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Sebastian Garmann[1]

Does the Nomination Scheme of the City Manager Matter for Urban Development Policies?

Abstract

This paper examines the causal effect of a change in the nomination scheme of the city manager from appointment by the local council to election by the citizens on urban development policies. Using the fact that the timing of the reform was as good as random in municipalities of the German state Hesse, I can utilize a difference-in-difference framework to estimate this causal effect. I find that when the city manager is elected by the voters, there is significantly less urban development than when the city manager is appointed by the municipal council.

JEL Classification: H7, Q15, R52

Keywords: Urban development policies; form of local government; land use regulations; building licenses; difference-in-difference estimation; natural experiment

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1. Introduction

Anecdotal evidence suggests that urban development policies are one of the most important responsibilities of (local) governments from the citizens' viewpoint. Gerber and Philips (2004) report that in the United States granting voters access to direct democratic institutions to influence the urban development process leads to significant different outcomes in the decision process. In Germany, Stuttgart 21, a rail-way and urban development project, has led to large protests from citizens. The biggest of numerous demonstrations was visited by more than 100000 people. In one of the demonstrations, hundreds of demonstrators were injured because the police used water cannons, pepper spray and batons against them. As a last example, the recent protests in Turkey, although later extended to several other topics and of course also reflecting a general dissatisfaction with the Turkish government, were initially started to contest the urban development plan for Istanbul's Taksim Gezi Park.

Accordingly, it is not surprising that the urban economics literature expects that citizens condition their political decisions in elections on urban development policies.[1] This has led to the concept of "homevoters" (Fischel, 2001), i.e. homeowners whose voting is guided by their concern for home values. Therefore, one should also expect that the motivation of city officials regarding development policies depends to a large extent on the role and importance of the voters in the political selection process. To test for this idea, I estimate the causal impact of a change in the nomination scheme of the city manager (from appointment through the city council towards election by the citizens) on urban development policies using data from the German state Hesse. My hypothesis is that

[1] In fact, many political spectators in Germany hold the view that, apart from the Fukushima catastrophe, the protests against Stuttgart 21 have decided the state election in the German state Baden-Württemberg of which Stuttgart is the capital.

urban development policies are significantly different depending on whether the voters can influence who holds office or not.

This hypothesis is supported by a simple model developed in Solé-Ollé and Viladecans-Marsal (2012a). In line with the existing literature, the model assumes that the median voter is a homeowner and therefore opposing urban growth due to the potentially resulting devaluation of his estate. For example, urban growth might cause negative externalities (crime, pollution, noise etc.) or might decrease the demand for already developed land. Furthermore, the model assumes that there is a land developer lobby that would benefit from promoting urban growth and tries to bribe the local decision-makers in exchange for relaxing urban growth boundaries. The local decision-maker thus has to trade-off his gains from re-election (for example pay and prestige from holding political office) against the potential contributions from the land developer lobby. In this paper, I compare the case in which the re-election motive and thus the above mentioned trade-off is absent (when the city manager is appointed by the city council) with the case in which re-election incentives are introduced (when the manager is elected by the voters) and thus the city manager faces the above mentioned trade-off. Assuming that the median-voter is a landowner (an assumption that also seems realistic in German municipalities), the introduction of elections for the city manager should lead to less urban growth.

Most of the very scarce existing empirical literature that tries to explain the emergence of urban growth policies disregards political factors (see, e.g., Bates and Santerre, 1994; 2001; Evenson and Wheaton, 2003; Glaeser and Ward, 2009). Exceptions are given by Kahn (2011) and Solé-Ollé and Viladecans-Marsal (2012a, 2012b). Importantly and in line with Solé-Ollé and Viladecans-Marsal (2012a, 2012b), my paper has the benefits

that it is able to precisely match the land use decisions to the local government that is responsible for these decisions and to estimate a causal effect of the political factor of interest, i.e. the nomination scheme of the city manager, on urban development policies. The paper also contributes to a large literature that estimates how different forms of local governments influence policy outcomes (see, e.g., Booms, 1966; Morgan and Pelissero, 1980; Rauch, 1995), especially settled in the United States. However, only very few empirical papers employ natural experiments to credibly estimate causal effects and none of these papers is concerned with urban development policies.[2] Note that although my case study regards German local governments, it is also of direct interest to the US analogue: In the US as well as in Germany, local governments with a council-manager system can either have an elected or an appointed executive.[3] Additionally, this paper is related to a literature investigating the effect of re-election incentives on policy outcomes that mostly compares behaviour of politicians with (i.e."lame ducks") and without term limit. For example, both List and Sturm (2006) and Fredriksson et al. (2011) find that environmental policies are significantly different depending on whether there is a re-election motive for politicians.

I use a natural experiment in the German state Hesse that occurred in the 1990s. To be concrete, there was a state-ruled switch in the nomination scheme of the city manager from appointment through the local council to election by the voters. This switch was not implemented in the municipalities at a single point in time, but when the term of the

[2] Recently, Egger et al. (2011), Ade (2013) and Garmann (2013) use natural experiments to estimate causal effects of changes in local government structures on fiscal policies in Germany. However, none of these papers considers urban development decisions.

[3] In general, the local government systems in the United States and in Germany are quite similar as in both countries, there are two predominant forms of local governments: Council-manager and mayor-council systems. According to the Municipal Form of Government Survey conducted in 2011, more than 90% of all US local governments operate either under the council-manager or under the mayor-council system.

last not elected manager ended which differed across municipalities. Importantly, these different dates are not related to urban development policies, but driven by casual factors in an as good as random manner. This natural experiment can be exploited in difference-in-difference estimations in which those municipalities that have not switched to the new system at a point in time can be regarded as valid counterfactuals for those municipalities that have already switched.

Using the amount of change in urban land in land use plans as well as the number of building licenses as outcome variables, I find that the introduction of an elected city manager significantly restricts urban development. The estimates for both outcome variables are highly significant and are robust to changes in the estimated model, the inclusion of further control variables and tests that are typical in a difference-in-difference design. Moreover, it is also interesting to consider which specific type of urban development is affected by the change in the nomination scheme. Due to the richness of my data set, I am able to measure the causal effect of the change in the nomination scheme on different land use types (residential, agricultural and commercial areas) as well as on different types of building licenses (licenses for residential buildings vs. licenses for non-residential buildings). Performing such a sub-analysis, I find that the election of the city manager only affects residential urban development policies. I am not aware of any paper that is able to use such detailed data on land use plans.[4]

The remainder of the paper is structured as follows. Section 2 describes the data set used. In Section 3, more information on the institutional framework, including the role

[4] In fact, Solé-Ollé and Viladecans-Marsal (2012a) write that it would be of great interest to analyze land use plans for different land use categories like residential or commercial, but that these data are not available for their case study.

of the political actors in the urban development process, is provided. Section 4 explains the empirical strategy, while Section 5 presents the results. Section 6 discusses the robustness of the results and Section 7 explores which sub-category of urban development is most affected by the introduction of the elections. Finally, Section 8 concludes.

2. Data

For this paper, I have compiled a large unique data set covering all municipalities from the German state Hesse. All data are available from the statistical office of Hesse. Summary statistics for the data used in this paper can be found in Table 1. In my analysis, I will mainly focus on two different outcome variables. First, as a kind of natural measure for urban development policies, I will use the change in urban land as indicated by a general land use plan that is set up by the municipality. I define urban land as commercial and residential land that is either already developed or marked developable in the land use plan of the municipality (scaled by the total land area of the municipality).

As discussed by Solé-Ollé and Viladecans-Marsal (2012a), focusing on the general land use plan set up by the local government has several advantages as compared to alternative approaches (for example, using regulatory indices): First, the change of urban land in the land use plan can easily be matched to the local government that took the decision. By contrast, alternative approaches like regulatory indices can also be the result of long-term decisions that have been taken by previous governments. Second, land use *plans* only reflect the decisions made by the local government. An alternative would be to use the land actually developed instead of the plan. However, how much land will actually be developed is not alone the decision of the local government. This will cru-

cially depend on supply and demand factors in the private sector such that the plan developed by the government surely is a cleaner measure for urban development policy. Further advantages of the land use plan data in this paper are that I have panel data which allows me to control for time-invariant unobserved heterogeneity and that the land use plan also contains information on sub-categories of land use (for example agricultural, residential and commercial). I am not aware of any paper that is able to use such detailed land use plans.

Figure 1 shows a distribution of the change in urban land over a 4 year period. As can be seen, most changes are quite small, with a sizeable fraction of the municipalities even implementing no change. In fact, in 106 of the 370 relevant cases, a municipality did not change the land use plan concerning urban land. The histogram in Figure 1 also shows one relatively large negative value where one municipality did change urban land by more than 17%. All estimation results below are unchanged even if one regards this observation as an outlier and excludes it from the sample. Thus, I will only report results with the whole set of observations.

Despite the above mentioned advantages, the land development plans also have a shortcoming that motivates the usage of my second outcome variable: land development plans in Germany have been only recorded by the Statistical Offices since the beginning of the year 1989 and are also only available every four years. To be precise, for the time period of interest I have urban development plans recorded at the beginning of the years 1989, 1993 and 1997. Thus, I cannot use all local governments for the difference-in-difference estimation. For example, if a local government changed in 1995 to the election of the city manager, then it is impossible to say whether the change in urban land

from 1993 to 1997 has mainly been taken place in the time period 1993 to 1995 when the manager was appointed by the local council or in the time period 1995 to 1997 when the manager was elected by the voters. Therefore, I can only use those local governments that have changed to the new system in 1993 or later than 1996. This strongly reduces the number of observations. Moreover, the unavailability of data for more than one pre-treatment period makes the standard placebo tests implemented in most difference-in-difference estimations impossible.

Due to the limitation of the above outcome variable, I complement the analysis with a second outcome variable that is yearly available from 1992 on. Specifically, I use the number of building licenses for residential buildings that a municipality has issued as the second outcome variable. Because of the yearly and pre-treatment availability, I will be able to perform placebo tests to analyze whether the key assumption of a common time trend in the absence of the treatment holds. Moreover, I also have data on the number of building licenses for non-residential buildings which I use to evaluate whether the effect of the nomination scheme on urban development is restricted to residential buildings. By contrast to the land development plans, the number of building licenses, however, is not only dependent on the urban development policy, but also on the actions of private forces.[5] Thus, none of the used outcome variables is without criticism, but I think that using both of them makes me able to compensate for each other's shortcomings. Figure 2 shows the distribution of the number of building licenses for residential buildings.

[5] Note that in a recent contribution, Kahn (2011) also uses the number of building licenses as measure for urban development policies.

In the next section on the institutional background, I will further explain basic facts about the local government system and its reform, the land use plans and the role of building licenses.

3. Institutional Framework

Local government system

Hesse is one of the most populous German states and consists of 426 municipalities. Of these, 5 municipalities have also a county status. These 5 municipalities have different responsibilities than the other municipalities and also different ties to the state controlling authority concerning land use decisions.[6] Therefore, I only use the 421 municipalities that do not have a county status. The local governments in Hesse have a two-tier system consisting of both a city manager and a city council (see Schneider, 1981; Dreßler, 2010). Both tiers are elected in separate elections. The local council can be seen as the local legislative, whereas the city manager is the head of public administration in Hesse that implements the decisions made by the council.[7] The city manager is not part of the local council and has accordingly no voting or veto rights.[8] However, as further discussed below, the public administration has some decision power when it comes to administration acts such as the issuance of building licenses. Urban development policies like the design of land use plans or the issuance of building licenses are significant and important topics at the local political level, but municipalities have also

[6] For example, the land use decisions of municipalities without a county status are supervised by the controlling authority of the county and the state level. Naturally, land use decisions of a municipality with county status can only be supervised by the controlling authority of the state.

[7] To be precise, the head of the public administration is a small, cabinet-style institution called magistrate (typically consisting of the two persons plus the manager) that is chaired by the city manager. For more on the municipal constitution of Hesse, see Garmann (2013).

[8] In the local constitution of Hesse, the city manager is labeled as *Bürgermeister* (mayor). However, as he is not part of the city council and has no voting or veto rights, his competencies are in line with what the theoretical literature on local governments (see, e.g., Coate and Knight, 2011) names city manager. To stay in line with this theoretical literature, I also use the terminology „city manager“ here.

some competencies in other policy areas. They are allowed to set the tax rates for three different local taxes and are, for example, responsible for road maintenance, elementary schools or child care.

Before 1993, in all municipalities the city council was responsible for appointing the city manager. In the year 1993, a reform of the local government system became law which changed (only) one aspect of the adminitrative system:[9] From the year 1993 on, the city manager must be elected by the citizens of a municipality in a run-off election.[10] In this paper, I use that the reform was implemented in such a way that the municipalities did not switch to the new system at the same point in time, but instead respected the term of the preceding non-elected manager. The date of the switch therefore depends on the date on which the term of last not publicly elected mayor ended. This date differed across municipalities and was not under the control of the municipalities, but determined by casual factors unrelated to urban development policies. To be precise, the date of the switch depends on the history of terms of all previous city managers that were appointed by the municipal council. This history differed across municipalities for two reasons: First, the 421 municipalities were the result of a process of municipal mergers in the 1970s in Hesse that took several years. Some municipalities were, for political reasons, formed earlier than others and were thus able to earlier appoint their first city manager, which of course has an effect on all dates of subsequent city manager appointments. Second, in the years between the first appointed city managers and the switch to the new system, not all city managers served their full term. Some died during

[9] The reform was decided on at the beginning of 1991.

[10] In the run-off election, a candidate is elected if he receives more than 50% of the votes. If this does not happen in the first round, only the top two candidates from the first round make it to the second round. By design, one candidate will win more than 50% of the votes in the second round for sure. The term length of the manager is 6 years. Moreover, it is also important to note that after the reform, the voters even have the right to vote the city manager out of office before his term ends.

their term, some retired, while others were replaced before the end of the term due to incompetence or because the majority in the city council has changed after council elections. This, of course, has also affected the dates on which the terms of all subsequent city managers have ended. Due to the dependence of the date of the switch on the whole history of terms, it is unlikely that this date depends on urban development policies, especially because the reform could not have been expected when the dates of the switch were effectively determined. See Table 2 for a distribution of the timing of the switch to the new system. Thus, I will use this natural experiment to estimate the causal effect of a switch in the nomination scheme on urban development policies. With this design, it is possible to disentangle general state-wide time trends in urban development policies from the introduction of the elected manager.

Land use plans

In Germany, land use plans (see Koppitz and Schwarting, 2005) are a central element for municipalities to navigate urban development and to set boundaries on urban growth. Passing detailed land use plans that describe for the whole municipal area how the land should be used is a municipal responsibility. An important feature of the German system, for example in line with the Spanish system as described in Solé-Ollé and Viladecans-Marsal (2012a), is that although individuals own the land, it cannot be urbanized unless this can been explicitly allowed by the urban planning process. Indicating land as developable in the land use plan of a municipality states the will of a municipality to develop this land in the near future. Practically, this means that a change in the classification of land in the general land use plan can have significant effects on land prices and marking land as developable is thus able to create sizeable rents for landown-

ers, which motivates the assumption in land use models (see, e.g., Glaeser at al., 2005 and Hilber and Robert-Nicoud, 2013) of a land developer lobby that tries to convince local politicians to change the land use plan to their benefit.

Importantly, while actual land use plans are binding for municipalities, municipalities can easily change these land use plans without creating compensation rights for the landowners. In other words, municipalities can take back the decisions to indicate land as developable (of course only if this land is not build up). Thus, the change in urban land in the land use plan can be (and sometimes is) negative, whereas in the case study of Solé-Ollé and Viladecans-Marsal (2012a), land that is marked developable can only be transformed back under significant costs for the local governments such that "back transformations" practically do not occur.

It is also important to shed some light on the process that leads to the adoption of a new land use plan. While the municipal council has the last word when a new land use plan is passed, the law prescribes that all relevant actors in politics and society should be informed as early as possible about the intend to change the land use plan. All relevant actors have the right to comment on the new plan and to propose amendments. Thus, passing land use plans is a bargaining process between all relevant actors and it is likely that the city manager will play an important role. Moreover, it should also be stressed that local politics becomes significantly more difficult if the city council and the city manager are under control of different parties. Thus, it should be the case that the city council acts more in line with the voters' preferences regarding land use policies when the city manager is elected instead of appointed. By contrast, when the city manager is appointed by the city council, the scenario of a kind of "divided government" is almost

impossible to occur. This is an additional channel through which the introduction of elections for the city manager might influence land use policies.

Building licenses

In Germany, it is strictly prohibited by law to construct buildings like houses unless the home-builder (or more general, the responsible person) has been granted a building license. When examining whether a building license can be issued, the responsible authorities not only check whether the planned building visually fits into the urban environment or causes any danger for the citizens, but also whether the rights of third parties are violated by the new building. The German law thus acknowledges that the issuance of a building licenses gives some rights to the developer, but that on the other hand, third parties may be affected, for example through a devaluation of their estate. Accordingly, these affected third parties play an important role in the issuance process. Commonly, potential builders ask their (potentially new) neighbors to sign that they do not see their rights threatened by the new building and thus agree with the building process. If the neighbors do not sign such a statement, a building license can nonetheless be granted. However, in this case affected third parties must be notified about the issuance of a building license by the municipality and have the right to raise an objection against this decision. Then, it must be examined once again whether a building license can be granted. Thus, existing homeowners have, to a certain degree, the possibility to block new development or at least to make new development much more complicated for the potential developer.

In Hesse, the public administration which is chaired by the city manager is both responsible for the issuance of the building license and the supervision of the whole building process. As the issuance of the building license has some discretionary elements, the public administration is likely to use this discretion especially in those cases in which third parties do not approve the building process. It should also be kept in mind that these third parties most likely reside in the municipality, are therefore vote-eligible and potentially even themselves actors in the political process, for example in relatively small towns, whereas the potential developers often do not reside in the municipality yet and are therefore also not vote-eligible. These considerations make clear that the city manager has some incentives to disapprove urban development in case that there are re-election incentives.

4. Empirical strategy

In this section, I will explain the empirical strategies. As the empirical strategies differ in some ways depending on which outcome variable I use, I will explain these strategies in separate subsections.

Land development plans as outcome variable

As explained above, land use plans are only available for the beginning of the years 1989, 1993 and 1997. This must be taken into account when developing the difference-in-difference strategy with which I want to test for the effect of the nomination scheme of the city manager. Specifically, I will use those municipalities that have changed at the beginning of the year 1993 to the system of elections (and have therefore held first elections at the beginning of 1993) as the treatment group, while I use all those municipali-

ties that have not changed until 1997 to the new system as the control group. In total, there are 141 municipalities in the control group and 44 municipalities in the treatment group for which I observe changes in urban land in the land use plan in the time periods 1989-1993 and 1993-1997. I estimate the difference-in-difference regression

$$\Delta \text{UrbanLand}_{it} = \mu_i + \lambda_t + \beta T_{it} + \delta X_{it} + \varepsilon_{it}, t = 1997, 1993 \qquad (1)$$

where μ_i is a municipality-fixed effect, λ_t is a year-fixed effect, T_i is an indicator variable that takes the value 1 if a municipality i has already switched to the new system in time period t and 0 otherwise and X is a set of control variables that might influence the outcome of interest. Note that using fixed effects allows me to control for any time-invariant unobserved heterogeneity. For the calculation of the dependent variable and other land use variables employed in this paper see Table 3.[11]

As control variables, I include population size, population density, the age structure of the population (proportion of people aged below 15 and proportion of people aged above 65), political competition[12], electoral turnout at the council elections, a dummy for an absolute majority in the municipal council to proxy for coalition governments and dummies indicating the strength of the political parties in the municipal council (distinguishing between an absolute majority for the center-left SPD, an absolute majority for the center-right CDU and no absolute majorities).[13] Moreover, I cluster the standard er-

[11] Note that the total land area of a municipality is as good as constant over time. There are only very small changes due to corrections in the cartography.

[12] Political competition is measured as the difference in vote shares between the first-placed and second-placed party at the municipal council election. Note that additionally to the included control variables, it would have been interesting to also include the party affiliation of the city manager. However, this party affiliation is only recorded after the change in the nomination scheme.

[13] One could think of other factors influencing urban development policies, for example total land area of a municipality and other geographical factors like the distance to central cities, the distance to the coast etc.. However, as I have a panel data set, these time-invariant factors can be captured by the municipality-fixed effects. Moreover, income is not available at the municipality level in Germany, but I do not believe

rors at the municipality level to allow for heteroskedasticity and serial correlation within a municipality in arbitrary form.

Building licenses as outcome variable

As yearly data for the time period 1992-2000 are available in the case of building licenses as the outcome variable, the empirical strategy now has to yearly redefine treatment and control groups. In this case, the treatment group consists of those municipalities that have already switched to the new system, while those municipalities that have not yet switched to the different nomination scheme are used as control groups. For example, if one considers only the year 1993, those municipalities that have switched to the new system in 1993 are in the treatment group, while those that switched to the new system in 1994 or later are in the control group. For the years to come, treatment and control group are always redefined accordingly. Formally, this means that I estimate the same model as in (1), with the only difference that the periodicity of the data is now yearly:

$$\begin{aligned} &\text{BuildingLicenses}_{it} = \mu_i + \lambda_t + \beta T_{it} + \delta X_{it} + \varepsilon_{it}, \\ &t = 1992, 1993, ..., 1999, 2000 \end{aligned} \tag{2}$$

I will include the same covariates as indicated above and again, standard errors are clustered at the municipality level.

A further complication for the analysis of building licenses emerges because building licenses are measured as counts.[14] To deal with this issue, I also estimate the poisson regression model besides the conventional OLS regression model. However, the stand-

that income is subject to large changes in the relatively short time periods I use. Therefore, fixed effects should take up much of the variation in this case, too.

[14] For a discussion of count data models, see Winkelmann (2003) and Cameron and Trivedi (2006).

ard poisson regression model has the shortcoming that it cannot capture overdispersion, i.e. a variance that is greater than the mean which is the case for building licenses (as Figure 2 suggests and the summary statistics indicate, for the number of building licenses the standard deviation is as large as the mean, which implies that the variance is far greater than the mean). To further investigate the robustness of my analysis, I thus also estimate the negative binomial regression which has been designed for count data in the presence of overdispersion. Note that there is a problem of overdispersion, but not of excess zeros for the number of building licenses. While both phenomena often appear at the same time (see, e.g., Becker et al., 2012), this is not the case for my analysis.

The availability of yearly and pre-treatment data offers better possibilities to test the fundamental common time trends assumption in the difference-in-difference framework. To provide evidence that this key assumption is met, I run the following placebo regression:

$$\text{BuildingLicenses}_{it} = \mu_i + \lambda_t + \beta_1 T_{it+1} + \beta_2 T_{it+2} + \delta X_{it} + \varepsilon_{it} \qquad (3)$$

Thus, I investigate whether the change of the nomination scheme in the future has any effects on the pre-treatment outcome variable. If so, this could mean that municipalities that will switch in the next time periods have fundamentally different time trends from those that do not and thus would raise doubts regarding the common time trends assumption. Note that I also experimented with more and less than two lead variables on the right-hand side. However, the results were not different.

5. Results

Table 4 shows the results for the case of the change in urban land as the outcome variable. From the included control variables, only electoral turnout at the municipal council

election is statistically significant. Note, however, that due to the inclusion of municipality-fixed effects, the coefficients of the explanatory variables measure the effect of changes in these variables rather than level effects. This might explain the non-significance of other control variables as the within-variation might be quite low. Further note that of course one cannot interpret the coefficients of the control variables as causal effects as none of these control variables is randomly assigned to the municipalities. For the main coefficient of interest for which one can claim causality, I find a negative effect that is significant at the 5%-level. Thus, in accordance with the mentioned hypothesis, introducing re-election incentives for the public administration reduces the development of urban land. To be precise, changing the system from an appointed to an elected city manager decreases the amount of urban land by 0.65 percentage points in terms of the total land area of a municipality. While this might seem to be a small effect at first sight, a deeper look reveals that this is not the case. While the median municipality in the sample has approximately no change in urban land over a four year period, decreasing the amount of urban land by 0.65 percentage points in terms of total land area would put a municipality at the lower end of the distribution of urban land changes (10%-quantile). Thus, the effect is both statistically and economically significant.

Table 5 shows the results for the number of building licenses for residential buildings as the outcome variable. First, I find a positive correlation between the number of licenses and an absolute majority of the right-wing CDU in the municipal council. This result is in line with Solé-Ollé and Viladecans-Marsal (2012b) who find a causal positive effect of right-wing parties on land conversion. Second, I find that the proportion of people aged below 15 and above 65 both significantly increases the number of building licenses which is in line with the fact that these population groups are particularly space-

dependent. Third, the effect of population size and population density changes signs across different specifications of the regression model. Given this, a note of caution is in order that these control variables again do not measure causal effects. Most importantly, I find a causal, significantly negative effect of the election of the city manager on the number of building licenses for residential buildings. In the fixed-effect specification in column (1), the estimate suggests that introducing an elected city manager reduces the number of building licenses by 2 per year.[15] Again, this is a large effect given that the mean number of building of licenses per year is 24. The poisson regression model in column (2) also finds a significantly negative effect. Note that the coefficients for the poisson and the negative binomial regression are marginal effects and must be seen as the difference between the logarithm of expected counts. This explains the difference between the magnitude of the coefficients in columns (1) compared to those in columns (2) and (3). As a last model, I estimate the negative binomial regression model in column (3) which should account for the fact that there is overdispersion in the number of building licenses. Again, I find a significantly negative effect. Thus, the estimated effect is robust to changes in the model specification due to the fact that building licenses are measured as counts.

Taken together, the analysis suggests that there is significantly negative effect of an elected city manager an urban development policies, regardless of whether I measure urban development policies as the change in urban land in the land use plan or as the number of building licenses. In the next section, I will further evaluate the robustness of these findings.

[15] In non-reported robustness checks, I also ran regressions where the dependent variable was the number of building licenses per capita. However, the results were similar.

6. Robustness checks

In this section, I investigate the robustness of the results. First, a potential threat to every difference-in-difference strategy as pursued in this paper is the presence of ashenfelter's dip, i.e. in the present case a change in the urban development policies of those municipalities which switch to the new system *shortly before* the switch. To test whether this issue might be able to explain the results found in this paper, I re-estimate the empirical model in the case of building licenses as the outcome variable excluding for every local government the last two years before the change in the nomination scheme took place. Note that it is not possible to conduct a similar analysis for the land use plan as the outcome variable as only one pre-treatment period is available. Table 6 shows the results of this robustness check. General conclusions do not change no matter which model I use. If anything, the magnitude of the coefficient estimates from Table 5 has been too low. The coefficient in the conventional fixed-effects regression is now only significant at 10%-level, but also of a much larger magnitude than before. The lower significance level can be explained by the fact that the standard errors increase, presumably because of the much lower number of observations due to the exclusion of two years of data.

Second, I implement the placebo regression (3) to provide evidence that the time trends in treatment and control group would be the same in absence of the treatment. Table 7 shows the results. As can be seen, the coefficients for both lead variables are insignificant and low. Although I am unfortunately not able to perform such an analysis for the land use plans as outcome variable, these results can be seen as evidence that the timing

of the switch to the new system was as good as random and most importantly not related to urban development policies.

A third robustness check is to perform another type of placebo regression. Specifically, I have compiled a second data set for the time period 2002-2010 and assume that in each municipality, the switch to the new system takes place exactly 10 years later than it has been the case in reality. Importantly, there are no municipal reforms in the time period 2002-2010. Thus, the coefficient estimate of interest from this regression should be small and insignificant. This helps to rule out the possibility that such large and significant coefficients as in my baseline analysis can be found by pure chance even when there is no effect. Table 8 shows the result from this robustness check. Indeed, I do not find any significant effects for the election of the city manager. The coefficient estimates are even positive and small in every specification.

Finally, it should be mentioned that the results derived above are also robust to the inclusion of further covariates that I have no put into the final specifications (1) and (2). Tables 9 and 10 show the results when I additionally include the percentage of immigrants, the number of employees in a municipality, the percentage of employees in the agricultural sector, the percentage of employees in the production sector, municipal debt per capita as well as open land area at the beginning of the years 1989 and 1993 (measured as the difference between the total land area and the build-up land, scaled by total land area). The idea of the latter control variable is that municipalities are restricted in their development possibilities if a large part of the total land area has already been developed in the past. I did not include these variables in the final specification because the within-variation in the relatively short time span is rather low such that the effect of

these variables is most likely absorbed by the fixed effects that I am able to use due to the panel structure of the data. As shown in Tables 9 and 10, the coefficients of interest drop slightly in size, but stay both statistically and economically significant. None of the general conclusions changes when I include these further covariates.

7. Different categories of urban development

So far, I have presented evidence that the change in the nomination scheme of the city manager significantly lowers the amount of urban land in the land use plan as well as the number of building licenses for residential buildings. A natural question that subsequently arises is whether all categories of urban development are equally affected by the introduction of the city manager or whether some categories are not affected at all. To shed some light on this issue, in this section I first estimate model (1) using different sub-categories of land use from the land use plan as outcome variables (residential, commercial and agricultural land). Second, I estimate model (2) using the number of building licenses for non-residential buildings as outcome variable.

Table 11 shows the results for different categories of the land use plan. From those two categories that were used to calculate the amount of urban land, i.e. residential land and commercial land, only residential land is statistically significant, while commercial land does not react to the change in the nomination scheme. The coefficient of interest is also insignificant for agricultural land as the outcome variable, but interestingly the coefficient is positive and of the same magnitude as the coefficient when residential land is the outcome variable. Thus, it seems to be the case that less land is planned to be con-

verted from agricultural to residential purposes when the manager is elected instead of appointed.

Table 12 shows the results when the number of building licenses for non-residential buildings is used as outcome variable. For all three models that I estimate, the coefficient of interest is insignificant and close to zero. Thus, also in the case of building licenses as a measure for urban development policies, the change in the nomination scheme only affects urban development for residential purposes. An interpretation for this finding would be that citizens opposing urban growth are only concerned about growth in the form of more residents. Citizens might not see their estate devaluated when more firms settle down in the municipality as this might, for example, lower unemployment rates and the negative consequences potentially arising from unemployment (for example, crime) or might increase wages.

8. Conclusion

The urban economics literature supposes that the vote decisions of citizens are to a significant extent driven by urban development policies. On the other hand, if this is true, it should also be the case that politicians concerned with re-election condition their urban development policies on the role of the voters in the political selection process. In this paper, I test for this idea by comparing urban development policies when the city manager, the head of the public administration, is appointed by the city council (and thus has not to appeal to the voters) with the case in which the city manager is elected directly by the citizens. Importantly, the design of a reform of the nomination scheme of the city manager in the German state Hesse allows me to give the estimates a causal interpretation. Using the change in *planned* urban land as well as the number of building li-

censes as outcome variables, I indeed find that urban development decisions are significantly different when the city manager is elected instead of appointed: The election of the city manager significantly restricts urban development. Moreover, I find that the change in the nomination scheme has an effect on residential urban development, but not on other sub-categories of urban development, for example commercial urban development. For future research, a natural extension of this paper would be to investigate whether changes in the urban development plans are able to deliver an incumbent a significant (dis)advantage in terms of vote shares in the electoral race.

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Tables

Table 1: Summary statistics
(a) Land use data set 1989-1997

Variable	Obs	Mean	Std. Dev.	Min	Max
ΔUrbanLand	370	-.068878	1.578269	-17.71812	5.426357
ΔResidentialLand	370	.0068453	1.085891	-11.94631	4.743833
ΔCommercialLand	370	1.528426	2.150126	0	13.45029
ΔAgriculturalLand	370	-.4006266	1.790377	-13.86391	7.065073
Area	370	4456.589	2642.743	440	12969
Population	370	10784.11	10734.09	766	77083
Pop. Density	370	3.261896	3.492863	.2555037	23.06591
% Old	370	15.41243	2.499519	8.9	25.4
% Young	370	16.56703	1.664322	12.2	21.3
Abs. Majority SPD	370	.6027027	.4900011	0	1
Abs. Majority CDU	370	.2864865	.4527316	0	1
Pol. Competition	370	.1886106	.1255004	.0004391	.5186029
Turnout	370	.7516764	.0583419	.5713812	.9230769
Coalition	370	.5837838	.4935978	0	1
% Immigrants	370	7.28225	4.507885	.0816993	32.69349
Number employees	370	3153.132	5599.91	36	49256
% ProductionSector	370	50.06268	16.58967	0	92.81642
% Agricult.Sector	370	1.168522	1.459435	0	10.73254
Open land	370	84.33603	8.17254	51.27353	95.7972
Debt per capita	370	.7591003	.4454012	.0015573	2.529306

Notes: Land variables are calculated as described in Table 3. Area is measured in hectare. Population density is defined as number of inhabitants per hectare.

(b) Building licenses data set (1992-2000)

Variable	Obs	Mean	Std. Dev.	Min	Max
BL Residential	3789	24.42016	21.76757	0	273
BL Non-Residential	3789	5.827131	5.16	0	66
Area	3789	4765.09	2882.713	405	14209
Population	3789	11015.11	10547.6	724	89156
Population density	3789	316.7585	339.3778	24.14943	2315.682
% Old	3789	15.76611	2.411423	8.4	27.1
% Young	3789	16.63038	1.563911	11.6	22.6
Abs. Majority SPD	3789	.6389549	.480367	0	1
Abs. Majority CDU	3789	.2570599	.4370704	0	1
Pol. Competition	3789	.1740259	.1168938	.0001936	.625
Turnout	3789	.7056681	.0594218	.5117735	.9230769
Coalition	3789	.6437055	.4789669	0	1
% Immigrants	3789	7.301603	4.71054	.0674992	32.88248
Number Employees	3789	3159.22	5570.307	31	53343
% ProductionSector	3789	47.79283	16.75278	0	92.81642
% Agricult.Sector	3789	1.384582	2.51785	0	50.84746
Open land	3789	84.49819	8.128151	43.58578	95.84561
Debt per capita	3789	.7651391	.4961697	0	3.931665

Notes: BL=building licenses. Land variables are calculated as described in Table 3. Area is measured in hectare. Population density is defined as number of inhabitants per hectare.

Table 2: Switch in the nomination scheme

Year	Number of municipalities that switched
1993	44
1994	75
1995	84
1996	77
1997	62
1998	66
1999	13
Sum	421

Table 3: Definitions of the land use variables

Variable	Definition
Urban Land	$\frac{\text{DevelopableResidentialLand+DevelopableCommercialLand+DevelopedResidentialLand+DevelopedCommercialLand}}{\text{TotalLandArea}} \times 100$
Residential Land	$\frac{\text{DevelopableResidentialLand+DevelopedResidentialLand}}{\text{TotalLandArea}} \times 100$
Commercial Land	$\frac{\text{DevelopableCommercialLand+DevelopedCommercialLand}}{\text{TotalLandArea}} \times 100$
Agricultural Land	$\frac{\text{DevelopableAgriculturalLand+DevelopedAgriculturalLand}}{\text{TotalLandArea}} \times 100$
Open land	$\frac{\text{TotalLandArea-DevelopedResidentialLand-DevelopedCommercialLand-TrafficAreas-Cemeteries}}{\text{TotalLandArea}} \times 100$

Table 4: Results for urban land as outcome variable

VARIABLES	(1) $\Delta \text{UrbanLand}_{it}$
Election of city manager	-0.653**
	(0.299)
Population size	0.000
	(0.001)
Population density	0.158
	(4.205)
Proportion of old, 65+	-0.415
	(0.402)
Proportion of young, 0-15	-0.470
	(0.349)
Political competition	0.052
	(1.822)
Dummy: Abs. majority SPD	-0.739
	(0.507)
Dummy: Abs. majority CDU	0.731
	(1.024)
Turnout	11.851**
	(5.961)
Dummy: Coalition Gov.	0.233
	(0.424)
Observations	370
Number of clusters	185

Robust standard errors clustered at the municipality level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1
All regressions include but do not report municipality- and year-fixed effects.

Table 5: Results for building licenses for residential building as outcome variable

VARIABLES	(1) Fixed-Effect Regression	(2) Poisson Regression	(3) Negative Binomial Reg.
Election of city manager	-2.004**	-0.094**	-0.053**
	(0.985)	(0.039)	(0.027)
Population size	-0.007*	-0.000**	0.000**
	(0.004)	(0.000)	(0.000)
Population density	0.323**	0.007**	-0.001***
	(0.140)	(0.003)	(0.000)
Proportion of old, 65+	3.224***	0.153***	0.102***
	(0.918)	(0.039)	(0.023)
Proportion of young, 0-15	1.252*	0.061*	0.098***
	(0.669)	(0.035)	(0.022)
Political competition	8.637	0.356	0.249
	(6.256)	(0.284)	(0.224)
Absolute majority SPD	1.444	0.051	0.034
	(1.537)	(0.062)	(0.045)
Absolute majority CDU	2.896*	0.118*	0.128**
	(1.687)	(0.069)	(0.052)
Turnout	-31.071	-1.550*	0.122
	(19.785)	(0.875)	(0.641)
Coalition gov.	-0.197	-0.023	0.020
	(1.415)	(0.057)	(0.045)
Observations	3789	3789	3789
Number of clusters	421	421	421

Clustered standard errors in parentheses. In columns (2) and (3), all coefficients are denoted as marginal effects evaluated at the sample mean. For the negative binomial model, standard errors are bootstrapped with 1000 bootstrap replications. *** p<0.01, ** p<0.05, * p<0.1. All regressions include but do not report municipality- and year-fixed effects.

Table 6: Testing for Ashenfelter's Dip

VARIABLES	(1) Fixed-Effect Regression	(2) Poisson Regression	(3) Negative Binomial Reg.
Election of city manager	-2.649*	-0.123**	-0.093**
	(1.505)	(0.056)	(0.044)
Population size	-0.007	-0.000*	0.000*
	(0.005)	(0.000)	(0.000)
Population density	0.332**	0.008**	-0.001***
	(0.154)	(0.003)	(0.000)
Proportion of old, 65+	2.633***	0.124***	0.085***
	(1.005)	(0.043)	(0.025)
Proportion of young, 0-15	1.070	0.055	0.097***
	(0.701)	(0.037)	(0.025)
Political competition	8.273	0.359	0.213
	(6.945)	(0.324)	(0.267)
Absolute majority SPD	2.123	0.083	0.039
	(1.837)	(0.071)	(0.053)
Absolute majority CDU	3.057	0.139*	0.126**
	(1.926)	(0.080)	(0.063)
Turnout	-27.031	-1.312	0.498
	(21.316)	(0.932)	(0.712)
Coalition gov.	-0.843	-0.038	-0.019
	(1.589)	(0.063)	(0.048)
Observations	2991	2991	2991
Number of clusters	421	421	421

Outcome variable is number of building licenses for residential buildings. Clustered standard errors in parentheses. In columns (2) and (3), all coefficients are denoted as marginal effects evaluated at the sample mean. For the negative binomial model, standard errors are bootstrapped with 1000 bootstrap replications. *** p<0.01, ** p<0.05, * p<0.1. All regressions include but do not report municipality- and year-fixed effects.

Table 7: Placebo difference-in-difference regressions

VARIABLES	(1) Fixed-Effect Regression	(2) Poisson Regression	(3) Negative Binomial Reg.
T_{it+1}	-0.485	-0.022	-0.025
	(0.950)	(0.038)	(0.029)
T_{it+2}	0.083	0.001	-0.013
	(0.989)	(0.038)	(0.029)
Observations	2947	2947	2947
Number of clusters	421	421	421

Outcome variable is number of building licenses for residential buildings. Clustered standard errors in parentheses. In columns (2) and (3), all coefficients are denoted as marginal effects evaluated at the sample mean. For the negative binomial model, standard errors are bootstrapped with 1000 bootstrap replications. *** p<0.01, ** p<0.05, * p<0.1. All regressions include but do not report municipality- and year-fixed effects as well as the same control variables as in Table 5.

Table 8: Placebo regression for the time period 2002-2010

VARIABLES	(1) Fixed-Effect Regression	(2) Poisson Regression	(3) Negative Binomial Reg.
Election of city manager	0.483	0.016	0.017
	(0.717)	(0.046)	(0.034)
Observations	3789	3789	3789
Number of clusters	421	421	421

Outcome variable is number of building licenses for residential buildings. Clustered standard errors in parentheses. In columns (2) and (3), all coefficients are denoted as marginal effects evaluated at the sample mean. For the negative binomial model, standard errors are bootstrapped with 1000 bootstrap replications. *** p<0.01, ** p<0.05, * p<0.1. All regressions include but do not report municipality- and year-fixed effects as well as the same control variables as in Table 5.

Table 9: Including additional control variables I

VARIABLES	(1) $\Delta UrbanLand_{it}$
Election of city manager	-0.514*
	(0.304)
Number employees	-0.000
	(0.000)
%Immigrants	0.212
	(0.189)
%Workers in producing sector	-0.019
	(0.015)
%Workers in agricultural sector	-0.005
	(0.138)
Debt per capita	-0.131
	(0.517)
Open land	1.020**
	(0.466)
Observations	370
Number of clusters	185

Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include but do not report municipality- and year-fixed effects as well as the control variables mentioned in Table 5.

Table 10: Including additional control variables II

VARIABLES	(1) Fixed-Effect Regression	(2) Poisson Regression	(3) Negative Binomial Reg.
Election of city manager	-1.918**	-0.087**	-0.053**
	(0.964)	(0.038)	(0.027)
Employees	0.000	0.000	0.000
	(0.002)	(0.000)	(0.000)
%Immigrants	0.692	0.050**	0.003
	(0.564)	(0.020)	(0.011)
%Workers in producing sector	-0.042	-0.003	0.002
	(0.041)	(0.003)	(0.002)
%Workers agricultural sector	-0.005	0.001	0.011
	(0.089)	(0.010)	(0.009)
Debt per capita	7.827**	0.320***	0.063
	(3.770)	(0.121)	(0.068)
Open land	-2.633	-0.075	0.018
	(1.635)	(0.054)	(0.018)
Observations	3789	3789	3789
Number of clusters	421	421	421

Outcome variable is number of building licenses for residential buildings. Clustered standard errors in parentheses. In columns (2) and (3), all coefficients are denoted as marginal effects evaluated at the sample mean. For the negative binomial model, standard errors are bootstrapped with 1000 bootstrap replications. *** p<0.01, ** p<0.05, * p<0.1. All regressions include but do not report municipality- and year-fixed effects as well as the same control variables as in Table 5

Table 11: Different categories of the land use plan

VARIABLES	(1) $\Delta ResidentialLand_{it}$	(2) $\Delta CommercialLand_{it}$	(3) $\Delta AgriculturalLand_{it}$
Election of city manager	-0.349*	-0.115	0.347
	(0.181)	(0.116)	(0.465)
Observations	370	370	370
Number of clusters	185	185	185

Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include but do not report municipality- and year-fixed effects as well as the same control variables as in Table 5.

Table 12: Number of building licenses for non-residential buildings as outcome variable

VARIABLES	(1) Fixed-Effect Regression	(2) Poisson Regression	(3) Negative Binomial Reg.
Election of city manager	-0.021	-0.001	-0.004
	(0.186)	(0.032)	(0.031)
Observations	3789	3789	3789
Number of clusters	421	421	421

Outcome variable is number of building licenses for non-residential buildings. Clustered standard errors in parentheses. In columns (2) and (3), all coefficients are denoted as marginal effects evaluated at the sample mean. For the negative binomial model, standard errors are bootstrapped with 1000 bootstrap replications. *** p<0.01, ** p<0.05, * p<0.1. All regressions include but do not report municipality- and year-fixed effects as well as the same control variables as in Table 5.

Figure 1: Distribution of the change in urban land

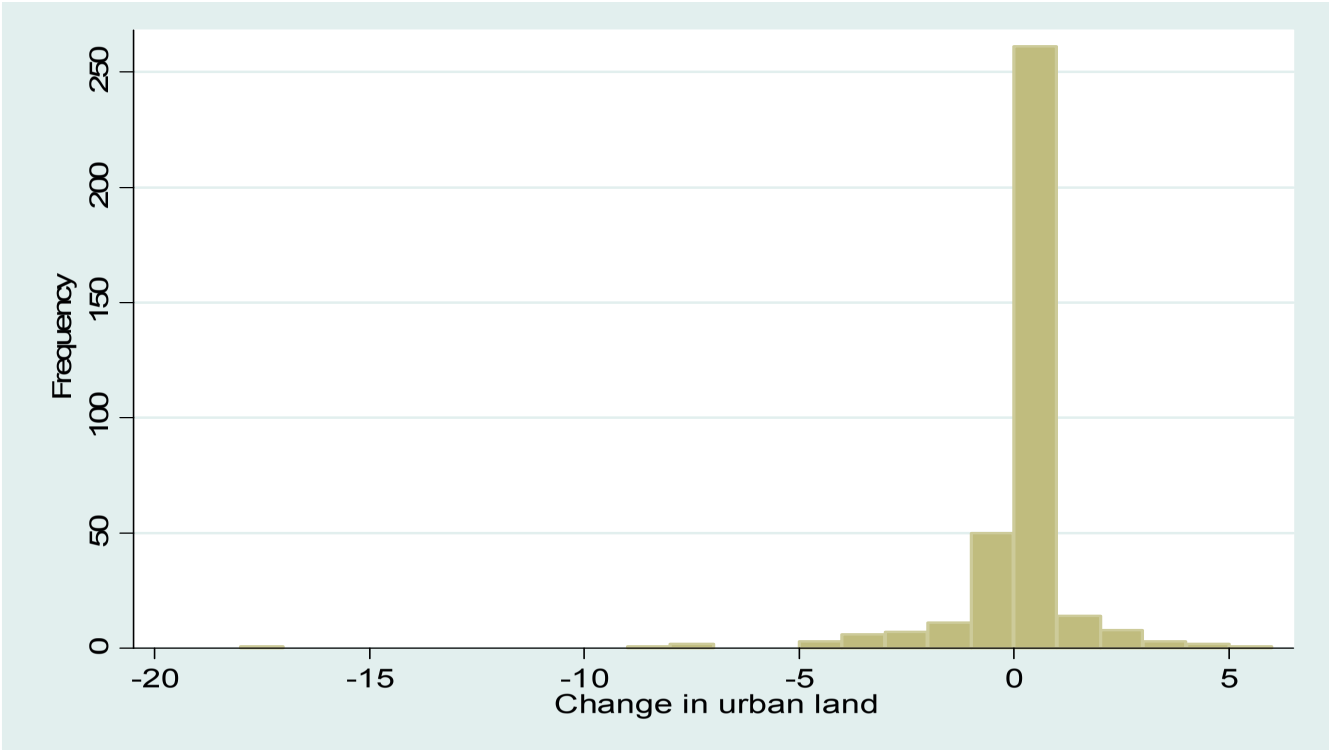

Figure 2: Distribution of the number of building licenses