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House price responses to a national property tax reform☆

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ABSTRACT

We show that house prices in general did not respond to a substantial cut in the national property tax in Sweden. The estimates are based on rich register data covering more than 100,000 sales over a time period of two and a half years. Because the Swedish property tax is national and thus unrelated to local public goods, our setting is ideal for causal identification of the property tax on house prices. We observe price increases only in a small segment of the market containing properties with very high tax values. We discuss, but can admittedly not empirically discriminate between, several potential explanations for why we find no evidence of capitalization except for the top segment of the market.

1. Introduction

Suppose you have decided to buy a house. Two houses appear to be nearly identical but one of them is slightly less expensive to own because the annual property tax burden is €100 lower compared with the other house. How much more would you be willing to pay for the lower taxed house? This question is at the heart of the standard capitalization theory (Oates, 1969; Yinger, 1982) where the price of a house is determined by the total stream of housing services minus the net present value of all costs of owning the house. When the property tax decreases, buyers realize that the cost of living also decreases and they are thus willing to pay a higher price for the house. If the supply of land and housing is fixed, the market price will increase with the full net present value of the tax reduction. Furthermore, if the housing market is efficient and

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individuals use all relevant information, prices will change immediately when information about future tax changes is made public (Palmon and Smith, 1998; Ross and Yinger, 1999).

Most earlier studies on property tax capitalization focus on local property taxes and support the prediction that lower property taxes lead to higher house prices (see e.g. the reviews by Ross and Yinger, 1999; Sirmans et al., 2008; Hilber, 2015). However, recent contributions, using richer data and more credible identification methods add important insights to the literature. Bradley (2015) finds that a temporary tax rebate leads to price increases that are much larger than the net-present-value of the rebate. He interprets this finding as being inconsistent with standard capitalization theory and as evidence of bounded rationality among house buyers. Moreover, Lutz (2015) and Hilber and Vermeulen (2016) find that capitalization degrees vary with the elasticity of the housing supply. The importance of paying attention to the supply side in empirical tests of capitalization theory is extensively discussed in Hilber (2015).

In this paper we empirically analyze how house prices responded to a reform that substantially reduced the Swedish national property tax on owner occupied residential properties (referred to as just houses or properties). The reform is frequently referred to as an "abolition" in Swedish policy discussions due to the extensiveness of the reduction. The reform is remarkable also in the sense that owners of very expensive properties got disproportionately large tax reductions. The tax was reduced in two steps; a preliminary reform including a medium-sized tax cut was introduced immediately after the center-right coalition had won the 2006 parliamentary election. A permanent and final reform including a larger tax cut was implemented on 1 January 2008. Prior to any of the two steps, the yearly tax payment equaled 1 percent of the property value as *assessed* by the Swedish Tax Agency (hereby referred to as the "tax value"). The final reform package included a decrease of the tax rate from 1 percent to 0.75 percent. But the most sweeping part of the reform was the introduction of a cap on yearly tax payments at SEK 6000 ($710 or €630).[1] The cap was binding for roughly half of all properties. The capping of property tax payments implies a disproportionately large decrease in the tax liability for properties above the cap.

We utilize this differential treatment in a difference-in-difference (DiD) approach, with uncapped properties in the control group and capped properties in the treatment group. Under the assumption of parallel trends in price developments for houses of different tax values, we estimate the causal effect of the tax cap on house prices. Our data is obtained from the official home ownership register and covers all (roughly 100,000) sales of single-family houses, mediated through a real estate agent connected to Svensk Mäklarstatistik AB (Our translation: Swedish Real Estate Agent Statistics, Inc.) in Sweden during the three years that span the reform period: 2006, 2007 and 2008.

Most of the earlier studies on property tax capitalization analyze local or regional cross-sectional variation in property tax rates. There are two fundamental identification problems with that approach, and these problems have been known and discussed since the seminal paper by Oates (1969). First, a higher tax rate implies higher tax revenues and consequently higher quality of public goods. Higher quality of public goods puts upward pressure on house prices, making it difficult to isolate the effect of the tax separately. Controlling for public goods quality has been the main concern so as to avoid biased estimates, but this task has proven difficult. Second, when local governments set their tax rate, areas with higher house prices, all else equal, are able to set a lower tax rate to collect a given amount of tax revenues. This creates a simultaneity bias between the property tax rate and house prices. A key advantage of our study is that the Swedish property tax rate is set at the national level, without concern for local house prices or quality of local public goods. With our approach, we avoid the two identification problems explained above. Using variation stemming from a national reform has an additional advantage, namely that we can track the effects of all reform events, including policy announcements.[2]

In this context it is important to note that responses to local and national property tax changes need not be identical. When a local property tax is raised in one jurisdiction, it is in principle possible for a buyer to buy an identical property in another jurisdiction with a lower tax burden. This decreases demand in the jurisdiction where the property tax was raised and increases demand in the other jurisdiction, with corresponding price adjustments. When a national property tax is changed, all identical properties are affected by the tax change. With a kinked national property tax schedule, as in the post-reform regime in Sweden, demand for properties with high tax values increases relative to demand for properties with tax values below the kink. However, since two properties with different tax values are arguably not identical, and hence not perfect substitutes, it is possible that the housing demand elasticity with respect to national property tax is lower than for a local property tax. If housing supply is perfectly inelastic, the demand elasticity does not affect capitalization degrees, but with elastic supply the capitalization degree might well be lower for a national property tax than for a local property tax. It should also be noted that while most of the earlier literature has used cross-sectional variation in property taxes and house prices, and hence implicitly compared long-run equilibrium outcomes, our reform evaluation approach makes it possible to study dynamic adjustment to changes in the taxes. Our study contributes to the previous literature estimating capitalization of property taxes in that we make use of a national reform and a large nation wide register based data set to identify price responses.

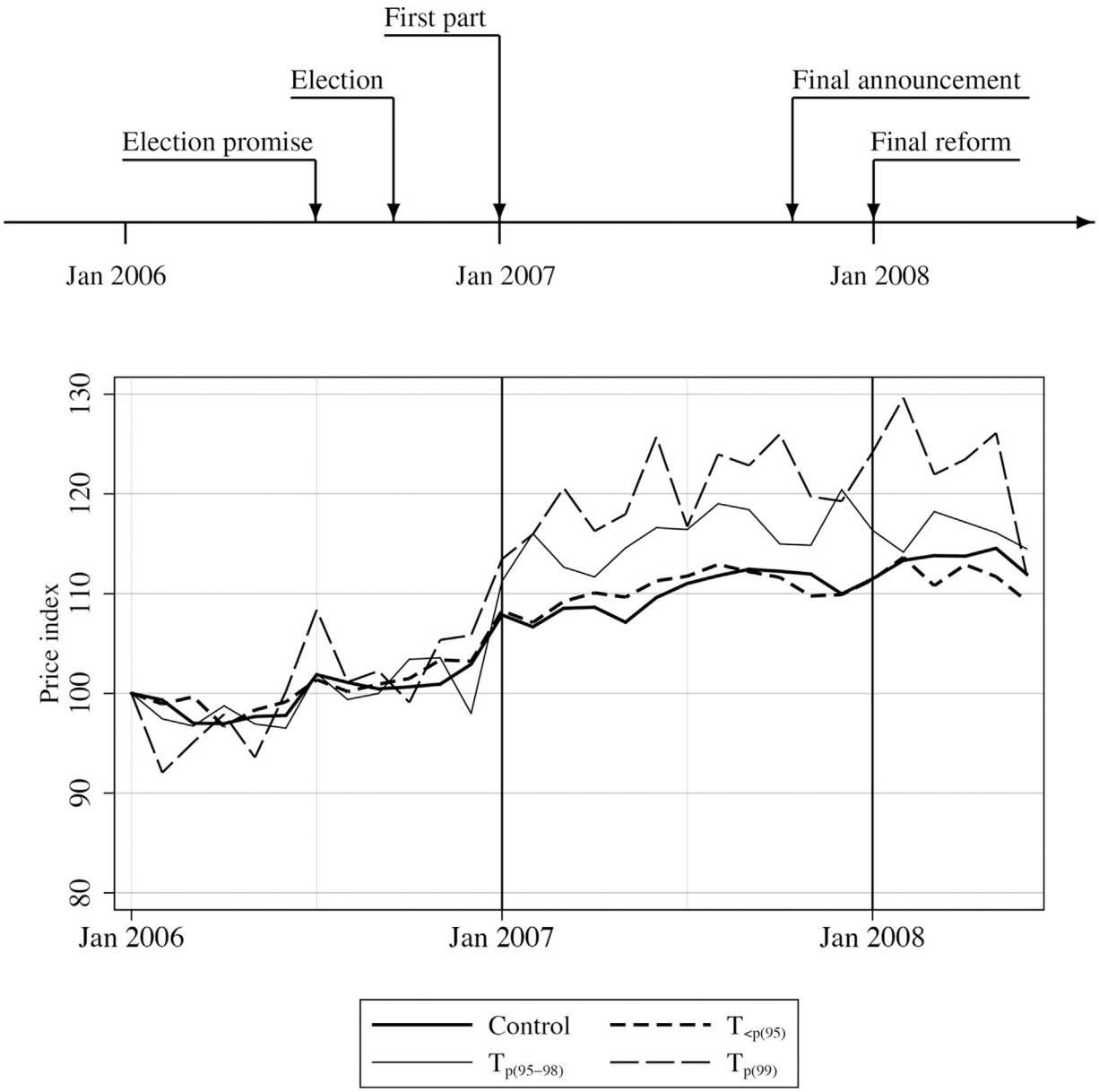

Fig. 1. Timing of events and price development of houses in control and treatment groups. *Note*: The data is collapsed on monthly level and seasonally adjusted with group specific calendar month dummies.

Fig. 1 shows the timing of reform events and the development of house prices for properties with tax values above and below the tax cap. Note that tax values were set in 2006 and were not changed until 2009, and are thus not endogenous to price changes induced by the reform. The solid thick line represents sales prices in the control group, i.e. properties with tax values lower than the cut-off which is implied by the cap. The price development in this group serves as a counterfactual for the properties in the three treated groups, which contains higher valued properties. In Fig. 1 we display three treatment groups; properties with the top 1 percent highest tax values, the top 5 percent (excluding the top 1 percent), and the remaining treated properties separately. Up until 1 January 2007, the price development is fairly similar in all four

[1] $1≈ SEK 8.45 and €1≈ SEK 9.51 (September 2016).

[2] A related, and quite large, literature investigates how other fiscal variables like transfer taxes (e.g. Besley et al., 2014; Kopczuk and Munroe, 2015; Best and Kleven, 2016; Slemrod et al., 2016), central government grants (Hilber et al., 2011), local income taxes (Boije, 1997), specific subsidies (Berger et al., 2000; Hilber and Turner, 2014) or neighborhood characteristics such as school quality (e.g. Black, 1999; Figlio and Lucas, 2004; Fiva and Kirkebøen, 2011), air pollution (Chay and Greenstone, 2005), or availability of religious amenities (Blind and Dahlberg, 2015) affects house prices.

groups. From that date, which coincides with the first part of the reform, the prices of the properties in the two highest valued groups diverges upwards, and the top 1 percent more so.[3] The largest of the treatment groups, which contains the remaining properties below the top 5 percent do not respond at all to any of the reform events. Our interpretation of the pattern shown in Fig. 1, which is supported by all the econometric analyzes, is that the introduction of a cap on property taxes did not lead to any house price increases for the vast majority of properties that were bound by this cap. Most treated houses share the same trend as the control group during the time period that we study, which contains all the important reform events. However, the tax cap seems to have had an effect for very highly valued properties. These houses are typically situated in areas in or around the three greater cities of Sweden, most notably around the capital of Stockholm. The effect size of the top 1 percent is a 8.4 percentage points increase relative to the control group. Although a substantial effect, the estimate is smaller than the expected price response (under the assumptions of rational expectations and fixed supply) which is 16.0 percentage points.

Our results are consistent with capitalization theory only if the supply of housing is perfectly elastic in all areas in Sweden except the areas in which the most exclusive houses are located. However, we find the regulatory, political and economic restrictions to be too serious to warrant such an assumption about the Swedish housing market as a whole. This statement is supported by studies of the Swedish housing market that have characterized the supply elasticity as low in general (see e.g. Hüfner and Lundsgaard, 2007). Instead, we have to look for complementary explanations for our zero-results. Besides variations in supply elasticity, we propose heterogeneity in saliency, financial literacy, frictions and expectations of policy reversal as potential mechanisms. Further studies on capitalization of property taxes are needed to confirm or reject the relevance of each of these mechanisms.

2. The Swedish property tax and the reform

Between 1985 and 2008, the Swedish property tax was uniform across the country and provided revenues to the national government.[4] The guiding principle of property taxation in Sweden is that property owners are liable to pay a percentage of the tax value of the property on a yearly basis. From 2001 until 2008 the tax rate was 1 percent of the tax value. The tax value is reassessed by the Swedish Tax Agency (Swedish: Skatteverket) every third year and is approximately 75 percent of the property market value at the time of assessment. Several variables enter into the algorithm that determines the tax value, such as: the average market price of surrounding properties, and individual property characteristics such as house size. The total tax value consists of the sum of the assessed value of buildings and the value of the land. A major advantage for our purposes is that a national assessment took place in 2006.[5] All the tax values in our data were consequently established before the reform could have had any effect on market prices, and therefore on the tax values themselves. The next national assessment took place in 2009. The property tax is paid by the owner of the house in connection with the annual tax settlement, which lists all of the individual tax liabilities pertaining to the previous year. The individual does not have to report house ownership and value since the tax authority retrieves this information from the property ownership register held by the Swedish land surveying office, which is the same register from which we retrieved data for this paper.

2.1. The property tax reform

Before the election to the national parliament in 2006 four opposition parties (the Moderate Party, the Liberal Party, the Center Party and the Christian Democrats) formed the coalition “Alliance for Sweden” (henceforth: the Alliance). In the campaign they promised that they would substantially cut the property tax, would they win the election. Immediately after the victory, the Alliance declared that they would deliver on their election promise to substantially lower the national property tax.[6] Measures were taken already in 2007 with the implementation of a temporary tax cut while the details of a permanent reform were discussed. The final reform, implemented on 1 January 2008, implied a reduction in the tax rate from 1 percent to 0.75 percent of the tax value and a cap on yearly tax liabilities at SEK 6000. The cap was binding for roughly half of all properties. The average yearly reduction in tax liabilities from the reform amounted to SEK 4900 and the average net present value of the tax reduction was SEK 243,000; 14 percent of the average market price.[7]

In 2006, before the reform, property tax revenues constituted slightly less than 3 percent of all tax revenues in Sweden. After the reform, the share was reduced to 2.3 percent. These property tax revenues include also property taxes on commercial and industrial buildings, as well as multi-family housing. Compared with the OECD average of 5.4 percent (or the US or UK which both collect more than 10 percent of their tax revenues from property taxes), reliance on property taxes was, and still

[3] We limit the analysis to the first half of 2008 since the financial crisis hit the market in the fall of 2008 and caused a huge drop in the number of sold properties. See Appendix A.2 for more discussion on the financial crisis and how it affected the Swedish housing market.

[4] For a historical review of the property tax in Sweden see Stenkula (2015)

[5] In 2005, home owners had to declare the characteristics of their house by filling in a form and send it to the tax agency. Home owners were then informed in the summer of 2006 about the final tax value.

[6] Like in the U.S. and elsewhere, the property tax is unpopular and regularly discussed also in Sweden. 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. See Cabral and Hoxby (2012) for a discussion on the unpopularity of the U.S property tax.

[7] Details about how the net present value is calculated is presented in Section 4.2.

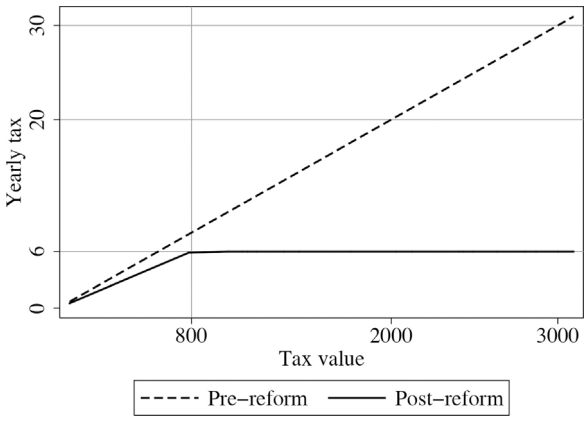

Fig. 2. Yearly tax payments. *Note*: The unit is SEK thousand.

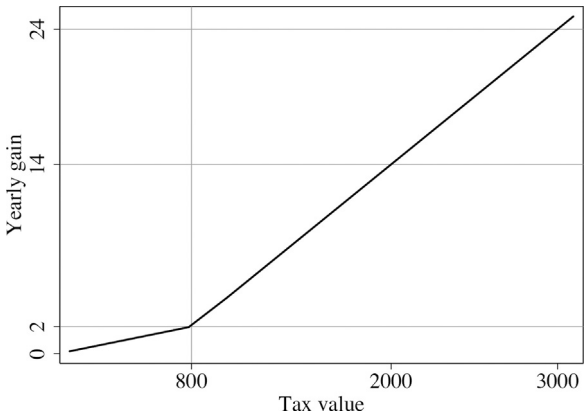

Fig. 3. Yearly reform gain. *Note*: The unit is SEK thousand.

is, very weak in Sweden. A recurring setup in developed countries is that local governments or cities set their own property taxes according to the local preferences for public goods such as education. In Sweden however, the property tax rate is determined at the national level. As a result of the reform studied in this paper, the actual revenues from the property tax on owner occupied residential housing were shifted from the national government to the municipal governments. However, during the first year of implementation the reform was financially neutral to each municipality since intergovernmental grants were decreased by the exact amount of the property tax revenue. Thereafter, the revenues from the property tax made a positive net contribution to municipal finances according to their jurisdictional property tax base, although the effect on the municipal budget was very limited. The revenue from the property tax constitutes only 2–3 percent of total municipal revenues, which consist primarily of income tax revenues and intergovernmental grants.[8] Also note that municipalities – before as well as after the reform – cannot affect the property tax schedule, but have to accept the rate (and cap) decided at the national level.

The reform was partly financed with an increase in the tax rate on the realized gains from a house sale from 20 percent to 22 percent, and partly by the introduction of an interest rate on delays on these tax payments. While the reduction in the property tax benefited owners of highly valued houses disproportionately more than the owners of lower valued houses, the tax on realized gains was unrelated to the tax value of the house. This difference is essential when we estimate the effect of the cut in the property tax. In Appendix C.3, we discuss in detail why the financing of the reform is not likely to confound our estimates of the effect of the property tax much.

In Fig. 2 we show how yearly property tax payments depend on the tax value before and after the reform. The dashed straight line with a gradient of 0.01 is the pre-reform proportional tax schedule. The post-reform tax schedule has a gradient of 0.0075 and a kink at a tax value of SEK 800,000, which illustrates that the final part of the reform contained two components: the proportional tax decrease of 0.25 percentage points and the cap at SEK 6000. We combine these two schedules in Fig. 3 in order to illustrate how the gain from the reform is related to the tax value. Clearly, the tax reduction was larger for highly valued houses both in relative and absolute terms, although all house owners benefited from the rate cut.

[8] For more details on the reform see Government Proposal, 2007/08:27.

2.2. From promise to implementation – when to expect responses?

As discussed by e.g. Blundell et al. (2011), when evaluating reforms it is important to track announcements that are leading up to actual implementation. If there are dynamic incentives to react to the reform already at the time of announcement, individuals may time their response accordingly, and an estimation of the effect of the reform at the time of implementation could be biased downwards. For us, the important question is when expectations about the implementation of a property tax reform – and about the details of that reform – changed due to newly released information.

Before the Alliance parties decided to make the property tax reform one of its most salient election promises during the election campaign in 2006, it was only the Christian Democrats, a small right-wing party, who had propagated for substantial reductions of the property tax.[9] However, the Alliance announced in the summer of 2006 that they intended to make across the board decreases in the property tax if they would win the election. As a first step, the Alliance promised that the yearly tax liability on the land part should not exceed SEK 5000. While the properties that were affected by the first part of the reform was largely the same properties that were affected also by the final part of the reform, the main difference was that the first part implied a smaller cut than the final reform.[10] This first part was intended to be temporary and eventually replaced with a "low" property tax collected by the municipalities in 2008. However no further details about the second and final part of the reform were mentioned. The Alliance also stated that the tax decrease would be partly financed within the housing sector. Representatives from the ruling Social Democratic party deemed the proposal as irresponsible due to the lack of a clearer financing plan, and also pointed out that the 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 small reduction of the tax rate was on the table.

In September 2006 the Alliance won the election and formed a majority government. The new government implemented the promised first part of the reform in January 2007. During 2007 there was a public discussion about the details of the final reform. Different placements of the cap were suggested. In September the final proposal of a cap at SEK 6000 and a tax decrease to 0.75 percent was publicly announced. The proposition was put forward to the parliament in October and was accepted as law in December. Fig. 1 showed the timing of announcements and implementation of different parts of the reform.

What could prospective house buyers and sellers expect at different points on in time? In the summer of 2006, when the Alliance announced that they would substantially reduce the property tax, it was a clear break from voters' earlier expectations. Before this election promise, a major reduction in the property tax was only given priority publicly by the Christian Democrats. However, it was expected that the coming election would be a close race and even if the Alliance would win, it was still not certain that the promise would be delivered on and what the details of the reform would be. In the presence of such high uncertainty we would not expect to see much of a response in house prices in the summer of 2006. After the Alliance won the election, and quickly thereafter re-announced that they would reduce the property tax, the probability of a substantial and permanent cut greatly increased. Their intentions were further substantiated as they implemented the first part of the reform going into 2007. Although the details of the final reform were unknown until the second half of 2007, owners of highly valued properties likely suspected a substantial cut in their property tax payments. Accordingly, we expect prices to have fully adjusted to the reform by the end of 2007.

Finally, from 1 January 2008, when the new tax schedule was implemented, the only uncertainty that remained was whether the tax would be changed again, e.g. if Social Democrats would revert the property tax, were they to win the election in 2010. However, the Social Democrats quickly adjusted to the new tax schedule, and in 2009 they admitted to only wanting to increase the property tax for extremely expensive houses.[11] House owners and prospective buyers, therefore, had good reason to believe that the property tax reduction would not be reverted with the Social Democrats in government. Furthermore, it turned out that the Alliance was able to maintain government power after the 2010 election. The minority coalition government consisting of the Social Democrats and the Green Party that came to power in 2014 has yet to make any suggestions of changes in the property tax. In 2014 the Social Democratic finance minister said that the property tax would not be raised.

3. 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 all instances where a house – intended for permanent or summer living – has switched owners during the time period 2006 to 2008, for all of Sweden. It contains information on tax liability, the date when the sales contract was signed, the final price, characteristics of the property, and precise location.

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

[10] Details about the distribution of tax decreases from this part of the reform are presented in Appendix A.1.

[11] In a leading Swedish newspaper the Social Democrats wrote that they would like to complement the newly introduced tax cap with a proportional property tax amounting to 1 percent on houses with tax values above SEK 4.5 million. There are only 160 observations with such high tax values in our data set, which is 0.16 percent of all observations.

From this extensive data set we select observations to be included in a relevant population based on a number of criteria. We leave out owner changes due to inheritance, distribution of marital properties, premarital settlements, gifts, purchases between family members and the like. Second, we only focus on observations that refer to one singular tax value unit, and which contains exactly one house and exactly one land unit. Third, only properties that had their latest tax value assessed in the mandatory tax value assessment in 2006 are included in the study population since tax 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. Assessments in 2007 and 2008 were only conducted for rebuilt houses. We leave out all sales during 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.[12] Finally, we miss information on value area, value year, tax value, living or total area for a few hundred sales.

From this population, we analyze all sales mediated through a real estate agents connected to Svensk Mäklarstatistik AB, which constitute 52 percent of all sales in the relevant population. The reason we focus on this group is that we only know the contract signing date, i.e. the date when the sales price was agreed upon, for sales that are mediated with a real estate agent that is connected to Svensk Mäklarstatistik AB. Fortunately, it is mainly smaller real estate agents that are not connected to Svensk Mäklarstatistik AB. However, in Table 3 in Appendix A.3 we show that the sample used in the estimations is largely representative of the entire population of sales. As expected, properties for which sales are not reported to Svensk Mäklarstatistik AB are more often located in non-urban areas and hence have slightly lower market value. The above restrictions leave us with a sample containing 101,449 observations.

4. Empirical strategy

As was shown in Figs. 2 and 3, the yearly tax liability is a deterministic function of the tax value both before and after the reform; and the tax reduction was proportionally larger for more expensive houses because of the introduction of a tax cap for houses with tax values over SEK 800,000. A simple cross-sectional regression of market price on tax reduction will therefore yield a positive coefficient as a direct consequence of the endogenous construction of the reform. Since we have data on house sales both before and after the reform we deal with this endogeneity issue in a Differences-in-Differences (DiD) framework, where the price development of properties below the tax cap servers as a counter-factual for houses with tax values above the cap.[13] We estimate the following empirical model in Eq. (1).

$$y_{ijt} = P_t + T_{ij} + D_{ijt} + \Omega \mathbf{X}_{ijt} + V_j + \varepsilon_{ijt}, \tag{1}$$

where y_{ijt} is the natural logarithm of the market price for house i, in municipality j, and in period t. We include period specific dummies in P_t and control and treatment group specific dummies in T_{ij}. Unless otherwise is stated, P_t is equal to one during 2007 and 2008, and zero during 2006. In some specifications, we divide the houses above the cap into three mutually exclusive treatment groups as in Fig. 1. However, we also show results where we include all houses above the cap in one single treatment group. The treatment group specific dummies are interacted with the period specific dummies to generate the variables of interest in D_{ijt}. If the reform takes place in time period $t = k$, and there are no prior announcements, the DiD-estimate of the reform effect is found in D_{ijk}. In most specifications we use one time period dummy to indicate pre- and post-reform, but we also show more dynamic effects in Section 6.3. Here we divide the pre- and post-dummies into half year long periods in order to study more specifically timed responses. Standard errors are clustered at the municipality level (288 clusters).

In the result tables we refer to Model 1 is a baseline DiD-model that only contains treatment- T_{ij} and time period dummies P_t, and interactions between the two D_{ijt}. In Model 2 we add control variables $\mathbf{X}_{ijt}$, which are the following: the size of the house (*living area*), the size of the land property (*total area*), a tax authority measure of house standard (*standard points*), and the latest year when extensive changes were made to the house, normally the year of construction, (*construction year*). In Model 3 we further add municipality fixed effects V_j, and in Model 4 we exchange the municipality fixed effects for the smaller (exclusively religious) jurisdiction of parishes.

The DiD-approach identifies the effect of the property tax cut under the crucial assumption that house prices in the treatment group would evolve similarly to the control group if there was no treatment. Since our treatment and control groups have generally different levels of prices we argue that the parallel trend assumption is more likely to hold for logarithms than for levels. According to our empirical specification, where the outcome variable is logarithmic, houses in the control and treatment groups should have similar *proportional* price changes if there was no reform.[14] The assumption is consistent with the idea that houses are capital investments that yield the same equilibrium rate of return within the fixed borders of a country. As could been seen in Fig. 1, the control and treatment groups follow a similar proportional trend during the pre-treatment period of 2006, which supports the validity of the parallel trend assumption. The parallel

[12] The collapse of the market in the end of 2008 can be seen in Figs. 10 and 11 in Appendix A.2.

[13] Given that there is a kink in treatment at tax value SEK 800,000, it is natural to suggest the use of Regression Kink Design (RKD) to identify the causal effect of the tax reduction. We have tried such a strategy but obtained estimates that were too 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, and close to, the kink. The results are presented in Appendix D.

[14] Or to be more accurate, if owners of properties with tax values above the cap also only received the proportional tax rate decrease.

Table 1
Theoretical effects under full capitalization.

	(1) First difference	(2) DiD-estimate
Control	0.074	Control group
Treated	0.154	0.080
$T_{<p(95)}$	0.146	0.072
$T_{p(95-98)}$	0.215	0.141
$T_{<p(99)}$	0.233	0.160

Note: Column 1 shows the first difference between the market price with and without the NPV of the reform for each group separately. Column 2 is the second difference net of the first difference for the control group.

trend assumption is violated if there are other policy changes or demand/supply shocks in 2007 or 2008 that affect the price development of houses in the control and treatment groups differently. In Appendix C.1 we provide additional support for the parallel trend assumption. In Appendix C.2 we show that there were no substantial endogenous supply responses to the reform. And in Appendix C.3, we thoroughly discuss three potentially confounding policy changes: the increased capital gains taxation, introduction of an earned income tax credit and the abolition of the wealth tax. To summarize, the conclusion is that each of these policy changes is likely to only marginally bias our estimates. One exception is the abolition of the wealth tax, which might bias the estimates in the top segments upwards. However, it is difficult to quantify the magnitude of that potential bias.

4.1. Expected price responses under full capitalization

If the property tax reduction is fully capitalized – of what order of magnitude do we expect the estimates to be? How large differences in price developments do we expect to see between our control and treatment groups? In this section we calculate expected price responses when the full value of the tax reduction is capitalized. We refer to these estimates as "theoretical estimates" since these are the benchmark estimates that we derive from the standard capitalization model. These theoretical estimates are obtained by performing simulated DiD estimations in the pre-treatment period since the market prices of these properties are not affected by the reform.

We begin by calculating the net present value (NPV) of the final reform tax reduction for each individual property that was sold in the first half of 2006.[15] We add the NPV to the actual market price of each property in order to get a market price under full capitalization; as if the 2008 reform was implemented already for the houses sold in the beginning of 2006. We then perform a DiD estimation comparing the differences in prices between groups before and after the value of the fully capitalized reform has been added to the market price.

The theoretical DiD-estimates under full capitalization and fixed supply are presented in Table 1. The first column displays the theoretical reform effect for each group separately, i.e. a difference between the actual log price and the log price with the added reform NPV. Under full capitalization, prices in the control group are expected to increase on average by approximately 7.4 percent (using differences in log as an approximation for percentage change). The corresponding price increase for all treated (capped) properties is 15.4 percent. The gain from the reform is clearly larger in higher valued segments. The price response in the three treated groups of properties is 14.6, 21.5, and 23.3 percent respectively. Column 2 displays the theoretical DiD estimates for the three treatment groups compared with the control group using Model 1. These theoretical DiD-estimates imply relative price increases amounting to 8.0, 7.2, 14.1, and 16.0 percentage points respectively. These theoretical estimates are an important point of comparison when we analyze our empirical results in Section 6. In some specifications, the sample and the identifying variation differs from what we have used when we calculated the theoretical effects presented in Table 1. When we change the specification or sample, we therefore calculate new theoretical effects and present them together with the results.

4.2. NPV calculations

The theoretical estimates depend on assumptions about the agents' expectations about the future 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:

$$NPV = \sum_{t=0}^{T} \frac{I_t}{(1+r+\pi)^t}, \tag{2}$$

[15] We also take into account that newly built houses were exempted from the property tax, both before and after the reform, for five years and a 50 percent reduction for another five years. However, this has a very small effect since only 4507 houses in the sample (4.4 percent) are exempted from the tax (are built after 1996).

where I_t denote the income in period t, or in our case the nominal annual tax reduction, r the real interest rate and π the inflation rate. The tricky part is to calculate I_t. According to the details in the property tax bill effective from 1 January 2008, the cap (initially at SEK 6000) 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 π+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:

$$NPV = I_0 \times T \tag{3}$$

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 and with an infinite horizon.

From this discussion, it should be clear that the theoretical benchmarks are sensitive to several assumptions. Importantly, they would be smaller with a shorter time horizon and/or with a higher real interest rate, and vice verse. Moreover if house buyers would expect the property tax to soon be increased again, the theoretical estimates could be too high. On the other hand, further cuts are also possible, and if house buyers put some probability on that scenario, the theoretical estimates may be too small.

5. Descriptive analysis

The distribution of properties according to their tax values is shown in Fig. 4. The distribution is positively skewed, with a tail (cut close to the 99th percentile for aesthetic reasons) of 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.

In Table 2 we show property characteristics using the variables in the data set. We show values for the full sample of properties, but also for the control group, the treatment group and three segmented treatment groups. As previously mentioned, the average gain from the reform is SEK 4907 in terms of yearly tax payments, and SEK 243,000 in terms of NPV, see descriptive statistics for the full sample in Column 1. For owners of houses in the top 1 percent, the NPV of the reform was substantially larger; roughly SEK 1.7 million, see Column 5. Owners of these houses went from paying almost SEK 40,000 every year to only SEK 6000 in property taxes. The NPV gain for the two other segmented treatment groups is SEK 994,000 and SEK 364,000. Houses in groups with higher tax values are larger, of higher quality (more standard points), and are located in more attractive geographic areas; the land share of the tax value is higher even though the total area is typically smaller than in the control group.

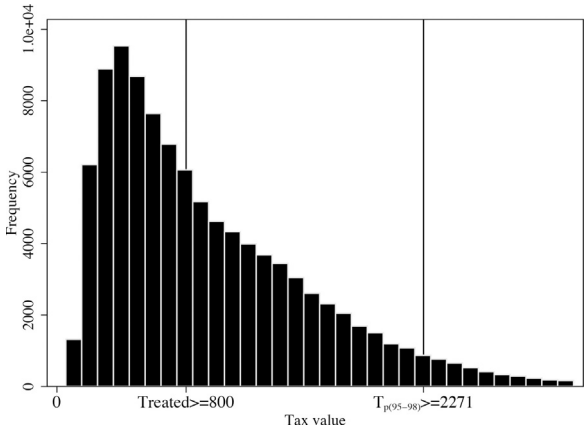

Fig. 4. Distribution of tax values. *Note*: The unit is SEK thousand, and the bin width is 100. Distribution is truncated at tax value 3200.

Table 2
Property characteristics by treatment status.

	(1) Full	(2) Control	(3) Treated	(4) $T_{<p(95)}$	(5) $T_{p(95-98)}$	(6) $T_{p(99)}$
Market price	1778	903	2726	2429	4721	7481
Tax value	959	462	1497	1336	2617	3963
NPV gain	243	58	444	364	994	1658
Yearly gain from reform	4.9	1.2	9.0	7.4	20.2	33.6
Yearly tax pre-reform	9.6	4.6	15.0	13.4	26.0	39.6
Yearly tax post-reform	4.7	3.4	6.0	6.0	6.0	6.0
Effective tax pre-reform %	0.54	0.51	0.57	0.57	0.58	0.56
Effective tax post-reform %	0.26	0.38	0.26	0.27	0.13	0.087
Living area, m^2	115	101	131	126	159	201
Total area, m^2	1473	1826	1091	1075	1162	1501
Standard points	29	27	31	30	32	34
Construction year	1963	1958	1969	1969	1969	1960
Land value share	0.37	0.36	0.38	0.37	0.44	0.48
Observations	101,449	52,750	48,699	43,617	4067	1015

Note: Prices, values, gains and taxes are expressed in SEK thousand nominal prices. Mean values.

6. Results

6.1. Baseline results

We start by presenting the results from DiD-estimations when dividing into two time periods. The pre-reform period covers year 2006, and the remaining time period (Jan 2007–Jun 2008) is the post-reform period since the first and final steps of the reform were implemented during this period. In this section we only use one control group of uncapped properties and one treatment group of capped properties (property characteristics of these two groups are shown in Column 2 and 3 in Table 2). We show results from this two-by-two DiD-estmation using all four empirical models that were previously discussed. As we control for more potentially confounding factors, the variation used for identification decreases and the theoretically expected price responses decrease. Fig. 5 shows the estimated effects as well as the theoretically expected effects for each of the four models (detailed regression results are presented in Table 5 in Appendix B).

The point estimates in each of the four specifications are slightly negative but very small. None of them suggest even a one percentage point response in house prices in the treated group compared with the control group. With point estimates as small as these we cannot statistically reject a zero effect. We can, however, reject full capitalization along with any other economically significant degree of capitalization. Contrary to what we would expect under standard capitalization theory, we therefore conclude that there is no substantial difference in price developments in the group that was treated with the property tax cap compared with the control group.

Because the point estimates appears to be stable across the specifications we will refer to Model 1, which is the most parsimonious and has the largest expected theoretical effects, as our main specification. The following subsections will therefore focus on results obtained from variations of Model 1. Results for the other specifications are presented in Appendix B.

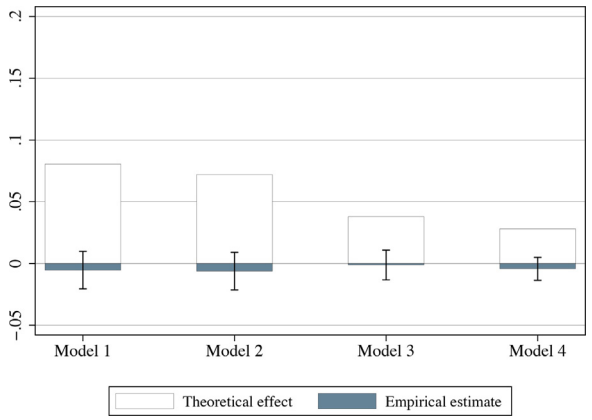

Fig. 5. Effect sizes and confidence intervals: one treatment group. *Note*: Ranges indicate 95% confidence intervals. Point estimates from left to right: −0.00535, −0.00616, −0.00119 and −0.00429, see Table 5 in Appendix B. Model 1 is a baseline DiD-model, Model 2 adds control variables, Model 3 further adds municipality fixed effects, and Model 4 adds parish, instead of municipality, fixed effects.

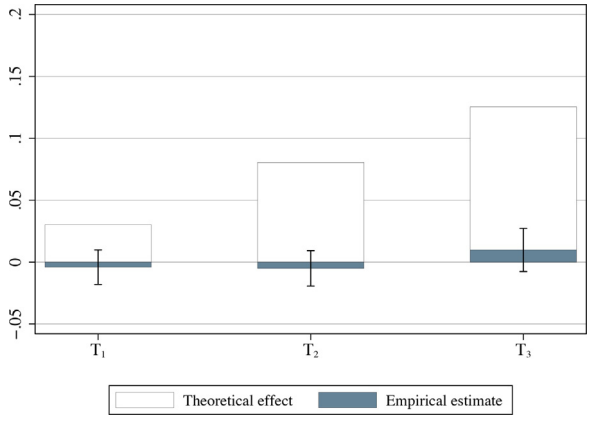

Fig. 6. Effect sizes and confidence intervals: three treated groups. *Note*: Ranges indicate 95% confidence intervals. Point estimates from left to right: −0.00412, −0.00505, 0.00985, see Model 1 (middle panel) in Table 5 in Appendix B.

6.2. Evidence from different segments of the market

We showed earlier that the gain from the reform in terms of decreased tax liability increases with the tax value of the property. We would therefore expect prices of houses with higher tax values to increase more than houses with lower tax values within the treatment group. We therefore divide the treatment group into three equally sized groups in terms of number of observations. In the first group T_1 tax values range from SEK 800,000 to 1,130,000. In the second group T_2 the range is SEK 1,130,000 to 1,595,000 and the third group T_3 contains houses with tax values above SEK 1,595,000. To estimate price responses separately for the three groups, we adjust Model 1, by splitting the treatment indicator into separate dummies for each of the three treatment groups. The results are presented in Fig. 6 (Detailed regression results are presented in Table 5 in Appendix B).

Contrary to our expectations, we find no evidence of capitalization of the tax cut for any of the three groups. This is quite noteworthy, as we would expect prices to increase by about 13 percentage points, relative to the control group, in the treatment group containing the houses with the highest tax values. In all three treatment groups, the point estimates are quite precisely estimated and close to zero. It is only in the group containing the houses with the highest tax values that we can see a positive, however very small, point estimate. To investigate if the tax cut is capitalized in house prices only among the very most expensive properties, we split the original treatment group into three new treatment groups in the following way. We extract the top 1 percent expensive properties (according to tax value) as one group $T_{p(99)}$. The second group $T_{p(95-98)}$ consists of the top 5 percent, excluding the top 1 percent. The final group $T_{<p(95)}$ consists of the remaining treated properties, i.e. treated properties with tax values below the 95th percentile. The 95th percentile is SEK 2,271,000 and the 99th percentile is SEK 3,280,000. Price developments in these three groups and the control group were previously shown in Fig. 1, the theoretical effects were shown in Table 1 descriptive statistics in Table 2.

Again, in Fig. 7 we see no evidence of capitalization up to the 95th percentile of the market. However, in the most expensive groups of houses we do find evidence of partial capitalization. Among the houses with tax values in the 95th to the 98th percentile we estimate price increases of about 4 percentage points. While this effect is both statistically significant

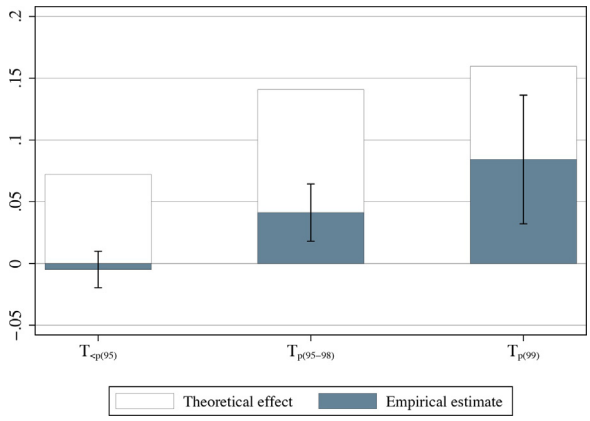

Fig. 7. Effect sizes and confidence intervals: top expensive groups. *Note*: Ranges indicate 95% confidence intervals. Point estimates from left to right: −0.00488, 0.0412, 0.0842, see Model 1 (bottom panel) in Table 5 in Appendix B.

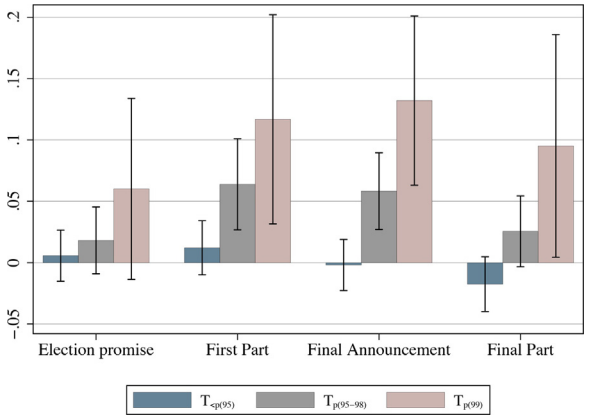

Fig. 8. Effect sizes and confidence intervals: top expensive groups. Note: Ranges indicate 95 % confidence intervals. Point estimates from left to right: 0.00573, 0.0181, 0.0601; 0.0122, 0.0639, 0.117; −0.0019, 0.0583, 0.132; −0.0176, 0.0255, 0.0952, see Model 1 in Table 6 in Appendix B.

and economically relevant, it is still a quite small effect compared with the theoretically expected increase of 14 percentage points. For the top one percent, we estimate a statistically significant price increase of 8 percentage points. A response of this magnitude is clearly economically important, but still only half of the theoretically expected 16 percentage points. However, as this segment of the market only contains 1 percent of the houses the group is naturally quite small, and hence the standard errors quite large. While we can reject full capitalization given current assumptions underlying the NPV calculations, minor changes in in the assumptions could change this conclusion. For instance, the obtained estimate for the most expensive group is consistent with a 25 year time horizon as opposed to our default of 50 years.

6.3. *Dynamic responses*

We now turn to the results from a more careful investigation of *when* price responses may have occurred. If prices increased already by the time of the election promise (second half of 2006), then the estimated price responses in Figs. 5–7 could be downward biased. The estimates could also be downward biased if prices increased only after the implementation of the final part of the reform (first half of 2008).

A visual inspection of Fig. 1 reveals that the groups evolve similarly up until January 2007, when the prices of the most highly valued houses suddenly increases more than the prices in the rest of the market. This suggest that prices did not respond at the time of the election promise and for the top segment the price increase shows up already in January 2007 and appears to stay at a higher level throughout the study period.

In order to formally estimate separate responses in different time periods, we split the study period of two and a half years into five time periods, each of six months:

- Control period (Jan 2006–Jun 2006)
- Election promise (Jul 2006–Dec 2006)
- First part (Jan 2007–Jun 2007)
- Final announcement (July 2007–Dec 2007)
- Final reform (Jan 2008–Jun 2008)

This split allows us to study price responses in all important stages of the total reform: from the first election promise in the summer of 2006, to the implementation of the final part of the reform in 2008. If house buyers anticipated the final details of the reform in the summer of 2006, we expect to see a reaction in the house price series of highly valued houses already before the implementation of the first part of the reform. If the implementation of the first part came as a surprise, or if individuals estimated that the risk was high that the Alliance would renege on their election promise, we expect to see responses only in the later periods.

The results of the formal tests, shown in Fig. 8, contain no surprises and confirms the conclusions from the visual inspection of Fig. 1. We find no evidence of capitalization in any segment of the market during the election promise period. Moreover, we find evidence of price increases in all post-periods for houses with tax values in the 95th to the 98th and in the 99th percentile. The estimated price response in those two groups are, however, smaller in the last period. This is entirely due to a fall in prices in the last month, which is visible also in Fig. 1. This could be a sign that the fall in house prices during the second half of 2008 started slightly earlier in the top segment of the market. For the other groups we find no evidence of capitalization in any period. Detailed regression results are provided in Table 6 in Appendix B. We therefore conclude that the analysis of the dynamics of the responses lend no support for the worries that the results from the previous analyzes would contain important biases.

7. Could the elasticity of supply explain our findings?

When calculating expected price responses from the reform (theoretical effects) we assume a fixed supply of housing. The fixed supply assumption is a simplification, and it gives us an informative upper bound on what to expect in terms of price responses. Our estimated price responses, however, seem to imply *the other* extreme in terms of supply, i.e. that housing in Sweden is perfectly elastic. A horizontal supply curve is the only shape that is compatible with the zero result we observe, given that actors on the housing market are rational. So is it really the case that housing in Sweden is perfectly elastic? At face value, land is abundant in Sweden. But land availability aside, there are crucial aspects of the Swedish housing market that put into doubt that housing supply is perfectly elastic.

The quotient between the market value of newly built houses and the corresponding construction costs (Tobin's Q) is a commonly used measure to analyze if an increase in demand would lead to increased investments in housing (Brainard and Tobin, 1968). If the market value is lower than the construction costs ($Q < 1$), it is simply not profitable to build new houses. Calculations by Swedish economists at the Institute for Housing and Urban Research at Uppsala University show that for most parts of Sweden the quotient is lower than one (in year 2006). This means that for large parts of Sweden supply is essentially fixed due to high construction costs relative to house values. In our sample, we have 77 percent of houses in the control group, and 15 percent of the treatment group, are sold in municipalities where new construction is not profitable.

In most areas where houses in the treatment group are located, financial incentives does not seem to hinder supply responses. But even if construction is profitable, supply is restricted by planning and regulation, the responsibility of which falls on the municipalities. Consider the fact that existing municipal residents face negative external effects of new construction; in terms of house price decreases and congestion. Municipalities that are politically accountable to existing residents therefore have incentives to keep construction to a minimum. Moreover, most of the additional income taxes that could be gained by expanding the housing stock and the population is redistributed to other municipalities in the so called "municipal equalization system" (Hüfner and Lundsgaard, 2007).

For legislative, political and economic reasons, supply of housing is more or less inelastic across Sweden.[16] Around medium-sized or smaller cities, where housing investments are profitable and where land is more available, housing supply is likely more elastic than in highly valued areas around larger cities, where land is more physically scarce, and residents in particularly affluent areas pay high prices for living in green areas close to the city center. This exclusive life style is maintained by municipal planning and legislation that protects certain areas with aesthetic and cultural values. Differences in supply elasticity could therefore explain a higher degree of capitalization in highly valued areas. But additional explanations are needed to explain the zero effect for most treated properties, since perfect elasticity of supply is highly unlikely on the Swedish housing market for the reasons discussed above.

8. Concluding remarks

For most of the properties on the Swedish market, we document a zero effect on prices from a substantial decrease in the property tax. Although the zero results are consistent with capitalization theory – if the supply of housing is perfectly elastic – we can conclude that perfect elasticity is very unlikely on the Swedish housing market, primarily because of restrictive legislation and the lack of municipal incentives to increase construction.

While there are several mechanisms that could explain why capitalization rates are lower than our theoretical benchmark, a fully satisfactory explanation should be able to explain both a zero effect for most of the market, as well as the partial capitalization result we find in the most exclusive segment of the housing market. A single such explanation is admittedly hard to find. For instance, the demand response to a national tax cut might be weaker than to a local tax cut, but arguably still existent. Similarly, while it is possible that some house buyers expected the tax to be increased again in the near future, we do not think it is likely that many expected an immediate policy reversal, which is required to explain the zero effects. Imperfections on the financial markets, such as crude bank lending policies that impose liquidity constraints on house buyers, are other potential explanation for lower capitalization. While supply and demand elasticities, expected policy reversal, and liquidity constraints could explain lower capitalization rates, each explanation cannot alone explain the zero result or the differential effects for different segments of the market.

What characterizes the most exclusive segment of the market, for which we find positive capitalization, is that: (1) the houses are situated in places where land is physically scarce, (2) the owners received a large tax reduction, and (3) the buyers of these houses are likely to be more financially literate. In other words, restrictions on new construction in exclusive areas, the saliency of the reform, and the ability to calculate the net-present-value from a decrease in the property tax could explain the differential effects. Since these factors are highly correlated, we cannot empirically separate them and hence not conclude which is most important. Moreover, the factors might interact, or partially contribute to the results.

Finally, it is worth commenting on the time frame of our investigation. While any information about a change in the property tax is expected by theory to be immediately capitalized into house prices on an efficient housing market, many of the potential explanations for lower capitalization discussed above may simply cause price adjustments to be slower, rather than not to materialize at all. Therefore, we cannot rule out the possibility that the property tax was eventually capitalized,

[16] Hilber and Vermeulen (2016) find that capitalization vary because of local differences in regulation, land scarcity and geography.

although extending the analysis beyond the first half of 2008 is very problematic since the financial crisis substantially affected the number of properties on the market and the sales prices of these properties.

Appendix A. The first part of the reform

A.1 The first part of the reform

The temporary land tax reform was introduced in 2007 as a stepping stone towards the more comprehensive final reform. In this section we show that the properties that were generously affected by the first part of the reform were the same properties that were generously affected by the final reform.

The first part of the reform implied that the tax on the land part was reduced to SEK 5000 or SEK 2 per m^2 depending on which one yielded the lowest tax payment. The tax stayed the same if the new rules did not yield a reduction in the tax. Of all the properties in our sample, 56 percent were treated with a tax reduction due to the temporary land tax reform. Before the first part was implemented, the treated properties paid a yearly tax of SEK 13,322 on average, and after the first part the yearly tax was decreased to SEK 9620; the average yearly gain from the first part of the reform was thus SEK 3702. The same properties went on to have an additional decrease in yearly tax liabilities of SEK 3950 when the final reform was implemented.

In Fig. 9 we show how the yearly gain of the first part of the reform correlates with the property tax value and compare it with the gain from the final reform. It is clear from Fig. 9 that the gain of first part was directed to the same kind of (expensive) properties as the final reform, only to a smaller extent.

A.2 The 2008 financial crisis and the housing market

In this section we illustrate how the financial crisis of 2008 affected the housing market. In Fig. 10 we show the evolution of the aggregate price index in our sample over three years, including the last part of 2008 when the financial crisis hit. In the autumn and winter of 2008 prices reached their lowest level during the time period that we study. In Fig. 11 we show

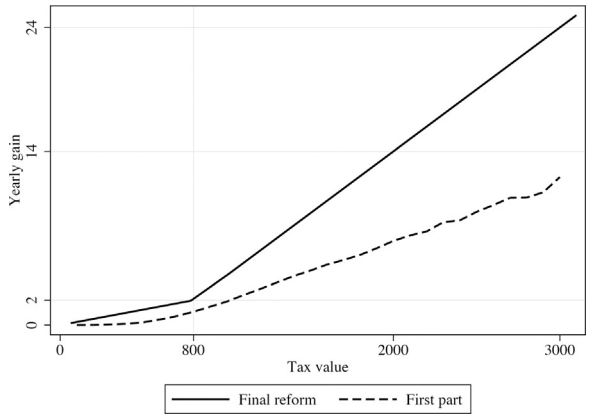

Fig. 9. Yearly gain: comparison between two parts of the reform. *Note*: The unit is SEK thousand.

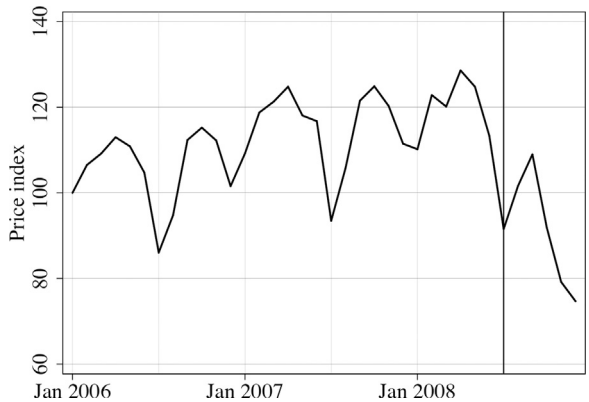

Fig. 10. Market price.

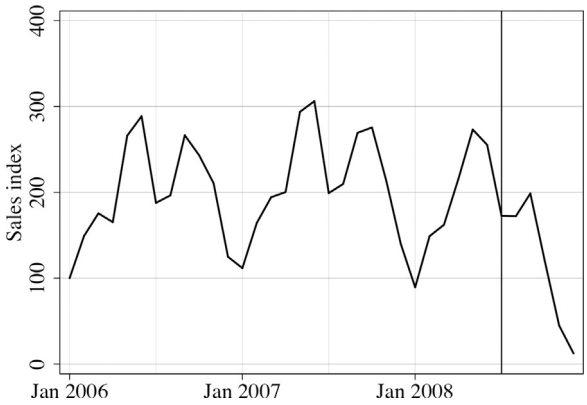

Fig. 11. Number of sales.

Table 3
Number of observations by group and time period.

Time period	Full	Control	Treated	T_1	T_2	T_3	$T_{p(95-98)}$	$T_{p(99)}$
2006h1	19,047	9515	9532	3019	3155	3358	876	217
2006h2	20,459	10,883	9576	3280	3171	3125	767	216
2007h1	21,148	10,817	10,331	3344	3414	3573	871	235
2007h2	21,749	11,765	9984	3522	3312	3150	783	170
2008h1	19,046	9770	9276	3066	3167	3043	770	177
2008h2	12,003	7491	4512	1685	1531	1296	298	56
Sales drop %	45	37	55	53	54	59	62	68

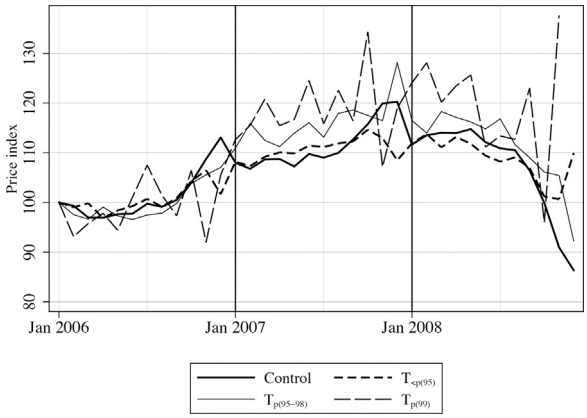

Fig. 12. Timing of events and price development of houses in control and treatment groups: including financial crisis. *Note*: The data is collapsed on monthly level and seasonally adjusted with group specific calendar month dummies.

an index of the number of sales, and this figure shows a dramatic fall in the number of sales at the end of 2008. The number of sales practically reaches zero in December 2008.

In Table 3 we show the drop in sales in each treatment group in 2008h2 compared to the same period the year earlier 2007h2. As can be seen in Table 3 the sales drop increases monotonically the more expensive the group is. This could mean that the financial crisis affected treatment and control groups in different ways.

Fig. 12, which is analogous to Fig. 1, shows the development of house prices up until the end of 2008. The limited number of sales in the end of 2008 contributes to imprecise estimates of the calendar month fixed effects over the entire period. Results based on the inclusion of the second half of 2008 (available upon request) are very similar to the estimates presented in this paper. However, because of the doubts on the validity of the parallel trend assumption, we find estimates obtained from analyses excluding the second half of 2008 to be, a priori, more credible for the purpose of this study.

A.3 Sample representativeness

Our sample of observations covers 52 percent of the observations of the relevant population. The reason for why we cannot use the full population is that we lack some information on these observations, most importantly the contract signing date.

Table 4
Representativeness.

	(1) Our sample	(2) Remaining observations
Tax value	959	851
Market price	1778	1436
Living area	115	112
Total areal	1473	1772
Standard points	29	28.1
Construction year	1963	1961
Share urban areas	0.27	0.24
Share northern region	0.11	0.15
Observations	101,449	93,863

Note: Prices and values are expressed in SEK thousand prices. Mean values unless otherwise stated.

Table 5
Baseline results.

	Model 1	Model 2	Model 3	Model 4
Estimation with one treatment group				
Post period × Treated	−0.00535	−0.00616	−0.00119	−0.00429
	(0.008)	(0.008)	(0.006)	(0.005)
Estimation with three equally large treatment groups				
Post period × T_1	−0.00412	−0.00234	0.00118	−0.00489
	(0.007)	(0.007)	(0.006)	(0.005)
Post period × T_2	−0.00505	−0.00838	−0.00955	−0.0100
	(0.007)	(0.007)	(0.007)	(0.005)
Post period × T_3	0.00985	0.00871	0.00713	0.00332
	(0.009)	(0.009)	(0.008)	(0.007)
Estimation with top expensive groups				
Post period × $T_{<p(95)}$	−0.00488	−0.00618	−0.00466	−0.00752
	(0.008)	(0.008)	(0.006)	(0.004)
Post period × $T_{p(95-98)}$	0.0412***	0.0396**	0.0371**	0.0254*
	(0.012)	(0.013)	(0.013)	(0.012)
Post period × $T_{p(99)}$	0.0842**	0.0862**	0.0849***	0.0693**
	(0.027)	(0.028)	(0.025)	(0.025)
Observations	101,449	101,449	101,449	101,449
Controls		√	√	√
Municipality fixed effects			√	
Parish fixed effects				√

Note: Dependent variable is log market price. The following controls are included where indicated: living area, total area, standard points, construction year, and monthly dummies. Clustered standard errors on municipality level in parentheses. The number of clusters is 288. Sizes of treatment groups is the following: Control: 52750, Treated: 48699, T_1: 16231, T_2: 16219, T_3: 16249, $T_{<p(95)}$: 43617, $T_{p(95-98)}$: 4067, $T_{p(99)}$ 1015.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.

Only those sales that are conducted with the help of a real estate agents that report statistics to Svensk Mäklarstatistik are included in our estimating sample. In Table 4 we report the average differences between the properties in our sample and remaining observations in terms of relevant characteristics. The properties in our sample are somewhat more expensive and are more often situated in urban areas, whereas properties among the remaining observations are more often situated in the northern region of Sweden.

Appendix B. Detailed results

Appendix C. Discussion of assumptions

C.1 Validity of the parallel trend assumption

The central assumption for our estimates of the property tax cut to be unbiased is the parallel trend assumption. We assess this assumption in several ways. First, a visual inspection of Fig. 1 suggests that the evolution of house prices in the different groups follow similar trends before the property tax was reduced. In fact, prices of most the houses follow the same trend also after the tax cut. Second, the results in Fig. 8 and in Table 6 contains a test of the parallel trends during 2006. Those results support the parallel trend assumption since we found no statistically significant differences in the house prices changes between the first and second half of 2006. Third, we divide the control group into two groups. One group

Table 6
Dynamic responses.

	Model 1	Model 2	Model 3	Model 4
Election promise × $T_{<p(95)}$	0.00573	−0.00426	0.00255	0.00333
	(0.011)	(0.010)	(0.009)	(0.007)
First part × $T_{<p(95)}$	0.0122	0.00497	0.00785	0.00408
	(0.011)	(0.011)	(0.009)	(0.007)
Final announcement × $T_{<p(95)}$	−0.00190	−0.00848	−0.000892	−0.00403
	(0.011)	(0.011)	(0.009)	(0.007)
Final reform × $T_{<p(95)}$	−0.0176	−0.0230	−0.0186*	−0.0188**
	(0.011)	(0.012)	(0.009)	(0.007)
Election promise × $T_{p(95-98)}$	0.0181	0.00885	0.0104	0.000577
	(0.014)	(0.016)	(0.015)	(0.014)
First part × $T_{p(95-98)}$	0.0639***	0.0537*	0.0592**	0.0401*
	(0.019)	(0.021)	(0.021)	(0.018)
Final announcement × $T_{p(95-98)}$	0.0583***	0.0567**	0.0496**	0.0288
	(0.016)	(0.019)	(0.018)	(0.016)
Final reform × $T_{p(95-98)}$	0.0255	0.0190	0.0150	0.00621
	(0.015)	(0.016)	(0.015)	(0.015)
Election promise × $T_{p(99)}$	0.0601	0.0227	0.00608	0.0108
	(0.038)	(0.040)	(0.035)	(0.034)
First part × $T_{p(99)}$	0.117**	0.0953*	0.0894*	0.0730*
	(0.044)	(0.044)	(0.038)	(0.032)
Final announcement × $T_{p(99)}$	0.132***	0.120**	0.126**	0.123**
	(0.035)	(0.042)	(0.039)	(0.041)
Final reform × $T_{p(99)}$	0.0952*	0.0803	0.0500	0.0320
	(0.046)	(0.049)	(0.034)	(0.039)
Observations	101449	101449	101449	101449
Controls		✓	✓	✓
Municipality fixed effects			✓	
Parish fixed effects				✓

Note: Dependent variable is log market price. Independent variables are group and time specific interactions. The following controls are included where indicated: living area, total area, standard points, construction year, and monthly dummies. Clustered standard errors on municipality level in parentheses. The number of clusters is 288. Sizes of treatment groups is the following: $T_{<p(95)}$: 43617, $T_{p(95-98)}$: 4067, $T_{p(99)}$ 1015.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.

contains houses with tax values under SEK 400,000 and the other group house with tax values between SEK 400,000 and 800,000. Fig. 13 shows that the evolution of house prices in these two groups follow the same trend for the entire period that we study. Using Models 1–4 to formally test for placebo DiD responses between the two groups, result in statistically insignificant differences (available upon request). Taken together, we have found no serious indications that the parallel trend assumption is not valid in our case.

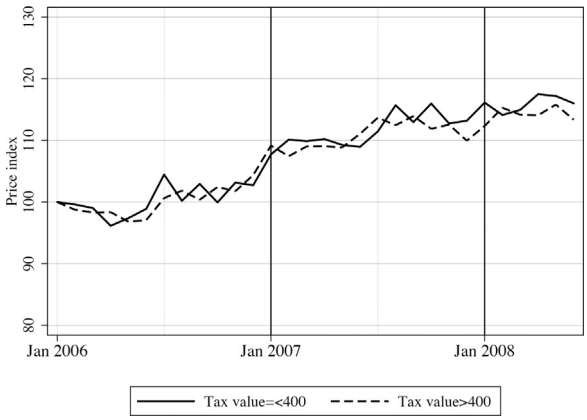

Fig. 13. Timing of events and price development of houses with different tax values. *Note*: Tax values are expressed in SEK thousand. The data is collapsed on monthly level and seasonally adjusted with group specific calendar month dummies. Jan07 marks the introduction of the first part of the reform and Jan08 the final part of the reform.

Table 7
Compositional effects.

	(1)	(2)
Living area	−0.779**	−0.736**
	(0.295)	(0.278)
Total area	26.10	24.61
	(16.737)	(16.408)
Standard points	−0.0427	−0.0462
	(0.039)	(0.037)
Construction year	0.276*	0.377**
	(0.135)	(0.123)
Observations	101,449	101,449
Municipality fixed effects		√

Note: Left column indicates dependent variable, and columns (1) and (2) show estimates for independent variable, which is post-period compared with pre-period. Column (1) is a simple OLS regression, and column (2) adds municipality fixed effects. Clustered standard errors on municipality level in parentheses. The number of clusters is 288.

* $p < 0.05$.
** $p < 0.01$.

C.2 Compositional effects

One could be worried that there are endogenous supply and compositional responses to the reform. In Table 7 we show that the characteristics of the properties sold do not change substantially from the pre-reform to the post-reform period. A change in living area of $-0.779\,m^2$ is only 2 percent of a standard deviation. The change in total area is not statistically significant and is only 1 percent of a standard deviation. The change in standard points is not statistically significant and is only 0.8 percent of a standard deviation. Lastly, the change in construction year of about three months is only 1.3 percent of a standard deviation.

C.3 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 three different policy changes may have affected demand for different properties and to what extent they could confound our estimates. The three policy changes are:

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

While the introduction of a higher capital gains tax could potentially introduce a downward bias, the abolition of the wealth tax could potentially introduce a small upward bias. However, after examining these policy changes we conclude that the each bias is small.

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 SEK 2 million 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 proportional return 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 The Swedish Tax Authority, 2008). Our assumption is thus that all gains belong to buyers of houses in the 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 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 2500 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.

17. 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 bill for all employed people. The absolute level of the tax credit increased with income up to an income cap at SEK 318,000 per year, at which the tax reduction was SEK 11,200. It was thus people with lower incomes who benefited most extensively in proportional terms. If spending on housing is a fixed proportion of disposable income, the demand for housing would increase relatively more for lower income households due to the tax credit. This can potentially add a negative bias to 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 each with a median yearly income among blue collar workers in the private sector (SEK 200,000 net of tax). The high income household consists of two adults each with a yearly median income among white collar workers in the private sector (SEK 240,000). Lets assume that households buying a house spend 30 percent of their net income on housing,[17] 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. The tax credit increased yearly disposable income for the blue collar workers with SEK 8900, and for white collar workers with SEK 10,700. Keeping the relative spending on housing constant at 30 percent, this would lead to price increases of around 4.5 percent for houses bought by both households.

It is clear from the example above that the EITC should have had nearly identical effects on the price development of houses typically bought by different income groups. Partly this is because the Swedish net of tax income distribution is pressed together, and also because of the EITC design. But let us take a more extreme example and exchange the high income family to a family with two adults, each with median income among *university educated* white collar workers in the private sector. This family has an even 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 are estimated to potentially have increased by 4 percent as an effect of the tax credit. Hence, even with this more extreme comparison the negative bias would be 0.5 percentage points only. Our conclusion is thus that we do not expect the introduction of the EITC to lead to anything but perhaps a slight negative bias of our estimated effects.

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. Our estimates would then contain a positive bias. 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 also partially exempted from the wealth tax. 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 prices of the vast majority of lower valued houses. An upward bias in the estimates in the segments of the most highly valued houses is possible, but hard to quantify.

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

Appendix D. Regression kink design

As a consequence of the reform the property tax schedule went from being linear to being kinked. The annual tax payment is a deterministic function of the tax value, and a sharp RKD is therefore suitable in this case. Since tax payments are determined by the tax value, we expect the relationship between the tax value and the market price to change at the kink. We show in Figs. 14 and 15 what the actual relationship between these two variables look like. Fig. 14 shows the relationship in a smaller bandwidth around the kink, while Fig. 15 shows the relationship over the full sample, truncated at tax value 3,000,000. The two figures show no change in the slope around the kink. As the bandwidth is extended farther and farther from the kink, the slope somewhat decreases. The decreasing pattern can be seen also in other years (not shown), and reflects the fact that the tax value as share of the market price is typically higher for more expensive properties. We insert in the two figures an estimation of the change in slope that is theoretically expected under full capitalization; the change in slope is quite small and effects will be more difficult to detect the closer we get to the kink.

We continue to show several RKD-estimates based on a linear model only, since the underlying relationship between market price and tax values is linear (through the tax assessment system). We estimate the following equation:

$$Y_i = \mu + \gamma(TV_i - k) + \delta(TV_i - k)\cdot D + \varepsilon_i,$$
$$\text{where} \quad |TV - k| \leq h,$$

where TV is tax value, Y is market price, k is the tax value at the kink, D is a dummy variable for being above the kink, h is the bandwidth, and ε_i is the error term. Our parameter of interest is δ which is the change in slope between tax value and market price for properties above the kink, compared to the slope γ for properties under the kink. In Figs. 16 and 17 we show RKD estimates and confidence intervals for a range of different bandwidths. The estimations are based on time period 2008h1, when the new tax schedule was adopted as law. As we move close to the kink, the number of observations naturally decreases. Estimates where $h < 50$ are extremely imprecise, and estimates where $h = 50$ are also very imprecise due to the small sample (1153 observations). Estimates close to the kink cannot reject very high degrees of capitalization, while they at the same time cannot reject zero capitalization. According to our NPV assumptions and calculations, SEK 1 in tax reduction should lead to SEK 50 increase in market price. This theoretical estimate implies a change in slope between tax value and market price of 0.375 (50×0.0075, where 0.0075 is the change in the deterministic tax schedule slope). The expected change

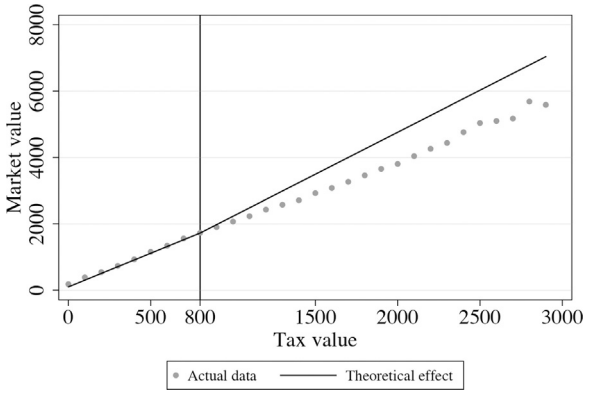

Fig. 14. Treatment period: Jan 08–Jun 08. *Note*: size 100 bins.

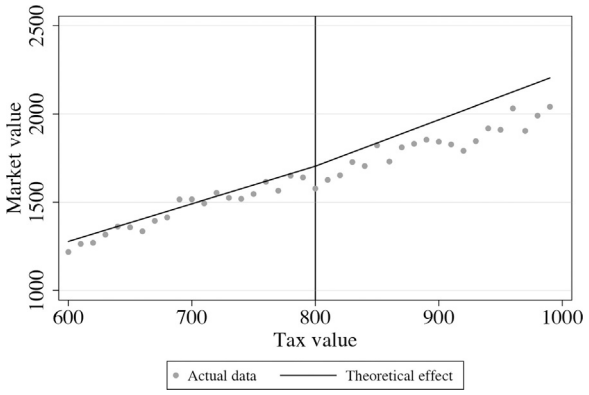

Fig. 15. Treatment period: Jan 08–Jun 08. *Note*: size 10 bins.

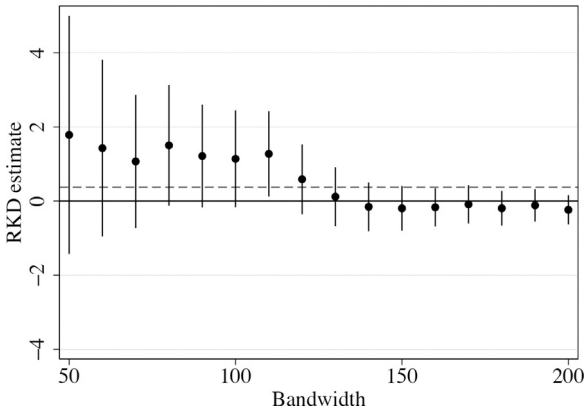

Fig. 16. Treatment period: Jan 08–Jun 08. *Note*: Linear RKD, standard errors are clustered on municipal level; number of observations: 1153 ($h = 50$), 2266 ($h = 100$), 3453 ($h = 150$), 4599 ($h = 200$).

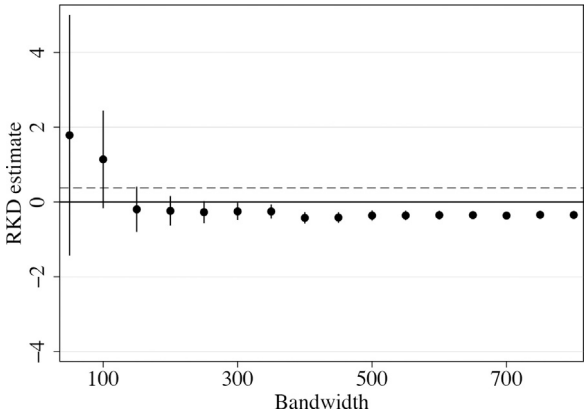

Fig. 17. Treatment period: Jan 08–Jun 08. *Note*: Linear RKD, municipal level; number of observations: 6915 ($h = 300$), 12,024 ($h = 500$), 15,452 ($h = 700$).

in the slope is included in the figures as a dashed line. Estimates at this line indicate full capitalization. From Figs. 16 and 17, it is clear that with narrow bandwidths the confidence intervals include both capitalization rates that are several times our theoretical benchmarks and zero (or even negative capitalization rates). Once we extend the bandwidth, the precision increases and estimates approach zero and for very large bandwidth becomes negative. Although we only show estimates without controls and without fixed effects, the estimates change somewhat when these are included; some estimates draw closer to zero, and the estimates become somewhat more precise.

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