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How do municipalities react to a revenue shock?

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How do municipalities react to a revenue shock?

Preliminary draft - please do not cite or circulate

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Abstract

We study the fiscal reaction of German municipalities to a revenue shock that stems from the adjustment of population counts in the 1987 and 2011 censuses. We focus on the choice of instruments municipalities use in order to offset the shock, namely municipal taxes, debt, and spending. Simple models of public finance suggest that municipalities should adjust all instruments marginally. In more complicated models that take into account the political economy, this does not need to hold. In regular instrumental variable (IV) regressions, we find that municipalities significantly adjust one or two instruments. Non-parametric IV regressions that rely on generalized random forests ([Athey et al., 2019](#)) provide evidence on strategic behaviour of municipalities.

Keywords: Municipal finance, revenue shock, census shock.

1. Introduction

Promoting equal living conditions in Germany is the predominant aim of regional policy. Apart from regional aid, the main instrument for redistribution between structurally strong and structurally weak regions are fiscal equalization schemes. Vertical transfer schemes exist both on state level (transfers from the federal government to the states) and on municipal level (transfers from the respective state government to the municipalities). Transfers to municipalities, which are frequently on the political agenda, are relevant in quantitative terms. Hence, as this implies that municipalities decide on a large share of the total public budget, analysing municipalities' reactions to additional resources is highly important. Insights gained from evaluating the benefits of vertical fiscal transfers might be of interest to decision-makers designing fiscal equalization schemes. In our paper, we exploit variation in municipal revenues due to population adjustments in the last two censuses in Germany to find answers to the questions of how municipalities react to unexpected shocks to their budgets. The last census was conducted in 2011, the previous one in 1987. In both cases, population adjustments led to sizeable adjustments in "key grants" ("Schlüsselzuweisungen") for a large majority of municipalities after the Census was published in November 1988 respectively May 2013. The adjustments in key grants were uncorrelated with local trends in economic fundamentals that could lead to fiscal reactions and grant changes at the same time (detailed information on our exogeneity assumption can be found in Appendix B).

In the empirical strategy, we use this "artificial" change in population as an instrument for the change in key grants. With this instrument, we study how the municipalities react to such a change in revenues by adjusting (property and business) taxes, debt or spending. We also focus on the question if heterogeneous reactions across municipalities can be explained on the basis of economic theory.

To motivate our empirical analysis, we set up a simple model that analyzes local tax and debt policy (see Appendix A). The model demonstrates that vertical grants may not affect capital taxation. It also shows that some jurisdictions will not tax land, but others prefer land taxation instead of debt. In particular, the model demonstrates that the political power of landowners and the burden of debt are crucial parameters.

The results of our empirical analysis show that the exogenous shock to municipal finances was relevant. Municipalities mainly reacted to the shock by adjusting one or two instruments. In general, there is a lot of heterogeneity across municipalities' reactions. Our heterogeneity analysis provides evidence on strategic behaviour of municipalities.

The results of our heterogeneity analysis provide important new insights into the political economy of financial behaviour of municipalities. Municipalities take into account voters' preferences when deciding whether to increase debt or not, for example, which is of interest with regard to the optimal design of municipal austerity programmes.

There is a relatively new literature on the effect of an exogenous shock to municipal finances due to census counts in the US. These papers attempt to estimate local fiscal multipliers. The institutional background differs markedly from our setting: In the U.S., many federal programs distribute funds based on population counts, whereas we study the effects of a fiscal equalization scheme, where funds are transferred directly to municipalities. [Serrato and Wingender \(2016\)](#) analyse the effect of a positive revenue shock on public spending and income as well as employment growth. [Kim and Nguyen \(2018\)](#) consider the effect of increased municipal spending on corporate investment, while [Hoffman and Mast](#)

(2018) focus on the effect of a positive fiscal shock on amenities, for example crime. Helm and Stuhler (2019) analyse dynamic fiscal responses after the Census 1987 in Germany. An important aspect for our analysis is the fact that in contrast to Spain, where municipalities are reported to have an incentive to overestimate their number of residents because of discontinuous jumps in allocated grants once certain population thresholds are exceeded (Foremny et al., 2015), there are no discontinuities in grant allocations depending on population thresholds in Germany (see appendix section "Exogeneity of the Instrument and Exclusion Restriction" for more information on the correlation between the population size and the Census shock in Germany).

Our research is related to the tax competition literature that considers the choice between a distortionary tax (corresponding to a more mobile tax base) and a less distortionary tax (corresponding to a less mobile tax base) (Bucovetsky and Wilson, 1991), (Braid, 1996), (Buettner, 2006), (Janeba and Osterloh, 2013) and (Buettner and von Schwerin, 2016). Holm-Hadulla (2018) analyses the tax setting behaviour of municipalities in response to an exogenous variation of municipal finances triggered by an institutional change of the fiscal equalization scheme in Northrhine-Westfalia. He finds that municipalities adjust property taxes to a larger extent than business taxes, which means that municipalities tend to use taxes where tax base effects are smaller rather than taxes where tax base effects are larger to adapt to the changed fiscal situation.

Moreover, our research can also be related to the polit-economic literature. The theory of the flypaper effect predicts municipalities to increase spending when receiving a larger amount of grants. Politicians also often tend to prefer increasing debt rather than tax rates when getting less money in anticipation of higher re-election probabilities (Besley and Case, 1995), (Wilson and Wildasin, 2004) and (Allers and Vermeulen, 2016).

The structure of our paper is as follows: The next section explains the institutional background regarding municipal fiscal equalization and the design of the censuses in 1987 and 2011. Section 3 presents our estimation strategies and results, while the final section concludes with suggestions for future research.

2. Data and Institutional Background

As far as municipal revenue in Germany is concerned, municipalities are allocated a fixed share of income tax revenue and VAT revenue. Municipalities also participate in other federal state tax revenue. In addition to that, they have the tax sovereignty over local consumption and expense taxes as well as property and business taxes. Apart from tax revenue, municipalities also get grants from the federal state. The purpose of these grant allocations is to redistribute between financially strong and financially weak municipalities. The major component of fiscal equalization schemes are key grants, which are allocated to those municipalities whose financial need exceeds their financial strength (based mainly on a comparison of tax bases). This allocation criterion is fulfilled by a large majority – more than 90 percent of all municipalities in Germany. There is some variation between federal states, in some states, all municipalities receive key grants (for example in Baden-Wuerttemberg and Hesse), whereas in Bavaria, for example, key grants were only allocated to around 85 percent of municipalities in 2012. The amount of key grants transferred to that municipality increases with the difference between financial needs and financial strength. This number is magnified for bigger cities to account for the fact that these cities offer

services also for residents from neighboring municipalities. The resulting amount is then multiplied by the number of residents (see [Lenk et al. \(2013\)](#) and the respective fiscal equalization laws of the federal states).

The resident population is determined by the latest census and updated by the municipality each year. Updating is done by adding births and in-migration, and by subtracting deaths and out-migration over the course of the year. Up to 1989, key grants were calculated based on numbers from the census 1970, while in 1990, the updated population count was based on the census 1987, leading to substantial deviations. A similar phenomenon occurred in 2011, when the updated population counts based on the census 1987 were replaced by population counts based on the census 2011. Notably, while the census 2011 counted about 1.5 million residents less than the population update for the same day based on the census 1987, the 1987 census counted about 1.0 million more residents. This shows that municipalities are not able to simply over-count their residents in order to secure a higher amount of transfers. One should also keep in mind that in contrast to 1987, where the total population was required to fill out a questionnaire, the census 2011 relied on surveys of a population sample. This implies that deviations between population estimations and population counts might also at least partly be attributed to statistical errors. In fact, several municipalities expressed doubts on whether the census 2011 correctly reflected the number of their residents and sued against the census results before the Federal Constitutional Court. However, in September 2018, the Court declared the statistical method applied in the census 2011 as being in accordance with the German Constitution.

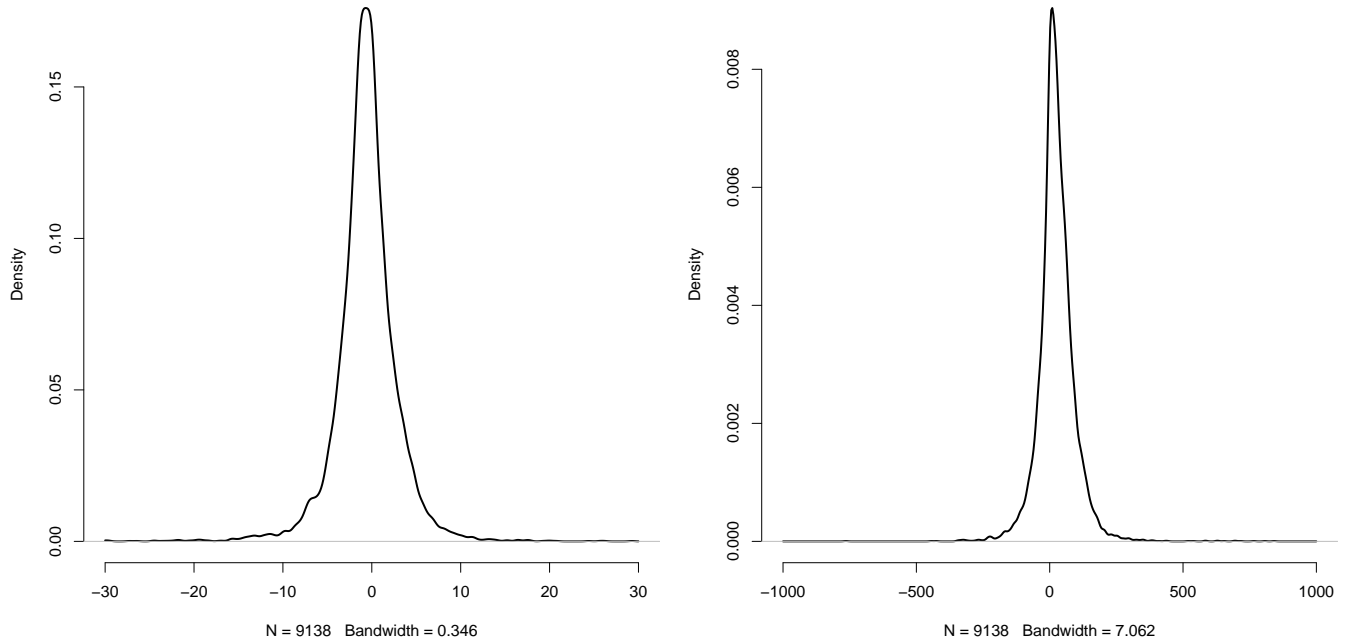
For municipalities that experienced a reduction of their population count, key grants were reduced accordingly (and vice versa). We exploit this variation in the empirical analysis. The mechanism was exactly the same in both instances, 1987 and 2011. Moreover, other transfers to the municipality do not depend on the census population count.¹

The census results for 2011 were published on May 31, 2013, two years after the census was conducted, and in most federal states, 2014 was the first year in which the number of residents of a municipality to determine its financial need was determined according to the updated population counts. We therefore consider changes from 2012 to 2014 in our analysis and restrict our sample to those municipalities that received key grants both in 2012 and 2014. Moreover, we excluded Rhineland-Palatinate and Saxony-Anhalt from our sample, since reforms of the fiscal equalization schemes took place in these two states in 2013. As we are interested in identifying the change in key grants and municipal finances that can be attributed to the adjustment of population counts in the Census 2011, we leave out those cases in which institutional changes between 2012 and 2014 could have led to changes in municipal finances.

With the same approach, we added data for the census 1987. The census 1987 was conducted in May 1987 and published November 25, 1988. We chose to compare 1988 (post-census-year) to 1990 (first year where the population adjustment became effective for key grants), to match the timing for the census 2011. Currently, we only have data for Bavaria included in the sample, but we will extend the sample in the future. In total, we have around 7000 observations from the census 2011 and around 2000 observations

¹For instance, municipalities receive a share of the federal income tax. The share is distributed according to tax revenue from income taxes at the municipality. This tax revenue is calculated based on tax returns. Although there are specific additional rules that effectively let tax revenue vary with population rather than income, it does not vary with population counts, but only when the actual number of residents changes.

Figure 1: Densities of the census shock and key grant growth



from the census 1987 for our empirical analysis, totalling 9138 observations in the full sample.

Figure 1 plots the distributions of the census shock (left panel) and the change in key grants per resident (right panel). The "Census shock" is defined as the percentage deviation between the population on the basis of the census and the population based on the estimation on 31st December 2011. The Census shock has a mean (median) of -0.89 (-0.86) percent and is distributed quite symmetrically. "Key grant growth" is defined as per capita changes in key grants from 2012 to 2014. Its mean (median) value amounts to 31 (29.6) Euro, the distribution is also quite symmetric. In general, both distributions are shaped similarly.

All data for our analysis are at municipal level. Data sources are the Regional Database Germany for data on taxes and spending and Statistical Offices of Federal States for data on key grants and debt. Control variables are from the Regional Database Germany and the Federal Employment Agency. Collecting comparable data both over time and across municipalities is quite difficult for data on debt. On the one hand, methodological changes took place such that the components of debt of municipalities do not remain constant over time (for example, further liabilities are added in one year), and on the other hand, comparability of debt across municipalities is limited because of different outsourcing possibilities of debt components. Moreover, due to different accounting methods of municipalities across federal states, we do not have data on spending for a considerable number of municipalities. For these reasons, results for debt and spending should be interpreted cautiously.

3. Regression Results

We estimate the following equation:

$$\Delta y_i = \alpha + \gamma \Delta g_i + \beta x_i + \varepsilon_i, \quad (1)$$

where Δg_i is the change in key grants per resident, from 1988 to 1990, and from 2012 to 2014. 1988 and 2012 are the years preceding the publication of the respective census results, and the population adjustment from the census became effective for the calculation of key grants in 1990 and 2014. x_i are controls, and ε_i is an error term. Δy_i is the change in an outcome variable. Specifically, we consider the *potential* change in tax revenue from property taxes² (applying to buildings) and local business taxes. We assume the tax base to remain constant in the short run, hence changes in our tax variables reflect potential tax revenue changes attributed to tax rate adjustments of municipalities. We define both tax variables as the (absolute) change in the respective potential tax revenue in 1988 and 2012, per capita. Moreover, we also consider changes in debt and spending per capita from 1988 to 1990 and 2012 to 2014. Our debt variable measures total debt levels of municipalities excluding debt of businesses owned by municipalities. The spending variable is defined as total gross expenditure of municipalities. Since Δg_i is endogenous, we instrument by the census shock, i.e. the percentage adjustment in population size in the 1987 and 2011 censuses.

We control for a variety of lagged changes, most importantly the lagged changes in population, in key grants per capita, and in debt per capita from the pre-census year ($t - 3$) to the year before the census results were published ($t - 1$). Note that our instrument is fixed in the census year and cannot be influenced by contemporaneous shocks that occur after that date. A potential source of bias could be changes in other important variables that are correlated with population count errors in a given year. If there is very high persistence in these changes, they might also correlate with the change in key grants per capita several years later, and thus with our endogenous variable. These factors should be captured by controlling for the lagged change of key grants per capita.

3.1. First stage results

Table 1 contains the results for the first stage. Generally, the instrument is tightly correlated with the change in key grants per resident. Column 1 shows results without control variables, in column 2 state-fixed effects are added and in column 3 it is additionally controlled for population, unemployment and debt changes prior to the census shock. Column 4 shows regression results when including a larger set of control variables. The size of the census shock coefficient is around three, irrespective of the specification, and the level of significance also does not vary much between specifications (the t-statistic is around ten in all cases). Hence, as the population count is adjusted upwards by one percentage point due to the census, the municipality receives additional three Euros key grants per capita. Although the magnitude might not seem very large in economic terms, municipalities have to react fully to this shock. Our interest lies in studying *how* municipalities adjust, rather than the general impact on the local economy (which might be small).

²Municipalities can tax agricultural land and built structures separately. We do not consider the agricultural tax (property tax A) because it is much less important in terms of revenue shares. In 2012, it only accounted for around three percent of total property tax revenues of municipalities.

Table 1: First stage results

| <i>Dependent variable: key grant per capita, change 2012–2014</i> | | | | |
|---|-------------------|-------------------|--------------------|-----------------------|
| | (1) | (2) | (3) | (4) |
| Census shock | 2.86*** (0.33) | 3.28*** (0.34) | 3.03*** (0.29) | 3.06*** (0.29) |
| $\Delta_{t-3,t-1}$ population | | | -1.70 (16.08) | -4.45 (16.16) |
| $\Delta_{t-3,t-1}$ key grants per capita | | | -0.40*** (0.06) | -0.40*** (0.06) |
| $\Delta_{t-3,t-1}$ debt per capita | | | 0.01 (0.01) | 0.01 (0.01) |
| $\Delta_{t-3,t-1}$ unemployed per capita | | | | 941.28*** (210.52) |
| $\Delta_{t-3,t-1}$ employment at residence per capita | | | | 268.65*** (77.75) |
| Observations | 9138 | 9138 | 9138 | 9138 |
| Residual standard error state fixed effects | 76.6 no | 72.0 yes | 67.7 yes | 67.5 yes |

Heteroskedasticity-robust standard errors in parentheses; * : $p < .05$, ** : $p < .01$, *** : $p < .001$.

Table 2: Basic regressions: taxation responses

| <i>Dependent variable:</i> | <i>property tax B</i> | | <i>business tax</i> | |
|--|-----------------------|----------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) |
| $\Delta_{t-1,t+1}$ key grants per capita | -0.021*** (0.006) | -0.020** (0.006) | -0.012 (0.013) | -0.012 (0.013) |
| $\Delta_{t-3,t-1}$ population | | -1.079 (1.444) | | -0.084 (0.491) |
| $\Delta_{t-3,t-1}$ key grants per capita | | -0.008* (0.003) | | -0.003 (0.005) |
| $\Delta_{t-3,t-1}$ debt per capita | | 0.004*** (0.001) | | 0.001 (0.001) |
| $\Delta_{t-3,t-1}$ unemployed per capita | | 4.277 (20.430) | | 1.294 (35.213) |
| $\Delta_{t-3,t-1}$ employment at residence per capita | | 2.263 (5.268) | | -14.983 (12.816) |
| $\Delta_{t-3,t-1}$ property tax per capita | | -0.103*** (0.022) | | |
| $\Delta_{t-3,t-1}$ business tax per capita | | | | -0.036* (0.017) |
| Observations | 9063 | 9063 | 9066 | 9066 |
| Residual standard error | 7.5 | 7.4 | 9.4 | 9.4 |

All regressions include federal state fixed effects. Regressions with control variables also include NA dummies for each control variable that capture cases where the control variable was not available. Coefficients of these variables are not reported in the table. Heteroskedasticity-robust standard errors in parentheses; * : $p < .05$, ** : $p < .01$, *** : $p < .001$.

3.2. Average responses

Table 2 displays the results for the change in potential tax income from the property tax (columns 1 and 2), and the business tax (columns 3 and 4). All tables in the remainder of this chapter show regression results for state-fixed effects, but no further controls (first column), and when adding a set of control variables (second column).

According to the regressions shown in Table 2, municipalities only adjust property taxes, but leave business taxes unchanged. Although the adjustment of the property tax is highly significant and negative, the magnitude is small: A one Euro increase in key grants leads municipalities to reduce potential property tax income (at fixed tax base) by around two cents. This number is very stable as control variables are added. The business tax adjustment is also negative, but even smaller in magnitude and not significant.

Table 3 shows regression results for debt (columns 1 and 2) and spending (columns 3 and 4) responses. Because data was not available for all federal states (see section 2), we lose several hundreds of observations. The main coefficients suggest that debt and spending were the two most important channels for

Table 3: Basic regressions: debt and spending responses

| <i>Dependent variable:</i> | <i>debt</i> | | <i>spending</i> | |
|---|-------------|------------|-----------------|------------|
| | (1) | (2) | (3) | (4) |
| $\Delta_{t-1,t+1}$ key grants per capita | -0.528* | -0.554* | 0.699 | 0.356 |
| | (0.252) | (0.268) | (0.554) | (0.648) |
| $\Delta_{t-3,t-1}$ population | | 17.649 | | -81.765 |
| | | (57.075) | | (67.345) |
| $\Delta_{t-3,t-1}$ key grants per capita | | -0.089 | | 0.414 |
| | | (0.237) | | (0.320) |
| $\Delta_{t-3,t-1}$ debt per capita | | -0.068 | | -0.163** |
| | | (0.068) | | (0.058) |
| $\Delta_{t-3,t-1}$ unemployed per capita | | 2966.066** | | -805.233 |
| | | (1043.452) | | (1566.587) |
| $\Delta_{t-3,t-1}$ employment at residence per capita | | 527.944 | | 173.580 |
| | | (319.912) | | (578.646) |
| $\Delta_{t-3,t-1}$ debt per capita | | -0.068 | | -0.163** |
| | | (0.068) | | (0.058) |
| $\Delta_{t-3,t-1}$ spending per capita | | | | -0.266*** |
| | | | | (0.040) |
| Observations | 8273 | 8273 | 6697 | 6697 |
| Residual standard error | 291.6 | 291.3 | 450.1 | 418.8 |

All regressions include federal state fixed effects. Regressions with control variables also include NA dummies for each control variable that capture cases where the control variable was not available. Coefficients of these variables are not reported in the table. Heteroskedasticity-robust standard errors in parentheses; * : $p < .05$, ** : $p < .01$, *** : $p < .001$.

municipalities. In terms of effect sizes, municipalities seemed to rely on spending and debt to comparable degrees. While the coefficients for the debt adjustment are significant in both regressions, the spending coefficients are not. According to the regressions, an increase in key grants by one Euro per capita led to a reduction of debt by 53–55 cents, and an increase in spending by 37–70 cents. The sum of these adjustments is reasonably close to one, and it clearly is not statistically different from one. This suggests that we only capture the direct effect of the census adjustment, and no other indirect effects from omitted variables.

3.3. How many instruments do municipalities mainly make use of?

The average effects measured in the preceding section might mask important heterogeneities. As a first step, we investigate how many potential instruments (taxes, debt, spending) municipalities significantly adjusted at once. If there are no adjustment costs, we would expect that all instruments are used simultaneously in order to prevent large changes that have greater distortive potential. On the other hand, if adjustment costs matter, it might be preferable to change only one parameter.

Instead of estimating a fully specified simultaneous equations model, we construct “change variables”. We focus on the change in property taxation, debt, and spending. We do not consider the local business tax since its coefficient in Table 2 was small and insignificant. First, we restrict the sample to observations where all three variables are observed. We then de-mean each variable and divide by its standard deviation. This makes the variables comparable. For each municipality, we then order the three scaled variable that was adjusted the most in absolute terms (relative to its standard deviation). We define the “strongest reaction” as the value of the scaled variable, while preserving the sign of the change. Similarly, we define the “medium” and “weakest reaction”. In order to align signs, we pre-multiplied the spending reaction by -1 .

Defined in this way, the three variables measure whether the strongest, medium and weakest change among the three instruments, property taxation, debt, and spending, were systematically related to the change in key grants per capita as caused by the census shock. That is, because we preserve the sign of

Table 4: Do municipalities adjust several instruments at once?

| <i>Change:</i> | <i>strongest</i> | | <i>medium</i> | | <i>weakest</i> | |
|--|---------------------|------------------------|-------------------|----------------------|-------------------|-----------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $\Delta_{t-1,t+1}$ key grants per capita | -0.470** (0.181) | -0.410* (0.194) | -0.124 (0.071) | -0.126 (0.075) | -0.031 (0.026) | -0.040 (0.028) |
| $\Delta_{t-3,t-1}$ population | | 31.938 (22.562) | | -0.198 (3.675) | | 1.696 (1.065) |
| $\Delta_{t-3,t-1}$ key grants per capita | | -0.256* (0.102) | | -0.064 (0.040) | | -0.026 (0.015) |
| $\Delta_{t-3,t-1}$ debt per capita | | 0.071*** (0.020) | | 0.017 (0.010) | | 0.011*** (0.003) |
| $\Delta_{t-3,t-1}$ unemployed per capita | | 1477.366* (706.312) | | 114.957 (303.692) | | -128.547 (111.371) |
| $\Delta_{t-3,t-1}$ employment at residence per capita | | 29.942 (201.855) | | 71.259 (81.165) | | 11.512 (33.301) |
| $\Delta_{t-3,t-1}$ property tax per capita | | 0.009 (0.514) | | -0.414 (0.216) | | -0.126* (0.061) |
| $\Delta_{t-3,t-1}$ debt per capita | | 0.071*** (0.020) | | 0.017 (0.010) | | 0.011*** (0.003) |
| $\Delta_{t-3,t-1}$ spending per capita | | 0.061*** (0.010) | | 0.004* (0.002) | | -0.001* (0.000) |
| Observations | 6294 | 6294 | 6294 | 6294 | 6294 | 6294 |
| Residual standard error | 148.4 | 141.7 | 60.6 | 60.4 | 22.3 | 22.3 |

All regressions include federal state fixed effects. Regressions with control variables also include NA dummies for each control variable that capture cases where the control variable was not available. Coefficients of these variables are not reported in the table. Heteroskedasticity-robust standard errors in parentheses; * : $p < .05$, ** : $p < .01$, *** : $p < .001$.

the change, these variable are significant if the direction of change matches the direction of the census shock.

The results can be found in Table 4. Clearly, the strongest change goes into the opposite direction as the census shock, and therefore offsets its effect on key grants (columns 1 and 2). The coefficient is significant and does not change when controls are added to the regression in column 2. The columns labeled “medium change” contain results for the variable that was changed the second strongest in absolute terms. It is smaller, thus less closely related to the census shock, but it is also negative and marginally significant. The weakest change in any of the three variables is even less systematically related to the census shock. On the other hand, if adjustment costs matter, it might be preferable to change only one or two parameters. Because transaction costs of tax changes are relatively high, many jurisdictions will not adjust taxes. Binding debt ceilings or zero debt levels might prevent jurisdictions from adjusting debt.

3.4. Sources of Heterogeneity

The preceding section showed that there is substantial heterogeneity in municipalities’ reactions to the census shock. This section explores the sources of this heterogeneity in greater detail. Instead of focussing on one or two particular variables, we make use of generalized random forests due to [Athey et al. \(2019\)](#). Generalized random forests are non-parametric estimators that also allow for instrumental variable regression.

As potential dimensions of heterogeneity, we consider the homeownership rate, the ratio of workers at workplace to workers at residence, the number of unemployed persons per resident, the municipality’s population, its property and business tax income per capita, and its debt per capita. One potentially interesting dimension of heterogeneity is the homeownership rate of municipalities. From a polit-economic perspective, homeowners are voters and prefer low property tax rates. Moreover, homeowners also prefer low debt-financed spending. Since they usually stay at the place where they live for a longer time period

than renters, they consider the financial situation of their municipalities from a longer-term perspective. As a result, they anticipate that debt will have to be repaid in the future which implies that tax rates will have to be increased one day in the future.

Similarly, the ratio of workers at workplace to workers at residence might matter. If a large share of employees does not work at the municipality where they live but commutes to work to another place and a large share of those employees that work at that municipality commutes to work from another place, higher business tax rates matter less to workers at residence who are voters from the perspective of municipal politicians. Commuters who do not profit from a business-friendly local tax rate care less about municipalities increasing business tax rates.

We first estimate the regular instrumental variable regression with state fixed effects and use the residuals as dependent variable in the non-parametric regression. The object of interest is $\tau(X)$, the treatment effect as a function of covariates X . Due to the estimation strategy, our estimate of τ measures deviations from the average effect. A test of the null hypothesis $\hat{\tau}(X) = 0$ for a specific X is thus a test for treatment effect heterogeneity at that value of X .

A general problem with high-dimensional non-parametric regression is that it becomes very difficult to interpret results. The dimensionality-reducing effect of the regression is lost if there are too many dimensions varying at the same time. We therefore propose the use of clustering algorithms in order to ease interpretation. After running the non-parametric regression, we predict each observation’s individual, heterogeneous treatment effect and calculate the 95% confidence interval. We drop observations where the treatment effect is not significantly different from the average effect (that is, from zero, since we analyze the residuals of the main regression), and divide the sample into observations with above- and below-average treatment effects. We then use a k-means clustering algorithm to find clusters of similar observations, where clustering is based on covariates (that is, the dimensions in which we allow for heterogeneity). The number of clusters is selected by visual inspection of a Scree-plot that displays the sum of the within-cluster sum of squared deviations.

Thus far, we only find significant effects for the property tax.³ For that reason, we only show results for this variable. Moreover, only the positive deviations happen to be significant in this case. Since the main effect is negative in the case of the property tax, see Table 2, municipalities with positive deviations adjusted the property tax less than the average municipality.

Based on the Scree test, we chose four clusters. For each cluster, the medians of the seven covariates relative to the overall sample median, and the deviation from the average treatment effect are displayed in Table 5. In total, the treatment effect is significantly smaller than the average effect in 2679 cases. Due to missings on several X variables, there are 6954 observations in total in this sample.

The four clusters have in common that the treatment effect is reduced to near zero, from a highly significant -0.035 average effect in the specific sample. Moreover, they all have below-average property tax B revenue per capita, and below-average debt per capita (as measured one year after the respective census).

The clusters also differ in various dimensions. The two largest clusters, 2 and 3, as well as the smallest cluster, 4, have an above-average homeownership rate and a below-average ratio of workers at workplace

³The confidence bands are just too wide in the other cases. We currently work on adding a greater number of observations from the 1987 census in order to reduce estimation error.

Table 5: Municipalities that do not adjust the property tax

| Cluster | (1) | (2) | (3) | (4) |
|--|-------|-------|-------|-------|
| homeownership rate | 0.90 | 1.07 | 1.07 | 1.03 |
| ratio workers at workplace to workers at residence | 1.62 | 0.87 | 0.84 | 0.95 |
| unemployed per capita | 1.00 | 0.85 | 0.77 | 2.43 |
| population | 1.75 | 0.66 | 0.73 | 0.49 |
| property tax B revenue per capita | 0.67 | 0.45 | 0.94 | 0.86 |
| business tax revenue per capita | 1.06 | 0.44 | 1.00 | 0.54 |
| debt per capita | 0.85 | 0.68 | 0.68 | 0.70 |
| $\hat{\tau}(X)$ | 0.034 | 0.035 | 0.031 | 0.025 |
| Observations | 519 | 985 | 989 | 186 |

to workers at residence. They are also smaller in terms of resident population. Clusters 2 and 4 also have lower business tax revenue. Cluster 4 differs from clusters 2 and 3 mostly because it has a high share of unemployed persons.

In contrast to the other clusters, cluster 1 has a below average homeownership rate and an above-average ratio of workers at workplace to workers at residence. Moreover, municipalities in this cluster are 75% bigger than the median municipality in the sample in terms of their population, and revenue from business taxes is also slightly higher than on average.

4. Conclusion

In this paper, we analyzed responses of municipalities to a change in grants assigned to a great number of municipalities in a fiscal equalization scheme. Relying on variation that was caused by census population adjustments following the censuses in Germany in 1987 and 2011, we found that municipalities primarily used debt and spending, as well as property taxes in order to offset the change in grants. Moreover, municipal behaviour exhibits important heterogeneities.

Future research should devote more attention to the effects of fiscal equalization schemes on economic outcomes. In order to study competition between municipalities for people and businesses, it is important to understand how municipalities react to income shocks in general, and to fiscal equalization schemes in particular. Sound answers to these questions are necessary when developing place-based policies that seek improve living conditions in the periphery.

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Appendix A: Model

To motivate our empirical analysis, we set up a simple static small-open region model with intergovernmental transfers and public debt.⁴ We consider a region with land endowment, L , where the immobile residential population, M , is made up of landowners, l , and tenants, t : $M = l + t$. Both landowners and tenants use one unit of land for housing at a cost of r . To produce the numéraire, capital, labor, and land are employed. The per-unit-of-land production function is denoted $y = f(k, n)$, where output, y , capital, k , and labor, n , are expressed per unit of land. The production function is strictly increasing, linearly homogeneous, and strictly concave, i.e., $f_k > 0$, $f_n > 0$, $f_{nn} < 0$, $f_{kk} < 0$, and $f_{kk}f_{nn} > f_{nk}$.⁵ Both immobile residents and mobile commuters provide labor, one unit each. It is assumed that due to a sufficiently small residential population workers from neighbor regions commute into the region. Returns to capital, ρ , and wages, w , are determined outside the region and taken as given, whereas the return to land, r , is endogenously determined. Profits per unit of land are $\pi = f(k, n) - \rho k - t_k k - wn - r - t_l$, where t_k and t_l indicate the tax on capital and the land tax, respectively. First-order conditions

$$f_k(k, n) - \rho - t_k = 0, \quad (2)$$

$$f_n(k, n) - w = 0, \quad (3)$$

$$f(k, n) - f_k(k, n)k - f_n(k, n)n - r - t_l = 0, \quad (4)$$

define the equilibrium values of capital, k , labor, n , and the return to land, r as functions of taxes, t_k and t_l , and also wages, w , with

$$\begin{pmatrix} k_{t_k} & k_{t_l} & k_w \\ n_{t_k} & n_{t_l} & n_w \\ r_{t_k} & r_{t_l} & r_w \end{pmatrix} = \begin{pmatrix} f_{nn}/\Delta & 0 & -f_{kn}/\Delta \\ -f_{kn}/\Delta & 0 & f_{kk}/\Delta \\ -k & -1 & -n \end{pmatrix}, \quad (5)$$

where $\Delta = f_{kk}f_{nn} - f_{kn}^2$. The land tax capitalizes in land returns and has no effect on capital, labor, and output. The capital tax affects capital and land returns negatively; higher outside options for mobile workers reduce labor and land returns.

The local jurisdiction provides a completely rival good, g , to its citizens and finances it with taxes, per-capita transfers from the federal government, z , and per-capita public debt, d . The budget constraint is

$$g = (t_k k + t_l)L/(l + t) + z + d. \quad (6)$$

Ad hoc, we assume non-negative tax rates: $t_l \geq 0$ and $t_k \geq 0$. To keep the model simple, we refrain from fully-fledged dynamic modeling. Instead, we assume that debt must be paid off in the same period. To capture the excess burden associated with financing debt repayment, we introduce a surcharge, e . Because tenants do not face transaction costs of selling land, tenants are more mobile than landowners, which implies that a tenant bears a possibly smaller share of debt repayment, $\beta \leq 1$. To balance the

⁴This model builds on [Holm-Hadulla \(2018\)](#).

⁵First and second partial derivatives are indicated by subscripts.

budget, a landowner's share is $1 + (1 - \beta)t/l$. Hence, income of a landowner, y_l , and a tenant, y_t , is

$$y_l = w + rL/l - r - t_l - (1 + e)d[1 + (1 - \beta)t/l], \quad (7)$$

$$y_t = w - r - t_l - (1 + e)d\beta, \quad (8)$$

respectively. Utility of landowners and tenants depends positively on income and the publicly provided good. Hence, their utility is $u(y_l, g)$ and $u(y_t, g)$. To obtain clear-cut results, we assume additive separable utility implying identical marginal utility of publicly provided goods for both population groups. The utility function satisfies $u_g > 0$, $u_y^t > 0$, $u_y^l > 0$, $u_{gg} < 0$, $u_{yy}^t < 0$, and $u_{yy}^l < 0$.

We assume that social welfare v is a weighted average of landowners' and tenants' utility: $v = \alpha u(y_l, g) + (1 - \alpha)u(y_t, g)$, with the weight taken from the unit interval: $\alpha \in [0, 1]$. Particularly interesting special cases include zero, unit, and population share weights of landowners. Weights can be derived from equity objectives, but also from political power. Taking the market equilibrium conditions, Equations (2)-(4), the government budget constraint, Equation (6), and the income conditions, Equation (7)-(8), into consideration, the local jurisdiction sets capital and land taxes and debt levels as to maximize social welfare.

Marginal effects of the control variables are

$$\frac{\partial v}{\partial t_k} = (1 - \alpha)u_y^t k - \alpha u_y^l k \frac{L - l}{l} + u_g \frac{L}{l + t} \left(k + \frac{f_{nn} t k}{\Delta} \right) \quad (9)$$

$$\frac{\partial v}{\partial t_l} = [u_g l - \alpha u_y^l (l + t)] \frac{L}{l(l + t)}, \quad (10)$$

$$\frac{\partial v}{\partial d} = u_g - (1 + e) \left[(1 - \alpha)u_y^t \beta + \alpha u_y^l \left(1 + (1 - \beta) \frac{t}{l} \right) \right]. \quad (11)$$

It depends on the excess burden, e , on the debt burden share of tenants, β , on the population share of landowners, $l/(l + t)$, and on the welfare weight of landowners, α , whether or not the government taxes land, takes on debt, or uses both instruments. For example, if $\beta = 1$, $y_l > y_t$ and, thus, landowners are better off than tenants implying $u_y^l < u_y^t$. If, in addition $\alpha = l/(l + t)$, $\partial v/\partial t_l > \partial v/\partial d$ and, therefore, the government would not take on debt, i.e., $d = 0$. Only if landowners' welfare weight, α , is disproportionately high and debt repayment burden, e , is rather low, the government runs into debt. If, however, $\alpha = 1$, the government has a strong preference for debt rather than land tax.

Because the immobile factor land bears the burden of the capital tax, the capital tax is an instrument for redistribution between landowners and tenants. The higher the welfare share of landowners is, the less likely the marginal welfare effect of the capital tax is positive.

For sufficiently high landowners' welfare weight, sufficiently low debt repayment burden, sufficiently high population share of tenants, and sufficiently high debt burden share of tenants, the government will

use all three instruments. Interior solutions for all instruments imply

$$\frac{u_g}{u_y^l} = \frac{\alpha(l+t)}{l}, \quad (12)$$

$$\frac{u_y^t}{u_y^l} = \frac{\alpha}{1-\alpha} \left[\frac{t}{l} - \frac{e(l+t)}{(1+e)l\beta} \right], \quad (13)$$

$$t_k = -\frac{\Delta k}{f_{nn}} \frac{l+t}{L} \left[1 - \frac{e}{(1+e)\beta} \right]. \quad (14)$$

Necessary conditions for interior solutions are $\beta > e/(1+e)$ and $t/(l+t) > e/[(1+e)\beta]$.

In this case, the capital tax only depends on the wage, the return to capital, the land endowment, the composition of the residential population, the debt repayment burden, and the tenants debt burden share, but is independent of the jurisdictions government budget constraint. Hence, any change in intergovernmental transfers does not affect the tax on capital.

If either the land tax or public debt was not used to finance the publicly provided good, the tax on capital would interact with the local jurisdiction's budget constraint and, therefore, intergovernmental transfers would affect the tax on capital.

Appendix B: Exogeneity of the Instrument and Exclusion Restriction (for the Census 2011)

Figure 2 illustrates the correlations between the census shock and the following demographic characteristics: population change, immigration and emigration change and change of the share of the population aged 65 years and older. If the precision of the population estimation in a municipality was systematically related to its population change, for example, the census shock could not be considered as exogenous for our analysis, since population change is very likely to be correlated with the economic situation of a municipality and hence also with its financial decisions. The vertical axes depict average yearly change rates of the respective variable during the time period from 2008 to 2010. Since the census shock "occured" in 2011 (it was only in 2013 that the census results were published), the considered time period ends in 2010. 2008 is chosen as start year for reasons of data availability and comparability. The scatterplots show no systematic correlations between the census shock and any of the four considered demographic variables. Apart from a few outliers, most observations are centered around 0 on both axes.

Our instrument could also not be considered as exogenous if it was correlated with the economic situation of a municipality, since the economic condition of a municipality most likely also has an effect on its financial condition and thus reaction. Figure 3 shows correlations between the census shock and the employment (subject to social insurance contributions) change as well as the change of the share of long-term unemployment. The latter can be considered as an indicator of the degree of the structural weakness of a municipality and is therefore of interest with regard to its economic situation. Neither the employment change nor the change of the share of long-term unemployment are obviously systematically correlated with the census shock. Hence, the scatterplots in Figure 3 confirm the picture that became obvious in Figure 2.

To check the robustness of the results presented so far, the correlations are analyzed for different time periods. Figure 4 depicts the correlations between the census shock and the annual change rates between 2010 and 2009 for one demographic variable (population change) and one economic variable

Figure 2: Correlations between census shock and demographic characteristics

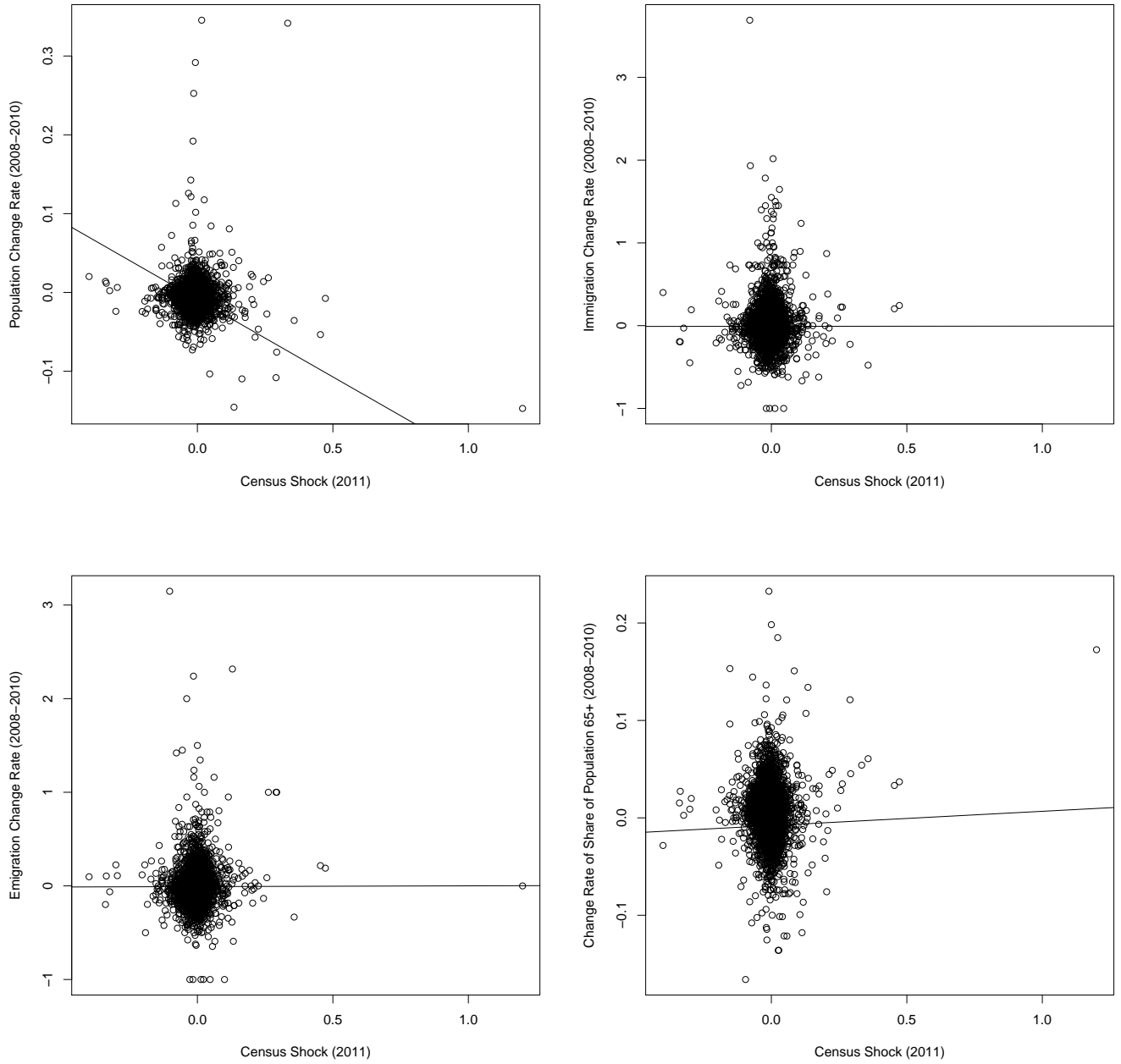
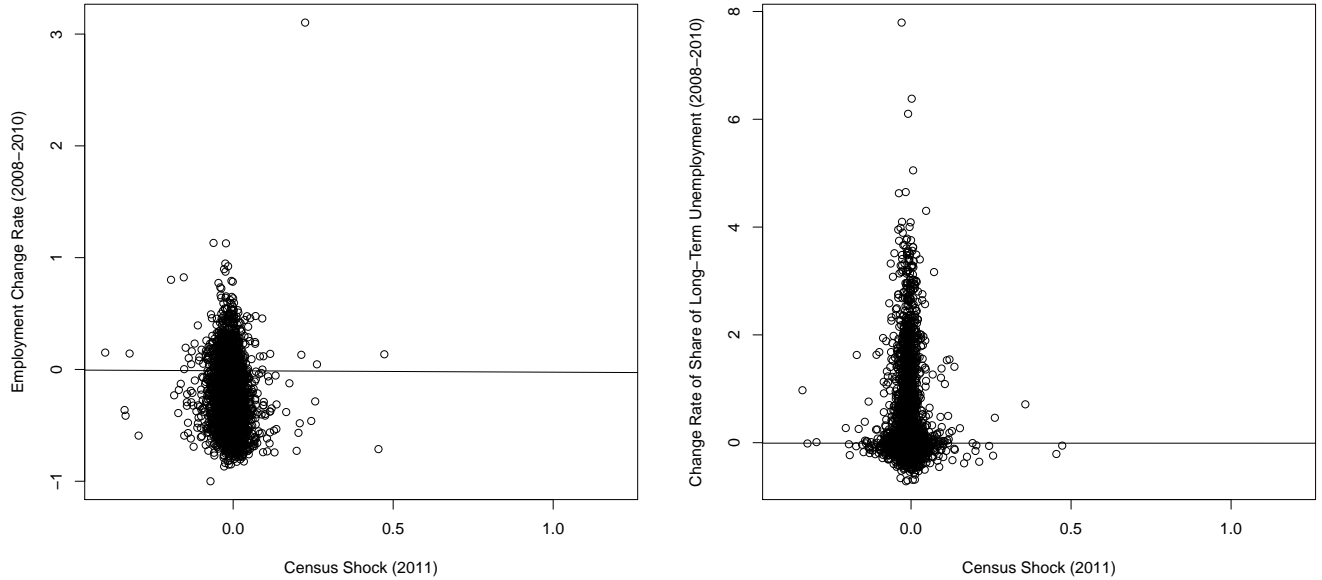


Figure 3: Correlations between census shock and economic conditions



(employment change). The observation of no correlations between the census shock and the considered variables remains the same also for this time period.

Instead of analyzing average yearly change rates from the period from 2008 to 2010, as done in Figures 2 and 3, the variables plotted on the vertical axes in Figure 5 are expressed in change rates from 2010 to 2008. No relevant change emerges compared to the previous figures.

To sum up the correlation results, the evidence suggests that the census shock is uncorrelated with demographic characteristics and economic conditions that are likely to have an effect on financial decisions of municipalities.

Another problem would arise if the Census shock was not distributed randomly across municipalities of different size. Figure 6 illustrates the correlation between the population size of municipalities according to the Census counts 2011 and our Census shock variable for municipalities with less than 200000 inhabitants. It becomes obvious that there is no systematic correlation between these two variables, the Census shock is distributed quite symmetrically around 0 independent of the population size. This means that municipalities equally over- and underestimated their number of residents. In Spain, municipalities are reported to overestimate their number of residents when being close to certain population thresholds due to discontinuous jumps in grant allocations once these thresholds are exceeded (Foremny et al., 2015). In contrast to that, in Germany, the financial need of municipalities is determined by multiplying the number of residents of municipalities by a certain factor which increases mostly linearly with population size.

Figure 7 shows correlations for smaller municipalities with a number of residents below 50000 (left panel) and for all municipalities (right panel). They confirm the picture from above.

Another important question is whether the effect of the population adjustment on the reaction vari-

Figure 4: Correlations for change from 2010 to 2009

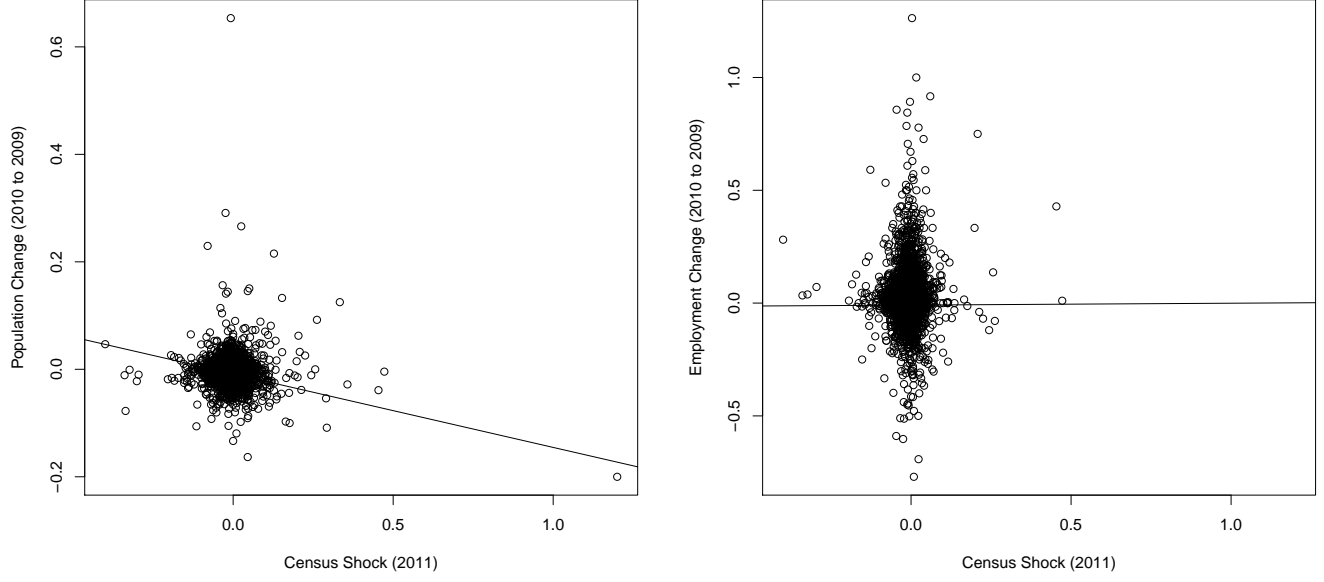


Figure 5: Correlations for change from 2010 to 2008

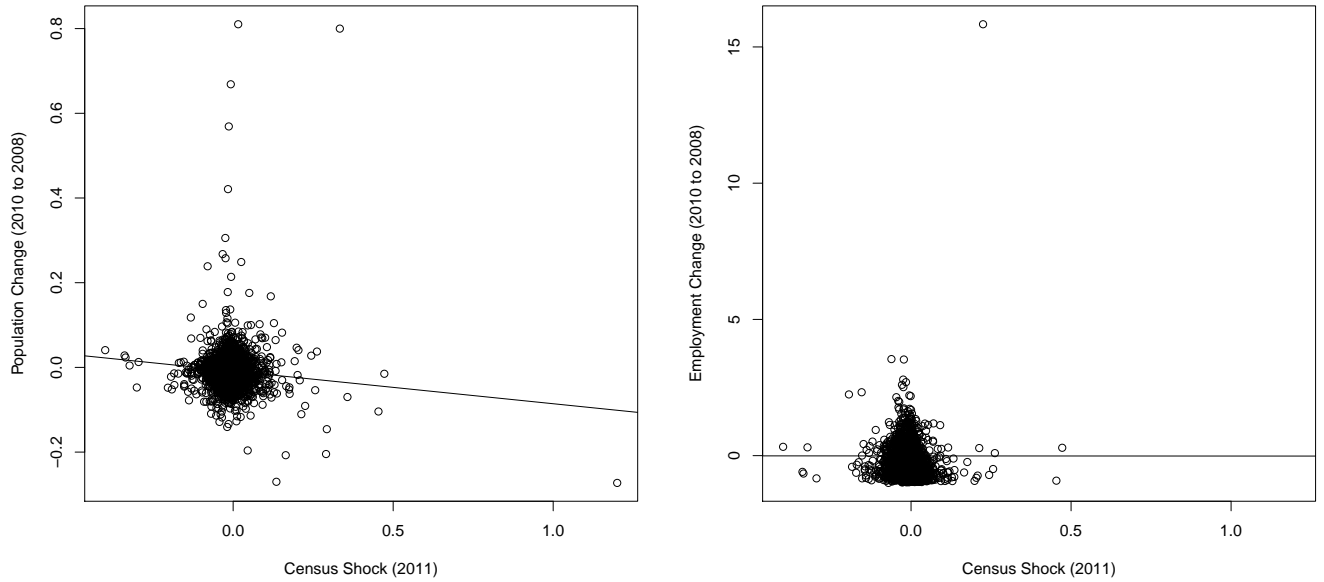


Figure 6: Correlation between population size and Census Shock for municipalities < 200000 residents

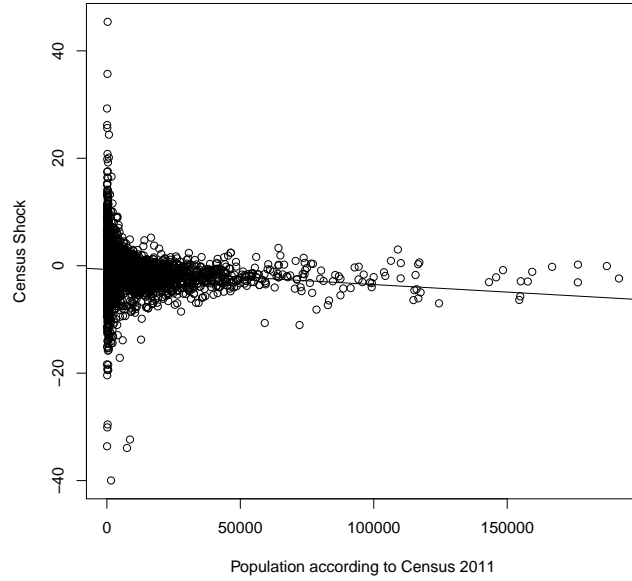
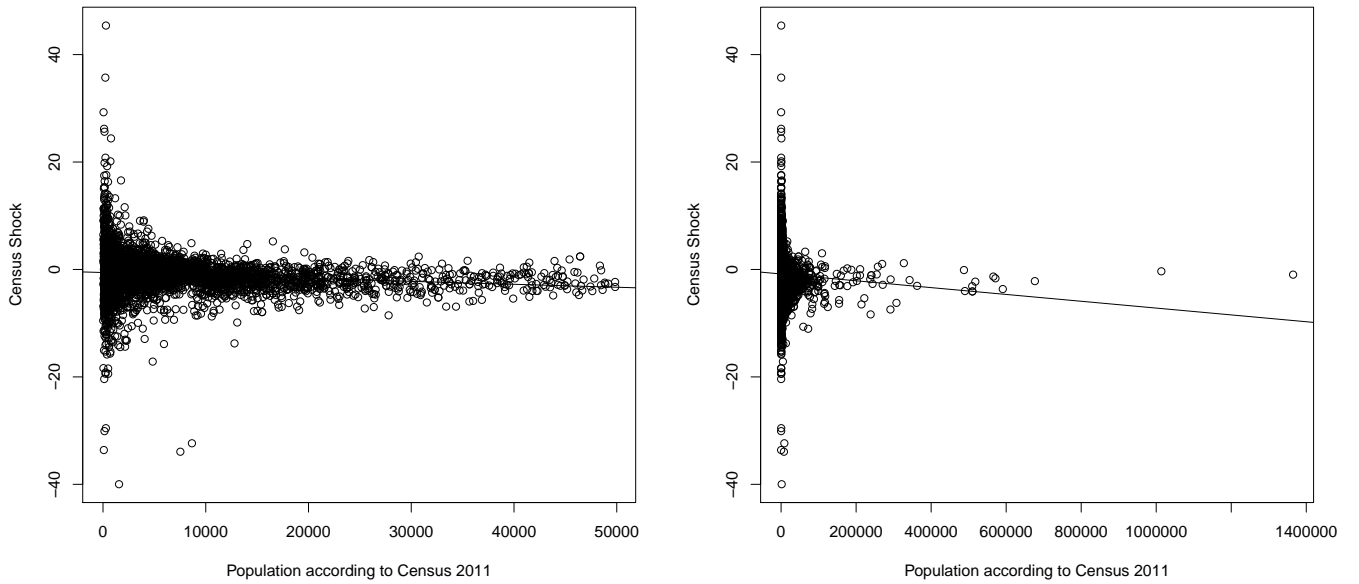


Figure 7: Correlations between population size and Census Shock for municipalities < 50000 residents (left panel) and total sample (right panel)



ables taxes, debt and spending occurs exclusively via a change of allocated key grants. This would not be the case, for example, if municipal spending was mechanically linked to population size. However, only very few exceptions exist in Germany where there is a direct link between the population size and the expenditures or revenues of a municipality. Municipalities with 80000 or more residents have to pay for expenditures for those parts of federal highways that pass through their municipal areas (see Law on Federal Highways "Bundesfernstraßengesetz"). Responsibilities and expenditures of municipalities also vary with the definition of a municipality as a central location. The status of a central location is attributed to municipalities on the basis of criteria of reachability and population size. However, a municipality is not automatically defined as a central location once its number of residents exceeds a certain threshold. As far as the revenue side of municipalities is concerned, network operators have to pay a concession fee to municipalities which is staggered according to population size (see Regulation on Concession Fees for Electricity "Verordnung über Konzessionsabgaben für Strom und Gas"). Since there are basically no relevant mechanical links between the number of residents of municipalities and their expenditures and revenues, the assumption of the exclusion restriction seems to be fulfilled.