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Did the refugee crisis affect local property prices?

The case of temporary housing sites in Gothenburg

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Abstract

We evaluate the property price development in neighborhoods surrounding temporary housing sites during "rumor phase" of construction. Property data from Booli.se and Lantmäteriet between 2014 - 2017 is used together with a simple hedonic pricing approach. We exploit the natural experiment setting of three simultaneous and unpredicted announced housing sites, targeting refugees, in Gothenburg and perform a difference-in-difference estimation to assess the effect. The results indicate that the single-family house market have been unaffected by the announcement while the market for apartments potentially have experienced a lower price development in areas surrounding the sites compared to the rest of Gothenburg. The results are however inconclusive and we highly encourage future research to replicate our study once the sites are up and running to evaluate the final effect as well as to identify the underlying determinants.

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1 Introduction

The investment in property, either apartment or house is often the largest financial decision for an individual during his or her lifetime. It is essential with stability and predictability in the housing market for such a long-term commitment to be feasible. Various unpredicted neighborhood changes could affect this economic commitment adversely. Despite scarce evidence in support of a negative price effect from refugee housing on nearby residential property values, this has been used as an argument when opposing specific temporary housing sites for refugees in Gothenburg (Göteborgs Posten, 2016). The lack of systematic analysis regarding the subject results in a debate based on feelings and beliefs rather than facts. This knowledge gap provides great difficulty in addressing this opposition rationally and risk jeopardizing every proposed site in the vicinity of residential areas. In an attempt to fill this gap, we examine the recent events in Gothenburg where the construction of twelve temporary housing sites were announced in January 2016, and try to answer the following research question:

Were property prices surrounding the temporary housing sites affected negatively after the announcement?

To the best of our knowledge the effect of temporary housing sites on surrounding property prices have not yet been investigated in Sweden. To do this 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 thesis builds on the hedonic pricing method, which assumes that the price of a good is determined by both internal (e.g. number of rooms and living area) as well as external factors (e.g. environmental and neighborhood factors). Due to the attention the announcement drew we argue that the locations were not anticipated and treat it as a natural experiment. We estimate a difference-in-difference model to examine if the price/m^2 trend has been different for the properties surrounding the announced locations after the announcement compared to the trend in the rest of Gothenburg.

We find no conclusive evidence of the announcement in January having an effect on the surrounding property prices. Properties are divided between apartments and single-family houses and we examine the two markets separately and conduct a number of robustness checks. All results obtained from the house market indicate that it has been unaffected by the announcement. For apartments the results are somewhat ambiguous. They indicate that there has been a negative effect on surrounding apartment prices and that these apartments have been sold for around 2 - 3 % less than apartments outside. However, the results are sensitive to the robustness checks and the number of observations surrounding the sites are relatively few. Additionally, there are some concerns about the level of clustering for apartments and the potential presence of type 1 errors. Due to this, we argue that it is too early to draw any conclusions about whether the sites have been successful and to make any major policy recommendations. With the sites yet to be built our recommendation is that future research replicate this study once the sites are up and running with a more extensive data set and evaluate the final effect.

The thesis will have the following outline. In section 1.1 we present a short background of the events in 2015 and 2016 to provide context for the thesis. In section 2 we will give a brief review of the published literature in the field. In section 3 the theoretical framework and the main hypothesis is presented. Section 4 describe the data and key variables of interest and is followed by our empirical strategy in section 5. The results along with the robustness checks is presented in section 6 and analyzed in section 7. Section 8 presents our conclusion of the thesis. All tables except summary statistics are found in the appendix.

1.1 Background

During 2015 there was a surge in migration into Europe due to the wars in Syria and due to emigration from northern Africa. A large fraction migrated to the northern part of Europe as a result of mainly two factors; (1) the Schengen system that allows for freedom of movement within the European Union and (2) the unwillingness to practice the Dublin regulation[1]. During the end of the summer in 2015 the inflow of migrants to Sweden increased rapidly. During November 2015 Sweden received around 10 000 applications for asylum each week, compared to around 2000 à week in 2014 (Migrationsverket, 2017b). By the end of 2015, 162 877 people had sought asylum in Sweden - compared to 81 301 in 2014 and 54 259 in 2013 (Migrationsverket, 2017a). This increase put a lot of pressure on the Swedish reception system and led to problems in terms of providing security and finding shelter for new residents as well as for Swedish citizens. Due to this, passport controls when entering Sweden were reinstated on the 4^{th} of January 2016 and on the 7^{th} of February 2017, as a result of a recommendation of the council of the EU, the passport control was extended until the 10^{th} of May 2017 (Regeringen, 2017a). The passport control was removed in May 2017 but the border control was further extended (Regeringen, 2017b).

[1]The EU member state's responsibility of examining asylum applications at first point of entry to the union (European Commission, 2017).

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, creating a large deficit of housing.[2] An estimated 700 000 new homes need to be built by 2025 to cover this deficit (Boverket, 2016). Further there are pronounced insider-outsider effects on the Swedish housing market. In order to address some of the more acute issues the city of Gothenburg announced, in the fall of 2015 that they were to build 1000 apartments on temporary building permits, as soon as possible, targeting newly arrived.

Locations for twelve temporary housing sites were presented on January 28^{th} of 2016. The locations were chosen considering certain characteristics such as communication, service and low share of refugees in that particular city area, the last one in order to combat segregation (Göteborg Stad, 2016). Since the announcement, all sites have been debated and further evaluated. As of today (19^{th} of May 2017) two sites (Kärralundsvallen and Askimsbadet) have had their temporary building permit approved and one of them is awaiting approval (Lemmingsvallen). The remainder of the suggested locations have for various reasons been rejected. A time line summarizing the events for all of the sites can be seen in figure 1. The number of apartments currently planned to build on these three sites are 158. Out of these, 44 are planned at Kärralundsvallen and 114 are planned at Askimsbadet and Lemmingsvallen, with an equal split (Förvaltnings AB Framtiden, 2017).

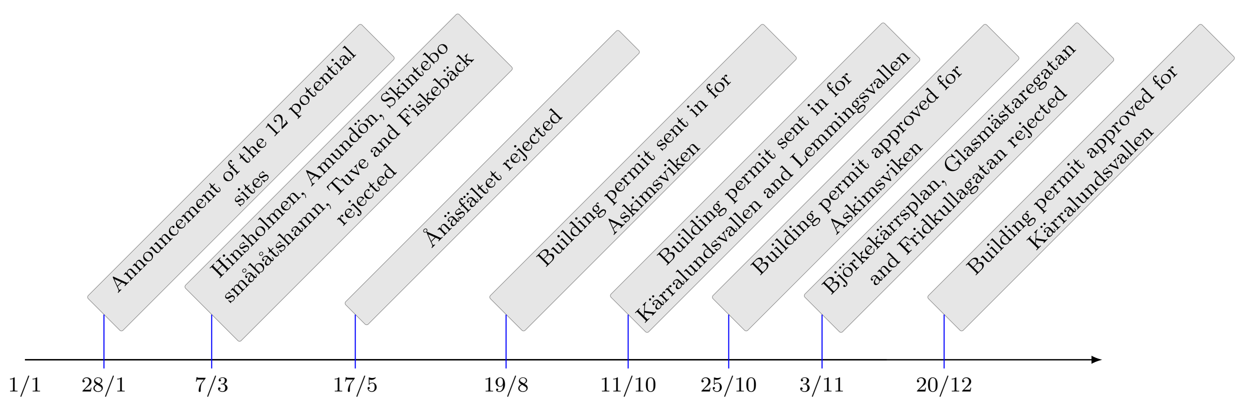

Figure 1: Time-line of events in 2016 (time-scale not proportional)

[2]For example, the average waiting time for a rental apartment have increased from about 2 to 4 years between 2012 - 2016 (Boplats Göteborg AB, 2017).

2 Literature review

Property prices is a subject that has been studied frequently in the past. Many of the previous papers aim to find the determinants of property prices and the willingness to pay for specific characteristics using the hedonic pricing method, made famous by Rosen (1974). According to Follain & Jimenez (1985) some of the commonly evaluated determinants in these papers are; living area, number of rooms, lot size, structural quality, public quality, air quality, distance to central business district, noise exposure, sanitary, dwelling quality, etc. The main focus of this paper will be to study the effect of distance to temporary housing sites and not the individual characteristics of these commonly used determinants. Some of them will however still serve as control variables in the analysis.[3]

Other papers are, similar to this, interested in examining how external characteristics affect property prices. Li & Brown (1980) find that visual quality (defined as the individuals' opinion of the esthetic in the neighborhood) and accessibility (to the ocean, highway and recreation area to mention a few) have a positive effect on price while congestion and pollution have a negative effect.

Though there is, to the best of our knowledge, a gap in the research field regarding how closeness to temporary housing sites affects property prices, similar research questions have been considered. Cobb-Clark & Sinning (2011) find that the housing price appreciation for immigrant homeowners was 41,7 % while the appreciation for natives was 59,4 % between 2001 - 2006 in Australia. The authors have a hard time explaining this result but conclude that the most likely explanation is that there are differences between neighborhoods within zip code districts which they could not control for. Saiz & Wachter (2011) use decennial data for US metropolitan areas on census tract level (small areas with on average 4000 inhabitants) to evaluate if having immigrant neighbors affect housing prices. They find that the appreciation of housing prices increased slower in neighborhoods where the fractions of immigrants were increasing. Saiz (2007) finds contradicting result in that areas where immigrants more frequently settled between 1983 to 1997 in US cities had a larger appreciation of housing prices due to an increased demand.

One of the goals with the temporary houses is to reduce segregation (Göteborg Stad, 2016). The effects of segregation in Sweden have been studied by Hällsten, Szulkin & Sarnecki (2013). The authors follow 63 462 individuals who have completed the last year of compulsory school (9^{th} grade) in Stockholm between 1990 - 1993. By collecting register data on all

[3] The interested reader can find out more about the effect of individual characteristics in Follain & Jimenez's paper.

individuals in the data set between the ages of 16 to 28 - 30 they conclude that children of immigrants are overrepresented in the criminal record. However, when controlling for a number of socio-economic factors a large fraction of this difference can be explained by family resources and neighborhood segregation. They also hypothesis that the remaining difference in observed recorded criminality could be due to discrimination in the legal system and from anti-social behavior (from trauma).

Keels, et al. (2005) investigate the so-called Gautreaux residential mobility program, which was a court ordered anti-segregation program in Chicago. The authors follow poor black families that, through the program, moved (between 1976 - 1998) from poor and segregated areas into higher socioeconomic status and low-criminal areas. After the program ended almost all families participating in the program moved to similar areas, with respect to income level and crime rates, as they were initially assigned to. Additionally, the families that participated in the program earned more and were more likely to be employed after placement, compared to families who did not participate. This effect was larger for those that were placed in the suburbs. We see many similarities with this program and the intention with the temporary housing project in Gothenburg.

Gautier, Siegmann & van Vuuren's (2009) paper is closely related our in terms of research question and empirical strategy. The authors divide Amsterdam into type 1 and type 2 neighborhoods, where type 1 is defined as neighborhoods consisting of more than 25 % inhabitants of an ethnic minority from a Muslim country. They investigate if the murder of the controversial filmmaker Theo van Gogh in Amsterdam in 2004, carried out by a radical islamist, affected house prices differently in these neighborhoods. Using a DiD approach, the authors discover that after the murder, house prices in type 1 neighborhoods decreased with around 0,07 % per week for a 10-month period compared to type 2 neighborhoods, which in total add up to a 3 % difference. The authors conclude that this was a result of the decreasing willingness of native Dutch individuals to live in type 1 areas. With the reduction of house prices going on for 10 months, the authors argue that the speculative component of the housing market was of importance. Over time individuals learned about the reduction of house values in type 1 neighborhoods, which added to the decreasing willingness to live in these areas. Additionally, their results indicate that segregation increased in Amsterdam after the murder.

That the effect can be long-term and changing over time is supported by Kiel and McClain. In two articles from 1995 they examine the effect of undesired activity on unused land (Kiel & McClain, 1995a) and how this effect can be studied and broken down into different stages (Kiel & McClain, 1995b). More specifically they study the effect of incinerator on surrounding housing prices. Even though there is a difference between this and temporary housing sites we borrow some of the terminology and methodology they discuss. In particular, we borrow their framework of different stages, which they argue can be divided into five parts; pre-rumor, rumor, construction, online and ongoing operation. In this paper the pre-rumor phase, prior to any discussion of temporary housing and the rumor phase, prior to any construction is taking place will be studied. Kiel and McClain's study force us to acknowledge that due to the slow adjustment of housing prices the full effect may not be observable until the fifth stage when the housing projects have been operating for some time. Further research can, and should be carried out once the full cycle with all five stages have passed.

Kiel and McClain (1995b) also discuss social welfare aspects and the potential of compensation due to the redistribution of wealth that property price changes cause. There are several issues associated with such compensation. The most prominent one in the setting of temporary housing is the choice of time for estimating the effect and when compensation would be paid out. The full effect, if any, would have had to materialized and to be known. If there is a significant decline in prices during the first stages of construction that is later reversed, there will be an economic redistribution from sellers to buyers. However, there will be no net effect on social welfare if prices fully recover. Kiel and McClain further argue that compensation in these cases would be done on an equity and not efficiency basis.

3 Theoretical framework & hypothesis

A commonly used method when determining property prices is the hedonic pricing method, which suggest that both internal and external factors determine the price of a good. There are a number of assumptions underlying this method (Follain & Jimenez, 1985). First, households base their purchasing decisions on a utility function such as $u = u(x, z)$, where z is a vector of internal and external property characteristics, $z = (z_1, z_2 ... z_n)$ and x is a composite good with unity price. Second, $p(z)$ is the hedonic price function of the property market and is assumed to be market clearing and non-linear. Third, households are utility maximizers, subject to the budget constraint $y = p(z) + x$. The first order condition of the hedonic model, $\partial p / \partial z_i \equiv p_i = u_{zi}/u_x$ where $i = 1, ...n$, simply means that the price of a property depends on how the different property characteristics are valued on the market and that this valuation is based on the relative utility a household gets from these characteristics compared to the composite good.

As most other markets, the market price is made up by a supply and demand schedule. However, according to Follain & Jimenez (1985) the commonly used estimation technique called the *simple hedonic approach* can estimate the market value of a property that stems from these supply and demand interactions without estimating them separately. In other words, the simple hedonic approach does not provide the marginal willingness to pay for a characteristic, instead it indicates how the marginal valuation of a property changes with an additional unit of a certain characteristic. Since this paper sets out to examine if the property price trend has been different between different areas rather than finding the willingness to pay for property characteristic we argue that the simple hedonic approach will be the best approach due to its simplicity.

The rest of the theoretical framework will explain how the model can be applied to the research question of this paper. As stated in the first assumption above, households makes their valuation of a property based on two factors, internal and external factors. Internal factors of a property is simply the characteristics of the property itself, such as living area, number of rooms and construction year, while external factors are characteristics of the neighborhood in which the property is located, such as the pollution level, distance to ocean and number of schools in the area. Hence, the price function for the housing market can be written as:

$$P_i = f(I_i, E_i) \qquad\qquad where \quad i = [1, n] \tag{1}$$

Where, I_i is internal characteristics and E_i is external characteristics. Using the simple hedonic approach, the marginal effect of an internal or external factor is the derivative with respect to the characteristic. In a simple OLS-regression the first derivative of a variable is simply the parameter value of that specific variable. Since the aim of this thesis is to evaluate one particular external factor, namely distance to temporary housing sites, the first order condition for this characteristic is:

$$\frac{\partial P_i}{\partial E_{distance}} = P_{distance}(E_{distance}) \tag{2}$$

If the arguments put forward against building temporary housing sites due to a negative effect on housing prices nearby in fact are true, the first derivative of a continuous variable would be positive and equal to the marginal valuation of an additional distance unit. If, however, we use a dummy variable in a DiD-framework, indicating if the property lies within an arbitrary distance of a site, the derivative would be negative and illustrate the average treatment effect (under certain assumption):

$$\frac{\partial P_i}{\partial E_{distance}} < 0 \tag{3}$$

If the results indicate that the announcement have affected price, then the reason for this would likely be due to a shift in demand from one neighborhood to another. As Gautier, Siegmann & van Vuuren (2009) points out, the reason for a shift could be due to different reasons. It could be that individuals generally have a negative preference to live nearby the temporary houses. However, it could also be because the market for properties are speculative. An individual being perfectly fine with living in these neighborhoods might choose to buy a property in another neighborhood if he/she believes that other individuals will value properties in the affected areas lower after the announcement. The focus of this thesis will be to evaluate if there have been different price trends between the neighborhoods, identifying the underlying reasons for the results would be a natural extension for future research using different methods. We will however briefly cover some of the potential reasons in the analysis.

Since the sites are yet to be built it will only be possible to evaluate the effect during the rumor face of construction and not the effect of the sites being up and running. To further conclude on the long-term effect, the analysis of this paper will have to be redone in a few years' time. The null hypothesis that will be evaluated in this paper is:

H_0= Temporary refugee housing sites, during the rumor phase of construction, does not affect surrounding property prices.

4 Data

4.1 Sample selection

We collect data from two different micro-level data sets of sales of properties, one on apartments and one on single-household houses. We use sales data of around 10 000 apartments 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 and indexes it, making it available for browsing. 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' web pages. This data only includes sales made by open ascending bids (the most common way to sell a property in Sweden), sales done by offers made before the bidding are not public and hence not available through their API. Sales of around 6 900 houses is provided by Lantmäteriet. It is mandated for everyone to report information to the agency every time a property is transferred from one owner to another. We separate the analysis between houses and apartments due to three reasons. (1) We cannot assume that the data generating processes behind the two data sets from Booli and Lantmäteriet are the same. (2) The data sets are structured differently, including different variables and different definitions of selling price. (3) Apartments and single household houses make up two easily distinguishable sub-markets on the Swedish housing market.

Both data sets cover the same geographical area; the municipality where the sites are (Göteborg) and where they border to (Mölndal and Partille)[4] as well as the same time period, January 2014 - February 2017. This means that the data consists of two years prior to the announcement of the temporary housing sites and one year after the announcement. A relative short time period 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.[5]

We use the coordinates for the three sites and the coordinates for the individual apartments (expressed in the WGS 84 standard) and single household houses (expressed in SWEREF 99 standard) to calculate a distance variable. Vincenty's formulae is used to calculate distance from individual apartments to the announced sites. This is a highly accurate mathematical formula for calculating distances between two points.[6] Pythagoras formula, which is slightly less accurate, is used to calculate the distance for individual houses. Using different formulas to calculate the distance is not optimal but Vincenty's formula cannot be applied to the SWEREF standard and vice versa. Moreover, the difference between the two ways of calculating the distance is less than half a meter (Lantmäteriet, 2012), making it negligible. These two techniques calculate the distance between two points in a straight line. An alternative way would be to measure it as the actual walking distance by the shortest route. This would however require more detailed information and advanced techniques not available to us.

[4]These two municipalities are included since at least one of the original twelve sites are within 1 km of the municipality border.

[5]Properties not suitable for year-round living include summer houses and houses that only have running water and electricity half of the year.

[6]Vincenty's formulae accounts for the flattening of Earth - assuming that it is not a sphere but rather a biaxial ellipsoid.

A few notes should be made about the data set and the Swedish market. First, apartments owned by individuals in Sweden normally follow a special type of arrangement where the individual owns shares of an economic association that owns the building and all the apartments in it. This arrangement is common in the Nordic countries but not elsewhere. Second, selling prices for apartments are collected from Booli's website as the last/highest bid and is used as the variable *price*. This will for the most cases correspond to the selling price and in those cases that it does not it will still serve as a good valuation of the property. There are two scenarios when the highest bid does not correspond to the selling price. (1) When the seller sells to a lower bidder or to someone else for various reasons. This does not constitute a problem in our analysis since the valuation derived from the highest bid is still consistent with the market valuation in the hedonic pricing model p(z). (2) When the highest bidder withdraws his or her bid or the seller does not accept any of the bids. This could be an issue for the analysis but we will assume that this is not a common behavior. If it were common, the arrangement of open ascending bidding would most likely be abandoned in the Swedish property market.

Further, the market for properties in Gothenburg can during the years in this thesis be referred to as a seller's market. There is a deficit of properties and the average time an object is for sale is relatively short. It is not unusual that objects are sold before bidding begins.[7] As previously mentioned these sales are not included in our data set.

4.2 Variables

The descriptive statistics for the dependent variable, *price/*m^2, and the control variables can be seen in table 1. Since the data is collected from two different sources the available control variables differ between apartments and houses. Control variables in the regressions for houses consists of *living area, construction year, lot size, value points* and *waterfront house*, while it consists of *living area, construction year,* m^2*/rooms, yearly rent/*m^2 and *floor* for apartments. The descriptive statistics are split up between houses and apartments and between houses situated within 1 km of one of the three still considered sites (Askimsbadet, Kärralundsvallen and Lemmingsvallen). From this point, only these three sites are included in our analysis if nothing else is mentioned. This is for the simplicity of the economic analysis due to the possibility that the rejection of the other sites (see figure 1) was predicted by the public.

[7] In 2016, 25 % of all properties in Gothenburg were sold before bidding began (Hemnet, 2017).

The distance variable, *within*, is a dummy variable equal to 1 if a property is located within a 1 km radius to the sites. We argue that this arbitrary distance is close enough to potentially be directly affected by the sites as well indirectly affected from the rumors going around about how the sites will affect neighborhoods. As can be seen in table 3 there are 86 houses and 190 apartments that are defined as ”treated” when 1 km is considered the cut-off distance. However, we will also consider other cut-off distances to investigate if this changes the results. With a cut-off distance of 0,5 km the number of observations within the treatment area is 23 for houses and 46 for apartments, while a 1,5 km distance results in a treatment group of 185 houses and 463 apartments.

Table 1: ***Descriptive Statistics***

	Houses within		Houses outside		Apartments within		Apartments outside	
	mean	sd	mean	sd	mean	sd	mean	sd
Price	5232.03	2227.98	4343.92	2192.59	2607.85	951.85	2620.27	1249.78
Price/m^2	42.74	12.95	35.54	16.25	45.19	9.34	41.05	14.26
Living area (m^2)	127.79	50.52	126.23	40.60	58.62	19.47	65.78	22.99
Construction year	1950.55	22.05	1963.62	99.94	1949.30	18.19	1956.63	31.33
Additional area (m^2)	53.16	38.10	30.39	37.51				
Lot size (m^2)	635.09	423.66	764.06	1830.04				
Value points	28.58	4.98	29.22	4.84				
Waterfront house	0.01	0.11	0.03	0.17				
Rooms					2.27	0.92	2.44	0.93
m^2/rooms					27.33	6.03	28.04	5.55
Rent					3375.28	990.59	3670.70	1155.67
Yearly rent/m^2					711.34	142.57	687.59	130.12
Floor					2.16	0.99	2.74	1.86
N	307		6574		459		9425	

Source: Lantmteriet.se (Houses), Booli.se (Apartments).

There is a fairly large price difference between houses *within* and houses outside both in terms of selling price and square meter price. This is not the case for apartments *within*, they are more expensive in term of square meter price but have a lower selling price. The aim was to build the temporary housing sites in areas with good communication and services and thus not increase segregation (Göteborg Stad, 2016). That properties in areas surrounding these sites on average have higher prices per square meter, compared to the rest of Gothenburg, is therefore not surprising.

As table 1 shows, the mean values of the control variables differ to some extent between living within 1 km from the sites compared to living outside for both houses and apartments. Houses outside are on average constructed 13 years later and have 130 m^2 larger lot size compared to houses *within*. *Value points* is a proxy for the quality of the house and is derived from the quality score of five different factors; exterior, energy usage, kitchen, sanitary ware and miscellaneous. It ranges from 4 to 55 in our sample and as can be seen in table 1 it is fairly similar for houses *within* and outside. *Waterfront house* is a dummy variable that equals to 1 if the house is situated within 150 meter to the shoreline and 0 otherwise. In total, 201 houses are defined as waterfront houses and as table 1 indicates, around 1 % of the houses *within* can be defined as waterfront houses compared to 3 % of the houses outside. Having a waterfront house is assumed to have a positive effect on price. When restricting the sample by eliminating the 1 % (5 %) most expensive houses only 178 (149) waterfront houses remains. It seems reasonable to assume that these properties are disproportionately expensive, and hence it is not surprising that more than 1 % (5 %) is removed when restricting the sample this way.

Apartments located outside the threshold distance are on average more than 10 % larger - resulting in the relationship previously mentioned about price and square meter price. There is also roughly a 10 % difference in rent, but since rent depends on living area this is most likely due to the difference in size of apartments. As table 6 show, the data consist mainly of apartments with two or three rooms (88 % of the data consist of apartments with three rooms or less). The correlation between living area and rent (0,775) as well as the correlation between living area and rooms (0,881), indicate that they all will, in some sense, measure the effect of size of apartments. To allow for the separate effect of *rent* we have altered the specification of it into yearly rent per square meter (*yearly rent/*m^2). We argue that this measure is more appropriate since a rational buyer would respond to a rent positively or negatively deviating from an average rent, given the size of the apartment. As can be seen the average yearly rent/m^2 is slightly cheaper for apartments outside (687,59) compared to those inside (711,34). *Rooms* is transformed into average room size, m^2*/rooms* to indicate if it is a big or a small apartment in relation to the number of rooms (e.g. if it is a big or a small two room apartment).[8] The alternation of these variables remove the high correlation between these variables, *living area* and *yearly rent/*m^2 has a correlation of (-0,3911) while *living area* and m^2*/rooms* has a correlation of (-0,0858).[9]

The natural logarithm of *price/*m^2*, living area, additional area, lot size,* m^2*/rooms* and *yearly rent/*m^2 will be used since the relative difference in these variables is a better measurement than the absolute difference and reduces the potential problem with non-normal distributions.[10]

[8] In Sweden, number of rooms exclude kitchen and bathrooms.

[9] The alternation of the variables does not change the outcome in terms of significance. It only changes the parameter value and the fit of model marginally, compared to when including them in their original form.

[10] For variables that take the value 0 we add 1 to this value, since the logarithm of 0 is not defined.

Lastly, table 1 also shows the number of observation within the cut-off distance. For houses, there are 307 (4,46 %) observations considered as *within* and for apartments there are 459 (4,64 %) observations *within* during the observed time period between January 2014 - February 2017. The total number of months in the data is 38 for both houses and apartments. Out of these, 25 are before the announcement and 13 are after. Postal codes will be included to control for external characteristics. The data set for apartments consist of 355 unique postal codes while the data set for apartments include 284 different 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.

5 Empirical strategy

The empirical strategy for this paper is to use the difference-in-difference (DiD) approach where the treated group are properties within a certain distance of the temporary housing sites and the control group is properties outside this distance. By using the DiD approach in a natural experiment setting it is possible to infer a causal relationship. The baseline model we use is the following:

$$\ln P_{ijt} = \beta_0 + \delta_1 D_i^{within} + \delta_2 D_i^{within} * D_t^{post} + \beta_3' \mathbf{I_i} + \alpha_j + \lambda_t + \epsilon_{ijt} \tag{4}$$

Where i indicate individual property, j indicate neighborhood and t indicate time. $ln(P_{ijt})$ is our outcome variable and is defined as $ln(price/m^2)$. The variable D_i^{within} is a dummy indicating if a property is within the threshold distance of temporary housing site (within a 1 km radius in the baseline model). D_t^{post} is a time dummy equal to 1 after the sites were announced (31^{st} of January)[11] and 0 before. The interaction term between the two dummies $D_t^{post} * D_i^{within}$ is the variable of interest, which captures the treatment effect. In other words, on average how the price/m^2 trend has been affected for households within the threshold distance after the announcement, compared to households outside the threshold distance, keeping everything else constant.

To isolate the treatment effect, a vector of internal characteristics (I_i) is included as well as time (λ_t) and neighborhood (α_j) fixed effects. We use $ln(price/m^2)$ to estimate a percentage change in price instead of an absolute change, since we argue that the effect should be proportional to the property price itself. A key assumption is that the announcement

[11]The actual announcement was 28^{th} of January but using the 31^{st} allow us to use full months. No property sales were made within 1 km of the sites between the 28^{th} to the 31^{st} of January, which means that this should have no effect on the result.

- the treatment effect - can be considered an exogenous event and not anticipated. The announcement of the twelve original sites received much attention by the public and media (SVT, 2016; Göteborgs Posten, 2016) and we argue that this strengthens the assumption that they were unanticipated even though the goal of building 1000 temporary apartments had been known since the fall of 2015.

As stated above, the DiD approach yields the possibility to obtain a causal treatment effect. However, there are some crucial assumptions for the DiD approach to be applicable. (1) There must be a parallel trend before the announcement between the control and the treatment group. This to justify that if no treatment was assigned the two groups would have followed a similar price development. The trends before treatment will therefore be investigated both graphically and statistically to get an idea about if the parallel trend assumption holds. (2) The DiD-method assumes that the sample composition before and after treatment is equal. This means that properties sold within the areas of interest have similar 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). Since attractiveness and similar characteristics cannot be directly observed we will test this assumption by comparing the mean values for all observable characteristics before and after treatment. If the composition in terms of observable characteristics have not changed we will assume that this is the case for unobserved characteristics as well. (3) There cannot be any spillover effect from the treatment on the control group. Since property cannot be moved, the problem with spillover effects is smaller here than in other DiD settings but simultaneously it may be harder to define the actual treatment and control groups. These will be tested using different cut-off values of the distance variable. If the assumptions of the DiD-approach do not hold inference is not possible. Further, since we aim to use the OLS estimator the usual Gauss-Markov assumptions must hold.

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 evolving over time but are the same for all geographical areas (such as interest rate). In the model, time fixed effect consist of monthly time dummies and neighborhood fixed effects are dummies for postal code.

With pooled cross-sectional property data where price is assumed dependent on geographic location and time, concerns regarding spatial auto-correlation arise. This is because unobserved characteristics of properties are likely to be geographically dependent.

Heteroskedastic-autocorrelation consistent standard errors (HAC), clustered on the level for this spatial auto correlation is one way to account for this (Stock & Watson, 2015). Given our data, we will assume that the geographic dependency occurs on a neighborhood level since properties having similar characteristics often are located close to each other. The neighborhoods will be proxied by postal codes and we argue that they can be included on three different geographical levels; the full five digit code, the first four digits or first three digits of the postal code. According to Cameron & Douglas (2015) the clusters should be as aggregated as possible up to the point where there is no concern about having too few clusters and when there is a small change in standard errors. There is no clear definition of what too few clusters are, but according to Angrist & Pischke (2008) 42 can be considered to be enough and well below 42 is too few. The number of clusters in the data set for apartments (houses) will be 355 (284) with five digit postal code, 72 (73) with four digit postal code and 21 (26) with three digit postal codes. Clearly three digit postal code might yield too few clusters. Meanwhile five digit postal codes include multiple areas with a single observation which result in no variation within the cluster. Further, they can be aggregated without the concern of having too few clusters. Since there is no perfect way of deciding what the best clustering level is, we will keep this in mind and opt for a conservative approach regarding the standard errors to try to avoid making a type 1 error, a false rejection of the null hypothesis.

To evaluate our findings, multiple robustness checks will be made. This will include testing different threshold distances (0,5 km, 1 km, 1,5 km) and varying the starting month, to asses if the treatment effect is sensitive to specification. The sample will also be restricted by removing outliers and we will restrict the control group by removing observations that are more that 5 km away, both approaches in order to investigate if the effect is driven by some sub-population.

6 Results

6.1 Difference-in-difference results

All the main results in this thesis can be found in table 3. In columns (1) - (4) the baseline regressions can be seen with robust standard errors and with clustered standard errors on postal codes. Following the arguments in section 5 about spatial autocorrelation, using HAC standard errors clustered on postal code level allow for properties within the same postal code district to be correlated by unobserved factors and produce consistent standard errors. The standard errors will be clustered using four digit postal codes - to avoid having too few clusters as well as avoiding them being too disaggregated. This is consistent with the argument about neighborhood characteristics in section 5, since four digit postal codes are relatively small areas where the properties share many characteristics while in the same time are big enough to include multiple observations. To be consistent between the data sets and throughout the paper all the subsequent regressions will therefor include clustered standard errors on four digit postal codes and the baseline model is thus represented by column (3). As expected, clustering yields larger standard errors for *withinpost* compared to the robust standard errors (column 1) and thus decrease the likelihood of type 1 errors. However, we will discuss the implication of the chosen level of clustering with respect to type 1 errors further.

The variable *within* in the models indicates whether there is an overall price difference between properties that are located within 1 km of the sites and those outside, throughout the entire period. Looking at table 3, this seems to be the case in most of our specification for apartments, *within* is significant and overall negative. However, the chosen level of clustering open up for an increased likelihood of type 1 errors for *within*.

The variable of interest, *withinpost*, is significant on 10 % level for apartments in the baseline specification in column (3). This indicates that the announcement had a negative effect on apartment prices within these areas compared to the control group. The economic interpretation is that apartments located within 1 km from a temporary housing site on average have been sold for 2,30 % less after the announcement compared to an apartment outside this area, keeping everything else constant. For houses the *withinpost* variable is positive but insignificant, indicating that there have been no significant price differences after the announcement between houses within and houses outside a 1 km radius.

The control variables in both the house and apartments sample all have the expected values. The variables *living area*, *construction year*, *yearly rent*/m^2 and m^2/*rooms* all have a negative effect on property price per square meter while the opposite effect is found for *floor*. A striking feature for houses is that the variable *value points* is highly significant and positive, indicating that it is a good determinant for quality of the houses. There are no similar control variables available for apartments but we have no reason to assume that there is a different relationship between quality and price. Lacking this variable in the data set for apartments is not optimal but the rent per square meter variable should catch at least some of this effect. When removing the effect of apartment size from *rent* we argue that what is left to pay for in rent is mainly the quality of apartment. Looking at the control variable for neighborhood effects, *postal code*, it is highly statistically significant in the vast majority of cases, both for apartments and houses, indicating that neighborhood characteristics are important for determining price. Further, the time dummies show a long-term positive and significant price trend and signs of seasonality in the Gothenburg property market.

6.2 Test of assumptions

In this subsection we will present two tests of the assumptions for the DiD approach presented in section 5. First we will discuss the parallel trend assumption and second the sample composition assumption.

6.2.1 Parallel trend

Figure 2 - 7 are graphical presentations of the parallel trend between the treated group and the control group. The vertical dashed line in all figures indicates the date of the announcement of the temporary housing sites and thus when the treatment occurred. Figures 2 and 3 present the monthly average selling price with a fitted line in the periods before the announcement, representing the parallel trend. Figures 4 and 5 basically present the same thing but with a three-month average in order to get rid of some of the noise that are present in figure 2 and figure 3 due to the small number of observations in each month. Both the graphs of monthly average and the graphs of three-month average price indicate that the parallel trend seems to hold. For houses, there seems to be a drop in prices in the periods just after the announcement. For apartments, the graphs indicate that the treatment group and control group diverge a little bit just before the announcement, with the control group having somewhat lower prices. However, these graphs are before controlling for anything and no conclusions about the treatment can be drawn from these graphs.

To make sure that the parallel trend also holds when controlling for internal factors, neighborhood effects and time, the residuals over time from the baseline model is graphically presented in figure 6 and 7. If there were any significant differences in the trend prior to the announcement between the treatment and control group this would be seen in the graphs by one of the lines following some systematic variation. As can be seen this is not the case, the residuals vary around zero and the parallel trend seems to hold. These two graphs further indicate that our model does not seem to have any big problems with autocorrelation. The error term seems to follow a white-noise pattern. A common concern in datasets with time-series is that the error term is correlated over time and that a lagged outcome variable should be included in the regression. Given the white noise pattern this does not seem to be necessary in our model.

Column (6) in table 3 presents a statistical representation of the parallel trend. We use the baseline specification and introduce a "placebo" treatment for the year 2015, called *withinpre*[12], by interacting *within* with a year dummy for 2015, making 2014 the baseline year. By including interaction terms of the treatment variable with periods preceding the actual treatment it is possible to test the validity of the parallel trend (Bejenariu & Mitrut, 2013). If the parallel trend assumption would be violated it would be shown by the interaction terms preceding the treatment years being significant. As can be seen for both houses and apartments, *withinpre* is not statistically different from zero, further strengthening that the parallel trend assumption holds.

6.2.2 Sample composition

The second crucial assumption that needs to be addressed is the similarity of characteristics before and after treatment for the treatment group. If this does not hold, the treatment effect might work through a change in sample composition. In tables 7 and 8 the mean of the observable characteristics for the treated group are presented before and after the treatment.[13] In the last two columns the differences in means are displayed together with the p-value from the t-test, which test the null hypothesis that the mean is not different from each other. As can be seen this cannot be rejected for any of the observable variables for apartments or houses and we retain the null hypothesis that there are no significant differences in the treated properties characteristics before and after the treatment. By assuming that this is also true for unobservable characteristics we conclude that the assumption of stable sample composition holds.

6.3 Robustness checks

In this subsection different robustness checks will be presented as mentioned in section 5. Separately, cut-off distance, announcement date and control group will be altered and outliers will be removed.

6.3.1 Cut-off distance

The baseline model's robustness regarding cut-off distances for the variable of interest are presented in column (7) and (8) in table 3. In column (7) the cut-off distance is set to 0,5 km and in column (8) the cut-off distance is 1,5 km. Changing the cut-off distance

[12] January 2016 is also included since the announcement was made the 28th of January 2016.

[13] The age of the property the year it was sold is used instead of *construction year* to get a more comparable variable.

tests how sensitive the model is to specification of the explanatory variable of interest. For houses the *withinpost* variable changes sign but remains statistically insignificant. This supports that the true parameter value varies around zero and thus that the treatment has had no significant effect on the market for houses. The variable of interest in the model for apartments on the other hand is consistently negative but varies in significance level. When restricting the cut-off distance to 0,5 km the explanatory variable of interest is significant on 5 % level and the parameter value increases to -0,029 compared to -0,023 in the baseline model. Increasing the cut-off distance has the complete reverse effect and leads to insignificant results. This indicates that it might only be the apartments closest to the temporary housing sites that are affected by the treatment. As can be seen in column (27) in table 5 the parallel trend is valid when the treatment variable is defined as being within 0,5 km to a site.

To further test the robustness of the result with 0,5 km cut-off distance for apartment the same tests as the baseline models are put up against are conducted and presented in table 5. Longer explanations of the intuition behind these tests are provided in Section 6.3.2 and 6.3.3. Columns (30) and (31) indicate that the result is robust against removing outliers in terms of the most expensive and cheapest apartments. Columns (28) and (29) imply that the effect also remains statistically significant throughout 2016, as the treatment variable is postponed three and six months. However, the number of observations that are treated are down to 46 in the baseline model when restricting the treatment to 0,5 km and when evaluating the robustness, the number of observations are even fewer (e.g. in column (29) the number of observations that are treated are 16). The small sample could lead to violation of the Gauss-Markov assumptions and bias the results and therefore any interpretation should be done with caution.

6.3.2 Announcement date

In column (9) - (11) the cut-off distance for the *withinpost* variable is changed back to 1 km but the date of the treatment is alternated. First, the treatment is defined as three months before the actual announcement took place. The reason for this approach is to see whether the announcement actually was exogenous or if the market predicted the placement of the sites after it was announced that the sites would be constructed somewhere in Gothenburg. If the market in fact predicted the placement, and had a negative reaction to the sites, this would be indicated by *withinpost* being significant in column (9). This is not the case for neither of the models, instead the p-value for both increase, which supports the assumption that the treatment was exogenous and/or the announcement having no effect on prices.

Second, in column (10) and column (11) the treatment is defined as three months and six months after the actual announcement. A possible scenario is that the market does not immediately react to the announcement due to the uncertainty regarding the actual construction. As time pass after the announcement and more information is released to the public about which sites are going to be pursued and to which sites building permits are sent in for, the uncertainty should be reduced. By removing the first months after announcement and thus restricting the treatment variable to the months where the locations of the sites were less uncertain a more distinct effect could potentially be seen. However, when restricting the treatment variable this way a lot of observations of the treatment group are removed. For houses (apartments) the number of observations that are treated decrease from 86 (190) to 64 (139) when moving the treatment date three months ahead and to 39 (93) when postponing it six months ahead. This means less reliable results that could be driven more by coincidence than by the announcement. Nevertheless, the treatment variable for apartments is significant on 5 % level with a parameter value larger than in the baseline regression when moving the treatment variable six months ahead. The treatment effect on the market for apartment could thus potentially be stronger once the uncertainty is reduced.

6.3.3 Outliers

In column (12) and (13) in table 3 it is presented how removing 1 % or 5 % of the most expensive and cheapest properties of the market affect the baseline model. Removing these properties decreases the probability of them having certain properties that does not belong to the same market. E.g. a property for 16 million SEK might attract different buyers compared to a property for 4 million SEK. Throughout our empirical strategy we rely on the assumption that all macro-economic factors (e.g. interest rate and stock market development) affect all observations in the sample similarly. For the most expensive and the most affordable properties this might not be true. If that is the case these two different markets could have displayed different price trend both before and after the treatment, which in turn could affect the results. Looking at the model for apartments, removing the outliers lowers the parameter values and increases the p-value to the point where the treatment variable no longer is statistically significant at any level. This indicates that the previous significant effects could be driven by outliers. The opposite and somewhat surprising thing occurs for houses. Removing 10 % of the observations leads to a significant and positive treatment variable.

6.3.4 Restricted control group

In table 4 the control group have been restricted while the treatment group is the same as before. The new control group consists of properties situated within 5 km to one of the sites. Thus, the data only consists of properties located closer to each other while neighborhoods further away are removed. Following past argumentation about neighborhoods sharing the same characteristics the new control group should be even more similar in unobservable characteristics to the treatment group. The downside is that the number of observations, and as a result the degrees of freedom, are reduced considerably. As column (19) in table 4 indicates, the parallel trend still holds and has an even higher p-value (which is not surprising since the control group and treatment group should be more alike). For houses the restricted control group does not change the result in any noteworthy way. However, all previous significant results for apartments become insignificant with the restricted control group. This could be a result of a reduced sample, but it could also mean that the previous significant result was driven by the neighborhoods that now have been removed. That is, if neighborhoods further away from the sites experienced a larger price increase after the treatment compared to the neighborhoods that are left in the dataset. The latter explanation is supported from the fact that the parameter value for *withinpost* is lower with the restricted control group in all specifications.

7 Analysis/discussion

The results from section 5 indicates that houses located close to the future temporary housing sites have been more or less completely unaffected by the announcement. The treatment variable varies around zero in all baseline regressions and robustness checks and is in the vast majority of cases insignificant. In the two cases where significant results are found (when cut-off distance is set to 1,5 km and when 5 % of the most expensive and cheapest houses are removed) the treatment variable have opposite signs, creating difficulties in making any interpretations from these results. For apartments, the interpretation of the results is more complicated. Contrary to the results of houses, the treatment variable for apartments is throughout all specifications (but one) negative but vary from statistically insignificance to being significance on 5 % level.

Focusing on standard errors, the baseline regression seems sensitive to the level of clustering. Four digit postal code yields the most conservative standard errors for houses. However, for apartments, both three digit and five digit postal codes provides larger standard errors and would lead to insignificant baseline results. As previously argued, four digit postal code clusters are appropriate due to a reasonable size and amount of clusters. There are other, more sophisticated methods that can be used to address spatial auto-correlation. These could potentially allow for correlation on different levels than the postal codes. However, this is outside the scope of this thesis and at this point we are satisfied with controlling and clustering at the given postal code levels.

Other than the baseline result there are two regressions where a significant result (on 5 % level) of the treatment variable is present for apartments. This is when the cut-off distance is set to 0,5 km and when the treatment date is postponed six months. Compared to the baseline result the parameter value of the treatment variable is somewhat larger in these specifications (-0,029 compared to -0,023). This indicates that apartment prices in the areas close to the temporary housing sites have had between 2 - 3 % lower price development compared to the apartments further away from the sites. It is important to remember that the areas within the threshold distance, just as the rest of the market for apartments, have had a positive price increase during the years of interest. The mean square meter price of apartments has on average increased 13,5 % between February 2016 to February 2017 for the entire sample. It should thus be interpreted as a smaller increase rather than a decrease of the values of apartments. The result when using a cut-off distance of 0,5 km is also tested and appears to be fairly robust. Unfortunately, there are concerns regarding the lack of observations within the treatment area when conducting these robustness checks, which makes it difficult to make any strong claims about the result.

The results when using a restricted control group (removing all observations further than 5 km from the sites) indicates that the significant results might have been driven by areas located further away from the sites. The parameter values for the postal code coefficients indicate that there is a rather large difference in property prices within Gothenburg. The price difference is, as mentioned, largely geographically dependent and restricting the control group to areas closer to the sites removes a lot of areas where property prices are considerably more affordable (Biskopsgården and Angered to mention two) than those in proximity to the sites. Prices in some of the removed areas require a fairly low increase in selling price to substantially outperform the areas closest to the refugee sites in relative terms - which is what we measure. This could drive the negative result in some of the specification presented in table 3 for apartments.

There are a number of concerns that have been raised previously in this paper that deserve to be mentioned further. (1) The data on apartments do not include any sales made before the bidding begins. This could be an issue if the share of apartments that are sold before bidding have increased/decreased within or outside the treated areas and if they are systematically different to the ones sold in open biddings. This would imply that the market in one area have become increasingly (un-)attractive even though we have not been able to observe it and that there has been an adverse selection in objects that are left for open biddings. However, if demand in one area increase, resulting in an increasing share of beforehand bids and sales this should logically also lead to higher prices for properties sold through open bidding, limiting this potential problem. Further, we test for differences in observable characteristics over time, see table 7 and table 8, and conclude that they are stable over time. This means that there are no systematic differences in observable characteristics (or unobservable characteristics)[14] over time, which supports the assumption that the market composition has not changed. (2) For the DiD-approach to be valid it is crucial that if no treatment occurs the counter factual would have followed the parallel trend. We control for external characteristics using neighborhood fixed effects that allow for variation across neighborhoods but not for neighborhood variation across time. The premium for living in one specific area may change over the course of three years, either due to changes in preferences or local development (e.g. building a new school). If any local neighborhood changes have occurred, other than the treatment itself, that would cause price to deviate from the parallel trend it would violate this assumption and bias the result. We are not able to control for all potential local changes but we have not observed any major changes that would have induced the counter factual to deviate from the parallel trend. Moreover, we argue that using a short window of time before and after treatment minimizes this potential problem. (3) An obvious limitation is that the sites have yet to be built, and as such, the total effect remains unknown. Some uncertainty remains about the building permit for Lemmingsvallen, and about when construction will begin. One can discuss and evaluate whether the construction have been successful first after the sites are finished and people have moved in.

So why do we observe different result for houses and apartments? As stated in the theory section, the aim with this thesis is not to identify the underlying reasons, but we have some ideas of possible explanations. Except for the fact that the two data sets are collected from two different sources and that the number of observations for houses is about two thirds of the number of observations for apartments we believe there are other important differences. The markets for houses and apartments are arguable two different sub-markets. We believe houses more typically is inhabited by families while small apartments, which table 6 imply that our data mainly consist of, often is bought by couples or single individuals. There is

[14]Based on the discussion in section 6.2.2 we assume that this is true for unobservable characteristics when the composition of observable characteristic are stable over time.

one main component that we assume differ between the average buyer of a house compared to the average buyer of an apartment, namely speculation. Having a family often means having a secure job and planned future, while living in an apartment can be a temporary solution until it is possible to find a bigger or better apartment/house. This means that the purchase of a house could be a more long-term investment compared to the investment in an apartment. The long-term investment should arguable be less affected by speculative components since the average investor tend to live there for a longer time during which the property value almost always increase. A short-term investment in an apartment, where the investor might only want to live for a shorter time period will thus be more sensitive to uncertainty since a temporary negative fluctuation in price could mean selling the apartment to a loss. The announcement of the temporary housing sites increase the uncertainty about the property value due to the fear that the neighborhoods might become less attractive to future buyers. Following the reasoning above this should have a larger effect on the market for apartments than on the market for houses and could thus be the reason for why the results differ between the two markets.

The potential price differences raise the issue of financial compensation. It is a difficult matter and we will only briefly discuss it here, mainly because as pointed out by Kiel and McClain (1995b), this is an issue that can only be solved after the full effect is known. Additionally, we see some issues with compensation due to the nature of the sites studied in this thesis. They are temporary and if there is an effect solely due to the temporary housing sites this would be reversed once they are removed again. As mentioned in section 2, the compensation would then be a matter of equity, redistributing wealth from those that have bought to those that have sold during these temporarily low prices. We argue however that such a scheme seems unfeasible.

8 Conclusion

In this thesis we tried to answer if property prices nearby temporary housing sites were negatively affected after the announcement of the sites by using a DiD approach. The results suggest that the market for houses have been unaffected by the announcement while the market for apartments have experienced a negative but inconclusive effect.

Due to the fact that the construction of the sites has not yet started and that what we find so far is inconclusive it is too early to evaluate the final effect and give any major policy recommendations. What can be interpreted from the results is that the market for houses more certainly than apartments have not experienced any lower price development due to the announcement. If homeowners' property values are considered when deciding on placement, it seems reasonable at this point in time, to place the sites in neighborhoods with a bigger fraction of houses than apartments.

It is noteworthy that many of the locations originally picked for the sites in many aspects did not reach the objectives formulated beforehand. Eight of the twelve original locations have been rejected on their own merits and some of these mistakes could probably have been avoided by further evaluation before announcement. This would likely decrease the irritation and attention regarding the remaining sites, regardless if they are rejected or not. Since the speculative component is an important part of the property market the media attention of the sites could potentially drive a price effect itself. With a lot of negative media attention on the sites that no longer are considered together with decision makers inability to rationally argue for the sites, the uncertainty and speculation about how the sites still considered will affect the market increase. However, with no conclusive evidence that the announcement has had any effect on price this is more of a theoretical concern.

Important to remember is that it is still early in the process and only the rumor phase has been investigated. Due to the uncertainty in the rumor phase, it would be appropriate if the study was to be conducted again with a more extensive data set (not available for us at this time) after the construction have been finished and the sites are up and running. This would give the opportunity to use more detailed data on individual characteristics of the properties (pools, garage/carport, lake view, Scandinavian stoves etc.) as well as using all sales during the period, not just those done by open bidding. A longer data set would also provide an opportunity to evaluate if the effect is different during different phases of construction as well creating bigger treatment groups that would generate more reliable results. Further, it could be possible to transform a bigger data set into a panel data from objects that have been sold repetitively.

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Appendix

Table 2: ***Variable definition and sources***

Variable	Description	Source
Price	The price for which the property was sold, in thousand SEK.	Booli.se, Lantmäteriet
Square meter price	$Price/Living\ area$	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 km) 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	$(Rent * 12)/Living\ area.$	Booli.se
Rooms	Number of rooms in the apartment excluding kitchen and bathroom(s).	Booli.se
m^2/rooms	$Living\ area/Rooms.$	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 lays within 150 meter 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

Table 3: ***Main Results***

Dependent variable - ln(price)

Houses

	(1a) Robust S.E.	(2a) Cluster 3-digit	(3a) Cluster 4-digit	(4a) Cluster 5-digit	(5a) Without Controls	(6a) Parallel Trend	(7a) Cut-off 0,5 km	(8a) Cut-off 1,5 km	(9a) Treatment t-3	(10a) Treatment t+3	(11a) Treatment t+6	(12a) 1% Outliers Removed	(13a) 5% Outliers Removed
within	-0.00465	-0.00465	-0.00465	-0.00465	-0.0172	-0.0165	-0.0370	0.00705	-0.00199	-0.00193	-0.00206	-0.00639	-0.0267*
	(0.0250)	(0.0356)	(0.0363)	(0.0358)	(0.0450)	(0.0429)	(0.0381)	(0.0175)	(0.0386)	(0.0353)	(0.0352)	(0.0337)	(0.0157)
withinpost	0.00549	0.00549	0.00549	0.00549	-0.0103	0.0165	0.00644	-0.0280*	-0.00296	-0.00463	-0.00644	0.0124	0.0358**
	(0.0233)	(0.0257)	(0.0276)	(0.0242)	(0.0230)	(0.0301)	(0.0414)	(0.0142)	(0.0253)	(0.0284)	(0.0297)	(0.0255)	(0.0162)
withinpre						0.0241							
						(0.0210)							
n	86	86	86	86	86	86	23	185	103	64	39	86	81
N	6881	6881	6881	6881	6881	6881	6881	6881	6881	6881	6881	6747	6209
R^2	0.717	0.717	0.717	0.717	0.529	0.717	0.717	0.717	0.717	0.717	0.717	0.785	0.805
adj. R^2	0.703	0.703	0.703	0.703	0.506	0.703	0.703	0.703	0.7 03	0.703	0.703	0.774	0.794

Apartments

	(1b) Robust S.E.	(2b) Cluster 3-digit	(3b) Cluster 4-digit	(4b) Cluster 5-digit	(5b) Without Controls	(6b) Parallel Trend	(7b) Cut-off 0,5 km	(8b) Cut-off 1,5 km	(9b) Treatment t-3	(10b) Treatment t+3	(11b) Treatment t+6	(12b) 1% Outliers Removed	(13b) 5% Outliers Removed
within	-0.0219	-0.0219	-0.0219**	-0.0219	-0.000532	-0.0311**	-0.0522***	0.0434*	-0.0257 **	-0.0260***	-0.0251***	-0.0246**	-0.0279***
	(0.0174)	(0.0144)	(0.0102)	(0.0177)	(0.0244)	(0.0156)	(0.00784)	(0.0244)	(0.0110)	(0.00958)	(0.00922)	(0.00946)	(0.00831)
withinpost	-0.0230**	-0.0230	-0.0230*	-0.0230	-0.0275	-0.0150	-0.0290**	-0.0112	-0.0114	-0.0158	-0.0286**	-0.0185	-0.00838
	(0.0114)	(0.0138)	(0.0123)	(0.0150)	(0.0189)	(0.0145)	(0.0114)	(0.0148)	(0.0132)	(0.0109)	(0.0115)	(0.0115)	(0.00966)
withinpre						0.0177							
						(0.0131)							
n	190	190	190	190	190	190	46	463	223	139	93	189	181
N	9883	9883	9883	9883	9883	9883	9883	9883	9883	9883	9883	9689	8919
R^2	0.902	0.902	0.902	0.902	0.829	0.902	0.902	0.902	0.902	0.902	0.902	0.901	0.898
adj. R^2	0.898	0.898	0.898	0.898	0.822	0.898	0.898	0.898	0.8 98	0.898	0.898	0.897	0.893
Postal Code	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered S.E.	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

n=number of treated observations

Table 4: ***Restricted Control Group***

Dependent variable - ln(price)

Houses

	(14a) Robust S.E.	(15a) Cluster 3-digit	(16a) Cluster 4-digit	(17a) Cluster 5-digit	(18a) Without Controls	(19a) Parallel Trend	(20a) Cut-off 0,5 km	(21a) Cut-off 1,5 km	(22a) Treatment t-3	(23a) Treatment t+3	(24a) Treatment t+6	(25a) 1% Outliers Removed	(26a) 5% Outliers Removed
within	-0.0115	-0.0115	-0.0115	-0.0115	-0.0185	-0.0196	-0.0324	0.00392	-0.00826	-0.0101	-0.00975	-0.0372	-0.0306**
	(0.0254)	(0.0397)	(0.0381)	(0.0372)	(0.0449)	(0.0461)	(0.0411)	(0.0181)	(0.0403)	(0.0369)	(0.0363)	(0.0250)	(0.0140)
withinpost	0.00645	0.00645	0.00645	0.00645	-0.0127	0.0141	0.00665	-0.0275*	-0.00374	0.00234	0.00134	0.0241	0.0263
	(0.0247)	(0.0300)	(0.0309)	(0.0272)	(0.0236)	(0.0347)	(0.0492)	(0.0160)	(0.0280)	(0.0305)	(0.0336)	(0.0229)	(0.0223)
withinpre						0.0166							
						(0.0235)							
n	86	86	86	86	86	86	23	185	103	64	39	86	81
N	2821	2821	2821	2821	2821	2821	2821	2821	2821	2821	2821	2768	2541
R^2	0.752	0.752	0.752	0.752	0.544	0.752	0.752	0.752	0.752	0.752	0.752	0.825	0.814
adj. R^2	0.736	0.736	0.736	0.736	0.517	0.736	0.736	0.737	0.7 36	0.736	0.736	0.814	0.801

Apartments

	(14b) Robust S.E.	(15b) Cluster 3-digit	(16b) Cluster 4-digit	(17b) Cluster 5-digit	(18b) Without Controls	(19b) Parallel Trend	(20b) Cut-off 0,5 km	(21b) Cut-off 1,5 km	(22b) Treatment t-3	(23b) Treatment t+3	(24b) Treatment t+6	(25b) 1% Outliers Removed	(26b) 5% Outliers Removed
within	-0.0276	-0.0276	-0.0276**	-0.0276	-0.00591	-0.0358**	-0.0578***	0.0384	-0.0313 **	-0.0308***	-0.0285***	-0.0302***	-0.0389***
	(0.0173)	(0.0161)	(0.0116)	(0.0174)	(0.0251)	(0.0175)	(0.00869)	(0.0247)	(0.0126)	(0.0108)	(0.0104)	(0.0109)	(0.00890)
withinpost	-0.0135	-0.0135	-0.0135	-0.0135	-0.0143	-0.00648	-0.0187	-0.000754	-0.00344	-0.00668	-0.0215	-0.00780	0.00267
	(0.0114)	(0.0160)	(0.0142)	(0.0155)	(0.0209)	(0.0169)	(0.0126)	(0.0165)	(0.0153)	(0.0129)	(0.0138)	(0.0127)	(0.0102)
withinpre						0.0136							
						(0.0133)							
n	190	190	190	190	190	190	46	463	223	139	93	189	181
N	6626	6626	6626	6626	6626	6626	6626	6626	6626	6626	6626	6496	5966
R^2	0.900	0.900	0.900	0.900	0.827	0.900	0.900	0.900	0.900	0.900	0.900	0.897	0.888
adj. R^2	0.896	0.896	0.896	0.896	0.820	0.896	0.896	0.896	0.8 96	0.896	0.896	0.893	0.883
Postal Code	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered S.E.	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

n=number of treated observations

Table 5: ***Robustness checks 0,5 km***

Dependent variable - ln(price)

Apartments

	(7b) Cut-off 0,5 km	(27) Parallel Trend	(28) Treatment t+3	(29) Treatment t+6	(30) 1% Outliers Removed	(31) 5% Outliers Removed
within	-0.0522***	-0.0502***	-0.0634***	-0.0575***	-0.0542***	-0.0666***
	(0.00784)	(0.0126)	(0.00622)	(0.00654)	(0.00742)	(0.00674)
withinpost	-0.0290**	-0.0307*	-0.00615	-0.0506***	-0.0256**	-0.0216**
	(0.0114)	(0.0166)	(0.00896)	(0.00826)	(0.0107)	(0.0101)
withinpre		-0.00448				
		(0.0186)				
Postal Code	Yes	Yes	Yes	Yes	Yes	Yes
Time Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Clustered S.E.	Yes	Yes	Yes	Yes	Yes	Yes
n	46	46	31	16	46	43
N	9883	9883	9883	9883	9689	8919
R^2	0.902	0.902	0.902	0.902	0.901	0.898
adj. R^2	0.898	0.898	0.898	0.898	0.897	0.893

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

n=number of treated observations

Table 6: ***Frequency of Rooms***

Rooms	Frequency	Per cent	Cum Dist
1	1,233	12	12
1.5	292	3	15
2	4,109	42	57
2.5	178	2	59
3	2,824	29	87
3.5	132	1	89
4	867	9	97
4.5	22	0	98
5	193	2	100
5.5	5	0	100
6	20	0	100
6.5	1	0	100
7	6	0	100
9	1	0	100
Total	9,883	100	100

Source: Booli.se

Table 7: ***Houses - Sample Composition***

	Pre		Post		Difference	
	mean	sd	mean	sd	diff	p-value
Living area	128.66	50.70	125.58	50.29	3.075	0.633
Age	64.64	22.15	63.65	21.84	0.987	0.725
Value points	28.65	4.92	28.38	5.17	0.268	0.673
Additional area	53.47	38.84	55.93	36.27	-2.464	0.612
Lot size	647.61	447.77	602.92	354.64	44.69	0.407
House type	1.50	0.83	1.60	0.90	-0.107	0.323
Waterfront house	0.01	0.12	0.01	0.11	0.00195	0.893
N	221		86			

Table 8: ***Apartments - Sample Composition***

	Pre		Post		Difference	
	mean	sd	mean	sd	diff	p-value
Living area	58.41	19.39	58.93	19.62	-0.520	0.778
Age	66.56	15.77	65.07	21.11	1.488	0.388
Rent	3365.11	970.92	3389.69	1020.21	-24.58	0.794
Rooms	2.28	0.93	2.26	0.91	0.0246	0.778
Floor	2.14	0.93	2.19	1.07	-0.0572	0.543
N	269		190			

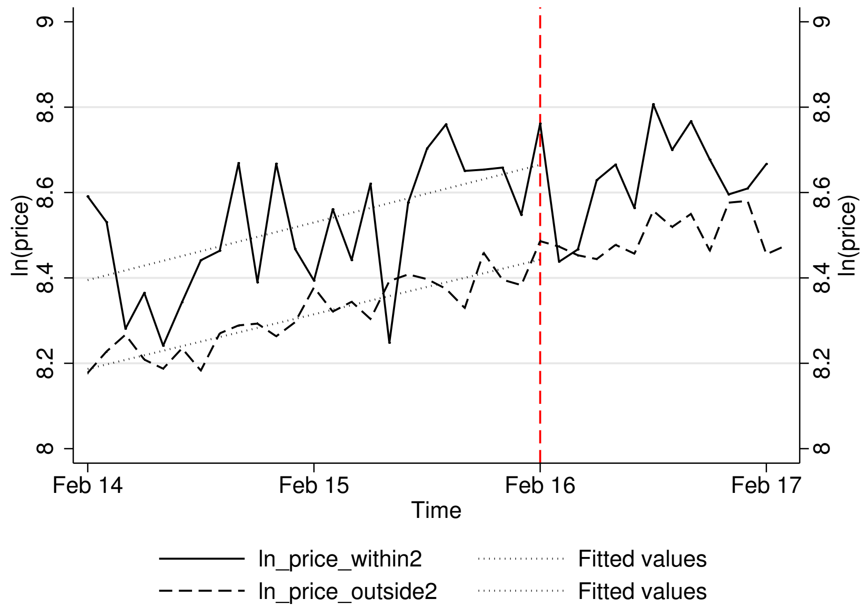

Figure 2: Parallel trend month - Houses

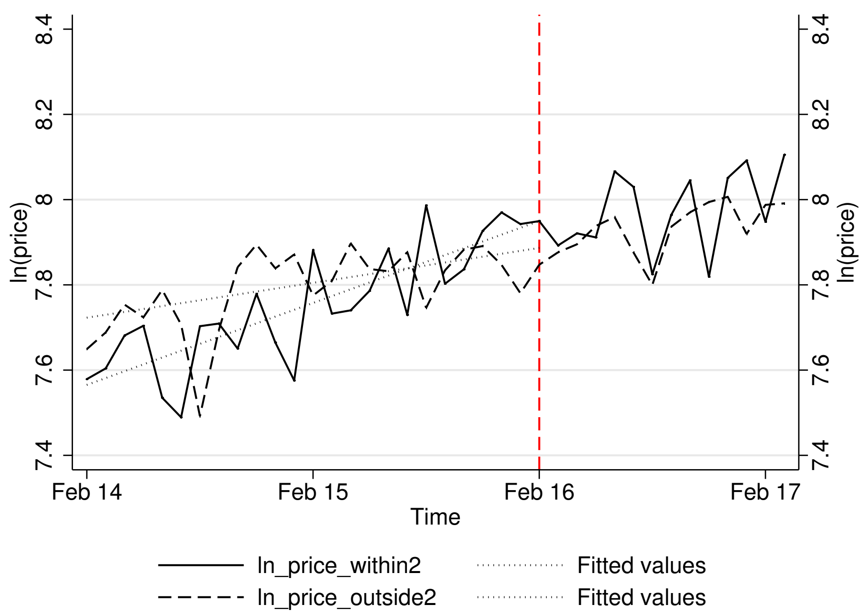

Figure 3: Parallel trend month - Apartment

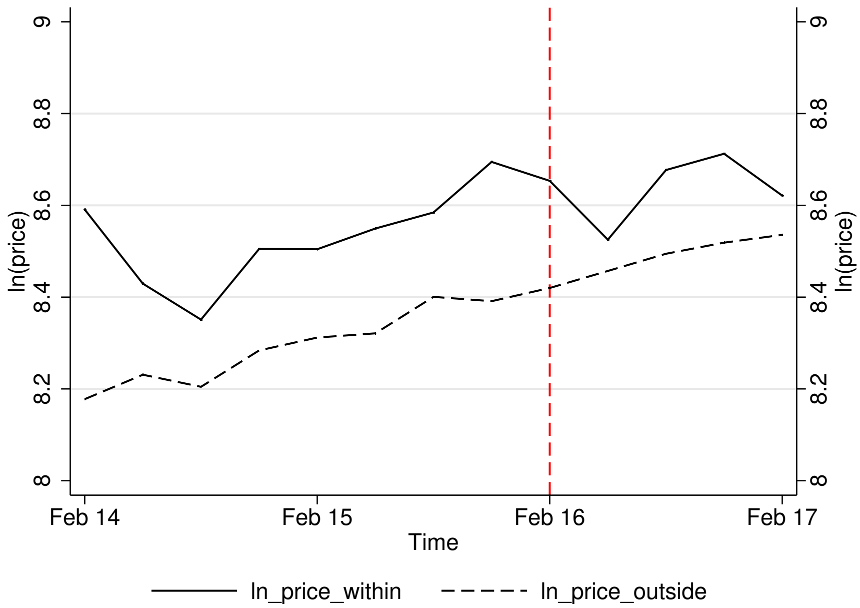

Figure 4: Parallel trend quarters - House

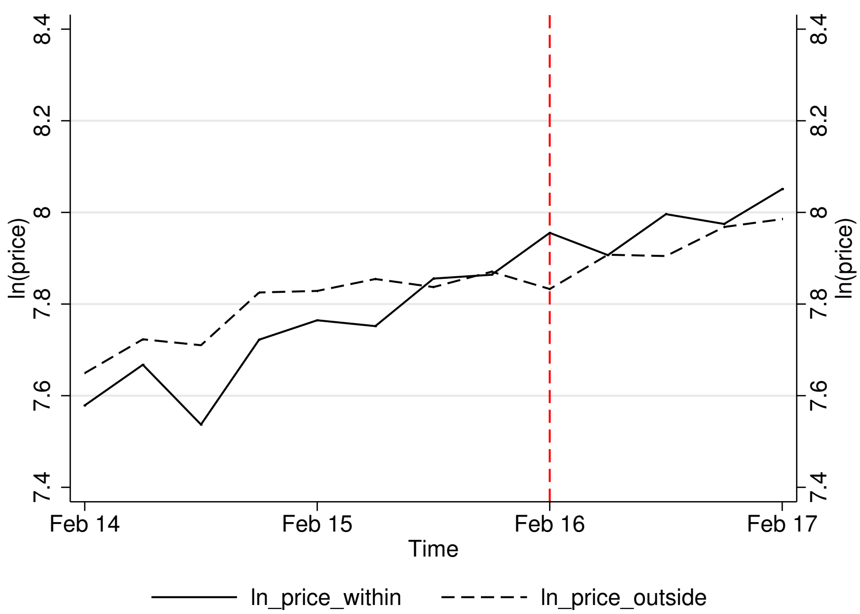

Figure 5: Parallel trend quarters - Apartment

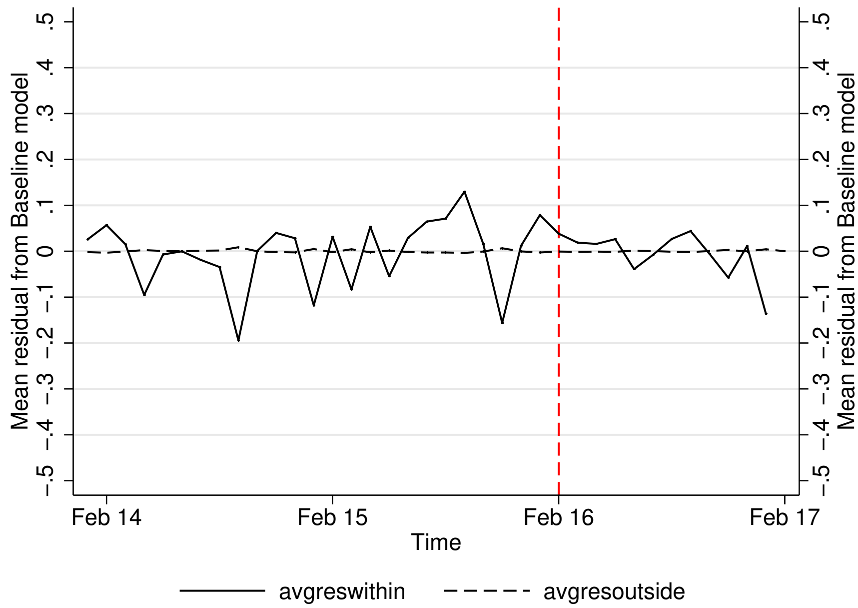

Figure 6: Parallel trend - House

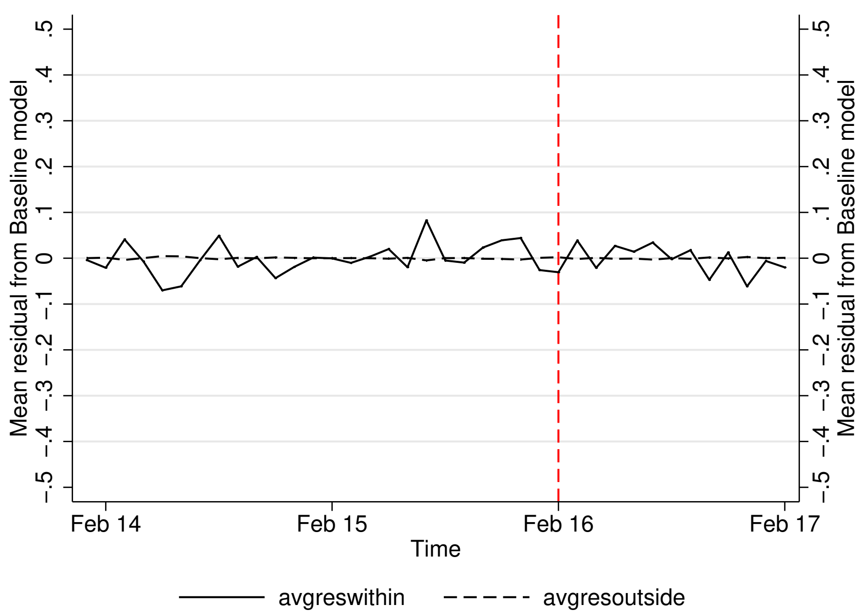

Figure 7: Parallel trend - Apartment

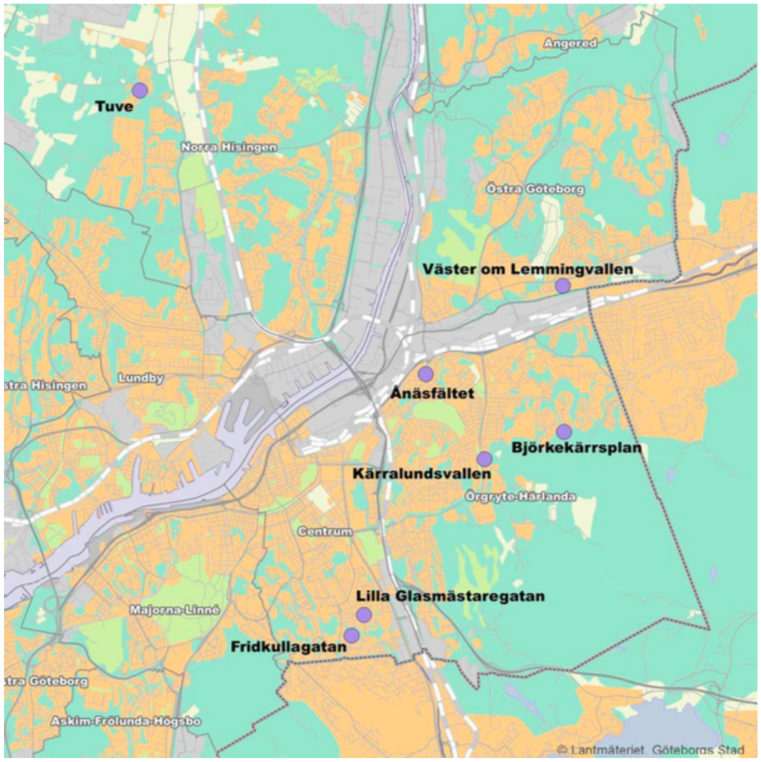

Figure 8: Location of sites - North (Göteborg Stad, 2016)

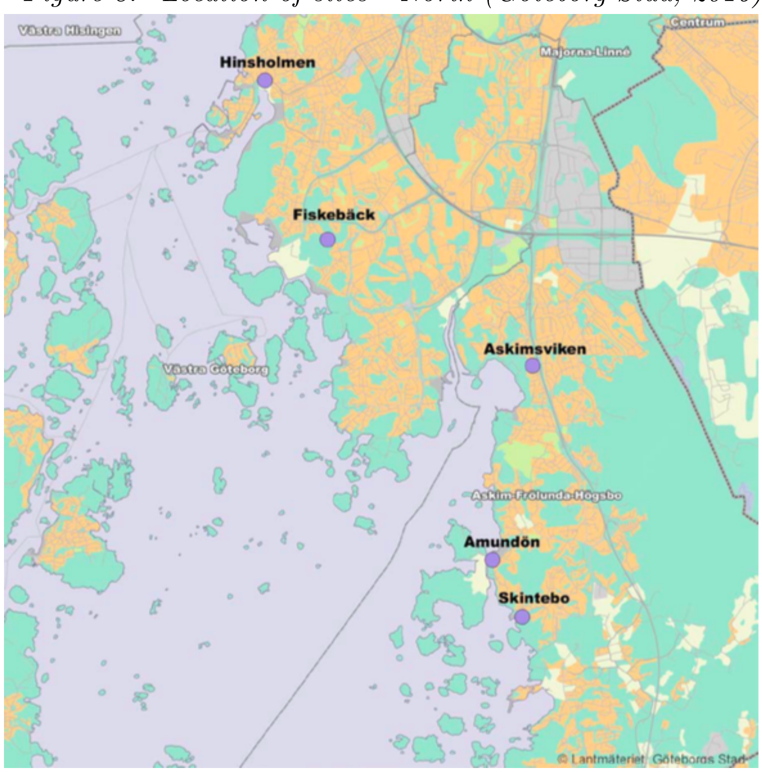

Figure 9: Location of sites - South (Göteborg Stad, 2016)