining whether the actual distribution of refugee center locations deviates from a randomly generated sample of refugee locations. The dotted lines represent, respectively, the lower and upper 5% global confidence band.

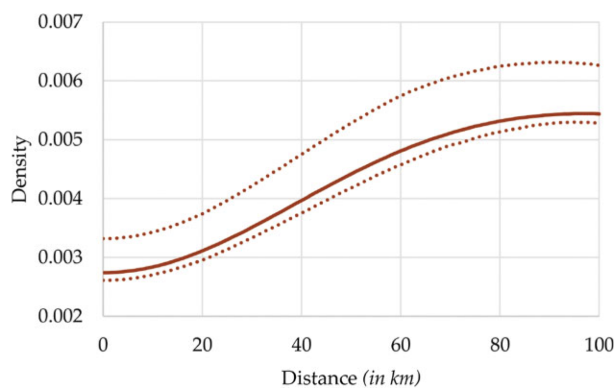

Figure A2. *K*-density for realized RCs.
Notes: The dotted lines represent, respectively, the lower and upper 5% global confidence band.

general policy of COA to evenly spread out refugee centers across the country. Interestingly, and in line with the outcomes of the *K*-density test, we do not find a correlation between the log of population density and whether an RC has been opened ($\rho = 0.0161$). However, larger RCs tend to be opened in more sparsely populated areas: the correlation between capacity and the log of population within 2 km of an RC is -0.344.

One may argue that the canceling of RCs may have been non-random, for example because protests may mainly arise in rural areas where households are more aware of the inflow of refugees. We therefore re-estimate the *K*-density, only when using the realized RCs. Figure A2 shows that realized RCs are a bit more dispersed, but still fall within the confidence bands.

B. Detailed descriptives

B.1 Detailed descriptives for the house price dataset

Table B1 shows the descriptives for the house price dataset. The observations are split between RCs: that were realized before 2015; those that were opened and closed before 2015;

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table B1. Descriptive statistics: house price data per refugee center category

	Realized <2 km		Planned <2 km		Closed <2 km		Canceled <2 km	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Transaction price	184,482	103,635	150,118	77,453	258,942	156,310	190,731	109,389
Refugee center opened, <2 km	0.622	0.485	0	0	0.113	0.317	0	0
Size in m^2	114.6	35.87	104.6	33.89	94.90	40.27	106.7	37.63
Number of rooms	4.285	1.294	4.065	1.241	3.396	1.456	4.099	1.352
Terraced property	0.361	0.480	0.285	0.451	0.112	0.315	0.265	0.441
Semi-detached property	0.264	0.441	0.220	0.414	0.0685	0.253	0.227	0.419
Detached property	0.0836	0.277	0.0786	0.269	0.0324	0.177	0.0917	0.289
Property has garage	0.290	0.454	0.214	0.410	0.113	0.317	0.260	0.438
Property has garden	0.976	0.153	0.978	0.148	0.983	0.130	0.974	0.158
Maintenance state is good	0.852	0.355	0.870	0.336	0.871	0.335	0.858	0.349
Property has central heating	0.889	0.315	0.857	0.350	0.845	0.362	0.878	0.328
Property is (part of) listed building	0.00503	0.0707	0.00971	0.0980	0.0368	0.188	0.0105	0.102
Construction year 1945–1959	0.0829	0.276	0.0704	0.256	0.0280	0.165	0.0859	0.280
Construction year 1960–1970	0.181	0.385	0.127	0.333	0.0255	0.158	0.199	0.399
Construction year 1971–1980	0.143	0.350	0.129	0.335	0.0235	0.151	0.135	0.341
Construction year 1981–1990	0.118	0.322	0.131	0.338	0.109	0.312	0.0908	0.287
Construction year 1991–2000	0.133	0.340	0.107	0.309	0.114	0.318	0.0913	0.288
Construction year >2000	0.0823	0.275	0.0762	0.265	0.0528	0.224	0.0775	0.267

Notes: This table shows the descriptive statistics of the house price dataset split up across the four different categories of refugee centers. Realized RCs are those that were present in 2015 and opened before that date. Planned RCs are those that were planned to be opened in 2016–2018. Closed RCs were opened and closed before 2015. Canceled RCs are RCs that were planned to be opened in 2016–2018 but were canceled. The number of observations for each category is 111,628, 36,265, 46,543, and 123,757, respectively. For our preferred baseline specification, we use observations from the first three categories. Each observation is classified based on the nearest RC in the overall sample period and the transaction being within 2 km. Each observation uniquely belongs to a particular RC category. The sample period is 1990–2015.

those that were planned to be opened after 2015 (in 2016–2018); and those that were planned to be opened after 2015 but were canceled. The observations of the first three categories sum up to the treatment group as used for our baseline regression results.

House prices are highest in locations where refugee centers will be or are closed (€258,942) and lowest where they are planned (€150,118). The realized refugee centers show an average transaction price (€184,482) that is lower than in the full sample. Housing characteristics across the different categories also differ a bit. This highlights that it is important to control for housing characteristics in the regression analyses. It also suggest that we carefully have to look at non-random placement of RCs. Our identification strategy (i.e. difference-in-differences, using the variation in timing of RCs, triple-differences) deals with such issues.

B.2 Representativeness of the RC sample

As we do not include temporary RCs in the analysis, there might be concerns that our sample of permanent RCs is not representative. To explore this further, we can compare the RC capacity with the actual refugee inflows (e.g. see Figure 1). In particular, we know the aggregate capacity in our sample and the actual refugee inflows and occupation (taken from COA), which we show in Figure B1. The permanent RC capacity has increased gradually over time.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

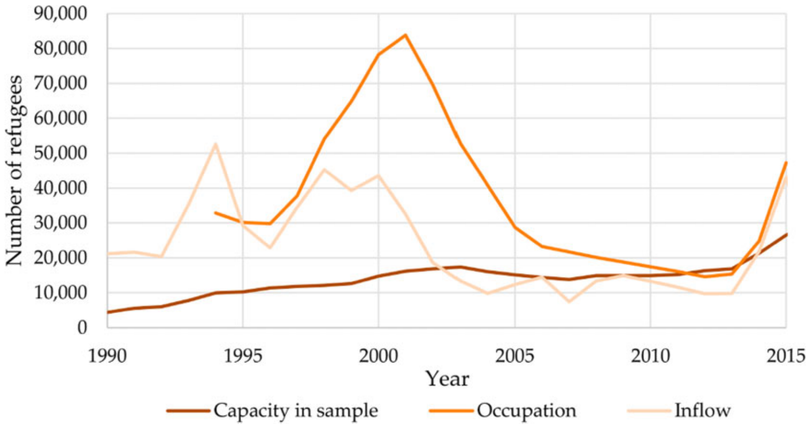

Figure B1. RC capacity, occupation, and inflow.
Notes: This figure shows the permanent RC capacity, the inflows of refugees (first asylum requests), and the general occupancy level (stock of refugees).

This may be a reflection of the survival probability of these RCs. As our sample also includes a hand-collected sample of opened and closed RCs, we can explore this potential selection issue further as, for example, RCs with large negative price effects are more likely to be closed. It indeed seems to be the case that only focusing on the opened RCs leads to a slight underestimate of the treatment effect (see Table 5, Column (5)).

Any peaks in refugee flows are dealt with by opening temporary RCs. These would typically not lie directly next to an existing RC, such that our estimate of the local treatment effect (particularly in the case we use a very local control group) should not be contaminated. As of 2005, the total capacity of permanent RCs captures the total amount of refugees in the Netherlands well.[26] Moreover, it is important to note that occupation rates of permanent RCs are fairly high throughout our study period. The general occupancy rate of RCs is typically between 85% and 93% (see e.g. COA 2003, 2004). Hence, the RC buildings in our sample are not referring to just empty buildings.

B.3 Representativeness of the house price data

In Table B2, the average transaction prices and transaction volumes for the *Land Registry* and the *Dutch Association of Realtors* (*NVM*) data are shown. By law, housing transactions are recorded in the land registry. The *NVM* data cover the full population of owner-occupied transactions particularly well in the last years of the sample where the coverage is 90% or higher. Before 2007, the average price in the *NVM* data is higher than the price based on the data from the *Land Registry*. The main issue is that a detailed set of housing characteristics is not available in the *Land Registry* data—one of the reasons we use the *NVM* data—so a further comparison of sample composition is difficult. It does imply that it is important to control for a wide set of housing characteristics to address potential sample selection issues. Finally, note that in every

26 As one may question the representativeness of our sample before 2005, because for example permanent RCs that are closed before 2005 are hard to find online, we emphasize that our main conclusions do not change if we use a subsample based on observations after 2005.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table B2. Representativeness of the *NVM* data

	Land registry data		NVM data		
	Price in €	Transactions	Price €	Transactions	Coverage (%)
1995	93,750	154,568	108,464	63,524	41
1996	102,607	175,751	117,180	75,993	43
1997	113,163	185,634	127,303	89,466	48
1998	124,540	192,622	138,719	104,208	54
1999	144,778	204,538	163,085	109,365	53
2000	172,050	189,358	186,540	114,733	61
2001	188,397	195,737	199,968	128,196	65
2002	199,752	198,386	210,754	130,540	66
2003	204,829	193,406	214,916	131,219	68
2004	212,723	191,941	222,579	136,532	71
2005	222,706	206,629	232,265	150,504	73
2006	235,843	209,767	242,299	152,488	73
2007	248,325	202,401	250,984	152,779	75
2008	254,918	182,392	251,129	130,067	71
2009	238,259	127,532	235,857	100,268	79
2010	239,530	126,127	243,262	106,180	84
2011	240,059	120,739	238,510	97,479	81
2012	226,661	117,261	223,682	100,365	86
2013	213,353	110,094	217,669	98,607	90
2014	222,218	153,511	226,471	143,783	94
2015	230,194	178,293	234,042	162,737	91

Notes: This table shows the average price and transaction volumes for the land registry (Kadaster) data and the Dutch Association of Realtors (NVM) data.

time period within our sample, we find a negative effect of the opening of RCs which, except for the period 1995–1999, is also statistically significant (see Column (6) of Table 5).

C. Other results

C.1 Event studies

In this Appendix, we re-estimate the event study, as depicted in Figure 4 for list prices. In Figure C1, we find a similar effect on list prices as we did with transaction prices. At the moment of opening of an RC there is an immediate negative effect on list prices. The effect seems to be slightly smaller than the effect on house prices. This suggests that the majority of the effect is already incorporated in list prices.

In Figure C2, we extend the event study for sales prices to 10 years before opening of an RC. All of the estimated coefficients before opening of an RC are statistically indistinguishable from zero at the 5% level.

C.2 Heterogeneity in the treatment effect

We further explore heterogeneity in the effect building upon Equation (1). In comparison to the non-parametric results, we only focus on implicit prices and the role of refugee center characteristics. That is, we add interaction terms between $\mathcal{RC}_{it}$ and whether the refugee

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

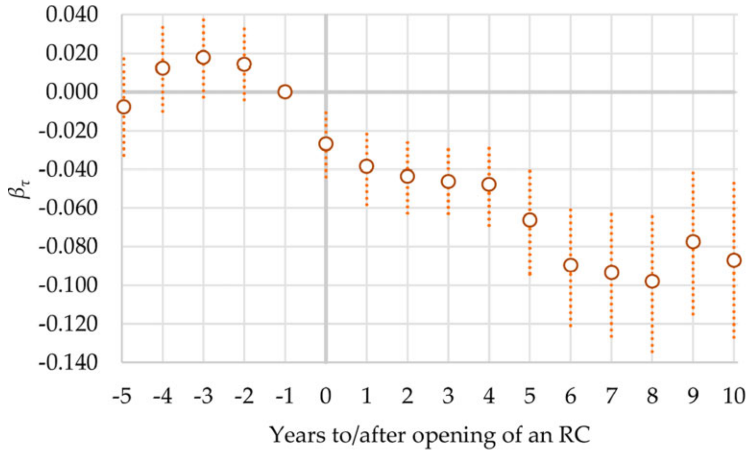

Figure C1. Event study (list prices).
Notes: We allow the effect of RCs to be dependent on the years to/after opening, see Equation (2). The event window ($\tau = -T$ to T) in our sample runs from −28 to 27 years. We report 5 years before, and until 10 years after, the opening of an RC. One year before treatment is the reference category. We use Equation (2), but with the log list price as dependent variable.

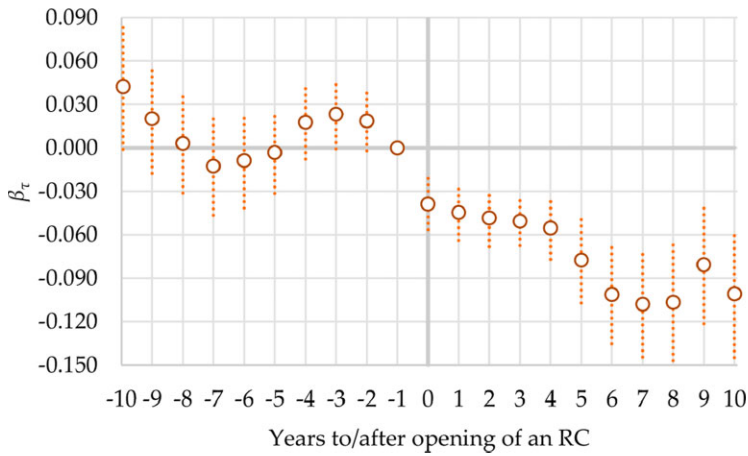

Figure C2. Extended event study.
Notes: We allow the effect of RCs to be dependent on the years to/after opening, see Equation (2). The event window ($\tau = -T$ to T) in our sample runs from −28 to 27 years. We report 10 years before, and until 10 years after, the opening of an RC. One year before treatment is the reference category. Table 5 reports the number of observations and R^2. Recall that only observations within 2 km of an (eventually) opened RC are included.

center is located in a rural area (versus an urban area), the capacity and capacity relative to the local population, whether the refugee center is located in a new building and the type of RC. The results are reported in Table C1.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table C1. Interaction effects: refugee center characteristics

Dependent variable: the log of house price

	(1) Rural	(2) Capacity	(3) Relative capacity	(4) New built	(5) +Extra controls	(6) RC type	(7) All
Refugee center opened	−0.0518***	−0.0383***	−0.0526***	−0.0318***	−0.0349***	−0.0655***	−0.0221
	(0.0118)	(0.0095)	(0.0110)	(0.0097)	(0.0094)	(0.0090)	(0.0149)
Refugee center opened × rural	−0.0255						−0.0343**
	(0.0172)						(0.0140)
Refugee center opened × high capacity		−0.0546***					−0.0305***
		(0.0154)					(0.0136)
Refugee center opened × high relative capacity			−0.0136				0.0028
			(0.0132)				(0.0138)
Refugee center opened × new built				−0.0638***	−0.0560***		−0.0465***
				(0.0173)	(0.0156)		(0.0131)
New residential buildings (log)					0.0136**		0.0126**
					(0.0056)		(0.0056)
New commercial buildings (log)					−0.0091*		−0.0083
					(0.0051)		(0.0051)
Refugee center opened × process RC						−0.0164	−0.0119
						(0.0090)	(0.0241)
Refugee center opened × family RC						0.0541**	0.0399*
						(0.0214)	(0.0209)
Housing characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Postcode-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year- and month-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	194,436	194,436	194,436	194,436	194,436	194,436	194,436
R^2	0.93	0.93	0.93	0.93	0.93	0.93	0.93

Notes: Standard errors clustered at the neighborhood level in parentheses.
***$p < 0.01$,
**$p < 0.05$,
*$p < 0.1$.

First, we added the interaction effect with an indicator of a house being located in an urban or rural area as defined by *Statistics Netherlands*. We would expect that a larger refugee center has a larger impact in a small village in comparison to a large city. There is a negative coefficient on the interaction term but it is not statistically significant at conventional levels, see Column (1).

In Column (2), we add the interaction with a high capacity dummy (>500 refugees), which is approximately equal to the average capacity in the sample. The results show that a high capacity is associated with an additional decrease in prices of –5.3%. We also include an interaction with a high (above median) relative capacity indicator variable in Column (3), which is the capacity relative to the population within 2 km of the property. The high relative capacity indicator, however, is not statistically significant.

Next, we add the interaction effect with a dummy indicating whether a refugee center is located in a new building in Column (4), which shows a strong additional negative effect. To control for the fact that this might just be reflecting (nuisance) as a result of new construction, we control for the (log) number of new residential and commercial buildings constructed in

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

the area in Column (5). The results indicate that the effect of RCs opened in newly built buildings is 5.4 percentage points more negative, relative to RCs opened in existing buildings. Finally, RCs that are dedicated for 'families only' have a smaller effect on house prices in comparison to central or processing RCs, see Column (6).

In Column (7), we include all interaction terms at the same time. The coefficients are generally similar to the previous specifications, but there are some notable differences. The effect on RCs opened in rural areas is now negative and statistically significant, while the effects of high capacity, new built RCs, and family RCs are somewhat less strong.

C.3 Other robustness checks and extensions

Table C2 shows some additional regression results. In Column (1), we re-estimate the baseline model using repeat sales. By including property-fixed effects, we control for time-varying unobserved housing and location characteristics. The number of observations in the repeat sales model does, however, decrease considerably and the repeat sales model is potentially subject to selection bias. Nevertheless, the results suggest that the effect is –5.1%. Also, it is still statistically significant at the 1% level.

Alternatively, there may also be unobserved developments in the implicit prices of the control variables, which our preferred specification does not allow for. We therefore also estimate a time-varying coefficient model in which all implicit prices *and the location fixed effects* are allowed to vary over time. That is, we add interaction terms between the independent variables and 5-year period dummies. We report the results in Column (2) and show that even with this very extensive model, the opening of a refugee center still has a negative and statistically significant effect on house prices of –2.9%.

In Column (3), Table C2, we use the difference between the log transaction price and log list price as dependent variable. The question is whether sellers take into account the price effect of the opening of a refugee center in setting the list price. Although sellers seem to anticipate the majority of the decrease in prices, buyers require an additional discount of 1.1 percentage points. This effect is statistically significant at the 1% level.

Table C2. Further robustness and extensions

	(1) Repeat sales	(2) Time-varying coef.	(3) Markup	(4) Time on market	(5) Number of refugees
Refugee center	-0.0521^{***}	-0.0299^{***}	-0.0109^{***}	0.1507^{***}	-0.0596^{***}
Opened, <2 km	(0.0099)	(0.0081)	(0.0023)	(0.0513)	(0.0090)
$\text{RC}\times(\log(\text{refugees})-$					0.0024
$\log(\overline{\text{refugees}}))$					(0.0096)
Housing characteristics	No	Yes	Yes	Yes	Yes
Postcode FE	No	Yes	Yes	Yes	Yes
Year, month FE	Yes	Yes	Yes	Yes	Yes
Observations	40,012	194,436	194,436	191,774	194,436
R^2	0.76	0.96	0.25	0.26	0.93

Notes: This table uses the data within 2 km of RCs (also see Specification (3), Table 4). Standard errors are clustered at the neighborhood level and in parentheses.
$^{***}p < 0.01$,
$^{**}p < 0.05$,
$^{*}p < 0.1$.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Opening of RCs may also impact the liquidity in the housing market (see for evidence on amenities and time on market, Koster and Van Ommeren 2019). We estimate the effect of the opening of a refugee center on the log of time on the market. The estimate in Column (4) suggests that there is a 16% increase in the time on market, which is in line with the observation that list prices are set relatively high (higher than buyers accept apparently).

Finally, Column (5) reports the interaction effect with the total number of asylum applications in a specific year. In particular, we interacted the treatment effect with the (log) difference between the number of asylum request (see Figure 1) and the average of that number over time. The idea is that if there are many refugees coming to the Netherlands households might be more aware of their presence, which may influence their attitudes. Although there are large differences in the inflow of refugees, the findings reported in Column (5) suggest that the number of refugees does not seem to affect the implicit prices of the opening of a refugee center.

D. Subjective well-being and unemployment

D.1 Neighborhood level data on subjective well-being and unemployment

We also examine the broader economic impact at a neighborhood level as an extension to the main analysis and to investigate whether those are in line with the effects on house prices. In particular, we collect additional data from the Dutch Housing Needs Surveys (*WoON*) on (living) satisfaction, the intention to move (within two years), and more subjective indicators on (neighborhood) nuisance and fear of crime. We also have information about unemployment and the amount of hours worked and a wide range of housing attributes (e.g. the size of the property, house type, and whether the household has moved within the last two years). We do not have information on actual crime rates.[27] For each property in the survey, we only know the location at the neighborhood level.

We combined five waves: 2002–2003, 2005–2006, 2008–2009, 2011–2012, and 2014–2015. Each wave consists of about 60,000 respondents and is considered to be a representative sample of the Dutch population. The descriptive statistics of the combined surveys are reported in Table D1. On average, about 7% of the respondents are dissatisfied with their neighborhood, 8% wants to move within two years, 5% experiences nuisances, and 8% a strong fear of crime.[28] The average employment is 94% and the head of the household works about 48 h a week. In only 2% of all cases a refugee center has been opened within 2 km. We further added several household-specific variables such as yearly income, cultural background, and type of households as additional controls.

D.2 Econometric framework

We will estimate the same model as in Equation (1) but at a neighborhood level and using a set of different dependent variables:

$$y_{rkt} = \tilde{\beta}\mathcal{RC}_{rkt} + \tilde{\delta}X_{rkt} + \tilde{\lambda}_k + \tilde{\mu}_t + \tilde{\epsilon}_{rkt}, \qquad \text{(D.1)}$$

where y_{rkt} is the dependent variable of interest (e.g. satisfaction and nuisance) of a respondent r living in neighborhood k in year t. We emphasize that we cannot track individual

27 For a detailed analysis of crime rates near Dutch RCs, see Achbari and Leerkes (2017).

28 Boumeester et al. (2015) show that 56% of the households that have an intention to move actually move within 3 years. Given that actual moving behavior is subject to financial and availability constraints, the intention to move can be argued to better represent preferences of households.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table D1. Descriptive statistics: Woon dataset

	(1) Mean	(2) Std. Dev.	(3) Min	(4) Max
Dissatisfied with neighborhood	0.0696	0.255	0	1
Move	0.0787	0.269	0	1
Nuisance	0.0480	0.214	0	1
Fear of crime	0.0799	0.271	0	1
Employed (works $\geq$12 h per week)	0.944	0.230	0	1
Hours worked	47.84	15.46	1	60
Refugee center opened, $<$2 km	0.0229	0.150	0	1
Gross yearly income	42,398	51,375	0	1,753,644
Age	50.82	16.81	17	107
Foreign	0.119	0.306	0	1
Single	0.613	0.487	0	1
Kids	0.349	0.477	0	1
Religion—Christian	0.457	0.498	0	1
Religion—Muslim	0.0389	0.193	0	1
Religion—other	0.0618	0.241	0	1

Notes: We also include 18 housing characteristics, including house type dummies, the floor of the apartment, the number of floors in the building, whether the building has an elevator, whether the property has central heating, a garage, the number of rooms and construction decade dummies. The number of observations is 285,031. For information on employment status, we have information on 129,097 observations because the data are missing in one wave of the survey (2008–2009).

respondents over time, implying that we cannot include respondent-fixed effects. Furthermore, $\mathcal{RC}_{rkt}$ equals one when the centroid of a neighborhood is within 2 km of a refugee center (after opening) and x_{rkt} are housing and household attributes.

We adopt the same identification strategies as outlined before. First, we use the whole sample. Second, we only include observations that are in a neighborhood with an RC or a planned/canceled RC. Third, we only exploit variation in timing implying that we include neighborhoods where there is an RC or will be one in the future (before 2015).

D.3 Results

Table D2 shows that the opening of an RC increases the probability of dissatisfaction in the neighborhood by about 1.4–2.0 percentage points, although this is not statistically significant at conventional levels in Column (3), where we only rely on temporal variation in the opening of RCs. The effect is substantial given the sample mean of dissatisfaction of 0.0696. In addition, the opening of an RC increases the probability that households want to move within 2 years by 1.9–2.6 percentage points, which is statistically significant at the 5% or 10% level.

In Panel B of Table D2, we investigate whether households also experience more nuisance. We find that the probability increases by 1.6–2.3 percentage points after an RC has been opened. There does not seem to be an increase in the fear of crime, which is in line with the previous literature that does not find effects on local crime rates (see Achbari and Leerkes 2017).

Finally, in Panel C, the opening of an RC seems not to statistically significantly reduce local unemployment as well as the number of hours worked. However, there may be effects outside of the local neighborhood: it is well known (i.e. reported by COA) that there may be

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023

Table D2. Regression results: perception and employment effects

Panel A: Satisfaction	Dep. var.: dissatisfied			Dep. var.: intention to move		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Refugee center in neighborhood	0.0170***	0.0196***	0.0139	0.0188*	0.0261**	0.0222*
	(0.00646)	(0.00740)	(0.00861)	(0.0109)	(0.0113)	(0.0123)
Household characteristics (9)	Yes	Yes	Yes	Yes	Yes	Yes
Housing attributes (16)	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	282,229	27,270	13,798	282,229	27,270	13,798
R^2	0.064	0.058	0.069	0.090	0.098	0.106

Panel B: Nuisance and safety	Dep. var.: nuisance			Dep. var.: fear of crime		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Refugee center in neighborhood	0.0160**	0.0230***	0.0214**	0.00539	0.00924	0.00566
	(0.00715)	(0.00775)	(0.00829)	(0.00920)	(0.00986)	(0.0106)
Household characteristics (9)	Yes	Yes	Yes	Yes	Yes	Yes
Housing attributes (16)	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	282,229	27,270	13,798	282,229	27,270	13,798
R^2	0.045	0.045	0.049	0.081	0.079	0.089

Panel C: Employment	Dep. var.: employed			Dep. var.: hours worked		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Refugee center in neighborhood	0.0017	−0.0043	−0.0006	−0.347	−0.345	−0.233
	(0.0160)	(0.0167)	(0.0181)	(0.858)	(0.864)	(0.876)
Household characteristics (9)	Yes	Yes	Yes	Yes	Yes	Yes
Housing attributes (16)	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	127,922	27,270	13,798	282,229	27,270	13,798
R^2	0.116	0.133	0.177	0.375	0.375	0.376

Note: Standard errors are clustered at the neighborhood level and in parentheses.
***$p < 0.01$,
**$p < 0.05$,
*$p < 0.10$.

many (also non-local) people working in an RC, which is something we do not directly measure. A similar story might apply to crime.

In sum, the effects of nuisance and dissatisfaction seem to be the dominant factors underlying the effect of RCs on local communities, which is in line with the reported results of RCs on house prices.

Downloaded from https://academic.oup.com/joeg/article/23/1/51/6574401 by Universiteit van Amsterdam user on 08 August 2023