You shall not build! (until tomorrow) [:] Electoral cycles and housing policies in Germany

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You shall not build! (until tomorrow)

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Abstract: This paper investigates whether local politicians adjust their approval of housing licenses during election times in Germany. For my research, I use a balanced Panel of 4,983 West German municipalities from 2002 to 2010. Due to the timing of local elections in Germany, I can disentangle the election effect from common time effects. My results suggest a decrease in new construction approvals for residential housing areas during election years of around 11 % evaluated at the mean, and an increase of similar magnitude in the years following the election. Furthermore, I am able to show that the decrease during election times is associated with the share of homeowners in a municipality. Hence, my research adds to the literature on political business cycles as well as to the existing literature on homevoters and their potential effect on housing policies during election times.

Keywords: Urban development policies, land use regulations, homevoter, political business cycle, housing policies
JEL classification: D72, R31, H79

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1 Introduction

Since the beginning of the year 2000, the German Real Estate Market has experienced an increase in both housing prices and rents. However, these developments differ across Germany; agglomerations experience a sharp increase while housing prices and rents in rural areas remain stable or even decrease slightly (BBSR, 2016). This uneven distribution is caused by an emerging reurbanization pattern combined with a low supply of living space in urban areas. The German Government has responded either with stricter rent controls or more relaxed approval policies for new construction projects to increase housing supply as policy tools to counteract this development.

This study contributes to the understanding of the reasons for a lack of housing supply in agglomerations by investigating one of the factors that might have contributed to this issue. Specifically, I examine whether construction approval policies change during election periods. My results show a decrease of housing approvals during municipal elections in West German municipalities. This is especially important since these adjustments might delay life cycle related immigration outflows from agglomerations to suburbs. It is likely that these potential homeowners then reside for a longer period in the agglomeration, which would exacerbate the shortage of housing supply and hence might contribute to the recent increase in real estate prices in urban areas.

The German housing market is an interesting study object, since German metropolitan areas still have comparably low rent to income ratios compared to other European metropolitan areas like Paris or London. Nevertheless, the German agglomerations seem to follow the same path as their European counterparts and converge towards similar real estate price levels. This gives an interesting opportunity to study the cause of rent developments in agglomerations and the underlying reasons.

Historically, Germany has a low homeowner rate which has been explained by comparably low subsidies, a strong social housing sector and a low rate of policy interventions. This led to stable prices and relatively high housing quality in Germany. Hence, the incentive to invest in private housing property was not as strong for individuals as in other countries (Voigtländer, 2009). Interestingly, the homeowner rate shows considerable spatial variation, where especially the West German federal states experience higher homeowner rates. Lerbs and Oberst (2014) investigated the spatial variation of homeowner rates and find that the relative price of renting over buying seems to drive this development.

Regarding life satisfaction of homeowners in Germany, Zumbro (2014) found that they are slightly happier on average. This finding is explained especially by the quality of dwellings and is more pronounced within lower income groups.

For my analysis, I use a panel of 4,983 West German municipalities between 1,000 and 20,000 inhabitants for the years 2002 to 2010. These study objects are not large entities like Frankfurt or Munich but small to medium sized municipalities located around these agglomerations. Using the approved residence square meters (sqm) per 1,000 inhabitants as dependent variable, I conduct fixed effects regression including municipality and year fixed effects. The timing of local elections across federal states allows me to identify the effect of elections on housing approvals. Evaluated at the mean, my results suggest an 11.4 % decrease of approved housing sqm during election times and a catch up effect of similar magnitude in the years after the election. These results remain economically and statistically significant throughout different robustness checks.

Furthermore, I investigate whether the share of homeowners in a municipality explains this disruption during election times. The negative effect on housing approval during elections seems to be strongest for municipalities above the 90th percentile share of homeowners. Furthermore, this effect only occurs in single and double family houses. This can be interpreted as suggestive evidence that this effect is caused by homevoters (Fischel, 2001), residential homeowners with a strong preference for the status quo in their neighborhood.

My study contributes to two strands of literature. First, it adds an additional policy dimension to the literature about political business cycles. Initially proposed by Nordhaus (1975), his theory suggests that politicians have an incentive to alter their fiscal policies during election times in order to increase their re-election probabilities. Usually, this literature is mostly concerned with governmental budgeting and spending during election times but my research shows that other parameters are also in the interest of politicians. This theory has been applied successfully to different political levels (e.g. national, state, county and municipal elections) and different countries((Alesina, Roubini, and Cohem, 1997; Brender and Drazen, 2005; Drazen and Eslava, 2010; Golden and Poterba, 1980; Klomp and De Haan, 2013; Schuknecht, 1999)).[1]

The literature also shows strong effects on governmental budgeting in Germany. However,

[1]For a literature overview of the relationship between election cycles and government spending see Foremny, Freier, Moessinger, and Yeter (2014).

it is important to distinguish which administrative level is subject to research in Germany, since governmental layers have different discretionary leeways and are affected by different kinds of elections. In Germany, elections take place on the federal level for the German Parliament, on the state level for the parliaments of the *Länder* and on the municipality level for the local governments.

Starting at the state level, Mechtel and Potrafke (2013) show that job creation schemes are more pronounced during election periods. Englmaier and Stowasser (2014) look at the allocation of loans by state owned banks during county elections and find an increase in loans during election times. Schneider (2009) analyses German NUTS1 regions and discusses different channels of manipulating the budgetary business cycle. Using a panel of West German cities, Furdas, Homolkova, and Kis-Katos (2015) examine spending patterns of West German cities during election times and find some evidence for adjustment of spending in different categories. Turning to the municipal level, Foremny and Riedel (2014) show that elections affect the tax setting pattern of municipalities regarding the business tax. Foremny, Freier, Moessinger, and Yeter (2014) examine spending patterns on a local level, where they distinguish the election effect of the legislative and the executive. To the best of my knowledge, there is no literature that shows whether the election cycle directly affects housing policies. Furthermore, it is also the first study for Germany that considers a local policy parameter that is not related to budgeting.

Second, my results also contribute to the ongoing discussion on how political alignment, council composition and the election system affect local housing policies. For Spain, Sole-Olle and Viladecans-Marsal (2012) show that mayors with a larger majority tend to declare more land for development in a municipality independently of the political affiliation. In addition, Sole-Olle and Viladecans-Marsal (2013) show that left wing governments in Spain have a tendency to convert less rural land into areas open for development. Garmann (2014) shows that mayors elected by the municipal council assign more land open for development than mayors elected directly by the constituency. The direct effect of local elections on housing policies has not yet been examined.

In assessing electoral effects, the question arises naturally which voters are most sensible to urban policies. The literature identifies the so-called homevoter. First introduced by Brueckner and Lai (1996) in a classic monocentric city model, they model the homevoter as landlord and tenant simultaneously. According to Fischel (2001) the homevoter pools his savings in his house which then serves as his only asset. Hence, homevoters approve

policies that increase the value of their house and, of course, try to avoid policy measures that decrease housing prices, which might occur via the supply side or by negative side effects of urbanization like pollution and crime. Therefore, homevoters usually disapprove of policies that increase housing supply and hence decrease the value of their asset[2]. The occurrence of increasing homeownership and resistance to urban growth is also described in Ortalo-Magne and Prat (2014).

So what should be the expected effect of the presence of homevoters during election times? Since homevoters will aim to preserve the value of their houses, all municipal projects that foster urbanization will be opposed. Local politicians should be aware of this occurrence and if homevoters are crucial for reelection it is likely that the homevoters' calculus is taken into account by local politicians during local election times. This should hence be reflected in lower housing approval rates during election periods. Whether this result occurs in the German case is worth exploring since the homeowner rate is lower in Germany than in the US and other European countries.

For the US, the literature finds general support for the homevoter hypothesis (Brunner, Sonstelie, and Thayer, 2001; Brunner and Sonstelie, 2003; Hilber and Mayer, 2009; Dehring, Depken, and Ward, 2008; Gerber, 1999; Gerber and Phillips, 2004). This effect is usually identified via public construction projects such as football stadiums or local school voucher initiatives.

Regarding the German context, the literature discusses different voting patterns between homevoters and leasevoters (citizens that rent instead of own an apartment). Furthermore, the literature finds a tendency against structural with regards to housing and urban development. Based on a referendum against part of the property investment project *Mediaspree* in Berlin (Germany), Ahlfeldt (2011) shows that the resistance against further construction is mostly driven by an expected loss of cultural amenities within cities. Ahlfeldt (2012) examines the pattern for a referendum on a soccer stadium in Munich in 2011 and finds that voters (regardless of being home- or leasevoters) in proximity to the project strongly voted against it, even though there was high support on the aggregated level. Ahlfeldt and Maennig (2015) apply the homevoter hypothesis to a public referendum on an airport concept for Berlin. They show that homevoters, unlike leasevoters,

[2]This statement applies in most cases but might be offset in cases where a certain population threshold enables the municipality to benefit from the provision of public goods like schools, kindergartens or public transport connections.

tend to vote more strongly in favor of topics that positively affect amenities surrounding their house. Where the literature on homevoters uses referendums to identify a possible homevoter effect, my study is the first to show that the homevoter might also affect policies during election times.

This paper is organized as follows: section 2 describes German municipalities, local elections and the underlying dataset. Section 3 describes my empirical strategy, shows my main results and applies robustness tests. Section 4 shows how homevoters seems to drive my main results and discusses alternative channels. Section 5 concludes my research.

2 Institutional setting and data

2.1 German municipalities

Municipalities are the lowest administrative tier in Germany. They provide a large variety of public goods (zoning, infrastructure, kindergartens and general public services) of which some are mandatory and other voluntary. Furthermore, some of these public goods are provided in cooperation with higher-tier administrative layers and others are solely the responsibility of the municipality. Most importantly, municipalities have a constitutional right to self-administration (*Kommunale Selbstverwaltung, Art. 28 Abs. 2 GG*).

This municipal self-administration is manifested by giving municipalities autonomy over land-use-planning as well as local taxation (business tax and two kinds of property taxes). Furthermore, municipalities have a high degree of autonomy for municipal land use plans, which is only limited by state and federal construction law. Revenues from local taxes account for 20% - 30% of the total municipal revenue. Hence, municipal funding is partly predetermined but municipalities have discretion here via local fees and local taxes and parameters on the spending side.

The permission for a construction project is in the hands of the building control authority (*Bauaufsichtsbehörde*), which checks whether the project fits the municipal zoning plans and if existing construction laws are violated. Therefore, municipalities do not have

discretion over the process of approval, although they have powerful tools (like zoning) to lay the framework in which the building control authority may decide. This is important since the building control authority is usually located in the county administration and not in the municipality itself.[3]

In general, municipalities have four tools to shape local land use and the building structure. The Land use plan for the next 15 to 20 years (*Flaechennutzungsplan, §5 Abs. 1 Satz 1 BauGB*) and the municipal construction plan (*Bebauungsplan, §8 Abs. 1 Satz 1 BauGB*) act more as general plans / macro tools, a development freeze (*Veränderungssperre, §14,15 BauGB*) and the municipal accord (*Gemeindliches Einvernehmen, §36 BauGB*) act as micro tools for the urban shape. In the following, these tools will be described in more detail.[4]

The most general tool is the land use plan which assigns the residential, commercial and agricultural land use within municipalities. This plan is usually developed by the local municipality and approved by a higher tier of administration, usually the county. Nevertheless, this plan is merely a planning tool and not legally binding. The construction plan on the other hand is legally binding. Where the land use plan has to be created for the whole municipal area, the construction plan gives specifics about parceled areas open for development. Here, municipalities have full discretion to develop these plans by taking existing laws, like environmental issues, into account.

If municipalities strongly oppose certain construction projects even though they fit into the current municipal construction plan, municipalities have micro tools at hand. The most common tool is the development freeze, which allows municipalities to freeze a construction approval process in order to adjust the municipal construction plan. Usually this freeze lasts for two years but can be extended on a yearly basis for up to four years. It is also important to note that a construction freeze requires legal justification, which

[3]Some big municipalities also have the building authority within their boundaries. This is, however, more of an exception. Any effects regarding the location of the building authority are negligible, since this paper focuses on smaller municipalities. Furthermore, standard errors are later clustered on a higher administrative level to take this effect into account.

[4]Detailed construction laws differ within West German federal states, although the legal tools described above are the same in all states. The later analysis will take this differences into account by using municipality fixed effects which include state fixed effects as well.

means it can not be prohibitive without a reason.[5]

A further micro tool is the municipal accord. When the building control authority receives the building application, it informs the municipality about the project. Then the municipality has two months to respond, otherwise the municipal accord is implicitly assumed. A municipality is legally not allowed to refuse consent for other than legal reasons. Furthermore, a municipality may give its consent with certain imposts, which might delay construction projects as well if legal concerns are valid. Nevertheless, it seems that in practice municipalities sometimes withhold consent for political reasons (Dirnberger, 2008), even though they might face legal consequences. Theoretically, the municipality has the best chance to withhold municipal consent if the parceled area does not have a municipal construction plan. Here, a municipality could argue that they need to adjust the construction plan if the building under construction might not fit into neighborhood.

In 2010[6], the federal court ruled that the building control authority was to be held accountable if municipal accord was illegally denied by the municipality. Previously, the accountability in case of an unlawfully declined construction project was not clearly divided between the county administration and the municipality. Hence, neither the building administration, nor the municipality were per se accountable and usually a court had to decide. Since then, municipally accords have not been used with such laxness because the county administration was controlling more strictly. This also justifies the use of the year 2010 as the upper bound of my panel, because it is likely that this effect diminishes.

So how may a municipality limit construction approvals in election times? Since the land use plan is rather imprecise and not legally binding, it does not allow a micro influence on certain projects. The municipal accord might be a potential angle but is also rather costly due to the potential legal consequences if denied for illegal reasons. The most promising tool might be the development freeze for specific development projects. Here, municipalities may either delay the approval until the elections are completed or change the municipal construction plan in such a way that inhabitants approve the process more or the project becomes unfeasible. In practice, this might for example mean that a

[5]All factors that can be determined in the construction plan are listed in §9 BauGB. In total there are 26 different factors that need to be determined. Specifically interesting for this paper are the amount of maximum apartments in one house, the amount of public housing, as well as environmentally protected areas.

[6]Specifically, the federal court ruled on this under case number *III ZR 29/10 from 16.09.2010*

new construction project is not allowed to have as many flats as initially planned or the construction plan should include more green areas in order to make the project unfeasible or at least be delayed.

2.2 Municipal elections

In Germany, municipal elections are usually held every five years, with the exception of Bavaria, where elections take place every six years. A summary of all municipal elections and their respective timings in the relevant period can be found in Figure 1. As one can see, municipal elections are not held simultaneously across the federal states during the period of my study. In 2014, the elections tend to cluster strongly, but this period is excluded from my research. It is interesting to note that even if local elections take place in the same year across different federal states, the timing within the year still differs. This is indicated by different lengths of the blue bars in Figure 1, where the length represents the share of the year until the election takes place.[7]

Local elections serve the purpose of electing the municipal council. The discretion of the mayor and the council varies slightly between the federal states but the municipality itself is always in charge of the local land use process. As pointed out, the timing of the local elections allows me to identify the variation of housing approval during election times and disentangles the election effect from common time effects.

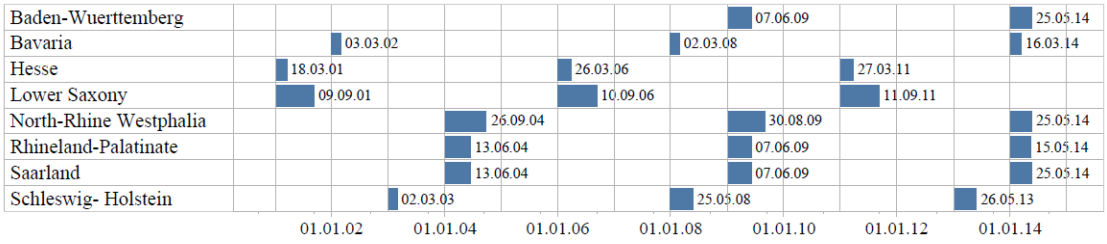


Figure 1: Municipal election dates and the intra-year timing in West Germany

[7] This intra-year timing will be taken into account by the econometric specification later on.

2.3 Data

The German Federal Statistics Office provides a rich data set on the municipal level, which allows a disaggregated analysis over time. Specifically, yearly data from all municipalities from 2002 until 2014 is provided. This enables me to observe housing approval policies over time and track their changes during election periods. All cross sections from 2008 onward were collected online[8], while all cross sections before 2008 were obtained from *Statistik lokal* CDs from the German Statistics Office. Furthermore, I enriched my dataset with information from the German census in 2011[9].

Regarding housing data, I collected the approved and finished housing and non-housing (*Nutzfläche*) area in square-meters (sqm). Using the approved sqm should provide sufficient variation to observe potential changes during election times. Furthermore, the data allows a distinction regarding finished construction and building licenses. Here, I gathered data on the absolute number of approved and finished houses and apartments. Furthermore the data allows me to track the approvals, completion and stock of houses with one, two or three and more apartments over time. My main dependent variable will be the ratio of approved sqm of housing units per 1,000 inhabitants. This should allow me to take different size effects of municipalities into account[10].

As independent variables over time, I include the population and the population density to take the total size and agglomeration differences into account. My data also allows me to model the demographic structure of the municipality over time by using the share of young (below 15 years) and old (above 65 years) inhabitants in the municipality. In order to proxy income differences across municipalities, I include the municipal share of federal income tax per capita. I collected the local tax rates and their respective tax bases, which are, however, not included. Foremny and Riedel (2014) show that business taxes are also manipulated during elections, which would feed into the error term and hence cause endogeneity. The two local property taxes A and B are at the discretion of local politicians, which leads me to the conclusion that these variables should also be excluded[11].

[8] www.regionalstatistik.de

[9] https://ergebnisse.zensus2011.de

[10] In further robustness checks, I will use different normalizations to verify my results.

[11] Initial political budget cycle regressions for the rate of the property tax B shows some adjustment during election times. This encourages me to omit the tax rates from the regressions. The results are available from the author upon request.

In addition, I gathered the share of homeowners from the national census in 2011 on the municipality level. This data allows a test of the homevoter hypothesis, as outlined above. Unfortunately, information about homeowners is only available for the year 2011, which of course allows only a limited test and implicitly assumes that the relative spatial distribution of homeowners among municipalities is constant over time. I do not think that this assumption is too strict, since the homeowner rate should have a low within variation and for this variable I am mainly interested in the between variation during election times.

My dataset covers all West German municipalities from 2002 until 2014. Nevertheless, I limit my panel to observations until the year 2010 since it is likely that the leeway for municipalities to delay construction approvals decreased from 2011 on due to the ruling of the federal court (see section 2.1). In addition, the municipal data was changed by census corrections from 2011 onwards, which would introduce a structural break in my dataset.

Municipalities that were subject to mergers were excluded to avoid confounding factors. Furthermore, I excluded municipalities with less than 1,000 and more than 20,000 inhabitants. I set 20,000[12] inhabitants as my upper boundary for the included municipalities, since it is the official threshold for small towns in Germany. Small municipalities with less than 1,000 inhabitants were excluded as well due to a lack of variation in the housing approvals, as I will explain the following paragraphs.

Using the area of housing approval as dependent variable provides sufficient variation to identify my effect of interest as one can see in Table 1. In the raw data, the housing sqm are coded in units of 1000 sqm, where the last two digits are missing (i.e. 1.2 instead of 1233). Therefore, this variable accumulates some measurement error which should be more severe in small municipalities. Furthermore, observations with less than 100 sqm were censored for reasons of data protection. Both types of censoring justify my approach removing the smallest municipalities from my estimation sample. Losing the last digits removes valuable variation, which matters most in small municipalities. Moreover, small municipalities have, in absolute terms, a lower value of approved sqm and are therefore more likely to be affected by the bottom censoring. However, the measurement error that remains within my observations due to the censoring is likely to be randomly distributed

[12]Even when increasing the upper threshold, the results remain similar. The results are available from the author upon request.

and should only increase the confidence intervals.

The descriptive statistics can be found in Table 1. Here, one can see that the dependent variable "Approved housing area in sqm per 1,000 inhabitants" offers a reasonable variation to identify potential election effects. The finished housing area sqm per 1,000 inhabitants offers a similar variation and hence it is natural to use this variable in a placebo test in the empirical section.

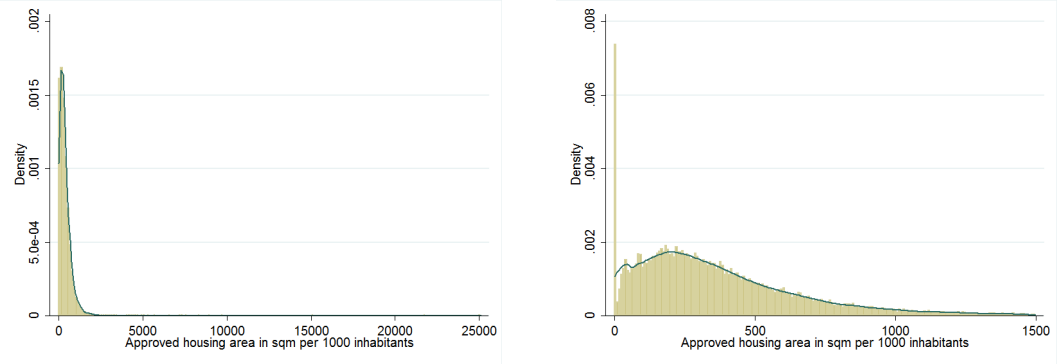
Table 1: Descriptive statistics of main dependent variables

	Mean	Std. Dev.	Min	Max	N
Approved housing area in sqm per 1,000 inhabitants	388	395	0	25,092	44,847
Finished housing area in sqm per 1,000 inhabitants	329	322	0	25,302	44,847
Population	5,025	4,286	1000	20,000	44,847
Share young(<15yrs.)	0.161	0.022	0.056	0.275	44,847
Share old(>65yrs.)	0.184	0.034	0.059	0.419	44,847
Population density	21.70	23.53	1.37	306	44,847
Share income tax per capita	295.52	84.15	0	779	44,847
Share of homeowners (0 - 100)	67.28	8.9	30.22	91.26	4,983

Source: Own calculations based on official statistics provided by the Federal Statistical Office.

When visualizing the dependent variable in Figure 2, one can see on the LHS in 2a, that the sample has some severe outliers which occur, however, in small numbers. Hence, I cut the distribution on the RHS to inspect the densest part of the data in more detail, which is shown in Figure 2b. Two features are striking; first, one can see that the dependent variable accumulates a certain amount of zeros (around 2500 observations) and that the distribution is otherwise smooth around the mean. This graphs shows the necessity to cut the distribution at the upper and the lower tail in robustness checks later on. Nevertheless, I keep these observations for my main analysis since this allows me to work with a balanced panel.

In the next step, I visualize the time pattern of my dependent variable. First, I plot the trajectories of the (mean) approved housing area by NUTS1 regions in Figure 3. It



(a) Full distribution, bins = 200 (b) Outliers excluded, bins = 200

Figure 2: Density of the dependent variable

is apparent that all federal states follow a similar macro pattern of decreasing housing approvals over time which is mainly driven by a decrease in governmental social housing policies during the period. However, sufficient micro variation within the states can also be observed.

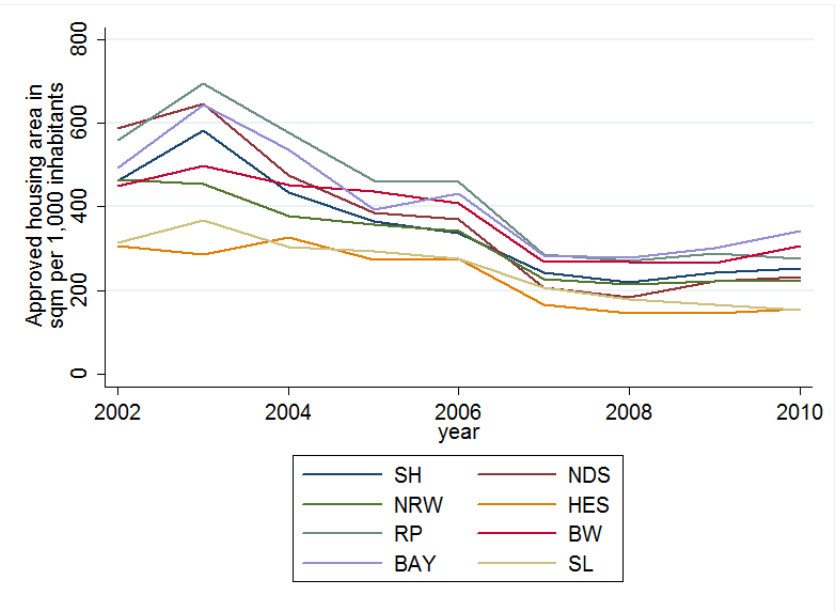


Figure 3: Development of approved housing area over time by Nuts1 regions

3 Empirical Strategy and Main Results

3.1 Empirical Model

My main empirical model looks as follows:

$$y_{i,t} = \sum elec_{i,t}\theta + \beta x_{i,t} + \mu_i + \tau_t + \epsilon_{i,t} \tag{1}$$

In (1), $y_{i,t}$ denotes the dependent variable, i.e. "Approved housing area in sqm per 1,000 inhabitants". $x_{i,t}$ is a matrix of the covariates as described in section 2.3. μ_i and τ_t indicate municipality and year effects, respectively. $\epsilon_{i,t}$ represents a well-behaved error term. The coefficient of interest is θ and defined as follows:

$$elec = \begin{pmatrix} Election_{t-1} \\ Election_t \\ Election_{t+1} \end{pmatrix} \quad and \quad \begin{matrix} = \\ = \\ = \end{matrix} \quad \begin{matrix} 1 \text{ in the pre} - \text{election year, } 0 \text{ otherwise} \\ \frac{days\ until\ election}{365} \text{ in the election year, } 0 \text{ otherwise} \\ 1 \text{ in the post} - \text{election year, } 0 \text{ otherwise} \end{matrix}$$

Since municipal election dates vary considerably between the federal states[13] politicians have different time budgets to adjust housing policies in election years. A simple binary dummy for the election year would blur this effect, i.e. an effect for an early election might be overestimated, whereas the effect for a late election could be underestimated. Hence, the dummy is replaced with a weight between zero and unity, given how far the election stretches into the year[14]. For example, if the election takes place on the 31st of December in the given year, this weight will be unity. Otherwise it will be a fraction of the days already passed in the given year. The precise weight of each specific election year dummy can be inferred from Figure 1.

This setup of the election dummies allows me to disentangle the election year effects from the post and pre election year effects. Since election dates vary considerably between the

[13] During the investigated period, the earliest election took place after 60 days had passed, whereas the latest election took place after 269 days had passed in the year

[14] In further robustness tests, I will also compare the performance of this specification with more traditional ones.

federal states, my approach resembles a Difference-in-Difference setting, where I compare municipalities that are subject to local elections to municipalities that do not experience an election in the given year. Furthermore, the timing of the municipal elections should allow me to disentangle the election effect from the common negative time trend.

Using (1), I will try to answer two hypotheses. First, I will verify whether the business cycle indeed exists for housing approval. This should be indicated by a negative coefficient of θ in the election year and a positive coefficient of θ in the year after the election. Second, I will examine whether homeowners in the respective municipality might drive these results. This hypothesis will be investigated in section 4. Specifically, I will distinguish between different types of houses (single/double family houses and houses with three and more apartments) and also interact the election year dummies with the share of homeowners in 2011 using the census data. If the adjustment is caused by homeowners, I would expect that the results are more pronounced for single and double family houses and that municipalities with a higher share of homeowners drive these results. Single and double family houses should be most affected by the presence of homevoters because similar buildings tend to cluster together within municipalities. Since homevoters oppose development policies in their direct environment, I would expect that the negative election effect should vanish if the number of apartments per house is increased (which of course increases strongly the likelihood that the inhabitants are not homeowners any more) because these projects are not conducted regularly in the same area of single and double family houses. One might still assume that homevoters should also oppose apartment buildings to the supply side effect on the housing market. But it is less likely that apartments and single / double family houses satisfy the same demand structure on the real estate market due to potentially different qualities of the estate.

Due to the fact that the tools to postpone construction plans need a majority in the municipal council I do not expect that all municipalities show this pattern during election times. I suspect that the effects, which are evaluated at the mean, will be of rather small magnitude since a suitable housing project needs to be present in the election year to conduct this kind of policy.

3.2 Main results

The main results can be reviewed in Table 2, where all estimated model resemble (1). Columns (1) - (5) use the approved housing sqm per 1,000 inhabitants as dependent variable, where (5) is my preferred estimation. Starting with a pooled OLS in Column (1), I subsequently add municipality effects in (2), year effects in (3), a combination of both effects in (4) and control variables in (5). For the remainder of this paper, (5) will serve as my benchmark. Starting with pooled OLS in (1), one can see descriptively that a reduction in approved housing area had already occurred one year before the elections, which remains similar when adding municipality FE in (2). However, when controlling for year fixed effects in (3), one can see that the pattern shifts to a reduction in the election year and a catch up in the post election year. Furthermore, when combining both kinds of fixed effects, this decrease and catch-up process becomes more clear in (4). The covariates add some explanatory power to the model in (5) but do not change the main result, which is a first indication of the main result's robustness.

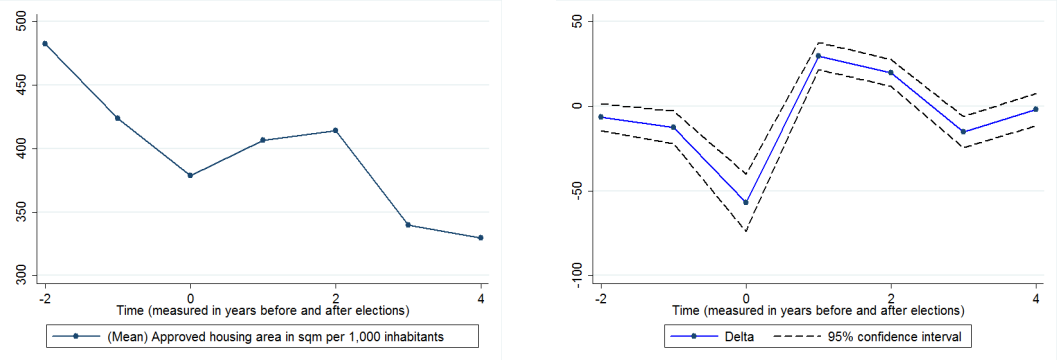
Regarding the economic significance of $Election_t$ in (5), the coefficient explains around 11.4 percent of the mean of approved housing area. For an average sized small town of 5,000 inhabitants, this translates to a decrease of around 200 sqm housing area during election times. This seems intuitive since the coefficient measures an evaluation at the mean. I suspect that not all municipalities use this tool during election times; it is merely a tool used when an application for a building permit is present.

In the next step, I plot the adjustment of housing approvals during election times. The results can be reviewed in Figure 4. Figure 4a on the LHS shows descriptive values where the time period is centered for the election year at $t = 0$. In Figure 4b on the RHS, I plot the regression results based on Table A.1 in Appendix A. Here, I regressed only one election coefficient at one point in time but with a longer time horizon regarding the election. By comparing both Figures, one can see the adjustment during election times. Furthermore, by controlling for common time effects, the overall negative trend on the LHS is removed on the RHS. Furthermore the results show that the catch-up process from delayed housing approvals persists up to two years after the election.

Table 2: Main results

	(1)	(2)	(3)	(4)	(5)
$Election_{t-1}$	-49.191***	-41.465***	-17.797***	-6.508	-5.614
	(5.609)	(4.892)	(5.513)	(4.941)	(4.977)
$Election_{t}$	-63.575***	-66.952***	-42.070***	-44.405***	-44.358***
	(10.594)	(8.502)	(11.194)	(8.931)	(8.909)
$Election_{t+1}$	7.376	8.886**	19.314***	22.367***	24.794***
	(5.094)	(3.694)	(5.184)	(4.187)	(4.137)
Municipality FE	No	Yes	No	Yes	Yes
Year FE	No	No	Yes	Yes	Yes
Control variables	No	No	No	No	Yes
Adjusted R^2	0.003	0.003	0.088	0.125	0.137
N	44,847	44,847	44,847	44,847	44,847

Notes: Dependent variable: Approved housing sqm per 1,000 inhabitants. Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.



(a) Raw trend without time effect

(b) Controlling for time effect

Figure 4: Housing approval pattern around election t = 0

3.3 Robustness

In the following subsection, I will conduct a series of checks to verify the robustness of my results. First, I will apply a series of placebo tests to examine whether my observed variation is caused by actions of municipalities during election times or merely a statistical artifact. This exercise is conducted in Table 3. Column (1) displays my main results as a benchmark. In (2), I replace the municipal election dummy with the state federal election dummy and (3) uses the finished, instead of the approved, housing area.

Regarding the state elections, I can not control for the year prior to and following the election since during my observation period some federal elections took place in two consecutive years in the same federal state. (2) verifies that there is no political adjustment of housing policies during state elections. (3) shows that the adjustment of housing approval during election times is not caused by random correlation. This is done by investigating the pattern of finished housing area during election periods. Finished housing area should not be at the discretion of local politicians because once the approval is given, the local landlord will follow his own agenda and schedule for the construction project[15]. Since the election coefficients are insignificant and of small magnitude in (3), I conclude that there is no willing adjustment of finished housing area but some alterations to the approved housing area. This is especially interesting, since approved and finished housing area have similar distributions as shown by the estimated density for both variables in Figure B.1 in Appendix B. These results verify that housing approvals are willingly adjusted during election times, where other elections from different tiers or finished housing area, which is not at the discretion of politicians, do not show any effect.[16]

In Table 4, I exclude single federal states to verify the stability of my results. One can see that magnitude and significance vary slightly but the sign of the coefficient remains unchanged. My results remain similar and therefore I conclude to observe a general phenomenon.

[15]Nevertheless, local politicians might still influence the finished housing area during election via timed housing approvals in the years before the election. Since each housing project follows a unique schedule, it is rather unlikely that housing approvals could be timed with such foresight.

[16]I also conducted regressions for finished non-housing area. Here, the confidence intervals of $Election_{t-1}$,$Election_t$ and $Election_{t+1}$ overlap strongly which indicates that there is no change of pattern during election times. I did not show the results since this regression has a very low R^2 of 0.023, where the significant coefficient might actually be misleading. The result is available from the author upon request.

Table 3: Placebo tests

Dependent Variable	(1) Approved housing per 1,000 inh.	(2) Approved housing per 1,000 inh.	(3) Finished housing per 1,000 inh.
Used elections	*Municipal*	*State*	*Municipal*
$Election_{t-1}$	-5.614		-6.343
	(4.977)		(4.321)
$Election_t$	-44.358***	4.919	5.408
	(8.909)	(4.027)	(9.747)
$Election_{t+1}$	24.794***		3.232
	(4.137)		(3.450)
Adjusted R^2	0.137	0.136	0.049
N	44,847	44,847	44,847

Notes: Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table 4: Exclude single federal states

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Excluded state	SH	NDS	NRW	Hessia	RPF	BW	Bay	SL
$Election_{t-1}$	-5.591	-5.204	-4.187	-12.294**	-9.010*	2.321	-3.312	-5.279
	(5.438)	(5.842)	(5.102)	(5.493)	(4.688)	(5.873)	(5.613)	(5.002)
$Election_t$	-50.666***	-43.272***	-40.702***	-48.893***	-37.420***	-53.734***	-36.590***	-43.624***
	(9.198)	(12.722)	(9.764)	(9.366)	(8.827)	(10.296)	(10.202)	(8.957)
$Election_{t+1}$	27.817***	24.640***	27.145***	20.670***	29.465***	18.399***	21.715***	25.326***
	(4.270)	(4.910)	(4.281)	(4.620)	(3.698)	(5.257)	(5.426)	(4.173)
Adjusted R^2	0.140	0.120	0.135	0.137	0.167	0.138	0.130	0.137
N	41643	39078	43308	41580	38925	36576	28323	44496

Notes: Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Afterwards, I cluster my main results on a higher administrative level in Table A.2 in Appendix A. Specifically, I follow the Classification for Territorial Units for Statistics NUTS (French: *Nomenclature des unités territoriales statistiques*). Here, I cluster on the NUTS1, NUTS2 and NUTS3 level.[17] In addition, I also cluster my standard errors on the NUTS2-year and the NUTS1-year level. The results remain significant throughout different kinds of clustering. Nevertheless the results for the NUTS1-year two-way clustering are only significant on the 3.9% level, which might lead to the conclusion that a part of the variation could be driven by federal state specific policies in certain years.

To examine this further, I repeated my baseline regression with linear and squared NUTS1 specific trends to see whether the results change. The results are shown in Table A.3 in Appendix A. In (1) and (2) I included the NUTS1 specific trends in a linear and quadratic dimension. In (3) and (4) I verified as well whether outliers drive my results and repeated the benchmark regression by omitting all observations with a higher value than 95% quantile in (3) and lower than the 5% quantile (i.e. I removed all zero entries) in (4). Throughout all tests, my results remain significant and of similar magnitude. Therefore I conclude that my results are not driven by federal state-specific policies nor outliers in my dependent variable. This increases the confidence in my results and I conclude that my observed pattern is not driven by some specific effects from a higher degree of administration like the counties or the federal state.

In a last series of robustness tests, I verify the stability of my results with regard to assumptions about the normalization of my dependent variable, the measurement of the election effect and the population threshold to include municipalities in the sample. First, I verify in Table A.4 in Appendix A whether different normalizations of the approved housing area show different patterns. (1) shows the baseline results for comparison, (2) simply shows the level of approved housing area in sqm without normalization, (3) is the log version of my baseline specification, (4) normalizes with the stock of housing area from the land use plan in 2004[18]. Throughout (2) to (4) the sign of the coefficients and the significances remain the same. Second, I change the specification of θ in Table

[17]NUTS units usually follow existing local administrative structures and consist of three levels. Usually the level of NUTS is assigned via population thresholds: NUTS1 corresponds in Germany to the *Länder*, NUTS2 to administrative districts (which do not have any governmental responsibilities in the German context) and NUTS3, which corresponds to German counties where the building authority is located.

[18]Land use data is only available in 4 year intervals before 2008 and on a yearly basis from 2009 on.

A.5 in Appendix A. In (1) I use my benchmark regression and add also a state election dummy. In (2), I combine $Election_t - 1$ and $Election_t$ into one coefficient and weight the indicator by the month in which the local election takes place. In (3) θ is weighted by the months that passed into the election year and (4) uses a traditional binary variable whether the given year is an election year or not. Throughout all specifications, my main results remain unchanged. Third and last in Table A.6 in Appendix A I vary the lower bound of the population threshold to include or exclude municipalities into the sample. When using any arbitrary population threshold for the lower bound, my main results remain unchanged. When I also include the smallest municipalities, i.e. municipalities with fewer than 500 inhabitants, the effect vanishes and the R-squared drops as well. This reflects the concerns regarding the bottom censoring I expressed in section 2.3.

4 Mechanism

So far, this paper has shown an adjustment of approved housing construction during election times in small to medium sized West German municipalities. This effect remains the same throughout different robustness tests. In this section, I will shed some light on the underlying mechanism behind this adjustment. I consider two explanations, which might be either an adjustment of politicians towards the homevoter or a general reduction in government activity during election times. An indirect test of political adjustment during election times would be to verify whether the adjustment could be explained by the presence of homeowners. If the results show a significant effect, I would interpret this as the first suggestive evidence in favor of the homevoter hypothesis. This would allow me to rule out reduced political activity during elections as an alternative explanation.

In order to test for my hypothesis, I conduct two empirical exercises. First, I verify whether the adjustment of approved housing licenses is driven by single family houses, which is more likely a concern of homevoters. Second, I use data from the census in 2011 and check whether the share of homeowners explains the adjustment during election times.

The first exercise is conducted in Table 5. (1) replaces the approved sqm with total sum of approved houses per 1,000 inhabitants. (2) uses the approved apartments instead of the houses, (3), (4) and (5) use only houses with one, two or three and more apartments

per house, respectively. First, one can see that the approved effect still persists in (1) and (2). Second, the effect is the most significant in economic and statistical terms in (3) and (4) for private residential buildings with only one or two apartment, which are most likely being constructed or owned by homeowners. It is interesting to note that the effect diminishes when housing categories with more than two apartments per house are considered in (5). This is first suggestive evidence for the homevoter hypothesis, since single / double family houses and multi-family houses usually tend to cluster together and homevoters care about their direct environment.

Table 5: Housing construction categories

Dependent variable	(1) Approved house licences per 1,000 inh.	(2) Approved apartments per 1,000 inh.	(3) House appr. with 1 app. per 1,000 inh.	(4) House appr. with 2 app. per 1,000 inh.	(5) House appr. with 3+ app. per 1,000 inh.
$Election_{t-1}$	-0.040	-0.050	-0.041	0.002	-0.011
	(0.035)	(0.042)	(0.034)	(0.010)	(0.019)
$Election_t$	-0.275***	-0.329***	-0.229***	-0.098***	-0.002
	(0.055)	(0.075)	(0.052)	(0.024)	(0.045)
$Election_{t+1}$	0.154***	0.189***	0.122***	0.052***	0.015
	(0.030)	(0.037)	(0.028)	(0.010)	(0.018)
Mean effect t	11.7%	11%	11.5%	17%	0.04%
Mean effect$t+1$	6.6%	6.3%	6.1%	9.5%	3.2%
Control variables	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.118	0.122	0.091	0.100	0.008
N	44,847	44,847	44,847	44,847	44,847

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

To examine this theory further, I verify whether the effect is more pronounced in municipalities with a higher share of homeowners. Therefore, I will interact the election year dummy with the share of homeowners in 2011 in the respective municipality. The modified version of(1) looks as follows:

$$y_{i,t} = \sum elec_{i,t}\theta + (elec_{i,t} \times v_{i,t})\,\eta + \beta x_{i,t} + \mu_i + \tau_t + \epsilon_{i,t} \tag{2}$$

In (2), all parameters are defined as before, where $v_{i,t}$ is a dummy variable that defines in which quantile the share of the homeowners from the respective municipality is. Here, I split $v_{i,t}$ at different quantiles (25%,50%,75%,90%). η denotes the estimation coefficient.

The results are shown in Table 6. In (1) $v_{i,t}$ indicates whether the respective municipality is in the first quartile of the homeowner shares, in (2) $v_{i,t}$ indicates whether the homeowner share is above the median, in (3) $v_{i,t}$ represents whether a municipality has a homeowner share in the fourth quartile and in (4) $v_{i,t}$ represents whether a municipality is above the 90% quantile. In (5) and (6) I only use municipalities with a share of homeowners in the first or fourth quartile.

The coefficients show the expected sign according to the homevoter hypothesis. Where lowest share of homeowners almost negates the election year effect $Election_t$, the effect becomes negative and increases in magnitude when the homeowner share is relatively high, i.e. in the 90% quantile. The effect for the 25% and 90% also shows statistic significance on the 10% value but the effect of the homevoter becomes more significant when comparing only municipalities with the highest and lowest share of homeowners in (6). Furthermore, the mean effect more than doubles when using only municipalities with a high homevoter share. I interpret these results to mean that my observed negative election year effect is driven by the homeowners share. Combined with the fact that the negative variation stems from single and double family houses, I regard my findings as suggestive evidence for the homevoter hypothesis.

Table 6: Interactions with share of homeowners

Dataset	(1) Full Sample	(2) Full Sample	(3) Full Sample	(4) Full Sample	(5) Only 1st and 4th Quartile	(6) Only 1st and 4th Quartile
$Election_{t-1}$	-5.557	-5.602	-5.637	-5.656	-2.494	-2.560
	((4.973)	(4.976)	(4.977)	(4.973)	(8.478)	(8.478)
$Election_t$	-52.341***	-35.538***	-38.492***	-39.366***	-12.250	-21.405
	(10.284)	(10.730)	(9.591)	(8.984)	(16.253)	(13.676)
$Election_{t+1}$	24.858***	24.829***	24.797***	24.794***	31.173***	31.132***
	(4.141)	(4.143)	(4.137)	(4.136)	(6.818)	(6.789)
Homeowner Quantiles interacted with $Elec_t$						
25% <	33.337*					
	(17.250)					
> 50%		-18.473				
		(16.389)				
> 75%			-27.593		-46.842*	
			(25.026)		(28.372)	
> 90 %				-66.620*		-77.935**
				(36.935)		(38.869)
Adjusted R^2	0.137	0.137	0.137	0.137	0.107	0.107
N	44,847	44,847	44,847	44,847	22,419	22,419

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

My approach provides a straightforward setup to test for the homevoter hypothesis during

election cycles. Nevertheless, having only one cross section of homeowner shares provides rather little variation, as can be seen in Table 1. A clear direct test of this hypothesis would be to verify whether the amount of development freezes or declined municipal accords for housing construction approval decreases during election times and whether a causal effect of the lower approval rate is linked to the share of homeowners. In terms of a natural experiment it would be useful to randomly assign different shares of homevoters to a municipality and observe whether this causes an increase in measures in municipal councils to postpone construction approvals. However, exhaustive data regarding declined municipal accords and development freezes is not available[19] and the spatial variation of homeowners is rather consistent over time and not available as a time series. Therefore, my results will provide suggestive evidence for the homevoter effect and show that the presence of homeowners actually accompanies political business cycles for housing policies during election times.

5 Conclusion

This paper investigated the adjustment of housing policies during municipal election years in West German municipalities. Using a sample of 4,983 West German municipalities from 2002 to 2010, this paper shows that housing construction is reduced during election times and increased afterwards. The results remain unchanged economically and statistically after a variety of robustness tests.

Furthermore, this paper provides suggestive evidence that the magnitude of the effect is driven by the share of homeowners and single and double apartment houses, which are more likely in the hand of homeowners. Homevoters, i.e. citizens who own their house instead of leasing it, have the strongest incentive to oppose development policies in their neighborhood since they have an incentive to maintain the value of their main house. My paper contributes to the existing literature regarding the impact of the homevoter by showing the influence not only through local referendums but during election times as well. This adds an important new policy parameter to the literature on political business cycles, which is so far mostly concerned with governmental spending and budgeting.

[19] Gathering the data manually would require the screening of all municipal council protocols from all 4,983 municipalities over nine years.

The aforementioned mechanism is important because this effect might delay the natural urbanization /suburbanization processes, which might put even more pressure on the recent rent increase in agglomerations and the surrounding areas. Hence, the results imply the importance of clear responsibilities in housing policies. As this paper clearly shows, a non-transparent division of liability between different layers of governments for housing policies might lead to inefficient low housing approvals during election times. This mechanism is most efficiently dealt with by establishing clear responsibility structures, e.g. through measures like the court ruling in Germany in 2010.

Future research should investigate whether the share of homeowners can explain the use of legal tools by municipal councils to delay housing construction. It is important to investigate whether this construction approval delay manifests itself in higher housing prices and, if this effect exists, the spatial extent. Another fruitful topic for future research is to dig deeper into a causal effect of homeowners on construction approvals. Is the electoral effect actually caused by the homeowner or an unobserved factor that is strongly correlated with the share of homeowners?

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A Appendix - Tables

Table A.1: Different election timing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year	$t=-2$	$t=-1$	$t=0$	$t=1$	$t=2$	$t=3$	$t=4$
$Election_t$	-6.445	-12.426**	-57.025***	29.731***	19.737***	-15.215***	-2.047
	(4.012)	(4.911)	(8.645)	(4.074)	(4.069)	(4.691)	(4.880)

Notes: Robust Standard errors in parentheses: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Source*: Own calculations.

Table A.2: Standard error clustering of benchmark results

	(1)	(2)	(3)	(4)	(5)	(6)
Clustering	Municipality	NUTS3	NUTS2-year	NUTS2	NUTS1-year	NUTS1
$Election_{t-1}$			-5.614			
(p-value)	(0.259)	(0.344)	(0.567)	(0.413)	(0.709)	(0.525)
$Election_t$			-44.358			
(p-value)	(**0.000**)	(**0.000**)	(**0.005**)	(**0.000**)	(**0.039**)	(**0.008**)
$Election_{t+1}$			24.794			
(p-value)	(**0.000**)	(**0.000**)	(**0.020**)	(**0.002**)	(0.146)	(**0.015**)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes

Notes: P-values in parentheses: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Source*: Own calculations.

Table A.3: Remove outliers

	(1) Nuts1 trend linear	(2) Nuts1 trend squared	(3) Remove top 5%	(4) Remove zeros
$Election_{t-1}$	-0.886	1.790	-5.769**	-5.908
	(5.084)	(5.028)	(2.487)	(5.543)
$Election_t$	-45.265***	-35.390***	-25.737***	-43.867***
	(9.037)	(9.255)	(6.338)	(9.199)
$Election_{t+1}$	20.429***	21.925***	16.530***	24.444***
	(4.321)	(4.403)	(2.461)	(4.252)
Adjusted R^2	0.143	0.145	0.207	0.130
N	44,847	44,847	42,605	42,413

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table A.4: Different specifications of dependent variable

	(1) Approved housing sqm per 1,000 inh.	(2) Approved housing sqm	(3) log Approved housing sqm per 1,000 inh.	(4) Approved housing sqm per stock sqm 2004
$Election_{t-1}$	-5.614	-0.022	0.018*	0.249
	(4.977)	(0.021)	(0.011)	(0.385)
$Election_t$	-44.358***	-0.160***	-0.051**	-2.666***
	(8.909)	(0.046)	(0.025)	(0.568)
$Election_{t+1}$	24.794***	0.088***	0.069***	1.890***
	(4.137)	(0.017)	(0.010)	(0.344)
Adjusted R^2	0.137	0.134	0.096	0.080
N	44,847	44,847	44,847	44,847

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table A.5: Different specifications of election dummy

	(1) State elections	(2) Election month and pre-year	(3) Election month	(4) Binary election dummy
$Election_{t-1}$	-4.798		-5.654	-7.067
	(5.394)		(4.977)	(4.951)
$Election_t$	-44.624***	-12.907*	-44.011***	-17.608***
	(8.822)	(6.777)	(8.819)	(3.764)
$Election_{t+1}$	24.828***	26.535***	24.752***	23.982***
	(4.132)	(4.106)	(4.140)	(4.249)
$State\ Election_t$	2.027			
	(4.339)			
Adjusted R^2	0.137	0.137	0.137	0.137
N	44847	44847	44847	44847

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table A.6: Results with different population thresholds

Pop. threshold	(1) -	(2) >500	(3) >1000	(4) >1500	(5) >2000	(6) >2500	(7) >3000	(8) >5000
$Election_{t-1}$	0.850	-3.066	-5.614	-5.120	-3.657	-5.529	-5.740	-6.314
	(5.091)	(4.564)	(4.977)	(5.504)	(6.163)	(6.265)	(6.833)	(4.398)
$Election_t$	-3.664	-30.912***	-44.358***	-39.791***	-36.852***	-33.910***	-28.550***	-29.403***
	(15.921)	(8.805)	(8.909)	(9.187)	(9.406)	(9.576)	(9.610)	(10.298)
$Election_{t+1}$	14.071***	19.530***	24.794***	26.006***	27.086***	25.289***	23.365***	19.694***
	(4.835)	(3.857)	(4.137)	(3.648)	(3.695)	(3.695)	(3.819)	(4.377)
Adjusted R^2	0.065	0.131	0.137	0.156	0.156	0.166	0.163	0.214
N	68938	56113	44847	36873	31275	26910	23841	15408

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

B Appendix Figures

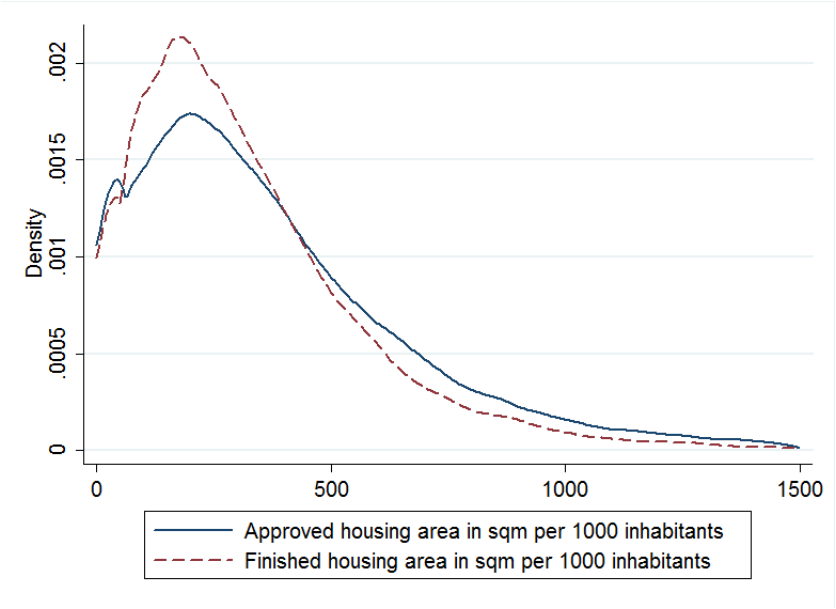

Figure B.1: Comparison of densities of approved and finished housing sqm. *Source*: Own calculations.