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## The Impact of the Announcement of Temporary Building Sites for Refugees on House Prices in Gothenburg

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## ABSTRACT

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# The Impact of the Announcement of Temporary Building Sites for Refugees on House Prices in Gothenburg\*

We evaluate the price development of apartments in neighborhoods surrounding temporary housing for refugees using the unpredicted announcement of three building sites, targeting refugees, in Gothenburg. More in particular, we look at the price development in the year after the announcement. We use a causal outcome model that takes account of time and postal-code fixed effects and we define an area to be affected by the announcement based on walking distance. We find support for a small and significant price effect. In addition, we find that the price effect of the neighborhood depends on the income level of the neighborhood. We interpret this as evidence for the fact that not having to live close to refugees can be seen as a luxury good.

**JEL Classification:** O15, O18, R31

**Keywords:** house prices, migration

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# 1 Introduction

Investments in property as dwellings are often the largest financial decisions for individuals during their lifetime. Therefore, stability and predictability of the housing market in terms of price and price developments are desirable and governmental policy should target such predictability. Nevertheless, governments often need to make decisions that can have unpredictable and negative impacts on the price development in some neighborhoods. One such decision is the construction of refugee housing and therefore, this has been used as an argument when opposing specific (temporary) housing sites for refugees <sup>1</sup>. Despite its importance, there are hardly any papers investigating the impact of refugee housing on neighboring house prices (Lastrapes and Lebesmuehlbacher, 2017, constitute an exception). This knowledge gap creates a great difficulty in addressing the question of where and whether to build refugee housing. Therefore, we examine the recent events in Gothenburg, where the construction of twelve temporary housing sites was announced in January 2016 and investigate its impact on house prices.

For this purpose, we collect data on property sales within Gothenburg between 2014 - 2017 and generate a distance variable with a unique value for every property by extracting information on coordinates from geo-coded data. Similar to previous research on the housing market, this paper builds on the hedonic pricing method, which assumes that the price of a good is determined by both internal (such as the number of rooms and living area) as well as external factors (such as neighborhood factors). Due to the attention (pattern) that the announcement attracted, we argue that the locations were not anticipated and treat it as a natural experiment. We estimate an empirical model with time and region fixed effects to examine if the price per square meter trend has been different for the properties surrounding

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<sup>1</sup>See Göteborgs Posten, “SD mobiliserar mot flyktingboenden: “Folk är rädda”“, February, 4<sup>th</sup>, 2016.

the announced locations after the announcement, as compared to the trend in the rest of Gothenburg. Our objective is to investigate whether the announcement can have any impact rather than to find out by exactly how much the house prices changed for individual houses. The latter is a much more difficult question to answer because of general equilibrium effects and the sensitivity to the exact specification of the econometric model.

In our analysis, we focus on the price per square meter for apartments. This is a relatively liquid and homogeneous asset and we would like to separate it from single family houses. The reason for this separation is also due to the special regulations regarding the ownership of apartments in Sweden (Hjalmarsson and Hjalmarsson, 2009). Our results indicate that there has been a negative effect on surrounding apartment prices. More in particular, we find that houses that were sold in the year after the announcement and that were within a 15-minute walking distance were sold at a price that was around 3 percent lower than the houses that were sold at a longer distance from temporary housing for refugees. We perform a number of robustness checks such as changing the cutoff value of a 15-minute walk to a 10- and 20-minute walk, respectively, changing the comparison group and we delete some outliers in terms of price. Although the robustness checks indicate that some of our results may have been driven by the extremely cheap and expensive houses, we find an overall, robust and negative impact of the temporary building sites. We interpret this as a new result, especially since the refugee housing was not even built in the period of our analysis.

Even though the number of papers that have been looking at refugee housing is far from abundant, there is ample research on the impact of the inflow of immigrants on house prices. Using US data, Saiz (2007) finds that there was a larger appreciation of house prices in areas where immigrants more frequently settled between 1983 and 1997 due to an increased demand. Saiz and Wachter (2011) use decennial data for US metropolitan areas at the census

tract level (small areas with on average 4000 inhabitants) to evaluate if having immigrant neighbors affects house prices. They find that the appreciation of house prices is negatively affected by the growth of a neighborhood's immigrant share. Sá (2015) adopts a similar method as Saiz and Wachter (2011) using the UK labor force survey. She concludes that a 1 percent increase in the stock of immigrants results in a 1.7 percent decrease in house prices. She also finds that this impact is driven by a negative impact in the poorest neighborhoods (with the lowest levels of education), while the negative impact is shown to mainly be driven by the fact that the highest earning families in the poor neighborhoods move after an increase in the immigrant population. One problem of all mentioned research is that immigration in general has two opposing impacts. First, immigration has a positive impact on housing demand, which is driven directly by the immigrants. Second, due to the potential outflow of families, there is a negative impact on housing demand. These two opposing impacts make the total impact ambiguous and the results somewhat hard to interpret. We do not have this problem in our analysis, since the building sites do not directly compete with the existing housing supply.

The only paper that studies the impact of refugees on house prices is from Lastrapes and Lebesmuehlbacher (2017). They mention that refugees are demographically different from many other types of immigrants (especially in terms of education). Moreover, refugees typically do not make their choice of residence based on the economic situation of the neighborhood and, in many cases, do not have any say at all in their location decision. Lastrapes and Lebesmuehlbacher (2017) find that there is an ambiguous impact of refugees on house prices in general. However, they find an unambiguously small and negative impact for low-priced houses and houses of low quality. One challenge in their research is that refugees are typically located in hard-to-let residential housing, which is usually located in neighbor-

hoods that are not doing well in the first place. They correct for this problem by using an instrument (*i.e.* the change in the share of subsistence-only asylum seekers located in neighboring districts). We do not have this problem in our analysis since the chosen locations in Gothenburg were selected based on the objective to omit segregation and are therefore randomly chosen by the municipality.

Gautier, Siegmann and van Vuuren (2009) is closely related to our paper in terms of research question and empirical strategy. They investigate whether the assassination of the controversial movie maker Theo van Gogh in Amsterdam in 2004, carried out by a radical Muslim, affected house prices differently in these neighborhoods. Using a difference-in-difference approach, the authors discover that after the murder, house prices in neighborhoods with a relatively large number of migrants from a Muslim country experienced around a 0,07% lower increase per week.

In the final part of the paper, we try to explain our results. Since the housing sites form a different market than the present housing stock, supply changes are not likely to explain these results. Hence, we expect them to be largely driven by demand changes of potential buyers in the affected areas. As potential explanations, we investigate immigrants density, education and income levels. We find that difference in income forms the most important determinant for the severity of the price decrease. That is, low income neighborhoods do not seem to suffer any price loss. Hence, we interpret our results as evidence for the fact that not having to live close to refugees can be seen as a luxury good which becomes important for higher income levels. We also find that neither education nor the number of immigrants has a large impact on the results.

The paper has the following outline. We present a background to the events in 2015 and 2016 in Section 2. Section 3 describes the data and the key variables of interest and



is followed by our empirical strategy in Section 4. The results are presented in Section 5 and we investigate the robustness of the results in Section 6. Section 7 discusses potential explanations of our results. Section 8 presents our conclusions.

## 2 Background

Due to the war in Syria and an increased migration from northern Africa, the inflow of migrants to Sweden increased rapidly during the second half of 2015. At its peak in November 2015, Sweden received around 10,000 applications for asylum each week, as compared to around 2,000 per week in 2014. By the end of 2015, 162,877 people had been seeking asylum in Sweden - as compared to 81,301 in 2014 and 54,259 in 2013. This increase put a great deal of pressure on the Swedish reception system and led to problems providing housing for the new residents.

The Swedish housing market in the major cities was affected by the large inflow of refugees, but the market had already been struggling for some time before 2015, thus creating a large deficit of housing.<sup>2</sup> An estimated 700,000 new homes need to have been built by 2025 to cover this deficit. Further, there are pronounced insider-outsider effects in the Swedish housing market.<sup>3</sup> In the fall of 2015, the city of Gothenburg announced that it was to build 1000 apartments on temporary building permits targeting the newly arrived in order to address some of the more acute issues.

Locations for twelve temporary housing sites were presented on January 28<sup>th</sup>, 2016. Since the day of the announcement, there has been a substantial newspaper coverage on the issue. As an illustration, Figure 1 shows the newspaper coverage in one of the most important

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<sup>2</sup>For example, the average waiting time for a rental apartment has increased from about 2 to 4 years in the period 2012 - 2016 .

<sup>3</sup>See for example an article published in the Gaurdian by D. Crouch, “Pitfalls of rent restraints: why Stockholm’s model has failed many ”, August, 19<sup>th</sup>, 2015.

newspapers in the city of Gothenburg using the Google search words temporary, houses and refugees. The locations were chosen considering certain characteristics such as communication, service and a low share of refugees in that particular city area, the last one in order to combat segregation (Göteborgs stad, 2016). Figures 3 and 4 show the locations of the temporary building sites. Since the announcement, all sites have been debated and further evaluated. At the end of May 2017, the temporary building permit was approved for three sites (Kärralundsvallen and Askimsbadet and Lemmingsvallen). The remainder of the suggested locations have been rejected for various reasons. A timeline summarizing the events for all of the sites can be seen in Figure 2. Therefore, we only use these three building sites for our analysis. The number of apartments currently planned to be built on these three sites were 158. 44 out of these were planned at Kärralundsvallen and 114 were planned (with an equal split) at Askimsbadet and Lemmingsvallen. At the end of September 2017, Kärralundsvallen and Lemmingsvallen decided to no longer use their building permit due to the high costs involved. However, this date is quite far from our observation window.

### 3 Data

We collected data for around 20,000 properties from the site Booli.se (using their API to retrieve it). Booli is an independent search engine for private properties. The site collects publicly available data from most real-estate agencies' websites. One does not need to actively advertise on Booli for the property to be available on the website; it only needs to be available on the real-estate agencies' websites. Apart from the houses that are currently for sale, Booli also presents houses that have been sold since 2012 and this is the information that we use for our analysis. This data only includes sales made by open ascending bids (the most common way of selling a property in Sweden). All sales that took place before the

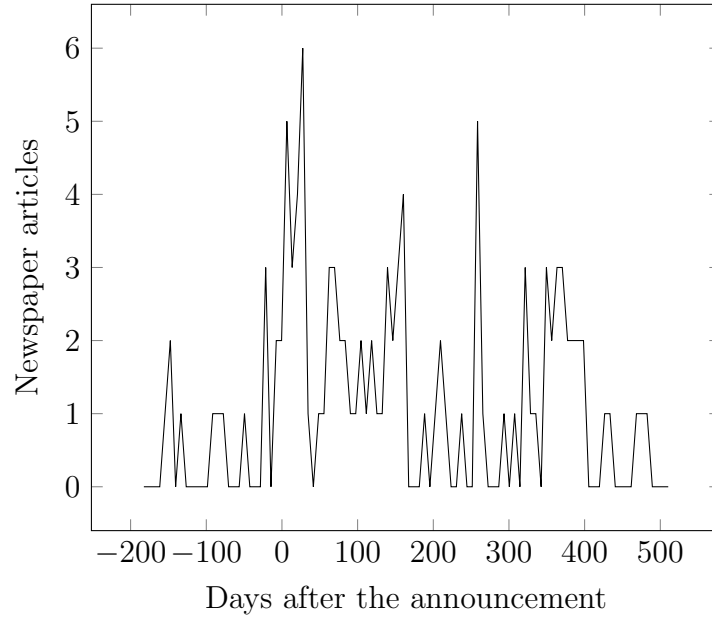


Figure 1: Number of newspaper articles published per week in Göteborgs Posten from 6 months before the announcement to one year after the announcement.

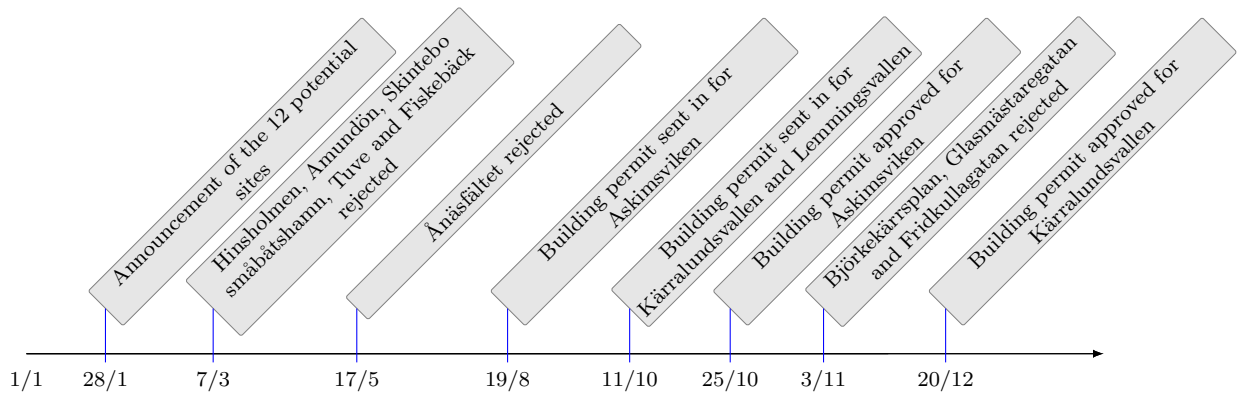


Figure 2: Timeline of events in 2016 (time-scale not proportional)

start of the bidding process are not public and hence not available through their API.

We use the open source routing machine (OSRM) to calculate the walking distances between the apartments and the temporary building sites. Using walking distances instead of using physical (Euclidean) distances has the advantage of accounting for the fact that there are many rivers, islands, forests and hills in Gothenburg and hence even though two sites might be very close to each other based on geographic location, the distance to move between these locations may take quite some time. Since we use rather short walking distances, *i.e.* 15 minutes for our baseline analysis, it can be expected that our choice does not depend to any great extent on the mode of transport. That is, the ranking between two locations is unlikely to be affected by this mode of transport. The reason for using walking distance is that we do not want our results to be affected by the fact that some houses are located very close to either a major railway station or a highway and are therefore closely related to a huge range of different locations including the temporary building sites.

In terms of geographical area, our data set covers the municipality where the sites are located (Gothenburg) and to which they border (Mölndal and Partille), while the time period in our most general specification is January 2014 - November 2017.<sup>4</sup> This means that the data consists of two years prior to the announcement of the temporary housing sites until almost two years after the announcement. However, since we typically look at the impact up to one year after the announcement, our baseline time window is from January 2014 to January 2017. A relatively short time period in our baseline analysis has been chosen to reduce the probability of things other than the treatment affecting the property prices. Further, both data sets have been cleaned from obvious misreports, missing values and properties not suitable for year-round living.<sup>5</sup>

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<sup>4</sup>Mölndal and Partille are included since at least one of the original twelve sites are within 1 kilometer of the municipality border.

<sup>5</sup>Properties not suitable for year-round living include summer houses and houses that only have running

A few notes should be made about the data set and the Swedish market. First, apartment buyers do not really *own* the property in Sweden. Instead, they acquire shares (or rights) of an economic association that owns the property. This is always the case for apartments, but also smaller houses (which are typical within Gothenburg) have this arrangement. The buyer of the shares obtains the right as a single user of the property, but she also has to pay a rent for using it. This arrangement is also common in other Nordic countries, but not elsewhere. Note that the rents are substantial and vary a great deal between the different properties (Hjalmarsson and Hjalmarsson, 2009). Hence, we cannot simply compare owned houses with houses that have these arrangements. This is the reason why we focus on apartments rather than on owned houses in our analysis. It implies that the total number of properties available for us is around 13,500. Second, selling prices are collected from Booli’s website as the last/highest bids. For most cases, this will correspond to the selling price and in those cases that it does not, it still serves as a good valuation of the property. There are two scenarios when the highest bid does not correspond to the selling price: (1) the seller sells to a lower bidder or does not sell at all and (2) the highest bidder withdraws her bid. Both reasons are not necessarily problematic in our analysis as long as the valuation derived from the highest bid is still consistent with the market valuation in the hedonic pricing model to be discussed in the next section. Third, the market for properties was booming in Gothenburg during our observation window. There is a deficit of properties and the average time that an object is for sale is short. It is not unusual that objects are sold before the start of the bidding process.<sup>6</sup> As previously mentioned, these sales are not included in our data set. This can potentially underestimate the (absolute value of the) impact of the temporary building sites if less houses are sold before the bidding starts in the areas close to these sites during

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water and electricity half of the year.

<sup>6</sup>According to the site Hemnet, 25% of all properties in Gothenburg were sold before the bidding began in 2016.

the period of our analysis. That is, some extremely good houses in these areas would not have been observed in our data set in the case that the building sites were not announced, because they would have been sold before the bidding starts.

As an alternative, we could have used the Swedish register for property in our analysis. However, apart from the fact that it will result in fewer control variables, this has two drawbacks. First, the majority of houses within the city of Gothenburg are apartments and these are not registered by the Swedish register. Second, the register only uses the date of the change of ownership, which is typically a couple of months after the date of the transaction. Booli uses the date at which the final offer was made and this is a more accurate date for our analysis than the date of changing ownership.

The descriptive statistics for the dependent variable, price per square meter, and the control variables are listed in Table 1. Control variables in the regressions consist of *living area*, *construction year*, *square meters per room*, *yearly rent per square meter* and *floor*. The descriptive statistics are split up between apartments situated at either within or outside the affected areas of the three sites that are still considered in May 2017 (Askimsbadet, Kärralundsvallen and Lemmingsvallen). From this point, only these three sites are included in our analysis if nothing else is mentioned.

We define whether a house is within the affected area by using walking distance to any of the temporary building sites. In our baseline analysis, we use a 15-minute walking distance. We expect that this distance is close enough to potentially be directly affected by the sites as well as indirectly affected by the rumors going around about how the sites will affect neighborhoods. As can be seen in Table 1, there are 495 apartments in the “affected” area that were sold during our time window. 150 of these were sold during the year after the announcement. We also consider other cut-off distances to investigate if this changes the

Table 1: Descriptive Statistics

	Apartments within		Apartments outside	
	mean	sd	mean	sd
Price	2874.18	1093.98	2725.35	1268.20
Price/ $m^2$	47.22	10.65	43.10	14.92
Living area ( $m^2$ )	61.91	21.13	65.43	22.98
Construction year	1951.62	23.25	1957.58	31.43
Rooms	2.37	0.96	2.43	0.93
$m^2$ /rooms	27.48	5.69	28.00	5.58
Rent	3410.08	1022.73	3667.86	1151.63
Yearly rent/ $m^2$	683.92	142.11	690.91	129.75
Floor	2.08	0.92	2.73	1.86
Number of observations	495		12907	

results. With a cut-off distance of 10 minutes, the number of observations within the affected area is 224 apartments (of which 65 were in the year after the announcement), while a 20 minute walking distance results in an affected group of 1073 apartments (of which 341 were in the year after the announcement).

Apartments in the affected areas are, on average, somewhat more expensive, especially when we look at the price per square meter. Remember that the aim was to build the temporary building sites in areas with good communications and services and thus not increase the segregation. It is therefore not surprising that properties in areas surrounding these sites on average have higher prices per square meter, compared to the rest of Gothenburg.

Apartments located outside the threshold distance are, on average, roughly 10% larger - resulting in the relationship previously mentioned concerning price and square meter price. There is also roughly a 10% difference in monthly rent, but it is also found that this difference can be completely ascribed to the difference in size; the rent per square meter is virtually identical for the two types of areas. Therefore, we use yearly rent per square meter as the control variable for our regressions. We do the same for the number of rooms that we alter

to square meters per room.<sup>7</sup>

We use the logarithm for the variables price per square meter, living area, square meter per room and yearly rent per square meter. This has the advantage that the relative difference in these variables is a better measurement than the absolute difference and also typically reduces heteroskedasticity (improving efficiency).

The total number of months in the data is 38 for both houses and apartments. 26 out of these are before the announcement and 12 are after. Postal codes will be included to control for external characteristics. For our baseline analysis, the dataset consists of 358 unique postal codes. Months and postal codes will be included as dummies in the empirical model in order to control for time and neighborhood fixed effects.

## 4 Empirical strategy

We use the following baseline model:

$$\log P_{ijt} = \beta_0 + \delta_1 D_i^{within} + \delta_2 D_i^{within} * D_t^{post} + \beta_3' X_i + \alpha_j + \lambda_t + U_{ijt}, \quad (4.1)$$

where  $i$  indicates individual property,  $j$  indicates neighborhood and  $t$  indicates time.  $\log P_{ijt}$  is our outcome variable and is defined as the log price per square meter. The variable  $D_i^{within}$  is a dummy indicating if a property is within the threshold (walking) distance to any of the temporary housing sites (*i.e.* within a 15-minute walking distance in our baseline model).  $D_t^{post}$  is a time dummy equal to 1 after the sites were announced and zero before. The interaction term between the two dummies  $D_t^{post} * D_i^{within}$  is the variable of interest: it measures the mean impact of the price per square meter for houses within the threshold

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<sup>7</sup>The alternation of the variables does not change the outcome in terms of significance. It only changes the parameter value and the fit of the model marginally, as compared to when including them in their original form.



distance after the announcement, as compared to houses outside the threshold distance, keeping everything else constant. We can interpret this impact as causal as long as  $U_{ijt}$  is independent from the regressors (including the fixed effects). Here, a causal impact implies that we can interpret it as the impact due to the announcement of the temporary building sites, *i.e.* the average observed houseprice minus the houseprice that we would have observed had there been no announcement.

In order to make the assumption of independence between  $U_{ijt}$  and  $D_i^{within}$  and  $D_i^{within} * D_t^{post}$  more plausible, a vector of house characteristics ( $X_i$ ) is included as well as time ( $\lambda_t$ ) and neighborhood ( $\alpha_j$ ) fixed effects. A key assumption is that the announcement - the treatment effect - can be considered as an exogenous and not anticipated event. As was already shown in Figure 1, the announcement of the twelve original sites received much attention by the public and media and we argue that this strengthens the assumption that they were unanticipated.

Our approach focuses on the investigation of whether there is any impact of the announcement of the temporary building sites on the price of the houses close to those building sites. We do not aim at estimating the exact impact or explaining such an impact based on distance or type of area or house. Hence, even though specification (4.1) may be restrictive, it suffices for our research question. For example, it may be possible that instead of (4.1), the actual relationship is as follows:

$$\log P_{ijt} = \beta_0 + \delta_2 f(D_i) D_t^{post} + \beta_3' X_i + \alpha_j + \lambda_t + U_{ijt},$$

where  $D_i$  is the distance to any of the temporary building sites and  $f$  is a decreasing function of this distance. Naturally, it may be interesting to estimate the function  $f$ , but it is clear that if  $f$  decreases sufficiently around the cut-off value, then our parameter of interest, *i.e.*

$\delta_2$  in (4.1), should be negative. Moreover, it is impossible to have a negative sign of  $\delta_2$  even in the case that  $f$  is completely flat for any value of  $D_i$ . Still, in order to obtain more confidence in our results, we do investigate whether our results are affected by changes in the cut-off value.

As stated above, our approach yields the possibility to obtain a causal treatment effect. However, there are some crucial assumptions for this approach to be applicable: (1) there must be a parallel trend before the announcement between the affected group and the remaining houses in order to justify the assumption that the trend would have been equal in the case of no treatment, (2) the method assumes that the unobserved characteristics before and after treatment are equal. This means that properties sold within the areas of interest have similar unobserved characteristics before and after the treatment. If this is not the case, for example, if the supply of properties in terms of their attractiveness is more skewed to the right after the treatment relative to before, a negative treatment effect could appear due to the fact that less expensive properties are bought at that time (an indirect treatment effect), (3) there cannot be any spillover effect from the affected to the non-affected areas. Although properties cannot be moved, this problem may arise due to general equilibrium effects, *i.e.* the fact that some areas become less attractive may increase the prices in other areas. Although this can have an impact on the precise value of our estimate, it will not affect the sign.

Since the model controls for time and neighborhood fixed effects, it controls for variables that differ across the different geographic areas but are constant over time (such as proximity to the ocean) and for variables that evolve over time, but are the same for all geographical areas (such as interest rate). Time fixed effects consist of monthly time dummy variables and neighborhood fixed effects are dummy variables for postal code.

## 5 Results

Table 2 lists the main results of this paper. The variable within “walking distance” implies that one of the three building sites is within a 15-minute walking distance from the apartment that is sold. The impact of the announcement of the building sites is measured by the variable that indicates that the house is within walking distance and sold after the date of the announcement. In our baseline analysis, we correct for a full set of dummy postal codes and hence the impact of the announcement of the building sites is identified from the fact that there are houses within a postal code that can either be shorter or longer than a 15-minute walk to the building site. We find that houses within this walking distance from the temporary building sites had a 3.3 percent lower increase in their house prices than houses situated further from those building sites. Based on the use of heteroskedasticity robust standard errors, as we do in the first column of Table 2, we can conclude that the impact is also significant. As a robustness check, we also provided the standard errors based on clustering of the postal codes. Note that the use of such clustered standard errors is only correct in the case that there is substantial heterogeneity in terms of the impact of the building sites between the postal codes (see Abadie, Athey and Wooldridge, 2017). If this is not the case, then these clustered standard errors are biased. In fact, the impact of the building sites is modeled to be homogeneous, *i.e.* specification (2), which makes the use of the standard heteroskedasticity robust standard errors more appropriate. Nevertheless, given the fact that we use three different building sites, which implies that the postal codes that are affected can be quite far from each other, it is possible that there is heterogeneity. As can be seen from the second column of Table 2, the clustered standard error of the impact is somewhat higher than the heteroskedasticity robust standard error, but the impact is still

significantly different from zero (based on a significance level of 5 percent).<sup>8</sup>

Note that our model is identified since there are postal code areas that have both affected and not affected houses, *i.e.* areas for which the cutoff line of a 15-minute walk crosses the postal code area. Both the use of a 15-minute walk and the use of the lowest level of aggregation of the postal codes are arbitrary. The assumption of a 15-minute walk is discussed in the next section. Here we discuss the use of the aggregation of postal codes. The third column of Table 2 lists our results in the case that we use only the first four digits of the postal code area. Surprisingly, our results are not affected to any extent by using such a level of aggregation although the standard error of the impact of the temporary building sites is slightly higher. When we use an even higher level of aggregation, then we are no longer able to say anything about the impact and the impact is quantitatively close to zero. However, it is questionable whether this is the right level of aggregation since it implies that many houses within a postal code can be quite different from each other. Remember that the temporary building sites are very good areas in Gothenburg and using a high level of aggregation implies that we compare very good areas that are within this long “walking distance” from the temporary building sites to lower ranked areas that are not close to the temporary building sites. This is exactly what happens when going from columns 1 and 2 to columns 3 and 4 of Table 2 when we look at the within walking distance dummy variable: it grows from virtually equal to zero to around 7 percent.

In order to investigate whether the temporary building sites had the same price development as the non-affected areas, we report the results of an extension of the model reported in columns 1 and 2 of Table 2 in column 5 of that table, where we also include an interaction dummy of the affected areas with the year 2015. This is to investigate whether our results

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<sup>8</sup>Note that the use of a more aggregate level of clustering would have been an error in this case. That is, the heterogeneity should be expected at the same level as the level used for the postal code dummy variables.

Table 2: Main results of the empirical model.

	Baseline (5 digit postal codes)	Baseline (clustered stand. error)	4 digit postal codes	3 digit postal codes	Parallel trend
Within walking distance	0.0050 (0.0190)	0.0050 (0.0155)	0.0583 (0.0139)	0.0698 (0.0124)	0.0003 (0.0211)
Within walking distance × Jan. 2015-Jan. 2016					0.0098 (0.0170)
Within walking distance × ≥ Jan. 2016	-0.0332 (0.0137)	-0.0332 (0.0172)	-0.0332 (0.0169)	-0.0096 (0.0184)	-0.0291 (0.0161)
Living area (in log $m^2$ )	-0.3794 (0.0068)	-0.3794 (0.0130)	-0.3960 (0.0101)	-0.3840 (0.0116)	-0.3793 (0.0068)
Construction year (× 100)	-0.0279 (0.0091)	-0.0279 (0.0265)	-0.0423 (0.0077)	-0.0065 (0.0086)	-0.0279 (0.0091)
Log rent per $m^2$	-0.1809 (0.0223)	-0.1809 (0.0310)	-0.2576 (0.0417)	-0.2982 (0.0468)	-0.1808 (0.0223)
Log $m^2$ per room	-0.0936 (0.0083)	-0.0936 (0.0117)	-0.0688 (0.0099)	-0.0428 (0.0118)	-0.0936 (0.0083)
Floor	0.0130 (0.0009)	0.0130 (0.0016)	0.0145 (0.0011)	0.0146 (0.0013)	0.0130 (0.0009)
Number of postal codes	358	358	72	21	358
Number of periods	37	37	37	37	37
Number of observations	10,067	10,067	10,067	10,067	10,067
$R^2$	0.901	0.901	0.834	0.734	0.901
Postal Code	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes

may be driven by a low increase in the house prices of these areas that was already initiated before the date of announcement. We find this not to be the case; if anything, it suggests that the house prices were increasing at a quicker rate in the affected areas, but the estimated impact is extremely small and far from significant.

The control variables have the expected signs. The variables living area, construction year, yearly rent per square meter and square meters per room all have a negative effect on the property price per square meter, while the opposite effect is found for floor. The neighborhood effects are statistically significant in the vast majority of cases, indicating that neighborhood characteristics are important for determining the price. Further, the time dummies show a long-term positive and significant price trend and signs of seasonality in the Gothenburg property market.

## 6 Robustness checks and extensions

We report a couple of robustness checks in this section. We look at the definition of affected areas in Section 6.1. We change the timing of the events (*i.e.* by looking at placebo treatments) in Section 6.2 and we delete outliers in Section 6.3. In Section 6.4, we change the definition of the non-affected control areas. We look at what happened in the period after our time window of interest in Section 6.5. Finally, we look at the impact among single-family houses. We report our results in Tables 3 to 7 and leave out the estimators of the control variables in order to improve the readability.

### 6.1 Change in the definition of the affected areas

Table 3 presents the results of our baseline model of Table 2 in the case that we use two different definitions of an affected area. The first new definition is based on a walking distance

of 10 minutes, while the second new definition looks at a walking distance of 20 minutes. Note that this does not only affect the definition of the affected areas, but also the notion of a comparison group. In general, the postal codes on which we base our identification, *i.e.* those postal codes that have houses from both the affected and non-affected areas, are at a longer distance from the building sites. This is reflected in the fact that the *Within walking distance* dummy variable goes from insignificantly negative to significantly positive. The impact of the building sites on the house price, *i.e.* the interacted dummy variable, is less affected by the change in the definition. Still, we find the impact to be reducing with distance. This is not surprising, since it can be argued that a 20-minute walking distance is too far for the temporary building site to have a large impact.

## 6.2 Change in the timing of the impact on the building sites

Table 4 lists the results of our baseline model of Table 2 in the case that we forward or delay the date of impact by one year. Naturally, the first exercise should not result in any impact unless there is something wrong with the date that we have chosen or with the choice of control and treatment group. The first column of Table 4 shows that there is indeed no impact with a point estimator virtually equal to zero. It is also very unlikely that there is an impact if we delay the date of the impact by one year. This implies that the dummy variable *Within walking distance* is now a mixture of the period before January 2016 and the period between January 2016 and January 2017. From our baseline results, we know that the first period was slightly positive and since this period is the longest by far, it is not surprising that the dummy variable is slightly positive as well. The impact variable of the temporary building sites is still somewhat negative, but it is far from significant.

	10	20
	minutes	minutes
Within walking distance	-0.0023 (0.0216)	0.0373 (0.0144)
Within walking distance $\times \geq$ Jan. 2016	-0.0356 (0.0212)	-0.0234 (0.0089)
Number of neighborhoods	358	358
Number of periods	37	37
Number of observations	10,067	10,067
$R^2$	0.901	0.901
Control variables	Yes	Yes
Postal Code	Yes	Yes
Time Dummies	Yes	Yes

Table 3: Robustness check: changes in the affected neighborhoods.

	1 year earlier	1 year later
Within walking distance	-0.0353 (0.0248)	0.0028 (0.0148)
Within walking distance $\times \geq$ Jan. 2016	0.0017 (0.0179)	-0.0082 (0.0125)
Number of neighborhoods	358	358
Number of periods	25	46
Number of observations	5,874	13,339
$R^2$	0.908	0.899
Control variables	Yes	Yes
Postal Code	Yes	Yes
Time Dummies	Yes	Yes

Table 4: Robustness check: impact of one year before and one year after the announcement.



### 6.3 Deletion of outliers

Table 5 shows the impact of our baseline results when we delete either 1 or 5 percent of the lowest and highest prices in our sample. Even though we do not expect a huge amount of measurement error in our analysis, it is still interesting to understand whether our results are based on observations that are located at the edges of the support of the price distribution. The point estimator in the case that we delete 1 percent of the highest and lowest prices is somewhat lower than the point estimator reported in the first column of Table 2. However, the difference is small and it does not really change our conclusions in a qualitative way. That is, we still have a negative and significant impact of the temporary building sites. This result changes whenever we delete the 5 percent highest and lowest prices in our data set, respectively. The point estimator is reduced even further and becomes insignificant. This result shows that our baseline results are somewhat affected by the choice of our data set. Nevertheless, a deletion of 10 percent of the observations is quite drastic and it is not surprising that the impact is not completely homogeneous with the price level.

### 6.4 Change in the control group

Our baseline model assumes that we can interpret the rest of Gothenburg, Partille and Mölndal as comparable to the affected areas in our analysis. This is an appropriate assumption as long as the coefficients of the controls (including the time trend) do not differ to any considerable extent between the affected and non-affected areas. However, especially the (poorer) northern part of Gothenburg differs considerably from the temporary building sites, which are located in prosperous areas in Gothenburg. Therefore, we use an alternative estimation technique in this subsection, where we only include houses that are located no further than a 60 minute walking distance from any of the temporary building sites. This

	1 percent deleted	5 percent deleted
Within walking distance	-0.0373 (0.0196)	-0.0006 (0.0082)
Within walking distance $\times \geq$ Jan. 2016	-0.0288 (0.0139)	-0.0196 (0.0176)
Number of neighborhoods	358	356
Number of periods	37	37
Number of observations	9,872	9,065
$R^2$	0.900	0.897
Control variables	Yes	Yes
Postal Code	Yes	Yes
Time Dummies	Yes	Yes

Table 5: *Robustness check: changes in the affected neighborhoods.*

excludes any of the poor neighborhoods in the northern part of Gothenburg, which can be reached only by car or public transport (due to the river that divides the northern from the southern part). The results of this exercise are listed in Table 6, where we have redone all estimations of Table 2. Note that we lose around 60 percent of our observations by using this approach. We find that the point estimator for our baseline model is negative and somewhat smaller in absolute value than what was reported in Table 2. Although the difference is not extreme, we also find that the coefficient is only significant in the case that we do not use clustered standard errors. As discussed earlier, not correcting for clusters means that we rule out strong heterogeneity in the impacts between the postal codes. Moreover, the results for higher levels of aggregation are no longer significant even though we remind ourselves that we have a strong preference for the low level of aggregation that we use in our baseline analysis. Finally, the parallel trend assumption is not violated for this estimation strategy.

	Baseline (5 digit postal codes)	Baseline (clustered stand. err.)	4 digit postal codes	3 digit postal codes	Parallel trend
Within walking distance	0.0027 (0.0186)	0.0027 (0.0159)	0.0516 (0.0147)	0.0610 (0.0128)	0.0089 (0.0204)
Within walking distance $\times < \text{Jan. 2016}$					0.0039 (0.0162)
Within walking distance $\times \geq \text{Jan. 2016}$	-0.0245 (0.0135)	-0.0245 (0.0181)	-0.0223 (0.0171)	0.0090 (0.0193)	-0.0229 (0.0153)
Number of neighborhoods	160	160	37	11	160
Number of periods	37	37	37	37	37
Number of observations	4,808	4,808	4,808	4,808	4,808
$R^2$	0.891	0.891	0.800	0.686	0.891
Control variables	Yes	Yes	Yes	Yes	Yes
Postal Codes	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes

*Table 6: Robustness check: restricted control group.*

## 6.5 Extension of the time window

Up till now, we have only looked at the period one year after the announcement. Obviously, this is the most important period for our analysis and it would be surprising if the (suspected) impact of the announcement would have had a time lag that is longer than one year. Still, it is interesting to investigate whether there was even an impact after a year. This is even more important since it was announced in September 2017 that the temporary building sites of Kärralundsvallen and Lemmingsvallen were canceled due to the high costs of these building sites. It implies that only the temporary building site of Askimsbadet was still planed after September 2017. Moreover, the impact of this building site may be small due to the fact that there are very few apartments within walking distance of this building site. Hence, even if there is any impact, it is unlikely that we would pick it up in our baseline analysis. Therefore, we look at two periods in Table 7: first the period from January 2016 to September 2017

and then the period from September 2017 to November 2017. The results from the same specifications as Table 2 are reported in Table 7. The results for the period from January 2016 to September 2017 are surprisingly similar to the earlier reported results. However, the impact after September 2017 is very small and far from significant.

## 6.6 Additional results for single-family houses

Our baseline results are only based on data from apartments which are relatively homogeneous. Nevertheless, it is interesting to investigate whether our results depend on this choice. Therefore, Table 8 lists the results for single-family houses. We apply the same methods as in the original Table 2, but we exclude floor and rent from the set of regressors. The results are in line with our earlier results. The regression coefficient of the impact of the temporary building sites is once more negative and even larger in absolute values than the results in Table 2. Apart from our baseline regression, the results are also significant at the 10 percent level. Based on these results, it is not possible to say that this contradicts our earlier results, even though the standard errors are somewhat too large to say that these results underline the earlier results.

In contrast to apartments, transactions of single-family houses are registered in Sweden and hence, it is interesting to investigate the impact of measurement error in the Booli dataset. That is, we can investigate the fact that some houses may not have been sold even after a final bid or they might have been sold, but at a substantially lower price. For this purpose, we merge our dataset with the records from the Swedish register. Unfortunately, we do not have a common registration number, nor do we have the exact address of the houses. Therefore, we decided to merge on basis of the GPS codes. We use the first six digits for this purpose. Based on this, we were not able to match 698 observations. This can be due to a

	Baseline (5 digit postal codes)	Baseline (clustered stand. err.)	4 digit postal codes	3 digit postal codes	Parallel trend
Within walking distance	0.0127 (0.0161)	0.0127 (0.0161)	0.0659 (0.0147)	0.0610 (0.0121)	0.0077 (0.0185)
Within walking distance × < Jan. 2016					0.0100 (0.0173)
Within walking distance × ≥ Jan. 2016-Sept. 2017	-0.0242 (0.0120)	-0.0242 (0.0148 )	-0.0218 (0.0143)	0.0017 (0.0155)	-0.0195 (0.0148)
Within distance × ≥ Sept. 2017	-0.0091 (0.0243)	-0.0091 (0.0181 )	0.0116 (0.0266)	0.0047 (0.0361)	-0.0039 (0.0260)
Number of neighborhoods	378	378	73	21	378
Number of periods	46	46	46	46	46
Number of observations	13,399	13,399	13,399	13,399	13,399
$R^2$	0.898	0.898	0.837	0.743	0.899
Control variables	Yes	Yes	Yes	Yes	Yes
Postal Code	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes

Table 7: Robustness check: longer time period.

	Baseline (5 digit postal codes)	Baseline (clustered stand. err.)	4 digit postal codes	3 digit postal codes	Parallel trend
Within walking distance	-0.0061 (0.0342)	-0.0061 (0.0412)	-0.0011 (0.0275)	0.1715 (0.0289)	-0.0185 (0.0185)
Within walking distance × < Jan. 2016					0.0226 (0.0480)
Within walking distance × ≥ Jan. 2016-Jan. 2017	-0.0735 (0.0470)	-0.0735 (0.0201)	-0.0816 (0.0488 )	-0.1034 (0.0522)	-0.0660 (0.0532)
Number of neighborhoods	272	272	71	23	272
Number of periods	36	36	36	36	36
Number of observations	2,233	2,233	2,233	2,233	2,233
$R^2$	0.816	0.816	0.707	0.581	0.816
Control variables	Yes	Yes	Yes	Yes	Yes
Postal Code	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes

Table 8: Robustness check: single-family houses after deletion of houses that were not merged with data from the register.

gap in the GPS coding, but it is also possible that some houses were not sold or were sold, but the change of ownership was after the period for which we have data (January 2017).<sup>9</sup> In addition, we also delete observations with a change in ownership before the transaction date on Booli. Such a transaction typically indicates that there were multiple transactions and the reported transaction on Booli was simply after the previous change of ownership. We also delete observations with a change in ownership one year or more after the transaction date from Booli or when the price on Booli does not match the price that was reported by the register. All these filters resulted in a deletion of 380 observations. The results of this exercise are reported in Table 9. We find that the results for the baseline analysis are not changed to any large extent, while the change in the other columns is somewhat larger. However, the large standard errors frustrate any definite conclusion here, most likely due to

<sup>9</sup>In fact, we had to do a recoding since the GPS coding of the register uses a coding system called SWEREF99 which was not used by the Booli website.

the low number of observations.

## 7 Discussion

The previous sections have shown evidence for a reduction in the apartment prices as a result of the announcement of the temporary building sites in the affected areas. In this section, we investigate some potential explanations for the drop in the apartment prices. Our preferred interpretation of the results is that supply effects did not play a large role since the houses that were planned to be built are unlikely to compete with the apartments that were for sale after the announcement of the temporary building sites. That is, prospective buyers of the apartments cannot buy a house in the temporary building sites, while it is unlikely to be the case that the municipality would have bought these (in general quite expensive) apartments for the housing of refugees in the case that the temporary building sites would not have been announced. This implies that the houses to be built on the temporary building sites are part of another market than the apartments that we investigate. Hence, if anything, it would reduce the prospective supply of apartments in these areas by reducing the opportunities for developers to buy the land in the future. However, that would have resulted in a price increase rather than a decrease. Therefore, it is more likely that demand effects have played a role. That is, the announcement of the temporary building sites lowered demand for the areas that were close to these sites since prospective buyers were averse to living close to the temporary building sites. Still, it is possible that the extent of the preference to live close to these sites depends on the attributes of the current and prospective residents as well as the characteristics of the neighborhood. In order to investigate this, we look at the impact of the number of existing immigrants, the education and the income level of the neighborhoods involved. For this purpose, we matched our dataset with data from the

	Baseline (5 digit postal codes)	Baseline (clustered stand. err.)	4 digit postal codes	3 digit postal codes	Parallel trend
Within walking distance	0.0648 (0.0505)	0.0648 (0.0588)	0.0207 (0.0375)	0.1510 (0.0331)	0.0419 (0.0583)
Within walking distance $\times < \text{Jan. 2016}$					0.0425 (0.0632)
Within walking distance $\times \geq \text{Jan. 2016-Jan. 2017}$	-0.0596 (0.0800)	-0.0596 (0.0802)	-0.0297 (0.0648)	-0.0306 (0.0488 )	-0.0428 (0.0835)
Number of neighborhoods	220	220	61	22	220
Number of periods	36	36	36	36	36
Number of observations	1,155	1,155	1,155	1,155	1,155
$R^2$	0.825	0.825	0.722	0.625	0.825
Control variables	Yes	Yes	Yes	Yes	Yes
Postal Code	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes

Table 9: Robustness check: single-family houses.

database of the municipality of Gothenburg and use the definition of primary neighborhoods (*primärområden*) which divides Gothenburg into 94 different neighborhoods. All our data is for the year 2016.

In order to investigate the impact of immigrants, we calculate the percentage of individuals in between 19 and 65 years of age who are born outside of Sweden and divide this by the total number of individuals within this age category who live in the neighborhood. Then, we use two different indicators for immigrant neighborhoods, those with more than 10 and those with more than 25 percent immigrants. In general, the impact of immigrants is not that clear a priori. Since the number of houses that is to be built in any of the temporary building sites is not that high in comparison to the present housing stock, it is not to be expected that the relative number of immigrants is changing a lot due to the building of these housing sites. Hence, it is possible that areas that do not have a lot of immigrants



are not affected by a large extent since the immigrants are still going to be in far minority. Economic theory states that the likelihood of integration of small groups is very high (Advani and Reich, 2015). On the other hand, going from a percentage of immigrants that is far below 10 percent (as is the case in many areas around Askimsbadet), to a percentage that is somewhat higher can at least psychologically be seen as a large difference, while going from a percentage well above 25 percent to a little bit higher can be interpreted as not a large difference at all.

Table 10 lists the results of our exercise. The first column reports the results of a regression with the 10 percent immigrants dummy variable as an additional variable. This column is introduced to compare it with our baseline results. Our results are not affected by any extend and we find a negative impact of the dummy variable of having more than 10 percent immigrants on the apartment price. Note that the sign and size of this dummy variable cannot be interpreted as causal as many unobserved neighborhood characteristics may be correlated with the number of immigrants residing in a particular neighborhood. The second column lists the main effects and shows that as soon as the percentage of immigrants is larger than 10 percent, then the impact is much smaller and only significantly different from zero for a significance level of 10 percent.<sup>10</sup> This suggests that our results are mainly driven by Swedish native buyers who prefer to be living in areas with a population that is almost entirely from a Swedish background. Prospective buyers who buy houses closer to immigrant dense neighborhoods seem not highly affected by the announcement of the temporary building sides. The third column presents results of a similar regression but here we also investigate the impact of neighborhoods with at least 25 percent of immigrants. We find here an even larger positive impact for the severely mixed neighborhoods even though the results are not statistically significant.

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<sup>10</sup>The t-value (not reported here) equals 1.64.

	Baseline + immigration rate	Immigration rate > 10%	Immigration rate ≥ 10 % and ≥ 25 %
Within walking distance	-0.0003 (0.0190)	0.0709 (0.0550)	0.0923 (0.0558)
Within walking distance × Immigration rate ≥ 10%		-0.0747 (0.0536)	-0.0973 (0.0547)
Within walking distance × Immigration rate ≥ 25%			-0.0150 (0.0384)
Within walking distance × ≥ Jan. 2016-Jan. 2017	-0.0317 (0.0136)	-0.0444 (0.0244)	-0.0449 (0.0249)
Within walking distance × ≥ Jan. 2016-Jan. 2017 × Immigration rate ≥ 10%		0.0178 (0.0289)	0.0143 (0.0297)
Within walking distance × ≥ Jan. 2016-Jan. 2017 × Immigration rate ≥ 25%			0.0514 (0.0493)
Immigration rate ≥ 10%	-0.0404 (0.0216)	0.0165 (0.0459)	0.0394 (0.0478)
Immigration rate ≥ 25%			-0.0276 (0.0081)
Number of neighborhoods	220	220	220
Number of periods	37	37	37
Number of observations	10,067	10,067	10,067
$R^2$	0.901	0.901	0.901
Control variables	Yes	Yes	Yes
Postal Code	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes

Table 10: Discussion: including the migration rate in the analysis.

Our second exercise is to investigate the impact of education on the price impact of the temporary building sites. We look at education levels within the neighborhood for the population in between 25 and 65 years of age.<sup>11</sup> From the ethnic and migration literature, it is known that higher levels of education typically reduce negative prejudice with respect to foreigners and hence we expect education to reduce the impact of the temporary building sites (see for example Hjerm, 2001). Also, it is suggested in the popular literature in Sweden that especially the lower educated are more likely to vote in favor of the anti-immigration party.<sup>12</sup> Here we use a dummy variable in the case that the neighborhood is ranked below the median of neighborhoods in terms of education level. The results of this exercise are listed in Table 11. The first column is again the baseline regression but now with the education dummy included. Low education levels in a neighborhood have a large negative impact on the apartment prices but again this can be interpreted as an effect of unobserved neighborhood characteristics rather than the presence of high educated individuals themselves. In the second column, we find, somewhat surprisingly, that a low level of education decreases the impact of the temporary building sites but the results are not significant. It is important to notice that this may be due to the fact that education is correlated with migration rates in the sense that low migration areas are typically areas with on average high education levels. Therefore, we include our immigration dummy together with the education levels in the third column of Table 11. Again, it is difficult to draw strong conclusions from this column, but we find that it is definitely not the case that low levels of education correlate with bigger impacts of the temporary building sites.

As a final exercise, we look at the impact of income on the price effect of temporary building sites by including a dummy that equals one in the case that the median income

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<sup>11</sup>Education levels are calculated in years of education, based on the highest grade obtained.

<sup>12</sup>See Aftonbladet, May, 26<sup>th</sup>, 2014.

	Baseline + education level	Education level	Immigration rate + education level
Within walking distance	0.0039 (0.0191)	0.0011 (0.0200)	0.0152 (0.0549)
Within walking distance × Education below median		-0.0062 (0.0253)	0.0164 (0.0304)
Within walking distance × Immigration rate $\geq 10\%$			-0.0307 (0.0551)
Within walking distance × $\geq$ Jan. 2016-Jan. 2017	-0.0260 (0.0181)	-0.0468 (0.0205)	-0.0425 (0.0243)
Within walking distance × $\geq$ Jan. 2016-Jan. 2017 × Education below median		0.0208 (0.0274)	0.0115 (0.0376)
Within walking distance × $\geq$ Jan. 2016-Jan. 2017 × Immigration rate $\geq 10\%$			0.0053 (0.0397)
Education level below median	-0.0412 (0.0080)	-0.0416 (0.0086)	-0.0419 (0.0086)
Immigration rate $\geq 10\%$			-0.0125 (0.0440)
Number of neighborhoods	220	220	220
Number of periods	37	37	37
Number of observations	10,067	10,067	10,067
$R^2$	0.901	0.901	0.901
Control variables	Yes	Yes	Yes
Postal Code	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes

*Table 11: Discussion: including the neighborhood education level in the analysis.*

is ranked among the 50 percent lowest medians over all neighborhoods. The results of this exercise are listed in Table 12. Again, the first column is for comparison where we include the dummy variable in the baseline analysis. The results are not affected to any large extent and the income dummy variable has an unsurprisingly negative impact on the price of the house. The second column extends our baseline by investigating the impact of the announcement in terms of the house price for different income levels. Even though the overall impact is negative and significant, the impact is insignificant and positive for low income neighborhoods. This is identical to the statement that our results are driven by above median income neighborhoods. Column 3 of Table 12 also includes education level and immigration rate into the analysis. We find these neighborhood characteristics to be of minor importance than income. Hence, our interpretation of the results is that not having to live close to refugees can be seen as a luxury good in a hedonic framework of housing characteristics. That is, for lower incomes, there are likely to be other characteristics that drive the value of the apartment, but as soon as individuals obtain a certain level of income, then having the opportunity not to live close to refugees becomes more important. We cannot speculate whether this is caused by high-income house buyers to be unwilling to buy houses close to immigrants or that they are only reluctant to buy a house close to refugees. We can equally not speculate whether these results are driven by a taste to live distant from refugees (or immigrants in general) or whether it is just a concern of the high-income buyers that their prospective investment might lose value. Again, we find education not to play any role: if anything, individuals in high-educated neighborhoods have the tendency to care even more about the negative impact of a potential high concentration of refugees on the neighborhood's apartment prices. Note that this is not necessarily in contrast with the earlier mentioned observation that the lowly educated are more likely to vote for anti-

migration parties. The highly educated have typically a high level of income and they can overcome the effects of high migration rates simply by living in areas with low concentrations of migrants. Hence, for these highly educated having high migration rates is not really an issue when it comes to their voting behavior. On the contrary, such behavior is impossible for the lowly educated. Also, the number of immigrants does not seem to play a role, but we should notice that high-income neighborhoods do not have a lot of immigrants in the first place.

## 8 Conclusions

This paper investigated whether property prices nearby temporary housing sites were affected after the announcement of the sites by using an empirical model with both postal code and time effects. We define the affected neighborhoods based on walking distance. The results suggest that the market for apartments has experienced a negative effect, although some of the robustness checks were insignificant.

Our results are especially interesting since the temporary building sites were not built during our observation window and, as it turned out, the majority of the building sites (2 out of 3) are not going to be built either. This implies that potential house buyers are forward looking and that only an announcement of a building site can even result in substantial price losses of the houses around that building site. Thus, this implies that governments should be careful with their announcements.

	Baseline + income level	Income level	Income + education level + immigration rate
Within walking distance	0.0043 (0.0191)	0.0006 (0.0211)	0.0176 (0.0554)
Within walking distance × Income below median		0.0286 (0.0328)	-0.0238 (0.0335)
Within walking distance × Education below median			0.0131 (0.0306)
Within walking distance × Immigration rate $\geq 10\%$			-0.0349 (0.0551)
Within walking distance × $\geq$ Jan. 2016-Jan. 2017	-0.0323 (0.0137)	-0.0357 (0.0138)	-0.0423 (0.0243)
Within walking distance × $\geq$ Jan. 2016-Jan. 2017 × Income below median		0.1508 (0.0824)	0.1475 (0.0827)
Within walking distance × $\geq$ Jan. 2016-Jan. 2017 × Education below median			0.0061 (0.0377)
Within walking distance × $\geq$ Jan. 2016-Jan. 2017 × Immigration rate $\geq 10\%$			0.0053 (0.0397)
Below median income	-0.0279 (0.0108)	-0.0280 (0.0108)	-0.0111 (0.0101)
Education level below median			-0.0378 (0.0090)
Immigration rate $\geq 10\%$			-0.0078 (0.0442)
Number of neighborhoods	220	220	220
Number of periods	37	37	37
Number of observations	10,067	10,067	10,067
$R^2$	0.901	0.901	0.902
Control variables	Yes	Yes	Yes
Postal Code	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes

Table 12: Discussion: including the neighborhood income level in the analysis.

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## Appendix

Table 13: *Variable definition and sources*

Variable	Description	Source
Price	The price <b>at</b> which the property was sold, in thousand SEK.	Booli.se, Lantmäteriet
Square meter price	<i>Price/Living area</i>	Booli.se, Lantmäteriet
Distance	Distance to closest refugee site expressed in kilometers, calculated using coordinates in the WGS 84 and SWEREF 99 standard	Booli.se, Lantmäteriet
Within	Binary variable, 1 if property located within a threshold cut-off distance - (0.5, 1 or 1.5 kilometer) of a refugee site, 0 otherwise	Booli.se, Goteborg.se, Lantmäteriet
Post	Dummy variable indicating 1 if the month of sale was after the announcement of placement of refugee sites, 0 otherwise.	Booli.se, Goteborg.se, Lantmäteriet
Living area	Surface of property measured in square meters.	Booli.se, Lantmäteriet
Construction year	Year construction of the property was finished.	Booli.se, Lantmäteriet
Floor	Floor on which the apartment is located in the building.	Booli.se
Rent	Monthly rent paid to the building association.	Booli.se
Yearly rent/ $m^2$	<i>(Rent * 12)/Living area.</i>	Booli.se
Rooms	Number of rooms in the apartment excluding kitchen and bathroom(s).	Booli.se
$m^2$ /rooms	<i>Living area/Rooms.</i>	Booli.se
Additional area	Area connected to the house but not defined as Living area, e.g. Basement and garage	Lantmäteriet
House type	Category of housetype where villa=1, chain house=2 and town house=3.	Lantmäteriet
Lot size	Size of lot on which the single-family house is situated in square meters.	Lantmäteriet
Value points	A measure of points for the standard of the single-family house, based on the buildings material and equipment.	Lantmäteriet
Waterfront house	Dummy variable, 1 if house <b>is situated</b> within 150 <b>meters</b> to the shoreline, 0 otherwise	Lantmäteriet
Postal code	Postal code. Specified as a 5-digit postal code or as 4- or 3-digit code, using the first 3 or 4 digits of the postal code.	Booli.se, Lantmäteriet
Time	On a monthly basis. Ranges from January 2014 to February 2017.	Booli.se, Lantmäteriet

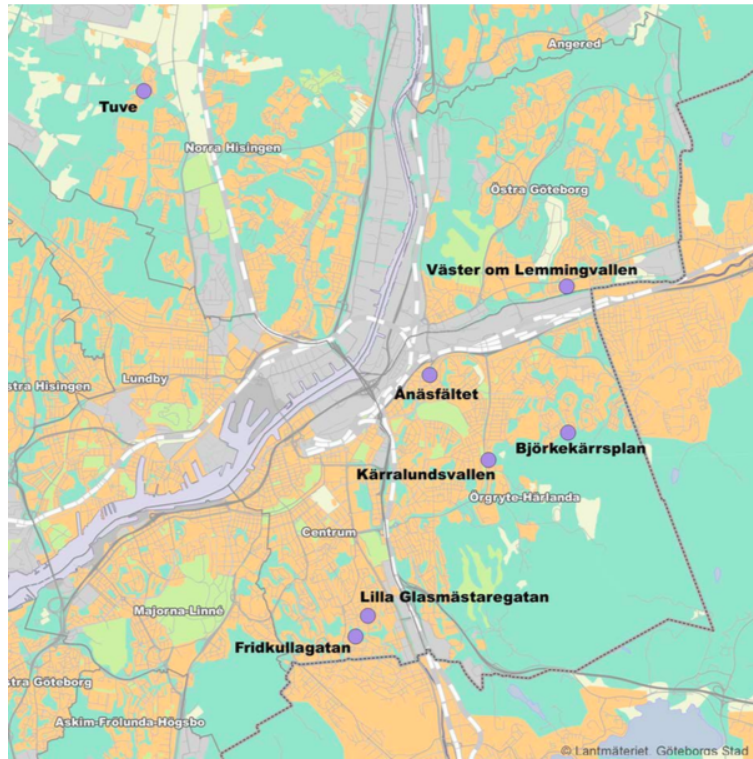


Figure 3: Location of sites - East

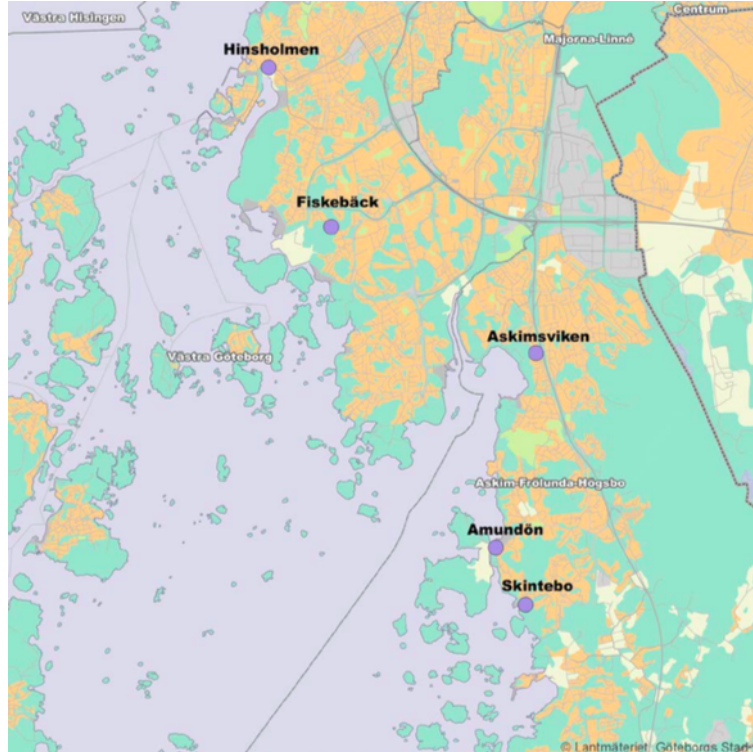


Figure 4: Location of sites - West