Homes for votes: prospective voting and housing privatization

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In this paper we study the impact of housing privatization on voting. We use a "right to buy" programme in Stockholm, Sweden, in which municipal homes were sold to tenants at a discount, that allows us to evaluate theories of voting behaviour and tactical privatizations. We exploit the variation in programme participation in electoral districts and use first-difference estimation to identify the pre- and post-policy effects on the support for the centre-right bloc that implemented the policy. Results indicate that the programme had a pre-policy effect on electoral support for the centre-right bloc in the 2006 local election. In districts affected by the policy, the local centre-right bloc increased its vote share by on average 0.48 percentage points. This result is consistent with theories of prospective, self-interested and informed voting. We do not, however, obtain statistically significant estimates of the post-policy effect on voting that support the models of strategic privatizations and transfers. Moreover, we do not find support for the model, which predicts that politicians use policy to reward their core supporters.

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"What I am desperately trying to do is to create one nation with everyone being a man of property …"[1]

Margaret Thatcher, 1983

How does policy interact with voter behaviour? The economic literature puts forward fundamentally different views on the motives and decision-making of voters and politicians. Whether politicians are predominantly driven by ideology, or a desire to maximize their popularity, makes a significant difference in theoretical models of electoral competition. Similarly, voters' motivations and ability to trust campaign promises is key in understanding the link between elections and policy.

Conventional models of electoral competition suggest that elections should be seen as a race between vote-maximizing parties who compete for the votes of rational, informed and forward-looking agents (Congleton, 2002). The immediate proposition of such models is that politicians will move towards a political middle ground in order to attract as many votes as possible. This gives rise to the well-known median-voter theorem. Later models have explored the implications of similar approaches to redistribution policy, suggesting that politicians can use transfers tactically to sway votes from certain groups (Lindbeck and Weibull, 1987). Such results formalize the type of behaviour, which is sometimes called *vote buying*, or *pork barrel*.

Another view suggests that politicians cannot, due to their ideological platform, make unrestrained promises that are credible in the eyes of the voter. Politicians may well desire votes and incumbency, but once in power there is nothing to stop them from implementing their preferred policies. Voters thus view campaign promises as a form of cheap talk. This view on politics implies that competition among political parties will give rise to diverging policies, rather than a collective move among parties toward a middle-ground platform (Alesina, 1988). The politicians' option in such an environment is then to attract voters to their own, rigid platform by using policy to change the incentives of the political process (Milesi-Ferretti and Spolaore, 1994). It has been suggested that right-wing governments can use voucher privatizations as a means to this end, by increasing voters' stakes in the market economy (Biais and Perotti, 2002).

In this paper we consider the effects on electoral support of a "right to buy" (RTB) privatization programme in Stockholm, Sweden. In the Stockholm RTB programme, some 21,000 municipal apartments were sold to tenants at a discount between 2007 and 2010. The programme was part of the second wave of an effort on the part of the

[1] In Jones (2002) p.60

centre-right local government to increase home ownership in the city. Its implementation was made possible by the election of the centre-right Alliance into national and local office in 2006.

Over the last decades, a majority of Western economies have seen a substantial rise in home ownership rates. While this in part can be explained by fundamental changes in housing and financial markets, a significant share of the rise in home ownership has been the result of active public policy (Andrews and Caldera Sánchez, 2011). One of the defining policies of the Thatcher era was indeed the right to buy programme, in which millions of tenants in the UK council housing bought their homes at a discount. Conservative and centre-right governments in e.g. France and Sweden have later introduced similar policies. Similarly, conservative governments in other countries have pushed for increased home ownership, using other tools. The "Homeownership Challenge", introduced by the George W. Bush administration, is one such an example.

Promoting home ownership appears as an interesting policy in light of the model of Biais and Perotti (2002), who suggest that privatizations might be a tactic for right-wing governments to permanently tie median-class voters to their platform. Friedrich Engels already in the 1800s argued that as soon as the worker becomes a homeowner, he ceases to be part of the proletariat (Engels, 1935). The political preferences of the homeowner are still, at least according to conventional wisdom, believed to lean more towards the right.

Moreover, the "right to buy" policy is interesting in light of the literature on tactical redistribution (i.e. Lindbeck and Weibull, 1987, Dixit and Londregan, 1996, Cox and McCubbins, 1986). Buying a home at a discount in a public right to buy programme is likely to make up a considerable indirect transfer from the government to participating households. The total contract value of the apartments sold in Stockholm between 2007 and 2010 in the Stockholm RTB programme exceeded EUR 3,7bn (Sjölin, 2012). Typically, transactions involve a significant, implicit rebate to tenants.

Drawing from these strands in the political-economy literature, the empirical part of this paper will focus on the decision making of voters in relation to the Stockholm RTB programme. In order to shed light specifically on the ability of voters to behave informed and forward-looking in elections, and politicians' ability to attract votes in the medium term by manipulating parameters in voters' objective functions, we test two research hypotheses on data from electoral districts in Stockholm:

(i) The RTB programme had a (positive) pre-policy effect on electoral support for the centre-right Alliance.
(ii) The RTB programme had a (positive) post-policy effect on electoral support for the centre-right Alliance.

Additionally, we look at the groups of voters, which were targeted in the programme in order to shed light on the possible strategic behaviour of politicians. We thus test two secondary research hypotheses:

(iii) The RTB programme targeted low-income households.
(iv) The RTB programme targeted core voters.

The first hypothesis thus relates to conventional models of political competition suggesting that voters are rational and forward-looking (prospective). The second hypothesis, in turn, relates to different models conjecturing that voters react rather to state variables and past events (and thus are retrospective). The third and fourth hypotheses, in turn, draws from results in Lindbeck and Weibull (1987) and Cox and McCubbins (1986) who propose different strategic consideration on the part of politicians.

Using first-difference estimates on electoral district data, we find a significant and positive pre-policy effect of RTB policy participation on the centre-right bloc vote share in the local election. Under our identifying assumptions, the centre-right Alliance in Stockholm thus managed to sway votes in the 2006 election from tenants who later bought their homes in the RTB programme. This result indicates that voters were indeed self-interested, informed and forward-looking when voting. We find negative but insignificant post-policy effects of RTB policy. The Stockholm right to buy policy does thus not, in the medium term, seem to have affected voters' ideology or other state variables which influence voter behaviour. Lastly, we find that the policy did not appear to target low-income areas, but areas were support for the centre-right Alliance was relatively low.

I. Literature review

When considering the links between policy and voting behaviour, several distinct views on the voter emerge. In this section we will first outline some key theoretical concepts in the political economy of electoral competition that may contribute to the understanding of the empirical tests we will later perform. We will also discuss some pieces of evidence on the various political economy theories, and some empirical facts about home ownership.

Forward-looking voters

The common framework in analysing majority decision-making has been, at least since Downs (1957), to consider politicians to be completely opportunistic and voters rational, materialistic and forward looking. In this class of models, the median voter theorem is central (Congleton, 2002).

Lindbeck and Weibull (1987) draw from the idea of the rational and informed voter and suggest a theoretical model of tactical redistribution as an outcome of electoral competition. In these types of models of pre-electoral competition with two political parties and probabilistic voting (see also e.g. Dixit and Londregan, 1996 and Persson and Tabellini, 2000), the voter typically maximizes a concave utility function that includes consumption and a political bias. Politicians are assumed to be opportunistic vote-maximizers that are able to make politically unrestrained promises to voters. Facing an election, voters observe campaign promises and vote for the option that gives them the greatest utility.

The key result of such models is that redistribution may work to sway certain voters. Politicians will find it particularly worthwhile to "buy" votes from individuals with a low income, because of their high marginal utility from consumption. Moreover, transfers will have a greater impact on election outcomes when they are targeted toward voters with a soft political bias, so-called swing voters (Lindbeck and Weibull, 1987). A contrasting result is obtained by e.g. Cox and McCubbins (1986). The additional assumption in their model is that voters with similar characteristics form political collectives, in order to advance their political objectives. These political collectives make up significant portions of the support for a politician; they will stay as supporters as long as the politician meet their objectives. A risk averse politician will avoid losing any such group of core supporters, and therefore instead focus transfer toward areas where support is already high.

Ideology-constrained politicians

Contrary to the proposition of the median-voter theorem, many political systems appear to be influenced by diverging policies in political parties, rather than policy convergence, the U.S. system being a prominent case of that (Poole and Rosenthal, 1984). This fact is explained by models suggesting that political parties, which themselves embody certain preferences, cannot commit to a tactic middle ground platform prior to an election (Alesina, 1988). Generally, political parties depend on their constituencies, and politicians are therefore trapped in an electoral dilemma: They have an incentive to announce a middle-ground platform prior to an election in order to maximize their popularity. However, once elected, the party wishes to implement their preferred policies and there are no mechanisms to stop them from doing so. When voters take this cheap talk dimension of campaign promises into account, politicians cannot credibly commit to middle ground agendas. The outcome is instead policy divergence (Alesina, 1988).

Close to this view is the notion of citizen candidates. The approach in such models, in Besley and Coate (1997), is to model politicians' motives differently. The three stages in the setting are as follows. First, citizens choose whether to become candidates or not, in order to represent themselves. Consequently, there is no automatic assumption about politicians' wish to maximize their popularity. Second, citizens elect the most popular candidate. Third and lastly, the candidate chooses and implements policies. This set-up implies that candidates cannot credibly commit to any other policy than he or she wishes to implement; voters know that they are candidates because they have political preferences, and therefore they will not automatically represent a middle ground platform when in government.

Voter manipulation and Machiavellian privatization

If pre-electoral promises are not credible in the eyes of the voters, because of the insufficient mechanisms to enforce promises, politicians are quite constrained in their prospects to sway votes. A conservative party cannot credibly promise to e.g. extend social welfare programmes to groups of swing voters, due to their political aversion to actually carrying out this promise in government.

Models drawing on the policy-divergence view of Alesina (1988) have suggested instead that governments use policy to manipulate political incentives and the interests of voters in order to retain power. Milesi-Ferretti and Spolaore (1994) present a model in which the incumbent government can change the conditions under which future governments are able collect funds, in order to constrain politics in the future. A similar model is proposed by Besley and Coate (1998).

In line with this view are Biais and Perotti (2002), who suggest a model of *Machiavellian privatization*. Motivated by the large-scale voucher privatizations carried out in post-communist central Europe, they present a model in which an incumbent right-wing government uses privatizations to align the interest of voters with their own platform. Voucher privatization schemes consist, in essence, of the transfer of shares in state-owned enterprises into the hands of citizens. Assuming that the voter's objective function contains consumption, and that consumption is affected by the performance of these stocks, such reforms potentially have an impact on voting. Assuming, additionally, that a left-wing government may affect stock values and returns adversely, right-wing governments can shift incentives toward their own platform by implementing voucher privatizations.

Uninformed voting

Motivated by empirical findings on the link between economic performance and support for the incumbent government (e.g. Kramer, 1971), yet a different theoretical view suggests that voters are rational but remain imperfectly informed about the quality of politicians. In a model by Rogoff, (1990), voters facing an election are able to infer the competency of politicians in government only from readily observable proxies, such as the current tax level and the status of public goods. Similarly, voters are unable to judge the abilities of opponents in an election. This type of voter thus becomes sensitive to prior changes in household finances and the macro economy. Such behaviour has become associated with the notion of *retrospective voting* (Fiorina, 1978).

Behavioural effects and voting

Experiments have shown that reciprocity, a tit-for-tat mechanism in social relations, have considerable implications to e.g. the labour market (Gneezy and List, 2006). Finan and Schechter (2011) suggest that there is a similar connection between reciprocity and vote buying in the literal meaning of pre-election cash for votes. In light of their empirical findings of post-policy effects on political support from a cash transfer programme in Uruguay, Manacorda et al. (2011) discuss the possibility that retrospective voting may partly be driven by reciprocity under certain circumstances.

Empirical evidence on the nature of electoral competition

A number of empirical studies provide support the propositions of the framework of tactical transfers and voting. Dahlberg and Johansson (2002) look at a temporary grants program in Sweden in order to identify tactical motives of the government,

recalling that the Lindbeck-Weibull model suggests that politicians wish to target transfers to swing-voters and low income voters. While the latter group of voters is straightforward to identify – the former, swing-voters, is not. Dahlberg and Johansson consider several proxies for the density of swing voters in different areas, and then estimate the likelihood of receiving grants in an area. They find strong support for the hypothesis that transfers are targeted to swing-voter areas. Case (2001) uses a similar method to look at grants distribution in Albania and finds similar results.

Turning to the behaviour of voters, Elinder et al. (2008) document forward-looking responses to campaign promises, using difference-in-difference estimation on survey data from Sweden. They find that parents with young kids, facing a campaign promise on the part of the Social Democratic government to cap the fees on childcare, reacted prospectively. Elinder et al. (2008) also measure retrospective voting in the same group, i.e. if support for the government increased after the reform, but find no evidence of this. Manacorda et al. (2011) document the effects of a cash transfer programme in Uruguay, and find instead a post-policy effect on political support. They use a regression discontinuity design that exploits the eligibility rule for the programme, and measure support for the government in a series of surveys among programme participants close to the cut-off. Recipients showed an increased support for the government and for transfer policies in general (Manacorda et al., 2011).

With regard to the evidence of policy divergence (drawing from e.g. Alesina, 1988), Lee et al. (2003) show that Democratic and Republican members of the U.S. congress have highly diverging voting records even when the election margin is very narrow. Using a regression discontinuity design, they show that the voting records of candidates are independent of the electoral pressure towards the middle ground in politicians' home districts. Politicians thus do not seem to respond to electoral competition by adjusting their policies, supporting the theory of ideology-constrained politicians.

A related, considerably broader area of empirical research concerns the link between economic performance and support for the incumbent, which is informally connected to the theory of uninformed voters. Notably, Kramer (1971) identified such a relationship. Lewis-Beck and Paldam (2000) provide a summary of this empirical literature. Overall, one third of the changes in votes appear to be attributable to prior economic changes. Voters appear to react primarily to changes in inflation and unemployment, and they do so more to previous periods rather than to expectations of the future. (Lewis-Beck and Paldam, 2000). Jordahl (2006) conduct a test of individual voting that distinguishes between macro and micro changes in a panel of

Swedish data. The results suggest that Swedish voters do react backward looking to economic changes, both on the macro level and the micro level. Related to the idea of uninformed voting is also an empirical body of literature suggesting that political support is sensitive to completely arbitrary prior events, such as shark attacks, giving rise to the term *blind retrospection* (Achen and Bartels, 2004).

The effects of being a homeowner

Home ownership has been documented to have important behavioural and microeconomic effects, being an important component of households' wealth and a factor in determining mobility, savings and labour force participation (Dietz and Haurin, 2003). There are, however, significant challenges in identifying these effects in practice. For instance, households might select their housing status depending on the need to be mobile. Moreover, renters might be influenced by their plans to become homeowners in the future when deciding how much to save and work today. Attempts to separate the true effect of being a homeowner are therefore quite constrained by endogeneity and the presence of unobserved characteristics of the household. Without trying to provide a full picture of this debate, we will highlight a few points on the effects of home ownership that appear relevant.

Findings that demonstrate lower mobility among homeowners appear to be robust to selection effects. Haurin and Gill (2002) demonstrate that homeowners are less likely to move than tenants, even when expectations on the need to move in the future are held constant. Lundberg and Skedinger (1999) demonstrate that taxes on housing transactions make homeowners less likely to move. This result supports the idea that higher costs of moving is the primary reason for greater immobility among homeowners.

Reduced mobility among homeowners has effects on other outcomes. Green and White (1997) and Aaronson (2000) document a significant impact of home ownership on schooling of children, arguing that this effect is linked to the stability of the household. DiPascale and Glaeser (1999) document, using survey data, that homeowners also invest more in social capital overall.

Homeowners, having a significant share of their wealth locked in housing, are in theory also incentivised to maximize the value of their home. This has given rise to the hypothesis of home voting, proposing that homeowners use their vote to influence home values. Dehring et al. (2008) study voting in a public referendum in Texas and demonstrate that homeowners were more likely to favour a public works project that was perceived to increase residential property values.

Although it is a widely held belief that home ownership is a source for certain political preferences, the evidence for a causal link is limited. Results from Gilderbloom and Markham (1995) indicate that the effect on political attitudes is minimal. Kingston and Fries (1994) and Kingston et al. (1984) find an insignificant link between home ownership and party identification and conservative ideology among households.

Ansell (2013) suggest instead that there is a link between home ownership and political attitudes, which works through the equity value of the home. In this theory, household assets, including the home, provide a personal stability that substitutes the demand for government-provided social security. The author's empirical results demonstrate that growing household values indeed have a negative impact on the support for social policy on the micro level.

II. Conceptual framework: The political-economy of RTB

The literature we have just discussed allows us to draw two separate hypotheses from the theory about voter behaviour. Broadly, we can say that the RTB programme may have had a pre-policy effect and a post-policy effect on electoral support. These hypotheses will later be the focus when we move on to the empirical analysis. A secondary evaluation will consider the possible strategy of the government. Specifically, we test if the RTB programme targeted any particular group of voters. In the following section we elaborate briefly on how theory relates to these possibilities.

Pre-policy electoral effects

If voters primarily behave according to conventional models of electoral competition, namely that they are rational, self-interested and prospective, RTB should have a pre-policy influence. Put simply, voters, before casting their vote, took note of the different campaign promises, evaluated the impact on themselves and trusted that promises would be kept. In these models, there is then no evident reason for the voter to take past events into account.

Post-policy electoral effects

We consider also the possibility of RTB having had a post-policy impact on electoral support. Generally, the literature provides three explanations of such an outcome: (i) uninformed voting, (ii) reciprocal voting and (iii) state-variable sensitive voting (in line with the *Machiavellian privatization* hypothesis).

Explanation (i) suggests that voters who were allowed to buy apartments in the RTB programme responded naïvely to the outcome and simply attributed the shock in their private economic status to the competence of the incumbent centre-right Alliance. This implies that voters embarked on a form of retrospective voting.

If we instead consider that buying a home in the RTB programme evokes some type of gratitude towards the centre-right parties, we are thinking in terms of explanation (ii). At least in connection to the U.K. right to buy programme, it appears to be a widely spread idea that the Tories have enticed voters in this manner. In a segment in CNN International (April 8, 2013), one participant says that "she did me an awful, awful good favour", regarding Margaret Thatcher's flagship social policy of the right to buy. Another, second generation homeowner said to BBC (April 10, 2013): "I have to say I'm grateful for the right-to-buy. I'm very happy.". To what extent

voters returned this favour remains nevertheless unclear.

Similar stories in media describe significant changes in the economic standing among participating households. “Your status went up”, says one homeowner to CNN International (April 8, 2013). Such anecdotes go more along the lines that tenure status might be relevant micro-level state variable to voting, and thus speak in favour of explanation (iii). Specifically, if the new homeowner considers low property taxes and low inflation important when voting, because these factors now have a greater impact on consumption, tenure status has an influence on the voting decision. Such an explanation implies not retrospection, but rather a contemporaneous effect of the voter’s assets on her voting.

The objectives of the government

Did politicians target the RTB programme to any particular group of voters in order to maximize support? Again, events in the U.K. provide a motivating example. Between 1987 and 1989 a Tory councillor focused right to buy policy on eight wards in which homes were sold at excessive discounts. A few years later, the councillor was accused of misconduct, in the so-called “homes for votes scandal”. An investigation found the councillor guilty of “disgraceful and improper gerrymandering”, due to the fact that the programme was specifically designed to sway votes from marginal constituencies. The theoretical framework of Lindbeck and Weibull (1987) explains such behaviour: Politicians can use transfers to attract votes from swing voters. One alternative proposition is suggested by Cox and McCubbins (1986): the RTB programme targeted instead the core supporters.

III. Housing policy and the right to buy in Stockholm

Housing policy and municipal housing in Sweden has traditionally stood out, from a Western perspective, in the sense that it has not primarily targeted low-income households (Boverket, 2008). Individuals still gain access to municipal housing via a non-discriminatory waiting list. The bulk of municipal housing is made up of apartments in regular city housing blocks and a fair share of the housing is considered quite attractive. Municipal rents are typically set according to a system of "utility value", which means that other parameters than i.e. size, standard and facilities, should play a minor role in rent levels (Boverket, 2008).

Between 1998 and 2002 and from 2007 and onward, tenants in Stockholm have had the right to buy their apartment from municipal housing companies, provided that a qualified (usually two-third) majority of tenants in the apartment block favour a conversion of ownership (Stockholm Stad, 2013). Tenants in an apartment block that is eligible for sale must first form a housing cooperative and submit an application to the municipal housing company. At this stage, at least 40 per cent of tenants in the house must support the application, although this is without prejudice to the final decision. The apartment block as a whole is then valued and offered to the cooperative. Subsequently, the cooperative holds a vote on whether to accept the offer. If the two-third majority is achieved, the apartment block is transferred to the cooperative and members either become owners[2] of apartments, or tenants under the cooperative. In 2009, around one fourth of tenants in RTB apartment blocks chose not to buy their apartment (Sjölin, 2012).

Apartment blocks that are considered for RTB sale are valued by independent consultancies, which are instructed to use as a benchmark the market for commercial, rented residential real estate. If no such benchmarks are appropriate, the value is instead based on the present value of the future rents minus maintenance costs of the municipal housing company (Stockholm Stad, 2013, Sjölin, 2012). Consequently, it is not entirely straightforward to estimate the potential rebate tenants receive on their purchase. Nevertheless, tenure status creates an evident wedge in real estate values. The reason being that the private market for rented apartments is subject to a system of rent-controlling regulation, while the market for tenant-owned apartment blocks is not (Boverket, 2008). As a result, the potential rebate primarily appears to derive from the rent-to-price ratio in the housing market.

[2] The most common form of apartment ownership in Sweden is not strictly in the form of a condominium, but a share in a private property cooperative that is tied to an apartment.

In Sweden as a whole, the long-run rent-to-price ratio (i.e. costs for renting over costs for owning) is estimated to be on average 0.77 according to The Economist Intelligence Unit (The Economist, December 30 2009) meaning that a the cost of renting a home is 23 per cent lower than owning it. We may consider this as an indication of the potential rebate or indirect transfer received by tenants.

The specific conditions for right to buy policies in Sweden are determined by national legislation and municipal policies. The next section will elaborate on this process.

The politics of RTB

Elections in Sweden are proportional for local assemblies as well as for the national assembly. In recent history, two political blocs have dominated both the national and local assemblies: The centre-right parties (the Moderate Party, the Christian Democrats, the Liberal Party and the Centre Party) and the red-green parties (the Social Democratic Party, the Left Party and the Green Party). In line with previous research (e.g. Jordahl, 2006 and Dahlberg and Johansson, 1998) we will henceforth discuss Swedish politics in terms of these two blocs.

The first steps toward the right to buy were taken during the centre-right national government of 1991-1994. However, it took until 1998, when the centre-right parties took office in Stockholm, for any large-scale programme to take place in Stockholm. During that period, Social Democrats held national office. While no legislative steps were taken to stop the sales at first, in 2002, the Social Democratic government passed a law that made it mandatory for municipalities to submit applications for all apartment blocks that were considered for RTB to the County Administrative Boards (Länsstyrelsen, a body of authorities under the national government). This law became known as a "Stop law" (*stopplag*), and acted as a fairly tight constraint on municipalities to implement RTB (Boverket, 2008).

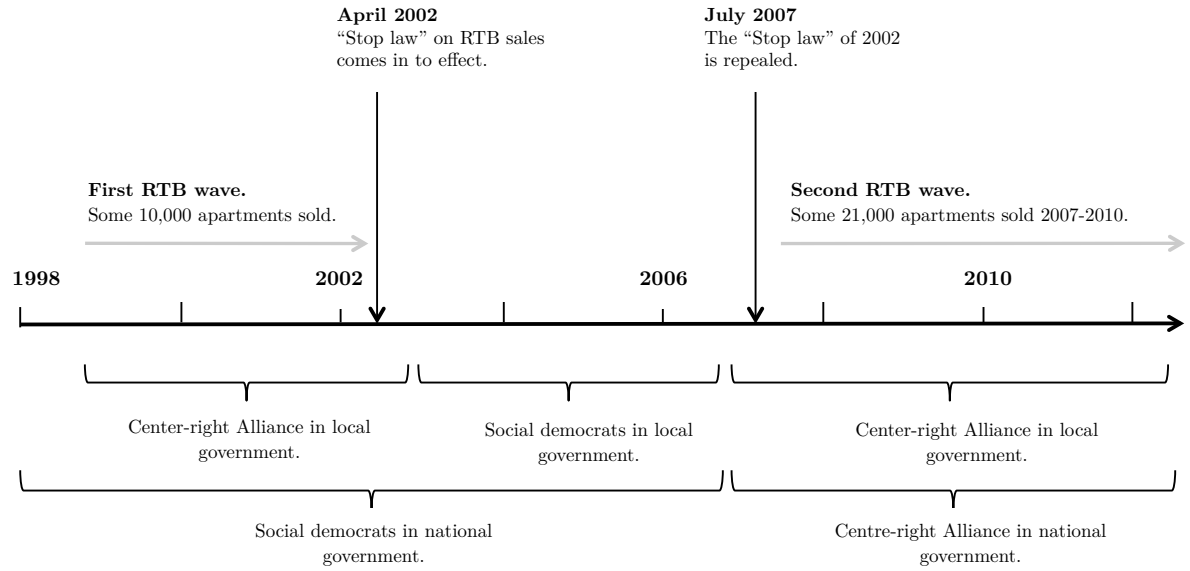
In 2006 the four centre-right parties launched a joint election platform under the name "Alliance for Sweden". The centre-right bloc subsequently won the 2006 election, and has held national government and local government in Stockholm since then. In its national manifesto of 2006, the Alliance proposed enhanced possibilities for RTB[3]. When the centre-right Alliance took national office, the Stop law was repealed and municipalities were allowed to continue RTB policy at their discretion. The Alliance in the city of Stockholm has since it took office championed reforms aimed at "achieving balance in housing tenure in the city" and improving possibilities

[3] 2006 joint manifesto: "Fler i arbete – mer att dela på. Valmanifest 2006".

to RTB (Stockholm Stad, 2013). That is, decreasing the share of rental apartments in the housing stock and increasing rates of home ownership.

Shortly after the election in 2006, RTB-applications in Stockholm increased rapidly. Around 21,000 apartments were sold to tenants in Stockholm municipality during the period between 2007 and 2010. Consequently, the Stockholm RTB programme was implemented during two waves; first 1998-2002, and after 2006 until now. The majority of RTB sales were made in 2008 and 2009, and a small number of sales have been made after 2010. Figure 1 displays the sequence of relevant events in this regard.

Figure 1: Timeline of the RTB policy



Source: Statistics Sweden (SCB), Municipal housing companies, Sjölin (2012)

To illustrate the political divide on RTB policy, Table 1 outlines the number and type of statements made by party officials on RTB policy in newspapers distributed in Stockholm. Comments asserting that RTB has had, or will produce, desirable outcomes are denoted as "praising", while comments criticizing some aspect of RTB policy or implementation are denoted as "negative". A fair number of comments were relating to technical issues and contained no value judgements, and were thus denoted as "neutral". We note also that local party officials made virtually all of statements regarding the RTB policy; we found only one comment made by a national party official.

Table 1: Bloc divide on RTB: Statements made by local party officials in print media 2006-2010

	Statement type		
	Praising	Negative	Neutral
Centre-right Alliance	11	0	28
Red-green bloc	0	32	3

Source: Retriever (Dagens Nyheter, Svenska Dagbladet, Expressen, Aftonbladet, Metro Stockholm; "Mitt i" media.)

In order for us to later obtain unbiased first-difference estimates, our identification will assume a stylized understanding of the politics of RTB. We make explicit the following interpretation:

1) Up until 2002, the implications of the right to buy were unknown to most voters. Specifically, the opportunities for tenants were not evident during the first wave.
2) Voters were, prior to 2006, unable to take RTB policy completely into account when voting; the political divide on RTB policy during the first wave was ambiguous.
3) Having seen the benefits of RTB in its first wave, tenants were able to acknowledge the potential gains from RTB only in 2006.
4) Having observed the consequences of the Social Democratic "Stop law" between 2002 and 2006 – and the campaign promises of the national and local Alliance – voters were only in 2006 able to identify the bloc divide on RTB policy.

IV. Dataset and variables

Data collection is carried out at two separate levels, the apartment block level and electoral district[4] level. First, we collect district data on election results – the outcome we are interested in – from the Swedish Election Authority. Election results for the national parliament and the City council in Stockholm are obtained from the same level and same points in time, namely the years 2002, 2006 and 2010. Additionally, we collect control variables from Statistics Sweden at the same district level as the election results, but only for the years 2006 and 2010. For 2006 and 2010 we obtain data on median income for each electoral district. In addition, we obtain two more control variables for 2010: the share of population that are born in another country and the share of population that has a secondary education in each district.

Table 2 shows some descriptive statistics of the electoral level dataset. We observe an increase in the per-district average vote share of the Alliance from 2002 to 2006 in both elections. Between 2006 and 2010, some of this increase in vote share was lost to the red-green bloc.

Table 2: Descriptive statistics - Electoral level dataset

	Year		
Total sample (n=235)	2002	2006	2010
Average vote share of centre-right Alliance, national election	45.7	52.5	51.2
Average vote share of red-green bloc, national election	52.1	42.8	43.5
Average vote share of centre-right Alliance, local election	44.7	50.9	49.0
Average vote share of red-green bloc, local election	51.3	44.2	46.5
Average per-district median income (SEK '000)	N/A	235.4	254.7
Average share of population born in a another country	N/A	N/A	15.7
Average share of population with a secondary education	N/A	N/A	47.7

Source: Statistics Sweden (SCB)

At the second level at which we collect the data, the apartment block level, we compile data on municipal apartment blocks and RTB applications from the records of the three municipal housing companies in Stockholm (Svenska Bostäder, Stockholmshem and Familjebostäder). This dataset is made up of 840 apartment blocks, containing 38,398 apartments. About half of the RTB applications – 21,353 out of 38,398 – during the period between 2007 and 2010 were successful and lead to the transfer of ownership from a municipal housing company to the tenants. The

[4] In the City of Stockholm the electorate is, for administrative purposes, divided into some 500 geographical districts, each consisting of 1,000-1,800 voters. In practice, we only use the 235 districts that had RTB applications.

other half, 17,045, were not sold, but retained by the municipal housing companies.

We obtain GPS coordinates from Google Geocoding API, a geographical data service, for all 840 apartment blocks in the dataset, based on their addresses. Having obtained the geographical coordinates of the electoral district borders from the Election Authority, this allow us to pinpoint the exact location of each of the 840 apartment blocks and match them to the correct electoral districts. In data management terms, we collapse the apartment block dataset into the electoral district dataset. In doing so, each electoral district is assigned variables for the number of apartments in the apartment blocks that have applied for the RTB programme, the number of sold apartments in the RTB programme, frequency weighted construction year of apartment blocks, median income (2006 and 2010), the share of individuals with a secondary education (2010) and the share of individuals born in a foreign country (2010).

The merged dataset is then a panel with three elections – 2002, 2006 and 2010 – in the latitudinal dimension and 235 electoral districts in the longitudinal dimension. In the 165 districts with successful RTB applications, the average number of RTB-sold apartments is 116, and the maximum is 572.

The 235 districts used in our analysis are not all the available districts; there are around 500 electoral districts in Stockholm municipality. We will however focus on the 235 districts affected by the RTB policy. Furthermore, we should note that, while no major changes in districts took place between the 2002 and 2006 elections, the city undertook some redistricting between 2006-2010. 45 new districts were added and 3 previous districts were removed. Even when we remove these 48 districts, the panel of 235 districts remains somewhat inconsistent due to the rearrangement of district borders. We will discuss the implications of the measurement error that arise from this fact in section VIII.

Groups and variable adjustments

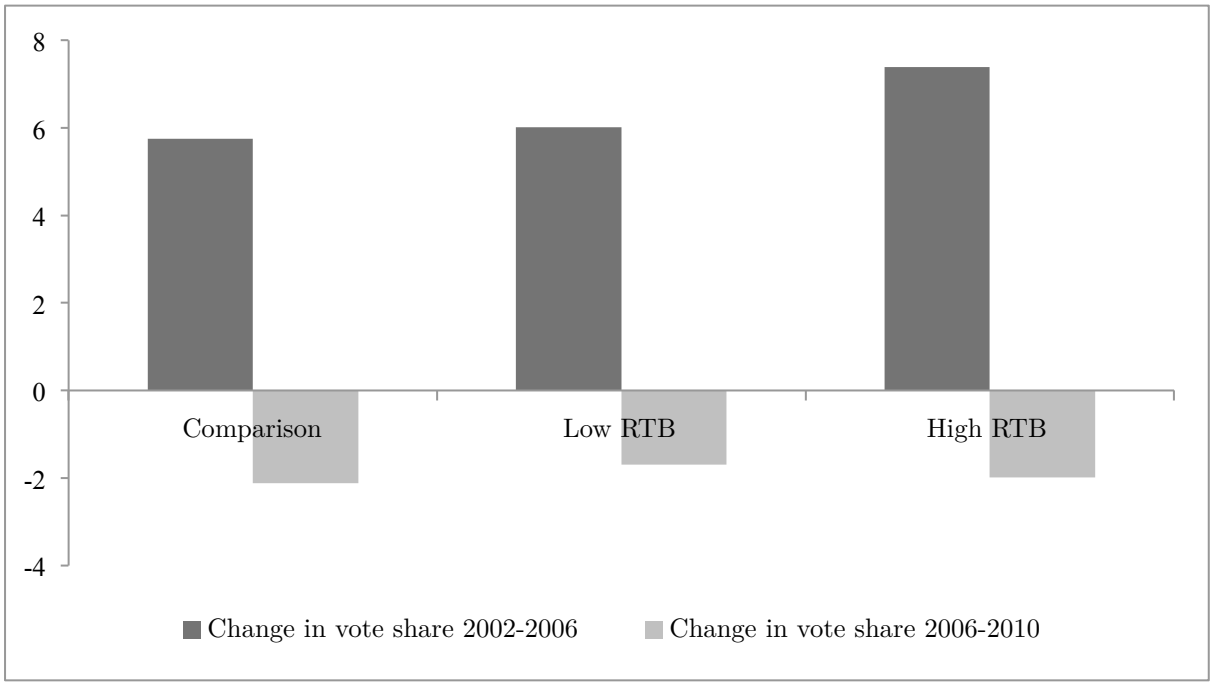
To measure the impact of the RTB programme on election results, we will consider two types of models. We will first discuss the evaluation in terms of a natural policy experiment with two dummy variables indicating district programme participation. This first model, Model 1, is thus a traditional difference-in-differences, specified as a first-difference equation with two dichotomous RTB dummies and one comparison group. We then move on to a second model, Model 2, which estimates effects within the group of districts with only successful RTB applications using a continuous measure of programme participation. We will discuss the advantages of the different approaches in the next section. Below, we outline briefly the adjustments of data we

carry out before moving on to estimation.

To estimate the first type of model, Model 1, we first divide the total sample (n=235) into the three groups. The two "treatment" groups are the districts with high and low RTB programme participation rates, while the comparison group is comprised of districts with only uncompleted RTB applications. We realize that the term "treatment group" carries some methodological weight, which we really cannot attribute to our groups of RTB districts. Therefore, we refrain from using this term, other than when referring to general methodological concepts, and instead use the terms "RTB group" or "RTB districts" when speaking of the group of districts affected by the programme. The groups "High RTB" and "Low RTB" simply refers to the "RTB group" split in two at the mean.

Figure 2 illustrates the percentage point change in the centre-right bloc's vote share for the RTB groups and comparison groups between the different elections. As the percentage point change in vote shares is the only dependent variable used in all regressions, the figure essentially illustrates the key relationship we are interested in graphically. We can see that the percentage point change in the vote share for the centre-right bloc between the elections in 2002 and 2006 is larger for the high RTB group than for the other two groups. The difference between the three groups when it comes to change in vote shares between the 2006 and 2010 elections is less striking.

Figure 2: Percentage point change in the centre-right vote share in the local election



Note: The comparison group has n=70, Low RTB has n=111 and High RTB has n=54

The second type of model, Model 2, will estimate the effect within the RTB group. As the independent variable we will use the number of sold apartments in the RTB programme. In order to be able to interpret the magnitude of the coefficients in any meaningful way, we make some adjustments to the continuous independent variable.

First, the implicit assumption will by necessity be that the number of individuals qualified to vote per apartment does not systematically vary between districts. In order to improve the measure of programme participation it is possible, however, to divide the raw variable (ranging from 6 to 572 apartments) by the number of individuals qualified to vote in each district. This procedure yields values ranging from 0 to 0.66. Official data from 2002 indicate that the average number of people per dwelling in Stockholm municipality is 1.8 (Regionplane- och trafikkontoret, 2005). This measure, however, includes all people, regardless of voting eligibility. Consequently, we multiply 1.8 with the share of eligible voters (77.1%) in the Stockholm municipality in 2002 and obtain 1.39. We continue to multiply the voters-adjusted programme participation ratio (ranging from 0 to 0.66) by a factor of 1.39 and obtain the variable RTB_PART.

Note that these last steps do nothing to improve or change the estimation of any regression using the variable. We merely multiply the variable with a scalar to make estimates and interpretation of programme participation variable (hence RTB variable) more intuitive.

Table 3: Descriptive statistics of RTB variable	Average	Median	Min	Max
RTB variable unadjusted	116	78	6	572
RTB variable adjusted for voters in district	0.094	0.061	0.005	0.661
RTB_PART variable adjusted for voters per dwelling	0.131	0.084	0.007	0.919

Table 3 shows some descriptive statistics of the continuous measurement of RTB programme participation. The last row shows the RTB_PART variable used to estimate Model 2 later on. It takes values between 0.007 and 0.919. In theory, a value equal to 1 represents the case in which all apartments in the district were previously owned by municipal housing companies, and all apartments were sold in the RTB programme during the period 2007 to 2010.

We conclude this section with a table explaining all the variables that will be used in the different models and regressions coming up.

Table 4: Variables

Variables	Explanation	Time period
CENTRE_RIGHT	The percentage point change in vote shares between two elections for the Centre-right Alliance	2002-2006 & 2006-2010
RED_GREEN	The percentage point change in vote shares between two elections for the Red-green bloc	2002-2006 & 2006-2010
RTB_HIGH	Dummy variable indicating if the district had a high RTB participation rate. High being above the mean participation rate (0.131). [Only used in Model 1].	2007-2010
RTB_LOW	Dummy variable indicating if the district had a low RTB participation rate. Low being below the mean participation rate (0.131). [Only used in Model 1].	2007-2010
RTB_PART	RTB participation. Continuous variable between 0.007 and 0.919. 1 is full RTB programme participation in the district. [Only used in Model 2].	2007-2010
L_MEDINC	Natural logarithm of district median income in levels.	2006 & 2010
MEDINC_CHNG	The percentage change in district median income between two election years.	2006-2010
CITY	Dummy variable, "1" if district is in the inner city of Stockholm.	Spatial
SOCIO	Vector containing two level controls: 1) the district share of individuals born in a foreign country, 2) the district share of individuals with a secondary education.	2010

V. Method

In order for the reader to better understand the empirical strategy following this section we now present the simple generalized versions of the two models used in this paper.

Model 1 – DiD with two dichotomous treatments

Model 1 can be written as the following general first-difference equation with two treatment terms (the exact empirical specification can be found under "Hypotheses and specifications" further below):

$$\Delta y_{ij} = \beta_0 + \delta_1 T1_i + \delta_2 T2_i + \beta_1 \Delta X_i + \Delta \varepsilon_i \qquad (*)$$

where Δy_{ij} is the change in outcome variable between two time periods (e.g. elections), for observation i for outcome j. The terms $T1_i$ and $T2_i$ are dummy variables indicating treatment group affiliation. ΔX_{i_i} is the change in a vector of time and space-varying control variables between the two time periods. Finally, the error term, $\Delta \varepsilon_i$, is the change in the idiosyncratic error from one period to the next. By assumption, this term has to be uncorrelated with the independent variables (exogeneity).

First-difference estimation on panel data has an important, obvious advantage over regular OLS estimation on pooled cross sections – it takes district fixed effects into account. This can be seen by writing equation (*) as two different equations, one for each time period, and then taking the first-difference.

$$y_{ij,t+1} = \alpha_0 + \beta_0 + \delta_1 T1_i + \delta_2 T2_i + \beta_1 X_{i,t+1} + a_i + \varepsilon_{i,t+1}$$
$$y_{ij,t} = \alpha_0 + \beta_1 X_{i,t} + a_i + \varepsilon_{i,t}$$

Notice that the two treatments are present only in the second period equation, i.e. post-policy. If we want to compare the pre- and post-policy effects of a particular programme we can subtract the second equation from the first and obtain

$$(y_{ij,t+1} - y_{ij,t}) = (\alpha_0 - \alpha_0) + \beta_0 + \delta_1 T1_i + \delta_2 T2_i + (\beta_1 X_{i,t+1} - \beta_1 X_{i,t}) + (a_i - a_i) + (\varepsilon_{i,t+1} - \varepsilon_{i,t})$$

which reduces to the FD equation we presented above.

$$\Delta y_{ij} = \beta_0 + \delta_1 T1_i + \delta_2 T2_i + \beta_1 \Delta X_i + \Delta \varepsilon_i \qquad (*)$$

The first-differencing allows for a difference-in-differences (DiD) type evaluation of the pre- and post-policy effects of the programme. Furthermore, by differencing over two periods, we remove the fixed effect, a_i, that is potentially biasing the OLS estimates (Wooldridge, 2009). It is reasonable to consider such fixed effects quite important in our estimation. For example, if certain district-specific time-invariant characteristics that are correlated with RTB sales also have significant impact on election outcomes, then failing to account for these would introduce a bias in the estimates. We may consider a number of socio-economic parameters as important in determining the likelihood of participating in the RTB programme and important factors in determining the level of support to a certain political party.

Nevertheless, the necessary assumption in difference-in-difference models remains (irrespective of the estimator) that there is a parallel trend. That is, in absence of the treatment at hand, the change in the treatment group would have been the same as for the comparison group. For our purposes, equation (*) assumes that there were no additional changes in policy toward households interested in RTB in 2006 and the comparison group, and no additional changes in policy toward RTB participants in 2010. This requirement is indeed difficult to verify. In essence, we rely on a careful choice of comparison group in this regard. As far as we can tell from the available data, our groups appear fairly similar when we look at the levels of income and education and the share of immigrants in districts (see Appendix A).

Evaluation of Model 2 - FD estimation with a continuous treatment

In Model 2, the key independent variable is continuous rather than dichotomous. This allows us to evaluate the effect in a simple linear specification within the treatment group. In this case, the necessary assumption becomes not one of parallel trend, but of exogeneity and correct specification.

Consider the slightly different equation:

$$\Delta y_{ij} = \beta_0 + \delta \Delta T\_CONT_i + \beta_1 \Delta X_i + \Delta \varepsilon_i \qquad (**)$$

where ΔT_CONT_i is now the change in treatment status from the first period to the second. The other variables remain the same as in equation (*). Just like we showed for Model 1, the FD set up of equation (**) will remove any fixed effects that are likely to introduce time-invariant bias in the model.

Estimating standard errors

Calculating standard errors that are robust to heteroskedasticity, generally yield asymptotically unbiased inferences even when the regression residuals are not homoskedastic in nature. However, the key assumption is asymptotic approximation, which requires large samples in both the time and space dimension (Angrist & Pischke, 2008). Problems in this regard often arise from the fact that there are dependencies between groups of observations in the sample within and across time periods. Failing to account for these dependencies may produce biased standard errors even though they are robust with respect to heteroskedasticity. The two most common problems in this regard arise from clustering (Moulton, 1986) and serial correlation (Bertrand et al., 2004). We believe that neither of these are apparent problems in our data.

The clustering problem occurs in data with a clear group structure, e.g. in data on test scores observed in different schools. In this case, the observations within schools are going to be correlated and not independent with respect to the other observations in the sample, thus biasing the standard errors and overestimating the statistical significance of estimates. Our regressor of interest, RTB_PART, is not fixed within a particular group structure, so clustering should not be a significant problem (Angrist and Pischke, 2008).

The serial correlation problem arises from the fact that observations for the same entity are usually highly correlated over time and are therefore not independent. Bertrand et al. (2004) show that many difference-in-difference estimations grossly underestimate standard errors by failing to account for serial correlation. However, this is primarily a problem in datasets with few cross-sectional observations and many observations in the time dimension. This is often the case in papers using DiD to measure policy impacts between two adjacent geographical areas, i.e. where $n = 2$. The observations used for estimations in this paper are in two-period panels spanning two elections. Depending on which specification we estimate, there are between 165 and 235 observations in the longitudinal dimension. Therefore, we do not deem serial correlation to be an important issue in our estimation and we simply calculate standard errors that are robust with respect to heteroskedasticity.

VI. Empirical strategy

Having organized the panel dataset described in section IV, we move on to outline the empirical strategy in order to identify the electoral effects at the district level of the right to buy reform. We use the two models explained in the previous section and apply them to our data.

In Model 1, we use the dichotomous variables RTB_HIGH and RTB_LOW and consider the case of a conventional policy experiment. This method will be equivalent to a difference-in-difference (DiD) strategy with district fixed effects (Wooldridge, 2009). The advantage of this approach is that we make sure, at least, that the number of municipal tenants is similar in the groups we compare.

We define the comparison group to be the districts with uncompleted RTB applications, so that we in effect estimate the difference between high participation/low participation and no participation. Recall, however, that there was some interest for the RTB policy in the latter group. Thus some tenants desired to purchase their apartment in the comparison group; they might have changed their voting behaviour accordingly, so we should be careful in interpreting the estimates too rigidly.

In Model 2, we use a continuous measure of programme participation in order to estimate the magnitude of the RTB programme within the RTB group. This will be a standard first-difference model with a continuous policy variable and district fixed effects. The advantage of this model is that we expect to estimate the effect of RTB participation, rather than the effect of different levels of potential interest and participation.

In section X, we expand on some factors, which might prevent estimates in these models to be unbiased. Below, we present the exact specifications of Models 1 and 2 for the two empirical hypotheses A and B.

Specifications: pre-policy estimation

For the pre-policy voting hypothesis, we consider first the following specification:

$$\Delta VOTE_{ij} = \beta_0 + \delta_l RTB\_LOW_i + \delta_h RTB\_HIGH_i + \beta_1 L\_MEDINC_i + \beta_2 CITY_i + \Delta\varepsilon_i \qquad (1.A)$$

where $\Delta VOTE_{ij}$ is the change in the vote share in district i for bloc j, where RTB_LOW_i take the value 1 if RTB participation was positive but smaller than the mean (0.13).

RTB_HIGH_i takes the value 1 if RTB was higher than the mean. L_MEDINC_i is the logged median income level in 2006 in each district and $CITY_i$ a city district dummy indicating central location.

In Model 1.A, we evaluate the difference between the RTB and comparison groups, where the latter is defined as the districts with uncompleted RTB applications.

For the pre-policy voting hypothesis, we consider also Model 2 with continuous RTB variable.

$$\Delta VOTE_{ij} = \beta_0 + \delta \Delta RTB\_PART_i + \beta_1 L\_MEDINC_i + \beta_2 CITY_i + \Delta\varepsilon_i \qquad (2.A)$$

where ΔRTB_PART_i takes continuous values between zero and one, representing the RTB participation measure. The other parameters remain the same as in Model 1.A. The sample used in Model 2.A includes some 165 districts affected by the RTB programme and no comparison group.

Specifications: post-policy estimation

For the post-policy voting hypothesis, we use the same approach as in Models 1.A and 2.A, but we include controls available for 2010.[5] We first estimate the following model with dichotomous RTB variables (corresponding to Model 1):

$$\Delta VOTE_{ij} = \beta_0 + \delta_l RTB\_LOW_i + \delta_h RTB\_HIGH_i + \beta_1 L\_MEDINC_i + \beta_2 \Delta MEDINC\_CHNG_i + \beta_3 CITY_i + \beta_4 SOCIO_i + \Delta\varepsilon_i \qquad (1.B)$$

where $\Delta VOTE_{ij}$ is the change in the vote share in district i for bloc j and where RTB_LOW_i and RTB_HIGH_i remain the same as above. L_MEDINC_i is the logged median income in 2010 in each district and $CITY_i$ a city district dummy indicating central location. For this period, three additional regressors are available: $MEDINC\_CHNG_i$, which is the percentage change in median income in each district between two election, and $SOCIO_i$, which is a control vector representing the share of foreign-born individuals in each district and the share of individuals with an secondary education in each district in 2010.

We consider also the model with a continuous RTB variable (corresponding to Model 2.A):

$$\Delta VOTE_{ij} = \beta_0 + \delta \Delta RTB\_PART_i + \beta_1 L\_MEDINC_i + \beta_2 \Delta MEDINC\_CHNG_i + \beta_3 CITY_i + \beta_4 SOCIO_i + \Delta\varepsilon_i \qquad (2.B)$$

[5] In the appendix, we present results from model 2.B with the exact same specification (controls) as in 2.A.

where ΔRTB_PART_i takes continuous values 0 to 1, following the RTB policy measure.

Hypotheses and specifications

Drawing from our conceptual framework we outlined in section II and the equations above, we propose the following empirical hypotheses about pre-policy and post-policy voting:

Table 5: Empirical hypotheses

	Hypotheses	
	Pre-policy voting (A)	Post-policy voting (B)
Centre-right Alliance	$\delta_A > 0$	$\delta_B > 0$
Red-green bloc	$\delta_A < 0$	$\delta_B < 0$

The expressions in Table 5 simply state that if we expect any of the two hypotheses about voting behaviour to be true, then the inequalities concerning that hypothesis should hold. If voters respond to the exclusively pre-policy (i.e. prospectively), then $\delta_A > 0$ and $\delta_B = -\ \delta_A$ should jointly hold. That is, there should be a positive effect before the policy, and a negative effect of the same scale after the policy, reflecting the fact that voters return to their initial preferences after having benefited from the programme. As stated in the beginning of the paper, our hypothesis is that homeowners are more prone to vote for the centre-right and therefore these coefficients are positive. Further, note that the pre- and post-policy voting hypotheses are not mutually exclusive. Voters can, in theory, both respond to promises and actual reform. Consider the case of pre-policy voting having a greater effect than post-policy voting, then $\delta_A > 0$ and $\delta_B < -\ \delta_B$ will hold. That is, there should be a positive effect before the policy, and a negative and lesser effect after the policy.

Note also that the only difference between the blocs should be that the inequalities have the opposite signs, which is a natural result of the zero-sum nature of elections dominated by two large blocs. In the results section below, we will display results for Models 1 and 2 for both blocs in the interest of clarity.

In practice, equation 1.A and 2.A are related to the pre-policy hypothesis, while equations 1.B and 2.B are related to the post-policy hypothesis. The equations will be estimated for the following time periods:

(A) $t = 2002\ and\ t+1 = 2006$
(B) $t = 2006\ and\ t+1 = 2010$

where (A) tests the pre-policy voting hypothesis and (B) tests the post-policy voting hypothesis. We add subscripts A and B for all terms in (*) and (**) so that δ_A is the average pre-policy effect and δ_B is the average post-policy effect.

VII. Results

The results presented in this section will draw from the specifications outlined above. Robustness checks and alternative specifications are relegated to Appendix B.

Pre-policy voting

First, we present the results concerning the hypothesis on pre-policy voting. Results for the election in 2006 are displayed in Table 6 and 7. The specifications follow from Models 1.A and 2.A. The dependent variable is always the change in vote shares for the two political blocs in the national and local elections, measured by the change in percentage points between the two elections.

Table 6: Model 1.A - FD estimates of pre-policy voting, *2002 – 2006*

	National election		Local election	
	CENTRE_RIGHT	RED_GREEN	CENTRE_RIGHT	RED_GREEN
RTB_HIGH (δ_h)	1.376**	-1.591***	1.663***	-1.673***
	(0.535)	(0.589)	(0.538)	(0.581)
RTB_LOW (δ_l)	0.631	-0.726	0.274	-0.450
	(0.418)	(0.483)	(0.394)	(0.440)
L_MEDINC 2006	0.337	1.243	-0.457	2.177
	(1.061)	(1.436)	(1.107)	(1.374)
CITY	YES	YES	YES	YES
SOCIO	N/A	N/A	N/A	N/A
Constant	1.539	-23.42	11.25	-33.73**
	(13.02)	(17.62)	(13.61)	(16.86)
Observations	235	235	235	235
R-squared	0.056	0.049	0.054	0.091

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Model 2.A - FD estimates of pre-policy voting, *2002 – 2006*

	National election		Local election	
	CENTRE_RIGHT	RED_GREEN	CENTRE_RIGHT	RED_GREEN
RTB_PART (δ)	1.276	-1.564	3.721**	-3.207**
	(1.534)	(1.534)	(1.463)	(1.568)
L_MEDINC 2006	-0.184	2.093	-0.931	3.119*
	(1.389)	(1.930)	(1.457)	(1.872)
CITY	YES	YES	YES	YES
SOCIO	N/A	N/A	N/A	N/A
Constant	8.563	-34.55	17.14	-45.52*
	(17.10)	(23.77)	(17.98)	(23.08)
Observations	165	165	165	165
R-squared	0.044	0.046	0.041	0.076

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

In Table 6 we show the simple FD estimations equivalent to a difference-in-difference estimation, and where we have split the RTB group into two: one for high RTB participation and one for low RTB participation. The RTB_HIGH estimates are significant and positive for the centre-right Alliance and significant and negative for the red-green bloc, while the RTB_LOW estimates carry the same signs but come out insignificant. Specifically, the differences in the centre-right Alliance vote share between the group in districts with high RTB participation and the comparison group are 1.38 and 1.66 for the national and local elections respectively. For the red-green vote share, the equivalent estimates are -1.59 and -1.67 respectively. The RTB_LOW estimates suggest that the difference in the centre-right vote share between the group with low RTB participation and the comparison group are 0.63 and 0.27 for the national and local elections. The same differences for red-green vote share are -0.73 and -0.45. We should however be somewhat careful in interpreting the estimates in Table 6, recalling that they measure the difference to the comparison group, in which voters may also have reacted positively to the outlook of RTB policy. We consider thus the estimates in Table 6 as an indication of policy having had a possible pre-policy effect on voting.

Table 7 displays the results from Model 2.A, which we specify to measure the effect of RTB policy as a continuous policy measure. We make the following interpretation when examining the coefficients for the RTB variable in Table 7. In the 2006 local election, the maximum RTB participation in districts (RTB_PART = 1) gives, on average, a 3.72 percentage point increase in the vote share of the centre-right

Alliance. For the red-green vote share, the equivalent estimate is -3.21. The mean RTB participation (RTB_PART = 0.13) produces, in turn, a 0.48 (3.72*0.13) percentage point increase in the vote share of the centre-right bloc and a 0.42 (3.21*0.13) percentage point decrease for the red-green vote share.

In the national election, estimates appear with the same signs, but come out insignificantly different from zero. The maximum RTB participation in districts (RTB_PART = 1) gives, on average, a 1.28 percentage point increase in the vote share of the centre-right Alliance in the national election. For the red-green vote share, the equivalent estimate is -1.56.

In Appendix B, we consider and test a number of alternate dependent variables (i.e. we manipulate the RTB measure in different ways), sub-samples and city-district controls in order to check the robustness of our specifications. The results above appear to be robust to such alternative approaches; we observe a significant effect on the local election, and a lesser and weakly significant effect on the national election.

If we consider our specifications to be correct and estimates to be unbiased the results, by and large, suggest that the RTB programme participation has a significant effect on the change in vote share of the centre-right bloc in the local election. This is irrespective of the sample, controls and measure of policy we use. Voters thus appear to have acted in an informed and forward-looking way in the local election, which speaks in favour of the pre-policy voting hypothesis. At the outlook of buying their apartments, voters were moved to favour the centre-right bloc to a greater extent, at least in the local election.

Post-policy voting

The results from the post-policy regressions are displayed in Tables 8 and 9 below. The specifications follow from Models 1.B and 2.B. The dependent variables are, just as before, the changes in vote shares for the two political blocs between 2006 and 2010 in the national and local elections. Note that we include the same control variables as above, but also add three additional district-level control variables that are available for 2010: change in median income between 2006-2010, share of the district population born in a foreign country and share of the district population with a secondary education.

Table 8: Model 1.B - FD estimates of post-policy voting, 2006 – 2010

	National election		Local election	
	CENTRE_RIGHT	RED_GREEN	CENTRE_RIGHT	RED_GREEN
RTB_HIGH (δ_h)	-0.258	0.552	-0.662	0.595
	(0.757)	(0.758)	(0.755)	(0.716)
RTB_LOW (δ_l)	0.0199	-0.155	-0.0702	-0.118
	(0.824)	(0.761)	(0.815)	(0.759)
L_MEDINC 2010	18.97***	-18.96***	16.46***	-16.17***
	(4.376)	(4.154)	(4.043)	(3.815)
MEDINC_CHNG	1.460	-0.136	1.674	-0.669
	(2.057)	(1.824)	(2.083)	(1.909)
CITY	YES	YES	YES	YES
SOCIO	YES	YES	YES	YES
Constant	-237.5***	234.1***	-208.2***	201.3***
	(52.97)	(50.30)	(49.00)	(46.22)
Observations	235	235	235	235
R-squared	0.176	0.159	0.209	0.145

Robust standard errors in parentheses

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

Table 9: Model 2.B - FD estimates of post-policy voting, 2006 – 2010

	National election		Local election	
	CENTRE_RIGHT	RED_GREEN	CENTRE_RIGHT	RED_GREEN
RTB_PART (δ)	-1.067	3.187	-2.884	3.304
	(4.597)	(4.422)	(4.666)	(4.439)
L_MEDINC 2010	26.12***	-25.11***	22.32***	-21.70***
	(4.468)	(4.314)	(4.548)	(4.279)
MEDINC_CHNG	2.540	-1.153	2.318	-1.387
	(2.763)	(2.408)	(2.772)	(2.471)
CITY	YES	YES	YES	YES
SOCIO	YES	YES	YES	YES
Constant	-322.6***	307.1***	-277.8***	267.3***
	(54.42)	(52.56)	(55.44)	(52.08)
Observations	165	165	165	165
R-squared	0.220	0.200	0.223	0.173

Robust standard errors in parentheses

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

We find that estimates are reversed in signs and of a somewhat smaller magnitude than estimates in the pre-policy voting. However, standard errors are now considerably higher, and make all estimates insignificant. Table 8 displays the results from Model 1.B, in which the difference in the centre-right vote share between the group of districts with high RTB participation and the comparison group are -0.26 and -0.66 for the national and local elections respectively. The equivalent estimates are 0.55 and 0.60 respectively for the red-green vote share. The difference between the group with low RTB participation and the comparison group are 0.02 and -0.07 for the national and local elections. The same differences for red-green vote share are -0.16 and -0.12.

Table 9 displays the results from Model 2.B. Thus, in the 2010 local election, the maximum RTB participation in districts (RTB_PART = 1) implies, on average, a 2.88 percentage point decrease in the vote share of the centre-right Alliance compared to the 2006 election. For the red-green vote share, the equivalent estimate is a 3.30 increase. In the national election, the maximum RTB participation in districts gives, on average, a 1.07 percentage point decrease in the vote share of the centre-right and a 3.19 increase in the red-green vote share. All estimates are, however, plagued with large standard errors and are not significantly different from zero.

Again, we consider and test a number of alternative specifications, which are displayed in Appendix B. Our tests appear to not change the results above. The estimates remain, with the same sign, i.e. negative for the centre-right vote share and positive for the red-green vote share, but with large standard errors. The post-policy effect remains, therefore, ambiguous and we are quite constrained in making any definite inference about the post-policy voting hypothesis.

Which voters were targeted by the RTB programme?

In addition to the tests on voting behaviour, we carry out a simple test on the targeting of the RTB programme. The characteristics of the areas with RTB participation are interesting factors, specifically in light of the models of Lindbeck and Weibull, and Cox and McCubbin. Recall that the result in the Lindbeck and Weibull (1987) model is that politicians will target swing voters and low-income households, whereas Cox and McCubbins (1986) suggest that politicians will target core supporters. We do not create a variable designed to capture the density of swing voters in districts (following e.g. Dahlberg and Johansson, 1998), in lack of historical data to appropriately calibrate such a measure. Instead, we focus on income levels and political support. For this test, we estimate:

$$\Delta RTB\_PART_i = \beta_0 + \beta_1 CENTRE\_RIGHT_i + \beta_2 L\_MEDINC_i + \varepsilon_i$$

where ΔRTB_PART_i is the programme participation and $CENTRE\_RIGHT_i$ is the vote share of the centre-right alliance in the 2006 election – note that this term is not differenced; it is the level of support in 2006.

The estimates of interest – β_1 and β_2 – follow from the two competing theories. If the centre-right Alliance used RTB to target core supporters, β_1 should be positive and significant. If the RTB programme was a way of targeting low-income households, β_2 should be negative and significant.

Table 10 displays the results from the above model, within the RTB group. We observe a negative and significant estimate of β_1, indicating that the RTB programme primarily affected districts with a low level of support for the centre-right Alliance in the 2006 election. This provides an interesting result as it highlights the fact that the RTB programmed did not affect the centre-right's core voters to the same extent as other groups of voters. Concerning the income level, we find no significant effect.

Table 10: Who was targeted by RTB?

Dependent: RTB_PART	OLS National	OLS Local
CENTRE_RIGHT 2006	-0.00311***	-0.00301***
	(0.000558)	(0.000552)
L_MEDINC 2006	-0.00781	-0.0167
	(0.0563)	(0.0582)
Constant	0.387	0.488
	(0.691)	(0.713)
Observations	165	165
R-squared	0.137	0.122

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

VIII. Threats to internal validity and possible remedies

The results we obtain above suffer from various weaknesses that are specific to the different estimations. In this section, we will discuss some threats to the validity of our findings – and possible remedies – thematically.

Bias arising from omitted variables

The obvious and probably most important weakness in our results is related to bias arising from time-dependent omitted variables. Specifically, this would induce correlation between the idiosyncratic error and the independent RTB variable, thus violating the strict exogeneity assumption.

In our analysis, we have attempted to rid the estimates of this kind of biases by including controls for median income in the districts, and for the change in median income between the 2006 and 2010 elections in the case of the post-policy evaluation. We have also added socio-economic controls (share of district population that is foreign-born and share of district population with secondary education) to the post-policy estimations. If we believe that omitted factors are time-dependent and correlated with the independent and the error, then the fixed effects estimation will still not solve the problem.

For instance, RTB policy did indeed target groups in which the support for the Alliance was relatively low. If we consider the test of pre-policy effects from RTB, we could think of the possibility that there exist some class of voters (characterized by some omitted variable) with generally low preferences toward the Alliance. If this class of voters was more likely to participate in RTB in 2007-2010 and also more likely to suddenly be enticed to the Alliance's platform in 2006, estimates will suffer from a positive bias. Such a contingency would imply that low support for the Alliance on the electoral district level is a proxy for some specific swing voter group, and in that case, first-differencing yields biased results. Motivated by this possibility, we estimate Model 2.A and control for the initial level of support to the Alliance (Appendix B, Table B2).

Even though results remain robust, we cannot completely rule out that some similar omitted variable might bias the estimates in the pre-policy models. As concerns the estimation of post-policy effects of RTB, we might suspect similar problems with omitted variables and endogeneity.

Measurement imprecision resulting from changes in district borders

Using electoral districts in a panel is associated with some measurement issues tied to the way the district entities are linked over time. Specifically, in order to keep the number of voters balanced in each district, authorities can adjust district borders. Only minor adjustments were made between 2002 and 2006, according to the Election Authority and our own assessment. Between 2006 and 2010, however, considerable redistricting was implemented. Thus, this problem concerns primarily the post-policy hypothesis.

Due to the way we construct data, changing district borders are a potential source of measurement error in the dependent variable as well as the independent. Measurement error in the independent variable gives rise to regression dilution bias (Frost and Thompson, 2000), which is negative. A potential remedy to this problem would be to gather data on a level, which is not plagued by redistricting. This is not a viable option, as it would greatly reduce the number of observations. Instead, we perform a traditional difference-in-difference (DiD) model using pooled OLS, i.e. treating the panel dataset as a cross-section so that redistricting is made irrelevant. This rules out the possibility to control for district fixed effects, but implies that districts are not linked over time and therefore are not plagued by measurement error. The results from the pooled OLS are presented in Table B5 in Appendix B. It turns out that standard errors are still too large to yield significant result. It appears, thus, that pooling does not solve the potential problems with measurement errors.

Programme-induced migration

Migration that is endogenous to policy often gives rise to bias in policy evaluations in aggregated panel data (Angrist and Pischke, 2008). We measure outcomes in electoral districts, between which households easily can move. For the period 2002-2006, we would expect very limited migration induced be the policy itself (i.e. that some individuals obtained a municipal housing contract for speculative purposes prior to the 2006 election).

However, for the later period (2006-2010), this may be a problem. After an RTB sale, apartments are privately owned and can be sold as normal real estate. Two problems arise. First, if an individual leaves her electoral district subsequent to having participated in the RTB programme, carrying with her an increased likelihood of supporting the Alliance, we are unable to identify this effect. This type of migration should give rise to negative bias in our estimates. Second, when the RTB participant leaves the district, a new individual moves in. If the new individual is even more likely to support the Alliance, such a tendency will induce a positive bias in our

estimates. This problem can, in principle, only be redeemed by obtaining data on individuals.

Effects prior to the 2006 election

Another source of bias in our results is the assumption that the RTB policy had no effect on elections prior to 2006. If we suspect that some voters already before the 2006 election had adjusted their decision to the prospects of RTB, we expect a negative bias in our pre-policy estimates. Due to lacking data on both election results and borders prior to 2002 on the district level, we cannot assess this potential problem in detail. Ideally, we would like to perform placebo tests i.e. on the 2002 and 1998 elections. Unable to do this, we rely on the interpretation of the policy made in section II.

IX. Discussion: Explaining the effects on electoral support

The estimates presented above indicate that the right to buy programme in Stockholm had a pre-policy influence over electoral support. That is, electoral districts with a high RTB participation rate in 2007-2010 voted at greater rate for the centre-right Alliance in the 2006 local election compared to the unaffected districts. However, the effect within the RTB districts is quite small. The centre-right Alliance only gained some additional 0.48 percentage points on average in vote shares from the districts affected by the programme. If indeed some opportunistic centre-right politician wanted to sway votes in 2006 using RTB to this end, then this has to be considered a very expensive strategy, at least in light of our findings. Looking at the post-policy effect on electoral support, results provide limited support for this hypothesis, at least in the 2010 election. Keeping in mind the caveats we consider above, we will in this section assess our results in view of the various theories of electoral competition and voter behaviour.

Although the right to buy programme is markedly different from many policies considered in the literature on tactical transfers, we argue that RTB indeed involves indirect transfers to tenants, which work via an implicit rebate. In essence, the rebate derives from the rent-to-price ratio in the housing market, which we recall to be 0.77 on average in Sweden. If we accept this proxy at face value, the rebate received by tenants is on average 23 per cent.

Nevertheless, RTB policy cannot be said to be unmistakably desirable among participants. A collective decision process in effect determines tenants' participation; they cannot individually choose whether to buy their home or stay on as tenants under the municipal housing company. Consider, for instance, households that are unable to obtain credit on the market. They may well recognize the bargain presented to them, but deem it unlikely that they will be able to buy their apartment when push comes to shove. The outlook for them is then becoming tenants under the cooperative. In effect, their neighbours will thus become their landlords; some households might be averse to such a prospect. If some households consider the outlook of RTB to be undesirable, this fact may also affect their voting decision in the opposite direction. The pre-policy effects of RTB policy should therefore be considered as a net effect among individuals.

An effect on the local election

Since 2006 the political divide over the right to buy has been apparent at the local level as well as at the national level, yet a significant pre-policy effect is only

observed for the local election. One conceivable reason for this is that voters merely perceive the political divide at the local level. We have already noted that the right to buy is an issue which primarily local party officials chose to focus on. It is likely therefore that voters indeed recognized the campaign promises and previous political signals in the local campaign, but failed to do so in the national election. Another possible reason for the discrepancy between the effects on the local and national elections is that voters, while fully recognizing the opposing preferences and intentions of the two blocs, were less motivated to bend their behaviour at the prospect of right to buy in the national election.

Rational and forward-looking voters

Influential theories of electoral competition draw heavily on the assumptions that election promises are credible and that voters are forward-looking (e.g. Lindbeck and Weibull, 1987). Nevertheless, politicians' campaign promises are clearly a form of cheap talk that should be ignored by voters; there are no mechanisms to enforce the promises in the short term. Models such as those of Alesina (1988) and Besley and Coate (1997) suggest that voters take this feature of elections into account, and judge politicians by their platform or "type".

Nevertheless, promises are indeed quite frequently delivered upon. During the period around the 1980's, up to 80 per cent of campaign promises were delivered by the election winners in the U.K., and 60 per cent of promises were delivered by winners in the U.S. (Royed, 1996). Why is this then? The case of RTB policy may well provide an example of a situation in which voters at least can be prospective and trust in election promises.

We argue that there was room for voters to be forward-looking about RTB policy in the 2006 election, considering two facts: First, the political blocs had revealed their preferences on the policy quite clearly before the election. Second, after the first RTB wave of 1998-2002, the policy became quite well known. In the framework of ideology-motivated politicians, politicians should be able to credibly speak to voters' pocketbooks if they stay within the boundaries of their own (or their cadre's) political preferences. The only additional assumption we should need to make is that voters can judge whether promises are consistent with party platforms. If politicians have signalled in the past an ability to carry out a specific policy, credibility should be less of a barrier in pre-electoral competition. Considering the specific context and design of the policy, we should however refrain from making any judgements about the validity of the notions of prospective voting and the credibility of election promises in general.

Our results are in line with those of Elinder et al. (2008), who study the effects on electoral support of a campaign promise on the part of the Swedish Social Democrats to put a cap on childcare fees. They also find evidence of prospective voting among voters with children in the age 0-4 (thus affected positively by this campaign promise later).

Machiavellian privatization

The theory of Machiavellian privatization (Biais and Perotti, 2003) suggests that incumbent governments can use privatizations to permanently change parameters in voters' objective function in order to stay in power. Specifically, a right-wing government can distribute stocks in companies to increase citizens' stakes in the performance of stocks. If then the policy of a left-wing government has adverse effects on the performance of stocks, voting behaviour will permanently altered in favour of the right-wing government.

Tenure status might, in theory, work similarly. The assumption then becomes that a left-wing government has adverse effects on house prices or the consumption of homeowners. Property taxes and preferences toward inflation arise as interesting factors in this respect. However, we do not know how relevant such considerations are in the Swedish context. Property taxes were indeed under debate during the 2010 election campaign, but voters in apartment blocks may well not recognize this as a decisive factor in voting. Moreover, there is little dispute over the independence of the central bank in current Swedish politics.

Alternatively, local politics might be a more interesting level to look at in this respect. This then becomes a topic closer to the home voter hypothesis, which simply states that the homeowner uses her vote to maximize the home value. Political parties in Stockholm may represent platforms, which influence home values in general quite differently; and aware of this, they might be tempted to use policy to make tenants into homeowners. It is technically possible to test such hypotheses on the RTB reform. However this would require a clear framework for analysing the platforms of the parties in the municipal council, and it is not clear that the two blocs are the relevant entities to consider in this respect.

Naïve retrospection and reciprocity

Having found no definite evidence for a post-policy effect on electoral support, we have no indication of naïve retrospection among voters affected by the RTB policy. In fact, prospective voting and naïve retrospection are in theory mutually exclusive; if voters are informed about the prospects of a particular policy and adjust their

decision rationally, they cannot realistically be uninformed at a later point. This result thus goes against the findings of e.g. Jordahl (2006), and notably Manacorda et al. (2011), which find support for uninformed voting subsequent to a cash transfer programme in Uruguay.

Similarly, we find no direct support for the idea of election reciprocity among RTB participants. It is interesting that narratives about reciprocal behaviour vis-à-vis the right to buy are quite abundant in the U.K., while this seems not to be the case in Sweden.

The objectives of politicians

In addition to the results on voting behaviour, we have shown that the RTB programme targeted areas in which the support for the centre-right Alliance was low. However, we find no targeting of low-income areas. The former result is in line with Dahlberg and Johansson (1998), who find that the Social Democratic government in a grants programme to municipals did not targeted core supporters, but rather swing-voter areas. Following the model of Cox and McCubbins (1986), we thus conclude that the centre-right bloc was not influenced by risk aversion.

X. Natural experiments embedded in the policy

Having sketched the drawbacks of our strategy, we consider in this section a number of alternative identification strategies that can be used to make causal inferences about the effects of the right to buy policy. Specifically, these strategies exploit quasi-randomization and exogenous variation in RTB participation and could thus help identifying the causal post-policy effects. In the absence of useful individual data we are, however, unable to carry out such strategies. The following section is consequently primarily in the interest of further research.

A regression discontinuity approach

In order to effectively identify the causal post-policy effects of becoming a homeowner in the right to buy programme, there is one obvious natural experiment embedded in the right to buy programme. The first option we face is to make use of the qualified majority, which co-ops must obtain in order to buy the property from the municipal housing company.

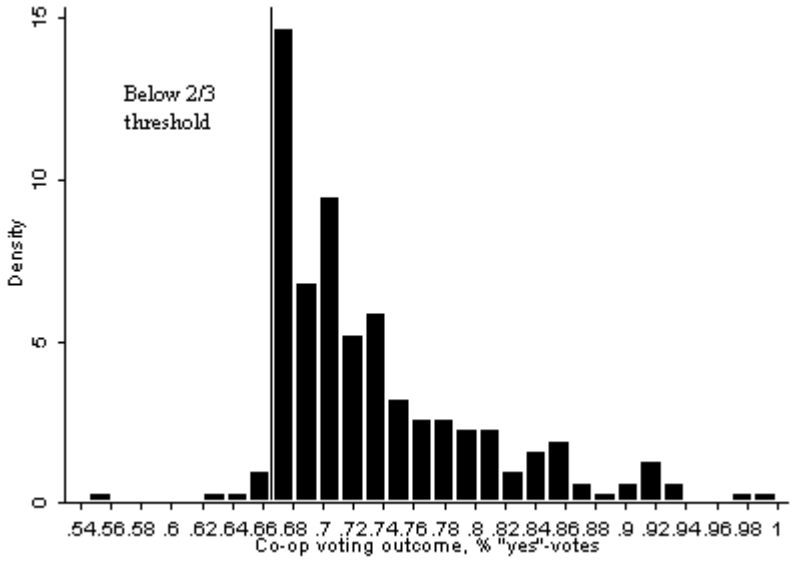
The procedural requirement of a two-third majority allows – in theory – for a regression discontinuity design (RDD). RDD techniques typically exploit some dichotomous decision, that is based on a continuous variable and impose a distinct outcome on individuals. The key assumption behind the RDD approach is that individuals cannot perfectly determine the value of the "forcing" variable (in this case, voting outcome) themselves, or that they are unaware of the decision rule (Lee and Lemieux, 2009). If individuals are not able to perfectly manipulate a variable, which govern some outcome, then those arbitrarily close to the cut-off will be assigned to "treatment" randomly. We are then permitted to make causal inferences.

Regression discontinuity designs are helpful in a range of situations where we expect some independent variable to be endogenous. In our case, we might suspect that the population of former tenants, that were successful in their RTB application, exhibit some unobservable characteristics that differ from the rest of the tenant population. We should, however, be able to root out any problems with unobserved characteristics and endogeneity if we compare the individuals that are located just below and just above the two-third-majority threshold. We may assume that each individual's co-op vote follows some probability distribution and that the outcome on the co-op is thus determined randomly at the margin. If this is true, we may consider assignment to RTB and home ownership as good as random around the threshold, and the outcomes above the threshold as a truly causal effect.

In reality, the prerequisites for a regression discontinuity approach in the RTB policy are not immediately fulfilled. First, co-ops that applied for RTB and had a vote but failed to achieve a two-third majority have not been required to submit the meeting minutes to the housing companies. We nevertheless attempt to collect a sample of meeting minutes that were available from one of the housing companies (Svenska Bostäder).

The distribution, which appears to be heavily skewed around the threshold, can be seen in Figure 3. The apparent bunching instantly above 67.7 per cent should make us worried about possible manipulation of the forcing variable. Considering that votes are subject to independent controls after the meeting, outright fraud is probably not the explanation. Instead, this distribution is likely the result of coordination among co-op members. If co-op members can be certain that a sufficient number of yes votes will be achieved in their absence, they may refrain from participating.

Figure 3: Distribution of co-op voting outcomes



Source: Compiled from the records of Svenska Bostäder

Before implementing an RDD strategy, this issue will have to be resolved. If coordination among co-op members is the true reason for manipulation of the running variable, then a sample of large apartment blocks could remedy the problem. In a small apartment block with, say, 20 households, it is quite likely that co-op members are able to acquire perfect information about the outlook of the meeting. This is much less likely in a large apartment block in the region of 150 households.

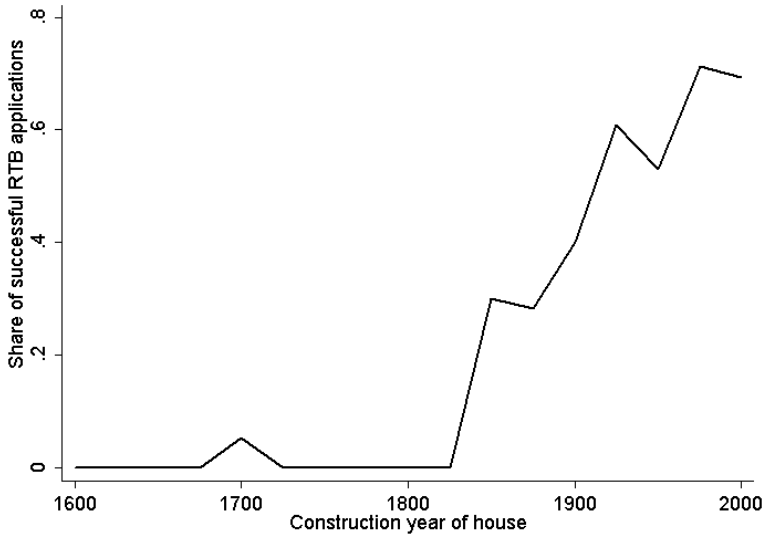
A fuzzy regression discontinuity approach

A second possible identification strategy presents itself by the fact that after the 2006 election, it took one year for the City council to determine the eligibility rules for RTB sales. During that time, co-ops submitted applications to the housing companies without knowing the restrictions. In 2007, municipal policy determined that no properties of certain cultural value could be subject to an RTB sale. Up until then, a large number of such properties were already waiting for RTB review.

It turns out that the implementation of the "cultural value" eligibility criterion exhibits a smooth discontinuity along the construction year of the properties. Within a certain range of construction years, the share of successful RTB processes changes rapidly. Figure 4 displays this relationship. Apartment blocks are divided into intervals along the construction year, and participation rate (or success rate) is calculated for each interval. A clear discontinuity in the likelihood of participating in the programme (the y-axis) is seen around the construction year (x-axis) 1850.

This fact might also be exploited in a regression discontinuity approach with a slight twist. Since the threshold is not dichotomous, but rather constitute a shift in the probability of assignment, we may use a so-called fuzzy RD approach. In this case, we must instrument assignment on the forcing variable (construction year) before we estimate the effect. In essence, the fuzzy RDD is an instrumental variable approach with a dummy (Angrist and Pischke, 2008). The simple idea is still that the rule applied by the municipal housing companies – of not allowing sales on properties of certain cultural value – is random at the margin. Despite that some individual characteristics may covary with the construction year of the home, we should be able to identify the causal effect around a threshold. Construction year is, moreover, an appropriate forcing variable since it cannot be tampered with by anyone.

Figure 4: Successful RTB applications and construction year



Source: Compiled from the records of Svenska Bostäder

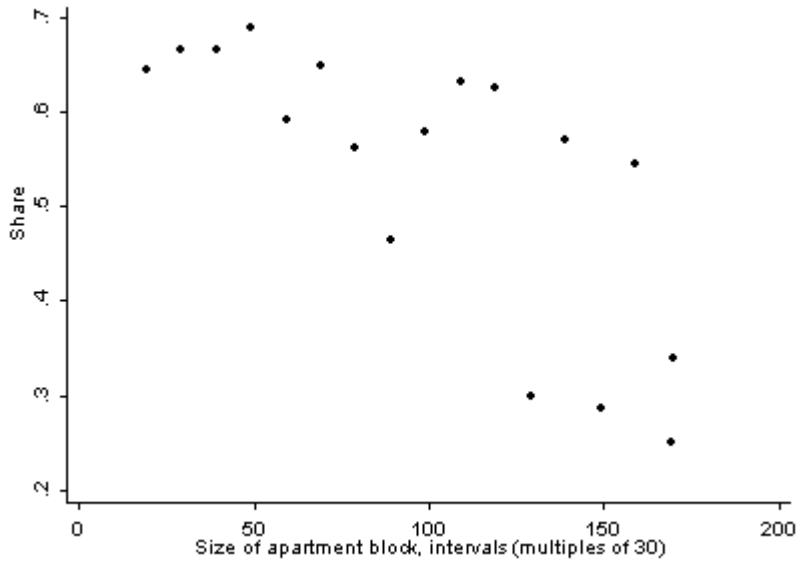
For our purposes, this feature of the data might be useful as an instrumental variable. We can obtain the weighted average of construction years in each electoral district. That would, in principle, contribute with some exogenous variation in the likelihood of succeeding with an RTB application, allowing for better inference about the post-policy effects.

Aggregation remains however a problem. In the house-level dataset, there are roughly 100 apartment blocks that were deemed "culturally valuable" and hence were denied the right to buy. However, these apartment blocks turn out to be concentrated to 24 electoral districts, which leave us with a sample that appear futile for meaningful inferences.

An instrumental variable approach

A third option in identifying the post-policy effect of the right to buy is using an interesting relationship between the size of apartment blocks and the likelihood to participate in the RTB programme. Figure 5 displays this relationship in the house-level dataset. We divide apartment blocks into intervals along their size (number of apartments) and calculate the share of participating apartment blocks in each interval. In Figure 5, we see that participation (on the y-axis) decreases when apartment blocks get bigger (on the x-axis).

Figure 5: Successful RTB applications and size of apartment block: House-level dataset



Source: Compiled from the records of Svenska Bostäder

There are several possible explanations for this relationship; we suggest that it might be driven by increasing difficulties to coordinate and build trust in large apartment blocks. Consider again the case of a small apartment block, with 20 households. In such an environment, it is possible for tenants to envisage their future as members of a housing co-op (which requires trust), easier to coordinate the RTB process and build social capital in the co-op. In a large apartment block, it may be more difficult to reach agreement as a co-op and coordinate the RTB process.

If this reasoning is valid, block size is likely to be an appropriate instrumental variable (IV). IV designs make use of variables that have an influence over the independent variable. That is, an instrumental variable must fulfil the exclusion restriction, i.e. that the instrument only has causal influence over the independent variable, and no such influence over the dependent variable. Apartment block size appears as quite likely to be an appropriate instrument. That is, if households do not select into block size according to their preferences, or if apartment block size is highly reliant on the type of housing in general.

Again, aggregation prevents us from exploiting the variation in the apartment block dataset. After aggregation, the block size variable does not demonstrate any relationship with programme participation; we obtain no first-stage regression that can be used for an IV.

Possibilities from individual data

Above, we show that there are at least three factors in the RTB policy that could potentially be used to make better causal inferences about the effects of right to buy policy in Stockholm. Specifically, it would allow the researcher to root out any endogeneity in programme participation, and identify the effects of becoming a homeowner in the RTB programme. It could thus be useful for inferences about post-policy effects on political preferences, voting and other micro-level outcomes. This requires, however, data that links outcomes to apartment blocks, or, preferably individuals. For the purpose of addressing political-economy hypotheses, collecting data from a survey would be a conceivable approach.

XI. Summary and conclusions

Promoting home ownership has been a priority for numerous right-wing governments, from Margaret Thatcher to George W. Bush to Sweden's current centre-right government. Between 2007 and 2010, the centre-right government in Stockholm sold 21,000 apartments to tenants at a discount in a right to buy programme. We ask whether this policy had any influence on voting behaviour, and in what way tactical motives can be a factor in governments' decisions to implement this form of housing privatization.

We draw hypotheses from the literature on prospective voting, retrospective voting and *Machiavellian privatization*. First-difference estimation is applied to election data from 235 electoral districts in Stockholm, in order to identify the pre- and post-policy effects of the right to buy programme.

Our results indicate that the programme had a pre-policy influence on electoral support to the centre-right bloc in the 2006 local election; in line with hypotheses of prospective voting. In districts affected by the policy, the local centre-right bloc increased its vote share by on average 0.48 percentage points. Our findings suggest that voters trusted in the campaign promise of the centre-right bloc, that they were informed about the policy and forward-looking when voting. Closest to our approach and in line with our results is notably Elinder et al. (2008). However, we do not find unambiguous support the hypothesis that housing privatization had a persistent, post-policy effect on electoral support, at least not in the 2010 election.

Moreover, the policy appears to have targeted non-core supporters of the centre-right parties, which we interpret to indicate risk neutrality on the part of the centre-right bloc. We conclude our analysis by outlining the natural experiments embedded in the Stockholm right to buy programme, and argue that there are several possible strategies to evaluate additional aspects of the policy. Especially, further research could benefit from quasi-randomization on the micro level.

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Appendix A – Group descriptives

Table A1: Descriptive statistics for the three groups by election year

High RTB	2002	2006	2010
Centre-right vote share % (national)	38.20	45.62	44.81
Red-green vote share % (national)	59.32	49.29	49.70
Centre-right vote share % (local)	37.36	44.74	42.76
Red-green vote share % (local)	58.63	50.31	52.55
Median income ('000)	N/A	231.2	242.2
Share of population foreign born %	N/A	N/A	19.31
Share of population with secondary educ %	N/A	N/A	43.98
Observations	54	54	54

Low RTB	2002	2006	2010
Centre-right vote share % (national)	48,62	55,43	54,22
Red-green vote share % (national)	49,33	40,08	40,55
Centre-right vote share % (local)	47,66	53,68	51,98
Red-green vote share % (local)	48,41	41,47	43,61
Median income ('000)	N/A	238.9	264.8
Share of population foreign born %	N/A	N/A	13,68
Share of population with secondary educ %	N/A	N/A	49,68
Observations	111	111	111

Comparison group	2002	2006	2010
Centre-right vote share % (national)	46,97	53,15	51,40
Red-green vote share % (national)	50,77	42,20	43,25
Centre-right vote share % (local)	45,59	51,35	49,22
Red-green vote share % (local)	50,17	43,64	46,26
Median income ('000)	N/A	233.1	248.2
Share of population foreign born %	N/A	N/A	16,03
Share of population with secondary educ %	N/A	N/A	47,41
Observations	70	70	70

Appendix B – Robustness checks

In the following Tables, B1-B5, we present robustness checks to the main pre- and post-policy regressions presented in Tables 6-9 in the results section. In all regressions, the dependent variable is the change in the centre-right alliance's vote share from one election to the next. For each robustness specification we run two separate regressions, one for the national election and one for the local election. We find that the main results are consistent over various specifications.

We test alternative estimations using different definitions of the programme participation measure, the RTB variable, e.g. logged and not adjusted for district population. We also check if the results hold when controlling for different subsamples of the city, in particular different areas in the "inner city". Using pooled OLS, we also check if the regressions testing the post-policy hypothesis suffer from measurement error due to redistricting. We do not find that the standard errors improve. Finally, we also test Model 1 where we extend the comparison group to include all districts, not just those with RTB-applications. The results still hold.

Table B1: Robustness checks Model 1.A

VARIABLES	Expanded comparison group (National)	Expanded comparison group (Local)	w/o any controls (National)	w/o any controls (Local)	Controlling for more city districts (National)	Controlling for more city districts (Local)
RTB_HIGH	1.104**	1.503***	1.240**	1.628***	1.344**	1.668***
	(0.453)	(0.465)	(0.542)	(0.531)	(0.546)	(0.556)
RTB_LOW	0.293	0.0609	0.630	0.260	0.607	0.271
	(0.345)	(0.316)	(0.415)	(0.390)	(0.424)	(0.405)
L_MEDINC	2.163***	1.341**			0.344	-0.476
2006	(0.546)	(0.548)			(1.076)	(1.131)
CITY	ONE	ONE	NO	NO	SEVERAL	SEVERAL
SOCIO	NO	NO	NO	NO	NO	NO
Constant	-20.71***	-10.72	6.179***	5.756***	1.482	11.49
	(6.666)	(6.706)	(0.334)	(0.309)	(13.20)	(13.90)
Observations	445	445	235	235	235	235
R-squared	0.077	0.045	0.026	0.052	0.057	0.055

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B2: Robustness checks Model 2.A

VARIABLES	Unadjusted RTB variable (National)	Unadjusted RTB variable (Local)	Logged unadjusted RTB variable (National)	Logged unadjusted RTB variable (Local)	Adjusted RTB w. more city districts (National)	Adjusted RTB w. more city districts (Local)	Controlling for vote share in levels (National)	Controlling for vote share in levels (Local)	Controlling for voter turnout (National)	Controlling for voter turnout (Local)	Controlling for socio factors 2010 (National)	Controlling for socio factors 2010 (Local)
RTB_PART	0.00176	0.00457***	0.169	0.680***	1.363	3.974***	1.642	3.556**	2.205	3.783***	2.718	4.093**
	(0.00159)	(0.00158)	(0.219)	(0.221)	(1.564)	(1.501)	(1.603)	(1.509)	(1.558)	(1.420)	(1.782)	(1.577)
L_MEDINC	-0.183	-0.955	-0.195	-0.882	-0.220	-0.999	-0.414	-0.817	-0.694	-0.973	-0.519	-0.853
	(1.391)	(1.464)	(1.396)	(1.481)	(1.417)	(1.488)	(1.424)	(1.475)	(1.245)	(1.436)	(1.193)	(1.334)
CENTRE-RIGHT 2006							0.0123	-0.00612				
							(0.0183)	(0.0166)				
TURNOUT 2006									0.0901**	0.00566		
									(0.0376)	(0.0347)		
CITY	ONE	ONE	ONE	ONE	SEVERAL	SEVERAL	ONE	ONE	ONE	ONE	ONE	ONE
SOCIO	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES
Constant	8.507	17.39	8.129	14.05	8.993	17.94	10.78	16.04	7.664	17.22	13.69	19.22
	(17.13)	(18.07)	(17.24)	(18.41)	(17.43)	(18.35)	(17.32)	(18.11)	(15.71)	(17.87)	(14.37)	(16.11)
Observations	165	165	165	165	165	165	165	165	165	165	165	165
R-squared	0.045	0.044	0.043	0.062	0.045	0.049	0.047	0.042	0.079	0.041	0.088	0.064

Robust standard errors in parentheses

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

Table B3: Robustness checks Model 1.B

VARIABLES	Expanded comparison group	Expanded comparison group	w/o any controls	w/o any controls	Controlling for more city districts	Controlling for more city districts
	(National)	(Local)	(National)	(Local)	(National)	(Local)
RTB_HIGH	-0.581	-0.932	-0.0473	-0.775	-0.197	-0.540
	(0.647)	(0.663)	(0.798)	(0.840)	(0.763)	(0.755)
RTB_LOW	-0.165	-0.191	1.024	0.870	0.128	0.0839
	(0.675)	(0.677)	(0.786)	(0.807)	(0.846)	(0.833)
L_MEDINC	7.677**	5.901*			19.91***	17.61***
	(3.144)	(3.043)			(4.479)	(4.113)
MEDINC_CHNG	0.670	0.861			1.394	1.630
	(0.888)	(0.789)			(2.062)	(2.092)
CITY	ONE	ONE	NO	NO	SEVERAL	SEVERAL
SOCIO	YES	YES	NO	NO	YES	YES
Constant	-96.95**	-77.60**	1.751***	2.124***	-248.9***	-221.7***
	(38.24)	(37.01)	(0.461)	(0.483)	(54.39)	(50.05)
Observations	447	447	235	235	235	235
R-squared	0.135	0.186	0.009	0.014	0.186	0.224

Robust standard errors in parentheses

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

Table B4: Robustness checks Model 2.B

VARIABLES	Unadjusted RTB variable	Unadjusted RTB variable	Logged unadjusted RTB variable	Logged unadjusted RTB variable	Adjusted RTB w. more city districts	Adjusted RTB w. more city districts	Controlling for vote share in levels 2010	Controlling for vote share in levels 2010	Controlling for turnout 2010	Controlling for turnout 2010	Same spec as Model 2.B	Same spec as Model 2.B	Medium term Δ 2002 - 2010	Medium term Δ 2002 - 2010
	(National)	(Local)	(National)	(Local)	(National)	(Local)	(National)	(Local)	(National)	(Local)	(National)	(Local)	(National)	(Local)
RTB_PART	0.00241	0.000923	0.695	0.411	-1.566	-3.334	-0.335	-1.758	-1.348	-3.546	1.140	-1.183	2.567	1.918
	(0.00487)	(0.00498)	(0.715)	(0.719)	(4.649)	(4.722)	(4.539)	(4.627)	(4.558)	(4.436)	(4.167)	(4.211)	(4.554)	(5.011)
L_MEDINC	26.18***	22.44***	26.67***	22.73***	28.11***	24.52***	23.95***	18.99***	24.59***	18.35***	15.29***	14.23***	30.91***	25.37***
	(4.410)	(4.501)	(4.472)	(4.602)	(4.543)	(4.583)	(5.223)	(5.517)	(4.623)	(5.139)	(4.263)	(4.170)	(5.622)	(5.755)
MEDINC_CHNG	2.563	2.371	2.590	2.388	2.299	2.067	2.700	2.564	2.347	2.007			3.773	3.598
	(2.717)	(2.738)	(2.741)	(2.755)	(2.772)	(2.783)	(2.861)	(2.904)	(2.769)	(2.805)			(2.718)	(2.769)
CENTRE_RIGHT 2006							0.0381	0.0586						
							(0.0361)	(0.0393)						
TURNOUT 2010									0.0838	0.188				
									(0.123)	(0.160)				
CITY	ONE	ONE	ONE	ONE	SEVERAL	SEVERAL	ONE	ONE	ONE	ONE	ONE	ONE	ONE	ONE
SOCIO	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	NO	NO	YES	YES
Constant	-324.0***	-280.0***	-333.1***	-285.5***	-347.1***	-304.7***	-296.8***	-238.2***	-310.4***	-242.8***	-191.0***	178.9***	-373.3***	306.0***
	(53.70)	(54.86)	(55.53)	(57.21)	(55.60)	(56.15)	(63.53)	(67.08)	(54.20)	(57.97)	(53.37)	(52.21)	(68.21)	(69.94)
Observations	165	165	165	165	165	165	165	165	165	165	165	165	165	165
R-squared	0.221	0.221	0.229	0.224	0.244	0.251	0.223	0.231	0.221	0.229	0.154	0.184	0.278	0.220

Robust standard errors in parentheses

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

Table B5: Model 2.B using Pooled OLS (2006-2010)

VARIABLES	Model 2.B Pooled OLS (National)	Model 2.B Pooled OLS (Local)
RTB_PART Effect*	2.208	-0.501
	(6.314)	(6.465)
Election dummy (=1 if 2010)	-4.393**	-4.610***
	(1.756)	(1.779)
RTB_CONT	-22.40***	-19.78***
	(4.924)	(5.064)
MEDINC_CHNG	33.24***	32.63***
	(4.953)	(4.942)
CITY	ONE	ONE
SOCIO	NO	NO
Constant	-359.5***	-353.5***
	(61.05)	(60.91)
Observations	363	363
R-squared	0.485	0.458

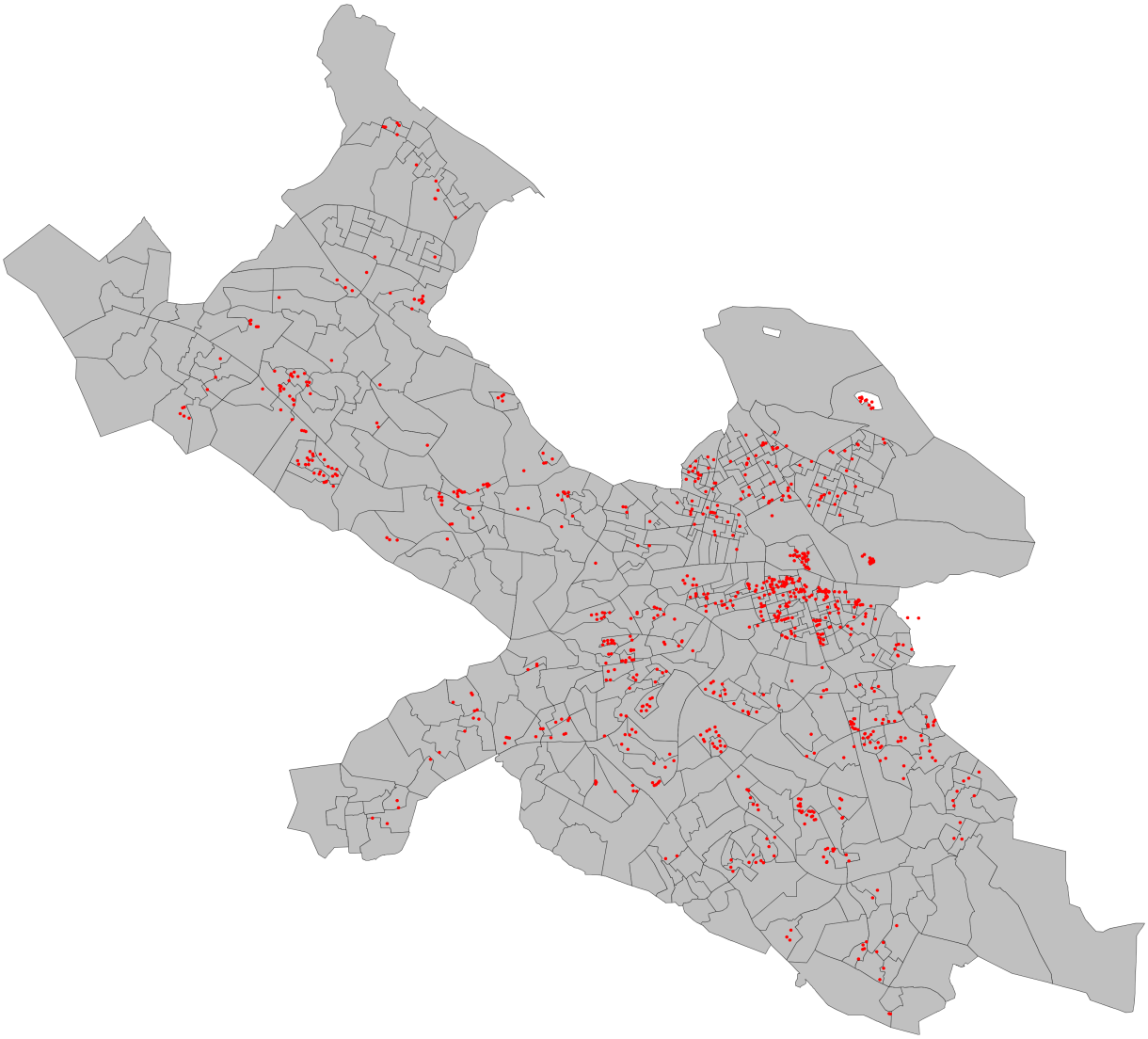
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

*Interaction between Election dummy & RTB(cont)

Appendix C – Mapping

The map below shows the electoral districts in Stockholm municipality (2010). Each red dot indicates the location of an apartment block that applied for the RTB programme. We should be careful when interpreting the apparent clustering of red dots in the city centre, as the sizes of these apartment blocks are generally smaller than the ones in the suburbs. What we measure in our regression analysis is not apartment blocks but number of apartments adjusted for voters per dwelling in each district. Thus, failing to account for the sizes of the apartment blocks and the sizes of the districts (larger districts are less densely populated), might lead to the wrong conclusion about the distribution of RTB participation in the city.

Figure C1: RTB-applications mapped into 2010 electoral districts



Sources: The Swedish Election Authority, Svenska Bostäder, Familjehem, Stockholmshem and Stockholms Stadsbyggnadskontor.