LUND UNIVERSITY SCHOOL OF ECONOMICS AND MANAGEMENT

The amortization requirement's effect on Swedish house prices

A second year Master's thesis about the policy's impact on the Swedish housing market with a difference-in-difference design

Abstract

This paper uses a common method for policy evaluations – difference-in-difference – to properly analyze Sweden's first amortization requirement that came into effect in the Summer of 2016. The aim of the thesis is to evaluate if the policy had any effect on house prices by comparing regional groups against each other. The results show that there is a significant difference between the treatment group and the control group after the policy was implemented. The municipalities in the treatment group experienced a larger increase in prices in 2016 and a larger decrease in prices in 2018 compared to the control group, and therefore compared to a scenario in which the policy had not been implemented.

Keywords: House prices, amortization requirement, diff-in-diff, Swedish housing market

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1. Introduction

Over the last decade, Sweden like many other countries have seen a dramatic surge in house prices and research suggest that house prices could be around 12 percent above its long-run equilibrium (International Monetary Fund, 2016). Although many agree that the main drivers behind the price increase are fundamentals[1], the combination of vast increasing prices and higher levels of household debt has been of great concern to policy makers. There are several examples showing that if household debt rises rapidly together with an increase in house prices, vulnerability in the financial system increases. In many of the countries hit the hardest by the global financial crisis of 2008, the crisis was led by rapidly increasing debts and house prices.

Sweden in particular, has seen household indebtedness rise from 90 percent of disposable income in 1995 to 181 percent in 2016 (OECD, 2018). Not surprisingly, countries that have not experienced a drop in house prices and an accompanying drop in household debt are attempting to identify feasible indebtedness reduction policies to lower the probability of a financial crisis. In Sweden, Finansinspektionen (The Financial Supervisory Authority, here on after referred to as the FSA) has for some time been in control over the design of macro prudential policy; and has played the leading role in addressing household indebtedness.

As early as 2010 the Swedish government decided to implement the FSA's first policy recommendation in an attempt to control and decrease household indebtedness. The policy was a mortgage cap which means that a firm that grants loans collateralized by a home should limit this credit so that the loan-to-value (LTV) for the home does not exceed 85 percent of its market value. Its objective when it was introduced was, and still is today, to counteract harmful lending practices in the mortgage market and thereby reduce risks for individual borrowers. The aim of the regulation is also to reduce the proportion of highly indebted households.

[1] See for example a report by Copenhagen Economics, 2017 or Burgert, et al., 2016

However, after many years of unchanged trends in increasing household debt and prices, the FSA came to the conclusion that an amortization requirement was necessary in order to adjust and slow down the market[2]. On November 2014 the FSA announced their proposal for an amortization requirement on new mortgages. The amortization requirement implies that new mortgage takers should amortize two percent of the entire mortgage until the LTV ratio is reduced to .70 of the home's value. Thereafter, they must amortize at one percent until the LTV ratio is lowered to .50 (Swedish FSA, 2015). As stated in the FSA memoranda (Swedish FSA, 2014) – the purpose of this policy is to reduce demand for housing and subsequently household indebtedness. A common worry revealed in both FSA documents and elsewhere is that the high level of indebtedness makes households vulnerable to shocks. If a household for instance becomes unemployed, its consumption may drop more if a substantial part of its budget is committed to mortgage payments.

The requirement was however stalled by the administrative court of appeal in Jönköping who detected some shortfalls in the policy. So instead of coming to effect in 2015, it was first implemented on June of 2016. The interesting part with this is the announcement effect that this comes with, as financial markets and households could be assumed to now be well prepared for this policy and that it did not come to most as a surprise.

The true effect of the first amortization requirement has been up for debate. In terms of indebtedness the country has seen some positive results, in which household debt have decreased for many groups and the levels of amortizations have increased. When looking at house prices the story is however a bit different. Shortly after the requirement not much change could be seen on the market and house prices all over Sweden continued to rise until the summer of 2017. Since then however, the majority of Sweden's

[2] Additional measures have been taken between 2010 and 2018 – such as a risk weight floor for mortgages at 15 percent, later increased to 25 percent, and a counter-cyclical capital buffer activated at one percent and thereafter increased to two percent.

municipalities have seen prices beginning to slow down and even decrease. It is still debatable if this is due to the amortization requirement or if there are other factors that have played a part in this.

Even though prices and debt begun to slow down the FSA determined that further measures needed to be taken. On March 2018 the FSA implemented a stricter amortization requirement saying that all new mortgage takers who take a mortgage above 4.5 times their pre tax yearly income must amortize an additional one percent every year. Perhaps the drop in prices a few months earlier has been a reaction to the announcement effect of the upcoming stricter requirement or perhaps the reason is a lagged effect of the first policy.

To understand the impact of a policy such as the amortization requirement is highly important, not only to policy makers but to other authorities, financial institutions and firms as well. By understanding its consequence on the housing market and particular on house prices, it can help policy makers when implementing new policies in the future, or authorities in other countries that are in a similar situation. Therefore, this paper will add to the extensive reports and controls that are made by the Swedish authorities, but with a slightly different approach that hopefully will broaden the view of the policy's effect and help others to better understand what the requirement's real consequences have been.

This paper is based on a commonly used method for analyzing policy implications, *difference-in-difference*, where two different groups are compared to each other over a certain period of time. This paper will argue that one group of municipalities in Sweden are more affected by the amortization requirement compared to a control group of municipalities that is argued to be unaffected. The empirical strategy is to divide the sample of municipalities based on various financial measures such as municipalities' average price- or debt-to-income ratios and whether or not a municipality belong to one of the greater city regions in Sweden.

The results show that the amortization requirement did have an effect on house prices and that prices in 2016 increased to a larger extend, perhaps due to some announcement effects prior to the policy, and that prices in 2018 decreased more for the treatment group compared to the control group.

The rest of the paper is constructed as follows; the next section contains some previous research on the topic and section 3 will explain the theoretical arguments. In section 4, the method for this paper is explained and section 5 explains the empirical strategy followed by the authors hypothesis in section 6. The paper continues in section 7 by describing the data and some descriptive statistics before presenting the results in section 8. The paper ends with a short discussion in section 9, followed by a part about the paper's limitations in section 10 and a final conclusion in section 11.

2. Previous research

Most previous research on this subject is conducted by governments, policy makers or other financial authorities. In Sweden, several reports from Riksbanken (the Swedish Central Bank) and the FSA are trying to properly analyze the effects of the requirement.

First off, a number of reports were made before the implementation of the requirement in an attempt to predict its effect on the market. In a report from Riksbanken in 2014 they used simulations of a DSGE-model with the results that real house prices would decrease with .5 percent to 1.3 percent on an aggregate level (Sveriges Riksbank, 2014). Their model does however not detect the risk that households might change their behavior, which could impact house prices even more. Similar reports were conducted in the following years in which similar results could be presented (Swedish FSA, 2014).

As of last year, the FSA reported that they expect the stricter requirement to result in households borrowing less and buying less expensive homes, much like after the implementation of the current amortization requirement. For the

country as a whole, the stricter amortization requirement is expected to slow the growth of debt by almost four percent and house prices by approximately 1.5 percent. In Stockholm, growth in house prices is expected to decrease by about three percent according to their report (Swedish FSA, 2017). Further analysis has been made on the effects that a decline in house prices would have on real GDP, consumption and the labor market (Konjunkturinstitutet, 2014) and continues stress test of the housing market is being made by the FSA (Swedish FSA, 2018b). These kinds of studies are important in order to better understand the macroeconomic effects that the amortization requirements would come with, and to help policy makers decide whether or not to implement such policies. They are however only projections of a policy that has not yet been implemented and the actual results could be very different.

Because the amortization requirement was implemented as late as June 2016, it does not come as a surprise that not that many studies have been made on its effect on the housing market. It is fair to argue that the timeline has been far too short and it is only now that we begin to see collected data from 2017 and 2018 being analyzed. There are a few reports that look at the descriptive statistics without drawing any conclusions to the casual effects. Yearly reports from the FSA in April 2017 and 2018 are describing the changes in household debt and prices, but none of the reports can show that the changes are due to the policy.

There is however one report from the FSA on April 2017 that is trying to analyze the effects of the amortization requirement on household debt (Swedish FSA, 2018). They do this by using micro data on households in Sweden, and comparing three groups against each other with a difference-in-difference design. The idea here, being similar to this paper, is that they can argue that one group of mortgage owners are unaffected by the amortization requirement and can therefore be used as a control group. Their results show that new owners take smaller mortgages than if the FSA had not implemented the amortization requirement. They are also buying less expensive homes. The groups that must increase their amortization payments the most are also

those that are affected the most. The report does however not draw any conclusion in to how the requirement effect house prices on an aggregate level. Although this report focuses more on the requirement's effect on household debt, the approach is still interesting and a similar method can be used in order to further analyze how the policy have effected house prices. It is also interesting to follow up with this report since it was conducted without data from 2017 and 2018.

The advantage of the FSA's report is the available micro data that the they possess. By analyzing data on individual level, it is easier to see what households that should belong to a certain group and then compare groups that we know are statistically different from each other. The disadvantage however is in the fallacy of the data being accurate since the micro data is a sample of the true population[3]. Since the data only covers a small sample of the true population, there is a possibility that the data might not properly reflect the real society[4].

There are many theoretical papers that support the use of a stricter LTV policy as a very effective measure for reducing household indebtedness, which lowers house prices as well[5]. There are however other papers which show that in a framework where both LTV limits and debt repayment limits (debt service to income ratio - DSTI) are imposed on borrowers, tighter LTV regulation may have no effect on household indebtedness ratios and may actually lead to an increase in house prices in equilibrium (Grodecka, 2017).

Following the available literature that analyze the policy's effect on aggregate and individual level, this paper continues the work of a report made by Riksbanken in June 2017 in which they choose to take the analysis on a regional level (van Santen, 2017). The report analyzes household debt on a regional level in order to better understand the current market situation. To

[3] For a closer description of the data being used see Swedish FSA, 2017
[4] Some evaluation of the dataset has however been done, see Grodecka, 2017.
[5] See both Chen et al., 2016 and Finocchiaro, et al., 2016

understand the regional differences in household debt and prices is interesting for a number of reasons. Firstly, by understanding the regional differences, one can also draw certain conclusions on the vulnerability of regions from a macroeconomic shock. Secondly, it is important to understand what specific types of regions that are being effected most, and perhaps therefore, certain regions should be targeted differently. As an example, recent macro policies in Denmark and Norway has been implemented by focusing on the greater cities alone, where the rise in house prices have been the largest.

This paper will therefore combine the idea of using a difference-in-difference design made by the FSA and the regional data set analyzed by Riksbanken in order to better understand the impact of the policy on house prices.

3. Theory

The theoretical background of this paper uses insights from life-cycle models in which households strive to achieve a smooth level of consumption across the life-cycle (Friedman, 1957)[6]. Since households are not bound by their own income, but instead can take part in functional borrowing and lending markets, they can achieve a higher level of living standards today and pay of debt in the future. In modern times, for individuals who are planning to buy a home this has become more or less praxis and there are today few individuals who own their home without having some part of the value as a mortgage. Individuals are however bound by certain constraints that will limit them in their demand for credits. Individuals must first possess access to financial markets, historically this would have been an obstacle for a large part of the population. In modern times however, with the introduction of internet and better communications, a clear majority of the developed world share almost equal available access to financial markets and borrowing and lending opportunities. As in this paper, when drawing conclusions from data of different municipalities in a developed country such as Sweden, one could

[6] Or see (Brumberg et. al., 1954)

be assured that there will not be any differences between communities or households.

Individuals are thereafter constrained by their own income, wealth and financial background. In order for lenders to minimize the risk of not getting future payments or total default of debts, borrowing is only given to individuals with a trustworthy credit record. One assumption that this paper will make is that the distribution of different types of mortgage takers looks similar for each municipality in the country. That is, if we observe data from two municipalities with similar statistics on e.g. average loan-to-value ratios or average house prices, we will then also assume that the underlying distribution of the data from these two municipalities look the same and that the distribution of different types of households are similar.

Assumptions also have to be made on the distribution of new mortgage takers between the different municipalities. In other words, we must assume that municipalities have the same ratio of new mortgage takers before and after the policy[7]. Furthermore, we must assume that new mortgage takers after June 2016 will have similar characteristics as previous mortgage takers. I.e. there is no difference in disposable income, consumption spending or savings between before and after the policy for the same group of people.

If we imagine that this was not the case, then we could have a municipality which according to theory should be highly effected by the policy, lets say Stockholm, but after the introduction of the policy did not have any new mortgage takers, or mortgage takers that were substantially different from the previous mortgage takers. On the contrary, a municipality that before June 2016 did not seem like it would be affected by the policy, could have new mortgage takers after June 2016 that were very different from the other individuals and were in fact highly affected. From the data used in this paper we can not draw any such conclusions and must therefore assume that the

[7] In the next section we describe how to overcome this problem by limiting the sample of municipalities.

population and types of mortgage takers did not change before and after the policy for each municipality. Some evidence for this could perhaps be found in the FSA's report from 2017 and 2018 which is built on individual micro data. Nothing in those reports suggest that individuals have changed their types or that any drastic changes have occurred between the two periods when it comes to households' characteristics.

Furthermore, we can of course note that there are many other factors that determine house prices in the economy, but due to the limitations of this thesis those will not be described here. These factors will in a difference-in-difference model also effect both groups in equal fashion and are therefore not of concern to the analysis[8].

3.1 Why prices should decrease

This part of this section will argue why in theory the amortization requirement introduced in 2016 should decrease house prices. First off, the direct effect is that new mortgage takers after June 2016 will be forced to take a part of their disposable income and spend it on amortization payments every month. It will therefore have an impact on how much debt a household can accumulate. Individuals who before was not forced to make this payment, would have more money left for interest payments and other monthly living costs. This means that when making budget decision on housing spending they could choose to spend less on amortizing their mortgage and have more money left for other costs. These types of individuals would after June 2016 be forced to find housing that were a little bit cheaper since they no longer had the option to not amortize. Such households now need to look for houses with lower prices if their monthly housing costs were to stay the same. We now have a situation in which demand for expensive housing is curbed. If we assume that there is a large enough part of the population that are affected by the policy, then we can also assume that after some time this will lead to house prices falling.

[8] These factors could be tax levels, living costs, interest rates, inflation or other macroeconomic variables.

There is a second reason to why the policy could decrease house prices. An amortization requirement that covers new mortgages could inevitably create a lock-in effect in the housing market. This is because households, insofar that they consider that their desired consumption level is affected by the amortization requirement over time, decide not to move to the same extent as they otherwise would. These households may decide not to move to avoid being subject to the amortization requirement. Such lock-in effects mean that mobility is reduced in the housing market and involve a socioeconomic cost. We thus get another reason to why demand for houses could decrease and therefore prices would drop.

If the effects do occur, it is more difficult to make any expectations on when prices should be falling, but a fair assumption would be that there is some adjustment time and that it could take some time before any effects are being observed.

Before we move on with the analysis we should state some theoretical arguments against the amortization requirement's effect on a fall in house prices. Since the policy only require households with mortgages to amortize, individuals could change to other types of loans and avoid the policy. This is something of a concern to policy makers and there are some who fear that the policy will have opposite effect and that households change to riskier types of debt with higher interest rates and that house prices therefore will continue to rise[9].

Another argument is that as long as interest rates remain low and disposable income continue to increase, living costs will as a result decrease. Thus the effect of the amortization requirement on house prices should be limited as demand for expensive homes continue to rise. Furthermore, if the number of households that are effected by the policy is to low, then it might be difficult to see any effects on an aggregate level.

[9] See for example (Svensson, 2016).

3.2 How to group municipalities

The next part of the theory section aims to describe a theoretical explanation behind why a certain group can be assumed to be affected by an amortization requirement and another group will not. The first part is strait forward since the amortization requirement clearly states who with respect to LTV ratios that should be effected and who should not. But by only using an estimate in which we observe LTV ratios might not be enough. A more thorough analysis might be necessary since we want to include individuals that might be in a risky situation and could be affected without having high LTV ratios. Think about the difference between a household who has a very large LTV ratio on their mortgage but the value of the house is small and their debt-to-income (DTI) ratio is very low. Will this household be more or less affected by the amortization requirement compared to a household with the opposite characteristics? We can argue that the first mentioned household will increase their relative amortization payments in a larger extent, but it is more difficult to say how their financial budget decisions will be effected. It is not straightforward to answer what specific households that will be more effected and every case will look very different, but to conclude, we can argue that it will not be enough to only look at the LTV ratio as a measurement, but instead a few other ones.

Let us begin by looking at the DTI ratio which is described as the total amount of debt over the yearly income for a household. The average debt ratio in Sweden have been rising for some time, but for private owned houses its been fairly stable around 385 percent over the last three years. According to theory, the higher DTI ratio a household poses, the more effected should they be by the amortization requirement. The reason is that the policy will put an extra burden on a household's financial budget if the debt is very high or the income is very low. One could argue that a high debt ratio does not say much about a certain household's possibilities to pay of its debt with the introduction of an amortization requirement, it is however difficult to argue that the effect would go the other way. A similar measurement that this paper will use is the share of households in a municipality with substantial high DTI ratios. The

argument here is that a municipality with a large share of households with high DTI ratios should be more effected by an amortization requirement compared to a municipality with a lower share.

Secondly we look at the level of households' mortgages. The argument here is that a household with a larger mortgage will be more affected by the policy. Because the amortization requirement directly affects the payments on the mortgage, it will become more expensive for households that posses a larger mortgage.

The third measure that this paper chooses to look at is the market value relative to income, or price-to-income (PTI). Theory suggest that a municipality with higher market values relative to income should be more effected by the policy than a lower one. The reason again being that if a household poses a mortgage on a house with higher market value to income, then being forced to amortize on that mortgage will be more difficult compare to a household with low market value to income. Again, one could argue that a certain household might not be effected at all since a high market value to income does not say anything about what type of mortgage or savings that they have or what payments that they need to make. It is however again more difficult to argue that the opposite would be true. This measurement of a household's vulnerability might be the most effective one as it includes information about several of the other factors, such as income and prices in a municipality.

Other variables that the paper will consider to have an effect on regional house prices are disposable income, gross regional product (a measure of regional growth) and changes in population for each municipality. Theory suggest that house prices will increase with disposable income and gross regional product - as more people can afford housing, demand rises and prices increase. In a similar fashion, a higher population should lead to more people competing for a fixed supply of housing which should increase prices.

4. Method

The method used in this paper is known as the difference-in-difference and is commonly used to compare the effect of a policy or reform for those affected (the groups subject to the requirement) to a comparable control group. Another common interpretation is that the method allows one to evaluate a policy compared to the scenario in which it was not implemented. The difference-in-difference method adjusts for factors that affect both the groups, and the results can then be interpreted as a causal effect of the policy. A normal application of difference-in-difference is the evaluation of political reforms[10] and for a review of the basic methodology see Imbens & Woolridge, 2009.

The main idea in a difference-in-difference model is that you have two groups that before a policy showed similiar trends in the dependent variable (that is they had a constant difference between them), but after the policy the difference between the two groups changes[11]. Since these groups are similar in all other fashions and are impacted by the same exogenous factors, one can say that the changes in differences between the groups are due to the policy and not some other factor. The standard difference-in-difference model will be constructed as follows;

$$\Delta P_{it} = \alpha + \gamma * Post_t + \lambda * Treated_i + \delta * (Treated_i * Post_t) + \varepsilon_{it} \quad (1)$$

Where ΔP_{it} is the dependent variable *yearly change in house prices* for municipality *i* in period *t*. $Post_t$ is a dummy variable that is set to 1 if a certain year is considered to be after the policy[12]. $Treated_i$ indicate if a municipality is in the treatment group or not, where 1 equals treated and 0 otherwise. The final variable is of most interest, $Treated_i$ * $Post_t$, this is a dummy variable

[10] See for example (Card, 1994)

[11] One of the most important criteria for the selected groups is that they show a common trend before the policy. This is confirmed for all tests in this paper.

[12] The post-period will be changed for robustness checks in the analysis.

that indicate if municipality *i* is in the treatment group during post-period *t*. Finally ε_{it} is an error term with the standard assumptions.

Besides the standard difference-in-difference model, a generalized version will be used in the analysis[13] and is constructed as follows;

$$\Delta P_{it} = \alpha + \beta_1 Y13_t + \beta_2 Y14_t + \beta_3 Y15_t + \beta_4 Y16_t + \beta_5 Y17_t + \beta_6 Y18_t + \beta_7 Y13_t * Treated_i + \beta_8 Y14_t * Treated_i + \beta_9 Y15_t * Treated_i + \beta_{10} Y16_t * Treated_i + \beta_{11} Y17_t * Treated_i + \beta_{12} Y18_t * Treated_i + \varepsilon_{it} \quad (2)$$

Where $Y13_t$ to $Y18_t$ are yearly dummy variables and $Y13_t * Treated_i$ to $Y18_t * Treated_i$ are yearly dummy variables for municipalities in the treatment group. That is, these dummies equal 1 if municipality *i* is in the treatment group and 0 otherwise. The model can therefore be interpreted as the extra effect that a municipality in the treatment group will have on the change in house prices for a specific year. These effects can be estimated from the variables $Y13_t * Treated_i$ to $Y18_t * Treated_i$. Note also that the dummy variable for year 2012 has been dropped and therefore the constant α can be interpreted as the starting year.

5. Empirical Strategy

The first step is to divide our sample into two comparable groups, a control group and a treatment group. The treatment group should consist of municipalities who we can assume and argue would be effected by the amortization requirement before 2016 and the control group the opposite. This is the key part of this paper because the result might be very different depending on how we choose to divide the groups.

[13] The necessity for a generalized model will be explained in section 5. and 8.

It seems fair to begin by following the strategy of the FSA in their analysis on the policy's effect on household debt. In their report they divide their data into groups that directly follow the thresholds of the requirement[14]. The data is divided into three different groups depending on their loan-to-value ratios.

The problem with this strategy when not analyzing data on individual level and not having data on new mortgages only, is that almost none of the municipalities have average LTV ratios below 50 or above 70 percent for any given year. Municipalities also seem to have very similar trends in their LTV ratios and it is difficult to find any clear outliers. Therefore, this strategy is ill-fitting for our analysis[15].

A more straightforward strategy is to divide our sample depending on the average price-to-income (PTI) ratio of households in each municipality and for every year. That is, if a municipality show numbers of higher PTI ratios than the country average for that year, then they are determined to be in the treatment group. On the contrary, if the PTI ratio is lower than the country average then the municipality end up in the control group.

The argument behind dividing our sample in to these two groups is that municipalities that show a high PTI ratio will be more effected by the amortization requirement compared to municipalities with lower ratios. A high PTI ratio show us that a household have either a very expensive house, very low income or high indebtedness. All of these three factors can be argued to have the characteristics of a household that will be effected if forced to amortize either one or two percent every year.

[14] The FSA use the same strategy in their report for determining the effect of the mortgage roof policy in 2010 on household debt. That is, they divide the groups by the given threshold of the policy.

[15] In the theory section there is also stated and argued why this measurement alone might not be the best to use in order to determine what municipality that is being affected.

We divide our sample into two groups depending on the PTI ratios for every year between 2011 and 2014 and run a total of four regressions based on the standard difference-in-difference method.

After evaluating the results from the first test we can conclude that a better assessed model is necessary in order to better comprehend the results. The problem that occurs when running the standard difference-in-difference model is that two different trends for the treatment group in the post period will offset each other, making it impossible to find any clear results.

By using a generalized model, we can more easily se the effect of the policy for every year after the event in 2016. The results, as we will see in the next part, clearly show us that the treatment group show two very different trends that obviously offset each other if they were to be analyzed as one trend.

After running the generalized difference-in-difference model based on the PTI grouping we make a number of robustness checks in order to see if the results hold with other groupings. The second method of dividing the groups are made based on a municipality's DTI ratio for every year before the policy (2011-2014). We place municipalities in the treatment group if they posses a DTI ratio higher than the country average for a specific year and in the control group otherwise.

The empirical strategy then continues with further robustness checks were we divide our sample based on if a large share of households in a municipality posses particularly high DTI ratios. The threshold is drawn at 400 percent, and if part of a municipality's population is above the country average for that year then they end up in the treatment group[16].

[16] E.g. in 2011 Boden had six percent of its population with a DTI ratio above 400 percent. Since this number is lower than the country average for that year (which was 25 percent) they end up in the control group. On the contrary, Värmdö had 51 percent of its population with a DTI ratio above 400 percent, which is above the country average and therefore end up in the treatment group.

The final check that this paper makes is to divide our sample based on if a municipality belong to one of the three large city regions in Sweden[17]. The argument here is that there is a vast majority that believe that these are regions which will be impacted the most, since these municipalities have both higher levels of debt and prices. This is the most straightforward method and could be the simplest one to argue for as it includes information on several other important variables.

6. Hypothesis

The hypothesis of this paper is that there should be a statistical significant difference between the control group and the treatment group due to the implementation of the amortization requirement on June 2016. The hypothesis is that the control group should not be effected by the policy and therefore will replicate the "true" change in house prices in Sweden if the amortization requirement had not been implemented. The treatment group however, will be effected by the policy and should therefore experience a different trend in the change of house prices.

Therefore the hypothesis is that δ in equation (1) should be statistically significant different from zero and have a negative sign, and that β_{10}, β_{11} and β_{12} in equation (2) should be statistically significant different from zero and all have a negative sign.

If the analysis can reject the hypothesis that there is a statistical significant difference between the two groups, then one could argue that the changes in prices over the last year has not been due to the amortization requirement but is due to some other common macroeconomic factor.

[17] The regions are; The Greater - Stockholm, Gothenburg and Malmö Regions.

7. Data and descriptive statistics

This paper solely focusses on private houses and not private housing cooperatives/tenant-owned apartments. The reason is because there are more available data for private houses and their households' financial situation and because there is a more even distribution of private houses being sold between municipalities in the country. Data has been taken from Riksbanken and covers all municipalities in Sweden between 2010 and 2016. The dataset includes information on mortgages for all households with loans at one of the eight largest banks in Sweden[18], households without a mortgage are therefore not included in the data. The data consists of yearly average in private housing market value[19], debt-to-income ratios, loan-to-value ratios and average mortgage rates for each municipality and year. Additional data on house prices from 2017 and 2018 is added from Svensk Mäklarstatistik (Swedish Real Estate Statistics). Furthermore, data on disposable income, regional price indices and gross regional product is taken from Statistiska Centralbyrån (Statistics Sweden) and data on amortization payments and other financial statistics are from the FSA.

One should note again that the data does not only cover new mortgage takers which might makes one question if the data matches the analysis properly. Since the amortization requirement does only effect new mortgage takers, it might be an advantage to only observe those types of households. This could be of some concern, but the assumption here is that it, if anything, should undermine the result. That is, if the hypothesis could be proven valid, then the effect should be greater if we only looked at households with new mortgages for every year and municipality[20].

[18] The eight largest banks are Danske Bank, Handelsbanken, Länsförsäkringar Bank, Nordea, SBAB, SEB, Skandiabanken och Swedbank.

[19] Unfortunately not available, it would be more ideal to use price per m^2 as explanatory variable.

[20] In the FSA's analysis with individual micro data they only look and compare new mortgage takers

The total number of municipalities in Sweden are 290, this paper does however only focus on the number of municipalities with households above 5000 for any year between 2011 and 2016. The reason for this is because we want to avoid including municipalities that had very few houses sold. For instance, in some municipalities only a few houses where sold between two years. Depending on the type of house being sold that year, we could observe considerable changes in house prices between the years which would not accurately represent the average yearly change for that municipality. By only observing municipalities that have a number of households above a certain threshold, we can be sure that the yearly change in house prices is not effected by a small amount of outliers. Upon further investigation, two other municipalities were removed due to very few sales in 2017 and 2018. The data does after this cover 104 municipalities.

The average number of private houses in the observed municipalities is 14.300 with the highest number being in Stockholm with 212.000 private houses in 2016 and the lowest being Ystad with 4.500 in 2011[21]. The average price in the 104 municipalities were 2.71 million Swedish kronor (SEK) with the highest average price being in Danderyd in 2016 (11.6 million SEK) and the lowest in Härnösand in 2012 (732.000 SEK).

One important aspect to look at is if the amortization payments differs a lot between regions and how this have changed before and after the requirement. This can be examined by observing data from the FSA's yearly housing market report (Swedish FSA, 2018). The report analyze data from yearly surveys on households' financial decisions like amortization among other things.

Graph 1 presents data on amortization payments for five different regions in Sweden between 2014 and 2017[22].

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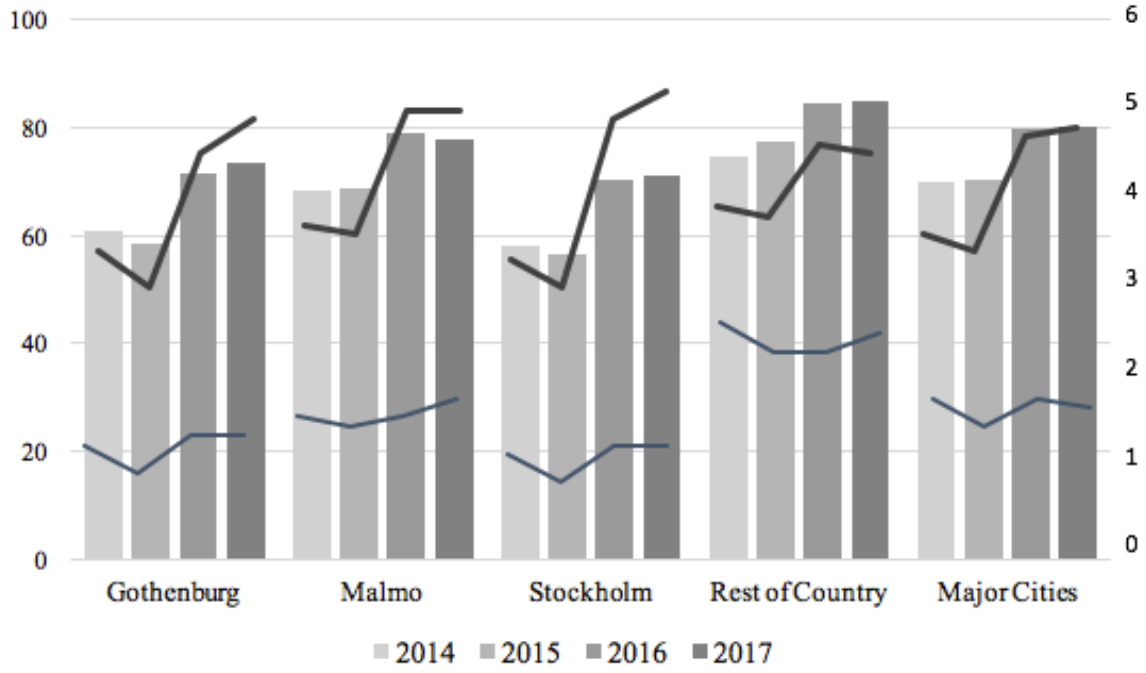
[21] Ystad (like some other municipalities) are included in the sample because they reached over five thousand in at least one of the years.

[22] In this report, Stockholm is represented by the 26 municipalities that is said to be the Greater Stockholm region, Gothenburg is the 13 municipalities that is the Greater

Graph 1 - Amortization payments in five Swedish regions (2014–2017).



Note - The bars show the ratio of the population that amortize (left axis), the darker lines show amortization payments as part of income (right axis) and the lighter lines show amortization payments as part of total debt (right axis). The graph is constructed by data from the FSA's yearly report on the Swedish housing market (Swedish FSA, 2018).

The share of households that amortize increased for all regions in 2016 when the policy was implemented, and remained higher in 2017 as well. The share of amortization payments relative to income did also increase for all regions in 2016 but amortizations relative to total debt remained fairly stable. This is important evidence to observe since it tells us that the policy did actually have an effect on households' amortization payments in the whole country. It also tells us that households after June 2016 spent more of their disposable income on amortization than before.

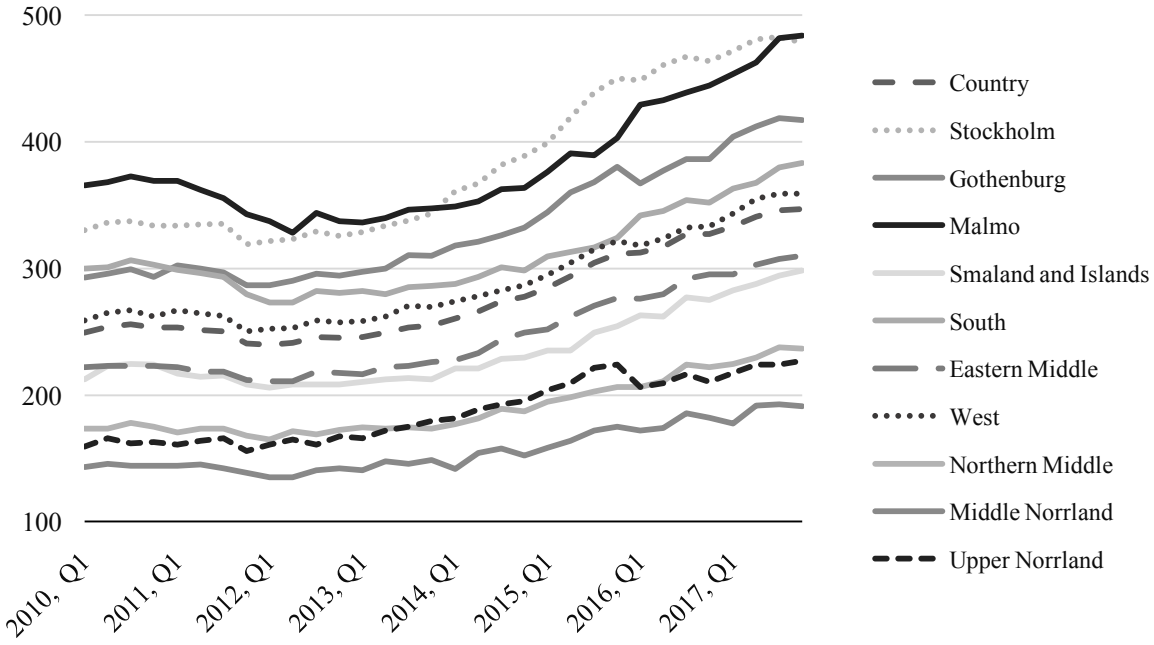
Gothenburg region, Malmo the 12 municipalities that is the Greater Malmö region, Major Cities are municipalities around the country that are considered large cities (these include Boras, Eskilstuna, Gavle, Halmstad, Helsingborg, Jönköping, Karlstad, Linköping, Norrköping, Sundsvall, Umeå, Uppsala, Västarås, Växjo and Örebro. The Rest of the Country are municipalities outside of the previously mentioned.

Another interesting aspect is that the data show that there are fewer households who amortize in the three major city regions compared to the rest of the country. These are also regions in which house prices are the highest. Before 2016, without the policy, fewer households in these regions choose to amortize compared to the rest of the country. These three regions saw a major change in amortization payments relative to income (all above five percent in 2017) which give us an idea that households in these regions would be more affected compared to the rest of the country after implementing the policy.

In Graph 2 below, one can also study how house prices have changed over the years for different regions in Sweden. Common for all regions is that house prices have been increasing until the last quarter of 2017, after which prices are beginning to stagnate. Note here that the house price indices are not the house prices that is used in the empirical analysis.

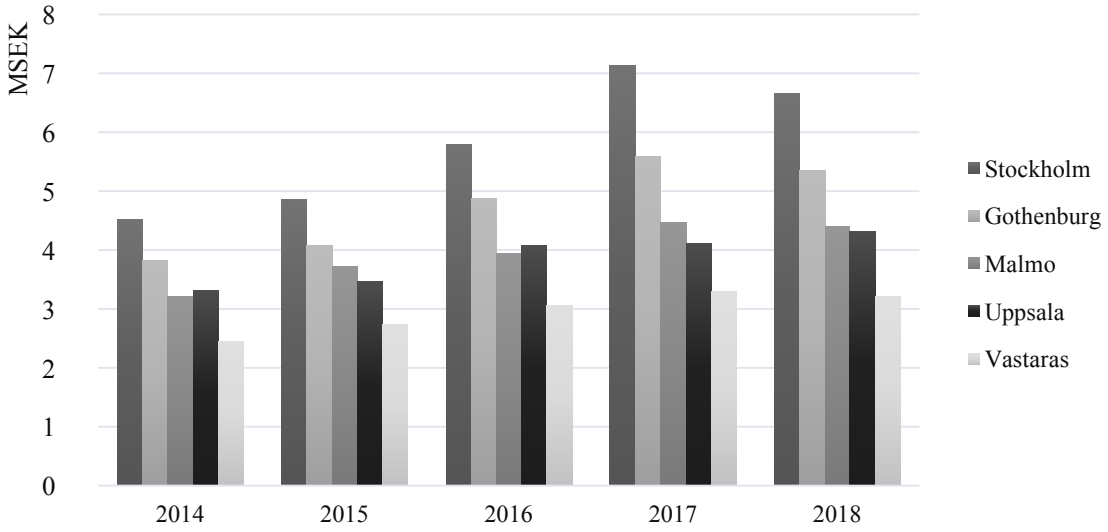
Graph 3 shows some updated data from 2014 to 2018 on the five largest cities in Sweden. It is clear that house prices after 2017 are beginning to decrease. This is important because it gives some evidence to this paper in that the amortization requirement indeed could have had an effect on house prices since they have decreased or stagnated some time after June 2016.

Graph 2 - House Price Indices for Swedish regions (2010-2017)



Note - House price indices for different regions in Sweden between the first quarter of 2010 and the fourth quarter of 2017 (1986 = 100). The indices are adjusted for inflation. The graph is constructed by data from Statistics Sweden.

Graph 3 - House Prices in Sweden's five largest cities (2014 - 2018)



Note – The graph shows actual average house prices in Sweden's five largest cities between 2014 and 2018 where prices are given in Million SEK. The graph is constructed by data from Statistics Sweden.

8. Results

The results from the empirical analysis are partly in line with what theory would predict, and the results hold for numerous robustness checks. There are however some unsolved issues with regards to the results not being valid when including cross-section fixed effects.

Let us begin by presenting the results from a standard difference-in-difference model where the sample has been divided based on if a municipality is part of any of the three Greater city regions in Sweden. The results are presented in Table 1. Three different tests are made based on different assumptions on the post-period, that is, we first assume that the post-period was the years 2015 to 2018, then 2016 to 2018, and finally 2017 to 2018. The argument behind including 2015 in the post-period is that the policy was announced as early as 2014 and therefore this year could be assumed to be impacted by some sort of announcement effect. The result shows us that the interaction variable ($Treated_i * Post_t$) is only significant for regression 3. The reason behind this is because when including 2016 as a post-period year the upward trend for the treatment group in that year offsets the downward trend in 2018 and therefore the total effect in collective post-period will be canceled[23]

Table 1 - Standard difference-in-difference results based on municipalities in the greater city regions

Dependent variable: Percentage change in house prices			
Variable	(1)	(2)	(3)
Constant	.03***	.05***	.06***
Post	.03***	.01	-.03***
Treated	.00	.00	.00
Treated x Post	-.00	-.00	-.03**
	(.0153)	(.0154)	(.0165)
Fixed effects	No	No	No
Post-years	2015-2018	2016-2018	2017-2018

NOTE. – Standard errors in parenthesis. Significant values are presented by: *** at the 1% level, ** at the 5% level and * at the 10% level. Post-years refers to the time period that is considered to be the time after the policy.

[23] This can be see from the results of the generalized model.

The next analysis in which a generalized version of the standard difference-in-difference is made will better show us why this is the case. In this model we are presenting variables for all years between 2013 and 2018 and include a dummy variable for all municipalities in the treatment group for the same years. The results of the first test where the sample is divided based on if a municipality belong to one of the three greater city regions, is presented in Table 2. The results show us that there is a significant positive effect for the treatment group in 2016 and a significant negative effect for the treatment group in 2018. This is evidence to why these two trends offset each other if we analyze this period as one collective post-period. The results are robust for including cross-section fixed effects. The results can be interpreted as follows; municipalities in the treatment group, who is more effected by the policy, experienced a larger positive increase in house prices in 2016 and a larger negative increase in 2018. According to the results, in 2016 the treatment group increased its house prices with around three percent more compared to the control group. In 2018, the treatment group experience a four percent larger decrease in house prices compared to the control group.

Table 2 – Generalized difference-in-difference results based on municipalities in the greater city regions

Dependent variable: Percentage change in house prices		
Variable	(1)	(2)
Constant	-.10***	-.10***
Y13	.25***	.25***
Y14	.16***	.16***
Y15	.19***	.19***
Y16	.22***	.22***
Y17	.16***	.16***
Y18	.10***	.10***
*Y13*Treated*	-.00	.00
*Y14*Treated*	.00	.01
*Y15*Treated*	.00	.01
*Y16*Treated*	.03***	.03**
*Y17*Treated*	-.00	-.00
*Y18*Treated*	-.04***	-.04**
Fixed Effects	No	Yes

NOTE. – Significant values are presented by: *** at the 1% level, ** at the 5% level and * at the 10% level. Fixed effects refers to cross-section fixed effects i.e. municipality fixed effects.

Table 3 show us the results from several generalized difference-in-difference regressions where the sample has been divided based on if a municipality had PTI ratios that were above the country average for a certain year. There are four regressions based on the years 2011 to 2014 and robustness checks for cross-section fixed effects are included for all models. The results here are similar to the first test; the treatment group show a significantly larger increase in house prices in 2016 and a significantly larger decrease in 2018. The results in 2018 are however not robust for including cross-section fixed effects, except for regression (4). The significant effects in 2016 does however remain for all regressions.

Table 3 – Generalized difference-in-difference results based on price-to-income ratios for four years (2011-2014)

Dependent variable: Percentage change in house prices								
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	-.10***	-.10***	-.10***	-.10***	-.10***	-.10***	-.10***	-.10***
Y13	.24***	.24***	.25***	.25***	.25***	.25***	.25***	.24***
Y14	.16***	.15***	.16***	.16***	.16***	.15***	.15***	.15***
Y15	.19***	.18***	.19***	.19***	.19***	.19***	.19***	.19***
Y16	.22***	.22***	.22***	.22***	.22***	.22***	.22***	.22***
Y17	.16***	.16***	.15***	.15***	.15***	.15***	.16***	.15***
Y18	.09***	.09***	.09***	.09***	.09***	.09***	.09***	.09***
*Y13*Treated*	.02**	.03**	-.00	.00	.00	.01	.00	.02
*Y14*Treated*	.00	.02	.01	.01	.01	.01	.01	.02
*Y15*Treated*	-.00	.01	-.00	-.00	-.00	.00	-.00	.00
*Y16*Treated*	.03***	.05***	.03***	.03**	.02***	.03**	.03***	.04***
*Y17*Treated*	-.01	.00	.00	.00	.00	.01	.00	.01
*Y18*Treated*	-.03***	-.01	-.03***	-.03**	-.03***	-.02	-.03***	-.02
Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Base Year	2011	2011	2012	2012	2013	2013	2014	2014

NOTE. – Significant values are presented by: *** at the 1% level, ** at the 5% level and * at the 10% level. Fixed effects refers to cross-section fixed effects i.e. municipality fixed effects. Base year refers to the year for which the groups where divided.

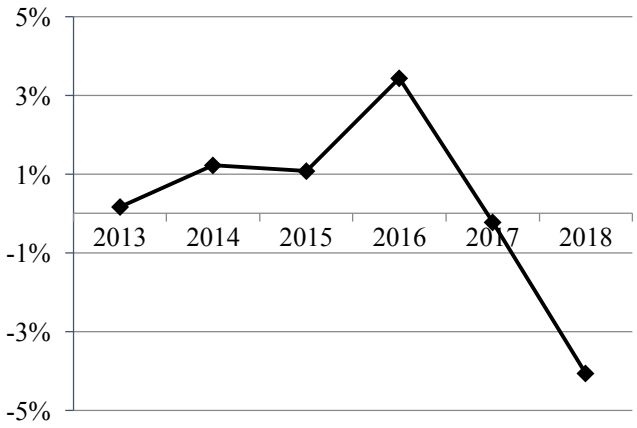
In the final two tests, the sample is first divided based on if a municipality had higher DTI ratios than the country average for a certain year. The results are presented in Table 4 in the Appendix and remain the same as in the second test. The results from the final test in which municipalities are divided based on if there is a larger share of households with DTI ratios above 400 percent compared to the country average for a certain year is presented in Table 5 in the Appendix. Again the results from these test are very similar to the previous test.

An important observation here is that the results show insignificant estimators for the treatment dummy variables β_7, β_8 and β_9 in all tests. This is in line with the hypothesis, i.e. before 2016 there are no significant difference between the groups. The hypothesis that these estimators are significantly different from zero is rejected by all the tests.

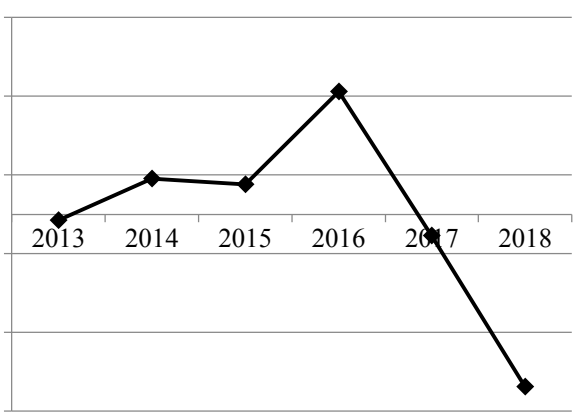
A visual view of the results is presented in the graphs below. The first part shows the difference-in-difference results from three tests where the sample has been divided based on if a municipality is (1) part of the three greater city regions, (2) had PTI ratios above average in year 2012 or (3) had high DTI ratios above average in year 2012. The graphs on the left hand side are including municipality fixed effects. The graphs show that there is a large positive difference in the change of house prices in 2016 and a larger negative difference in 2018. The graphs also show that before 2016 there was a fairly stable difference between the groups and that the difference is close to zero for all tests in 2017. This paper abstains from drawing to large conclusions on the magnitude of the differences, but according to the results the difference could be around three percent in 2016 and negative three to four percent in 2018. Graph 5 shows the change in house prices for the treatment- and control group for the same set of tests. Important to note here is the similar trends prior to the policy implementation. The results clearly show that there was no significant difference between the two groups prior to the policy. After the amortization requirement we do however get that the groups are significantly different from each other.

Graph 4 – difference-in-difference results

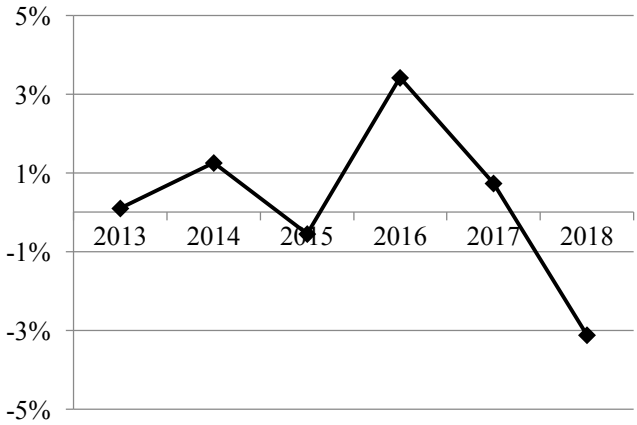
Greater city regions with cs-fixed effects



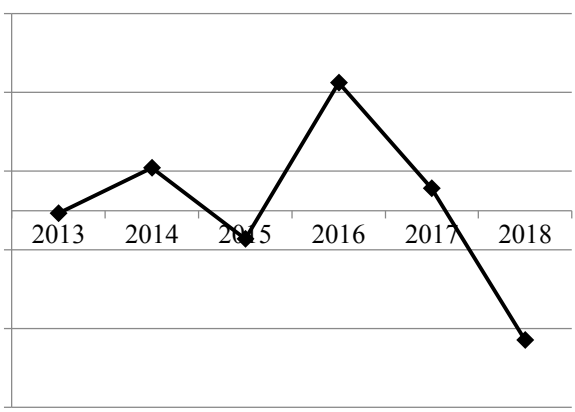
Greater city regions



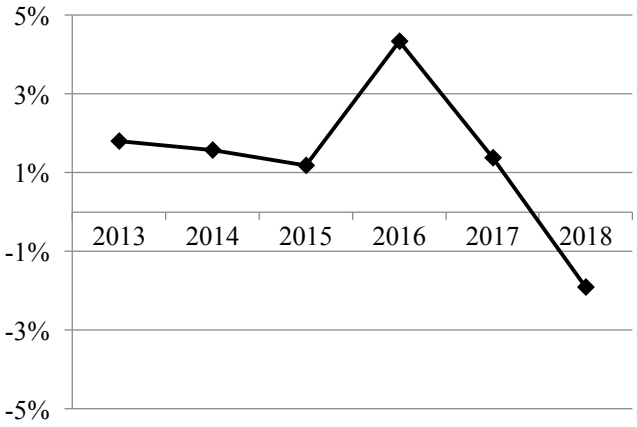
PTI in 2012 with cs-fixed effects



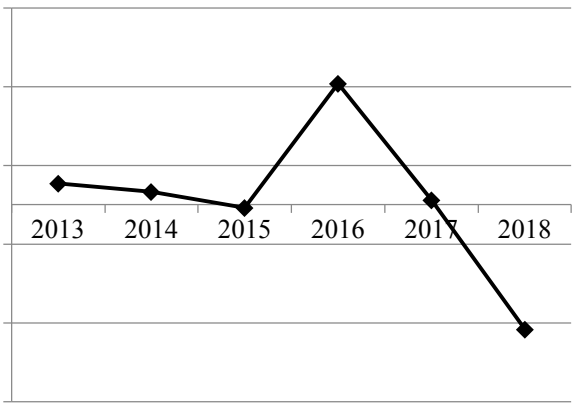
PTI in 2012



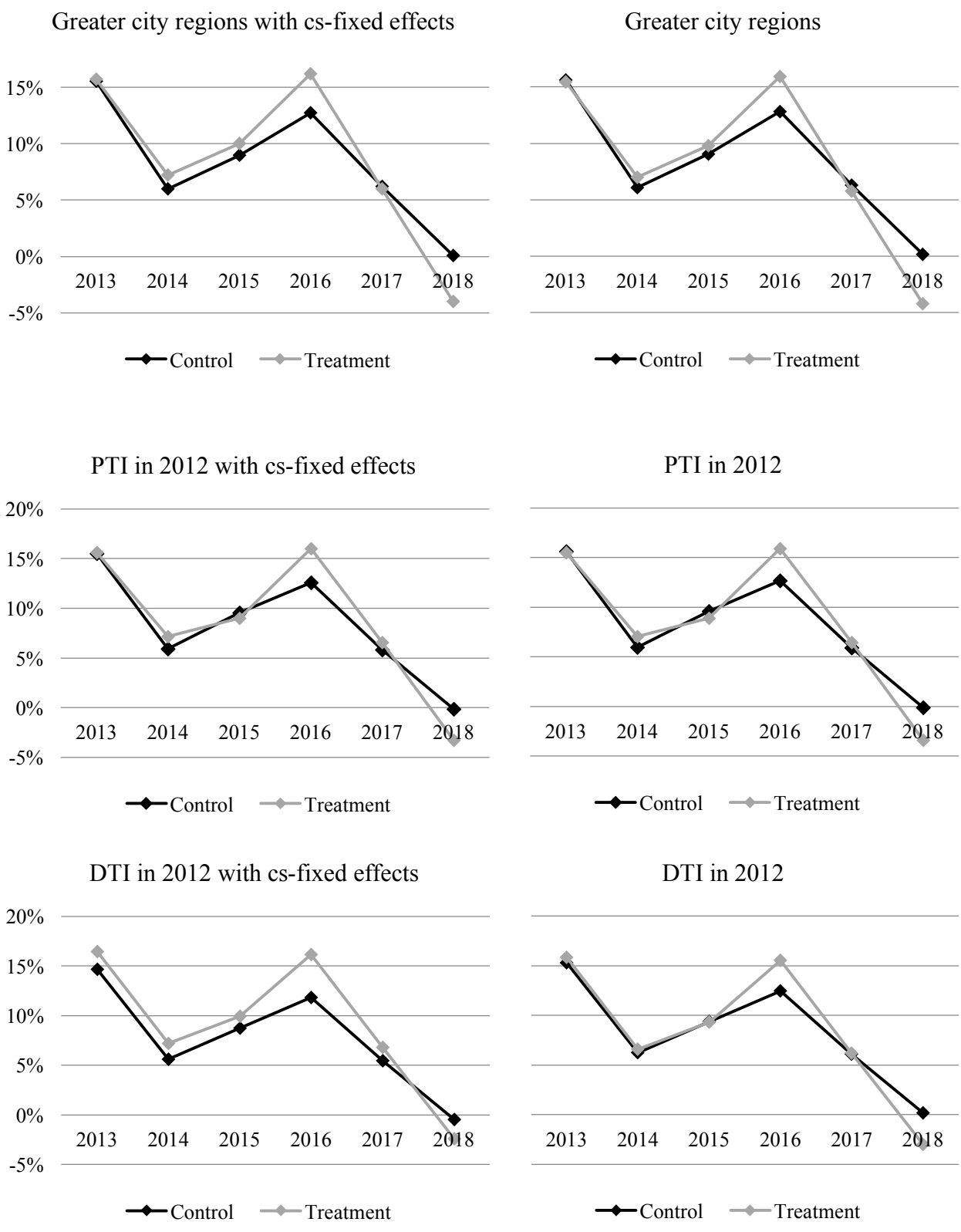
DTI in 2012 with cs-fixed effects



DTI in 2012



Graph 5 – difference-in-difference results, treatment- and control group



9. Discussion

The main results from the empirical analysis show that there is a significant difference in the yearly change in house prices following the implementation of the amortization requirement in 2016. We do however get two offsetting effects from the results. The municipalities who we have argued would be more impacted by the policy show a larger positive increase in the change in house prices in 2016 and a larger negative increase in 2018. According to theory and our hypothesis, house prices in the effected municipalities should decrease compared to municipalities in the control group or compared to a scenario in which the amortization requirement was not implemented. Therefore, the results that we get for 2018 are in line with what theory predicts, although the results are often not robust for when including municipality fixed effects.

The reason to why we get an opposite effect than predicted in 2016 could be explain by what occurred with house sales shortly prior to the implementation in June the same year. Several reports over house sales for that year show that there was a huge increase in sales during the Spring of 2016 which caused house prices to rise as demand increased. The main explanation behind this could have been that households who were thinking of buying a new home in this period rushed to find housing before June in order to avoid the new requirement. This result is also important evidence that show us what will happen with effected groups even prior to a policy's implementation. In a situation without any announcement effects the story might have been different, but in this situation it does not come as a surprise that households reacted by the announcement and rushed to by houses before the amortization requirement came into effect. After the implementation, it is possible that prices then began to decrease in the impacted municipalities, but the aggregate yearly effect still remains positive.

An important aspect is that the results for the treatment group in the years prior to the implementation, is not significantly different from zero. These result show that there was no difference between the two groups before the

policy and that they followed the same trend. This is again in line with what theory predicts and confirms the hypothesis.

It is also clear from the results that there was no significant difference between the groups in 2017. Even though some municipalities were more impacted compared to others, the trend in house prices seems to have been almost the same. The explanation for this could be that the market first experienced an announcement effect prior to 2016 which then made the market stagnate in 2017, and that there was a similar trend across the country.

As a lagged effect, it then took over a year before the market began to adjust and that is the outcome that this paper has been able to capture. The amortization requirement did therefore have a negative effect on house prices in 2018 but not prior to that.

One unclear and unresolved issue is whether or not the large negative difference in 2018 is due to the amortization requirement in 2016 or an announcement effect of the previously mentioned stricter amortization requirement that came into effect on March 2018. Since this paper argues that the treatment group are municipalities who before 2016 would be impacted by the first policy, it would also be fair to assume that the same regions are affected by the stricter requirement as well. Due to the limitations of this paper, any further analysis of this have not been made, but it remains and interesting aspect for further researchers to look at.

It also remains some what troubling that the effects from the results in 2018 are often not robust for when including municipality fixed effects. In many of the test presented in this paper, there was often issues for including variables such as regional growth, disposable income and change in population in the models. A more thorough analysis could perhaps be made by other researchers to properly handle this problem. If one is to question the validity of these results, then the conclusion should be that there was no significant difference in the change in house prices due to the policy in 2018.

10. Limitations

There are some limitations with the method and strategy that is conducted in this thesis and these are important to address. First of, as mentioned in the previous sections, this paper tries to analyze the effect of a policy that is only targeting new mortgage takers but the available data covers all mortgage takers. If no effect can be found, it might be because the amount of mortgage takers is too small and that their impact on the housing market become negligible.

This paper is also relying on aggregate data for each municipality, which will further decrease the impact of the policy since most mortgage takers are far away from being affected compared to households that are in a riskier situation. That is, it might be difficult to find out what municipalities that are more affected since households in a risker situation is covered up by a large majority of households with a sounder financial situation.

Finally, because this paper analyzes data with yearly changes in house prices it might be difficult to capture the true effect of the policy after June 2016 since the data from 2016 is accumulated to one year. It would be wise for other researchers to try the same method but using quarterly or monthly data on the change in house prices instead.

11. Conclusion

This paper has used a common method for policy evaluation, *difference-in-difference,* in order to determine if the Swedish amortization requirement on June 2016 had any impact on house prices. The results from the empirical analysis suggests that there was a significant difference in the change of house prices between affected municipalities compared to a control group. The results are however not completely in line with what theory would predict. Results show that in 2016 there was a significant positive difference between the groups. That is, municipalities who arguably would be more affected by the policy saw a larger positive increase in the change in house prices. This could perhaps be explained by an announcement effect prior to June 2016 as many households in the impacted municipalities rushed to purchase new homes to avoid being effected by the requirement. This surge in demand could have impacted house prices and led to prices in these municipalities to increase more.

The results also show that there was no difference between the groups prior to 2016 which is in line with the hypothesis and there is a similar trend for the treatment and control group. The same results are found for 2017 which could be due to the large positive increase for the treatment group in 2016 which left that part of the market in a stagnated stage.

Further results from the analysis does however show that theory holds. In 2018 there is a significant difference between the two groups and the results show that there was a larger negative change in house prices for the affected municipalities compared to the control group. These results are however not robust for including municipality fixed effects. But if considered valid, then ceteris paribus, the amortization requirement had an effect on house prices in Sweden and caused them to decrease.

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Appendix

Table 4 – Generalized difference-in-difference results based on debt-to-income ratios for four years (2011-2014)

Dependent variable: Percentage change in house prices								
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	-.10***	-.10***	-.10***	-.10***	-.10***	-.10***	-.10***	-.10***
Y13	.25***	.25***	.25***	.24***	.25***	.25***	.25***	.25***
Y14	.16***	.15***	.16***	.15***	.16***	.15***	.16***	.15***
Y15	.19***	.19***	.19***	.18***	.19***	.19***	.19***	.19***
Y16	.22***	.22***	.22***	.21***	.22***	.22***	.22***	.22***
Y17	.15***	.15***	.16***	.15***	.16***	.15***	.16***	.15***
Y18	.10***	.09***	.10***	.09***	.10***	.09***	.10***	.09***
*Y13*Treated*	-.00	.00	.00	.01	.00	.00	.00	.00
*Y14*Treated*	.00	.01	.00	.01	.00	.01	.00	.01
*Y15*Treated*	-.00	.00	-.00	.01	-.00	-.00	-.00	-.00
*Y16*Treated*	.03***	.03**	.03***	.04**	.03***	.03**	.03***	.03**
*Y17*Treated*	.00	.01	.00	.01	.00	.01	.00	.01
*Y18*Treated*	-.03***	-.02	-.03***	-.01	-.02***	-.02	-.02***	-.02
Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Base Year	2011	2011	2012	2012	2013	2013	2014	2014

NOTE. – Significant values are presented by: *** at the 1% level, ** at the 5% level and * at the 10% level. Fixed effects refers to cross-section fixed effects i.e. municipality fixed effects. Base year refers to the year for which the groups where divided.

Table 5 – Generalized difference-in-difference results based on large share of households with high debt-to-income ratios for four years (2011-2014)

Dependent variable: Percentage change in house prices								
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	-.10***	-.10***	-.10***	-.10***	-.10***	-.10***	-.10***	-.10***
Y13	.25***	.25***	.25***	.24***	.25***	.25***	.25***	.24***
Y14	.16***	.15***	.16***	.15***	.16***	.15***	.16***	.15***
Y15	.19***	.19***	.19***	.18***	.19***	.19***	.19***	.19***
Y16	.22***	.22***	.22***	.21***	.22***	.22***	.22***	.21***
Y17	.15***	.15***	.16***	.15***	.16***	.15***	.15***	.15***
Y18	.10***	.09***	.10***	.09***	.10***	.09***	.10***	.09***
*Y13*Treated*	-.00	.00	.00	.01	.00	.00	.00	.01
*Y14*Treated*	.00	.01	.00	.01	.00	.01	.00	.01
*Y15*Treated*	-.00	.00	-.00	.01	-.00	-.00	-.00	.00
*Y16*Treated*	.03***	.04**	.03***	.04***	.03***	.03**	.03***	.04***
*Y17*Treated*	.00	.01	.00	.01	.00	.01	.00	.01
*Y18*Treated*	-.03***	-.02	-.03***	-.01	-.02***	-.02	-.03***	-.01
Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Base Year	2011	2011	2012	2012	2013	2013	2014	2014

NOTE. – Significant values are presented by: *** at the 1% level, ** at the 5% level and * at the 10% level. Fixed effects refers to cross-section fixed effects i.e. municipality fixed effects. Base year refers to the year for which the groups where divided.