DISCUSSION PAPER SERIES

DP16137

The Dynamic Response of Municipal Budgets to Revenue Shocks

Ines Helm and Jan Stuhler

PUBLIC ECONOMICS

CEPR

ISSN 0265-8003

The Dynamic Response of Municipal Budgets to Revenue Shocks

Ines Helm and Jan Stuhler

Discussion Paper DP16137
Published 10 May 2021
Submitted 09 May 2021

Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

The Dynamic Response of Municipal Budgets to Revenue Shocks

Abstract

We study the fiscal and tax response to intergovernmental grants, exploiting quasi-experimental variation within Germany's fiscal equalization scheme triggered by Census revisions of official population counts. Municipal budgets do not adjust instantly. Instead, spending and investments adapt within five years to revenue gains, while adjustment to revenue losses is more rapid. Yet, the long-run response is symmetric. The tax response is particularly slow, stretching over more than a decade. Well-known empirical “anomalies” in public finance such as the flypaper effect are thus primarily a short-run phenomenon, while long-run fiscal behavior appears more consistent with standard theories of fiscal federalism.

JEL Classification: H71, H72, H77, E62

Keywords: Intergovernmental Grants, Fiscal Transfers, government spending, Local taxation, Census Shock, Flypaper effect

Ines Helm - ines.helm@ne.su.se
Stockholm University and CEPR

Jan Stuhler - jstuhler@eco.uc3m.es
Universidad Carlos III de Mardid and CEPR

The Dynamic Response of Municipal Budgets to Revenue Shocks

Ines Helm* and Jan Stuhler‡

May 6, 2021

We study the fiscal and tax response to intergovernmental grants, exploiting quasi-experimental variation within Germany's fiscal equalization scheme triggered by Census revisions of official population counts. Municipal budgets do not adjust instantly. Instead, spending and investments adapt within five years to revenue gains, while adjustment to revenue losses is more rapid. Yet, the long-run response is symmetric. The tax response is particularly slow, stretching over more than a decade. Well-known empirical "anomalies" in public finance such as the flypaper effect are thus primarily a short-run phenomenon, while long-run fiscal behavior appears more consistent with standard theories of fiscal federalism.

Keywords: Intergovernmental grants, fiscal transfers, government spending, local taxation, Census Shock, flypaper effect
JEL Classification: H71, H72, H77, E62

* Stockholm University (email: ines.helm@ne.su.se)
† Universidad Carlos III de Madrid (email: jan.stuhler@uc3m.es)
‡ We thank Deborah Cobb-Clark, Christian Dustmann, Dirk Foremny, Ronny Freier, Steve Gibbons, Andreas Lichter, Eva Mörk, Nora Gordon, Marko Koethenbuerger, Kathrine von Graevenitz, Andreas Peichl, Nico Pestel, Gregor Pfeifer, Davud Rostam-Afschar, Dominik Sachs, David Seim, Albert Solé-Ollé, Daniel Sturm and Juan Carlos Suarez Serrato, as well as seminar participants at the Urban and Regional Economics Seminar at LSE, the Berlin Applied Micro Seminar, the Regional Economics Committee of the German Economic Association, the DFG-SPP 1764 workshop, CReAM Workshop in Piemonte, UEA Conference in Düsseldorf, Uppsala University, Stockholm University, Barcelona Institute of Economics (IEB), University of Stuttgart-Hohenheim, University of Cologne, University of Munich, and the Institute for the Study of Labor for their valuable comments. We thank Sergio Correia and Ben Jann for providing the reghdfe and coefplot packages, and Juan Antonio Montero de Espinosa Reina and Ivan Logrosan Tercero for excellent research assistance. Financial support from the Thyssen Foundation, the Ministerio Economía y Competitividad (Spain, MDM 2014-0431 and ECO2014-55858-P), and the Comunidad de Madrid (MadEco-CM S2015/HUM-3444) is gratefully acknowledged.

1 Introduction

Since many public goods are provided at the sub-national level, local public finances are important for the overall functioning of government. Accordingly, a rich literature explores how local governments make fiscal decisions, and how those decisions are distorted by the division between different layers of government (*fiscal federalism*). Several key predictions, however, have only limited empirical support. One prominent example is the observation that municipal governments that receive lump-sum grants tend to spend most of them – the so-called "flypaper effect" (see Hines and Thaler, 1995, or Inman, 2008) – a finding that is at odds with standard models of collective choice, which predict that governments should redistribute a portion of the grants by reducing taxes (Bradford and Oates, 1971a,b).

We argue that this and other well-known anomalies may in part be explained by the *dynamics* of fiscal adjustments. Theoretical work tends to focus on steady-state arguments and to abstract from the timing of the adjustment process. Empirical work based on vector error correction models reveals intertemporal statistical associations, but is less informative about the causal response to fiscal transfers (Martin-Rodriguez and Ogawa, 2017).[1] And while a recent quasi-experimental literature provides snapshots of that response, the underlying variation is rarely suitable to identify long-run effects.[2] Gordon (2004) and Cascio et al. (2013) are important exceptions, as they demonstrate a delay in the fiscal adjustment to a specific type of grant.[3]

In this paper, we study the dynamic municipal response to a general intergovernmental grant, based on a broad and representative sample and a long time window. Specifically, we study the evolution of municipal budgets after a sudden shift in revenues within Germany's municipal fiscal equalization scheme. To isolate variation that is not endogenous to local economic trends we exploit that (i) the allocation of transfers within the equalization scheme depends on local population counts, and that (ii) after a Census, register-based projections are abruptly replaced by direct counts from the Census. As this "Census Shock" triggers revenue gains *and* losses, the setting also allows us to study the symmetry of fiscal decisions.

The core identification problem in the literature stems from the endogeneity of fiscal transfers to local fiscal and economic conditions, and the political economy between different layers

[1] Contributions include Buettner (2009) for Germany, Buettner and Wildasin (2006) and Holtz-Eakin et al. (1989) for the U.S., Dahlberg and Johansson (1998) and Dahlberg and Johansson (2000) for Sweden, Solé-Ollé and Sorribas-Navarro (2012) for Spain, and Bessho and Ogawa (2015) for Japan. Overall, this literature suggests that important dynamic interrelationships do exist, but that they occur within a limited time period.

[2] Knight (2002) and Baicker (2001) study variation in federal spending across U.S. states, and several recent studies exploit population or other thresholds in grant allocation formulas at the county or municipal level, as in Dahlberg et al. (2008), Litschig and Morrison (2013), Gennari and Messina (2014) or Liu and Ma (2016).

[3] Gordon (2004) and Cascio et al. (2013) find that U.S. districts receiving a federal education grant adapt their educational expenditures over a two- to three-year period.

of government (e.g., Knight, 2002, Dahlberg et al., 2008). However, the "fiscal need" of a municipality within Germany's fiscal equalization scheme depends on local population counts, which allows us to adapt an identification strategy proposed by Serrato and Wingender (2014) to our context. After a Census, register-based population projections are replaced by physical counts from the Census, leading to sudden, large, and permanent jumps in official population statistics – and thus in municipal transfers. We combine such quasi-experimental variation from the 1987 Census with newly digitized information on municipal revenues, spending, investment, and taxes for more than 4,000 municipalities. Since the next Census did not occur until 2011, this setting is uniquely suited to study the dynamic adjustment of municipal budgets over a long time period.

We first show that municipalities that "gained" inhabitants in the Census received additional transfers within the fiscal equalization scheme, leading to a permanent spatial reallocation of revenues. On average, a Census Shock in population counts of one standard deviation (4.1 percent) increased transfers by 7 percent (around 12 EUR per capita). The transfers were not earmarked and thus entirely fungible. Since these shifts were permanent, they led to a reallocation of billions of euros in the years after the Census. The "Census Shock" is spatially dispersed, and unrelated to economic conditions and fiscal trends in the period before the adoption of the new Census counts. Its distribution is also unrelated to two important subsequent events, German reunification and the 1993 recession.

Accordingly, we use the Census Shock as an instrumental variable to study how municipalities respond to a permanent shift in fiscal transfers. On average, transfers are neither used to restructure finances (e.g. through debt repayments or the reduced take up of loans), nor to increase public employment. Instead, municipalities invest: they acquire more assets and invest in local infrastructure. These responses are, however, neither immediate nor always monotonic. In particular, infrastructure investments peak five years after the permanent shift in transfers, before settling at a permanently higher level. These spending decisions are in line with the response patterns on the revenue side, pointing to dynamic feedback effects between spending and revenue streams. Revenues from fees and contributions increase strongly and also peak around four years after the exogenous increase in fiscal transfers. Because German municipalities have a legal obligation to pass on part of their investment costs to those who benefit, this increase is likely a direct consequence of the increased infrastructure investments. Likewise, the response in tax revenues follows a hump-shaped profile, with initial revenue gains reversing over time, reflecting the staggered and opposing effects of spending and tax decisions.

After five years, the shifts in total revenues and total expenditures are nearly twice as large as the initial shifts in transfers. We therefore find a strong multiplier effect: each additional euro in transfers increases local budgets by about two euros in the medium to long run. Around 50 percent of this effect is driven by the mechanical link between municipal investments and revenues from fees and contributions in the German system. However, municipalities also increase their take-up of loans and their revenues from other sources. We therefore find that intergovernmental grants have a positive causal effect on own revenues, even though prior time-series evidence documents a negative correlation (Martin-Rodriguez and Ogawa, 2017). These results also suggest that fiscal equalization schemes may equalize revenues and investments to a greater degree than their nominal size would suggest.

We then consider municipal tax setting. In Germany, municipalities set multipliers for a business tax and two types of property taxes. We find that they adjust all three: for each percentage gain in official population counts, the property tax multipliers decrease by about 0.41 percentage points within the following decade, while the business tax multiplier decreases by 0.26 percentage points. However, these adjustments do not take place immediately. Instead, municipalities adjust their tax rates slowly, with adjustments continuing even one decade after the revenue shock. Consequently, studies that only estimate the immediate effects would incorrectly conclude that municipal tax rates do not react to transfers, while those estimating the dynamics over three or four years might miss the bulk of the response.

Since the Census Shock triggers both transfer gains (positive shock) and losses (negative shock) we can assess whether the fiscal response is symmetric.[4] We find that in the long run, municipalities respond symmetrically. Expenditures, however, respond more quickly to transfer losses, especially administrative expenses and infrastructure investments. These results are in line with the notion that municipalities seek to immediately balance their budgets following a negative shock, while having greater flexibility with respect to when to invest the additional revenues from transfer gains.

Finally, we study how budget responses differ with initial conditions, such as the municipality's unemployment rate, debt level, or population size. Larger municipalities tend to respond more strongly along the tax dimension. As a consequence, the *long-run* multipliers of fiscal transfers on revenues and spending are much greater in small municipalities. Municipalities with low unemployment or low debt levels predominantly invest additional funds, while those with high unemployment or debt levels tend to reduce the tax multipliers, especially

[4] While much work has been conducted on potential asymmetries (e.g., Gramlich 1987, Gamkhar and Oates 1996, Heyndels 2001, Rattsø and Tovmo 2002, or Deller and Maher 2005), there exists little causal evidence based on quasi-experimental variation.

the business tax multiplier – perhaps to gain competitiveness and to attract businesses. These responses differ not only in their magnitude but also their timing.

Overall, our results suggest that the evolution of local fiscal budgets is more protracted than previously found. Given the dearth of prior evidence, our main objective is to characterize the dynamic adjustment process per se. However, the explicit contrast between short- and long-term responses also provides a new perspective on much-debated "anomalies" in local public finance. In the final part of our study, we interpret our results in the context of two such debates, on the relationship between intergovernmental grants and tax setting and on asymmetries in the response to fiscal gains and losses.

A standard prediction from collective-choice theory is that intergovernmental grants should be partially transferred to local inhabitants via tax reductions (Bradford and Oates, 1971a,b). However, this prediction has not found much empirical support. Instead, most studies find strong evidence of the so-called *flypaper effec*t and thus that "money sticks where it hits", with fiscal transfers triggering additional spending rather than tax reductions (see e.g., Dahlberg et al. 2008, Lundqvist 2015, or Leduc and Wilson 2017). This apparent contradiction between theoretical predictions and empirical evidence has motivated a vast body of work (Hines and Thaler 1995, Bailey and Connolly 1998, Inman 2008). It also raises some troubling concerns about fiscal federalism, suggesting that local officials might follow self-serving instead of collective objectives (Oates, 2008). Yet, existing empirical studies capture only a snapshot of the tax response to intergovernmental grants.

We find that the initial tax response is indeed small, but also show that tax rates continue to decline in subsequent years. The total response after one decade is a magnitude of order larger than the initial response. Moreover, the strength of the *flypaper effect* depends on the degree of political and tax competition a municipality faces: the tax response is more pronounced and quicker in municipalities facing low tax competition or strong electoral competition. Our results are therefore twofold. First, we argue that prior empirical work on the short-term response may have underestimated the degree to which fiscal transfers trigger tax decreases, and overstated the flypaper effect. Second, our results suggest that local governments do respect the fiscal preferences of their inhabitants as prescribed by standard theories, but also point to tax competition and insufficient electoral competition as two factors that mute the tax response.

Another question on which theoretical and empirical evidence are conflicting is whether policymakers respond *symmetrically* to positive and negative shocks in revenues. In contrast to the implications of standard models, many applications find that the response is asymmetric

(Deller and Maher, 2005). While the initial response is asymmetric in our setting, it becomes increasingly symmetric in subsequent years. In the long run, the spending and tax responses to transfer gains and losses are near-mirror images of each other. In particular, there is no *bias towards fiscal expansion* in our setting, which has been a particular concern with respect to the long-term sustainability of decentralized fiscal systems.

Our findings are therefore more supportive of standard theoretical models than the existing literature. A more explicit consideration of the timing of municipal decision making, and the dynamic implications of these decisions, might thus help to reconcile central theoretical predictions on fiscal federalism with the empirical evidence. Nevertheless, we do confirm that important "anomalies" exist in the short run. The observation that these anomalies are less pronounced in the long run may help to discriminate between competing theories of municipal decision making.

The paper is structured as follows. Section 2 describes the data. Sections 3 describes how the Census affects official population counts in Germany and transfers within the municipal fiscal equalization scheme. Section 4 presents our empirical framework. Our main results on the dynamic response of municipal revenues and expenditures are reported in Section 5 and on taxes in Section 6. Section 7 provides evidence on the heterogeneity of the response. Section 8 discusses the implications with regard to known anomalies in public finance, while Section 9 concludes.

2 Data Sources

Population and Census Counts. Population statistics are published by the German Federal Statistical Office. We obtained the official population counts for December 31st of each year from 1983 to 2000 for all municipalities in West Germany. Since there has been restructuring over the years, we drop municipalities for which borders changed or that merged with other municipalities. Specifically, we exclude municipalities whose area changed by more than 3 square kilometers from one year to another, losing 44 out of 7,898 municipalities and leaving us with a balanced sample of 7,854 West German municipalities, excluding Berlin. The average municipality size is 7,518 in 1986 (see Table 1, Panel A), though municipalities range in size from villages with only a few residents to the largest city in West Germany (Hamburg, with more than 1.5 million residents). Our *main sample* contains 4,373 municipalities, with a slightly larger average size of 11,330 inhabitants. These are all municipalities in the five federal states for which we were able to obtain complete fiscal budget data (see next section). The Statistical Offices of these federal states also provided us with the actual Census counts, which reflect the population of each municipality as measured in the Census on May 25, 1987.

Table 1: Summary Statistics: Population Growth and Fiscal Revenues

	Mean	Sd	Min	Max
Panel A: Population and Population Growth				
Population 1986 (West Germany, N=7,854)	7,518	35,859	8	1,571,267
Population 1986 (Main sample, N=4,373)	11,330	39,735	93	1,274,716
Population Growth 1986	0.004	0.013	-0.314	0.081
Population Growth 1987 (Census Year)	0.002	0.042	-0.409	0.198
Population Growth 1988	0.008	0.013	-0.227	0.117
Census Shock (May 25, 1987)	0.000	0.041	-0.346	0.217
Panel B: Municipal Government Revenues and Expenditures				
Total Revenues (per Capita)	1,321.16	652.06	534.08	33,714.51
Capital Budget	467.53	324.01	5.50	12,272.15
Administrative Budget	851.99	414.42	284.18	21,442.36
Municipal Fiscal Transfers	161.43	79.97	0.00	943.68
Share Transfers / Total Revenues	14%	7%	0%	46%
Total Expenditures (per Capita)	1,250.14	642.29	291.94	31,587.97
Capital Budget	488.26	422.85	29.77	22,019.54
Administrative Budget	760.74	347.33	164.90	9,568.43

Notes: Panel A reports the mean, standard deviation, minimum and maximum of population levels for all West-German municipalities (excluding Berlin) and population levels, log population growth and the Census Shock for the main sample (municipalities in Baden-Wuerttemberg (BW), Bavaria (BY), Hesse (HE), Lower Saxony (NI) and North Rhine-Westphalia (NRW)). The Census Shock is defined as the log difference between the Census count on May 25, 1987 and the population projection on December 31, 1986. Panel B reports the mean, standard deviation, minimum and maximum of municipal revenues and expenditures per capita in our main sample in 1986 (measured in EUR).

Municipal Fiscal and Tax Data. We combine the population data with detailed fiscal and tax data at the municipal level. The Statistical Offices of Baden-Wuerttemberg (BW), Bavaria (BY), Hesse (HE), Lower Saxony (NI), and North-Rhine Westphalia (NRW) were able to provide municipal fiscal budgets for the years 1983 to 2000. These are the five most populous German states, accounting for 84 percent of the overall West German population in 1986.[5] The data contain total revenues and expenditures by municipality and year, but also break down into more detailed revenue and expenditure categories. Panel B of Table 1 and Figure A.1 report summary statistics. In 1986, municipal fiscal revenues were, on average, 1,321 EUR per capita.[6] The capital budget, which includes contributions and other payments, investment grants, and revenues from the take-up of loans, accounts for about one quarter of total revenues. The remaining three quarters originate from the administrative budget, which includes revenues

[5] We focus on West Germany, as East German States only participated in the fiscal equalization scheme from 1995 onwards. We also digitalized budgets from the *Statistisches Jahrbuch deutscher Gemeinden* (Statistical Yearbook of German Municipalities). These yearbooks contain detailed fiscal information for all federal states, but only include large municipalities with more than 20,000 inhabitants. Estimates based on this data source are in line with our main estimates, but less representative and more noisy, and therefore not reported here.

[6] All revenues and expenditures are inflation-adjusted to their 1990 values.

from taxes, fees, and fiscal transfers from the national and state governments. Municipal spending includes expenditures for personnel, current expenses, repayments of loans, and building and infrastructure investments. Since municipalities are, in principle, required to balance their budgets, expenditures mirror revenues with an average of 1,250 EUR per capita.

3 The Census Shock and Fiscal Transfers

In a nutshell, our identification strategy exploits the sudden change in official population counts triggered by the 1987 German Census, which in turn led to a sudden shift in municipal fiscal transfers and revenues. Before formalizing our empirical approach in Section 4, we describe the institutional background that motivates this approach: the relationship between the Census and official population counts, the definition of the *Census Shock,* and the relationship between population counts and fiscal transfers within Germany's fiscal equalization scheme.

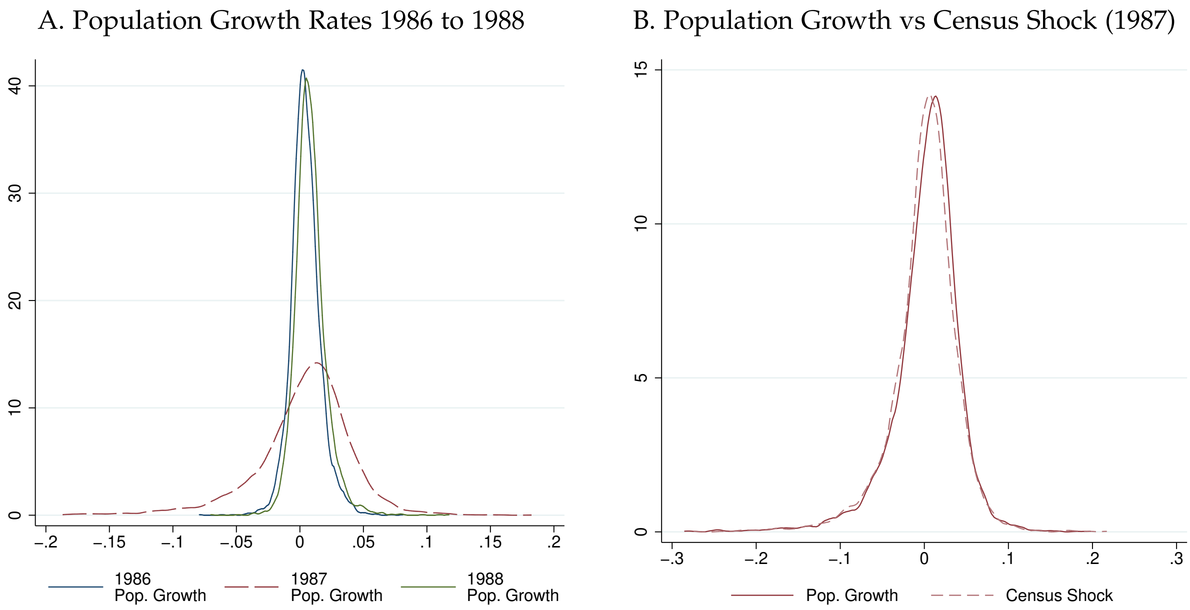
3.1 Official Population Counts and the 1987 Volkszählung

Census counts provide information about a country's regional population distribution, which is an essential determinant for the spatial allocation of public expenditures. Because Censuses are rare events, the Statistical Offices of the German Federal States provide yearly updates of official population counts based on birth and death registers, as well as internal and international population movements. When a new Census is conducted, however, these yearly projections are abruptly replaced by physical counts from the Census, which can differ considerably from the register-based projections. One likely source for such projection errors are imperfect linkages between the various registers.

In West Germany, post-WWII Censuses (*Volkszählungen*) took place in 1950, 1961 and 1970. These were full counts, with every resident required to participate by law. While the following Census was originally scheduled for 1981, it was delayed twice. A dispute between the national and federal state governments over its financing forced a first postponement to April 1983. In that same year, a protest movement formed arguing that its implementation was not in accordance with data protection laws. Several complaints were filed at the constitutional court, which decided that the Census law (*Volkszählungsgesetz*) did not sufficiently protect individuals' privacy rights. This further delayed the Census to May 1987. The 1987 Census was, in fact, the last full count to be implemented in Germany. The next (register-based) Census took place only in 2011, when Germany was required to participate in a European-wide Census.

The long gaps between the Censuses before and after the 1987 Census offer a key advantage

Figure 1: Distribution of Population Growth Rates



Notes: The figure plots kernel density functions of log population growth and the Census Shock. Panel A compares the distribution of municipal log population growth in West Germany (excl. Berlin) in the years 1986, 1987 (the census year) and 1988. Panel B compares the distribution of log population growth in our main sample (dashed red line) with the distribution of the Census Shock (solid red line). The Census Shock is measured as the log difference between the Census count on May 25, 1987 and the population projection on May 25, 1987. For readability we drop observations with population growth below -30 or above 30 log points (less than 1% of observations).

for our study. Because errors in the population projections accumulated over nearly two decades, the deviations between the projections and the Census count were large and unlikely to reflect current economic trends in the Census year (we return to this point in Section 4.3). Furthermore, because the next important revision in official statistics did not occur until 2011, over two decades later, this setting is ideal for assessing the dynamics of the causal response of municipal spending and tax decisions over long time periods.[7]

How did the 1987 Census affect official population statistics? Table 1, Panel A, reports the log growth in official population counts for the years 1986 to 1988. The *average* log population growth in 1987 (in which the Census-based counts were incorporated retroactively) differs only slightly from the projected growth in the adjacent years (1986 and 1988), but its *standard deviation* is four times larger (0.04 vs 0.01). This is further illustrated in Figure 1, Panel A, which shows that the distribution of the growth rates of municipal population counts are centered around zero in all three years, while the spread is considerably larger in the Census year. The Census thus led to considerable shifts in the population distribution across municipalities, without differing substantially from the yearly projections in the aggregate.[8] The similarity

[7] The use of the 1987 Census has additional advantages over the 2011 Census. The impact on fiscal budgets was more sudden, since several federal states only gradually incorporated the 2011 Census counts into official population statistics or implemented legislative changes to lessen its impact on transfers.

[8] For West Germany as a whole, 61.083 million inhabitants were counted in the Census as compared to 61.154

in aggregate counts also suggests that there was no systematic manipulation in municipal population projections.[9]

3.2 The Census Shock

The observed 1987 population growth reflects a combination of the true *Census Shock* (the gap between the Census counts and the official projection on the day of the Census), which is a valid source of identifying variation, and true population growth over the year, which could be endogenous to fiscal trends. We thus define the Census Shock in municipality m as the log difference in population on the day of the Census (May 25, 1987) and the last observed register-based population projection on December 31, 1986:

$$CensShock_m = log(Pop_{m,1987,census}) - log(Pop_{m,1986,register}). \quad (1)$$

This measure is less likely to pick up true population growth, as it abstracts from population growth in the second half of the Census year.[10] Because our measure may still reflect true population growth in the first five months of 1987, some concern remains. However, the distributions of the Census Shock and the 1987 population growth rate are very similar (see Figure 1, Panel B), suggesting that the latter is primarily driven by deviations between Census and register-based counts, and not by actual population growth. More importantly, our estimates remain very similar when defining the Census Shock net of imputed population growth for the first five months of the year (see Section 5.3), and the Census Shock as defined in (1) is largely uncorrelated with economic and fiscal trends (Section 4.3).

Due to the long gap to the previous Census, the 1987 *Volkszählung* led to a substantial revision of municipal population counts. The Census shock has a standard deviation of 4.1 percent (see Table 1, Panel A), and its 5th and 95th percentiles correspond to a drop in population counts of 7 and an increase of 5.4 percent, respectively. Revisions of more than 10 percent occurred in 119 municipalities in our sample. Notable examples include Munich, which lost 7 percent of its official population (89,295 inhabitants) and Göttingen, which lost 10.4 percent (13,981 inhabitants). Meanwhile, Frankfurt am Main saw its population count jump by 4.4 percent (25,855 inhabitants) while Krefeld gained 7.2 percent (15,540 inhabitants).

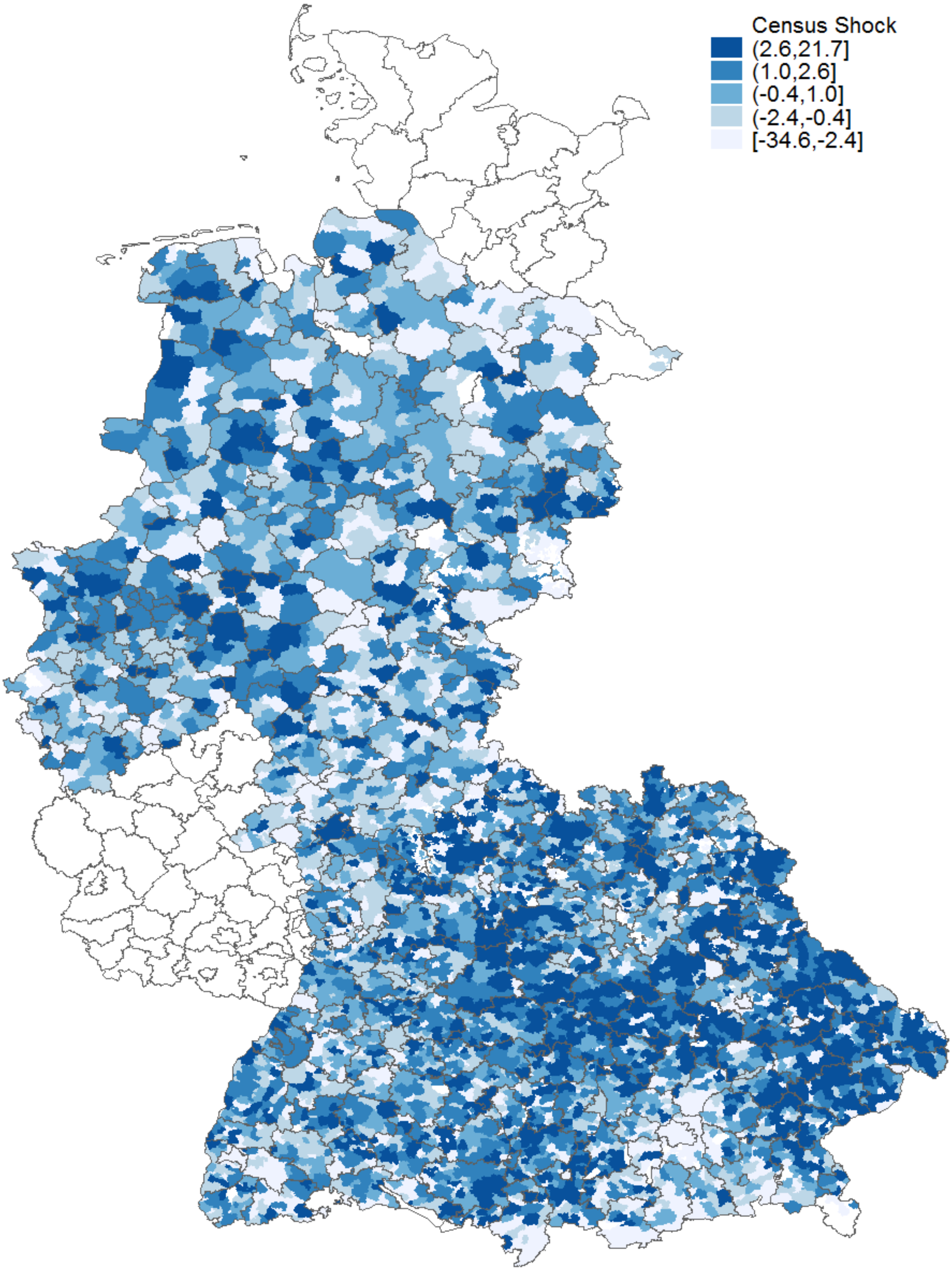
Figure 2 plots the regional distribution of the Census Shock in our main sample. The shock

millions in the projections (measured on Census day) – a difference of about 71,000 inhabitants.

[9] While equalization schemes may incentivize such manipulations (Foremny et al. 2017), manipulative sorting appears far less severe in Germany than, for example, in France or Italy (Eggers et al., 2017).

[10] Similarly, Serrato and Wingender (2014) use the US Census Bureau's "error of closure", i.e. the difference between two concurrent estimates of the population in the same year.

Figure 2: Regional Distribution of Census Shock by Quintile Groups



Notes: The figure shows the distribution of the Census Shock in population counts by quintiles (see legend in top right corner) across municipalities in the five federal states in our main sample (Baden-Wuerttemberg, Bayern, Hessia, Lower Saxony and North Rhine-Westphalia). Darker colors imply a stronger Census Shock for the respective municipality.

varies substantially within federal states and between municipalities within the same district (black lines). Regressing the Census Shock on fixed effects at different local levels, we further show that it does not vary much between federal states (Appendix Table A.1, column 1), and that more than 80 percent of its variation occurs within rather than between commuting zones or districts (columns 2 and 3). Similarly, the population-weighted Census Shock of the five nearest neighbors is not a strong predictor (column 4). This spatial dispersion is an important advantage, since it allows us to verify that economic trends in narrowly defined areas do not affect our results. It also suggests that deviations between the population projections and the Census counts are due to idiosyncratic factors that do not relate to local labor markets as a whole or to the proximity between municipalities. In Section 4.3, we show that the Census Shock is as good as random with respect to other determinants of municipal budgets.

3.3 Municipal Fiscal Budgets in Germany

In most countries, local governments depend heavily on grants and transfers from upper-level governments (*fiscal federalism*). Germany, like some other countries, administers such transfers via redistributive schemes that equalize revenue capacities across local governments. By mitigating the fiscal consequences of local economic shocks, these schemes may safeguard the provision of public goods more effectively than systems that depend on vertical grants or local taxation.[11]

Specifically, German federal states are required by constitutional law to distribute a proportion of their revenues – that is, their share of the joint tax revenues from income, capital gains, value-added and corporate taxes – to their constituent municipalities (*Finanzausgleichsmasse*). These transfers are administered through the municipal fiscal equalization scheme (*kommunaler Finanzausgleich*), which aims to reduce budget disparities across municipalities through intergovernmental transfers. The bulk of these transfers is allocated according to a fixed spending formula (*Schlüsselzuweisung*) to reduce the difference between a municipality's fiscal need (*Finanzbedarf*) and its fiscal capacity (*Steuerkraftsumme*). A municipality's fiscal capacity is determined by the tax base of the local business tax and other revenue sources, mainly the local share of the statewide income tax.[12] The main determinant of fiscal need is a municipality's population size.[13] This implies that, holding fiscal capacity fixed, a municipality's share of

[11] Buettner (2009) estimates that the contribution of intergovernmental transfers to restoring fiscal balance after a revenue shock is two to three times larger in the German context than in the U.S.. Local economic shocks might also have greater fiscal consequences in the U.S.. For example, Feler and Senses (2017) demonstrate that exposure to Chinese import competition in U.S. localities led to a substantial deterioration of local public finances and services.

[12] Importantly, hypothetical statewide rates are used to determine the fiscal capacity of a municipality rather than the actual municipal business and property tax rates.

[13] For some states, the transfer formula also depends on other factors. For example, municipalities in BY that were close to the former East-German or Czechoslovakian borders received a higher weight, while transfers in NRW also depend on the number of school pupils and the unemployed. In all cases, however, the local population size is by far the most important factor. In a typical municipality, the equalization scheme compensates more than two-thirds of the gap between fiscal need and fiscal capacity (Buettner, 2009).

funds from the state budget increases in its official population count. We provide more detailed information on the allocation formula in Appendix D.

Importantly, the transfers allocated under this spending formula are not earmarked, but are fungible and can be spent as municipal governments see fit. On average, fiscal transfers amount to 161 EUR per capita and year, and are thus an important component of municipal revenues (see Panel B of Table 1).[14] But the dependency on transfers varies considerably across municipalities. A small share receives no transfers at all, while others receive up to 46 percent of total municipal revenues (see Table 1 and Figure A.1). Municipalities provide a wide range of public goods and services. These include expenditures for personal and materials and the provision of local services (including schools and hospitals), infrastructure (such as roads, electricity and water) and social and transfer payments. While municipalities have to provide certain services, they have considerable freedom in how they allocate their funds. Budget decisions are made by the municipal council using simple majority rule. To ensure that budgets are set in accordance with a municipality's financial capacity, they are supervised by the federal states (*kommunale Finanzaufsicht*).

3.4 The Census Shock in Municipal Transfers

By legislation, federal states calculate a municipality's fiscal need on the basis of counts from two years prior rather than on contemporaneous counts. As such, the Census affects fiscal transfers from 1989 onwards.[15] Anticipatory effects are unlikely to be important in our setting, as the German Federal Statistical Office published the provisional Census counts only in late 1988 (Statistisches Jahrbuch, 1988), and the final counts in 1989.[16]

The institutional setting is also informative about the functional form between the Census Shock and changes in transfers. In Appendix D, we show that non-linearities in the transfer formula have only limited implications and that shifts in a municipality's fiscal need are well approximated by a linear function of population.[17] Consequently, a percentage change in the official population count should have an approximately linear effect on fiscal transfers per

[14] Abstracting from the double accounting of shifts between the administrative and capital budgets (see Section 6.2), the share of fiscal transfers on overall revenues is 17%. Our definition of "fiscal transfers" abstracts from other types of transfers within the municipal fiscal equalization scheme, such as investment grants.

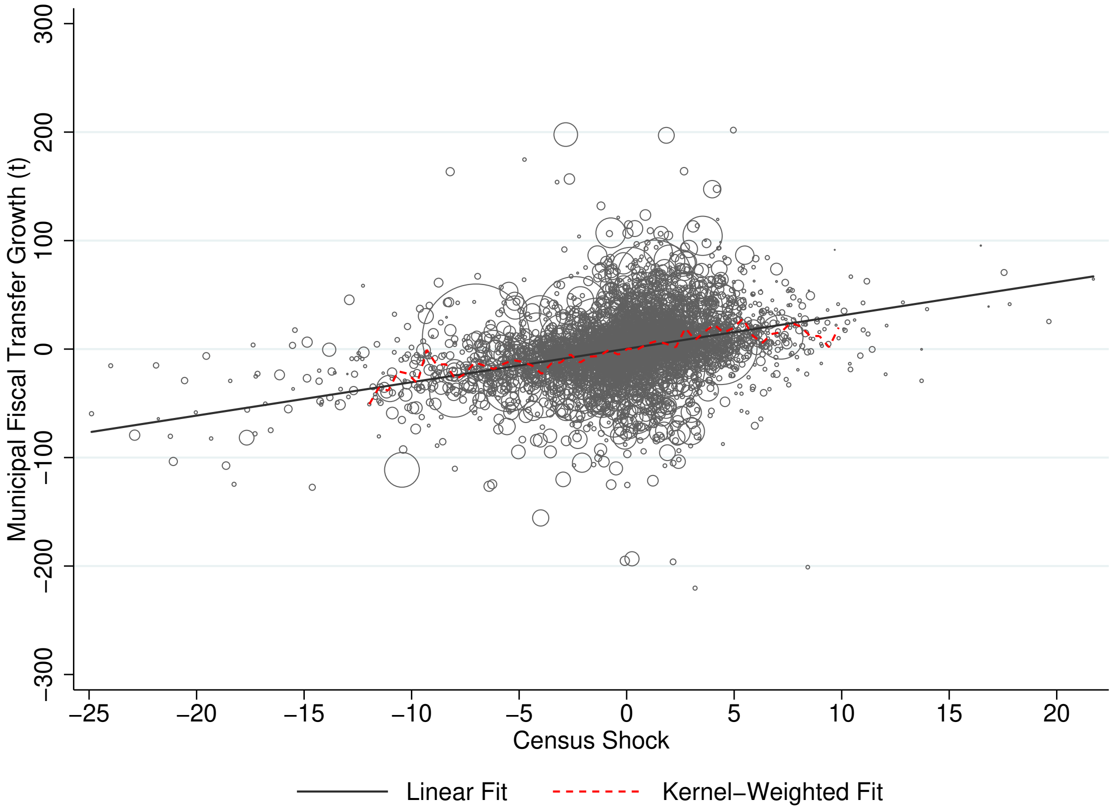
[15] The regulations can be found in Verordnung zum Finanzausgleichsgesetz Bayern (1988), Gesetz über die massgebende Einwohnerzahl Baden-Württemberg (1988), and Gemeindefinanzierungsgesetz Nordrhein-Westfalen (1988). Lower Saxony incorporated the new population counts only in 1990, since full results were not yet available in 1989 (Stenographischer Bericht der 78. Sitzung, 1989). Our event study approach takes this into account. For simplicity, however, we describe the shock throughout this paper as if it took place in 1989 in all federal states.

[16] Municipal councils debate the fiscal budget (*Haushaltsplan*) towards the end of the preceding year, which then comes into effect on January 1. 1989 is therefore the first year in which the Census counts affect budgets.

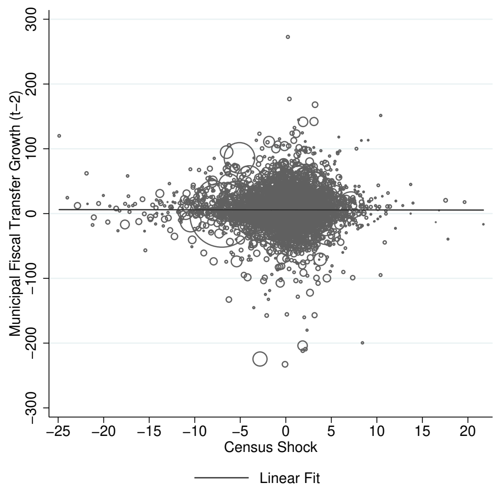
[17] Non-linearities exist due to an inherent asymmetry in how fiscal surplus and shortages are treated, and because larger municipalities receive a higher population weight.

Figure 3: Census Shock and Municipal Fiscal Transfer Growth

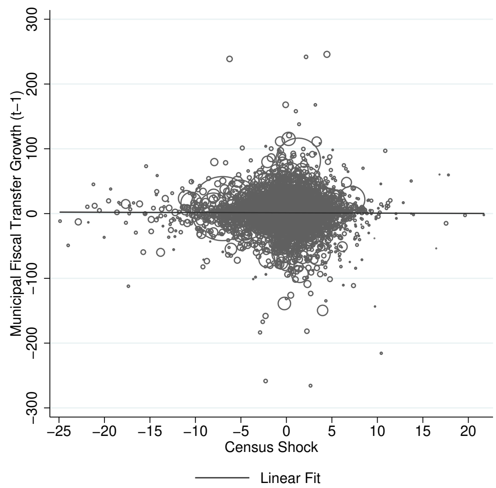
A. Year of Implementation of Census Counts (1989)



B. Two Years before Implementation (1987)



C. One Year before Implementation (1988)



Notes: The figure plots municipal fiscal transfer growth per capita against the Census Shock as defined in eq. (1). For readability the plot excludes observations with fiscal transfer growth below -300 EUR or above 300 EUR per capita, or Census Shocks below -25 or above 20 log points. OLS and locally-weighted polynomial (degree 1 with kernel half-width of 0.02) fits are weighted by population size in 1986.

capita. To test this hypothesis, we provide a non-parametric representation of the relationship between the Census Shock as defined in equation (1) and the yearly growth in fiscal transfers per capita in municipality m in 1989,

$$\Delta Trans_{m,1989} = \frac{Trans_{m,1989} - Trans_{m,1988}}{Pop_{m,1986}} \tag{2}$$

where we divide by the 1986 population count to avoid the contamination of population statistics by the Census revision itself. Panel A in Figure 3 shows a scatter plot, as well as a linear and a kernel smoothing (local polynomial) estimator. As expected, we observe a strong positive effect of the Census Shock on the growth in municipal fiscal transfers. The locally weighted polynomial reproduces the linear regression line well, providing support for a linear approximation.

4 Empirical Strategy

The key econometric challenge when studying the municipal response to fiscal transfers is that transfers depend on local economic and fiscal conditions. To address this endogeneity problem, we employ a two-stage estimation procedure, instrumenting municipal fiscal transfers with the Census Shock in official population counts triggered by the 1987 Census. After outlining our empirical strategy, we provide evidence supporting its validity in Section 4.3.

4.1 First Stage and Reduced Forms

To predict changes in municipal fiscal transfers due to changes in population counts triggered by the 1987 Census, we first estimate the regression

$$\Delta Trans_{m,1989} \quad = \quad \alpha_s + \sum_{s=1}^{5} \beta_s CensShock_m \mathbb{1}[FedState_m = s] + \epsilon_{m,1989}, \tag{3}$$

where $CensShock_m$ is the Census Shock variable, as defined in equation (1), $\mathbb{1}[FedState_m = s]$ are indicator variables indicating whether municipality m is in federal state s, and α_s are federal state fixed effects. $\Delta Trans_{m,1989}$ (as defined in equation (2)) represents the per capita change in municipal fiscal transfers in municipality m between 1988 and 1989 – the first year in which the Census had an effect on transfers (see Section 3.4). The functional form of equation (3) is motivated by the institutional setting, which stipulates an approximately linear relationship between changes in per capita fiscal transfers and the census shock (see Section 3.3). However, we allow the first stage to differ by state, as federal states vary in the degree to which the equalization scheme closes funding gaps.[18] In an extension, we further distinguish between positive and negative Census Shocks (Section 7.1).

4.2 Second Stage – Event Study

We then analyze the dynamic response of municipal budgets to fiscal transfers, estimating

$$\Delta Y_{m,t} = \alpha_{s,t} + \sum_{k=1984}^{1988} \beta_k \Delta Trans_{m,1989} \mathbb{1}[t=k] + \sum_{k=1989}^{T} \gamma_k \Delta Trans_{m,1989} \mathbb{1}[t=k] + \delta_t X_{m,t} + \epsilon_{m,t}, \quad (4)$$

where $\Delta Y_{m,t}$ represents the per capita change in the outcome variable (total revenues $\Delta Rev_{m,t}$, total expenditures $\Delta Exp_{m,t}$, or detailed subcategories) in municipality m and year t, $\alpha_{s,t}$ represents federal state x year fixed effects, $\Delta Trans_{m,1989}$ is defined as in equation (2), $X_{m,t}$ are potential control variables, and β_k and γ_k are our coefficients of interest. Equation (4) is estimated by two-stage least squares using the Census Shock as an instrument for changes in fiscal transfers in 1989, see equation (3).[19] We define the event period τ relative to the event year (i.e., $\tau = 0$ in 1989), and refer to event period τ and year t interchangeably.[20] In an extension, we interact the Census Shock with an indicator for its sign, to study if positive and negative shocks have different effects on municipal budgets.

We estimate cumulative versions of equation (4), in which the reference period for the outcome variable $\Delta Y_{m,t}$ is 1988, the year before the Census counts were first applied. Specifically,

$$\Delta Y_{m,t} = \frac{Y_{m,t} - Y_{m,1988}}{Pop_{m,1986}}, \quad (5)$$

where the denominator is population in 1986 to avoid contamination by the revision of Census counts itself. The coefficients β_k and γ_k consequently reflect cumulative growth relative to the base year 1988, with $\beta_{1988} = 0$ by construction. As explained in Section 3.4, the Census Shock is expected to *permanently* affect fiscal transfers from 1989 onwards. Our aim is to estimate the dynamic response to this permanent shock over a long time period. We thus pool data for five years before and ten years after the onset of this treatment, i.e., from 1984 to 2000 ($= T$). To account for heteroscedasticity due to variation in population size and location-specific shocks, we weight observations based on a modified Breusch-Pagan test.[21]

---

[18] However, our results are similar when we restrict this relationship to be constant ($\beta_s = \beta \; \forall s$).

[19] To reduce the sensitivity of the estimates to outliers, we winsorize all outcome variables at the 1st and 99th percentile, replacing observations below or above those percentiles with the values at the cutoff.

[20] We set the initial event period to 1990 for Lower Saxony, as Lower Saxony only incorporated the new population counts in that year. For simplicity, we ignore this deviation in the presentation.

[21] Specifically, we regress the square of municipal per-capita revenue growth in 1988 on inverse population size, and construct the weights as the inverse of the fitted value. Our results are similar if instead we top-code population weights (e.g., at the 75th or 90th percentile) to avoid an outsized influence of large cities.

In our baseline specification, we control for federal state x year fixed effects, but not for additional control variables $X_{m,t}$. However, we present robustness checks in Section 5.3, in which we flexibly control for pre-treatment levels and trends in local demographic and economic characteristics, or include finer fixed effects to exploit only within-commuting zone x year variation.[22] Following Serrato and Wingender (2016), we further present inverse-probability weighting (IPW) and regression-adjusted IPW (IPWRA) estimates.[23] In the next section we show that the Census Shock appears as good as random with respect to budgetary and economic trends.

4.3 Validity of the Instrument

Since the event-study design abstracts from time-constant differences between areas, our key identifying assumption is that the Census Shock does not correlate with other factors affecting the growth of fiscal outcomes. This assumption may fail to hold if deviations between the Census counts and registry-based projections reflect local economic trends that extend into our analysis period, or if they correlate with the effect of other important events (such as German reunification). The long time gap between the 1987 Census and the preceding Census of 1970 mitigates such concerns.[24] Moreover, the Census Shock is spatially dispersed (Section 3.1), and therefore cannot be explained by factors that apply to local labor markets as a whole. To provide more direct evidence, we study its pre-treatment relationship with fiscal, demographic, and economic variables. Here we exploit that the level of transfers is based on two-year lagged population counts, which breaks the simultaneity between treatment *assignment* (the Census in 1987) and *implementation* (from 1989 onwards). This allows us to test whether the Census Shock was unrelated not only to pre-Census conditions, but also to conditions during and immediately after the Census year.

Pre-Trends in Fiscal Budgets. Figure 3 provides non-parametric evidence on the relationship between the Census Shock and changes in municipal fiscal transfers. While there exists a strong relationship in the implementation year of 1989 (Panel a), no such relationship is present in either of the pre-treatment periods 1987 (Panel b) and 1988 (Panel c). The upper part of Figure 4 extends this analysis to other fiscal variables and tax multipliers. Panel A reports

[22] Moreover, the hypotheses $H_0: \beta_k = 0$ for $k < 1989$ represent valid falsification tests to support our identifying assumption that the Census Shock was unanticipated and not correlated with other sources of revenue and spending growth.

[23] The IPWRA estimator has the "double robust" property and yields consistent estimates of the treatment effects when either the regression adjustment model for the outcome or the propensity score model for the treatment are correctly specified.

[24] Because projection errors could accumulate over nearly two decades, they are unlikely to reflect concurrent economic trends around the Census year.

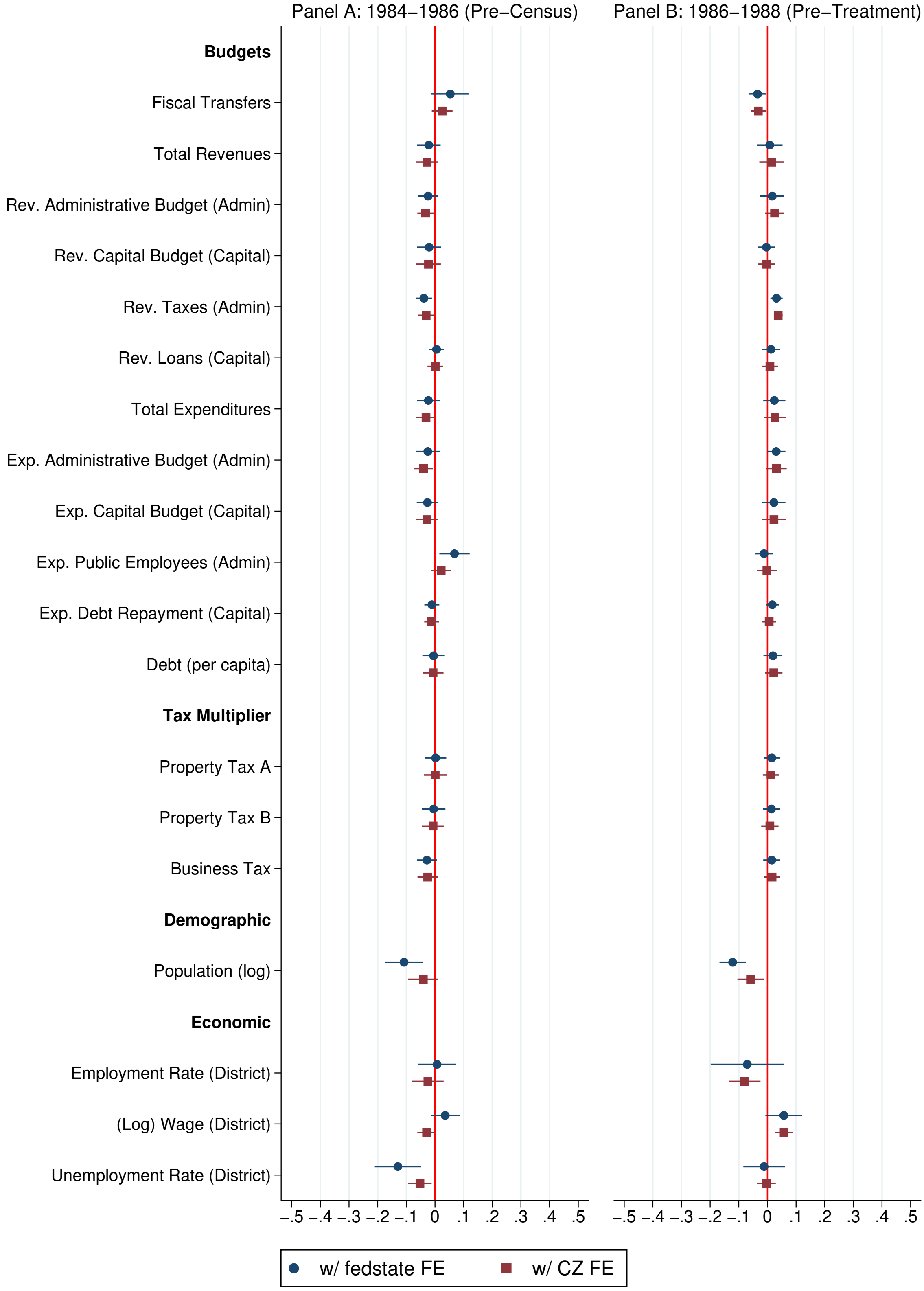
correlations between the Census Shock and the growth in the respective variable between 1984 and 1986 (before treatment assignment), while panel B reports the corresponding results for the growth between 1986 and 1988 (before treatment implementation). We report correlations net of federal state fixed effects (blue) and net of commuting zone fixed effects (red). They are all well below 0.1 and, despite the large number of municipalities, only 2 out of 60 correlations are statistically different from zero. This pattern extends to the local tax multipliers. Overall, the estimates suggest the absence of any meaningful correlations between the Census Shock and pre-trends in fiscal budgets and tax setting. They also imply the absence of anticipation effects: municipal governments either did not anticipate the impending revisions in population counts and transfers, or chose not to act on them. In Appendix Figure A.2, we show that the Census Shock does not correlate much with the *levels* of municipal budgets either.

Pre-Trends in Demographic and Economic Variables. The lower half of Figure 4 reports pre-treatment trends in economic and demographic variables, since these may have fiscal implications in later periods, even in the absence of pre-trends in fiscal outcomes. Municipalities with a larger Census Shock have somewhat lower pre-treatment population growth rates on average.[25] However, this correlation is small (around -0.1), remains similar in the years before and immediately following the Census, and is halved when partialling out commuting zone fixed effects. The relationship is also economically small: municipalities with a one percent larger Census Shock had, on average, 0.025 percent lower yearly population growth prior to the Census (against a standard deviation of 1.5 percent). Figure A.3 confirms that this relationship is negligible compared to the size of the Census Shock and further shows that there are no discernible pre-trends for municipalities that lost population in the Census. We next report the corresponding correlations for the local employment rate, wage, and unemployment rate (all measured at the district level). There is no significant pre-trend in the employment rate (normalized by 1986 population) or wages in the pre-Census period and only a small correlation (<0.06) with wage growth post-Census (but pre-treatment). Similarly, there is only a weak pre-trend in the unemployment rate in the pre-Census period that vanishes post-Census. Finally, we provide evidence on the correlation with the *levels* of demographic and economic variables in Appendix Figure A.2.[26] The Census Shock tends to be slightly more positive in rural areas and negative in student towns. These correlations are however small and our estimates hardly change when controlling for such pre-treatment differences.

[25] The results reported in Panel B reflect the correlation of population growth net of the Census Shock itself, since population growth between 1986 and 1988 would otherwise be mechanically related to the Census Shock.

[26] As the outcome in equation (4) is differenced, such correlations represent no direct threat to the internal validity of our research design. However, they might affect the interpretation and external validity of our results.

Figure 4: Census Shock and Pre-Treatment Trends



Notes: The Figure reports correlation coefficients between the Census Shock and pre-treatment growth rates in municipal fiscal, economic and demographic characteristics. We consider pre-Census growth between 1984 and 1986 in the left panel and post-Census (but pre-treatment) growth between 1986 and 1988 in the right panel. Correlations are net of federal state fixed effects (blue dots) or net of commuting zone fixed effects (red squares). Population growth in 1986-88 is net of the Census Shock. Unemployment rate growth in the left panel reflects only 1985-86 growth. The horizontal bars show 95% confidence interval based on standard errors clustered at the commuting zone level.

In sum, the Census Shock is not correlated with fiscal trends and is broadly uncorrelated with economic trends. These conditions motivate the event study design described in Section (4), which does not rely on a selection-on-observables assumption, or the inclusion of particular sets of control variables. To illustrate its robustness, however, we do perform various falsification and sensitivity tests and include different sets of fixed effects or demographic and economic control variables in the estimation, such as direct controls for population pre-trends (see Section 5.3).

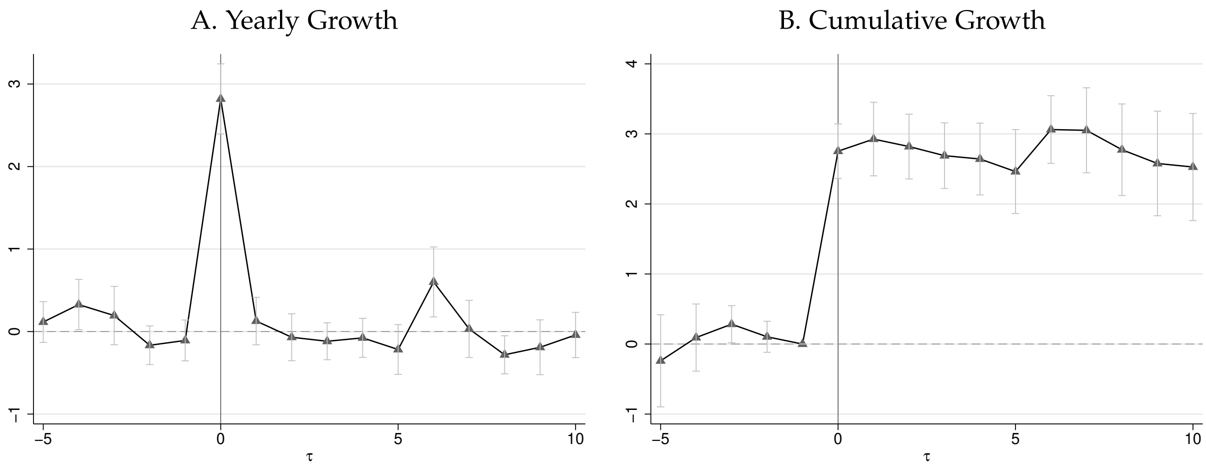
Other Events. Our analysis period coincides with German reunification in 1990 and the subsequent recession of 1993. East German federal states then joined the municipal fiscal equalization scheme in 1995, six years after the Census Shock first affected municipal budgets. In principle, these events could affect the internal validity of our identification strategy if their consequences were correlated with the Census Shock at the municipal level. Given the spatial dispersion of the Census Shock (Figure 2), this is however unlikely. In addition, we show in Section 5.3 that our results are insensitive to controlling for proxies of the local effects of reunification, such as distance to the inner-German border or East-West migration flows. Similarly, they are insensitive to recession controls, and hold conditional on commuting zone x year fixed effects. Nevertheless, the overall change in fiscal conditions during the recession may have affected municipal spending decisions, and thus also how municipalities allocate additional transfers they received because of the Census Shock during that period. For the interpretation of our results it is therefore important to understand the extent to which Germany's post-reunification recession has reduced municipal budgets. In Appendix Section C, we show that this impact was delayed and staggered, affecting budgets up to five years after the start of the recession.

Another event falling into our analysis period was the repeal of the business capital tax in 1998. We do not expect this reform to affect our results, as (i) revenues from the business capital tax only constituted a small share of overall business tax revenues (around 15%), (ii) the policy only took place towards the end of our observation period (in $\tau = 9$), and (iii) it should affect all municipalities irrespectively of the Census Shock.

5 Results: Fiscal Budgets

We begin by studying the effects of the Census Shock on municipal transfers (i.e., unconditional intergovernmental grants) in Section 5.1 (first stage). Section 5.2 then focuses on the dynamic response of municipal budgets to intergovernmental grants in detailed revenue and expenditure

Figure 5: The Census Effect on Municipal Fiscal Transfers



Notes: The Figure plots the estimated effects of the Census Shock on municipal fiscal transfer growth based on equation (3), controlling for federal state x year fixed effects. In Panel A, the dependent variable is the per capita change in municipal fiscal transfers from $\tau - 1$ to τ; the coefficients represent effects on yearly growth. In Panel B, the dependent variable is the per capita change in municipal fiscal transfers relative to 1988; the coefficients represent cumulative growth. The underlying regressions are heteroscedasticity-weighted. The horizontal bars represent 95% confidence intervals based on standard errors clustered at the commuting zone level.

categories (two-stage least squares), and in Section 5.3 we probe the robustness of our results.

5.1 First Stage

Figure 5A plots the estimated impact of the Census Shock on the *yearly* growth in municipal fiscal transfers. These estimates are based on a variant of equation (3) in which the dependent variable represents the yearly per capita growth rate of fiscal transfers (e.g., $\Delta Trans_{m,1989}$ as defined in equation (2)) and the slope coefficients are assumed to be constant across federal states. We find no relationship between the Census Shock and yearly fiscal transfer growth before 1989 (i.e, in event periods $\tau < 0$). The coefficients are precisely estimated and clustered around zero. We thus fail to reject our falsification tests represented by the null hypotheses $H_0 : \beta_\tau = 0, \forall \tau < 0$, including in the year of the Census itself (1987 or $\tau = -2$). This observation supports our identifying assumption that the Census Shock is not correlated with other sources of local transfer growth (see also Section 4.3). Further, the post-treatment coefficients are small and, with one exception, not statistically different from 0, illustrating that the Census Shock led to a one-time, *permanent* shift in transfers.

Figure 5B plots the impact of the Census Shock on the *cumulative* growth in municipal fiscal transfers relative to 1988, the last year before the implementation of the Census-adjusted population counts (see Section 4). A one percent increase in local population counts increased transfers per-capita by 2.75 EUR. Put differently, each additional person found in the Census led to an additional transfer of 275 EUR/year.[27] This effect remains approximately constant over the following years, confirming that the Census Shock triggered a *permanent* shift in fiscal transfers. Accordingly, a Census Shock in population counts of one standard deviation (4.1 percent) increases per-capita transfers by 12 EUR (4.1×2.75), or 7 percent compared to the average transfer (161 EUR, see Table 1). Severe cases include cities such as Munich (7 percent loss in population count, or 89,295 inhabitants) and Göttingen (10.4 percent loss, or 13,981 inhabitants), which respectively experienced a sudden loss of revenues of 26.2 and 4.1 million EUR per year. Transfers changed by at least 20 percent in more than 300 municipalities. The size of the revenue shock was therefore substantial. According to our estimates, the 1987 Census triggered a reallocation of about 447 million EUR of transfers per year within the municipal fiscal equalization scheme.[28] Since this shift was permanent, the total reallocation amounts to about 5 billion EUR over the ten-year period considered in our analysis.

5.2 Two-Stage Least Squares

What is the dynamic response of municipal budgets to unconditional intergovernmental grants, and which budget categories are most affected? To answer these questions, we make use of our detailed revenue and expenditure categories. We estimate equation (4) by two-stage least squares (2SLS), instrumenting the change in municipal fiscal transfers per capita with the Census Shock. As described in Section 4.2, we control for federal state x year fixed effects and estimate the effects relative to the last year before the Census counts were implemented.

Total Revenues and Expenditures. Because the various budget categories interact, it is instructive to discuss revenues (Table 2) and expenditures (Table 3) jointly. *Total revenues* and *total expenditures* increase immediately in response to an exogenous increase in municipal transfers (column (1) in Tables 2 and 3). However, unlike transfers, they continue to increase over the next few years. A 1 EUR increase in fiscal transfers per capita leads to an immediate increase in per-capita revenues of 1.4 EUR ($\tau = 0$), which then grows steadily to about 3 EUR in subsequent years. The pattern is similar for total expenditures: an increase of 1 EUR in fiscal transfers leads to a 1.1 EUR increase in per-capita expenditures in $\tau = 0$, growing to 3.3 EUR in subsequent years. This indicates that municipalities fully spend the additional revenues they receive.[29]

Why does the Census Shock have a considerably larger impact on total revenues than on

[27] The marginal effect of an additional inhabitant on transfers is therefore much larger than the average transfer per capita (161 EUR/year, see Table 1).

[28] Our estimates are in line with those from official sources at the time, which suggest that the corrections

Table 2: Dynamic Effects of Fiscal Transfers on Revenues (2SLS)

	Total Revenues	Total Revenues (Net)	Administrative Budget					Capital Budget					
			Total (Admin)	Fiscal Transfers	Taxes	Fees and Charges	Other Revenues	Total (Capital)	Fees and Contrib.	Investments	Loans	Other Revenues	Admin to Capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	=(3)+(8)	(3)+(8)-(12)	(4)+(5)+(6)+(7)					(9)+(10)+(11)+(12)					part of (12)
τ = -5	0.17	0.09	0.14	-0.01	0.05	0.10	-0.08	0.12	0.04	-0.24	-0.12	0.32	0.48**
	(0.44)	(0.35)	(0.14)	(0.10)	(0.17)	(0.06)	(0.08)	(0.38)	(0.10)	(0.19)	(0.18)	(0.25)	(0.20)
τ = -4	0.29	0.04	0.28**	0.08	0.09	0.10*	0.02	0.01	0.03	-0.11	-0.19	0.20	0.40*
	(0.42)	(0.36)	(0.14)	(0.08)	(0.15)	(0.06)	(0.06)	(0.36)	(0.10)	(0.18)	(0.17)	(0.24)	(0.22)
τ = -3	0.19	0.06	0.32***	0.11***	0.07	0.06	0.09*	-0.04	0.00	-0.19	-0.19	0.25	0.23
	(0.39)	(0.29)	(0.12)	(0.04)	(0.09)	(0.06)	(0.05)	(0.35)	(0.09)	(0.15)	(0.17)	(0.25)	(0.25)
τ = -2	-0.13	-0.33	0.10	0.04	0.03	0.02	0.06	-0.08	-0.12	-0.22*	-0.09	0.09	0.13
	(0.35)	(0.24)	(0.10)	(0.04)	(0.07)	(0.05)	(0.05)	(0.31)	(0.09)	(0.12)	(0.15)	(0.24)	(0.21)
τ = -1	.	.	.	.	.	.	.	.	.	.	.	.	.
τ = 0	1.35***	0.85***	1.13***	1.00***	0.08	0.04	-0.13**	0.33	0.02	-0.08	-0.06	0.44*	1.00***
	(0.32)	(0.23)	(0.11)	(0.00)	(0.07)	(0.04)	(0.06)	(0.32)	(0.08)	(0.14)	(0.12)	(0.23)	(0.15)
τ = 1	2.07***	1.10***	1.26***	1.07***	0.10	0.02	-0.07	0.86**	0.18*	-0.10	-0.15	0.77***	1.13***
	(0.41)	(0.30)	(0.16)	(0.05)	(0.08)	(0.05)	(0.09)	(0.35)	(0.11)	(0.18)	(0.15)	(0.27)	(0.22)
τ = 2	2.61***	1.41***	1.27***	1.04***	0.19**	-0.02	-0.05	1.46***	0.23*	0.00	0.07	1.26***	1.14***
	(0.41)	(0.35)	(0.19)	(0.05)	(0.08)	(0.05)	(0.09)	(0.42)	(0.12)	(0.20)	(0.17)	(0.30)	(0.20)
τ = 3	3.02***	1.77***	1.33***	1.00***	0.21	-0.06	-0.01	1.79***	0.40***	0.14	0.12	1.16***	1.00***
	(0.41)	(0.41)	(0.22)	(0.06)	(0.13)	(0.07)	(0.11)	(0.42)	(0.13)	(0.18)	(0.17)	(0.27)	(0.18)
τ = 4	3.06***	2.17***	1.29***	0.98***	0.24*	-0.07	-0.06	2.04***	0.64***	0.03	0.34*	0.96***	0.74***
	(0.45)	(0.45)	(0.27)	(0.06)	(0.14)	(0.08)	(0.14)	(0.41)	(0.15)	(0.19)	(0.20)	(0.25)	(0.27)
τ = 5	2.91***	1.95***	1.24***	0.93***	0.01	0.04	0.10	1.90***	0.52***	0.01	0.29	1.04***	0.62***
	(0.48)	(0.51)	(0.28)	(0.07)	(0.14)	(0.10)	(0.13)	(0.42)	(0.17)	(0.19)	(0.20)	(0.26)	(0.21)
τ = 6	1.92***	1.51***	0.95***	1.08***	-0.10	-0.03	-0.11	1.12**	0.45**	-0.05	0.27	0.40	0.43*
	(0.53)	(0.46)	(0.27)	(0.08)	(0.15)	(0.11)	(0.16)	(0.52)	(0.18)	(0.21)	(0.19)	(0.26)	(0.22)
τ = 7	1.72***	1.12*	1.11***	1.10***	-0.01	-0.01	-0.15	0.86*	0.51***	-0.24	-0.09	0.64**	0.50**
	(0.54)	(0.58)	(0.30)	(0.08)	(0.15)	(0.12)	(0.15)	(0.44)	(0.13)	(0.22)	(0.18)	(0.30)	(0.24)
τ = 8	2.78***	2.03***	1.21***	1.02***	0.14	-0.02	-0.20	1.69***	0.55***	0.03	0.40**	0.73***	0.37
	(0.56)	(0.50)	(0.29)	(0.09)	(0.14)	(0.12)	(0.16)	(0.46)	(0.17)	(0.24)	(0.19)	(0.26)	(0.26)
τ = 9	3.09***	1.96***	1.27***	0.95***	0.13	0.05	-0.05	1.75***	0.51***	-0.15	0.14	0.96***	0.74***
	(0.46)	(0.46)	(0.36)	(0.10)	(0.18)	(0.14)	(0.16)	(0.50)	(0.14)	(0.23)	(0.19)	(0.33)	(0.22)
τ = 10	2.97***	2.10***	1.54***	0.93***	0.20	0.12	0.02	1.46***	0.44***	0.01	0.24	0.69**	1.15***
	(0.58)	(0.55)	(0.40)	(0.10)	(0.22)	(0.14)	(0.15)	(0.43)	(0.15)	(0.23)	(0.22)	(0.28)	(0.25)
Cumulative	24.54	15.87	12.06	10.16	1.00	-0.06	-0.71	13.79	4.00	-0.40	1.32	8.34	7.67
States	all	no HE	all	all	all	no HE	no HE	all	no HE	no HE	all	no HE	BW&NRW
N	69,968	63,152	69,968	69,968	69,968	63,152	63,152	69,968	63,152	63,152	69,968	63,152	23,920

Notes: The table reports 2SLS estimates of the effects of fiscal transfers on municipal revenues based on equation (4), instrumenting the change in municipal fiscal transfers per capita with the Census Shock. The dependent variable is the per capita change in the variable reported in the top row relative to 1988. All columns control for federal state x year fixed effects and observations are heteroscedasticity-weighted based on a modified Breusch-Pagan test. "Cumulative" stands for cumulative effects over 10 years. Standard errors are clustered at the commuting zone level. Significance levels: 1% ***, 5% **, 10% *.

Table 3: Dynamic Effects of Fiscal Transfers on Expenditures(2SLS)

	Total Expenditures	Total Expenditures (Net)	Administrative Budget				Capital Budget				
			Total (Admin)	Public Employees	Material	Other Expenditures	Total (Capital)	Debt Repayment	Acquisition of Assets	Other Expenditures	Infrastructure Investments
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	=(3)+(7)	(3)+(7)-(6)	(4)+(5)+(6)				(8)+(9)+(10)				part of (10)
τ = -5	0.02	0.29	0.07	-0.07	0.12	0.18	0.09	-0.09	0.02	0.34	0.37
	(0.45)	(0.46)	(0.18)	(0.05)	(0.08)	(0.15)	(0.38)	(0.09)	(0.22)	(0.29)	(0.32)
τ = -4	0.11	0.20	0.22	0.03	0.10*	0.14	-0.01	-0.04	-0.14	0.24	0.25
	(0.45)	(0.44)	(0.15)	(0.03)	(0.06)	(0.16)	(0.39)	(0.09)	(0.22)	(0.28)	(0.31)
τ = -3	-0.01	0.10	0.06	0.02	0.08	0.10	-0.09	-0.09	-0.01	0.02	0.03
	(0.43)	(0.44)	(0.14)	(0.02)	(0.07)	(0.15)	(0.37)	(0.08)	(0.18)	(0.30)	(0.31)
τ = -2	0.24	0.17	0.27**	0.00	0.04	0.18	-0.02	0.05	0.06	-0.10	-0.14
	(0.36)	(0.34)	(0.13)	(0.02)	(0.04)	(0.11)	(0.31)	(0.07)	(0.16)	(0.21)	(0.19)
τ = -1	.	.	.	.	.	.	.	.	.	.	.
τ = 0	1.14***	0.71*	0.83***	-0.01	0.05	0.59***	0.40	-0.01	0.22	0.41	0.20
	(0.36)	(0.37)	(0.13)	(0.02)	(0.04)	(0.12)	(0.32)	(0.07)	(0.19)	(0.25)	(0.24)
τ = 1	1.63***	0.79*	1.18***	0.01	0.12**	0.91***	0.38	-0.08	0.33	0.46	0.49
	(0.42)	(0.42)	(0.17)	(0.03)	(0.06)	(0.13)	(0.37)	(0.09)	(0.23)	(0.31)	(0.34)
τ = 2	2.70***	1.78***	1.25***	0.00	0.21***	0.91***	1.43***	0.07	0.68***	0.95***	0.89**
	(0.45)	(0.45)	(0.20)	(0.04)	(0.07)	(0.16)	(0.40)	(0.07)	(0.25)	(0.32)	(0.39)
τ = 3	2.66***	1.66***	1.26***	0.01	0.20*	1.05***	1.22***	0.14	0.58**	0.90***	0.76**
	(0.47)	(0.46)	(0.22)	(0.06)	(0.10)	(0.15)	(0.41)	(0.09)	(0.25)	(0.30)	(0.34)
τ = 4	3.36***	2.44***	1.36***	-0.02	0.17*	1.00***	2.02***	0.15*	1.18***	0.94***	0.98***
	(0.49)	(0.47)	(0.26)	(0.08)	(0.10)	(0.18)	(0.44)	(0.09)	(0.31)	(0.33)	(0.34)
τ.= 5	3.16***	2.12***	1.44***	0.03	0.25*	1.04***	1.60***	0.10	1.11***	0.75**	0.66
	(0.55)	(0.51)	(0.23)	(0.07)	(0.13)	(0.17)	(0.46)	(0.10)	(0.38)	(0.35)	(0.40)
τ = 6	2.36***	1.19**	1.42***	0.01	0.22	1.11***	0.83*	0.04	0.74**	0.22	0.35
	(0.57)	(0.57)	(0.24)	(0.08)	(0.14)	(0.18)	(0.49)	(0.09)	(0.35)	(0.36)	(0.39)
τ = 7	1.88***	1.12**	1.18***	0.02	0.26*	0.81***	0.75*	0.17*	0.74**	0.39	0.28
	(0.54)	(0.53)	(0.25)	(0.07)	(0.14)	(0.18)	(0.44)	(0.10)	(0.31)	(0.37)	(0.36)
τ = 8	2.76***	1.78***	1.21***	0.03	0.22	0.92***	1.41***	0.13	0.88***	0.49	0.66*
	(0.55)	(0.52)	(0.27)	(0.08)	(0.16)	(0.18)	(0.46)	(0.09)	(0.30)	(0.37)	(0.34)
τ = 9	3.26***	2.00***	1.42***	0.01	0.27	1.06***	1.58***	0.10	0.81***	0.77**	0.44
	(0.52)	(0.50)	(0.29)	(0.09)	(0.17)	(0.22)	(0.45)	(0.09)	(0.25)	(0.34)	(0.34)
τ = 10	3.25***	2.06***	1.78***	0.06	0.30*	1.35***	1.52***	0.35***	0.72***	0.54	0.45
	(0.60)	(0.55)	(0.34)	(0.09)	(0.18)	(0.24)	(0.46)	(0.09)	(0.27)	(0.40)	(0.41)
Cumulative	24.89	15.59	12.55	0.09	1.98	9.40	11.63	0.82	7.28	6.30	5.71
States	all	no HE	all	all	no HE	no HE	all	all	no HE	no HE	no BY
N	69,968	63,152	69,968	69,968	63,152	63,152	69,968	69,968	63,152	63,152	37,344

Notes: The table reports 2SLS estimates of the effects of fiscal transfers on municipal revenues based on equation (4), instrumenting the change in municipal fiscal transfers per capita with the Census Shock. The dependent variable is the per capita change in the variable reported in the top row relative to 1988. All columns control for federal state x year fixed effects and observations are heteroscedasticity-weighted based on a modified Breusch-Pagan test. "Cumulative" stands for cumulative effects over 10 years. Standard errors are clustered at the commuting zone level. Significance levels: 1% ***, 5% **, 10% *.

fiscal transfers? As we show below, intergovernmental transfers trigger *multiplier effects* by increasing investments that, in turn, raise revenues. However, another reason is that under the system of fiscal accounting during our analysis period (called *Kameralistik*), revenues moved from the administrative to the capital budget are counted twice: as expenditures in the administrative budget (in *other expenditures (admin)*, Table 3, column (6)), and as revenues in the capital budget (in *other revenues (capital)*, Table 2, column (12)). Indeed, in the year in which the Census counts are implemented, *other expenditures (admin)* increase by 0.59 EUR, while *other revenues (capital)* increase by 0.44 EUR for each additional euro of transfers. Both categories increase further in the following years, suggesting a close to one-to-one shift of revenues generated by the Census Shock from the administrative to the capital budget.[30]

To compute the response of total revenues and expenditures net of these accounting effects, we create measures of total revenues and expenditures net of *other revenues (capital)* and *other expenditures (admin)*, respectively (see column (2) in Table 2 and Table 3, *Net Total*). These estimates can be interpreted as multiplier effects of fiscal transfers on total revenues. In the first year, an exogenous 1 EUR rise in transfers increases net revenues less than proportionally by 0.85 EUR, and expenditures by only 0.71 EUR. The effects then increase steadily in the following years, suggesting a long-run multiplier of about 2. Our estimates therefore suggest that unconditional grants have a more than proportional effect on total revenues and expenditures. We examine the fiscal responses that contribute to these large multiplier effects below.

Detailed Revenue and Expenditure Categories. In the long run, fiscal transfers affect total revenues and expenditures more than proportionally. But which budget categories adjust, and how fast? We next consider the dynamic response in detailed budget categories.

Municipalities do not use the increased transfers to restructure their finances; that is, they do not increase *debt repayment* by much (Table 3, column (8)), nor do they reduce their take-up of *loans* (Table 2, column (11)). Instead , the take-up of loans tends to increase in the medium term. On average, they neither use these funds to increase *public employment*, since salary expenditures do not respond (Table 3, column (4)). Instead, municipalities invest: they

amounted to 350 million EUR in cities of 100,000 inhabitants or more (Source: Destatis).

[29] The temporary drop of coefficient estimates six years after incorporation of the Census counts can be explained by the fact that Germany experienced a recession in 1993, which depressed municipal revenues and expenditures in the subsequent years (see Appendix Section C for a more detailed discussion).

[30] For two federal states (BW and NRW) we can directly observe transfers between the two budget categories, confirming this interpretation. As shown in column (13) of Table 2, *transfers from administrative to capital budget* increase by around 1 EUR per capita in 1989 for each additional euro in fiscal transfers in these states, largely remaining at that level over time. Furthermore, *transfers from the administrative to the capital budget* are the largest single component of *other revenues (capital)* in the two states, comprising a share of 64 percent.

increase the *acquisition of assets* (Table 3, column (9)), which can be both capital investments and investments into tangible assets such as properties, land, or buildings, but also *other expenditures (capital)* (Table 3, column (10)), of which investments in infrastructure are the most important sub-category.[31] Consistent with Cassidy (2019) we find a hump-shaped response, with investments peaking around five years after the initial shock and remaining at a permanently higher level thereafter.

The response in revenues is consistent with these expenditure patterns. The revenue category that reacts most strongly is *fees and contributions* in the capital budget, which increase slowly but steadily, to around 0.4 EUR for each additional euro of transfers four years after the exogenous shift (Table 2, column (9)). This response is likely a direct consequence of the increase in infrastructure investments. In Germany, municipalities must pass a substantial part of the costs of building new roads onto those who benefit from them, that is their own inhabitants.[32] Also the timing of effects is consistent with this interpretation, since both *other expenditures (capital)* and revenues from *fees and contributions* increase steadily over the first four years following the Census Shock.

Finally, we find indicative evidence that revenues from *taxes (admin)* increase temporarily before dropping in later years. However, the coefficient estimates are rather small and imprecisely estimated.[33] The response in tax revenues also reflects the endogenous response of local tax rates to transfers. In Germany, municipalities set tax multipliers for two types of property taxes and the business tax. In Section 6, we study if and how municipalities' tax schedules react to exogenous increases in fiscal transfers, and how this rationalizes the response in tax revenues.

In sum, municipal budgets take time to adjust. In our setting, the adjustment process takes at least four years, with different budget categories responding at different speeds. It is therefore important to track this process over several years. Approaches that only consider the immediate response may instead miss the bulk of the response and hence come to qualitatively different conclusions about the functioning of public finances (see Section 8).

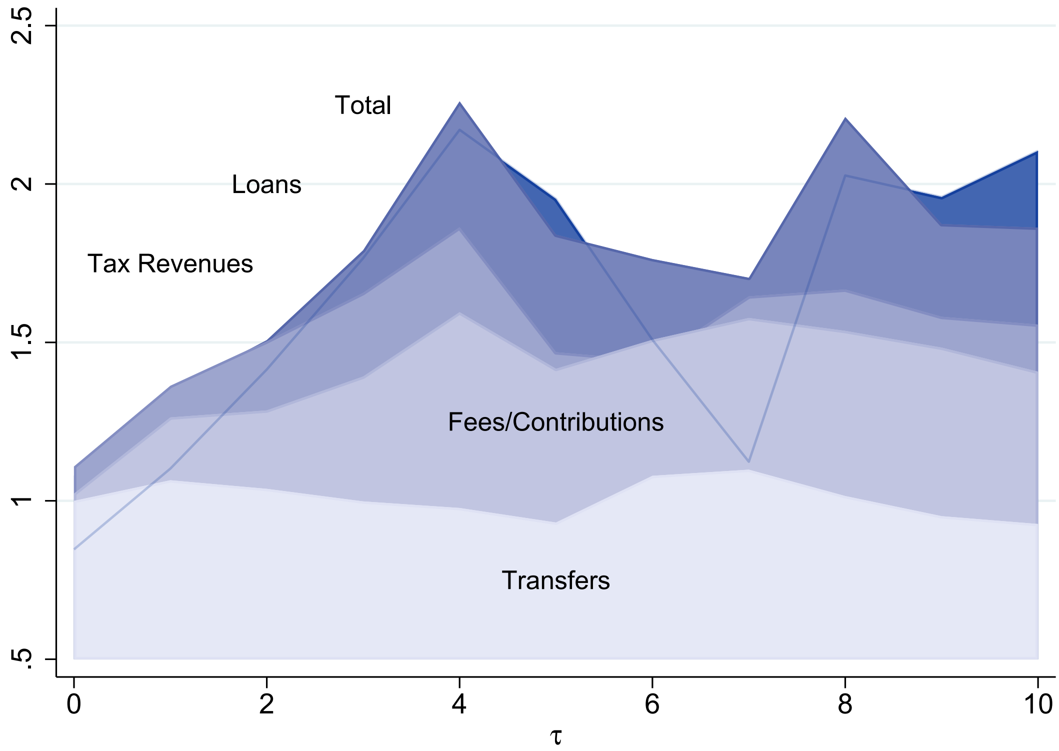
The Multiplier Effect of Intergovernmental Grants on Fiscal Budgets. One implication of our findings is that intergovernmental grants can have important multiplier effects on fiscal

[31] We were able to obtain direct information on infrastructure investments for all federal states in our sample except for BY. Using this data, we confirm that a large share (50 to 80%) of the response in *other expenditures (capital)* is driven by *infrastructure investments* (Table 3, column (11)).

[32] These contributions are included in the capital budget category *fees and contributions*. *Fees and charges* in the administrative budget instead include revenues from charges for garbage collection or the wastewater charge.

[33] These coefficients are estimated more precisely in robustness checks controlling for commuting zone x year FE or the average Census Shock in the other municipalities in the same district (see Figure A.5 and A.6).

Figure 6: Multiplier Effects of Transfers on Revenues



Notes: Decomposition of the multiplier effects of fiscal transfers. The underlying regressions are equivalent to the ones in Table 2, however observations from Hessia (HE) are excluded in all categories for better comparability. Sub-category effects of the respective revenue categories shown in the graph are stacked on top of each other. The shaded blue line shows total revenues (net of double accounting). The temporary drop in the multipliers after $\tau = 4$ coincides with a general, recession-related drop in municipal revenues and expenditures (see Appendix C).

budgets. We estimate a long-run multiplier of transfers on total revenues of about 2 (see Table 2, column (2)).[34] Figure 6 plots the dynamic composition of this multiplier based on the estimated effects discussed above. Around 50% of the long-run multiplier can be attributed to the mechanical increase in fees and contributions (which, in turn, is a response to infrastructure investments). While less precisely estimated, the remainder largely stems from increases in tax revenues and the take-up of loans. As the approval of loans for infrastructure investments depends on the general state of a municipality's finances, this indicates that increased transfers may allow municipalities to take on additional debt to finance (potentially lumpy) infrastructure investments.

The notion that fiscal adjustment takes time, and the existence of dynamic interrelationships between different budget components, is consistent with the existing evidence on municipal fiscal adjustments based on vector error correction models (VECM). However, this literature reports a negative response (crowding out) of own revenues to grants (Martin-Rodriguez and

[34] This large multiplier is not a mechanical effect of economic growth over time. Accounting for the mean revenue growth across municipalities (see Appendix Figure C.1), the estimated multiplier is still greater than 1.9.

Ogawa, 2017), suggesting that transfers from higher-level government are not very effective in stabilizing local expenditures.[35] Instead, we find a positive effect on own revenues and a large multiplier effect on total revenues and expenditures. Two factors might contribute to these conflicting findings. First, our estimates capture the response to a permanent shift in transfers, while VECM estimates also reflect the response to transitory fluctuations. Second, estimates based on VECM do not distinguish the source of transfer innovations and may be driven by factors depressing own revenues, such as responses to adverse economic shocks (Martin-Rodriguez and Ogawa, 2017).

5.3 Robustness

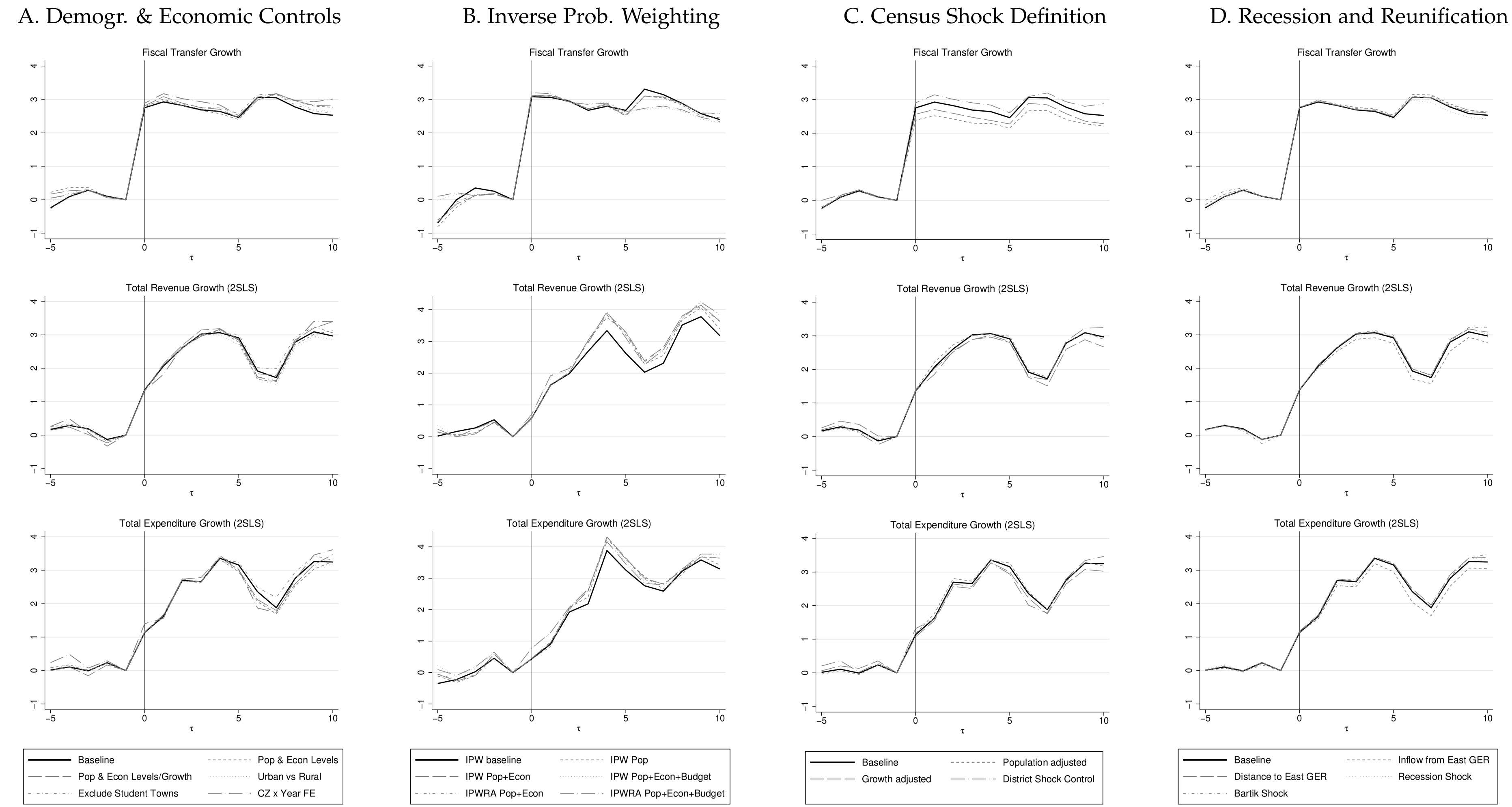
Demographic and Economic Controls. In Section 4.3, we showed that the Census Shock does not correlate with fiscal pre-trends, and that its correlations with demographic and economic pre-trends are small. In this section, we report five robustness checks to verify that these correlations are indeed negligible for our purposes. First, we control for pre-treatment levels in (log) population and economic variables (employment rate, unemployment rate and log wage). Second, we control for both pre-treatment levels and (three-year) growth rates in these variables. Third, we add control variables that classify a municipality as urban or rural. In each case, control variables are interacted with year fixed effects to flexibly allow for a differential impact of these variables over time. Fourth, we exclude student towns from the regression. Lastly, we control for commuting zone x year fixed effects in order to account for potential differential trends at the local labor market level.[36]

Panel A of Figure 7 presents results for our three main outcomes *fiscal transfers*, *total revenues*, and *total expenditures*, over the entire analysis period (from $\tau = -4$ to $\tau = 10$). For comparison, the figure also includes our baseline estimates, which only control for federal state x year fixed effects (thick black line). All estimates are robust to the inclusion of additional control variables or finer fixed effects. None of the specifications presents a pre-trend, and the post-treatment estimates are very similar to, and not statistically different from, the baseline results. In Appendix Figure A.5, we present the effects for all other outcome variables. For the sake of brevity, we only present the effect in period $\tau = 10$. Again, the estimated coefficients are largely similar to the baseline estimates (dashed grey line). The estimates tend to be more precise when controlling for commuting zone x year fixed effects, thus confirming the small positive effects on tax revenues from the main specification.

To further mitigate concerns about selection-on-observables, we complement our baseline

[35] In line with this evidence, the raw correlation between the change in own revenues and the change in fiscal

Figure 7: Robustness



Notes: The figure reports estimates of the effects of the Census Shock on municipal fiscal transfers, revenues and expenditures, and illustrates their robustness to the inclusion of demographic and other controls in Panel A, to using IPW and IPWRA estimators in Panel B, to alternative definitions of the Census Shock in Panel C and to recession and reunification controls in Panel D. The top panels show reduced-form estimates based on variants of equation (3) while the other panels show two-stage least square estimates based on variants of equation (4). Control variables are interacted with event year. In panel B, treatment is binary (above vs below median) and coefficients represent the 2SLS effects implied from the respective first-stage and reduced-form IPW and IPWRA regressions. To simplify comparisons, the first-stage coefficients are normalized by the average difference in the Census Shock between the two groups. All regressions are heteroscedasticity-weighted.

approach by using inverse-probability weighting (IPW) and regression-adjusted IPW (IPWRA) treatment effects estimators, which necessitates replacing the continuous Census Shock variable with a binary treatment indicator (above or below median).[37] We first reestimate the baseline specification using this approach, controlling only for federal state fixed effects (by event year). We then consecutively add pre-treatment trends in population, economic outcomes (employment rate, unemployment rate and log wage) and fiscal outcomes. The corresponding results are presented in Panel B of Figure 7. Our results are robust to this alternative choice of estimator.

Definition of the Census Shock. We provide three additional robustness checks with respect to the definition of the Census Shock. First, as one may be worried that our Census Shock measure picks up population growth in the first five months of 1987, we create an alternative measure that accounts for this growth by imputing the January to May 1987 population growth. Second, we construct an alternative measure that accounts for non-linearities in the relationship between the Census Shock and fiscal transfers, since the assumed linear relationship in our baseline model represents a simplification of the true functional relationship (see Appendix D). Third, to account for fiscal transfers at the next-highest level of government (the district level) and spillovers across municipalities within a district, we estimate a specification controlling for the average Census Shock in the other municipalities in the district.[38] The results are very robust with respect to these alternative definitions (see Figure 7C and Appendix Figure A.6), which are further described in Appendix B.1.

Reunification and Post-Reunification Recession. Our analysis period coincides with German reunification in 1990 and the subsequent recession of 1993. To rule out that these events affect the validity of our estimates (see Section 4.3), we provide four additional checks. First, to account for the sizable migration flows from East Germany in the wake of German reunification, we control for (municipality-level) distance to the inner-German border or for the actual (district-level) migration flows from East Germany between 1991 and 1994. Second, to account for potential correlations with the 1993 recession shock, we control for the 1991 to 1993 change in local unemployment at the district level or for Bartik shocks as a proxy for

transfers is negative in our data.

[36] This specification accounts for any shocks that affect some local labor markets more than others, such as population movements or spatial changes in economic activity.

[37] We estimate first stage and reduced-form regressions of the respective outcome variable on the treatment indicator, separately for each event period. We then use these estimates to infer the implied effects of an increase in fiscal transfers on the respective outcome.

[38] In principle, transfers at the district level should not directly affect municipal budgets, since the districts have their own budgets.

industry-related local demand shocks. As shown in Figure 7D, our results are robust to these additional control variables, reflecting that neither local consequences from reunification, nor local recession shocks are correlated with the Census Shock.

6 Results: Tax Setting

Most taxes in Germany are set at the federal level. Municipalities, however, levy three types of local taxes: *property tax A*, levied on property used for agricultural purposes; *property tax B*, levied on developed and developable land; and a *business tax*. Tax decisions are made by the municipal councils using simple majority rule. In particular, municipalities set local tax multipliers, which multiplied with the basic federal rate determine the total tax rate.[39] These taxes represent an important source of revenue, most notably the business tax, which on average contributes 41 percent to overall municipal tax revenue. Property taxes contribute around 12 percent, with the remainder resulting from the municipalities' share of the federal income tax.

6.1 Tax Response

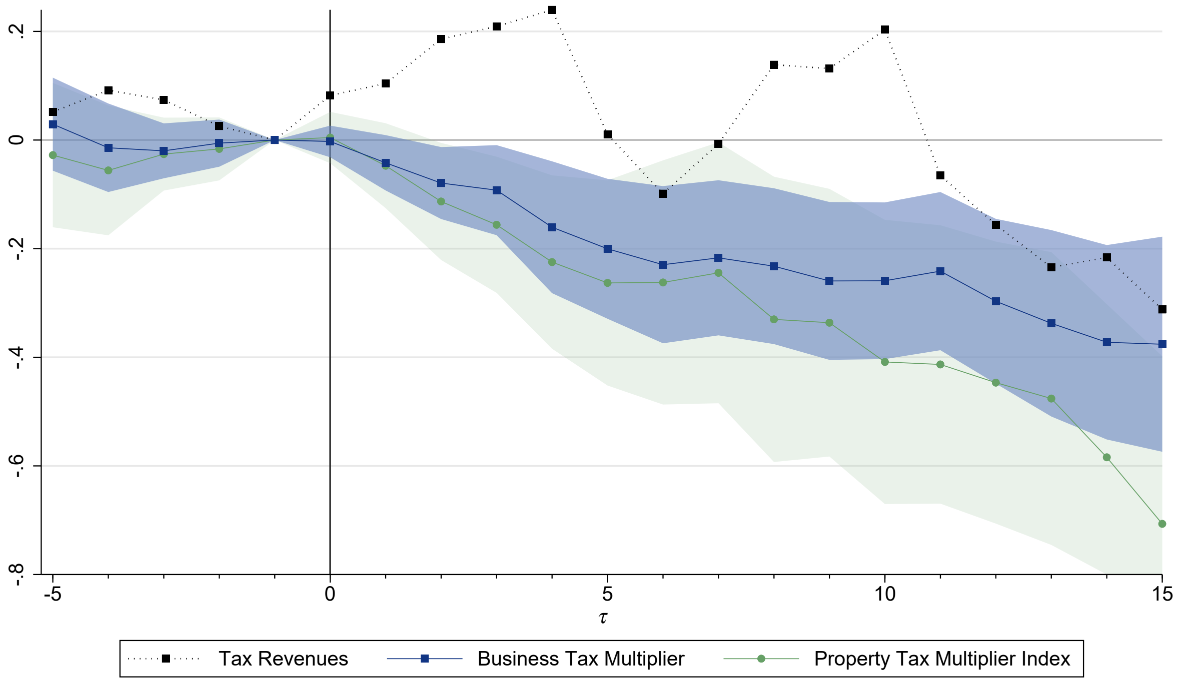
To study the tax response, we estimate the reduced-form equivalent of equation (4), regressing changes in the local tax multiplier (relative to $\tau = -1$) on the Census Shock.[40] The results are reported in Figure 8A and Appendix Table A.2, column (1). To simplify the exposition, we report an index of the two property tax multipliers weighted by their respective share on overall tax revenues. We find that municipalities do adjust all tax types. A Census Shock in population counts of one standard deviation (4.1 percent) reduces the *property tax index* multiplier by 1.7 percentage points ten years after the Census Shock and the *business tax* multiplier by 1.1 percentage points. Tax multipliers decline further in the following years. These responses are considerably stronger in larger municipalities and in municipalities with less favorable economic or local conditions (see Section 7.2).

However, this adjustment does not take place immediately. Instead, municipalities only gradually adjust their tax multipliers. Consequently, a study that only estimates the short-term response would incorrectly conclude that local tax rates do not react to transfers. Studies that assess the dynamics over three or four years would still miss the bulk of the response. The

[39] To our knowledge, municipal tax decisions have not been restricted by caps during our study period.

[40] We focus here on the direct tax response. In the presence of tax competition, the Census Shock may additionally affect local tax rates through spillovers induced by neighboring municipalities' Census Shocks. We analyze such spillovers in a companion paper. As the own and neighboring municipalities' Census Shocks are largely uncorrelated, our results do not pick up such spillovers (see Table A.1 and Figure 7).

Figure 8: Tax Multipliers and Tax Revenue



Notes: The figure plots the estimated effects of the Census Shock on tax revenues (dotted line, 2SLS estimates) and local tax multipliers (solid lines, reduced-form estimates) based on variants of equation (4). Regressions are heteroscedasticity-weighted and the areas represent 95% confidence intervals. For readability the confidence interval for the tax revenue coefficients are omitted.

prolonged response in tax rates has also implications for the evolution of tax revenues. Figure 8A shows that after initial gains, tax revenues exhibit a decline that mirrors the response in tax multipliers. This non-monotonic response reflects the staggered and opposing effects of spending and tax decisions over time, and is more pronounced for those municipalities that respond more strongly at the tax margin (see below and Section 7.2).

We provide a back-of-the-envelope calculation for the share of transfers that is returned to the local inhabitants via tax reductions.[41] A one-percent Census Shock shifts the per-capita tax revenues of local tax type j (property tax A, property tax B, business tax) by

$$\frac{\text{Tax Revenue per Capita}_{j,1988}}{\text{Tax Multiplier}_{j,1988}} * \hat{\beta}^j_\tau,$$

where $\hat{\beta}^j_\tau$ is the estimated effect of the Census Shock on the respective tax multiplier in period τ (see Table A.2). Because we cannot decompose revenues from the three tax types for each municipality, we use the average national per-capita revenue for tax type j (and thus the

[41] This calculation reflects an upper bound, as local tax instruments do not reflect direct transfers. Fuest et al. (2018) demonstrate however, that in Germany, workers bear about one half of the local business tax burden and Löffler and Siegloch (2018) show that property taxes are fully passed through to renters in the medium term.

average national tax multiplier).[42] These calculations imply that municipal governments return around 11 percent of their transfer gains after 10 years (0.3 EUR compared to 2.75 EUR per capita, see Section 5.1) and 16 percent (0.45 EUR vs. 2.75 EUR) after 15 years, with a steady upward trend. Yet, these results also suggest that even 15 years after the initial shock there still exist important frictions.

6.2 Mechanisms

Why do municipalities take such a long time to adjust tax rates, and why does the response remain weak even in the long run? One might surmise that municipalities fail to optimize against changes in their fiscal conditions if those changes are not sufficiently important (*rational inattention*). However, the speed of the response is not very correlated with the size of the fiscal shock.[43] We therefore consider two other mechanisms stressed in prior work on municipal tax setting.

One mechanism is *political* competition. Foremny and Riedel (2014) demonstrate that tax rates follow electoral cycles, with the business tax multiplier being more likely to increase following municipal elections than just before. Indeed, we observe such *political budget cycles* also in our sample, and consistent with this general pattern find that the Census Shock has little impact on tax multipliers when elections are imminent. However, while municipal elections take place every four to six years, the tax response continues to amplify in later years. Political cycles could therefore explain an initial delay, but not why the tax response is so long-lasting.

Not only the timing of elections, but also the degree of electoral competition may matter. To probe this explanation, we split our sample into two groups based on the incumbent government's vote share pre-treatment. As shown in Figure 9A, the tax response is substantially stronger among incumbent administrations with "weak" majorities (vote share below the 75th percentile) than among administrations supported by "strong" majorities.[44] These findings point to insufficient political competition as one important reason for weak tax responses.

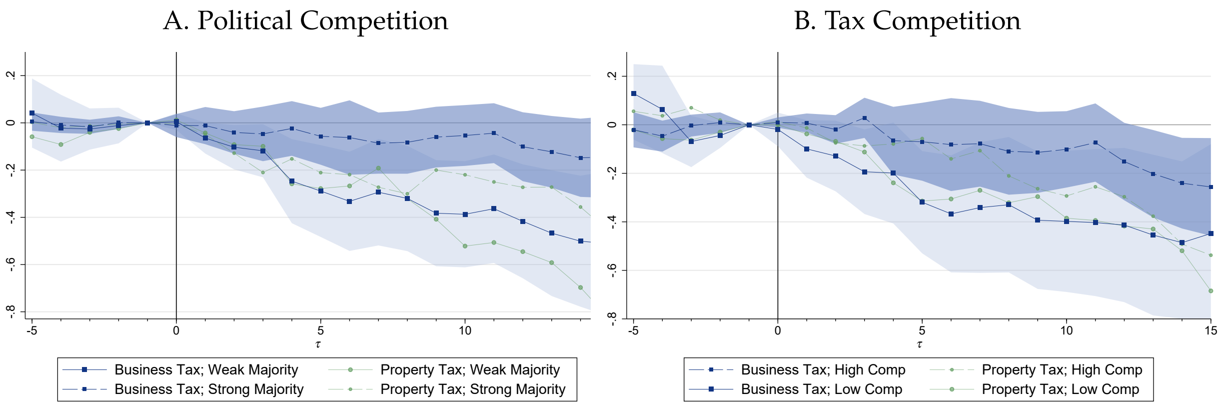
A second important mechanism is *tax competition*. Tax changes affect not only the allocation of resources between private and public consumption, but also the tax base – if firms or capital

[42] In 1988, property tax A revenue is 4 EUR per capita, property tax B revenue 65 EUR per capita, and business tax revenue 290 EUR per capita. The average local tax multipliers are 262, 303, and 362 percentage points, respectively. While the average size of tax changes in our analysis period is around 15 pp., changes of just a few percentage points are common. The occurrence of such minor adjustments suggests that municipalities do not incur large fixed costs when changing their tax multipliers.

[43] Splitting our sample into municipalities that experienced a modest or a more major revision of their official population count (less or more than 5%), we find if anything a quicker response for the former (Appendix Table A.2, column (3)).

[44] The difference of the business tax multiplier coefficient estimates at event period $\tau = 5$ and $\tau = 10$ are statistically significant at the 5% level.

Figure 9: Tax Setting Mechanisms



Notes: The figure plots reduced form estimates of the Census Shock on local tax multipliers based on variants of equation (4). Regressions are heteroscedasticity-weighted and the areas represent 95% confidence intervals. For readability the confidence intervals for the property tax coefficients are omitted.

owners are mobile and thus avoid high-tax jurisdictions. Inter-jurisdictional competition might therefore restrict a municipality's capacity to change tax rates in response to fiscal shocks. To illustrate this implication, consider a standard model of tax competition in which the government in municipality i maximizes the quasi-linear utility of a representative household,

$$u_i = c_i + \alpha_i v\left(z_i\right),\ v' > 0,\ v'' < 0,$$

where c_i is private and z_i public consumption. Municipal revenues depend on the tax rate t_i, the tax base k_i and fiscal grants g_i, such that $z_i = t_i k_i + g_i$. As shown by Buettner (2006), the optimal tax rate in such model is determined by a familiar first order condition that equates the marginal benefit from public consumption with the marginal cost of raising public funds,

$$\alpha_i v'\left(t_i k_i + g_i\right) = \frac{1}{1 + \frac{\partial k_i / k_i}{\partial t_i} t_i}, \tag{6}$$

where $\frac{\partial k_i / k_i}{\partial t_i}$ represents the semi-elasticity of the tax base with respect to the tax rate. The marginal costs thus consist of direct costs from shifting private to public consumption, and indirect costs related to the negative effect of taxes on the tax base ($\frac{\partial k_i / k_i}{\partial t_i} \leq 0$).[45]

How do fiscal transfers affect tax setting? Given the concavity of v, fiscal transfers g_i decrease the marginal benefit from public consumption $v'(z_i)$. To restore optimality, the tax rate t_i must decrease ($\frac{\partial t_i}{\partial g} < 0$). The intuition is that the representative household faces a trade-off between private and public consumption, so shocks in municipal revenues increase private

[45] As noted in Section 3.3, a municipality's tax base (*fiscal capacity*) in turn affects transfers within Germany's equalization scheme. The negative effect of tax hikes on the tax base is therefore partially offset by an increase in fiscal transfers (Buettner 2006, Egger et al. 2010).

consumption via a reduction in taxes (see Section 8). However, the optimal reduction in t_i is greater when tax hikes trigger only small losses in the tax base ($\frac{\partial k_i/k_i}{\partial t_i} \to 0$) as compared to when competition for the tax base is strong ($\frac{\partial k_i/k_i}{\partial t_i} \ll 0$). When tax competition becomes more severe, a municipality becomes more concerned about retaining its tax base (indirect costs of raising public funds) and less concerned about the trade-off between private and public consumption (direct costs).

The tax response to fiscal grants should therefore be weaker if tax competition is more severe. To probe this implication, we split our sample into two groups based on a proxy for the degree of tax competition that a municipality faces. As municipalities subject to greater tax competition are more likely to mimic the tax setting of their neighbors, we define low tax competition municipalities as municipalities whose tax rate deviates by at least 5 percent from the district average.[46] Figure 9B reports the estimates. In line with the model, municipalities facing lower tax competition adjust local tax multipliers more quickly and strongly. A Census Shock of one standard deviation (4.1 percent) leads to a business tax (property tax index) multiplier reduction of about 1.6 (1.6) percentage points in low-tax competition municipalities, but only a reduction of 0.4 (1.2) percentage points under high tax competition.[47] Municipal governments with low tax competition thus return a considerably higher share of their transfer gains to the local inhabitants. These differences become larger when considering a stricter definition of high tax competition (at least 10 percent deviation from the district average; see Appendix Table A.2, column (2)).[48]

A number of additional observations suggest that the slow adjustment of taxes to fiscal conditions is a more general phenomenon. First, municipalities adjust their tax rates infrequently. In our sample, only around ten percent change tax rates in a given year and only half change the business tax multiplier over a ten-year period. Consistent with this observation, the mean response hides considerable heterogeneity. For example, the response among municipalities that did alter their tax rates during our observation period is at least 50 percent larger (Appendix Table A.2, column (4)) than the mean response in the full sample. Further, while we might expect fiscal shocks to trigger *additional* tax changes, we instead find that transfer gains tend to *decrease* their frequency (Appendix Table A.2, column (5)). This pattern can be rationalized by the observation that only a few municipalities decrease their tax multipliers

[46] Municipal tax rates are heavily clustered. The tax multipliers of neighboring municipalities are highly predictive of a municipality's own multiplier, and more than half of the municipalities in our sample choose the exact same business tax multiplier as one of their three closest neighbors.

[47] The difference of the business tax multiplier coefficient estimates at event period $\tau = 5$ and $\tau = 10$ are statistically significant at the 10% level ($p = 0.06$ and $p = 0.08$, respectively).

[48] These results are robust to controlling for population size interacted with the Census shock and event period. Indeed, the response pattern by municipality size further supports the interpretation that tax competition dampens the tax response to fiscal transfers (see Section 7.2).

during our study period (14 percent of all changes). As such, the negative response of tax rates to fiscal transfers primarily reflects the cancellation or postponement of *future* tax increases.

In sum, municipalities adjust their tax rates infrequently and avoid short-term adjustments, in particular if subject to weaker electoral competition or a greater degree of tax competition, or if the direction of those adjustments are at odds with long-term trends. These findings are an important qualifier for empirical studies on the relation between unconditional grants and tax setting, which have primarily focused on short-term or cross-sectional relations. In Section 8 we describe these implications in more detail.

7 Variability in the Fiscal and Tax Response

In this section, we analyze heterogeneities in the municipal response to fiscal transfers. We first study whether the response to an increase in transfers (positive shock) differs from that to a transfer loss (negative shock). We then examine whether the fiscal response depends on local economic and fiscal conditions such as the unemployment rate or the municipality's debt level.

7.1 Asymmetries: Transfer Gains vs. Losses

While recent studies provide quasi-experimental evidence of the response to unconditional grants (see e.g., Knight 2002, Dahlberg et al. 2008, or Leduc and Wilson 2017), most exploit variation in grant *increases*.[49] However, the Census Shock leads to both gains and losses within the fiscal equalization scheme, allowing us to compare their dynamic implications on budgets and tax setting.

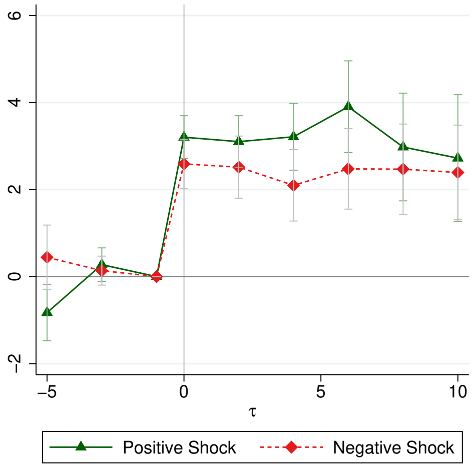
We first construct indicator variables on whether a municipality experienced a positive or negative Census Shock. We define these indicators relative to the average shock in the federal state, since a municipality's relative gain depends on whether its change in official population counts was larger (or smaller) than the state's average shock. We then estimate variants of our baseline specification, interacting the indicator variables with the Census Shock in the first-stage equation (3) and with the transfer variable in the second-stage equation (4). We pool the data over two-year periods to increase precision and simplify the presentation.

We present the results for our main outcomes in Figure 10 and for other categories in Appendix Tables A.3 and A.4. Figure 10A shows that fiscal transfers are more sensitive to positive than to negative shocks: on average, an additional person counted in the Census

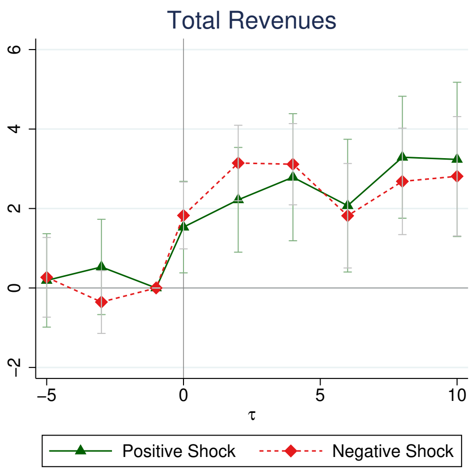
[49] Other studies consider the joint response to positive and negative transfer changes in transfers, but do not account for their endogeneity (e.g., Gramlich 1987, Gamkhar and Oates 1996, Heyndels 2001, Rattsø and Tovmo 2002, Deller and Maher 2005, or Melo 2002).

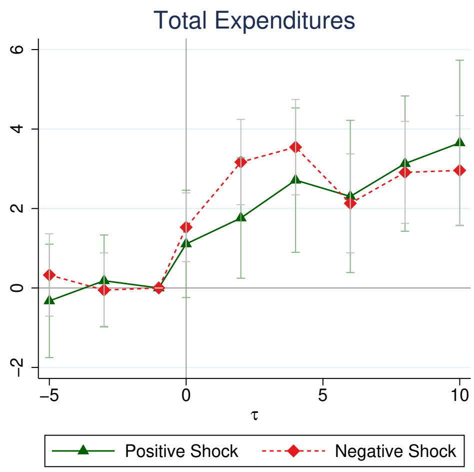
Figure 10: The Response to Positive and Negative Shocks

A. Fiscal Transfers

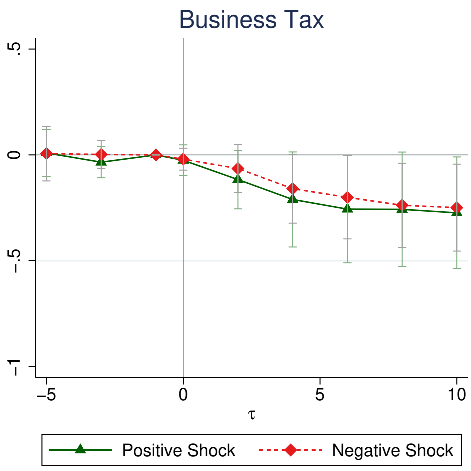


B. Fiscal Budgets





C. Local Tax Multipliers



Notes: The figure reports coefficient estimates and 95-percent confidence intervals of the effects of the Census Shock on municipal fiscal transfers in Panel A, revenues and expenditures in Panel B, and local tax multipliers in Panel C. Panels A and C show reduced-form estimates based on variants of equation (3), while Panel B shows two-stage least square estimates based on variants of equation (4). Regressions are heteroscedasticity-weighted.

leads to a 315 EUR increase in transfers, while the loss of an inhabitant decreases transfers by about 258 EUR. This asymmetry is not driven by different characteristics of municipalities (the Census Shock is largely uncorrelated with municipality characteristics, as shown in Section 4.3). Instead, some federal states appear to cushion the impact of negative shocks.[50] Accounting for this asymmetry in the 2SLS estimator, we find that total revenues and expenditures respond symmetrically to transfer gains and losses in the long run (see Figure 10B). Five to six years after the onset of the Census Shock, the estimated effects are near-mirror images of each other. However, municipalities respond more quickly to negative shocks, such that the fiscal response is asymmetric in the short run. Expenditures drop rapidly within three years after a reduction in transfers, while they increase more steadily after transfer gains. These results are in line with the notion that negative shocks force municipalities to react immediately to balance their budgets, while positive shocks leave them with greater flexibility. We expand on this argument in Section 8.2.

Tables A.3 and A.4 show the fiscal response across sub-categories, which appears largely symmetric. *Investments in infrastructure* and revenues from *fees and contributions* show strong reactions to both negative and positive shocks. However, in line with the overall effects on spending, municipalities experiencing a drop in transfers adjust their investments more quickly. Fiscal transfers also have a symmetric effect on tax setting: while positive shocks trigger a decrease of tax multipliers, municipalities that experience fiscal losses increase taxes (see Panel C of Figure 10). The magnitude of these responses are similar in the short and long run.

7.2 Fiscal Transfers and Local Conditions

We next analyze whether the fiscal response depends on local economic and fiscal conditions. We focus on heterogeneities along three dimensions: population, unemployment, and debt levels. We split our sample based on the pre-treatment value of the respective variable and estimate equation (4) and the corresponding first stage separately for each sub-sample. Table 4 reports the estimates for our most important outcome variables and for three two-year periods representing the short- ($\tau = 0, 1$), medium- ($\tau = 4, 5$) and long-run response ($\tau = 9, 10$). In Appendix B.2, we provide more detailed evidence, allowing the response of municipal budgets to vary smoothly with baseline characteristics by interacting the change in fiscal transfers with a restricted cubic spline of the respective characteristic of interest.

In Panel A, we split municipalities into two groups based on their 1986 population level into the top population tercile (corresponding to a population of about 7,500 and over) and bottom

[50] In particular, some federal states established a floor level of spending, and replace 100% of the funding gap between fiscal need and fiscal capacity up to this floor (Egger et al. 2010).

Table 4: Heterogeneity of the Effect on Revenues and Expenditures

(a) Revenues

	Total Revenues (Net)		Administrative Budget				Capital Budget			
			Taxes		Fees and Charges		Fees and Contributions		Loans	
	(1.1) High	(1.2) Low	(2.1) High	(2.2) Low	(3.1) High	(3.2) Low	(4.1) High	(4.2) Low	(5.1) High	(5.2) Low
Panel A: by Population Level										
τ = 0/1	1.22***	0.85***	0.35***	-0.03	0.04	0.02	0.14	0.07	-0.44**	0.12
	(0.34)	(0.29)	(0.12)	(0.07)	(0.06)	(0.05)	(0.09)	(0.12)	(0.21)	(0.14)
τ = 4/5	0.45	2.60***	0.03	0.20	-0.04	0.03	0.30**	0.60***	-0.24	0.57**
	(0.63)	(0.53)	(0.20)	(0.16)	(0.15)	(0.08)	(0.14)	(0.18)	(0.25)	(0.22)
τ = 9/10	0.40	2.77***	-0.02	0.39**	-0.20	0.24	0.11	0.56***	-0.26	0.42*
	(0.83)	(0.53)	(0.31)	(0.16)	(0.26)	(0.16)	(0.11)	(0.15)	(0.25)	(0.22)
Panel B: by Unemployment Rate										
τ = 0/1	1.01***	0.96***	-0.04	0.24**	0.04	0.01	0.15*	0.05	-0.12	-0.12
	(0.33)	(0.32)	(0.09)	(0.10)	(0.04)	(0.06)	(0.09)	(0.13)	(0.18)	(0.16)
τ = 4/5	1.83***	2.34***	-0.02	0.36*	0.10	-0.12	0.54***	0.57**	0.27	0.31
	(0.48)	(0.65)	(0.16)	(0.18)	(0.11)	(0.11)	(0.16)	(0.21)	(0.22)	(0.27)
τ = 9/10	1.56***	2.72***	0.09	0.29	-0.04	0.21	0.44***	0.45**	0.21	0.13
	(0.54)	(0.70)	(0.20)	(0.27)	(0.18)	(0.20)	(0.13)	(0.19)	(0.22)	(0.27)
Panel C: by Debt Level										
τ = 0/1	1.09***	0.85**	0.08	0.10	0.04	0.00	0.03	0.19	-0.13	-0.03
	(0.34)	(0.38)	(0.08)	(0.11)	(0.05)	(0.06)	(0.09)	(0.13)	(0.18)	(0.15)
τ = 4/5	1.85***	2.41***	0.08	0.23	0.05	-0.12	0.59***	0.62**	0.24	0.47*
	(0.50)	(0.73)	(0.15)	(0.22)	(0.12)	(0.12)	(0.14)	(0.25)	(0.22)	(0.25)
τ = 9/10	1.43**	2.93***	0.33**	0.01	0.07	0.13	0.40***	0.63**	0.00	0.53*
	(0.55)	(0.70)	(0.16)	(0.39)	(0.18)	(0.18)	(0.13)	(0.25)	(0.20)	(0.30)

(b) Expenditures

	Total Expenditures (Net)		Administrative Budget				Capital Budget			
			Public Employees		Material		Acquisition of Assets		Other Expenditures (incl. infrastructure)	
	(1.1) High	(1.2) Low	(2.1) High	(2.2) Low	(3.1) High	(3.2) Low	(4.1) High	(4.2) Low	(5.1) High	(5.2) Low
Panel A: by Population Level										
τ = 0/1	0.93**	0.66	0.03	-0.03	0.11	0.09	0.03	0.43	0.65*	0.36
	(0.42)	(0.50)	(0.03)	(0.03)	(0.07)	(0.06)	(0.28)	(0.30)	(0.38)	(0.33)
τ = 4/5	0.72	2.65***	0.04	-0.03	0.16	0.26*	0.52**	1.28***	0.10	1.14***
	(0.50)	(0.64)	(0.09)	(0.08)	(0.14)	(0.14)	(0.24)	(0.49)	(0.39)	(0.43)
τ = 9/10	0.34	2.65***	-0.10	0.07	0.21	0.37**	0.05	1.01***	-0.02	1.00**
	(0.66)	(0.66)	(0.13)	(0.11)	(0.27)	(0.18)	(0.23)	(0.36)	(0.44)	(0.46)
Panel B: by Unemployment Rate										
τ = 0/1	0.28	1.17**	0.02	0.01	0.13***	0.07	0.33	0.22	-0.14	0.95**
	(0.52)	(0.54)	(0.03)	(0.02)	(0.05)	(0.07)	(0.30)	(0.25)	(0.29)	(0.42)
τ = 4/5	1.80***	2.85***	0.05	0.05	0.33***	0.12	0.96**	1.27***	0.45	1.29**
	(0.58)	(0.71)	(0.07)	(0.11)	(0.09)	(0.18)	(0.44)	(0.45)	(0.34)	(0.53)
τ = 9/10	1.34**	2.96***	0.02	0.16	0.29**	0.31	0.69*	0.86**	0.10	1.29**
	(0.63)	(0.81)	(0.09)	(0.13)	(0.13)	(0.29)	(0.37)	(0.34)	(0.37)	(0.54)
Panel C: by Debt Level										
τ = 0/1	0.43	1.12**	0.01	-0.02	0.11**	0.06	0.28	0.30	0.42	0.49
	(0.43)	(0.55)	(0.03)	(0.03)	(0.06)	(0.08)	(0.23)	(0.33)	(0.28)	(0.41)
τ = 4/5	2.08***	2.65***	0.04	-0.03	0.36***	0.05	1.14***	1.22***	0.80**	0.96*
	(0.58)	(0.65)	(0.06)	(0.13)	(0.11)	(0.18)	(0.40)	(0.46)	(0.36)	(0.49)
τ = 9/10	1.26**	3.06***	0.04	0.08	0.45**	0.11	0.49	1.17***	0.49	0.87
	(0.62)	(0.70)	(0.09)	(0.15)	(0.18)	(0.24)	(0.33)	(0.34)	(0.35)	(0.57)

Notes: The table reports 2SLS estimates of the effects of fiscal transfers on revenues and expenditures based on equation (4). Sample sizes are reported in Tables 2 and 3. We split the sample by municipal population levels in 1986 (top third versus bottom two thirds, Panel A), the mean unemployment rate over the 1986-88 period, or the mean debt level (median split, Panels B and C). All columns control for federal state x year fixed effects and observations are heteroscedasticity-weighted. Standard errors are clustered at the commuting zone level. Significance levels: 1% ***, 5% **, 10% *.

two thirds. The effect of intergovernmental transfers varies substantially with population size. In large municipalities, we observe a strong short-run effect with an over-proportional and immediate impact of transfers on *total revenues* and a one-to-one impact on *total expenditures*. Additional revenues are spent primarily on infrastructure investments (*other expenditures, incl. infrastructure*). Subsequently, revenues, expenditures, and investments decrease over the following years.

The inverse pattern applies to small municipalities. Their fiscal response is delayed, and it takes several years until additional transfers are incorporated into expenditures. However, the long-run revenue and spending multipliers are large. This finding reflects a more sustained increase in investments, with each euro of transfers increasing *investments (incl. infrastructure)* by approximately one euro. Moreover, *tax revenues* increase steadily from a (precise) zero short-run effect to a substantial long-term increase (0.39 EUR for each additional euro of transfers). Consequently, fiscal transfers initially have a strong effect on tax and total revenues in large municipalities, but reductions in local tax rates diminish these revenue gains over time, while the expansionary effect for small municipalities is more sustained.[51]

These opposing trends in revenue and spending patterns can therefore be explained by differences in municipalities' tax decisions: large municipalities decrease their tax rates by considerably more than small municipalities, especially the business tax, for which the response is five times larger than in small municipalities (see Appendix Figure A.4). The weak tax response in small municipalities is consistent with our finding that local tax competition dampens the tax response to fiscal transfers (Section 6.2), as the tax base in smaller jurisdiction is more sensitive to neighboring jurisdictions' tax decisions.[52]

The next two panels of Table 4 show that the impact of intergovernmental transfers depends on local economic and fiscal conditions. We split our sample at the median of the average 1986 to 1988 unemployment rate (measured at the district level, panel B) or municipal debt level (as a share of municipal revenues, panel C), respectively. Transfers have a much more expansionary effect in areas with favorable economic or fiscal conditions. Municipalities with unfavorable economic or fiscal conditions instead decrease their tax multipliers. In particular, municipalities with high unemployment aggressively cut the business tax multiplier – possibly to gain competitiveness in attracting businesses and therefore jobs (see Figure B.2).[53]

[51] We find a similar pattern in data from the *statistical yearbooks*, which contain fiscal information on municipalities with more than 20,000 inhabitants from all West German states (rather than the five most populous ones).

[52] This implication follows from the literature on asymmetric tax competition, and has been confirmed empirically in the German context (Buettner, 2003). Larger cities may however not only compete with their neighbors, but also with other large cities in more distant locations (Janeba and Osterloh, 2013).

[53] Fuest et al. (2018) demonstrate that in Germany, workers bear about one half of the corporate tax burden. As such, a decrease in the business tax increases both the wage of local workers and the attractiveness of a location

Municipalities with high debt levels decrease instead their property taxes (see Figure B.3).

In sum, budgetary and tax responses differ substantially with local demographic, economic, and fiscal conditions – both in their magnitude and their timing. As a consequence, they can have a very different cross-sectional pattern in the short and long run. The contrast is particularly striking when comparing large and small municipalities, which experience opposing dynamics in their spending multipliers. This also has implications for the recent literature on local fiscal multipliers (e.g., Serrato and Wingender 2014, Auerbach et al., 2019), suggesting that both the magnitude and the dynamic pattern of fiscal multipliers should vary with local characteristics.

8 Interpretation

The evolution of local fiscal budgets appears more protracted than previously shown. The explicit contrast between short- and long-term responses also provides a new perspective on some well-known "anomalies" in public finance. In this section, we interpret our results in the context of two such debates: that on the relationship between intergovernmental grants and tax setting, and that on asymmetries in the response to fiscal gains and losses. We argue that traditional theories of collective choice and fiscal federalism better explain the long-run than the short-run decision making of local governments. But most empirical studies have focused on the short run, suggesting that the inconsistency between theoretical predictions and empirical "facts" may at least partially reflect a temporal mismatch.

8.1 The Flypaper Effect

In standard models of collective choice, intergovernmental grants have a similar effect on fiscal policy and public spending as an equivalent increase in private incomes (Bradford and Oates, 1971a,b). In the median voter theory, for example, the median voter determines fiscal policy and therefore the allocation of funds between private and public consumption. The allocation of these funds is then not determined by their origin, but rather by the median voter's marginal propensity of consumption. However, in practice it seems that the origin *does* matter: intergovernmental grants result in greater spending and lower tax reductions than what standard models predict. In other words, "*money sticks where it hits*," an empirical regularity that has become known as the "*flypaper effect*" (Hines and Thaler, 1995, Inman, 2008).

This apparent contradiction between theoretical predictions and empirical evidence has

for firms. Indeed, Buettner (2003) shows that changes in the business tax rate have strong effects on the local tax base.

triggered a vast body of work. Some authors have developed extensions of the standard theory that seek to rationalize the observed insensitivity of taxes to transfers. Others have scrutinized the empirical evidence. Knight (2002) argues that prior work has not sufficiently accounted for the potential endogeneity of fiscal transfers, and that the flypaper effect might be an artifact of this endogeneity problem. However, recent studies based on quasi-experimental research designs still find evidence in support of the flypaper effect (see e.g., Dahlberg et al. 2008, Leduc and Wilson 2017, Liu and Ma 2016, or Lundqvist 2015). One exception is Lutz (2010), who finds that intergovernmental grants are treated largely equivalent to private income in a particular setting in which citizens are more directly involved in fiscal decisions.

In this paper, we set forth a different reason for why the flypaper effect may have been overstated in empirical work. Both the theoretical and empirical literature have been relatively silent about the timing of the policy response to intergovernmental grants. While the former focuses on equilibrium concepts, the latter captures snapshots – typically relating fiscal transfers in year t to taxes or expenditures in the same year.[54] If fiscal adjustments do not take place immediately, this approach identifies only the short-term effect of transfers (or, in cross-sectional studies, a combination of the short- and long-term response). In comparison, we track the fiscal response over a long time window. We find that tax rates adjust only slowly to a change in fiscal conditions, much more gradually than municipal budgets (see Sections 5 and 6). While most of the adjustment in spending occurs within the first four years, the response of local tax multipliers takes a decade or longer. Indeed, the short-term effect of intergovernmental grants on municipal tax setting is an order of magnitude smaller than the long-term effect (see Table A.2). This finding suggests that the empirical literature may have underestimated the extent to which fiscal transfers result in tax decreases and, consequently, overstated the flypaper effect.

Why do local taxes respond so slowly to a change in municipal budgets, and what does this observation tell us about political and fiscal decision processes? Our results point to the degree of tax competition as an important source of friction, as we found a particularly slow and weak tax response among municipalities facing high tax competition. Our findings suggest however also that local governments do respect the fiscal preferences of their inhabitants as prescribed by standard theories, in particular if subject to strong electoral competition, but that those preferences have a delayed effect on municipal decision making.

[54] Important exceptions are Gordon (2004) and Cascio et al. (2013), who track the fiscal adjustment to a specific type of educational grant over several years.

8.2 The (A)symmetry of Spending Decisions

Theoretical predictions and empirical evidence are also at odds with respect to whether policymakers respond *symmetrically* to positive and negative shocks.[55] Standard models of collective choice, such as the median voter theory, predict a symmetric spending response. However, the empirical literature has found little support for this prediction. Many recent applications find an asymmetric response, with grant reductions triggering only small, if any, reductions in spending (e.g., Deller and Maher, 2005).

To rationalize these asymmetries, the theoretical literature emphasizes institutional and political complexities (Bailey and Connolly, 1998), as well as behavioral phenomena (Hines and Thaler, 1995). A prominent example is the theory of *fiscal illusion*, which posits that local taxpayers are not fully aware of the extent to which grants contribute to municipal revenues. As a consequence, voters support the expansion of public services because they underestimate their cost. Many alternative mechanisms have been suggested, but no theoretical consensus has thus far emerged (Heyndels, 2001).

Whether spending responds symmetrically also matters from a policy perspective. Understanding how local governments spend intergovernmental transfers is essential for the design of efficient fiscal policy (Inman, 2008). At the heart of the debate is the concern that fiscal federalism and fiscal equalization schemes create inefficiencies and distortions. One particular concern is the *bias towards fiscal expansion* (also called *fiscal replacement*), whereby municipalities that receive additional grants spend most of the funds, but do not reduce spending when grants decrease, choosing instead to increase taxation (e.g., Gramlich 1987, Oates 1999, Heyndels 2001, Rattsø and Tovmo 2002). Such a pattern would contribute to a slow but persistent drift towards higher spending and higher taxation, threatening the long-term sustainability of redistributive fiscal schemes in federal systems.[56]

In line with the empirical literature, we do find asymmetric responses in total expenditures in the short run. Municipalities experiencing a negative shock adjust their finances more quickly than those that experience a positive shock (see Section 7.1). In contrast to earlier studies, our results however suggest that the overall effects of spending decisions *are* symmetric in the medium to long term. Six years after the initial shock, total expenditures decrease by nearly the same amount in response to a negative shock as they increase in response to a positive shock (see Figure 10). In particular, we do not find evidence for a "*fiscal bias towards*

[55] According to Volden (1999), research on this question has suffered from both "*facts without theory*" and "*theory without evidence*".

[56] Such drift is also a concern in the German context, where local tax rates have increased steadily over the last seven decades, and where some federal states and municipalities are highly indebted. For example, Fuest et al. (2018) note that 93 percent of all changes in the local business tax rate between 1993 and 2012 were tax increases.

expansion": intergovernmental grants do not lead to a permanent expansion of the public sector, at least not in as direct a manner as some studies have postulated.

9 Conclusions

Using quasi-exogenous variation triggered by the German Census, we study the dynamic fiscal and tax response to intergovernmental grants. Municipalities that "gained" inhabitants in the Census received additional transfers within Germany's fiscal equalization scheme, triggering a reallocation of billions of euros across municipalities. Municipalities use the additional revenues predominantly to increase investments, which peak around five years after the shift in fiscal transfers. The response pattern in expenditures and a long-run multiplier in revenues of around two point to dynamic feedback effects between spending and revenue streams in subsequent years. Municipalities also adjust their local tax rates, but not immediately – the tax response only becomes sizable several years after the shock in transfers. Tax rates adjust therefore much more slowly to fiscal conditions than municipal spending. Both the magnitude and timing of these responses differ with local economic conditions.

Our results thus suggest that fiscal adjustments are more protracted than previously shown. This observation in turn provides a new perspective on some of the key issues in the literature on fiscal federalism. Various empirical regularities appear at odds with standard models of collective choice, raising the question if municipal fiscal and tax decisions can be rationalized by traditional economic theory. Two such "anomalies" have attracted particular attention: (i) fiscal grants are observed to increase expenditures more and reduce taxes less than predicted by theory (the so-called *flypaper effect*), and (ii) the response to positive and negative shocks appears to be asymmetric, with expenditures more elastic to transfer gains than losses (implying a *fiscal bias towards expansion*).

Our results, however, suggest that the prior literature may have underestimated the extent to which transfers cause tax reductions, and overstated the *flypaper effect*. Most prior studies only capture the immediate or a cross-sectional snapshot of the fiscal and tax response. While the tax response is indeed negligible in the first years after the transfer shock, the adjustment continues in subsequent years. The response after one decade is a magnitude of order larger than the initial response, and amplifies further thereafter.

Our results further indicate that the asymmetries in the response to transfer gains and losses as reported in previous work may reflect a short-run phenomena. We do find fiscal asymmetries in the first years: municipalities adjust their finances more quickly in response to a negative shock in transfers than a positive one. However, the response is symmetric in the long run. This is a useful piece of evidence to differentiate between competing theories of local public finance, and suggests that municipal governments do not have an "expansionary bias" in the German setting.

Overall, we find that standard models explain better the long-run decision making of local governments than their short-run fiscal decisions. Since most empirical studies capture the short run, this observation suggests that the apparent mismatch between theoretical predictions and empirical "facts" can be partially explained by a mismatch in their temporal perspective. The important question of what determines these dynamic patterns remains. The most striking finding is how slowly tax rates adapt, with adjustments continuing one decade after the permanent shift in transfers. This finding calls for an examination of *why* local taxes respond so slowly and what this may tell us about fiscal and political decision processes. Our results point to tax competition and low electoral competition as two important factors dampening the tax response. A greater understanding of these and other dynamic patterns may provide valuable input for future work.

References

AUERBACH, A. J., Y. GORODNICHENKO, AND D. MURPHY (2019): "Local Fiscal Multipliers and Fiscal Spillovers in the United States," NBER Working Papers 25457, National Bureau of Economic Research, Inc.

BAICKER, K. (2001): "Government Decision-making and the Incidence of Federal Mandates," *Journal of Public Economics*, 82, 147 – 194.

BAILEY, S. J. AND S. CONNOLLY (1998): "The Flypaper Effect: Identifying Areas for Further Research," *Public Choice*, 95, 335–361.

BESSHO, S.-I. AND H. OGAWA (2015): "Fiscal Adjustment in Japanese Municipalities," *Journal of Comparative Economics*, 43, 1053–1068.

BRADFORD, D. F. AND W. E. OATES (1971a): "The Analysis of Revenue Sharing in a New Approach to Collective Fiscal Decisions," *The Quarterly Journal of Economics*, 85, 416–439.

——— (1971b): "Towards a Predictive Theory of Intergovernmental Grants," *The American Economic Review*, 61, 440–448.

BROER, M. (2003): "Möglichkeiten zur Stabilisierung der kommunalen Einnahmen," *Wirtschaftsdienst – Zeitschrift für Wirtschaftspolitik (1949 - 2007)*, 83, 132–136.

BUETTNER, T. (2003): "Tax Base Effects and Fiscal Externalities of Local Capital Taxation: Evidence from a Panel of German Jurisdictions," *Journal of Urban Economics*, 54, 110 – 128.

——— (2006): "The Incentive Effect of Fiscal Equalization Transfers on Tax Policy," *Journal of Public Economics*, 90, 477–497.

——— (2009): "The Contribution of Equalization Transfers to Fiscal Adjustment: Empirical Results for German Municipalities and a US-German comparison," *Journal of Comparative Economics*, 37, 417 – 431.

BUETTNER, T. AND D. E. WILDASIN (2006): "The Dynamics of Municipal Fiscal Adjustment," *Journal of Public Economics*, 90, 1115–1132.

CASCIO, E. U., N. GORDON, AND S. REBER (2013): "Local Responses to Federal Grants: Evidence from the Introduction of Title I in the South," *American Economic Journal: Economic Policy*, 5, 126–159.

CASSIDY, T. (2019): "How Forward-Looking Are Local Governments? Evidence from Indonesia," Tech. rep., mimeo.

DAHLBERG, M. AND E. JOHANSSON (1998): "The Revenues-Expenditures Nexus: Panel Data Evidence from Swedish Municipalities," *Applied Economics*, 30, 1379–1386.

——— (2000): "An Examination of the Dynamic Behaviour of Local Governments using GMM Bootstrapping Methods," *Journal of Applied Econometrics*, 15, 401–416.

DAHLBERG, M., E. MÖRK, J. RATTSO, AND H. ÅGREN (2008): "Using a Discontinuous Grant Rule to Identify the Effect of Grants on Local Taxes and Spending," *Journal of Public Economics*, 92, 2320–2335.

DELLER, S. C. AND C. S. MAHER (2005): "A Model of Asymmetries in the Flypaper Effect," *Publius: The Journal of Federalism*, 36, 213–229.

Egger, P., M. Koethenbuerger, and M. Smart (2010): "Do Fiscal Transfers Alleviate Business Tax Competition? Evidence from Germany," *Journal of Public Economics*, 94, 235–246.

Eggers, A. C., R. Freier, and V. Grembi (2017): "Regression Discontinuity Designs Based on Population Thresholds: Pitfalls and Solutions," *American Journal of Political Science*.

Feler, L. and M. Z. Senses (2017): "Trade Shocks and the Provision of Local Public Goods," *American Economic Journal: Economic Policy*, 9, 101–43.

Foremny, D., J. Jofre-Monseny, and A. Solé-Ollé (2017): "'Ghost Citizens': Using Notches to Identify Manipulation of Population-based Grants," *Journal of Public Economics*.

Foremny, D. and N. Riedel (2014): "Business Taxes and the Electoral Cycle," *Journal of Public Economics*, 115, 48 – 61.

Fuest, C., A. Peichl, and S. Siegloch (2018): "Do Higher Corporate Taxes Reduce Wages? Micro Evidence from Germany," *American Economic Review*, 108, 393–418.

Gamkhar, S. and W. E. Oates (1996): "Asymmetries in the Response to Increases and Decreases in Intergovernmental Grants: Some Empirical Findings," *National Tax Journal*, 49, 501–512.

Gemeindefinanzierungsgesetz Nordrhein-Westfalen (1988): "Gesetz zur Regelung der Zuweisungen des Landes Nordrhein-Westfalen an die Gemeinden und Gemeindeverbände im Haushaltsjahr 1988, Drucksache 10/2252," .

Gennari, E. and G. Messina (2014): "How Sticky are Local Expenditures in Italy? Assessing the Relevance of the Flypaper Effect through Municipal Data," *International Tax and Public Finance*, 21, 324–344.

Gesetz über die massgebende Einwohnerzahl Baden-Württemberg (1988): "Gesetzblatt fuer Baden-Württemberg, No 10," .

Gordon, N. (2004): "Do Federal Grants Boost School Spending? Evidence from Title I," *Journal of Public Economics*, 88, 1771 – 1792.

Gramlich, E. M. (1987): "Federalism and Federal Deficit Reduction," *National Tax Journal*, 40, 299–313.

Heyndels, B. (2001): "Asymmetries in the Flypaper Effect: Empirical Evidence for the Flemish Municipalities," *Applied Economics*, 33, 1329–1334.

Hines, J. and R. Thaler (1995): "The Flypaper Effect," *Journal of Economic Perspectives*, 9, 217–226.

Holtz-Eakin, D., W. Newey, and H. S. Rosen (1989): "The Revenues-Expenditures Nexus: Evidence from Local Government Data," *International Economic Review*, 30, 415–429.

Inman, R. P. (2008): "The Flypaper Effect," NBER Working Papers 14579, National Bureau of Economic Research, Inc.

Janeba, E. and S. Osterloh (2013): "Tax and the City - A Theory of Local Tax Competition," *Journal of Public Economics*, 106, 89–100.

Knight, B. (2002): "Endogenous Federal Grants and Crowd-out of State Government Spending: Theory and Evidence from the Federal Highway Aid Program," *American Economic Review*, 92, 71–92.

Leduc, S. and D. Wilson (2017): "Are State Governments Roadblocks to Federal Stimulus? Evidence on the Flypaper Effect of Highway Grants in the 2009 Recovery Act," *American Economic Journal: Economic Policy*, 9, 253–92.

Litschig, S. and K. M. Morrison (2013): "The Impact of Intergovernmental Transfers on Education Outcomes and Poverty Reduction," *American Economic Journal: Applied Economics*, 5, 206–40.

Liu, C. and G. Ma (2016): "Taxation without Representation: Local Fiscal response to Intergovernmental Transfers in China," *International Tax and Public Finance*, 23, 854–874.

Löffler, M. and S. Siegloch (2018): "Property Taxation, Housing, and Local Labor Markets: Evidence from German Municipalities," Tech. rep., mimeo.

Lundqvist, H. (2015): "Granting Public or Private Consumption? Effects of Grants on Local Public Spending and Income Taxes," *International Tax and Public Finance*, 22, 41–72.

Lutz, B. (2010): "Taxation with Representation: Intergovernmental Grants in a Plebiscite Democracy," *The Review of Economics and Statistics*, 92, 316–332.

Martin-Rodriguez, M. and H. Ogawa (2017): "The Empirics of the Municipal Fiscal Adjustment," *Journal of Economic Surveys*, 31, 831–853.

Melo, L. (2002): "The Flypaper Effect Under Different Institutional Contexts: The Colombian Case," *Public Choice*, 111, 317–345.

Oates, W. E. (1999): "An Essay on Fiscal Federalism," *Journal of Economic Literature*, 37, 1120–1149.

——— (2008): "On the Evolution of Fiscal Federalism: Theory and Institutions," *National Tax Journal*, 61, 313–334.

Rattsø, J. and P. Tovmo (2002): "Fiscal Discipline and Asymmetric Adjustment of Revenues and Expenditures: Local Government Responses to Shocks in Denmark," *Public Finance Review*, 30, 208–234.

Serrato, J. C. S. and P. Wingender (2014): "Estimating Local Fiscal Multipliers," Tech. rep., mimeo.

——— (2016): "Estimating Local Fiscal Multipliers," Tech. rep., National Bureau of Economic Research.

Solé-Ollé, A. and P. Sorribas-Navarro (2012): "The Dynamic Adjustment of Local Government Budgets: Does Spain Behave Differently?" *Applied Economics*, 44, 3203–3213.

Statistisches Jahrbuch (1988): *Statistisches Jahrbuch deutscher Gemeinden*, Deutscher Städtetag.

Stenographischer Bericht der 78. Sitzung (1989): "Niedersächsisches Landtag," .

Verordnung zum Finanzausgleichsgesetz Bayern (1988): *Bayerisches Gesetz und Verordnungsblatt, No 19*, Bayerische Staatsregierung.

Volden, C. (1999): "Asymmetric Effects of Intergovernmental Grants: Analysis and Implications for U.S. Welfare Policy," *Publius: The Journal of Federalism*, 29, 51–73.

Online Appendix

A Additional Tables and Figures

Table A.1: Spatial Variation in the Census Shock

	Dependent variable: Census Shock			
	Federal State Fixed Effects	Commuting Zone Fixed Effects	District Fixed Effects	Nearest Neighbors
	(1)	(2)	(3)	(4)
R2	0.017	0.164	0.185	0.085
Residual Variation (SD)	0.040	0.037	0.037	0.039
# fixed effects	5	173	265	5
# municipalities	4,373	4,365	4,304	4,373

Notes: The table reports the R2 and residual variation from an unweighted regression of the Census Shock on federal state (column (1)), commuting zone (columns (2) and (4)), or district fixed effects (column (3)). Column (4) includes the population-weighted average Census Shock of the nearest five neighbors as an explanatory variable (in addition to federal state fixed effects). The Census Shock is defined as the log difference between the Census count on May 25, 1987 and the population projection on December 31, 1986.

Table A.2: Dynamic Effects of Fiscal Transfers on Local Tax Rates

	Baseline		Tax Competition (10% deviation)				Size of Shock				>0 adjustments		Tax Changes (any tax)
	Business Tax	Property Tax (Index)	Business Tax		Property Tax		Business Tax		Property Tax		Business Tax	Property Tax	
			High	Low	High	Low	Small	Large	Small	Large			
	(1.1)	(1.2)	(2.1)		(2.2)		(3.1)		(3.2)		(4.1)	(4.2)	(5)
τ = -5	0.029	-0.028	0.018	0.147	-0.047	0.133	-0.027	0.058	-0.275**	0.086	0.046	-0.039	-0.002
	(0.044)	(0.068)	(0.044)	(0.208)	(0.058)	(0.116)	(0.075)	(0.052)	(0.107)	(0.073)	(0.069)	(0.088)	(0.005)
τ = -4	-0.014	-0.056	-0.019	0.060	-0.075	0.102	-0.113*	0.022	-0.290***	0.053	-0.022	-0.077	-0.005
	(0.042)	(0.061)	(0.042)	(0.186)	(0.049)	(0.111)	(0.058)	(0.056)	(0.097)	(0.071)	(0.066)	(0.081)	(0.004)
τ = -3	-0.020	-0.026	-0.022	-0.055	-0.010	-0.014	-0.053	-0.015	-0.123*	0.013	-0.033	-0.035	-0.003
	(0.026)	(0.034)	(0.025)	(0.070)	(0.043)	(0.049)	(0.053)	(0.026)	(0.067)	(0.033)	(0.041)	(0.047)	(0.003)
τ = -2	-0.006	-0.016	-0.006	-0.036	-0.016	0.008	-0.019	-0.015	-0.115**	0.012	-0.010	-0.022	-0.002
	(0.022)	(0.030)	(0.018)	(0.063)	(0.034)	(0.039)	(0.041)	(0.019)	(0.053)	(0.026)	(0.037)	(0.041)	(0.002)
τ = -1	.	.	.	.	.	.	.	.	.	.	.	.	.
τ = 0	-0.003	0.005	0.008	-0.090	0.028	-0.040	-0.011	0.005	-0.023	0.020	-0.005	0.006	-0.001
	(0.015)	(0.024)	(0.015)	(0.064)	(0.024)	(0.047)	(0.032)	(0.020)	(0.042)	(0.030)	(0.024)	(0.033)	(0.002)
τ = 1	-0.042	-0.047	-0.019	-0.110	-0.015	-0.040	-0.103*	-0.024	-0.090	-0.022	-0.071*	-0.067	-0.004
	(0.026)	(0.040)	(0.026)	(0.093)	(0.037)	(0.079)	(0.062)	(0.028)	(0.067)	(0.048)	(0.042)	(0.056)	(0.003)
τ = 2	-0.079**	-0.113**	-0.03	-0.317*	-0.056	-0.095	-0.160**	-0.061	-0.155*	-0.078	-0.128**	-0.158**	-0.006*
	(0.034)	(0.055)	(0.031)	(0.174)	(0.050)	(0.090)	(0.066)	(0.037)	(0.079)	(0.068)	(0.055)	(0.080)	(0.004)
τ = 3	-0.092**	-0.156**	-0.013	-0.359*	-0.088	-0.116	-0.189**	-0.055	-0.234*	-0.097	-0.149**	-0.220**	-0.007
	(0.042)	(0.064)	(0.041)	(0.212)	(0.068)	(0.111)	(0.092)	(0.042)	(0.122)	(0.077)	(0.068)	(0.092)	(0.005)
τ = 4	-0.161**	-0.225***	-0.08	-0.435	-0.082	-0.360**	-0.350***	-0.118**	-0.370**	-0.171*	-0.252***	-0.316***	-0.011*
	(0.062)	(0.081)	(0.059)	(0.273)	(0.071)	(0.167)	(0.126)	(0.059)	(0.151)	(0.095)	(0.087)	(0.111)	(0.006)
τ = 5	-0.200***	-0.263***	-0.1	-0.686**	-0.116	-0.415**	-0.370***	-0.140**	-0.519***	-0.137	-0.317***	-0.373***	-0.012*
	(0.066)	(0.096)	(0.066)	(0.278)	(0.084)	(0.172)	(0.133)	(0.068)	(0.178)	(0.115)	(0.091)	(0.132)	(0.007)
τ = 6	-0.230***	-0.262**	-0.117	-0.765**	-0.121	-0.475**	-0.381***	-0.158**	-0.440**	-0.171	-0.368***	-0.376**	-0.012
	(0.074)	(0.115)	(0.076)	(0.302)	(0.114)	(0.194)	(0.139)	(0.076)	(0.181)	(0.133)	(0.098)	(0.154)	(0.008)
τ = 7	-0.217***	-0.244**	-0.103	-0.801**	-0.116	-0.379*	-0.367***	-0.162**	-0.398*	-0.173	-0.351***	-0.357**	-0.01
	(0.073)	(0.123)	(0.072)	(0.336)	(0.131)	(0.199)	(0.134)	(0.077)	(0.208)	(0.140)	(0.097)	(0.162)	(0.009)
τ = 8	-0.232***	-0.330**	-0.121*	-0.797**	-0.141	-0.512**	-0.309**	-0.195**	-0.406*	-0.280*	-0.377***	-0.477***	-0.011
	(0.073)	(0.134)	(0.071)	(0.339)	(0.141)	(0.208)	(0.134)	(0.078)	(0.214)	(0.154)	(0.098)	(0.172)	(0.010)
τ = 9	-0.259***	-0.336***	-0.126*	-1.036***	-0.204	-0.413*	-0.393***	-0.215***	-0.449**	-0.284**	-0.419***	-0.484***	-0.011
	(0.074)	(0.126)	(0.075)	(0.352)	(0.128)	(0.227)	(0.139)	(0.079)	(0.221)	(0.143)	(0.099)	(0.155)	(0.010)
τ = 10	-0.259***	-0.409***	-0.116	-1.042***	-0.225*	-0.575**	-0.397***	-0.219***	-0.467**	-0.386**	-0.418***	-0.583***	-0.012
	(0.074)	(0.134)	(0.071)	(0.360)	(0.128)	(0.237)	(0.137)	(0.079)	(0.225)	(0.153)	(0.103)	(0.161)	(0.011)
Federal States	all	all	all		all		all		all		all	all	all
N	69,504	69,501	68,256		68,253		69,504		69,501		42,928	50,173	69,968

Notes: The table reports estimates of the effects of the Census Shock on local tax multipliers in columns (1) to (4) and on the number of tax changes in column (5) based on the reduced-form equivalent of equation (4). The dependent variables represent the absolute change relative to 1988 of the variables reported in the top row. In columns (2.1) and (2.2), low tax competition municipalities are defined as municipalities whose tax rates deviate by at least 10 percent from the district average. In columns (3.1) and (3.2), large shocks are defined as municipalities with a Census Shock of at least 5 percent. Columns (4.1) and (4.2) restrict the sample to municipalities that adjust tax rates at least once in the analysis period. All columns control for federal state x year fixed effects and observations are heteroscedasticity-weighted based on a modified Breusch-Pagan test. Standard errors clustered at the commuting zone level. Significance levels: 1% ***, 5% **, 10% *.

Table A.4: Asymmetric Effects - Expenditures

	Total		Administrative Budget							
			Total (Admin)		Public Employees		Material		Other Expenditures	
	(1.1)	(1.2)	(2.1)	(2.2)	(3.1)	(3.2)	(4.1)	(4.2)	(5.1)	(5.2)
	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative
τ = -5/-4	-0.32	0.33	0.23	0.09	-0.09*	0.03	0.02	0.15**	0.50**	-0.10
	(0.73)	(0.53)	(0.22)	(0.23)	(0.05)	(0.05)	(0.11)	(0.07)	(0.23)	(0.21)
τ = -3/-2	0.18	-0.05	0.36*	0.00	0.00	0.02	0.02	0.07	0.44*	-0.13
	(0.59)	(0.48)	(0.21)	(0.18)	(0.02)	(0.02)	(0.07)	(0.06)	(0.24)	(0.17)
τ = -1	.	.	.	.	.	.	.	.	.	.
τ = 0/1	1.11	1.53***	0.88***	1.09***	-0.05	0.03	0.12	0.05	0.80***	0.69***
	(0.69)	(0.44)	(0.18)	(0.22)	(0.03)	(0.03)	(0.09)	(0.06)	(0.17)	(0.15)
τ = 2/3	1.76**	3.17***	0.94***	1.45***	-0.14**	0.11	0.18	0.22*	1.02***	0.90***
	(0.77)	(0.55)	(0.23)	(0.31)	(0.06)	(0.07)	(0.12)	(0.11)	(0.20)	(0.23)
τ = 4/5	2.71***	3.54***	1.18***	1.60***	-0.17*	0.12	0.23	0.24	1.13***	0.91***
	(0.93)	(0.61)	(0.34)	(0.30)	(0.09)	(0.10)	(0.15)	(0.15)	(0.24)	(0.22)
τ = 6/7	2.30**	2.13***	1.28***	1.40***	-0.18*	0.15	0.40**	0.16	1.17***	0.83***
	(0.98)	(0.64)	(0.36)	(0.33)	(0.10)	(0.09)	(0.18)	(0.18)	(0.25)	(0.26)
τ = 8/9	3.13***	2.91***	1.26***	1.44***	-0.27**	0.21**	0.46**	0.12	1.17***	0.90***
	(0.87)	(0.66)	(0.38)	(0.38)	(0.13)	(0.10)	(0.21)	(0.21)	(0.27)	(0.28)
τ = 10	3.65***	2.96***	1.72***	1.84***	-0.25*	0.25**	0.58**	0.09	1.44***	1.24***
	(1.06)	(0.70)	(0.46)	(0.46)	(0.14)	(0.11)	(0.24)	(0.23)	(0.34)	(0.34)
States	all		all		all		no HE		no HE	
N	69,968		69,968		69,968		63,152		63,152	

	Capital Budget									
	Total (Capital)		Debt Repayment		Acquisition of Assets		Other Expenditures (incl. Infrastructure)		Infrastructure Investments	
	(6.1)	(6.2)	(7.1)	(7.2)	(8.1)	(8.2)	(9.1)	(9.2)	(10.1)	(10.2)
	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative
τ = -5/-4	-0.52	0.43	0.04	-0.15	-0.08	-0.05	0.10	0.41	-0.16	0.68*
	(0.61)	(0.46)	(0.15)	(0.14)	(0.36)	(0.23)	(0.45)	(0.33)	(0.51)	(0.38)
τ = -3/-2	-0.15	-0.07	-0.02	-0.02	-0.02	0.04	0.14	-0.23	0.03	-0.12
	(0.46)	(0.40)	(0.11)	(0.10)	(0.26)	(0.21)	(0.35)	(0.33)	(0.34)	(0.28)
τ = -1	.	.	.	.	.	.	.	.	.	.
τ = 0/1	0.13	0.53	0.07	-0.08	0.32	0.22	0.27	0.46	0.00	0.59**
	(0.61)	(0.38)	(0.10)	(0.11)	(0.26)	(0.28)	(0.48)	(0.31)	(0.50)	(0.29)
τ = 2/3	0.75	1.62***	0.04	0.16	0.29	0.82***	1.04*	0.71**	0.42	1.16***
	(0.69)	(0.47)	(0.12)	(0.11)	(0.34)	(0.31)	(0.54)	(0.35)	(0.64)	(0.40)
τ = 4/5	1.51**	1.89***	0.34**	0.01	0.71*	1.43***	0.88	0.70*	0.6	0.96**
	(0.74)	(0.56)	(0.15)	(0.12)	(0.40)	(0.42)	(0.61)	(0.36)	(0.65)	(0.38)
τ = 6/7	1.07	0.66	0.27*	0.00	0.48	0.95***	0.72	-0.03	0.32	0.39
	(0.78)	(0.53)	(0.15)	(0.14)	(0.42)	(0.36)	(0.59)	(0.40)	(0.65)	(0.40)
τ = 8/9	1.73**	1.26**	0.32**	-0.03	0.64*	0.98***	0.86	0.39	0.49	0.59
	(0.69)	(0.53)	(0.13)	(0.13)	(0.38)	(0.32)	(0.54)	(0.41)	(0.54)	(0.37)
τ = 10	1.86**	1.25**	0.46***	0.28**	0.80**	0.65*	0.91	0.18	0.34	0.59
	(0.83)	(0.54)	(0.17)	(0.12)	(0.40)	(0.34)	(0.65)	(0.50)	(0.62)	(0.45)
States	all		all		no HE		no HE		no BY	
N	69,968		69,968		63,152		63,152		37,344	

Notes: The table reports 2SLS estimates of the effects of fiscal transfers on growth in the respective expenditure category based on variants of equation (4). The dependent variable is the per capita change in the variable reported in the top row relative to 1988. All columns control for federal state x year fixed effects. Regressions are heteroscedasticity-weighted based on a modified Breusch-Pagan test. Standard errors are clustered at the commuting zone level. Significance levels: 1% ***, 5% **, 10% *.

Table A.3: Asymmetric Effects - Revenues

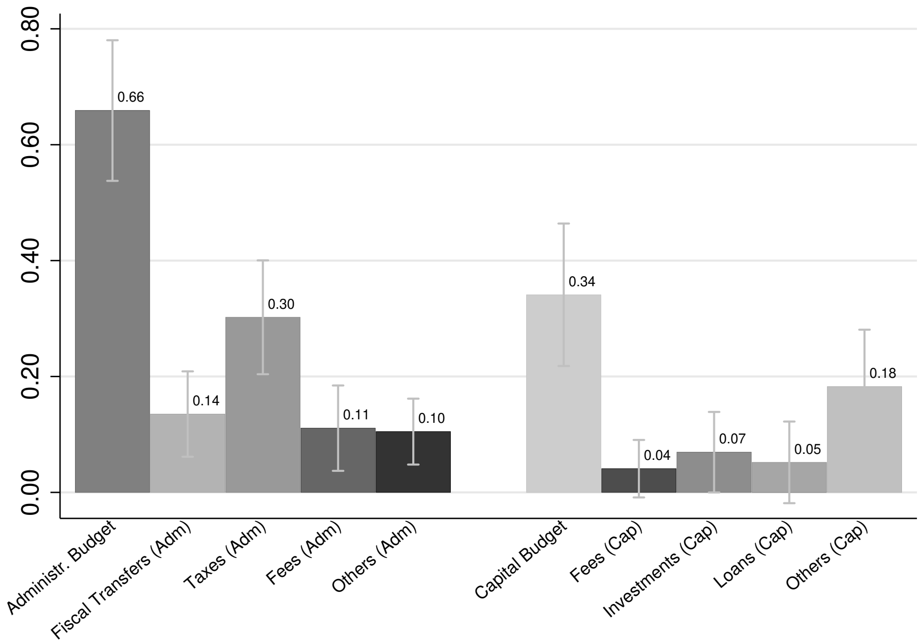
	Total		Administrative Budget							
			Total (Admin)		Taxes		Fees and Charges		Other Revenues	
	(1.1) Positive	(1.2) Negative	(2.1) Positive	(2.2) Negative	(3.1) Positive	(3.2) Negative	(4.1) Positive	(4.2) Negative	(5.1) Positive	(5.2) Negative
τ = -5/-4	0.19	0.27	0.40**	0.10	0.43***	-0.10	0.10	0.04	-0.15	0.06
	(0.60)	(0.51)	(0.20)	(0.18)	(0.16)	(0.24)	(0.08)	(0.08)	(0.09)	(0.08)
τ = -3/-2	0.53	-0.35	0.60***	-0.05	0.38**	-0.14	0.02	0.02	0.02	0.11*
	(0.61)	(0.40)	(0.20)	(0.15)	(0.16)	(0.11)	(0.07)	(0.06)	(0.06)	(0.06)
τ = -1	.	.	.	.	.	.	.	.	.	.
τ = 0/1	1.53***	1.83***	1.15***	1.27***	0.04	0.15	0.06	-0.02	-0.05	-0.14
	(0.59)	(0.43)	(0.16)	(0.16)	(0.12)	(0.11)	(0.07)	(0.05)	(0.07)	(0.12)
τ = 2/3	2.22***	3.14***	1.07***	1.46***	-0.02	0.35***	-0.06	-0.01	0.00	-0.06
	(0.67)	(0.48)	(0.23)	(0.26)	(0.16)	(0.13)	(0.10)	(0.09)	(0.11)	(0.13)
τ = 4/5	2.79***	3.11***	1.16***	1.39***	-0.11	0.23	0.03	0.02	0.06	0.01
	(0.82)	(0.52)	(0.33)	(0.34)	(0.23)	(0.18)	(0.12)	(0.15)	(0.15)	(0.16)
τ = 6/7	2.07**	1.82***	0.97**	1.19***	-0.19	0.02	0.18	-0.11	-0.05	-0.20
	(0.85)	(0.67)	(0.39)	(0.37)	(0.24)	(0.20)	(0.15)	(0.19)	(0.18)	(0.18)
τ = 8/9	3.29***	2.68***	1.25***	1.33***	0.03	0.20	0.23*	-0.10	0.02	-0.24
	(0.78)	(0.68)	(0.37)	(0.43)	(0.25)	(0.21)	(0.13)	(0.22)	(0.21)	(0.21)
τ = 10	3.24***	2.81***	1.50***	1.60***	-0.07	0.36	0.50***	-0.15	0.15	-0.08
	(0.99)	(0.77)	(0.48)	(0.51)	(0.26)	(0.30)	(0.17)	(0.23)	(0.22)	(0.19)
States	all		all		all		no HE		no HE	
N	69,968		69,968		69,968		63,152		63,152	

	Capital Budget									
	Total (Capital)		Fees and Contributions		Investments		Loans		Other Revenues	
	(6.1) Positive	(6.2) Negative	(7.1) Positive	(7.2) Negative	(8.1) Positive	(8.2) Negative	(9.1) Positive	(9.2) Negative	(10.1) Positive	(10.2) Negative
τ = -5/-4	-0.30	0.33	0.05	0.03	-0.33	-0.08	-0.55*	0.08	0.52	0.08
	(0.50)	(0.43)	(0.16)	(0.10)	(0.25)	(0.23)	(0.28)	(0.21)	(0.35)	(0.31)
τ = -3/-2	-0.05	-0.11	0.04	-0.14	-0.18	-0.22	-0.53**	0.09	0.55	-0.13
	(0.45)	(0.38)	(0.12)	(0.10)	(0.19)	(0.14)	(0.22)	(0.21)	(0.39)	(0.31)
τ = -1	.	.	.	.	.	.	.	.	.	.
τ = 0/1	0.64	0.52	0.18	0.04	-0.16	-0.02	-0.15	-0.12	0.61**	0.57*
	(0.50)	(0.38)	(0.13)	(0.11)	(0.22)	(0.17)	(0.20)	(0.19)	(0.30)	(0.33)
τ = 2/3	1.09*	1.89***	0.41**	0.23	0.16	-0.04	-0.3	0.32	1.07***	1.26***
	(0.59)	(0.54)	(0.18)	(0.15)	(0.26)	(0.23)	(0.23)	(0.22)	(0.38)	(0.41)
τ = 4/5	1.68**	2.11***	0.60***	0.56***	-0.03	0.06	0.24	0.33	0.89**	1.08***
	(0.65)	(0.51)	(0.18)	(0.20)	(0.29)	(0.22)	(0.31)	(0.24)	(0.36)	(0.31)
τ = 6/7	1.05	1.05**	0.57***	0.41**	-0.09	-0.18	0.16	0.01	0.53	0.59
	(0.67)	(0.53)	(0.18)	(0.18)	(0.29)	(0.26)	(0.34)	(0.24)	(0.37)	(0.39)
τ = 8/9	1.97***	1.47***	0.80***	0.32	-0.01	-0.14	0.25	0.23	0.73**	0.96**
	(0.64)	(0.54)	(0.18)	(0.20)	(0.28)	(0.32)	(0.26)	(0.24)	(0.35)	(0.38)
τ = 10	1.67**	1.31**	0.79***	0.18	0.18	-0.13	-0.24	0.58**	0.75**	0.6
	(0.73)	(0.53)	(0.21)	(0.21)	(0.38)	(0.31)	(0.32)	(0.26)	(0.37)	(0.41)
States	all		no HE		no HE		all		no HE	
N	69,968		63,152		63,152		69,968		63,152	

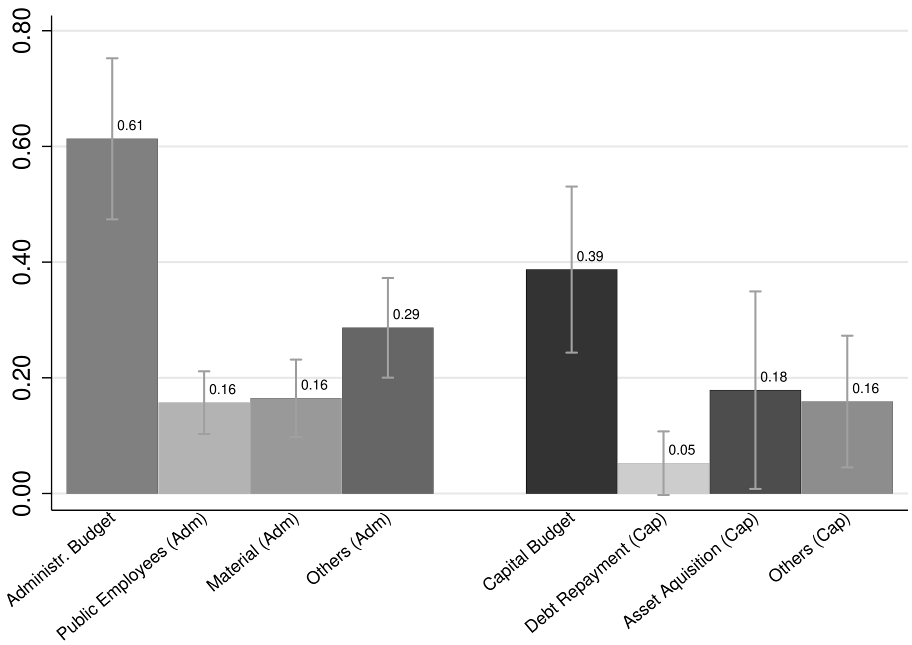
Notes: The table reports 2SLS estimates of the effects of fiscal transfers on the respective revenue category based on variants of equation (4). The dependent variable is the per capita change in the variable reported in the top row relative to 1988. All columns control for federal state x year fixed effects. Regressions are heteroscedasticity-weighted based on a modified Breusch-Pagan test. Standard errors are clustered at the commuting zone level. Significance levels: 1% ***, 5% **, 10% *.

Figure A.1: Municipal Budget Shares

A. Revenue Categories

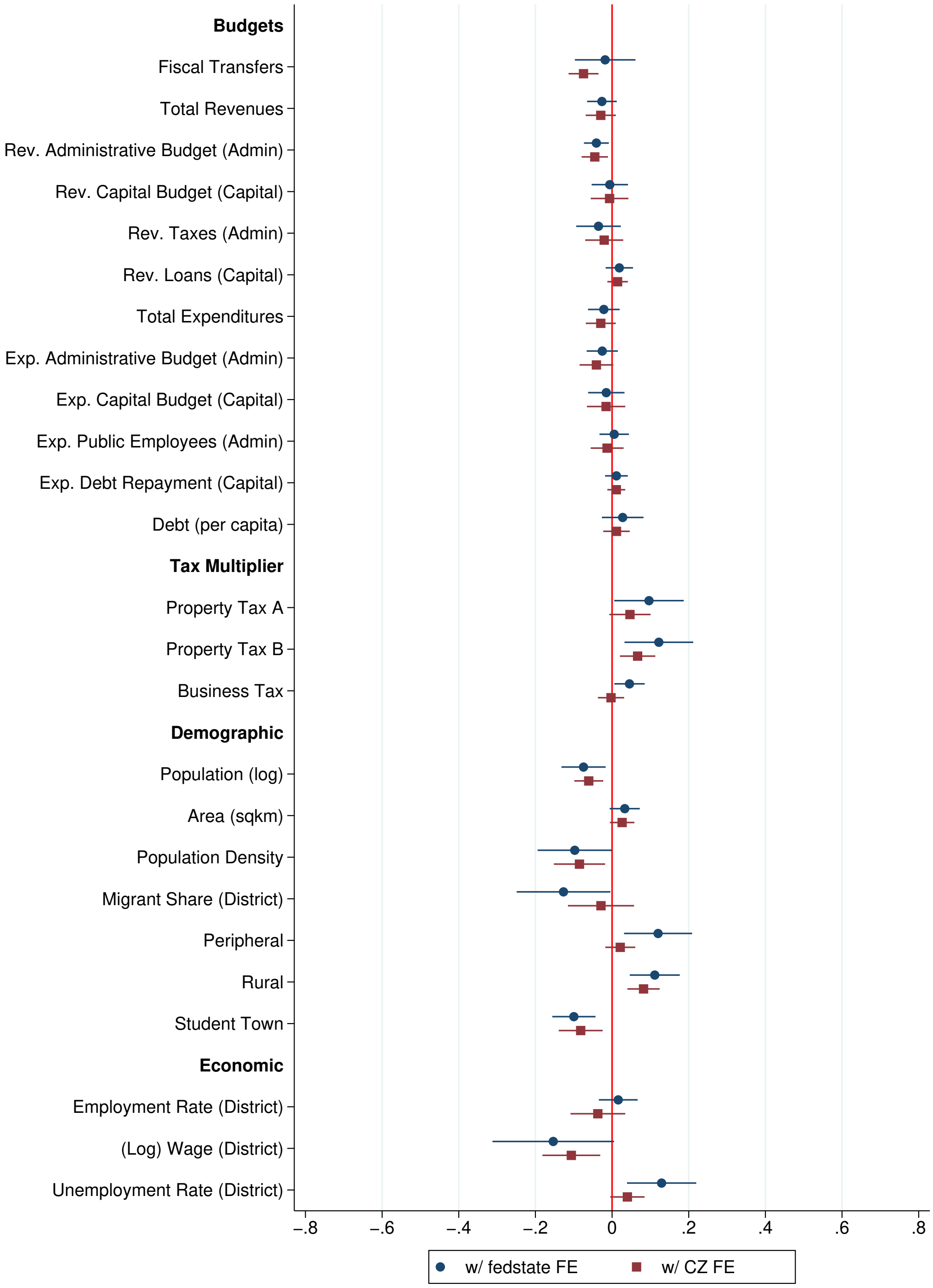


B. Expenditure Categories



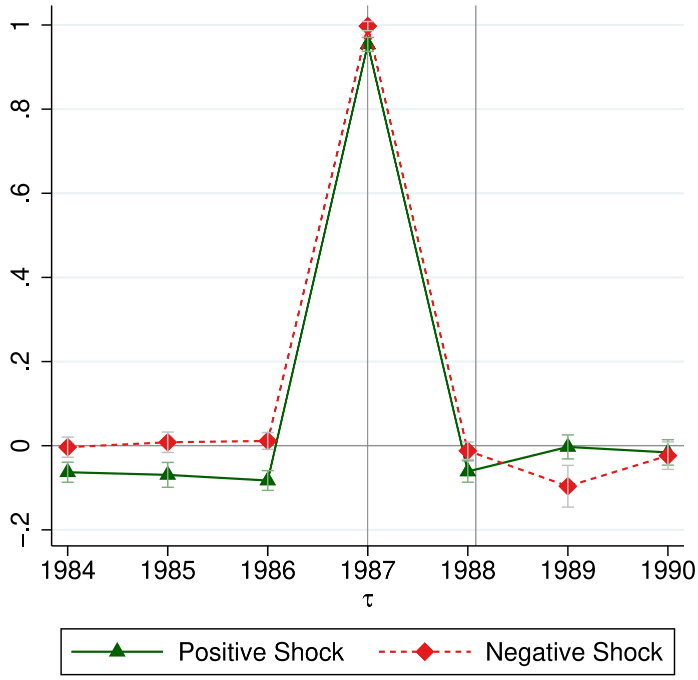
Notes: The figure plots municipal mean budget shares and standard deviations for the main sample in 1986. Panel A reports shares of the various municipal revenue budget categories, separately for the administrative budget and the capital budget, Panel B reports shares of the various municipal expenditure budget categories, separately for the administrative and the capital budget. Mean shares are weighted by municipal population in 1986.

Figure A.2: Census Shock and Pre-Treatment Levels



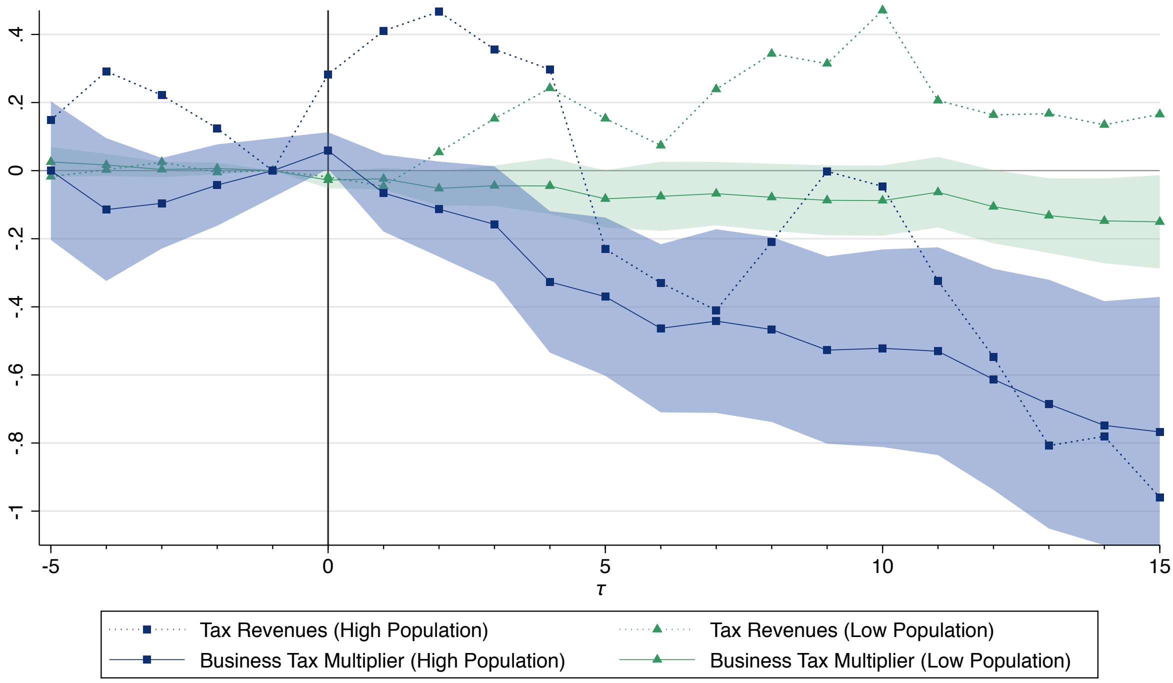
Notes: The Figure reports weighted correlation coefficients between the Census Shock in official population counts and 1986 levels in municipal fiscal, economic and demographic characteristics. Correlations are net of federal state fixed effects (blue dots) or net of commuting zone fixed effects (red dots). The horizontal bars show the 95% confidence interval based on standard errors clustered at the commuting zone level.

Figure A.3: Census Shock and Population Growth



Notes: The figure reports coefficient estimates and 95-percent confidence intervals of the relationship between the Census Shock and population growth based on variants of equation (3). Regressions are heteroscedasticity-weighted.

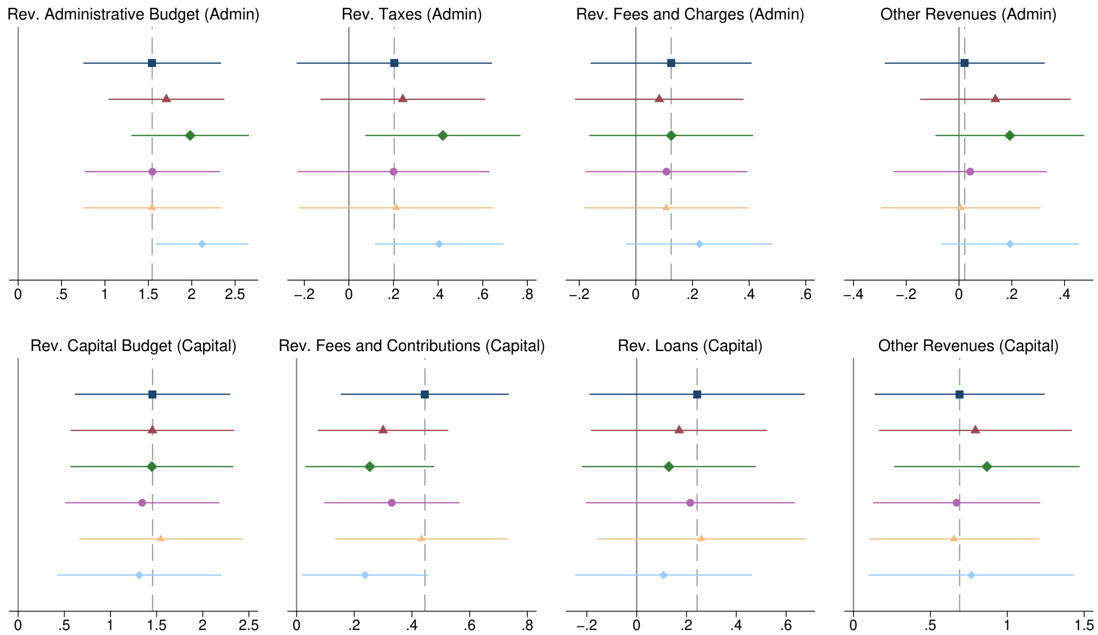
Figure A.4: Tax Multipliers and Tax Revenue - By Population Level



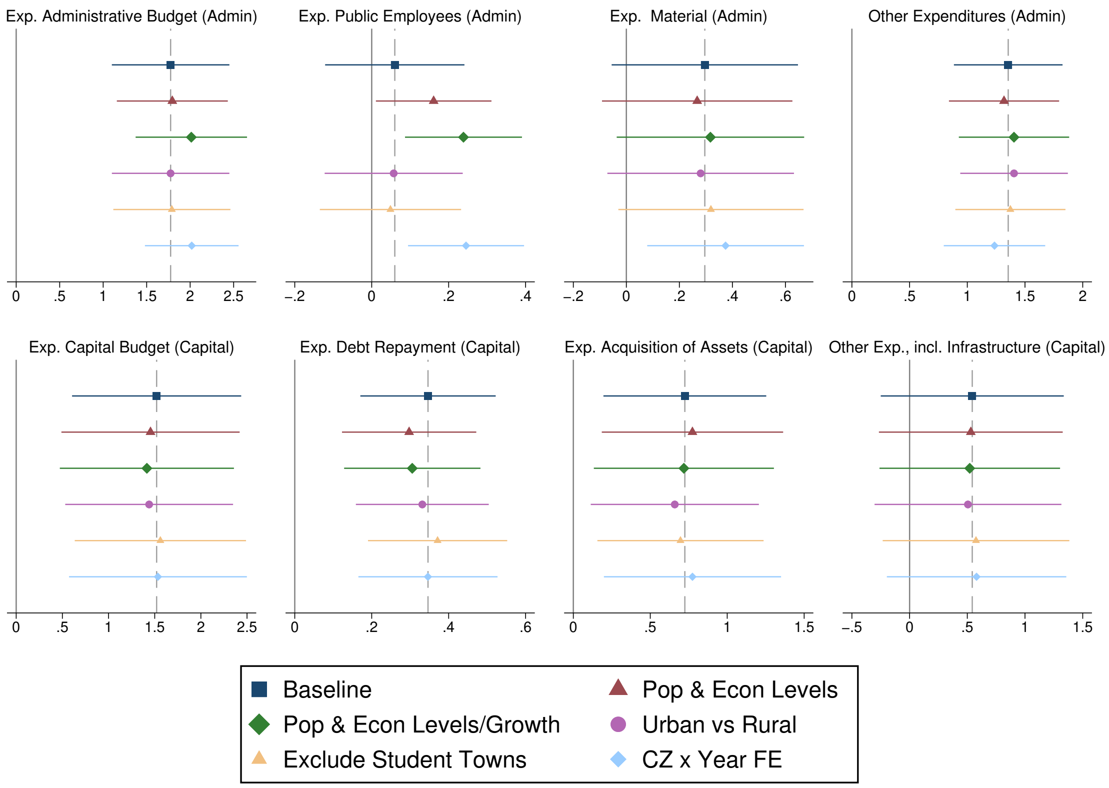
Notes: The figure plots the estimated effects of the Census Shock on tax revenues (dotted lines) and local tax multipliers (solid lines) for municipalities in the top third of population levels in 1986 (squares) versus the bottom two thirds (triangles). The effects on tax revenues are estimated by two-stage least squares, the effects on local tax multipliers are reduced-form estimates. Regressions are heteroscedasticity-weighted and the areas represent the 90% confidence intervals.

Figure A.5: Robustness I - Demographic and Economic Control Variables

A. Revenue Categories



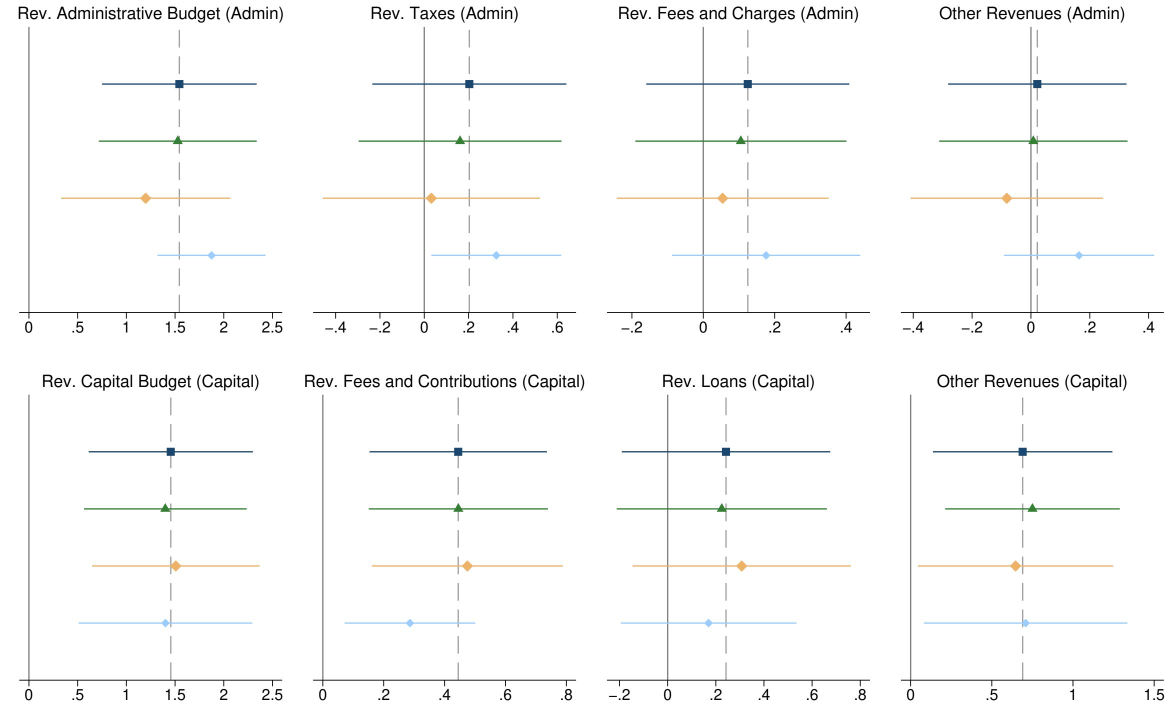
B. Expenditure Categories



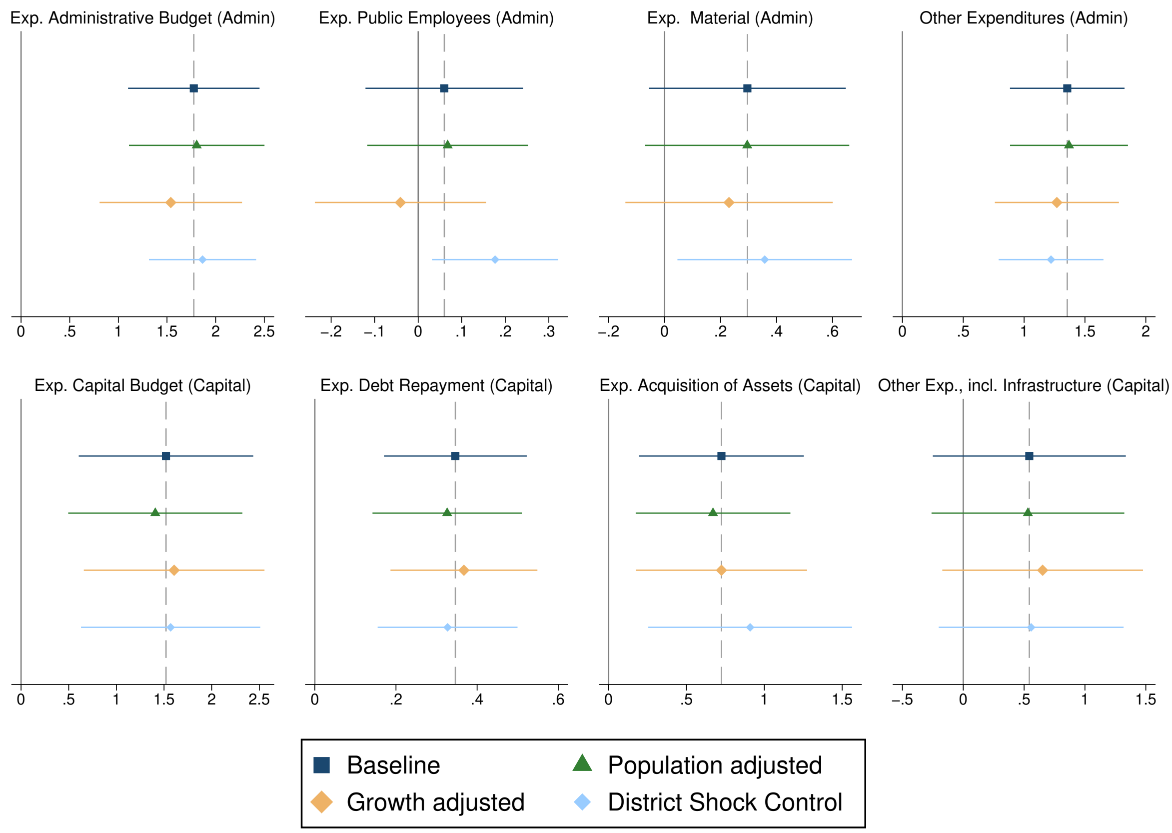
Notes: The figure reports two-stage least squares estimates and 95% confidence intervals of the effects of an increase in municipal fiscal transfers on revenue and expenditure growth in event period $\tau = 10$ based on variants of equation (4). Regressions are heteroscedasticity-weighted.

Figure A.6: Robustness II - Census Shock Definition

A. Revenue Categories



B. Expenditure Categories



Notes: The figure reports two-stage least squares estimates and 95% confidence intervals of the effects of an increase in municipal fiscal transfers on revenue and expenditure growth in event period $\tau = 10$ based on variants of equation (4). Regressions are heteroscedasticity-weighted.

B Supplemental Empirical Analyses

B.1 Definition of the Census Shock

As described in Section 3.2, we define the Census Shock as the log difference between population counts on Census day (May 21, 1987) and the last observed register based counts (December 31, 1986). In this section, we provide two alternative Census Shock definitions and test whether our results are robust to these definitions.

First, while our Census Shock definition accounts for population growth from June to December of 1987, one may still be worried that our Census Shock measure picks up population growth in the first 5 months (January to May). To account for this, we impute population growth from January to May of 1987 based on the assumption that average monthly population growth in this period is the same as average monthly population growth from June to December 1987. We then create an alternative Census Shock measure that abstracts from population growth in the first 5 months by deducting the imputed population growth from the original measure.

Second, as described in Appendix D, the assumed linear relation between the Census Shock and fiscal transfers in our baseline model represents a simplification of the actual functional relationship. In particular, the weight of each additional person in the fixed spending formula to determine fiscal transfers tends to increase with population size (see Figure D1). We construct an alternative measure of the Census Shock that accounts for this non-linearity by incorporating the state-specific functions that determine the population weights used to calculate *fiscal needs*. We then reweight both the population counts on Census day and the last observed register based counts according to these weights w_s, i.e.,

$$
\begin{aligned}
CensShock_{m,s} \quad = \quad & log\left[w_s(Pop_{i,1987,census}) \cdot Pop_{m,1987,census}\right] - \\
& log\left[w_s(Pop_{i,1986,register}) \cdot Pop_{m,1986,register}\right].
\end{aligned}
$$

Third, there are also fiscal transfers at the next higher level of government (the district level) that depend on local population counts and are consequently affected by the Census Shock. In principle, these transfers at the district level should not directly affect municipal budgets, as districts have their own budgets. One may worry however, that there are nevertheless spillovers across municipalities within a district. For that reason we provide an additional robustness check estimating a specification where we control for the weighted average Census

Shock in the other municipalities in the district. That is, we control for

$$CensShock_m^{district} = \sum_{j \in District \setminus m} w_j^{pop} CensShock_j,$$

where $CensShock_j$ is defined as in equation (1) and $w_j^{pop} = \frac{Pop_{j,1986}}{\sum_{k \in District \setminus m} Pop_{k,1986}}$ are population weights.

For our main outcome variables we present again estimates over the whole analysis period (see Figure 7D-F), while we only report the effect in $\tau = 10$ for the other revenue and expenditure categories (Figure A.6). The estimated coefficients adjusting for population growth from January to May (long dashed line) are slightly smaller than the baseline results (thick black line), but largely similar and not statistically different. When accounting for non-linearities in the relationship between the Census Shock and fiscal transfers (EWR, short dashed line), the first stage becomes slightly smaller. As we describe in Appendix D, that result is expected because the weight of each person in the "fiscal needs" formula tends to increase in population size. As such, our original Census Shock measure understates the population weighted Census Shock, which implies that the former should yield a larger first-stage coefficient than the latter. However, the 2SLS estimates in Figure 7e and 7f remain virtually unchanged, suggesting that our decision to estimate a linear relationship does not introduce any bias. Intuitively, the population weights can be explicitly integrated into the definition of the Census Shock or picked up implicitly by the first-stage coefficients (our baseline choice). Lastly, the results are also largely robust to controlling for the average Census Shock in the other municipalities in the district. If at all, this specification slightly increases precision. Interestingly, these results largely mirror the estimated effects from the specification controlling for CZ x Year FE (see Figure A.5), confirming the indicative evidence for small positive effects on tax revenues, public employment and material expenses from the main specification.

B.2 Fiscal Transfers and Local Conditions: Additional Evidence

In this section, we provide additional evidence on how the response to fiscal transfers varies with local demographic, economic and fiscal conditions. We allow for a non-linear effect by estimating a variant of equation (4) in which we interact the change in fiscal transfers with a restricted cubic spline with four knots of the respective variable (located at the 5th, 35th, 65th and 95th percentile). Specifically we estimate the following specification separately for two

two-year periods representing the short ($\tau = 0, 1$) and the long-run response ($\tau = 9, 10$),

$$\Delta Y_{m,t} = \alpha_{s,t} + \gamma_t \Delta Trans_{m,1989} + \sum_{j=1}^{3} \gamma_t^j \Delta Trans_{m,1989} \times Spline_m^j + \sum_{j=1}^{3} \omega_t^j Spline_m^j + \epsilon_{m,t}, \qquad \text{(B.1)}$$

where $Spline_m^j$ represents the restricted cubic spline variables and we instrument $\Delta Trans_{m,1989} \times Spline_m^j$ with the interaction between the Census Shock and the splines. This method allows the response of municipal budgets to vary smoothly with baseline characteristics while taking heterogeneities in the first stage into account.

We focus on heterogeneities along three dimensions: population unemployment and debt levels. The complete set of results are presented in Figures B.1 to B.3. In each of the figures we overlay the effect (and confidence interval) in period $\tau = 0, 1$ (orange) with the effect in $\tau = 9, 10$ (blue). This allows us to focus on the long-run effects while still being able to detect stark differences between the short- and long-run response.

We first analyze whether the fiscal response varies with respect to local population levels (measured in 1986). Figure B.1A shows that the Census Shock impacts transfers more in large municipalities. This result is expected, as Germany's fiscal equalization scheme assumes a higher per-capita "fiscal need" – and puts a higher weighting factor – on each inhabitant in larger municipalities (see Appendix D).[57] The long-run multiplier in revenues and expenditures is however smaller in larger municipalities. This is partially driven by lower investments into infrastructure (Figure B.1D, fourth panel), which in turn leads to lower revenues from fees and contributions (Figure B.1C, third panel). More importantly, it is driven by the stronger reduction in tax multipliers in larger municipalities (Figure B.1D). As a consequence, large municipalities experience declining tax revenues after a positive transfer shock (Figure B.1B, first panel).

We next study whether the fiscal response varies with respect to the local unemployment rate. As illustrated in Figure B.2A, the Census Shock has a stronger effect on fiscal transfers in municipalities with high unemployment (measured as the average rate at the district level between 1986 and 1988). This is expected, as the equalization formulas depend on the local unemployment rate in some states. We observe more pronounced non-linearities in total revenues and expenditures: fiscal transfers increase total revenues and expenditures in municipalities with low unemployment, but do not expand spending much in municipalities with high unemployment. This contrast is driven by differences in how municipalities use the additional transfers. Municipalities with favorable economic conditions invest most of

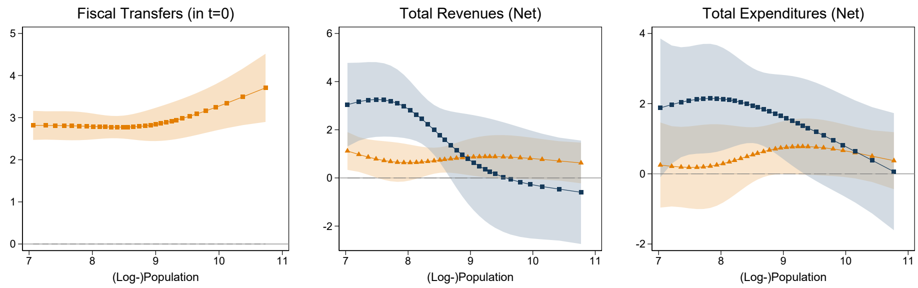
[57] Note that our 2SLS estimates take such non-linearities into account, as also the Census shock is allowed to vary with population.

the additional funds into assets or infrastructure (see Figure B.2C, fourth panel) – amplified by an increase in the take-up of loans (Figure B.2B, fourth panel). Municipalities with high unemployment instead reduce the tax multipliers, in particular the business tax multiplier (see Figure B.2D). Given that local capital taxation has strong effects on the tax base (Buettner, 2003), this reduction of the business tax may reflect an attempt to attract business, to increase labor demand and reduce unemployment.

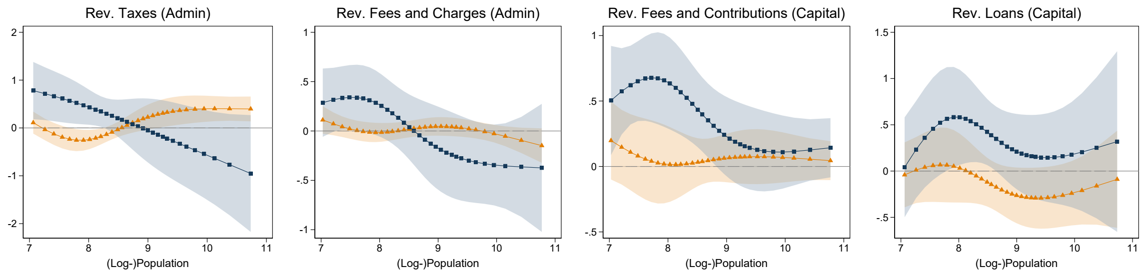
Figure B.3 presents variation in the fiscal response with respect to the pre-treatment local debt level, expressed as a share of total revenues. Because standard errors are very large below a debt share of 20 percent, we focus on the estimated effects above that level. We do not observe strong heterogeneities in the effects of the Census Shock on fiscal transfers. The short-run effect on total revenues is similar, but the long-run multiplier is larger among municipalities with low debt levels. Again, municipalities with higher debt level seem to invest less into infrastructure (Figure B.3C, right panel) and instead reduce their tax multipliers (Figure B.3D). This response seems intuitive given that tax multipliers and debt levels are positively correlated across municipalities —- municipalities may have to raise local taxes to serve the debt, but the receipt of additional transfers would alleviate that constraint.

Figure B.1: Response Heterogeneity by Population

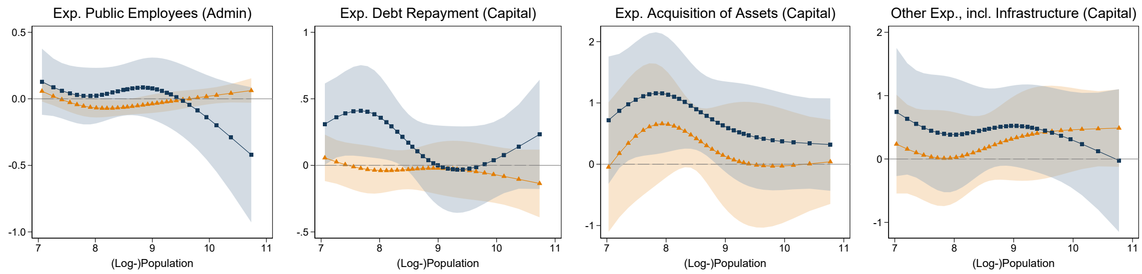
A. Transfers, Total Revenues and Expenditures



B. Other Revenue Categories



C. Other Expenditure Categories

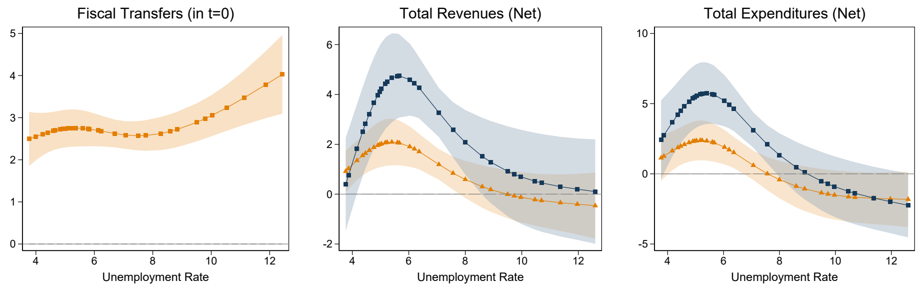


D. Local Tax Multipliers

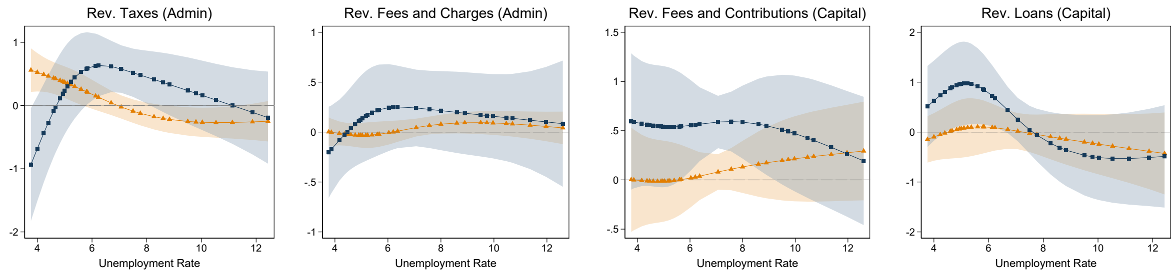
Notes: Variation in the estimated municipal response with respect to the population level (measured as share of total revenues). All estimates except the ones in Panel A, first panel, are from two-stage least squares regressions based on equation (B.1) in which the change in fiscal transfers in 1989 is interacted with a restricted cubic spline with 4 knots in the local population level (at the 5th, 35th, 65th and 95th percentile). The estimates in Panel A, first panel, report the corresponding first-stage estimates. The local population level is measured pre-Census in 1986. In each of the figures we overlay the effect in period $\tau = 0, 1$ (orange triangles) with the effect in $\tau = 9, 10$ (blue squares). The triangles and squares represent marginal effects measured at each 3rd percentile between the 5th and the 95th percentile of the distribution. The orange and blue area represent 90% confidence intervals clustered at the commuting zone level.

Figure B.2: Response Heterogeneity by Unemployment Rate

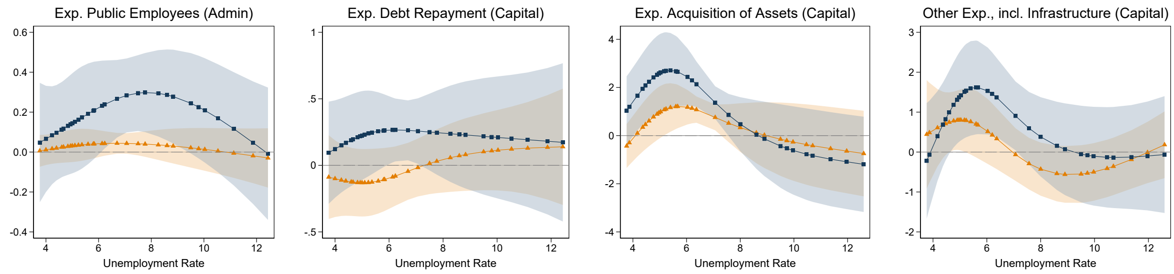
A. Transfers, Total Revenues and Expenditures



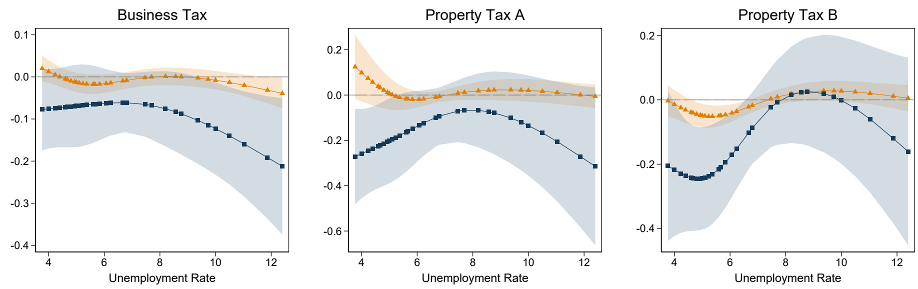
B. Other Revenue Categories



C. Other Expenditure Categories



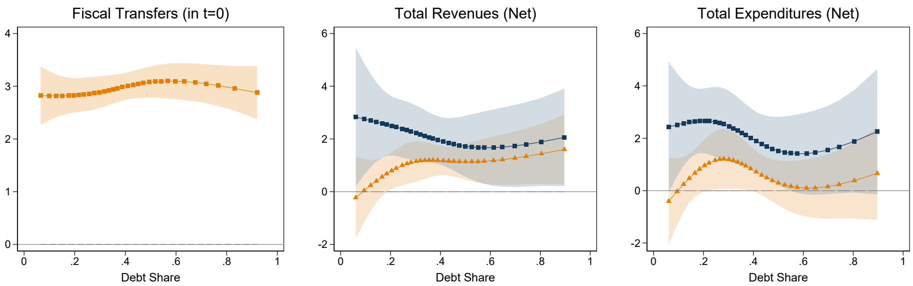
D. Local Tax Multipliers



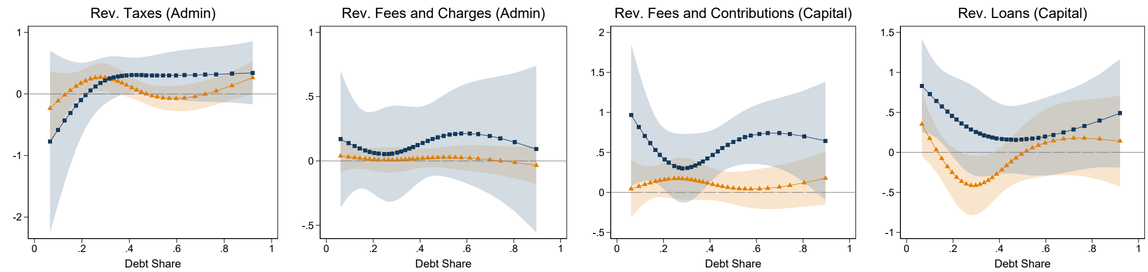
Notes: Variation in the estimated municipal response with respect to the unemployment rate (measured at the district level). All estimates except the ones in Panel A, first panel, are from two-stage least squares regressions based on equation (B.1) in which the change in fiscal transfers in 1989 is interacted with a restricted cubic spline with 4 knots in the local unemployment rate (at the 5th, 35th, 65th and 95th percentile). The estimates in Panel A, first panel, report the corresponding first-stage estimates. The local unemployment rate is measured as the pre-treatment average between 1986 and 1988. In each of the figures we overlay the effect in period $\tau = 0, 1$ (orange triangles) with the effect in $\tau = 9, 10$ (blue squares). The triangles and squares represent marginal effects between the 5th and the 95th percentile of the distribution. The orange and blue area represent 90% confidence intervals clustered at the commuting zone level.

Figure B.3: Response Heterogeneity by Debt Share

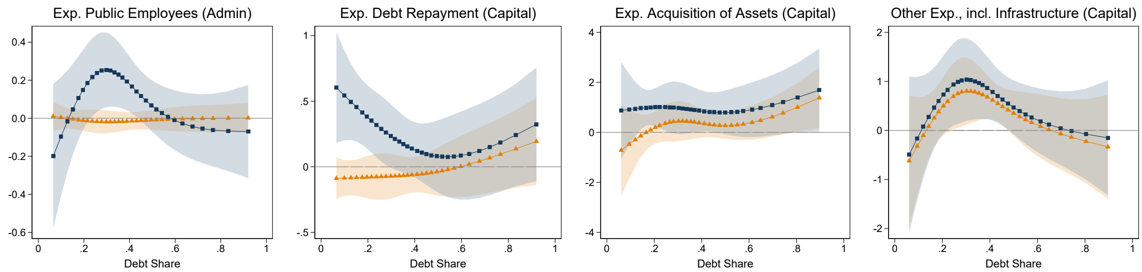
A. Transfers, Total Revenues and Expenditures



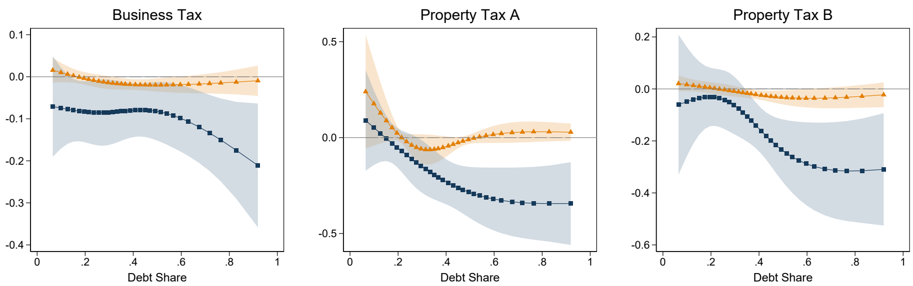
B. Other Revenue Categories



C. Other Expenditure Categories



D. Local Tax Multipliers



Notes: Variation in the estimated municipal response with respect to the debt level (as share of total revenues). All estimates except the ones in Panel A, first panel, are from two-stage least squares regressions based on equation (B.1) in which the change in fiscal transfers in 1989 is interacted with a restricted cubic spline with 4 knots in the local debt level (at the 5th, 35th, 65th and 95th percentile). The estimates in Panel A, first panel, report the corresponding first-stage estimates. The local debt level is measured as the pre-treatment average between 1986 and 1988. In each of the figures we overlay the effect in period $\tau = 0, 1$ (orange triangles) with the effect in $\tau = 9, 10$ (blue squares). The triangles and squares represent marginal effects between the 5th and the 95th percentile of the distribution. The orange and blue area represent 90% confidence intervals clustered at the commuting zone level.

C Recession and Municipal Budgets

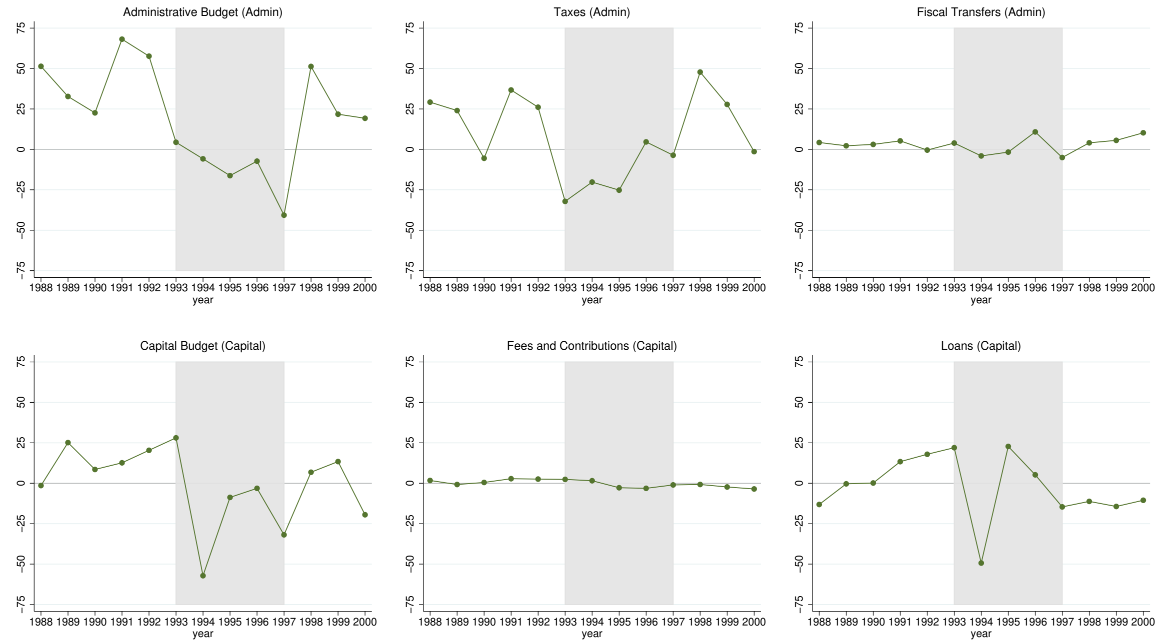
Municipal revenues are more sensitive to cyclical fluctuations than revenues from higher levels of government (Broer 2003). For the interpretation of our results it is therefore important to understand the staggered and delayed impact of Germany's post-reunification recession which started in 1993, even if the Census Shock is not correlated with the recession at the municipal level (and thus does not invalidate our identification strategy, see Section 5.3). This is because municipalities may also readjust how they allocate additional revenues they received because they gained population in the Census, when they readjust their budgets in response to the recession.

Panel A of Figure C.1 plots the average absolute growth rate of per-capita revenues in our sampled federal states over time. After a steady period of growth, revenues in the administrative and capital budgets stall in 1993, and decline in the following years. The response is staggered because different components decline at different times. Tax revenues drop already in 1993, the year of the recession and continue to decline for several years thereafter, while the take-up of loans continues to grow in 1993, before falling strongly in 1994. Instead, the reactions in fiscal transfers and revenues from fees and contributions are only small and do not contribute substantially to the overall drop in revenues. Other sources of revenues decline only from 1994 onwards.

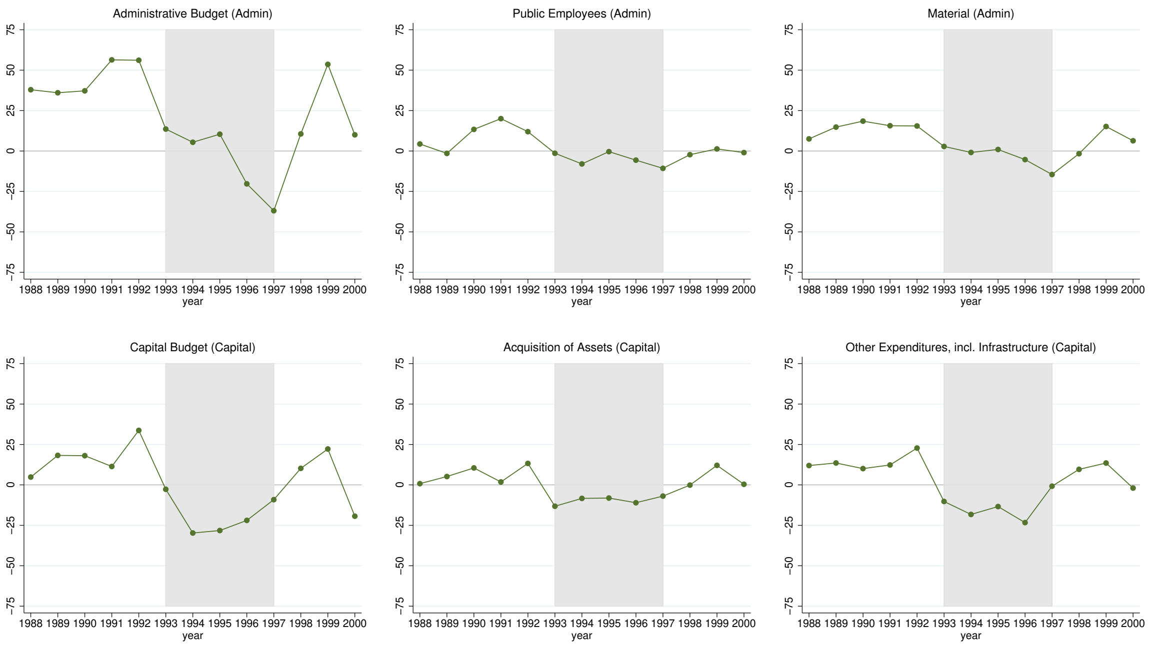
Panel B of Figure C.1 plots the average absolute growth rate of per-capita spending and its sub-categories over time. In line with revenues, the adjustment in expenditures is staggered over multiple years. Administrative budget expenditures only react delayed, with expenditures for personal stalling at pre-recession levels and expenditures for material only declining in the recession aftermath in 1996 and 1997. Instead, the capital budget reacts strongly, with investments into assets and infrastructure dropping considerably over a five-year period between 1993 and 1997. While economic growth recovers more quickly, municipal revenue and spending continue to fall, improving only from 1998 onwards. The impact of the recession on municipal budgets is therefore felt from 1993 to at least 1997, corresponding to event periods $\tau = \{4, 5, 6, 7, 8\}$.

Figure C.1: The Effect of the 1993 Recession on Municipal Budgets

A. Revenues



B. Expenditures



Notes: The figure plots the average absolute growth rate of per-capita revenues and expenditures over the years 1988 to 2000. *Acquistion of Asssets* and *Other Expenditures* missing for HE.

D Determinants of Municipal Fiscal Transfers

In this section, we describe in more detail how fiscal transfers within the municipal fiscal equalization scheme are distributed. As mentioned in Section 3.3, the bulk of transfers is distributed according to a fixed spending formula (*Schlüsselzuweisung*). The calculation of these transfers depends on four main factors: The amount of revenues to be distributed (*Schlüsselmasse*), the compensation rate (a_s), the municipality's fiscal need ($Need_{i,t}$), and the municipality's fiscal capacity ($Capacity_{i,t}$). The amount of revenues to be distributed, and therefore the compensation rate a_s, varies across federal states. For example, in 1988, the compensation rate was 0.55 in BY, while it was 1 in NRW.

A municipality receives transfers according to the fixed spending formula if the municipality's fiscal need is larger than its fiscal capacity. If a municipality's fiscal capacity instead is higher than its fiscal need, the municipality will not receive any transfers, but does not have to make payments either. This implies that

$$Transfers_{i,t} = max\left\{a_s \cdot (Need_{i,t} - Capacity_{i,t}), 0\right\}. \tag{D.1}$$

Fiscal capacity is determined by the tax base of the local business tax (calculated by applying a hypothetical statewide tax rate) and other revenue sources (mainly the local share of the statewide income tax revenue). In contrast, the main determinant of fiscal need is the official population count.

More specifically, fiscal need is calculated as the product of a base amount $Base_{s,t}$ and weights $w_{s,t} = w_{s,t}(Pop_{i,t-2})$, which themselves depend on population size, such that:[58]

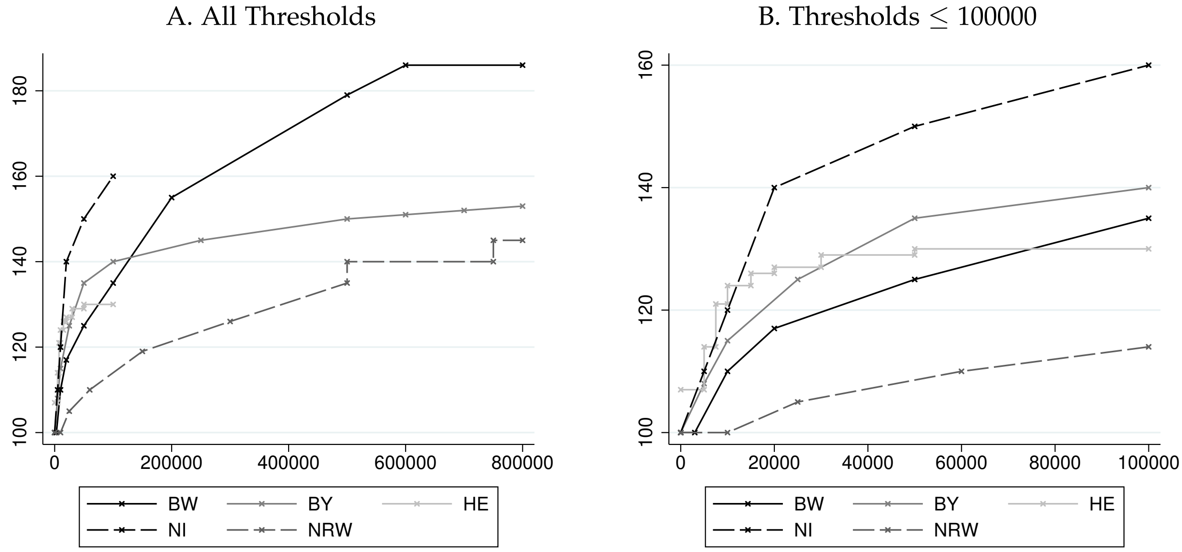
$$Need_{i,t} = Base_{s,t} \cdot w_{s,t}(Pop_{i,t-2}) \cdot Pop_{i,t-2}. \tag{D.2}$$

The base amount $Base_{s,t}$ is endogenously set such that the full *Schlüsselmasse* will be spent amongst the municipalities. Population size enters with a 2-year lag in the calculation of fiscal needs. The weights $w_{s,t}$ increase in population as larger municipalities are assumed to have a higher per-capita fiscal need than smaller municipalities, partly because they may function as a center for surrounding communities. For example, in Bavaria the weight for a municipality with 5,000 inhabitants is 108%, while for a municipality with 25,000 inhabitants each person is weighted at 125%. The specific weighting function varies across federal states, as shown in Figure D1.

The Census Shock has therefore a direct and linear effect on fiscal transfers via the change

[58] The formula in (D.2) is a simplification of the actual formula, which takes other factors into account. For example, municipalities in BY that were close to the former East German or Czechoslovakian border received a weight that was 10 percentage points higher (until 1999), while transfers in NRW also depend on the number of school pupils and unemployed. Further, in most federal states independent cities receive additional transfers. Population size is however by far the most important determinant of fiscal needs, and these other factors will not be affected by the Census Shock, as they do not depend on official population counts (e.g. the number of school pupils is taken from school registers).

Figure D1: Population Weights



Notes: The figure plots the population weights used to calculate a municipality's fiscal need within the municipal fiscal equalization scheme for the year 1986.

in official population counts $\Delta Pop_{i,t-2}$, and an additional non-linear effect via the implied change in weights $\Delta w_{s,t}(Pop_{i,t-2})$. Given (D.1) and (D.2), and ignoring the zero lower bound in equation (D.1), the per-capita change in fiscal transfers triggered by a change in official population counts equals therefore

$$\frac{\Delta Transfers_{i,t}}{Pop_{i,t-2}} = a_s Base_{s,t} \left[w_{s,t} \frac{\Delta Pop_{i,t-2}}{Pop_{i,t-2}} + \Delta w_{s,t} + \Delta w_{s,t} \frac{\Delta Pop_{i,t-2}}{Pop_{i,t-2}} \right]. \tag{D.3}$$

Because the weighting functions are known, both the linear and non-linear components in square brackets can be calculated. However, the weighting function $w_{s,t}$ has only a minor effect on the overall impact of the Census. First, while the effect of the percentage change in population in the first term in the square brackets is scaled by $w_{s,t}$, this matters little in practice. The mean absolute value of the scaled and unscaled population growth is nearly the same, and the correlation between the two variables is greater than 0.99. Second, the indirect effect from the change in weights $\Delta w_{s,t} = \Delta w_{s,t}(Pop_{i,t-2})$ in the second term in square brackets is negligible. For the federal states included in our analysis, this term contributes less than five percent to the size of the overall shock in transfers – the mean absolute value of $\frac{\Delta Pop_{i,t-2}}{Pop_{i,t-2}}$ is 2.78 percent compared to 0.16 percent for $\Delta w_{s,t}$. Because the weights $w_{s,t}$ play such a negligible role, we simplify equation (D.3) to the definition of the Census Shock given in our main text in equation (1). We confirm in Section 5.3 that this has very little consequences for our coefficient estimates.