IFN Working Paper No. 1029, 2014

Property Taxation, Bounded Rationality and House Prices

Mikael Elinder and Lovisa Persson

Research Institute of Industrial Economics
P.O. Box 55665
SE-102 15 Stockholm, Sweden
info@ifn.se
www.ifn.se

Property taxation, bounded rationality and house prices*

Mikael Elinder†
Lovisa Persson‡

June 18, 2014

Abstract

In 2008, the Swedish property tax was reformed and a cap on yearly tax liabilities was introduced. A large fraction of owner occupied houses was subject to a substantial decrease in the tax. When the reform was announced, most analysts projected - in line with tax capitalization theory - that the tax decrease would lead to significant increases in house prices. We estimate price responses and capitalization degrees, using various DID strategies, in which the price dynamics of houses that were subject to a generous tax reduction are compared to the price dynamics of houses with a more modest reduction. Our results are largely inconsistent with capitalization theory. For the majority of properties, we find no evidence that the tax cut led to increases in house prices. However, we find evidence of partial capitalization in sub-markets with highly valued properties, highly educated citizens and were it is especially difficult to increase supply. We argue that theories of bounded rationality can help explain why house buyers may fail to take a tax decrease into account in the valuation of houses.

Keywords: announcement effects, capitalization, financial literacy, housing market, inattention, saliency

JEL codes: D01, D03, D04, D12, H22, H24, R21, R38

1 Introduction

In 2008, property taxes were substantially reduced for a large share of Swedish owner occupied residential properties (henceforth referred to as just houses or properties). In line with standard capitalization models, most analysts predicted that the tax cut would lead to increases in house prices. In this paper we estimate to what extent the reduction

*We would like to thank Aron Berg and Nina Öhrn Karlsson for excellent research assistance, Niclas Berggren, Per Engström, Henrik Jordahl, Che-Yuan Liang, Eva Mörk, Håkan Selin, Erik Spector, Oskar Tysklind and seminar participants at The Research Institute of Industrial Economics (IFN), The Ministry of Finance and Uppsala University for valuable comments, and the Swedish Research Council for financial support.

†Department of Economics and Uppsala Center for Fiscal Studies (UCFS), Uppsala University, and the Research Institute of Industrial Economics (IFN); Email: mikael.elinder@nek.uu.se

‡Department of Economics and Uppsala Center for Fiscal Studies (UCFS), Uppsala University; Email: lovisa.persson@nek.uu.se

in the Swedish property tax was capitalized into house prices. To identify causal effects of the tax cut, we compare the price dynamics of houses that received a small decrease in taxes due to the reform, with the price dynamics of houses that received a larger decrease. Since both groups had very similar price developments prior to the reform, we find our strategy reliable. Overall, our results suggest that for the vast majority of properties the reduction in the property tax had no effect on house prices, neither at the time of announcement, nor in the time period after implementation. We discuss whether the absence of general price responses is due to bounded rationality of house buyers. We find substantial capitalization only in sub-markets characterized by highly valued houses, large reductions in the property tax and highly educated citizens. These empirical results are in line with theories of bounded rationality. The property tax may be neglected in the valuation of houses as a way of simplifying a complex decision.

In capitalization models (see for instance Oates (1969) and Yinger (1982)), a prospective house buyer considers both house characteristics and factors affecting the cost of living when deciding what price to offer the seller. The market value of the current and future stream of these characteristics determine the market price. When a property tax is lowered, buyers take into account that the cost of living has decreased and they will therefore be willing to make higher price offers. If the supply of land and housing is fixed, the market price will increase with the net present value of the reduction in present and future tax payments (Oates (1969)).[1] If the housing market is efficient, and individuals use all relevant information in order to value a house, prices will change immediately when new information about future tax changes are made public (Ross and Yinger (1999)).

Capitalization models rely on strong assumptions about the degree of sophistication among house buyers as they calculate the net present value of all aspects of a property when determining the value. This includes aspects like location, size, attractiveness, maintenance costs, taxes and much more. However, for many individuals, buying a house is a new type of decision problem, of which they have limited experience. There are two potential sources of optimization errors in this situation. First, recent studies have documented that large groups of consumers lack the financial ability to make rational investment decisions (see Lusardi and Mitchell (2014) for a review of the financial literacy literature). It may be difficult for many individuals to manage to perform the task that capitalization theory demands from them, namely to assess the net present value of lower property taxes. Second, optimization errors may also be catalyzed by the characteristics of the decision making environment. When there are many aspects to take into account (competing stimuli), such as when buying a house, potentially important information could be neglected

[1] If the property tax is viewed as a tax on capital, full capitalization is expected when the property tax is uniform within a nation and capital cannot move freely between sectors, see Mieszkowski (1972) for the "capital tax" approach.

by the consumer in order to simplify the decision, see DellaVigna (2009). A boundedly rational consumer may neglect less salient or less important (such as small costs) aspects, and instead focus on the prioritized characteristics or the heaviest costs. The property tax could then be a relatively non-salient aspect for most houses buyers, especially if the property tax liability is low.[2] When taxes are not salient for the consumer, conclusions about tax incidence need to be modified, and Chetty (2009) therefore derives formulas that characterize the welfare implications in such a situation. In an experiment where commodity prices in a grocery store are varyingly displayed with or without taxes included, Chetty *et al.* (2009) show that non-saliency in commodity taxes increases demand by 8 percent. In this paper we argue that both limited financial literacy and the relative non-saliency of the property tax are likely to mitigate house price responses to the tax change.

The empirical literature on property tax capitalization has typically found full or partial capitalization in house prices (see reviews by Ross and Yinger (1999), Sirmans *et al.* (2008) and Hilber (2011)). Most studies have estimated property tax capitalization in a local government context with cross-sectional variation in property tax rates. Two important identification problems arise when studying the property tax in a local public sector context. First, the level of public goods is positively correlated with the property tax level, and both the tax level and the public goods level independently affect the house price level. If the tax differential that remain after controlling for public services is equal to the house price differential, there is said to be full capitalization of the property tax. There are however severe difficulties concerning how to measure the level of public services in a way as to avoid biased estimates. Second, when local governments set their own tax rate, areas with a high house price level, all else equal, are able to set a lower tax rate to collect a certain amount of tax revenues. This creates a simultaneity bias between the property tax rate and house prices. Both of these identification issues have been known and discussed since the seminal paper by Oates (1969).

The above identification issues are dealt with in Cushing (1984) by comparing blocks of housing on opposite sides of the boarder of two jurisdictions with different property tax rates. This reduces the endogeneity problem since the compared houses largely share the same type of services and neighbourhood amenities. The results indicate roughly full capitalization of the tax differentials, but is based on only 86 observations. Palmon and Smith (1998) use a similar approach and estimate house price effects from cross-sectional variation in tax rates within a district with similar local public services. Their estimates suggest full capitalization and they conclude that agents on the market rationally incorporate tax liabilities in their valuation of houses. While solving the most critical endogeneity problem, their analysis is also based on a limited data set with only about

[2]This is quite different from property taxes being a relatively salient tax to existing house owners, and in relation to other taxes, which is discussed by Cabral and Hoxby (2012).

500 observations, and the estimates may still contain some bias due to an inability to fully account for differences in unobserved factors, like neighborhood characteristics and amenities, that may be correlated with the tax rate.

Like Cushing (1984) and Palmon and Smith (1998), we solve the simultaneity and measurement problems by estimating capitalization in a context where the property tax rate is unrelated to local public services. The Swedish property tax is determined at the national level and do not vary between municipalities. However, unlike most previous studies, we rely on variation stemming from a national property tax reform affecting the tax payments of house owners differently depending on the assessed tax value of their house. Starting as an election promise in the campaign of the 2006 parliamentary election, the property tax rate was lowered in 2008 from 1 to 0.75 percent and a cap on property taxes was set at SEK 6,000.[3] As a consequence, a substantial fraction of house owners received a large reduction in yearly tax liabilities. We rely on, and show that our setting is suitable for, difference-in-differences (DID) estimation as the policy change affected houses with previously similar price developments differently. Our analyses are based on a rich data set covering roughly 100,000 sales of owner occupied single-family houses in Sweden during 2006, 2007 and 2008. The data set contains information about tax liability, as well as the exact date when the sales contract was signed, the final price, characteristics of the property, location and more.[4]

We make an important contribution to the previous literature on tax capitalization in that we estimate responses to both announcements and actual changes in the property tax. To our knowledge we are the first to estimate responses to an announcement of a property tax change. Due to our large and nationally encompassing data set we are also able to study heterogeneity in price responses in several interesting sub-markets and to relate our findings to the growing literature on the empirical relevance of bounded rationality. Although we cannot provide direct evidence of bounded rationality, we are able to study price responses in sub-markets, where individuals are likely to be more informed about changes in the tax schedule and where the education level is higher (and hence financial literacy). Since we find significant capitalization estimates in these markets only, we suggest that theories of bounded rationality may explain why we find negligible capitalization rates overall. In this sense our paper also contributes to the growing literature on how cognitive limitations and information costs influence responses to public policies in general, and to complex tax codes in particular (see e.g.Chetty *et al.* (2009), Kling *et al.*

[3]SEK 6 ≈ USD 1 and SEK 9 ≈ EUR 1.

[4]The studies most related to ours, using Swedish data, are Berger *et al.* (2000), finding that government subsidized interest rates on mortgages in the 80's and early 90's were fully capitalized into property prices, and Boije (1997) who finds that the local income tax rate is partially capitalized into property prices. Other related papers that analyze responses in the housing markets to transfer taxation are Best and Kleven (2013) and Kopczuk and Munroe (2014)).

(2012), Sahm *et al.* (2012) and Chetty and Saez (2013)).

2 The decision problem - what to pay for a house?

In this section, we present a framework for interpreting our empirical results. We start out by presenting standard capitalization theory and then incorporate insights from models of inattention and bounded rationality, see (DellaVigna (2009) and Chetty (2009)). The typical view in capitalization models is that property prices reflect the present value of future housing services net of costs (see e.g. Sirmans *et al.* (2008)). This means that prices are determined entirely by the demand for different properties, implicitly (or sometimes explicitly) assuming that supply is inelastic. A simple equation can thus illustrate how prices are formed:

$$P = \frac{S_1}{r} + \frac{S_2}{r} + ... + \frac{S_M}{r} - \frac{C_1}{r} - \frac{C_2}{r} - ... - \frac{C_N}{r}, \tag{1}$$

where P is the price of the house, S_i is the value of service i, r represents the relevant discount rate, M denotes the number of different services the property provides. $\frac{S_i}{r}$ and $\frac{C_j}{r}$ thus represent the net present value of service i and cost j respectively, assuming an infinite horizon. In principle, S can denote any aspect of the property that consumers value, like location, size, attractiveness, access to good schools etc. C denotes costs associated with the property and N the number of relevant costs. C can be e.g. capital costs, heating costs, and of course: tax liability, which we will focus on. In principle, this equation could be estimated using the following empirical model:

$$P = \theta_1 \frac{S_1}{r} + \theta_2 \frac{S_2}{r} + ... + \theta_M \frac{S_M}{r} - \beta_1 \frac{C_1}{r} - \beta_2 \frac{C_2}{r} - ... - \beta_N \frac{C_N}{r} \tag{2}$$

The standard view in capitalization models is that $\theta_i = 1 \quad \forall i$ and that $\beta_j = 1 \quad \forall j$, meaning that all aspects of a property are fully valued at their net present value. However, if we take into account the context and the process by which an individual makes a decision about how much to pay for a house, it is evident that optimization errors might occur. The prescribed view in Simon (1978) is that rationality should be view in a broader sense than the strict sense of maximization that was prevailing in the economic sciences. A weaker definition of rationality takes into account the decision environment and the cognitive limitations of the individual decision maker. This weaker form of rationality is typically referred to as "bounded rationality". As is also put forward by Chetty (2009) and DellaVigna (2009), it may be boundedly rational to neglect less salient aspects when an individual is in a complex decision making environment. Chetty (2009) presents a framework for how saliency can be incorporated into a model of commodity demand. Incorporating these insights into our capitalization framework would mean that for some

i, $\theta_i < 1$ and for some j, $\beta_j < 1$. If we rank the aspects and costs according to how salient consumers perceive them, with S_1 and C_1 being the most salient quality and cost aspects respectively we get the following predictions:

$$\theta_1 > \theta_2 > ... > \theta_M \text{ and } \beta_1 > \beta_2 > ... > \beta_N$$

We argue that in the Swedish context, the property tax liability was not among the more salient aspects (neither before nor after the tax reduction) when a prospective house buyer is considering how much to pay for a house. Instead consumers are likely to find the aspects that are typically highlighted in advertisements as more salient. The more salient quality aspects would then be location (possibly including access to good schools), living area and plot size. Less salient aspects would perhaps be the attractiveness of the garden in the winter (assuming that it is marketed during summer), behavior of neighbors and occasional disturbance from heavy traffic on a nearby road. Since costs always put downward pressure on the final market price, real estate agents have incentives to not emphasize the costs of living in the house as much as they would emphasize quality aspects. Additionally, we argue that the saliency of a particular type of cost depends on how high the cost is in relation to other costs of owning a house. For an average house and buyer, interest payments on mortgage loans perhaps constitute the largest cost of owning a house. In Sweden, heating and maintenance costs are also very high for many houses. Water, sewage, and insurance costs are typically smaller and hence, we argue, less salient costs. Property tax liabilities could before 2008 be larger than for instance heating costs for very expensive houses, but were for most houses lower, but still higher than, for instance, insurance costs. This makes us think of the property tax as neither the most nor the least salient cost of owning a house. If some consumers do not perfectly consider all these aspects when deciding how much they are willing to pay for a house then we should expect estimates of θ and β to be considerably lower on less salient aspects.

If the price is determined through an auction process, which is common in the market for residential housing in many countries, then it is crucial whether fully rational or boundedly rational consumers are more likely to put the highest bid. If inattentive consumers neglect a certain cost aspect, then they would be willing to pay a higher price for a house than fully rational consumers. In such a case, a reduction in that particular cost would increase the price fully rational consumers would be willing to pay for the house. However, inattentive consumers would still be willing to pay more (as long as the neglected cost is not reduced to zero), and hence win the auction and determine the price.

Taken together, we argue that it would be quite expected that property taxes would not be fully capitalized, at least in tax regimes were the annual tax liability does not put property taxes on top of the list of housing costs.

3 The Swedish property taxes and the reform

Between 1985 and 2008, the property tax was uniform across the country and provided revenues to the national government.[5] Both industrial and residential properties were subject to the tax, although the focus of our study is on owner occupied residential properties. In 2006, the tax revenues from this group of properties amounted to SEK 13.1 billion, or about 1 percent of total tax revenues. The guiding principle of property taxation in Sweden is that property owners are liable to pay a percentage on the tax value of the property. From 2001 until 2008 the tax rate was 1 percent of the tax value. The tax value is determined by the tax authority and is approximately 75 percent of the market value of the property (at the time of the assessment). Each property belongs to a"value area" according to characteristics of the common surroundings, and the average market price in each value area is one of the factors used in order to calculate the tax value for a single property. Properties also receive so called "standard points" from the tax authority which take into account individual property characteristics. Since 2001, tax values are reassessed every third year. Consequently, when market values rise, in between assessments, the tax value as a share of market price decrease. Increases in tax liabilities, due to reassessment of tax values, were phased in over a period of three years, such that owners only had to pay one third of the increase in tax liability the first year after a reassessment.[6]

When determining the appropriate property tax rate, an important principle is that it should be neutral in relation to the tax on capital (or rental) incomes. If home owners receive a rental income amounting to 3 percent of the market value of the residential property, a tax rate of 1 percent of the tax value, which is 75 percent of the market value, is equal to a 25 percent capital income tax. This tax rate can be compared with the 30 percent tax on capital incomes in Sweden. In addition, when a property is sold above its purchase price, the seller have to pay a capital gains tax amounting to 20 percent of the profit.

3.1 The 2008 reform

On January 1, 2008, the property tax on residential housing in Sweden was reconstructed. The tax rate was lowered from 1 percent to 0.75 percent of the tax value and a cap on the yearly tax liability was set at SEK 6,000.[7] The tax revenues were shifted from the national government to the municipal governments. However, grants from the national government to the municipalities were lowered to cancel out the revenue increase. The property tax reform was thus intended to be neutral in terms of the municipal budget.

[5]For a historical review of the property tax in Sweden see Stenkula (2014)

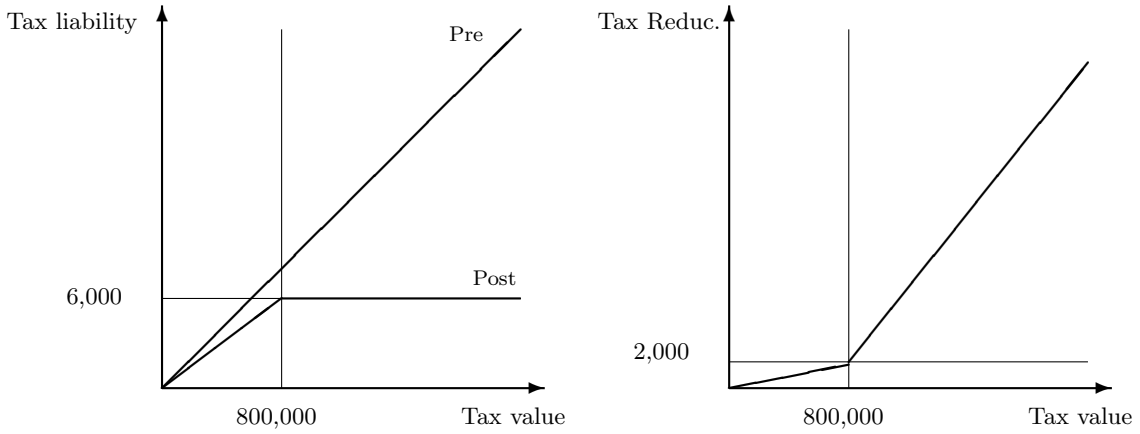
[6]A "limitation rule" was introduced in 2001, such that households with a yearly income of less than SEK 600,000 would not pay more than 5 percent of their income in property taxes.

[7]The limitation rule was kept but would only apply to senior citizens.

Unlike what is common in other countries, Swedish municipalities are not allowed to set their preferred local property tax rate. The tax rate was kept uniform across the country even after the 2008 reform. The average yearly reduction in tax liabilities from the 2008 reform amounted to SEK 4,900, and the average net present value of the tax reduction was SEK 245,000.[8] With the new property tax system, tax revenues from owner occupied residential properties decreased from SEK 13.1 billion in 2006 to SEK 10.5 billion in 2008. The tax decrease was partly financed with an increase in the capital gains tax from 20 percent to 22 percent, and partly by the introduction of an interest rate on delays on these tax payments.

[8]Details about how the net present value is calculated is presented in section 5.2

Figure 1: The final tax reform 2008 (values in SEK)



(a) Yearly tax liability pre- and post-reform

(b) Reduction in yearly tax liability

In figure 1(a) we show how yearly property tax payments depend on the tax value, both before and after the reform. The straight line with a gradient of 0.01 is the pre-reform tax schedule, and it shows how the tax used to be strictly proportional to the tax value. The post-reform tax schedule, however, has a gradient of 0.0075 and a kink at SEK 6,000 in yearly tax liabilities, which illustrates clearly that the reform came in two parts; the proportional tax decrease of 0.25 percentage points and the cap at SEK 6,000. Combining these two schedules, we get a kink in the yearly gain from the tax reform, which is shown in figure 1(b). The figures illustrates that the tax reduction was larger for highly valued houses. The relationship is piecewise linear in the tax value; the higher the tax value the higher the gain, although all house owners clearly benefited from the reform in some way.

3.2 From promise to implementation

Like in the U.S. and elsewhere, the property tax is unpopular and regularly discussed also in Sweden.[9] House owners lobby for a complete removal, economists support the tax for efficiency reasons, and politicians want the revenue it brings but also the support of the voters. Between 1994 and 2006, the Social Democrats ruled as a minority government in Sweden. During this time, one of the small opposition parties, the Christian Democrats[10] repeatedly propagated for a removal or large reduction of the property tax. The other parties in the opposition were also somewhat positive towards a reduction of the property tax, but it took until 2006 before the opposition jointly decided to give it priority.

In the summer of 2006, a few months before the election in September, the newly

[9]See Cabral and Hoxby (2012) for a discussion on the unpopularity of the U.S property tax.

[10]The Christian Democrats received roughly 9 and 6 percent of the votes in the 2002 and 2006 parliamentary elections.

formed coalition of center-right opposition parties – the Alliance for Sweden (Swedish: Allians för Sverige) - jointly announced a large reduction of the property tax as one of their most tangible election promises. On 4 July, The Alliance parties summoned a joint press conference at the Almedalen Week (a highly publicized annual conference where interest groups, media representatives and political parties discuss politics) to announce their agreement on the future of the property tax. The proposal was in two parts: in 2007, the tax values were to be fixed at the 2006 level and the yearly tax liability on the land part of the property should not exceed SEK 5,000. In 2008, the national property tax was to be replaced with a "low" property tax collected by the municipalities. The Alliance parties did not say how high the tax would be, but the Christian Democrats' proposal at the time was a cap at SEK 2,800 in yearly tax payments. No change in the tax rate was mentioned. It was also clearly communicated that the property tax should not increase for anyone, and that it should be partly financed within the housing sector.

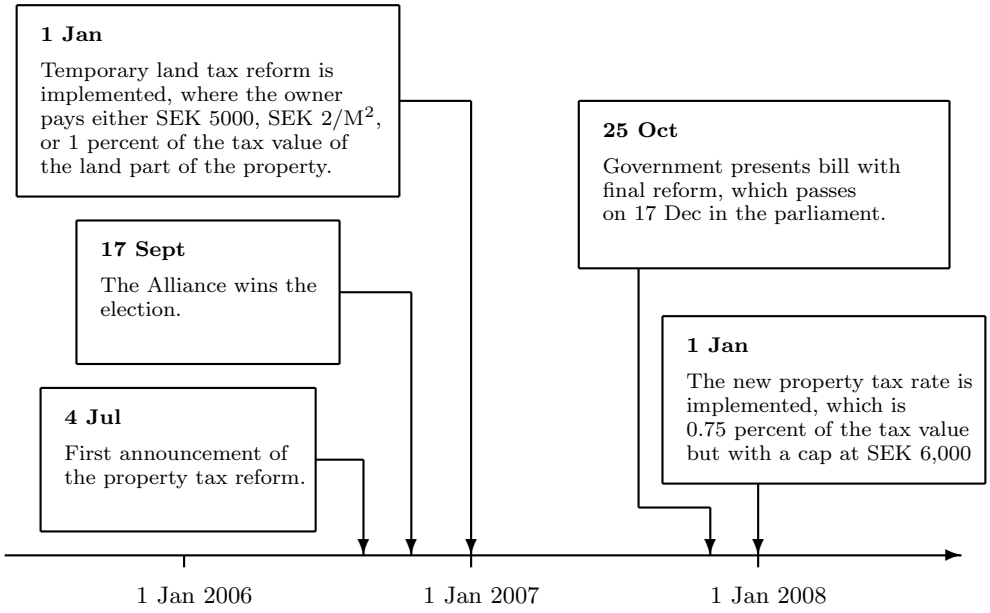
After the announcement, economists appeared in the media, defending the tax. Social Democratic party representatives deemed the proposal as irresponsible due to the lack of a financing plan of the tax cut, and also pointed out that the proposed tax reduction would predominantly benefit owners of highly valued properties. The Social Democratic response to the Alliance proposal was that the system should stay mainly as it was, although a reduction of the tax rate was on the table.

In the election manifesto, released on 23 August, 2006, the promise to abolish the property tax in its current form was repeated. In the manifesto, The Alliance referred to the tax as being "unjust" and "unpredictable", and they expressed an ambition to abolish the tax by 2008. Until then, they wished to implement the cap on the land part of the tax at SEK 5,000 and keep tax values at the 2006 level, as earlier announced at the press conference in July. On 17 September, 2006, the Alliance won the election and formed a majority government. In connection with the inauguration, the new government repeated the long-term promise to reform the property tax, and also the short-term promise to reduce the tax liability through the implementation of the temporary land tax reform.

The new government implemented the promised temporary land tax reform on 1 January 2007.[11] The land tax reform was estimated to cost SEK 2.9 billion in lost revenues. During 2007 there was a public discussion about where the final cap should be set. On 20 September the final proposal of a cap at SEK 6,000 and a tax decrease to 0.75 percent was finally publicly announced. On 25 October the proposition was put forward, which was later on accepted by the Parliament on 17 December. Figure 2 shows the timing of the reform. Since 2008, the cap has been raised according to increases in general income.

[11]The land tax reform affected taxes retrospectively for 2006 as well, but it was not part of the promise and could not easily have been anticipated by the agents in the housing market. Thus it should not affect our capitalization estimates.

Figure 2: Timing of the property tax reform



3.3 What could prospective house buyers and sellers expect?

On 4 July 2006, when the Alliance jointly announced that they would substantially reduce the property tax, it was a clear break from what voters could have expected earlier about changes in the property tax. Before that date, a major reduction in the property tax was only given priority by the Christian Democrats – a small opposition party. However, it was expected that the election would be a close race and even if the Alliance would win, it would still not be certain that the promise would be delivered on. In a highly efficient market with rational agents, this kind of news would still immediately increase asset prices, at least to some degree. After the Alliance won the election and re-announced that they would implement their promise to reduce the property tax, the probability of a reduction again increased. Finally, from 1 January 2008, when the new tax schedule was implemented, the only uncertainty that remained was whether the Social Democrats would revert the property tax, were they to win the election in 2010. However, in the summer of 2009 it became evident that the Social Democrats would only like to increase the property tax for houses with very high tax values.[12] House owners and prospective buyers, therefore, had good reason to believe that the property tax reduction would not be reverted for a long time. Still to date, only the Left Party, which is in the opposition

[12]In a leading Swedish newspaper the Social Democrats wrote that they would like to implement an additional property tax amounting to 1 percent on house values above SEK 4.5 million. *Dagens Nyheter*, 30 June 2009.

and has less than 6 percent of the votes, has proposed to make substantial increases in the property tax.

4 Data

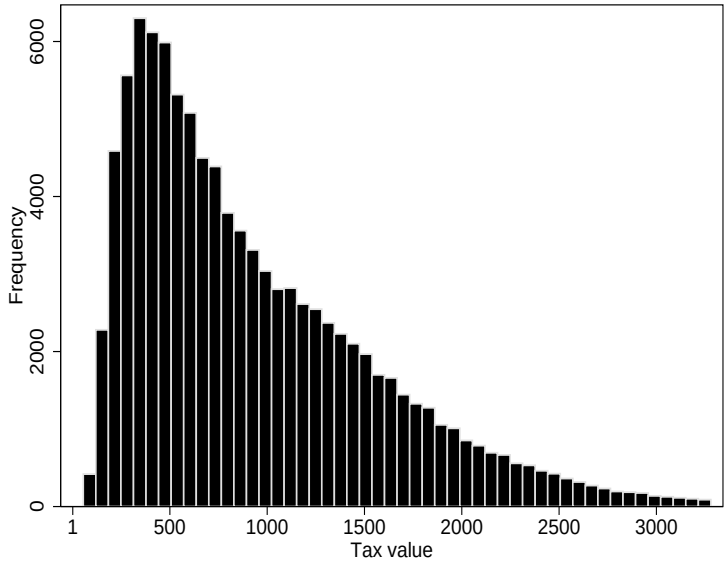
We have at our disposal a combined data set originating from the Swedish land surveying office and Svensk Mäklarstatistik AB (Our translation: Swedish Real Estate Agent Statistics, Inc.). The data set contains 124,563 observations of instances where a house – intended for permanent or summer living by the owner – has switched owners in the land register during the time period 2006 to 2008, for all of Sweden. We have the following information about the properties included in our data set: date of contract signing, market price, tax value, zip code, year of construction, and other house and property characteristics such as living and plot area. The data set covers 41 percent of the total population of owner changes in the Swedish housing market. The reason why we do not cover the whole population is that we lack information on sales of properties mediated without a real estate agent and agents that are not connected to Svensk Mäklarstatistik AB (mainly smaller real estate agents), and thus we cannot include these sales in our sample.

In the empirical analysis we will mainly work with a sample of 101,449 observations. We leave out 23,114 observations due to the following deliberate sample restrictions. First of all, we leave out a few (131 observations) owner changes that are due to inheritance, distribution of marital properties, premarital settlements, gifts, purchases between family members and the like. Second, we only keep observations that refer to one singular tax value unit, and which contains exactly one house and exactly one land unit (2,476 observations). Third, only properties that had their latest tax value assessed in the mandatory tax value assessment in 2006 are included in the sample since tax liability values as a rule depend on market prices in the value area and tax value assessments after 2006 will therefore be endogenous to the tax reform (1,005 observations). Assessments in 2007 and 2008 were only conducted for rebuilt houses. Fourth, we also drop observations where: the purchase concerns leases instead of actual ownership (5,727 observations), and cases where the tax value is not completely related to the property that has been sold (1,551 observations). A few observations lack information on value area, value year, tax value, living area and plot area, and these observations are therefore dropped (221 observations). The most extensive sample restriction (12,003 observations) is due to the exclusion of the second half of 2008, when very few houses were sold especially at the very end of the year, due to the extraordinary event of the financial crisis.

4.1 Descriptives

The distribution of tax values in our data set is shown in figure 3. The distribution is positively skewed, with a long tail of relatively highly valued properties. A majority of properties have a tax value below SEK 800,000 (52 percent), and were affected by the reform only to the extent that the tax rate was decreased from 1 percent to 0.75 percent of the tax value. Owners of properties with a tax value above SEK 800,000 received a more favorable treatment from the tax reform because the marginal tax rate above SEK 800,000 was set to zero.

Figure 3: Distribution of tax values



Tax values are in SEK 1,000

In table 1 we show some descriptives of the main variables used in the data analysis. The average house in the data set costs SEK 1,778,000, and has a tax value of SEK 959,000. The average net present value gain from the final 2008 reform is SEK 245,000. In terms of yearly tax payments, the average gain is SEK 4,900. The average house has 115 m^2 of living area, and has a plot size of 1500 m^2. The so called "standard points" is a point system used by the tax authority in order to estimate the tax value taking into account several additional house quality characteristics such as: tiled stoves, kitchen appliances, and the number of bathrooms. The standard points of the properties in our sample range from 6 to 54 with a mean of 29.

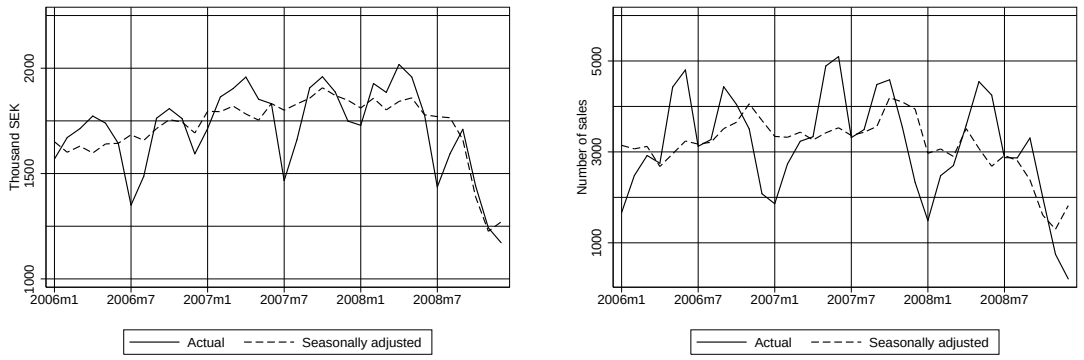
Figure 4a shows the evolution of markets prices and figure 4b the number of sales over the time period that we study. They show both the actual time series and seasonally adjusted series. The fluctuations in the sales prices indicate a monthly seasonal pattern. House prices fall during midsummer and winter, while they increase during spring and

Table 1: Descriptive statistics

	Mean	Std.dev.	Min	Max
Market price (SEK)	1,778,000	1,330,000	10,000	24,000,000
Tax value (SEK)	959,000	692,000	55,000	11,576,000
Net present value gain (SEK)	245,000	295,000	7,000	5,488,000
Yearly gain (SEK)	4,900	5,900	138	110,000
Living area m^2	115	41	12	1,680
Total area m^2	1,500	2,500	25	162,000
Standard points	29	6	6	54

Monetary values are in nominal prices. All values are rounded. 101,449 observations.

Figure 4: Time series



(a) Market price

(b) Number of sales

autumn. This seasonal pattern is due to the type of houses sold, and the number of sales in each month. In both cases it is clear that market prices show an upward trend, which is broken by the end of 2007. There is also a clear seasonal pattern in figure 4b which shows the number of sales each month. The sales quantity is pretty stable over time until the financial crisis in 2008 when the number of sales decreases sharply.

5 Empirical strategy

As was shown in figure 1, yearly tax liabilities are a deterministic function of the tax value, both before and after the reform. The tax value is of course correlated with the market price through the characteristics of the house. We also know that the reduction in tax liability is larger for more expensive houses. A simple cross-sectional regression of market price on tax reduction would therefore yield a positive coefficient as a consequence of the construction of the tax reform itself. In other words, (tax reduction) treatment is correlated with house characteristics and is therefore endogenous. Since we have data on

house sales both before and after the reform, we can deal with this endogeneity issue in a Differences-in-Differences (DID) framework.

We use three different DID approaches, where the first one is a conventional two-group DID estimation where we compare the evolution of market prices of properties for which the cap is binding, with properties that were only subject to a decrease in the tax rate. In other words, the price development of properties with tax values below SEK 800,000 (control group) serves as counterfactual for the price development of properties with tax values above SEK 800,000. In the second approach we divide the treatment group into three parts according to tax value. By doing this we allow for heterogeneity in treatment effects in line with predictions from capitalization theory and from how the reform is designed.. We can also observe whether properties with higher tax values are closer to their theoretical effects than properties with lower values, which is what we would expect if saliency of the property tax increases in the magnitude of the tax liability. In the final approach, we utilize the full extent of variation that we get from the reform and perform a DID with continuous treatment. With the latter approach we can estimate the relationship between the tax reduction and the price response, taking into account that each property received a unique treatment.[13]

5.1 Specification of empirical models

In the two-group DID case we estimate variations of the following empirical model

$$y_{igjt} = P_t + T_g + D_{gt} + \Omega \mathbf{X}_{igjt} + V_j + \varepsilon_{igjt}, \tag{3}$$

where y_{igjt} is the natural logarithm of the market price for house i, in group g ($g = 1$ for the treated group and $g = 0$ for the control group), region j, and in period t. We include period specific dummies in P_t and group specific dummies in T_g (T_g equals 1 if g=1 and 0 otherwise). The parameter(s) of interest are D_{gt}, which are interaction variables between the group dummy (T_g) and period dummies (P_t), $D_{gt} = (T_g \times P_t)$. The interaction variables D_{gt} equal 1 if the house is in the treated group in period t, and is 0 in all other cases. D_{1k} represents the effect of the tax reform in period $t = k$. $\mathbf{X}$ represents a vector of control variables. We also estimate the model with fixed effects V_j at the county or municipality level.

The important assumption underlying all DID procedures is the parallel trend assumption. Given that we have chosen to specify our model in logarithms, we assume that properties in both the control and treatment group would evolve in the same way in

[13]Given the kink in the treatment at SEK 800,000 in tax value, it would be natural to use a regression kink design to identify the causal effect of the tax reduction. We have tried such a strategy but obtained estimates that were to imprecise to be informative. This is not surprising given that the difference in the tax reduction, and hence the expected difference in price response, is quite small around the kink.

percentages in the case with no property tax reform. If properties on all price levels are capital investments yielding the same rate of return, this is more plausible than assuming parallel trends in the market price level. The parallel trend assumption is violated if there are general equilibrium effects on the property market as a result of the property tax decrease. For example, there is the possibility of a negative cross-price-elasticity effect, such that prices of properties in the control group decrease as a result of a positive demand shift for properties in the treatment group. The parallel trend assumption is not directly testable. However, we show, in Figure 6 in section 5.3, that there is no drop in prices of the control group that can be attributed to equilibrium effects and that prices of houses above and below the cap evolve similarly before the first announcement in Almedalen.

To somewhat relax the parallel trend assumption and to increase the precision of the estimated treatment effect we include control variables for the size of the house (*living area*), the size of the land property (*plot area*), and a measure of the quality or standard of the house (*standard points*) in $\mathbf{X}_{itgj}$. When including control variables, we rely on the slightly weaker assumption that the two groups would have evolved equally conditional on the control variables. Including fixed effects in the models further allows us to deal with unobserved heterogeneity, and should thus decrease potential bias in the estimated price responses. However, once we include fixed effects, we utilize variation only within specific areas to identify price responses. This comes with the cost that several fixed effects unit will not contribute with identifying variation because they contain properties from the treatment only or the control group only. In practice this means that the theoretically expected estimates under full capitalization will be lower. This is because the variation in tax values, in areas with properties from both groups, on average is smaller than the variation in the country as a whole. We acknowledge this when we calculate capitalization degrees and interpret our findings.

In the two-group DID case we assign properties below the cap (0-800' in tax values) to the control group, and properties above the cap (>800') to the treatment group. In the four-group DID case we split the treatment group into three groups according to the following tax values, 800'-1600', 1600'-2400' and >2400'. The empirical model looks the same as in equation (3), although we now have three dummy variables in $\mathbf{T_g}$.

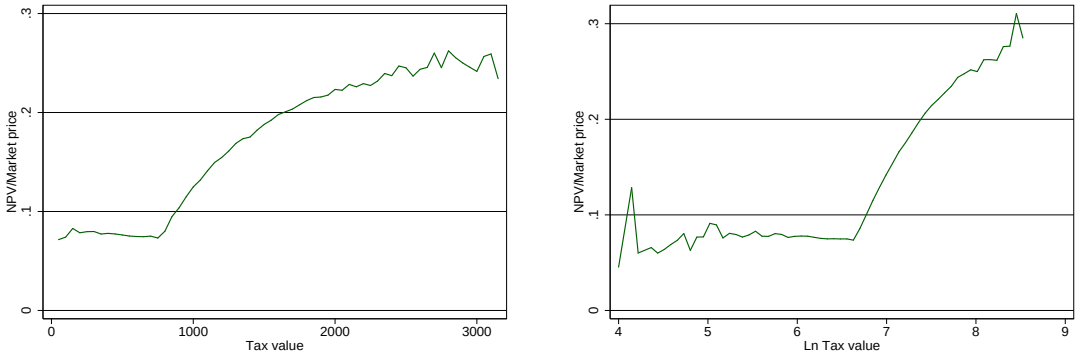
In the case with continuous treatment we estimate the following empirical model

$$y_{igjt} = P_t + T_g + \tau_{ijt} + (P_t \times T_g) + (P_t \times \tau_{ijt}) + (T_g \times \tau_{ijt}) + V_j + D_{gt} + \Omega \mathbf{X}_{igjt} + \varepsilon_{igjt}, \quad (4)$$

Where $D_{gt} = (P_t \times \tau_{itj} \times T_g)$, and τ_{ijt} is the natural logarithm of the tax value. In other words, the model is similar to a DDD (triple differences framework). We again rely on a logarithmic specification, in both market price and tax value. The expected price response under full capitalization is concave in the tax value for houses with tax values

above 800' , i.e. houses with higher tax values should get a higher percentage increase in house prices from the reform, but this effect is diminishing. The concave relationship is illustrated in figure 5a, where it is also clear that the properties below the cap all got the same proportional treatment.

Figure 5: Continuous treatment



(a) Tax value

(b) Logarithm of tax value

Figure 5a illustrates that the relationship between market price and the theoretically expected price response (see next subsection) under full capitalization. As can be seen in Figure 5b, the logarithm of the tax value and the price response in the natural logarithm of the price is approximately piecewise linear. Using a log-log specification we can then expect the effect of the property tax to be linear in the tax value for houses that are above the cap. For houses below the cap there is no variation in proportional treatment and hence expected proportional price responses. The houses below the kink are not used to identify the effect of the property tax when using continuous treatment, but can be included to increase the precision of the control variables. The parallel trend assumption under the case with continuous treatment is that the relationship between log market price and log tax value would be the same for all tax values without treatment. In order words, property prices evolve in the same way – in percent – for all levels of the log tax value.

We split our sample period of two and a half years into five time periods P_t, each of six months:

- Control period (Jan 2006 - Jun 2006): No annoncements had been made
- First announcement period (Jul 2006 - Dec 2006): Announcement at Almedalen and election win by the Alliance.
- Land tax reform period (Jan 2007 - Jun 2007): The land tax reform is effective
- Final announcement period (July 2007 - Dec 2007): The final reform is announced and passed by the Parliament

- Post reform period (Jan 2008 - Jun 2008): The final reform has been implemented

The above split allows us to study price responses in all important stages of the reform, from the first election announcement in Almedalen in 2006, to the final implementation of the reform in 2008. If house buyers anticipated the final reform already before the implementation, we would expect to see a jump in the house price series of highly valued houses already before the implementation of the final reform. Such responses could occur after the first announcement in Almedalen, after the Alliance won the election, or when the final reform was announced in the second half of 2007. If the final implementation came as a surprise we should see responses only in the last period.

5.2 Expected price responses under full capitalization

If the tax decrease is fully capitalized – in what order of magnitude do we expect the estimates to be? In this section we calculate theoretical estimates that are expected when the full value of the tax decrease is capitalized. We begin by calculating the net present value (NPV) from the 2008 reform for each property (as detailed below) based on its tax value. We add the NPV to the actual market price in order to get a market price under full capitalization. We then perform a simulated DID estimation using both the log market price under full capitalization as outcome variable and the actual market price. In the case with two-group DID, we first take the difference between log market price with treatment and the actual market price within each group separately. Lastly we take the second difference, the difference-in-differences, between the two groups in order to arrive at an estimate which theoretically would emerge under full capitalization. This procedure perfectly replicates the empirical specification in the previous section, which is important in order to get a credible comparison between our empirical and theoretical estimates. Note that when performing these calculations, we restrict the sample to the pre-treatment period of the first six months of 2006, as the market prices of these properties are not affected by the reform. Also, in the empirical analysis the difference in the first half of 2006 constitutes our baseline comparison.

Table 2: Theoretical effects under full capitalization

	Reform effect	Theoretical DID-estimate
≤800'	0.074	control group
>800'	0.155	0.081
800'-1600'	0.130	0.056
1600'-2400'	0.194	0.120
>2400	0.224	0.150

The theoretical DID-estimates under full capitalization are presented in table 2. The first column displays the reform effect for each group separately, expressed in log differ-

ences. Under full capitalization, the proportional tax decrease for the control group, the ≤800' group, implies an increase in prices of approximately 7.4 percent. For properties above the cap, the >800' group, the reform implies a 15.5 percent increase in prices. Dividing the treated group into three new groups, we can clearly see that the reform effect is increasing in the tax value. The second column displays the second difference, in other words, the theoretical DID estimates for the various groups. In the two-group DID-estimation we expect to see an estimate of 0.081 under full capitalization. The treated group is expected to increase by 8.1 percentage points more than the control group due to the reform. In the four-group DID we expect to see the estimates 0.056, 0.120, and 0.150 respectively.

In the case with continuous treatment we first estimate the pre-reform relationship between the log tax value and the log market price. We then add the NPV gain of the tax reduction to each property and re-estimate the relationship. The difference in the slope coefficients between the two models reveal the expected continuous DID estimates under full capitalization. We stated earlier that there was no variation in proportional treatment among the properties below the cap. This statement is confirmed in table 3 where we show that adding the NPV of the reform does not change the relationship between log market price and log tax value in this group of properties. The reason is of course that the treatment from the reform for these properties was a proportional tax decrease. For the properties above the cap we expect to see a change in the relationship between the log tax value and log market price according to table 3. For the group of properties with tax values above 800' the coefficient on log tax value from a regression on log market prince will increase from 0.94 to 1.06, which implies an expected theoretical continuous DID-estimate at 0.12.

Table 3: Theoretical continuous DID-estimates under full capitalization

	Before treatment	After treatment	Theoretical DID-estimate
≤800'	1.00	1.00	0.00
>800'	0.94	1.06	0.12

Note: Column 1 and 2 show the relationship between log tax value and log market price.

The theoretical estimates depend on assumptions about the agents' expected development of the property tax, time horizons and discount rates. A short reflection on each of these assumptions is needed. First, we note that the net present value of a stream of incomes can be written as:

$$NPV = \sum_{i=0}^{T} \frac{I_i}{(1+r+\pi)^i}, \tag{5}$$

were I_i denote the income in period i, or in our case the annual tax reduction, r the

real interest rate and π the inflation rate. The tricky part is to calculate I_i. According to the details in the property tax bill effective from 1 Jan 2008, the cap which was initially set at 800' SEK would be adjusted on a yearly basis according to both inflation and the real growth in wage incomes (g). Assuming that tax values (and hence tax payments under the old property tax regime) also increase at the rate $\pi+g$, we get $I_{t+1} = I_t \times (1 + g + \pi)$. With the simplifying assumption that the real interest rate equals the growth in real wages we get that:

$$NPV = I_0 \times T \tag{6}$$

The value of the tax reduction thus depends critically on the time horizon T. With no future changes in the tax policy, the time horizon should be very long. In standard net present value calculations, the flow of incomes is certain. However in our case, the property tax is likely to change again at some point in time. Both further reductions and increases are possible. This means that a rational agent may discount the gain from the reform with a risk premium. We assume a risk premium of 2 percent and an infinite discount horizon. This is equivalent to a case with no uncertainty and that the property tax is reverted after 50 years.

Our NPV calculations assume that the agents understand the details about how the tax payments would evolve, both under the old and new policy, and that they adjust for the political uncertainty. If we instead assume that agents simplify the problem and expect the first year's reduction to be constant in real terms, we would obtain exactly the same NPV, with a real interest rate of 2 percent.

The most critical assumption underlying the theoretical DID estimates is the choice of time horizon (or political uncertainty). But the choice of real interest rate is also important. The theoretical estimates would be smaller with a shorter time horizon and with a higher real interest rate.

5.3 Validation of identifying assumptions

As we have shown in figure 1, the treatment from the reform increases in the tax value. In the following empirical analysis we will compare the price developments of properties that were subject to both the tax rate reduction and the cap with properties that were subject to only the tax rate reduction. Since the control group also gained from the property tax reform, the counterfactual will be the case of a proportional decrease in the property tax. Descriptives for the two groups are shown in in table 4. Naturally, the average market price and the average tax value are higher in the treated group. A property in the control group has an average tax value of about SEK 462,000 and was on average sold for SEK 903,000, whereas an average property in the treated group has a tax value of SEK 1,497,000 and

was sold for SEK 2,726,000. The houses in the treated group are 31 m^2 larger than in the control group, and they are also of higher standard, as reflected in having more standard points. The average plot size in the control group is 1800 m^2, but only about 1100 m^2 in the control group. This difference follows from the fact that more properties in the control group are located in the countryside where plot sizes are larger than in urban areas. The group sizes are well balanced.

Table 4: House characteristics by treatment status

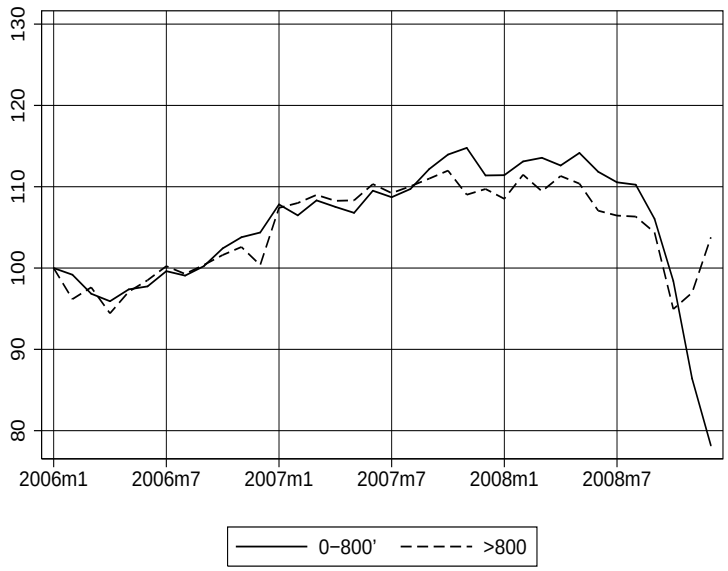
	Control ≤800	Treatment >800
	Mean	Mean
Market price (SEK)	903,000	2,726,000
Tax value (SEK)	462,000	1,497,000
Yearly gain (SEK)	1,200	9,000
Net Present Value (SEK)	58,000	449,000
Living area m^2	100	131
Total area m^2	1800	1100
Standard points	27	31
Observations	*52,750*	*48,699*

SEK in nominal prices. All values are rounded.

A DID approach allows for differences in the characteristics of the treated and untreated group, and therefore also in the levels of the outcome variable. Instead, a DID strategy relies on the assumption of parallel trends, i.e. that the market price in the treated group would evolve according to the control group in the case of no treatment. In figure 6 we show (monthly) seasonally adjusted market price indices by group status. The solid line represents the control group and the dashed line represents the treated group. The two groups evolve according to a seemingly similar trend until mid 2007, when the market price in the control group starts increasing slightly faster than in the treated group. A potential problem for our identification strategy is if there is general equilibrium effects, such that demand for properties in our control group is also affected by the reform. In this case we would see a divergence in prices between the two groups, such that prices in the control group decrease due to the reform. This effect is however not visible in the figure.

Already with this simple descriptive illustration we can see patterns that are not expected under the prediction from capitalization theory. Prices of expensive houses do not seem to increase more than lower valued houses, neither before nor after the reform. Under full capitalization, and no pre-reform responses, we would expect to see the prices of the treatment group to increase with 8 points relative to the control group by 1 January 2008. This is clearly not visible in figure 6.

Figure 6: Seasonally adjusted indices by group



Seasonal adjustment: log market prices were regressed on a full set of calendar monthly dummies, for each group separately. The mean of the series was added to the residuals from this regression.

6 Results

We now turn to the empirical estimates of the price responses of the property tax reduction. We start by presenting our baseline results from the two-group DID model. These results show the average treatment effects of the average treatment difference between the control and treatment group. Moreover, we also show the dynamics of price responses - i.e. pre-reform (or announcement effects), as well as post-reform responses. We then continue by studying responses using three stratified treatment groups according to tax value. We thus allow for treatment heterogeneity in line with the heterogeneity in expected theoretical effects, see table 2. Thereafter, we present continuous DID estimates, which allows us to exploit the full extent of variation in treatment. Moreover, we discuss the robustness of our findings with respect to different assumptions about expectations, and the possible influence of other policy changes occurring during our sample period. Finally we present estimates of price responses in several interesting sub-markets.

6.1 Two-group DID

In table 5 we show the results from our baseline DID estimations using two groups and five time periods. The control group consists of properties below the cap, and the treatment group consists of properties above the cap. The columns display the results from running different variations of equation (3). The group dummy and time period interaction

variables D_{gt} are named after the most important reform event happening within the six month time period. Model 1 is the simplest version of equation (3), where there are no controls or fixed effects, in model 2 we add property characteristics as controls, and in model 3 and 4 we add county and municipality fixed effects respectively.

It is certainly not trivial to know what expectations of the property tax reform buyers might have had in the "First announcement" period. Although, keeping in mind that this period covers both a generally stated election promise and a subsequent win by the Alliance for Sweden, it would not be a stretch to say that we expect to see at least some announcement or anticipation effect at this point. However, the results show that there are no substantial differences in the price changes between the two groups in this period. The statistically insignificant estimates in the four models range between -0.5 and $+0.7$ p.p difference in price development. These results should be compared with the theoretically expected estimates which are 7.9 p.p for model 1 and 3.9 p.p for model 4 under full capitalization of the 2008 tax reform and perfect foresight.

In the following period, going into 2007, the land tax reform is in place and the details of the final reform is being discussed. Neither in this time period do we see any clear indications of capitalization of the property tax, although estimates are somewhat larger than in the previous period. The highest estimate of 1.5 p.p is found in model 3, an effect size which should be compared with the theoretical estimate of 5.6 p.p. Hence, none of the estimates for the "Land tax reform" period are statistically significant or economically substantial.

The third six-month period is when the theoretical assumptions about the final reform completely converges with the expectations of the voters. When the reform is announced in it's final form, we expect to see clear capitalization effects since there is no longer any uncertainty regarding the details of the reform. What we see in the results however, are even smaller estimates than in the previous period. The additional certainty about the coming reform has accordingly not caused the price dynamics between the two groups to diverge, rather the opposite, they converge to the same proportionality as in the beginning of 2006.

Estimates in the last "Post reform" period, when the reform is a law in effect, are negative and hence in the opposite direction of what we expect from our theoretical assumptions. The estimates range from -2.5 p.p to -1.4 p.p, and they should be compared with the theoretical estimates of 3.9 p.p to 7.9 for each model respectively.

Taken together, the results from our baseline DID-estimation using two groups provide no evidence of capitalization of the property tax reduction. From a theoretical perspective we expect all estimates to be positive, but to grow over time as the uncertainty about the finally implemented tax change decreases. Instead, we find very small or no effects during

the three periods leading up to the "Post reform" period where we, opposite to what we expected, find negative effects but substantially small effects.

Table 5: Two-group DID

	(1)	(2)	(3)	(4)
First announcement	0.00631	-0.00494	0.00732	0.00631
	(0.012)	(0.011)	(0.010)	(0.008)
Land tax reform	0.0127	0.00471	0.0147	0.0123
	(0.012)	(0.011)	(0.011)	(0.009)
Final announcement	-0.00349	-0.00883	0.00580	0.00546
	(0.011)	(0.011)	(0.010)	(0.009)
Post reform	-0.0209	-0.0249*	-0.0144	-0.0151
	(0.012)	(0.012)	(0.011)	(0.009)
Theoretical estimate	0.081	0.073	0.057	0.039
Observations	101449	101449	101449	101449
Adjusted R2	0.584	0.613	0.530	0.427
Controls		✓	✓	✓
Municipality fixed effects				✓
County fixed effects			✓	

Dep. var: Log House Price. Clustered standard errors on municipality level in parentheses. *p<0.05, **p<0.01, ***p<0.001. 52,750 observations in the control group, and 48,699 observations in the treatment group.

The observant reader will notice that we have a small problem of decreasing power as we add controls and municipality fixed effects. The worst case is found in model 4 in the "Land tax reform" period, where we cannot reject the null hypothesis of no capitalization and not reject an estimate of 3 p.p, which could – given some weaker assumptions about the information set voters has at time – be a reasonable estimate to expect. However, we can well and good reject full capitalization given the theoretically expected estimate of 3.9 p.p. Of course, we make our conclusions based on the whole set of estimates and over all time periods, and taken together they point in the direction of no effects, at least this far in our empirical analysis. However, we still attempt to solve our power problem by introducing treatment heterogeneity such that the treatment dose is allowed to increase for some groups of properties.

6.2 Four-group DID

In the four-group DID model we divide the treated group into three groups according to intervals of 800', and perform the same estimations as in the case with two groups. The estimates in table 6 show the price development in each treatment group (and in the said time period) relative to the control group. Since the tax reform is designed in such a way that highly valued properties receive a larger treatment, we are in effect increasing the treatment dose for these properties. If saliency is important for explaining why we find no capitalization results in our baseline estimations, we expect to see that estimates for the

highly valued properties are closer to their theoretical estimates than cheaper properties.

Table 6: Four-group DID

	(1)	(2)	(3)	(4)
First announcement × (800'-1600')	0.0186	0.0105	0.0132	0.00942
	(0.010)	(0.011)	(0.010)	(0.009)
Land tax reform × (800'-1600')	0.00573	0.00145	0.00822	0.00584
	(0.010)	(0.010)	(0.010)	(0.009)
Final announcement × (800'-1600')	0.0134	0.00727	0.0120	0.00696
	(0.010)	(0.010)	(0.010)	(0.009)
Post reform × (800'-1600')	-0.0108	-0.0137	-0.00810	-0.0141
	(0.011)	(0.011)	(0.011)	(0.009)
First announcement × (1600'-2400')	0.0246*	0.0138	0.0158	0.00922
	(0.011)	(0.012)	(0.012)	(0.011)
Land tax reform × (1600'-2400')	0.0224	0.0151	0.0152	0.0106
	(0.012)	(0.013)	(0.013)	(0.013)
Final announcement × (1600'-2400')	0.0204	0.0135	0.0115	-0.000987
	(0.012)	(0.013)	(0.013)	(0.011)
Post reform × (1600'-2400')	-0.00757	-0.0137	-0.0134	-0.0179
	(0.013)	(0.013)	(0.013)	(0.011)
First announcement × (>2400')	0.0404*	0.0248	0.0309	0.0148
	(0.017)	(0.018)	(0.017)	(0.015)
Land tax reform × (>2400')	0.0709***	0.0625**	0.0720***	0.0660***
	(0.019)	(0.020)	(0.020)	(0.019)
Final announcement × (>2400')	0.0800***	0.0792***	0.0828***	0.0770***
	(0.020)	(0.020)	(0.020)	(0.019)
Post reform × (>2400')	0.0422	0.0383	0.0429	0.0305
	(0.025)	(0.026)	(0.025)	(0.020)
Theoretical estimate 800'-1600'	0.057	0.054	0.048	0.040
Theoretical estimate 1600'-2400'	0.120	0.116	0.104	0.092
Theoretical estimate >2400'	0.150	0.143	0.127	0.115
Observations	101449	101449	101449	101449
Adjusted R2	0.661	0.673	0.578	0.455
Controls		✓	✓	✓
Municipality fixed effects				✓
County fixed effects			✓	

Dep. var: Log House Price. Clustered standard errors on municipality level in parentheses. *p<0.05, **p<0.01, ***p<0.001. Group sizes from control to highest valued treatment groups: 52,750; 32,584; 12,092, and 4,023.

What we observe in table 6 is somewhat in line with the reasoning above. Estimates are higher for properties that receive larger treatment from the reform. Although, this is only a stable result concerning a small group (4,023 properties) of top valued properties with tax values above SEK 2,400,000. Already in the first announcement period do we find positive estimates in the range 1.5 p.p to 4 p.p. Estimates increase in the following periods, where the null hypothesis of no capitalization is rejected, and it becomes increasingly difficult to reject full capitalization. In the final announcement period we find estimates in the range

of 7.7 p.p to 8.3 p.p, and we are not able to reject full capitalization in any of the four models. In the last period we find positive effects, but they are not statistically significant, and neither are they in the same effect size as in earlier periods. Additionally, we also see that the estimates for the top valued properties are closer to what we would theoretically expect, which is a result that is in line with the saliency explanation.

6.3 Continuous DID

In this section we make use of the whole range of variation and estimate a continuous relationship between reform treatment and market price. Instead of interacting the period dummies with group dummies, we now interact with the (logarithm of) the tax value. In other words, the proportional relationship between tax value and market price is allowed to change over time. In the case of no reform we rely on a continuous version of the parallel trend assumption, namely that proportional trends should be the same for all levels of the log market price. The proportional relationship is theoretically expected to change due to the reform according to what we have shown in figure 5b. Since only properties above the cap have variation in expected treatment effects, we fully interact with a dummy indicating whether the property is below or above the cap.

The results from four models with varying specifications and samples are shown in table 7. The first column with model 1 displays results from estimations using the whole sample and the DDD approach described above. In the first period we get a negative estimate of -1.5 p.p, but we find higher and relatively stable estimates around and above 3 p.p in following time periods. These finding should be compared with the theoretically expected change in the proportional relationship between treatment and market price which is 0.12. The results remain largely stable when we add controls and municipality fixed effects in model 2. In model 3 we perform a robustness check by excluding properties with tax values below 800' and we estimate a regular DID with continuous treatment. If our assumptions hold, and there is no proportional treatment effect for properties below 800', we should not observe very different results comparing with model 2. Estimates remain relatively intact in model 3, although the standard errors decrease somewhat. Remembering earlier analyses where we found highly values houses to be closer to their theoretical effects, we also perform an analysis when excluding the top valued properties. According to the results in model 4, the positive results in the three first columns with treatment effect around 3 p.p seemed to be largely driven by the top valued properties, that received a much larger treatment from the reform and where the cost of not being informed about the reform is higher.

Table 7: DID estimates with continuous treatment

	(1) Full sample	(2) Full sample	(3) > 800'	(4) 800'–2,400'
First announcement	-0.0146	-0.0156	0.00123	-0.00486
	(0.016)	(0.015)	(0.010)	(0.011)
Land tax reform	0.0356	0.0306	0.0388*	0.0149
	(0.020)	(0.019)	(0.015)	(0.015)
Final announcement	0.0379*	0.0397*	0.0317**	0.00412
	(0.018)	(0.016)	(0.011)	(0.012)
Post reform	0.0313	0.0303	0.0279*	0.00443
	(0.020)	(0.019)	(0.014)	(0.015)
Theoretical estimate	0.12	0.12	0.11	0.13
Observations	101449	101449	48699	44676
Adjusted R2	0.865	0.703	0.667	0.575
Controls		✓	✓	✓
Municipality fixed effects		✓	✓	✓

Dep. var: Log House Price. Clustered standard errors on municipality level in parentheses. *p<0.05, **p<0.01, ***p<0.001.

7 Robustness

As we will show below, the general no capitalization result is not likely to be explained by miss-specification of the expectations of house buyers or by other simultaneous policy changes.

7.1 Specification of expectations

If house buyers responded to the information given in early announcements of the reform, not anticipating the final design, we need to slightly adjust our definitions of the control and treatment groups to more properly identify such anticipation effects. We redesign our model to allow for more precise detection of responses to information given at the Almedalen press conference and around the election day. This information reflects the changes that were implemented in the temporary land tax reform. The analysis below assumes that agents in the market believed that the land tax reform would be permanent. Although most properties were subject to further decreases in the tax after the final implementation of the property tax reform, a few got increased taxes. We acknowledge and discuss the possibility that a small part the treatment group in the previous analyses received a "disappointment" effect at the announcement of the final placement of the cap. A "disappointment effect" would occur if earlier suggestions of lower caps would have been credible, or if the 2008 reform had the effect of increasing tax liabilities compared to the land tax reform.

The land tax reform was announced at the press conference in Almedalen, and the proposal was re-announced in the election manifesto by the Alliance in August, 2006. The

land tax reform implied that the tax on the land part of the property would be reduced to SEK 5,000 or SEK 2 per m^2 depending on which one yielded the lowest tax. The tax would stay the same if the new rules did not imply a reduction in the tax.

In our sample, 56 percent of all properties were subject to a tax reduction due to the temporary proposal of a limit on tax on land. We define these properties as being treated in our analysis of early announcement effects. The average gain of the proposal in this group is SEK 3,700 per year, which translates into a net present value of SEK 185,000 assuming that the reform would be permanent. The remaining properties that did not gain at all from the temporary land tax decrease constitutes our control group.

Table 8: DID estimates with land tax reform

	(1)	(2)	(3)	(4)
First announcement	0.00332	-0.0124	0.00231	0.0106
	(0.014)	(0.012)	(0.010)	(0.008)
Land tax reform	0.0156	-0.00191	0.00553	0.0116
	(0.013)	(0.011)	(0.011)	(0.009)
Final announcement	-0.0162	-0.0182	-0.00285	0.00736
	(0.013)	(0.012)	(0.012)	(0.009)
Post reform	-0.0182	-0.0229	-0.0129	-0.00789
	(0.014)	(0.012)	(0.011)	(0.009)
Theoretical estimate	0.080	0.063	0.053	0.045
Observations	101449	101449	101449	101449
Adjusted R2	0.440	0.561	0.509	0.428
Controls		✓	✓	✓
Municipality fixed effects				✓
County fixed effects			✓	

Dep. var: Log House Price. Clustered standard errors on municipality level in parentheses. *p<0.05, **p<0.01, ***p<0.001. 44,162 observations in the control group, and 57,287 observations in the treatment group.

Table 8 displays our capitalization results when using the temporary land tax reform as a way of defining treatment and control group. If the news about a tax reduction was capitalized already at the announcement, we would expect a small but positive DID estimate in the first announcement period when the Alliance had won the election and subsequently implemented the land tax reform. If house buyers responded to the new information about the final design of the property tax reform given in the second announcement period, the estimates would further increase. However, estimates in all four periods are rather small and many are negative. The theoretical estimates under full capitalization range from 4.5 to 7.2 p.p.

When comparing the information given in the early announcements and the final design of the property tax reform, it is clear that it was largely the same properties that were affected. Only a few thousands of houses were "disappointed" by the 2008 tax reform (3,883 in the whole sample). Properties above the cap got on average a further reduc-

tion amounting to SEK 4,900 in yearly tax liabilities from the 2008 property tax reform compared with the land tax reform. Houses treated by the land tax reform gained an additional SEK 4,000 from the final reform. The small but negative estimates for the final reform period cannot be explained by a disappointment effect. It would simply require very large negative price responses in this group and no responses in the rest of the treatment group that were much more affected by the reform.

To conclude, specifying our models to better be able to detect price changes in response to the information given in early announcements, and closely resembling the temporary land tax reform, does not change our main findings from the previous section. For most properties, we have found no evidence of capitalization.

7.2 Potentially confounding policy changes

For our capitalization estimates to be unbiased, there cannot be other policy changes taking place at the same time as the tax reduction that can affect the price development of the treatment and control groups differently. Below we discuss how four different policy changes may have affected demand for different properties and to what extent they could confound our estimates. The four policy changes are:

- Increased capital gains taxation on properties
- Introduction of an earned income tax credit
- A rising interest rate
- The abolition of the wealth tax

The conclusion from this exercise it that each bias is small, but typically goes in the same direction, namely that the prices of houses in our treatment group would decrease relative to the control group during the first half of 2008. A downward bias amounting to between 1 and 2 percentage points in our post-period estimates is possible.

7.2.1 Increased capital gains taxation

Two minor changes in the taxation of returns from residential housing investments were introduced on 1 January 2008. First, the tax on realized capital gains were increased from 20 to 22 percent. An owner who sells a house for 2 million SEK and bought it for SEK 1 million, should now pay SEK 220,000, instead of SEK 200,000, in taxes on realized capital gains. Second, the tax payment could be postponed if it was reinvested in a new house. But, from 1 January 2008, individuals who postponed the tax payment had to pay a yearly interest amounting to about 2.5 percent on the postponed tax liability. Let us now discuss if these policy changes are likely to have had different effects on the price development

of expensive and less expensive houses. Such a difference could bias our estimates of the effect of the reduction of the property tax on house prices.

The primary effect of an increase in the tax on realized capital gains, from residential housing investments alone, is that it lowers the returns on investments in housing capital. However, it lowers the returns on investments equally for all houses independent of the price of the house before the tax was increased. But, if buyers of expensive houses are more likely to also be sellers of a house that has increased in value, compared with buyers of less expensive houses, it may be a problem for us. Let us see how substantial this concern may be. Assume that buyers of houses in our treatment group on average have a taxable gain amounting to SEK 500,000 and buyers of houses in our control group have no gains. The average taxable gains among all house owners with a taxable gain was about SEK 500,000 by the end of 2007 (see Skatteverket (2008)). Our assumption is thus that all gains belong to buyers of houses in our treatment group, which would cause the largest bias in our estimates. The average increase in the tax payments on a gain amounting to SEK 500,000 is SEK 10,000. With the extreme assumption that this would lower the willingness to pay for new houses with the same amount, we would see a reduction of house prices in our treatment group of about a third of a percent (10,000/2,726,000). This can be seen as an upper bound on the bias in our estimates due to this contemporaneous policy change.

The introduction of interest costs on postponed liabilities may also affect the price developments of expensive and less expensive house differently if buyers of expensive houses more often have have sold a house that has increased in value. Assume again an average taxable gain amounting to SEK 500,000 among buyers of houses in our treatment group and no taxable gain among buyers of houses in our control group. The increased cost of postponing the tax liability amounts to SEK 2,500 per year. This is equal to the interest costs of another SEK 62,500 in mortgages (assuming a 4 percent nominal interest rate). Assuming that all buyers of houses in the treatment group lowers their willingness to pay for houses with 30 percent of SEK 62,500 we get a reduction of house prices in this group by 0.8 percent. This bias is not negligible, but can be seen as an upper bound of the bias. In reality, we believe that it is lower, since it is not exclusively buyers of expensive houses that have sold a house with profit.

7.2.2 The introduction of the earned income tax credit

From 1 January 2007 the government introduced an earned income tax credit (EITC), which lowered the tax payment of all employed people. The tax credit increased with income up to a income cap at SEK 318,000 per year, at which the tax reduction was SEK 11,200. For people with lower incomes, the tax reduction was more modest. The tax reduction can be viewed as an income effect and thus increase demand for housing. As the

tax reduction was higher for people with high incomes, demand for higher valued properties may also have increased more than for lower valued properties. This can potentially bias our estimates, but as we shall see the bias is likely to be negligible.

To get an idea of the size and direction of the bias, we illustrate the income effects for two types of households. The low income household consists of two adults with median income among blue collar workers in the private sector (SEK 200,000 net of tax). The high income house hold consists of two adults with median income among white collar workers in the private sector (SEK 240,000). The earned income tax credit for the low income family amounted to SEK 8,900 and for the high income family to SEK 10,700.

Lets assume that households buying a house spend 30 percent of their net income on housing,[14] an interest rate of 4 percent and that interest payments amount to half of the housing costs. Together, this means that the low income family would pay SEK 1,5 million for a house before the EITC was introduced. The high income households on the other hand would pay SEK 1,8 million. Assume further that each household is willing to pay 30 percent of the EITC in increased interest payments. This would lead to a price increase of 4.46 percent for the houses bought by the low income households and 4.47 percent increase for houses bought by the high income households. It is quite clear from this example that the EITC should have nearly identical effects on the price development of houses typically bought by different income groups. Let us take a more extreme example and exchange the high income family to a family with two adults, both with median income among *university educated* white collar workers in the private sector. This family has now a higher income, and thus typically buys more expensive houses (SEK 2 million), but received the same EITC as the previous example of a high income family. Houses demanded by this group would then increase by 3.97 percent. Our conclusion is that we do not expect the introduction of the EITC to lead to anything but perhaps a slight negative bias of our estimated effects.

7.2.3 Rising interest rate

One could argue that prices of properties with high loan-to-value ratio (LVR) are more sensitive to changes in the interest rate. Furthermore, if LVR is correlated with the market value, then changes in the interest rates over time could affect the two groups differently. The 5-year interest rate increased between Jan 2006 and Jan 2008 from 4.1 percent to 5.3 percent.[15] Our case for parallel trends may thus be questioned if the LVR is correlated with market values.

To see how substantial this concern is, we compare LVRs for different groups of prop-

[14] This is slightly higher than the average household in the population which spend 25 percent according to Statistics Sweden.

[15] See historical interest rates on SBAB's website (www.sbab.se), accessed 12 May 2014.

erties. The data on LVRs has been collected by the Swedish supervision authority for financial activities (see Finansinspektionen (2013)). According to their data, the LVR is 70 percent for the properties that we study, owner occupied single family properties. We can compare the LVR for Stockholm (where properties are highly valued) with the LVR for the rest of the country, in order to get an idea of whether LVR varies with market values. It turns out that the difference is not substantially large: in Stockholm the LVR is 65 percent, and in the rest of the country (excluding all cities) the LVR is 73 percent. Although this is not a perfect test of the relationship between market value and LVR, it tells us something about the variation in the country.[16] Our conclusion is that changes in the interest rate over the studied time period is not likely to substantially bias our estimates.

7.2.4 Abolition of the wealth tax

On 1 January 2007, the wealth tax was abolished. Previously, wealth in excess of SEK 1.5 million (3 million for couples) was subject to a 1.5 percent tax. The abolition of the wealth tax could potentially lead to an increase in demand for highly valued properties. We would then overestimate the effect of the decrease in the property tax on highly valued houses. In practice, however, very few households paid the wealth tax –only about 3.6 percent of all households. One reason is probably that several types of assets were exempted from taxation. Among the exempted assets were stocks listed on certain stock markets and agricultural properties. The abolition of the wealth tax may thus have caused an increase in demand for housing as people reallocate their assets from previously non-taxed assets to assets that are no longer subject to the tax. However, the wealth tax is calculated from the tax value, which is lower than the market price. This means that houses were in a sense beneficially treated also when the wealth tax was in place. Hence the incentives to shift wealth to housing property may not be very strong. It is, however, difficult for us to quantify the potential bias this may lead to, but we find it unlikely that the abolition of the wealth tax had any substantial effect on the vast majority of lower valued houses.

7.3 Capitalization in sub-markets

In our baseline estimations we find no evidence of property tax capitalization, except for very expensive properties. We now continue and split our sample according to three different criteria and analyze the extent of capitalization in six sub-markets. First, if our no-capitalization result is driven by a factor such as low financial literacy, i.e. a limited ability to calculate the value of the tax decrease, we expect to observe differences in

[16] The data sample includes 18,178 newly issued mortgages under the periods 8 Aug - 4 Sept and 26 sept - 30 Oct of 2012. In the sample, the average market value of a property in Stockholm is SEK 3,014,600, and SEK 1,432,700 in the rest of the country.

capitalization between areas with highly educated citizens and areas with lower educated citizens. Second, since the result of our capitalization theory rests on the assumption that supply is fixed, we expect to observe differences in capitalization in city-center areas, where it is very difficult to build new houses, and more peripheral areas where exploitation of land for newly-built houses is easier. Third, we analyze the sub-market of Stockholm (the Capital). In Stockholm aspects of both financial literacy and fixed supply are combined, since highly educated people live and work in Stockholm and the supply of housing is notoriously inelastic, in part due to the special geographical conditions that the archipelago implies. It should be noted that sub-markets with highly educated citizens, typically also are characterized by high values and limited possibilities to build new houses. This makes it difficult for us to disentangle which of these characteristics that are most important for property taxes to capitalize into house prices.

Table 9: Sub-markets

	(1) High Education	(2) Low Education	(3) Centre	(4) Periphery	(5) Stockholm region	(6) Other regions
First announcement	0.0119	-0.0137	0.0154	-0.00107	0.0103	-0.0151
	(0.012)	(0.017)	(0.019)	(0.010)	(0.016)	(0.013)
Land reform	0.0647**	0.000916	0.0808*	0.0310*	0.0675**	-0.00843
	(0.022)	(0.016)	(0.032)	(0.014)	(0.020)	(0.011)
Final announcement	0.0639***	0.00623	0.0485	0.0302*	0.0547**	-0.0114
	(0.015)	(0.019)	(0.025)	(0.012)	(0.017)	(0.014)
Post reform	0.0589**	-0.000189	0.0620	0.0225	0.0440	-0.0191
	(0.018)	(0.018)	(0.031)	(0.013)	(0.021)	(0.013)
Theoretical estimate	0.12	0.12	0.12	0.12	0.12	0.12
Observations	22980	25719	5748	42951	14737	33962
Adjusted R2	0.743	0.592	0.765	0.646	0.759	0.627
Share with high education	0.25	0.14	0.24	0.19	0.22	0.18
Share of newly built houses	0.126	0.141	0.055	0.144	0.128	0.136
Mean tax value	1,700	1,300	1,800	1,500	1,900	1,300

Estimation method: Continuous DID (see 5.1). Dep. var: Log House Price. Sample restriction: Tax value >800'. The estimations includes controls and municipality fixed effects, corresponding to column 4 in previous result tables. Clustered standard errors on municipality level in parentheses. *p<0.05, **p<0.01, ***p<0.001. The "high education" group includes the quartile of properties that are located in municipalities with the highest eduction levels. The "centre" group includes properties that are situated in larger cities and in central parishes, see Appendix for a full list of included parishes. The "Stockholm region" group includes properties in the county of Stockholm excluding the peripheral municipalities: Norrtälje, Nynäshamn, Nykvarn and Södertälje.

The results from the analysis of sub-markets is shown in table 9. We analyze the differences between the groups using continuous DID without any of the properties below 800,000 in the estimation. The main reason for excluding lower valued properties is that in the highly educated areas, city centers and in Stockholm, there are extremely few properties with a tax value below 800'. We begin by looking at the criterion of high education, where we have split the sample on the municipality level according to the share of inhabitants with a high education. "High education" is defined as at least three years of

college or university studies. Properties that are in the "high education" group are located in a municipality that put them in the top 75 percentile of the education distribution over all properties; 32 municipalities are included in this group. The "low education" group includes the remaining properties in the distribution. There are 22,980 observations in the "high education" group, and the average share of highly educated citizens is 0.25; whereas there are 25,719 observations in the "low education" group and the average share of highly educated citizens is 0.14. The differences between the groups are clear and in the expected direction, namely that properties that are sold in municipalities with a larger share of educated citizens display an economically important degree of property tax capitalization, the DID-estimate is stable around 6 p.p from the land reform period to our last post reform period. At the same time, properties in municipalities with a lower share of highly educated citizens does not display any property tax capitalization at all. This gives us a hint that the ability to calculate the value of the financial return of the tax decrease may be important for whether or not the lower property tax is capitalized into property prices.

Our second criterion for sub-market analysis concerns the fixed supply assumption. Our baseline no-capitalization result could be due to a failure of the fixed supply assumption, i.e. if the supply of properties increases (or is expected to increase) due to the tax decrease, the price increase will be dampened. We have divided properties into two groups "Centre" and "Periphery" according to whether the property is situated in a larger city and in the most city central parishes (of the Swedish church). In this way we can compare areas with very limited possibilities for building new homes – city centres – with areas where the possibilities for exploitation of new land is easier – the periphery. Since city centres most often consists of condominiums, our sample restriction generates only 5,748 observations in the "Centre" group, while there are 42,951 observations in the "Periphery" group. The point estimates indicate that properties in city centres show a higher degree of capitalization than in the periphery. The point estimates indicate similar capitalization degrees in city centers as in highly educated areas. However, the estimates for city centers are not statistically significant.

Our third sub-market analysis is based on whether or not the property is located in the Stockholm region. The migration into the Stockholm region is extensive, and the popular notion is that the supply of properties do not keep up with migration pace. On a county level, the Stockholm sub-market is obviously the largest of sub-markets and of high interest for policy makers. Our Stockholm sub-market contains 14,737 observations, while the other regions contain 33,962 observations. Again, the difference between the two groups run in the expected direction. The group of properties in the Stockholm region show economically substantial levels of capitalization, at around 4.4-6.8 p.p. The point

estimate for the post reform period is, however, statistically insignificant. Properties in other regions display no capitalization.

Out of the six sub-markets, we find partial capitalization in the three expected markets, and no evidence of capitalization in the other three. The three sub-markets, for which we find evidence of capitalization, share several features that make us a priori believe that we would find significant capitalization. All three are markets with highly valued properties, highly educated citizens and areas in which it is difficult to increase the supply of houses. Given that the results in these three markets all indicate the same level of capitalization, they cannot inform us much about which factor is most important for capitalization.

8 Concluding remarks

We have utilized a rich register based data set in order to estimate how house prices responded to a substantial cut in property taxes. The identification of capitalization in house prices is based on comparisons of house price dynamics of houses that were subject to large and small tax cuts respectively. Our results are largely inconsistent with capitalization theory. For a majority of properties, we find no evidence that the tax cut led to increases in house prices. The estimated price responses are typically close to zero. We find positive capitalization degrees only in markets with highly valued houses, highly educated citizens, and in city centers. In these markets we find that capitalization occurs before the final implementation of the reform, but not before the Alliance had won the election and taken a first step to show that they would permanently lower the property tax.

We have argued that capitalization theory relies on strong assumptions about the rationality of house buyers. Incorporating insights from the literature on bounded rationality, we argue that the non saliency of the property tax, in combination with the complex task to calculate the net present value of the lower tax burden, may explain why we find overall weak evidence of capitalization. When a prospective buyer decides which house to buy and how much to pay for it there may simply be too many aspects to consider to be able to make a perfect valuation of all of them. The net present value of lower tax liabilities in the future may be such a neglected aspect. However, it is possible that if the property tax was substantially larger than it is (or were) for most of the properties in our sample, more buyers would consider how the tax influences the cost of owning the house. This is consistent with our finding that among properties with the highest tax values, we find substantial capitalization rates. Our interpretation of when capitalization is likely to occur, may also explain why i.e. Berger *et al.* (2000) find full capitalization, as the subsidies on mortgage interest rates where highly salient, easier to calculate and associated with less uncertainty about the duration of the subsidy.

We should note at this point that we have estimated short-run capitalization rates, focusing on pre-reform responses and responses during the first six months following the property tax reform. While any information about a change in the property tax is expected to be immediately capitalized into housing prices in an efficient housing market, this market may contain frictions making the adjustment slower. Such frictions can come from boundedly rational consumers, but also from transaction costs or slowly evolving norms about how much to pay for certain properties. Therefore we cannot rule out the possibility that the property tax eventually will be capitalized.

References

Berger, T., Englund, P., Hendershott, P. H. and Turner, B. (2000). The Capitalization of Interest Subsidies: Evidence from Sweden. *Journal of Money, Credit and Banking*, **32** (2), 199–217.

Best, M. C. and Kleven, H. J. (2013). Housing Market Responses to Transaction Taxes: Evidence From Notches and Stimulus in the UK. *Mimeo*.

Boije, R. (1997). *Capitalization, Efficiency and the Demand for Local Public Services*. Ph.D. thesis, Department of Economics, Uppsala University.

Cabral, M. and Hoxby, C. (2012). *The Hated Property Tax: Salience, Tax Rates, and Tax Revolts*. Working Paper 18514, National Bureau of Economic Research.

Chetty, R. (2009). *The Simple Economics of Salience and Taxation*. Working Paper 15246, National Bureau of Economic Research.

—, Looney, A. and Kroft, K. (2009). Salience and Taxation: Theory and Evidence. *American Economic Review*, **99** (4), 1145–1177.

— and Saez, E. (2013). Teaching the Tax Code: Earnings Responses to an Experiment with EITC Recipients. *American Economic Journal: Applied Economics*, **5** (1), 1–31.

Cushing, B. J. (1984). Capitalization of Interjurisdictional Fiscal Differentials: An Alternative Approach. *Journal of Urban Economics*, **15** (3), 317–326.

DellaVigna, S. (2009). Psychology and Economics: Evidence from the Field. *Journal of Economic Literature*, **47** (2), 315–72.

Finansinspektionen (2013). Den svenska bolånemarknaden 2013.

Hilber, C. A. L. (2011). *The Economic Implications of House Price Capitalization: A Survey of an Emerging Literature*. Discussion paper 91, Spatial Economics Research Centre.

Kling, J. R., Mullainathan, S., Shafir, E., Vermeulen, L. C. and Wrobel, M. V. (2012). Comparison Friction: Experimental Evidence from Medicare Drug Plans. *The Quarterly Journal of Economics*, **127**, 199–235.

Kopczuk, W. and Munroe, D. J. (2014). Mansion Tax: The Effect of Transfer Taxes on the Residential Real Estate Market. *Mimeo*.

Lusardi, A. and Mitchell, O. S. (2014). The Economic Importance of Financial Literacy: Theory and Evidence. *Journal of Economic Literature*, **52** (1), 5–44.

Mieszkowski, P. (1972). The Property Tax: An Excise Tax or a Profits Tax? *Journal of Public Economics*, **1** (1), 73–96.

Oates, W. E. (1969). The Effects of Property Taxes and Local Public Spending on Property Values: An Empirical Study of Tax Capitalization and the Tiebout Hypothesis. *Journal of Political Economy*, **77** (6), 957–971.

Palmon, O. and Smith, B. A. (1998). New Evidence on Property Tax Capitalization. *Journal of Political Economy*, **106** (5), 1099–1111.

Ross, S. and Yinger, J. (1999). Sorting and Voting: A Review of the Literature on Urban Public Finance. In E. S. Mills and P. Cheshire (eds.), *Handbook of Regional and Urban Economics*, *47*, pp. 2003–2060.

Sahm, C. R., Shapiro, M. D. and Slemrod, J. (2012). Check in the Mail or More in the Paycheck: Does the Effectiveness of Fiscal Stimulus Depend on How it is Delivered? *American Economic Journal. Economic Policy*, **4** (3), 216–250.

Simon, H. A. (1978). Rationality as Process and as Product of Thought. *The American Economic Review*, **68** (2), 1–16.

Sirmans, G. S., Gatzlaff, D. H. and Macpherson, D. A. (2008). The History of Property Tax Capitalization in Real Estate. *Journal of Real Estate Literature*, **16** (3), 327–343.

Skatteverket (2008). Tax Statistical Yearbook of Sweden 2008.

Stenkula, M. (2014). Taxation of real estate in sweden from 1862 to 2010. In M. Henrekson and M. Stenkulal (eds.), *Swedish Taxation: Developments Since 1862*, New York: Palgrave Macmillan.

Yinger, J. (1982). Capitalization and the Theory of Local Public Finance. *Journal of Political Economy*, **90** (5), 917–943.

Appendix A: List of parishes

0180xx Stockholm
148001 Domkyrkoförs. i Göteborg
148009 Göteborgs Annedal
148013 Göteborgs Carl Johan
148010 Göteborgs Haga
148008 Göteborgs Johanneberg
148012 Göteborgs Masthugg
148004 Nylöse
148011 Göteborgs Oscar Fredrik
148033 Göteborgs S:t Pauli
148025 Tyska Christinae
148007 Göteborgs Vasa
148016 Örgryte
128001 Malmö S:t Petri
128002 Slottsstaden
128003 Kirseberg
128004 Malmö S:t Pauli
128005 S:t Johannes
128006 Möllevången-Sofielund
018101 Södertälje
038001 Uppsala domkyrkoförs.
038002 Helga Trefaldighet
048002 Nyköping
048402 Eskilstuna
058001 Linköpings domkyrkoförs.
058102 Norrköpings S:t Olof
068001 Jönköpings Sofia-Järstorp
078001 Växjö stads- och domkyrkoförs.
088001 Kalmar domkyrkoförs.
108001 Karlskrona stadsförs.
128101 Lunds domkyrkoförs.
128302 Helsingborgs Gustav Adolf
129001 Kristianstads Heliga Trefaldighet
129301 Hässleholm
138001 S:t Nikolai
138301 Varberg
148501 Uddevalla
148801 Trollhättan
149001 Borås Caroli
149601 Skövde
178001 Karlstads domkyrkoförs.
188001 Örebro Nikolai
198001 Västerås domkyrkoförs.
208001 Falu Kristine
218001 Gävle Heliga Trefaldighet
228101 Sundsvalls Gustav Adolf
228401 Örnsköldsvik
238001 Östersund
248001 Umeå stadsförs.
248201 Skellefteå S:t Olov
258002 Luleå domkyrkoförs.