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'Not in my Backyard!' The 2015 Refugee Crisis in Germany

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This paper exploits the sudden mass arrival of refugees to Germany in 2015 to study potential price penalties suffered by residential property in vicinity of refugee reception centers (RRCs). Using novel data on exact locations of publicly-run RRCs in 2014 and 2015 and monthly offers of single-family homes for sale from Germany's leading online property broker *ImmobilienScout24*, we find strong evidence in spatial DiD regressions for a sizeable negative effect on house price growth in proximity to such sites. Detached and semi-detached houses located within a 15-minute walking distance of RRCs exhibit, on average, 13 percentage points lower price growth than comparable dwellings beyond this threshold. We corroborate our finding in a battery of robustness tests and additional explorations, including sample restrictions that consider exclusively property on offer for sale within 40 minutes walking distance to RRCs and exogenous variation in the exposure to such sites. *'Not in my backyard' (NIMBY)* stances among the resident population may explain our finding.

Keywords: refugee immigration, refugee housing, house prices, Germany
JEL Classification: R23, R31, R38

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1 Introduction

The refugee crisis of 2015, fueled in particular by the conflict in the Syrian Arab Republic, sparked a momentous event in European history with potentially large and multifaceted consequences for local economies in major refugee recipient countries (Tumen, 2016). In 2015, Germany witnessed an abrupt and sizeable inflow of around 900 thousand refugees predominantly towards the end of that year, with monthly arrivals peaking throughout September till December.[1] This inflow was unforeseen and the largest refugee inflow the country has witnessed since the early 1990s, providing various challenges for state and local authorities, particulary immediate and sizeable logistical problems in providing sufficient, suitable as well as affordable accommodation to house these arrivals. In late 2015, capacities of existent refugee reception centers (RRCs) were rapidly exhausted, which necessitated the acquisition and opening of several new outlets and affiliated branches thereof. Nevertheless, authorities at federal-state level took quite diverse approaches in housing these new arrivals in centralized or decentralized accommodations.[2]

In the heyday of the crisis, this challenging situation created concerns about potential ghettoization, communicated both in the early political debate and in (social) media (Kather, 2015). Others saw a risk that socially deprived areas might develop (van Suntum, 2015) and that refugee group quarters might adversely affect the value of privately owned property in their immediate vicinity. There is some anecdotal evidence suggesting that local residents indeed perceived the opening of a new refugee camp in their immediate neighborhood as a disamenity that could adversely affect the value of their residential property (FOCUS, 2015b; Stoldt, 2014). In addition, Liebe et al. (2018) provide evidence that is consistent with this view in a repeated choice experiment to study the attitudes of Germans towards refugee shelters in their neighborhoods. The authors find that the majority of Germans rather disapproved of refugee accommodations in the immediate vicinity of their neighborhoods and that this perception persisted over twelve months since November 2015, i.e. the time which saw the internationally much-lauded "culture of welcoming" (*Willkommenskultur*) of the German public at its peak, a culture that received much coverage in the media and that was actively promoted by politicians (Dewast and Chaturvedi, 2015; Sueddeutsche Zeitung, 2015). The study also revealed that about one-fifth of the surveyed population, which had formerly rather approved of the establishment of refugee accommodations in their vicinity, no longer did so in 2016. According to Liebe et al. (2018), this change in attitude may, at least in part, be attributable to growth in *'not in my backyard' (NIMBY)* perceptions (Dear, 1992) among the surveyed population.

However, little is yet known on the actual scale of any price penalties for residential property (houses on offer for sale) caused by the potential shock to real estate markets from the 2015 inflow of refugees to Germany. Kürschner Rauck and Kvasnicka (2018) find that the mass exodus of refugees to Germany in 2015 adversely affected rental price growth of residential apartments (at county level) in major destination regions. Price growth penalties, however, turned out partly attenuated if a larger share of refugees was housed in decentralized as opposed to centralized accommodations in the heyday of the crisis. The authors also provide evidence which suggests that local residents have perceived large scale refugee settlements in vicinity of their property (at municipality level) as a disamenity, in particular when such settlements took the form of large scale group quarter accommodations, such as RRCs. The study by Kürschner Rauck and Kvasnicka (2018), however, is confined in focus to a segment of the property market (apartments for rent) that may have been faced also with demand-side driven price increases, caused by the rapid acquisition of additional property by authorities that needed to house the arrivals. Such increases in demand may have counteracted price penalties caused by disamenity perceptions of refugee settlements. In fact, associated concerns about supply shortages of rental property for both natives and refugees in certain localities, as well as the fear that residents might get evicted

[1] We consider as 'refugees' all individuals who arrived in Germany in 2015 and registered informally as seeking protection with the intend to lodge a formal asylum application, irrespective of their later residence status and the outcome of their asylum application. We hence, and henceforth, use the terms 'refugee' and 'asylum seeker' interchangeably.

[2] Centralized refugee accommodations comprise RRCs and other types of shared housing (i.e. general group quarters), and decentralized refugee accommodations comprise non-shared dwellings, in particular individual flats (Destatis, 2016).

from apartments, such that landlords could reap the often disproportionate rents offered by public authorities, became a vital part in the public debate at the time (FOCUS, 2015a). Further empirical evidence is hence required to better understand the nature of potential *'not in my backyard'* perceptions of natives and their role for real estate market outcomes. For property other than rental apartments, however, such crowding-out effects stemming from direct demand by local authorities and refugee migrants themselves are of no, or only minor concern. In particular, demand for detached and semi-detached single-family homes for sale should not have received a direct stimulus from refugee immigration, at least in the heyday of the 2015 crisis. Analyses of offer prices for such property would hence aid the identification and the detailed study of local demands for segregation and *NIMBY* perceptions that make refugee settlements a disamenity harming property price growth. However, no statistical evidence exists yet on the magnitude of such potential price penalties.

In this paper, we provide first evidence on the price penalties suffered by such property for single-owner occupancy offered for sale in immediate vicinity of RRCs during the 2015 European Refugee Crisis in Germany. We use a spatial difference-in-differences framework and detailed information on exact locations of publicly-run RRCs in 2014 and 2015, provided for this research project by state authorities of the federal states of Baden-Wuerttemberg, Rhineland-Palatinate and Mecklenburg-Western Pomerania, and monthly geo-referenced individual data on houses for sale between January 2012 and September 2016, provided by Germany's leading online property broker *ImmobilienScout24*. Our findings show that houses located within 15 minutes walking distance of RRCs exhibited an average 13 percentage points lower price growth between the pre- and (post-)crisis period than property located beyond this distance threshold. A battery of robustness tests, including sample restrictions that confine the focus to property on offer for sale within 40 minutes walking distance to RRCs, corroborates the validity of this result.

The 2015 refugee inflow to Germany was unexpected, abrupt, and large in size, which limits the scope and scale of confounding factors that might threaten identification. Nevertheless, we aim to address in additional explorations any remaining concern about identification and potential anticipation effects. First, we restrict the estimation sample to soley new RRC openings in the wake of the crisis. In further analyses, we exploit for identification exclusively variation in exposure to RRCs that is arguably exogenous to (prospective) local residents' preferences by considering only property located in counties adjacent to counties that housed RRCs, which eliminates the possibility that local lobbyists' in treatment, respectively control regions, exerted an influence on the decision of county-level officiating bodies where exactly to place new RRCs within administrative county regions. These explorations corroborate our finding of a negative effect on house price growth in vicinity of RRCs, which adds to and complements related findings on refugee centers in the Netherlands (Daams et al., 2019; Dröes and Koster, 2019), refugee shelter opening announcements in the city of Gothenburg in Sweden in the wake of the 2015 European Refugee Crisis (van Vuuren et al., 2019), and associated findings of a negative impact of the scale of refugee immigration in late 2015 on rental price growth for residential housing (at county level) in Germany (Kürschner Rauck and Kvasnicka, 2018). These findings may all be a reflection of *NIMBYism* related to regional differences in natives' perceptions of potential adverse externalities associated with large scale refugee settlements, particularly if they are concentrated, and they bear implications for the design of public policies in handling future receptions of refugees, i.e. the spatial dispersion of appropriate shelters for the housing of arrivals already at early stages of the asylum process.

The paper proceeds as follows. Section 2 reviews the existing literature on (forced) migration and housing markets. It also provides background information on the mass inflow of refugees to Germany in 2015 and information on RRC openings in the three federal states studied in the analysis. Section 3 describes the different data sources we use in the main analysis and outlines our empirical strategy. Section 4 presents and discusses our main findings, probes their robustness in several ways, and considers some additional explorations. Finally, Section 5 summarizes our main findings and discusses potential policy implications.

2 Background

2.1 Previous Research

There is a growing body of literature that is of broader relevance for this paper. Research on the consequences of (regular) immigration for real estate markets has started about two decades ago and is proliferating rapidly.[3] Most of the empirical evidence produced suggests that immigration (its scale) tends to increase house prices, respectively housing rents, in migrant recipient regions (see, amongst others, Saiz (2007) for the U.S., and Gonzalez and Ortega (2013) and Degen and Fischer (2017) for Europe). There is also some evidence, however, that immigration and property prices may be negatively related, or not correlated at all, depending on the price segment or level of aggregation one considers (see Sá (2014) and Braakmann (2019) for evidence on the UK, Saiz and Wachter (2011) for evidence on the U.S., and Accetturo et al. (2014) for evidence on Italy). According to these studies, negative impacts can materialize due to mobility responses of natives related to immigration and associated changes to local neighborhoods, respectively changes in local amenities.

Empirical research on irregular migration phenomena also produced evidence of positive effects at times, but in the majority found negative effects of immigration on property prices. The latter group largely consists of studies of refugee migration related to the Syrian refugee crisis, whereas the former considers also two distinct major migration episodes which occurred in the early 1990s, i.e. the Mariel boatlift to Miami (Saiz, 2003) and German internal migration following the reunification of the country (Kürschner, 2016). Research on the recent mass inflow of Syrian refugees to Turkey also found positive price effects, particularly for rental property in the upper price segment and property located in native-dominated residential areas, which may be attributable to an increase in natives' demand for residential segregation with respect to ethnicity (Tumen, 2016; Balkan et al., 2018). Studies that have found negative price effects of forced migration for residential property explored the link between (general) refugee migration over the years 2004 to 2015 and UK house prices (Lastrapes and Lebesmuehlbacher, 2018), price penalties suffered by Dutch property in vicinity to asylum seeker reception centers over the years 2009 to 2017 (Daams et al., 2019) as well as 1990 to 2015 (Dröes and Koster, 2019), and refugee migration fuelled by the Syrian refugee crisis and perceived housing quality and predicted rents in Jordan (Alhawarin et al., 2018) or apartment prices (van Vuuren et al., 2019) and housing rents (Kürschner Rauck and Kvasnicka, 2018) in major European refugee recipient countries.

Amongst the inquisitions into the consequences of the 2015 European Refugee Crisis, Kürschner Rauck and Kvasnicka (2018) provide evidence for a negative effect of refugee immigration on housing rents (at county level) for Germany, the largest refugee recipient country in absolute terms in the EU (Daldrup, 2016). They find a one percentage point increase in the 2015 end-of-year county-level refugee stock (normalized by the 2014 county population) to be associated with a 0.38 percentage points lower average rental price growth in the last quarter of 2015, and a 0.75 percentage points lower average rental price growth in the first quarter of 2016. The authors also find evidence that adverse price effects were attenuated in the heyday of the crisis (in late 2015) if a larger share of refugees was accommodated in decentralized housing, rather than in centralized facilities. Additional explorations into effect heterogeneity of county-level refugee immigration by local exposure to refugee populations and different types of refugee accommodation (at municipality level) suggest that demand for residential segregation by natives and perceptions of large-scale refugee settlements as (a source of) disamenity may sign responsible for these findings. The study by van Vuuren et al. (2019) for the city of Gothenburg in Sweden also finds sizeable price penalties of around 4% for apartments for sale within 5 minutes walking distance to building sites for RRCs which were announced in the heyday of the crisis. According to the authors, the negative impact is largely driven by dwellings with high priced features, increases in migrant density and education level and declines in income at neighborhood level, and it materialized immediately when building permits for RRC sites were issued.

[3] See Kürschner Rauck and Kvasnicka (2018) for an elaborate overview.

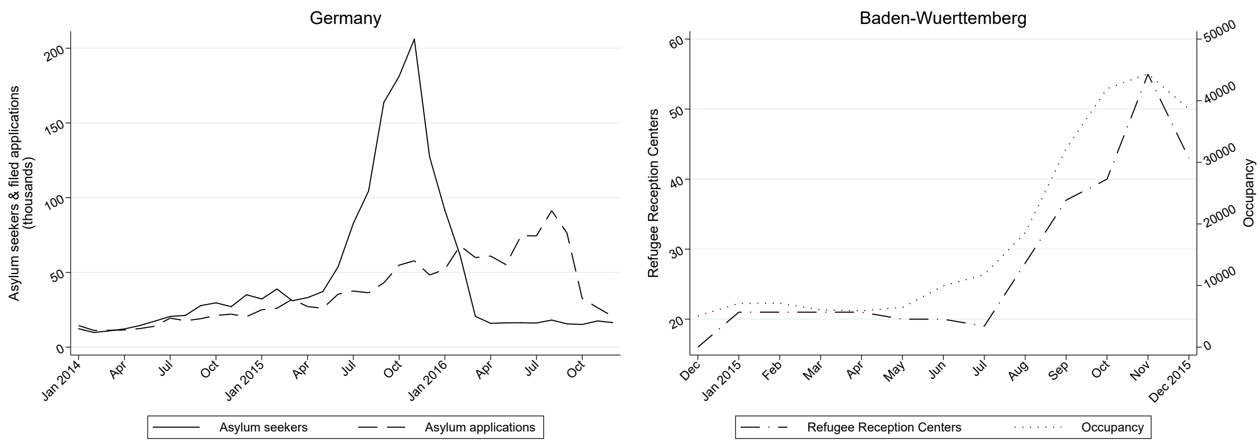
The two other studies that are closely related to the present paper, Daams et al. (2019) and Dröes and Koster (2019), also find sizeable negative price effects for houses located in proximity to refugee centers in the Netherlands, but cover time spans that start well before the 2015 European Refugee Crisis. Employing data on housing transactions over the years 1990 to 2015, Dröes and Koster (2019) provide evidence for a 3 to 6% decline in house prices of property located within two kilometers of refugee centers. Utilizing additional information on home buyers' characteristics, the authors furthermore show that negative attitudes towards refugee centers tend to be attenuated for foreign-born households, and elevated for high income households. There is also evidence that households' willingness to pay for avoiding to live near refugee centers increased over time and was largest in absolute terms (-10.4%) throughout the last five years of the observation period, which may be a result of changes in local buyers' attitudes towards asylum seekers that went hand-in-hand with the increasing popularity of Dutch nationalist political parties and media coverage of the European refugee crisis as well as criminal incidents involving refugees (Dröes and Koster, 2019). This finding is further supported by Daams et al. (2019) who find that prices of non-urban single-family houses located within 0.5 to one kilometer air distance to asylum seeker reception centers fell by 5.2%, respectively 9.3% nearby facilities with a hosting capacity of 500 spaces or more, over the years 2009 to 2017.

Altogether, these results and associated findings in van Vuuren et al. (2019) suggest that natives do indeed perceive the placement of a RRC in their immediate neighborhood as a local disamenity and that such perceptions may have been fuelled by changes in public attitudes towards the reception of asylum seekers, in particular in the wake of the recent refugee crisis in Europe. Perceptions of the establishment of RRCs in one own's neighborhood, i.e. in one's 'backyard', but not elsewhere as a disamenity, commonly referred to as *'not in my backyard' (NIMBY)* syndrome (Dear, 1992), may result in price penalties for residential property located in vicinity of RRC sites. Evidence for the NIMBY syndrome has been found in different contexts, such as the announcement and construction of garbage incinerators (Kiel and McClain, 1995), and the announcement of building sites for sports stadia (Dehring et al., 2007). The fact that Kürschner Rauck and Kvasnicka (2018) find a negative effect of refugee immigration on residential rental price growth, i.e. price penalties for a property segment that faced also stimuli for demand-side driven price increases at the time, at an even higher level of spatial aggregation (at the county level) than the one considered in the three pertinent studies reviewed above, suggests that sizeable price growth penalties may have materialized also for single-family houses on offer for sale in vicinity of RRCs in Germany. This paper contributes to the strand of literature summarized above by providing first empirical evidence on the existence and magnitude of such potential price penalties caused by the 2015 refugee crisis.

2.2 Germany and the 2015 European Refugee Crisis

"[T]he world's largest humanitarian crisis since World War II" (ECHO, 2015), soon coined the 2015 European Refugee Crisis, was triggered by a mass exodus of migrants from the Middle East and the Islamic World who fled from political instabilities, armed conflicts, state repressions, and the outbreak of the civil war in the Syrian Arab Republic in 2011 and ISIS's proclamation of the caliphate in the region (Keijzer and Schraven, 2015; Middelhoff, 2015). Much of this mass migration was heading for Europe both seaborne and overland, a movement that gained momentum when German chancellor Angela Merkel, faced with large numbers of refugees stranded at the Hungarian border, declared "[w]e will make it!" (*Wir schaffen das!*) (Federal Government, 2015b) in a press conference on 31st August, which was followed on 4th September by her announcement that there were "no limits on the number of asylum seekers" (The Irish Times, 2015) the country would admit, which de facto opened the border to those stranded migrants. Immediately after, Germany found itself in an exceptional and precarious situation, as monthly arrivals of asylum seekers jumped up dramatically (as shown by the solid line in the left graph in FIGURE 1) in the last four months of 2015, a mass inflow that came to a halt only after the *Balkan Route* was closed in March 2016 (European Council, 2016; BMI,

FIGURE 1: MONTHLY ARRIVALS OF ASYLUM SEEKERS AND ASYLUM APPLICATIONS (GERMANY), REFUGEE RECEPTION CENTERS AND RRC OCCUPANCY (BADEN-WUERTTEMBERG)



NOTE: Left graph: The solid line depicts the total number of monthly arrivals of asylum seekers to Germany in the period January 2014 to December 2016 as recorded by the EASY initial registry IT-system (BMI, 2016a,b, 2017; bpb, 2018), i.e. the number of informal registrations of individuals who intended to file an asylum application in Germany; the dashed line depicts the corresponding total number of actual asylum applications filed in Germany in this period, i.e. the total number of formal registrations of individuals seeking asylum in Germany (BAMF, 2015, 2016a, 2017). Right graph: The dash-dot line depicts the total number of refugee reception centers (RRCs) in the federal state of Baden-Wuerttemberg in the period December 2014 to December 2015, respectively; the dotted line depicts the total number of occupied spaces in these facilities in the same period.

2016a,b, 2017; bpb, 2018).[4]

Data on monthly refugee inflows come from the EASY-system, in which all *informal registrations* of requests for asylum were recorded already upon arrival of refugees to Germany. Refugees merely had to report to a state organization that they were seeking asylum, either directly at the German border or with a state or public authority (e.g. the Police or a RRC) immediately after their arrival (BAMF, 2016b). This informal registration initiated the first step of the official asylum procedure (Burger and Stoldt, 2015), which qualified refugees for benefits in accordance with the *Act on Benefits for Asylum Seekers (AsylbLG)* and the *Asylum Act (AsylG)* (Bundestag, 2015).[5] Based on EASY records, asylum seekers were distributed onto the sixteen federal states according to a predetermined quota defined by the *Koenigstein Key* (Federal Government, 2016; BAMF, 2016b).[6] TABLE A-1 in the appendix contrasts state-level allocation quotas based on the *Koenigstein Key*[7] and actual regional distributions of asylum seekers as recorded in the *Statistic on Asylum Seekers' Benefits* on 31st December 2015.[8] In spite of the chaotic circumstances that prevailed in the last months of 2015 as a result of the unexpectedness and scale of the vast migrant inflow to Germany, state allocation quotas resemble actual state-level distributions of asylum seekers fairly closely at the end of 2015. In the three federal states studied in our analysis (Baden-Wuerttemberg, Rhineland-Palatinate, and Mecklenburg-Western

[4]See Kürschner Rauck and Kvasnicka (2018) for a more detailed chronology and overview of the series of events that led to the 2015 Refugee Crisis in Germany.

[5]Benefits are mainly in kind, such as food, medical services and shelter in RRCs or subsequent housing, but can also take the form of financial allowances.

[6]The acronym EASY refers to ***E****rstverteilung der* ***Asy****lbegehrenden*, i.e. the initial distribution of asylum seekers.

[7]The Koenigstein Key *(Königsteiner Schlüssel)* is a pre-defined quota based on demographic and economic characteristics of the 16 federal states of Germany. The key is calculated annually, based on tax revenues (receiving a weight of 2/3) and population sizes (receiving a weight of 1/3) of the penultimate year (GWK, 2017; German Federal Social Insurance Authority, 2016).

[8]See Destatis (2016) for a detailed description of these statistics.

Pomerania), differences between state quotas and actual refugee inflow shares lie in the narrow range of -0.530 to 0.242 percentage points (or between -4.08% to 5.01%).

Upon arrival at a designated RRC in an assigned state, refugees often had to persevere for several weeks before they could *formally register* by filing an application for asylum with the *Federal Office for Migration and Refugees (BAMF)* (Bundestag, 2016; Federal Government, 2016; Daldrup, 2016).[9] This pent-up demand is visible in the left graph of FIGURE 1, which shows that the total number of asylum applications filed with the BAMF (dashed line) in late 2015 is lagging behind the corresponding monthly figures of arrivals of asylum seekers to Germany, as recorded in the EASY-system (solid line). Because of the large numbers of arrivals and pending asylum applications, existent capacities of RRCs became in most instances rapidly exhausted in the advent of the crisis. This pressure on RRC capacities was further intensified with the passage of the *Act on the Acceleration of Asylum Procedures (AsylVfBeschlG)* on 24th October 2015 (§47 AsylG), which increased the maximum residency requirement of refugees in their allocated RRC from three to six months.[10] Authorities hence suddenly found themselves confronted with the challenging task to acquire at short notice sufficient and suitable accommodations to extend existing RRC capacities (Federal Government, 2015a). The state of Baden-Wuerttemberg, for instance, already housed pre crisis (in 2014) a considerable proportion (approximately 70%) of asylum seekers in centralized accommodations (i.e. RRCs and other refugee group quarters). It opened 35 new RRCs in 2015, of which 30 facilities where still open and 28 also occupied by the end of the year. The right graph of FIGURE 1 documents for the federal state of Baden-Wuerttemberg the total number of RRCs (dash-dot line) and the occupied spaces in those facilities (dotted line) in the months December 2014 to December 2015. As is evident, both lines in the right graph move in tandem with the solid line in the left graph of FIGURE 1, which documents monthly inflows of asylum seekers in this time period. The acquisition and opening of new facilities hence started suddenly in the latter half of 2015 and proliferated rapidly until December 2015.[11]

State-level authorities, however, exhibited strikingly different approaches in their usage of refugee group quarter accommodations (including RRCs) and decentralized accommodation of refugees both pre and (post) crisis. FIGURE A-1 in the appendix illustrates for the whole of Germany in 2014 and 2015 county-level shares of refugees in centralized housing and the percentage point change in these shares between the two years. TABLE A-2 in the appendix furthermore provides summary statistics for each of the three federal states we consider in our analysis. The state of Baden-Wuerttemberg (in the very south-west of Germany) features very high shares of asylum seekers that are housed in centralized accommodations, both in 2014 (on average 70%) and in 2015 (on average 85%). In Rhineland-Palatinate (center south-west), centralized refugee accommodation shares also increased by an average of 14 percentage points. However, a much lower share of refugees in Rhineland-Palatinate was housed centrally already in 2014. In Mecklenburg-Western Pomerania (top right), in contrast, respective shares remained relatively persistent.

At the end of 2015 (2014), the total number of RRCs was 43 (16) in Baden-Wuerttemberg, 26 (1) in Rhineland-Palatinate, and 6 (1) in Mecklenburg-Western Pomerania. Approximate locations (at municipality level) of these facilities are depicted by federal state in FIGURE 2 for the years 2010, 2014 and 2015. Comparing the RRC locations in 2010 (left maps) and 2014 (center maps), it is evident that the pre-crisis allocation of reception facilities was highly persistent.[12] The setting at hand is therefore advantageous for identification as it largely precludes the anticipation of the extensive number of rapid RRC openings in late 2015.

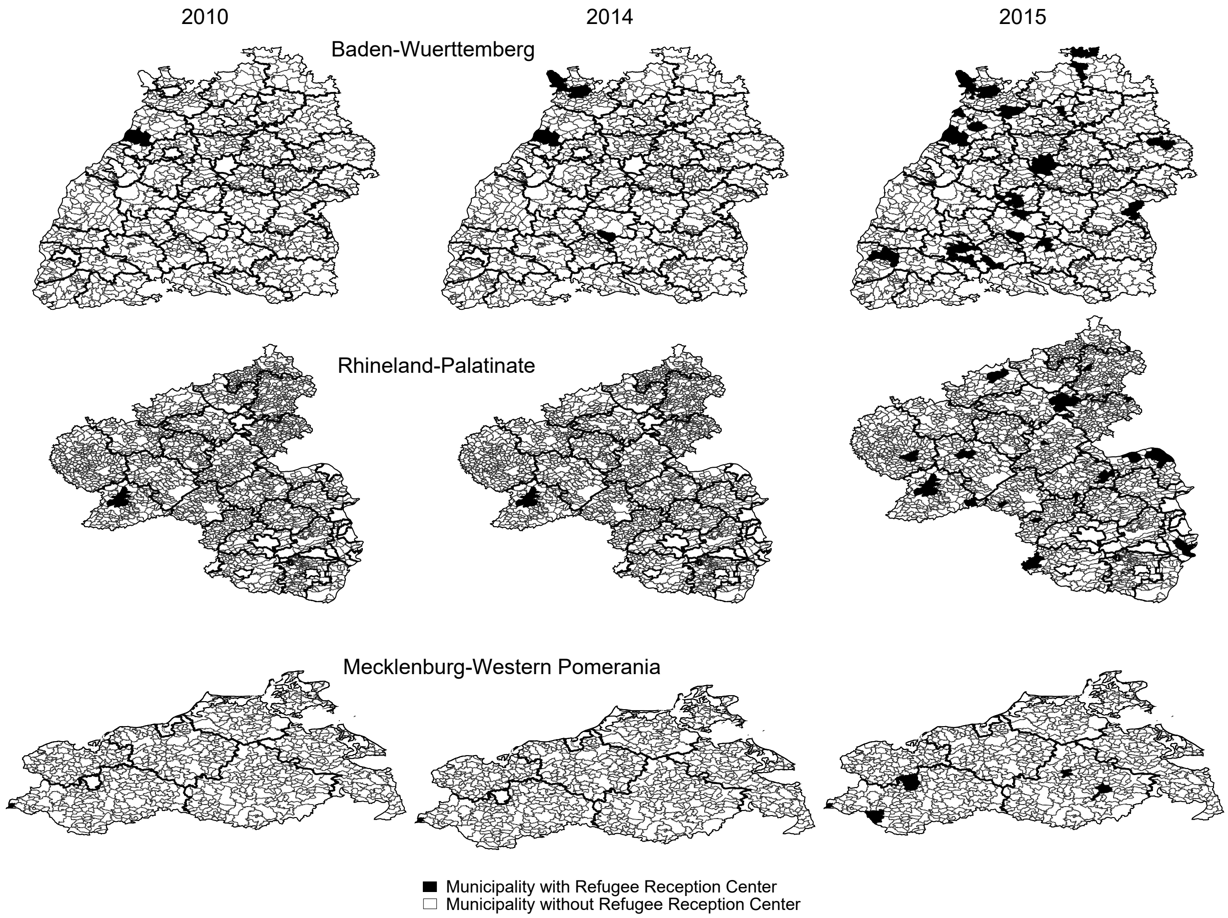
[9] In September 2015, the number of unprocessed asylum applications already amounted to approximately 300 thousand (Hoesch, 2018).

[10] Asylum seekers from save origin countries had to reside in their designated RRC for the entire duration of their asylum procedure (§47 AsylG; AsylVfBeschlG).

[11] New RRC openings in the federal states of Rhineland-Palatinate (25) and Mecklenburg-Western Pomerania (5), which all were in use by the end of 2015, also took place in the latter half of 2015 with but one exception that opened already with partial capacities in May (Federal State Government (RLP), 2017; Federal State Parliament (MWP), 2017).

[12] Only the federal state of Baden-Wuerttemberg opened new establishments in three municipalities between 2010 and 2014.

FIGURE 2: REFUGEE RECEPTION CENTER LOCATIONS (31ST DECEMBER 2010, 2014 & 2015)



NOTE: Municipalities in black shades depict approximate (i.e. municipality-level) locations of refugee reception centers for the federal states of Baden-Wuerttemberg (top), Rhineland-Palatinate (center) and Mecklenburg-Western Pomerania (bottom) on 31st December in 2010 (left), 2014 (center) and 2015 (right).

3 Empirical Strategy and Data

3.1 Data and Setting

In the empirical analysis, two main sources of data are employed, property data from *ImmobilienScout24*[13], Germany's leading online property broker,[14] and regional data on refugee reception centers (RRCs) and their capacity utilization.

Regional data on RRCs were provided for this research project by the respective officiating authorities of the three federal states of Baden-Wuerttemberg, Rhineland-Palatinate and Mecklenburg-Western Pomerania. The data contain exact locations (postal addresses) of RRCs on 31st December 2010, 2014 and 2015, as well as occupancy figures for

[13] See https://www.immobilienscout24.de/. A detailed description of the data is provided in Bauer et al. (2013).

[14] According to the *Unique Visitors* ranking of *comScore Media Metrix*, a market research institution, *ImmobilienScout24* had a market share of approximately 63% of the German online property market in March 2016 (ImmobilienScout24, 2017). This figure, however, understates *ImmobilienScout24*'s true market coverage, as the overlap between ads posted on *ImmobilienScout24* and competing online real estate brokers as well as alternative means to property marketing is large, as landlords and letting agents typically market available property via several available channels.

late 2015.[15] FIGURE 2 depicts the regional distribution of those locations at municipality level. TABLE A-3 in the appendix provides corresponding summary statistics at municipality and county level for pre-crisis socio-economic features of regions with RRCs, regions with new RRCs, and regions with no RRC in 2015. The original raw RRC data were complemented by own research on approximate opening periods throughout 2015, and geo-referenced in order to calculate walking distances of the shortest routes between each pairwise combination of RRCs and property observations in the estimation sample (described below), using the Stata module *osrmtime* (Huber and Rust, 2016).

The *ImmobilienScout24* data we use covers the universe of monthly geo-referenced individual offers of single family homes for sale that were posted on the broker's website between January 2007 and September 2016. Comprising on average 85 thousand observations on detached (77.4%) and semi-detached (22.6%) houses per calendar month, the data set covers a large part of the German housing supply for single-owner occupancy and is suitable for the analysis of property prices even at fine spatial levels (Bauer et al., 2013; Georgi and Barkow, 2010).[16] House prices and information on property characteristics stem from offers posted by private owners and real estate agents on the platform. Information on property on offer, e.g. its price, may be modified at any time during the purchased posting duration,[17] facilitating the analysis of instantaneous responses of property prices to region specific shocks. These offer postings are pooled across the three federal states into a pre and a post period, which we use in difference-in-differences (DiD) regressions with spatial fixed effects at a very small regional unit (1km grid). As illustrated in FIGURE 3, which summarizes in a timeline the framework of our analysis (which is formally described in Section 3.2), the pre-crisis period covers the months January 2012 to March 2015, and the (post-)crisis period the months October 2015 to September 2016. In the simplest variant of settings considered, observations on property located within (beyond) walking distance of at least one locality that housed an open RRC on 31st December 2015 are assigned to the treatment (control) group.[18] Our dependent variable is the log price of a house offered for sale. We exclude from our estimation sample property listings for which information on house characteristics is missing or for which there is no other listing in its 1km grid in the total 51 month period under investigation. We also exclude special properties, such as farmsteads, villas and bungalows, as well as offers with very extreme characteristics.[19] Our final estimation sample consists of approximately 847 thousand monthly observations on about 267 thousand dwellings. The size of the estimation sample and the individual information on houses contained in these data allow to control for standard items of property characteristics traditionally considered in hedonic house price regressions, such as the age of the house [in years], its base area, and its living space [m^2] (amongst other items). A complete list of these property-specific characteristics by distance to RRCs is provided in TABLE 1 for the pre- and (post-)crisis period, respectively. Average offer prices for houses located within a 15-minute walking distance of RRCs decreased pre- to (post-)crisis (-17,777 Euro), whereas those of property located beyond that threshold increased (+25,664 Euro). The difference in these two differences, i.e. the unconditional DiD estimate, is statistically significant and amounts to -43,441 Euro (see column (11) of TABLE 1). This suggests that houses near RRCs have suffered sizeable price growth penalties. The same applies to log house

[15] In December 2015, occupancy levels in the 73 operating RRCs (among the 75 open RRCs) ranged from 52 to 4,901, with a mean occupancy level of 724 asylum seekers (std. dev. 915.98).

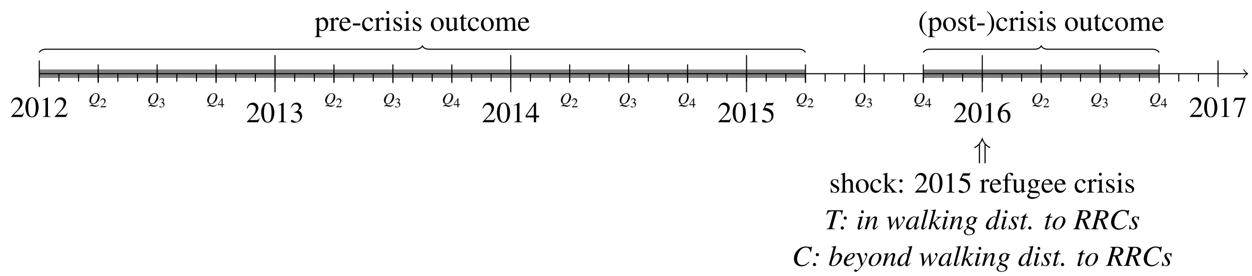
[16] Recent empirical studies have utilized *ImmobilienScout24* data to study, amongst others, the consequences of location-specific exogenous shocks for regional property prices in Germany, such as nuclear power plant closures following the Fukushima Daiichi incident in 2011 (Bauer et al., 2017), the establishment of wind turbines (Frondel et al., 2019), or county-level refugee immigration (Kürschner Rauck and Kvasnicka, 2018).

[17] Fees payable depend on the posting duration (two weeks, one or three months) and offer type (houses, respectively apartments on offer for sale or rent).

[18] Data covering the months April to September 2015 are excluded from the estimation sample, as information on neither announcements of building sites nor the exact timing of openings and closures is provided for all RRCs throughout this period.

[19] We exclude houses with a listed price below 1,000 or above 10 Mio. Euro, houses with a living space below 25 and above 500 square meters, houses with a reported base area of less than 50 or more than 500 square meters and houses for which the number of rooms exceeds 11 or is less than one. We also exclude houses, which are dilapidated, aged 200 years or more, and houses under construction with an expected duration until completion exceeding two years.

FIGURE 3: TIMELINE OF SETTING, FRAMEWORK OF ANALYSIS AND DATA SAMPLING STRATEGY



prices, our dependent variable (-9.4 percentage points). Furthermore, unconditional DiD estimates for the property characteristics are all negatively signed and statistically significant with but two exceptions, the share of detached houses and the share of houses under construction. However, when we condition on neighborhood (1km grid-level) fixed effects, these differences in characteristics abate (see column (12) of TABLE 1).

3.2 Empirical Framework

To identify the effect of refugee reception centers on residential house prices, we apply a spatial difference-in-differences approach by estimating variants of the following hedonic price function:[20]

$$\log HousePrice_{ijt} = \mu_j + \tau_t + X_{it}'\beta + \gamma WALK_i + \delta POST_t \times WALK_i + \varepsilon_{ijt}, \tag{1}$$

where the dependent variable, $\log HousePrice_{ijt}$, is the log offer price (in Euro) of property i in region j in month t. All regressions include fixed effects for time (quarterly dummies) and region (1km grid), denoted by τ_t and μ_j, to capture the piecewise common time-trend in asking prices between regions as well as time-invariant region-specific attributes, and control for a set of house-specific characteristics X_{it} to address potential changes in local (within-grid) property compositions across time (TABLE 1 provides a complete list of these characteristics by distance to RRCs, i.e. separately for treatment and control regions). $POST_t$ and $WALK_i$ are binary variables that equal one for listings posted in the (post-)crisis period (October 2015 through September 2016), respectively houses that are located within 15 minutes walking distance to a RRC.[21] The main explanatory variable is given by the interaction of these two indicators. Its coefficient δ captures the treatment effect.

FIGURE 4 illustrates the empirical strategy graphically. Listings of houses within the 15-minute distance threshold (within the dash-dot line) are assigned to the treatment group, listings located beyond that threshold to the control group.[22] Additional specifications consider as control group exclusively property located in 15 to 40 minutes walking distance to RRCs in order to obtain comparably more homogenous treatment and control regions (solid line). In either case, the main coefficient of interest, δ, captures the percentage point difference in the pre- to (post-)crisis

[20] See Rosen (1974).

[21] Since it is a priori not clear at which distance threshold RRCs may exert an effect, exploratory piece-wise constant specifications consider multiple indicator variables for less than 15 minutes, 15 to 30 minutes and 30 to 40 minutes walking distance bands.

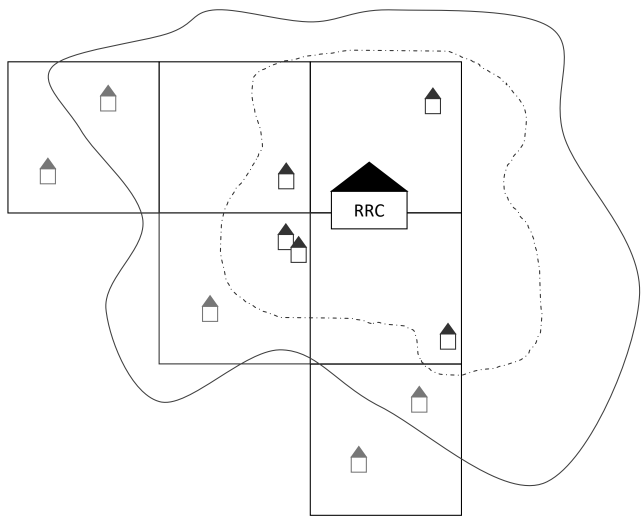
[22] A 15-minute walking distance threshold corresponds to approximately 1.2 km (0.7 miles) walking distance. This resembles roughly the walking distance between Times Square and the Empire State Building in New York City (U.S.), the walking distance between the London Eye and Westminster Abbey in London (UK), or half the walking distance between the Eiffel Tower and Arc de Triomphe in Paris (France).

Table 1: Summary Statistics by Distance to Refugee Reception Centers, Pre- and (Post-)crisis Period

	Pre-crisis					(Post-)crisis					DiD	
	all	≤ 15 min.	> 15 min.	> 15-40 min.	> 40 min.	all	≤ 15 min.	> 15 min.	> 15-40 min.	> 40 min.	[(7)-(2)]-[(8)-(3)]	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
House price [EUR]	267,052	368,971	266,423	354,950	263,896	292,586	351,194	292,087	392,555	289,286	−43,441***	−25,657
	[181,858]	[243,638]	[181,234]	[213,905]	[179,576]	[185,988]	[190,128]	[185,873]	[279,391]	[181,789]	(5,428)	(16,829)
log House price	12.290	12.643	12.287	12.615	12.278	12.410	12.669	12.408	12.686	12.400	−0.094***	−0.084*
	[0.676]	[0.596]	[0.676]	[0.592]	[0.675]	[0.627]	[0.430]	[0.628]	[0.627]	[0.626]	(0.020)	(0.044)
Living space [m^2]	159.763	169.662	159.702	167.860	159.469	157.683	150.782	157.742	167.338	157.475	−16.920***	−12.434**
	[56.571]	[61.578]	[56.534]	[58.886]	[56.448]	[51.740]	[45.921]	[51.783]	[59.504]	[51.526]	(1.650)	(5.980)
Base area [m^2]	772.525	677.887	773.109	622.614	777.404	787.639	605.945	789.187	711.077	791.365	−88.020***	−6.726
	[787.632]	[657.315]	[788.335]	[571.756]	[793.229]	[732.474]	[349.709]	[734.688]	[702.501]	[735.448]	(23.052)	(52.919)
# Rooms	5.733	6.024	5.732	5.845	5.728	5.556	5.257	5.559	5.732	5.554	−0.595***	−0.400*
	[1.674]	[1.724]	[1.674]	[1.711]	[1.672]	[1.560]	[1.478]	[1.560]	[1.559]	[1.560]	(0.049)	(0.218)
Age [years]	39.608	34.501	39.640	36.937	39.717	29.737	17.750	29.840	30.304	29.827	−6.951***	−4.240
	[37.134]	[34.098]	[37.150]	[33.850]	[37.237]	[38.105]	[31.544]	[38.140]	[36.712]	[38.179]	(1.110)	(3.939)
Under construction [%]	0.046	0.056	0.046	0.071	0.045	0.108	0.184	0.107	0.132	0.106	0.066***	−0.023
	[0.209]	[0.230]	[0.208]	[0.256]	[0.207]	[0.310]	[0.387]	[0.309]	[0.339]	[0.308]	(0.007)	(0.036)
Detached house [%]	0.805	0.798	0.805	0.755	0.807	0.920	0.933	0.920	0.869	0.922	0.021*	0.014
	[0.396]	[0.402]	[0.396]	[0.430]	[0.395]	[0.271]	[0.250]	[0.271]	[0.338]	[0.269]	(0.011)	(0.032)
Region fixed effects											no	1km grid
Observations	658,703	4,042	654,661	18,162	636,499	188,147	1,590	186,557	5,059	181,498	846,850	846,850

NOTES: Columns (1) to (10) show the mean of each variable for the entire pre- and (post-)crisis estimation sample (columns (1) and (6)), for property located within 15 minutes walking distance to RRCs (columns (2) and (7)), and for property located beyond that threshold (columns (3) and (8)). Among the houses located beyond 15 minutes walking distance, we further distinguish between property located beyond 15 and below 40 minutes walking distance (columns (4) and (9)) and property located beyond 40 minutes walking distance (columns (5) and (10)). Averages are calculated for the pre-crisis period January 2012 to March 2015 (columns (1) to (5)) and (post-)crisis period October 2015 to September 2016 (columns (6) to (10)). Standard deviations are reported in square brackets (columns (1) to (10)). Standard errors and cluster robust standard errors (at 1km grid level) are reported in round brackets in columns (11) and (12). ***, **, * denote statistical significance at the 1%, 5% and 10% level.

FIGURE 4: IDENTIFICATION STRATEGY



NOTES: The figure illustrates the assignment of individual observations on houses for sale in the estimation sample to the treatment group (within 15 minutes walking distance to a RRC), respectively control group (beyond 15 minutes, respectively 15 to 40 minutes walking distance to a RRC) according to their exact (geo-referenced) locations. The dash-dot (solid) line indicates the 15 (40) minutes walking distance threshold from a RRC. Squares indicate the level of regional (neighborhood) fixed effects at the 1km grid level.

change in average asking prices for houses located within and beyond 15 minutes walking distance to RRCs. Causal interpretation of this coefficient relies on the main identifying assumption that, conditional on the above mentioned controls, price trends would have evolved similarly in both treated and control regions absent treatment. To corroborate the validity of this assumption, we contrast the results of our baseline estimation and of associated tests for pre-crisis differences in price growth trends between treatment and control regions against results from extended specifications that include (treatment and control) region-specific (linear) time trends.

Due to the inclusion of a full set of time indicators at the level of individual quarters, the model does not identify the counterfactual time trend, i.e. the coefficient on the indicator for the (post-)crisis period, $POST_t$. Similarly, since regional fixed effects are included at the level of 1km grids (see squares in FIGURE 4), the coefficient capturing the time-invariant mean level difference in asking prices between property located within and beyond the 15-minute walking distance threshold, γ, is identified only from 1km grids that contain observations both in the treatment and control region. TABLE A-4 in the appendix lists by distance to RRCs pre-crisis (2013) socio-economic and compositional property characteristics of neighborhoods (at 1km grid level), which were obtained from the RWI-GEO-GRID (Breidenbach and Eilers (2018); microm Consumer Marketing (2016)). Of these, the unemployment rate (RWI and microm (2017b)) and the foreigner share (RWI and microm (2017a)) are included amongst other potential confounders at various levels of aggregation (individual property, municipality and county) in a series of robustness tests, entering both in levels and in interactions with the indicator for the (post-)crisis period to capture potential differences in neighborhood-specific house price growth trends related to these factors. The same applies to pre-trends (between 2011 and 2013) in these variables.

Notwithstanding the advantageousness of our contextual setting for identification, the use of time and region fixed effects at a fine spatial grid level, and the multitude of house attributes considered, there is still a possibility that the placement of RRCs was endogenous to unobserved changes in pre-crisis region-specific characteristics or may have coincided with unobserved changes in local (dis-)amenities other than RRCs in late 2015, which could cause bias in our estimate of δ. As mentioned above, we aim to address such concerns by including lagged values of socio-economic neighborhood characteristics, respectively pre-trends therein, that enter both in levels and interactions with the (post-)crisis period indicator to capture differential trends in region-specific house price growth. Moreover, identification may be complicated by endogenous RRC placements (both pre crisis and in the heyday of the crisis) that are systematically related to unobservable preferences of local residents concerning the hosting of refugees in their immediate neighborhoods, as well as residential mobility responses to pre-crisis RRC placements (selection). Similarly, anticipatory effects may have prevailed particularly in regions near existing RRCs. Our research design aims to reduce the possibility of anticipatory effects of any kind by excluding the months April to September 2015, i.e. the time period during which monthly inflows of asylum seekers began to increase gradually (see left graph of FIGURE 1) and such anticipatory effects (if indeed present) most likely materialized. In addition, we also use two modified versions of our research design. First, to address anticipatory effects nearby existing RRC locations as well as residential mobility responses to those pre-crisis sites, we restrict the analysis to exclusively new establishments of RRCs in late 2015. Second, although the acquisition of new RRCs was of utmost urgency in the advent of the crisis, new placements may have been (partly) endogenous to local residents' preferences towards the reception of refugees in their immediate neighborhood. However, the power that lobbyists may have exerted on officiating bodies (at county, respectively municipality level) with regards to the exact localities in which to place new RRCs within those administrative regions should arguably not have extended beyond their own local communities' administrative boundaries (i.e. the municipality, respectively county above that). We therefore consider in additional explorations exogenous variation in the local exposure to RRCs based on a restricted estimation sample that considers exclusively property located in treatment and control regions that are located in neighboring counties to those that actually housed RRCs.

4 Results

4.1 Main Results

We begin our analysis by estimating a simplified variant of the DiD model specified in equation (1) where we disregard property-specific characteristics. We do control, however, for regional (1km grid) and time (quarter) fixed effects (see column (1) in TABLE 2). The estimated treatment effect of -0.0833 indicates that houses located within 15 minutes distance to RRCs experienced on average 8.3 percentage points lower price growth from the pre- to (post-)crisis period than houses beyond that threshold.[23] House prices may, however, not be directly comparable between the relatively narrow treatment and comparatively large control regions, as these may systematically differ in their average characteristics over time. Therefore, we next add as controls a multitude of characteristics of houses to the regressions (for a list of these characteristics, see TABLE 1). As depicted in column (2) of TABLE 2, adding these hedonic items to the empirical model substantially increases the within R^2, from 0.028 to 0.558,[24] and reduces the absolute magnitude of the estimated treatment effect, which is still negatively signed and statistically significant at the 10% level, by approximately 2.8 percentage points.

Since it is a priori not clear at which distance RRCs exert an effect, we include a set of distance indicators for less than 15, between 15 to 30, and 30 to 40 minutes walking distance to RRCs along with corresponding interaction

[23] Full tabulated regression output for all tables in this study can be obtained from the author upon request.

[24] The overall R^2 is 0.84 or larger in all regressions that include a full set of region dummies and the set of house-specific characteristics.

TABLE 2: TREATMENT EFFECTS - BASELINE RESULTS

	Dependent Variable: log *HousePrice*							
Treatment Effects:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Regions:								
$WALK_{i,15min}$	0.0237 (0.0494)	0.0476 (0.0321)	0.0450 (0.0308)	0.0176 (0.0440)	−0.0574 (0.0527)	−0.0031 (0.0386)	0.0005 (0.0367)	−0.0327 (0.0486)
$WALK_{i,15-30min}$				−0.0355 (0.0313)				−0.0354 (0.0313)
$WALK_{i,30-40min}$				−0.0055 (0.0252)				−0.0055 (0.0252)
Treatment Effects:								
$POST_{Q4'15-Q3'16} \times WALK_{i,15min}$	−0.0833** (0.0425)	−0.0553* (0.0295)	−0.0716** (0.0327)	−0.0545* (0.0295)	−0.1969*** (0.0562)	−0.1263*** (0.0455)	−0.1343*** (0.0499)	−0.1250*** (0.0454)
$POST_{Q4'15-Q3'16} \times WALK_{i,15-30min}$				0.0279 (0.0274)				0.0275 (0.0273)
$POST_{Q4'15-Q3'16} \times WALK_{i,30-40min}$				0.0171 (0.0247)				0.0170 (0.0247)
linear time trend$\times WALK_{i,15min}$					0.0038*** (0.0014)	0.0024** (0.0010)	0.0021** (0.0011)	0.0024** (0.0010)
Grid-level fixed effects (1km)	yes	yes	yes	yes	yes	yes	yes	yes
Time fixed effects (quarters)	yes	yes	yes	yes	yes	yes	yes	yes
Property characteristics	no	yes	yes	yes	no	yes	yes	yes
Control sample	beyond 15 min.		>15–40 min.	beyond 40 min.	beyond 15 min.		>15–40 min.	beyond 40 min.
# fixed effects (1km grid level)	20,802	20,802	680	20,802	20,802	20,802	680	20,802
Obs.	846,850	846,850	28,853	846,850	846,850	846,850	28,853	846,850
within R^2	0.028	0.558	0.571	0.558	0.028	0.558	0.571	0.558

NOTES: The dependent variable is the log of the nominal house price posted. There are 5,632 observations within 15 minutes, 12,952 observations within 15 to 30 minutes, and 10,269 observations within 30 to 40 minutes walking distance to a RRC. Standard errors are clustered at the 1km grid level. ***, **, * denote statistical significance at the 1%, 5% and 10% level.

terms of these indicators with the (post-)crisis period indicator to explore the piece-wise constant distance gradient in the treatment effect of interest (see column (4) of TABLE 2). The control region in this spatially staggered version of the model specified in equation (1) comprises houses located beyond 40 minutes walking distance to RRCs. Positively signed, yet statistically insignificant coefficient estimates on these added interaction terms suggest that the negative treatment effect disappears at walking distance bands larger than 15 minutes.[25]

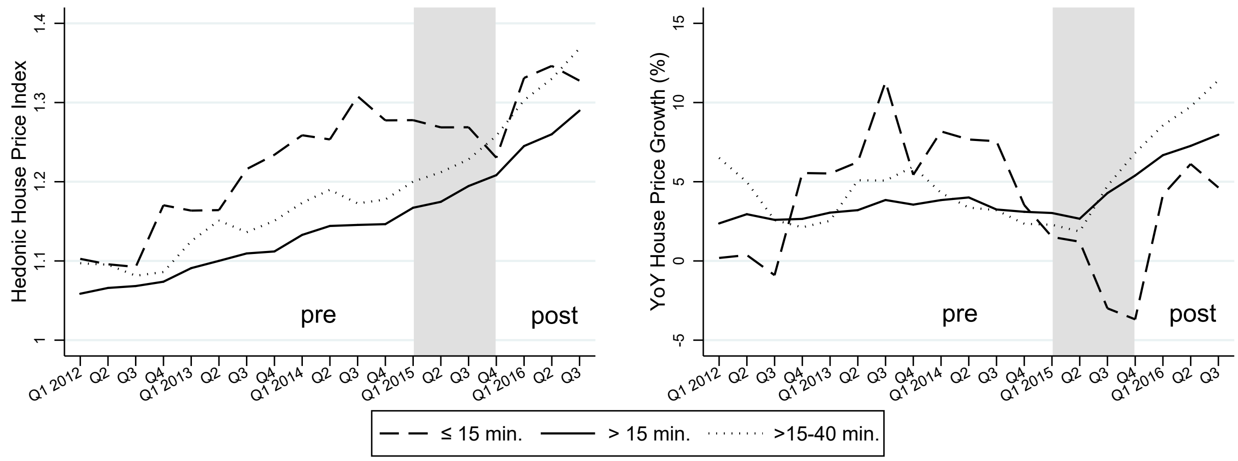
The three specifications discussed above employ the entire estimation sample, where all houses that are located beyond either the 15 or the 40 minutes walking distance threshold constitute the control region. Next, we utilize a restricted estimation sample to obtain more homogenous treatment and control regions. Specifically, and as depicted by the solid line in FIGURE 4, we restrict the control sample to houses located between 15 to 40 minutes walking distance from RRCs. Disregarding observations on property listed for sale beyond 40 minutes walking distance to RRCs reduces the estimation sample to 28,853 observations. Regression results for this restricted sample (see column (3) of TABLE 2) show that the treatment effect gets absolutely larger (now estimated at approximately -7.2 percentage points) when houses within a narrow spatial sub-market are considered, i.e. houses that are located in more similar regions.[26]

Furthermore, the pre-crisis level difference in average house prices between treatment and control regions is positive (albeit statistically insignificant throughout specifications (1) to (4)), which may indicate that houses located in regions near RRCs have been offered at higher prices than comparable houses at further distance. It has to be noted, however, that the coefficient on the region indicator, $WALK_{i,15min}$, is identified only from 1km grids that housed both treated and control units, which are relatively few in number (90). Nonetheless, visual inspection of quarterly region-specific hedonic house price indices in the years 2012 to 2016 suggests that a positive level difference indeed prevailed

[25] Additional inquisitions in which we included indicators also for walking distance bands of less than 10 as well as 10 to 15 minutes produced also negative coefficient estimates for both sub-regions. For the former band, the effect is estimated only imprecisely. The number of observations in these sub-regions, however, is also relatively small (2,171 in 10 minutes, and 3,461 in 10 to 15 minutes walking distance, out of which 585 and 1,005 are (post-)crisis period observations, respectively).

[26] For a list of pre-crisis neighborhood characteristics of treatment and control regions, see TABLE A-4 in the appendix.

FIGURE 5: HEDONIC HOUSE PRICE INDEX AND YEAR-ON-YEAR (YOY) HOUSE PRICE GROWTH (QUARTERS: Q1 2012 - Q3 2016)



NOTES: The figure shows the evolution of quarterly hedonic house price indices for treatment (within 15 minutes walking distance to a RRC) and control regions (beyond 15 minutes, respectively 15 to 40 minutes walking distance to a RRC) from the first quarter of 2012 until the third quarter of 2016 (left graph) as well as the corresponding year-on-year (YoY) growth rate in the quarterly house price indices for the three region-groups throughout this period (right graph). Group-level house price indices were calculated using the hedonic time-dummy method and house price data from *ImmobilienScout24* from 2010 to 2016, following the methodological approach outlined in Bauer et al. (2013). The hedonic regressions were applied to stratified samples at the treatment, respectively, control region level, including 1km grid-level fixed effects and the full set of property characteristics employed in the main analysis of this paper. The total number of RRCs is 18 (75) on 31st December 2014 (2015). The time period shaded in gray, between the first and fourth quarter of 2015, indicates monthly pooled cross sections excluded from the estimation sample (i.e. April until September 2015).

between treatment and control regions (see left graph of FIGURE 5).[27] Above all, level-differences seem to be the result of an elevated trend in quarterly year-on-year (seasonally adjusted) house price growth in the treatment regions pre crisis (see right graph of FIGURE 5). What is more, associated figures on average year-on-year house price growth in treatment regions do not just fall below those of the respective control regions, but turn even negative in the heyday of the crisis (in late 2015). If this pre-crisis divergence in house price growth between treatment and control regions prevailed absent treatment, the main identifying assumption of a common trend would not be satisfied. However, the observable diverging trends pre crisis are not of the type that could explain or invalidate our result of a negative effect of proximity to RRCs on house price growth. They only suggest that the size of the estimated treatment effects in columns (1) to (4) of TABLE 2, where we do not control for differential trends pre crisis, should be viewed as lower bounds.

We address this differential price growth trend by including a group-specific linear time trend in our regression model in equation (1). Columns (5) to (8) of TABLE 2 show the corresponding results to columns (1) to (4) of TABLE 2 after including these trends. Estimated coefficients for the treatment effect of interest more than double and are much more precisely estimated after the inclusion of these trends, which are positive for both treatment and control regions

[27] These indices were calculated for stratified samples of the treatment and control regions using the hedonic time-dummy method and data from *ImmobilienScout24* for the years 2010 to 2016, following the methodological approach outlined in Bauer et al. (2013). Hedonic regressions included the above mentioned controls for house characteristics and region fixed effects at 1km grid level.

TABLE 3: PLACEBO TEST - EXOGENEITY OF PRE-TREATMENT OUTCOMES (PROPERTY DATA: JAN. 2012 – MAR. 2013 & OCT. 2013 – SEPT. 2014)

	Dependent Variable: $\log HousePrice$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Regions:								
$WALK_{i,15min}$	−0.0028	0.0366	0.0355	0.0595	−0.1001	0.0116	0.0052	0.0345
	(0.0515)	(0.0276)	(0.0272)	(0.0468)	(0.0631)	(0.0409)	(0.0410)	(0.0557)
$WALK_{i,15-30min}$				0.0205				0.0205
				(0.0386)				(0.0386)
$WALK_{i,30-40min}$				0.0295				0.0295
				(0.0321)				(0.0321)
Treatment Effects:								
$POST_{Q4'13-Q3'14} \times WALK_{i,15min}$	0.0995***	0.0703***	0.0678**	0.0706***	−0.1130	0.0161	0.0008	0.0163
	(0.0368)	(0.0247)	(0.0264)	(0.0247)	(0.0800)	(0.0578)	(0.0620)	(0.0578)
$POST_{Q4'13-Q3'14} \times WALK_{i,15-30min}$				0.0063				0.0062
				(0.0179)				(0.0179)
$POST_{Q4'13-Q3'14} \times WALK_{i,30-40min}$				0.0082				0.0082
				(0.0190)				(0.0190)
linear time trend $\times WALK_{i,15min}$					0.0112***	0.0029	0.0035	0.0029
					(0.0036)	(0.0031)	(0.0033)	(0.0031)
Grid-level fixed effects (1km)	yes	yes	yes	yes	yes	yes	yes	yes
Time fixed effects (quarters)	yes	yes	yes	yes	yes	yes	yes	yes
Property characteristics	no	yes	yes	yes	no	yes	yes	yes
Control sample	beyond 15 min.		>15–40 min.	beyond 40 min.	beyond 15 min.		>15–40 min.	beyond 40 min.
# fixed effects (1km grid level)	17,587	17,587	587	17,587	17,587	17,587	587	17,587
Obs.	448,718	448,718	15,277	448,718	448,718	448,718	15,277	448,718
within R^2	0.003	0.553	0.580	0.553	0.003	0.553	0.580	0.553

NOTES: The dependent variable is the log of the nominal house price posted. The table reports results from placebo tests which employ pooled cross sectional data of houses on offer two years prior to the refugee crisis. Specifically, observations on houses on offer from January 2012 till March 2013 and October 2013 till September 2014 are assigned to a fictitious pre- and post-treatment period. There are 2,710 observations within 15 minutes, 7,224 observations within 15 to 40 minutes, and 5,343 observations within 30 to 40 minutes walking distance from a RRC. Standard errors are clustered at the 1km grid level. ***, **, * denote statistical significance at the 1%, 5% and 10% level.

and do indeed feature a positive (statistically significant) differential for the treatment group.[28] Columns (6) and (7) depict the results of our preferred specifications (among those, preference is given to specification (7), which considers the restricted control sample), which employ the full set of controls for house characteristics, quarter and neighborhood (1km grid) fixed effects, as well as a linear time trend for treatment, respectively control regions. Offer prices for houses located within 15 minutes walking distance to RRCs (operating in late 2015) grew on average 12.6 (13.4) percentage points less than offer prices of comparable houses listed for sale at farther (15 to 40 minutes) walking distance.

In a next step, we employ several placebo regressions to formally test conditional on controls for the exogeneity of pre-treatment outcomes to treatment, both with and without linear trends. These tests employ pooled cross sectional data on houses listed for sale from January 2012 till March 2013 and from October 2013 till September 2014, which are used as fictitious pre- and post-treatment periods, respectively. The results of these tests are shown in TABLE 3. To ease comparison, regression specifications and their order are analog to the ones considered in TABLE 2. Consistent with our preliminary finding from the visual inspection of FIGURE 5, a positive pre-crisis differential in house price growth is observable for property located within 15 minutes walking distance to RRCs (see columns (1) to (4)). This difference between treatment and control regions disappears, however, once group-specific linear trends are accounted for (see columns (5) to (8)). Therefore, in the remainder of the paper, which scrutinizes the validity of our above findings in various ways, all regressions will account for group-specific trends at the level of treatment and control regions or at an even lower level (i.e. county level).

[28] Although the visual inspection of the two graphs in FIGURE 5 is indicative of group-specific linear trends, we ran further specification tests, which additionally include quadratic and cubic trends. The corresponding coefficient estimates on these trends are, however, not statistically significantly different from zero. The associated coefficients on the treatment effect of interest remain negatively signed, but are now only imprecisely estimated.

4.2 Robustness Tests

In the following, we will probe the robustness of our findings in a battery of sensitivity tests. The results of these tests are reported in TABLES 4 to 6. The baseline model we consider as a point of reference in these tests is the most elaborate specification from our main analysis, which includes both quarter and neighborhood fixed effects (at 1km grid level), group-specific time trends for treatment and control regions, and the set of house-specific characteristics listed in TABLE 1. To ease comparison, columns (1) and (2) in TABLES 4 to 6 reproduce the estimated treatment effects of these (preferred) baseline specifications, which stem from columns (6) and (7) in TABLE 2.

Results reported in TABLES 4 and 5 consider a broad range of potential confounding influences at various levels of aggregation. As a first robustness check, we include lagged values (from 2013) of socio-economic characteristics of neighborhoods (i.e. at the highly spatially disaggregated level (1km grid) at which the regional fixed effects are applied), respectively pre-trends in these characteristics (from 2011 to 2013). Characteristics we consider for this purpose include the unemployment rate and the share of the foreign-born population, which enter both in levels and in interaction terms with the indicator for the (post-)crisis period to account for neighborhood-level differences in regional house price growth trends related to these factors. As shown in columns (3) to (6) in TABLE 4, estimated treatment effects change little from those in the baseline specifications, both for the estimation sample that considers the broader control region (approx. 0.5 percentage points increase in columns (3) and (5) compared to column (1)) and the estimation sample that considers the geographically more restricted control region (approx. 0.4 percentage points increase in columns (4) and (6) compared to column (2)).

Second, we change the level of spatial fixed effects that we consider from 1km grids to the larger, albeit still relatively small, regional unit of municipalities. The motivation for this test is twofold. First, the administrative units of municipalities constitute (partially) spatially integrated housing markets. Second, final decisions on exact RRC placements are made at this level (or the associated county level). Allowing for fixed effects at the level of these administrative regions, and clustering standard errors at that same level, we can address potential housing market spill-overs within these entities. The corresponding results are shown in columns (7) and (8). Estimated treatment effects turn out smaller in absolute magnitude (-8.32 and -9.64 percentage points), but are still large and also statistically significant. This decline in the effect size may be attributable to the prevalence of housing market spillovers,[29] as well as the fact that we control for regional fixed effects at a less spatially disaggregated level of regional fixed effects. To account for fixed effects at a fine spatial grid level may, however, be crucial for our analysis. The great importance of (otherwise unobserved) local neighborhood amenities for property prices has long been recognized. This importance finds expression in the appellative saying 'location, location, location' (repeated three times for emphasis) to stress the importance of locality for the value of real estate. In columns (9) and (10), we add the above mentioned controls for lagged neighborhood characteristics (from 2013) to our regressions to account for neighborhood-level heterogeneity in socio-economic factors. This changes the results only little from those shown in columns (7) and (8), which is suggestive that housing market spill-overs did indeed prevail.

Returning to our baseline model with grid-level fixed effects, we allow in a third and similar sensitivity test house price growth between the pre- and (post-)crisis period to vary by lagged values (from 2014) of tax income per taxpayer and living space per capita in municipalities (see columns (5) and (6) of TABLE 5). This sensitivity test is motivated by the concern that these attributes of municipalities may have been crucial for RRC placement decisions. The results of this test, however, do not suggest that such correlations lead to bias in our estimates of the treatment effect of interest. The coefficient estimates on these new controls show the expected sign.

Fourth, we replace the group-specific linear trends by distinct trends at the administrative region level above that

[29]Note that house price growth in the control regions experienced an expansion in the advent of the crisis and thereafter (see FIGURE 5). Nonetheless, we cannot tell with certainty that house price growth would not have fared similarly in the treatment regions had they not been treated.

TABLE 4: TREATMENT EFFECTS - ROBUSTNESS TESTS I

	Dependent Variable: $\log House Price$									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment Regions:										
$WALK_{i,15min}$	-0.0031 (0.0386)	0.0005 (0.0367)	-0.0012 (0.0381)	0.0032 (0.0349)	0.0488 (0.0366)	-0.001 (0.0378)	0.0033 (0.0344)	0.0262 (0.0405)	0.0437 (0.0365)	0.0485 (0.0322)
Treatment Effects:										
$POST_{Q4'15-Q3'16} \times WALK_{i,15min}$	-0.1263^{***} (0.0455)	-0.1343^{***} (0.0499)	-0.1315^{***} (0.0453)	-0.1386^{***} (0.0502)	-0.1327^{***} (0.0458)	-0.1385^{***} (0.0520)	-0.0832^{*} (0.0434)	-0.0964^{**} (0.0404)	-0.0907^{**} (0.0454)	-0.0825^{**} (0.0395)
Regional fixed effects	1km grid	1km grid	1km grid	1km grid	1km grid	1km grid	municipality	municipality	municipality	municipality
Pre-crisis neighborhood characteristics (1km grid)	no	no	yes	yes	trend: 2011-13	trend: 2011-13	no	no	yes	yes
Region-specific linear time trends (T,C)	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Time fixed effects (quarters)	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Property characteristics	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Control sample	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.
# fixed effects	20,802	680	20,802	680	20,769	680	3,980	100	3,980	100
Obs.	846,850	28,853	846,850	28,853	846,532	28,853	846,850	28,853	846,850	28,853
within R^2	0.558	0.571	0.558	0.572	0.558	0.572	0.568	0.605	0.568	0.609

NOTES: The dependent variable is the log of the nominal house price posted. To ease comparison, columns (1) and (2) reproduce the treatment effects of the baseline specifications in columns (6) and (7) in TABLE 2. Lagged (pre-crisis) neighborhood characteristics at 1km grid level (from 2013), respectively pre-trends in these (from 2011 to 2013), include the unemployment rate and the share of the foreign-born population. Information on neighborhood characteristics from 2011 is missing for 33 1km grids (318 observations) due to legal data protection requirements. There are 5,632 observations within 15 minutes, and 23,221 observations with 15 to 40 minutes walking distance to a RRC. Standard errors are clustered at the 1km grid level in columns (1) to (6) and at the municipality level in columns (7) to (10). ***, **, * denote statistical significance at the 1%, 5% and 10% level.

TABLE 5: TREATMENT EFFECTS - ROBUSTNESS TESTS II

	Dependent Variable: $\log House\,Price$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Regions:								
$WALK_{i,15min}$	-0.0031 (0.0386)	0.0005 (0.0367)	-0.0003 (0.0389)	-0.0039 (0.0368)	-0.0014 (0.0386)	-0.0044 (0.0372)	0.0427 (0.0327)	0.0439 (0.0306)
Treatment Effects:								
$POST_{Q4'15-Q3'16} \times WALK_{i,15min}$	-0.1263^{***} (0.0455)	-0.1343^{***} (0.0499)	-0.1263^{***} (0.0455)	-0.1352^{***} (0.0500)	-0.1310^{***} (0.0455)	-0.1285^{***} (0.0490)	-0.0574^{**} (0.0288)	-0.0787^{**} (0.0346)
$dist.\,population\,center_{ic}$			0.0092^{***} (0.0030)	-0.0135 (0.0126)				
$tax\,income_{m2014}/taxpayer_{m2014}$					0.0018 (0.0027)	0.0046 (0.0123)		
$living\,space_{m2014}/pop_{m2014}$					-0.0033 (0.0032)	-0.0491^{***} (0.0185)		
$POST_{Q4'15-Q3'16} \times tax\,income_{m2014}/taxpayer_{m2014}$					0.0015^{***} (0.0005)	0.0087^{***} (0.0029)		
$POST_{Q4'15-Q3'16} \times living\,space_{m2014}/pop_{m2014}$					-0.0013^{***} (0.0005)	-0.0012 (0.0035)		
Grid-level fixed effects (1km)	yes	yes	yes	yes	yes	yes	yes	yes
Region-specific linear time trends	T,C group	T,C group	T,C group	T,C group	T,C group	T,C group	county	county
Time fixed effects (quarters)	yes	yes	yes	yes	yes	yes	yes	yes
Property characteristics	yes	yes	yes	yes	yes	yes	yes	yes
Control sample	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.
# fixed effects	20,802	680	20,802	680	20,784	680	20,802	680
Obs.	846,850	28,853	846,850	28,853	846,549	28,853	846,850	28,853
within R^2	0.558	0.571	0.558	0.571	0.558	0.573	0.560	0.582

NOTES: The dependent variable is the log of the nominal house price posted. To ease comparison, columns (1) and (2) reproduce the treatment effects of the baseline specifications in columns (6) and (7) in TABLE 2. Columns (3) and (4) include as additional control the driving distance [km] from each house to its county-level population center. Lagged municipality-level characteristics (from 2014) added in columns (5) and (6) include tax income per taxpayer [tsd. EUR] and living space per capita [m^2]. Information on taxable income per tax payer is missing for two municipalities due to legal data protection requirements. There are 5,632 observations within 15 minutes, and 23,221 observations within 15 to 40 minutes walking distance to a RRC. Standard errors are clustered at the 1km grid level. ***, **, * denote statistical significance at the 1%, 5% and 10% level.

of municipalities, i.e. at county level. This test is motivated by the possibility that house price growth may have evolved differently among sub-regions at this administrative level and that such differential growth may be correlated with RRC placements in late 2015. Dispelling any such concerns, however, estimates of the treatment effects, shown in columns (7) and (8) in TABLE 5, turn out virtually identical to those in columns (2) and (3) in TABLE 2, i.e. to those of our very first analyses, where we disregarded linear trends of any kind.

Fifth, even at the level of neighborhoods, certain locations of property may be considered superior by prospective buyers. To address this issue, we add a house-specific location attribute, the driving distance (the shortest route in kilometers) of property i to its county-level population center in 2015.[30] Note that houses in treatment regions tend to lie in more proximate location to population centers. This may (at least in part) account for the positive difference in pre-crisis price growth trends and speaks once more against the possibility that average differences in local amenities, such as *remoteness*, explain our finding of a negative treatment effect. This view is corroborated by the corresponding estimation results, shown in columns (3) and (4), which hardly show any change both in terms of magnitude and precision.[31]

The same holds true also for our final sensitivity test, in which we aim to take into account potential confounding influences brought about by broader facets of the 2015 refugee crisis. Specifically, we add controls for the scale of refugee immigration and the structure of refugee accommodation (the share of refugees housed decentrally in flats) at county-level, as well as indicators for municipality-level exposure to refugees, respectively refugee group quarter accommodations other than RRCs. These data are drawn (partly in the form of special data extracts for this project) from the *Statistic on Asylum Seekers' Benefits*, which is a compulsory count of all asylum seekers who receive any kind of benefit in accordance with the *Act on Asylum Seekers' Benefits (AsylbLG)* on the survey date (31st December 2015). Columns (3) to (6) of TABLE 6 report the estimation results. Our finding of a negative treatment effect of RRC exposure proves highly robust in these tests. What is more, refugee immigration (at county level) turns out negatively related to house price growth, with statistically significant coefficient estimates in the full estimation sample (see columns (3)

[30] Driving distances have been calculated using the Stata module *osrmtime* (Huber and Rust, 2016).

[31] The mean driving distance [in km] of houses in the estimation sample is 15.853 (std. dev. 9.856). For dwellings located in treatment regions, the mean driving distance is 9.584 (std. dev. 6.471). For dwellings in the control regions, the respective distance is 15.895 (std. dev. 9.862).

Table 6: Treatment Effects - Robustness Tests III

	Dependent Variable: $\log House\,Price$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Regions:								
$WALK_{i,15min}$	-0.0031	0.0005	-0.0027	-0.0023	-0.0034	-0.0012	-0.0026	0.0010
	(0.0386)	(0.0367)	(0.0385)	(0.0365)	(0.0384)	(0.0372)	(0.0384)	(0.0377)
Treatment Effects:								
$POST_{Q4'15-Q3'16} \times WALK_{i,15min}$	-0.1263^{***}	-0.1343^{***}	-0.1247^{***}	-0.1354^{***}	-0.1248^{***}	-0.1314^{***}	-0.1301^{***}	-0.1256^{***}
	(0.0455)	(0.0499)	(0.0461)	(0.0504)	(0.0463)	(0.0491)	(0.0460)	(0.0483)
$refugees_{c2015}/pop_{c2014}$			1.2462	11.8258^{**}	3.0309	13.8869^{***}		
			(4.5123)	(4.9486)	(4.9635)	(5.3529)		
$decentr.share_{c2015}$					0.0799	0.3516		
					(0.0978)	(0.2530)		
$refugees_{m2015}$							0.0400	0.0945
							(0.0333)	(0.1255)
$group\,quarter_{m2015}$							-0.0422	0.1125
							(0.0371)	(0.0687)
$POST_{Q4'15-Q3'16} \times refugees_{c2015}/pop_{c2014}$			-3.3696^{***}	-0.9873	-3.2812^{***}	-2.6495		
			(1.0397)	(2.1602)	(1.0396)	(2.2373)		
$POST_{Q4'15-Q3'16} \times decentr.share_{c2015}$					0.0114	-0.0937^{**}		
					(0.0085)	(0.0453)		
$POST_{Q4'15-Q3'16} \times refugees_{m2015}$							0.0182^{*}	-0.0061
							(0.0099)	(0.0821)
$POST_{Q4'15-Q3'16} \times group\,quarter_{m2015}$							-0.0112^{*}	0.0827^{***}
							(0.0065)	(0.0301)
Grid-level fixed effects (1km)	yes	yes	yes	yes	yes	yes	yes	yes
Region-specific linear time trends (T,C)	yes	yes	yes	yes	yes	yes	yes	yes
Time fixed effects (quarters)	yes	yes	yes	yes	yes	yes	yes	yes
Property characteristics	yes	yes	yes	yes	yes	yes	yes	yes
Control sample	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.
# fixed effects	20,802	680	20,802	680	20,564	680	20,802	680
Obs.	846,850	28,853	846,850	28,853	838,671	28,853	846,850	28,853
within R^2	0.558	0.571	0.558	0.571	0.558	0.572	0.558	0.573

Notes: The dependent variable is the log of the nominal house price posted. To ease comparison, columns (1) and (2) reproduce the treatment effects of the baseline specifications in columns (6) and (7) in Table 2. Columns (3) and (4) include as additional control the county-level end-of-year refugee stock in 2015, which we normalize by 2014 county population (refugee immigration), and columns (5) and (6) in addition the county-level share of refugees in decentralized housing (refugee accommodation). Columns (7) and (8) add indicators that equal one if a municipality housed refugees, respectively refugee group quarters other than RRCs in 2015. Data used to construct these variables are drawn (partly in the form of special data extracts) from the *Statistic on Asylum Seekers' Benefits*. There are 5,632 observations within 15 minutes and 23,221 observations within 15 to 40 minutes walking distance to a RRC. Standard errors are clustered at the 1km grid level. ***, **, * denote statistical significance at the 1%, 5% and 10% level.

and (5)). An increase in the county-level end-of-year refugee stock equal to one percent of the initial (2014) county population is associated with an average 3.3 percentage points lower house price growth. The restricted estimation sample, which considers a spatial housing sub-market within 40 minutes walking distance of RRCs, produces once more negative, albeit statistically insignificant, coefficient estimates for refugee immigration, whereas the housing of both a RRC and a comparably high share of refugees in decentralized housing (conditional on the scale of refugee immigration) accelerates associated price growth penalties.

Associated treatment effects also prove highly persistent when municipality-level exposure to refugees and general group quarter accommodations for refugees are taken into account (see columns (7) and (8)). Municipalities exposed to refugees in decentralized housing are characterized by price growth premia averaging 1.82 percentage points. These premia are attenuated by an average 1.12 percentage points if a municipality is also exposed to a refugee group quarter other than a RRC. However, the presence of such a facility within the same spatial sub-market of RRCs is positively associated with house price growth (the price growth differential equals 8.27 percentage points).

Overall, therefore, the estimated treatment effects of our baseline specification prove highly robust in the battery of sensitivity tests we carried out, both qualitatively and, in most instances, also quantitatively. Moreover, regional housing market spill-overs seem to account (in part) for the house price growth differential between treatment and control regions. We also find evidence for adverse effects on average house price growth of refugee immigration at county level (its scale), as well as of municipality-level exposure to general refugee group quarters. However, in the restricted estimation sample, which considers only houses within 40 minutes walking distance from RRCs, the presence of an additional group quarter (at municipality level) has a positive effect on house price growth. These auxiliary results align well with associated findings in Kürschner Rauck and Kvasnicka (2018) for housing rents in Germany. The study also finds a negative effect of county-level refugee immigration (its scale) in its analyses of rental price growth of apartments at county level. Furthermore, and as expected given the different type of property that is considered in this study, this effect is smaller in magnitude (i.e. -0.38 and -0.75 percentage points in the last quarter

TABLE 7: TREATMENT EFFECTS - SAMPLE RESTRICTIONS

	Dependent Variable: log *House Price*					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Regions:						
$WALK_{i,15min}$	-0.0031 (0.0386)	0.0005 (0.0367)	-0.0113 (0.0409)	-0.0088 (0.0387)	0.0283^{**} (0.0111)	0.0691^{***} (0.0139)
Treatment Effects:						
$POST_{Q4'15-Q3'16} \times WALK_{i,15min}$	-0.1263^{***} (0.0455)	-0.1343^{***} (0.0499)	-0.1489^{***} (0.0471)	-0.1558^{***} (0.0387)	0.0457 (0.0389)	-0.1089^{**} (0.0372)
Regional fixed effects	1km grid	1km grid	1km grid	1km grid	municipality	municipality
Region-specific linear time trend (T,C)	yes	yes	yes	yes	yes	yes
Time fixed effects (quarters)	yes	yes	yes	yes	yes	yes
Property characteristics	yes	yes	yes	yes	yes	yes
Control sample	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.
Sample restrictions	none	none	new RRC	new RRC	RRC neighbor county	RRC neighbor county
# fixed effects	20,802	680	20,780	658	1,906	12
Obs.	846,850	28,853	846,275	28,278	462,499	2,301
Obs. in $\leq$ 15 min. walking distance	5,632	5,632	5,057	5,057	608	608
within R^2	0.558	0.571	0.558	0.573	0.574	0.584

NOTES: The dependent variable is the log of the nominal house price posted. To ease comparison, columns (1) and (2) reproduce the treatment effects of the baseline specifications in columns (6) and (7) in TABLE 2. Standard errors in columns (1) to (4) are clustered at the 1km grid level, respectively, at the county level in columns (5) to (6). ***, **, * denote statistical significance at the 1%, 5% and 10% level.

of 2015 and first quarter of 2016, respectively) than the one we find.

4.3 Additional Explorations

The results of our baseline estimations and associated robustness tests show a consistent pattern of evidence of adverse effects of local exposure of single-owner-occupier houses to RRCs on the price growth of such property in the heyday of the 2015 refugee crisis. In this section, we probe further the validity of this finding by considering various sample restrictions and by inquiring into potential causal pathways that may underlie our results.

Concerning identification, the use of controls for quarterly and spatial fixed effects at neighborhood (1km grid) level and numerous house characteristics may still be insufficient for a causal interpretation of the estimated treatment effect of local RRC exposure on house price growth. Notwithstanding our expedient results from placebo tests for the exogeneity of region-specific pre-trends in house price growth to treatment, systematic, yet unobserved and uncontrolled for, selection of RRC placements by local authorities, potentially lobbied by selected communities, may still confound the relationship of interest. Moreover, identification may be complicated by endogenous location responses of local residents to RRC placements pre crisis. Such selection based on residents' (unobserved) preferences concerning the hosting of refugees in their neighborhoods may induce upward bias in the treatment effect of interest. The same applies to potential anticipation of elevated occupancy utilization in the advent of the crisis, as well as the possibility that capacities may be expanded nearby established (pre-crisis) RRC facilities, which may have found expression in pricing responses for nearby offer postings already pre crisis.[32]

To address these concerns, we use two modified versions of our research design that consider restricted estimation samples. Results are shown in TABLE 7. To ease comparison, columns (1) and (2) reproduce once more the treatment effects of our preferred baseline specifications in columns (6) and (7) in TABLE 2. First, to address anticipatory effects near existing RRC locations and associated concerns about endogenous relocations of residents near such sites, we exclude offer postings located within 15 minutes walking distance to pre-crisis (as of 31st December 2014) RRC sites

[32] Note that our conceptual research design already aims to reduce the possibility of such anticipatory pricing and mobility responses in the wake of the 2015 refugee crisis by disregarding offer postings throughout the months April to September 2015, i.e. the months in which asylum seeker inflows began to increase (see left graph of FIGURE 1) and such effects (if any) most likely materialized.

from the estimation sample.[33] The number of offers that are eliminated by this restriction is relatively small (575). The resulting increase in estimated treatments effects from -12.63 and -13.43 percentage points (see columns (1) and (2)) to -14.89 and -15.58 percentage points (see columns (3) and (4)), however, is remarkable and indicates that the actual impact of the shock (when unanticipated and not subject to ex ante endogenous resident selection) even exceeds that found in the baseline scenario.

Second, although the acquisition and opening of new RRCs was of utmost urgency in the heyday of the crisis, the locations of new RRC sites may have been (partly) endogenous to local residents' preferences concerning the allocation of refugees in their immediate neighborhood. However, any influence that lobbyists may have exerted on officiating bodies and their exact location choice of new RRC sites within administrative regions should arguably not have extended beyond their own local communities' administrative borders at municipality, respectively county level, which generates exogenous variation in local exposures to RRCs. To exploit this variation, we consider a sub-sample of house listings that are situated in treatment and control regions but solely lie in neighboring counties to those that actually housed a RRC. This restriction leads to a sizeable reduction in the estimation samples. The estimation sample with the broad control group (beyond 15 minutes walking distance of a RRC, yet in a neighboring county to the one that housed the RRC) shrinks to 462,499 observations, the estimation sample with the restricted control group (within 40 minutes walking distance of a RRC, yet in a neighboring county to the one that housed the RRC) falls to 2,301 observations. The number of observations in the treatment region (i.e. houses located within a 15 minutes walking distance of a RRC, yet in a neighboring county to the one that housed the RRC) is 608, out of which 520 and 88 offers have been posted in the pre- and (post-)crisis period, respectively. Due to this reduction in the number of observations, we elevate the level of regional fixed effects to municipalities. The corresponding results are shown in columns (5) and (6). Considering the estimation sample that includes the broader control region, we obtain a positive, albeit not statistically significant coefficient estimate for the treatment effect. Note that the weight attached to observations in the treatment regions is very low in this scenario. Restricting the sample further to only housing sub-markets within 40 minutes walking distance to RRCs that are adjacent to RRC hosting counties produces again a negatively signed and precisely estimated coefficient estimate (-0.1089) for the treatment effect of interest. Although lower than our baseline estimate in absolute terms (by approximately 2.54 percentage points (see column (2)), which considers regional fixed effects at the level of 1km grids, the estimated treatment effect exceeds by approximately 1.25 percentage points that of our robustness test in column (8) of TABLE 4, which also considers regional fixed effects at the level municipalities and may therefore be more directly comparable.

Next, we inquire into potential drivers of our findings. For this, we consider effect heterogeneity at various levels of aggregation and changes in the housing supply by treatment and control region as well as changes in the type of property posted on *ImmobilienScout24* in the observation period. First, fortified regional anti-refugee (more broadly anti-foreign) sentiments, both old and new, and corollaries thereof, such as demonstrations and violence against foreigners or refugee group quarters, may have been major triggers of disamenity perceptions of RRCs and hence drivers of our results. To explore this potential mechanism, we consider spatial treatment effect heterogeneity by exposure to both pre- and (post-)crisis criminal activities of the kind described above. For this purpose, we make use of two additional data sources. First, data from Krueger and Pischke (1997) on county-level crimes against foreigners committed between January 1991 and June 1993, originally derived from newspaper and magazine reports compiled by the *Archiv für Sozialpolitik* in Frankfurt, Germany; and second, records on anti-foreigner crimes (including attacks against refugee group quarters) committed in 2014 and 2015, which we obtained from a chronicle that is also based on news-

[33] TABLE A-3 in the appendix shows that regions with new RRC sites and regions without any RRC site are more similar than regions that housed RRCs already pre crisis with respect to several pre-crisis characteristics at municipality and county level, such as population and GDP per capita. Moreover, there are no indications that overall RRC placements were slanted towards remote regions. RRCs were rather placed in municipalities with bigger populations and less living space per capita. One in five RRCs was placed in urban counties.

TABLE 8: HETEROGENOUS TREATMENT EFFECTS - EXPOSURE TO ANTI-FOREIGNER CRIMES AT ADMINISTRATIVE-REGION LEVEL

	Dependent Variable: log *House Price*					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Regions:						
$WALK_{i,15min}$	−0.0016 (0.0735)	−0.0021 (0.0705)	−0.0194 (0.0429)	−0.0196 (0.0413)	−0.0261 (0.0438)	−0.0253 (0.0417)
$region_y^{anti-foreigner crime}$	0.0428 (0.0356)	−0.2479*** (0.0583)	0.0411 (0.0686)	0.3243*** (0.0963)	−0.0275 (0.0366)	0.3206** (0.1436)
$WALK_{i,15min} \times region_y^{anti-foreigner crime}$	−0.0026 (0.0745)	0.0019 (0.0728)	0.0920** (0.0457)	0.1035** (0.0438)	0.1086* (0.0583)	0.1212** (0.0564)
Differential House Price Growth in Crime Regions (no RRC):						
$POST_{Q4'15-Q3'16} \times region_y^{anti-foreigner crime}$	0.0155** (0.0061)	0.0649* (0.0348)	0.0238 (0.0163)	0.0227 (0.0390)	0.0176* (0.0093)	0.0287 (0.0358)
Treatment Effects by Type of Region (RRC in 15 min.):						
$POST_{Q4'15-Q3'16} \times WALK_{i,15min}$	−0.1598*** (0.0548)	−0.1347** (0.0627)	−0.1388*** (0.0468)	−0.1449*** (0.0515)	−0.1231*** (0.0470)	−0.1246** (0.0527)
$POST_{Q4'15-Q3'16} \times WALK_{i,15min} \times region_y^{anti-foreigner crime}$	0.0914* (0.0481)	0.0176 (0.0574)	0.1039* (0.0616)	0.0926 (0.0675)	0.0013 (0.0560)	−0.0080 (0.0619)
Year (y) committed	1991–93		2014		2015	
Administrative region	county		municipality		municipality	
# obs. in 15 min. walking distance of RRC by region	2,913		1,263		1,066	
(post)	(483)		(208)		(326)	
Grid-level fixed effects (1km)	yes	yes	yes	yes	yes	yes
Region-specific linear time trend (T,C)	yes	yes	yes	yes	yes	yes
Time fixed effects (quarters)	yes	yes	yes	yes	yes	yes
Property characteristics	yes	yes	yes	yes	yes	yes
Control sample	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.
Obs.	846,850	28,853	846,850	28,853	846,850	28,853

NOTES: The dependent variable is the log of the nominal house price posted. Administrative-region indicators at county (columns (1) and (2)), respectively municipality level (columns (3) to (6)), are equal to one if an administrative region witnessed in 1991–93, 2014 or 2015 at least one anti-foreigner crime with media coverage, in newspapers and magazines as compiled by the *Archiv für Sozialpolitik* in Frankfurt, Germany (archive data) for the years 1991–93 and used in Krueger and Pischke (1997) (columns (1) and (2)), or in the public chronicle *Courage against right wing violence (Mut gegen rechte Gewalt)* for the years 2014 and 2015 (columns (3) to (6)). Data used to construct these indicators stem from Krueger and Pischke (1997) (archive data) and a collation of two non-governmental organizations, *PRO ASYL* and the *Amadeu Antonio Foundation* (PRO ASYL and AAF, 2018), (public chronicle data). The number of observations is 5,632 within 15 minutes, and 23,221 within 15 to 40 minutes walking distance from a RRC. Standard errors are clustered at the 1km grid level. ***, **, * denote statistical significance at the 1%, 5% and 10% level.

paper reports which was initiated by the project *Courage against right wing violence (Mut gegen rechte Gewalt)* and which is frequently updated by two reputable non-governmental organizations, *PRO ASYL* and the *Amadeu Antonio Foundation* (see PRO ASYL and AAF (2018)).

As shown in TABLE 8, however, we do not find evidence for any commemoration or scaring effects from past exposure to anti-foreigner violence (at county level) in the early 1990s (columns (1) and (2)), nor do we find evidence that property in treatment regions which witnessed incidents against foreigners (at municipality level) in 2014, a year prior to the European refugee crisis (columns (3) and (4)), or in 2015, the year that saw refugee inflows at their peak (columns (5) and (6)), faced accelerated price penalties during the (post-)crisis period. Coefficient estimates on the associated triple interactions rather indicate that negative treatment effects are attenuated (if anything) in these regions. Moreover, estimated treatment effects for property in regions that were not exposed to such criminal activities, yet situated within 15 minutes walking distance of RRCs (see double interactions $POST_{Q4'15-Q3'16} \times WALK_{i,15min}$), are all negatively signed, precisely estimated, and exceed, in columns (1) to (4), those from our baseline estimations.

In similar vein, we investigated whether municipality-level differences in expressions of left- or right-wing sentiments, as measured by vote shares for 'The Left' party *'Die Linke'*, respectively the right-wing 'National Democratic Party of Germany' *'Nationaldemokratische Partei Deutschlands (NPD)'* in the federal state elections of 2011 sign responsible for our findings. The results from these explorations, however, do not provide any evidence for spatial effect heterogeneity related to these factors.[34]

The same holds true also for differences in treatment intensity as measured by occupancy figures of RRCs. Columns (1) to (4) of TABLE 9 show associated regression results for specifications that employ triple interactions using binaries that indicate top, respectively bottom quintiles of RRC occupancy utilization on 31st December 2015

[34] Tabulated results of these explorations are available from the author upon request.

TABLE 9: HETEROGENOUS TREATMENT EFFECTS - RRC OCCUPANCY (TOP & BOTTOM QUINTILES IN 2015)

	Dependent Variable: $\log House\,Price$			
	(1)	(2)	(3)	(4)
Treatment Regions:				
$WALK_{i,15min}$	0.0645 (0.0474)	0.0701 (0.0483)	-0.0151 (0.0449)	-0.0124 (0.0429)
$WALK_{i,15min} \times Indicator_i$	0.0905 (0.0567)	0.0917 (0.0572)	0.0587 (0.0616)	0.0599 (0.0606)
Treatment Effects by Type of Region:				
$POST_{Q4'15-Q3'16} \times WALK_{i,15min}$	-0.1080 (0.0677)	-0.1372^{*} (0.0708)	-0.1305^{***} (0.0465)	-0.1360^{***} (0.0508)
$POST_{Q4'15-Q3'16} \times WALK_{i,15min} \times Indicator_i$	0.0267 (0.0714)	0.0004 (0.0709)	0.0403 (0.0511)	0.0272 (0.0484)
Indicator	RRC Occupancy per 2014 county population			
Quintile	top		bottom	
# obs. in 15 min. walking distance of RRC by region	1,311		1,145	
(post)	(233)		(314)	
Grid-level fixed effects (1km)	yes	yes	yes	yes
Region-specific linear time trend (T,C)	yes	yes	yes	yes
Time fixed effects (quarters)	yes	yes	yes	yes
Property characteristics	yes	yes	yes	yes
Control sample	beyond 15 min.	>15–40 min.	beyond 15 min.	>15–40 min.
Obs.	846,850	28,853	846,850	28,853

NOTES: The dependent variable is the log of the nominal house price posted. Indicators are equal to one if a RRC belongs to the top, respectively bottom quintile of the distribution of RRC-level occupancy utilization on 31st December 2015, normalized by the 2014 county population. Data used to construct these indicators come from information on RRCs provided by state authorities of the three federal states of Baden-Wuerttemberg, Rhineland-Palatinate, and Mecklenburg-Western Pomerania. The number of observations is 5,632 within 15 minutes and 23,221 within 15 to 40 minutes walking distance to a RRC. Standard errors are clustered at the 1km grid level. ***, **, * denote statistical significance at the 1%, 5% and 10% level.

(normalized by 2014 county-level population). We also considered several additional specifications of this kind in which the occupancy indicators are defined according to thresholds of more than 1,000, respectively more than 1,500 and more than 2,000 spaces. The results for these specifications also do not provide evidence for a differential treatment effect at the intensive margin.[35]

In the following, we consider the possibility that regional differences in housing supply may be systematically linked to treatment. For this purpose, we graphically inspect the evolution of total offer postings, the average elapsed dwell time of offers (in months) on *ImmobilienScout24*, as well as the type of house listings (new inflows, existing offers, and outflows of offer postings) on the online broker's platform throughout the observation period (Q1 2012 to Q3 2016) by federal state, respectively treatment and control regions considered. The top left graph of FIGURE A-2 in the appendix depicts the distribution of houses on offer for sale (total offers) across time (quarters) by federal state.

[35]In further explorations, we considered effect heterogeneity by county-level exposure to characteristics of refugee populations in late 2015, which may serve as a proxy for characteristics of refugee populations accommodated in appendant RRCs. Corresponding results solely indicate that price growth penalties attached to houses located in vicinity of RRCs may have been attenuated if exposure to young refugees (aged 15 to 39) is particularly low (bottom quintile). This age group may be perceived by natives as one that is particularly engaged in criminal activities. What is more, these regions were at the same time characterized by a comparably low male to female sex ratio of refugees in that age group (on average 1.89 in the bottom quintile (std. dev. 0.12) compared to an average 3.25 (std. dev. 1.52) in the remainder of the treatment region). Data used to construct these indicators come from special data extracts from the *Statistic on Asylum Seekers' Benefits*. Due to legal data protection requirements, information on these characteristics is unavailable for 13 counties. These results are hence based only on a sub-sample of 2,444 observations on house listings in the treatment region (out of which 374 belong to the (post-)crisis period) and should therefore be interpreted with caution. Tabulated results of these explorations are attainable from the author upon request.

The pre- and (post-)crisis distribution of offers evolves very much in tandem across states, whereas the quantity of offers in the first two quarters of the (post-)crisis sample (Q4 2015 and Q1 2016) exceeds that of the entire pre-crisis period and falls below the same thereafter (Q2 2016 and Q3 2016).[36] What is more, the temporal distribution of offer postings in both control regions, i.e. beyond 15 minutes (bottom left graph) and in 15 to 40 minutes (bottom right graph) walking distance to RRCs, is similar to the one just described. However, in treatment regions, total quarterly offer postings in the entire (post-)crisis period exceed considerably those in the pre-crisis period (top right graph). The average number of offers per quarter increased by 28% (from 311 to 398) from the pre- to (post-)crisis period. Moreover, this absolute increase is largely driven by an increase in the number of existing offer postings by an average 90% (from 125 to 237).

Furthermore, the solid line depicts, for each region, the average dwell time (in months) of house listings on the online broker's platform. Whilst the average dwell time of offers is quite persistent for property listings in the broader control region (see bottom left graph), both the treatment and the comparably narrow control region (see top and bottom right graphs) show a similar pattern of decline in the average dwell time pre crisis. During this period, listings of houses located in proximity to 2015 RRCs spent on average 0.17 months longer on *ImmobilienScout24* than those of houses in 15 to 40 minutes walking distance. However, during the post-crisis period, offers in the restricted control sample spent on average 0.85 months less on the platform than offers in immediate vicinity of RRCs, which may be indicative of changes in the relative supply of and demand for single-family homes within this specific housing sub-market (surrounding RRCs).

Finally, since information on houses listed for sale on *ImmobilieScout24* can be modified at any time during the purchased posting term,[37] we investigated whether pricing responses were systematically linked to treatment. For this purpose, we employed two additional outcomes for the frequency of up- and downward price adjustments in DiD regressions. The results of these explorations do not provide evidence for any systematic changes in the differential number of price adjustments between the treatment and control regions from the pre- to the (post-)crisis period.[38]

5 Conclusion

This study investigated the impact of the 2015 mass arrival of refugees to Germany on house prices of single-family homes for sale in vicinity of operating refugee reception centers (RRCs) in late 2015. Using novel data on exact RRC locations provided by authorities at federal-state level and data on monthly offer listings of detached and semi-detached houses from Germany's leading online property broker *ImmobilienScout24*, we found strong evidence in difference-in-differences regressions for a negative effect of local exposures to RRCs on house price growth. Various sensitivity analyses and additional explorations corroborated these findings, including sample restrictions that exploit for identification arguably exogenous variation in local exposures to RRCs based on administrative boundaries at county level. These findings suggest that the much lauded "culture of welcoming" (*Willkommenskultur*), a term coined in the advent of the 2015 refugee crisis in Germany to characterize the reigning spirit and gestures of welcoming towards refugees among large parts of the public, a phenomenon which received much global press coverage and attention (Sueddeutsche Zeitung, 2015), may not have been all that strong, at least among resident populations in the backyard of RRCs (within 15 minutes walking distance) in the three federal states considered in our analysis.

Our finding of adverse consequences for residential property of a sudden mass inflow of migrants stands in contrast

[36]From Q1 2012 through Q1 2015 the average number of offers per quarter is 50,669. During Q4 2015 and Q1 2016, total quarterly offers increased by an average 5.2% (to 53,284). During Q2 2016 and Q3 2016, total offers dropped to an average of 40,790 per quarter.

[37]Fees payable depend amongst the type of property (houses, respectively apartments on offer for sale or rent) on the posting duration (two weeks, one or three months).

[38]Tabulated results of these explorations are attainable from the author upon request.

to related evidence on the relationship between mass immigrations and housing rents (Saiz, 2003; Kürschner, 2016). However, it is consistent with and complements the finding of Kürschner Rauck and Kvasnicka (2018) that county-level refugee immigration (its scale) in late 2015 had a negative effect on county-level rental price growth of apartments in Germany. In one of our sensitivity tests, we provide auxiliary evidence for a negative impact of county-level refugee immigration also on house price growth. Our finding furthermore aligns well with the evidence produced by van Vuuren et al. (2019), which consider also the consequences of the 2015 European Refugee Crisis for property prices. The study finds for the city of Gothenburg in Sweden that solely the announcement of building sites for refugees can affect prices of apartments in nearby regions and that such effects instantaneously materialized at the time building permits were issued. The detailed data on monthly offer prices we exploit in our paper is particularly well suited to unearth similar kinds of immediate impacts, such as instantaneous pricing responses to the opening of RRCs or their expanded occupancy utilization in the heyday of the 2015 refugee crisis. In addition, considering property prices in vicinity of refugee centers over longer time spans (i.e. 2009 to 2017 (Daams et al., 2019) and 1990 to 2015 (Dröes and Koster, 2019)), associated research for the Netherlands provides evidence that is consistent with that of the inquiries into the consequences of the 2015 refugee crisis in the two major European destination countries summarized above.

Estimated treatment effects appear highly persistent and homogenous across RRC localities, irrespective of the composition of the local voter population in 2011 and municipality-level exposure to anti-foreign violence throughout 2015. Considering treatment intensity according to end-of-year occupancy utilization of RRCs in 2015, we also do not find evidence for a differential impact of RRCs at the intensive margin. However, we do find that the quantity of offer postings increased between the pre- and (post-)crisis period by an average 28% in the treatment regions and that the average elapsed dwell time of offers in these regions relative to that of houses located in nearby control regions with up to 40 minutes walking distance to a RRC increased by a factor of five.

Altogether, our findings point towards an unequal distribution of the costs that are born by resident populations in regions in which refugee settlements are more concentrated, in particular in the form of RRCs. Notwithstanding the extraordinary circumstances authorities faced in late 2015, these costs were, at least in part, determined by policy, i.e. the distributional policies and housing decisions of state authorities. Costs of this kind can be avoided or at least lessened in the future handling of (large scale) refugee arrivals to Germany. Distortions of regional housing markets related to the placement of RRCs need to be taken into account when siting decisions are made. This may require potential redistribution of the costs through public transfers, including investments in local amenities and the provision of public goods. Moreover, existing RRC residency requirements for refugees could be profitably shortened to allow for a timely and more equitable re-allocation of refugees to appendant accommodations, in particular to decentralized housing. In the advent of the 2015 refugee crisis, however, RRC residency requirements were prolonged significantly, not shortened (Act on the Acceleration of Asylum Procedures (AsylVfBeschlG)). In view of the rapid exhaustion of existing capacities in late 2015, this measure appears counter-productive and casts doubt on whether benefits from a more timely, equitable and dispersed allocation of refugees may be reaped in the future.

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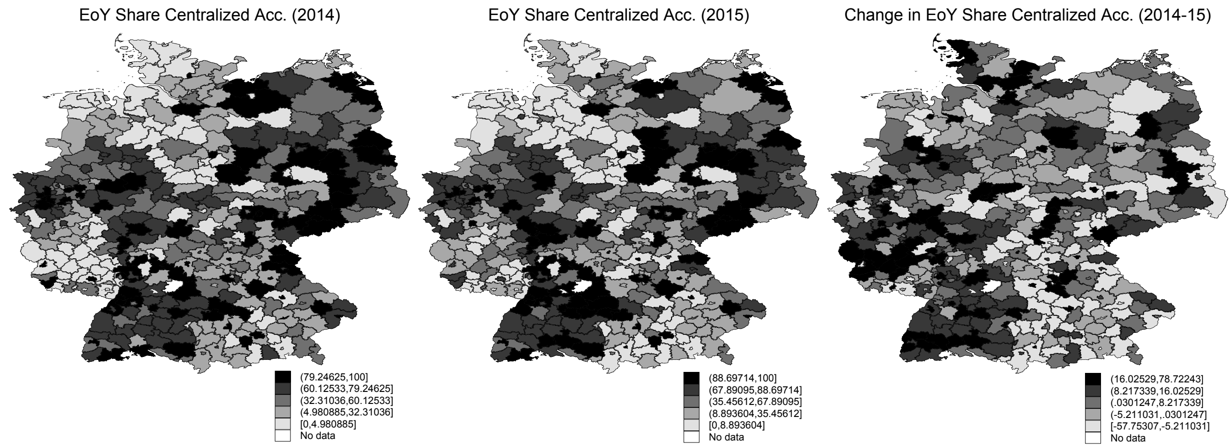
A Appendix

TABLE A-1: END OF YEAR ASYLUM SEEKERS IN 2015

	(1)	(2)	(3)	(4)
	Koenigstein Key	Asylum Seekers		(3)−(1)
Federal State:	%	total	%	%-pt.s
Baden-Wuerttemberg	**12.975**	**121,280**	**12.445**	**-0.530**
Bavaria	15.330	126,185	12.949	-2.382
Berlin	5.046	49,654	5.095	0.050
Brandenburg	3.081	29,339	3.011	-0.070
Bremen	0.941	11,159	1.145	0.204
Hamburg	2.528	23,051	2.365	-0.162
Hesse	7.316	68,122	6.990	-0.325
Mecklenburg-Western Pomerania	**2.042**	**20,332**	**2.086**	**0.045**
Lower Saxony	9.357	101,251	10.390	1.033
Northrhine-Westphalia	21.241	224,089	22.995	1.755
Rhineland-Palatinate	**4.835**	**49,475**	**5.077**	**0.242**
Saarland	1.216	10,411	1.068	-0.147
Saxony	5.101	45,749	4.695	-0.406
Saxony-Anhalt	2.858	30,073	3.086	0.228
Schleswig-Holstein	3.388	35,935	3.688	0.300
Thuringia	2.748	28,401	2.914	0.166
Total	100	974,506	100	

NOTES: The table shows for each federal state the total number of asylum seekers on 31st December 2015 recorded in the *Statistic on Asylum Seekers' Benefits* (column (2)), a state's share of refugees in the country (column (3)), and the federal state quota of refugees to be allocated according to the *Koenigstein Key* (column (1)). Percentage point differences between the two shares are rounded to the third decimal space (column (4)).

FIGURE A-1: REGIONAL VARIATION IN END OF YEAR (EOY) SHARE OF REFUGEES IN CENTRALIZED ACCOMMODATION AT COUNTY LEVEL (GERMANY)



NOTES: The figure depicts the county-level share of asylum seekers that were accommodated in centralized housing (i.e. any type of group quarter accommodation) on 31st December 2014 (left map), respectively 2015 (center map), as recorded in the *Statistic on Asylum Seekers' Benefits*, as well as the percentage point change in these shares between 2014 and 2015 (right map). County values are grouped into quintiles and depicted by different shades of gray (darker colors indicate larger values).

TABLE A-2: SUMMARY STATISTICS ON COUNTY-LEVEL SHARES OF REFUGEES IN CENTRALIZED ACCOMMODATION BY FEDERAL STATE

Variable	Obs	Mean	Std. Dev.	Min	Max
Baden-Wuerttemberg					
centralized share '14	42	69.236	18.076	3.472	94.825
centralized share '15	42	84.107	13.284	30.794	97.782
Δ centralized share	42	14.871	10.535	-0.477	60.162
Rhineland-Palatinate					
centralized share '14	36	16.807	31.928	0	100
centralized share '15	36	30.902	29.234	0	97.172
Δ centralized share	36	14.096	20.325	-21.887	59.150
Mecklenburg-Western Pomerania					
centralized share '14	8	52.908	33.546	0	92.829
centralized share '15	8	51.067	37.546	0	96.376
Δ centralized share	8	-1.841	11.066	-25.034	13.663

NOTE: The table shows for the three federal states in the estimation sample summary statistics on the county-level share of asylum seekers who were accommodated in centralized housing (i.e. any type of group quarter accommodation) on 31st December 2014, respectively 2015, as recorded in the *Statistic on Asylum Seekers' Benefits*, and the percentage point change in these shares.

TABLE A-3: PRE-CRISIS CHARACTERISTICS OF REFUGEE RECEPTION CENTER LOCATIONS AT MUNICIPALITY & COUNTY LEVEL

	RRC '15	new RRC '15	no RRC '15	*t*-test [(3)-(1)]	*t*-test [(3)-(2)]
	(1)	(2)	(3)	(4)	(5)
Municipality level (year: 2014):					
population	61,798	49,782	3,390	−58,408	−46,392
	[109,403]	[101,932]	[8,692]	(−27.36)	(−22.17)
taxpayer	31,226	25,402	1,698	−29,529	−23,705
	[56,650]	[53,161]	[4,382]	(−26.96)	(−22.01)
taxable income/taxpayer [tsd. EUR]	37.019	36.946	35.624	−1.395	−1.322
	[5.283]	[5.283]	[7.016]	(−1.37)	(−1.22)
flats p.c.	0.501	0.507	0.494	−0.007	−0.014
	[0.069]	[0.049]	[0.061]	(−0.77)	(−1.44)
living space p.c. [m^2]	45.734	46.892	53.239	7.506	6.347
	[6.538]	[5.002]	[12.086]	(4.29)	(3.40)
County level (years: 2013 & 2011):					
foreigner share [%]	8.936	8.332	8.914	−0.022	0.582
	[4.774]	[4.106]	[4.643]	(−0.02)	(0.59)
GDP p.c. [EUR]	36,393.21	34,961.68	32,618.22	−3,774.98	−2,343.45
	[14,853.32]	[14,274.53]	[10,581.62]	(−1.39)	(−0.86)
refugee share [%]	0.233	0.216	0.239	0.005	0.023
	[0.125]	[0.069]	[0.048]	(0.27)	(1.79)
property market: vacancy rate 2011 [%]	4.639	4.731	4.680	0.042	−0.051
	[1.573]	[1.564]	[1.279]	(0.14)	(−0.16)
property market: building permits (flats)/tsd. pop.	2.865	2.973	3.154	0.289	0.181
	[1.446]	[1.458]	[1.275]	(1.00)	(0.60)
(dis-)amenities: annual guest arrivals (tourism)/pop.	2.045	1.961	2.179	0.134	0.218
	[1.138]	[1.131]	[1.993]	(0.37)	(0.58)
(dis-)amenities: crimes/tsd. pop.	63.584	59.511	58.049	−5.535	−1.462
	[27.027]	[25.930]	[22.832]	(−1.04)	(−0.27)
# municipalities	48	42	3,932	3,980	3,974
# counties	39	34	49	88	83
# urban counties	12	8	11		

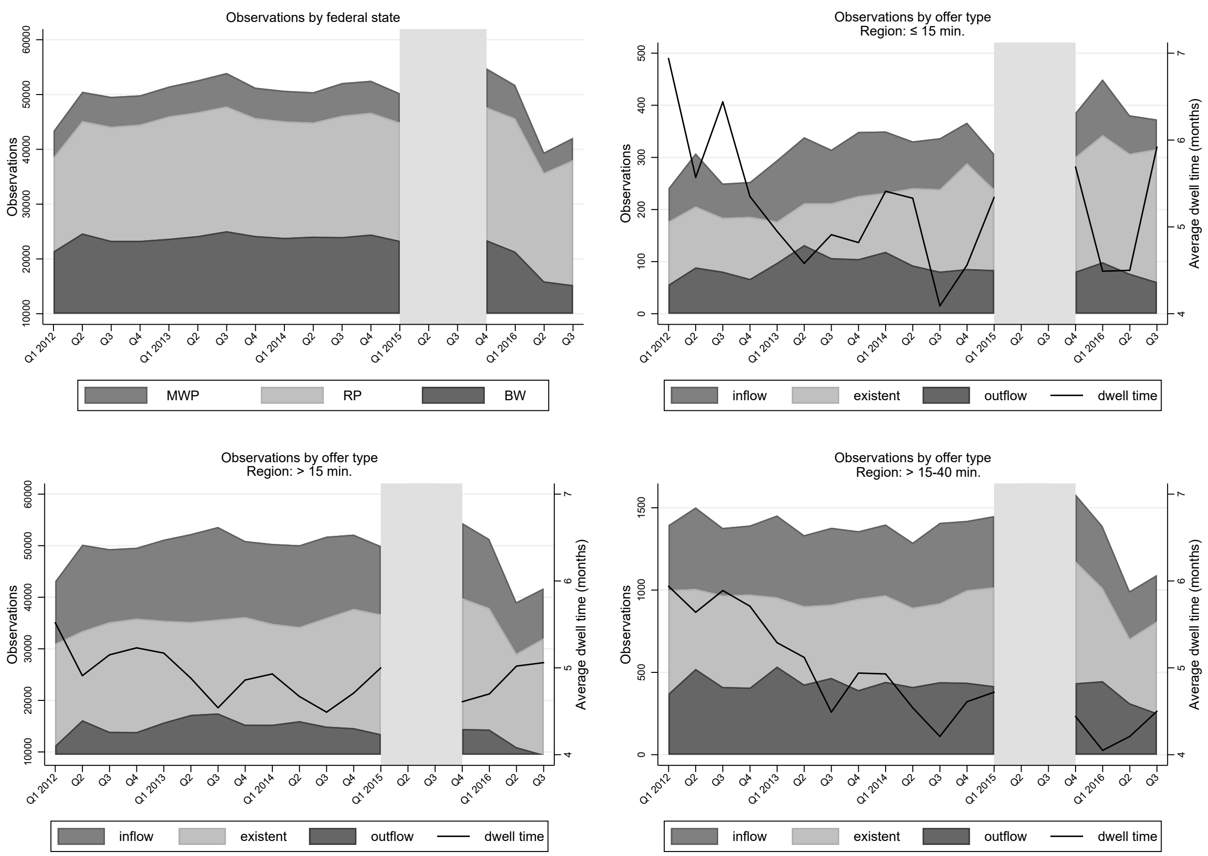
NOTES: Information on the number of taxpayers and taxable income per tax payer is missing for one, respectively two municipalities due to legal data protection requirements. Standard deviations are reported in square brackets (columns (1)-(3)). *t*-statistics are reported in round brackets (columns (4)-(5)).

TABLE A-4: PRE-CRISIS NEIGHBORHOOD CHARACTERISTICS BY DISTANCE TO REFUGEE RECEPTION CENTER (1KM GRID LEVEL, YEAR 2013)

	$\leq$ 15 min.	> 15 min.	> 15-40 min.	t-test [(2)-(1)]	t-test [(3)-(1)]
	(1)	(2)	(3)	(4)	(5)
Socio-economic characteristics					
unemployment rate	5.684	5.005	5.055	−0.679	−0.629
	[3.139]	[4.074]	[3.127]	(−2.09)	(−2.21)
foreigner share	11.990	5.217	9.964	−6.773	−2.026
	[7.793]	[6.002]	[7.856]	(−14.09)	(−2.85)
Household structure					
singles	41.726	31.063	35.986	−10.662	−5.739
	[17.806]	[20.032]	[18.870]	(−6.67)	(−3.39)
couples	31.059	32.901	30.946	1.842	−0.113
	[17.914]	[21.334]	[18.712]	(1.08)	(−0.07)
families	27.216	36.036	33.068	8.820	5.852
	[16.826]	[24.157]	[19.300]	(4.58)	(3.44)
Age-groups: males					
0-10 years	4.347	4.375	4.358	0.028	0.011
	[0.440]	[0.934]	[0.564]	(0.37)	(0.22)
10-20 years	5.014	5.445	5.053	0.431	0.038
	[0.654]	[1.214]	[0.760]	(4.46)	(0.58)
20-35 years	10.718	8.308	9.976	−2.410	−0.742
	[2.372]	[1.619]	[2.287]	(−18.56)	(−3.54)
35-50 years	10.779	11.070	10.710	0.291	−0.069
	[1.177]	[1.449]	[1.062]	(2.52)	(−0.70)
50-65 years	9.868	11.596	10.225	1.728	0.357
	[1.266]	[2.056]	[1.451]	(10.55)	(2.79)
$\geq$ 65 years	8.289	8.780	8.550	0.491	0.261
	[1.591]	[1.850]	[1.593]	(3.33)	(1.80)
Age-groups: females					
0-10 years	4.096	4.165	4.169	0.069	0.073
	[0.481]	[0.903]	[0.544]	(0.96)	(1.52)
10-20 years	4.868	5.098	4.813	0.230	−0.056
	[0.718]	[1.191]	[0.734]	(2.42)	(−0.84)
20-35 years	10.009	7.698	9.629	−2.311	−0.380
	[2.275]	[1.733]	[2.614]	(−16.65)	(−1.65)
35-50 years	10.465	10.847	10.439	0.382	−0.026
	[0.898]	[1.333]	[1.013]	(3.59)	(−0.29)
50-65 years	10.167	11.329	10.536	1.163	0.370
	[1.222]	[1.741]	[1.457]	(8.38)	(2.90)
$\geq$ 65 years	11.383	11.289	11.541	−0.093	0.158
	[2.289]	[2.511]	[2.263]	(−0.47)	(0.77)
Default risk					
low	44.173	57.634	50.516	13.461	6.343
	[21.497]	[26.511]	[23.827]	(6.37)	(3.00)
medium	34.571	29.390	32.833	−5.181	−1.739
	[13.503]	[19.836]	[15.535]	(−3.28)	(−1.27)
high	21.256	12.976	16.651	−8.281	−4.605
	[18.329]	[17.092]	[16.577]	(−6.06)	(−2.98)
Property composition					
detached & semi-detached house	37.350	65.208	43.416	27.858	6.066
	[21.298]	[21.607]	[23.069]	(16.15)	(2.95)
multi family house: 3-9 dwellings	39.021	26.693	36.702	−12.328	−2.319
	[12.813]	[15.765]	[15.220]	(−9.80)	(−1.74)
multi family house: $\geq$ 10 dwellings	19.740	5.122	16.110	−14.618	−3.630
	[17.453]	[10.527]	[16.708]	(−17.27)	(−2.37)
commercial building	3.890	2.977	3.772	−0.913	−0.117
	[6.554]	[6.500]	[8.335]	(−1.76)	(−0.16)
# 1km grids	158	20,644	522	20,802	680
Obs. (estimation sample)	5,632	841,218	23,221		

NOTES: The number of 1km grids that house properties in: a. exclusively the treatment group (i.e. within 15 minutes walking distance to a RRC) is 68 (column (1)), b. both the treatment and the control group is 90 (column (1)) and c. exclusively the control group (i.e. beyond 15 minutes walking distance) is 20,644 (column (2)). The number of 1km grids that contain exclusively houses in the restricted control group (i.e. located between 15 to 40 minutes walking distance to a RRC) is 522 (column (3)). Standard deviations are reported in square brackets (columns (1) to (3)). t-statistics are reported in round brackets (columns (4) to (5)).

FIGURE A-2: TEMPO-SPATIAL DISTRIBUTION OF OBSERVATIONS, OFFER TYPE BY LONGEVITY AND AVERAGE DWELL TIME (ESTIMATION SAMPLE)



NOTES: The figure depicts the distribution of houses on offer for sale (observations) across time (quarters) by federal state (top left graph), respectively by location of houses on offer for sale within 15 minutes (top right graph), beyond 15 minutes (bottom left graph), and between 15 to 40 minutes (bottom right graph) walking distance to a RRC. The abbreviations MWP, RP and BW correspond to the federal states Mecklenburg-Western Pomerania, Rhineland-Palatinate and Baden-Wuerttemberg, respectively. The solid line depicts, for property in each region (i.e. by walking distance to the nearest RRC), the average elapsed dwell time (in months) of offers on *ImmobilienScout24*.

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