DISCUSSION PAPER SERIES

DP14551
(v. 4)

A WORLD DIVIDED: REFUGEE CENTERS, HOUSE PRICES, AND HOUSEHOLD PREFERENCES

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INTERNATIONAL TRADE AND REGIONAL ECONOMICS

ISSN 0265-8003

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Discussion Paper DP14551
First Published 02 April 2020
This Revision 15 December 2020

Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

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JEL Classification: E02, O18, R31

Keywords: Immigration, House Prices, refugee centers, household preferences

Martijn Dröes - m.i.droes@uva.nl
University of Amsterdam

Hans Koster - h.koster@vu.nl
Vrije Universiteit Amsterdam and CEPR

Acknowledgements
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 thank Brainbay for providing the Dutch Association of Realtors (NVM) data on housing transactions. We also thank Boris Velthuijs for valuable research assistance.

A World Divided:

Refugee Centers, House Prices, and Household Preferences*

Martijn I. Dröes[†] Hans R.A. Koster[‡]

December 15, 2020

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[†]Corresponding author. Amsterdam Business School, Faculty of Economics and Business, University of Amsterdam, Plantage Muidergracht 12, 1018 TV Amsterdam, e-mail: m.i.droes@uva.nl. Martijn is also affiliated to the Amsterdam School of Real Estate (ASRE) and the Tinbergen Institute.

[‡]Department of Spatial Economics, Vrije Universiteit Amsterdam, De Boelelaan 1105, 1081 HV Amsterdam, email: h.koster@vu.nl. Hans is also research fellow at the National Research University, Higher School of Economics in St. Petersburg and the Tinbergen Institute, and affiliated to the Centre for Economic Policy Research.

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). Refugee flows have a multitude of underlying causes such as wars, famines, and economic deprivation.[1] For example, the European Union witnessed a sharp increase in the number of asylum applications from about 300 thousand in 2012 to 1.3 million in 2015 due to the war in Syria. Although the influx of refugees to Europe has decreased since, due to the refugee deal between Turkey and the EU, immigration remains high on the political agenda of many Western countries. When refugees come to the EU they have to be accommodated to await the outcome of their asylum procedure, which in some cases can take years. Some refugees stay in large camps at the point where they entered the European Union (*i.e.* Greece, Italy, Spain), but there are also a considerable amount of refugees that are placed in dedicated refugee centers (RCs) within European Union 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 main aim of this paper is to estimate the willingness to pay (WTP) to live close to a refugee center. We do so by exploiting variation in the opening of RCs and relating them to changes in housing prices using hedonic price techniques. 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. This disamenity effect captures both the general *attitudes* of incumbent households towards refugees, as well as *negative external effects*. The latter may, for example, arise due to more traffic, noise pollution, and increased crime levels. The former, may be related to the incumbent households' political affiliation and

[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. The point is that for many countries it is a priori unclear what type of asylum seeker they are dealing with and they have to house them for some time.

[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 fights between the police and protesters (see *e.g.* Algemeen Dagblad 2016, Bakker 2016).

own ethnic background. We aim to study these effect in more detail.

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, when the WTP not only captures a negative externality (*e.g.* related to nuisance, the size of an RC), but also attitudes of incumbent households towards refugees, we would 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. Nevertheless, we would expect that the attitudes towards refugees are, on average, negative as households generally prefer to live near households of their own type (Schelling 1969).

The analysis in this paper is based on about 2.6 million housing transactions taken from the Dutch Association of Realtors and covering the whole of the Netherlands between 1990-2015.[3] Besides transaction prices, the dataset includes information on list prices, the time on market, and an extensive set of housing attributes. We match this data to the locations of opened, planned and closed RCs. Our main dataset is including all permanent RCs that were present in 2015, based on official government sources.[4] Using a variety of internet sources, we further add a subsample of RCs that were opened and subsequently closed and we added RCs that were planned to be opened after 2015, some of which were canceled. We will discuss the representativeness of the RC sample later on. To investigate heterogeneity in the WTP related to the attitudes of the local population living near RCs, 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.* nuisance, willingness to move, and unsafety) to substantiate our findings.

There are two main identification challenges when measuring the causal effect of RCs on house prices. First, RCs are not randomly allocated across space and time. RCs may for example be opened in locations where the available land is cheaper or where the opposition against opening

[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] We focus on permanent RCs as temporary ones are typically only open for a year or two. As housing markets take time to adjust; and permanent changes in amenities are more likely to capitalize into house prices, it makes sense to focus on permanent RCs to examine the externalities and attitudes associated with RCs.

an RC is less severe. To address this issue, we use a difference-in-differences (DID) methodology in which house price changes within a short distance of a realized refugee center are compared to 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 (something we will discuss later on) 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 difference-in-differences model this would not be possible as there is no variation in the timing of treatment. Our approach does require that the timing of the openings of RCs are 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. Hence, note that our approach avoids that we have to make a strong parallel trends assumption, prevalent in a standard DID approach (for a more elaborate discussion on the problems associated with this assumption see Bertrand et al. 2004, Abadie 2005, Donald & Lang 2007). Moreover, we use the existing road network to create (100m 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). We utilize this additional information to estimate the treatment effect using a triple-differences approach comparing local price developments near RCs, but outside those corridors, with the price developments within the corridors.

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 towards refugees and does not necessarily correspond to underlying household-specific willingness to pay parameters. That is, many studies of this sort focus on the *average* treatment effect. 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 & Benkard (2005). This allows us to measure variation in the individual households' willingness to pay. We address the issue that the WTP for RCs is not point-identified because the variable of interest (the presence of an RC) is dichotomous (see Bajari & Kahn

2005).[5]

Our results show that the opening of a refugee center decreases house prices by 3-6% on average, which is economically sizable.[6] The statistical evidence suggests that the effect is confined to 2km. The effect is still there 10 years after the opening of a refugee center, hence the effect seems to be permanent. Closing a refugee center has about the same, but positive, effect on house prices. The triple-differences approach based on travel corridors between refugee centers and the nearest shopping street further suggests that the effect is not equidirectional. Moreover, the effect is lower during the Yugoslavian civil war and higher towards the end of the sample period (Syrian war) and corresponds with the rise of nationalist parties in the Netherlands. Indeed, we find that each percentage point increase in the local share of nationalist votes in the previous Dutch national election is associated with an increased effect of RCs by 0.45 percentage points.

Further results indicate that there is considerable heterogeneity in the WTP of households. The WTP distribution is left skewed. The median WTP after the opening of a refugee center is about −€16 thousand (about 7% relative to average house prices). Interestingly, only about 0.5% of the households have a positive willingness to pay, although this is statistically insignificant in most cases. 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 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 towards 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 willingness to pay 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 as 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

[5] We make use of a suggestion by Ekeland et al. (2004) to mitigate simultaneity problems in the second stage, by instrumenting house size with its value conditional on household characteristics. The identifying assumption is based on the inherently nonlinear nature of the hedonic price function.

[6] Our preferred baseline estimate is essentially 6% (*i.e.* −5.8%), while the triple-differences approach (*i.e.* −2.8%) defines the lower bound estimate of 3%.

areas with higher shares of foreign-born people and families.

Finally, to provide further support for the results on housing prices, we examine in Appendix D whether there is an effect of RCs on the subjective well-being of the incumbent population within the local neighborhood. We focus on measures as residential satisfaction, intention to move and indicators on nuisance and the feeling of safety. We also have information on the amount of hours worked. We show that the probability to move and the probability that households experience dissatisfaction and nuisance increase when an RC is opened. These effects are in line with the hedonic price analysis. By contrast, we do not find effects on feeling more unsafe and the hours worked.[7]

Related literature. Our paper contributes to several strands of literature. In particular, this is not the first paper that examines the effect of refugee centers on house prices in the Netherlands. Using data from several Dutch provinces from 1997-1999, Theebe (2002) and Theebe & Eichholtz (2003) find no effect of RCs on house prices. In a more recent contribution and using data from 2009-2017, Daams et al. (2019) do find hat only prices of singe-family homes in rural areas decrease by about 5% within 0.5-1.0km of an RC. For RCs exceeding a capacity of 500 refugees, the effect doubles. The identification strategy of Daams et al. (2019) is based on a difference-in-differences approach using 76 housing sub-markets and spatial matching techniques. Hence, there seems to be some discrepancy between the results by Theebe (2002), Theebe & Eichholtz (2003) and Daams et al. (2019).

We improve on these papers in several ways. First, our sample period encompasses both Theebe (2002), Theebe & 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 is associated with a more negative WTP for RCs. Second, our methodological approach improves upon Theebe (2002), Theebe & Eichholtz (2003), Daams et al. (2019), and other similar studies. Instead of using matching on observables and classical 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. Instead, we use a *structural*

[7]Interestingly, a detailed study on the effects of RCs on actual crime rates shows that there were no more crimes in close vicinity to RCs in comparison to other areas in the Netherlands. There are, however, relatively more crimes being committed *within* RCs (Achbari & Leerkes 2017).

hedonic approach to measure preferences. This allows us to show that the willingness to pay varies considerably not only across areas, but also across households with different characteristics. The effect of RCs is, thus, certainly not confined to households living in single-family homes in rural areas, even though many RCs are placed in such areas.

This paper contributes to a small but growing literature studying the impact of refugees on the housing market. Tumen (2016), for example, uses variation in the recent inflow of Syrian immigrants into Turkey. The results show that refugee inflows increase the rents of higher quality housing units, while there is no effect on lower quality units. Recent evidence shows that Syrian refugees may lead to openings of firms and growth in gross profits in Turkey (Akgündüz et al. 2018). A paper by Dustmann et al. (2018) 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 coincides with a divide in attitudes to refugees between urban and rural populations. This very much relates to our findings, where foreign-born households appear to be less opposed to RCs. Moreover, we find that RCs in urban areas have less of an impact 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 & 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 US and Spain.[8] Using data from Spain, Moraga et al. (2019) find that immigrant inflows to existing, dense, neighborhoods causes natives to move but also increases real estate development. In suburban areas there were even no observed changes in segregation. Ottaviano & Peri (2006) argue that housing prices are higher in places with high inflows of immigrants. By contrast, using the same framework, Bakens et al. (2013) argue that the net amenity effect for the Netherlands is negative. More specifically, Bakens et al. (2018) show evidence for a trade-off in which access to ethnic restaurants (*i.e.* a positive external effect) partly compensates for the effect of the presence of immigrants (*i.e.* an effect related to attitudes)

[8] Saiz & Wachter (2011) further show that places with an increasing share of immigrants become less attractive to natives. They in turn show that neighborhoods with a high immigrant share are not becoming relatively less attractive because they are populated by immigrants per se, but because they are more likely to contain households with a low socio-economic status in terms of education or ethnicity (also see Cortes (2006)).

on house prices.

We further contribute to a large empirical hedonic pricing literature aiming to identify implicit prices of amenities. Omitted variable bias is a profound issue in hedonic price analyses, and researchers have met identification challenges with varying degrees of success. As mentioned, we use an arguably convincing identification strategy to identify the disamenity impacts of RCs. We specifically add to a growing literature on hedonic pricing aiming to identify households' preferences. To identify structural parameters in hedonic models, rather than only implicit prices, Rosen (1974) originally suggested a two-step procedure. However, it has been shown that preference parameters are only identified given arbitrary functional form assumptions, such as the assumption of homogeneous preferences (Brown & Rosen 1982, Ekeland et al. 2002). Ekeland et al. (2004), however, argue that the marginal willingness to pay is generically a non-linear function of household's characteristics and housing attributes. This non-linearity provides information that rules out collinearity between an endogenously-chosen characteristic and its marginal willingness to pay and enables identification of structural parameters in a single market, given the assumption that marginal utility is additive and, importantly, that unobserved attributes of individuals (*e.g.* ability, race) are independent of observed attributes of individuals. Bajari & Benkard (2005) and Bajari & Kahn (2005) 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 & Kahn (2005), who shows that housing preferences can be identified, even though the variable of interest is dichotomous.[9]

The remainder of this paper is structured as follows. In Section 2 we discuss the policy context regarding RCs. Section 3 introduces the data, followed by the research framework in Section 4. Section 5 reports the regression results. Section 6 provides a conclusion and discussion.

[9]Recent papers by Bishop & Timmins (2018) and Bishop & Timmins (2019) show alternative ways to estimate preferences. Bishop & 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 & 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.

2 International, European, and national policy on refugees

Directly after World War II there were many 'displaced' persons in Europe and around the globe. The term refugee was formally defined in the 1951 Refugee Convention. This treaty, originally signed by 144 states, only arranged the status of refugees as a result of events before 1951 and mainly focused on European refugees. In 1967, this treaty was broadened to include all persons. Currently, the treaty is ratified by 145 states.

Although the European Union has a long history in terms of migration policy (for an overview see, Obdeijn & Schrover 2009), the 1990 Dublin Convention marked one of the first attempts to improve the immigration process. The central aim was to reduce the number of multi-country applications by asylum seekers by having the application being processed by the first country of entry. It took until 1997 before the treaty was signed. One of the main problems of the treaty was that also in case of irregular entry (*e.g.* by land or sea) the first country of entry remains responsible for the application procedure. This has led to an unequal burden on some countries at the border of the European Union, which created so called 'hotspots' in Greece and Italy. Therefore, a reallocation scheme was implemented to redistribute the refugees across member states (European Commission 2015).

The 1951 Refugee Convention was ratified by the Dutch government in 1956. In 1965 this led to the Law on Foreigners (in Dutch: *Vreemdelingenwet*). The responsibility of taking care of the refugees was given to the municipalities. With the increase of the number of refugees (and financial burden) over time, the Dutch government decided to change the regulation. This regulation (in Dutch: *Regeling Opvang Asielzoekers*) led to the establishment of the first officially dedicated refugee centers in 1987.

As time progressed, the government bodies responsible for the asylum procedure became gradually more independent and eventually, in 1994, a law was passed that led to the creation of a separate government institute (*COA*) responsible for the processing of refugees including the opening of RCs. Due to privacy consideration it is not publicly known where refugees with a particular cultural background are housed. In addition, refugees cannot choose their own refugee center location. Besides regular RCs, there are a couple of specialized RCs. 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. Although the exact reasons why an RC is opened at a particular location is rather opaque and subject to negotiations with local municipalities, *COA* explicitly aims to distribute RCs evenly over the country and between different provinces.[10] Although the local population can protest and 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 (*i.e.* we will examine anticipation effects later on and do not find any).

The asylum application itself is evaluated by the Immigration and Naturalisation Service (INS) and the length of the procedure depends on the probability that an application is successful. By law, a decision should officially be made within 6 months but there have been quite a few cases known where the procedure has taken years, for example due to the possibility to appeal the decision (NRC 2018). In addition, RCs accommodate anyone who applies for asylum including, for example, economic migrants. Identifying whether asylum seekers are actually eligible for a residence permit can, therefore, be a daunting task. It is because of these reasons that asylum seekers can stay in RCs for years. 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).

The number of refugees that arrive in the Netherlands has varied considerably over time and there are a variety of underlying causes that explain the inflow of refugees. Figure 1 depicts the number of asylum seeker requests from 1990 until 2017 and shows several distinctive peaks. The peak 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 was also a considerable amount of Eritrean refugees (17%), fleeing because of political repression.

[10] 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 estimates we will show can be considered to be a conservative estimate.

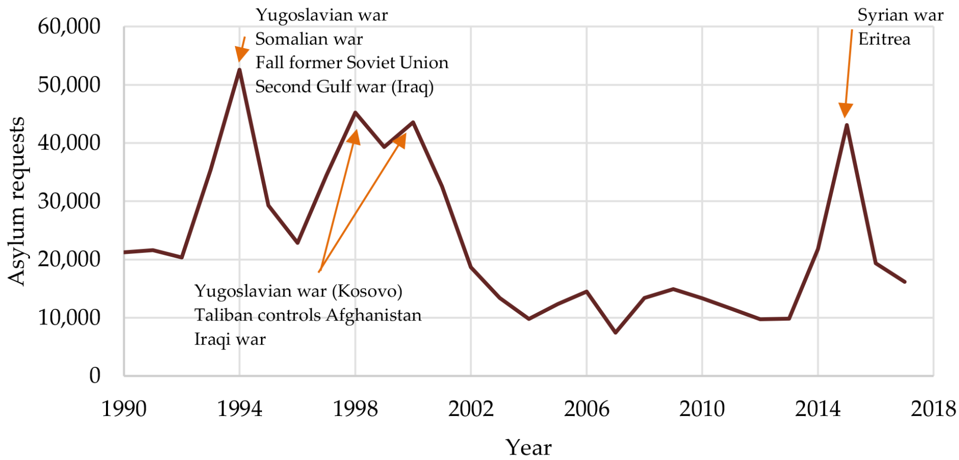


Figure 1 – Inflow of refugees in the Netherlands

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

According to Figure 1 the inflow of refugees and, consequently, the need for capacity to house those refugees is a recurring event and will most likely remain a political and societal challenge in the future.

3 Data and descriptive statistics

3.1 Data

Three main sources of data are combined: housing transactions data, household level data, and data on locations and openings of refugee centers. The housing transactions data are taken from the *Dutch Association of Realtors* and is provided by Brainbay. It covers the period 1990-2015, and captures about 60-70% of the market. The dataset contains information about the sales price, list price, time on market, and standard hedonic characteristics: house size, number of rooms, construction year, type of property (apartment, terraced, semi-detached, detached), presence of garage and garden, whether the property is well maintained, has a central heating system, and is listed as cultural heritage. We also know the exact location as well as the (6-digit) postcode, which covers about 15-20 addresses each. These will be used as location fixed effects in the regressions. The full dataset contains 2.6 million transactions.

The second dataset is from *Statistics Netherlands*. We use data from the *Sociaal Statistisch Bestand* (SSB), which provides basic information on demographic characteristics, such as age, country of birth, and gender.[11] 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 characteristic, 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 data to have information on characteristics of the buyer. The household level data is only available as of the year 2000. We use this data to examine the heterogeneity in the WTP related to household characteristics.

The third dataset contains the information on Dutch refugee centers. We distinguish between four categories: realized, planned, closed and canceled RCs. For all refugee centers we have the opening date (unless canceled), its capacity, its general type (process accommodation, central accommodation, and family accommodation), and we know whether the RC is realized in an existing or a new building. The data is taken from the website of COA, `www.coa.nl`, and contains all realized (permanent) refugee centers that were still open in 2015. There are 51 of such centers. We added to this sample 48 refugee centers from `www.nrc.nl` that were planned to be opened in 2016 and 2017.[12] Most of them (33 RCs) did not open eventually, we refer to those as 'canceled' RCs. We will use housing transactions nearby these RCs as one of our control groups based on the assumption that the same selection process has been used for opened RC versus planned and subsequently canceled ones. Yet, one may argue that canceling an RC is not necessarily an exogenous event. It may for example be related to severe protest against the planned opening of a new RC. Based on news (gathered online) about such protest surrounding RCs we could find that there was protest in relation to 24% of the canceled RCs against 21% of all RCs (including those planned) that were there in 2015. We will therefore also show results using other control groups. Finally, also based on online sources (*e.g.* news articles), we hand-collected 10 centers that were opened before 2015 but were closed before this date. This allows us to also examine the effect of closing of RCs.

[11] In contrast to countries like the United States or the United Kingdom, the Netherlands does not undertake censuses to register their population, but the register is constantly updated when people move or when household composition changes.

[12] 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 this data and complemented it using various internet sources.

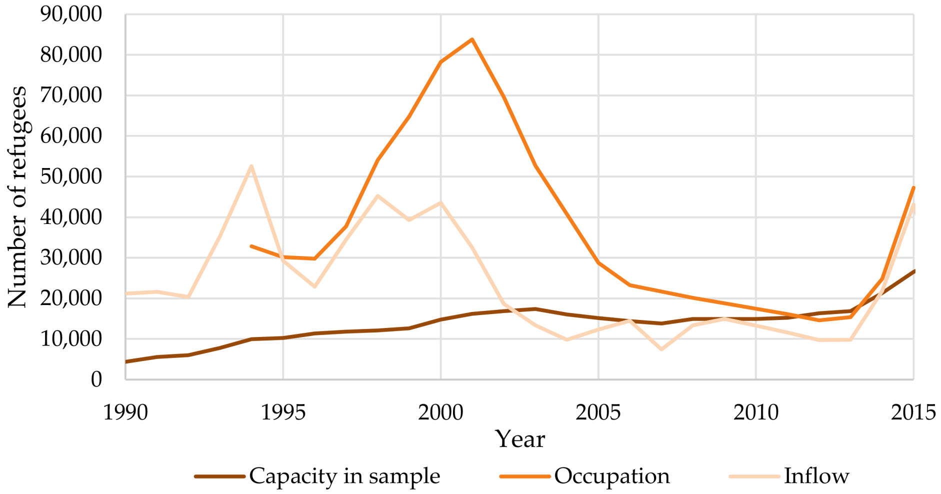


Figure 2 – RC capacity, occupation, and inflow

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

As mentioned, the exact number and allocation of asylum seekers per RC is not publicly known due to privacy considerations. However, we do know the aggregate capacity in our sample and the actual refugee inflows and occupation, which we show in Figure 2. The permanent RC capacity has increased gradually over time. 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 permanent RCs also seem to fairly reasonably capture the total amount of refugees.[13] 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 85-93% (see *e.g.* COA 2003, 2004). Hence, the RC buildings in our sample are not referring to just empty buildings.

There is anecdotal evidence that exposure to refugees is concentrated in corridors between the RC and the nearest shopping center, as the refugees walk trough these corridors to the shopping street to obtain clothes, food, and other items (Kuppens et al. 2017). 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 (100m wide) from the RC to the nearest shopping center

[13] As one may question the representativeness of our sample before 2005, because *e.g.* 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.

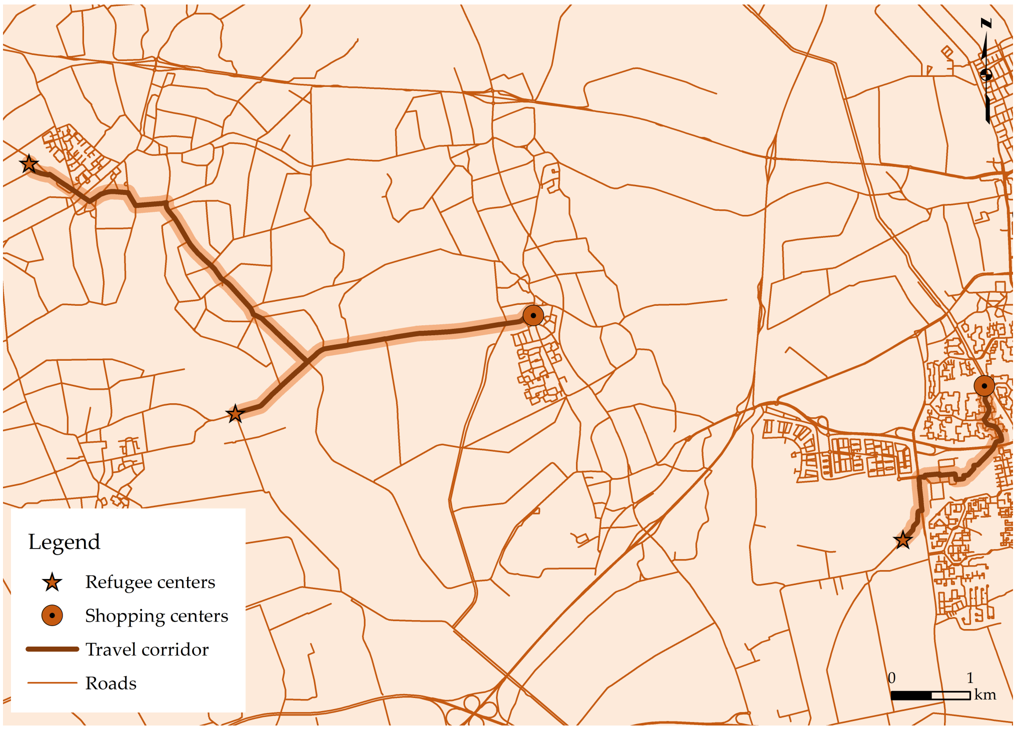


Figure 3 – Corridor between refugee centers and nearest shopping street

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

(consisting of more than 25 shops).[14] This is based on the existing road network in 2015. Figure 3 shows an example of such corridors. The average length of a corridor is 1.9km, 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, as a supplementary data source we use individual information about neighborhood satisfaction, willingness to move, feeling of unsafety, 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 empirical results based on the survey data in Appendix D.

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

Table 1 – Descriptive statistics: refugee center dataset

	Realized		*Planned*		*Closed*		*Canceled*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	mean	sd	mean	sd	mean	sd	mean	sd
Refugee center capacity	532.4	322.0	496.7	136.9	413.5	221.5	434.8	173.4
Year of opening refugee center	2005	10.35	2017	0.799	1996	6.620	—	—
Year of closure refugee center	—	—	—	—	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 refugee centers.

3.2 Descriptives

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 refugee centers has increased gradually over time although towards 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.

Figure 4 depicts the spatial allocation of RCs. They seem to be quite uniformly distributed across space. To check whether this is indeed the case, we compare the observed spatial distribution of realized, planned, canceled, and closed RCs with a randomly distributed sample using the Duranton & Overman (2005) measure for spatial concentration. This entails estimating Kernel densities for different distances and investigating whether they deviate significantly from a randomly generated spatial distribution (for a full description see Appendix B.1). The results of this measure are depicted in Figure 5. Given that the estimated K-densities fall well within the 95% confidence bands, this indeed suggests that the spatial distribution of refugee centers is close to random. This seems to be in line with the 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 2km of an RC is -0.344. Although the overall spatial distribution appears random, if we focus on the RC locations there

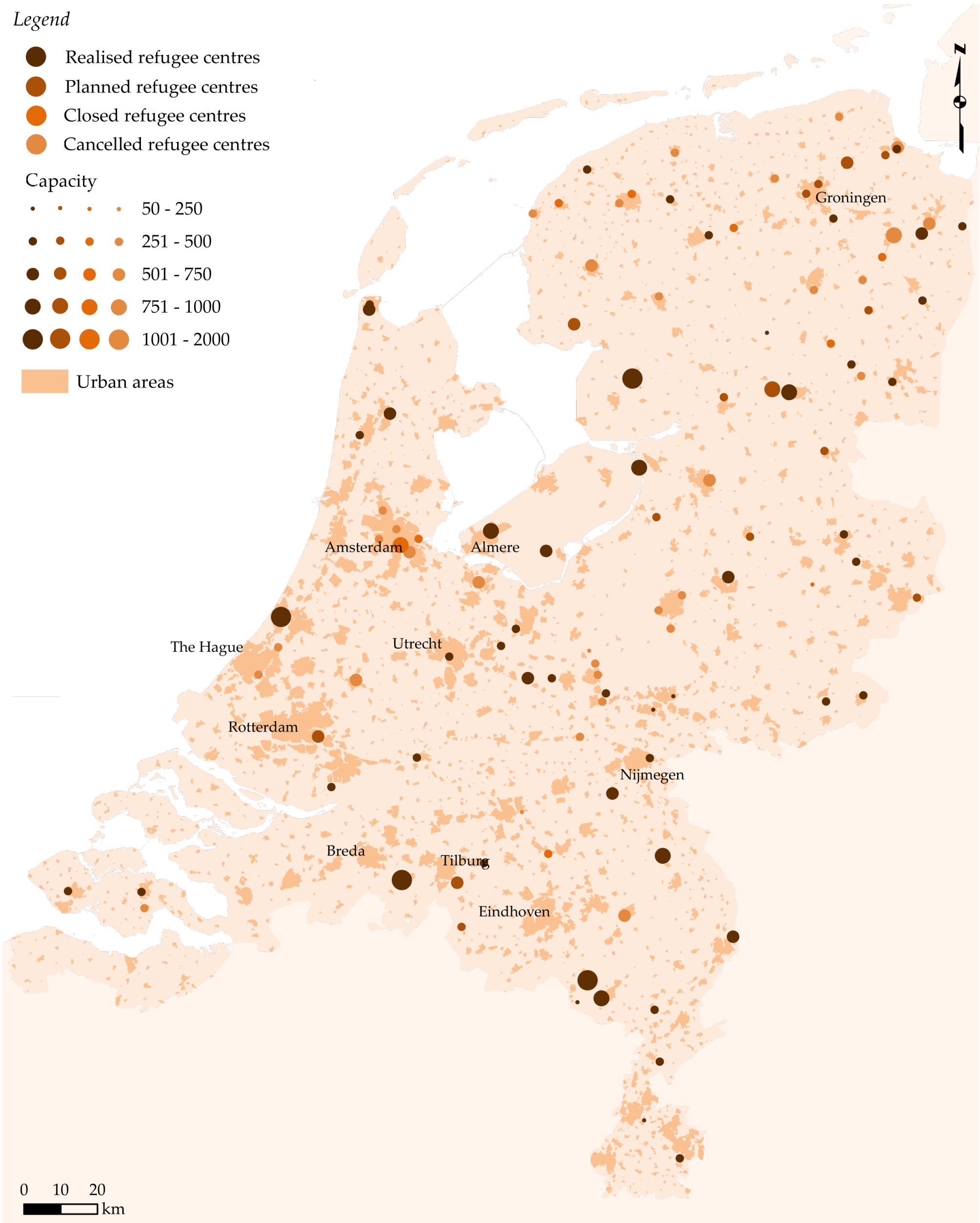


Figure 4 – Spatial distribution of refugee centers

Notes: This figure shows the location and size (capacity in persons) of refugee centers in the Netherlands. The refugee centers 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, those that were realized before 2015 and closed somewhere before this date, and centers that we planned to be build after 2015 but were canceled. For the first three categories we have the opening date which we use to measure the effect of refugee centers on house prices.

are some notable differences in terms of house prices and housing characteristics between different types of RCs in comparison to areas without RCs.

Table 2 contains the descriptive statistics for the house price dataset. The average house price is

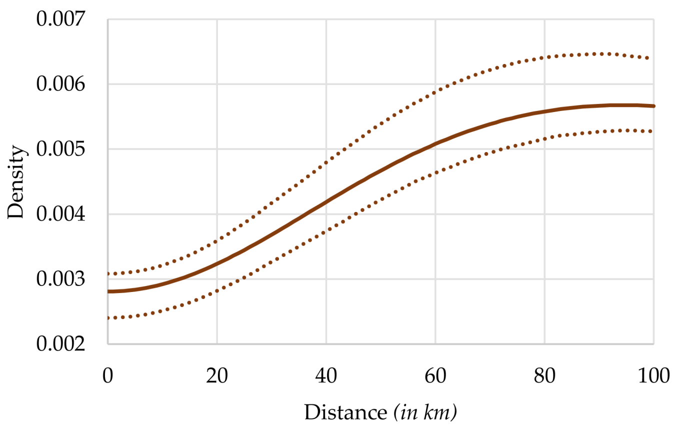


FIGURE 5 – SPATIAL CONCENTRATION OF REFUGEE CENTER LOCATIONS

Notes: This figure uses the Duranton & Overman (2005) methodology by examining whether the actual distribution of refugee center locations deviates from a randomly generated sample of refugee locations. For a detailed explanation, see Appendix B.1.

€203, 626 in the overall sample. Because we have the exact locations of the houses that have been sold and the refugee centers we can calculate for each property the distance to the nearest refugee center. Because the spatial extent of the effect of RCs on house prices is a priori unclear, we will start by using a 2km threshold to estimate the treatment effect. We will test whether there are no effects beyond 2km and we will also report effects when we assume the effect to be more local. Given a 2km threshold, the baseline treatment group consists of 116,310 houses and 148,553 transactions. About 2.8% of housing transactions (74,976 observations) are within a 2km radius of a refugee center *after* it has opened (see Table 2). Finally, only about 0.1% of the observations are inside corridors after the opening of an RC.

House prices and housing characteristics are somewhat different close to RCs. In Appendix A.1 the housing transactions dataset is split into four categories of refugee centers (realized, closed, planned, and canceled). Average house prices are highest in locations where refugee centers will be or are closed (€226, 333) and lowest where they are planned (€181, 549). Households close to realized refugee centers, however, show an average transaction price of €203, 030, which is very close to that in the full sample. Housing characteristics across the different categories also differ a bit. This highlights that it is potentially important to control for housing characteristics in the regression analyses.[15] In any case, our identification strategy will address any concerns

[15] Not surprisingly, given the size of the dataset, all of the differences in the means across the refugee center categories are statistically significant.

Table 2 – Descriptive statistics: house price dataset

	(1) mean	(2) st.dev.	(3) min	(4) max
Sales price *(€)*	203,626	114,657	25,000	1,000,000
List price *(€)*	216,367	124,536	22,916	1,400,000
Time on market *(days)*	135.1	185.7	0	1,825
Refugee center opened, <2km	0.0283	0.166	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	0.00606	0.0776	0	1

Notes: The number of observations is 2,649,070. The dataset also includes 6 construction decade indicators which we will use in the regression analysis. Apartments are the reference category for the type of house dummies. The corridors between RCs and shopping areas (> 25 shops) are 100m wide and based on the road network in 2015. The corridor indicator is one after a RC gets opened. The sample period is 1990-2015.

regarding potential non-random placement.

The descriptive statistics for the matched dataset, containing housing transactions and household characteristics, is reported in Table 3. We focus on observations within 2km of an RC. The average house price is somewhat higher than in the full sample (about 12.5%) because the data is 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 European Union.

4 Research framework

4.1 Reduced-form specification

4.1.1 Baseline specification

We first aim to estimate the treatment effect of refugee centers (RCs) on house prices. In this subsection we explain our identification strategy to measure implicit prices. In the following subsection we extend this framework by using a non-parametric hedonic price approach to

Table 3 – Descriptive statistics: matched dataset

	(1) mean	(2) sd	(3) min	(4) max
Sales price *(in €)*	228,837	118,179	32,000	1,000,000
RC opened <2km	0.433	0.496	0	1
Capacity of nearest RC	493.6	248.4	75	1,700
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	6,019	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 2km of a realised or planned RC. The dataset also includes 6 construction decade indicators which we will use in the regression analysis. The sample period is 2000-2015.

identify household preferences.

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 $\bar{d}$ km of the property. We aim to estimate:

$$\log P_{it} = \beta_1 \mathcal{RC}_{it} + \beta_2 X_{it} + \lambda_j + \lambda_t + \epsilon_{it}, \tag{1}$$

where X_{it} are a set of housing attributes (*e.g.* house size, construction year), λ_j are (6-digit) postcode fixed effects, λ_t are year and 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 refugee centers 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).

We initially start with $\bar{d} = 2$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 & Eichholtz 2003, Gibbons & Machin 2005, Davis 2011, Dröes & Koster 2016, Daams et al. 2019). Because the choice of 2km is arbitrary, *(i)* we will test whether the effect reaches beyond 2km 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.

With regard to equation (1), we are particularly interested in the parameter β_1 to examine whether there is a negative treatment effect on house prices. We estimate three different versions of equation (1). First, we estimate a standard difference-in-differences (DID) specification using all available data. Note that the treatment group dummy is absorbed by postcode fixed effects λ_j. The DID framework takes into account that RCs may potentially be opened in locations with lower house prices. An important assumption in a DID framework is the parallel trend assumption, which requires that in the absence of the treatment, the difference in prices between the treated and control observations is constant over time.

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. Therefore, in a second specification, we only include observations that are either within 2km of an actual RC (that is already opened or will be opened in the future) or within 2km of RCs that are planned to be opened after 2015 (in 2016, 2017) but were canceled. These latter areas should be comparable in terms of unobserved traits that are potentially correlated with the decision to open an RC.

Using this as control group may be problematic when RCs are canceled non-randomly (*e.g.* because of public opposition, less demand for RCs, or lack of space). Hence, we therefore employ a third approach were 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 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. This assumption is violated if the *timing* of the construction of RCs within eventually treated areas is non-random and occurs primarily in areas where prices are declining relatively. 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. 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 transitory or permanent effect on house prices *and* whether pre-existing trends are important. We do not find any evidence for such trends.

4.1.2 Revisiting identification

To the extent one is worried that unobservable trends may still bias our results, we consider an alternative identification strategy here. As an improvement on using uniform treatment areas, we consider effects of RCs to be restricted to corridors. As mentioned, the hypothesis is that the potential nuisance from RCs is concentrated in these corridors as the refugees walk trough these corridors to the nearest shopping district.

We define corridors using the shortest route from each RC to the nearest shopping center and we choose the corridors to be only 100m wide. Using this definition, we estimate:

$$\log P_{it} = \beta_0 \mathcal{RC}_{it \in \mathcal{C}} + \beta_2 X_{it} + \lambda_j + \lambda_t + \epsilon_{it}, \tag{2}$$

were $\mathcal{RC}_{it \in \mathcal{C}}$ now equals 1 if a transaction is within the corridor *and* within 2km of a realized RC. We use the same three control groups as in the previous analysis: the whole of the Netherlands, corridors near planned but canceled RCs, and areas in which an RC will be placed.

Although using different control groups might alleviate endogeneity concerns, the corridor analysis is still subject to the same possible concern of non-random timing of placement of RCs within treated areas, as the analysis based on a circular impact area. To mitigate this issue, we combine both approaches:

$$\log P_{it} = \beta_0 \mathcal{RC}_{it \in \mathcal{C}} + \beta_1 \mathcal{RC}_{it} + \beta_2 X_{it} + \lambda_j + \lambda_t + \epsilon_{it}, \tag{3}$$

In the above equation we estimate a triple-difference specification where we measure the change in prices in corridors within 2km of an opened RC conditional on being within 2km of an RC. Hence, β_0 now still captures the treatment effect. When the effect within 2km of an RC is partly capturing some local price trends, this is unlikely to be the case for the difference between the corridor and the other observations within 2km of an RC. Note that because the treatment effect may extend beyond the corridor, we may underestimate the treatment effect using this approach. Hence, this estimate can be interpreted as a lower bound estimate. To mitigate this issue we will exclude observations between 100m and 1km from the road towards the shopping center.

In the empirical analysis we also consider additional robustness checks and extensions. For example, we test whether closings have the opposite effect of openings, we obtain the treatment effect using repeat sales, 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.

4.2 Identifying preference parameters: a non-parametric hedonic price approach

We further explore the heterogeneity in the effect. A considerable literature on hedonic pricing focuses on recovering estimates for marginal changes in characteristics and estimates average effects for the population. It is possible to simply add interaction terms between $\mathcal{RC}_{it}$ and the characteristics of a refugee center (like its capacity), *as well as* household characteristics, and measure the additional effect on marginal prices.[16] However, because the number of potential interactions grow large very quickly, such an approach would suffer from the curse of dimensionality (Yatchew 2003). More importantly, interaction effects do not necessarily identify the underlying individual preferences as marginal prices and quantities are simultaneously determined (Ekeland et al. 2004). Hence, we go beyond estimating the average marginal effect and aim to recover household-specific willingness to pay (WTP) estimates for RCs. Following Bajari & Kahn (2005), we regress these WTP on characteristics of RCs and individual characteristics to explain the heterogeneity in WTP across households. We employ a two-step approach.

First, assume that the hedonic price function is given by:

$$P_{ijt} = \gamma_{1j}(W_{it}, X_{it})\mathcal{RC}_{it} + \gamma_{2j}(W_{it}, X_{it})X_{it} + \lambda_j + \mu_t + \epsilon_{it}. \tag{4}$$

The implicit prices γ_{1j} and γ_{2j} are flexible functions of RC attributes, W_{it} (such as RC capacity

[16] We show the results of a standard interaction effects model with only RC characteristics in Appendix C.1.

and whether it is new built), and housing attributes, X_{it}. To investigate how the willingness to pay for RCs relate to households' preferences, let us assume that the underlying utility function of household j occupying property i in year t is:

$$U(\mathcal{RC}_{it}, X_{it}, Z_{jt}) = \alpha_{0j} + \alpha_{1j}\mathcal{RC}_{it}W_{it} + \alpha_{2j}\mathcal{RC}_{it}Z_{jt} + \alpha_{3j}\mathcal{RC}_{it}X_{it} + f(Z_{jt}) + g(X_{it}) + C_{jt}, \quad (5)$$

where α_{0j} is a constant, α_{1j}, α_{2j} and α_{3j} are the preference parameters of interest, Z_{jt} are household characteristics (such as age and 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 (5) by $I_{jt} - P(\mathcal{RC}_{it}, X_{it})$.

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

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

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.[17]

4.3 Estimation of non-parametric hedonic price models

The first step to identify latent preferences for RCs is to estimate the non-parametric hedonic price function as per equation (4). We follow a similar approach as Bishop & Timmins (2018).

[17] 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 households 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.

We start with conditioning out the postcode and time fixed effects:

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

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 & Kahn 2005, for the unobserved housing attribute). We then use local linear regression techniques to estimate γ_{1j}:

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

where ℓ are (other) individuals, γ_{1j} are the parameters of interest, and $V_{i\ell t} = \left\{\frac{W_{it|\ell}-\bar{W}}{\sigma_W}, \frac{X_{it|\ell}-\bar{X}}{\sigma_X}\right\}$, where $k = 1, ..., \mathcal{K}$ are the number of variables to be included in the kernel function. 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 previous applied literature usually just picks a somewhat arbitrary value of around 3 (see Bajari & Kahn 2005, Bishop & 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 1$^{\text{st}}$ percentile and above the 99$^{\text{th}}$ percentile to mitigate the issue that the outcome is affected by outliers.

The second step of the estimation is to identify the preference parameters $\{\alpha_{1j}, \alpha_{2j}, \alpha_{3j}\}$ using the estimates of γ_{1j} and data on $\{W_{it}, X_{it}, Z_{jt}\}$. Given equation (5), we estimate:

$$\gamma^*_{1j} = \alpha_1 W_{it} + \alpha_2 Z_{jt} + \alpha_3 X_{it} + \mu_{jt}, \tag{11}$$

where γ^*_{1j} is the (latent) willingness to pay for an RC.

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:

$$\begin{aligned} \underline{\gamma}_{1j} &= \mathcal{RC}_{it}\hat{\gamma}_{1j} + (1 - \mathcal{RC}_{it})\min_j(\hat{\gamma}_{1j}), \\ \overline{\gamma}_{1j} &= \mathcal{RC}_{it}\max_j(\hat{\gamma}_{1j}) + (1 - \mathcal{RC}_{it})\hat{\gamma}_{1j}. \end{aligned} \tag{12}$$

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 (11) we use the following maximum likelihood function:

$$\begin{aligned} (\hat{\alpha}_0, \hat{\alpha}_1, \hat{\alpha}_2, \hat{\alpha}_3) = \underset{\alpha_0,\alpha_1,\alpha_2,\alpha_3}{\arg\max} \sum_{j=1}^{J} \log \Bigg(\Phi\left(\frac{\overline{\gamma}_{1j} - \alpha_0 - \alpha_1 W_{it} - \alpha_2 Z_{jt} - \alpha_3 X_{it}}{\sigma}\right) - \\ \Phi\left(\frac{\underline{\gamma}_{1j} - \alpha_0 - \alpha_1 W_{it} - \alpha_2 Z_{jt} - \alpha_3 X_{it}}{\sigma}\right)\Bigg). \end{aligned} \tag{13}$$

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

Bajari & 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 $\mu_{jt} \sim N(0, \sigma^2 J)$, 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.[18]

[18] As discussed earlier, we will also estimate a version of the model where we instrument for house size. In the

Table 4 – Baseline results
(Dependent variable: the log of house price)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Full sample	*Planned and canceled*	*Only RC locations*	*Event study*	*Distance profile*	*Within 1km*	*Within 750m*
Refugee center opened, <2km	-0.0303***	-0.0524***	-0.0599***	See Fig. 6	-0.0814***		
	(0.0077)	(0.0086)	(0.0089)		(0.0146)		
Refugee center opened, 2-5km					-0.0487		
					(0.0350)		
Refugee center opened, 5-10km					0.0152		
					(0.0147)		
Refugee center opened, <1km						-0.0620***	
						(0.0138)	
Refugee center opened, <750m							-0.0552***
							(0.0257)
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	2,640,378	318,193	194,436	194,436	194,436	52,838	28,101
R^2	0.92	0.93	0.93	0.93	0.93	0.94	0.93

Notes: The estimates are based on the Dutch association of realtors data between 1990-2015. Our preferred specification in column 3 only focusses on observations near refugee centers. Standard errors are clustered at the neighborhood level and in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

5 Regression results

5.1 *Baseline estimates*

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 a refugee center has, on average, an effect on house prices of $e^{-0.0303} - 1 = -3.0\%$. This effect is statistically significant at the 1% level. One may be worried that the placement of RCs is not random over space. That is, price developments in the rest of the Netherlands are different from those in areas near RCs, which is particularly so if there are unobserved differences between the RC locations and the rest of the Netherlands. In column (2), we therefore limit our sample to observations that are within 2km of an RC that has been opened or will be opened, as well as observations that are within 2km of a planned but canceled RC. The latter observations are used as control group. Using this specification, the effect is -5.1%.

first stage, we regress house size on individual characteristics. In the second stage, we include a control function of the first stage residual in equation (13) (Blundell & 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.

There is some evidence that RCs that are canceled 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 2km of an RC that has been opened (treatment group) or will be opened (control group). Hence, the identifying assumption is that the timing of openings of RCs is random. The results are reported in column (3). The coefficient indicates that prices decrease by -5.8% once an RC is opened.[19]

We examine whether the effect on house prices is transitory or permanent by undertaking an event study. We do not impose any structure on the temporal effects but allow for year-specific effects for our sample. The results of this specification are reported in Figure 6. The results show that at the moment of placement there is a very clear discrete negative jump in prices of -3.9%, while the effect is statistically insignificant in the years preceding treatment. Hence, we do no find (anticipation) effects before the opening of an RC. After a few years, the effect seems to become more negative (up to -8.9%, 10 years after opening of an RC), but the coefficients are also less precisely estimated albeit still statistically significant at the 5% level. Jointly, the null hypothesis that the treatment effect is constant is rejected although the associated F-value of 6.94 is not particularly large. Overall, the results suggest that the effect of the opening of an RC on house prices is permanent.[20]

Next, we investigate whether the treatment effect extends beyond 2km. Because distance to the nearest RC varies over time, we can also identify those effects. The results in column (5) seem to confirm our baseline estimate and shows that the effect decays over distance. Within 2km the effect is -7.8% which is highly statistically significant. At 2-5km the effect is still negative but no longer statistically significant. The point estimate is also considerably smaller than within 2km. Between 5-10km we find a relatively small positive coefficient that is, however, also not statistically significantly different from zero.

Finally, columns (6) and (7) show the average treatment effect at 1km and 750m, respectively. The effect within 1km is -6.0%, which is statistically significant at the 1% level. Using a 750m

[19] If we just include data after 2005, for which the RC sample captures the majority of the refugee flows, again see Figure 2, the effect is -5.1% and statistically significant at the 1% level.

[20] The effect 5 years before opening is -1.9% and just statistically significant. However, excluding the (small) sample of refugee centers that were opened and subsequently closed during the sample period this effect becomes negligible and statistically insignificant.

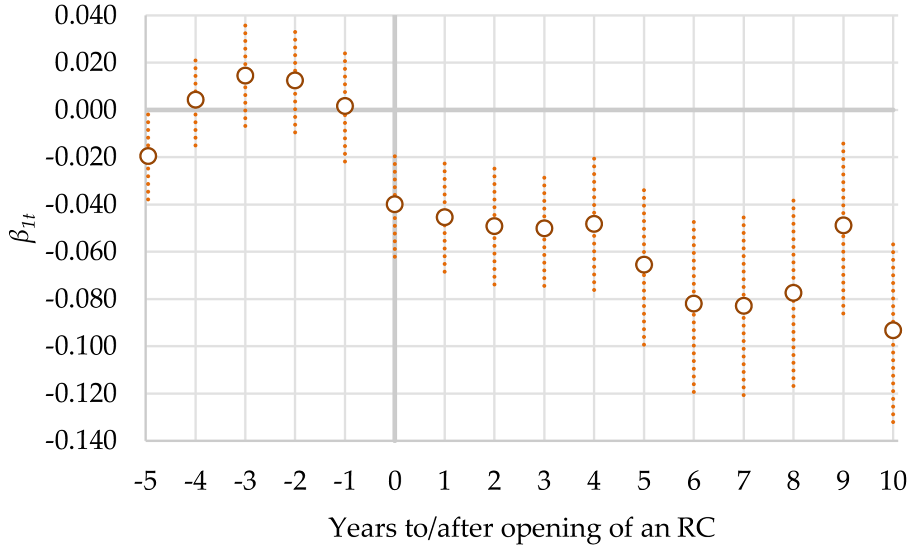


FIGURE 6 – EVENT STUDY

Notes: This figure re-estimates the specification reported in column (3), Table 4, but allows the effect of RCs to be dependent on the year to/after opening.

threshold we still find a statistically significant effect of -5.4%, even though there are only 28 thousand observations left.[21] These result 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 seems to be relatively robust.

5.2 *Extension: corridor analysis*

Here, we consider an alternative approach to measure the impact of RCs on house prices. We construct 100m wide corridors to the nearest shopping area based on the existing road network (see equation (2)). It seems more reasonable that the effect is located inside of those corridors and outside these corridors the effect might be due to RCs being opened in places that were declining in price for some other unobserved reason. We again estimate a specification including the whole of the Netherlands, with planned but canceled RCs as benchmark, and using the time variation in the opening dates of RCs only, just like in Table 4. Table 5 reports the results.

Based on the full sample we find a negative effect on house prices inside the corridor and within 2km of an RC of -5.6%. The second specification is in line with the preferred baseline estimate, albeit somewhat stronger. We think this points towards the fact that effects within corridors

[21] Choosing even lower thresholds (*e.g.* 500m) leads to imprecise results due to a lack of observations. Nevertheless, the estimated effect is still negative and in line with the results we present here.

TABLE 5 – REGRESSION RESULTS USING CORRIDORS TO THE NEAREST SHOPPING STREET
(Dependent variable: the log of house price)

	(1)	(2)	(3)	(4)	(5)	(6)
	Differences-in-differences			*Triple-differences*		
	Full sample	*Planned and canceled*	*Timing opening of RCs only*	*Full sample*	*Planned and canceled*	*Timing opening of RCs only*
RC opened (<2km)	-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)
RC opened (<2km)				-0.0228**	-0.0559***	-0.0643***
				(0.0100)	(0.0101)	(0.0104)
Housing characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Postcode fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year and month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	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	0.94

Notes: The corridors are between RCs and the nearest shopping center of at least 25 shops. For all specifications the treated observations are conditioned to be within 2km of an RC (so houses in corridors beyond that distance are not considered to be treated). In addition, between 100m and 1km 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.

may be stronger. We find a negative effect of -7.9%. Finally, using the variation in the timing of RCs only, the effect becomes -8.3% which is again somewhat stronger than our previously reported preferred estimate.

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 2km of a recently opened RC (see equation (3)). We report the results of this analysis in columns (4)-(6). If placement of RCs is correlated with declining prices due to unobserved reasons, assuming that this decline occurs in both the corridor and circle, we can consistently estimate the treatment effect by taking the difference between both. If there actually is an effect within 2km 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 2km 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 reported in columns (5) and (6). It seems very unlikely that just in the direction of shopping centers we pick up a spurious negative price trend. This strengthens our claim that the effect we find is causal.

5.3 *Further robustness checks and other extensions*

This subsection discusses several robustness checks and extensions, which are reported in Table 6. The specification in column (1) 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 only slightly smaller compared to the baseline estimate (−5.8%). Alternatively, we show in column (2) that, conditional on the RC being opened, the effect of closing an RC is *positive* (5.1%). This is of the same order of magnitude as the opening of an RC. This provides additional support that the estimates we provide measure a causal effect of RC openings/closings on house prices. It is highly unlikely that the timing of these events (opening and subsequent closing of an RC) exactly corresponds, for example, with some unobserved local policy intervention or unobserved economic shocks.

Next, 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 reported in column (3), Table 6, 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 (4) 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 (5), Table 6, 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.

Opening of RCs may also impact the liquidity in the housing market (see for evidence on amenities and time on the market, Koster & 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 (6) suggests that there is a 14% 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).

Column (7) 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 (7) suggest that the number of refugees does not seem to affect the implicit prices of the opening of a refugee center.

More generally, to study in more detail whether there has been a shift in attitudes regarding refugees, we examine whether the treatment effect systematically varies over time. We interacted the treatment dummy with 5-year period dummies. The results, stated in column (8), show that particularly at the end of the 1990s (−2.2%, not significant) and the beginning of the 2000s (−2.8%) the effect was less negative than the baseline estimate. This was a period when the Yugoslavian war occurred on the European mainland, with the Netherlands directly involved in the peacekeeping force, resulting in a large inflow of refugees. Apparently, the opening of refugee centers was less of an issue at that time.[22] After that, the effect seems to become more and more negative 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 LPF (founded in 2002) and PVV (founded in 2005). It may also relate to the increasing media coverage of the refugee crisis and (criminal) incidents where refugees were involved.[23]

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. This data is publicly available

[22] This is in line with the findings of Theebe (2002) and Theebe & Eichholtz (2003) who show that refugee centers had no effect on house prices during this period.

[23] 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).

Table 6 – Robustness and extensions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Opened RCs only	*Closings*	*Repeat sales*	*Time-varying coef.*	*Markup*	*Time on market*	*Number of refugees*	*Over time*	*Political vote*
Refugee center opened, <2km	-0.0520***		-0.0521***	-0.0299***	-0.0109***	0.1507***	-0.0596***		-0.0600***
	(0.0142)		(0.0099)	(0.0081)	(0.0023)	(0.0513)	(0.0090)		(0.0088)
Refugee center closed, <2km		0.0494***							
		(0.0155)							
$\text{RC}\times\big(\log(\text{refugees}) - \overline{\log(\text{refugees})}\big)$							0.0024		
							(0.0096)		
$\text{RC}\times D_{1990-1994}$								-0.0647**	
								(0.0265)	
$\text{RC}\times D_{1995-1999}$								-0.0218	
								(0.0171)	
$\text{RC}\times D_{2000-2004}$								-0.0287***	
								(0.0106)	
$\text{RC}\times D_{2005-2009}$								-0.0687***	
								(0.0135)	
$\text{RC}\times D_{2010-2015}$								-0.1096***	
								(0.0180)	
$\text{RC}\times\big(\text{share nationalist} - \overline{\text{share nationalist}}\big)$									-0.0045***
									(0.0008)
Housing characteristics	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Postcode fixed effects	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Year and month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	147,839	105,439	40,012	194,436	194,436	191,774	194,436	194,436	194,436
R^2	0.93	0.94	0.76	0.96	0.25	0.26	0.93	0.93	0.93

Notes: This table uses the data within 2km 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.

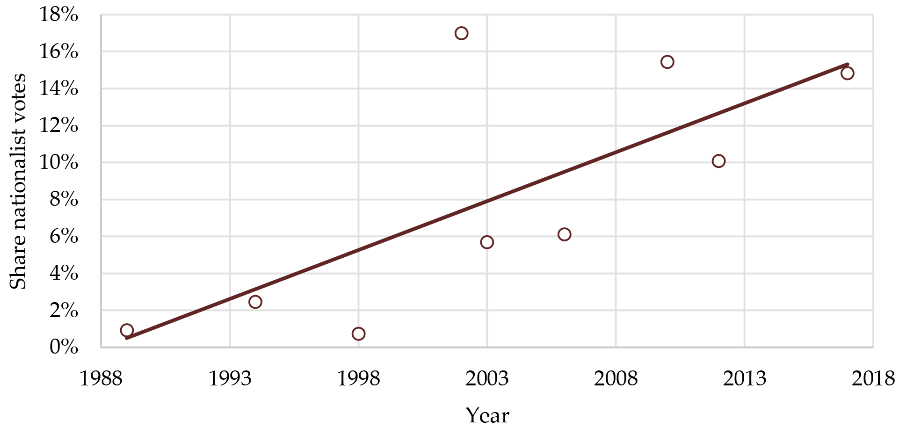


Figure 7 – Share of nationalist votes

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

per municipality from the electoral council. The share of nationalist votes comprises the votes of several parties (*i.e.* Centrum Democraten (CD), Lijst Pim Fortuyn (LPF), Partij Voor de Vrijheid (PVV)) that are considered to be anti-migrant. Figure 7 shows how the average share has increased over time. 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. Merging the political votes data to our transaction database, the local votes in national elections for nationalist parties in the merged dataset varies between 2.2% and 16%. The interaction effect is evaluated at the average share of nationalist votes in the elections data, which is 6.4%, and we use the share in the election *before* a specific RC is opened.

The results in column (9) show that the marginal effect of the opening of an RC for the average share of nationalist votes is 5.8%. For every percentage point increase in the share the marginal effect is 0.45 percentage points 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). Yet, in terms of house prices we do not observe strong anticipation effects (*i.e.* the process of opening RCs is relatively opaque in the Netherlands) and we use the lagged share of political votes. Nevertheless, changes in voting behavior could still just be a proxy for underlying characteristics (preferences) of households. We examine preferences in more detail in the next subsection. Keeping these caveats in mind, the result do seem to be in line with the idea that underlying attitudes towards refugees affect the extent to which house prices decline by the opening of RCs.

In general, the effect on house prices might reflect changes in subjective well-being. To further support our findings we also examined the effects of RCs on satisfaction, perception of safety and employment using data from the Dutch housing demand survey. The results of this supplementary analysis are discussed in Appendix D. 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 feel more unsafe and unemployment within the local neighborhood is not affected by the opening of an RC.

Finally, in Appendix C.1 we examine several extensions based on interaction effects between RC characteristics and the dummy indicating the opening of an RC. The results suggest that the treatment effect is more negative 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, and seems to be less negative for family RCs.

5.4 Results of non-parametric hedonic price models

In the previous analysis we do not identify the household demand function (WTP) but just differences in the marginal price for RCs. Instead, we aim to measure whether the WTP varies by RC attributes and household characteristics as to identify households' demand for RCs.

In Appendix C.2 we report the results for a linear hedonic price model. For the restricted sample (2000-2015, for which we have data on household characteristics) we find an average WTP of $-€16,020$, which is 7.0% of the average house price. This is very close to the estimated treatment effect of our specification listed in column (3) in Table 4.

Next, we estimate the non-parametric hedonic price model (see equation (7)), of which the estimated WTP parameters are reported in Figure 8. In what follows we exclude the estimates

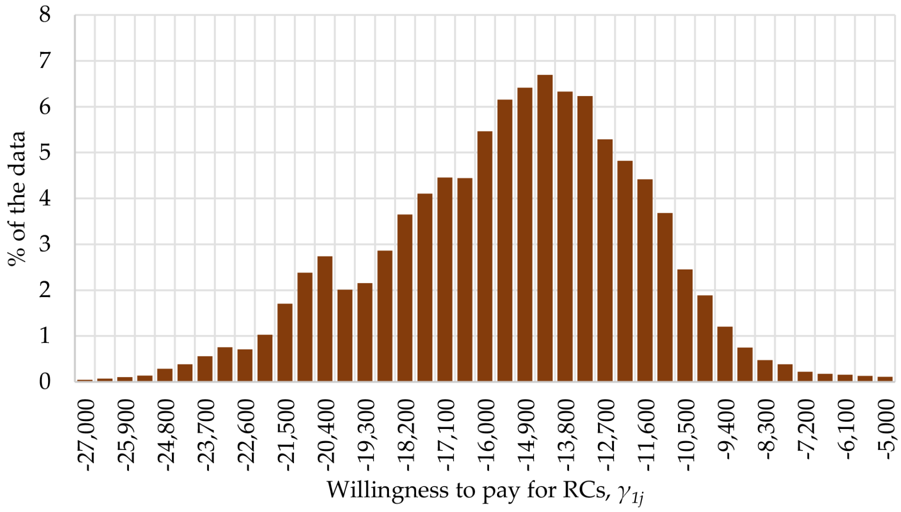


FIGURE 8 – WILLINGNESS TO PAY FOR REFUGEE CENTERS

Notes: We report here the willingness to pay for refugee centers based on the estimates of equation (7).

below the 1^{st} and above the 99^{nd} percentile to ensure that our results are not driven by outliers. We find an average WTP for RCs of $-€15,562$. The results further show that there is substantial heterogeneity in preferences to live nearby an RC. Almost all values are negative (99.6%), in line with expectations.

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 (13) to be able to recover the utility parameters $\{\alpha_{1j}, \alpha_{2j}, \alpha_{3j}\}$. In column (1) we explain the variation in WTP-parameters only by the main attributes of refugee centers. In line with expectations, we find that the effects are stronger for larger RCs. When the capacity of the nearest RC increases by 100 (about *one-third* of a standard deviation), the WTP decreases by €700, which is 4.5% relative to the mean WTP (€15,562). More specifically, for a small WTP of 50 refugees, the estimated WTP would be $-€350$, while for a large RC of $1,000$ refugees, the WTP would be $-€6998$. The relative capacity as compared to the population does not seem to have an effect over and above the effect of absolute capacity.

In column (2) we add household characteristics. The effect of opening an RC in a new building is now marginally statistically significant and $-€2288$, which implies that the effect is about 14.7% of the average WTP. We find weak evidence that households with a higher disposable income have a more negative WTP. For example, a standard deviation increase in income (about

TABLE 7 – EXPLAINING HETEROGENEITY IN THE WTP FOR RCS

(Dependent variable: the willingness to pay for refugee centers, $\hat{\gamma}^*_{1j}$)

	Maximum likelihood			*+ Control function*
	(1) ML	(2) ML	(3) ML	(4) ML-CF
RC capacity *(in 100s)*	-700***	-677***	-506**	-540**
	(246)	(245)	(241)	(245)
RC is newly built	-1,959	-2,288*	-2,167**	-2,129*
	(1,377)	(1,378)	(1,099)	(1,097)
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		-1,036	-513	-80
		(0,668)	(416)	(511)
Age $\geq$ 70		-1,868*	-31	273
		(1,031)	(917)	(956)
Non-western foreigner		1,280**	1,188**	1,030**
		(508)	(469)	(459)
Household size		426*	210	375*
		(230)	(160)	(201)
Household – couple		116	-276	-21
		(0,544)	(484)	(472)
Household – kids		1,224***	906***	1,086***
		(317)	(306)	(313)
Household – share male		148	58	91
		(198)	(187)	(189)
Housing attributes	No	No	Yes	Yes
Number of observations	57728	57728	57728	57728
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 2km of an RC. Bootstrapped standard errors are clustered at the neighborhood level and in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

€23,642) implies a decrease in the WTP of only €684, which is relatively small. We also find that foreign-born people do care less about the opening of an RC. Their willingness to pay is €1280 more positive (8.2% relative to the mean WTP). This makes sense: foreign-born people are more likely to be of a similar ethnicity or might have a refugee background themselves, which makes it likely that they are more favorable towards the opening of an RC nearby. We also find that families with kids are more favorable towards RCs. The effect is in the same order of magnitude.

Column (3) adds housing attributes as additional controls. We find that the previous results related to RC capacity, newly built RCs, income, family structure, and being foreign-born are robust, implying that interactions of the willingness to pay with housing attributes do not critically matter. Finally, we report the results where we allow for the potential endogeneity of house size in column (4). Since this is the most comprehensive specification, we consider this as the preferred second-stage estimates. 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 €1,382 (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. In line with our previous findings, we find that foreign-born have a €1,030 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 confirm that families with kids are more favorable towards RCs. Their WTP is around €1,086 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 when, for example, the shares of foreign born people and families are higher. The added value of our analysis from a policy point of view also becomes more clear when we use our results to examine in more detail where RCs should be opened.

5.5 Where to build RCs?

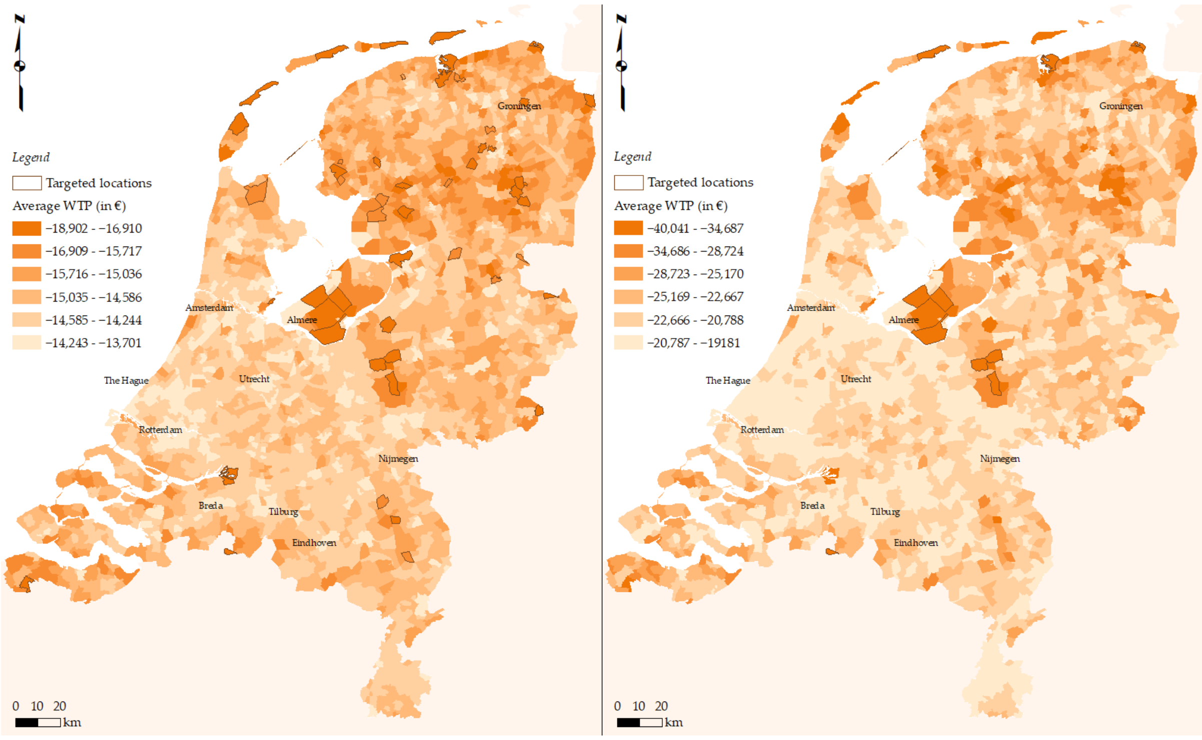
Given the preferred WTP estimates (see Table 7, column (4)) and the average demographics in an area we can determine what is the best location to open RCs. Let us consider an inflow of 12,500 additional refugees (which is about the standard deviation of asylum applications throughout the years). We consider 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 be opened in the centroid of neighborhoods. There are roughly 4000 neighborhoods in the Netherlands. We draw circles of 2km 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 the current practice.[24]

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 a 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, 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. We do not take into account 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 9. In Figure 9a 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. So, despite overall negative attitudes, opening RCs in sparsely populated areas seems to be preferred.

We also consider the alternative scenario where 10 new RCs will be opened with a capacity of 1250, see Figure 9b. The average WTP per location is much more negative than before 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

[24] Otherwise, because of household sorting, the resulting outcome would be that RCs are concentrated in only a few places. Note that for simplicity all neighborhoods are considered as potential location even if they already have an RC based on the current situation.



(a) RC capacity of 250 (b) RC capacity of 1250

Notes: We report the average willingness to pay 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.

Figure 9 – The WTP for RCs

negative average WTP values, the *total WTP* is still less negative in those areas.

In Figure 10 we vary the capacity of RCs and show the total WTP across all targeted RC locations for different capacity levels. Figure 10 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 dislike large RCs, it seems preferable to build a few large RCs in sparsely populated areas. Although it is contentious to let a few people carry a heavy burden, it is in line with current practices (*i.e.* recall the correlation of -0.344 between RC capacity and log population).

Yet, the government (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 placing RCs in locations with high shares of foreign-born people and families.

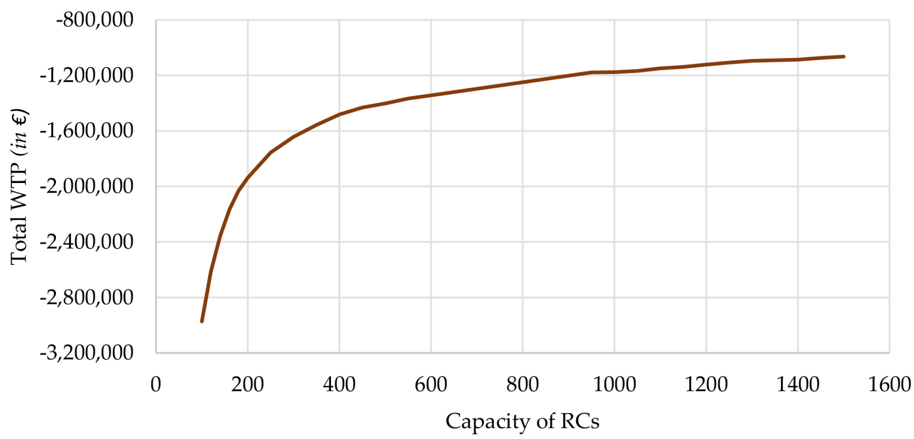


Figure 10 – Total willingness to pay for an inflow of 12,500 refugees

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.

It is clear that the policy maker's problem of where to build RCs is complex. Currently, COA seems to aim to spread RCs across the country and also state on their website that they want public transport to be nearby. Ultimately, the question is to what extent these and other factors are taken into account in COA's objective function. Although other considerations such as integration of refugees, economies of scale, fair distribution of RCs, etc., arguably also plays an important role in the decision process, our results imply that willingness to pay is an informative additional measure to guide policy decisions regarding placement of RCs.

6 Conclusion

The number of refugees around the world has increased steadily in the last decade to 25.9 million refugees in 2018 (UNHCR 2019). This has had a profound impact on many countries, regions and cities. Many of these refugees have to await their asylum procedure in dedicated refugee centers. In this paper we use data on refugee centers (RCs) opened in the Netherlands between 1987 and 2015 and house prices to measure the disamenity effect of RCs. This disamenity effect captures both negative externalities caused by RCs as well as attitudes of locals towards refugees. More specifically, using detailed house price data we examine how much households are willing to pay to avoid living near refugee centers.

The results show that house prices within 2km of a refugee center decline by about 3-6% after opening of a refugee center. Closing down an RC has a similar but positive effect. The effect on house prices is persistent over time and is present even 10 years after opening. The effect seems to be particularly negative towards the end of the sample period, corresponding to an increased popularity of nationalist parties. In line with this result, we find that the effect increases with the local share of nationalist votes. This implies that the effect not only captures a negative externality effect but possibly also incumbent households' attitudes towards refugees. Our results are robust to using a triple-differences strategy where we compare price changes in corridors to local shopping areas to price changes outside those corridors but within close distance of an RC. We back these results up by survey evidence on subjective well-being: we find that households have an increased intention to move, have a higher probability of experiencing neighborhood dissatisfaction, and experience more nuisance when RCs are opened.

We further estimate non-parametric hedonic price regressions to identify individual preferences regarding the opening of refugee centers. The mean WTP is about $-€16$ thousand but we show that there is considerable heterogeneity in the WTP. For example, the WTP is about $€1,400$ lower for a standard deviation (250 persons) increase in the capacity of a refugee center. Households have a less negative WTP when RCs are in existing buildings (about $€2,000$). Both foreign-born people and families have a less negative WTP of about $€1,000$, so they seem to be more tolerant towards the opening of RCs near their properties.

Our counterfactual analysis shows 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 in neighborhoods with higher shares of foreign-born people and families. Of course, the decision to open refugee centers relates to other factors than just households' preferences, such as general humanitarian concerns as well as possibilities for future integration. Although such considerations are important, we show that the disamenity effect of refugee centers can be reduced considerably by carefully choosing locations. Of course, an alternative policy could be to mitigate the disamenity effect by aiming to change the attitudes of incumbent households towards refugees and/or refugee centers. 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 towards refugees remains to be seen.

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Online Appendix A

A.1 Descriptives per refugee center category

Table A1 shows the descriptives for house price dataset, which we split between observations near RCs that were realized before 2015, those that were planned to be opened after 2015 (and had a reported opening date), those that were opened and closed before 2015, and those that were planned after 2015 but were canceled.

House prices are highest in locations were refugee centers will be or are closed (€$226,333$) and lowest were they are planned (€$181,549$). The realized refugee centers, however, show an average transaction price (€$203,030$), which is very close to that 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.

Unsurprisingly, given the size of the dataset, all of the differences in the means across the refugee center categories are statistically significant. In any case, our identification strategy will address any potential non-random placement.

Table A1 – Descriptive statistics: house price data per refugee center category

	Realized		*Planned*		*Closed*		*Canceled*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	mean	sd	mean	sd	mean	sd	mean	sd
Transaction price	203,030	110,218	181,549	100,808	226,333	129,554	207,923	120,347
Size in m^2	120.8	36.37	115.0	37.39	109.5	41.98	114.4	37.76
Number of rooms	4.446	1.310	4.254	1.299	3.964	1.470	4.296	1.321
Terraced property	0.347	0.476	0.293	0.455	0.216	0.411	0.314	0.464
Semi-detached property	0.317	0.465	0.255	0.436	0.199	0.399	0.249	0.432
Detached property	0.137	0.343	0.124	0.330	0.110	0.313	0.102	0.303
Property has garage	0.372	0.483	0.303	0.460	0.293	0.455	0.278	0.448
Property has garden	0.971	0.169	0.975	0.155	0.976	0.153	0.975	0.155
Maintenance state is good	0.868	0.338	0.864	0.343	0.876	0.330	0.858	0.349
Property has central heating	0.903	0.295	0.883	0.321	0.878	0.328	0.889	0.314
Property is (part of) listed building	0.00498	0.0704	0.00484	0.0694	0.0176	0.131	0.00569	0.0752
construction year 1945-1959	0.0718	0.258	0.0803	0.272	0.0454	0.208	0.0747	0.263
construction year 1960-1970	0.152	0.359	0.159	0.366	0.135	0.342	0.155	0.362
construction year 1971-1980	0.188	0.391	0.166	0.372	0.129	0.335	0.157	0.363
construction year 1981-1990	0.156	0.363	0.137	0.344	0.128	0.334	0.138	0.345
construction year 1991-2000	0.145	0.352	0.128	0.334	0.117	0.321	0.121	0.326
construction year > 2000	0.0908	0.287	0.0763	0.265	0.0805	0.272	0.0833	0.276

Notes: This table shows the descriptive statistics of the house price dataset split up across the four different categories of refugee centers. The number of observations for each category is 1,188,941, 335,731, 179,153, and 945,358, respectively.

Online Appendix B

B.1 Concentration of refugee centers

In this Appendix section 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 to test for the concentration of RCs, which exploits the fact that our data is continuous over space.[25] More specifically, we employ the method introduced by Duranton & Overman (2005, 2008). Their concentration index controls for overall agglomeration, is invariant to scale and aggregation and, importantly, provides an indication of statistical significance. Below, we briefly discuss the procedure. For more details, we refer to Duranton & Overman (2005, 2008).

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

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

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

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

Equation (B.2) implies that we use a normal density function. Following Duranton & Overman (2005, 2008) and Klier & 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.[26]

[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.

[26] There are almost an infinite ways to construct counterfactuals (such as using postcode locations or correct

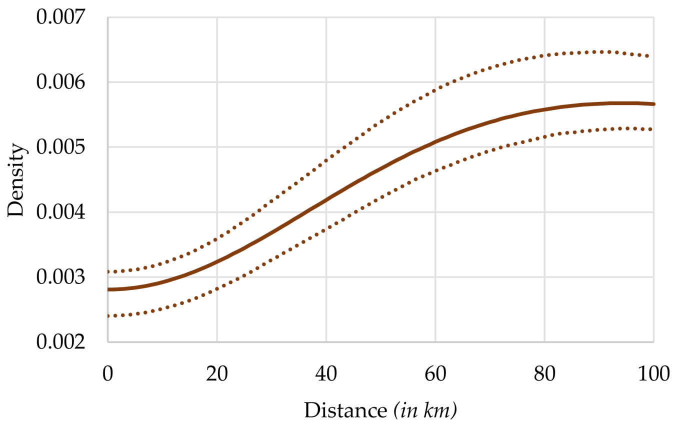

Figure B1 – K-density for all RCs

Notes: The dotted lines represent respectively the lower and upper 5% global confidence band.

To investigate whether there is 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 & 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 (B.1) for each distance below the median bilateral distances between RC location. In Figure B1 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.

One may argue that the canceling of RCs may have been non-random, *e.g.* 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 B2 shows that realized RCs are a bit more dispersed, but still fall within the confidence bands.

for density in drawing counterfactuals). The results of some of these exercises are available upon request.

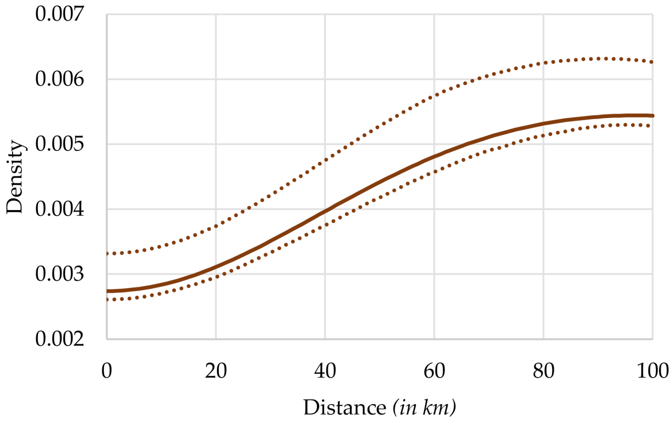

Figure B2 – K-density for realized RCs

Notes: The dotted lines represent respectively the lower and upper 5% global confidence band.

Online Appendix C

C.1 Heterogeneity in the effect: parametric estimation

Having established the average effect, we further explore heterogeneity in the effect using the baseline version of equation (1), as reported in Table 4, column (3). 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 center is located in a rural area (versus urban area), the capacity and relative (to the population) capacity, whether the refugee center is located in a new building, and the type of RC. The results are reported in Table C1. We focus the discussion on the final specification reported in column (6).

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 (also see Figure 4). 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 interaction term and it is marginally statistically significant. On top of the baseline effect of -2.2%, which is no longer statistically significant, there is an additional negative effect of -3.4% for rural areas.

Next, we added the interaction with a high capacity dummy (>500 refugees), which is approximately equal to the average capacity in the sample. We also include an interaction with a high (above median) relative capacity indicator variable. The relative capacity is the capacity relative to the population within 2km of the property. The results show that a high capacity is associated with an additional decrease in price of -3.0%. The high relative capacity indicator, however, is not statistically significant.

We also added the interaction effect with a dummy indicating whether a refugee center is located in a new building. 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 the area. The results indicate that the effect of RCs opened in newly built buildings is -4.5 percentage points more negative, relative to RCs opened in existing buildings. Finally, RCs that are dedicated for 'families only' have a less negative effect on house prices in comparison to central or processing RCs.

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	-0.0112
						(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.

C.2 Nonparametric estimation – first stage results

We report the results of linear models for our sample including households characteristics in Table C2. When we rely on OLS, we find in column (1) that the average willingness to pay for refugee centers 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.

In column (2) we estimate 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. Figure C1 shows that the Root Mean-Squared Error is minimized when the bandwidth equals 2.728. Given this bandwidth we obtain the mean estimate reported in column (2). Looking at the standard error of the estimate, we find evidence for considerable heterogeneity in the willingness to pay for refugee centers.

Table C2 – Linear and non-parametric models
(Dependent variable: the log of house price)

	(1)	(2)
	Linear model	*Local linear model*
Refugee center opened, <2km	-16,020***	-15,562***
	(5,403)	(6,998)
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 2km of an RC. Bootstrapped standard errors are clustered at the neighborhood level and in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

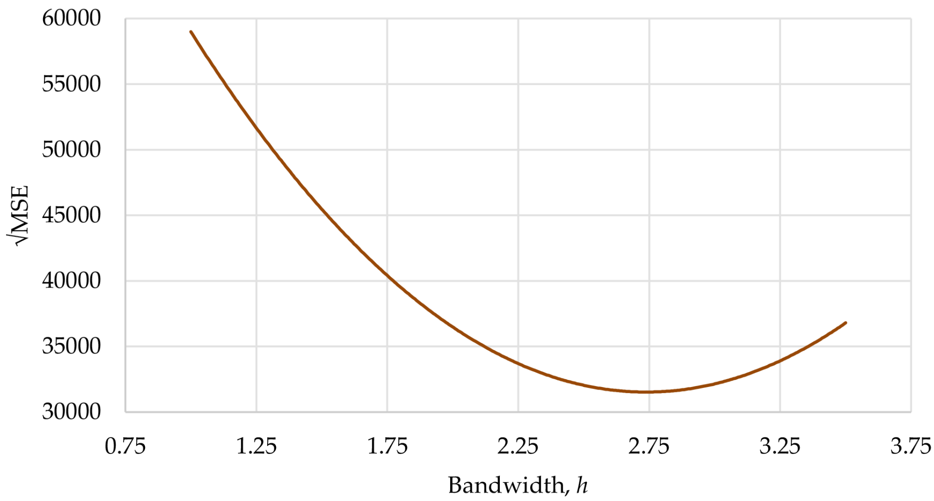

Figure C1 – Root mean-squared error for different bandwidths

Online Appendix D

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 Surveys (*WoON*) on (living) satisfaction, the intention to move (within two years), and more subjective indicators on nuisance and feelings of safety. 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% feels unsafe. The average employment is 94% and the head of the household works about 48 hours a week. In only 2% of all cases a refugee center has been opened within 2km. 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:

$$y_{rkt} = \tilde{\beta}\mathcal{RC}_{rkt} + \tilde{\gamma}x_{rkt} + \tilde{\lambda}_k + \tilde{\mu}_t + \tilde{\epsilon}_{rkt}, \tag{D.1}$$

where y_{rkt} is the dependent variable of interest (*e.g.* satisfaction, nuisance) of a respondent r living in neighborhood k in year t. We emphasize that we cannot track individual respondents over time, implying that we cannot include respondent fixed effects. Furthermore, $\mathcal{RC}_{rkt}$ equals

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

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
Unsafe	0.0799	0.271	0	1
Employed (works $\geq$12 hours per week)	0.944	0.230	0	1
Hours worked	47.84	15.46	1	60
Refugee center opened, <2km	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 is missing in one wave of the survey (2008-2009).

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 percent 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.

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 centre 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
Neighbourhood 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.: unsafe)*		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
Refugee centre 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
Neighbourhood 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 centre 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
Neighbourhood 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

Notes: Standard errors are clustered at the neighbourhood level and in parentheses; *** $p < 0.01$, ** $p < 0.5$, * $p < 0.10$.

There does not seem to be an increase in the feeling of unsafety, which is in line with the previous literature that does not find effects on local crime rates (see Achbari & 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 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 in line with the reported results of RCs on house prices.